GLEANINGS

IN

SCIENCE.

JANUARY TO DECEMBER,

1830.

VOL. II.

The Gleaners spread around, and here and there,
SPIKE AFTER SPIKE, their *scanty* harvest pick.

THOMSON.

In the knowledge of bodies we must be content to *glean* what we can from particular experiments; since we cannot, from a discovery of their real essences, grasp at a time whole sheaves, and in bundles comprehend the nature and properties of whole species together.

LOCKE.

CALCUTTA:

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1830.

TO

THE RIGHT HONORABLE

LORD WILLIAM CAVENDISH BENTINCK, G. C. B.

Governor General,

&c. &c. &c.

THE LIBERAL AND ENLIGHTENED PATRON

OF EVERY PROJECT CALCULATED TO PROMOTE AND DIFFUSE

USEFUL KNOWLEDGE THROUGHOUT THE EXTENSIVE EMPIRE

Of British India,

THIS SECOND VOLUME

OF

GLEANINGS IN SCIENCE

IS,

WITH THE HIGHEST RESPECT,

AND BY HIS LORDSHIP'S PERMISSION,

Inscribed,

BY HIS MOST OBEDIENT HUMBLE SERVANT,

THE EDITOR.

CALCUTTA,
April, 1831.

PREFACE.

We cannot permit our second volume to go forth without a few words to our readers, although we may seem to have exhausted the little we had to say, in our first volume.

A second year has given rather more maturity to our plan, and, as we predicted, increased materially the contributions to the work. But owing to an accidental circumstance, which is not likely to be again attended with the same consequences, we have been throughout the year, in arrears of the regular time of publication, and have consequently been less able to avail ourselves of the many valuable papers placed at our disposal. Besides the appearance which this has occasioned of neglecting our Correspondents' favors, it has to others seemed as if occasioned by a want of materials, so that this irregularity in publication has been, we fear, every way injurious to the work. Notwithstanding the having to struggle against this difficulty, the work has been steadily advancing, as the present list of Subscribers, compared with our former one, will serve to show.

Besides this increase of support from our Subscribers, generally, the Government have extended their patronage to the work, by considerately remitting, pending a reference to the Honorable the Court of Directors, the postage hitherto levied on it, which, though slight, was doubtless an impediment to its more general circulation, amounting, as it did, to one-fourth the price of the work. We are not the less sensible of, or grateful for the liberal views manifested on this occasion, by their conditional nature, inasmuch as we recognise the difficulty of making an exception in favor of one publication, amongst so many, unless it could be shown, that the Government had an interest in doing so. As the price of the boon granted to us, we have undertaken the task of digesting and publishing, free of expense to Government, the many valuable contributions to Geography, and general science, known to exist in the public offices of this Presidency. These papers will, we doubt not, raise the character of our work; they have been hitherto virtually lost to the public, and would soon, in a climate, the influences of which are so destructive, be so altogether, but for this judicious resolution. We shall thus derive a twofold benefit from the enlightened and liberal views of Government; and as we intend to devote any surplus arising from the work, to its improvement, we hope to make it, every year, more and more worthy of the attention of the public. In particular, we shall devote our attention to reports of the progress of European science, and large extracts from the European journals. Being no longer confined to the narrow compass of 32 pages, we shall be more able to do justice to this part of our Editorial labours.

While thus adverting to the increase of patronage, public and private, which it has been our good fortune to experience, it would be unpardonable to omit our acknowledgements for the notice taken of our labours by the Class of Natural History and Physics of the Asiatic Society. Besides subscribing liberally to the work, they have selected it as a vehicle for publishing all the minor communications made to them, the ephemeral interest attaching to which, renders them unsuitable to publication in the regular volume of their Researches, on account of the necessary delay of printing a large work. These will, then, be given, in future, in our pages, and they will, we are confident, be found greatly to enhance the interest and value of our work, while we doubt not, that in return the authors, finding their views given quickly to the public, will thereby be stimulated to fresh exertions; a reaction being thus established, which may have a beneficial effect on the Society itself, and add energy and interest to its proceedings.

Of the great utility of periodical publications, few, in the present age, have any doubt. Were any proof wanting, the extraordinary progress of European science, since these publications have become more general, would be sufficient. To construct, from our own materials, a complete treatise on any subject, however limited, falls within the ability or industry of few men. But to cast a solitary ray of light on any particular branch of a subject, is within the power of all, and the inclination of many. To collect and retain these scattered rays, till they can be combined into one strong focus of light, is the object of works devoted to periodical publication. Where such are fully established, and ably conducted, knowledge must be progressive, cannot be retrograde; every particular is laid up for use, and the philosopher, when he proceeds to his task of framing theories, has thus a rich storehouse of facts from which to draw his illustrations of new views, or refutations of the old ones. Any one, who will refer to the many volumes of scientific journals, published within the last 20 years, must be struck with the mass of information they contain, which requires but to be methodised, and arranged; to be but freed from its dross, and smelted down, to exhibit the rich metal of real science.

Another great advantage secured by periodical publication, in a cheap form, is the general diffusion of knowledge, and the consequent application of it to the common purposes and business of life. The time is past, when it was thought, that science was too great a mystery, to be within reach of the bulk of mankind; when the philosopher, in the pride of his occult knowledge, exclaimed, *Procul este profani*. That knowledge which is only fit for the inquiries of the speculative in his closet, and has no reference to the general wants, whether moral or physical, of mankind, is little thought of in the present day. Utility, in its enlarged sense, is the grand test; and as that which is known to few, cannot be of extensive utility, communication is one of the duties impressed upon us by the growing spirit of the age. There is something peculiarly convenient too in the periodical recurrence of short communications; the most busy may find time for their perusal, while those least disposed to reflection or study, are lured into an attention they had otherwise never manifested. Information is thus acquired almost unconsciously, by those who would otherwise startle at the very name of science, forgetting, that after all, science is but the knowledge and classification of facts; and that he who can perceive the connection of two or more facts, and can separate that which is common to them, is so far acquiring science, and a knowledge of first principles. In this, as in all other cases, men have been imposed upon by words; and it is not one of the least benefits attributable to these works, that they have, in some degree, tended to introduce just notions on the subject, and to familiarize us with discussions which were vulgarly considered to be beyond the reach of ordinary readers.

It will be obvious that we do not claim such high merits for our humble publication. Its title sufficiently indicates its character, and the views with which it has been projected. And if its tone appear to correspond less than could be expected, with the general views we have alluded to, it will, we doubt not, be taken into consideration by the candid, that it is a first attempt in a country where, till lately, every first attempt was certain to fail; that it has been carried on by the unassisted means of a private individual; that the editorial duties are conducted by one who has other and large claims on his time; and finally, that with the utmost possible encouragement we could look to, funds could never be realized so as to admit of remuneration for able contributions. When, therefore, it is recollected, that the Proprietor, Editor, and Contributors, are all volunteers, who neither look to gain by the sale of the work, nor receive any pecuniary return for their assistance, it will, perhaps, be admitted, that if we fall short of accomplishing all that periodical publications, under more favorable circumstances, have effected in other countries, we have at least some excuse, for our failure. It is something to have established a point of connection amongst those who take an interest in the pursuit of useful knowledge, and to have offered to them a means of recording any facts they may become acquainted with, and which, otherwise, from the dispersed state of European society in this country, and the little communication amongst us, would be lost. If it be conceded to us, that we have effected thus much, it is all that we claim ; all that we ever expected to perform. Hereafter the work may attain a maturity which may entitle it to the notice of the scientific public in Europe.

We have added considerably, in the present volume, to the list of subjects treated in our last, and yet how many of the greatest importance remain untouched. To give a list of the desiderata in Indian Science, is an attempt we are unequal to; but it may not be amiss to glance at some of the more useful tasks, which many of our Correspondents, we doubt not, have the means of performing. A good account of the timber trees of India is much wanted. A reversed vocabulary of Indian plants, of which the list of synonimes in our June number might form the basis, would be very useful. Chemical examinations of the several oils, resins, gums, and gum resins of India, would be useful, if only to attract the notice of chemists in Europe to a subject full of curiosity and interest. Of Geographical information, as of Geological, we have as yet failed to collect the ample stores we had hoped at our outset to become possessed of. Many of our readers must possess details of this kind, which it is a subject of regret should be lost to the world. We would also suggest the subject of Biographical notices, as one worthy of attention. If to these be added the many questions springing from the two great departments of practical science,—public works and manufacturing chemistry,—we think those who wish to aid us need never be at a loss. We would also invite the communication of popular views of the several branches of science, as well as historical statements of the progress made in any particular department; notices of new views, or recent improvements, &c. &c.

We must not conclude without offering our thanks for the liberal mention which the conductors of the daily and weekly press have continued to make of our labours. With our limited funds we found we could not afford the charge of advertising the work, so as to give it a fair chance of becoming generally known; we are, therefore, the more beholden to them for their very considerate notice of it, in a prominent part of their publications, to which circumstance, we doubt not, is mainly attributable the great increase in our subscription list since the publication of our first volume. Their favourable opinion of our humble efforts, next to the support of our Correspondents, has been our chief incentive to persevere in the task we had undertaken.

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No. 15. MARCH.

CORRIGENDA.

Page 2 Line 22 *for* ' objections,' *read* ' objection.'
3 ,, 3 *Note* 4, *for* ' 20,' *read* ' 20″.'
— ,, 12 —— 5, *for* ' observations,' *read* ' observatories.'
4 ,, 20 *for* ' points,' *read* ' point.'
16 ,, 9 *from b. for* ' 1, 7,' *read* ' 17.'
18 ,, 16 *for* ' 60°' *read* ' 6°.'
— ,, 23 *from b. for* ' medical,' *read* ' medicinal.'
20 ,, 23 ——— *for* ' fitted,' *read* ' filled.'
21 ,, 19 *for* ' on connait,' *read* ' on ne connait.'
29 ,, 7 *from b. for* ' waters,' *read* ' water.'
30 ,, 20 *for* ' in, ' *read* ' of.'
— ,, 36 *for* ' palace,' *read* ' place.'
31 ,, 28 *for* ' is,' *read* ' are.'
32 ,, 5 *dele h.*
33 ,, 15 *for* ' to or from, with,' *read* ' to or from, which.'
36 ,, 24 *for* ' one of which,' *read* ' of one of which.'
37 ,, 7 *supply* comma *after* ' is.'
— ,, 31 *for* ' chocked,' *read* ' choked.'
39 ,, 14 *for* ' ponton,' *read* ' pontoon.'
— ,, 27 *from b. dele* ' a' *before* ' secondary strata.'
40 ,, 5 ——— *for* ' or,' *read* ' on.'
45 ,, 12 *for* ' embedded,' *read* ' imbedded.'
51 ,, 1 *for* ' rises,' *read* ' rise.'
— ,, 3 *for* ' its,' *read* ' their.'
53 ,, 6 *from b. after* ' proportion,' *substitute a period for the colon.*
56 ,, 28 ——— *after* ' detail,' *supply a comma.*
64 ,, 28 ——— *for* ' have little,' *read* ' have a little.'
65 ,, 20 *for* ' 6. 16*s*.' *read* ' 16*s*. 6*d*.'
— ,, — *for* ' 52*d*.' *read*, 5*s*. 2*d*.'
79 ,, 15 *for* ' Bangalore,' *read* ' St. Thomas' Mount.'
— ,, — *for* ' 12° 54′ *read* ' 13° 00′.
— *Note* 2 *after October insert* ' 1800.'
92 *Line* 17 *from b. after* ' gorge,' *a comma; after* ' river,' *a semicolon.*
93 ,, 6 ——— *for* ' river's bed,' *read* ' river bed.'
95 ,, 19 ——— *for* ' flour,' *read* ' floor.'
104 ,, 2 ——— *for* ' coluismn,' *read* ' column is.'
115 ,, 5 *for* ' Pusputnuth, ' *read* ' Paspatnáth.'
— *in the table, mean minimum temperature, in the column Dárjiling, for* ' 54' *read* ' 57.'
116 ,, 16 *for* ' doled,' *read* ' dozed.'
119 ,, 33 *for* ' 2294, ' *read* ' 7294.'
133 ,, 16 *for* ' bevilled,' *read* ' bevelled.'
137 ,, 25 *for* ' limit,' *read* ' limb.'
140 ,, 21 *from b. for* ' affected,' *read* ' effected.'
141 ,, *last line, for* ' 14°, 5′,' *read* ' 14°,5.'
142 ,, 23 *for* '78°, 5,″ *read* ' 78°,5.'
144 ,, 2 *from b. for* ' marly,' *read* ' marles.'
— *for* ' of the loose,' *read* ' or the loose.'
— *for* ' calcared, argillaceous,' *read* ' calcareo-argillaceous.'
147 ,, 13 *for* ' crop,' *read* ' cross.'
150 ,, 9 *from b. for* 3000,° ' *reaa* ' 300°.'
156 ,, 32 *for* ' elements,' *read* ' element.'
158 ,, 4 *from b. for* ' bricks,' *read* ' brick.'
159 ,, 25 *for* ' Puhárpúr,' *read* ' Pahárpúr.'
— ,, 27 *for* ' compost,' *read* ' compact,'
— ,, 17 *from b. for* ' musjids,' *read* ' masjids.'

Page 162	Line	28	*for* 'salt,' *read* 'salts.'
—	,,	18	*after* 'furnish,' *insert* 'himself with.'
163	,,	21	*for* 'grates,' *read* 'bars.'
—	,,	12	*for* 'and,' *read* 'which.'
170	,,	23	*dele comma after* 'surface.'
172	,,	18	*dele comma after* 'tumour.'
—	,,	25	*from b. dele* 'the,' *after* 'from.'
174	,,	11	——— *for* 'vertebra,' *read* 'vertebræ.'
175	,,	8	*for* 'inner vertebral,' *read* 'inter-vertebral.'
179	,,	12	*from b. for* 'deep,' *read* 'steep.'
—	,,	5	——— *for* 'master,' *read* 'muster.'
183	,,	19	——— *for* '8643,' *read* '4643.'
195	,,	5	——— *for* 'such much,' *read* 'so much.'
209	,,	8	*Note, for* 'suppose,' *read* 'believe.'
213	,,	14	*for* 'priesthood,' *read* 'priests.'
216	,,	25	*from b. for* 'people,' *read* 'the people.'
218	,,	6	*for* 'water line,' *read* 'the water line.'
224	,,	23	*for* 'tentaculæ,' *read* 'tentacula.'
—	,,	7	*from b. for* 'Dr.' *read* 'do.'
226	,,	33	——— *for* 'between,' *read* 'amongst.'
231	,,	19	——— *for* 'Mr. Baggage,' *read* 'Mr. Babbage.'
232	,,	30	*for* 'regatded,' *read* 'regarded.'
—	,,	26	*for* 'M. Enche,' *read* 'M. Encke.'
—	,,	10	*for* 'observations in this case;' *read* 'observations; in this case.'
237	,,	12	*for* 'or,' *read* 'of.'
247	,,	17	*from b. for* 'variety,' *read* 'vanity.'
254	,,	9	*for* 'chamfired, *read* 'chamfered.'
—	,,	24	*for ditto, read ditto.*
271	,,	15	*for* 'agriculturalists,' *read* 'agriculturists.'
—	,,	19	*from b. for ditto, read ditto.*
—	,,	18	——— *for* 'proprietory,' *read* 'proprietary.'
274	,,	20	——— *for* 'thati n' *read* 'that in.'
287	,,	13	*for* 'largest,' *read* 'abscissa.'
295	,,	29	*from b. for* 'English-spirited,' *read* 'English; spirited.'
314	,,	18	——— *for* '$Am^t + \beta t^2$,' *read* '$Am^{\alpha t} + \beta t^2$.'
—	,,	17	——— *for* 'm & β,' *read* 'm, α, and β.'
319	,,	31	*for* '$m\,a^{at} + \beta t^2$,' *read* '$m\,a^{\alpha t} + \beta t^2$.'
337	,,	25	*for* '$= (t)$,' *read* '$= \phi\,(t)$.'
—	,,	12	*from b. for* '$m\,a^t\,(a^t-1)$,' *read* '$m\,a^t\,(a^t-1)$.'
338	,,	5	*for* '2,073,' *read* '2,037.'
339	,,	14	*for* '(a^t-1),' *read* (a^t-1).'
340	,,	27	*for* '$m\,at$,' *read* '$m\,a^t$.'
342	,,	5	*for* '$(at-1)$,' *read* '(a^t-1).'
—	,,	26	*dele second V.*
—	,,	29	*for* '$(at-1)$' *read* '(a^t-1).'
—	,,	30	*for ditto, read ditto.*
350	,,	19	*after* 'sketch' *supply* (see Pl. VII.)
353	,,	10	*from b. dele* 'be.'
355	,,	2	*for* 'Longitude,' *read* 'Logarithmic.'
—	,,	17	*for* '$2n-1$,' *read* '2^n-1.'
371	,,	14	*for* 'd,' *read* 'a.'
373	,,	13	*in the numerator and denominator both—for* '$t\,(b-1)$,' *read* '$_t(b-1)$.'
381	,,	25	*for* 'the shots dispose themselves,' *read* 'the shot disposes itself.'
388	,,	21	*from b. for* 'Mr. Telfier,' *read* 'Mr. Telfair.'

LIST OF PLATES,

WITH DIRECTIONS TO THE BINDER.

Through inadvertence Plate I. was not numbered, and Plate II. *et seq*, with the exception of V. were consequently numbered erroneously. Plate VI. was, from another circumstance over which the Editor had no controul, numbered Plate III. Vol. III. but in consequence of the delay in preparing the Title page and Index, it is now restored to the volume to which it properly belongs. As each plate is referred to the paper of which it forms the illustration, this circumstance can occasion no inconvenience to the reader, who is requested, however, for his own satisfaction, to alter the numbering as above indicated; the references to them may also be corrected.

GLEANINGS

IN

SCIENCE.

No. 13.—*January,* 1830.

I.—*On the general Principles of Geodesy, and on the several methods by which may be constructed a Map of any country.*

1. The object of every map may be stated to be a representation, on a flat surface, of a portion of the earth, on which all the lines or distances shall, as far as the difference of the surfaces permit, bear the same proportion to one another as those in nature do. Accuracy is of course essential to it; but the value of accuracy is like that of other things, comparative, and is always to be judged of by the cost of its production. Perfect mathematical accuracy is as unattainable as it would be useless, but that degree of it which is likely to be practically useful is fortunately within our reach.

2. In considering the subject of transferring lines given in position, to a plane on a reduced scale, TWO GENERAL METHODS present themselves. In the first, one line being assumed, another is linked to it; a second to that; and then a third; and so on, till the whole subject be filled in. In the other, each point is considered independent of every other; being referred to two lines given in position, by their distances, from which, its place is determined. The first has evidently a tendency to produce an accumulation of error. The second not so; as each point being fixed independently of every other, if an error be committed in any position, it is not transferred to another. The first may be called the trigonometrical[1] method, as depending on the description of triangles; the second, the method by coordinates.

3. The latter is then evidently superior, abstractedly considered. But in practice we continually find that we must sacrifice to circumstances, and that very often the most promising projects in theory are either impracticable or at least barren of the advantages expected from them. The excellence of the principle in the method of coordinates is so counterbalanced by objections that could not be seen *a priori*, that it becomes necessary to modify our opinion of its superiority. The character of either of the methods, practically speaking, is conditional; and depends on so many considerations, that it is necessary to take a full view of all the circumstances of the case in which we are to decide, before we can say which is the preferable one. These considerations will generally have reference, 1. To facility of operation. 2. To the degree of accuracy required. 3. To expedition, in which is contained also the question of economy. I shall consider each of the methods in these three lights, and shall begin with the trigonometrical.

§ 1. *Trigonometrical Methods.*

4. In this method, the points are determined by choosing them so, that on connecting them by lines, the surface in question shall be covered with a net-work of triangles. And it has been found, that when these triangles are required to be extended in every direction, the most advantageous arrangement is that in which they shall be as nearly as possible equilateral. The whole problem then resolves itself into the construction of triangles similar in position and species[2] to a given series. This problem is easily resolved by the 22nd prop. of Euclid. b. i. in which he shows that the base of the triangle being given, the summit is easily found if we know the sides, by drawing circles from each extremity of the base as a centre, and with radii equal to the given sides. The intersection of the circles is the summit of the triangle.

[1] From *trigón*, a triangle; and *metron*, a measure.
[2] A triangle given in species has its angles or sides given.

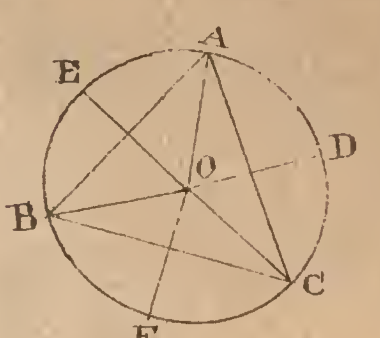
5. The object is then to obtain the length of the sides ; in other words, the distance of every pair of points required to be mapped. This may be effected two ways ; 1st, by measuring, directly, those distances by the application of the linear unit ; or secondly, by measuring only one of them, and determining the inclinations to that of the two lines joining, to form the summit of the triangle, of which the measured line was the base ; in other words, the angles of the triangle. The same operation is pursued with respect to the next triangle, except as to the measurement of one of its sides, which is supposed to be obtainable from the preceding operation. The measurement of angles, is thus substituted for the measurement of lines. A third or mixed method may be stated to consist in the combination of both.

6. The first or actual measurement of all the lines would be an excessively laborious operation, and by no means a correct one. It is indeed found that one of the most difficult problems to execute is that of measuring with accuracy a given distance on the surface of the earth ; and to be at all successful, we must have the choice of the ground, and we shall even then find it to be a most tedious and troublesome one. It is true, the error on each measurement might not be so considerable as to be of moment, singly considered ; but in a method in which each error is transferred to the next result, thence to be carried with any accession of error to the following, it is obviously inadmissible. The method of direct measurement can only be applied with advantage to the principle of coordinates ; where the positions are independent of each other ; supposing, I mean, the tediousness of the operations to be no objections. To their application to triangles we object on two grounds ; 1. The great labour and delay ; and 2. The great accumulation of error they would cause without any means of check or discovery[3]. To counterbalance these it does not offer the shadow of an advantage.

7. It is otherwise with the measurement of the angles or inclination of the sides. It is an operation easily, and most conveniently performed ; while from the great perfection of the instruments used, it is to be executed to a degree of accuracy, which would astonish the uninitiated. In strictness, it is a kind of measurement on a small scale, which depends chiefly for its application on the facility with which a straight line may be traced through the air by means of a telescope ; yet it is so disguised by the circumstances of the case, as to merit being considered a distinct method.

8. In the measurement of angles we use some kind of circular instrument, in which a telescope or pair of sights, to define the line, the inclination of which we are seeking, turns on a centre ; and by observing what proportion of the whole circuit the telescope, or connected sight-vanes, pass over when moved from the direction of one line to that of another, we obtain the means either of laying down those inclinations on paper, by means of a somewhat similar instrument ; or what is better still, we calculate the sides of a triangle having the same angles, from which, by a simple statement of proportion, combined with the known value of one of the sides, we obtain the actual lengths of the others ; and are therefore prepared to describe the circles, the intersection of which will be the summit of the triangle.

9. The manner in which the proportion of the sides of triangles is obtained, the angles of which are given, is simple enough. In the adjoining figure, the three angles at the centre of the circle, are, each, double the angles at the circumference, resting on equal segments, i. e. the angles of the triangles. Draw the Radii OD. OE. OF. bisecting the sides. It is evident that each side is then the double sine of half the angle at the centre, in other words, of the opposite angle of the triangle. So that if we have previously calculated, and arranged in a table, the values of the sines of angles, or the chords of their doubles ; we may, by inspection, perceive the proportion of the several sides to each other. This is the method by which triangles are most usually resolved.



10. We are now to enquire, what is the degree of accuracy of which the method by angles is susceptible. It has been already stated, that in reality it is a measure-

[3] It may appear to many, that the writer is here fighting with a shadow which might have been dismissed with a mere mention, did it not appear, not only to have been practised by Land Surveyors in England, but recommended by Hutton, in his Course of Mathematics (vol. ii. p. 72.) The latter fact will occasion less surprize than the former, inasmuch as it is more within general experience, that an able theorist should be mistaken in a practical question, than that a professional man, as the term is, should so lose himself. Whoever has read an able article in the Westminster Review, *On the scientific Education of the*

ment on a small scale, to be afterwards magnified to that of the map we are to execute. In proportion then to the accuracy with which the measurement on the limb of the instrument is made, in that proportion will the error of each individual result be. This error will of course vary with the price and dimensions of the instrument, and the care and skill of the observer.

11. We may obtain, however, a sufficiently correct idea of the question from the following statements. The size of the circle employed in the Trigonometrical Survey of England, was 3ft. and perhaps a larger instrument could not be conveniently used. It is then to be considered, that the longest line or greatest distance in that survey, had its distance determined by comparison made amongst the chords of that circle, the greatest of which could not exceed 3ft. and the one actually compared was, most probably, less than half that quantity. Now the accuracy with which the divisions of the circle enable us to measure a line of 3ft. is the highest measure of the accuracy with which the longest distance in the survey of England (about 64 miles) could have been determined, I mean, as far as the method of angles alone is concerned. Roughly, we may put an inch for two miles, and consequently one thousandth of an inch will be equal to 3,5 yards. But one thousandth of an inch is a quantity many times greater than what is supposed to be the error of good divisions[4] This is, however, the accuracy obtainable from the largest means, and the greatest facilities in every way[5].

12. If we take a circle of one foot diameter, which may be considered the instrument of a second rate operation, we shall have one thousandth of an inch, answering to 10½ yards on the same distance. With third rate means, such as good engine-divided five inch theodolites, the possible error might be 25 yards. But in using instruments of this size the triangles would scarcely be so large, in which case, the error being always proportionate to the lengths of the sides, would be less. If these were but 30 miles, it would be 12½ yards; if 20, 8; if 10, 4. And though in this case there would be an increase in the number of errors, *(which*

Upper Classes, will be at no loss, however, for reasons to support the opposite opinion.

[4] *Instrument* makers assert, and *many* of their purchasers believe them, that divisions on a circumference of 1ft. diameter will be true to less than 10″ about $\frac{1}{4000}$ of an inch. In engine divided instruments, 20′ is taken as the possible error, which on a diameter of five inches is about the same quantity. But that these are favourable representations, will be obvious to any one who will consider the following results of an examination of a Troughton's circle of 10 inches diameter, made with the greatest care, and comprising 162 readings of the Nonii. From the examination it appeared

1mo. That the uncertainty of the readings on any one of the three Indexes does not exceed 10″, and is generally much less.

2do. That as long as the Indexes are moved in the same direction, the angular distance remains the same; or in other words, that they do not separate or approach by bending.

3to. That the errors of division, on a mean of three Indexes, amount to about 30″ or 40″ the difference of the same nonius, supposing one a standard, running to 2′ 30″.

4to. That at the same place, the readings for several degrees on each side of these anomalies appear to be pretty regular.

[5] The distance of Beachy Head from Dunnose in the Isle of Wight (vide Trig. Sur. of England and Wales, vol. 1, p. 288.) as given by different series of triangles is as follows:

Yds.
113131 ,5
1 ,7
3 ,1
3 ,8

The extreme difference here is only 2, 3yds. instead of 3,5; but it is to be considered, that not one of the four may be quite correct, and the extreme error *may*, therefore, still amount to the quantity indicated. It *probably* exceeds it.

Since writing the above note, I perceive in the account of operations conducted by Captain Kater, for the purpose of connecting the observations of London and Greenwich, (Gl. Se. p. 153) in which several of Gen. Roy's measurements were repeated, the extreme differences are between ,0001 and ,00003 of the distances; the first would be on the preceding distance 11 yards, the latter 3,3. These differences do not include any error of the case whether as regards measurement or comparison of the measuring unit with the standard, as both triangulations depend on the same determination of the same distance. They are chiefly attributable to uncertainty of the angular observations, either from imperfect bisection of the object, imperfect adjustment of the instrument, lateral refraction, or incorrect divisions. If we suppose General Roy's extreme errors to be three times as great as Captain Kater's, which is perhaps as favorable a supposition as we have a right to make, this would still authorize our attributing ,000015 as the extreme error of the most perfect operation, conducted with the most consummate skill. This is about the proportion I have assumed above.

increase, would be in the proportion of the above diminution in the value of each) yet as the errors might lie different ways, it by no means follows that the aggregate error, on the whole, would be equal to the average error multiplied by the number of results. The consequence usually expected, is, that it should be, at the very most, equal to half this quantity ; and in many cases it would be much less. So that a greater number of smaller triangles, measured by a something inferior instrument, might give equal accuracy with a smaller number of larger triangles, measured with a better instrument. The increase of labour prevents this principle being applicable to any great extent in practice.

13. In art, 8. I stated, that either the angles observed in the field could be laid down by the intervention of a protractor on the paper, or the radii of the intersecting circles might be calculated by the assistance of the theorem in Art. 9, and the tables founded thereon. There is still a third method that ought to be noticed. It is that in which the inclinations of the several lines are drawn on a table in the field at the moment of observation, by means of a ruler so fitted up that its edge is in a vertical plane with the line of sight connecting the two points. This is the method called plane table surveying.

14. Surveying with the plane table is not susceptible of equal accuracy with the preceding method, and for the following reasons. The paper is an uneven surface, and the pencil by which the line of inclination is drawn, has its points constantly varying. It is not then possible to draw lines with equal accuracy with such materials as is done by the instrument makers on brass, with the assistance of a graver[6]. But yet in the case in which angles observed in the field are laid down with a protractor, in this case, I think, the plane table may compete with observation by circular instruments, and indeed, I think it pretty clear, that in numerous cases, the plane table affords *as much accuracy as we are in search of*[7], while it has in every case a facility and convenience to recommend it, rendering it highly deserving of attention. It is no small advantage, the method of check it affords at every step, by treble and multiple intersections. And we think, that as yet this instrument has not been done justice to. For correct detail it has no equal.

15. The mixed method requires but few words. In this the inclination of some of the lines are observed, but not of all ; and to supply this deficiency, measurements are also made. This method, to be even tolerably accurate, requires great labour, and for the reasons mentioned in art. 6 ; and as it is, even then, destitute of any general principles of check[8], it is scarcely entitled to any consideration, excepting as being applicable to cases in which other methods cannot be resorted to, or as affording means of filling up detail, as when performed without scrupulous attention to accuracy, it is perhaps more rapid in operation than any other. But in this case, it should be confined within narrow limits, and used merely for filling in detail. To employ it as a means of check or verification is to mistake its nature altogether, and the principles on which it depends. The method of circuits in land surveying, and route surveys in the military branch, are examples of it. In the latter it is supposed not to be susceptible, as hitherto practised, of a greater accuracy than one part in 50 : in the former, one part in 350 or 400[9]. Even in these, the accuracy is by no means in proportion to the labour, whether in the field, or the closet

2. *The Method by Coordinates.*

16. Having fully considered the trigonometrical methods, their facilities, and their degrees of accuracy, we are now to enter upon the method by coordinates, in which each point is laid down independently of every other.

[6] I have said nothing on the superior accuracy with which an alignment may be observed with a telescope compared with a pair of plain sights which have been hitherto used with the plane table, and for this simple reason, that the plain sights are no essential part of the instrument. It is just as easy to apply telescopic sights to the ruler of a plane table, as to the upper plate of the theodolite.

[7] And it is to be noted, that where results are required only for the purpose of laying them down on paper, this method is probably the most correct, as it is certainly the most direct. If, however, we want the results to subject them to some futher calculation, then the plane table is inadmissible, except for very secondary purposes.

[8] By this I do not mean that there are no means, at the termination of the work, of proving its accuracy, or otherwise ; but that the proof can only be applied at the conclusion of a considerable part of the labour (on the closing of the circuits for instance); when, if the work be proved erroneous, the whole of it is to be repeated. A principle of check, to be valuable, must comprise applicability at each step of the operation.

[9] This proportion is derived from the difference of two positions, determined by two distinct operations of this nature.

17. The most obvious method of applying this principle to practice, is to mark out, or trace on the ground, two lines forming any angle, (a right one would be the most convenient,) and then proceeding along these lines with an instrument for letting fall perpendiculars[10] measure as we go along the distances to the points at which perpendiculars to the lines from the several objects would fall. Then drawing these on a proper scale on paper, their intersections would give the positions of the several points.

18. Such a method is excellent, as far as it goes[11], but it requires that all the positions should be in sight, from the two lines forming the origin of the coordinates; if we wish, at least, to preserve the efficiency of the principle, the independence of the results. This condition of course seldom obtains, and we must therefore have despaired to apply the method on the great scale, did not practical Astronomy supply the desideratum.

19. It is evident, that if we consider the Equator and one of the meridians to represent the axes of the coordinates, then will the latitude of a place be one of them, and the distance from the meridian (which is only another expression for the longitude) the other. By determining then our latitude and longitude, we afford data for drawing the coordinates, the intersection of which will fix the place of any point.

20. The facility of the method is great, at least in the case of the latitude. The altitude of a star being observed, gives us this element; and the labour of observation and calculation is equally trifling. But with regard to the accuracy of the method, there is a limit necessarily placed to it, in the smallness of our instruments, which for the time, are made to represent the whole meridian or circuit of the globe. The scale is, therefore so much smaller than that of any map, that very trifling errors become magnified into such as are scarcely admissible in *every* map. With regard to the amount of this error it will depend upon the instruments used; a Troughton's reflective circle is one of the most ordinarily employed, but the accuracy attainable with such an instrument is, perhaps, scarcely sufficient, except for very general maps. A Troughton's altitude and azimuth circle of 15 or 18 inches, furnished with three, or five, or even seven miscroscopes[12] would be doubtless capable of giving much more correct results; but though portable, such an instrument is troublesome to transport, and consumes much care and time in the use: it is also very expensive. The former might be depended on, perhaps, to 20″ in a single observation, the latter to 10,″ 8,″ or 6,″ with one reversal, according to the number of microscopes used: a mean of many observations would, probably, be correct to half these quantities. But the preferable instrument to either, is the repeating and reflecting circle. This instrument, which was invented by Borda, has never been much patronised by English makers, or English observers, owing, perhaps, to the prejudice in favour of Troughton's circle. Some years ago I ordered one from Dollond, and though circumstances prevented my giving the instrument a fair trial at the time, and occasioned me immediately afterwards to part with it, I saw enough of it to be convinced that it was capable, in the hands of an able observer, of giving, perhaps, as accurate results as could be had with much larger and less portable instruments. The method of reducing any of the heavenly bodies to the meridian when near it, so well explained and illustrated by Delambre, enables us to multiply the observations to any desirable extent, so that in a few hours a result may be obtained with this instrument, of

[10] The reflecting cross, as it is called, or still better, the pocket sextant, set to an angle of 90°.

[11] For a plan of small extent, I cannot imagine a better method, unless it be a something similar one, which is indeed in principle the same. Measure three distances, or more, and put up signals at each point. Then, with a compass, take the bearings of as many of these points as can be conveniently had at each position you wish to lay down. Three bearings would be a sufficient check on each secondary position, and if the primary stations are fixed by trigonometrical proceedings, this method would be a very correct one of filling in detail, the principle of the independence of the results being fully apparent. A small pocket sextant would be still better than the compass, as by means of the angles subtended by three objects being taken, the intersections of two circles, the radii of which are easily found, would give the place of observation. If again a plane table be substituted, we should have the very best method of land surveying that can perhaps be devised. And yet, strange to say, it has never been either practised, or even described.

[12] Perhaps when so many as seven are used, Verniers would answer the purpose. The inventor of this instrument has, I am aware, given it as his opinion, (Trans. Astron. Soc. vol. 1, p. 38) that more than two microscopes cannot conveniently be attached to the vertical circle. Some little awkwardnes may certainly be occasioned in reading them, but not sufficient to authorize our rejecting so capital an improvement.

equal accuracy with that which by any other instrument would consume the whole night, or perhaps more than one. I think, therefore, I am warranted in fixing 4″ or 5″ [13] as the utmost error which latitudes, determined by it, need be subject to. This is equal to one-thirteenth or one-tenth of a mile.

21. Were the longitude determinable with equal correctness, there would be little doubt as to the superiority of the astronomical method for laying down a large tract of country, where no minute accuracy is required for special purposes. But this is not the case. Even with this admission, however, the facilities of this method are so great, and the advantage of its leading principle is so obvious, as to make it always a question of circumstances which method we are to employ; and to afford some grounds for judgment, I shall go into a little detail on the subject, and explain as succinctly as I can, the several methods of determining the longitude.

22. They are in number seven. 1. The moon's eclipses. 2. Immersions and emersions of Jupiter's satellites. 3. The sun's eclipses. 4. Occultations of the fixed stars. 5. The moon's transits. 6. The moon's distance from the sun or fixed stars. And 7, what is commonly called, The transference of time; whether effected by transporting chronometers, rapidly from place to place, or by signals visible at the same time at the two stations of observation.

23. The first is not susceptible of much accuracy, on account of the ill-defined edge of the earth's shadow, rendering it uncertain when the eclipse begins or ends. This objection it is attempted to obviate, by observing and recording the entrances and exits of the several spots on the moon's face into the shadow, and doubtless, the mean of many observations of the spots will be nearer the truth than the mere commencement and end of the eclipse. But it is, after all, far from giving satisfactory results, add to which (a vital objection in our view of the subject) these phenomena occur so seldom as to be totally inapplicable to the business of mapping a country.

24. The eclipses of Jupiter's satellites occur oftener, and are susceptible of a considerably higher degree of accuracy. Nevertheless, they fall short of our purpose, and are better fitted to determine very distant positions when time is at our disposal, than contiguous points at each of which our stay is limited. A mean of several results of immersions and emersions of the 1st satellite is capable of giving the longitude to a very tolerable degree of accuracy; but to obtain such a series, would consume months even in the favorable climate of India.

25. The sun's eclipses, and occultations of the fixed stars, are superior in principle to either of the preceding, and only inferior in practice on account of the slow motion of the moon, on whose position they depend. The disappearance of a fixed star is instantaneous, and if the commencement of a solar eclipse be not observable with such extreme accuracy, yet as observations may be multiplied during its continuance, the mean result may be expected to be even perhaps more accurate than the solitary observation of the star. But the moon's motion is so slow that any uncertainty is greatly multiplied in the resulting longitude: an error of even half a second in the time of the phenomenon will occasion an uncertainty of about 12 s. in the longitude, or 3′ of space; equivalent in India to three miles. I think it very doubtful whether an interval so small as one fourth of a second can be observed with any thing like certainty; and this would answer to half the above quantity, or 1,5 mile.— The same objection occurs to each of these methods which we mentioned as applying to the preceding ones; infrequency of occurrence. They are therefore more suited to the determination of very distant points than to those of a single country.

26. Lunar distances and lunar transits, depending also on the moon's motion, have not the latter objection. Transits may be observed almost every day, wea-

[13] In a series of 100 altitudes of the sun, taken while I was yet quite new to the instrument, I obtained the following results. They were taken in sets of 10 altitudes each, and the result of each set was separately calculated.

23rd Nov.	1st	Set.	31.	18.	38,7	
24.—	2nd	do.			35,5	
30.—	3rd	do.			31,0	
——-	4th	do.			38,2	
——-	5th	do.			34,7	Greatest difference from the mean 4″,4.
1st Decr.	6th	do.			33,4	
———	7th	do.			37,0	
———	8th	do.			37,8	Extreme difference 7″,7
———	9th	do.			32,6	
———	10th	do.			35,4	
Mean of 10 sets, or 100 obs.			31.	18.	35,4	

ther permitting,and lunar distances repeatedly in the course of the same day or night. The facility and rapidity given to these observations, by the employment of the reflecting and repeating circle of Dollond, already mentioned, enhances very much the value of this method. In the first number of the Gleanings, a suggestion of mine was published, recommending a method of employing lunar distances, so as to be independent of the tables, i. e. by successive or by corresponding observations. The circle of Dollond adds great practical value to this suggestion[14]. In fact I am persuaded that in this way, or by correcting the tables, a longitude may be determined by a proper series of distances to within 2′; which in the mean parallel of India is equal to about two miles. Owing to the slowness of the moon's motion, however, it is scarcely to be expected that observation can come much within this quantity, unless in a very much prolonged series. We may, therefore, proceed to the consideration of the next method, the transference of time.

27. In reality, all methods of determining the longitude, may be said to consist in the transference of time; but that which I am now going to describe seems to have been considered to depend more immediately or directly on that principle. And though I could easily show that this opinion is incorrect, and that the methods are all the same in principle, differing merely in the phenomenon or appearance by which the same absolute instant of time is indicated at two places, to be compared there with the account as derived from the appearance of the heavens, different to each observer; though I say this be evident to any one who will consider the subject, yet as the term is sanctioned by use generally intelligible and convenient, I shall not refuse to employ it, though it seem to indicate a distinction without a difference.

28. There are two ways in which the results usually classed under this head may be obtained. In one, the principle is perfect, but the application limited. In the other, no difficulty occurs in the application, but the result is rendered doubtful by the necessary reliance placed on the accuracy of the work of man's hand. The doctrine of chances teaches us to estimate the amount of this uncertainty, and it is possible by increase of means to diminish it greatly, so as in fact to raise a question as to the comparative value of the means, and the accuracy they are intended to secure.

29. The first depends on the display of a signal, such, that it may either appear or disappear suddenly, and the instant of its appearance or disappearance be observed by two spectators, furnished with the means of determining their time at that instant; in other words, the appearance of the Heavens as referred to their horizon. The distance at which such signals are visible, evidently opposes a limit to this method, which renders it applicable only to short distances. The distances, attainable in practice are found, however, to exceed what might be supposed *a priori.* For though in the case of an opaque object, it must be fixed at an inconvenient, perhaps an unattainable height, if it be required to be visible to any distance on account of the curvature of the earth, yet it is different with regard to a luminous body, which throwing out rays in every direction, is reflected from the mass of our atmosphere, so as to be distinctly visible even when the object itself is below our horizon and actually concealed by the intervening swell of the globe. By elevating on a stage 50feet high the luminous signal, the sphere of its visibility would be increased; or by employing, as in the operations carried on between Greenwich and Paris, rockets, the explosion of which should be considered the phenomenon to be observed, a still greater distance might be given to the stations. In a country where the undulation of the ground should be at all considerable, still greater facilities would be afforded, as allowing the application of Lieut. Drummond's powerful signal, which being concealed at intervals, would afford a favorable phenomenon to observe. Under any circumstances, however, it has its limits in practice, and to determine so great a difference of longitude as 10° or 12° would no doubt require several operations. This of course vitiates the principle of the coordinates in some measure, I mean the independence of results; but it may be worth inquir-

[14]. The accompanying pamphlet contains a description of this circle by Dollond. It has been recently received from Lt. Col. Hodgson, late Surveyor General of India, who recommends the instrument strongly as adding so much to the facility and the correctness of the lunar method. Col. Hodgson, whose opinion as the most practised geographical surveyor in India, is entitled to great weight, always thought favorably of lunar distances. His chief objection to them was occasioned in the use of Troughton's circle by the necessity of making frequent readings, three for each distance, thereby often occasioning hurry, and always disturbing the eye. This objection is completely obviated by Dollond's circle. If the Editor could find room for the substance of the pamphlet, and particularly for Captain Sabine's remarks, in the Gleanings, I think he would do a service to many by publishing them. *Our correspondent will find in Art. II. the substance of his pamphlet.—Ed.*

ing in what degree this would be the case, and what would be the amount of the possible error.

30. The only errors to which the determinations are subject, are, 1st. The uncertainty in noting the exact instant of the phenomenon; 2nd. The error which may exist in the determination of the time of the places of observation; in other words, uncertainty in the error of the clocks. The first may be reduced by multiplying observations certainly to one-tenth of a second in time; the latter to about the same. The first observation would be required once at the extreme, and twice at the intermediate stations. The second would only be required at the extreme stations. Supposing the distance of the stations to be about 25 or 30 miles, we should have on a distance of 60 miles, six observations, in which the error might amount to one tenth of a second. On the very unfavorable supposition that five errors lay one way, and the sixth the other, we should have four-tenths of a second in time =, 1′ in space; or about 176 yards, as the error on a difference of longitude amounting to 1°. On a difference of 10° or 12° the errors would balance each other more or less, so that the error would not probably exceed half a mile [15].

31. The second way of transferring time is independent of the display of any signal or its visibility, and is, consequently, less restricted in its application. I fear, however, we cannot say that the independence of the results is really, though it is apparently preserved. The operation consists in determining the time at one of the points to be fixed, and then transporting the chronometer to which the time was referred to the other, repeating the operation there. The chronometer being now referred to the account of time at each place, is obviously capable of shewing us the difference between the two accounts, or the difference of longitude. In this operation the chronometer is supposed to go with perfect exactness or regularity, for it little matters whether it gains or loses, provided it gains or loses equally in equal intervals of time. It is in the nonaccordance of this supposition with fact, that the objection to the method consists. Were it possible to make such a chronometer as we have above supposed, the method would be perfect.

32. That this has not yet been effected, is sufficiently known, notwithstanding the munificent rewards that have been either given or promised to the partial, or the complete solution of the problem, by the several governments of Europe: yet, though perfection has not been attained, a near approach to it has been made, and it remains to show how far the imperfections still adhering to these beautiful efforts of human skill, affect the question in the consideration of which we are engaged; the determination of the longitude.

33. Chronometers have been made which will steadily preserve, for a considerable period, as long as they are not exposed to shocks or irregular motion of any kind, their rate, (as it is called;) that is the daily quantity by which it exceeds or falls short of a supposed perfect clock, agreeing with the mean state of the heavens. These are of course not exactly what we want. Our object is to have such a one as will bear being transported from place to place, and will not be liable to change its rate, though it occasionally meet with a little rough usage. Such a chronometer is to be had, but they are not very common, or very cheap. It is found, however, that the best chronometers yet made, though exceedingly satisfactory for a period, have yet suddenly and unaccountably altered their rate, and it is the possibility of this change which is the only objection to their being employed in determining the longitude. A good chronometer will go sufficiently regularly as to admit of its rate being known from day to day to within one quarter of a second. Supposing us then to determine at one point, the error of this chronometer on mean time, and then to travel rapidly a distance of 100 or 200 miles, repeating our determination of its error, on mean time at the new station, it is obvious that the difference of these errors corrected for the rate of the watch during the interval, will be a measure of the difference of longitude: so much the more accurate, as the rate has been steady, and as the interval of time is short.

34. With regard to the steadiness of the rate, that is generally judged of from the comparison of the rates determined at the two places. If they do not differ more than the results at each place do amongst themselves, then we have a right to consider it exceedingly probable that the rate has not changed during the transport. With regard to any uncertainty in the rate which there may appear to be, this will be

[15] As an example is always better than any estimate, however judicious,—I shall here present the result of the operations carried on between Greenwich and Paris, by Mr. Herschel and his associates.—The number of stations was four, and the difference of longitude was found to be 9 m. 21, 46 s. differing only ,28 s. from Captain Kater's trigonometrical determination. This is much below the above estimate.

reduced within narrow limits, by confining our dependence on the steadiness of the rate to the shortest possible period. With an uncertainty, as above supposed, of one fourth of a second on the daily rate, the above distance, which may be travelled in two days, might be determined to within one furlong, a proportion of 1 in 1600.

36. There would still, however, be this doubt affecting the result. The rate of the watch may have changed; there is no absolute proof it has not; the result may then be erroneous. Against this we have only to alledge the probability of no such thing having taken place. This probability has its value, and can be expressed in figures. If indefinitely increased it becomes the next thing to certainty. It is evident that using two watches instead of one, if they both agree in giving the same result, the probability of the result being correct will be increased two fold. If three, three fold, &c. As to the original value of the probability, it will be expressed by a fraction, the numerator of which is 1, and the denominator the number of good watches, divided by the number of instances in which their rate has changed within two days. Let us suppose this is to be $\frac{1}{10}$, that is, that 1 watch in 10 will be found to fail us in a period of two days. If we use two, the chances we are right (they both agreeing) will be 20 to 1, if three, 30 to 1, and so on. By using more watches, we then increase the probability so much, that we can have every necessary degree of confidence in the result. The employment of a greater number of chronometers is calculated also to reduce the inevitable errors of observations, which were fixed at $\frac{1}{4}$ of a second, and to estimate this reduction in the most moderate manner, we may safely suppose it one half. I would say then, that 10 chronometers would determine the whole longitude of India, within two or three seconds of time, half or three quarters of a mile. This is one part in 3000 nearly [14].

37. Thus we see that operating with proper means, we may determine the longitudes with an accuracy of $\frac{1}{2}$ a mile, however great the distance, and the latitudes to a sixth of a mile, or even less; no distance need then be erroneous to so much as one mile [15], however great it may be. I say *need be*,—as indicating the necessity of the work being properly performed.

38. Having thus briefly described the several methods which have been, or may be employed for mapping a given portion of the earth's surface; I shall, as this communication has already swelled to such an extent, defer my observations on the comparative economy and expedition of each method, as well as my ideas of the circumstances and conditions under which each is applicable, to a future occasion. It was necessary to establish the preceding detail, to refer to in the course of what I have further to urge; as I know of no work to which I could appeal for authority, with regard to many of the statements I shall have to bring forward. D.

II.—*Description of an improved Repeating Reflecting Circle, by Mr.* Dollond, *with observations by Captain* Sabine, *extracted from his work on the Pendulum.*

The reflecting circle, being constructed upon the same principles as the sextant, requires the same process for making the observations. The circle having the advantage of enabling the observer to take the angle on, what is usually termed, the ON arc; that is, he may first take the angle precisely in the same manner as with the sextant; and then, by reversing the face of the instrument, take the angle on the OFF arc, or the other side of the zero. By this method, he will correct any error that may arise from a want of parallelism in the glasses, or in the adjustment of the

[14]. Dr. Tiarks determined the difference of longitude between Falmouth and Madeira to be, by seventeen chronometers, 0*h*. 47*m*. 28*s*.2, the extreme difference was 20*s*, nine of them gave a mean 0*h*. 47*m*. 28*s*. 2, and of these the extreme difference was only 3*s*, 7. *Vide Phil. Trans. for* 1824, *p*. 365.

[15]. In the case of a difference of longitude, the error on the distance will be equal to the error on the longitude. In the case of the latitude, equal to that of the latitude, and intermediately will be between these extremes. The maximum will be when $m=p$, in which case the error on the distance will be 1, 4 times that of the latitude + 1,4 times that of the longitude= a little less than a mile.

verniers to the zero; and by adding the angles, so taken, to each other, and dividing them by two, the correct angle will be obtained.

There are two verniers on each index, which read from the centre division, according to the arc upon which the angle is taken, and the one may be read backward as a check upon the other.

There is another and a greater advantage to be derived from the circular construction of the instrument, which is the power of repeating the observation, and thereby correcting the errors arising from the eccentricity of the centering or dividing[1].

In the instrument here described, there is a very considerable improvement for facilitating the operation of repeating the observations; it principally consists of the two sets of figures, one upon the inner circle, and the other upon the outer or divided circle; the first set are for reading off the angle, which is taken with the index B; and the second for reading off the number of the degrees, &c. which are to be divided by the number of repetitions.

The method of observing with the instrument is as follows:—

If it is to be used as a sextant, the vernier A, which is attached to the inner circle, must be set to a primary division, (it will be the most correct to set it to 720,) for which purpose there is, as a rough guide to the figures, a line upon the outer chamfered edge of the vernier A, which corresponds with the zero of that vernier. Having adjusted this vernier correctly to 720, the angle must be taken by moving the index B forward, in the same manner as with the sextant, and the angle contained between the two objects will be seen upon the circle by the vernier belonging to that index, and the figures on the inner circle will be the guide for reading off. The index error should, in this case, be found as with the sextant, by taking a small angle on each side the zero. If there should be time for a second observation, the same angle may be taken with the face of the instrument reversed by moving the index B, the contrary way to the former, or upon the OFF arc; in this case the index error need not be attended to, as the half of the two angles will be the correct angle.

The circle may also be used for taking a series of observations, by setting the vernier A to 720, and proceeding, in the first instance, by moving the index B forward, and then the inner circle, telescope, &c. and so on, each time reading off the angle, or noting the number of observations; and dividing the amount of the degrees, &c. passed through by the number of the observations; and if the number of the angles taken are equal, or nearly so, to the whole circle, it is evident that the errors of division and entering will have been taken into the account:—the index error must, in this case, be noticed.

When the instrument is used as a repeating instrument, the vernier A should be very correctly adjusted to 720; and the index B moved forward until the two objects, the angle of which is to be taken, are brought into contact; read off, and note the amount, then leaving the index B fast, and unclamping the telescope, &c. move it through the space equal to twice the angle, or until the figures on the inner circle shew the amount of the first angle; on the contrary side of their zero, bring the objects into contact, and it will here be evident that the space passed over will be equal to twice the angle. Note down two, and without disturbing the last adjustment, move the index B forward twice the angle, and bring the objects again into contact; then move the telescope, &c. forward as before, and note down two in addition to the former. In this mode of observing, if the face of the instrument is continued always one way, it will be requisite to present the telescope, first to the left hand object, and secondly, to the right; but if the face is reversed alternately, the telescope should be presented to the same object. Proceed with the observations, and each time the telescope, &c. is moved forward, note two; and when the vernier A has either approached or exceeded the whole circle, or 720, read off by that vernier, and divide the amount by the number of the angles taken, and the quotient will be the correct angle; it will here be clearly evident, that all errors of division, centering, and observation, will have been taken into the account, and, therefore, the angle will have been correctly obtained.

Observations by Captain Sabine, *from his work on the Pendulum.*

In the spring of 1821, Mr. Dollond, to whom practical astromony has so many obligations, was kind enough to show me the design of a repeating reflecting circle,

[1] This cannot surely apply to the ordinary reflecting circle.—ED.

which was then in progress of execution: as this instrument appeared, so far as could be judged from the design, to promise to supply precisely what was wanting, I requested him to make a second on my account, intending to give it an extensive trial[2].

When used as a sextant only, this instrument possesses the following advantage over sextants of the ordinary construction; first, it enables the angle to be measured alternately on each side of zero, whence the index error is compensated, and the liability to those of imperfect division and centering diminished; secondly, by clamping the vernier of the circle successively at primary divisions, about a third of the circle apart, in succeeding pairs of observation, the errors of centering may be destroyed; and thirdly, the angle which may be measured is not limited by the extent of the arc, but may be carried to the utmost amount in which the relative position of the glasses will admit of reflection.

When used as a circle, the following additional advantages are gained; the process of observation is shortened at least a half, by dispensing with the reading off and writing down the angle at each repetition. The errors which are frequently introduced in those operations are avoided; those of imperfect graduation and eccentricity are rendered insensible; and in night observations, especially, the eye is spared the alternate reference to a strong artificial light, necessary for reading the arc, but extremely prejudicial to the most favourable state of the eye for observation.

An incidental advantage arising from thus shortening the process of observation is, that it places the whole operation within the power of an individual to accomplish by himself; whereas it previously consisted of too many distinct parts, and was consequently too laborious and fatiguing for accuracy. The subjoined observations were made (with very few exceptions) without an assistant, the times being noted by the beats of a chronometer. The satisfaction is great to an observer to have all the parts of an observation thus within his own command; it is convenient also, because assistance is not always to be obtained; and it is conducive to accuracy, because the attention of an assistant is rarely equal to that of the observer.

In the subjoined tabular abstract, the " Time by the Chronometer" is a mean of the number of observations expressed in column 4, the details of the time corresponding to each observation being omitted. The correction of the chronometer No. 423, to the mean time at the several stations is inserted in column 3; in column 5 is shewn, the whole arc passed through by the vernier (A) of the circle in the process of repetition; and in column 6, being the whole arc divided by the number of repetitions, is the apparent distance corresponding to the mean chronometer time in column 2. Column 7 and 8 exhibit the apparent altitudes of the moon and sun, or star, calculated for the known apparent time at the station. The corrections for refraction, or the differences between the true and apparent altitudes, have been computed for the states of the atmosphere shewn in columns 9 and 10, by Dr. Young's table in the " Nautical Almanac," for 1822; much pains was taken to obtain the true temperature of the air, uninfluenced by radiation on the thermometer from the surfaces around; for which purpose the thermometer was enclosed in a highly polished metal cylinder, pierced with holes in the top and bottom, and placed in the shade. The true distances in column 11 have been deduced by Dr. Maskelyne's method, published in the preface to " Taylor's Logarithms," with corrections introduced,—of the horizontal parallax on account of the ellipticity of the earth,—and of the distance where the oblique semi-diameters were sensibly affected by refraction[3].

In deducing the time at Greenwich, corresponding to the true distances, from those inserted in the " Nautical Almanac" for every third hour, the second differences of the moon's motion, in relation to the sun or star, have been duly taken into the account. It sometimes happens that the second difference of the distances of the moon and stars, inserted in the Nautical Almanac for every third hour, amounts to more than one minute of space; in such instances, the correction due to the second difference will exceed six seconds of space during more than half the

[2] A description of the instrument here follows, but as it would be a repetition of what has preceded, it is here omitted.—Ed.

[3] These tables contain all the details of 1350 observations, but as they would occupy so much room, we have been obliged to omit them, especially as all the information interesting to the reader is contained in the text, and in the small table given in the succeeding page.—Ed.

intermediate interval, and consequently, if neglected, will occasion an error of about three minutes of longitude in the deduction. This circumstsnce is thus specially adverted to, because its notice is omitted in the very useful summary of the minute corrections, requiring attention where precision is desired, published by the Secretary of the British Board of Longitude, in the Journal of the Royal Institution, for July 1820.

The circle with which the distances were observed, was ten inches in diameter, and weighed five pounds; the telescope was furnished with a magnifying power of fourteen. The observations at Sierra Leone were not strictly its first employment, as I had observed sixty-four distances with it at Madeira, on the outward passage; with the exception of these, however, the use of the circle was new to me at Sierra Leone, and the awkwardness which attends the employment of a new instrument was still to be overcome.

The sixty-four distances at Madeira, of which forty were of Regulus, west of the Moon, and twenty-four of the Sun, east of her, made the British Consul's house at Funchal, in 16° 55′00′ W.; the longitude of the Consul's garden has since been ascertained, by the mean of sixteen chronometers, specially sent for the purpose, at the direction of the Commissioners of Longitude, and has been found 16° 54′. 45″,3, W.

The preceding tabular statement * comprises the results of 1350 distances, divided into 123 sets, and distributed through seven stations. The following table collects, in one view, the mean results, and exhibits a summary of the differences of the individual sets on the general mean at each station.

STATIONS.	No. of Distances.	No. of Sets.	Individual Sets differing from the Mean.							MEAN LONGITUDES.
			Less than 1 mile.	Less than 2 miles.	Less than 3 miles.	Less than 4 miles.	Less than 5 miles.	Less than 6 miles	Less than 7 miles.	
Sierra Leone,	318	23	4	7	4	7	0	0	1	13 15 26,8 W.
St. Thomas,	150	11	2	4	1	2	2	0	0	6 45 00,4 E.
Ascension,	164	16	5	2	2	5	2	0	0	14 23 35, W.
Bahia,	128	14	5	4	3	2	0	0	0	38 32 39, W.
Maranham,	158	16	12	3	1	0	0	0	0	44 21 25,5 W.
Trinidad,	162	16	5	6	5	0	0	0	0	61 36 15, W.
Jamaica,	270	27	15	7	4	1	0	0	0	76 53 15, W.
Total,	1350	123	48	33	20	17	4	0	1	

Whence it may be inferred that in similar circumstances of observation,—i. e., on shore, and within the tropics, the observer being previously accustomed to lunar observation with sextants, and furnished with a correct knowledge of the time at the station,—it is about 2 to 1, that a single set composed of 10 or 12 distances, observed with Mr. Dollond's circle, will give a result within two miles of the longitude, deduced from an extensive series, including the various states of the atmosphere occurring in such climates, and at different periods of the Moon's age; that it is about 2 to 3 that the result will be within one mile; and that a difference, amounting to so much as between 4 and 5 miles, may not be expected to occur oftener than about once in 25 sets.

The improvement which practice will make in the habits of observation, (and consequently on the inferences that have been stated,) is evident, on an inspection of the table; for if the three last stations only are regarded, the chances will appear more than equal, that the result of a single set is within one mile, and 4 to 1 that it is within two miles of a general mean; whilst the extreme difference, occurring only once in 59 sets, is under four miles.

No attempt has been made to correct the distances inserted in the Nautical Almanac, by a more exact knowledge of the Moon's place, derived from the Greenwich

* The statements are omitted, as the result is so clearly expressed in the above table.—ED.

observations; because the design has been to afford a practical inference of the degree of accuracy which an observer may expect with the means with which he is furnished on the spot. It may be proper to mention also, that the table includes every set of distances observed at the stations to which it refers.

The conveniency of the circle in observation, and the facility with which it may be managed by those who will accustom themselves to its use, may be judged by the observations at Jamaica; where it may be seen that 60 distances of the Sun and Moon were observed within the hour, or one in each minute, including the observation and entry of the time of each distance, and the reading off at every tenth distance, and writing down the arc passed through. In the repetition of the same process with the Moon and Aldebaran at night, the number of distances observed in an hour was 50, or, on an average, one in a minute and twelve seconds. Of the six sets, into which the distances of the Sun and Moon under notice were divided, three are within one mile of the combined result of twenty-seven sets, and the two others within two miles; and of the five sets of the Moon and Aldebaran, four are within one mile, and the fifth within two miles: this is stated to shew that accuracy was not sacrificed to expedition; both the instances were without the advantage (in expedition certainly) of an assistant.

The observation of the angular distance of the Moon, from certain fixed stars, has long and universally been regarded as the best means of deducing the longitude of a vessel on the ocean from celestial phenomena; but it has not been so generally recognised as it deserves to be, as the most eligible of all the methods which present themselves to the choice of the geographer, or the practical astronomer, for determining positions on land, wherever time or the conveyance of instruments form a part of the consideration. It combines, in a degree far beyond comparison with any other method, the very important qualities of convenience, expedition, and accuracy. The whole apparatus which is required,—a circle, a chronometer, and an artificial horizon,—does not weigh twelve pounds; no temporary observatory is required for its protection, and all situations are equally convenient for its use; the latitude and longitude may both be determined in the first 24 hours after the arrival at a station, during three-fourths of every lunation; and as the observations by which the determinations are accomplished, may be multiplied within that interval at the pleasure of the observer, so as to comprise, in respect to latitude, every important variety of circumstance, and almost every variety in regard to longitude, no sacrifice of accuracy to expedition is called for, but the precision will be proportionate to the labour which is bestowed.

There are occasions in which the qualities of convenience in portability, expedition, and accuracy in determination, are almost equally essential. Such is the design which is understood to be entertained, of forming the bases of a survey of central India, by the celestial determination of the geographical position of stations, selected at proper intervals, over that very extensive portion of the globe. Admitting the space to contain 130 or 140 square equatorial degrees, and the stations to be, on an average, 100 miles apart, above eighty such determinations must be made. Those who are acquainted with the apparatus which would be required, in any other mode of deducing the position from celestial observation than the one under notice, and will pursue, in detail, the consideration of the conveyance of such an apparatus over such an extent of country, independently of the accidents and interruptions to which it would be liable,—and who can appreciate the time which would be occupied in obtaining an equally precise determination at each station, as lunar distances would give in 24 hours,—will, I think, arrive at the conclusion, that it is only by lunar distances that the design is likely to receive its accomplishment.

III.—*On the Tides of the River Húgli.*

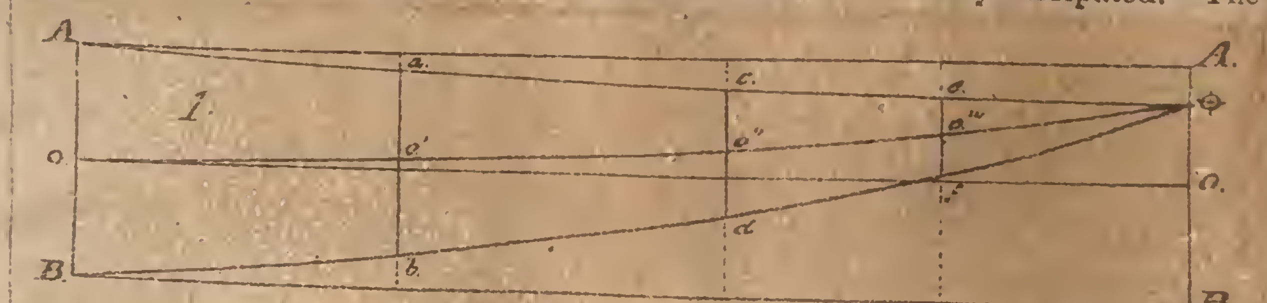
In a former paper on the subject of tides, which appeared in your eleventh number, while drawing a comparison between the tides observed at Calcutta, and those observed at the mouth of the river in the island of Ságar, I was led into a train of conclusions upon the general principles affecting the level of the tides in their progress inland, which may, perhaps, appeal more forcibly to the understandings of your every day readers, who may not have considered hydraulic subjects with great attention, after some additional illustration. The subject itself would indeed readily bear dilating into a much greater compass than would probably suit the nature of your valuable little periodical; perhaps, also, infinitely beyond the patience of your readers; and my fear of trespassing on the bounds of either, may very easily have wrapped in enigma what a very little explanation cannot fail to render perfectly intelligible.

The notice, in your tenth number, of Mr. Kyd's labours, rendered it unnecessary to recite the peculiarities of the tide, and I, therefore, confined myself to the reduction of his observations to mean results, and the comparison of these mean results with other tides at the mouth of the river, and in the Salt Lake immediately in our neighbourhood.

In drawing this comparison, it was impossible to avoid recognising the two principles which I endeavored to illustrate by the two marginal diagrams inserted in the paper; first, that in a tide channel or creek through which there is no discharge or drainage of upland water, and where the elongation of its course is sufficient to dissipate the effects of the diurnal tide at the mouth of that creek, the surface of the water at this place, where the influence of the daily tide is not perceptible, (the point ⊕ of the diagram) will be below the level of the highest flood tide at the mouth of the creek. Secondly, that in the mouths of rivers, or in tideways, through which there is any constant and considerable discharge of upland water into the sea, the surface of the water at the limit to which the influence of the daily tide extends up that river, (the point ⊕ of the 2nd diagram) must be above the flood tide-level at the mouth.

This latter principle has been already very generally noticed, although, without any minute elucidation of the former. In my own limited researches I can find no recognition in any works upon the subject, but from some casual notices upon the water levels at New Orleans, a place situated precisely in the same manner as Calcutta, within the tide-way of the Mississipi, and with the salt or marine lake Poutchartrain immediately behind it, the river under its walls, being subject to an annual fresh or swelling of its waters, while the lake behind exhibits a nearly even and constant level, with still a small daily fluctuation from the gulf tides, with which it communicates directly. Also from the elaborate accounts of the tides in Holland, reduced to the Amsterdam standard given in Lalande, I almost conceive it impossible that the principle could have been overlooked. And some careful enquirer among the works of the Dutch engineers, may, very likely, discover much more curious and even better detailed particulars than I have been able to collect.

The favorite cases which are most generally adduced in theoretical treatises on tides, have all some opportunity of the application of lunar attraction. Such as the Mediterranean, Baltic, German, and Northern seas, which from their extent are supposed to be sensibly influenced thereby. In all tide estuaries, mouths of rivers, and especially in the various confined and extended tide creeks of the larger river deltas, there can exist no possible disturbing cause beyond the diurnal rise and fall of the surface of the sea at the mouth. It matters not, as described of the Amazon and other rivers and estuaries, whether more than one tide interval elapses between the mouth and the point ⊕ where the effect of the tide is finally dissipated. The

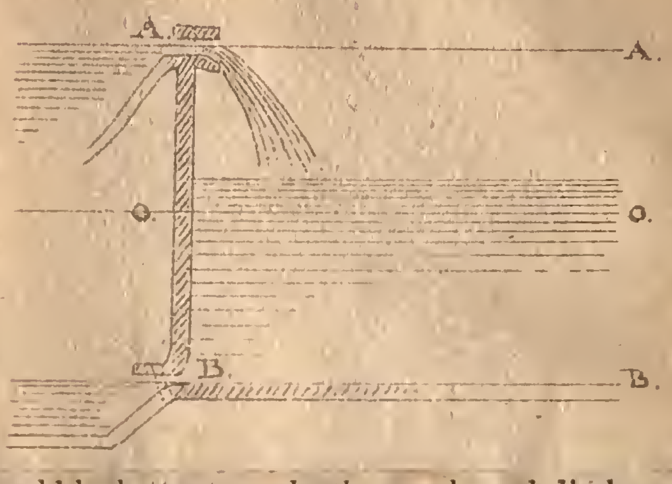


general principle, which I have in my former diagram 1. (here repeated for facility of reference,) endeavoured to represent, will not be vitiated by the circumstance. It may be better understood, perhaps, by the following illustration.

Suppose the A and B of the preceding diagram to be replaced by two cocks, and the part above, from A O B, towards ⊕, to represent a cistern, the first cock A, supplying the cistern from a body of water standing at the level of A, the cock B discharging the water of the cistern into a reservoir at that level B.

The cocks to be alternately opened and shut at stated equal intervals, and to be so constituted, that the supply through A, during the period of its being open, which we will suppose six hours, may not be sufficient to raise the surface of water in the cistern to an equal level with A, and that the discharge, during the same periods of six hours through B, may also be insufficient to depress the surface of water in the cistern to B.

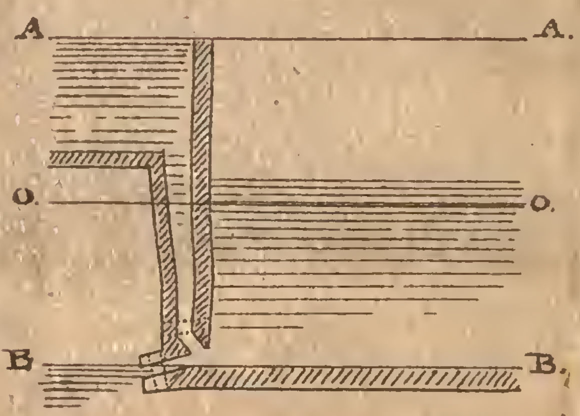
2



Commence with the water of the cistern standing at a supposed mean level ⊕ and open A for a half interval or three hours, then shut A and open B for six hours, after which the alternation may proceed at regular intervals.

It is plain, that if the two cocks had equal orifices, situated as in figure 2, the expenditure for six hours would not be equal through each. Such, however, is not a natural state of things, and it would be better to make the supply and discharge cock at B, as in figure 3, in which case, as the average head of water would be the same to the supply and discharge, an equilibrium must evidently take place in the undulation of the surface of the cistern, consequent upon the alternate manœuvring of the cock. And in this case, all other things being equal, the average height of the water in the cistern would correspond with the mean between A and B or O.

3



To bring the illustration, however, nearer to nature, it becomes necessary to suppose the orifices of the two cocks to alter every moment during their action, the supply cock increasing in section in a very rapid geometrical ratio to the time of high water, and the discharge cock to decrease nearly in the same rapid ratio to the time of low water. It is also necessary, in applying the illustration to a tide which has to travel over any distance, to allow for the time of passing, or in other words, to suppose the supply cock to open before the discharge cock has ceased its action: and in the like manner, the discharge cock to open before the supply cock is closed. The effect of this elapsing time is, however, only partially represented by such a supposition, inasmuch as the check formed by the first flood at the mouth of a creek to the water still ebbing inside, will, on the principle of afflux, create an artificial level, while the commencement of the ebb, at the mouth of the creek, can have no parallel influence upon the level inside the creek. So that it is evident, that although, by the above illustration, while the orifices of discharge and supply are equal, or even changing in equal ratios, it may be difficult to prove that the average surface in the cistern will vary from the mean height between A and B at O; when this latter circumstance of the check created by the first flood is taken into the consideration, the balance will ever be in favour of the flowing tide, and the average height in the cistern will be above O, the mean height between A and B.

The application of the above illustration is of course simplest with maritime lakes and gulfs, such as the lakes of the delta of the Mississipi, or perhaps the Chilka lake in the Cuttack district, and Pulicat, or Madras lakes, in all of which the body of the lakes is only separated from the sea by short inlets of greater or less transverse section. The bed of these inlets, although generally considerably below low water, still so much partakes of the nature of a bar, that the mean level of the lakes is almost universally raised, as shown in the diagram, above the mean level outside;—and here the lake will resemble the cistern of the illustration very closely.

The extended tide creeks of the Sunderbunds, on the other hand, can only be represented, by supposing a series of such cisterns extending to infinity, or otherwise, according as the point ⊕ of final dissipation is attained or not.—The bar, which is so general in the former case, is also prevalent here, although not, perhaps, to the same extent; but it may be said that, the converging of the section of the tide-creek will itself act much in the same manner in raising the level of ⊕.

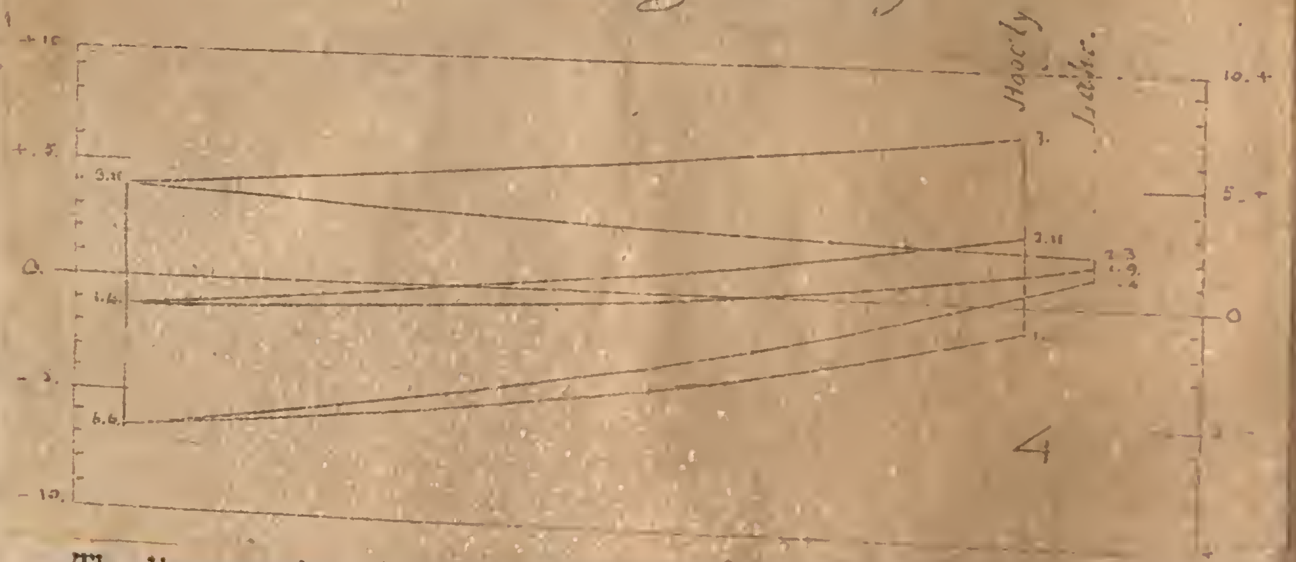
The final dissipation of the daily tide is very far from being general to all the creeks of the Sunderbunds, and to the delta streams of other large rivers. Few of the creeks are sufficiently extended, and others converge too rapidly in section to allow of it. This circumstance is of most common occurrence in those creeks which possess the great uniformity of section throughout their lengthened course; such as the deserted outlets of the main river by which there is now no discharge: also in those creeks which terminate in large j'heels or salt marshes, such as the salt water lake in our own vicinity. In this lake, the point of dissipation is not attained in every state of the tide, a trifling daily fluctuation being sensible at the remotest extremity of the lakes, in the seasons when the bay tides are the greatest in quantity. This may be perfectly understood by the diagram, where, supposing A B variable, it is evident that the distance to ⊕ must vary with it, and most probably the elevation of ⊕ above O also in the same ratio. Indeed this last appears very evidently to obtain in the lake, and in many other cases that I have met with, not excepting the river Húgli, where in the immediate neighbourhood of the tide limit or point ⊕, a very sensible fluctuation is perceptible between the neap tides and spring tides, and no sensible daily fluctuation, except occasionally in spring tides.

It is much to be regretted that no observations exist to guide us to the perfect developement of the whole problem in the river Hoogly. Perhaps some of your many correspondents will be induced to supply what is most particularly wanted, namely, the exact position of the ⊕ under all different circumstances of time and season, beside a few registers of daily tide at intermediate points, such as Chinsura and Sooksagur, or Bánsbaria.

I subjoin, what perhaps may be interesting, a diagram of the state of the tides at Calcutta in the river and lake, compared with those at the mouth of the river at Ságar island, for the month of January (fig. 4); also a diagram for August, the extreme of the freshes. (fig. 5.)

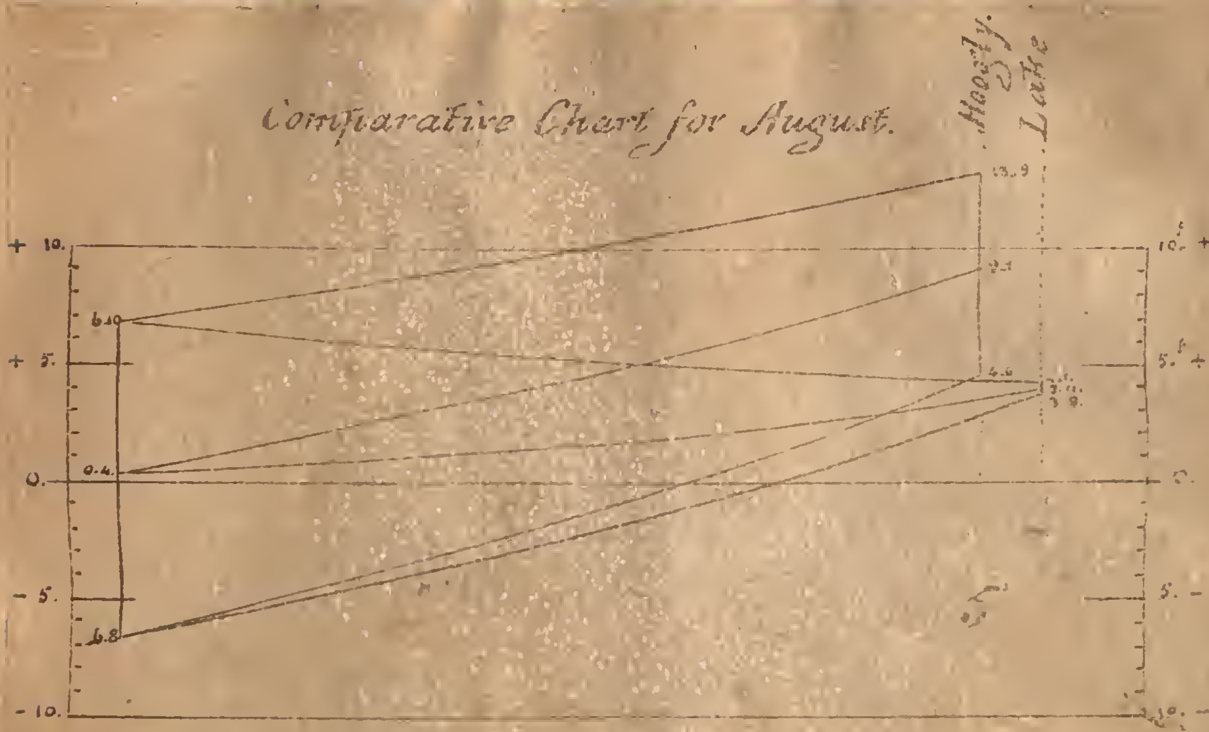
The tides are, each, general means of the whole months, and they have been reduced to their relative level, agreeable to the scheme of connection, given by the dotted base to my former chart, No. 2.

Comparativa Chart for January.



The diagrams show the state of the problem in all cases, and also show what, in the river, ought to be considered the descent or slope from which the discharge of up-land water is to be calculated. In January, the path of mean level shows a descent of 4 ft. 3in. for the whole distance of 62 miles, which would give 8 inches per mile. In August the descent is 8 ft. 9in. giving a slope of 1,7 in. per mile, or more than double what it is in January. The proportion also between the tide at the mouth and at Calcutta, is sensibly diminished by the raising of the mean level in August.

With regard to the lake, the parent tide of which, or mouth of the Mutwal creek, is not precisely the same as the Ságar tide, although it cannot differ much in January, and in the dry months, it becomes necessary to deduct a certain portion of what is given as the elevation of the mean tide at Ságar, due to the season in August. The descent, in January, of the lake mean path is 3ft. 1in., while in August it is only 3ft.7in. by the diagram, a difference of only 6 inches for the influence of drainage upon the slope. With this allowance included the difference of the January and August descent, by the mean path, cannot amount to 2 feet.



In corroboration of what I have advanced upon the subject, I may add, that I have observed frequent instances of two tide creeks, proceeding from the selfsame mouth, approaching close to each other, after a long course (of 80 or 90 miles sometimes), one perhaps with a freer channel, a more circuitous course, or more converging section than the other, and presenting very different tide phenomena, *both as to the time and quantity.* The investigation in these cases invariably *proved some trifling modification*, if not in precise keeping with the above rules.

The influence of such rules in the configuration and surface of such a district as the Sunderbunds, as also in its physical history, may well be conceived ; and it cannot be otherwise than interesting to trace the progress of the marine gulf into a lake with its inlets, the lake into a salt marsh, and the salt marsh into a fen, only inundated in certain tides, and now fit for agricultural purposes. The tide is the moving cause in such changes, and a knowledge of the principles by which it acts is essential in the search. The configuration of the lower deltas of different rivers will be found to accord strangely with the nature of the tides which prevail in the seas before them. Thus, the disturbed and violent tides which prevail in the Bay of Bengal, accelerate the progress of lake into marsh, and marsh into fen; and the maritime lakes of the Mississipi delta, which have a depth of 15 to 18 feet water, the tides of the gulf being only 4 to 5 feet, are replaced in the delta of the Ganges, before which tides of 15 or 20 feet prevail, by fens and salt marshes. It is also very easy to explain another curious circumstance in such countries, namely, that the country under cultivation, considerably inland, is several feet below the level of the land immediately on the coast, and yet by no means so liable to suffer from overflow, during extraordinary tides, unless their duration could be supposed to last a very unusual time.

The level of the land formed by the deposits of the tide, in the same manner with the level of a river's banks, will be universally influenced by the high-water mark, to which it ever bears a certain relation ; and if in the course of ages, by the gain of the delta upon the sea, the tides at any particular position have altered during that time, according as there may or may not be sufficient of sediment in the tide, will the changes in the level of the ground of that position be found to have kept pace with the other change.

But I have said more than enough for some of your readers to form their own conclusions and follow up the subject.

P. T.

Calcutta,
December, 1829.

IV.—*Remarks on* Daniel's *Hygrometer, as connected with the Purity and Strength of Ether in India.*

To the Editor of Gleanings in Science.

SIR,

In a paper of mine, on Hygrometry, published in your second number, p. 46, occurs the following passage: "But there is a still more serious objection to the use of Mr. Daniel's Hygrometer in India. It is the almost total want of action under which it labours, owing to the impossibility, I suppose, of keeping our ether in any thing like a state of purity in temperatures, which for some part of the year exceed that of its boiling point, and for the greater part are not removed 10° from it. It is evident that no stopper will confine the vapour of ether in a temperature of 96°; even at 86, it exerts a force equal to 25 inches of mercury. Accordingly, I have never yet been able to obtain a correct result with it. So far from the depression reaching the dew-point, which is, I believe, occasionally, even 60° below the temperature of the air, I have never been able, however liberal of my ether, to obtain a greater depression than 60°."

A friend, who had purchased a Daniel's Hygrometer with its supply of ether, the property of the late Dr. Abel, and who knew me to be the author of the above paper, expressed his surprise at this statement, which he declared to be entirely contradicted by his experience. He suggested that the ether I had used might have been impure, i. e. a mixture of alcohol and ether, in which case my explanation would apply; but that it was not applicable to good or pure ether, which might be diminished in quantity, but could not be deteriorated in quality, by any evaporation. He offered to convince me, by actual experiment, that, even in the dampest weather, ether would produce, in India, a depression of 30 or 40°.

The ether I had used, and to which the above extract refers, was of three different sorts; I first tried part of a stock which belonged to a chemical test chest. Thinking this might possibly have been filled up in the country, I wrote for some to England: it was no better. I next tried some that had been procured from the Honorable Company's Dispensary, it had no more power than either of the others. I was then led to the conclusion above stated, that the best ether of commerce must be a mixture of alcohol and pure ether, and that the latter, evaporating in consequence of the high tension of its vapour in this country, left a weak mixture of alcohol and ether, which was evidently powerless when applied to Daniel's Hygrometer. Finding my view of the subject controverted, I was glad to have an opportunity of witnessing the experiment.

My friend had two sorts of ether; that purchased with the hygrometer, and some procured from a dispensary. The experiment was performed towards the end of the rains, when the weather was very damp and close. The temperature of the air was 86°, the moist-bulb thermometer marked 80°. We began by trying the effect of the ether I had before used: it did not reduce the temperature of the dew-ball 2°. We then tried his supply of medical ether, which had a little more effect, but very little. When that belonging to the instrument, however, was used, the depression became suddenly much greater, and in a few seconds the ball was covered with dew. The temperature was found to be at this moment 74° or 12° below the temperature of the air, and this in very damp weather. The experiment was not pushed farther with the hygrometer, as I wished to try the difference of the ethers in a still more striking manner, and in such a way that the results might furnish some guide as to judging of the value of ether when required for the purposes of hygrometry.

A thermometer, with its bulb coated with cambric muslin, was used for this purpose. Being at the temperature of the air, ether was poured on it, and the maximum depression occasioned by the evaporation was marked. In the case of my ether, the thermometer sunk to 69°, the air being 86, and a moist bulb thermometer marking 80°. In the case of Dr. Abel's ether, it sunk to 30°; or 56° degrees below the temperature of the air, instead of 17°, the effect of the other. This was sufficient to show the difference of the two liquids. I next tried a very strong brandy, it gave 73°, being 13° below the temperature of the air, and within 4° of the effect of the bad ether. This further strengthened my conjecture that the ether I had been using was a mixture of alcohol and ether, in which the latter bore but a small proportion.

We may draw the following conclusions from these experiments; 1. That it is an error to suppose that pure ether may not be kept even in a temperature of 96°. The above extract from my paper is therefore erroneous; and the conclusion, that Daniel's Hygrometer is useless in India, is *pro tanto* invalidated. 2. That ordinary ether is a mixture of alcohol and ether, and being, in consequence of the heat of the climate and the greater volatility of the ether, subject to deteriorate, becomes, in a short time, perfectly powerless for all purposes of hygrometry. 3rd. That a simple and infallible test is at hand, by which the purchaser of ether may always ascertain its value (I mean as a hygrometrical agent). It is, to take a thermometer with a small bulb, coat it with cambric muslin, and then pouring ether on it, observe the maximum depression produced. If this does not amount to between 50° and 60° in the dampest weather, the ether is not so good as it might be. If it does not amount to 40° he had, probably, better not meddle with it. In dry weather the depression will be something greater.

Though it be evident, from what precedes, that I had so far underrated the value of Daniel's Hygrometer in India, I am still of opinion, that it is an instrument fitter for the laboratory or the closet, than for the purposes of the traveller or the every day observer. As a standard instrument, it is both elegant and effective; but for observations to be frequently repeated, it is (to say nothing of the expense of the ether, or even of the difficulty of procuring it of a sufficiently good quality in India,) far from convenient. In this respect, the moist bulb thermometer is certainly preferable; but let the indications of the latter be checked by frequent comparisons with the former. That it is not easy to procure ether of the proper quality I know, from the testimony of several friends who have been equally disappointed with myself in attempting to make use of this instrument. In your third number, p. 82, another of your correspondents makes the following remark: "In atmospheres, nearly deprived of moisture, Daniel's method becomes nearly useless, from the great degree of cold required to produce deposition." This remark would not, perhaps, have been made, had such ether, as that with which we experimented, been available.

Daniel's instrument, certainly does require the most powerful ether, from the indirect manner in which it is applied to produce the effect. It is applied not to the part of the instument required to be cooled, but to a distant part connected with it; much of the power of refrigeration is by this means lost. The circumstance has, in fact, been made an objection to this form of the instrument. In your 10th number, you gave insertion to a short notice I sent you of some improvements proposed on this view of the subject. Of these, I am inclined to prefer Mr. Cumming's, which there *is little doubt would afford results*, even with the worst ether, perhaps even with pure alcohol; a still simpler arrangement would be the following:—Suppose an exceedingly thin glass capsule, of such depth and surface as would ensure the ether poured into it speedily cooling many degrees below the temperature of the air; a very delicate thermometer suspended in this would indicate the precise temperature at which moisture should become visible on the sides of the glass capsule. In this way no more ether would be used than what was absolutely necessary, the apparatus would be exceedingly portable, and it is probable it would be an effective instrument even with very bad ether. A hemispherical cup, similar to the half of the dew-ball of Daniel's instrument, with a thermometer, having a very small globular bulb, would, I should think, answer completely; saline mixtures would, as mentioned in my first paper, also answer; but I have found an objection in practice to this method, that it is difficult to hit the exact point at which moisture begins to deposit itself. Ether from its *gradual* evaporation, would enable the observer to be more accurate, with less attention . the former often requires several trials.

Another conclusion of, perhaps, real importance, is derivable from the above experiment. I mean the great impurity of the ether used in this country as medicine. Its power, when pure, is so great as well as peculiar, that we can easily imagine a mixture of alcohol and ether may fail to produce equivalent effects. This is a subject, however, which I must leave to your medical readers, merely suggesting, that if the view here indicated be worth attention, the test, already described, will always enable them to distinguish pure from alcoholic ether. This test has even a wider range of usefulness,—for it is equally applicable to the determination of the strength of alcohol or spirit, in every stage of dilution. This particular application of the thermometer I have long had in contemplation, and have been engaged in a series of experiments on the subject, from which I hope to show that it is one of the most convenient hydrometers that can be used; and not inferior in accuracy, for any of the purposes required in commerce, to the more expensive and frequently troublesome instruments commonly employed. I hope to give you, ere long, the result of my meditations on this subject for publication in your work.

I am, Mr. Editor,

Your obedient Servant,

D.

Postscript.

Since writing the above, I have met with a paper, by Mr. Adie, in Brewster's Journal of Science, No. 1. N. S. on this subject, the results of which are important. I think Mr. Adie deserves great credit for the communication and the experiments on which it has been founded; for, to detect and expose the errors of an instrument generally confided in is scarcely a less benefit to those who give their time and attention to observation, than the invention of a new and perfect instrument. The details brought forward in this paper, form an additional reason too for observers to take nothing on trust, but "to try all things and hold fast by that which is good."

In all that I have advanced on the subject of Daniel's Hygrometer, I never questioned the accuracy of the instrument, provided it could be made to act. The fact was, that never having succeeded in obtaining a result from it, on account of the badness of my ether, I could not judge whether its indications would be correct or not. Mr. Adie at once asserts, that it is not only objectionable in principle, but that it actually gives erroneous results; never scarcely showing the true dew point, and in a mean of 28 observations, giving a dew point 3° higher than the truth.

Mr. Adie begins his paper by adverting to Mr. Daniel's objections to Mr Jones' simplification of his instrument, briefly noticed in my communication, published in the number of the Gleanings for October, p. 309, and asserts, that the same objection applies to his own, but in a reverse sense. He observes, "The ether inclosed in the bulb on which the deposition is observed, being cooled by the evaporation from its surface, the whole mass must acquire the temperature from the conducting power of the fluid alone; and as the enclosed thermometer is half immersed in the ether, and half exposed to the temperature of its vapour, while the deposition takes place only on a zone at the surface of the ether, a zone only, on the bulb of the enclosed thermometer is exposed to the dew point temperature, the other part retaining the temperature of the ether below, and of the vapour above; thus the instrument gives a dew point always at a higher temperature than the truth."

This point, he states, was noticed by Mr. Foggo, of Leith, who in consequence recommended the simplification proposed by Dr. Coldstream of Leith, nearly the same as that of Mr. Jones'. This is the arrangement mentioned in my communication, p. 309, but attributed to Mr. Foggo. But Mr. Adie shows, that the objections applicable to the instrument of Jones applies also to this modification; as there is no certainty that the whole of the bulb will be of the same temperature, when only half of it is subjected to the refrigerating power of evaporation, the other half being exposed to the atmosphere. In the case of a globular bulb, the error was 4°,75 on a mean of 28 observations; in the case of a cylindrical one 6°,6.—The method by which these deductions were obtained, is that of Le Roy, described in my paper on Hygrometry, p. 46, erroneously attributed by Mr. Adie to De Saussure.

In conclusion, Mr. Adie proposes the following arrangement, as preferable either to Mr. Daniel's or Mr. Jones' instrument. A globular case of black glass is provided to fit over the bulb of an ordinary thermometer, with an adjustable collar for fixing it. The space between the bulb is fitted with alcohol, and the outer bulb being covered with black silk (excepting a small spot about a quarter of an inch in diameter), ether is applied to it. The thermometer, being then agitated by the hand, ensures a speedy and complete distribution of heat, and when the temperature is sufficiently reduced, moisture is deposited on the open spot of the black bulb. On a mean of 28 observations, the error of this instrument was only 1° and the greatest error 5°; whereas in Mr. Daniel's instrument, the error was occasionally 7°, and frequently 5°.

The following remarks may, I think, be made on Mr. Adie's communication. *First.* It is quite clear that he has very much simplified Daniel's instrument; thus, rendering it not only cheaper and more portable, but also enabling all those who have a common thermometer to convert it at once into a dew point hygrometer. And this has been effected, certainly, without any sacrifice of accuracy. *Secondly.* It does not appear to me, that the above objections of Mr. Adie, to Daniel's Hygrometer, are insuperable; for may not the same agitation of Daniel's instrument, which he requires with his own, be sufficient to ensure an equally accurate result. This remark, even supposing it well founded, does not diminish the value of Mr. Adie's suggestion, however, in the view of convenience, portability, and economy; but I think it right to notice the subject, as well for the sake of doing justice to all parties, as for the satisfaction of those who may have purchased this instrument, which supposing the above suggestion to be well founded, will for the laboratory, perhaps, continue to be preferred, though for the purposes of a traveller less convenient, whatever its accuracy. *Thirdly.* I should think a cylindrical bulb more likely to give satisfactory results than a globular one, in as much as the globe has, of all solids, the least surface in proportion to its quantity of matter. Now it is through the surface that the cooling process is to take place. *Fourthly,* I should have preferred a metallic to a glass surface. It appears by the experiments of Gay Lussac and of professor Leslie that metallic surfaces have a greater attraction for water, in the liquid state, than glass; and I have verified the fact myself, having repeatedly obtained deposition on a metallic surface that had its temperature 1 ° higher than a glass one undewed. The non-conducting nature of the glass is an objection, although, perhaps, not so great as might be at first thought, as the heat of the thermometer is also to be abstracted through glass.

In conclusion, I may say, that, perhaps, the arrangement I have suggested in the body of this letter, may be as convenient as Mr. Adie's : it will certainly be as accurate. If made of metal it might be preferable. This point, however, requires some further elucidation.

V.—*Description of the Animal of Melania,*

A genus of fluviàtile Testacea ; with remarks on its habits, and the characters of five species inhabiting the fresh waters of the Gangetic Provinces.

The following description of the animal inhabiting a shell, regarding which Cuvier remarks " on connait pas bien l'animal," will hardly require further apology for its introduction. The only notice of the animal, given by Lamarck, in his description of the family to which it belongs, is contained in the statement that the shells of which it consists have two tentaculæ and a horny operculum, a description which is rather vague, and calculated to lead to its classification with many testacea very differently constructed.

Melania. Animal with two setaceous tentaculæ proceeding from the broadest part of the head, tentaculæ bearing the percipient points[1] on their upper sides, at a short distance from the base.

Head panduriform ; snout elongated, bilobed anteriorly ; the mouth occupying the sinus between the lobes, and extending longitudinally underneath ; lobes short, rounded.

Foot or *disk* subquadrate, rounded posteriorly, anteriorly clypeate, punctate underneath.

Mantle furnished with short filiform processes [2], which clasp the right lip while the animal is crawling, perhaps occasioning the transverse furrows observable on the exterior of the shell.

Operculum horny, thin, concentric lamellar.

In the position of the percipient points, Melania differs considerably from Paludina, which also has two tantaculæ furnished with percipient points. In Melania they are situated on the posterior or upper side of the tentaculæ, which are not disproportionately swelled below the points ; while in Paludina they are salient from the exterior side of the tentaculæ, which appear as if double below them.

The Melaniæ are found in sandy and gravelly parts of rivers, seldom in clay or mud. They appear to delight in clear streams, and are partial to a shelving shore shaded by a high bank. I have never met them in jheels or standing waters, so that they may be strictly called fluviatile. Their occurrence in the great lakes of North America does not militate against this fact, as those waters are connected by

[1] Sir Everard Home having ascertained, by dissection, that the organs, usually called eyes in snails, are merely the termination of a nerve, the term " ocular point," which is still used to specify them, is as objectionable as the word " eye ;" no other appellation having been substituted, I propose the term " percipient point" as sufficiently precise, and expressive of the nature of the service which, there is reason to believe, this organ is meant to render to the animal.

[2] The description of the animal was taken from the small species of the Betwa and Cén, as I did not examine minutely the animal of the large species, at the time that I had an opportunity of taking them alive. I cannot remember observing the fimbriated mantle, which the smaller species exhibit ; but, from the dark colour of the animal and the exterior of the shell, it may easily have escaped observation. The larger species of Paludina exhibit, at times, the same appearance on the mantle. I have not been able to discover in Melania the breathing tube which, in Ampullaria, is formed by the mantle.

running streams, and are generally in a state of agitation. When placed in a vessel of water, and furnished with sand, they quickly bury themselves. This habit accounts for their not being easily met with in calm weather. During the prevalence of a stiff breeze, the rippling of the water beats up the sand into ridges and furrows, and discovering the retreats of the Melania, they may be observed crawling close to the lee shore, endeavouring to secure themselves by adhering to stones; or when the breeze has ceased, they may be traced by the furrows which they form upon the surface of the sand, in their progress to deep water. The small pools, left by the rivers in their fall, sometimes abound with them. I once saw a Melania bury itself by first inserting the summit of its spire in the sand, and then crawling gradually backwards: more usually the foot is the first part buried, the shell following by degrees. When they have made good their retreat under the sand they do not remain motionless, but continue to burrow parallel with the surface; probably seeking their food, which, from the situation in which they usually occur, I do not believe to be vegetable, unless very minute, and in the form of fecula.

The utility, to the Melaniæ, of this habit of burrowing is obvious. Were they not endowed with it, they would be liable, during the height of the rains, when the rivers are rushing down like torrents, to be swept away to the sea; as it is, they securely abide their fury, protected by their sandy covering.

The Melaniæ are ovo-viviparous. I have taken between 40 and 50 minute shells from the body of the large species marked A; the animal having ejected them, when plunged into boiling water for the purpose of freeing it from the shell.

I shall here give the characters of the five species which I have met with, refraining from naming them for the reason stated in my notice on Ampullaria.

Sp. A. Shell elongate-turreted, solid; epidermis varying from piceous to olive brown; whorls convex, with longitudinal ribs, which are nodulous on the shoulders of the lower whorls; several obsolete transverse ridges or rugæ on the last whorl, the central one most prominent. Interior of the aperture violaceous-grey, with a satiny lustre. In the young shell there were two reddish-brown bands, running transversely on the centre and base of the lower whorl, which become fainter as it approaches maturity. The foot and spiral extremity of the animal are of a blackish colour, the centre of the body being of a light orange red.

This species attains the length of three inches; it occurs plentifully on a shelving bank of calctuff-gravel (*bájri*) in the Gumti, at Juanpúr, keeping close to the edge of the water. I have also received specimens from the waters North East of Ayoodhya (Oude.)

Sp. B. Shell subulate-turreted, translucent; whorls depressed, with longitudinal and transverse rugæ, which give a decussated appearance to the upper whorls. Colour pallid, with bands of red-brown dots, or irregular longitudinal streaks of the same colour, sometimes altogether devoid of markings.

This shell occurs alive in the Gumti, Yamuna, Betwa and Cén rivers. I have met with the exuviæ in the Ganges. In this and the next described species the foot is of a pallid colour, with brownish black markings. The body is light verdigris-green.

Sp. C. Shell ovate-oblong; whorls convex, ribbed on the spire, and crossed by numerous transverse rugæ; ribs coronated by tubercles; colour pallid olivaceous, with irregular longitudinal red-brown streaks.

In a variety from the Cén, the tubercles of the shoulders are produced into spires, giving the shell somewhat of the appearance of *M. amarula*, and causing such a variation in general form, that, were not the two extremes gradually connected by a series of specimens in my possession, I should be disposed to regard them as separate species. This species occurs in the Gumti, Betwa, and Cén.

Sp. D. Shell ovate-conical; whorls flattened, with obsolete longitudinal ribs, and numerous distant transverse rugæ, of which the upper one is more prominent, and nodulous, with a groove corresponding with it in the interior of the shell. Epidermis blackish-brown.

I have never met with a living specimen of this shell.

It occurs in the Gumti, at Juanpúr.

Sp. E. Shell elongate-turreted; whorls convex, longitudinally ribbed, and crossed by transverse rugæ; shoulders tuberculated. Colour as in the third mentioned species.

This shell was brought to me from the waters N. E. of Ayoodhya. It appears to be intermediate between the species marked B. and C. I have only a single specimen.

It is probable, that a shell which Le Gentil mentions as common in the river d'Embale, in Madagascar, and which he describes as "une grande vis" three inches in length, with 13 whorls, decollated, furnished with long spires, a brown epidermis, and an operculum, is referable to this genus. He says: "it lives in the sand, whence it is drawn by the blacks at low water, and is eaten in great quantities." Perhaps the large Melania of the Gumti might afford a delicate morsel to our Eastern "*gourmets*;" Le Gentil, however, does not inform us how they are prepared.

It is desirable that the animal of Pirena, which inhabits the marshes of Amboyna, and, perhaps, also of Southern India, and which Lamarck, from inspection of the shell, placed in the same family with Melania, should be compared with the description of the latter, and their points of similarity or difference noted.

W. H. B.

Bundelkhand,
October 1829.

VI.—*On the Scale of Temperature; with Remarks on the Graduation of Leslie's differential Thermometer.*

To the Editor of the Gleanings in Science.

Sir,

The writer of the remarks, in your number for September, on the scale of temperature, has, in recommending the adoption of the universally condemned Daltonian scale, given a partial view of the subject.

Did all bodies, like water, suffer a diminution,—and the *same* diminution,—of capacity for caloric, concurrent with the rise of their temperatures, the Daltonian and common scale would be on an equality in point of truth; the former indicating the *actual*, and the latter the *potential* or *sensible* quantity of heat: but as probably all substances differ, not only in their capacities for caloric at one temperature, but each in its own capacity at different points of the thermometric range, no useful information could be derived from a scale adapted to the varying capacities of any one substance, unless that substance were the very fluid constituting the thermometer; and in the case of a mercurial thermometer such a scale would not sensibly differ from the common division into equal parts, the modifications required by the slightly varying capacities of mercury being so minute as to fall within the limits of error in ordinary graduation.

Mr. Dalton's proposal, however, differed widely from this. With the increase of their temperatures it is probable, that all bodies acquire a small progressive increase of capacity for caloric,—all except water, which with the increase of temperature suffers a great diminution of capacity for caloric; and water, the striking exception, Mr. Dalton would make the general rule, dividing the scale according to the quantities of heat which it *alone* required for attaining a given temperature. He might with equal advantage have perplexed the scale with some representation of the gradual shrinking of water during its passage from 32° to 40°, both circumstances being mere exceptions to the general laws of heat, and perhaps both equally referable to the undermentioned final cause.

The latter anomaly has been frequently pointed out as one of the instances in which the Author of Nature has, to attain some beneficent end, reversed the rule which he had previously ordained for the government of the universe; and the former may, perhaps, be regarded as another portion of the same great design. As the fixing of the maximum density of water at 40° preserves, at that temperature, the deeper parts of the seas, lakes, and rivers of cold climates, which would otherwise be annually frozen throughout, so does its simple chemical constitution (its elements having very small combining weights, and the compound an inversely great capacity for caloric,) enable it to resist, with slight variations of its own temperature, the great changes of heat and cold which the varying influence of the sun would occasion in a fluid having a smaller capacity for caloric. And, finally, it is still further adapted, at moderate temperatures, for the residence of the finny tribes, by the reversal of the usual law, and made less susceptible of those changes precisely at those temperatures which best fit it for this purpose; the whole system being not less beautiful than that by which atmospheric climates, from the equator to the pole, are attempered.

These reflections ought to make us satisfied with considering water as, in this particular, a solitary exception, and to prevent us from endeavouring to extend its phenomena to other bodies, by experiments planned and executed with a bias, like those on mercury mentioned by your correspondent.

This subject tempts me to offer some remarks on the scale of Leslie's Differential Thermometer, which protean instrument, in its hygrometric form, you have discarded from your meteorological table in favour of the moistened bulb thermometer,—notwithstanding the extravagant anonymous commendations of it by its inventor, if he may so be called, as the only difference between it and Van Helmont's Thermometer consists in an alteration which is no improvement to its accuracy.

Van Helmont's Thermometer was in the shape of the letter U, one end being open to the atmosphere, while the other was furnished with a bulb, which bulb being filled with air, and the tube *under* it with a coloured fluid, any increase of temperature in the bulb was accompanied by a rise of the coloured fluid in the *opposite* tube. While the atmospheric pressure continued uniform, this instrument would form, bating the slight variations of hydrostatic pressure in the open tube, a perfect thermometer, each additional degree of heat acquired by the bulb being indicated by an equal rise of the fluid in the open tube. But as the volume of the air in the bulb varied not only with the temperature, but with the pressure also of the atmosphere, its value, as a thermometer for general use, was thereby greatly diminished.

An obvious mode of excluding this variable pressure was to shut the open end of the tube; adapting to it, of course, a bulb full of air, to allow, by its compressibility, of the rise of the coloured liquid. This was Leslie's alteration ; but as he still continued to attach a scale of *equal* parts to the instrument, it was any thing but an improvement : in fact the instrument then ceased to be a thermometer, sinking into a mere thermoscope, which indicates a difference of nearly 60 centesimal degrees, when the actual difference of temperature between the two bulbs is only 50° ; thus erring by 18° of Fahrenheit's scale ! I have had no opportunity of experimentally verifying this conclusion ; but its truth, though hitherto unnoticed, will appear from the following considerations.

Two points, in Leslie's scale, are necessarily correct, the zero being marked when the temperatures of the two bulbs is the same, and 1000° when the difference between their temperatures is 100° of the Celsian scale, or 180° of Fahrenheit's, the former decupled having been adopted by Leslie, probably to facilitate the introduction of his instrument upon the continent. But he divides the space between 0° and 1000° into *equal parts*, forgetting, that while the liquid rises in the scale-tube, the increasing re-action of the compressed air in the bulb of that side diminishes the extent through which each successive degree of temperature, added to the warmer ball, is able to propel the fluid ; and that the scale ought therefore to be *logarithmically* graduated, the lowest degrees being made about twice as large as the highest.

The ratio of diminution is thus ascertained. All gases in being heated through 180° of Fahrenheit's scale, acquire an addition of $\frac{3}{8}$ to their volume, when exposed to a uniform atmospheric pressure. Therefore, as 8 : 11 : : 1000 : 1375 ; the index which rises through 1000° would rise to 1375° of Leslie's equal degrees, if the tube were long enough and open at the top, as in Van Helmont's original construction ; the two half degrees,—0°,5 + 0°,5 of the logarithmic scale should together be made equal to 1,375° of Leslie's ; and the succeeding degrees up to 1000° be so divided that the point 1000° of the new scale shall coincide with the point 1000° in Leslie's.

The subjoined table, constructed on the above data, shews the respective indications of Leslie's arithmetical and the proposed geometrical division, at each 10° of the latter, throughout the ordinary hygrometric range ; stating also, in degrees of Fahrenheit, the amount of error in the former.

Geometrical Division in millesimal degrees.	Arithmetical division in millesimal degrees.	Difference in degrees of Fahr.	Geometrical division.	Arithmetical division.	Diff.
0°	0°	0°	90°	119°,7	5°,3
10	13,7	0,6	100	132 ,5	5 ,8
20	27,3	1,3	110	145 ,9	6 ,3
30	40,8	1,9	120	157 ,9	6 ,8
40	54,2	2,5	130	170 ,4	7 ,8
50	67,5	3,1	140	182 ,8	7 ,7
60	80,7	3,7	150	195 ,1	8 ,1
70	93,8	4,2	160	207 ,3	8 ,5
80	106,8	4,8	1000	1000	0 ,

How is this to be reconciled with the "refined" calculations, on the scale of this "nice" and "delicate" instrument, in the Supplement to the Encyclopædia Britannica? Will your able correspondent D. favour us with his opinion on the subject?

D. B.

September 26th.

VII.—*Census of Benares.*

A careful census of the population of this celebrated city has just been completed, from which it appears that the accounts formerly published of its magnitude have been very greatly exaggerated.

In the year 1800 a numeration of the houses was taken, from which it was calculated that the population amounted to 600000: and a subsequent estimate, made a few years afterwards, raised this extravagant result to 800000: in both of these cases the number of houses seems to have been correctly ascertained, and the error lay in the rate of inhabitants assumed for each species of house, which exceeded all bounds: thus, for a six storied pukha house, 150, and for a single kucha house 60 persons were set down.

From the present census it appears, on an average of the whole, that six inhabitants is a fair rate for all sorts of houses, whether in the town or the vicinity; and this accords pretty well with Mr. Bayley's published register of the population of Burdwan.

It must be remarked, however, that "house" does not exactly represent "chouk" or "quadrangle," the expression used in the register: a large house generally consists of several chouks, which are either occupied by different branches of a family or let to different lodgers.

From a fear of exciting suspicion in a town hitherto considered to be rather tender of interference and scrutiny, it was not thought advisable to permit the natives employed to reckon separately the males and females: but on a revision of 17 muhullas with a view to determine its accuracy, this point was also effected without exciting the slightest ill-will.

As far as so small an average permits me to draw conclusions, the male and female population are nearly equal. There is a disproportion of female children, however, which must be attributed to the reluctance with which the natives mention that branch of their family, frequently including the girls under the general term of "lurkè."

The proportion of children, as might be expected, is much less in the city than in the villages around Secrole. The proportion of lodgers to proprietors is on the contrary much greater in the town. Benares is also a place of continual resort for travellers of every description, who may not, perhaps, be included in the census. Upon religious occasions and melas there is a very great afflux of visitors, who cover the ghâts with their little encampments. On one occasion an attempt was made to count the people who flocked in by the principal roads and ferries for three days previous to a solar eclipse; the number actually counted was nearly 40,000, and the probability is that it exceeded 50,000.

In round terms, the population of Benares may be safely called two hundred thousand, so that it is still entitled to the name of a first-rate city, being on a par with Edinburgh and Bristol; four times as large as Brussels or Rotterdam; and eight times greater than Geneva.

The number of houses has increased about 1½ per cent. since 1800: there are, however, a number of houses in a ruinous condition. It is singular that the number of musjids counted should be precisely one-third of the Hindoo temples. Many of both must, however, have escaped insertion where they were insulated, or in the outskirts of the town.

A check census of the different castes and trades obtained through the *chaodris*, &c. is now under revision, whence will be found the proportion of Hindoos and Moosulmans, &c.

It does not fall within the province of a Scientific Gleaner to enter more at large on a subject of the present nature, and we must be contented to present our readers with the following abstract of the results, distinguishing those which appertain to the city itself, from those of the European station of Secrole.

Summary.	*In the city.*	*In Secrole and the vicinity.*
Number of Inhabitants,	1,81,482	18780
Number of *Chauks* or houses,	30,205	2880[1]

1 This does not include the servants' houses in European estates, nor the soldiers' huts, &c. in the lines; although these are reckoned in the population.

Muhallas, or divisions,	369	21
Pukha houses (of brick and stone,)	11325	73
Kucba houses (of mud,)	16552	2639
Kucha pukha ditto (of mixed nature,)	2328	88
Houses of one story in height,	12590	2444
,, two, ,,	11838	282
,, three, ,,	2996	2
,, four, ,,	1019	
,, five, ,,	200	
,, six, ,,	7	
,, seven, ,,	1	
Ruins cf houses and building spaces,	1498	72
Gardens,	174	
Siwalas, or Hindoo temples,	1000	7
Musjids, or Moosulman Mosques,	333	5
Comparison of the accuracy of the census, by re-exmination personal,	in 1827-28 17 muhullas contg. 8932; in 1829,	8814
Proportion of Proprietors to Lodgers in second examination.		
Proprietors,	4310 } equal,	7753 } =5 : 1
Lodgers,	4510 }	1684 }
Proportion of Males to Females.		
Men,	3424	3354
Women,	3564	3151
Boys,	1085	1698
Girls,	741	1234
Proportion of Adults to Children,		
Adults,	6988 } =4 : 1	6505 } =$\frac{2}{4}$: 1
Children,	1726 }	2932 }
Proportion of Inhabitants per Chouk,	6	6
Europeans and East Indians in Secrole, (including 125 artillery,)	399	

Postscript.

The population of Benares, as since ascertained from the principal people of each caste and trade, may be distinguished under the following heads.

HINDOOS.

Brahmuns,	Maharashtr,	11 castes,	12000	
	Nagur,	7 ,,	3000	
	Mor,	12 ,,	600	
	Oudeech,	7 ,,	1200	
	Gour,	8 ,,	2000	
	Kunoujia,	4 ,,	7000	
	Kherewal,	7 ,,	1600	
	Bengalee,		3000	
	Gungapootras,		1000	
	50 small castes together,		3600	35,000
Khutree Caste.	Rajpoots,		6500	
	Bhooihars,		5000	
	Khutrees, 5 sects,		3000	14,500
Bueshya Caste.	Ugarwalas,		2000	
	Kusrwanees,		2500	
	20 small castes together,		3500	8,000
Shoodr Caste.	Kaeths,		7500	
	Koerees,		8500	
	Aheers,		5500	
	Kuhars,		5000	
	Kulwars,		6500	
	55 other trades and castes,		37000	70,000
Hindoo Fuqeers of 11 denominations,				6,500
Total Hindoo Population,				1,34,000

MOOSULMANS.

Ruees, or persons of independent income, Shekh, Sueed, Moghul and Patān,	10,000	
Joolaha, or Weavers,	15,000	
Tuwaeefs,	1,800	
41 different professions and trades,	4,500	
Fuqeers, of 80 Tukeeas,	500	
Chundāl, or lowest cast,	800	
Total Moosulman Population,		32,600
Remainder, supposed to be composed of travellers, visitors, &c. and children omitted in the Choudree's computations, with the unavoidable omissions of the catalogue itself,		13,400
Population of the City of Bunarus as by house census,		1,80,000

VIII.—*On the Geological and Mineralogical Structure of the Hills of Sitabaldi, Nagpùr, and its immediate Vicinity: by the late* H. W. Voysey, *Esq., H. M.* 67*th Foot.*

[From the 17th volume of Asiatic Researches.]

The hill of Sitabaldi, although agreeing in form and interior structure with other basaltic hills in its neighbourhood, merits a more particular description on account of some peculiarities in the composition of the main rock, hitherto unnoticed by geologists, and for the opportunities afforded by its extensive quarries of studying the varied structure of the rocks of the trap family, which is rarely to be seen in so distinct a manner.

The mass of the hill is composed of porous basalt, with a semi-columnar appearance, derived from numerous vertical fissures. It passes in some places, both in a gradual and abrupt manner, into a coarse porous wacken or indurated clay, which in its turn changes in a similar manner to the nodular basalt or wacken, of which the northern and southern summits of the hill are composed. At the junction of these rocks, the passage is sometimes so gradual as to give the intermediate rock an indeterminate character partaking of the nature of both. At others it is abrupt; yet, notwithstanding the abruptness of the change, the vertical and horizontal fissures are prolonged into each, and cross the line of junction. I shall not here enter into a greater detail of these appearances, but shall content myself with observing that the most satisfactory explanation of these phenomena is derived from that theory which ascribes to the trap rocks an igneous origin, under pressure of a great body of water.

The semi-columnar basalt forming the greater part of the hill is very porous, containing numerous amygdaloidal cavities, which are, for the most part, merely lined with a peculiar mineral, which I presume to name conchoidal augite; sometimes, however, they are nearly filled with it, or with calcedony, semi-opal, or carbonate of lime; the calcedony being usually covered with a coating of green earth. The rock itself is composed of hornblende and felspar, with the augite, so profusely disseminated, as to claim a right to be considered, in some cases, as a constituent of the rock. This is, I believe, the first time that conchoidal augite has been found entering into the composition of basalt. The rock is fusible, and is of considerable specific gravity, notwithstanding its porosity. The vertical and horizontal fissures are not always straight, but are at times waved; they are also, sometimes, lined with an infiltration of calcedony coated with clay and chlorite.

This rock is succeeded by an indurated clay or wacken, which at its junction with the basalt, frequently partakes of its semi-columnar structure, the vertical fissures being prolonged into the wacken, and the schistose structure of the latter extending into the basalt: these changes may be favourably seen in the fosse which surrounds the fortification.

The indurated clay or wacken seems to form but a small proportion of the hill, as it is not seen in the hollow between the greater and less elevation, the semi-columnar basalt being there uncovered by any rock. It also contains crystallized carbonate of lime and semi-opal; but calcedony, coated with green earth, is the most common mineral found in it.

This rock passes into the nodular wacken, which, on a casual inspection, appears to be a collection of stones rounded by attrition, and involved in a matrix of clay; when examined more carefully, it is evident that this appearance is owing to a peculiar modification of the concretionary structure, developed by decomposition. Nuclei, of various sizes, are enveloped by concentric lamellæ, which peel off as decomposition destroys their cohesion. They are consequently seen in various states of decay, and of sizes, varying from several feet in diameter to several inches. But their true nature is easily discovered by the mutual indentation of the different lamellæ, which surround their respective nuclei, the centres from which this pseudo-crystallization has proceeded. The existence of the vertical and waved fissures need scarcely be adduced as farther proof that they are not the product of alluvial *detritûs*.

It appears most probable that they owe their forms to molecular magnetic attraction, since they contain a very large proportion of oxide of iron, (nearly twenty-five per cent.) as may be perceived by the great specific gravity of hand specimens.

The nodular wacken or basalt, is one of the most common forms of trap in the extensive districts, composed of the rocks of this family, south of the Nermada.

It occurs perpetually in the extensive and lofty range of mountains situated between the Purnea and Tapti rivers, and appears to form their principal mass. It is found equally abundant throughout the whole of Berar, part of the provinces of Hyderabad, Beder, and Sholapùr, and appears to form the basis of the great western range of trap hills, which separate the Kenkan from the interior of the Dekhin.

It is probably one of the main sources, when decomposed, of the black diluvial soil to which Hindoostan owes so much of its fertility. The valley or extended plain of Berar, that of Hasanabad, of Seronj, of Nandiala, south of Krishna, of the Palnad, bordering the Krishna, and numerous others, all lie near the course of rivers, which at some former period have covered these plains and formed their extensive deposits of alluvium. Whether the deposition originated in some sudden and partial inundation, or whether it was owing to the gradual subsidence of the waters of the great deluge, I think may be determined by cautious investigation. I am at present inclined to think that the most probable cause was the latter.

The hill of Sitabaldi offers favorable opportunities, if the quarries are extended, of ascertaining positively whether the basalt is merely a superficial deposit, or is deeply connected with a mass beneath. It is surrounded on all sides by gneiss, or slaty granite, which is found at the base of the hill a few feet deep. Perhaps Nagpùr affords more opportunities than any other part of India, of studying the geological history of these rocks, as it is situated near the junction of the primary and overlying rocks. Numerous opportunities must arise during the excavation of wells and baths, of ascertaining the connexion of the strata beneath. A well in Mr. Alex. Gordon's garden, near the base of the hill, of about forty feet depth, penetrates through three or four feet of black soil, succeeded by a magnesian siliceous clay, which appears to owe its origin to the decomposition of the gneiss, by which it is immediately followed, and which continues to the bottom of the well.

From the summit of the hill of Sitabaldi the difference in the outline of the rocks eastward is very perceptible. The flattened summits and long flat outline with the numerous gaps of the trap hills, are exchanged for the ridgy, peaked, sharp, outline of the primary rocks. At Ramtek and its vicinity the rocks are of granite and gneiss. At Dungari, at Palora and Parsùni, are found crystallized marbles passing into gneiss, capable of receiving a fine polish: some of them contain a small quantity of carbonate of magnesia. At Khorari, a dolomite or magnesian marble is found, also in gneiss. At Nayakund, Parsùni, and the bed of the Pesh river, granite and gneiss of various kinds, also quartz, rock, and sandstone; and foliated black manganese ore is in great quantity.

IX.—*On the Quantity of Water raised by the* Dence.

To the Editor of the Gleanings in Science.

SIR,

It is with much gratification that I have seen so much of the attention of your correspondents devoted to the most efficacious means of raising water, believing that the fertility of the valley of the Ganges may be carried by improvements in irrigation to an extent beyond present credibility. The first point, however, towards any improvement is to settle the value of the simple machines now employed by the natives for raising water, and I have pleasure in being able to communicate to you the enclosed extracts from the observations of a gentleman during a tour in Patna and Behar on the comparative merits of the lever and *mót*. The former I should consider as in general much underrated. When water is to be raised from short depths—say from 10 to 20 feet—its extreme cheapness, and the facility with which it may be erected and transferred, and the means it thus affords of being able to multiply upon a particular spot any number of levers that may be required for even very temporary purposes, render it, I conceive, a valuable machine, and deserving of more notice than it appears to have met with in the neighbourhood of Calcutta; but I have seen no part of India where irrigation is so much neglected as in Bengal, and particularly in the vicinity of Calcutta. Notwithstanding the immense quantity of ground within 10 or 15 miles of the city that is dedicated to the culture of vegetable crops, it is very rarely that recourse is had to any other means of watering the gardens than by hand, with common earthen pots; and this system is even the prevailing one in irrigating most of the extensive potatoe crops west of the Húgli, although the quantity of water required for the potatoes in the sandy ground between the Húgli and Damúda is very great, and the waste of labour must be in consequence enormous. The same is also applied to the watering of the soot crop. To the S. E. of Calcutta, after the rice crop is gathered from the lower lands, almost the whole of these lands are allowed to remain uncultivated for the remainder of the year; though I conceive there is not a doubt but the greater part could be made to return a good cold-weather crop of pulse or vegetables, if proper attention was paid to preserving the rain waters, or of obtaining it by sinking wells, and to the means of raising the water advantageously. In many high or badly-watered parts of Bengal, the rice crop is very precarious: and in one of these spots, not long ago, I observed that the farmers had lost their rice, almost entirely, over a very extensive tract of country, and there was no attempt made to raise any other crops to supply the deficiency of grain, which was severely felt by the inhabitants, owing, I presume, to their supposed inability of assisting what they might then sow by irrigation. But the water was, I should imagine, abundant; the tanks were large and numerous; and judging from the bed of the Damúda, I suppose water would be found in wells at a very high level, for the stream of that river is scarcely any where 15 feet below the level of the country.—The most usual machine employed by the Indigo planters for raising water, is I believe, the China pump. I do not recollect seeing any account of its performances, but it will be in the power of many of your readers, no doubt, to afford you the means of comparing it with the other machines which have already been described in the Gleanings.

I. S.

Patna.—"By far the most usual method of raising water from wells, in this district, is by means of a pot suspended from a lever, which is here called *latha*. It is always here wrought by one man, who stands under the end of the lever, on the sides of the well next to the fulcrum or post. About one half of the pots used here are iron; an iron pot costs a rupee, and the lever and rope cost about 4 annas. Although one man works each lever, yet it is usual for three levers, at least, to be wrought in the same well at the same time, and still more, if practicable, is advantageous, because one man can distribute the water raised by four or five *lathas*, and one man is necessary where only one *latha* is employed. Still however one *latha* is often used, because the two men, alternately relieving each other at the lever, work with ease the whole day, while four or five men, with only one relief, can scarcely continue to work so long without much fatigue. Many *lathas* are employed, where the well is from 25 to 30 cubits deep; but when the depth exceeds 15 cubits, it appears to me that this is a very tedious operation, and vastly inferior to the leathern bag raised by oxen descending on an inclined plane (*Mót*), but, except near the Ganges such are seldom used, and the kind which requires a man to empty the bag is alone employed; but in some places I perceive that one man suffices to work this machine.—He stands by the well, and empties the bag while he has trained the oxen to go down the slope, and to return, without being driven. In the interior, indeed, the wells are seldom deep, but near the Ganges the *latha* is vastly more common than the *mot*, even in the deepest wells. The reason seems to be the want of forage for the cattle in the working season. I tried an experiment on the comparative effects of these two methods of raising water. A *latha* from a well in which it was 36 feet to the water from the surface, in half an hour drew 1357 lbs. avoirdupois of water; two men usually work from sunrise until 9 o'clock, and from 3 o'clock to sunset, or rather until dark. Where the depth is moderate three men, with two *lathas*, waters from about $\frac{2}{5}$ of an acre to $\frac{1}{3}$ daily.

"Three men and two oxen work a *mot* from morning until evening, with a refreshment only of about $\frac{3}{4}$ of an hour. In a well 33 feet from the surface to the water, a *mot*, in half an hour, drew lbs. 7210, but such a superiority over the *latha* is not admitted by the natives, who contend that three *lathas*, wrought by four men, are equal to a *mot* wrought by three men and two oxen. This, however, I have no doubt is a mistake, unless where the water is very near the surface."

X.—*On the Rent and Net Produce of Land in India.*

To the Editor of the Gleanings in Science.

SIR,

A good deal of discussion has lately taken place in the Calcutta papers regarding agricultural matters, and the practicability of Europeans succeeding in a speculation of this nature, either in investing property in the purchase of lands, or as a mere farmer or labourer who rents as much soil as he can undertake to manage with his ordinary and limited means; and as some interest seems to be excited on the subject, permit me to say a few words, which, coming from this quarter, may prove of some service in elucidation of the point under discussion.

A *pergannah* here consists of about 100 villages, 8 or 10 of which pay 3 or 4 rupees per *bíga* annually to the Government, while the rest pay about 12 annas: the former villages appear to have no particular advantage as to soil, canals, rivers, wells, or natural products, and yet the inhabitants seem much more comfortable than the latter. These high rented and comfortable villages are generally possessed by Jats, who, without doubt, are the best farmers in the Upper Provinces; and if they can manage to raise the produce of the land so superior to their neighbours, may we not suppose capital could be advantageously employed in the remaining part of the pergunnah, which appears to be so backward, and consisting in the greater part in culturable waste, and by outstripping the Jat, derive a profit sufficient to satisfy every moderate wish.

For those who wish to invest capital in the purchase of land, I may add, that land liable to the Government assessment is to be had here for about 1 Rupee per *bíga*, while lands not liable may be valued at 6 or 7 Rupees per *bíga;* the latter not procurable in any quantity, and the rights probably not safe. A number of people have invested property in the former; and although their villages are by no means well managed, yet I am assured they derive a very ample revenue after paying the amount of the assessment made by Government. In purchasing lands, care should be taken to secure the *Bisawadaree* as well as *Zumeendaree* rights, both separate interests here; at least it is a disputed point, and persons who have merely purchased the former are likely to get into trouble hereafter.

It is doubted, whether the European labourer could compete with the native in this country, in consequence of the superior accommodation and food required by the former, as well as the disadvantages of climate. To the objection on the score of accommodation and food, I have to offer my opinion, that a farmsteading, such as would be considered a palace of accommodation by most of the small farmers in Scotland, could be erected in this country for £20 or 30, including a proper residence for himself and family, and huts, &c. for cattle . and as to the food, I am inclined to believe it will be found to rest in favour of the natives; his cakes well steeped in *ghí*, and accompanied by a pot of boiled vegetables, are at least equal to the Scotchman's hard water porridge, and the native has the buttermilk to himself, which in Scotland is generally carried to the market; add to this, the natives on Thursdays make the whole produce of milk into *khír*, (milk boiled up with rice and sugar,) which forms a banquet for that and a near two succeeding days. As to flesh meat, small farmers have little of that, except on particular occasions; such as it is in this country, it is selling here in the bazars at the rate of one maund or 80 lbs. for the Rupee. I suspect your Calcutta writers on the subject take their specimen of emigrants from the Dandy European artisans, occasionally to be seen on the Calcutta course, sporting their buggies, and who drink their claret; and while I make the following extract from the Supplement to the Encyclopædia Britannica, of what is considered food for a family of six for one week in Ireland, I expect they will be inclined to take a somewhat different view of the subject, or at least be induced to make further enquiries. Food for a family of six for a week, 3½ bushels of potatoes, 14 herrings, 9 quarts of butter milk, and one pound of salt. It may be supposed by people unacquainted with the country, that this allowance is scanty; I can, however, assure them, the people of Ireland are, at least, as robust as any in the world.

With regard to the remaining objection of the European not being able to stand the sun and climate, the excessive heats during the ploughing months of July, August, and September, must be very trying to any constitution. I have been employed with Europeans and natives at all seasons, and I feel confident the former are as capable of bearing fatigue as the latter, under exposure; and I have heard it confidently stated, that the Europeans on the Madras and Bombay establishments are more hardy than those at Bengal, who are kept tattied in their barracks;—an instance of something of the kind I witnessed myself in a battery, manned by men from Bengal and Madras, where five or six of the former expired from the effects of the sun, while the Madras establishment lost only one officer from a similar cause. It has also been stated, that although the European constitution may bear the climate for a time, yet it suffers in the end, as has been instanced in the campaigns of Lord Lake and the Marquis of Hastings, during which few Europeans died of diseases attributed to the climate, while afterwards deaths were frequent, supposed to have been influenced by the previous exposure.

The produce of the country has been much underrated ; I beg leave to offer the following table of a comparison of a number of crops of Wheat and Barley of this part of the country, in various seasons, with the average produce of Ireland.

Per Eng. acre.	Wheat.	Barley.
Ireland,	1380 lbs.	1772 lbs.
Upper Dooab,	1486 lbs.	1693 lbs.

It is a fact that nothing has yet been done to elucidate the resources of the country in agricultural matters ; a new turn, however, seems to be taken by Government and the public, which if persisted in must be of great advantage to the interests of both, and I trust we shall no longer hear of such mirthful stories as that of a European affirming the impossibility of raising hops in India, from a failure he experienced himself, although he took every care, and used, as seed, the best hops that could be procured out of the dregs of a butt of Hodgson's superior Pale Ale !! Ridiculous enough, Mr. Editor, but I am afraid the jest still lies in its being applicable to the want of practical men. *Fit fabricando faber.*

Upper Dooab,
November, 1829. Z.

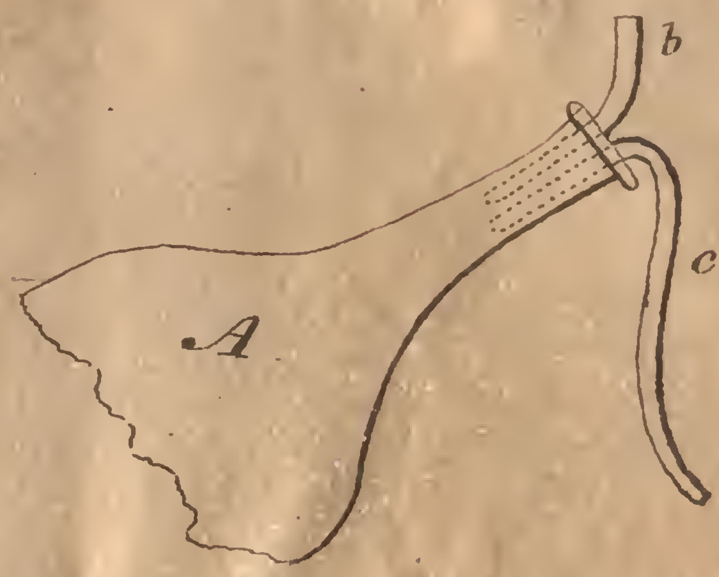
XI.—*Miscellaneous Notices.*

On a Cheap Substitute for the Safety Tube of Welter.

In a country where the price, and often the impossibility of procuring chemical apparatus and re-agents is next to a prohibition on the pursuit, whatever tends to simplify apparatus is doubly advantageous. The following contrivance will in all common cases supply the place of a Welter's tube, and has been found even steam-tight under considerable pressure.

1. Grind the end of a piece of quill-tube, an inch long, perfectly smooth.
2. Cut a small piece of bladder, in the shape of *a*, of which the round part is of the size of the tube, and soak it in water.
3. Tie this, wet, on the ground end of the tube, with a waxed silken thread, and try it with the mouth ; if properly done, it will be a perfect valve while wet.
4. Bend and draw out a piece of tube (large enough to admit the quill-tube with the valve, so that the bladder does not touch its sides) in the form *b* ; make it of any convenient length,—say seven inches in all,—leave a small orifice at the point.
5. Insert the valve-tube at the top of this, closing *the space between the two* with a good cap-cement, but leaving the valve-tube open, as in *c*.
6. Dry the bladder which forms the valve, and put the point of *b* through the cork where it is to serve ; *b* may, of course, be strait if required ; when on the point of using it, put a few drops of olive oil in at the top of the valve-tube: this will render it perfectly flexible and tight, and the oil which oozes, or is forced through the valve will, by being retained about it by the capillary action of the two tubes, protect it from the action of corrosive vapours

It is evident that when any absorption takes place, a portion of air will be admitted by the valve, which h will bear an internal pressure strong enough to force out any common cemented cork. The first thus made was in action during thirteen hours in the mouth of a common bottle, used as a retort for the production of sulphuretted hydrogen, by inserting two tubes into the cork, of which one was the safety-tube just described; it therefore œconomises both the retort and the Welter's tube for any common use.



A. bottle. *b.* safety-tube. *c.* gas-tube.

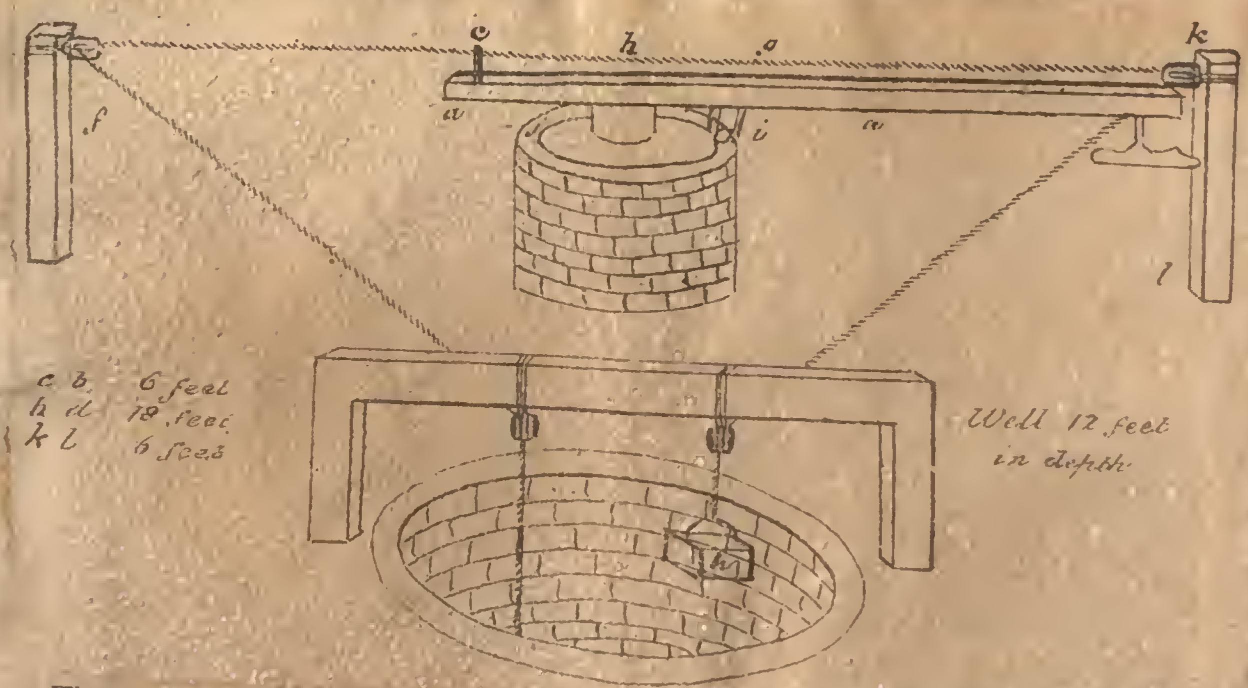
30*th November*, 1829. H. P.

2.—*On Raising Water by the* Mót.

To the Editor of Gleanings in Science.

SIR,

The difficulty in raising water by bullocks, owing to the loss of time and danger in turning, has for some time occupied my attention; and as I believe I have discovered a method by which it may be avoided, I inclose you a plan of the same. Nothing of the kind, I think, can be made cheaper or more simple—two great objects to the farmers of this country.



The machine being a simple crank, its operations must be at once obvious to every eye. The beam *a a* turns on the point *b*, its fulcrum. When *e* the yoke, is dragged by the bullocks round to *f*, it is obvious the point *c* will be at *g*; the bucket *h* will be in the well, and the bucket on the other side will be up: *i* is a small roller supporting the beam *a a*, and traversing on the frame *j*.

Having not yet put this method into practice, your opinion upon it will be esteemed a favour.

I am, &c. I. B. R.

N. B. The beam *a a* must of course be bushed with iron at the point *b*; an iron ring is also requisite to fasten the ropes, to which must traverse round the pivot *b*.

3.—*On Deducing a Mean Result from Observations of a broken Number of the Wires in a Transit Telescope: by* Mr. V. Rees, *of the Surveyor General's Office.*

Many years practice, in finding the mean result from the observed transits of a heavenly body, over the several wires of a transit telescope, have led me to investigate formulæ calculated to give to every observation an equal share in the result, when from accident, inattention, or other cause, one or more of the wires may not have been observed.

Some observers, in this case, reject the observation of the wire which corresponds to the deficient one, and thus, instead of one, lose two observations:—the one rejected being, very possibly, the best. Again, if the 1.2.5. or 1.4.5. wires be observed, a similar inconvenience follows; while in the case of the 1. 2. 3. or 3. 4. 5. wires being observed, the whole is rejected, or else the middle one only taken. Others supply the deficiency of one, or even more lost wires, by adding to the preceding or subtracting from the following wire the mean difference; but a little consideration will show, that should the wires to or from, with this mean difference, be added or substracted, be taken too great or too small, (and this supposition amounts almost to a certainty,) so this supplied wire will bear exactly the same error; which error of one wire will be taken in the sum twice or three times, as one or two wires have been supplied.

To obviate these objections, I have drawn up the following formulæ, which I have been in the habit of using in calculating the observations made in the Surveyor General's Office. They may be of use to the practical Astronomer, and as such are offered.

1st Wire wanting, mean result $=\frac{b+c+d+e}{4}-\frac{e-b}{6}$

5th $\frac{a+c+d+e}{4}-\frac{e-a}{16}$

2d $\frac{a+b+c+e}{4}+\frac{e-a}{16}$

4th $\frac{a+b+c+d}{4}+\frac{d-a}{6}$

1st and 2d $\frac{c+d+e}{3}-\frac{e-c}{2}$

4th and 5th $\frac{a+b+c}{3}+\frac{c-a}{2}$

1st and 4th $\frac{b+c+e}{3}-\frac{e-b}{9}$

2d and 5th $\frac{a+c+d}{3}+\frac{d-a}{9}$

1st and 3d $\frac{b+d+e}{3}-\frac{2(e-b)}{9}$

3d and 5th $\frac{a+b+d}{3}+\frac{2(d-a)}{9}$

2d and 3d $\frac{a+d+e}{3}-\frac{e-a}{12}$

3d and 4th $\frac{a+b+e}{3}+\frac{e-a}{12}$

4. *Supply of Wholesome Water.*

Among the first cares which ought to occupy the attention of the founders or inhabitants of a great city, one would suppose that a supply of good and wholesome water would occupy the principal place. Strange to say, however, such is not always the case, and amongst others, London has still to reckon this amongst her desiderata. But from the attention lately given to the subject, it appears, that though a late yet an effectual remedy will be applied to remove the evils under which her inhabitants at present labour on this score. A Mr. Wright appears to have engaged in the question, with a commendable zeal and intelligence; and from some notices on the subject we have lately seen, the inhabitants appear desirous of acknowledging his services by some appropriate means. Questions have also been brought forward for discussion and enquiry before the Society of Civil Engineers, having for their object to throw light on the subject of boring to a great depth for overflowing springs.

The subject is not without its interest in Calcutta; for if London be bad in this respect, Calcutta is worse. We wish some Mr. Wright would start up amongst us, and entitle himself to the gratitude of his fellow citizens, by persuading them to combine for the attainment of a plentiful supply of wholesome water. We think the statements published in the Gleanings on this subject offer well grounded hopes of discovering overflowing springs. Certainly there are grounds sufficient to warrant a trial; and as it is fully to be expected that Government would so far co-operate as to authorise the use of one of the boring engines now in the Fort, it is obvious the only expense would be the labour. This expense must be trifling, and would, we should suppose, be cheerfully borne by the Physical Committee of the Asiatic Society, who might also delegate certain of their members to superintend the operation. The most favorable season is now approaching, and we trust will not be allowed to pass away without something being done.

5. *Repair of Violin Bows.*

Musical amateurs who use the violin discover often to their great annoyance, that no precautionary measures will secure them against the insidious attacks of a little caterpillar insect, which contrives to disarm them, by destroying their bow-strings, and this often in places where the damage cannot be repaired but through the efforts of their own ingenuity and patient industry.

To those lovers of harmonious sounds who, like me, wanting a hint as to the right method of setting about it, have wasted hours in a vain endeavor to adjust a new roll of hair to their fiddlesticks, it would be conveying information scarcely less interesting than a new memoir on the geology of Bundlekund to instruct them how this may be accomplished. That there is a proper and particular method I am convinced, by observing the beautiful evenness of a new Dodd compared with my own best imitations.

I have compleletly failed in an attempt to bring *each single hair separately* to the same degree of tension; and though I have found that sufficient evenness may be had temporarily by drawing the whole through a close hard brush, there still remained the difficulty of securing them in that good order, owing to the thickness of the bunch where tied, bending the little knot in its box, and so drawing the hairs upon the surface much tighter than the lower half.

I hope this may meet the eye of some of the more ingenious of your readers, who may be able to give some information on the subject to, Sir, your's obediently,

Phil-Harmonicus.

6. *Hot Spring.*

At Sonuh, near Dehli, there is a hot spring, (sulphureous) which attracts from the surrounding country myriads of people for the purpose of bathing: the bath is constantly filled with as many people as it can hold (except perhaps for a few hours during the night), in the day time by men and at night by women; most of the inhabitants of the town itself are in the habit of bathing in it daily, and it is probably to this habit that they are indebted for the cadaverous and unhealthy appearance so common amongst them. The temperature of the spring, in January last, was 103°; but it varies, for in July 1826 I observed it as high as 110°: the flow of water varies also very considerably.

T.

7. *Contraction and Expansion of Paper.*

The following may be interesting to many of your readers, as an instance, though not an extreme one, of the contraction and expansion of paper.

I had a few months since laid down with the greatest care on a sheet of fresh drawing paper, some distances measured from an excellent scale by Ramsden, with a good beam compass. The paper has not ever been subjected to any drier atmosphere than that of the interior of the house, but now having occasion to lay off more distances on the same sheet, I find that my former measurement of 52, 942 inches is now but equal to 52.700 inches. The difference is nearly $\frac{1}{4}$ inch in 53 inches, or 1 part in 212.

W.

8. *Notice of Hail.*

In a thunder storm, which occurred in April last year at Serampore, some hail-stones were picked up as large as a hen's egg. They were observed, when broken, to have a concentric lamellar structure, being formed of successive layers similar to the coats of an onion! The nucleus was of a whiter colour than the exterior. I confess I am quite at a loss to imagine the cause of this curious appearance; can any of your readers give me any information?

E. M.

XII.—*Proceedings of Societies.*

1. Medical and Physical Society.

Saturday, November 7, 1829.

Mr. Bacon was elected a Member.

Mr. Newmarch withdrew his name, on the same plea as that urged by former seceding Members.

A letter was read from Dr. Hays, Editor of the American Journal of Medical Science, Philadelphia, transmitting several numbers of that work for the Society's Library.

The following communications were submitted :

A case of dislocation of the thigh-bone, by Mr. Spry.

A history of a case of poison, producing excessive constitutional irritation, by Mr. Raleigh.

Remarks on the state of disease in the Meerut circle of superintendence, by Superintending Surgeon Langstaff, (communicated by the Medical Board.)

Report of Disease in the Sirhind division, by Officiating Superintending Surgeon A. Murray.

A paper on the occlusion of the Biliary Ducts, by Mr. Twining.

Saturday, December 5, 1829.

Messrs. H. Taylor and F. Hartt were elected members.

An extract of a letter from Dr. A. Duncan, of Edinburgh, was read, relative to the medical powers of Madar, and several copies of a paper published by him on the subject, were presented for the use of the Society.

The following communications, received since the preceding meeting, were then submitted by the Secretary :

A memorandum on the native operation of Lithotomy, by Mr. King of Patna.

Some remarks on Cholera, by Dr. A. Smith, of H. M. 44th Regiment.

A case of tumour on the leg of a native, by Mr. Dempster, of Buxar ; and a clinical report on the effects of Blood-letting in the cold stage of Intermittent Fever, by Mr. Twining.

Several numbers of the Edinburgh Medical and Surgical Journal ; and a copy of the Supplement of the Edinburgh Dispensatory, were presented for the Library, by Dr. Duncan of Edinburgh.

Mr. King's Memorandum, Mr. Twining's Clinical Report, and Mr. Raleigh's Observations on Cholera, were then read and discussed by the Meeting.

2. Agricultural and Horticultural Society.

A Special Meeting of this Society was held on Monday, 28th December, 1829 ; Sir E. Ryan, President, in the chair.

Lord Viscount Combermere was elected a Member of the Society.

The following Gentlemen were also elected ; viz.

Roy Saltee Kinker Ghosal, Richard Holdsworth, Esq., William Blenkin, Esq., E. S. Ellis, Esq., Dr. Frith, Major Burney ; Mr. Richard Barnes, of Purneah ; and Mr. Eglintone.

Mr. John Abbott was duly elected Treasurer of the Society, in the room of Mr. Ballard, now in England.

Read a letter from E. Molony, Esq., Deputy Secretary of Government, to the Secretary, intimating that Government had resolved to grant the sum of Sicca Rupees 20,000 for the purposes specified in the letter of the Society ; but that before acceding to the proposition of granting a further sum annually to be distributed as prizes to those whose exertions in agricultural improvements appeared most deserving of reward, the Governor General in Council was desirous of receiving particular information in regard to the conditions upon which the Society proposed to grant premiums for the most approved specimens of Cotton, Tobacco, &c. as well as upon several other points therein enumerated.

On the motion of the President, it was resolved to refer the matters contained in the two first paragraphs of Mr. Deputy Secretary Molony's letter to the Agricultural Committee, which would be requested to report to the Society the best mode of distributing premiums, and the conditions under which the competition ought to take place. To refer the points in the 3rd paragraph to the Committee of Finance appointed last year, consisting of Mr. Hurry, Mr. Bruce and Captain Jenkins. To refer the points in the 4th paragraph to the Horticultural Committee ; and that so soon as these several Committees furnished their reports, a new Committee should be named to draw up a reply to Mr. Molony's letter, then under consideration.

Resolved, that the Garden Committee be requested to undertake the arrangement for the distribution of prizes and medals to Native Gardeners on the morning of Wednesday the 13th, at 9½ o'clock.

Read a letter from Mr. Wood, proprietor of the Asiatic Lithographic Press, presenting to the Society a copy of the Agricultural and Commercial Resources of the Presidency of Bengal.

Resolved, that the thanks of the Society be offered to Mr. Wood.

Wednesday, January 13, 1830.

The Society had an exhibition of prize vegetables to-day at the Town Hall, and the prizes, consisting of a silver medal and a sum of money not exceeding 40 Rupees, were delivered to those *malis* who produced the best show of Cauliflowers, Potatoes, Asparagus, Peas, Turnips, Beet-Root, Jerusalem Artichokes, Carrots, Onions, and Nolkole.

XIII.—Analysis of Books.

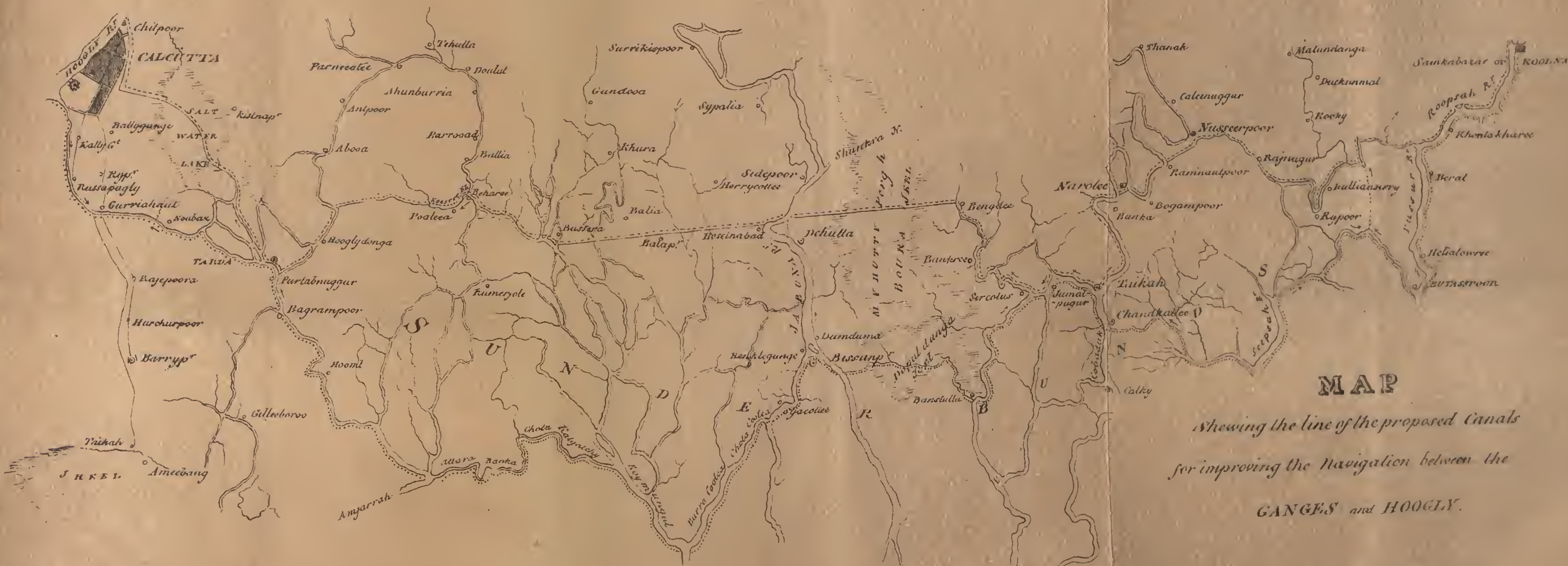
Memoir on a Plan for the Formation of an easy and permanent Communication by Water between the Upper and Eastern Provinces of India and Calcutta during the dry Season: by Lieutenant J. A. Schalch, 14th Regiment N. I., D. A. Q. M. G.

This is a pamphlet of 19 folio pages, written by the late Major Schalch, whose promising career was so prematurely cut short by his fall before Chamballa stockade, when attacked by the flotilla under Commodore (now Sir J.) Hayes.

The subject is one of the greatest interest: and as Major Schalch's statements have proved influential in establishing the system of canals now carrying on, one of which we gave a short notice in our fourth number, we think it will not be without its use to introduce to the notice of our readers this little work, which we believe is far from being so generally known as it ought to be.

The object of the Memoir is stated in the following opening passage :—" The want of a free navigation between the Eastern and Upper Provinces and Calcutta, during the months of January, February, March, April, and May, when most of the rivers which branch off from the Ganges become too shallow for large boats, has long been felt; and considerable expense has been incurred to remedy the evil, but hitherto without effect; every succeeding year bringing with it additional complaints of the obstructed state of the rivers and the increased delay in the navigation." Various plans, it appears, have either been proposed or resorted to, but owing to their being founded on erroneous views (as Major S. thinks) without any prospect of success. Some of these plans, such as that of straightening the rivers flowing from the great Ganges into the Bhagirettee, and employing machinery to free their beds of sand, are pregnant with consequences, which, though of slow growth, may be eventually of very fatal moment. For it is evident that in this case the sand being forced downwards to that part of the river between Calcutta and the sea where the periodical interruption of the current, occasioned by the tides, allows of its being deposited, a continual increase is ensured of those sands which render the navigation of the Húgli so dangerous, and which must, in time, under such a state of things, materially obstruct the port of Calcutta. Major S. thinks, in fact, that, with reference to this important consideration, it would be preferable, if it were feasible, rather to encourage the windings of these rivers and the deposit of sand at their efflux from the Ganges, by which means he considers the Húgli would resemble the Isamattia, Cubadák and other Sunderbund rivers, which, at the same distance from the sea as Calcutta, are free from sand, and their beds so stationary, that soundings taken in 1817 agree with those given by Rennel 40 years before.

But supposing there were no objections on this score to the measure of preventing the accumulation of the sand at the heads of the communicating rivers, Major Schalch considers it useless, as it would be impossible to keep them open for any time. In support of this opinion Major Schalch has brought forward the statements of Major Rennel and Col. Colebrooke, who assert that the soil must form an insuperable obstacle to keeping open any thing like a straight channel with a current such as would be required. And this, either in the case of a river straightened, or a canal. In fact, it is sufficiently obvious, that the figure or direction of any flowing body of water must, in a nearly level plain, be determined by the two circumstances of velocity of current, and tenacity of the materials of which the bank is composed. As the latter diminishes or the former increases, the sinuosities will be greater, till by the slackening of the current the frail banks are enabled to with-



MAP

Shewing the line of the proposed Canals for improving the Navigation between the GANGES and HOOGLY.

Scale. 8 Brd. Miles = 1 Inch.

REMARKS.

The line .... shows the old passage by the Sunderbunds.

The line - - - - - - shows the new.

The line ———— shows the proposed Canal.

The arrows point in the direction of the tides.

Commt. Lith. Press. 1830

stand the diminished wasting power. Unless, then, the depositions which are to be removed are of less tenacity than the banks, it is quite obvious that the current will, by its inevitable effect on the banks, destroy itself, so that in time it shall be no longer able to effect its purpose. The banks then become stable, i. e. comparatively, but with them the sands and shallows are also made permanent, whatever machinery be used to disturb them. When, therefore, it is considered what the nature of the soil is through which these rivers take their course at their afflux from the Ganges, it does not seem exaggerating the difficulties of the problem to say, that in a *natural* channel it is not practicable to keep these rivers open.

Major Schalch having shown, as well the inutility as danger of attempting the cure of the evil in that direction, next turns to the object of his Memoir, which is to propose a canal of communication between the Húgli and the great river, which shall not only be free of these objections, "but be likely to repay, in a short time, the expense of excavation, and ultimately become a source of considerable revenue to Government or any body of individuals who may embark in the speculation." This canal he proposes to carry across the several rivers which cover with their anastomosing branches the tract called the Sunderbunds, but on such a line as to be within the influence of the tides: the advantages of giving this direction to the canal he considers will be the following—The surface of the ground being so little elevated above the level of high water, the expense of excavation will be trifling. For the same reason, no locks will be required, as the numerous inlets from the sea will always afford a sufficient supply of water. Nor will the banks be liable to change, as the alternate flux and reflux of the tide seems favourable to the durability of such channels.

To the present route by the Sunderbunds, which proceeds by Tolley's Nullah, there are many objections besides its circuitous direction. Thus from the great breadth of the Raimangal and Sipsa (the Marjatta of our maps) rivers, the passage is not unattended with danger to boats heavily laden. There is also the danger of tigers when cooking ashore; and still more, there is the delay and other great inconveniences attending the passage of Tolley's Nullah. This cut, though sufficient for the commerce of Calcutta when first excavated, is no longer so. At the very season when it is most required, it is literally chocked with boats to a distance of 3 or 4 miles beyond Garriahath; and many of the boats, deeming their further progress hopeless, are in the practice of landing their cargoes to be conveyed to Calcutta by a land carriage of 8 miles. All these inconveniences and objections will be obviated by taking the direction proposed, and making the cut of sufficient width. Advantage may be taken of the branches of these rivers which run in the proposed direction; and these are more numerous and of greater length than might be at first supposed, considering that this direction is at right angles to the general course of the rivers.

The accompanying plate exhibits both routes, the old one being marked by a round dotted line, and the proposed one by a line of elongated dots.

The navigation of the Garái (which in its course to the sea is successively named the Baráshi, the Múdumatti and Huringatta) and by the Attarí banke nullah and Bairab, is perfectly safe until reaching Culna. From this point, or rather from the mouth of the Bytaghata Khál, at Hatbár, the new route, it is proposed, should diverge; it will proceed by the Baítaghatta, Salta, Féli, Káli, Bangaria and Nazzerpúr Khál to the back of the village of Cabalmani, on the banks of the Cubádak, with which a junction will be formed by a canal, as in the map. A few trifling operations will render the above line perfectly unexceptionable. Hence, to Téka it may follow the Cubádak; or the passage may be greatly shortened by cutting through the isthmus at Naruli, which is only 1368 yards across, by which a tide will be gained; thus far, a saving in time from Culna of 18 hours will be made, and, what is of more consequence, one of the most dangerous rivers of the Sunderbund navigation will be avoided, while the line passes through a highly cultivated country the whole way.

The new line now joins the old one as far as Bissautpùr, and then across the country by a canal to Bassera, through the Ghaskáli, the Behari, the Kusri, and Bangar nallah to the letter E. near the village of Gabtalla; from thence through another canal to the Bedadúri nullah, which leads to Tarda, where it again joins the present route. There is a great deal of very valuable and detailed information on this part of the line, which we regret our limits will not allow us to do justice to. From Tarda, after passing Bamingatta, it proceeds through the Salt-lake and up the eastern canal to within about 2000 feet of the Circular Road, where it turns to the right, and running parallel with it, forms a junction with the river a little above Chitpúr.

Major Schalch estimates the expense of excavating the several canals, including the charge for bridges, roads, and other works connected with the undertaking, as amounting to 9,50,000 Rupees; while the annual income, he supposes, will amount to 1,65,600 Rupees, being upwards of 18 per cent. on the capital.

A feature of the plan finally adopted by Major Schalch, was the formation of a harbour for boats in the canal, immediately above its communication with the Húgli. This harbour it is calculated, as affording shelter from the dangers of the Húgli during storms, and facilities in loading and unloading, would be a great public convenience. Supposing a toll of one anna per 100 maunds per diem to be levied, the yearly amount on 3,30000 maunds, which it is calculated to hold, will be 75281 Rupees: a further toll on boats passing through the canal in either direction, of 1 Rupee for every 100 maunds, it is calculated, on the present tonnage engaged in the trade of Calcutta, would yield annually 80,000 Rupees; but as the trade is likely to increase by these facilities, the above estimate is probably much within the income it will eventually yield.

Besides the increase to the internal trade of Calcutta, this plan, Major Schalch considered, would hold out a prospect of the following advantages: the certainty of the navigation, the lessening the number of wrecks, the prevention of loss of property and damage arising from boats running frequently aground in the present passages, the convenience of the harbour for loading and unloading, and affording a safe retreat from the dangers of the Húgli in stormy weather.

The advantages to Government he considered obvious, and requiring no enumeration. To the city, the improvement in value of the ground near the Circular Road from the head of the eastern canal to near Chitpúr, and an increased salubrity, owing to a system of effectual drainage to be connected with the proposed canals. This latter subject, which is one of vital concern, particularly to the inhabitants of a city like Calcutta, he has discussed at greater length in an Appendix, which we shall republish in a future number at full length. The very great deficiencies under which our city labours, as far as regards the public health, make it desirable to use every means of drawing the attention of our fellow citizens to the subject of a medical police, in the hope, however slender, of seeing some of the many very obvious improvements that are called for, adopted.

To return to the subject of our analysis: Major Schalch, after a fair, and by no means exaggerated statement of the advantages of his plan, proceeds to consider also the objections. The first of these has a reference to the possibility of the same state of things occurring in the Gárai and Chandna rivers, on which it may be seen the above navigation depends, as have occurred in the Bhágirettee and Matabhanga. But the answer to this is easy: the former rivers have never been shut, and the reason is obviously the greater tenacity of the soil through which they take their course, which is a black mould instead of a loose sand, as in the case of the latter rivers. This having sufficient power to resist the action of the current, the latter is effective in keeping the channels clear of sand or mud banks. The narrow channel in which the Ganges is confined below Chocula, and its great depth of water, sufficiently prove the fact of an entire change in the nature of the alluvia it traverses; and this is further confirmed by the fact, which might have been inferred, *àpriori*, of the mouth of these rivers within the influence of the tides being choked by sands. It may then very safely be taken for granted, that no change of the kind contemplated is likely to occur.

The second objection is of even less force. It may be said that the Bhágirettee and other rivers of the upper navigation may again become navigable in the process of time; but independent of the absurdity of waiting for such a problematical event, it may be safely said that the whole course of human experience goes to support the opposite proposition. That the tendency of a river bed is to raise, not lower, itself, is a fact familiar to all who have studied this branch of physical geography; and it has been satisfactorily established by Major Schalch, that the obstructions which impede the navigation of these rivers are not confined to their heads, where they leave the great one, but extend pretty uniformly the greater part of their length. As to any canals that could be made to assist the navigation of these rivers, the nature of the soil through which they flow, as has already been shown, is fatal to such a scheme. It has indeed been proposed to carry a canal from Rajmahl through a tract of stiffer soil into the Bhágirettee below Suti. But even if the expense of such an undertaking were not in itself a sufficient objection, which it most certainly is, there are others which appear insuperable. For as the line in question is intersected by many mountain streams, these would infallibly cut up the canal, and fill its bed up with the detritus brought down in the rainy season. Add to which, that the sands and shallows *below* the point of junction would be rather increased than diminished by the canal. The project is in fact chimerical, and may be at once dismissed.

That Major Schalch's estimate of the increased value of the land along the line of the canal is not unreasonably favorable, is evident from the fact that the land contiguous to the Eastern Canal had risen from 16 and 20 Rs. a cotta to 3 and 400. In his estimate of the expenses, he appears to have been equally on the right side, judging from a comparison he has drawn between his results and those of the Forth and Clyde canal. He concludes by giving as his opinion, that the plan proposed by him offers no difficulty that may not be overcome by moderate ability and attention; and, as he justly remarks, his knowledge of the country and frequent visits to the tract in question, qualified him to form an opinion on the subject. His Memoir appears to have attracted that attention which it deserved, and we need not inform our readers was followed, by the Government sanctioning the project and engaging in the execution of it. The disastrous war with the Burmese put a stop to this and many other projects of great public utility, and Major Schalch, whose ardent and active mind would never allow him to remain idle, having organized a ponton system for the use of the army in Arracan, was appointed to the head of that department. His health having suffered, he was recommended to try the sea air, and he accordingly proceeded in the *Research*, with Commodore Hayes. In the unfortunate attempt which was made against the stockade of Chamballa he was mortally wounded, and having lingered in great pain, the following morning breathed his last. Thus terminated a life which began under the fairest promise, and thus was cut short a career which in its progress would have secured to the individual an honorable distinction; to the country, a series of useful labours and great public improvements. Amongst the many names distinguished for talents and acquirements, the loss of which India has had within the last few years to record and to deplore, that of Schalch will hold a conspicuous place. He was no common man, who at his age, and as yet a subaltern in his regiment, so recommended himself as to obtain from Government the distinction of the brevet rank of major, and this without exciting the dissatisfaction of many who might, in ordinary circumstances, have thought themselves aggrieved by his advancement. Nor is it less obvious from considering his career, so brilliant though so short, as well as that of many others, that in India at least talent need not repine; and that its possessor must, in whatever situation placed, sooner or later, work out for himself, not only distinction, but independence.

XIV.—Notices of European Science.

1. *Junction of Granite and Sandstone.*

In a paper with this title in the 8th No. of the Journal of Science, N. S., Dr. MacCulloch gives his opinion, that granite solidified after eruption at the bottom of the sea may have a secondary strata deposited on it, and then the whole be raised above the level of the sea by a new eruption at a still greater depth. He considers that he has demonstrated the *successive* production of granite, and that solid rocks have been raised by it, is, he affirms, one of the facts on which the science of geology is based. That it may, therefore, raise solidified granite is not more surprising than that it should raise quartz rock or mica slate; he asserts that Scotland is full of examples of this truth: the sandstone and lignite formation over it, having been deposited on consolidated granite, the whole being afterwards upheaved in a solid form by a later eruption.

The facts which have given occasion to this statement of his views are certainly curious, and may, he thinks, at first sight, be very easily turned by a Wernerian into a confirmation of the opinion, that granite, like many other rocks, has originated in deposition from water and is stratified. Fairly interpreted, however, he considers them to lead merely to the above modification of the opinions that have been latterly gaining ground, as to the igneous origin of granite and the upheavement of the more recent strata. The appearances we shall endeavour briefly to describe; though, as he well observes, it is not possible for description to do justice to such phenomena. To be fully appreciated and understood they must be seen.

With the exception of the present instance, he says he knows of but one example of the junction of granite and sandstone in Aberdeenshire; but in that case the appearances are unimportant, and lead to no conclusion beyond the mere fact of the junction. In the present case it is otherwise; there is the most perfect exposure of the whole junction through a space of many hundred yards; while all the rocks are as clean as if recently cut by a tool, and absolutely free of any incumbrance capable of introducing obscurity or doubt into the observations.

The place in question is in Sutherland, where the granite of that country comes in contact with the sandstone of Caithness; the latter he considers to be synonimous with the old red sandstone, the oldest and lowest of the secondary rocks. The granite he considers to be of the same æra as that found in every part of Scotland. It is inferior to all the primary strata, and is occasionally covered by extensive tracts of gneiss. As to their mineralogical composition, the granite is very variable : it exhibits, in particular, the appearance which some observers hold to be particularly *genuine*. The sandstone is sometimes grey and arenaceous, or else compact, of the same color ; sometimes red, of various tints ; is in some situations simple, in some argillaceous, and in some calcareous. It is remarkable for containing beds of argillaceous schists, sometimes so abundant as to occupy entire tracts of the surface, to the exclusion of the arenaceous strata. These beds sometimes resemble clay slate, more frequently greywacke; very seldom the shales found in the white or superior sandstones, or those of the red sandstone of Arran.

The junction of the sandstone with the subjacent granite is visible for some distance : the former has a dip to the eastward of north, the inclination being about 40°. The strata are perfectly even, and the line of junction of the two rocks corresponds with their seams. In a very few cases there is some slight irregularity in the surface of the granite ; and in these, the sandstone strata are affected in a corresponding manner. No fissures or other marks of disturbance are visible in the sandstone, nor is it penetrated by any granite veins.

With regard to the modification in the mineral characters of these rocks produced at the junction, the most striking is the apparent approximation of two rocks of such different characters. Where the mass of granite approaches the sandstone, the vertical or prismatic structure, visible in other places, first disappears, and there are then displayed solid masses, rounded by the action of the weather, and not satisfactorily distinguishable by the eye from the neighbouring sandstones which have been subjected to the same influences. As it comes close to the sandstone, it resembles a stratified rock, and in one place there are even partings of an argillaceous character to mark the strata. These are in all their thickness, however, not more than one foot, and similar laminæ may be observed, though of less extent, on the surface of the uncovered granite. The granite has, in many cases, the most ordinary aspect, but where it approaches the sandstone, it consists of quartz and felspar. In some places these minerals are equally intermixed in large grains; in others, large grains of each are united in a sort of general basis or paste of finer sand. In one variety of remarkable appearance, the large grains of felspar are of a high red colour, when that mixed with the quartz is white. This rock approaches so nearly in general aspect to the finer conglomerate or gravel stone of the strata which consist of the same materials, that in the detached specimens it is at first sight difficult to know which the subject under examination is. Another remarkable variety contains schistose clay, which is either in small particles, or in larger resembling fragments, or else merely communicates a grey tinge to the stone. These specimens similarly approximate in character to that variety of the sandstone which contains small fragments of shale.

As to the sandstone, it is generally reddish, very compact, resembling quartz rock, and consisting chiefly of quartz sand with a few fragments of felspar. This graduates into a sandstone of the same colour without fragments. Then follows purple schist, which in some places is arenaceous, in others fine and fissile, succeeded by a blue shale resembling grey wacke. After this there occur, in irregular order, a series of sandstones, red, purple, blue, and brown, intermixed with the same blue and purple grey wacke shales. In some places the schists predominate, in others the sandstones. The order is not however always the same. In many parts of the junction there are found conglomerates of various kinds, but generally of a moderate sized structure, or consisting of small fragments : one of these consists of quartz and felspar, compacted to a state as hard as the granite which has been already described as being similar in composition. A coarser conglomerate consists of small fragments of granite, or of quartz and felspar cemented by argillaceous schists. Other varieties contain fragments of argillaceous schist or various fragments united by sand; and some are formed purely of fragments of schist, united by an argillaceous cement.

We have been thus particular in extracting this description, as it may prove interesting to some of our readers who may have an opportunity of studying similar appearances (so rare in Great Britain) in this country. The expense of books, so great in England, is so much enhanced in India, that many of those who may have the power of throwing light on questions of this kind, will probably never see the journal from which the above notice is taken. If we mistake not, there are several examples of the super-position of sandstone or granite in India, and it will be seen from Dr. Macculloch's paper what interest attaches to the subject, and how valuable a detailed and faithful description of the phenomena at the several places would be. We hope our notice may be the means of awakening the attention of any of those of our readers who might be able to furnish such description.

GLEANINGS

IN

SCIENCE.

No. 14.—*February*, 1830.

I.—*Directions for the Guidance of those desirous of making Geological and Mineralogical Observations.*

In considering the great interest which is attached to geological enquiries, and the wide field which is open to the observer in this part of the world, we cannot but think that many would be disposed to assist in the collection of the necessary facts, were they aware how very simple and elementary the knowledge is which would enable them to do so. With little of technical knowledge, beyond what one of ordinary intelligence may very easily acquire, it is in the power of a faithful observer to co-operate materially in laying a foundation for a sound and philosophical theory of the earth. In Europe much has been done to approximate to a full and correct description of geological appearances as they are; the first step towards attempting an account of what they have been. But Europe is a small part of the world, and though it be probable that the study of the geology of even that limited portion of the globe will not lead to any very erroneous assumptions, as to its general history, still we must remember that this is but a probability, and one, the truth or falsehood of which cannot be established without a greater or less accumulation of facts from all parts of the world. And even were we quite certain of the universality of all the facts of this science, it would still be a question, whether there could be too many observers, particularly in one which, like this, is in its infancy. When, however, we know that the contrary is the case; that very many curious facts are found to be of local and partial occurrence; when we see that many of the links wanting in one country to the chain of geological evidence are to be sought for in another; we cannot deny but that the progress of geology would be considerably accelerated by a general and simultaneous effort on the part of our countrymen scattered over the eastern world. Though there may be few amongst us capable of emulating a Cuvier, a Smith, or a Buckland, we must not infer that our efforts would be, therefore, useless. Every fact, however trifling or unconnected it appear to the superficial observer, has its value; and every man possessing ordinary powers may be the recorder of a fact, however incapable of generalising or constructing a theory.

With such views, and in the hope of inducing some of the many whose leisure and opportunities will allow of their adding a stone to the geological edifice, we gladly avail ourselves of the permission of a friend to publish the following clear and elementary instructions compiled for the use of the officers attached to the Ordnance Survey now in progress in Ireland. A few trifling and merely verbal alterations have been made, simply with the view of adapting the instructions to our readers, and rendering them of more general application.

The first object of a person desirous of collecting geological information should be to obtain a general idea of the nature of the rocks which prevail in the tract under examination; and if they are in strata or beds, of the alternation and super-position of these strata with each other; and also their dip or inclination to the horizon, and the direction or bearing in which they cut it; in order to judge when the same beds may again probably come to the surface, since they often preserve the same plane over a considerable extent.

It is almost needless to observe, that the beds of rivers, cuts for roads, cliffs and quarries, are the situations where such observations can, in general, best be made, particularly the latter, since the fresh fracture of the sound unaltered rock is there exposed, and in some kinds of rock the change taking place from decomposition extends to a considerable depth, sometimes of several feet.

Care must be taken to distinguish boulders or rolled masses from the out-croppings of the edges of the strata: the latter, being in their original position, are termed in situ; whilst the former merely lie on the surface, and are often found at great distances from any rock of similar characters from which they may be supposed to have been derived.

In examining the boulders, and also the pebbles of the beds of gravel, attention should be given to ascertain the correspondence or similarity of their characters to the rocks of the neighbouring mountains.

After having obtained a general idea of the rocks which prevail, specimens should be taken from the different beds or masses which are the most characteristic of the whole. It will be desirable that these specimens be kept to nearly the same size; 3 inches by 2 by 1 may be taken as average dimensions.

In the beds of pebbles, a few of the different pebbles may be taken, and also specimens of the beds of sand and clay; but in these cases, description will be more requisite.

Besides the specimens giving the general character of the whole, others should be taken of any particular varieties which occur; as when the component simple minerals are larger and more distinct, or when minerals of a different appearance and character present themselves, either imbedded in the mass, or affixed on the edges of the cavities which happened to be in it.

When such minerals are crystallized, it is an object to obtain all the different varieties of form, and particularly to select such as have the faces distinct and numerous, with the apex unbroken, and the crystal fixed in its original position in the rock, which is termed its matrix or gangue. In the cavities of the basaltic and amygdaloidal rocks, crystallized minerals are of frequent occurrence: they are generally zeolites, or species belonging to the genus *Kouphone Spar* of Mohs. In collecting crystals, the minute ones are not to be neglected, for they have commonly their faces and edges the best defined and glancing; and hence, from being capable of being measured by the reflective goniometer, are the most instructive to the mineralogist or crystallographer. Each specimen must be labelled with a reference to the locality from which it has been taken; for which purpose a rough tracing must be made from the plan or paper which will bear ink. On this tracing, for the geological plan, will be marked the points at which the strata come to the surface, with their dip and bearing, changes of direction, intersection of veins or dykes, &c.

The line of intersection of the plane of stratification with the plane of the horizon is the bearing, and the angle formed by the above two planes is the dip, which must therefore be measured in a plane perpendicular to the line of bearing, as it is evident that it is the direction of the greatest slope on which a ball or water would fall down the plane in free descent.

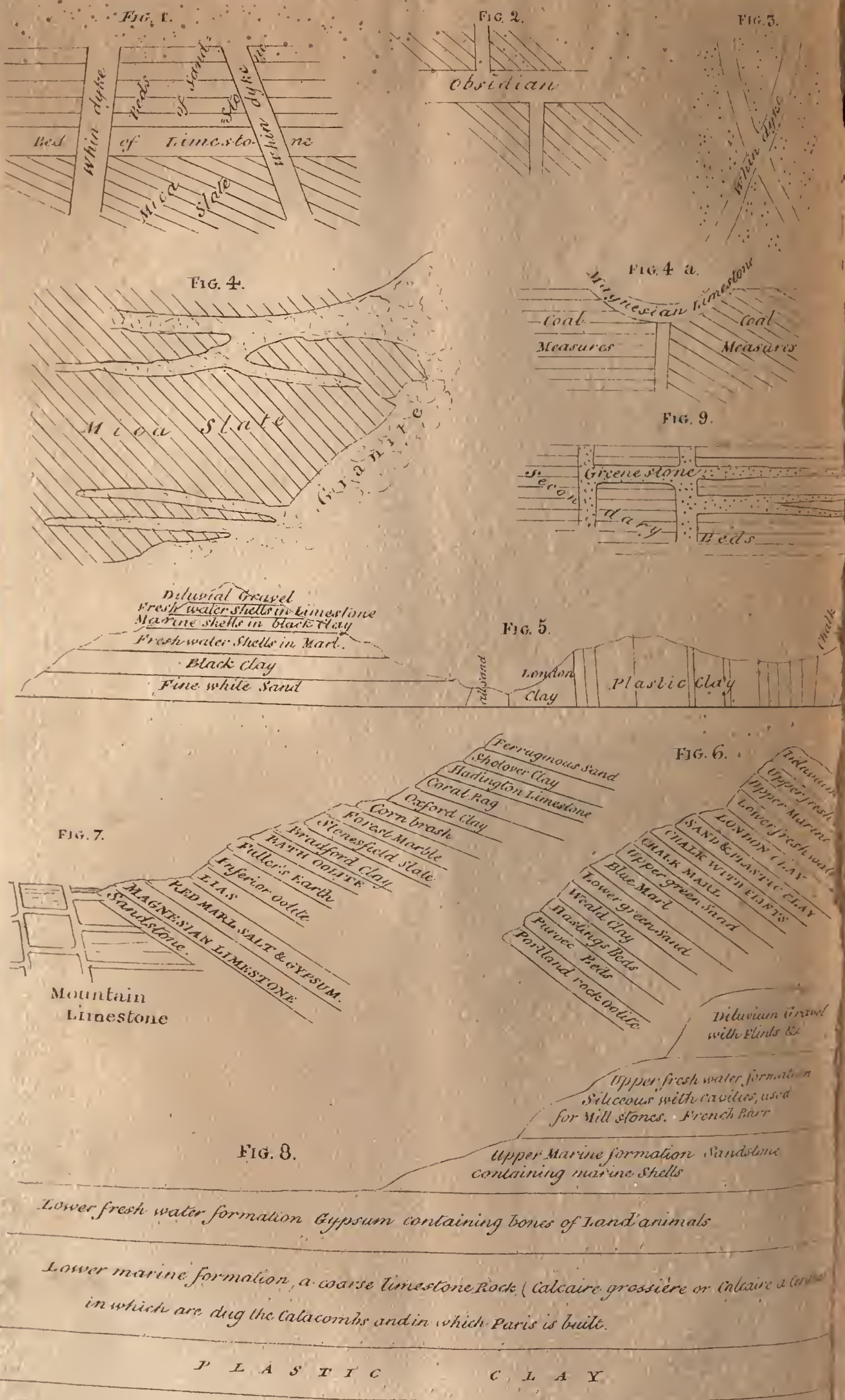
In measuring the above angle, it will be sufficient that it be done with a small wooden quadrant and plumb-line, or other simple contrivance, since the irregularities of the strata do not admit of great nicety. The thickness of the beds should also be marked, but, for the same reason, measurement will not be required, provided the dimensions be judged accurately whilst on the ground. When a number of these observations are collected, it is evident they will give the data, to make a geological section across an extent of country.

A section of the strata should also be made, when it can be done, on each of the geological plans to which the specimens are referred, and the same letters used, so that the label on the specimens may refer to both the plan and section.

The portions where the strata or rock cannot be observed, should be left blank, as no supposition or uncertain observation or information is to be admitted. These blanks, however, may contain any remarks, either as to communication received, or inference made, with the reason assigned.

A section to some depth may be obtained when a shaft is sinking for a well, or other object, in which case a sketch of the beds, according to their order and thickness, should be given, with specimens from each.

Sections of the strata are often exposed in the cliffs along the sea shore, or in vallies, in which cases a sketch should be made, with specimens from the beds, their different approximate thicknesses marked, and their true inclination and bearing. The true inclination or dip, it is evident, can scarcely ever be the same as that shown in the sketch: such situations often exhibit remarkable contortions in the strata, or the junction of rocks of a different kind, as basalt and limestone, sandstone, &c. or the intersection of veins or dykes through the strata, (see fig. 1. 2. 3. Pl. 1.) accompanied by slips and shifts; the former signifying a depression or change of level of the beds in the different sides of the dyke in a vertical direction; the latter a simi-



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lar change of their corresponding parts, formerly in contact sideways, or in a horizontal direction. The rock also immediately in contact with the dyke, is frequently altered; to all which circumstances attention should be paid, as being curious geological facts, which lead to inferences that will be afterwards mentioned.

A vein may be defined a narrow seam which appears to have been formed in a rock or fissure passing through the strata. The depth to which the vein extends is quite unknown, and the longitudinal extent can seldom be ascertained. Veins of basalt, greenstone, porphyry, and granite, are generally termed dykes.

The term strata it is unnecessary to define: eminent geologists differ, however, as to the thickness which some beds may have to be called strata or masses; and also distinguish fissures parallel to each other, which are found in some rocks, as being different from stratification. For these and other minute distinctions, reference must be made to the works on geology.

The outlines of mountains may also be given, and frequently on their sides the washing off of the earth has laid bare a portion of the rocks, which in some cases shew the junction of unstratified with stratified rocks, and sometimes veins from the former running into the latter, (as in fig. 4.)

When the planes of the upper beds are not parallel to the planes of the beds on which they rest, they are termed *unconformable*, an example of which is seen in the figs. 4*a*, and 7, when the sand and magnesian limestone beds lie on the edge of the coal, and seem to indicate different periods of formation, as will be afterwards mentioned. Such instances, when shewn in the above mentioned situations in the vertical section, should be carefully observed; or when only in the horizontal junction, such should be traced along the place with the dips of the strata, as in fig. 1. The dips being marked by an arrow with figures to read in its direction, as is done in the sketch.

The organic remains and impressions contained in the strata are of great interest, but the specimens are generally found in accidental digging, and, at any rate, to make a collection would require much time and expense. It will, therefore, be considered as desirable, merely to ascertain the beds, and the general nature of the remains which they contain, accompanied, when it can be done, with the few specimens that can be procured of undoubted authenticity, which can seldom be the case with bought specimens. The remains in the solid rocks have been changed in their nature, mineralized or petrified. Those in the gravel and sand are but little altered; the same with those found in the peat bogs, as of the gigantic Elk and others.

When the rocks are worked for economical purposes, as slate, lime, gypsum, (plaster of Paris) such should be stated, with some account of their extent or value, and the market to which they are sent.

The names given to the beds by the inhabitants should also be stated, as such names have frequently reference to the purposes for which they are used; as firestone, from being employed to line furnaces, millstone grit, &c.

By observing walls and buildings, a knowledge of the nature and durability of the rocks, and of their value as stones for building, will be obtained.

The specimens should be shaped on the ground, and care taken to get a fresh fracture: they should always be labelled or numbered, referring to a descriptive catalogue, and wrapt up separately in paper, whilst on the ground, to avoid the chance of mistake.

On bringing the specimens home, they should be re-examined and proper labels fixed with gum-water. When packed to be sent to any distance, each specimen should be in a double paper, and the whole firmly packed together with hay, moss, or tow, in a deal box or cask.

By looking at and examining the specimens, and at the same time referring to the works on mineralogy for the character by which they are distinguished, the student may, of himself, be enabled to prosecute the subject, and, it is hoped, be induced to take an interest in the pursuit. Hardness, according to a certain scale of minerals, being one of the chief distinguishing characters employed in the system of Mohs, a set of minerals arranged so as to give the means of applying its characteristic, and understanding its use, would facilitate his enquiries. The specific gravity can be taken with small scales and weights, but the most convenient instrument for a person not resident, is a hydrometer, termed Nicholson's, from the inventor. A goniometer, for measuring the angles of the crystals, will also be required by such as wish to pursue the study of mineralogy.

The minerals forming the great portion of the rocks, are quartz, felspar, hornblende, mica, augite, limestone or calcareous spar, gypsum, diallage, talc.

The mode in which these are found combined, and from that combination, or from the structure, give names to the different rocks, will be found in the works on geology, as also the transition which exists in one species of rock into another; from the absence of one or more of the composing minerals, or presence of others; or from a change in the structure. This transition, as must be evident, frequently causes a doubt in the application of the names.

To avoid, however, the necessity of a constant reference to these works, the following table, in which will be found described all the principal rocks, or at least those of common occurrence, has been made out.

Some of the terms employed in it, primitive, transition, &c., refer to the place of the rocks or strata in the series, as to superposition, which will be afterwards explained.

Granite is composed of quartz, felspar and mica; the proportions of each are various. Hornblende also sometimes enters into granite.

Gneiss is composed of the same three minerals, but instead of being granular as granite, it has a schistose structure, from the plates of mica lying in the same direction.

Mica Slate is composed of the same three minerals, or frequently of only mica and quartz, and has the same structure as gneiss, but the mica is in excess.

Talc Slate and Chlorite Slate, are similar to mica slate, the talc or chlorite, which is a green variety of talc, having taken the place of the mica. These two minerals are, however, often, with difficulty, distinguished from each other when disseminated in rocks.

Clay Slate, (the slate used in roofing,) the best belongs to the primitive class, and appears to be composed of the same minerals as talc and mica slate, but they are so intimately mixed that they cannot be detected by the eye. As the structure changes, a transition takes place from granite to gneiss, and from gneiss to the slates.

Sienite is composed of hornblende and felspar, in well defined crystalline masses: quartz is also generally one of its component parts. It therefore differs from granite by the substitution of hornblende for mica; but, as it has been seen, that granite sometimes contains crystals of hornblende, it will be evident there is a transition from one rock into the other.

Greenstone or Whin, the Diabase of the French, is composed of hornblende and felspar, and sometimes a small portion of quartz; the component minerals are generally so minute as to assimilate the appearance of the rock to a compact crystalline mass. When the greenstone is composed chiefly of hornblende, is highly crystalline, and is found alternating with the crystalline rocks of the *primitive* class, it is generally termed hornblende rock.

Basalt is composed of hornblende and felspar, but still more compact than the greenstone: some varieties contain augite, and large crystals of hornblende and augite are frequently imbedded in basalt: when these are in considerable number, it is termed *porphyritic*, as will be afterwards explained.

Obsidian and Pitchstone belong to this class, but their composition appears to vary, and often to be chiefly silex. These rocks are generally found forming veins: they are vitreous or pitchlike; their specific gravity is less than that of quartz; their distinguishing characters will be found in the work of Mohs, as some of their varieties appear to be simple minerals.

Lava differs from basalt in composition, only from the prevalence of augite, and in structure from being generally porous, therefore, less dense; but all the lavas, when sufficiently magnified, shew them to be composed of minute crystals, which appear to be of felspar and augite, or hornblende and crystals of leucite. Mica and oxide of iron frequently occur in lava. Pumice stone is a very porous lava, chiefly consisting of vitrified felspar or of quartz. Scoria and volcanic sand appear to be particles of the same nature as pumice; and since volcanoes in action emit a great quantity of such matter mixed with steam, which in its condensing unites the ashes or sand into a solid mass, which is termed volcanic tufa. It is from the volcanic tufa that the cement called Pozzalano is formed; it is found in rocks of a volcanic origin at Pozzuola, in the neighbourhood of Naples and near Rome, and also from similar rocks in Auvergne in France, and on the Rhine near Coblentz; from which last, the Dutch cement, termed Trass, is obtained.

Trap is the general term for the greenstone and basalts, amphibolites of the French, from the hornblende, by them named *Amphibole*, being the chief ingredient.

Trachyte is the name given, particularly by French geologists, to rocks of volcanic origin, but derived from volcanoes no longer in action; such as are found in Auvergne, Hungary, and the Lower Rhine: these are vitreous and cellular, white glassy felspar forming a great portion of the mass, and they melt into a white glass. Trachyte is, therefore, merely a variety of lava, excepting as to the date of formation, and there is a transition through all the above trap and trachyte rocks, and from them into sienite and porphyry.

METEORIC STONES are, many of them, very similar to varieties of greenstone, but mixed with iron; others are composed of iron nearly pure or native, so as to be malleable; sometimes the iron is in so great a proportion as to form almost the mass. The metal nickel is always found in meteoric iron, and is considered to give the property of resisting oxidation in the atmosphere.

WACKE appears to be a basalt half decomposed, or semi-indurated, or a hard ferruginous clay; it is sometimes amygdaloidal.

PORPHYRY is composed of crystals, embedded in and disseminated through a compact mass, which is called the base or paste. These crystals are of cotemporaneous formation with the base or mass, since they have their angles and faces of crystallization perfect. These crystals are of felspar, and the base may be considered a compact granite or greenstone, felspar being the predominating ingredient. Such are the rocks to which the name porphyry is particularly given: but any rock having crystals so disseminated through it, is a porphyry, and may be named according to its composition, as porphyritic claystone, obsidian, &c. The base of the porphyry just mentioned, is the EURITE of the French; and when hornblende predominates, it is their OPHANITE.

AMYGDALOID has the same, or nearly the same composition as the paste forming the abovementioned rock, but instead of crystals disseminated through its mass, as in porphyry, it has round or elongated nodules, generally of calcareous matter, which appear to have been formed in cavities of the above figures previously existing by water, containing lime in solution, percolating through the rock; when these cavities are not filled, they have frequently crystals of calcareous spar or zeolite implanted on their sides.

SERPENTINE is chiefly composed of silica and magnesia; it is compact, but variegated in its colour; it is the matrix of many magnesian minerals, of asbestos, ecume de mer, &c. The green colour is given by the protoxide of iron, and the chromate of iron. It may be remarked, that the different oxides of iron appear to give the colouring matter to nearly all the minerals, rocks, and clays.

DIALLAGE ROCK is composed of diallage, or diallage and felspar; it is the SCHILLERFELS of the Germans, and is found with beds of gneis and other rocks of the primitive series.

QUARTZ ROCK is quartz mixed generally with a little mica; it is the GNEISEN of the Germans: their WEISEN is felspar and mica; therefore, granite without the quartz.

PEGMATITE is composed of quartz and felspar, or granite without the mica. Flints and chalcedony are pure silex, and are merely varieties of quartz. Siliceous tufa is the deposit from water containing silex in solution. The Geiser fountain in Iceland appears to be almost the only deposit of this nature now in action: the mineral formed has less specific gravity than quartz, and comes nearer to obsidian or the opal.

SANDSTONES are considered to be formed from the débris of former rocks; the particles are commonly quartz, which may be supposed to have resisted decomposition more than the other minerals of which the rock was formed as granite. These grains are united into one mass by a cement of silex, argil, or lime; the cement of lime will be known by its effervescing with acids; that of argile is less hard than the siliceous, but these are often mixed in the same.

PUDDING STONE, or CONGLOMERATE, is of the same composition as sandstone, only the parts are larger and always water-worn or rounded.

BRECCIA is also of the same composition as sandstone, but differing from the conglomerate in being formed of angular fragments of rocks, as its name indicates, instead of water-worn or rounded pebbles cemented together.

GRAUWACKE′ is a sort of breccia or sandstone, containing fragments of primitive slate, and some organic impressions; it is the oldest sandstone in the series; it lies under the coal formation, and is, therefore, in the transition class; it has a siliceous or argillaceous cement. It corresponds with the beds of OLD RED SANDSTONE of the English geologists.

GRAUWACKE SLATE is a variety of the above which has passed into a schistose structure. GREENSTONE SLATE is similarly connected with greenstone, and in the same manner. By some geologists gneiss has been termed SCHISTOSE GRANITE.

SANDSTONE SLATES are in similar relation to the different beds of sandstone. Among the sandstones, particularly in the lower portion of the series, there are some beds which are highly crystalline; that these have been formed by the débris, or by the particles of older rocks cemented together, must be considered doubtful; they would rather lead to the idea that they may have been formed in the same manner as the crystalline rocks, as greenstone, primitive limestone, &c. &c.

Limestone is a carbonate of lime, that is, composed of carbonic acid and lime; besides its external character, described in works on mineralogy, it is most readily known from its effervescing with acids, by the disengagement of the carbonic acid gas, the lime remaining in solution in the acid.

According to the position of the beds of limestone in the series which will be afterwards explained, and also according as its structure varies, it receives different names.

Primitive limestone is found alternating with granite gneiss and mica slate. It is highly crystalline, being composed of a number of small crystals united together; these, when separated, shew the rhomboidal clearage, as in calcareous spar. Statuary marble is the finest variety; the color is, however, often grey and bluish-grey or veined. The primitive beds do not contain shells or organic remains; all beds which contain shells or organic remains, are superior to the primitive beds. Beds of crystalline limestone, which contain shells, &c. are termed *transition* limestone.

Mountain limestone, which forms so great a portion of the English series, (see fig. 7.) and on which the great coal formation rests, is a transition limestone. It is generally of a dark-grey or brown color, but never yellow; it contains frequently a great deal of bitumen, so as to be black, and cavities in it are frequently found filled with mineral pitch or oil; it contains a great many shells and corals, often running through it in seams.

Lias limestone, also forming a portion of the English series, is a compact limestone, with a dull fracture, compared to the primitive; it contains generally a portion of alumina, is best known from the fossils which it contains, and is sometimes called *gryphite* limestone. It is generally of a dark-grey or blue color, but some of the seams nearly white. This latter variety has been employed for lithography, but not found to answer so well as the Bavarian stones.

Oolite, which gives name to a large portion of the English rocks, has its name from being composed of small rounded grains like eggs, or the roe of a fish; it is a pure carbonate of lime, like primitive limestone, but its granular composition is quite different from the crystalline appearance of the latter.

Chalk requires no description; it is a pure carbonate of lime, but less dense or compact, than the preceding.

Marl is a carbonate of lime, mixed with argil or other substance.

Calcareous Tufa is formed by the deposits of springs and rivers containing lime in solution, and is therefore a formation now in progress; it is generally porous and of a dead earthy appearance; it often forms round leaves, twigs of trees, &c. of which it retains the impression after their being removed.

Stalactite is formed on the roofs of caves and fissures, by water containing lime in solution, percolating through the roof, and depositing it in the form of icicles; when these drops come too quick to part with all their calcareous matter in the roof, and form a similar deposit on the ground, they are termed *stalagmit*.

Magnesian limestone, called also *dolomite*, is a carbonate of lime and magnesia, containing 20 per cent. of the latter; it is harder than the carbonate of lime, and does not effervesce easily with acids, unless when heated. It does not form so good a mortar as pure limestone, but is considered a durable building stone; it is considered injurious as a manure, excepting in small quantity; the colour is generally yellow and aspect sugary, with frequently rhomboids crystallized through it. No shells are found in this limestone. It may be observed, that there is an imperceptible transition of these beds into each other, from primitive limestone to marl; and many of the limestone beds are impure, mixed with earthy matter. When it is wished to ascertain the quantity of lime in such beds, it may be done by weighing the limestone, and a quantity of acid sufficient to dissolve it; and on weighing it again, after it is dissolved, it ought to have lost, if it be pure carbonate of lime, 43 per cent.; hence, the excess of weight above this will give the quantity of earthy matter.

Gypsum, the sulphate of lime, is generally found nearly pure: from it is made the plaster of Paris, by drawing off the water of crystallization by heat; the powder is ready to reabsorb water, and on being mixed with it becomes hard, or sets. Gypsum is distinguished from the carbonate by not effervescing with acids, and by being less hard: it always accompanies the beds of rock salt, and is thus found in the red marl formation of England.

ROCK SALT requires no description; its form of crystallization is the cube into which it is generally cleavable.

COAL. There are three kinds of coal; namely, *anthracite*, *bituminous* coal, and *lignite* or wood coal.

Anthracite is found in the primitive series; it is without bitumen, and, therefore, chemically, is nearly the same as charcoal or coke, or the diamond. Seams of anthracite are also sometimes found in the coal formation, that is, along with beds of bituminous coal.

Graphite or Plumbago may be considered a variety of anthracite, but it generally contains also iron. The beds of bituminous coal in that part immediately in contact with whin dykes, are frequently without their bitumen; hence in this portion like anthracite.

Bituminous coal is the coal of the great and useful coal of formation; it never contains in itself vegetable impressions or immediate indication of a vegetable origin; it is, however, considered by many geologists, to be derived from vegetable matter. There are, however, in the beds which accompany it, as the BITUMINOUS SCHALE, many vegetable remains, as reeds, palms, &c. &c.

The beds which always accompany the bituminous coal, and hence termed the coal formation or coal measures, are chiefly sandstones, but also SLATE CLAY, CLAYS, SHALES, and in some of the formations, beds of limestone; but the latter are not found in the Newcastle beds. In the slate clays are found the IRONSTONE in nodules containing about 33 per cent. oxide of iron: it is from this ore that the furnaces of England are supplied, limestone being used as a flux, and in some places basalt, which besides its easy fusion has the advantage of containing a portion of iron.

The beds of sandstone in the coal formation, generally contain a portion of mica, and have an argillaceous cement; they are commonly white, and have black spots of coaly matter. These sandstone beds are often worked for a building stone, termed *freestone*, and when the portion of quartz is large, for millstones, then called *millstone grit*. When the proportion of mica is large, these sandstones are sometimes used for furnaces, and termed *fire-stone*.

Lignite, or wood coal, is found much higher in the series than the bituminous coal, being found in the oolite, and beds still higher, (see fig. 6 and 7.) It is generally brown, or even, when black, the powder from it is brown; it bears evidence of having been formed by vegetable matter, since the fibres of the trees can be traced in it. It is sometimes worked in the Continent, but scarcely ever in this country, and then only for factories, being disagreeable for domestic use; the seams are also generally thin. The finding of this coal, as must be evident from its position in the series, gives no indication of being in the neighbourhood of the great coal formation, but the contrary.

Jet is a lignite highly bituminized.

Cannel coal is nearly the same as Jet; it is said to have its name from the word candle, as being used to give light.

PETROLEUM, NAPHTHA, MINERAL PITCH and OIL, found in various parts of the world, in pits and wells, are considered, in some cases, as being derived from the coal; in others, the derivation is not at all established.

Of ORES, IRON, and COPPER PYRITES, LEAD GLANCE, and the various ORES of IRON, are the most likely to be met with. Some of the latter are, however, often in a state in which they cannot be determined by their external character, being mixed with earth, or in a state of decomposition. The *bog* and *meadow iron* are found in masses or lumps on the surface, and are supposed to be formed by deposition from water, having iron in solution. This ore is, in many situations, used to a considerable extent, and affords a good metal. For the character distinguishing iron and copper pyrites from each other, and also the various ores which may occasionally be met with, reference must be had to works on mineralogy.

It will be seen from the preceding descriptions, that it is chiefly the crystalline rocks which are distinguished by their mineralogical composition: these lie lowest in the series, and in following it from them to the upper beds, the general character is, their becoming less distinct and crystalline, or more mixed and earthy as we ascend. This has led to divisions in the series; and although the names thus given are dependent on a theory or supposed order of formation, now almost abandoned, still the terms are retained by most geologists, as being understood from their having been so long established.

The divisions alluded to are those introduced by the school of Werner, of PRIMITIVE TRANSITION and SECONDARY, which may be defined as follows:—

The PRIMITIVE rocks or strata, are such as do not contain in themselves, nor rest upon any beds which contain organic impressions, and they were hence considered as having been formed previously to the existence of animals or vegetable life. They consist of granite, gneiss, mica slate, clay slate, primitive limestone, sienite, hornblende rock, and some of the porphyries.

The TRANSITION rocks extend from the beds of clay slate where organic impressions are first formed, to the old red sandstone or grauwacke of the Germans, or to our mountain limestone, which lies immediately under the coal formation. Others make it extend to the coal inclusive: this difference arises from the coal beds in Germany being unconformable to the strata on which they rest, which makes them be considered as the commencement of a new formation, whilst in England they are conformable to the transition.

The SECONDARY rocks extend from the last mentioned point, up to the chalk, inclusive.

TERTIARY is the term now generally given to the beds above the chalk: these extend from the chalk to the uppermost compact strata. Above these lie beds of detritus, gravel, sand, &c. &c., such as we now see forming in vallies or at the mouths of rivers, from the matter brought down by them, which latter beds are termed ALLUVIAL; but since the former often cover the tops of hills and elevated ground, and seem to have been occasioned by some extensively acting cause which has ceased, they are termed DILUVIAL.

DILUVIAL beds, as above mentioned, consist of detritus: they have been formed previously to the excavations of the present vallies: in them are found the bones of animals not mineralized, but of species distinct from any now inhabiting the surface of the earth, though sufficiently near to be referred to the same genera, particularly of the elephant and hyppopotamus. Above these beds, as has been already mentioned, come the ALLUVIAL beds. The formation of these is still in progress, from the decomposition of mud and gravel at deltas and in other situations. An advantage from the above divisions in the nomenclature which it introduces, is that it at once gives the mineralogical name of the rock, and refers to its place in the series which is its geological or geognostic position, as a primitive limestone or granite, transition, &c. &c.

An eminent geologist has lately endeavoured to introduce a new nomenclature of rocks, which refers only to their geological position, so that the same rock as granite and gneiss, if found in one point in the series, has a quite different name from what it would have in another; but this does not seem likely to become general.

II.—*Letter from the Himmalaya.*

The following letter was written by a friend several years ago. Although the topics it touches on have all been previously brought before the public, yet, as there are many of them new to the generality of our readers, we have thought they would not be unwilling to see them treated in a familiar and interesting manner.

"I have before remarked that several very eminent snowy peaks are called *kylas*, by the mountaineers, one of which is near the Mansarowa lake; another a very magnificent three peaked mountain I saw and nearly approached to in Kunaur up the bed of the Satlej in 1818; its base is washed by that great river, where it leaves the broken plains of Bhót or Tartary, and enters the Himmalaya; but there is one *kylas* celebrated in the extravagant legends and fabulous mythological stories of the Hindoos, which may perhaps be grounded on geographical facts, though much obscured by nonsense. This *kylas*, or *cailasa*, is supposed, by them, to be the chosen Olympus of Mahadeo and his heavenly choir, where they sing, dance, dally, and drink nectar, and otherwise enjoy themselves much in their fashion. There, according to them, the Ganges rises at the feet of three peaks, and clad in snow, and lofty beyond all human measure; their summits shining with gold, diamonds, and all manner of precious stones. If this rhodomontade is founded on any matter of fact, the place we are now at may be the *kylas* in question, and Mahadeo's votaries will not thank us for reducing his altitudes. Many of the Hindoos were of opinion, that the earthquake at Gangotri was a demonstration of his displeasure at our prying into his secrets; but we attempted to convince them that it was only his method of giving us welcome, as it could not be supposed he could receive us without noise and *dhumdam*, and that neither demons nor devils had any power over Britons. When we returned to Gangotri, I assured an old Bramin whom I left here, that I had seen Mahadeo. Instead of laughing at me, he seemed to take it gravely. I have since repeated the fact. We accounted for the natives with us not being honored with a sight, from the circumstance of their want of new light to see the same. The measures we took of the peaks St. George, &c. must not be considered as decisive of the heights of the Himmalaya: those measures were not taken under favorable circumstances either in the barometrical or geometrical parts of the operations, but I judge that the error does not exceed 200 feet on the whole. The question of the heights of the snowy peaks is determined from a very extensive trigonometrical operation proceeding, instituted for the purpose, and carried on with excellent instruments, much care and labour, and upon just principles. An account of these operations will be published in all its detail for the satisfaction of the scientific. The height of some of the peaks in this survey is about 23000 feet above the level of the sea, but there is one in the Camaún survey, upwards of 25000, which is, as far as we know, the highest mountain on the earth. People in Europe are unwilling to believe that the Himmalaya are higher than the Andes, but the examination of our data will satisfy them that every precaution has been taken to confine the effects of terrestrial refraction within very narrow limits, by modes which it would take up too much room to describe here, but the chief principles of which are to take reciprocal observations of elevation and depression, and so to dispose the terrestrial arcs that they may be only of a moderate extent, and the angles of apparent elevation as large as possible. We find that at low elevations, as 1° 40′ and 2°, and at distances of 40 to 60 miles, mountains of from 6500 to 12000 feet are subject to be affected by refraction in the ratio of nearly $\frac{1}{12}$ of the subtended arc. In only one instance, and that on a long arc (Karnál and the Chúr peak) and in dark weather too, it was so great as $\frac{1}{5}$. Within the mountains, where the air is light, clear and drier, it is $\frac{1}{18}$ and $\frac{1}{20}$, a small quantity where the arcs are only of 15 or 20 miles and the angles of elevation 5°, 8° and often more degrees. You are to understand by this that the arcs are expressed in feet and turned into arcs of oblique circles; but, for the sake of explanation, we may roughly consider as geographical miles the distances from station to station, and take a large ratio of refraction as $\frac{1}{10}$th. Thus, if a peak at the distance of 120 such miles appears elevated above the horizon 2°: or 120′, its apparent elevation is to be reduced 12′, its true angle then is 1°. 48′; and 12′ being a large proportion when the quantity of refraction is only *assumed*, it is not satisfactory to rely on such long arcs and small angles of elevation of objects seen through a moist and dense medium, as is the atmosphere of the plains. But they are of great use in comparing the heights determined on the whole arc, with the sums of those given by smaller arcs and greater angles of elevation; and the comparison proves, that even giving to calculations made hitherto, often on long arcs, an extreme quantity of refraction, the peaks of the Himmalaya surpass the heights of the loftiest of the Andes by some thousands of feet, which that prince of travellers and excellent observer, M. F. Humboldt, will prove if he comes, and I wish he were here to do so. His researches also will throw great light on the geology of our mountains, and their vegetable and mineral productions, and other subjects beyond our skill: but as to the measurements of their distances, their latitudes, longitudes, and heights, I think he will fully confirm what we have done. Perhaps in England they think that officers of the army are unequal to the task, but really it is not mysterious; good instruments, time, care, perseverance, and a moderate skill in calculation, are all that is required. We have followed the methods of those skilful observers, Roy, Mudge, Dalby, and Lambton, in the English surveys, and those of Delambre and Le Gendre in the French, in calculation, principally the latter, as their calculations are the most ready: but they all give the same results. In the observations themselves, we adhere to the English practice, which is adapted to our instruments. Excellent as the French repeating circles no doubt are, and easy to transport, their construction requires several corrections and calculations, which take up time, and may lead the unwary and less skilled into mistakes when much is to be done; but the English circles, by Troughton, which give, when well adjusted, direct angles, horizontal and vertical, are, though less portable, more direct and downright in use. We returned to Gangotri, of course, by the same route, and as all things are more or less wild by comparison, that edgeway, and by us once considered remote place, seemed like a welcome home. We found our people all well, but rather hungry. Thence we went by Bhairoghatie and Deráli to Súki. Having now no object in view, except to return, the ways seemed rougher, and we were more fatigued. We arrived at Súki on the 6th of June, when the rains began in earnest; it rained night and day till the 13th, with a degree of violence I never before witnessed, and our situation was very unpleasant, having no other shelter than our very little tents, smaller than those used by the jemadars of the native infantry. There were a few ruinous and dirty stone houses, but we durst not inhabit them on account of the earthquakes which happened almost every day. I had a portable stove, which there, as elsewhere, was our greatest comfort; however, we were in a safe place, not overhung by any cliffs. We were generally shrouded in cloud and mist, but heard more than we could see. The tremendous and unceasing crashes, caused by masses of rock, loosened by the rains and melting snow, were awful. By day and night this uproar went on, but we got used to it; though sometimes, when some great piece of cliff from the steeps across the river was precipitated, the noise was really alarming; and even the apathy of the natives was roused, and they ran out to try to see through the thick gloom if the end of the world was coming. Occasionally the mist cleared away a little, and we could see the vast bounds made by the falling rocks, and the havoc they had caused among the pines; and how much the face of the mountain across the river was altered! It rose steeply from the stream to the height of several thousand feet. The river was about 1000 feet below us, and its roar contributed to the confusion around. We saw, with some alarm, that it rose rapidly, fearing it might take away some of the *sanghas*, when we should have been prisoners, and our grain, (a supply of which had been luckily sent from Raítal,) was nearly expended, and no one durst go to Raítal along the bed of the river while the rocks were falling. For ourselves we got a few *monáls* (*Phasianus Impejanus*), but were relieved from our anxiety on the 13th, when the rain ceased, and, making a few observations, we set off for Raítal, finding the *sanghas* all in good order. Right glad we were to see, at the distance of several miles, the old Union waving over it. We were absent from that village 23 days. From Raítal we returned by Barahát to the Dún and Seharanpúr, and then I afterwards joined the reserve of the grand army, under Sir D. Ochterlony.—This letter has now run to such a length, that I must defer giving you an account of my excursions in other parts of the mountains up the Satlej within the Himmalaya in Canaúr, of my passage over the snowy range, in June 1816, and journey to the source of the Jumna at Jumnotri, in April 1817; but I will, in a general way, notice, among many other peculiarities of the mountains, some of the most remarkable. And first, of the earthquakes, which are much more frequent in their occurrence, and more destructive in their effects than in the plains. In one month, in 1817, about the same time we experienced so many shocks, forty, I understand, were counted in the Camaún mountains, all slight I believe, and not felt in the plains, except that of the 21st May, which alarmed us so much at Gangotri, and which was smartly felt over the north-west provinces of Hindustán. You may have heard of the earthquake of 1803, which was considered violent in this country, and many buildings were damaged over the whole extent, from Bengal to the Penjáb, but in the mountains its effects were terrible, and a great part of the population perished; whole villages having been buried by the fall of cliffs and sliding down of the faces of the hills. The scenes of that havoc have often been pointed out to me. The imagination can hardly form an idea of a more terrible event than such a catastrophe! What can be the cause of these more frequent and violent shocks in the mountains than in the plains? We saw no volcanoes, nor heard of any, and I believe there are none. Thunder and lightning are much less frequent in the upper mountains than in the plains, and I do not recollect any, except once on the way to Jumnontri, at a place near the bed of the river, and not above 8000 feet above the level of the sea. The earthquake at Gangotri was by far the most alarming phenomenon of nature I ever experienced, and the frequent, almost daily, recurrence of shocks, though slight, made us uneasy, as it showed there was some active agent at work, perhaps, under our feet, which might at any instant bring down the cliffs under which we scrambled along on our hands. To avoid a few falling blocks is difficult, and might be impossible; but no activity could save the traveller from his fate in those extensive falls, among the ruins and rubbish of which our path very generally lay. You know how keenly the question is agitated among some philosophers of Europe, whether certain appearances on the surface of the earth have been caused by the action of water or fire? For my part, so far from presuming to give any opinion on such subjects, I confess that I have so little geological knowledge, that I am not able to describe accurately, or in terms of science, the nature of the various rocks and soils which compose the mountains from the plains of Hindustán to the heart of the Himmalaya. But one small range of hills, that which is next to the plains,—as for instance, that which divides the Dún valley from the low country,—certainly appears as if it had been the deposition of water. It is about 6 miles in depth, and the height of its various sharp eminences were from 500 to 1300 feet. This particular range extends from the Ganges, at Haridhuara, to the

Jumna, at Padsháhimahl; but the same sort of hills rises from the plains, the north-eastern frontier of Bengal as far as the Satlej, and probably further to the north-west, its direction being nearly parallel to the great Himmaláya, or from east 25 south, to west 25 north, and the features of these small hills have, in most places, a miniature resemblance to the snowy peaks, their *apices* point the same way, *i. e.* about 25° to the west of south, the south western profile being steep, and the north-eastern less so. In going to the Dún from the Doáb plains, we pass through broad, strong water-courses upon slight acclivities, more than two-thirds of the way, and then gaining the crest of the pass, descend to the Dún valley, but the descent is shorter than the ascent. The water-courses are bounded by precipitous walls of soft sandy rock and large rounded stones of granite, quartz, and gravel: these components are arranged in strata, alternating several times; some of the layers are only 3 or 4 feet deep, others 30 or 40, and they point upwards in angles of from 25 to 35, and perhaps 40 degrees. The rounded form of the stones, one is apt to imagine, must have been caused by the powerful and long continued action of water; and the general appearance of the strata is not unlike what would, in miniature, be represented by a section cut through the sand and gravel at the high-water mark of the sea. Can these small hills have once been the boundary of the ocean, when, as we may suppose to have been the case, the plains of the Gangetic provinces were yet under the waters? These plains, you know, contain few stones, and are so little elevated, that Seháraupúr, though at a great distance from the sea, is only 800 feet above its level. Supposing such to have been the case, the Dún may have been a safe harbour; call it the *Downs,* (Doona sigifies a valley.) The Dún between the Ganges and Jumna is about 44 miles in length, and in breadth 11 generally. Though somewhat uneven, it is a very beautiful valley; the slopes are shaded with the saul and other forest trees; it is well watered by the Soang and A'san rivers, and many brooks; and some parts of it are carefully cultivated. Its southern and western boundaries are the small hills above mentioned; but on the N. E. side, larger mountains, as Bhadráj, Súrkanda, and others, of the heights of 5000 to 8000 feet, rise abruptly from it. During the winter months, the summits and parts of the sides of these are covered with snow. From the base of these commence that huge jumbled mass of mountains, which fills up the whole space to the feet of the grand Himmaláya, towards which, they, for the most part, increase in height, but the summits are not so sharp as those of the snowy peaks or of the little range which rises from the plains. From the higher elevations of Bhadráj, &c. we enjoy a noble view, which commands the admiration, and rivets the attention of the most phlegmatic, of the towering pinnacles of the Himmaláya, shining with pure and brilliant snows, and rising far above the intermediate irregular mass of mountains, which resemble the billows of a stormy ocean; and to the S. E., S., and S. W. are seen the plains of Hindustán stretching far away, and entwined by the shining streams of the Ganges and Jumna, and other rivers. An unrivalled Panorama! It appeared to me that the rock at the bases of these intermediate mountains, where laid bare by the action of water, was chiefly of granite with much quartz and mica intermixed, under an outer coating of soil and friable sandy stone; and occasionally large masses of calcareous rock present themselves, both near the beds of the torrents below, and on the sides and summits of ranges of 5500 feet high. Of this nature is the Sain-ka-Dhár, between Jaítac and the Chúr: some of the points of 6500 and 7000 feet high, are chiefly composed of coarse slate and quartz, as is Bairát. Both the base and summit of the Chúr, which, though only 27 miles in direct distance from the plains, is one of the most remarkable, if not the highest of the mountains of what I will call the second order, (i. e. from 12000 to 15000 feet high) is chiefly composed of coarse granite, with large nodules and bands of quartz and other ingredients, though in the flanks and sides there is much soft sandstone and shining particles, and small sheets of talc, but little lime. Several of the mountains abound with iron ore; the iron is of a good quality, and some is exported to the plains. Every where, I think, quartz is to be found. Of the nature of the rock of the Himmaláya, I have taken notice as I passed along, and I sent specimens of it to Calcutta: it proves to be granite, of one sort or other, as I suspected. Lead is found in the hills above the Tóns in tolerable abundance, and there is copper in some places, but it is difficult to work, and the population is so limited that people enough cannot be spared from the labours of agriculture to make that of mining advantageous. You know that the red wood used for black lead pencils is usually called cedar: it is really a species of juniper, *(Cedrus Juniperus,)* and red cedar is a small relation of the family: it is found in Canaúr. I found it at our bivouac near the source of the Bhágiretti, at the height of

12914 feet above the sea. It there had the form of a large creeper, (not of a tree;) some of the branches were 6 inches in diameter and of a considerable length; in some places they were above the spongy soil, and in others below the surface. We used it as fuel; the wood has the same red colour, brittle and soft grain, and pleasant smell, as the pencil wood. But Lieutenant Herbert, when he went up the Jahnavi river, found this juniper cedar in the form of a small tree. Botanical writers also mention expressly that the wood used for pencil is a juniper. But the tree which I have in this note denominated cedar, is the Great *Pinus Cedrus*, the cedar of Lebanon, with the description of which it agrees in every particular;—the cones, the leaves, the spreading branches, great size of the tree, the durability yet brittleness of the wood, and its peculiar smell. This noble tree, which towards the Satlej is called *Cailon* or *Cailang*, but in Garhwál and the eastern mountains, Deodár, flourishes on the N. W., N., and N. E. faces of the mountains, and at the elevation of from 6000 to 9000 and 10000 feet, though occasionally below and above both those limits; its nature seems to suit best with an elevation between them. The northern faces of the mountains are very generally shaded by large forests of cedars, and there the snow always lies from 2 to 6 months in the year; the northern faces of the mountains are always less steep than those of the southern ones, and have more soil on them. I have frequently measured the larger trees, and found them 24 feet in circumference, or 8 feet in diameter, at 6 feet from the ground; but those of about 18 feet in circumference are more common; their height, though great, is not, I think, quite in proportion to their thickness, and they are, perhaps, exceeded by some of the more slender pines: one of these last, which had fallen, I measured, and found it to be 169 feet. It was of the *Rai* kind, and exceeded by others standing near it. The largest cedars often separate into two upright branches at the height of 30 or 40 feet, but the middle sized trees generally have but one bole, and are very straight. The wood, of which I will send you specimens, is nearly similar in colour to deal, but rather darker: has a fine, but brittle grain, and a peculiar though not unpleasant smell, which it retains for ages: it is reckoned the most durable of all timber, and most valuable in house building, but it is too brittle for ship's masts. No insect will eat it. When the bark is cut, a fine white resin distils in large quantities from the tree: this resin and the oil obtained from it are much esteemed, and it is said it was used in Syria to preserve dead bodies from corruption. Much oil is also obtained from the cones, which are of an ovate form, about 3½ inches long and 2½ in diameter, the scales close pressed, the cones stand at right angles from the branches. The leaves are in small bunches, of a deep green, bristly and 1¼ inches in length; the bark coarse, and about ⅝ of an inch thick. The branches shoot off nearly horizontally, with sometimes a slight sweep downwards in the center, recovering their straightness towards the extremities; the lower arms are the longest, but where the trees grow near each other, the bole has often no branches, but only the marks where they have been when the trees were younger and occupied less space; as there is little underwood in the forests of the cedars, the shaded and solemn passage under them is seldom obstructed. It is needless to say that it is impossible to transport large trees from the mountains, but cedar planks may be carried from the north side of the Chúr to the plains. Many kinds of large timber grow in these regions, and one regrets the impossibility of removing them: some of the stately pines afford the finest deal, and would make excellent masts; their wood is far superior and very different from that of the small pines, called *Chír*, which were cut in the low hills near Haridhuara and sent to Calcutta: even those spars were thought of some value.

I. A. H.

III.—*On the Different Methods of Shading Mountain Land.*

A map may be considered to be an orthographic projection (reduced to a small scale) of any portion of the earth's surface, sufficiently limited to be synonimous with a plane, at least as to sense. In this projection the eye is supposed to be at an indefinite distance, and consequently the rays are all parallel.

But as the surface of the ground in mountainous countries is far from even, and consequently cannot coincide with a plane even as to sense, it is evident that an orthographic projection of points in such a map will not give a correct idea of their relative position. Thus, in the annexed diagram, representing a section of the earth's surface, the places A B will be represented in a map at *a. b*, but it is evident that the distance of the points *a b* gives a very inadequate idea of the distance A B.

This difficulty, which involves the representation of all the inequalities of the proposed portion of the earth's surface, and consequently the features of a mountainous country, has been variously got over. In the very old maps a congregation of conventional signs, each intended to represent a mountain, was introduced and arranged so as to give some idea of the direction in which the mountain land was disposed. Those who have seen maps executed on this principle, will acknowledge how very imperfect is the information it gives of the inequalities of the ground, either as to arrangement and connection, or elevation above the general plateau of the country. Rude as the method is, and destitute of precise meaning, I have known many who affected to prefer it to the modern method of deep hatching, which they assert has the effect of rendering a map illegible without being a whit more precise. There is some ground, it must be confessed, for forming this opinion—at least if we confine ourselves to English maps; but those executed on the continent do not deserve this censure. Arrowsmith's Map of India is a splendid example of this cheatery of the eye. To form any thing like a correct idea of the surface of India from his map, thickly as it is covered in parts with these "caterpillars," as I have heard them called, is impossible. Very often it will be found that what the peruser of the map supposes is a high ridge, is in reality a low one, and vice versa; while vallies are elevated into ridges, and these again sunk to vallies. A certain quantity of ink has been applied to the paper, but to what purpose, save that of blackening it, it would puzzle Œdipus himself to say. Nor are the maps of other publishers much better, and, still more extraordinary, I fear, that even to many manuscript maps executed in this country, much of this censure must attach.

The map-makers on the continent have higher ideas of their art, and do not trust so important a feature of their work to an ignorant draughtsman, who knows no more of the principles which should guide him in the representation of ground, than the brush, by the aid of which he annually destroys so much paper. It is evident that a map, to be any thing, ought to be precise; it is otherwise worse than useless. To represent hills where there should be vallies, and vallies where there should be hills; or to give an erroneous idea of the comparative heights of different points above the general level, is to take from the map all the value it could ever have; and it would be much better and less troublesome, not to attempt at all the delineation of the surface features, than to represent them erroneously, inasmuch as ignorance is preferable to error.

"They manage these things better in France," as I before observed, and in Germany too. Some of the continental maps are quite curiosities in this respect; I mean considered with regard to the accuracy of representation with which mountain ground is delineated. On the continent they always consider it necessary to determine the three co-ordinates of every position, but we are satisfied with two; and indeed much of the foregoing censure is applicable as much to the geodest or surveyor, as to the draughtsman. The latter cannot frame a representation, the data of which are wanting, and he is, therefore, often compelled to draw on his imagination. It is no wonder then if we occasionally see such grotesque productions.

Supposing all three co-ordinates known in any given portion of a country, it may be asked, how can more than two of them be inserted in a map which is a plane surface, and, consequently, can have but two dimensions, length and breadth? To this it may be answered, by a similar artifice to that employed in a picture, in which objects of three dimensions are represented on a plane, and with such effect as to enable the eye to judge with considerable accuracy of figure and proportion: by an application of the principles of light and shade, it is possible to give to a plane surface, all the appearance of projections of various heights; nor is the application of colour necessary to obtain perfect distinctness, as far as mere figure and situation are concerned. The map, in fact, becomes a perspective representation of the country, the eye being supposed at an indefinite distance.

This is sufficiently obvious. The question is, how we are to regulate our shading so as to produce an accurate representation of all the inequalities of the ground we are employed in mapping? To answer this question, we must first settle the direction in which the light is supposed to fall, as on that circumstance the arrangement of the shadows depends.

Out of the many directions which might be assumed, two only have been generally adopted. In the one, the rays of light are supposed to fall at an angle of 45° from the N. W. corner of the map, the shadows being all directed south-east; and those planes facing the N. W. at an angle of 45°, being in brilliant white light. In the other, the source of light is assumed to be in the same position with the eye, at an indefinite distance, and the rays, therefore, fall perpendicularly on the map. Every horizontal surface will, in this method, have a brilliant white light, while perpendicular surfaces alone will be quite black. I ought to observe, that I use the word shadow as equivalent to more or less deprivation of light, and not as indicating what are commonly called projected shadows.

The first of the above methods is that practised by the draughtsmen attached to the Ordnance Survey, and by them taught to such of the Company's Engineer Officers as are allowed to attend that survey. When well executed, it has a fine effect, and indeed some specimens I have seen executed from models, emulated all the force and truth of a pictorial representation. The gradation of tints or effect of aerial perspective, is added to make the deception the greater, but I think injudiciously, inasmuch as it takes from the geometrical simplicity of the principle, and renders it less subject to the regulation of the scale and compass. This latter is, in my opinion, absolutely necessary to attaining that precision both in the representation and perusal, which can never be the result of judgment by the eye alone.

Another objection to it, in my opinion, is that, properly speaking, all the plain country should be shaded, and the white paper only left for those surfaces which, being inclined at an angle of 45°, face the north-west quarter. For as the number of rays which fall on equal portions of surface will be as the sines of inclination at which the light falls, i. e. the inclination of surface + 45°; so in horizontal surfaces and those inclined 45° the proportion will be as sin. 45°: 1. But the inclined surface is projected into a lesser one in the proportion of the co-sine of inclination. The light will then be still more concentrated on its orthographic projection, so that the difference will be even greater than the above, and will be equal to sin. 45 cos. 45°: 1. Now in no map or drawing that I ever saw executed on this principle, has this point been attended to; nor, indeed, am I aware that those who follow it, are aware of such a result flowing from their principle.

The second general method, which is, I believe, the practice of the German school, depends on the supposition that rays of light fall perpendicularly on the surface. Projected shadows, therefore, there will be none, but every surface will have a gradation of shade proportional to the deprivation of light occasioned by its inclination to the direction of the rays. It is inferior in effect, considered as a picture, inasmuch as where the steepness is equal on two sides of a ridge, the shading is the same. But it appears better adapted for the purposes of a map, which is to afford precise information. Now in a map executed on this principle, the peruser of it knows that every equally deep shade is an equally steep surface,—a point of the utmost consequence in a military view, and one which can only be gathered by guess or loose inference from a map executed in the English style. So precise are the Germans in their practice of the method, that the proportion of black to white, or ink to paper in these shades, where the method of hatching is used, is regulated by the consideration of the light which would fall on each surface compared with a horizontal one, the latter being represented by a full light or the white paper. Thus, from white to black we may have an innumerable series of shades, all accurately representing corresponding inclinations between 0° and 90°, or horizontal and perpendicular. If we suppose the lines to be of the same force, then their number may be as the diminution of light; *i. e.* as 1 : cos. inclination. Or they may be the same in number in every shade, but proportioned in thickness, so that the surface of paper shall be as the light, or as the cosine of inclination, and the thickness of the lines as the difference between radius and that to sine.

The combination of both will generally be required in hatched shades. But in shading with Indian ink great facility is obtained. Thus, if we have a series of 11 cups, the two extreme ones containing ink and water, the first will be the shade for a perpendicular surface, which is, in fact, represented by a line: the latter for a horizontal surface, which is the paper now in the cup next the water; if we put one of ink and 9 of water, in the next 2 of ink and 8 of water ; then 3 and 7, 4 and 6, and so on till we come to 9 of ink and 1 of water, being the next to the pure black shade; then these 9 tints will represent the following inclinations

No.	Ink.	Water.	Angle of Inclination.
1	1	9	25°. 51′
2	2	8	36. 53
3	3	7	45. 34
4	4	6	53. 03
5	5	5	60. 00
6	6	4	66. 25
7	7	3	72. 33
8	8	2	78. 28
9	9	1	84. 15

To apply these tints in the method of shading by hatches, is more difficult. If we assume that the thickest line which can be admitted is about $\frac{1}{60}$ of an inch, and conversely the broadest space, then we shall have the following scale:

Angle of Inclination.	Breadth of Line.		Breadth of Space.	
	Inch.	Inch.		
25. 51	,002	$\frac{1}{500}$	,018	$\frac{1}{55}$
36. 53	,004	$\frac{1}{250}$	,016	$\frac{1}{66}$
45. 34	,006	$\frac{1}{166}$	,014	$\frac{1}{71}$
53. 08	,008	$\frac{1}{125}$	,012	$\frac{1}{83}$
60. 00	,01	$\frac{1}{100}$	,01	$\frac{1}{100}$
66. 25	,012	$\frac{1}{83}$	,008	$\frac{1}{125}$
72. 33	,014	$\frac{1}{71}$	,006	$\frac{1}{166}$
78. 2	,016	$\frac{1}{66}$	,004	$\frac{1}{250}$
84. 15	,018	$\frac{1}{55}$	,002	$\frac{1}{500}$

It must be confessed that the principle of this method is excellent, in as much as it affords a definite scale by which to measure the inclination of the surface. In practice it is said to be not difficult, and to be capable of representing with wonderful detail and certainty every the minutest undulation of the ground. But we may easily perceive that this eulogium is to be understood *cum grano salis.* For it appears perfectly hopeless ever so to apportion the thickness of the lines and spaces, as to represent with any thing like fidelity the actual inclination of each surface. If then it be found so easy in practice, it must evidently be from this fidelity being disregarded in a certain degree. In fact, it is not pretended that the inclination of every surface is actually determined, and also the depth of shade by which that inclination should be expressed ; but rather that, having fixed a few positions by which the outline of the feature is obtained, the eye is depended on for an estimate of the slopes, as well as for the depth of shade necessary to represent them. Practice will give great facility, there is little doubt, in these operations ; still there can be no question that the eye will often make mistakes, for it is proved that nothing is so deceptive to the spectator, or so difficult to judge of with accuracy, as an inclined surface.

But as these objections apply equally to what may be called the *pictorial* representation of ground, there can be no question of the preference due to the former over the latter. When most loosely executed, it represents equally precise information, and in a character which is not only easily read, but which is definite to a degree exceeding that of the information to be conveyed. It is, at the same time, capable of representing, with perfect accuracy, all that can with any degree of trouble be ascertained, so that it will always keep pace with observation. No particulars collected need be lost for want of means to represent them. The pictorial method has none of these claims on our attention. It is, in fact, only fit for those who consider a map as rather meant to please the eye than to satisfy the judgment. When well executed it must be allowed to give a tolerable notion of the general condition of the surface, but that notion is one rather of effect than of detail; it is general, not particular; nor will it bear any verification or minute examination by the application of any standard. And it is to be considered, that to give even the rough notion here indicated, requires a talent in using the brush, not to be found with every one. For one draftsman capable of such work, ten will be found incapable. And as the expression of ground in this method, is entirely a matter of tact or practice, corrected by a natural taste, and does not depend on the application of any mechanical contrivance or rule, it is evident that none but proficients can attempt to do any thing that shall be useful in this way. The German system can be taught to any one, and even the first or rudest essay, will often be superior (as conveying more exact information) than perhaps the most successful essays in the pictorial style.

Yet, though I find no difficulty myself in giving the preference to the German method, as superior in principle to the other, as being capable of always keeping pace with the utmost accuracy of observation, I must not conceal from the reader, that the pictorial style has its advocates, and these no common ones. In this country, all who have studied under Mr. Dawson, of the Ordnance Survey, are enthusiastic in its praise, and it is pronounced impossible to withhold assent to the excellence of the system after examining his surveys. Mr. Dawson himself, who has been so many years employed in representing ground, must be allowed to be a witness in its favor worthy of some credit. But the most powerful champion I have yet met in its favor, is M. Puissant, the author of two treatises on *Geodesie* and *Arpentage*. In a pamphlet which he has published, he ably states the merits of his favorite system and the demerits of the German; and he has, upon the whole, made out a better case for the pictorial method than *à priori* I should have judged possible. Yet the strongest objection that he has brought against the German style is the difficulty of distinguishing between a hollow and a projection. Thus, suppose a cone of a certain angle and height, and a conical depression of the same angle and depth, it is evident that they would be represented by a circle uniformly tinted with that shade which corresponds with the angle of their sides. But though it must be confessed that the method has no means of discriminating in these cases, supposing the two instances to be artificial—models in fact—yet, in nature there is always this assistance, that the course of the streams will tell in which direction the surface is inclined. Such a hollow, in fact, would, supposing no outlet, inevitably have water at the bottom; and this was the answer given by a draughtsman of this school, who was at first puzzled when appealed to, to say whether the surface was in depression or in relief. Another objection made by M. Puissant, and it is not without its force, is the difficulty of distinguishing the detail names of towns, rivers, &c., in a map finished in this way, in consequence of the very dark shade which is so much spread over the map. This objection, though applicable also to the pictorial style, has only half the force it has in the other. Yet, finally, in spite of M. Puissant's evident leaning towards his own favorite style, the force of truth compels him to acknowledge that the draughtsman ought to practise and be familiar with both. And he allows that for military topography, particularly in a country where the inclination of surface is not considerable, the German method has wonderful power. What tells more in its favor, is that a commission of the first mathematicians in France having been ordered to report on both systems, and to recommend one for general adoption in the *Bureau des Cartes*, the numbers were so nearly equal for each, that no final report could be drawn up. This is saying much for the German, if we reflect that the French are not often found preferring foreign suggestions and foreign systems to their own. And finally, Puissant is obliged to acknowledge, that to enable the pictorial system to give precise information, it must be combined with a series of lines of equal level, which is almost equal to giving up the question altogether. In fact, though I acknowledge M. Puissant to be an able advocate for the method he prefers, I have not found my confidence at all shaken in the other, by any thing he has advanced; and it must be admitted by the most prejudiced in favor of the pictorial system, that the other is the most scientific, and consequently the most definite. But which is actually to be preferred in any particular case, must depend upon the character of the person who has to choose, and the nature of the information he has collected.

While I have no hesitation in preferring it, then, to the other, I am not insensible of the force of the objections which may be made to both. It is, in fact, the object of this paper to notice them, and to enquire if there be no other method of expressing the three co-ordinates besides these two, which might yet be free from the objections to which they are liable. The following are what appear to me to be objections to that which is, in my opinion, the preferable of the two.

The German method applies itself to represent surfaces, expressing, by the depth of shade used, the inclination of that surface to the horizon. This is always an important feature in the delineation of ground; and to obtain accurate information on this subject, is certainly, at least to military men, a principal object in consulting a map; I mean a topographical one. The knowledge of the acclivities in a mountainous country naturally lead us to infer the relative heights of the different positions; so that in a map executed in this manner, the person who consults it may, with a little consideration, satisfy himself of the relative height of every point in it. It must, however, be acknowledged that these particulars are not forced on our notice: it requires, as I said, some attention to discover them, and what is of more consequence, it is only the *relative* heights that can be thus known. When we look at a map of this kind we do not immediately perceive which is the high ground and which the low, but with a little attention these particulars become visible. No attention, however, will enable us to say HOW high any point is. Yet, information of this kind is often required in a map, and there is little question that were they capable, in general, of conveying such information, their utility would be very greatly increased. I may add, that the German method is incapable of expressing those delicate undulations and differences of level which are often to the engineer the most important of all. These are certainly exceptions to our unqualified admiration of the method, and it must, in fact, be acknowledged, that it is no more capable than the other of representing, in a generally intelligible manner, ALL THREE CO-ORDINATES of any point on the surface of the earth.

It may be replied by the advocates of the method, that as the slope of every part of the ground is expressed, and also the horizontal projection, a combination of these elements should, by calculation, enable us to fix the absolute height of any point in the map. But besides that, this would be an enquiry into which an ordinary consulter of the map could not be expected to enter; it would be so great a labour, that I can confidently affirm, no one would, however qualified, engage in it. And besides this labour, there is the further objection, that it is not possible any map could ever be executed on this principle with such accuracy as to enable it to be applied to such a use. If the map could express the heights accurately, the slopes would be almost obvious to the eye, and could, with a very trifling application of thought, be conceived with the utmost accuracy; but the reverse of this is so far from being true, that it is, I will venture to say, scarcely possible.

To these objections may be added, that a method which employs shading to express inclination of surface must have the effect of rendering a map of a mountainous country such a map of shade, that no other particulars will be sufficiently visible. Nothing is commoner than to hear complaints on this head against such productions. Not only are all the other features of the ground, in topographical delineation, concealed, but villages and towns, with their names, and sometimes even the rivers, where the hatching is very deep. In such a map you cannot distinguish whether a mountain be bare or covered with forest, cultivated, or a barren waste; water-courses are dried up, and wells or springs are looked for in vain; while any information to be conveyed in writing is altogether lost. Nor is such system of shading much better for geographical maps. In fact, it is evident, that in adopting shade as the expression of any general feature of a country, we lose the principal advantage of exhibiting clearly to the eye the other particulars; many of which are equally valuable. It is as though we used a dark paper on which to draw our map. In the case of a geological map, the one, perhaps, in which the expression of the arrangement of the surface is most valuable, the method is altogether inapplicable, on account of interfering with the colours employed to represent certain rocks.

These objections, which are not made in a spirit of cavilling, certainly take from the merit of the system. In considering their force, I have been led to enquire whether a method could not be found that should represent accurately the level above the sea of every point in the map, which, could it be done in a manner so as to strike the eye at once, would also be capable of communicating all the other particulars of information required in the perusal of a map of a mountainous country. I cannot say that I have quite satisfied myself on the subject, but an account of my own crude thoughts and unsuccessful schemes may possibly enable some one, more qualified, to strike out a better. The present communication being however a sufficient exercise of the reader's patience, I must defer, to a future opportunity, what I have to offer on this head.

D.

IV.—*On the Package of Valuable Drugs, and especially of Opium.*

The importance of the subject on which I am about to offer some details, partly with the view of attracting attention to it, and partly with the view of doing justice to the ingenious views and able suggestions of a very meritorious individual, is so obvious, as scarcely to require much illustration. The monopoly of this drug supplies a very considerable item in the Honorable Company's revenue, and there is little doubt but that any really important improvement in the management of it will not only receive every attention, but earn for the author something more solid than mere approbation. The late Mr. Fleming, who introduced the great, yet obvious improvement of packing the drug in its own leaves (petals) instead of those of tobacco, which had till then been used, received from the Honorable Court, in testimony of the advantages reaped from his suggestion, the sum of 50,000 Rupees. Nor will this sum be judged too great by any one who will consider the improvement in price which was effected by so simple an arrangement, which yet had escaped the sagacity of all, till then, connected with the Opium Department. The trade is now of far greater importance than it was then, and were it possible (as indeed few, capable of judging, doubt) to make the whole of the trade pass through the Government agencies, the improvements proposed would be of still greater consequence. Fifteen thousand chests have been sold in China alone, to which is to be added the demands of India and of Europe. Even the above quantity, at but 1000 Rupees a chest, amounts to 150 lacks, or 1½ crore. It is to be considered too, that every means of improving the drug or its appearance in the market, must render more feasible the scheme of drawing the whole of the opium into the hands of Government. By producing a superior article in larger quantity, they can at any time ruin the smugglers; and as they can afford to sell it cheaper than it can be had any where out of their provinces, it is obvious that they could, in a few seasons, make the opium of the Company's provinces supersede every other.

In the improvement of the drug, or rather of its commercial value, a very important consideration is the mode of package. To enable your readers to understand the subject, I shall first give some account of the present mode of packing it, and of the objections to which it is liable. I shall show, that besides the enormous expense, many other evils spring from it: and that to obviate these, by producing a package that shall not be liable to them, would be to perform a very considerable service to the opium trade.

The opium is made up into solid balls of 1 *sr.* 10 *ch.* and each ball packed in a case made of poppy leaves and PASTE. This paste is nothing more or less than opium, and the quantity used is said to be one eighth of the contents of the ball. This amounts, on the whole produce, taken as above, at 150 lacks, to 19 lacks per annum. The saving of this sum alone would be no inconsiderable advantage to any method of packing which should dispense with the paste.

But there is another objection to this leaf-package, which is perhaps even more important:—it is this. The balls so prepared are packed in boxes, so that there is necessarily much air in the interstices of the balls, and the consequence of this is, that chests of opium, so packed, are liable, after a time, particularly if exposed to heat, to deteriorate: the cause of this deterioration I am not aware has ever been pointed out, though the fact of old opium being much inferior to new is generally known. Some suppose that a kind of fermentation takes place amongst the balls, assisted by the leaves, which we know will, if exposed to air, moisture, and heat, rapidly undergo change. The result being the increase of bulk, by the disengagement of gas, the covers are burst; and thus not only the very object of the package defeated, but fresh power given to the deteriorating process, whatever that may be[1]. The reader may judge of the value of this objection when he is told, that opium of one season can not be sold in another, without a fall in the price in the China market; and in the London market, old opium is almost unsaleable.

It may be said, perhaps, in answer to the first objection, that the paste of the shell is not lost, but contributes to the price of the ball, agreeably to the conjecture of some, that in preparing their smokeable extract the Chinese use the shell as well as its contents. This is a point not clearly ascertained, and, therefore, no weight can be laid upon such an answer to the objection. It is quite clear, that if they reject the

[1] This sometimes proceeds to such an extent, that on opening the chest in China, the contents are found to be one mass of leaves and opium, in which every trace of distinct balls has disappeared. In one particular instance, the provision of 1819-20, we know of 97 defective balls having been taken out of 4 chests (160 balls) *in Calcutta.*

shell, one eighth of the whole provision is lost, and adds nothing to the price. On the other hand, supposing it to be used in preparing their extract, it will be equally available, if added to the contents of the ball, while the mixture of the leaves (in the case of the leaf shell) must tend greatly to detract from the high flavour and other qualities of the extract which they prepare. It is known that the petals of the poppy (by the opium dealers termed *leaves*) furnish an extract which has no narcotic properties, or if any, in a very slight degree; but, on the contrary, irritate considerably, producing pains in the stomach and bowels. Supposing, then, the shell to be used at present, it is evident that it would conduce to the improvement of the drug and the raising its character in the market to adopt some other package; while if it be not used, such a measure would at once occasion an increase of nearly 18 lacks of Rupees in the produce of the two agencies of Patna and Benares.

This little detail will at least show the importance of the subject, and enable the reader the better to appreciate the merit of the following suggestions. These plans have been proposed for the purpose of obviating the above objections, and superseding the present mode of package, both in India and Europe. Of these, the first is, perhaps, more particularly adapted to the China market, in which a drug of less consistence is preferred than what is sent to Europe. As the importance, too, of this part of the opium trade far exceeds the other, I shall begin by examining the advantages which the adoption of this plan promises.

It is proposed that the leaves shall be altogether rejected and cloth substituted, the opium being still made up in balls as at present, and the paste either added to the contents of the present balls, or reserved for making others, as may appear most expedient. The cloth may have consistence given to it by employing several folds, and using an adhesive paste, which may be made of various materials. Should it appear that the extract of the petals is thought essential to the opium used in China, it would itself be convertible to this purpose, and I have seen specimens of balls prepared in this way. This would afford all the contents of the present package without any of the objections. The great superiority of the new mode of package is most obvious on inspection. It forms a hard and impervious shell, which promises to afford a much better defence than the old one, which, indeed, has only to be seen to be condemned. The utmost, in fact, that can be said of it is, that it is better than the exploded arrangement of the tobacco leaves. The cloth package is free from that disposition to absorb moisture from the atmosphere, for which the leaf-shell is so remarkable: though it will sufficiently absorb the moisture from its contents, the enclosed opium, and give it out by evaporation to the atmosphere, so as to ensure sufficient solidity to the opium, and consequently be an effectual cure for that sweating and bursting of the balls which is so often a subject of complaint. This will be obvious to any one who considers that various substances have various degrees of attraction for moisture, and so powerfully hygrometric is opium (the chief material of the shell) that it has been known to absorb in 24 hours 8½ per cent. of moisture.

The package, in balls formed of cloth, cemented by means of an adhesive paste, has been suggested more under the idea that it might be objectionable to depart too much from the established and customary mode of bringing the drug to market, than as thinking this to be the best package that could be devised. But as it is evident that the new package could be introduced by degrees, and not to any extent till the preference of the Chinese was clearly manifested, this consideration appears of less moment. Something indeed of this kind has already taken place; and this very package, that I am about to describe, having been introduced to their notice, had, in a small parcel, the effect of enhancing the price of the *very same opium in every respect*, 25 per cent. When I say the very same opium I mean of course as it left the agency here; but that the drug will not keep in the leaf-shells, is a fact which has already been mentioned, and a reason for it assigned. And herein consists a not inconsiderable advantage of the new method of package, that the drug will keep for almost any period in it, and this even in the moist state in which it is sent to the China market. Let no one smile at the suggested mode of package as being so ridiculously obvious;—the most effective inventions are frequently the most simple, and after their utility has been clearly established, each person is heard expressing his wonder that he never thought of it. It is in all matter of invention, the old story of Columbus and the egg.

The method proposed is to pack the opium IN SMALL KEGS. In this way there is no possibility of the drug absorbing moisture, or of its undergoing that change or fermentation, by which the balls are burst and the value of the article in the market so much deteriorated. Nor would there be any difficulty in keeping opium not merely for one season, but for several, in such a package. Its convenience in other respects is so obvious as to require no observation. It would add increased facilities to the trade in China, as being, if necessary, so easily sunk without any risk to the article, to be fished up again when required. It is quite evident, that nothing of this kind could be done with the present chests. The great advantage it would have over the present mode of package would be the annual saving of the paste, amounting to 18 lacks of Rupees. Over the proposed method of the cloth-shells it would not have this advantage, but it would that of greater security and greater convenience. And that these advantages are none of them illusory, it has been found, as before stated, that the same opium sent in shells and in kegs, sold in China, the latter for 25 per cent. advance on the former. If to this we add the fact of the former having for the same nominal weight one-eighth of opium more than the latter, we shall see that the real difference of returns is 38 per cent. in favor of the keg-package over the present or leaf-shell package.

But the most ingenious of the three proposed improvements is that which I am now to describe; and as it is applicable to any costly drug as well as to opium, I am in hopes my description may not be without its use. It has this advantage over either of the others, that the package itself would form a very good remittance either in the Chinese or European market. It is particularly adapted too for keeping small quantities of any substance likely to deteriorate from the contact of air. In particular I should think it would be admirable for camphor, asafœtida, rhubarb, ipecacuanha, jalap, and indeed for all substances that require to be kept well closed in order to preserve their virtues.

This package is made of BEES' WAX. By means of a mould formed of two square boxes made of the required size, one fitting within the other, the space between being equal to the required thickness of the wax package, a box intermediate in size to the two wooden ones is cast by pouring melted wax between them. The inner one has three small nails driven into the bottom, on which it rests, so as to keep it at a proper distance, and both boxes are well damped previously to pouring in the wax, that it may not adhere. On removing the inner wooden box, a similar but smaller wax one is found lining the outer box, from which it is easily detached. The lid is then cast separately, and at the time of closing the package, is cemented by a hot iron. Previously to introducing the contents of the package, it is lined with plates of mica, which is a substance procurable in every bazar. This is to prevent the contact of the drug with the wax, and so fitted up, it really seems to me to form one of the least exceptionable packages for costly drugs that can be devised. Opium, in such packages, will keep for any length of time without deterioration, and, by parity of reasoning, so will any other drug. The former has been opened after two years, and found as fresh as when first gathered. When it is considered too, that this envelope has no properties calculated to impair the opium, that it is itself a valuable article, the price of which will be added to that of the drug, its great superiority over the leaf-shell will be at once acknowledged. I have seen packages of this kind prepared by the inventor, and holding 5 seers of opium, which were obviously superior to the old package. Being made of a square form, they pack conveniently, and with an equal bearing all round; whereas the balls, by being in contact only at one point on each side, the weight or momentum, when moved, of the whole chest is co-operating with any fermentation or absorption of damp to break the balls. Once broken, though but a few, the evil must increase; and indeed the wonder is that much greater losses are not sustained with this most imperfect method of putting up a valuable drug. The new method not only obviates all these objections, but offers the same facility as the keg, of sinking a cargo of opium; as it is quite evident that with such envelopes opium might remain under water for almost any period without suffering injury.

The wax packages described, were generally small, seldom capable of containing more than 2 seers, but they may be used of very considerable size, merely by strengthening them with an external wooden case. When this kind of package is required, the outer box of the mould must form the external covering of the wax; and to make the latter adhere to it, it is not damped, but only the interior part of the mould. When the latter is removed, the wax will be found to form a lining to the wooden box, rendering it quite impervious to moisture, and no doubt strengthening even the wooden box. The plates of talc being now affixed, it becomes an unexceptionable package.

AN OPIUM EATER.

V.—*On the Manufacture of the Sylhet Lime.*

The Sylhet Lime is manufactured at Chattac and Chuna Ganj, two large villages on the Surma River: the first about 16 miles in a direct line, N. W. of Sylhet, and the other about 32 miles W. N. W. of that place. Chattac is laid down in Rennell's atlas, plate No. 6, and Chuna Ganj may be readily found, being immediately below, and adjoining the village of Solagar, above and below which the lime kilns extend several miles on both banks of the river.

The stone is brought down by water during the rains from Panduah Laúr, Pharapunji, and the nearest points in the first range of hills to the northward, where it is found on the surface in large rocks and masses, and is said to be inexhaustible. The Laúr quarries produce the best stone, and in the greatest quantity.

The several quarries or tracts producing the stone are rented from the Cásiah and Páuduah Rajahs, by Mr. Terraneau, and Messrs. Inglis and Co.; those in Laúr are rented by Mr. Terraneau on the part of Government for about 140 or 150 Rs. per annum, on a lease renewable every four or five years, and a small annual present to the chiefs. The stone is broken and rolled down to the boats by the Cásiahs and hill people, and costs, when laid down at the ghaut or kilns, from 16 to 18 and 20 rupees per 1000 maunds, when it is saleable to the *Beparies* or native lime-traders, of whom there are great numbers established at and in the vicinity of the marts above-mentioned, at from 30 to 40 Rs. per 1000 maunds.

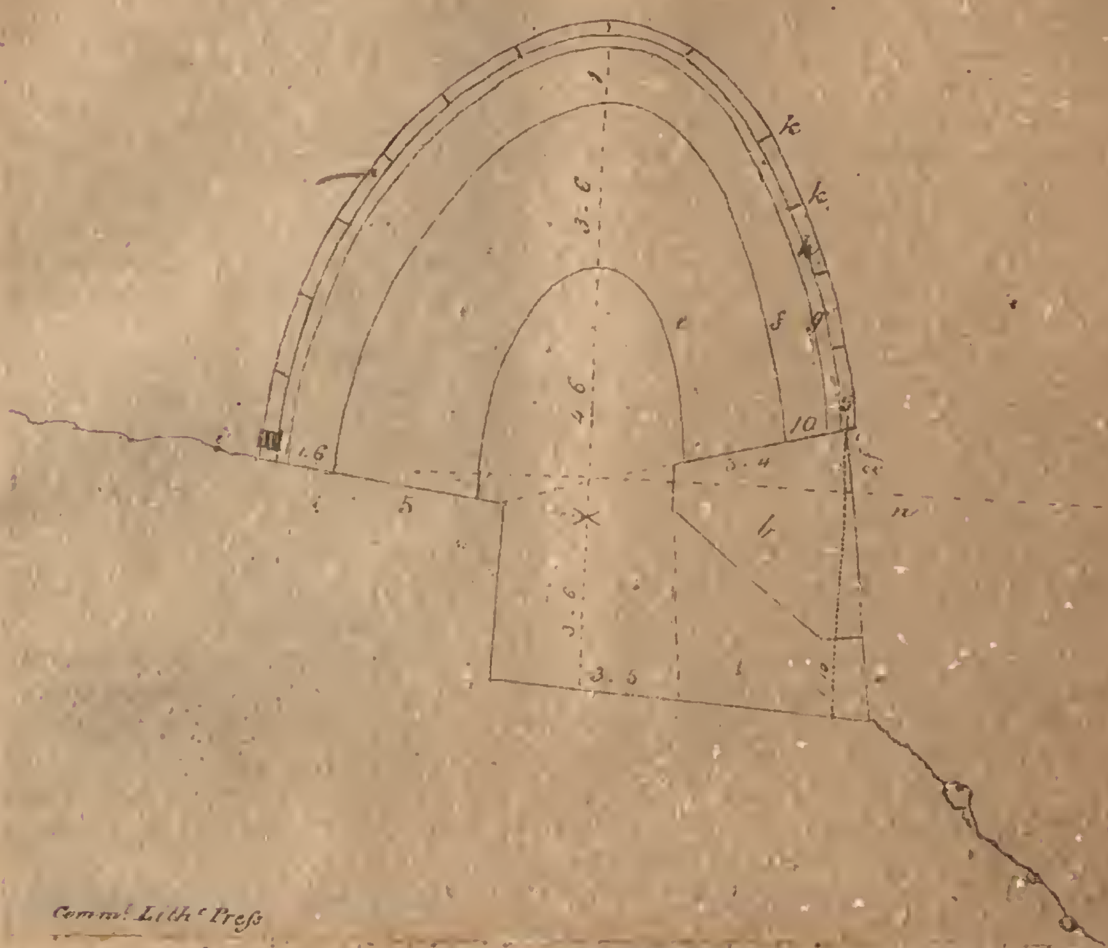
The fire-wood is procured from Bánscandi, Dúdpatli, and other parts of Cachár, east of Sylhet. It is generally contracted for with *taúdars*, or wood-merchants, and costs, in the green state, when landed at the kilns, from 35 to 40 Rupees per 1000 maunds. It is chiefly made use of at Chattac, where the *nal* or reed is not so abundant as lower down the river, the banks of which may be said to be covered with it from Azméri Ganj to some distance above Chúna Ganj. The wood, however, must not be used until perfectly dry: and as the rains in that part of the country continue nearly nine months out of twelve, or from April until October, the burning of the lime cannot commence at Chattac until the latter end of January or beginning of February; so late as January even they are frequently obliged to dry the wood over fires; whereas at Chúna Ganj, the burning commences in November or December, or as soon as the banks become sufficiently dry to allow of the kilns being constructed, and continues during the whole of the cold season, or until the end of March; during all which period the reed alone is used for fuel. But this is not found to answer so well in the rains, which, as before observed, commence in April; and should any lime, therefore, be burnt after that period, wood must be used, and the kilns are protected by slight moveable roofs.

The *nal* is very similar to the *sirkandah* of the upper provinces, and grows to the height of from 12 to 15 feet or more, and to the size of a thick rattan, or about ¾ths of an inch in diameter. It is cut down some months before it is required, and when sufficiently dry, is tied up into bundles of 2½ to 3½ feet in circumference each, and taken up to the kilns by water.

The kilns are generally placed on the edge of a bank, which is cut down in front nearly perpendicularly to the depth of about 4 or 5 feet, as shewn in the accompanying elevation, and occasionally on the level ground in rear, when space sufficient is not to be found immediately on the bank. In such case, an excavation is made 15 or 18 feet in diameter, and 4 or 5 feet deep, and the kiln erected as before, on the edge facing the prevailing wind.

The figure next page fully explains the manner in which the fire-place is constructed, and ground laid out previous to the kiln being loaded; the usual size of a kiln burnt with wood is such as to contain from 700 to 800 maunds of stone; but when the *nal* or reeds are used, the kilns seldom exceed 500 or 600 maunds, and vary from that to 400 maunds. The figure shews the dimensions of one to to contain 500 maunds, which differs little from the other, except in the thickness to which the stones are piled.

Kiln to contain 500 maunds of stone.



References to the plan.

e. The large stones, as brought down from the hills, which weigh from ½ to 1¼ maunds each, the largest being placed next to the fire place, and gradually decreasing in size towards the outside.

f. The small stones, broken on the spot to about the size of a man's fist or more, and weighing from 1 to 1½ and 2 seers each: the smallest of these are in same manner reserved for the outer course.

g. The null or reeds placed on end round the kiln to the thickness of 1½ or 2 inches, to protect the outer course of stones from the coating of tempered clay or mud *h*, which covers the whole. The reeds are of course soon consumed, but not before the clay becomes nearly half burnt, and this forms a crust which remains uninjured to the last, suspended as it were at the distance of about 1½ inch from the stones all round, by which the ventilation through the draft holes *i. k.* is greatly increased.

h. Outer coating of mud or clay well tempered and worked up with grass, which must also be carefully attended to in constructing the artificial part of the kiln marked *l.* The outer coating *h.* is carried up gradually all round, to the thickness of 4 or 5 inches, and as it dries, every crack that appears must be carefully closed by repeated applications of mud plaister in nearly a liquid state. Before it becomes quite dry, the air or draft holes *i.* are formed 4 inches wide, the same in height, and from 3 to 3½ feet apart. Those marked *k.* are pierced with a stick 2 inches in diameter, at the distance of from 12 to 15 inches one from the other, as shewn in the side elevation and section, and serve also to pass off the smoke.

Fuel, whether wood or reeds, must be used in a perfectly dry state: the former is rafted down on bamboos from the hills, in logs of from 2 to 3 and 4 feet long, and from 6 to 12 and 15 inches in diameter, which are subsequently split into two or three pieces each; the proportion allowed to a kiln is about 16 or 17 per cent. more than the weight of the stone to be burnt; thus from 800 to 820 maunds of wood are required for a kiln loaded with 700 maunds of stone. Much, however, depends on the season; and if the weather be dry and favorable, equal weights of wood and stone are found to be sufficient; in addition to which, there must be 2 or 300 bundles of reeds, or a proportionate quantity of dry brushwood, to assist in heating the kiln, keeping up the fire, particularly during the first two or three days.

The reed is brought to the kilns tied up with two or three bindings into bundles from 10 to 12 and 13 feet long each, and from 2½ to 3 feet in circumference. From 2000 to 2500 such bundles are required for a 700 maunds kiln, and from 12 to 1500 for one loaded with 500 maunds of stone; much depending, as with wood, on the state of the weather.

In favorable weather, and when wood is employed as fuel, a kiln, whether of 5 or 700 maunds, is allowed to burn four days and nights, during which time the fire must, of course, be constantly attended to and fed; when the reed is used, 24 hours more are allowed, or five days and nights; the average rate, therefore, of expenditure at a 700 maunds kiln, will be about 20 bundles per hour, or one every three minutes; the bundles are put in whole, the root end first, and gradually pushed forward to the centre of the fire-place as they consume.

The stone is considered sufficiently burnt when it glows with a white heat, and the interior and orifices of the small draft holes *k.* have become covered with a white incrustation, intermixed with small patches of the colour of sulphur. The native lime-burners appear to attach much importance to the latter sign, and I certainly observed it no where but in kilns that had been burnt and were cooling. The mouth of the fire place *m.* is now filled up with clods of earth or stones, but not so as to exclude the air altogether, at the same time the draft holes *i.* are carefully stopped with mud, and those marked *k.* loosely closed with small lumps of clay, which are gradually removed as the kiln cools. At the end of the second or third day, the outer crust or coating *h.* is stripped off, and the stones spreadout to be slacked, when they are all found to be equally well burnt throughout, slacking freely, throwing out a great deal of heat, and falling into a fine white powder. There was no appearance of vitrification to be seen at any of the kilns.

The loss of weight in burning is about 16½ per cent.[1], 1200 maunds of stone being calculated to produce 1000 maunds of pure slacked lime, the cost of which on the spot is from 16 to 18 rupees per 100 maunds.

The failure of a kiln, owing to the stone falling in, or the front *l.* giving way, is a very rare occurrence; nor did I observe more than two or three such accidents throughout Chúna Ganj and Chattac. For the first there is no remedy but to re-make the kiln, but the second is often averted by shores, or props, placed at A. *or* stone is sometimes built in across the mouth of the fire-place at *o.* and is more frequently seen at Chúna Ganj, where the *nal* is in general use; it serves to support the reeds as they are thrust forward to the fire, and regulates also the draft of air into the kiln.

T. R.

VI.—*Description of* Novaculina, *a New Genus of Fresh-water Bivalves, inhabiting the Ganges and its branches. By* W. H. Benson, *Esq. B. C. S.*

ORDER, *Conchifera dimyaria*; Division, *Crassipeda*; Family, *Solenaceæ*.

GENUS. *Novaculina*[2]. Shell subinequivalve, inequilateral, transversely elongated; ligament external, communicating with the interior of the shell by an oblique channel. Beaks prominent. Hinge-line nearly straight. Two narrow entering teeth under the beak in one valve, generally three in the other. Syphonal scar very long. Extremities of the shell gaping. Epidermis easily detached when dry, folding over the edges and extremities of the shell, and connecting the hinge-margins. Interior glossy or dull, never pearly.

Animal. Mantle with the basal-edges united, forming a tube which encloses the animal, longitudinally constricted at the suture. Foot proceeding from the anterior extremity, short, thick, cylindrical, and very muscular; enlarged at the extremity into a disk, with a convex surface, the plane of which is at right angles with the axis

[1] This is a curious fact, and deserves verification, inasmuch as it is not reconcileable with any probable value of the combining weights or *prime equivalents* (as they are more generally termed) of the bodies. Thus 1200 maunds of limestone (*if pure,*) would afford 672 maunds of quick lime; and this, if slaked or converted into hydrate, would gain in weight 216 maunds, or would become 888 instead of 1000 maunds. Possibly the above numbers may not have been accurately ascertained.—ED.

[2] Novacula, a razor.

of the foot and shell. Syphons separate, as long as the shell, when fully extended; the anal one, or that nearest the hinge, half the thickness of the other; apertures constricted, not ciliated.

Inhabits the Jumna, Gumti, and Ganges.

When I first discovered this shell, I was induced, from its lengthened form and gaping extremities, to suspect its affinity to the *Solenaceæ*, but the bad state of the deserted shells which I found, in which the sharp, delicate teeth were worn down to slight tubercles, which appeared of little importance, and the semi-internal ligament induced me to refer it, provisionally, to *Anodonta*, accompanied only by a mark of doubt. The teeth are so delicate that they are only to be found in the shell when taken with the animal, and the slightest cleaning breaks them down.

The shell is generally open as wide as the mantle and the epidermis (which is folded over the edge of the shell, and is soldered to the mantle) will permit. I placed between 40 and 50 live shells in a tub of water, with a piece of strong slaty clay, but none of them attempted to perforate it, possibly on account of its hardness, although kept for several days. The animal at times spirts a strong stream of water from the anal syphon. It inhabits cylindrical holes in clay, which it probably excavates with its powerful foot, which is always downwards. The holes descend to the depth of half a foot and more. I should not consider the shell, the extremity of which is defended by the lapping over of the epidermis, as sufficiently strong to aid in any way, except as a fulcrum for the operations of the foot. They are found by digging below the surface of the water, in the margins of banks, where they appear to have been perforated.

The worn shells are not uncommon in the beds of the Jumna and Gúmti, when the waters are retiring. They are rarely found in holes in *cancar* rock. In this case the shell is distorted, if confined in an irregular hole, to the sinuosities of which it, in a measure, conforms itself; thereby shewing that the residence was chosen accidentally, and that the abode was not formed by the animal itself. In the clay the shells are more symmetrical.

As in the *Solenaceæ*, the edges of the mantle are soldered together at the base, forming a tube which confines the animal, and gives more support to its muscular foot, the exertions of which are principally required in the direction of the axis of the shell. In its habits, *Novaculina* also resembles *Solen*, clay being merely substituted for sand, in which the latter genus delights to burrow vertically. The animal differs from *Solen*, in having its syphons free, instead of occupying a common tube; and in having an expanded, instead of a conical, termination to the foot.

To *Solen Ensis* the shell seems, at first, to have little resemblance; but it has more characters in common with *Solen Legumen*, which is less linear, and has comparatively prominent beaks situated towards the centre of the hinge,-margin. In *Solen Legumen*, also, the ligament, although external, has like our fluviatile shell, a chanel (not mentioned by Lamarck), communicating with the interior of the shell: and it appears deserving of forming a separate genus intermediate between *Solen Ensis*, and its affinities, and the genus under consideration.

Our shell is easily distinguished from the *Solens* which most nearly approach to it by its prominent beaks, its irregular form, and the great length of its syphonal scar. At times some of the teeth become obsolete, as in *Solen*; and both the cardinal and basal edges are subject to slight emargination.

Long before meeting with the live animal, I had predicted the extraordinary length of the syphons from the appearance of the syphonal scar, which, as Mr. Gray has well observed, in the Zoological Journal, is a good auxiliary character for the classification of bivalves.

Except Mr. Gray's new Chinese genus *Glauconome*, no other fresh-water shell has a long syphonal scar. The remaining *Conchæ Fluviatiles*, and the whole of the *Naiadæ* having but a slight emargination in the submarginal impression, and their ciliated syphons scarcely projecting beyond the extremities of the shells.

This shell is chiefly interesting as being the first of the family of *Solenaceæ*, or even of the *Crassipeda*, which has been ascertained to inhabit fresh water, and must be peculiarly so to the geologist, who can ill pronounce upon the nature of the medium which was inhabited by a fossil shell under investigation, until all the genera which inhabit fresh water are known. I must confess, however, that it has appeared to me, that in geology too much stress has been laid upon shells, and that the water which deposited them has often been hastily assumed as fresh, from the examination of the exuviæ found in a particular stratum, to which currents and other extraneous causes might easily have conveyed them from some vast antediluvian river.

Mr. G. B. Sowerby, to whom I forwarded dead, and therefore imperfect, specimens of the shell, in 1828, will have hardly failed to characterize the genus, as far as the specimens and remarks furnished would admit of, should he have received them safely; but as that may not have been the case, and as it is expedient, for the sake of reference, that the shell now described should not go forth unnamed, I have given it the provisional appellation of *Novaculina*,—a name, I believe, as yet unoccupied in Natural History. The only species known may appropriately be called *Gangetica*: the following is its description.

Shell oblong, with truncated extremities, white, slightly violaceous internally, epidermis olivaceous.

VII.—*Further Notice of the Produce of Land in India.*

In my last I mentioned that, considering the prices of wheat and the wages of labour in this part of the country and in England, it appeared that the value of money to the cultivating classes in this part of India was about seven times greater than its value to the same classes in England. It may be added, that the charge for interest of money to those classes is here not less on the average than 30 per cent. It varies from 24 to 40 per cent. I do not know what rate of interest these classes would pay in England.

The average purchase price of land, formed on 56 transfers by private contract, is 6, 16*s.* per acre. The highest price was 22s. 1½d.; the lowest 5,2*d.*

Seventy estates, comprising 31711 acres, contained 14203 souls—about 2¼ acres to an inhabitant, or 287 souls to a square mile.

Average prices of agricultural produce—Quantity sold for a Rupee.

	Of 10 years to 1827.	In 1829.	Mean of the foregoing.
	lbs.	lbs.	lbs.
Gúr,	64,148	80,31	72,2
Cotton, (unpicked)	33,46	33,46	33,46
Rice, (uncleaned)	141,881	174	157,943
Baira,	116,683	230,93	179,359
Wheat,	88,341	133,85	110,095
Barley,	139,204	187,39	163,297
Gram,	108,757	147,235	127,496

The following are the results of experiments made during the last *Rabbi* Harvest; which, however, was a bad season.

Division.	Number of Experiments.	Article.	Average Produce per Acre. Bushels.	Pounds.
No. 1,	44	Wheat,	9	35
	33	Barley,	11	46
	21	Gram,		473
No. 2,	239	Wheat,	11	47
	147	Barley,	15	27
	147	Gram,		770
No. 3,	237	Wheat,	12	10
	143	Barley,	15	
	142	Gram,		731
No. 4,	27	Wheat,	22	6
	15	Barley,	27	35
	5	Gram,		1504
No. 5,	9	Wheat,	13	38
	2	Gram,		824
No. 6,	15	Wheat,	11	12
	1	Gram,		843
No. 7,	41	Wheat,	19	51
	11	Barley,	30	44
	8	Gram,		1006

A mistake occurred in my writing or in printing the former notice; the proportion taken as rent should have been two-fifths, and not two-thirds, which could not be paid without actual starvation.

January, 1830.

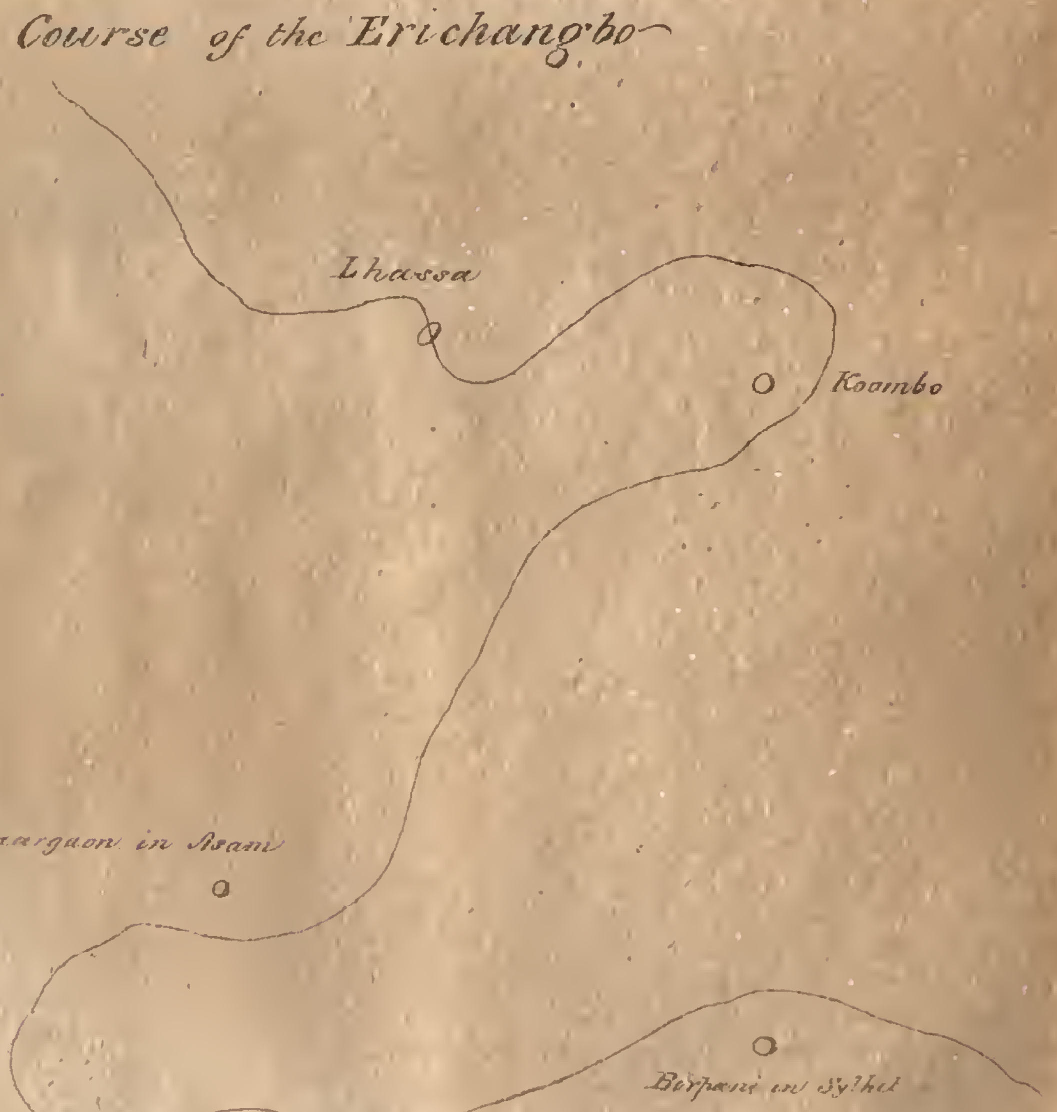
H. S. B.

VIII.—*On the Identity of the Sanpu and Irawadi Rivers.*

In our first number we gave a short notice of a controversy which has originated on the subject of the identity of these rivers between our Calcutta geographers and M. J. Klaproth, editor of the *Journal Asiatique* of Paris. In that notice we hinted at the contemplated publication of some details on the subject, which bid fair to satisfy M. Klaproth's doubts, and convince him that the Sanpú is not the Irawadi but the Brahmaputra. The details in question form part of a paper on the Geography of Assam, by Lieut. R. Wilcox, lately employed there as Surveyor, and of whose researches and joint travels, with the late Lieut. Burlton, there appeared some very interesting accounts in the Oriental Quarterly Magazine. The paper in question, which was presented to the Asiatic Society, is now, we believe, in course of publication, and will, doubtless, ere long, put the finishing stroke to this controversy, and add another to the many proofs we have of the sagacity of the father of our Indian Geography, Major Rennell, whose very guesses appear better founded than the laboured erudition of other men.

In the mean time we have much pleasure in laying before our readers the following note and enclosure, for which we acknowledge ourselves much indebted to a gentleman whose contributions to the various branches of natural science have been so often recorded in our work.

My dear Sir,—I got the enclosed an hour ago from a fine young Chinese priest, who for the last 10 years has been visiting all the holy places of his religion. I gave him no hint as to what I wanted, but merely asked his route from Lhassa to Bengal; and when he named the Eri-changbo, or river of Tibet, I desired to know what its course was from Lhassa to Assam. He asked a pencil, and instantly drew the sketch I send you. He was not learned, but seemed ingenuous and candid, and I am inclined to think his sketch not bad evidence of the course of the river at the point controverted.



Sketched by Kho-shang Lama, of the monastery of Oni-on-si, in the town of Thin-ta-phoo, 20 days W. of Peking.

He travelled from Lhassa to Koombo, from Koombo to Deva Dharma, from Deva Dharma to Assam, from Assam to Bengal. Says the road from Lhassa to Koombo lies occasionally on bank of river, occasionally over mountains.

IX.—*On Boring for Overflowing Springs.*

To the Editor of Gleanings in Science.

Sir,

Some doubt may perhaps be entertained of our being able to command never failing supplies of fresh water in the central parts of Bengal by means of boring, as practised now so largely in London and elsewhere in England, from the great distance of the hills, and the supposed depth that it may consequently be necessary to bore to reach the stratum of rock or diluvial clay retaining springs capable of rising to the surface, or nearly so. But there could be no doubt, I imagine, that on the confines of the valley of Bengal and Behar many spots would afford abundant springs of fresh water capable of overflowing; and I have pleasure in being able to send you the accompanying extracts from unpublished papers of Dr. Buchanan, which, if I err not, point out such situations, where the process of boring would be attended with the most successful results. As no precautions were taken to prevent the hole perforated in the bottom of the tank (mentioned in the first extract) from choking, it is not surprising that the spring appeared to have failed; it may, I conceive, be considered as quite certain, that wherever the water rises in the wells with a gush, on the clay at the bottom being tapped, unfailing springs of water may be commanded, by securing the hole with a metallic or other case, and that a necessary consequence will be that a higher level for the water will be obtained, that it will rise near the surface or even above it (if the tube be carried up), as the sources of the springs may be more or less elevated above the situation of the wells.

Throughout the greater part of Behar, Patna, and Shahabad, it seems probable from these extracts, that boring would be attended with the most successful results; and I recollect having read that the digging of wells in most parts of the Rungpore district was attended with the same sudden rise of water, as described in these extracts, the wells being finished in precisely the same manner, by tapping through the last layer of clay with a sharpened stake, and that district may, therefore, be added to the others, in which, from the facts recorded of the phenomena of the springs, little doubt can be entertained that experiments of boring, from the little depth at which the springs are met with, will, if properly conducted, completely succeed and repay the adventurers with ample supplies of water, and on a level much above that of the present wells.

C. I.

Of Springs and Wells in the Bhagalpúr District.

" Near the Ganges, in most parts the wells are deep, and their water is often hard and very indifferent, especially if found in red sand or clay.

At Gopalpúr, near Surayagarha, (Suragegurra, Rennell's atlas,) about seven years ago, a tank was dug 45 cubits deep, and no water having been found, a well was sunk 4 or 5 cubits further. A stake was then driven 2 cubits into the ground, when the water gushed out, and in about 3 hours filled the tank. It was expected that the water of this tank would have been uncommonly good; but the spring seems to have failed, as in the dry season the tank does not contain above 8 or 10 cubits of water, and that, as usually, exceedingly dirty."

Behar and Patna. " At a little distance from rivers, the water of the wells, in these districts, is in general very good, although often found in clay even of a loose black nature. It very often happens, that after digging far through clay, the people neither procure water nor come to any change of substance. In this case they expect that the water will rise with a rush (bhur); and in order to escape the inconvenience of this, a stake is driven into the bottom, and pulled up by a rope when the workmen have come from the well. This sudden gush is expected whenever the workmen have dug, somewhat below the depth at which water is usually found in the vicinity, especially when the whole substance dug through has been a clay of one kind."

Shahadabad and Arah. " In this district also, the water in wells often rises with a sudden rush (bhur); and this is here expected, either when a well has been dug to the usual depth at which water is found in the vicinity, without coming to any substance but clay, or when after passing a bed of sand without procuring water, the workmen come to clay. In both cases the water is procured by driving a stake into the bottom of the well. In the divisions of Ekwari and Karangja most of the wells are of this nature."

X.—*Miscellaneous Notices.*

1.—*Population of Madras.*

To our last contribution in Indian statistics, the population of Benares, we have now to add the following, and trust that the publication of these papers will induce some of our correspondents to favor us with a similar statement of Calcutta and other of our principal Indian cities.

Population of Madras in 1823, *from a census made by the Government.*

Black Town, and the Villages within the Jurisdiction of Madras.	No. of the Houses.	Men.	Women.	Boy's.	Girls.	Total.
Black Town, ...	14093	33789	45720	21305	19832	120646
Chindatripettah, ...	597	5207	7083	3821	3343	19454
Perya Metto, ...	897	4605	4834	3260	3088	15787
Erroongoonnam, ...	315	1699	2222	1524	1380	6825
Rosspettah, Narrainpolyam, Paramboor, Peterpettah, Vyasarvady, and Casapettah,	1940	7926	8999	7603	7193	31721
Porsavaukum, ...	611	3604	3878	2415	2405	12302
Egmore, ...	271	1138	1500	1124	725	4487
Poodoopettah, ...	169	423	628	363	342	1756
Comalasweren Covel, and Mannarsawmy Covel,	308	2710	3736	1713	1727	9886
Noongumpankum, ..	90	957	1205	560	559	3281
Priplieane, Trenetuseurumpet, Poodoopauk, Vallabagraram, Chapauk, & Narasingapooram,	3769	22550	24236	15318	14821	76928
Mylapoor Kistnapettah, Royepetah, Meersaihpet, Alvarpet, ... Tanampet China Taunampet and Paracharry, ...	2056	16819	19735	13342	12817	62713
Washerpettah, Sunjiverayenpet, Careapet, Tondiarpet Royapoora, and Paracherry,	1673	7819	21140	7644	13362	49965
	26786	109246	144916	79992	81597	415751
Houses, Varandas, Huts, of Khandans and their servants,	2826	0	0	0	0	46300
	29612	109246	144916	79992	81597	462051

2.—*Cedar of the Himmalya (Pinus Deodara of Roxburgh.)*

To the Editor of Gleanings in Science.

SIR,

In the 13th vol. of the Edinburgh Philosophical Journal, p. 377, is given a short account of this tree, which is undoubtedly the king of the vegetable tribes. It is well observed there, that the wood of it is almost imperishable, whether from the effects of the weather or the depredations of insects. It is stated,—and it is partly for the sake of this piece of information that I quote,—that several plants were then (October 1825,) growing both in England and Scotland. Of these the finest specimens were in the arboretum at Hopetoun House, the seat of the Earl of Hopetoun. "They were raised in the year 1818, by Mr. Smith, his Lordship's intelligent gardener, from seeds communicated by Dr. Govan, then of Saharunpore." The largest of the specimens at Hopetoun House was then (1825) 3 feet 9 inches high, and had produced a shoot 6 inches long. During two winters Mr. Smith kept them sheltered by a mat from the north winds.

It is further stated in this notice, that "From the observations of Lieutenant Herbert, it would appear that the ultimate limit of the Deodara extends even beyond 13000 feet above the level of the sea, which," it is added, "would make it perfectly hardy in our own country." But this must be some mistake, for in the account of his journey up the Sutluj, the only published paper I have seen by that officer, *the upper limit of the Deodar* is expressly fixed at between 11000 and 11300 feet, and the lower limit at 7000 feet. I apprehend, however, that these levels are peculiar to the above tract, and that in the southern parts of the mountains, from 6 to 8000 feet will be found its most congenial site. H.

3.—*Abuse of Calomel.*

A paper on this subject, by J. Tytler, Esq. was read before the Medical and Physical Society, as noticed in the Gleanings for April, of which the following seems to be not without interest to the general reader.

"Mercury has a power of affecting the constitution in two ways. One is, that of true or common salivation, the symptoms of which are well known, and which is a salutary state tending to the removal of disease and the restoration of health: the other is directly the reverse, tending to the destruction of the constitution; and its leading symptoms are as follows:—The countenance is haggard and sallow, the pulse quick and low, the skin hot and moistened with perspiration; the bowels violently affected, the patient has eight or ten stools a day, very copious and liquid, of a black and dark green color; loss of appetite and extreme dejection; the gums and tongue are red, inflamed, sore, and sometimes ulcerated, a strong coppery taste in the mouth, fœtid breath and coated tongue; yet the secretion of saliva is not increased, the mouth is parched and dry, and the fluid it contains is a thick, viscid and frothy mucus, which it is very distressing to the patient either to swallow or to eject. As this combination of symptoms has not as yet received any name, the author calls it *false* or *spurious* salivation, or *pseudo-ptyalism;* so that true salivation affects chiefly the salivary glands, and spurious, the great intestines. The writer then proceeds to illustrate this distinction by the relation of some cases of cholera, in which, after the cessation of the original disease, the symptoms of pseudo-ptyalism came on. He then relates the case of an officer, attacked with jungle fever, for which he took a quantity of mercury, sufficient to produce true salivation; in consequence of which, the fever seemed to have left him, and farther medicine appeared almost unnecessary. During the succeeding night, unfortunately, the windows of his bed-room were left open, and in the course of the night a violent storm of wind and rain beat into his room for a considerable length of time. In consequence, in the morning every symptom of true salivation had left him, and those of pseudo-ptyalism were fully established, and the fever had returned in all its violence: no means that were tried proved of any benefit, and he died in a few days afterwards. The writer's own case is next given, in which he narrowly escaped with life by true salivation coming on, and the case of an indigo planter, who died under pseudo-ptyalism; and he then proceeds to relate the case in which he was first led to observe the distinction between true and false salivation, and the proper treatment to be observed in the latter. This case was that of a Naick in hospital, labouring under jungle fever, who had fallen into a state of pseudo-ptyalism, insomuch, that his case appeared almost desperate, and he refused to take any medicine whatever. In consequence, Mr. T. was obliged to abandon mercury, and, after trying the patient with several medicines, succeeded in coaxing him to take a little tincture of bark. This change of measures seemed attended with the happiest effects, and in a few days the man was dimissed cured. From this and other cases of a similar nature, he was led to draw the following rule for the treatment of fever; that, when true salivation should come on, attended with increased flow of saliva and diminution of fever, the use of mercury is to be prudently continued till the disease be subdued; but should spurious salivation appear, attended with increase of fever and inordinate action of the bowels, mercury is immediately to be abandoned, and recourse had to tonics, and, if necessary, to stimulants, to restore the constitution to a state of health."

4.—*Heights of Places in the Cásia Mountains.*

Determined Barometrically by Lieut. Fisher.

Bairang,	1334 *ft.*	above Pundua.
Crest of the steep ascent at which the table land commences, and where the change of climate becomes very sensible,	3471	
Hill in the road between Músmai and Chirra,	4051	
Chirra, near the Sanatarium,	4299	

The following were determined trigonometrically.

Tingye, ... 918
Mamálu, ... 4009
Mallung, ... 2434

The following observations were made to determine the elevation of Pandua, April 17th, 1827.

Hours.	A. Th.	D. Th.	Barom.	Remarks.
7 A. M.	71	70	30,394	Clear sky
10 A. M.	85	86	30,394	do.
11 A. M.	87	89½	30,436	Fleecy clouds
12	89	92	30,436	do.
1 P. M.	91	93	30,436	do.
2	90	92½	30,436	Wind fresh from South.
3	91	94	30,374	
4	92	94	30,374	
6	88	90	30,312	
7	80	80	30,300	

The barometer is of the common Englefield kind, made by Jones: its internal diameter between $\frac{1}{10}$ and $\frac{2}{10}$ inch, and its results require to be corrected by $+\frac{1}{57}$ as a compensation for the want of a guage point. I have no reason to think that any air was included at the time the above observations were made. This correction was made in computing the altitudes now given.

5.—*Desiderata in Indian Geology.*

The following is an extract from a letter addressed by a gentleman at home, of high scientific attainments, to a correspondent in this country.

At the Warapilly Ghaut on the Kistna, on the road from Hydarabad to Nellore, the river seems to have forced its present passage out of the bed of a great lake, or rather perhaps the breach may have taken place at Ibrahím pattan, or Basarrah some miles further down; but at Warapilly the ground on the north side of the river rises abruptly, and on the south side very gradually; so that, according to my recollection, you have in half a coss, on the one side, the cropping out, or edges of the strata of deposition cut through by the river, while on the other side you travel many coss before you come to what must have been the boundary of the lake. A succession of specimens of these deposits from the different sides marked with notes of their relative height above the present bed, and their horizontal distance, would be interesting, as their hardness and composition appear to differ according to the pressure they have been subjected to. Some immense loose blocks like pure white marble are found lying on the south side. Specimens from the face of the hill at Bezwarrah, where it has been cut down by the river, might also be interesting.

I find among some memoranda the following sketch of a hill I saw in passing, when I had not an opportunity of getting at it to examine it. Perhaps if any of your

correspondents be in that neighbourhood they may give you an account of it. "A remarkable hill surrounded by a square rock as seen from the road 2 coss S. E. from Narrainpoor (north side of the Kistnah), the hill apparently 3 coss distance S., or S. by W.

6.—*Wood Engraving.*

This art has certainly been brought to a great degree of perfection in England. Our readers are doubtless most of them familiar with Bewick's masterly productions. They yield in merit, however, to the modern efforts of this branch of engraving, which are, we confess, quite surprising and delightful. What can be done in this way may be seen in the illustrations of that interesting work, *The Tower Menagerie*, in which, we do not know whether to admire most the beauty and truth of the drawing, or the force with which these are transferred to the productions of the printing press. In Lowdon's Natural History Magazine this art has appeared to great advantage, and to particularise we need only refer to the figure p. 13, in the March number, of a man bestriding a crocodile. We may safely say that no book has more beautiful illustrations than this, and we fully agree with the Editor, in the opinion that publishers would be wise to confine themselves to this cheap and excellent mode of illustrating a subject instead of meddling with copper plate. For examples of successful illustration, look at his *Encyclopedias* of *Gardening*, of *Agriculture*, and of *Plants*, three of the cheapest and best books in the English language. In India we believe there are none capable of executing wood cuts in a style at all approaching to the English school. We wonder at this, for we think it a method of illustration so decidedly popular that a competent artist would have every prospect of receiving encouragement. Subjects too for vignettes and for illustrations in natural history are, in India, inexhaustible. Our clumsy method of supplying the want of wood cuts in the Gleanings is sufficiently known to our readers. All we can say in its favor is, that it is the only one practicable here.

XI.—*Proceedings of Societies.*

1. Asiatic Society.

Wednesday, the 6th January.

Sir Charles Grey, President, in the Chair.

Major Walpole was elected a Member of the Society.

The Meeting then proceeded to the annual ballot for Vice-Presidents and Committee of Papers, when those for the preceding year were re-elected.

A letter was read from Mr. Huttmann, Acting Secretary to the Royal Asiatic Society, enclosing copy of an unanimous resolution on the part of the Royal Asiatic Society, authorising the Counsel of the same to invite the Asiatic Society of Bengal to unite with the R. A. S. on the same terms as have been agreed upon in relation to the Bombay Literary Society. Resolved, that as far as the 2d, 3d, and 4th articles extend, the Asiatic Society is willing to combine with the Society at home.

Read a letter from the Acting Secretary to the Royal Asiatic Society, acknowledging the receipt of the 16th volume of the Researches.

The following donations were received :

The vertebræ and cranium of a whale, presented by Mr. Swinton.

The 22d volume of the Archœologia, presented by the Society of Antiquaries of London.

The 46th volume of the Transactions of the Society of Arts, presented by the Society.

A copy of the printed edition of the Shah-Nameh, presented by the Editor, Captain Macan.

A treatise on Hydrophobia, by Dr. Sully, presented by his son.

The Meteorological Registers of October and November, presented by Major Walpole.

There being no other business before the meeting, it adjourned.

Class of Natural History and Physics.

Wednesday, the 27th January.

Sir Edward Ryan in the Chair.

A letter was read from Captain Franklin to the President, giving the results of his late Geological Researches in Central India.

With reference to the birds (about two hundred in number,) collected and preserved by Captain Franklin, for the Society, (whose property they are,) it was agreed that they should, for the greater convenience of classification, &c., accompany Captain Franklin to England, together with all the drawings and notes respecting them: the latter, however, to be returned to the Society, should but very few of the specimens turn out to be new to Ornithologists.

Five papers on certain birds of Nepal, from Mr. Hodgson, were read, each being accompanied by an excellent coloured drawing.

2. Medical and Physical Society.

Saturday, the 2d January.

The annual election of the Vice-President and Office Bearers took place, when the following gentlemen were declared duly elected:

Vice-Presidents—H. H. Wilson, Esquire, (re-elected.)

Secretary and Treasurer—Dr. John Adam, (re-elected.)

Assistant ditto ditto—W. Twining, Esquire, (re-elected.)

Committee of Management:—P. Breton, Esq., J. Grant, Esq., Dr. Waddell, C. C. Egerton, Esq.

Committee of Papers:—P. Breton, Esq. (re-elected,) J. Grant, Esq. (re-elected,) J. Tytler, Esq., Dr. Waddell, (re-elected,) C. C. Egerton, Esq. (re-elected,) Dr. D. Stewart.

The following communications, received since last meeting, were submitted by the Secretary; viz. A case of Hernia, successfully operated upon, by Dr. A. B. Webster.

Observations on the cure of Intestinal Wounds, by Mr. T. A. Wise.

Mr. Raleigh's notes of a case of poisoning, and Mr. Spry's case of Luxation of the Femur, were then read and discussed by the meeting.

3.—Agricultural and Horticultural Society.

Tuesday, the 12th January.

Sir Edward Ryan, President, in the Chair.

The following Gentlemen were duly elected Members of the Society:—

Benjamin Harding, Esq. Calcutta, and J. L. Turner, Esq. of Colgong.

Charles Paton, Esq. Assistant Commissioner, Arracan, and Lieutenant William Martin, of the Mug Levy.

Captain J. Colvin, of Engineers.

Upon a scrutiny of lists, the following Members were declared to be elected as the Office Bearers of the Society, and Members of Committees, during the ensuing year, viz.

President—Sir Edward Ryan.

Vice-Presidents—Rev. Dr. Wm. Carey, Nathaniel Alexander, Esq. Baboo Radacanth Deb, and His Highness Nawab Soulut Jung Bahadoor.

Treasurer—John Abbott, Esq.

Secretary—C. K. Robison, Esq.

Native Secretary and Collector—Baboo Ram Comul Sen.

Foreign Secretary—Henry Piddington, Esq.

1st. Agricultural Committee.

President—Sir Edward Ryan.

Joint Secretaries—W. C. Hurry and W. Patrick, Esqrs.

Members—James Calder, Esq.; James Kyd, Esq.; Sir Robert Colquhoun, Bart.; Dr. Carey; Captain Johnston; Jas. Kyd, Esq.; Nathl. Alexander, Esq.; William Bruce, Esq.; John Abbott, Esq.; Captain Jenkins; C. K. Robison, Esq.; Henry Piddington, Esq.; Rajah Kalee Kissen Bahadoor; Baboos Radacanth Deb, Obychurn Bonnerjee, Radamadub Bonnerjee; His Highness Nawab Soulut Jung Bahadoor; Baboos Dwarkanath Tagore, Prussunah Koomar Tagore, and Ram Comul Sen.

2d. Horticultural or Garden Committee.

President—Rev. Dr. Wm. Carey.

Secretary and Superintendent of Garden at Allipore—Sir Robert Colquhoun, Bart.

Members—J. Minchin, Esq.; Jas. Kyd, Esq.; J. Master, Esq.; Nathl. Alexander, Esq.; Wm. Patrick, Esq.; C. K. Robison, Esq.; H. H. the Nawab Soulut Jung Bahadoor; Rajah Kalee Kissen Bahadoor; Baboos Radacanth Deb, Cossinath Mullick, Prussunah Koomar Tagore, Dwarkanath Tagore, Ram Comul Sen, and Sibchunder Doss.

3d. Committee of Papers and Translations.

President—Rev. Dr. Wm. Carey.

Secretary—C. K. Robison, Esq.

Members—Sir Edward Ryan; H. H. Wilson, Esq.; Capt. Jenkins; W. C. Hurry, Esq.; H. Piddington, Esq.; John Abbott, Esq.; Nathl. Alexander, Esq.; James Calder, Esq.; Sir Robt. Colquhoun, Bart.; Wm. Patrick, Esq.; C. K. Robison, Esq.; John Abbott, Esq.; Baboos Radacanth Deb, Dwarkanath Tagore, Ram Comul Sen; His Highness the Nawab Soulut Jung Bahadoor; Baboo Cossinath Mullick.

GLEANINGS

IN

SCIENCE.

No. 15.—*March,* 1830.

I.—*Biographical Sketch of the late Col.* Lambton, *Superintendent of the Trigonometrical Survey of India.*

Amongst the many useful and interesting topics which at our outset we proposed the plan of the Gleanings should embrace, none appeared to us more worthy of attention than Biography. To collect and record some traces of the career of those who, in India, have distinguished themselves, either by their scientific acquirements, their useful labours, or their curious researches, has been long a desideratum. However barren an Indian life may appear of incident, it is always interesting to learn the little which may be known of those who are remarkable amongst their fellows. And in every country the lives of men devoted to the sciences, pass in a noiseless tenour, affording little of that bustling incident or developement of character which may be thought necessary to excite the reader's curiosity, or to fix his attention. Such men have little to relate beyond facts connected with the progress of their enquiries and their victories over difficulties: but even these are far from uninteresting to the general reader; while to the student they are highly useful, both as a stimulus to improvement in showing what may be done, and as beacons to guide the way in showing how. That the biography of those who have been engaged all their lives in scientific pursuit may be made interesting even to the unscientific reader, is evident, from the success which the last volume of the *Library of Entertaining Knowledge*[1] has met with. And notwithstanding the objections that have been made to what has been called a fanciful division of knowledge into useful and entertaining, (the critic asserting all knowledge to be entertaining as well as useful,) it is still certain that the division of biography is more generally entertaining than any other branch of knowledge. As our work too has got the character of being more abstruse and less inviting than necessary, we propose to open our present number with the lighter subject of biography, though we shall be reduced to the necessity of telling a twice told tale.

Of the many names which might be selected, it would be difficult to find a more suitable one for the commencement of our task than that we have chosen: we mean rather as regards his well earned fame and general celebrity, than as attaching much importance to the very few particulars which are known concerning him. These are, indeed, unusually scanty: nor would it be possible, perhaps, to obtain the attention of the reader for them, were it not that they relate equally to the history of that great undertaking in which he spent twenty years of his life, and by which he gained for himself a rank amongst the first mathematicians of his day. We need scarcely inform our readers, that the particulars of our narrative are taken from a series of letters published in the *Madras Government Gazette*, and reprinted in the *Bengal Hurkaru.* These letters were written by one who lived on terms of intimacy with the subject of them for twenty years. The style of them, as well as many of the expressions, seem those of a foreigner not quite familiar with the idiom of the English language[2]. Instead, therefore, of reprinting them, we have recast the whole, and have introduced such remarks as

[1] Containing the life of Fergusson.

[2] If our conjecture, as to the author of these letters, be correct, he appears to have paid the debt of nature himself very soon after he had performed this last duty to his friend's memory.

seem called for by the occasion, with one or two trifling particulars, which we happen to have acquired through the medium of some private letters of the late Colonel, to which we had access through the kindness of a friend.

Col. Lambton appears to have been remarkable for great reserve even towards his friends; at least in all particulars relating to his origin or to his family. It is not known where he was born, or what was the condition of his parents, further than that his silence, perhaps, warrants us in supposing the latter to have been humble; which supposition is in some measure confirmed by a casual admission made to his friend, that much of his early savings had gone to support one of them. Even the date of his birth is not known with any certainty: the writer of the letters fixes it in 1753, on the credit of the following anecdote, which, as it throws light on his character, we shall give in his friend's words.

"Being on duty with him in the Coorg country, in the year 1803, Captain Lambton told me that, a few days before, at a dinner party at Morikera[1], the Raja of Coorg, Veer Rajender Wooriar (a well known personage) came, about desert time, with his suite, as he was wont to do, to converse with the company: when, from an odd whim, he proposed that every body present, himself not excepted, should declare their age; and to set the example, His Highness mentioned his own forthwith. The ladies who were present, met the challenge handsomely, as did every body else in the company, excepting the Philosopher, who rejected it as an instance of ridiculous curiosity. 'What would you have said (he observed to me) if I had acknowledged *Fifty!*'"

Although the place of his birth be not accurately known, his friends think he was either of Lancashire or Durham; and that if not born at Darlington, he at least resided there during his youth. As that town was the residence also of the famous mathematician Emerson, it is not improbable that he owed his initiation into mathematical studies to that eminent compiler. This is rendered more probable by his being fond of repeating anecdotes of Mr. Emerson, having relation to his singularities of disposition and person. But however this may be, his education was known to be chiefly his own work; nor was he ever heard to acknowledge himself indebted to any teacher for what he had acquired. He had the best teacher in his own steady attachment to his pursuit, and in his zeal to make himself master of it.

That his thoughts were early directed to these pursuits, appears by his having been employed in 1784, soon after his arrival in America, with the 33rd regiment, to which he belonged, as a surveyor, to measure the grants of lands passed by the Government to the new settlers. During this service he suffered, according to his own account, a severe injury in his eye. Employing a common theodolite to observe a solar eclipse, he omitted to attach any coloured glasses to the eye piece, the consequence of which was a cauterization of the retina of the left eye by the sun's image falling on it. This accident, though it did not deprive him of the sight of the eye, yet it occasioned the view by it to be distorted.

Soon after this accident Mr. Lambton's friends in Europe (and particularly the late Sir Brook Watson, commissary general of the army in North America) procured him the appointment of barrack-master of the province of New Brunswick, with a salary of £400 per annum; a provision which seemed to bury him for life in the forests of North America: In reality, however, it proved the cause of that celebrity which he subsequently acquired in Asia; for it was during his sequestration of thirteen years in that wild country, that, either from choice, or from an abundance of leisure, he applied himself to the study of the mathematics, and (to use his own words) "laid the foundation of that knowledge, which was one day to bring him to the notice of the world."

Independently of the civil appointment which he thus obtained, Mr. Lambton was suffered to retain his ensigncy in the 33d regiment; though, from want of personal attendance, he obtained no promotion; and was superseded during several years. It was at last the Honorable Lieutenant-Colonel Wellesley[2] (then Wesley) who, during his command of the regiment, recommended him for a Lieutenancy. Thus chance first gave an opportunity to that illustrious nobleman to promote Mr. Lambton's progress in life.

In the year 1795, His R. H. the Duke of York having resolved on reforming the British Army, and raising it to that degree of efficiency which has made it since unrivalled in Europe, determined to clear it from all its useless members; and ordered that all officers who held civil appointments, independently of their military commissions, should declare by which service they meant to abide. Lieutenant Lambton,

[1] The capital and principal residence of the Coorg Rajahs.
[2] The present Duke of Wellington.

on this emergency, consulted his old patron Sir Brook Watson, who, impressed with a persuasion (very common in those times in England) that to go to India, and to acquire a fortune there, were the same thing, advised him to prefer his Lieutenancy; and having sent him a letter of introduction to Lieutenant-General Sir Alured Clarke, then Commander in Chief in India, Lieutenant Lambton joined the 33d regiment at Calcutta, in the year 1797, after an absence of thirteen years from the corps.

His disappointment was great, on his arrival in Fort William, to observe the real state of affairs. A subaltern's prospects (he having no means of purchasing promotion, and, as he then thought, no interest to obtain it in any other way) appeared to him any thing but brilliant, even in India, when viewed closely. He appeared to have regretted the sacrifice he had made, and to have feared "that he had heedlessly cast off his sheet anchor," as he expressed it. A barrack-master in New Brunswick on £400 a year might well think he had made a bad choice, when he found himself snugly settled down to a subaltern's duty, and a subaltern's *pay* in the garrison of Fort William.

However, Sir Brook Watson's recommendation to Sir Alured Clarke, on which Lieutenant Lambton had placed little reliance, proved to be of more service than he had anticipated; for the appointment of Brigade Major to the King's troops under the presidency of Fort St. George having become vacant, His Excellency presented it to him. By the 33rd Regiment having at the same time been ordered to the Coast for the purpose of being employed in the war against Tipu Sultán, Lieutenant Lambton saw new prospects open to his view, and he had the advantage of coming round in the same ship with Colonel Wellesley, who having by mere accident been instrumental before in forwarding his views, was now about to give him his support on the grounds of personal knowledge and on a much more important occasion.—The Fitz-William, East Indiaman, (on board of which the head-quarters of the 33d regiment had embarked,) having struck on Saugor Sand, on coming out of the river in stormy weather, they were, with the other passengers, nearly lost on that occasion. The storm, however, having abated, and the wind veering round, the danger ceased. One may see, in such circumstances as these, on what slight accidents sometimes hang the destinies of nations. It would be curious, though useless, to speculate what might have been the present state of Europe had the wind then not veered round at the Sand-heads in Bengal.

Col. Wellesley seemed to take little notice of Brigade Major Lambton during their voyage, nor did he pay him any attention beyond that which all the officers of the regiment received from him. But on their arrival at Madras, Lambton being disappointed in the expectation he had formed of living in the Commander in Chief's family, was invited by Col. Wellesley to reside with him; a proof, notwithstanding his reserve, that he had formed a favorable opinion of the Brigade Major. This opinion was expressed some days after to an officer of the regiment, who was also addicted to mathematical studies[1]. The Colonel asked him what he thought of Lambton's attainments; to which, having replied that they were very respectable, he rejoined, that, though no judge himself on such subjects, he could easily believe him to be a proficient, by what he had observed of his acquirements in other pursuits. Yet his manner continued, to the subject of his enquiries and eulogy, so reserved, that Lambton had fully made up his mind to leave his hospitable roof. In discussing the matter with the same officer, being advised not to be too hasty, and told that he was sure the Colonel had a high opinion of him, and if an opportunity of serving him should occur, he was sure he would seize it; he answered, "I would believe it if he would do me the honor to speak to me." He allowed himself, however, to be prevailed on by his friend, and continued Colonel Wellesley's guest all the time the regiment remained at Madras; that is to say, till the opening of the Mysore campaign.

When the army under Lieutenant-General Harris, which was destined for the siege of Tipu's capital, was organized, Brigade Major Lambton found himself attached to the 1st Brigade, 2d Division, under Major-General (afterwards Sir David) Baird. During the siege he lost no opportunity of making himself useful, and particularly during the storm, where he rallied the left column, the progress of which had been checked by the enemy. The particulars of this service are to be found in Major Beatson's *Account of the Mysore Campaign*, and we think they justify the opinion, that had circumstances required or allowed of his turning his attention exclusively to military affairs, he would have become an able and a distinguished officer.

[1] The writer of the letters from which this sketch is compiled.

A curious circumstance occurred during the siege, which serves to show the practical value of even a very elementary knowledge of astronomy. On the 4th of April, 1799, General Baird received orders to proceed, during the approaching night, with part of the right division, and scour a large orchard, where it was supposed Tipú had established an advanced post. After repeatedly traversing the orchard without finding any one, the general resolved to return. It so happened, however, that from the various windings the detachment had made, the column took a direction which was not the one intended. Brigade Major Lambton, who of course accompanied General Baird as his staff, observed by the position of the pole-star, that instead of proceeding south, in which direction their camp was situated, they were marching north, that is to say, upon Tipú's whole army. He communicated his opinion to the general, with the grounds on which it rested: but the latter, who was no astronomer, answered, "He knew very well where he was going without consulting the stars." Presently the detachment fell in with one of the enemy's outposts, which was soon dispersed. The General now began to think that Lambton's opinion might be, after all, correct. A light was ordered to be struck, and on referring to a pocket compass, it was found *that the stars were right!* If we mistake not, we have heard a somewhat similar story of an officer on the Bengal side of India, who conducted a detachment across the western desert to their destination by a reference to the position of the pole-star, during a night-march, and when such a thing as a pocket or any other compass was not available. Such knowledge as this can be scarcely dignified with the name of astronomy—every peasant in England, we should suppose, knows *Charles' wain,* and could readily, from its position, point out the north; the wonder is to find men of education, and, above all, military men, ignorant of such simple matters.

It was after the successful termination of the war with Tipú, that Brigade-Major Lambton brought forward his plan of a geographical survey of part of the peninsula of India. This work subsequently became the nucleus of the Great Trigonometrical Survey of India, which, carried on for 20 years, at the expense of the Honorable Company, has spread a net-work of triangles over nearly the whole of the peninsula, and, advancing into the Bengal dependencies, has reached the 24th degree of latitude:—a work that has earned for the author of it well-merited eulogiums from the first mathematicians of the day, and obtained for him the distinction of being elected member of the two first learned Societies in the world: —A work too, which reflects almost equal credit on his employers, as steady patrons of science. The Honorable Company's Government have, in fact, in this effort for the promotion of sound geography, and the obtaining a knowledge of the dimensions and figure of the earth, thrown the regal governments of Europe into the shade; and we agree with the writer in the *Edinburgh Review,* in his notice of this very survey, "that their liberal and enlarged views cannot be too highly commended."

The first idea was, however, more circumscribed, and was confined to the throwing a series of triangles across from Madras to the opposite coast, for the purpose of determining the breadth of the peninsula in that latitude, and the fixing the latitudes and longitudes of a great many important places, the geography of which was supposed to be very erroneous. His plan being laid before Colonel Wellesley, the latter handed it up to Government, with his recommendation and support. Mr. Josiah Webb, then secretary to Government, had also a favorable opinion of the undertaking; and in consequence of the representations of these two gentlemen, the first patrons of the project, Lord Clive and his council sanctioned it, and directed Major Lambton to prepare the necessary estimates and information for the passing the special orders of Government.

The juncture was every way favorable; for it happened that at that time some superior instruments, which Lord Macartney had taken to China, under the idea that they would prove very interesting to a people who pride themselves on their knowledge of astronomy, had found their way to India, and were immediately available for the purposes of the survey. The emperor of China having taken no notice either of the instruments or of the astronomer, Dr. Dinwiddie, in whose charge they were, Lord Macartney had presented them, on his return from China, to that gentleman, who remained in India. Brigade Major Lambton, who had, when stationed in Fort William, an opportunity of examining them, had ascertained that they were fully adapted to the prosecution of his survey. They consisted of a zenith sector of 5 feet radius; a 100 feet steel chain, by Ramsden; a levelling instrument, similar to that used in General Roy's operations in England; and a chronometer, by Arnold. An altitude and azimuth circle, with a theodolite like that used by General Roy, were in addition ordered from England. In 1801 every thing requisite had arrived, and about the same time, on Captain Lambton's application, Lieutenant Warren, H. M.'s 33d Regiment, was appointed his assistant.

The instruments received from Calcutta were, however, from disuse and neglect, some of them in bad order. In particular the object glass of the zenith sector could not be moved so as to adjust the focus. No observation could be made with the instrument. Nor was there a possibility of procuring the assistance of any skilful mechanic to correct any of these defects or supply any trifling deficiencies. But Captain Lambton himself possessed considerable mechanical skill; his genius was fertile in resources; nor was he ever unprepared for these or any other difficulties. He contrived to adjust the sector so as to render it fit for taking the most delicate observations; and its performance in the course of the survey is the best proof of his success. A still more puzzling difficulty occurred, soon after the commencement of the angular observations with the great theodolite, which was near interrupting the progress of the survey for some time. In hoisting the instrument to the top of one of the lofty pagodas, which formed the stations in the Tanjore country, the guy rope having broke, the instrument was dashed against the building, and it was at first supposed irreparably injured. Notwithstanding the intricacy and complexity of the frame work of the instrument, which it is said contained 2000 screws, he determined to take it to pieces, and finding the axis uninjured, and only a small part of the limb damaged, he was able to restore it so as to obtain equally good observations with it as before the accident. In fact, without this power, for which he was remarkable, of providing for every exigency, his work must have been subject to continual interruptions. In Europe such matters are easily managed, and occasion no embarrassment to the surveyor; but it is otherwise in India. To conduct such a work as this of Captain Lambton's, it was necessary to have not only the acquirements of the mathematician, but in some degree those of the instrument maker.

Had Captain Lambton been opposed by no obstructions but these,—had his attention been only directed to obviate such difficulties, his progress must have been as satisfactory as it was rapid[1]. But there were, unfortunately, others less easy to be surmounted, and which, renewed from time to time during the whole progress of his survey, must, from their very nature, have been irksome in the extreme. We allude to the prejudice and hostility which the subject of our paper had to encounter at his outset, and even subsequently, owing to the general ignorance which prevailed, amongst influential personages, (a few excepted,) of the importance, or even nature of his operations. What we do not understand, we are naturally disposed to undervalue; and in this way may be accounted for much of the opposition he met with. It was desirable to obtain the sanction of the Court of Directors for the prosecution of the work, but so incorrect an idea had been given of the nature of it in England, that Major Rennel, who had been consulted on the subject, gave it as his opinion, " that there being already a topographical survey of Mysore, instituted under Captain Colin Mackenzie of the engineers, he saw no necessity for having two at the same time, because the same surveyor who delineated geographically (topographically) the country, might very well carry on the necessary astronomical observations for correcting the position of the principal stations." We think very highly of the father of our Indian Geography, and yield to no one in admiration of his sagacity and skill in combining heterogeneous materials, and extracting from conflicting statements results so nearly approaching to truth, as to leave little for his successors beyond the task of confirming his statements. But we assert,—and a reference to his memoir will bear us out in the assertion,—that science was not his forte; nor had he carried his acquirements in mathematical learning to the level of his cotemporaries in Europe. Colonel Wellesley might justly observe, in comparing his opinion with that set forth in Captain Lambton's prospectus, " that one or the other must be very ignorant." Fortunately, however, this prospectus had been forwarded to Dr. Maskelyne, then Astronomer Royal, through his relative Lord Clive; and he having explained to Major Rennel the real nature of the survey, the latter, very handsomely, came forward and declared to the Court, that he had been misinformed; and wrote also to Captain Lambton to urge him to prosecute his labours.

It appears that he had many other opponents beside Major Rennel;—men who, though totally ignorant of the subject, were disposed to think, in the pride of office,

[1] In 18 months a series of triangles was carried across from the Coromandel to the Malabar Coast, extending between the parallels of 12 and 14; this work included the measurement of three bases.

nothing too great for their grasp. Others again, with a short sighted and narrow economy, could find no inducement to sanction an expense for the attainment of objects falling so little within the compass of their ideas of utility. It has been made a question before now, whether men in office might not render their services more valuable to the state, if furnished with some portion of useful knowledge; if possessed of some notion, however faint, of the nature and objects of science. "It is very little known or considered how deeply government and its officers are called on for scientific knowledge, and how widely and seriously they are daily engaged in carrying on operations which depend often purely on science, often on technical knowledge, or knowledge of the arts, and on a different kind of education from that which is considered an education in business; operations also which cannot be conducted without these kinds of knowledge whence-ever it is to come[1]." To particularize, we need only mention such questions as those of the currency, the mint, the bank, extensive plans of public improvement, such as drainage of marshes, improvements in navigation, manufacture of gunpowder, &c. No one can seriously suppose that questions of this kind can be satisfactorily treated by men whose acquirements, if they have any, are purely literary, whatever may be their knowledge of official routine. To understand and judge of such subjects, knowledge, not learning is required; and it is obvious to all how much a government may go astray and become the dupes of interested or crack-brained projectors, as they may often also lose the services of useful and able men, if not competent to decide on their attainments by their own lights.

The members of the Finance Committee at Madras appear to have had great difficulty in comprehending the object of Captain Lambton's survey. The manner in which one of their leading members illustrated the opinion of the Committee, is sufficiently original to be worthy of preservation in the history of the operation. "If any traveller," he says, "wished to proceed to Seringapatam, he need only say so to his head palankeen bearer, and he vouched that he would find his way to that place without having recourse to Col. Lambton's map." A most undeniable truth! Whether however the sole use of a map, or of correct geographical knowledge, is to enable a dawk traveller to find his way to his station, appears to admit of some doubts. To us it appears that it is perhaps one of the least importance: the gentleman in question, however, thought otherwise, and accordingly decided against the Survey. This committee, as it may be supposed, plagued Captain Lambton with endless absurd questions and comments; and he having consulted his feelings rather than his judgment in some of his answers, offence was taken, and the matter reported to Lord W. Bentinck, at the time governor of Madras. His Lordship, who patronised the work, out of kindness, warned him against giving way to his feelings, in a public correspondence; but he would make no concession, and declared "that if he were to be placed, any how, under the control of persons who could not possibly understand the nature of his business, and who acted with ill-will towards him, he begged to withdraw from his undertaking." Lord Bentinck was pleased to overlook this proof of sturdiness, and even promised him his support, provided he would learn to temporise and attend to the decorum of official forms.

Such was the opposition he met with, and such the prejudices he had to overcome. But on the other hand he was happy in obtaining the patronage of men of enlarged views, and, fortunately for him, of sufficient influence, to defeat opposition and neutralise this prejudice. Besides his steady patron Colonel Wellesley, there were others who distinguished him by a uniform support, and whose support was often of the greatest use when the fate of the Survey hung trembling in the scales. The late Mr. William Petrie, formerly member of council and acting Governor of Fort Saint George, and subsequently Governor of Penang, who so justly obtained the title of *The Mæcænas of Southern India*, was a steady patron of the work, and gave it his support, not only officially but privately, through the means of an extensive correspondence carried on with England. Lieutenant-Colonel John Munro, late Quarter Master General at Madras, was also enabled to perform a very important service to the cause of the survey and of science during the eventful presidency of Sir G. Barlow. Having heard that the Government contemplated the abolition of the Survey, he waited on the Governor for the purpose of representing the utility of the operations in a military point of view, more especially as exhibited in the results of the Topographical Survey then carrying on, the triangles of which rested on the positions determined by Captain Lambton. This survey was the work of the Military Institution which had been established under the superintendence of Captain A. Troyer, for the purpose of teaching

[1] Westminster Review, vol. ix. p. 346.

officers of the army the method of topographical surveying,—an acquirement, we may say, absolutely necessary to military men. He had the merit of stating his case in so clear and convincing a manner, that the intention to abolish the Survey was relinquished. Captain Lambton had also a warm friend and admirer in Mr. Andrew Scott, of the Madras Civil Service,—perhaps the man in India best qualified to appreciate his labours. He was first judge of the court of appeal, but, owing to his well known attainments, was generally consulted by the Government on all such questions as had any connection with science. His favorable opinion of Captain Lambton, and the countenance he gave the Survey, were doubtless of great value in supporting it against the ill founded objections of the ignorant. To these names we may add those of Lords Minto and Hastings, from both of which noblemen he received that encouragement and support which their enlarged minds and well-known liberality entitled him to expect.

On the 10th April, 1802, the work was commenced[1] by the measurement of a base line of 40006,4 feet, near Bangalore, in 12° 54′ N. Latitude. This, it is well known, is an operation of the greatest delicacy, and requiring all the attention of the observer,—all the refinements of modern science; inasmuch as it is the *base* or ground-work of the whole operation, which, as that is, will be correct or otherwise. The first base line, measured with any thing like an approach to accuracy, that of Picard's survey in France, was measured with wooden rods, painted to protect them from the changes attributable to variation of the weather. De Lambre and Mechain again used rulers made of platina and copper, which were insensible to ordinary changes of weather, and by their indications gave the elements of the correction for temperature. General Roy, in the English survey, began by using deal rods, but found them so variable, owing to atmospheric influence, that he was induced to re-measure the base on Hounslow Heath with glass rods. The latter apparatus, though accurate, was troublesome in use, and a steel chain, jointed like a watch-chain, which was found equally accurate and much more convenient, was substituted in the continuation of the English operations. Captain Lambton also used a chain similar to that used in England, and with the same precautions. The chain was laid in coffers or long boxes, supported on stout pickets driven into the ground, and their heads dressed even by means of a telescope. At one end of the chain was the draw-post, to the head of which the hither end of the chain being fastened, could be moved a little backwards or forwards by means of a finger screw. Near the handle of the chain, and at the point where its measuring length was supposed to commence, there was a brass scale, with divisions, which was fixed to the head of another picket, distinct both from the draw-post and from those supporting the coffers. This scale could, by means of a screw, be moved backwards and forwards on the head of the post till it coincided with the mark on the chain. A similar arrangement was made at the other end, but the handle of the chain, instead of being firmly attached to the *weigh* post, as it is called, has a rope passing over a pulley; and to this rope is appended a weight of 28lbs. to keep the chain stretched. This arrangement, it is obvious, enables the measurer to move his chain backwards or forwards with the greatest nicety, and when satisfied that it is correctly placed, to keep it there perfectly steady; while by means of the registers he marks exactly the places of the two extremities of the chain. The chain is then taken forward, and the hither end being adjusted to the scale which had before marked the fore end, a new chain's length is laid off, and so on till the base be finished. Thermometers are placed in the coffers to determine the temperature of the chain; and the rate of expansion being previously determined by experiment, the necessary corrections may be made for the varying temperature of the measurement. The quantity of this correction had been found by Colonels Williams and Mudge, to be on 100 feet ,0075 inch for every 1° of Fahrenheit; but Captain Lambton, by some experiment, performed with the chain itself, in October 1800, found ,00725[2], which quantity he applied as the correction of his measurement. Small as the above difference may appear to be, it would yet occasion, on a base of seven miles, a dif-

[1] While waiting for the arrival of the instruments from England, Captain Lambton had, in the latter end of October, measured a base near Bangalore in 12° 54′ N. Latitude, partly to serve as a base of verification,—partly for continuing the series to the Malabar Coast. The ground was such as to occasion many breaks in the line, and though carefully measured, it was afterwards deemed expedient, on finding that a line of equal length could be had without any impediment, to reject it altogether and to measure a new base, which was done in 1804. The real commencement of the Survey was, therefore, as above stated.

[2] As. Res. vol. viii.

ference of nearly two feet, supposing the change of temperature to be 20°, which in India it might well be. And this may serve to give some idea of the nature of these operations and of the extreme nicety required.

Besides the chain originally belonging to Dr. Dinwiddie, a second was obtained from England, exactly similar. Its length had been fixed in the temperature of 50°. This chain was preserved as the standard, and to its indications were reduced all the measurements made with the other. The length of this standard chain was afterwards corrected for a trifling discrepancy detected by Captain Kater, when comparing standards at home, previously to establishing a uniform system of weights and measures.

The next operation was to establish the triangles, and make the angular measurements. These, in the older surveys, had been effected by the employment of a quadrant furnished with two telescopes, which by means of a proper mounting and stand, could be placed in the plane of the objects. But the quadrant has long given way, both in this particular work and in astronomical observations, to the full circle, which is free from several errors inherent in the former. The introduction of the circle naturally led the way to Mayer's capital improvement, by the application of the repeating property; and Borda having devised a very convenient form of instrument, it was used by De Lambre and Mechain in their survey. The continental observers appear to have rated this instrument too high; nor do we wonder at it, when we consider how satisfactory in theory the principle of their construction is, and how independent it renders them of bad division. General Roy again, who thought he might depend upon the skill of our artists to divide the limb of the instrument correctly, preferred one of a different construction. This was the great theodolite, a plate of which may be seen in Adam's Graphical Essays.

The theodolite has its principal circle in the plane of the horizon, and its telescope, like that of a transit instrument, fitted for describing verticals. The angles are, therefore, taken at once as fitted for calculation, and do not require reduction, as those observed with the French repeating circle, being in the plane of the objects and inclined to the horizon. This is certainly a great advantage. Another advantage which the English observers secured, was that of placing the centre of the angular instrument, exactly over or under the centre of the signal observed from other stations—whereas in the French survey, it was almost always placed on one side, and in consequence the observed angles required a second reduction to bring them to what they would have been if observed at the centre of the station. Captain Lambton, using a theodolite similar to that of the English survey, was generally guided in all respects by the example of the conductors of it.

In May 1804, a base of verification of 39793,7 feet was measured by Lieutenant Warren, Captain Lambton's assistant, near Bangalore; and though the distance was 160 miles nearly, the computed and measured lengths of this base differed only 3,7 inches, or about half an inch in a mile: a proof of the great care and accuracy with which the work had been conducted. The series of triangles were continued across to the other coast, along a belt of country extending 2° in latitude, detecting several errors in the positions of very principal places, and in the breadth of the peninsula one of about 40 miles.

In the 10th volume Asiatic Researches will be found a list of the positions determined by this series of triangles, amounting to 256. It would lead us too far into intricate and technical discussions to explain his methods of calculation; suffice it to say, they were the same as those employed in the English survey, and included the consideration of the figure of the earth. But as some doubt existed with regard to this point, it was necessarily determined from data furnished by the Survey itself. Along with the general series of triangles, therefore, which spread over the country in every direction, for the purpose of fixing geographical positions, there was a meridional series, arranged and measured with every attention to accuracy; and at certain points along this line, zenith distances were observed, for the purpose of determining the value of the celestial arc in degrees and minutes, while the triangles furnished the length in feet. In 1801 and 1802, a series had been established on a meridian about thirty-five miles west of Madras, and the arc which was 1° 34′ 56″ gave 60494 fathoms for the length of the degree, in lat 12° 32′ and 61059 for that of the perpendicular to the meridian. But this series was afterwards rejected, and the meridian of Dodagontah chosen. The series on this meridian was begun in 1805, at Paugur and Yerracondah, depending on the Bangalore base. In 1806 it was carried down to the Coimbetoor, where a base of verification was measured. From this series the length of the degree of the meridian in latitude 12° 55′ was found to be 60498 fathoms, and that of the perpendicular

60748, the latter differing considerably from the former result. This series was afterwards continued to the southward as far as Punnae, near Cape Comorin; a base of verification of 30507,5 feet, being measured near Tinevelly, in February 1809. The arc now extended from Punnae to Dodagontah, having an amplitude of 4° 50′. This arc gave the value of the meridional degree, in latitude 10° 34′ as 60496 fathoms. But observations having been made also at Paugur and Bonasundrum, north of Dodagontah, the length seemed to be 60462 and 60469, for latitudes 11° 4′ and 11° 8′. This difference led to the suspicion of derangement of the plummet by some secret influence at Dodagontah, and, the meridional arc being in 1811 continued north from Paugur to Gootee, where a new base was measured; the length was now 6° 56′, and the degree for the middle point or latitude, 11° 38′—60480 fathoms[1]. In 1815 the series was still further prolonged to Daumergidda, and a base of 30806,2 measured near Beder, for the check and verification of the operations. It had now become the largest arc ever measured in any country, having an amplitude of 9° 53′ 45″[2]. The value of the degree in latitude 13° 6′ appeared to be 60480, and the perpendicular degree is deduced by determining the ellipticity or figure of the earth, which a comparison of the measurements in India, and in England, France and Sweden, would give. This is found to be $\frac{1}{304}$, from which, as a datum with the mean dimensions of the spheroid, the lengths of the degree of latitude, of the perpendicular to the meridian, and of longitude, from 0 to 30° are calculated. These are given in the 13th volume of the Asiatic Researches, and with them a very full table of geographical positions computed from them.

It must not be supposed that from 1802 to 1815, Major Lambton was occupied solely with these meridional measurements. They were in fact but a small part of his labours. He had covered the peninsula as high as 15°, with a net-work of triangles. "The whole of the peninsula is now completed from Goa on the west, to Mausilapatam on the east, with all the interior country from Cape Comorin to the southern boundaries of the Nizam's and Marhatta's territories. In that great extent of country, every object that could be of use in geography or in facilitating the detailed surveys of the provinces, has been laid down with precision. All the great rivers sketched in in a general manner, and all the great ranges of mountains slightly depicted[3]." In fact, it is to Colonel Lambton we owe all that we know of precision in the geography of the south of India, and if the northern parts or the Bengal presidency can furnish no map of equal accuracy, it is because we have had no Lambton. Or perhaps it would be more just to say, because we have had no patrons like those which it was Colonel Lambton's good fortune to meet with; for we have the authority of the poet to say, that

Sint Mæcenates non deerunt Marones.

The published accounts of Colonel Lambton's operations conclude with the Beder base and the above arc of 9° 53′. But it is known, that in 1822 he had extended the arc to near Ellichpoor, a little north of the parallel of 24°, thus completing an arc of nearly 16°. And a base of verification of 37914, was measured near Takulkhera. The other particulars, however, of this section of the arc are not known. From his private letters it appears, that he looked forward to carrying on the work to the banks of the Jumna, which he supposed his meridian would intersect near Agra. But Col. Lambton was no longer a young man, and twenty years of such work as he had been engaged in could not have improved his constitution. In 1810 he appears to have contracted an asthmatic disorder, from which he had latterly suffered much, and which was probably aggravated by the remedies which he was obliged to have recourse to, to allay the distress and exhaustion occasioned by its attacks. He was not fated to complete the extensive plan he had sketched; for in January 1823, a severe attack of his disorder put an end to his labours and his life together. He died at Hingin Ghat, 26th January, on the road from Hydrabad to Nagpoor, whither he was proceeding at the time. His first assistant, Captain Everest, as well as the medical officer attached to the survey, (the

1 The discrepancies found in comparing consecutive degrees in all the great surveys of England, France and India, has been attributed either to irregularities of the earth's figure or to disturbances of the plummet. Were this the place to dilate on the subject, we are prepared to show that a very large share of these discrepancies is due to unavoidable errors of observation, and to nothing else.

2 The French arc, from Barcelona to Dunkirk, was only 9° 40′. It has been since prolonged to Formentera, making an extent of upwards of 12°.

3 As. Res. vol. xiii. p. 7.

late Mr. Voysey,) were absent at the time, having been detached for the prosecution of a subordinate operation, which was intended to connect the presidency of Bombay with the general series of triangles. But he had the attendance of two of the sub-assistants of the survey, Mr. De Penning and Mr. Rossenrode, whose grateful attentions must no doubt have soothed his last hours. Thus died, at an obscure village in central India, Lieutenant-Colonel William Lambton, H. M. 33d Regiment, aged 70[4]. It was suggested we believe to the Government to erect some small, but lasting monument to mark the spot where his remains had been deposited. Whether the suggestion was ever attended to, we know not. His best and most durable monument will be the history of the great work in which were spent so many years of his life.

Colonel Lambton's stature was above the common size; his complexion was fair, and his hair tending to red. His face wanted expression, and the accident mentioned p. 74. gave a cast to his eye which rendered his looks rather vulgar and unmeaning. He was never married, though his friends appear to think that his wishes some-times pointed that way latterly.

To a considerable portion of general knowledge and a respectable share of erudition, he united much simplicity of character; so much so, as to give many people a very inadequate idea of his powers of mind and knowledge of the world. Some peculiarity of manner too adhered to him, from having lived so long out of the world, and he is said in consequence to have appeared to disadvantage in mixed companies, and particularly in the company of women. But to those who could, through this singularity, discern merit, his conversation was found alike entertaining and instructive. He had strong prejudices, yet no man was more quick in discovering talent or ready in acknowledging it; and of this a remarkable instance occurred in the case of a gentleman, who having been appointed his assistant without his being consulted, was regarded with no very favorable feelings. But the gentleman having joined the survey, Colonel Lambton was not long in remarking his talent. He acknowledged to a friend "that he had been completely mistaken in his prepossessions; that he was a genius of no common stamp, and that he would certainly shine one day conspicuous amongst the scientific men of his time." A prediction that was fulfilled to the letter, the individual in question having become a member of almost every Academy in Europe; been employed on every business of national research; appointed a member of the Board of Longitude; and finally elected vice president of the Royal Society.

He was of a quick and hasty temper apparently, yet in reality most kind and considerate. His servants were affectionately attached to him, and grew old in his service; and of his public followers he counted (as he wrote to a friend in 1822) three generations in his camp. The young men attached to the survey as sub-assistants he treated with uniform kindness and with much consideration; and in return they looked up to him as to a father. No stronger testimony could be borne to his excellence, than the unaffected sorrow of these people, when his demise took place. They felt that in the master they had lost a friend.

He read the Latin, French, and Italian authors, at least those who treated on science, with the same fluency as those of his own language. He was not, however, a good classical scholar, nor had he much taste for the fine arts or even for literature. His official style was neither eloquent nor yet lucid. He expressed himself with plainness, but not always with clearness;—a fault we think we have observed in other mathematicians. His private correspondence bore marks of haste and negligence, and he seldom attempted to correct a letter. He was, we are told, a first rate mathematician, and as such was in correspondence with many very celebrated philosophers in Europe, amongst whom we may mention Messrs. De Lambre and La Place in France; Dr. Young, Captain Kater and Mr. F. Fallowes in England. He was a corresponding member of the French Institute, and a Fellow of the Royal Society of London.

[4] According to the date of his birth established by the story, p. 74. A notice of his death, given in the *Government Gazette* of that time, makes him 75.

II.—*Narrative of the Route marched by the 18th Regt. M. N. I. from Pekang-yeh, on the I'rrawadi River, to Aeng, in Aracan. By Lieut.* W. Bisset, Q. M. G. D.

The detachment of the 18th Regiment N. I., 50 pioneers, and 36 elephants, arrived at Pekaug-yeh from Yanpabú on the 13th March, after a pleasant march of eight days; and immediately commenced crossing the river, by means of canoes furnished by Moonya, the Thendywé-Wún, who was employed by the Burmese Government for the purpose of giving us safe conduct through their territories.

The elephants being sent up the bank some considerable distance, crossed a narrow channel to an island, from whence they swam over without difficulty. The 14th was employed in crossing the remainder of the commissariat and baggage, which was effected towards evening, with the loss of only three ponies and six bullocks. The breadth of the river is 1150 yards.

We marched on the afternoon of the 15th, and passing through a highly cultivated country, arrived at Sembigyún, a distance of six miles. This once extensive village is situated in a fine plain on the banks of the Chilén river, and was wantonly burnt to the ground by the Burmese army on their retreat from Melún, to prevent the inhabitants remaining and affording us any supplies or assistance: it is surrounded by gardens, affording plenty of fruit and vegetables. The country produces Indian corn, China *raggi*, *cholum* and *coulti*, together with abundance of rice. The roads in the neighbourhood are extremely good, but must be totally inundated during the rains. The best ground for encampment is on the banks of the river to the southward of the village, nearly opposite the village of Moktén.

We marched on the morning of the 16th, and arrived at Chilén Myú, distance six miles; the country remains nearly the same, and as well inhabited. The village, as well as the houses in the stockade, have suffered the same fate as Sembigyún, and from similar reason. The fort is of too great antiquity to trace with any certainty the date of its origin, in a country where early history is lost in fiction; but it is, no doubt, of great antiquity. A stockade was built in 1824, round the site of the old fort, and embraces in many places the remains of a brick wall, now fast crumbling to decay. The position is excellent, and might form a complete island at the will of the besieged, by merely cutting the *band* of the great tank, which is only 200 yards from the south gateway. The weakest point is on the north face, where the besiegers would have good cover behind a cluster of pagodas within 300 yards of the walls. We encamped on the *band* of the great tank, on the south side of the stockade; the ground is good, and produces great quantities of rice: we were informed two crops annually.

On the 17th we marched to Paungtehang, distance 9 miles, 2 furlongs, and encamped on the banks of an extensive *jhíl*, formed by the inundations of the I'rawadi, which is not more than three miles distant from this; the road, which is tolerably good, winds along the banks of the *jhíl* which we then forded—it is about 240 yards broad, and runs inland towards Patodio. Half a mile further we recrossed it, and ascended a low *ghát* to Manishatwah pagoda, from whence we descended to Cuázi, a large village, having some boilers for saltpetre, which is found in the neighbourhood: from thence to Cónzaúng the country is well cultivated. This village is on the Mú river, and contains 3000 inhabitants. The river (which is the southern boundary of the Chilén district), is fordable in the dry season, but is navigable for large boats during the monsoon. This district is accounted the largest and richest in the Burmese empire, and contains 200000 inhabitants. On the breaking out of the war it furnished 10000 soldiers, 5000 of whom went down the country with Maha Bandúla; few, however, have yet returned. We arrived that evening at Lehdin: total distance 15½ miles. This village (formerly very extensive, and which gave its name to a district containing 24 villages and 10000 inhabitants,) is now of little consequence: it is well situated, and contains some handsome pagodas. The surrounding plain is covered with rice cultivation, which appears very luxuriant.

We marched on the 18th to Hívensah, a distance of 14½ miles, and passed through a highly interesting country, studded with villages and covered with cultivation; the road is also particularly good. Yuah-seh-khung is a large village, well inhabited: between it and Pénsloh the road leads over a small artificial nullah, the water of which is brought from the Maní river, by means of embankments; it not only serves to irrigate the neighbouring country, but is also used as a canal, by which rafts of bamboos are floated down, which is no inconsiderable traffic, as that article is in great demand, all the houses being constructed of that material. Pénsloh, Chémálah, Kíngún, Shúgyun, are all neat villages: they are watered entirely from wells, which are very numerous. It was particularly interesting to observe the curiosity of the inhabitants of both sexes and all ages, who flocked from their villages to see us pass; they all squatted down at our approach, (which is their posture of respect to superiors,) and testified their feelings at the novelty of the sight, by exclamations of wonder and delight. Shúgyun is a Shán village, and contains about 3000 inhabitants.

On approaching Kevensah the country becomes more wild, and on the banks of the Maín river is a complete forest. This village is very small, and is watered by the same artificial stream already mentioned. The ground for encampment is good, though rather enclosed. In the evening we had a view of the mountains as the sun set behind them, and traced their dark and decided outline: we hailed them with joy, as about to surmount the difficulties they had long presented. We were about to decide on the probability of an object much wished for, yet much questioned; besides we were about to reap information of interest to geographers, and advantageous to the interests of our Government.

The extensive and richly cultivated plains we had just passed, producing, as they do, such quantities of paddy, is a sufficient proof of the erroneous ideas we had formed of the resources of the country, when we supposed that the Delta alone was productive in that grain, and that by gaining possession of the principal emporiums by Rangún and Bassén, we were cutting off the supplies of the enemy, who were, in fact, totally independent of the means to be derived from the districts we had conquered.

The country for four miles beyond Kivensah is very woody. At the Kēnbúm *Choki*, a small open spot, we began to ascend the lower ridges of the mountains; the road is very good, and passes over three successive ranges of no great height;—between each are small narrow vallies, where there has been formerly halting places and water; but Chinaji, Mokaín, Lotsé, and Kényuah, are now totally deserted, and the little water formerly procurable by damming up the small stream that winds through, is now dirty, stagnant, and unwholesome. The descent of the last ridge is rather abrupt, but it had every appearance of the tracks of wheel carriages, and those recent. After crossing the Maín river, a clear and beautiful stream, we halted at the romantic village of Shú-chútoh. This place is situated on the banks of the river which flows through a confined but picturesque valley, bounded by high hills, covered with vegetation. At the west entrance of the valley, the hills terminate in one of a conical form about 700 feet in height, covered at the summit with a cluster of neat temples and a magnificent gilt *kiúm* of most chaste workmanship. I ascended by a flight of 970 stone steps, covered by a canopy and carved wood work supported by pillars of teak, which rises progressively with the ascent, and has a very imposing effect. In the centre of a small quadrangular enclosure, in the Shú-Kyún, is the print of the foot of Gaúdamah, of great antiquity and supposed sanctity, and is the resort of pilgrims from all parts of the empire. A tax is levied by Government on the higher orders of pilgrims: the sum is from 20 to 50 *tickals;* these may enter the quadrangle, but the poorer visitor is only allowed to pray outside. The Pungís, who take care of the establishment, are supported by the donations of the pilgrims, such as rice, fruit, fish, clothes, beetle, &c., but they never receive money. The hill is a fine military position, commanding the entrance of the valley from the westward, on which side it is defended by a cliff, or perpendicular scarp of a rock, about 200 feet in height: on the other side it is likewise very steep.

We left Shú-chútoh on the 21st, and after crossing the Maín river and alternately mounting its banks and descending into and winding in its bed, we reached a small open spot called Háulataín, from whence we struck across a small range of hills covered with bamboo jungle; the road, however, still remains tolerably good. We then descended into a fine rich valley, through which the Maín river winds, and supplies irrigation with its waters: we crossed it about a mile from the bottom of the descent, and entered the stockaded village of Napoh-myú. This place, which gives its name to a district containing 19 villages and 10000 inhabitants, is situated on a gentle ascent in the centre of the valley. The Maín river flows round the north and east face of the stockade, which is fallen to decay. An abatis and rail-work appear, however, to have been lately added to its defence. The village is neat and clean, and the people have a particular healthy appearance. It furnished 300 men for the defence of the passes of the mountains, who have now quietly returned to their homes. It now belongs to the *jaghir* of our guide Munza, who here furnished us with five days' provisions. One mile behind the village the road passes over a succession of low ridges, and descends to a small valley, in which is situated the village of Doh, inhabited by a few families of Kyanns, and is the best cultivated spot on the eastern side of the mountains: it is watered by a beautiful clear mountain torrent, which empties itself into the Maín river.

We left Doh on the 22d; the road, for the first mile, passes over some low hills, and then enters the bed of the Maín river, which is now confined in a narrow valley, bounded on each side by high and precipitous mountains, which descend abruptly to its banks; the waters are often confined in a narrow channel, and form numerous cascades and small falls. The whole appearance of this defile is at once romantic and grand; in many places the summits of the mountains on each side seem to meet, and the lofty forest trees with which they are clothed, appear to entwine their branches across the chasm, and darken the depth beneath. The bed of the river through which the road runs, is composed of columns of basalt and blocks of trap, which have been washed down the mountain, and must consequently change their position every rainy season. We marched through these difficulties about 8 miles, when the bed of the river became totally impassable. We were accordingly obliged to ascend the face of a mountain, by a road which had recently been made with some care; it was however very steep and abrupt, but fortunately of no extent. We again entered the bed of the river, and after having crossed it thirty-one times since the morning, we determined to halt, judging that the baggage would be retarded, from the nature of the road we had passed over. We had no ground for encampment, and were obliged to pitch a few tents in the bed of the river amongst the rocks. The distance was 10 miles.

We started the next morning, the 23rd. The road to Aeng is the same as the day before. At this place, (which has been a small military post of the great range,) the Maín river divides, one branch running in a northerly direction, the other southerly, collecting their tributes from the various mountain torrents which fall from the main range. We now began to ascend, and the mountains which rose in majestic grandeur before us, presented no very inviting appearance to the traveller. The first ascent to Dabrokíng being exceedingly abrupt;—the road, though well planned, has suffered much, being cut up by numerous mountain torrents and water courses now dry. At this spot water is procurable by descending about 200 yards on the north side; the spring issues from a rocky ravine overhung by lofty trees, and surrounded with ferns, the first we had yet seen. One mile further, still ascending, is the small stockaded position of Kyúpilaú: it commands the summit of a high precipitous ridge, along which the road runs for half a mile: its breadth, from brink to brink, varies from 12 to 15 feet, and was nearly covered the whole distance by an abatis. Continuing to ascend 4 miles by a road which must have been originally tolerably good, but now in many places washed away through a succession of rainy seasons, without undergoing any repair, we arrived at the summit of the Pokung-Roma range of mountains, now the boundary of our extensive Eastern empire. The mountain we had just ascended is the highest of the range, and is called Mareng-ma-teng Tung: it is covered to its very summit with a fine forest of lofty trees, amongst which, the walnut is particularly luxuriant. For three miles further the road winds along the crest of the mountain, and arrives at the strong stockaded position of Nari-jaín, which completely commands the road, and if properly defended, might throw great obstacles to the advance of an army from the westward.

We experienced great privation here from the great scarcity and difficulty of procuring water—it was particularly distressing, as our cattle had come a long and harassing march. The only spring is about 600 feet down a ravine on the northern angle, but the access to it is so steep and abrupt as to be totally impracticable for any kind of cattle, although every attempt and exertion was used to remove the difficulty. From this elevated position we commanded a fine view of the whole range of mountains, which, as far as the eye could reach, rose in majesty around us. Their features are well defined, and the summits being covered with thick wood, retain an unbroken outline. The main ridge runs N. 20° W., whilst the ridge that falls progressively towards the sea, bear S. 70° W.

The descent to Korukrí, 6 furlongs, is exceedingly abrupt and very precipitous, but the angles and windings of the road have been chosen with such judgment as to overcome these difficulties. At this place is a small stockade commanding a narrow ridge, along which the road runs like the stockade of Kyupilaú; the position is good, and the road is completely covered with an abatis in the same manner. Here is likewise a small open spot, surrounded by a fine forest, and a spring of delicious water, which might soon be collected in some quantities. From this to Nodún the road is pretty good, and gradually ascends: from this latter place to Kanajú, it winds along the side of high conical mountains; the precipices on the outer side are very steep, and the road in some places has been washed away: this might soon be remedied, as the slope down to the inner side is not very steep, and the soil is a red loose clay. There is also a small spring on the north side, and some good forage for cattle. Descending one mile further through a deep bamboo jungle, we arrived at an open spot called Waddah, where we halted. This day's march, though only 6 miles 2 furlongs, was the most fatiguing;—first the descent of the mountain, then the various obstacles the enemy had placed in the way; such as trees felled across the road, abatis, &c. &c., detained us until 5 in the evening. The baggage did not come up until 9 or 10 o'clock, and a number of bullocks dropped down with thirst and fatigue, and, together with three elephants, were left behind and eventually abandoned in spite of every exertion that was used to bring them on. We marched next morning to Seróah on the Aeng river: the road runs along, in general, the slopes of the ridge, and is well made with occasional ascents, but the descents were more gradual: the distance is 10 miles 6 furlongs, and the joy we felt at seeing once more the clear waters of a river after two day's privation, may be easily conceived. The ground is open, and affords excellent pasturage; the jungle abounds in wild elephants, a herd of which were seen by the guides, and their traces were discernible in every direction: the wild plantain tree is in great plenty, and affords them food.

On leaving Seróah the road ascends an abrupt declivity, and then winds along a succession of low ranges, and crosses eight mountain torrents, over which remarkable well constructed bridges of teak wood had been thrown, but they are now going fast to decay, and some are in complete ruin. Two miles further we re-crossed the Aeng river, having the hills entirely to the right. The road then runs through a flat country, covered with deep jungle and numerous plantain trees. Three miles further we again crossed the river; the road is a perfect bowling green, and passes through a deep forest, abounding in wild elephants, and continues to Aeng; total distance 15 miles 3 furlongs.

This village, which from its situation, has been the emporium of commerce between Burmah and Aracan, is now nearly deserted. All its former Burmese inhabitants have fled, justly dreading the retaliation which their own despotic injustice demanded. About 20 Mug families and a few Kyanns are now its only inhabitants: it is situated on a peninsula formed by a river of the same name and some other streams. The former is navigable at high water for boats drawing from 4 to 5 feet water. Vessels of greater burden can approach within 5 or 6 miles to this place. We cut a road through a deep jungle, and embarked on the 5th of April, on board three gun boats and four flotillas, sent by Commodore Hayes, and arrived at Amherst Harbour on the morning of the 10th, when the detachment embarked, the right wing under Captain Ross, and public followers on board the transport Robarts,—the left wing and pioneers on board the Mermaid. We took our final departure from Chedubah on the 19th April, and arrived in Madras Roads on the 23rd May, 1826.

General Remarks on the origin of the Road; on the Commerce between Burmah and Aracan; and on the Customs and Manners of the Kyánns, the Inhabitants of the Mountains.

This road, which may be safely called the Simplon of the East, was begun in the year 1816, during the reign of Mendaráji Prá, who employed 560 men for that purpose under the superintendence of the Thendwé-Wún and the other chiefs through whose territories it passed, the whole plan being previously laid down by the engineers of the king. In the beginning of 1817, the road towards the summit from the westward was nearly finished, when 200 men in addition were employed, who completed the work to the eastward as far as Shú-Chútoh. In the commencement of 1818, the different *beoparis* and merchants, who carried on the commerce between the two countries, finding a facility of conveyance for their merchandize, became more adventurous and enterprising, forming regular caravans for crossing the mountains during the months of January, February, March, and April, after which the monsoon shuts out all communication. The Burmese Government, with a policy no less advantageous than well timed, instead of exacting any pecuniary tax, only stipulated that merchants should carry with them implements for working, which they should employ in widening the road in various places, in repairing it, and in cutting pathways to the different watering places that could be found. Thus in a few years, through the policy of the

Government and exertions of individuals, an easy access was gained into the conquered provinces, which used annually to be frequented by nearly 40000 people. The discovery of this important route, which we accomplished in 12 days, (distance 150 miles, 5 furlongs,) over a range of mountains hitherto totally unknown to Europeans, will tend to obviate, in the event of a future war, the many difficulties which have presented themselves on the invasion of the Burmese territories to the southward, the total distance to the capital of Burmah from Aeng being only about 270 miles, of which only 50 miles of road may be deemed bad, and even that might soon be surmounted and rendered passable for an army and light field artillery. Besides this advantage, in time of peace it may lay open a commercial intercourse between the two nations, highly beneficent to the interests of both.

Before the commencement of the present war the commerce between Aracan and Burmah was very considerable; the former importing piece goods, silks, muslins, and broad cloth, and other European and Indian manufactures, besides salt, *ugâ pi*, and beetle, their home produce. The Burmese in return exported ivory, silver, copper, palm sugar, tobacco, and oil, the produce of their upper provinces, together with wax and lackered boxes of all sizes, the manufacture of Pegu.

The original history of the Kyanns, the present undisputed possessors of these mountains, is so lost in fiction as to be little worthy of belief; and even those parts which are known to the present race, are nothing but vague traditionary legends. They relate however, that in former times the rich plains of Burmah and Pegu were peopled by their ancestors, who were governed by a race of kings, when an irruption of the Tartars from the north overthrew their dynasty. The conquerors, seizing the Kyann chiefs who disputed their authority, put them to death; using this argument, that it was incompatible with nature that two kings could reign over two separate races of people, inhabiting peaceably the same country. Thus compelled to seek independence amidst the lofty mountains of Aracan, Ohinah, and Siam, and naturally dreading all intercourse with their former invaders, a succession of years beheld them mere children of nature, and all trace of their former kings lost in oblivion. Left without any leader or chief, the interest of social intercourse pointed out the necessity of having some one to whom they could look up, and the elder of each tribe was generally chosen as the arbitrator between them in all disputes, which are settled according to established custom, they having no code of laws. A soothsayer named Pasín, who inhabited a mountain called Pújaú, at the source of the Maín river, seems to have been looked upon by all as their director in sacrifices, marriages, &c. &c. and all things that they thought required any supernatural agency. His descendants, both male and female, to any distant degree of relationship, have fulfilled the duties of his office ever since. They deliver their mandates by word of mouth, the use of letters being perfectly unknown: their chief office is in performing sacrifices for the healing of the sick, and assisting at the burial or burning of the dead.

The Kyanns acknowledge no Supreme Being; nor have they the most distant idea of the creation. They worship a tree named by them *Subri*, which produces a blackberry of which they are fond. They suppose a peculiar substance is sent from above for their worship, which is searched after and adored with superstitious awe. As soon as a thunder storm has ceased, and nature becomes calm, they repair in a body to the spot, where, from the destruction of any tree, the substance is supposed to have fallen, and commence digging for it with great care: on being found, a hog and cow are immediately sacrificed and eaten, when it is given to the Pasín, who uses it as a talisman in the cure of the sick, they possessing the most sovereign contempt for all kinds of medicine. Their ideas of right and wrong are confined to their relative care of their flocks and families. The good man is he who takes care of his father and mother, looks after his hogs and cattle, eats the most meat, and enjoys himself in drinking a liquor distilled from grain:—the bad man is the abstemious, as he is thought an unworthy wretch for not enjoying to the utmost of his power the blessings nature has bestowed. Of this latter class there are very few. Of future rewards and punishments they appear to have some vague idea—good would attend the good, they said, and evil the bad, but where and by whom inflicted they know nothing.

Their only belief of any future state is confined to transmigration, and even that very indefinite, as they have no scruple in killing any animal either for food or sacrifice.

Yehántáng, a hill from the summit of which the whole world is supposed to be seen, is looked upon with peculiar sanctity. To this place the bodies of their dead are carried: if the superiors of any tribe or village, they are burnt, and their ashes being collected in a basket of bamboo, are then interred; a small house is erected over the spot, and covering the grave is a rudely carved image of the deceased, laid horizontally, which is supposed to ward off evil spirits. If the deceased is poor, he is buried without any distinction of place, unless in the immediate vicinity of the holy mountain. Those tribes inhabiting the tract of mountains near the Maín river, carry and burn their dead at the hill of Hánlataín, which is likewise deemed sacred.

Death is not looked upon as an event to be regretted; on the contrary, on the demise of any member of a family, the whole assemble and testify their joy, in feasting, drinking, and dancing: in fact, every event through life, from their birth to their demise, a marriage, a divorce, a virgin's purity or impurity, or religious ceremonies, are all taken advantage of as pretences for indulgence in sensual pleasures; a feast being always the finale to any thing extraordinary.

Marriage is a mere tacit agreement between the parties, and is annulled by the offending party paying a fine to the other. Adultery is not looked upon as very criminal, the damages to the injured husband being an ox, a spear, and a string of cowries, he taking back if he pleases the faithless fair one, who is thus restored to her original purity. The bridegroom, having chosen his future helpmate, makes a present to her father of an ox, a spear, a pig, a sword, tabor, and a gourd of liquor; the bride is then handed over to him, and the day is spent in rejoicing and feasting; all the village, young and old, being invited.

Unlawful intercourse between the sexes is punished by the male offender paying the fine of an ox to the parents of the female delinquent: if she proves pregnant, and her seducer does not choose to marry her, he is obliged to keep the child and pay to her the forfeit of a bullock, which latter arrangement restores her to her fair fame: marriage, however, is the general result. If the daughter of a chief is seduced, three bullocks is the price of her chastity, and the same law holds good in the event of her pregnancy; only if no marriage takes place, the man is mulct in damages of three oxen. Incest is a very rare and uncommon crime, but is absolved by the offenders paying an ox to their father.

Murder is punished by the offender giving up three men as slaves to the friends of the deceased; if he be unable to do so, which is generally the case, 30 rupees (or *tickals*) a head is taken as a substitute; should he be so poor as to be incapable of doing either, he is taken as a slave himself, nor can he afterwards be ransomed. Should a murderer escape and take refuge in any village, it is immediately required to give him up, and seldom refuses; but should it do so, the friends of the murdered person, assisted by their fellow villagers, carry arms against it, and never cease until one party is completely defeated, their village burnt, and totally ransacked and plundered. Should the murderer be retaken, he does not share the fate of his defenders, but is only kept in continued slavery, his original punishment.

Theft of grain is punished by the thief paying 30 *tickals*, if the value of the article is below that sum; if above it, 60 *tickals*, and so on in proportion. If unable to pay, he is taken as a slave by the person from whom the grain was stolen, nor can he redeem his liberty.

Silver coin, which is used among them in a very small proportion, is obtained from the inhabitants of the plains in exchange for the scanty produce of the mountains, which consists in honey, bees' wax, iron ore, and smoked fish; together with a coarse cloth, manufactured from the wild cotton by the women, who take charge of all domestic concerns, and are consequently very valuable, which is a principal reason for the men prefering marriage in case of a *faux pas*, to the mulct of keeping the child or paying the ox: in fact, these forfeitures are merely nominal, as not one probably out of a hundred of these poor people could pay 30 *tickals* for theft or murder; and their slavery is merely service; nor are the crimes which have been enumerated, at all common among them. The faces of the females are rendered particularly hideous from being tattooed completely over with a blue colour, which extraordinary practice had its origin in the following curious circumstance.

On their ancestors being driven from their fertile plains to seek an asylum from oppression amongst the wild recesses of the mountains, the tyranny of the Burmese Government still followed them, and demanded an annual tribute; which they being unable to pay in specie, a certain number of young women were taken as concubines, to grace, with their mountain freshness, the harem of the king. To such an extent was this barbarity carried, that it promised shortly the extinction of the race, till at last the women, determining to sacrifice their beauty at the shrine of liberty, submitted to be tattooed and disfigured. The first presentation of these newly featured nubiles to his golden presence, had the desired effect of exciting horror and disgust; and in their place girls of a tender age were demanded. The Kyanns sent young female children, who were immediately returned as too young, and ever since no further demand has been made. Still this practice remained in force until within these last few years, since when it is merely a matter of taste; and it is a remarkable instance of the capricious nature of the latter, when we see even now, men tattooing the faces of their wives without being compelled, either by human law or mandate of religion. I have seen some girls about sixteen who had not yet arrived at the general age for this disfiguration, whose features were uncommonly fine: they were far superior to the Burmese women in figure and fairness of complexion.

Their dress is a frock coat, with long sleeves of blue cotton cloth, which reach to the knee; the breast is open as low down as the waist, and is lined with strings of cowries, or some varied coloured fringe; a small blue handkerchief binds the hair, and a basket is suspended from the back of their heads by a thong which reaches round the forehead: in this they carry their provisions or the wild cotton they gather amongst the mountains. The men wear a frock of the same colour, but much shorter, reaching to the waist; their loins are girt in a white cloth, ornamented at the borders by stripes of red; their legs are entirely bare; a pouch is suspended from the shoulder by a belt ornamented with cowries and pieces of silver and pebbles; in this their valuables are kept, but it is, more properly speaking, a tobacco pouch; they possessing more of the latter than the former. Both males and females are particularly partial to smoking: they all carry a long reed pipe, generally tipped with a piece of silver or coral. Their weapons of defence are spears, swords, and cross bows, from which they project arrows of reed or bamboo, the points of which are hardened in the fire and dipped in vegetable poison, which seldom fails inflicting instant death.

The Kyanns are nominally tributary to the Burmese, who however derive little benefit from their wild and untaught vassals, except from those who have been allowed to enter the plains and have there settled. I saw many who seemed very happy, and, to do the Burmese credit, were not at all oppressed.

To speak generally of their character, from what I have heard from those who have been in the habits of daily intercourse with them for these last 20 years, I would say that their civilization would be of much importance to us, and could be accomplished without much difficulty. Conciliation is the only means. The Kyanns amongst themselves, pleased with their natural freedom, are rather a social race: they have, from the strongest of reasons, been taught to look on strangers as enemies. The Burmese, the only people they ever knew, they have only known as their oppressors. But now within the territories and under the protection of a Government famed for its liberality, temper, and mildness, and whose policy is grounded upon the principle of moderation, the Kyanns will find protection, and, gradually gaining confidence, may become useful subjects, and worthy of our consideration.

III.—*Particulars of a Visit to the Siccim Hills, with some account of Dárjíling, a place proposed as the site of a Sanatarium or station of Health.* By *Captain* J. D. Herbert, *D. S. G.*

Favorable accounts having reached Government, of the climate of the Siccim country, and of the advantages which would attend the establishment of a Sanatarium or station of health at Dárjíling, it was suggested to me, that my personal examination of the spot might lead to a more correct appreciation of these advantages; and in particular that my knowledge of the western mountains might suggest some useful comparisons of the features or peculiarities of the new station with the old ones, so as to give a more perfect idea of them, and enable the public to determine how far the former might, under particular circumstances, be eligible as a residence for invalids.

Mr. Grant of Málda, who had first drawn attention to the subject, and who was enthusiastic in praise of the country and of the people, had determined on a second visit, and it was proposed that I should accompany him; a proposal I very gladly accepted, for besides the curiosity to see a people of whom I had heard such very interesting accounts, I had long wished to verify the identity of the geological formations within the tract in question, and our mountains to the northwest. In particular, I wished to put to the test the truth of some views noticed in my paper on the coal of the Himmalaya, published in the 15th vol. of the Asiatic Researches, and which to me appeared not only speculatively interesting, but to have some reference to public utility.

On the 6th February, pursuant to arrangement with my fellow traveller, I left Calcutta by dawk for Málda, where I arrived on the 8th, at 2 P. M., having stopped a few hours at Berhampore for refreshment. On the 9th we quitted Málda in company, and reached Dinájpúr the following morning early. Málda, it is known, is in the vicinity of the extensive ruins of the ancient city of Gaúr—ruins so extensive, that they give the country an undulating and almost hilly appearance. Of the actual remains of building, I saw few in my line of road—but the dimensions of one of the tanks, not the largest, as I afterwards understood, perfectly surprised me, and gave me a lively idea of the former magnificence of a place which is now almost a desert. A well raised causeway runs from Gaúr through Málda to Dinájpúr. On leaving Málda we entered the Parwa jungle, as it is called, the site of an ancient and still more extensive city than Gaúr. From the road scarcely a trace is visible beyond an occasional undulation in the surface, the whole being dense but not lofty jungle. A very magnificent ruin called the Edína Masjid, of the history of which I could gain no account, was visited by us. It is situated on the road side about the middle of the jungle, and is a place of pilgrimage to the superstitious Musulmáns of the surrounding districts. As Parwa was a Hindú city, it is not very obvious how the mosque came there, unless we suppose, what the discovery of some Hindú sculptures in a corner, which had been built over, appears to entitle us to do—viz. that it may have been originally a Hindu temple, and seized and converted into a mosque, by some of the fanatical emperors. This opinion is confirmed by the character of the architecture, particularly of the pillars, which are quite in the ancient Hindú style. The tomb of Sikandar Sháh, or rather the remains of it, is at no great distance.

Whatever its origin, whether Hindú or Musulmán, it is a magnificent ruin, and, in my opinion, the most worthy of attention of any I have seen in India. The style, as I have said, resembles in some degree the older Hindu buildings in the north west of India, or, perhaps still more strongly, some of the structures in Egypt. The roof is a congeries of domes, and this at first I supposed to be more characteristic of the Mahomedan school; but that the dome is a feature of Hindu architecture also is proved by the Bishenpad at Gya. It consists of two stories, the columns of each story being of different orders, both most beautiful. The members of these order exhibit admirable proportions—all the ornaments are in character, and there is a unity of effect felt in contemplating the building, that stamps it the work of a cultivated people. The whole of a side wall, which is still standing, has been covered with the most elaborate tracery, with which is occasionally mixed the usual Arabic sentences from the Koran, executed in relief. This part of the work was probably contributed by the Musulmáns. It is built of a dark gray almost black basalt, derived from the Rájmahl hills—a stone apparently admirably adapted for the most delicate ornamental work. The upper floor is formed of slabs of granite, of great thickness, supported on the pillars. The building is fast falling to decay; only four of the domes with their pillars remaining perfect—but the remains of pillars and heaps of rubbish show its extent to have been considerable. It is said to have had 700 domes, each dome surmounting a square of 20 or 30 feet. I never so much regretted want of leisure to execute some measurements and memorandum sketches of this building—so striking in its peculiarity of character as well as beauty of architecture. It is indeed well worthy of a Daniel or a Grindlay to illustrate its beauties.

At Dinajpúr we halted a day to allow of the tents and servants making progress, and on the 12th, in the afternoon, getting into our palkees, found ourselves next morning about 10 o'clock at Titalya. Titalya has been only recently abandoned as a cantonment. The buildings are still in good order, and they would be useful in the event of an establishment being formed at Dárjíling, as the cantonment would most likely be made a resting place, and new point of departure for visitors proceeding thither. Supplies from the plains too might be lodged here till an opportunity offered of conveying them into the hills. Titalya is situated in a fine open, high and dry country, on the eastern or left bank of the Mahanaddi, which is said to be navigable in the rains to within a very short distance, even for boats of 700 maunds. The place is said to have been unhealthy; yet from what cause, if the fact was so, it it is exceedingly difficult to say. Assuredly no site could promise better, as far as our examination extended. What the character of the vicinity may be, I have no information or means of judging.

We did not stop at Titalya, but pushed on to Nijántra, on the left bank of the Balásan Nuddi, a small river which, flowing by Dinájpúr, at length joins the Máhanaddee. Opposite the village is a low undulating sandstone hill, on which, during the war, the Goorkhas had established a stockade, notwithstanding the vicinity of the cantonment, where was then stationed a battalion under the late Major Latter. Here we found pitched for us the tents which my fellow traveller had sent on, and his servants waiting to receive us. A moonshee, the medium of correspondence on this frontier with the Siccim Rája, waited on us to know what assistance he could give. He hinted at the difficulties of our proposed journey, of the bad feeling of the Rája, and of the obstacles which would be thrown in the way of our obtaining porters for the carriage of our baggage, as also provisions for the people. The latter, Mr. Grant told him, he had taken care to bring with him; and the other objections he would not listen to, thinking it very unlikely that any indisposition on the part of the Rája would be manifested towards two English gentlemen travelling on a friendly mission, and by desire of that Government to whom he owed not only his country, but the means of subsistence[1]. Ample notice had been given, not only of our intended visit, but of the number of porters we should require. As the number was small, and such as there could be no difficulty in collecting, we would not allow ourselves to anticipate any disappointment. Orders were therefore given for proceeding the following day.

The hill on which the stockade was situated is of some little extent, and of an elevation amounting perhaps to 50 or 60 feet. The surface, which is undulating, is covered with grass and bush jungle, which effectually conceals the rocky strata. In some of the gullies and small water courses, however, I observed accumulations of rounded stones and gravel, which I concluded to be derived from the conglomerate beds of a sandstone similar to that which is found to flank the great mountain district to the north-west. This conjecture was strengthened by observing in a low bank where the surface had been broken, indications of sandstone strata, though the portion disclosed was too small for me to judge positively. The elevation of Nijántra above Calcutta is 336 feet. The surrounding country is almost flat, the hill above described being the only one within many miles. The thermometer descended during the night to 43°, showing the effect of radiation to be very great at this place.

On the 14th we proceeded on the elephant to Goshaínpúr, also on the eastern bank of the Balásan, the distance about eight miles. The country having a very gentle acclivity, has, to sight, the appearance of a flat, but from the barometer the ascent appears to be 50 feet. It is quite open and interspersed with villages and patches of cultivation, though much ground appears reserved as pasturage. At this place, the Rája's *zamíndár* had erected huts for us and our followers, but we found the tent a more comfortable dwelling. In the afternoon a change of weather seemed to threaten, and the clouds collecting in heavy masses, the outline of the mountains began to be faintly discernible. They appeared to have considerable elevation, even allowing for our proximity. The following morning the clouds had again dispersed, and all was haze in that direction. But they had interfered so much with the radiating process, that the thermometer was only down to 57°, being 14° above what it had fallen to at Nijántra. As a consequence, there was no dew, though at that place it had been heavy.

On the 15th we proceeded to Singamárí, four or five miles within the mouth of the Nágrí pass, which is in fact the bed of the Balásan river. The road is in the *cádir* of the Balásan, and is consequently low, and otherwise objectionable. But the higher ground appeared to afford a very eligible line of road, rising latterly more rapidly into a sort of side range, which kept the direction of the course of the river. As the road we travelled repeatedly crosses the river or some of its branches, and frequently lies in its bed, I do not think it could be kept open in the rains. But no difficulty need arise on this score, as the bank-like elevation above noticed, affords an unexceptionable line, gradually rising, till, at the entrance of the pass, it is already many hundred feet above the present road. It would however cross a branch of the Balásan (as does the present line), which goes to meet the Máhanaddee here, the other branch continuing its course, till it joins below Dinájpúr. This would require a chain bridge perhaps in the rains. At the season we crossed it, though wide, it was shallow. The bed is covered with rounded stones of every size, from 8 or 9 inches in diameter downward; though at Gosháínpúr, but 8 miles below it, it is

[1] Siccim itself is too poor to maintain half a dozen serving men. It is on the tract of plain country, at the foot of the hills, ceded by our Government, that the Rája supports himself.

quite sandy.—Rassadhúra is the name of a halting place here, on its banks in the middle of the forest. This forest is, however, by no means thick, the elephant having found no difficulty in getting through it. At Núnumatti, which is the last station in the plains, our conductor had intended us to halt, and had erected huts for our accommodation. But the place was dirty, dusty, dark and dismal, and the huts small and inconvenient. We therefore determined to proceed onward a little, and even take our chance in the river bed, though unprovided with tents, rather than be smothered with dust at the miserable place they had fixed on. We could not help making the same remark which I have often made when employed in the northern mountains—viz. the total insensibility of the natives of the plains to the charms of rural scenery, they invariably choosing the most objectionable spot to place a camp in, and passing by or stopping short of scenes of the greatest natural beauty.

On proceeding a few hundred yards we emerged from the forest and entered the bed of the river, which had now become a mountain stream : its water clear as crystal, and its course obstructed by huge round stones. The elephant made its way very slowly over these, and after proceeding with great difficulty about a mile and a half, we came to a place where the river, collecting itself in a deep pool, is surmounted on one side by a rocky ledge, over which the path lies. Here, therefore, we were obliged to descend and send the elephant back. The small ones belonging to our conductor, however, passed over this rocky defile, with as much boldness and certainty as a goat would have done. This pool, or natural basin, being nearly thirty feet in diameter, and from five to eight feet deep, offered a fine opportunity for bathing, which as the day was warm, would have been a great refreshment. But as the place had no very inviting features for passing the night in, and as Singamári, where it was known there was a comfortable house, was represented to be at no great distance, we pushed on, and in about an hour and a half we reached it. The baggage however did not come up till nightfall, and then only part of it ; so that we had at one time the rather disagreeable prospect, after our day's travel, of going dinnerless to bed, upon a hard flooring, formed of bamboo laths.

The rock above the pool was the first we had seen, and I was curious, having found sandstone so far from the entrance of the hills, to examine it. Agreeably to expectation, it proved to belong to the second zone of rocks : that found succeeding the sandstone in the mountains to the north-west : being an argillaceous gneiss, exactly similar to that constituting the upper ascent of the Ghágar on the road to Almórah. In that mountain it however attains an elevation of 5 or 6000 feet, whereas here it was not above 1000. But it is worthy of notice, that the sandstone is also deficient in elevation ; at least if we suppose the low hill at Nijántra to be the only indication of it in this quarter. Now that the latter formation had not disappeared from the effect of wasting, is evident from the total absence of debris either sandy or pebbly, the only trace of the latter being on the hill itself. May we then venture to assume, that those rocks have not risen to the level they have attained in other quarters, simply because the elevating force was less powerful or had more resistance to overcome?

Singamári, elevated 1300 feet, is on the right bank of the Balásan, which we had crossed three times ; the last time at the place. It is on one of those small flats seen in all mountain rivers at intervals, and is about 50 feet above the river bed. The valley is narrow, being in fact a mere gorge ; the breadth of the river and the mountains on each side rise to a great height, being covered with thick forest. No pines were visible ; a singular difference from the other quarter, where they are the only tree seen on first entering the hills. There is no village, or at least there were no inhabitants. We therefore took possession of the principal house, built of bamboos and thatched, and which we found a very comfortable one. It was raised about 3 feet from the ground, the flooring being formed of split bamboos. There were two rooms, the dimensions of which were much more convenient than in the houses of the plains, and quite sufficient to allow of an upright posture. And what I never saw in any native's house—a kind of sideboard or table was constructed of split bamboos in one corner—in another was a very fine raised platform intended for the bed. There were abundance of smaller huts for our people.

On the 16th we left Singamári at 9 o'clock, and proceeding in the bed of the river (which we crossed several times) for a distance of about a mile, turned to the left, and ascended by the bed of a steep torrent which here joins the Balásan. The ascent is latterly rather steep to Jamdári Ghát, elevated 1795 feet above Calcutta. From this pass the Sinchal mountain is visible, bearing N. 20 E. From the pass the path leads through a bamboo forest, occasionally ascending, occasionally descending to a stream, whence there is a small descent to Dimáli Góla; where we arrived in a heavy shower of rain. This is one of the stations where the mountaineers barter their *manjít* for the good things of the plains or for money: we had therefore a pretty full meeting to stare at and welcome us. The *jemadár*, as they call the officer or head man who is here on the part of the Rája, cleared out a house for us, and we soon found ourselves very comfortably settled.

This was my first interview with the Lepchas, and I saw immediately that they were the same people whom I had met with at Nialang, at Jahnabbi, at Shipei on the Satlej, in Hangarang, and at Lári in Ladác. They are in fact the people who have been erroneously called Chinese Tartars, and are in reality of the same race as the Thibetians, being a family of the great division of Eleuth Tartars or Calmucs. Yet the Lepchas distinguish between themselves and the Bhótiahs or Thibetians, and the languages, though resembling each other sufficiently, have yet a difference. I imagine however this distinction to be rather that of the new and old settler, and the difference of the languages to have originated in the same circumstance. There is certainly not the least difference in their appearance, or manner, or character, as far as we could see into it, or habits, or prejudices, and their religious worship is actually the same.

The peculiarity of feature that marks this race is very striking. A broad, flat face; the nose little elevated, but with expanded nostrils; the eyes small and set obliquely in the head, the inner angle being depressed; a rather large mouth, but with thin lips; and a great deficiency of beard; form the elements of a countenance, which though it cannot, according to European notions, be pronounced handsome, is yet often, from the expression of intelligence and good humour that distinguishes it, more prepossessing than the regular features of the Hindustani. Their character answers to their looks: they are cheerful, frank, full of curiosity; bold, yet not presuming in their address; and to all this is added a simplicity of manner, as well as of feeling, that must render them favourites with Europeans. Their curiosity was not to be satisfied; they crowded around us, while we were dressing, and what seemed greatly to interest them was the process of removing the beard, a part of the human face divine, which requires little trimming with them. That little is effected by plucking it out by the roots in most cases, and the few who cultivate it are not improved in their appearance, as it is so very scanty. Our telescopes attracted much attention, as did a pocket compass, and a watch; the latter being held at a distance, and a long stick touching it, brought in contact with the ear of one of them, he seemed greatly delighted, and called out to his companions that it said tick, tick, tick! using the very word that we do to express the sound.

The Lepchas are able bodied men—they are short square thick-set muscular looking figures. One of them will carry as much as two Bengális, and this without grumbling or complaint. Their legs exhibit proportions which might do honour to an Irish coalheaver or chairman. Their complexion is of a lighter tint than that of the Hindustani, or rather would be, could they be persuaded to remove the thick coat of dirt that obscures it. This leads me to notice their only fault; at least the only one we could discover in our short acquaintance with them. I mean their excessive filthiness; and this is such as to surpass belief. Notwithstanding this drawback, many of them appear remarkably fair, and exhibit considerable colour. They all, men and women, allow their hair to grow, some wearing it loose on the neck, others plaiting it into a tail which hangs down behind, and to the end of which cowries are often attached. The dress of the two sexes is precisely the same. These circumstances of the similarity of the hair and dress, added to their smooth faces and want of beard, give the men a very effeminate appearance, and several of them were constantly mistaken by us for women—the voice alone enabling us to distinguish.

The rocks in this day's journey were gneiss, apparently dipping to north-west, and lying at an inclination of 45. In the bed of the river it continued of the argillaceous type, but on Jamdárí Ghát it consisted of the ordinary ingredients, the felspar being reddish.

On the 17th we left Dimáli Góla for Sámdong. The road descends through a bamboo forest to the river's bed, in which it proceeds, crossing it by a bridge formed of a single bamboo, with another to hold by. A little beyond this, at a place called Gulgulia-muni, the river is collected in a deep and extensive pool overhung by a lofty precipice, which even towards noon keeps the spot in shade. Here appears to reign always a cool and refreshing air—too cold indeed; for heated as we were with our walk, the breeze from the pool chilled us. The precipice is of great height and steepness, and the river, which takes a bend at the place, washes its foot; the surface as even and unruffled as that of a lake. It is evidently of great depth, and is full, the people say, of fish. We could have wished to have halted here instead of at Dimálí-góla, but were obliged to make our wishes bend to circumstances. A little beyond this the Rámbong river joins the Balásan from the left. The road continues in the bed of the latter, crossing again to the right bank by a similar rude bridge to that before described, and then ascends the steep face of the mountain. This was the most fatiguing part of the stage, and occupied us 40 minutes. At Nagri-long-jók, elevated 2718 feet, the road branches off to Nagri stockade, where we had a detachment formerly from Titalya. Our path was now level for some distance, or with easy descent along the face of the mountain. We had a less confined view of the country too, than when groping in the river bed; yet we could see no villages, nor any thing in fact but thick forest, which seemed to overspread the country in every direction. Here and there a small cleared tract was visible, having a hut in the middle; but these efforts to overcome the exhuberance of nature seemed, like man himself, to bear no proportion to the vast features of these mountains. Gradually descending, we again found ourselves in the bed of the Balásan, which we crossed a third time, on a bridge exactly like the two former. After proceeding in the bed a few hundred paces, we crossed back again, and had then about a mile and a quarter of very unpleasant road, through a thick jungle of the small bamboo; the ground very uneven and wet, and covered with decayed leaves. A fifth crossing of the river was then effected, and we found ourselves, on ascending the bank, at Sámdong. This was rather a fatiguing march, having occupied us five hours.

Sámdong, though elevated 2751 feet, is scarcely superior to Dimálí-góla as a halting place. On the side of a steep mountain, and surrounded with thick jungle, there is no seeing any thing beyond a few yards, while the quantity of even ground is extremely confined. The place is rather dreary looking, I confess; and we had an unfavorable day to contemplate it, as it began to rain immediately on our arrival. But we were comfortably housed, and we pleased ourselves with the idea that we should here really start for Dárjíling, as our Lepcha porters were here to come into play, and the Bengális be discharged. Accordingly, being informed that the Rájah's *dewán*, (as they called a dirty Bhótia with a silk dress,) was waiting to pay his respects, Mr. Grant ordered him to be admitted, and we immediately entered on business. A smart little Lepcha, in a scarlet vest or cloak, something like the Spanish *poncho*, acted as interpreter, and afforded us some amusement by the pertinacity with which he prefaced every speech, however short, with the never varying declaration of *Ghulám bintee kurta, khudáwand mulk áp ka, áur áp ka hukmse*, &c. &c.; after which he would conclude perhaps by refusing to give us half a dozen porters. They began by aking us the intention of our journey, to which, instead of replying by any mystification, Mr. Grant at once declared the full aim and object of our mission. We thought it might simplify and cut short the negociation by satisfying them, that nothing was in contemplation from which they could by any possibility extract any cause for alarm. But with those accustomed to tortuous and crooked methods of arriving at their object, such openness perhaps does not advance one much. Unaccustomed to the thing themselves, they cannot understand the object of practising it; and, as is always the case with weak minds, what they cannot understand or fathom, they are sure to suspect and fear. The royal ambassador testified considerable alarm, and exhibited the only visage (to do the people justice) in which any thing sinister or disagreeable was observed. Like all diplomatists, he seemed never tired of telling lies; and assured us in the strongest terms of the respect and deference his master felt for the British Government all the time he was making a difficulty of allowing us to proceed a step farther. He wished for time, being apparently aware that *time*, like knowledge, *is power*. He declared we had hurried too much, though his lazy master had actually had nearly a month's notice of our approach. All his excuses and protests we set aside at once, and told him plainly, that if by morning we were not furnished with porters to proceed to Dárjíling, we would retrace our steps, and leave him and his Rája to explain their neglect and incivility the best way they could. Mr. Grant was very mild but firm, and as he appeared to have inspired them with favorable impressions, what he said had the more weight. After a little more delay they declared we should have as many as could be got together by morning. In a short time, nearly a dozen able bodied fellows came to examine and prepare the loads, and we were not a little pleased to find that we had some prospect of seeing Darjíling. Eight annas a stage was agreed to, as their daily hire, not including return; and taking into consideration the severity of the marches, as well as the extraordinary loads they carried, it was moderate. A few bottles of brandy were given to put them in good humour, for they are extravagantly fond of spirits. Nor was the ambasador or the interpreter forgotten, and even the worthless Raja had a royal and sufficient share assigned to him, which the people promised to forward.

On the 18th we left Sámdong, after breakfast, commencing with a pretty good ascent up a lateral ridge, where the forest seemed a little more open, and from which we could distinctly trace the several ramifications of the mountain Sinchul plainward. The road we had come had proved by no means good, but it was objectionable, still more from the nature of the country it passed through, and the impediments which occurred in every part of it. Of these the principal were the repeated fords over the Balásan, which could not be expected to be passable in the rainy season; or indeed in any season after a heavy fall of rain. Every one who has lived for any time in a mountainous country, knows the sudden impetuosity which even insignificant rills will acquire from the effects of a pretty heavy shower; and that it requires a bridge for the passage of every stream, if a road is to be kept open all the year rouud. But even with bridges this road could not be kept open, as great part of it lies in the bed of the stream, and must be under water after every heavy fall of rain. Add to which, that during great part of the year a considerable portion of this road must be decidedly unhealthy. But a very unexceptionable line of road is to be traced from the ridge above Sámdong. One of the ramifications of the mountain above-mentioned, exhibits a uniform ascent from the plains to its parent ridge, without break or valley to interfere; as far at least as we could distinguish: and with very few windings, fewer, in fact, than are found in the present road. So unexceptionable did the suggested line appear to us, that we could not help exclaiming against the apparent perverseness of the people, who will always (or at least had done so here) choose the worst possible direction in which to carry their roads; not adverting to the fact, that in all half settled countries like this, roads must pass by the villages, however circuitous and otherwise objectionable the line be, from the necessity of having shelter and supplies at each stage. We both agreed however that if Dárjíling is ever to become a place of resort, it will require some other means of access than the present; and we saw no reason to doubt, either then or afterwards, the great superiority of the line which had recommended itself to us.

The ascent from Sámdong to Tikri-bong is almost continued, and so steep as to be very fatiguing. The first part is partially cleared, with a solitary hut in one or two places, and an attempt at cultivation; but the latter half is through a thick forest, frequently over a bed of decayed leaves. Soon after we set out it began to rain, and continued more or less heavily till we reached our halting place. This latter was a spot in the forest, where water was procurable. There were no huts, but our Lepcha porters who had preceded us had erected a sort of wigwam of boughs of trees, the roof being covered with the smaller branches, which, however, did not constitute a very water-proof sort of house. But by putting up blankets in the inside we contrived to shelter ourselves from the rain, which continued to fall nearly all night, though far from heavily. A platform had been erected for our beds, on which we slept pretty comfortably, in spite of the rain and cold wind. In fact, we took the precaution of lighting a blazing fire within the hut, on the earthen flour beyond our sleeping platform, and with our heads directed to this excellent companion and the blankets over head, we passed as comfortable a night as if we had been in a palace; and this in a place which, on a first view, appeared the most wretched and unpromising I ever saw. The elevation of Tikri-bong is 5559 feet, and the thermometer had sunk the following morning to 46°.

19th. The morning was not more promising than the evening had been; but as there was no inducement to remain where we were, we determined to push on in spite of the thick mantle of cloud which enveloped the whole of the mountain, and effectually concealed from us every object. The road lay along the summit of the ridge, through the same kind of forest as the preceding day; the bed of decayed leaves rendering the path very disagreeable, and sometimes even dangerous. We had a good deal of ascent, as may be judged by the elevation we attained, 8080 feet, being upwards of 2000 feet above Tikree-bong. But we had much more than this; for the most tedious part of the road was a series of ascents and descents, sometimes very steep, and over a road so bad that I think I have never seen in any part of the hills a worse. Yet I must say that it could, with a very small expenditure of means, have been made a very good one. After reaching the highest point we began to descend through a thick jungle, I will not call it forest, of the small bamboo, in which the stems were so intertwined that it was with some difficulty we could make our way. The former part of the road was about the worst I had ever seen, but it yielded to this. For miles we could see nothing but these stems interlaced in every direction; while it was often a matter of considerable difficulty, picking our way through them over the mass of decayed leaves or slippery clayey soil, where the ground was at all visible. The day was one of the most dreary that can be imagined, and doubtless, by the sombre colouring it threw over every thing, gave us an exaggerated idea of the difficulties. For the greater part of the distance we could not see even the sky, the forest forming a thick covering over head; while the density of the cloud in which we were enveloped, afforded us little more light than might be called darkness visible by which to see our way. But it must be noticed, that in speaking of the badness and difficulty of this road, the ordinary features of bad mountain roads are not to be understood; but rather such as might belong to any unfrequented track, even in a plain country, through similar jungle and in similar weather. After a long march, the tediousness of which was rendered worse, by the difficulties and discomforts mentioned, we emerged from the forest, and found ourselves on a part of the ridge entirely cleared, marked by a small square erection of a few feet, with a pyramidal top, which they called Paspatnáth. In front no trees were visible, while the prospect opening gave us a very general view of the country, and showed us to be within a considerable basin, the sides of which were formed by lofty mountains. The cleared spot on which we stood was DÁRJÍLING[1].

IV.—*Report of the Committee appointed by the Council of the Royal Society, to consider the subject referred to in Mr. Stewart's Letter, relative to Mr.* Babbage's *Calculating Engine; and to report thereupon.*

Your Committee, in this their Report, have no intention of entering into any consideration of the abstract mathematical principle on which the practicability of such a machine as Mr. Babbage's relies, nor of its public utility when completed. They consider the former as not only sufficiently clear in itself, but as already admitted and acted on by the Council in their former proceedings. The latter they regard as obvious to every one who considers the immense advantage of accurate numerical tables in all matters of calculation, especially in those which relate to astronomy and navigation; and the great variety and extent of those which it is professedly the object and within the compass of Mr. Babbage's engine to calculate and print with perfect accuracy.

The original object of the present machine was to compute any tables which could be calculated by six orders of differences and twelve figures in each, and sixteen figures in the table itself, in such a form that by bestowing a very moderate degree of attention on their publication, it would be impossible for a single figure to be erroneous; and supposing any person employing them to entertain a doubt whether that moderate degree of care had been bestowed, he might in a short time himself verify the tables. The machine was intended to produce the work stamped on plates of copper or other proper material. Besides the cheapness and celerity of calculation to be expected from it, the absolute accuracy of the printed results being one of the prominent pretensions of Mr. Babbage's undertaking, the attention of your Committee has been especially directed, both by careful examination of the work already, executed, and of the drawings and by repeated conferences with Mr. Babbage, to this point. And the result of their enquiry is, that such precautions appear to have been taken in every part of the contrivance and work which they have examined; and so fully aware does the inventor appear to be of every circumstance which may by possibility introduce error, that they have no hesitation in saying they believe these precautions effectual, and that whatever the engine does, it will do truly.

In the actual execution of the work they find that Mr. Babbage has made a progress, which, considering the very great difficulties to be overcome in an undertaking so novel, they regard as fully equalling any expectations that could reasonably have been formed; and that although several years have now elapsed since the first commencement, yet, that when the necessity of constructing plans, sections, elevations and working drawings of every part; that of constructing, and in many

[1] We have been obliged to leave off here, and to reserve the continuation of this paper for our next number.

cases inventing tools and machinery of great expense and complexity (and in many instances of ingenious contrivance, and likely to prove useful for other purposes hereafter) for forming, with the requisite precision, parts of the apparatus dissimilar to any used in ordinary mechanical works,—that of making many previous trials to ascertain the validity of proposed movements, and that of altering, improving, and simplifying those already contrived and reduced to drawings.

Your Committee are so far from being surprised at the time it has occupied to bring it to its present state, that they feel more disposed to wonder it has been possible to accomplish so much.

The drawings form a large and most essential part of the work: they are executed with extraordinary care and precision, and may be regarded as among the best that have ever been constructed. On these all the contrivance has been bestowed, and all the alterations made; so that scarcely any work, excepting drawing, has been thrown away. When it is mentioned that upwards of 400 square feet of surface are covered with drawings, many of them of the most elaborate description, it will easily be understood that a very great expense of time, thought, and capital, must have been incurred in producing them; but without which your Committee consider that success would have been impossible.

Nearly the whole of this department of the work (according to Mr. Babbage's statements, probably nine tenths) is completed, and what remains is of a nature to afford no difficulty, on the score of contrivance; so that there is no reason why the execution of the work (hitherto necessarily retarded till the completion of the drawings) could not now proceed with rapidity; and according to what the Committee have been enabled to gather from Mr. Babbage's statements and their own observations, and supposing no unexpected cause of delay, they regard a further period of three years, as probably sufficient for its completion.

In judging of the adequacy of Mr. Babbage's work, to complete the objects for which it was intended, these are two distinct questions,—the adequacy of the contrivance, and that of the execution.

On the former point every information has been afforded by Mr. Babbage; and both the drawings and the work executed have been unreservedly subjected to their discussion, and have been such as to excite a well grounded confidence. The movements are combined with all the skill and system which the most experienced workmanship could suggest.

But in so complex a work, in which interrupted motions are propagated simultaneously, along a great variety of trains of mechanism, it might be apprehended that obstacles would occur, or even incompatibilities arise from the impracticability of foreseeing all the possible combinations of the parts, and of which, in a mere inspection, your Committee could not be expected to form a judgment. But this doubt, should it arise, your Committee consider as fully and satisfactorily removed, by the constant employment by Mr. Babbage, of a system of mechanical notation, devised by himself, and explained in a paper in the Transactions of this Society, which places at every instant, the progress of motion through all parts of this or any other mechanism distinctly in view; and by an exact tabulation of the times required for all the movements, renders it easy to avoid all danger of two contradictory impulses arriving at the same instant at any part.

Of the adequacy of the machinery to work, under all the friction and strain to which it can ever be fairly exposed, and of its durability, your Committee have not the least doubt. Great precautions are taken to prevent the wear of the parts by friction, and the strength, solidity, and equilibrium in the whole apparatus, ensure it from all danger on the score of violence or constant wear.

It ought also constantly to be borne in mind, that in all those parts of the machine where the nicest precision is required, the wheel-work only brings them, by a first approximation (though a very nice one) to their destined places. They are then settled into accurate adjustment, by peculiar contrivances, which admit of no shake or latitude of any kind.

The machine consists essentially of two parts,—a calculating part and a printing part. These are both equally essential to the fulfilment of Mr. Babbage's views; for the whole advantage would be lost, if the computations made by the machine were copied off by human hands, and transferred to type by the usual process. The actual work of the calculating part is in a great measure constructed, although not put together, a portion only having been temporarily fitted up for the inspection of the Committee: and from its admirable workmanship, they have been able to form a confident opinion that it will execute the work expected from it. At the same time the Committee cannot but observe, that had inferior workmanship been resorted to, such is the number and complexity of the parts, and such the manner in which they are fitted together, the success of the undertaking would have been hazarded; and they regard as extremely judicious, although of course very expensive, Mr. Babbage's determination to admit of nothing but the very best and most finished work in every part; a contrary course would have been false economy, and might have led to the loss of the whole capital expended on it.

In the printing part less progress has been made in actual execution than in the calculating: the reason being, the greater difficulty of its contrivance; not for transferring the computations from the calculating part to the copper or other plate, ultimately destined to receive them, but for giving to the plate itself the number and variety of movements which the forms adopted in printed tables may call for in practice. The movements necessary for effecting this, being entirely such, as might at any time be decided on, were purposely allowed to stand over till the more difficult parts should be fully developed. Taking the calculating and the printing part together, and regarding the tools and machinery already erected, as available for the performance of what remains, the Committee regard it as not improbable that three-fifths of the work may be already completed; but they cannot be expected to state this with any degree of certainty.

With regard to the expense incurred and likely to be incurred, Mr. Babbage states the sum already expended by him at £6,000; £1000 of which, he states to have been laid out in preliminary trials, which have not formed any object of enquiry with the Committee. Taking into consideration the extent of the work and drawings which they have examined, and judging entirely from the general knowledge of the cost of these and similar works, which the professional experience of several individuals of the Committee has enabled them to acquire, they are no way surprised at the outlay that is stated to have been incurred. With regard to the future cost, they have of course less means of judging than of the past, of which they see the results, and the tools with which they were produced. A probable conjecture might be grounded on the proportion of $\frac{3}{5}$ths assumed as the proportion of the work already done; but this would require to be received with great latitude. Finally, taking into consideration all that has been already said, and relying not less on the talent and skill displayed by Mr. Babbage as a mechanician in the prosecution of this arduous undertaking for what remains, than on the matured and digested plan and admirable execution of what is accomplished, your Committee have no hesitation in giving it as their opinion, that "in the present state of Mr. Babbage's engine, they do regard it as likely to fulfil the expectations entertained of it by its inventor."

(Signed) J. T. W. Herschel, *Chairman.*

Minutes of the Council of the Royal Society, relating to the Report of the Committee on Mr. Babbage's Calculating Machine.

[February 12, 1829.

The Report of the Committee appointed to consider the letter of Mr. Stewart, relative to Mr. Babbage's Calculating Machine, was received and adopted.

Resolved, That the thanks of the Council be given to the Committee for the pains they have bestowed upon the subject referred to them, and for their able report.

Resolved, That the following answer be sent to Mr. Stewart, viz.

The Council of the Royal Society, having taken into consideration Mr. Stewart's letter, dated December 1828, requesting their opinion, "Whether the progress made by Mr. Babbage, in the construction of his machine, confirms them in their former opinion, that it will ultimately prove adequate to the important objects which it was intended to attain," appointed a Committee, consisting of the president and secretaries—Mr. Herschel, Mr. Warburton, Mr. F. Baily, Mr. Burton, comptroller of the mint, Mr. Brunel, F. R. S. civil engineer, Mr. Donkin, civil engineer, Mr. Penn, and Mr. G. Rennie, F. R. S. civil engineers—from the result of whose examination of the drawings, the tools employed, and the work already executed, as detailed in the annexed report, they have not the slightest hesitation in pronouncing their decided opinion in the affirmative.

The Council of the Royal Society cannot conclude without stating their full concurrence in the report of their Committee, comprising, as it does among its members, several of the first practical engineers and mechanicians in the country: nor without the expression of a hope, that while Mr. Babbage's mind is intently occupied on an undertaking likely to do much honor to his country, he may be relieved as much as possible from all other sources of anxiety.

V.—*Statistical Account of Anupsheher.*

A short account of this town and the neighbouring country may prove interesting, as having been at one time the emporium of a considerable trade in indigo, cotton, and wood. The town is situated on the west bank of the Ganges, very conveniently for carrying on a trade with the Duáb and Rohilkhand districts, and there can be little doubt of its being again brought to notice in the mercantile world.

Population,		
	Hindús,	6206
	Musulmáns,	1866
	Total,	8072

Divided into 1723 families, as follows :

Castes.	Employment.	Castes.	Employment.
18 Bráhman,	Farmers.	1 Bilóch,	Servant.
19 Rájpút,	Ditto.	7 Chípi,	Chintz makers.
23 Gadi,	Ditto.	10 Rangréz,	Dyers.
12 Lódah,	Ditto.	11 Mirdha,	Land-measurers.
19 Máli,	Ditto.	18 Ráj Mistri,	Masons.
3 Shékh,	Soapmakers & Farmers.	23 Barhaí,	Carpenters.
8 Ját,	Farmers.	4 Lohár,	Blacksmiths.
1 Ahír,	Ditto.	16 Kumhár,	Potters.
18 Lódah,	Labourers.	3 Kúzgar,	Make fancy dolls.
40 Máli,	Ditto.	2 A'tashbáz,	Fireworkers.
14 Ját,	Ditto.	2 Tírgar,	Arrow makers.
5 Ahír,	Ditto.	1 A'bkár,	Sells spirits.
100 Chamár,	Ditto.	14 Darzi,	Tailors.
39 Mihtar,	Ditto.	2 Khómra,	Make mill stones.
45 O'dh,	Ditto.	3 Shékh Dupáli,	Musicians.
116 Bráhman,	Merchants.	1 Ditto,	Farrier.
32 Ditto,	Priests.	1 Ditto,	Basket maker.
139 Ditto,	Servants.	34 Náí,	Barbers.
20 Rájpút,	Ditto.	21 Télí,	Sell oil.
18 Gadi,	Sell Milk.	7 Pómba,	Cotton cleaners.
191 Bania,	Merchants.	2 Manihár,	Glass makers.
23 Káíth,	Servants.	13 Méo,	Watchmen.
6 Ditto,	Scriveners.	16 Dhóbi,	Washermen.
12 Khatri,	Merchants.	18 Bihishtí,	Water carriers.
6 Ditto,	Servants.	2 Kistpaz,	Brickmakers.
7 Béaúrah,	Bankers.	1 Sikligar,	Polishes steel.
15 Sonár,	Goldsmiths.	10 Bharbunja,	Parch grain.
11 Taga,	Servants.	56 Mala,	Boatmen.
6 Garería,	Shepherds.	21 Kahár,	Bearers.
10 Tambóli,	Sell *páns.*	12 Kunjra,	Sell vegetables.
66 Gujuráti,	Priests.	16 Qasáb,	Butchers.
4 Acharáj,	Attend funerals.	17 Khaterh,	Labourers.
13 Bhát,	Minstrels.	21 Tawáp,	*Kunchanee.*
8 Gusáin,	Mendicants.	8 Bhatiára,	Cooks.
17 Dahaút,	Ditto.	12 Mírási,	Musicians.
11 Faqír,	Beggars.	1 Hijra, or Eunuch.	
26 Shékh,	Servants.	1 Nat,h,	Tumbler.
3 Saíad,	Perfumers.	93 Juláha,	Weavers.
12 Móghal,	Servants.	15 Móchi,	Shoemakers, &c.
29 Pathán,	Ditto.	1 Bári,	Cutler.
4 Ditto,	Well-diggers.	1 Kurabi,	Porter.
1 Kalál,	Servant.	7 Ahiria,	Kill game.

There are 14 *Chaókidárs* in the town, receiving 42 rupees per month; and 893 *havélis* or tenements.

There is a trade and manufacture in coarse and fine cloth, blankets, boats, hackeries, soap, shoes, *jamdáni*, cotton, and indigo; and a considerable trade in the produce of the neighbouring country, and of wood and bamboos at the ghaut. The trade in former times must have been considerable: the *dák mūnshi* informs me he used to receive 200 rupees per month of postage, and now it seldom amounts to more than 3 or 4 rupees.

The town is on the very banks of the river, and part of it has been carried away at times in the rainy season ; it is surrounded by deep and irregular ravines, particularly on the south, where the manufactory now unoccupied stands, below which are the ruins of a fort, the former residence of the late *Rája* Tara Sing and his ancestors, and which was built by Anee Rao 291 years ago, in Akber's time. He was *mansebdár* to this prince, and a man of great consequence. About 40 years ago, the family quarrelled among themselves, and the *Ráni*, rather than submit to some degradation, blew up the fort, and perished in the ruins, along with several of her attendants. Five years afterwards, the destruction of the place was completed by Asaf Daúlah's army, when the family seems to have fallen from their former prosperity. Treasure was concealed in the place, but, it is said, has been abstracted. At this time, persons employ themselves in the night in digging, it is supposed with little success ; but from the attempts having been so long continued, something must be occasionally found ; the Rajaship appears to have consisted of 126 villages, and what was left to the family, on the occupation of the country by the British Government, has been sold to pay off debts, on the most disadvantageous terms, and the *Ráni* is now living in the town in a comparative state of penury ; Jód Rái *Brahman*, has a large manuscript volume, containing the whole history of this family, and, like the minstrels of old, gains a subsistence by writing the tale.

On the west of the town, near where troops usually encamp, there is the tomb of an officer, who was killed 32 years ago, at the fort of Khailía ; 5 coss hence, two of the garrison of Khailía were hanged on a couple of trees, mango and tamarind, near the grave. From the statement of the natives, the officer was killed in a treacherous manner. There are other tombs to the south-west of the town, and also another Christian burying-place to the north, in the lands of Jáferabád, where there was formerly the cantonment of troops, and the only mark now left of its existence. There is a *méla* held three days before the last day of *Kartik* at the Ganges; there is no particular day for the market, which is always well attended. There are three schools attended by 32 boys, and they appear to have 31 rupees per month. Many people may recollect when the islands in the river, and jungle on the banks, afforded cover to numerous tigers : there is nothing of the kind now. The sportsman, however, if he be industrious, may find a few wild hogs, deer, black partridge, and occasionally a florikin. Wild cows, of a beautiful white color, are found on one of the islands ; but no person I suppose would shoot them, who had any respect for the feelings of the natives. A respectable Musulman informed me he caught two young ones with a great deal of trouble ; they were perfectly wild and quite untameable; and one killed a servant, by literally kicking his brains out. At the urgent request of the Hindús he let them go again.

The Duáb above Aliger,h is divided into villages or estates, averaging about 800 acres each, paying a yearly rent to Government of 1 R. 9 a. 7 p. per acre of land under tillage. Of the whole superficial contents, there appears to be $\frac{617}{1000}$ of the lands under cultivation ; $\frac{349}{1000}$ under the denomination of fit for cultivation ; and $\frac{34}{1000}$ as barren. The population appears to be 268 to the square mile, there being one Músulmán to $\frac{3}{10}$ Hindús. The hire of a labourer, when employed near his own home, is $1\frac{1}{2}$ annas per day, or $2\frac{1}{2}$ pence, taking the value of a rupee at two shillings.

The soil may be divided into two kinds—*bángar* and *khádir*. 1st. *bángar*, or lands not liable to be covered with water at any time, which may be divided into two sorts ; first, that where the subsoil will permit of wells being easily dug : it consists of a fine mould, in some places 10 or 15 feet deep, and calculated for any crop; and second, that which cannot afford wells, having the subsoil of sand : the latter soil is known by the name of *khéki* or *bhúr*, the former *chahi*, and of course are sub-divided into different others, as superior, inferior, and middling ; almost all bearing different names in different *pergannahs*.

2nd. *khádir*, or lands in the vicinity of rivers, or otherwise low and occasionally covered with water in the rains, and which also may be divided into two sorts; first, a light sandy soil, improved by alluvial deposit, calculated for the production of wheat and barley, without irrigation : these lands lying low, the crops are liable to injury from occasional floods, and can only be cropped once in the season, and that only during the *rabi*, or spring.—Second, the higher *khádir* lands, consisting of a rich loamy soil, calculated for *chari*, sugar cane, and, where stiff clays predominate, gram : these lands are (except in cases of sugar cane) often cropped three times, and always twice a year.

The following is a table of the quantity obtained from an English acre in average lbs. of the staple produce of the country.

Wheat	Bar-ley.	Gram.	*Jáur.*	*Bájra.*	*Urd.*	*Mót,h.*	Mus-tard.	Tobac-co.	Cotton	*Gúr* or coarse Sugar.	Manufactured Indigo.
1486	1693	1160	685	542	542	542	542	1087	237	2037	17; or 5 acres to 1 maund.

The indigo manufactories seem to be very thriving at present, from a new plan they have adopted of purchasing the plant from the cultivator, the average price of which at the beam may be 26 rupees per 100 maunds. From 280 to 300 maunds of plant are required for one maund of dry indigo, and a *kacha bíga* is calculated to produce 10 maunds. Indigo seed is now selling for only 2 rs. per maund, and the late rains and hail storms, having partially injured the *rabi* crop, there is no expectation of large quantities being sown.

The seer in use in the Mírat and Bulúndsheher divisions is 84 sonats, the weights being ascertained by rupees that have been in circulation. I find this seer to weigh 2 lbs. 1 oz. 15 drs.; the maund of 40 seers is therefore 84 lbs. 13½ oz. or 3305½ sicca weight.

The seer in use at Sahárunpúr, Aligerh, and some parts of Muzafarnagar, is 90 rupees, weighing 2 lbs. 4oz. 5½drs. and the maund 90lbs. 14½ oz. or 3541¼ sicca weight; and this is the seer in use in the town of Anupsheher. But it will generally be found the *Bania's* weights are all short several rupees per seer, which, with their proficiency in humouring the scales, makes the retail rate so profitable to them.

J. B.

Camp near Anupsheher,
1*st March,* 1830.

VI.—*Notice regarding the male and female Orang-outang, in the possession of* George Swinton, *Esq. of Calcutta, in a letter to Dr.* Brewster.

[From Brewster's Journal, vol. i. N. S. p. 369.]

"Last year I sent you an account of my Orang-outang[1]. I have lately got a female companion for him, apparently of the same age. She wants the thumb nails of the lower extremities, which confirms me in the opinion that this is a distinction of sex, not of species. The young female carried home by Lady Amherst, wanted these nails. My male, and the great Sumatran Orang, described by Dr. Abel, has them. The thumb of the foot in the female, looks as if the upper joint had been chopped off below the nail, and the skin had healed over the wound."

Mr. Swinton goes on to mention the deportment of the two Orangs on their first introduction to each other. They tumbled about like children, but without any symptoms of sexual desire, which he attributes to their being so very young. The following notice of the female, in a letter from Captain Hull to Mr. Swinton, with Mr. Swinton's remarks, will be read with interest; and we hope Dr. Grant, whose able description of the male appeared in this journal, will find leisure to draw up a similar account of the female. In case of the death of one or both of the animals, their bodies are to be preserved and sent to England for dissection.

"This female stands two feet six inches in height; is extremely docile and playful; has been in the possession of Mrs. B. for nearly twelve months, during which period it became the constant playmate of the children. The only information I can give respecting the abode of this animal, is that it was sent here by Mr. B. from Macassar, who is residing on the Celebes. I conclude this animal is a native of Borneo, which island lies adjacent, distant only a few miles across the straits, and most probably it came from the woods near Bangirwassin.

"This animal must be very young, from the appearance of the teeth. The number of grinders in each jaw is four. In the adult, described by Dr. Abel, the grinders are ten in each jaw. It differs in external appearance[2], in some points, from

1 Vol. ix. O. S. p. 1. We shall take an early opportunity of publishing this very interesting paper.—Ed. Gl.

2 Very slightly; when together, the female can only be distinguished by a more slender and feminine appearance. If any thing, she is rather taller.—G. S.

the Orang-outang which I saw at Mr. Swinton's. The head is more thickly covered with hair, and hangs down much longer on each side of the checks, and is more bushy. The nose is a more prominent feature[3]; the lips are thicker, especially the under[4], and turns more outward than in any other of the species I have seen, one of the marked distinctions between this order and man. The nail on the great toe is wanting; this is an essential difference[5]. Its gait or mode of moving about the room, is generally at a walk in an upright posture; whereas the animal which I used to observe at Mr. Swinton's, scarcely ever attempted to move in an upright posture[6]. On the contrary, his manner of moving was in a stooping position, pushing himself along the ground with his hands like a cripple bent double. It is worthy of remark, that in accelerating his motions in this manner, he always used the back of the hand; thus bending the wrist in a contrary direction to the human species.

"Anatomical subjects of the species *Simia Satyrus* will now be a desideratum, because the naturalists who have inspected the female subject, which I sent to Sir Stamford Raffles from Sumatra, have described it to be of a different species to the animal already designated and described under the genus *Simia Satyrus*, or Orang-outang of Borneo[7], in Linnæus' System. I have not seen the paper myself, which has been read before the Society in London, in delineation of the specimen which I transmitted. But I believe one essential difference in the structure of the Sumatran animal, which distinguishes it from the Borneo specimens which have hitherto been sent home for examination, is in the number of spinal bones being greater in the Sumatran ape. The naturalists in England have described the Sumatran animal to be of a different species of *Simia*, which they allege Dr. Abel, in his description of the animal, brought to Calcutta by Captain Cornfoot[8], has erroneously classed with the Orang-outang of Borneo. What a pity it is, there is now so little prospect of obtaining another specimen of this wonderful inhabitant of Sumatra. I do not see how the difference of opinion can be set at rest, without obtaining a perfect subject with all the fleshy parts and viscera for examination[9]. If I meet with an opportunity of returning to Bengal by the way of Sumatra, I shall certainly endeavour to get up to the northern parts, and spare no trouble or expense to procure another subject."

A model of the male Orang, in the possession of Mr. Swinton, has been sent by that gentleman to the Royal Society of Edinburgh.

VII.—*Miscellaneous Notices.*

1. *Crocodile of the Nile and* Ghariál *of the Ganges.*

A story, related by Herodotus, of the Crocodile of the Nile, had been very generally disbelieved by the naturalists of Europe, when a communication was presented, in January 1828, by M. Geoffroy St. Hilaire, to the Academy of Sciences, confirming the general truth of the story, though correcting some of the details. Herodotus says, that this reptile's mouth is infested with a creature which he calls *bdella*, (generally translated leech;) and that a bird, called *trochilos*, entering his mouth, frees him from this plague. In return, the crocodile never harms this bird.

[3] The nostrils are more defined and raised; but can hardly be called *prominent*. If any thing so flat can be called a *nose*, I would say that her nose is handsomer than his. —G. S.

[4] I see no difference in the under lip. It is perpetually varying in thinness or thickness from the action of the muscles, just as we can make the lip thick or thin by contracting or stretching it.—G. S.

[5] Not an essential, but a sexual difference, I am strongly inclined to believe. Dr. Montgomerie informs me, that the female he dissected at Singapore wanted this nail. This then is the third female in which the nail has not been found.—G. S.

[6] The female may have been taught to stand upright. In playing together, they move exactly in the same way; but she can balance herself better on her legs than he can.—G. S.

[7] This is the one already alluded to in some of my former letters. She was about five feet high, and was killed near the same place, where the great male, described by Dr. Abel, was found.—G. S.

[8] Only the hand, foot, and lower jaw and skin brought to Calcutta.—G. S.

[9] I have given a commission to the captain of a vessel, trading with Sumatra, to endeavour to get one, dead or alive.—G. S.

The story had been, in latter times, either explained away or disbelieved altogether, notwithstanding the very general repetition of it by the writers of antiquity. M. G. St. Hilaire, during his long residence in Egypt, had opportunities repeatedly of verifying this account, but with the change of *gnats* for *leeches.* The bird is in fact the *Charadrius Egyptius* of Hasselquist, and is very like, if it be not identical with the small ringed plover of France[10]. It is further stated that the Crocodile of St. Domingo, which resembles closely that of the Nile, (particularly in having, like the latter, its tongue fixed,) derives similar services from a small bird which frees its mouth of the gnats that torment it.

The story is certainly a curious one, and it became a question, immediately, whether any thing of the kind had ever been observed of our alligators in the Ganges. The natives of the country are not aware of any thing of the kind, and a gentleman who found some interest in the inquiry in a river voyage, took opportunities of examining the animal, when basking on sandbanks or on the shore, with an excellent telescope "capable of showing *a beetle* at the distance used." Out of 369 individuals, he did not see one in which there was any appearance that could give rise to such a story. "In many cases there were paddy birds, pelicans, divers, &c. near them, probably attracted by the same cause which induced the alligators to visit the place, the abundance of fish; but although they were frequently very near, they did not at all interfere with each other. In one solitary case there were a few sand plovers, which answer to the description of M. G. St. Hilaire's *trochilos,* about ten yards from a party of alligators, but apparently paying them no sort of attention. Of the above number of alligators, only thirteen were seen with their mouths open, all in the afternoon, none being observed in that state before 3 P. M."

The letter from which the above is extracted adds—"The natives distinguish the *Gharriál* of this river into three kinds, called *Taría*, *Ghótiál*, and *Jalgúi.* The two former they consider as males, and the latter as females. The *Taría* and *Ghótiál* have knobs, the former small, and the latter very large, at the extremity of the snout, through which they make a humming noise on respiring after being long under water. They are both vicious, seizing men, geese, &c.; but the *Jalgúi* is not so, and on this account I should think that they must be of a different species altogether, although a very experienced fishermen declares that they are all produced from the same batch of eggs. I shall make further enquiry on this head, and give you some positive information on the subject soon. I had no idea that the *Gharidls* were so numerous in this river, until I yesterday and the day before set people to look out, and put down the number examined with the telescope. By the bye, has this instrument been much used by naturalists in studying the manners of birds, &c.? It appears to me to be of very great use in this way, the action and courage of the birds, &c. being much more free and unconstrained when at liberty, than when cooped up in a cage, and equally distinctly seen."

2.—*Method of Preserving Oranges, from one Season to another.*

Calcutta is abundantly supplied with oranges from the hills adjacent to Sylhet, in the months of December, January and February, and at a very moderate price. The fruit is by no means bad, though it is believed that the trees are growing quite in a state of nature, no pains whatsoever being bestowed upon their cultivation, with a view to the improvement of the fruit; and no further care taken of the groves, than to secure the valuable rents they even now yield in their present wild neglected state. The groves are farmed to the merchants who supply the Calcutta market, and are said now to be mostly held by a European firm. As the oranges only reach our tables during the three coldest months in the year, they prove but unpalatable fruit to most people, for oranges can only be truly relished in the hottest weather; and it has long been a desideratum with all horticulturists, to introduce an orange that should afford fruit at a more seasonable period of the year. However, all attempts having been hitherto ineffectual, it seems desirable to endeavour to preserve the fruit by artificial means; and the following method, which is reported to be in common use with the Chinese, appears well adapted to that purpose. As an excavation in this part of Bengal might be objectionable, small cylinders of earth or masonry might be constructed above ground, that would doubtless be equally efficacious. A cool shaded spot should be selected, and the walls made thick to preserve the fruit in as cool a state as possible; and the better to effect this, the walls within might be lined with straw and coarse blankets, or any other non-

10 M. G. St. Hilaire's paper may be seen in the *Revue Encyclopedique*, or an abstract of it in Brewster's *Journal of Science*, vol. ix. O. S. p. 68.

conductors of heat as are usually had recourse to for the same purpose in the pits. After closing the mouth of the pit and making it quite air tight, the mound or cylinder might be well coppered over. Should this mode prove successful in preserving oranges, only for one month after the setting in of the hot winds, when there is no longer a single orange in the Bazar, there could be no doubt but the price then obtainable, would most amply repay the cost of the experiment. Should the plan even so far answer, it would seem to hold out the prospect of much more general utility. Mangoes, Pineapples, and most descriptions of fruits, would bear the same treatment; and in a far greater degree, potatoes and all tuberous rooted vegetables.

" First dig a trench or pit, to the depth of seven or eight feet, in a dry bank, sheltered from rain. Make a bed at the bottom, two or three inches thick, of chopped rice straw, of the leaves of the pine tree, well dried, and lay well picked, sound Oranges on the bed, so as not to touch each other. The first layer being thus completed, make a second upon a stage of bamboos a little above the under layer, and so on a third and fourth, &c. each being on a separate bamboo stage. All the Oranges being thus arranged, stop the mouth of the pit with a tile, securing its sides with several coats of clay that no air may be admitted to the Oranges. In this way Oranges can be kept perfectly fresh for six, eight, or ten months."—*Grosier's China*, vol. 2, p. 448.

3.—*Madras Astronomical Observations.*

Several volumes of observations, made by the Honourable Company's astronomer at Madras, have been given to the public. They extend back twenty years, and will doubtless be considered of much value and interest by the astronomers of Europe, but more particularly by our Indian surveyors, to whom indeed they must prove a mine of geographical results. To the latter, the most interesting of course are the observations of Jupiter's satellites, of the transits of the moon, and of occultations of fixed stars. We fear, however, that there are many important misprints, which if it be the case, is much to be regretted. Our reason for saying so, is founded on the following comparisons, which were made to determine the difference of longitude between Madras and a meridian, which, by comparison of a great number of observations with the calculated times, as given in the Nautical Almanac, appeared to be 5h. 9m. 54s. E. of Greenwich. The following table gives the particulars.

Emersions, Jupiter's 1st Satellite.

Year.	Month.	Date.	M. T. at unknown meridian.			M. T. at Madras.			Difference.		
			h.	m.	s.	h.	m.	s.	h.	m.	s.
1814	April, ...	25	7.	58.	29.	8.	5.	31.6	0.	7.	02.
	May, ...	2	9.	53.	45.	10.	4.	57.	0.	11.	12.
1816	June, ...	17	8.	27.	27.3	8.	39.	33.1	0.	12.	05.8
1817	Do.	13	9.	23.	37.	9.	34.	37.9	0.	11.	00.9
	Do.	30	7.	35.	25.9	7.	53.	21.6	0.	19.	55.7
1819	October, ..	23	7.	39.	48.1	7.	50.	32.9	0.	10.	44.1
	November,	15	7.	56.	02.	8.	7.	37.7	0.	11.	35.7
	December,	1	6.	16.	24.4	6.	27.	33.2	0.	11.	8.76
1820	November,	10	9.	17.	01.3	9.	28.	10.4	0.	11.	09.1
			Immersions.								
1817	April, ...	10	14.	6.	43.	14.	17.	52.4	0.	10.	52.4
1817	May, ...	12	10.	39.	26.6	10.	50.	01.3	0.	10.	34.7
1819	April, ...	18	14.	46.	16.6	14.	57.	59.8	0.	11.	43.2
	Do.	25	16.	39.	44.4	16.	51.	48.	0.	12.	03.5
	June, ...	3	15.	04.	29.	15.	16.	10.5	0.	11.	41.5
1820	May, ...	22	14.	46.	15.6	14.	57.	43.5	0.	11.	27.9

The differences, in both these sets, are so capricious and irregular, that it is difficult to know what conclusion to draw. That the errors belong to the Madras coluismn evident, from the close agreement which the observations in the other column exhibit, when compared with the N. A.

GLEANINGS

IN

SCIENCE.

No. 16.—*April,* 1830.

I.—*On the Polarization of Light.*

When the particles of light travel through a crystallized body, endowed with the property of double refraction, they are supposed to experience certain movements of rotation on their centres of gravity, dependent on the nature and direction of the forces exerted upon them by the atoms of the crystal. In certain cases the effect produced is, to arrange all the atoms of the luminous ray in one direction, with their similar faces or poles parallel.—This circumstance, which was first observed by Malus, is called POLARIZATION, from its analogy to magnetic phenomena. Thus, if a ray of light fall upon the surface of a plane glass mirror, at an angle of 35° 25′, the ray which is reflected, is found to be in this condition; all its component atoms are prepared to shew the same submission to any subsequent disturbing force, resembling so many minute magnets, suddenly brought within the influence of a powerful directive attraction, to which they arrange themselves conformably;—those which were right at first, of course remaining steady, while the rest undergo oscillations, until they also become polarized.

Although these functions of the minute invisible atoms of light, may be deemed purely conjectural, still they serve so well to explain the facts observed, and to render intelligible the various modifications of which they are susceptible, that, where brevity and perspicuity are desired, it becomes convenient at once to assume them as established laws. Indeed, the velocity and the times of oscillation are considered, in many cases, capable of calculation, as well as the depths to which the action continues in different bodies! But my object at present is not to enter into any subtle theoretical discussions; I merely wish to give a statement of the principal facts hitherto developed in this curious and little-known department of optics, with the aid of such simple diagrams as, I hope, will facilitate their comprehension, and very much lessen the task of explanation.

fig 1

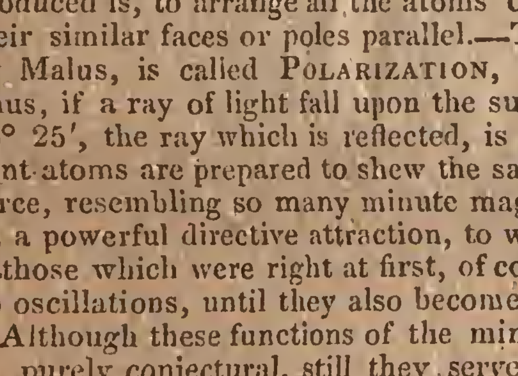
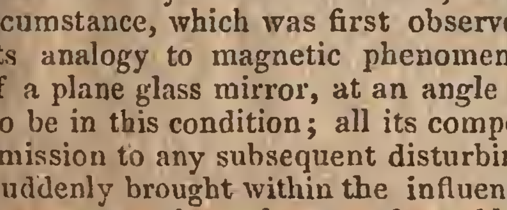


fig 2

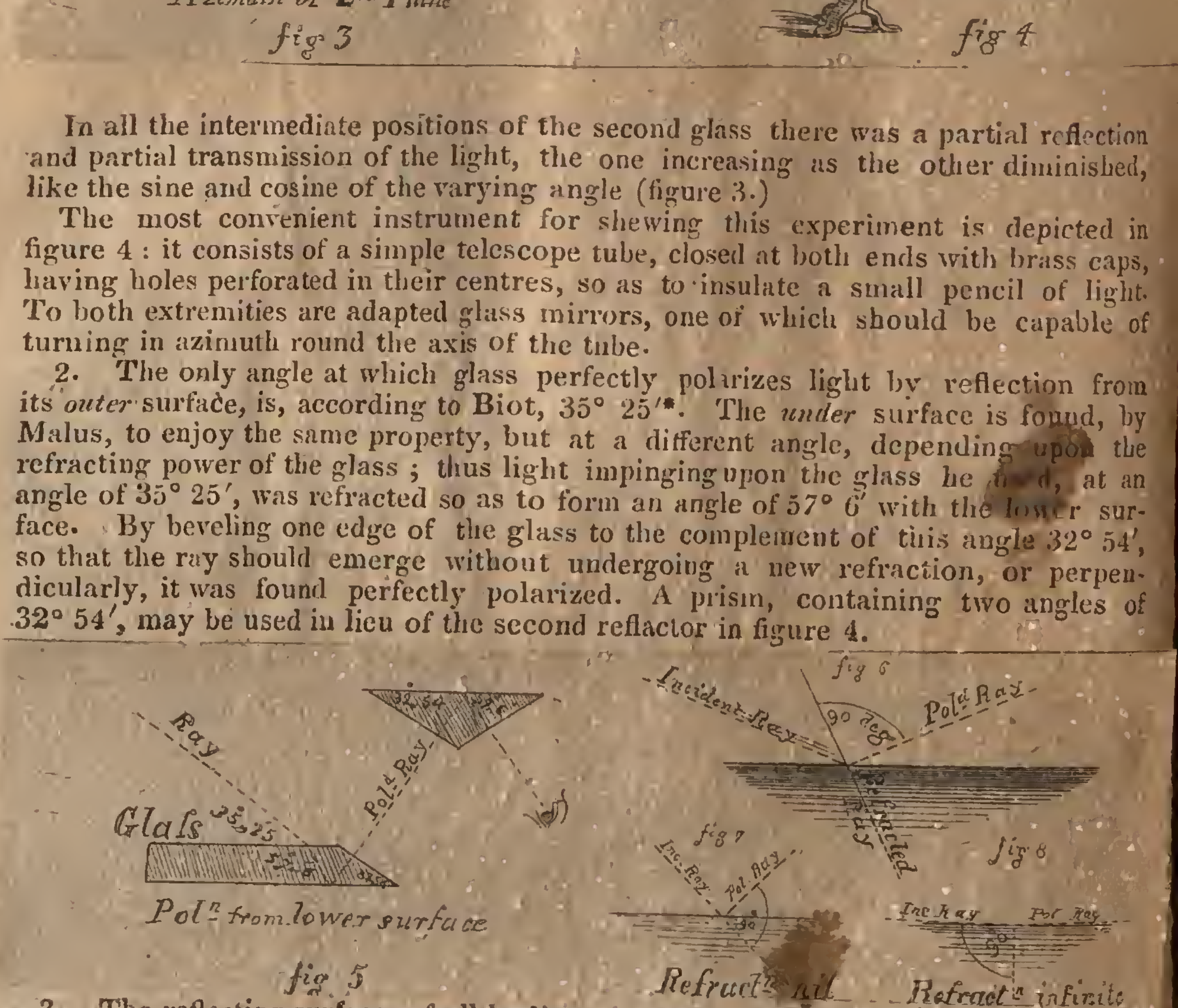


1. Malus observed, that when a ray of light impinged on a glass mirror, at an angle of 35° 25′, and was reflected thence, if another mirror were presented to the reflected ray at the same angle, so as to cause a second reflection *in the same plane,* the whole ray was, as might have been expected, *reflected,* and became visible to a spectator properly situated (figure 1, 2); but if the second reflecting plane were turned round, so as to form a right angle with the first, (retaining its angle of 35°.25′ with the ray,) then no light whatever underwent reflection, but on the contrary the whole ray passed through the glass, and was visible in a straight direction, as if no glass intervened. (fig. 4.)

In all the intermediate positions of the second glass there was a partial reflection and partial transmission of the light, the one increasing as the other diminished, like the sine and cosine of the varying angle (figure 3.)

The most convenient instrument for shewing this experiment is depicted in figure 4 : it consists of a simple telescope tube, closed at both ends with brass caps, having holes perforated in their centres, so as to insulate a small pencil of light. To both extremities are adapted glass mirrors, one of which should be capable of turning in azimuth round the axis of the tube.

2. The only angle at which glass perfectly polarizes light by reflection from its *outer* surface, is, according to Biot, 35° 25′*. The *under* surface is found, by Malus, to enjoy the same property, but at a different angle, depending upon the refracting power of the glass ; thus light impinging upon the glass be found, at an angle of 35° 25′, was refracted so as to form an angle of 57° 6′ with the lower surface. By beveling one edge of the glass to the complement of this angle 32° 54′, so that the ray should emerge without undergoing a new refraction, or perpendicularly, it was found perfectly polarized. A prism, containing two angles of 32° 54′, may be used in lieu of the second reflector in figure 4.



3. The reflecting surfaces of all bodies, crystals, liquids, and even metals, have been ascertained to exercise the power of polarizing light, each at an angle peculiar to itself. Biot and Malus found these angles to diminish as the refractive powers increased.

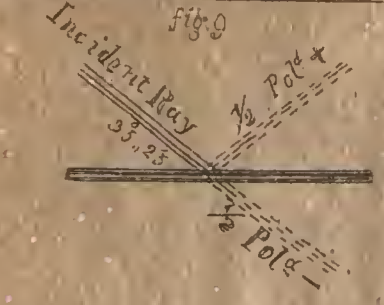
Water polarizes at	36°	58′, and its refraction is	1,3359
Whale Oil, ...	34	30	1,4666
Amber, ...	33	25	1,5555
Sulphate Barytes,............	32	06	1,6428
Topaz,.......	31	00	1,6520
Native Sulphur,..............	29	00	2,0278
Diamond, ...	22	54	2,43905

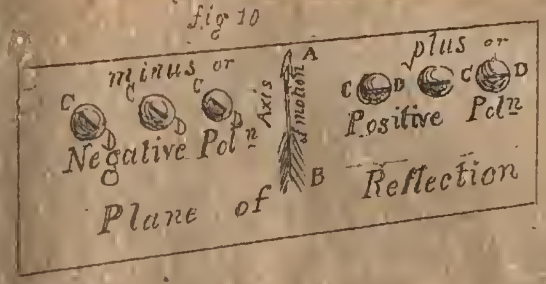
4. From these experiments Brewster deduced a very simple law ; namely, *that the polarized ray reflected from any surface, is a perpendicular to the refracted portion of the incident ray,* (figure 6.) Where the refraction vanishes, as in air, the angle of polarization will become 45° (figure 7), which it can never exceed. When on the contrary the refraction is infinite, the angle of polarization becomes 0° (figure 8.)

5. Hitherto we have noticed the portion of light *reflected* from the surface of glass at 35° 25′: but of the whole ray which arrives at this surface, one half only suffers reflection ; the remaining half passes through the glass, and proves, after transmission, to be completely polarized, in a sense opposite to that of the reflected half. A little reasoning will supply the cause of this ; for every natural luminous

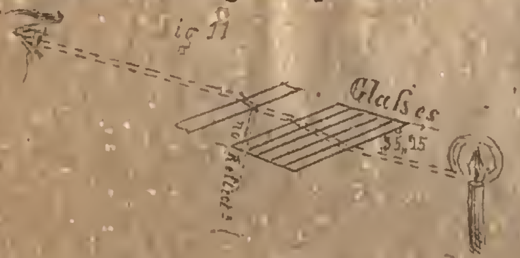
* Brewster makes this angle 34°, and I have myself found the latter angle succeed best. Probably the difference is due to the quality of the glass.

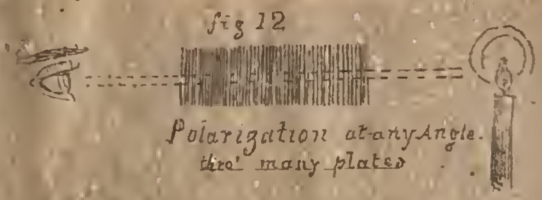
pencil may be supposed to be made up of a multitude of atoms, whose polar axes are in every possible direction, so that an equal number should be disposed to be diverted into either of the two planes, by the action of the forces inherent in the glass, of whatever nature these may be; analogy leading always to assign to them a close connection with the forces producing reflection and refraction themselves, which are attributed to attractions and repulsions exercised by the atoms of any substance upon opposite poles, as it were, of the atoms of light.





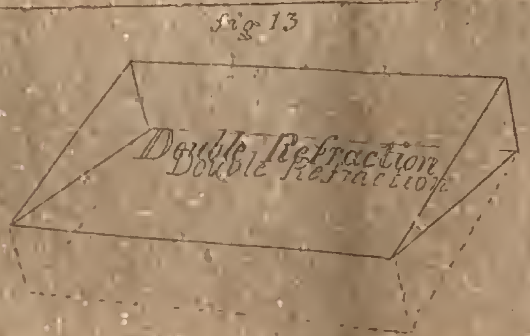
6. To render clearer the conception of the *opposite states* of polarization mentioned above, each atom of light should be regarded as having the power of revolving round its axis of motion, like a magnet upon its pivot; then when its poles (C D, figure 10) are situated parallel to the plane of reflection in which the atom is moving forward, it is said to be *positively* polarized: when they form a right angle therewith, the term negative polarization is employed.





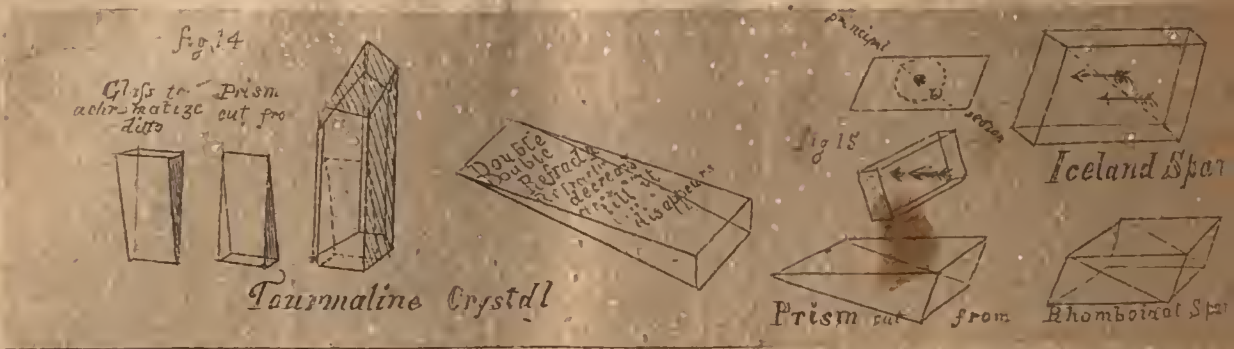
7. Light once transmitted at an angle of 35° 25′, having all its axes of polarization fit to escape reflection, will not suffer further diminution: if a number of glasses be applied to the back of the first, a candle will look equally bright; and if the last glass be drawn back (as in figure 11) it will be seen that there is no light reflected.

8. When the number of plates is increased to 30 or 40, the transmitted light is found to be polarized at any angle of incidence, although feeble in brightness; because each succeeding glass tends to rob the ray of the atoms fittest for reflection, and to transmit the remainder. In practice, the glasses should not be too close, otherwise they may act as a single glass.



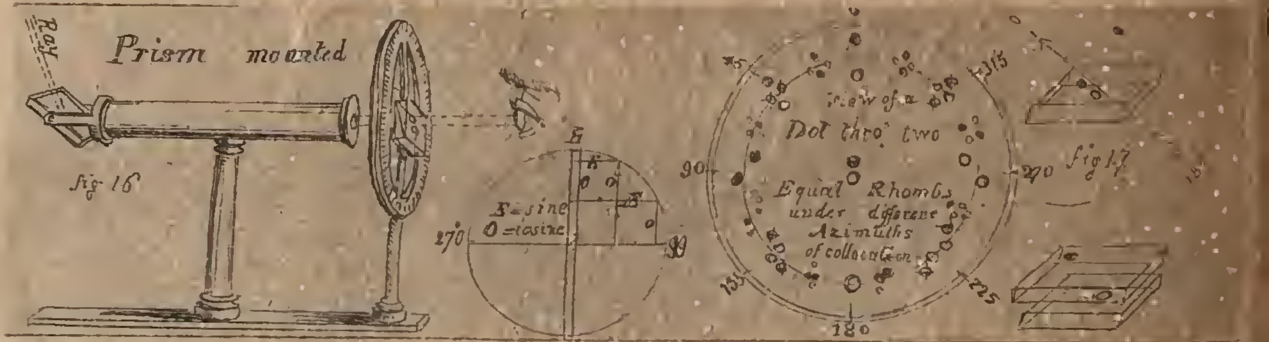
9. Having now noticed the chief circumstances of the polarization of light by surfaces of glass, I may proceed to detail the peculiar modifications of the same action, developed by crystallized bodies. Most of these have the curious property of dividing a transmitted ray of light into two portions, so as to render two images visible. These are very distinct in rhomboidal, or as it is hence called, double-refracting spar; but it will be more convenient to speak first of the tourmaline, which itself furnishes a mode of analyzing the polarized light of other substances.

10. If a thin prism be cut from the polished side of a tourmaline crystal, and a polarized ray be viewed through it, two images are seen towards the thinnest part of the prism. The ordinary image is not white like the other, but of a greenish yellow colour, and it grows fainter as the tourmaline increases in thickness until it disappears totally: a plate of tourmaline of this last thickness, therefore, will only



transmit the polarized rays of *one* denomination: this effect holds only when the ray under examination is polarized, and when the perpendicular axis of the tourmaline is situated in the plane of polarization. We have, therefore, through its means, a mode of determining both the position of the polarizing axis of any other crystal, and the nature of the polarization, whether positive or negative. Place the tourmaline over the image to be examined, and turn it in azimuth, until the image vanishes; then if the side of the prism is parallel to the axis of polarization, the nature of the latter is *positive* or *ordinary*.

11. The two images produced by a clear rhomboid of Iceland or calcareous spar, are sensibly equal in intensity and colourless. A line drawn through the two, is always parallel to the "principal section" or "short diagonal" of the rhomboid, (fig. 15). When the crystal is made to revolve, one image seems moveable: this last is the ordinary ray. Now, by analyzing the two images by the tourmaline plate, the ordinary ray, O, is found to have its polar axis *parallel* to the diagonal, and the extraordinary one, E, *perpendicular* thereto. The original natural ray, therefore, by traversing the crystal, has been precisely affected in the same way as when it met the surface of a glass plane, at an angle of 35° 25′, and in every instance one may be substituted for the other.

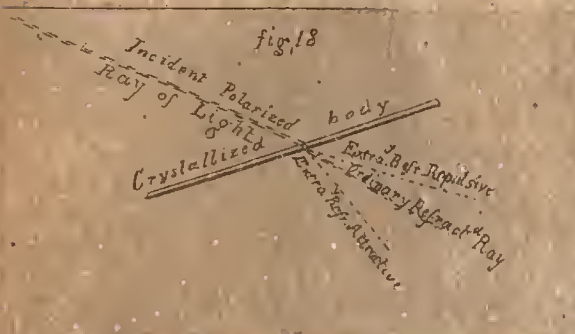


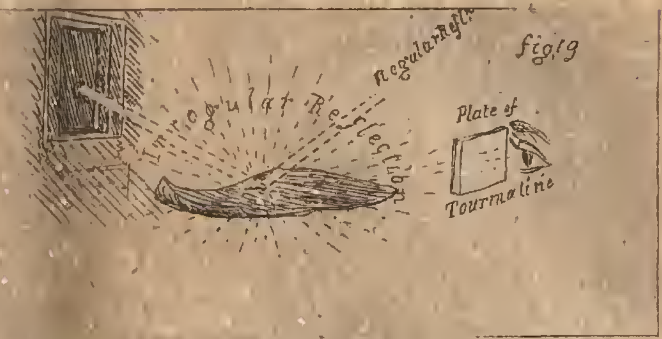
When the rhomboid (or what is better, a prism of the spar, achromatized by a prism of glass) is used instead of the second reflector of our instrument, then with the short diagonal parallel to the plane of reflection, we shall have but one image transmitted, (the ordinary or positive.) Turning the crystal round to 90 and 270°, or at right angles to the first reflecting plane, the polarized ray of light again traverses singly, as a negative or extraordinary image. In all other azimuths, two images will be visible, whose luminous intensity will vary as the sine and cosine of the angle of deviation, just as was stated to happen with the reflected and transmitted ray in figure 4.

12. If one rhomboid be placed over another, and an image be viewed (as the dot in figure 17), then a duplex action ensues, on making the upper rhomboid to revolve: for there are now two figures oppositely polarized to be acted on. At 0° the two images will be merely further separated, as if the crystal were of double thickness: at 180° they will be brought together and coincide, should the rhomboids be of the same dimensions: at 90° and 270° the two *original* images will have vanished, but two new ones, the reciprocals of each, will have taken their place: at 45°, 135°, &c. *four* images of equal intensity will be visible, two of which will become fainter, on approaching either of the quadrants.

13. On analyzing the images seen through the *rhomboid* by means of the tourmaline, the most deviating image was found to be polarized parallel to the axis or short diagonal, and the less refracted or extraordinary ray, at right angles to the same; but in a prism of silex or rock crystal, it is just the reverse. This is ascribed to the double refraction of the former being *repulsive*; of the latter *attractive*, fig. 18. The extraordinary ray, properly so called, of both bears, therefore, really, the same species of polarization, i. e. perpendicular to the *axis of crystallization*, and

this is a rule with all crystals, so that a method is hereby obtained for determining whether the double refraction of a substance be attractive or repulsive.

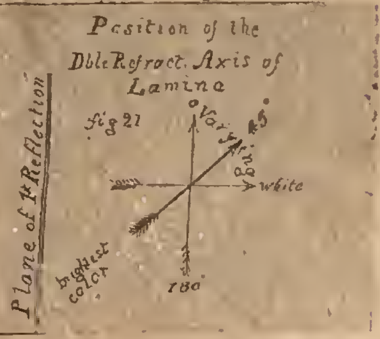




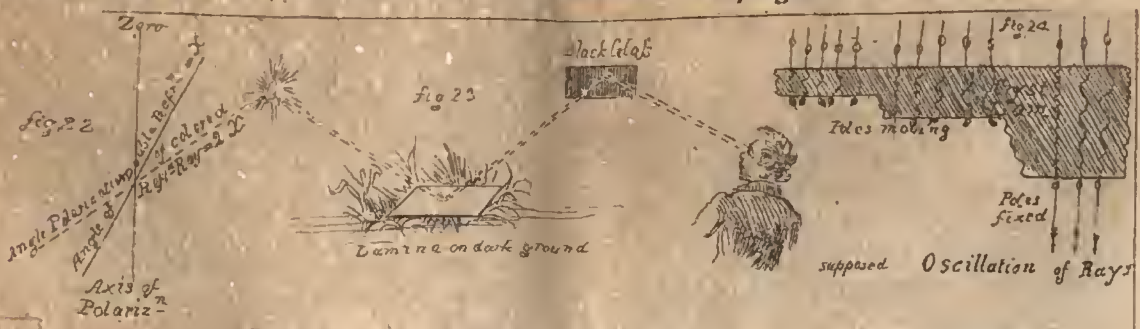
14. The tourmaline demonstrates, moreover, that the greatest portion of light which escapes in all directions from any body by *irregular* reflection, has a contrary polarization to that portion which is regularly reflected. This experiment should be performed in a dark room, by admitting a solar ray upon any substance, (figure 19.)

15. Agate resembles tourmaline somewhat in its action upon light. Brewster discovered that, when thin slices were cut perpendicular to the natural strata, they suffered all light indiscriminately to pass, but gradually as the stone became thicker, all the light was absorbed, but such as was polarized in the direction of the laminæ. In pure tourmaline, no laminæ can be perceived, but their existence is rendered evident by this same optical phenomenon. It resembles what was described in section eight, of the action of a number of glass plates.

16. In sulphate of lime or gypsum, the axis of double refraction forms an angle of 16° 13′, with the shortest side of its parallelogramic crystals, being also at right angles to the axis of crystallization, as are the laminæ of the crystal, which may be cut off to any degree of thinness.



17. The facility of procuring extremely thin plates of gypsum, led Arago to the investigation of a new species or modification, which he called "*moveable polarization*", because in its production, the atoms of light are supposed to be in a state of oscillation. The general fact is, (and it holds good for thin plates of all other crystals,) that when such a lamina is interposed before the second glass of the polarizing instrument, (supposed to be fixed at 90°, or in a position for transmitting the ray of the tube,) then should the crystalline axis of the lamina be brought parallel to the first plane of reflection, no portion of light will be diverted from its previous course, but should the lamina be turned in azimuth (figure 20), a portion of light will instantly be abstracted from transmission, and be reflected from the glass. This effect has been improperly termed *depolarization*, for it appears from examination by the tourmaline or other tests, that the reflected light has not acquired fixed polarization at *right angles* to that transmitted, but that it carries its axis of polarization in an azimuth double of the deviation of the axis of crystallization of the lamina, from the plane of first reflection or zero, figure 22.



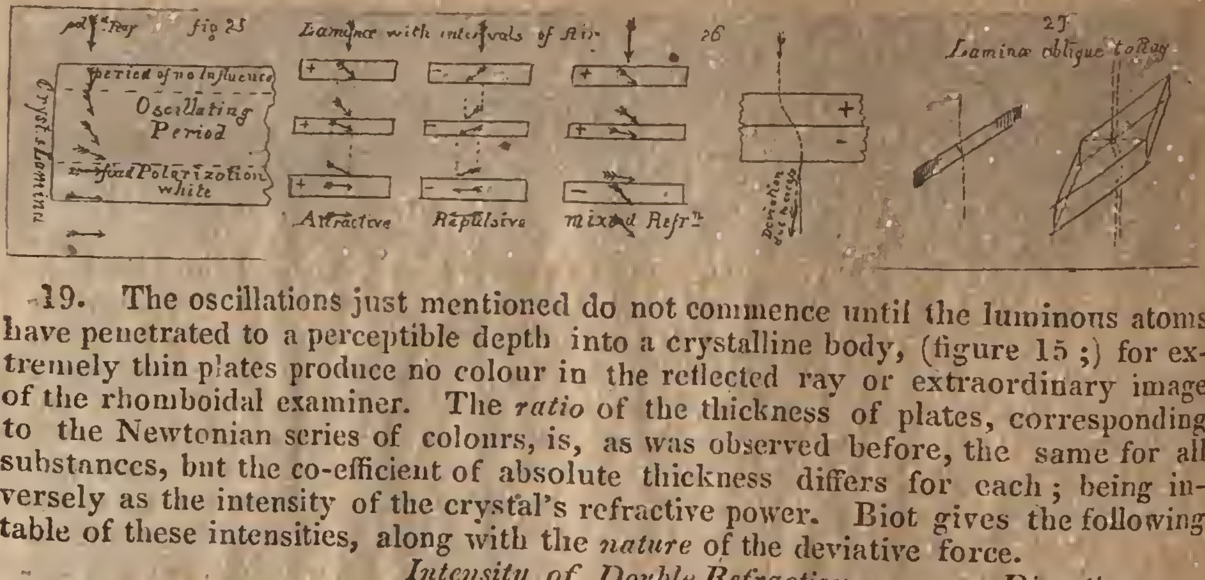
Moreover, while the reflected portion is thus situated, with regard to its poles, the complementary light has its poles turned to an equal angle on the opposite side of the crystalline axis X, or in other words, remains at *zero*, still in a state fit for transmission. When the lamina is very thin, the ray which it diverts is always coloured, and the colour depends on the thinness, the transmitted ray being always of the complementary colour to that reflected. When the lamina is turned round to the azimuth of 45°, the diverted portion has its polar axis at 90°, which, as we have before seen, is the fittest for reflection: accordingly, this azimuth is found to give the maximum intensity of the coloured reflected ray. One mode of shewing it is by laying the lamina horizontally on a dark ground, letting it receive light from white clouds, at an angle of 35°, and placing a black glass so as to *transmit* the rays reflected therefrom:—a portion of the latter will escape transmission, and be reflected by the glass to the spectator, of a brilliant colour due to the dimensions of the lamina (figure 23).

18. The cause of this singular division is attributed to the oscillations, more or less rapid, which the differently coloured atoms of light experience on entering the substance of the crystal (fig. 24), or while subjected to the attractive or repulsive energies of its axis.

If the depth to the lower surface of the lamina (for it is proved that the outer surface has nothing to do with the phenomenon) be insufficient to allow the oscillation to cease, the atom will reissue with its pole in that attitude in which it had just reached the lower surface, and every change of dimension will induce a different series of atoms, or colours, ready to issue *polarized for reflection*, (figure 24.) In fact, the experiments of Biot prove that the colours and dimensions of plates precisely follow the order of the coloured rings observed by Newton, or his "fits of easy transmission and reflection:"

Thus, a plate of ,0000151 inch thick, gives a bright blue, by reflection.
one of ,0000231 yellow green.
one of ,0000136 purple, &c.

The near analogy of these properties, shows the close alliance of all the branches of optics; and suggests the existence of some general law for the relative actions of the molecules of matter and of light, which may at once provide for the cases now distinguished under the several heads of reflection, refraction, dioptrics, chromatics, and polarization.



19. The oscillations just mentioned do not commence until the luminous atoms have penetrated to a perceptible depth into a crystalline body, (figure 15;) for extremely thin plates produce no colour in the reflected ray or extraordinary image of the rhomboidal examiner. The *ratio* of the thickness of plates, corresponding to the Newtonian series of colours, is, as was observed before, the same for all substances, but the co-efficient of absolute thickness differs for each; being inversely as the intensity of the crystal's refractive power. Biot gives the following table of these intensities, along with the *nature* of the deviative force.

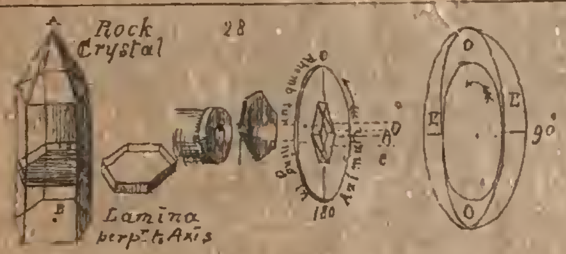
	Intensity of Double Refraction.		*Direction.*
Carbonate of Lime,......	17,75	....	Repulsive
Sulphate of Barytes, ...	1,25	....	Attractive
Sulphate of Strontian, ..	1,07	....	
Sulphate of Lime, ...	1,00	....	———
Rock Crystal,.........	11,00	....	Attractive
Beryl,...............	10,523	....	Attractive
		....	Repulsive

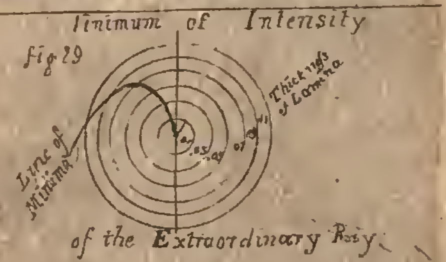
The thickness at which *moveable polarization* has not yet commenced in sulphate of lime and rock crystal, is about ,00046 of an inch.

21. Hitherto, the lamina interposed before the second glass or rhomboid, (fig. 20,) has been supposed to be perpendicular to the axis of the tube, or of the polarized ray. Should the plate now be made to incline from this position, (figure 31,) the colour reflected will be seen to vary according to the order of Newton's rings. In fact, the thickness of the plate goes on increasing as the tangent of the inclination,

and moreover, the reflective forces of the axis of double refraction, which in the first position of the lamina was perpendicular to the ray, now change their direction: but if allowance be made for these two influences, the colour and intensity of the depolarized ray will be found to be guided by the same law as under the perpendicular incidence.

22. Thin plates of rock crystal, cut perpendicular to the axis, (figure 28,) have also the power of depolarizing a portion of light: that is, they produce a coloured *extraordinary image*, (which we will in future designate by E, and the ordinary one by O,) in the rhomboidal eye-piece: the image, as might be expected, does not change when the plate is turned in azimuth, as happened in our foregoing examples, because now the axis of the deflecting force remains parallel to the ray in all azimuths; but if the rhomboidal eye-piece itself be made to perform a revolution in azimuth, the whole series of complementary colours will pass in review in each quadrant:—those belonging to O in the first, becoming after 90° the series of E, and vice versâ. With some specimens of rock crystal, it is necessary to turn the rhomboid from right to left, in others the reverse; and if two equal plates of this contrary disposition be superposed, they entirely neutralize the effect.





23. Again, taking the first quadrant of revolution, the image E varies continually in *brightness*, and its minimum intensity is found at an angle more and more distant from 0°, as the thickness of the plate augments: thus when a lamina is

0,0156	inch thick, the angle of minimum is	9° 45,	and colour blue.
,0190		11 30	ditto.
,0402		25 00	indigo.
,0806		50 00	blue.
,0120		70 00	ditto.
,1357		80 00	indigo.

When the plate is little less than 0,200, the minimum ceases to be observable*.

24. The peculiarities thus briefly described, cannot be explained by the theory of oscillatory polarization, for by that, E at its minimum should always be colourless, inasmuch as the only effect of interposing the plate of rock crystal, while the rhomboidal eye-piece stood at zero, having been to divert a small portion of the blue light from O towards creating a faint image, E, O remaining sensibly colourless, the effect of turning the rhomboid should be to throw more white rays into E, until at 45° it should have been equal to O *plus*, the original blue light abstracted. Nothing of this kind happens: on the contrary, the extraordinary image first decreases in intensity, to a certain angle dependant upon the thickness of the plate, (fig. 29,) and afterwards runs through the descending series of colours, while turning from o to 90°.

It would involve more space than I can spare to explain the theory upon which Biot rests this new property: suffice it to say, that he supposes a secondary force inherent in the crystal acting perpendicularly to its axis, and tending to make the molecules of light rotate on their centres: the violets, being the lightest, will be those which will first undergo depolarization.

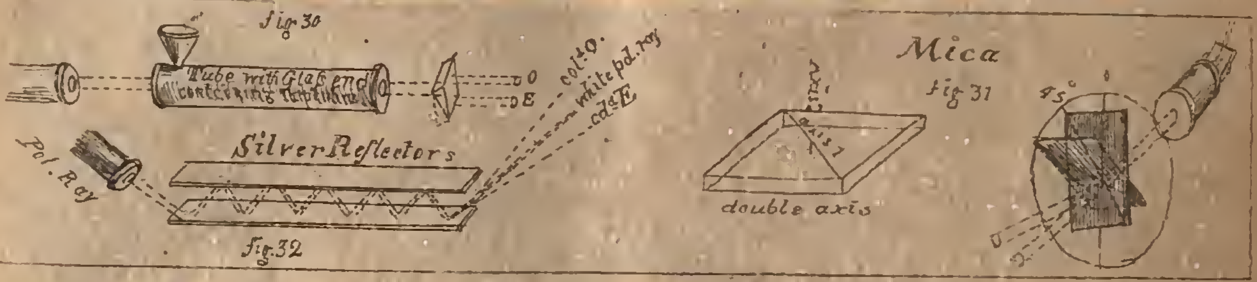
25. This rotative force must be independent of crystallization, since several liquids exercise the same. Turpentine enclosed in a tube, with glass ends, possesses $\frac{1}{80}$th of the power of rock crystal, and gives a series of colours to E, when the

* Some laminæ, made up by myself from Delhi crystal, gives nearly similar results, viz:

thickness	0.032	angle of minimum 16°	colour violet blue, direction from left to right.
	.036	20	
	.062	36	
	.082	52	
the 3 last together	or 0.150	86	
a plate of Beryl,	0.045	5	colour white.

rhomboid is turned from right to left. Essential oil of laurel does the same; but camphor, dissolved in alcohol, induces a series from left to right, so that a mixture of one part turpentine, and three of camphoric solution neutralize one another.

Neither water nor alcohol alone exhibit any depolarization on the rhomboid.



26. Mica presents, under a perpendicular incidence, just the same characters as sulphate of lime, and rock crystal plates cut *parallel* to the axis; but it also exhibits anomalies of its own, affecting all cases of oblique incidence of the polarized ray. These are susceptible of explanation, by allowing to mica *two* polarizing axes; one in the plane of its laminæ, the other at right angles to the first; both being repulsive. The yellow imperfect mica is frequently found without the first axis, and then, if coupled with sulphate of lime, (which possesses only this axis,) it entirely serves to explain the compound nature of the clear mica: and in the same way many other combinations may be made and analyzed.

The imperfect mica to which we have alluded, produces no E image in the rhomboid under any azimuth, unless it be inclined obliquely on the ray, with the azimuth not in the quadrants. When the plane of incidence is at 45° (the laminae being inclined at the same time) the colours of E are most vivid, (fig. 31,) and they disappear when the azimuth is brought to 0° or 90°, which proves that the axis of polarization is perpendicular to the plane of the crystal. Keeping the azimuth at 45°, and inclining the mica gradually on the ray, the whole descending series of Newton's colours appear.

The other, or principal polarizing axis of mica, corresponds with one diagonal of its primitive rhomboid: its influence modifies the colours visible under oblique incidences in a regular manner, increasing up to the angle 35° 11′, and thence retrograding;—but the cases are too complicated to be embraced in this cursory examination.

27. Such bodies as have not their molecules arranged in *a definite manner*, that is, are uncrystallized, as glass, liquids, &c. may be imagined to be compounded of numerous regular systems, whose tendency is to neutralize one another: if on such bodies some disturbing force be elicited to check their natural arrangement, it may follow that some of these systems (or laminæ as it were) may remain unneutralized; and these will then tend to act upon a polarized ray in all respects like a crystalline body. Brewster has paid much attention to this species of research, and has shewn that glass, heated red hot, and suddenly cooled, (whereby its uniformity of aggregation is prevented,) depolarizes a certain portion of coloured light, depending on the form of the glass. Glass, compressed in a vice, does the same; and even jelly, when pressed, shews a slight power of depolarization. Borax, melted, and suddenly cooled repeatedly; salt rapidly dried; and gum arabic, quickly evaporated, also depolarize. Horn, bone, ivory, &c. are sufficiently uniform not to need forcible means: they may be regarded in some measure as crystalline bodies.

28. The polarization induced by reflection from metallic surfaces was considered doubtful, until Biot and Arago succeeded in rendering it evident, by multiplying the reflection of a polarized ray between two polished silver plane mirrors. Arago first ascertained that the tendency of a molecule of light, on approaching a metallic surface, is to arrange its axis of polarization parallel to this surface, in the plane of incidence; while Biot demonstrated, that oscillations of the molecules also took place exceedingly close to the metal, similar to those already noticed within transparent crystalline laminæ. Thus, a ray, after repeated reflection from the silver mirrors, (fig. 32,) yields to the rhomboidal eye-piece a finely coloured E image: and an increased number of reflections acts like an increased thickness of crystalline laminæ, in changing the order of the colour; an augmented angle of incidence produces a minor effect of the same nature.

29. Having alluded frequently to the series of coloured rings, analysed by Sir Isaac Newton, and shewn how the thickness of different plates of mica, &c. may be found from the colour of the depolarized image which they induce, it will be con-

venient to insert a table of Sir Isaac's measurements, and follow it up with an example or two of application.

	Colour reflected.	Thickness of laminæ in millionths of an inch.		
		of air.	of water.	of glass.
Order I.	Very black,	½	0.33	0.31
	Black,	1	0.75	0.61
	Blue,	2.4	1.8	1.5
	White,	5.2	3.8	3.4
	Yellow,	7.1	5.3	4.6
	Orange,	8	6.	5.2
	Red,	9	6.7	5.8
Order II.	Violet,	11.2	8.4	7.2
	Indigo,	12.8	9.7	8.2
	Blue,	14.	10.5	9.
	Green,	15.1	11.3	9.6
	Yellow,	16.3	12.2	10.4
	Orange,	17.2	13.	11.2
	Red,	18.3	13.7	11.7
	Scarlet,	19.6	14.7	12.6
Order III.	Purple,	21	15.7	13.5
	Indigo,	22.1	16.7	14.2
	Blue,	23.4	17.5	15.1
	Green,	25.2	18.9	16.2
	Yellow,	27.2	20.3	17.5
	Red,	29.	21.7	18.7
	Blueish red,	32.	24.	20.6
Order IV.	Blue green,	34	25.5	22.
	Green,	35.4	26.5	22.7
	Yellowish green,	36.	27.	23.2
	Red,	40.3	30.2	26.
Order V.	Greenish blue,	46	34.5	29.6
	Red,	52.5	39.4	34.
Order VI.	Greenish blue,	58.7	44.	38.
	Red,	65.	48.7	42.
Order VII.	Greenish blue,	71	53.2	45.8
	Reddish white,	77	57.7	49.6

Rock crystal, mica and sulphate of lime differ very little from glass in the above scale. To apply this table to practice, suppose we fix upon the following five plates of mica for measurement. We place them succesively in the interposition-circle of our instrument, (figure 33,) bring their axis of crystallization to the azimuth 45° from the plane of first reflection :—then gently incline them to the ray, by turning the index of the horizontal circle below, and note down the series of colours, from which we find the Newtonian colour of the plate when uninclined, and the consequent Newtonian measurement, viz.

No.	Colour of O	Colour of E,	Rising series of E, on inclining the mica	Supposed Newtonian colour	Thickness corresponds.
1	Orange,	Bl. white,	wh. or red. vio. ind. bl	wh. of 1st order	4
2	Bl. white,	Orange wh.	red. pur. bl. gr. or. red, viol. blue.	cr. yell. 1st order	7.7
3	Blue,	Orange,	scr. red, pur. bl. gr. yell. red, blue.	orange, 2d order	27.2
4	Red,	Green,	———	green, 3d order	2.5
5	Yellow,	Indigo,	gr. yell. red, blue, gr. red, green.	indigo of 3d order	22.1

But the last column here only denotes the thickness of a plate of air which would *reflect* the colours displayed by E. To reduce thence the thickness of mica plates *transmitting* the same, the numbers have only to be multiplied, according to Biot, by ,00023, which gives the resnlt in inches. We shall have then

No. 1	=	0.00092	inch.
,, 2	=	0.00254	,,
,, 3	=	0.00396	,,
,, 4	=	0.00575	,,
,, 5	=	0.00508	,,

which agreed as nearly as might be with actual measurement.

30. The measurement of thin crystalline plates is not the only useful purpose to which the polarizing instrument may be practically applied: a more important one is the ready means it affords of finding the axis of double refraction or crystallization in any crystal, a main point in mineralogical crystallography, and in constructing what are called double image micrometers of crystal. It is useful, in a minor way, to detect false gems, without scratching them. It affords useful hints as to the best disposition of glass reflectors: but these are trivial matters; the real point of utility gained by the discovery of polarization, is the knowledge of a fundamental law of light, which goes far to explain the rationale of reflection and refraction—two co-existent effects, which always seemed at variance with one another —an attraction and a repulsion simultaneously at work on the surface of bodies:— it also gives very strong support to the theory of the materiality of light, and confirms all the subtle reasoning of the great philosopher who first analyzed the prism, and pronounced the relative weight, number, and velocity of atoms, which, but for his researehes, would, perhaps, never have been acknowledged to possess a material form or existence.

II,—*Particulars of a Visit to the Siccim Hills, with some account of Dárjíling, a place proposed as the site of a* Sanatarium *or Station of Health. By Captain* J. D. Herbert, *D. S. G.*

[Continued from page 96.]

Dárjíling is on the southern side of a great hollow or basin, being that of the Ringít river, which falls into the Tísta, a few miles east of the place. To the north the view is open, and exhibits the usual succession of range beyond range, all irregularly ramifying in every direction, and in apparently inextricable confusion. It terminates in the snowy range, which is here equally as magnificent an object as to the north-west, and there is some reason to suspect, includes peaks of even greater height than those measured in the surveys of Garhwál and of Kamaún. Unfortunately, during the two days we halted, the weather was unfavorable; a mass of clouds almost continually obscured them, and it was only by an occasional glimpse of a peak that we were enabled to trace out their great extent, or guess at their superior elevation. To the westward, the view is confined by a lofty range at the distance of about 10 miles; intermediately is a low ridge connected with that of Gángla, which is again a part of the Sinchal mountain; on the top of this ridge is the small village of Changtong, separated from Dárjíling by a deep valley. To the eastward appears the valley of the Tísta, the boundary of Siccim and Bután; and on each side of it is the confused assemblage of mountain ridges as to the north. Above the head of the Tísta may be seen the opening of the Féri pass—that, I imagine, by which Captain Turner visited Táshi Lumbú. To the left of it the high peak Chamalári, noticed also by that traveller, is visible; and west of it the highest summit in this quarter, called Kanching-jinga. This is the peak which is mentioned in a communication published in Brewster's Edinburgh Journal, and conjectured to be a volcano[1]. It is said to have been measured, and found to be 27000 feet high.

To the south, Dárjíling has the Sinchul peak, elevated about 9000 feet, and the Gardan-kattar range, which is a ramification of it. These mountains are completely clothed with forest from the top to the very bottom, and owing to consequent sameness of tint and want of break or variety in the surface, they form

[1] I have not been able to learn any of the particulars of the measurement, further than that it was in some degree only approximate, and by no means rigorously exact. It is visible as a very conspicuous object from Dinájpúr, which cannot be less than 150 miles distant in a direct line. This is, in itself, a presumption of great height.

rather sombre features in this landscape, especially in cloudy weather. Dárjíling is, as before mentioned, situated on the shoulder of this great mountain.

The extent of the cleared part of the ridge, the site originally of a Lepcha town, and afterwards of a Gúrkha cantonment, is in a northerly direction about 400 yards. The southern extremity, marked by the small building called Pusputnuth, is a narrow neck of land, having on one side a steep declivity, covered with thick forest; on the other a more gradual one, with the forest open. From this point the hill rises into a broad and almost flat summit, having on it the remains of a *Gumbu* or Lama monastery. The northern and eastern sides sink down precipitously, but to the west and south the declivity is easier. On the western side, there is, at the foot of this summit, a considerable tract of level ground, which passes round from south to north, and at the latter corner throws off a broad and tolerably even topped ridge as a ramification to the westward. On the highest summit, round its western base, and along this ramification, will be found ample room, even for a small town. Water is plentiful and not distant, there being two springs close to the place; and should more be required, some of the innumerable rills, which are found in the higher but connecting range of Sinchal, could easily be conducted in narrow channels along the face of the mountain, as is practised in every part of these hills.

Of the climate it is impossible to speak too favorably. During our stay of two days, 19th and 20th February, the range of the thermometer was 39 to 49°. Both days were cloudy; and doubtless, had it cleared up, the thermometer would have risen higher than 49. But from a single observation of this kind, nothing can be learned of course as to the temperature of the hot months, which would be the period for invalids visiting Dárjíling. We can, however, determine, from knowing the elevation of the place, what would be the difference of temperature between it and Calcutta, as it has been found, by a very extensive induction, that an ascent of about 300 feet occasions a fall in the temperature of the air, amounting to 1° of Fahrenheit's thermometer[2].

The elevation of Dárjíling appears, by a mean of two cotemporaneous observations, to be 7219 feet. Divided by 300, this gives 24° as the difference of temperature between Calcutta and Dárjíling. When the thermometer is at 80° at the former place, it would be 56° at the latter; when 90°, 66°; and in the very rare cases in which it reaches 100° in Calcutta, it would be but 76° at Dárjíling. The latter would then be the highest temperature out of doors: but in a house it could never rise even to 70° in the hottest weather; while during greater part of the hot weather and rains it would not much exceed 60°. Let any dweller in our city of palaces picture to himself the establishment of a cold weather suddenly in the middle of the rains, and he will have some idea of the change in his feelings and health, which a visit to Dárjíling would produce. The lowest temperature ever felt in the house in Calcutta is 62°, and this at Dárjíling would be about the temperature of the hottest season of the year. The following table will put the difference of climate in a clearer point of view.

Comparative Temperatures.	Calcutta.	Dárjíling.
WITHIN DOORS.		
Mean temperature of the year,	78°	54°
Mean temperature of hottest month,	87	63
Mean temperature of coldest month,	65	41
Mean maximum temperature of hottest month,	93	69
Mean minimum temperature of ditto,	81	54
Mean temperature of rainy season, (July, August and September,)	83	59
Maximum temperature, (June,)	95	71
Minimum temperature, (January,)	62	38
OUT OF DOORS.		
Maximum temperature,	101	77
Minimum,	48	24

[2] Mr. H. Atkinson, the author of a very elaborate paper on the theory of Astronomical Refractions, gives as the result of his very extensive induction, $T^{\circ} = H \div n$, in which T is the difference of temperature due to any difference of elevation H, and n a variable divisor, the value of which may always be found, by adding $\frac{1}{200}$ part of the difference of elevation to the constant 251. This would give as the rise due to 1° in the present case 287 feet, and the difference of temperature 25°. *Mem. Astr. Soc. vol.* ii. *p.* 1.

In the cold season it appears then that the temperature would sink below the freezing point. Snow might then be expected to fall; and this agrees with the experience of Captain Lloyd, who visited the spot in 1827, and found snow in the neighbourhood even in February. We did not observe any snow even on heights of 10,000 feet; but it is to be considered that there is a great difference even at home in different years, and the present year began here with an unusually mild winter[3]. Though we saw no snow lying, except on the very elevated ranges in the neighbourhood of the Himalaya, yet we saw it falling on a neighbouring ridge, not much higher than Dárjiling; and indeed from the temperature observed at the latter place, 39°, it is evident that a very trifling fall in the thermometer would have brought snow.

But it is not so much the mere temperature of a mountain station, (though that is a great point,) that renders it so delightful a retreat to the debilitated European, who for twenty years or more has suffered under the fervors of an Indian sun. There is a lightness and a buoyancy in the air, or rather in our spirits, in mountain regions, that to him who has doled away years in the apathetic indolence, inevitably induced by the climate of the plains, and particularly of Calcutta, feels like taking a new lease of life, or rather like passing into a new and superior state of existence. Instead of that listlessness in which we of the city of palaces pass our lives, apparently insensible even to extraordinary stimuli; the dweller in the mountains feels an energy and vigour, a power of exertion and a freshness of feeling, which is not found in the plains even in countries sufficiently cold. This exhilarating effect of the mountain breeze has been often noticed, but never, that I am aware of, satisfactorily accounted for. Perhaps it is the purity of the air,—perhaps the greater dryness, owing to increased evaporation,—perhaps neither the one nor the other. That it is not the lightness of the air, seems pretty clear from a well known fact, that our spirits every where rise with the barometer, i. e. as the air becomes heavier. But whatever be the cause, the fact is certain, and I appeal to those who, after suffering from the heat of the plains, have escaped to our northern sanctuaries, Semla or Landaúr, whether they did not feel renovated in mind and body by the transition. It is alone in mountainous countries that we experience that delightful sensation which renders mere passive existence a high enjoyment.

That the advantages of a residence at Dárjiling will be equally great as at the northern stations of Semla, Landaúr, or Almórah, can admit, I imagine, of no doubt. The elevation being within less than 300 feet of the former, must give it a temperature at all times within 1° of Semla. The latitude is certainly lower by 3°, but it is very doubtful whether the difference in geographical position would amount to so much as the former. To which is to be added, that Semla, having a southern aspect, with nothing to defend it from the heated winds of the plains, would probably, on that account, appear to have even less than this little advantage of climate over Dárjiling, which, facing the north, is well screened by the Gardan-kattar range, situated to the south, and the direction of which is nearly E. W. But this range, as noticed by Captain Lloyd, will act a still more important part in meliorating the climate of the place. For the rising fogs and exhalations of the plains will be checked in their progress northward by the cold air, which must always rest on the summit of this mountain, while the winds will be turned off; so that if there be any thing deleterious in the air of the country at the foot of the hills, it would be neutralized as far as Dárjiling is concerned; being in fact prevented reaching that place by the skreen afforded by this range[4]. The efficacy of a mountain range to modify climate

[3] It appears to be thought by many that snow lies in considerable quantity every year at Sémla; but this is not the case. The difference of different years may be judged of by the following two facts. In the year 1815 snow fell at Nahan elevated 3000 feet, and at Kálsi 2500, and lay deep also on the low sandstone range that bounds the Dehra Doon to the south (2000 to 3500.) In 1819 there was no snow fell on elevations of 9000 feet.

[4] This consideration involves a most serious objection against the only other mountain station in the vicinity of Calcutta; I mean that at Nanklaúl, in the Cásia hills. An extensive tract of low marshy ground, with much of the worst kind of jungle, borders that table land to the north and to the west. When the wind proceeds from that quarter it must bring with it malarious exhalations; and that it does so and inevitably occasions the place to be less healthy than it otherwise would be, is the opinion of those who know and have lived in the country. The difference of elevation between Dárjiling and Nanklaúl is however too great (2200 feet=7° Fah.) to allow of any question being made between them, even if there were not the above great advantage on the side of the former.

is acknowledged; and were it doubtful, the case of the great Himálaya chain would show it most powerfully in the fact, that there is no rainy season to the north of that great belt of elevated land. And I feel disposed to believe, that this great range of Sinchul must have something of a proportionate effect on the tract to the north of it, and the rainy season would be much less violent than at Semla and at Landour, open to the full blast of the great congregation of vapours swept from the plains. And even supposing the question of malarious exhalations to be worth nothing, it would still be certain that Dárjíling would have the advantage of those places as a residence in the rainy season, at which time they are exceedingly dreary.

The relative temperatures of these several stations is best learned by comparing their elevations, a very trifling allowance being due to those in the north-western mountains, as before remarked, for their higher latitude[5], and perhaps to Dárjíling for its northern aspect. Keeping in mind that 300 feet in elevation is equivalent to 1° of temperature, we may by a glance at the following table, obtain a perfect idea of the differences of these places, with reference to temperature. We may see by it that Semla is nearly 1° colder than Dárjíling; Landaúr, or rather Masúri, 2° hotter; and Almórah 6° hotter.

Semla.	Dárjíling.	Masúri[6].	Almórah.
7486	7218	6500	5520

As a locality for a Sanatarium, Dárjíling has then many claims on our notice. Temperate climate, a sufficiency of level ground, a sufficiency of water, in which it has the advantage of Semla and Landaúr, and of building materials, as far, at least, as stone and timber are concerned. In the deficiency of limestone it is no worse than were Semla and Almóra when first established. And that a little careful research will succeed as it did at those places, in detecting the mineral at no great distance, appears to me very probable, from the fact of the grey-wacke formation (with which the limestone is associated) being found in the vicinity. That we did not discover it, is not even a presumption against its existence, for we had not the means to do justice to the enquiry, being obliged to hurry through the country as fast as we could, and prevented going off the particular line of route we had chosen to follow. In the article of rides and walks Dárjíling offers great advantages. Connected with a lofty range, which throws out its ramifications in every direction, a level road of any desirable extent may be cut with little trouble. And though the immediate spot itself be inferior in romantic beauty to Semla or Landaúr, it has many beautiful places in its vicinity. The forest scenery on the Gardan-kattar range is very magnificent, and the descent to Ging, which is on the same ridge with Dárjíling, is a very picturesque ride. No place can boast of a more extensive view of the snowy range, if only on account of the peculiarity of the position, as will be evident by considering the description before given. Accordingly nearly a third of the horizon is occupied with these lofty pinnacles, some of which are considered, and not without reason, amongst the most elevated points of this stupendous chain.

The ground is sufficiently cleared to allow of building being immediately commenced on, little more being requisite than burning down some jungle grass that has grown up rather luxuriantly. But in the construction of a road to the place, some assistance would be required from the people of the country; the present road, as I have already stated, being utterly useless as a means of convenient or even regular communication. The line which appeared, as far as we could see, eligible, is the greater part of it through thick forest, the clearing of which would be the principal part of the work. In effecting this object, the co-operation of the people of the country would be very useful. They are a hardy and athletic race, and would be glad to join us if permitted.

They are at present living within the Gúrka territory, whither they fled to avoid the tyranny and oppression of the Raja of their country. But they are not satisfied with their position. The Gúrkas being rigid Hindús, they find themselves subject to various disagreeable prohibitions; and are made to feel, in fact, that they are what the knavish brahmin calls outcasts. Free from every sort of injurious prejudice and absurd restriction themselves, they cannot but feel the yoke of a bigotted and superstitious race, who seem imbued with all the worst spirit of the Hindú system.

[5] Dárjíling is in 27°, Almórah in 29°. 30′, Landaúr in 30° 20′, and Semla in 30° 40′.

[6] I cannot refer to the elevation of Landaúr; but it is, if any, very little above that of Masúri.

They are, therefore, very anxious to return to their country, which is in this neighbourhood, and have been lately making every effort to obtain the permission and guarantee of our Government to that effect. The Gúrkas, however, are aware of their value, and endeavour to prevent any communication with us.

There are said to be 1200 of them in the Gúrka territory under their chief Eclátoc, whose brother Barajít was murdered by the Siccim Raja in the most treacherous manner. Barajít's wife and children were also put to death, as they fell into his hands, with the exception of one son, whom the Raja has spared, but keeps in an honorable confinement. By means of this young man he is endeavourng to inveigle his stray subjects back, but much as they dislike the Gúrkas, they will not venture to trust him without our guarantee. It is not that they are afraid of him, but of us; for were they certain of our indifference to their squabbles, they could drive him out of the country to-morrow, as their numbers exceed those of his party. The Raja's tyranny and injustice has the additional stain of the basest ingratitude, for at the time that the Gúrkas took possession of his country, he owed his safety as well as subsistence for many years to this very man, whom he afterwards so treacherously murdered. The absence of the people who belonged to this part of the country for so many years, has occasioned it to become a perfect wilderness; and even Dárjíling itself, once the site of a flourishing town, will ere long lose all trace of its former state. The establishment of a *Sanatarium* there, connected with the recal of the people, would however soon give a very different aspect to affairs; and I should not despair to see, in a few years, the bazár and *Bania's* shops, which it once boasted. At present, the only traces of its having ever been inhabited, besides the extent of cleared ground, are the remains of a *Gumbu* or Lama monastery on the summit, and of *cázi* Barajít's house on the even strip below to the west.

The geology of this country is that of the north-western mountains. In the last three stages gneiss, of ordinary character, was the only rock observed. Owing to the thick coating of vegetation however, the rock is very seldom visible, and never to any extent. The chief difference in the arrangement of these mountains, and those between the Satluj and Káli, appears to lie in the small developement of the sandstone formation in this quarter, and the absence altogether, in the route we had followed, of the clay slate. These circumstances, with the prevalence of gneiss, seemed to me additional reasons for doubting that any thing like the true shale of the coal formation had been found in this quarter, as stated in the Geological Transactions. That the specimens of coal, found by Mr. Scott in the beds of the Sabac and Tísta rivers, belonged to the same class as those so common in our sandstone to the north-west, I had always been inclined to believe, and to infer, consequently, that they were entirely unconnected with the true coal formation, notwithstanding the use of the term *shale* in the paper above referred to. The little insight which our journey so far had afforded me, confirmed me in this view; and I was now chiefly anxious to see the places described by Mr. Scott, more for my own information and satisfaction, than as having any doubt of the conclusions I had arrived at. I wished, in fact, to examine this so called shale *in situ*, and to compare the sandstone with which it was associated with that which I had studied in the north-western mountains, and which I supposed equivalent with the *newer red sandstone* of Europe, and consequently to *overlie* that formation in which coal is found.

With a geology absolutely identical, and a climate the same in every respect, whether of temperature or arrangement of seasons, it was a subject of surprise to me to find so great a difference in the forest features of the country. Of the five species of pines found in the north-western mountains, not one is here visible,—a deficiency which is particularly striking on entering the hills, the lower ranges to the north-west being literally covered with the *Pinus longifolia*, or where it is wanting, in the lower sandstone hills, being seen within the first 10 miles in considerable numbers. Of the *Pinus Deodara*, the king of the forest tribes, we could neither see nor learn any thing. There is the same deficiency of oaks, a genus of which there are six species in the north-western mountains, and of which I only saw one individual in our journey. The character of the landscape, which in those mountains depends chiefly on these trees, is, it may be supposed, quite different. Of the three species of *Rhododendron* found there, not one was seen by us in Siccim. Most of the trees I saw were new to me; the most remarkable exceptions were the wild date, the wild plantain, the tree fern, the rattan, and a reed, the name of which I do not know, growing about 30 feet high, seldom so thick as the wrist at the base, forming an excellent material for mats, and growing also in the north-west, where it is used for the same purpose. The bamboo is found in great perfection in these hills, and at a greater height than I should have supposed it could bear. It grows much thicker than the bamboo of Bengal, though not equal to the enormous bamboo of Martaban. The Lepchas, however, find their bamboo large enough to serve them instead of jars to keep a supply of water in their houses. They cut them into lengths of about five feet, and cut away, or otherwise remove the partitions at the knots. Such a bamboo will hold about three gallons. The shrubs and herbaceous plants seem to have a greater resemblance than the trees to the productions of the other mountains. The several species of *Rubus* are found the same, amongst which I particularly remarked a herbaceous one, with shoots like the strawberry, which bears a raspberry of very tolerable flavour, and which, I am persuaded, would improve by cultivation. The wild strawberry is also found, from which I would infer that this fruit might be brought, by cultivation, to great perfection here ; judging at least from the excellent specimens which are every year produced in the plains,—a climate certainly less congenial to it than one where it grows wild. Of flowers I only saw the violet, and one or two unimportant species ; but the season was not sufficiently advanced to entitle us to expect to see any There is the same variety of ferns, mosses, lichens, and fungus, that we have to the northwest ; and in this department of botany my fellow traveller made an excellent collection.

Our two days,—to which period we had limited our stay,—soon passed away, and, to our great disappointment, without any improvement in the cloudy state of the atmosphere. I was unable to determine even the latitude of the place, though provided with the means ; and the arrangements I had made for settling its longitude by chronometer became nugatory. What was still more mortifying, we could not get a fair view of the snowy range, or even of the high peak Kanching-jinga, so as to take an azimuth and altitude, which with similar observations in the plains, combined with latitudes and the elevations of the two places by barometer, would have given the means of fixing the position and elevation of the peak within sufficiently narrow limits. The only observations made, were those of the barometer and of temperature. The former stood, on the 19th February, at 4 P. M. 23,056, Therm. 49,7 ; and on the 20th, at 4. P. M. 23,134 att. th. 47 det. th. 46. These observations being calculated, give 7134 and 2294 as the height of Dárjíling; the mean is 7218, which cannot be very erroneous. The temperature, by a register thermometer, was each day minimum 39°, maximum 49°.

On the 21st, finding the weather still unpromising, we determined to descend. With some difficulty we got a sufficient number of porters, for those we had brought from Samdong had there stipulated that they should be free to return from Dárjíling. Some of them, however, were induced to accompany us, and the full number was made up by a few that had come in from the neighbourhood. It had been thought adviseable that we should return by another route, in order that we might be prepared to say which was preferable. The route by the Sabbak pass, near the *debouche* of the Tista river, was said to be the best, and by that we accordingly determined to return.

Our first march was to Takdak, a small hamlet, the residence of a Lama, situated in a north-easterly direction from Dárjíling, and on the declivity of the Gardan-kattar range. The first six miles was a very easy descent, the road excellent, and the scenery far superior to any thing we had yet seen. The road was evidently a made one as far as Ging (about four miles ;) and so broad, and of so easy a descent, as to render this part of our march most agreeable. At Ging there is a small square building, surmounted by a pyramidal top, and called Ging-chûten or Paspatnath, but no other trace of the village which was once here. Two miles beyond Ging, the road which had led down the crest or back of the ridge, turns to the southward to descend to the bottom of the glen which separates the Dárjíling ridge from that of Gardan-kattur. This part of the road is at first tolerable, but gradually gets worse, and finishes with so steep and difficult a descent, that excepting there was little or no danger, I scarcely ever saw a worse. The first part was well cleared, and was indeed altogether such a road as a person would travel for pleasure ; but the latter part was through a thick jungle, in which the long and luxuriant grass was particularly the source of much annoyance and difficulty. As we got near the bottom the heat was quite dreadful, we having left Dárjíling about half past 9, and, therefore, had the hottest part of the day to get over the worst part of the road. Great was the satisfaction with which we at length descried the beautiful stream that ran at the bottom of this most fatiguing descent, which had occupied us an hour and three quarters. The mere sight of the water, and the shady and sequestered spots that bordered it, soon made us forget our recent toil, while the clear and sparkling fluid was in its refreshing coolness doubly welcome to our thirsty and parched lips.

From this stream, which is 5000 feet below Dárjíling, the road ascends to Takdad, about 1000 feet, and is far from good. The distance is not above two miles; when the proximity of the village is indicated by the improvement of the road and the increase of open and level ground. The village spring, with its rude spout of wood, was next passed; and here I had the first opportunity of seeing one of their beautiful breed of cows, far superior to any I had ever seen in the north-western mountains, and indeed only inferior to our English animal. Immediately after, we found ourselves at the village; one of the most comfortable houses of which, was assigned as our residence.

The Lama, we were told, was prevented paying his respects by illness; and we were so fatigued by our long and difficult march, that we were very glad to excuse him. We arrived late in the evening, and were glad, after being on our legs all day nearly, to have a little rest.

The following morning the weather was still as cloudy as ever, but towards 8 o'clock the atmosphere began to clear up, and we thought we should at last see the snowy peaks while yet not at too great a distance. But the hope was delusive: they appeared at intervals, and so imperfectly as only the more to excite our curiosity to see them in their full and unclouded glory. Of four of them I was able to observe the bearings, but only of one the altitude, and even of this imperfectly, as before it could be properly taken, the peak was again covered with cloud. The bearings were as follows:

No. 1.	Broad topped mountain,	310°	15′
		341	00
No. 2.	Kanching-jinga,	345	15
No. 3.	Sharp peak, hollow to left,	347	30 alt. 5°.22′
No. 4.	Chamalári,	353	50
	Feri Pass, the head of the Tísta,	1	
	Sulukfok, bare near peak, no snow,	9	
	Dárjíling,	235	
	Ging,	255	

Finding that the weather had no appearance of clearing, we were obliged to leave Takdak. Mr. Grant, however, went previously to visit the sick Lama, who, it appeared, had broken his arm. He had evidently attempted to set it, as it was found bound up with splints; nor did he wish to have it examined: but he expressed a desire for some medicine, which was furnished him. He appeared to know something of medicine, and perhaps of elemental surgery. He was intelligent and superior in his manner to any of the people we had yet met with. All our followers, as well as his own, seemed to treat him with great reverence and respect. Turner mentions the great influence which those of his class possess. They are not, however, a distinct caste; for of any such division or distinction these people have no notion. The Lamas are taken indifferently from every class,—at least in Thibet; and are educated to fit them for the duties they have to perform. Like the priests of the Roman Catholic Church, they make a vow of chastity; and this attempt to shake off the feelings of human nature, is, as in other unenlightened countries, repaid with the credit of great sanctity. Much of their influence with these people is derived from the belief that they have power over the evil spirits of the country; for here, as in other mountainous districts, we find superstition people each wild spot with its peculiar demon. Yet the Lepchas have more reason, and even philosophy, in their superstition, than might at first seem compatible with this offspring of ignorance and mental darkness. The *kelpie* or *bhút* of Siccim inhabits the deep glens and narrow vallies, the tracts of dank and luxuriant vegetation. His anger is shown by visiting his victim with an intermittent fever; so that he is, in fact, not a mere *ens rationis*, but may be considered rather to be the embodied spirit of *malaria*. This spirit, it is supposed, the Lamas have the power of conjuring far away,—possibly into the red sea; and such is the confidence of this people, that they never seem to enquire whether there is a *bhút* (spirit) in such a locality, but whether a Lama resides there. The latter, it is supposed, excludes the former. Of the existence of the *bhút* they are perfectly convinced; nor do they allow his invisibility to be any argument against their belief. The death of several people in a village from fever, is considered to be quite sufficient evidence of there being something there that ought not to be; and their philosophy is satisfied with the explanation which the Lamas give of the matter.

22d. On leaving Takdak, the road leads up the face of the Gardan-kattur range, and is steep, but not difficult. It occupied us three hours, not including halts. On this mountain is a fine field for a botanist, the whole of it being covered with thick forest, in which appears a great variety of productions. The top of the ridge is broad and quite flat: it would furnish an excellent site for an experimental garden; and as it is connected with the mountain, over which the road to Dárjíling would pass, a branch road could easily be made to it. The soil, as in all mountain forests, is a rich vegetable mould. The elevation is 6600 feet, or about 600 feet lower than Dárjíling. If the latter place should be found to be deficient in even ground, the top of this range, which extends several miles in length, and is upwards of 200 yards in width, would afford ample. A road might be easily cut along the top of this ridge to lead round the head of the glen to Dárjíling; and as the forest scenery on it is superb, such a road would afford a fine ride to the invalids at that place. It would extend several miles, and be almost level the whole way.

From the summit we had an easy descent of 20 minutes to an open spot on the declivity of the range, whence we had a view of the plains, but dim and indistinct, owing to the unfavourable state of the atmosphere. The stream of the Tista was distinguished flowing to S. 40° E. Caliámpung, a fort in Támsang, a district of Bhután, or the country of the *Dherma Rája*, was pointed out to us: it bore N. 92° E. We sat down here on a grassy bank, and idled away a half hour pleasantly enough, gazing on the various features of the scene spread out before us, remarking particularly how very different a country it looked from that through which we found our way to Dárjíling. An hour's further descent brought us to a part of the ridge where we observed some substantial huts, as well as various signs of a vigorous system of clearing being in operation. Here we expected to halt, but owing to some objection, which we could not very well understand, they took us on about half an hour's farther walk, where, just below the crest of the ridge along which our descent had latterly lain, we found a most comfortable and substantial farm-house, the best half of which was given up for our accommodation, while the family retired to the other. The day was cloudy and bleak; and notwithstanding our warm clothing and the annoyance of the smoke, we were glad to light a fire in our room. The people of the house very sociably joined our fireside, and took the opportunity of contemplating us at their leisure.

Here we began to fear our progress would terminate,—at least for some days; the porters who had come on from Sámdong positively refusing to proceed any farther. The delay threatened to involve us in very serious inconvenience, as we had been obliged to leave Sámdong but ill provided either with clothes or food, and the articles left behind had not, as promised, been forwarded. After much discussion, we at length agreed to give them a day to collect porters, and if not forthcoming by that time, that the remainder of the Sámdong party must go on with us. It was now we began to feel how little we were indebted to the *Rája* or his arrangements for the progress we had hitherto made; and it began to be a subject of regret, that we had not furnished ourselves with some more pressing introduction to him than that we had received. We had no idea that we should get away under several day's detention, for we knew not where the new hands were to come from, the country appearing to us quite deserted; and as to those who had accompanied us, though they were well satisfied with the treatment they had received, and though to induce them to exert themselves on the occasion, we told them they must go on, failing the relief, yet it is very doubtful I think if they would have stirred a step further. There was, however, no help for it, and we were obliged to content ourselves with repeated injunctions to have the new men ready for the 24th.

The following day part of our difficulties were removed by the arrival of the baggage left behind at Sámdong, and the gloom, for it was a miserable rainy day, was further dispelled by the arrival of a dawk. Upon the whole, our day passed off better than we had expected it would, and by the bustle of new arrivals towards evening, we guessed we should be able to move the following day. Two pigs were given by Mr. Grant to our host, or rather hostess, for it was a woman who appeared to be the head of the family and mistress of the mansion. I mention the circumstance, for the purpose of noting their method of slaughtering animals. They were shot with an arrow, and so skilful was the archer, and so powerful his bow, that the same arrow sufficed for both. I could scarcely have believed that an arrow would pass through a pig's body with sufficient force to kill a second animal standing close to him. Some other good things were added, particularly a bottle or two of brandy, of which these people are immoderately fond.

On the 24th we found all in readiness for our proceeding,—or at least were told so; and we accordingly started after breakfast. Our route lay along the crest of the ridge with very little descent, till we came to a solitary farm house. Here we found we were on the edge of a steep and most fatiguing and tedious descent; and it was represented that as the baggage was all in the rear, and the day now pretty well advanced, it was doubtful whether the porters would reach the foot of the descent where we were to encamp, by night-fall. On further questioning them, it appeared that the new recruits were only half the number we had demanded, and that they had kept this circumstance to themselves, expecting to be able to return for a second load in sufficient time. Finding, however, that this was not the case, they proposed we should halt here, by which means they would be able to effect this arrangement. We were vexed at this unlooked-for delay, though we could not be angry with the people, not only from the motive of their silence, which was to avoid troubling us, but also from the good will they had manifested in wishing to work double tides. But as complaint and vexation were equally unavailing, we were compelled to halt, at least till the people had returned for the extra loads, when we thought we could by a better arrangement, and by dispensing altogether with the most bulky and useless part of the baggage, manage to proceed the following day with even our diminished number of porters. We therefore gave orders for a halt, and sent back the men as they arrived to bring up the several articles left behind.

The only house at the place which was comfortable, though not large, was tenanted by a very interesting family; and short as our intercourse was with them, they established such favorable impressions as not to be soon forgotten. The *gudeman* was out when we arrived, but his wife welcomed us equally cordially. The principal room was soon cleared out for us, and the two young boys set to work to light the fire. Of these one was lame, from some hurt in his thigh, and though evidently, from his countenance, occasionally in great pain, yet shewed more equanimity and fortitude than I should have expected from so young a child. We did not observe the slightest display of pettishness or fretfulness. The other, who was younger, was a funny little fellow, and often making us laugh at his strange comical ways, we nicknamed him *Scaramouch*. At the same time the rest of the family were busy in making room for us, our host's sister was sent to the spring with one of the large bamboos I have mentioned, to bring water. Another was busy pounding rice, assisted by the oldest of the children, a little girl about eight or nine years of age. This latter, though, like the other two, far from regular featured, had yet a very expressive and prepossessing countenance, and her behaviour fully answered to her looks. We tried to recommend ourselves to the children at dinner by offering them a biscuit, glass of wine, &c. but whatever we gave was immediately carried off to the mother, whose permission was always thought necessary; or perhaps they wished to share these gifts with her. The behaviour of these children, without being so riotous as those of Europeans, was extremely natural and interesting, forming a most striking contrast to any thing we had ever observed of children in the plains. That they were grateful for the notice we took of them, was evident on our departure the next morning, as they stood looking after us with very serious countenances as long as we could be seen. Little *Scaramouch*, in particular, seemed very much to regret our departure; and few as were the links of sympathy between us and these rude and neglected people, yet such is the charm of natural goodness and simplicity of feeling that I really believe the regret was mutual.

These people are all rather square built; some so much so as to be clumsy. This is often the case with the women. Of the two sisters of our host, one was about as broad as she was long. She was really a bouncer, and would have formed a fine contrast to one of our modern wasp-shaped belles, equally removed from symmetry though on the other side. The other sister was not quite so much in the Dutch style, though still far from possessing the form described as "fine by degrees and beautifully less." But she had a very expressive countenance,—one the farthest possible removed from that of a Hindustaní beauty[1], yet one which, every time it was seen, would be thought to improve. There was a mingled air of sweetness and gravity, which gave a charm to features that, taken singly, were, perhaps, every one of them, faulty. It was, in fact, a European face, and one of much meaning. This girl came the nearest to what I may call a Lepcha beauty of all that we saw. She was betrothed to one of the young men who accompanied us, who appeared every way worthy of her. He was a fine active good

[1] The Hindustaní, in all that regards form and feature, is a Greek; only with a darker skin. I remember an engraving of a Greek girl, which every one who saw it mistook for a Hindustaní.

humoured and intelligent young fellow, and good looking withal. They had been betrothed many years, and had no immediate prospect of being married, inasmuch as he had not yet made up the present, which it is usual for the parent, or he who stands *in loco parentis*, to exact from the suitor. In fact, in the enquiries suggested by the interest we took in this woman's history, it was completely established that they *buy* their wives. The price of the article in question, which it is evident was rather above par, was 100 rupees. Mr. Grant, desirous of assisting the lovers, asked how much yet remained to be paid, and was told 40 rupees; but to his offer to advance the money, it was answered, that though not paid, the lover had collected the sum, and that the wedding would now take place immediately. Apparently there was some feeling of delicacy that interfered with the acceptance of the offer; nor will those who have seen the people, deem it chimerical to ascribe such and even greater delicacy of feeling to them.

During the betrothment the lovers have every facility of meeting, which is a politic measure, inasmuch as it must tend to hasten the period of the payment. They do not, apparently, like the term *buy* being applied to this singular arrangement. In fact, they pretend it is merely a present to cover the expenses which the guardian is subject to, both in providing the marriage feast, and in endowing the bride with her proper share of goods and chattels. They appear to have learned, in their intercourse with the plains, that it is a custom confined to themselves; and having been, I suppose, rallied on the subject, they try to hide the real nature of the transaction from themselves, or at least to disguise it. It is worthy of remark, that the same custom, with many others, probably borrowed from Thibet, is to be found in our north-western mountains, though Hinduism is fully established there. Of these the most singular is Polyandry. It would be a curious inquiry to ascertain how women came to have such opposite relations amongst these people to what they have in every other nation: having money paid for them instead of conveying dower to the husband, and the allowance of several husbands to one wife, instead of, as elsewhere, to one husband several wives. Doubtless these national discrepancies had their origin in some peculiarity of situation or history, which it might be worth tracing.

The following morning, having previously reduced the baggage to the lowest possible compass, we left Gyal, and immediately commenced the steep descent of the ridge. The road was very bad, and in some places not even quite safe, so precipitous was it. But for the trees which conceal the danger, it would perhaps, to many, appear impassable. Certainly it could never be made a good road for general travelling. The approach, therefore, to Dárjíling by the Tísta side, was no longer a question. Towards the foot of the descent the heat became very oppressive, and we were delighted at last to find ourselves in the river bed, and a beautiful natural basin of great extent and depth, as smooth as a millpond, and with sandy bottom, offering us the great refreshment of the bath—a refreshment which, in these mountains, is almost always within the reach of the heated and jaded traveller. Our camp was only about a mile beyond this beautiful spot, on the bank of the river, but in the middle of a thick jungle, the elevation being little beyond that of the plains. We had as usual a wigwam to sleep in, but preferred spreading our table for dinner on the fine level and gravelly beach of the river, with no other canopy but that of the sky. At night we trusted to a blazing fire and two Lepcha sentries, to keep off wild animals, if there were any. It is probable there are tigers.

On the 26th we marched, and there being no village, were obliged to bivouac as the preceding day, in the jungle. Our route at first ascended the lofty ridge, which here shuts in the river valley, and then pursued its course along the face of a higher range, passing round the several shorter glens or ravines by which it was intersected. Of rocks we had hitherto only met with gneiss, but here the grey-wacke slate began to prevail. We did not see any that was likely to be useful for roofing; nor did we, as I expected, meet with any limestone. But as our researches were confined to the immediate line of route, it is not the less likely to be found associated with this rock as to the north-west. Our route gradually descended, till we halted in the bed of a stream at a place called, Salam-góla.

While on the road, Mr. Grant received two musical boxes that he had ordered from Calcutta, when leaving Malda, and which he had intended as a present to the *Rája*. Highly as we thought of the intelligence of these people, and great as we had observed their curiosity to be, the interest and admiration expressed by them on hearing these toys in action, exceeded what we had anticipated. At first they stood and listened in breathless admiration, not one of them venturing to speak. At last one of the most enthusiastic burst out into a loud laugh of wonder and delight. He threw himself on the ground, and appeared quite in an ecstacy of enjoyment; nor were the others much less affected. We could not but contrast the natural and unsophisticated behaviour of these wild mountaineers, with what would have been that of any number of Hindustanís, whether high or low, under the same circumstances. After the first edge of wonder was worn off, they began to look more closely at the box. One of them, who seemed more intelligent than the rest, undertook to explain the matter, as he understood it, to his less clever companions; and to judge by their countenances, (for we did not understand what passed between them,) the lecturer must have acquitted himself pretty well. But the discovery of the cause of the music did not abate the pleasure they took in listening to it. At meals, when we generally had one of the boxes playing, or when we stopped on the march to rest, as soon as ever the silver tones of these beautiful little toys were heard, there was a group of most attentive listeners assembled round us. Nor would one of them leave us as long as the box continued to play.

The following morning we marched for Sabac-góla, which was to be the term of our mountain travels, the place being situated, as we understood, at the gorge of the Sabac pass or river, where it quits the hills to join the Tísta. The route was, the first half, a rather steep descent to the river bed; the remaining half was easier, being in the bed of the river, and consequently almost level. Sandstone began to be observed in the descent, but in the river bed it became fully established; enormous strata of this rock appearing to compose the huge walls, many thousand feet in height, which composed the banks of this river. The first glance at this rock showed that it was the same I had supposed, and confirmed my opinion as to the little value of the coal that had been found in it. As we advanced, the specimens of coal began to show themselves, evidently mineralized logs of wood, their nature being perfectly evident, as viewed in their native sites, though in the only specimens we could detach it be rather obscure, most of them, though bituminised and of a black colour, have yet a nearer resemblance to stone than coal, being fully as heavy and not less hard. Sometimes these kernels, as they may generally be called, are of a grey colour, and look like indurated mud. This latter substance, when in great quantity, assumes the form of a vein; and in this case its substance is fissured in every direction. This is what appears to be called *slate clay* in the Geological Transactions; though it is certainly not slaty in its structure. I would as little think of calling the blacker varieties *bituminous shale.* But not to dispute about names—it may be sufficient to say, that the substance in question is not the bituminous shale of the true coal formation;—that, on the contrary, the sandstone in question is, if not the *newer red sandstone*, one still more recent; and that there appears to me no prospect of discovering coal in this neighbourhood;—I mean in any thing like profitable quantity.

In the evening we went on the elephant to visit the pass or debouche of the Tista. Just before leaving the hills, it collects itself into a smooth and level sheet of water, more resembling a lake, from its great breadth, than a river. The mountains, thickly wooded on each side down to the river's edge, add to the deception; and on first coming in sight I could not be persuaded that it was not a mountain lake. From this beautiful and calm expanse it precipitates itself at one corner by a rapid, which I found it difficult to believe had ever been ascended by a canoe. Below it, about half a mile, there is a second, after which the river, though still having a strong current, is, I should think, navigable. Its breadth here is about eighty or ninety yards; its depth probably ten or twelve feet. On the extensive sands forming its shore, particularly near the patches or islands of jungle grass, we saw numerous impressions of tiger's feet; and returning home, we heard the deer calling in every direction.

On the 28th we marched to Silgúri, about five or six miles through an open forest, in which the elephant had no difficulty in making progress. The remainder, about ten miles, was through a well cleared, high, and latterly cultivated country, the Mahanundee being to our right and at no great distance after emerging from the forest. The following day we marched to Phánsi-déwa, through a well peopled and well cultivated district. On the 29th we reached Titalía, and took up our residence in one of the bungalows there. By evening the dawk bearers arrived, and we left Titalía for Dinajpúr the following morning early. At Dinajpúr I saw the hill raspberry, in Mr. Ellerton's garden, raised from seeds communicated by Mr. Grant. The plant was exceedingly thriving, and would, I should conclude, bear fruit this year, or at furthest the next. From Dinajpúr we proceeded to Malda, whence I came on by Berhampore and Kishenagur, and arrived in Calcutta on the 8th of March, having been just thirty days absent.

III.—*Further Remarks on the Property of enduring Drought, and the carnivorous Propensities of a Species of* Paludina.

In the 363rd and following pages of the 1st vol. of the GLEANINGS, I related some facts connected with the power of enduring drought, possessed by a small species of *Paludina*[1], and ventured several suppositions to account for the appearances observed. I have now the pleasure to offer subsequent observations, continued through several months, illustrating the subject, and if not absolutely confirming, at least strongly supporting one of my conjectures. I shall relate my experiments in the order in which they occurred.

On the 17th September, 1829, I observed, that out of seven *Paludinæ* procured from the pool formerly mentioned, and left in a tumbler of water, one had fixed itself to the side of the glass by the outer surface of the operculum, which was perfectly closed: in which situation it was left dry by the evaporation and fall of the water. The animal having, apparently, selected this situation of its own accord, I thought it a good opportunity of trying whether it would outlive the exposure for a short time; and accordingly poured off the remainder of the water, leaving the shell adhering to the glass in the situation which it had chosen. On the 24th September I poured water into the tumbler until the shell was immersed, and, on examining it three hours after, had the pleasure to find the animal alive and vigorous, and crawling about the tumbler. During the interval, the weather was perfectly dry, and on the last day, the wind was quite hot, in consequence of the unusual drought.

On the 4th October I took four of the specimens above mentioned, and placed them in a glass vessel, covering them to the depth of one inch with earth, which I wetted to make it adhere firmly. The vessel was nailed up in a small box, and was carried on a hackery, exposed to the sun, to a distance of 40 miles, when it was placed in a godown and neglected for some days. On the 1st November I took up all the specimens and found the earth so dry that it was capable of being reduced to powder between the fingers. No rain had fallen in the interval. I placed two of the shells in a tumbler of water, and enclosed the other two in earth as before. In the course of twenty-five minutes I had the satisfaction of finding one of the *Paludinæ* crawling about in the water, as if nothing unusual had occurred, and shortly after the other followed its example. This I considered almost an *experimentum crucis*, but resolved on still further trying the powers of the remaining two animals.

On the 17th January, 1830, three months and a half having elapsed since the two remaining shells had been deprived of water, I immersed them in their native element: one shell immediately rose to the surface, the animal having died, and having been completely desiccated and withdrawn into the apex of the shell: but the other shell appearing to be still filled by the animal, I continued to keep it separate in water, although with faint hopes of seeing it revive. However, as its former companions, which had been kept in water since their release, were then torpid, probably in consequence of the coldness of the weather, I thought a chance was left and changed the water at intervals. On the 3rd February the weather having become warm, in consequence of the continued drought since August, I was surprized, a few minutes after renewing the water, to observe the little *Paludina* moving about, after a torpidity of four months, during three and a half of which it had been destitute of water, so indispensable to most aquatic shells. The same heat caused its two former companions to move about in their vessel of water.

These experiments strongly support the probability of my first supposition being correct; viz. that the animal, in its perfect state, has the property of retaining within itself sufficient moisture to sustain vitality in the clay, under the bed of the pool, until the return of rain; but to place the matter beyond all doubt, it will be necessary to institute fresh experiments during the three months of the hot winds, and to place *Paludinæ* in the earth, at various depths, in places exposed to the sun and hot wind.

My remaining observations relate to an unusual, although not altogether an unknown occurence; viz. that of a shell, belonging to a family esteemed phytiphagous, possessing a zoophagous propensity. This habit occurs in the same animal upon which the foregoing experiments were made.

[1] This shell I described, without a name, in vol. i. page 363. As no other Paludinæ is known to possess a calcareous operculum, it is doubtless a new species; I shall therefore call it *P. Cerameopoma*, from Κεραμεος testaceus, and Πωμα operculum.

I had first occasion to observe this property in some specimens which I had gathered in August, 1828, and had put in a glass vessel with some tadpoles. One of these being transformed, and dying two days after, I found the *Paludinæ* fastened on the carcase of the frog, and apparently feeding on it, as it shortly disappeared altogether: the other tadpole dying, was served in like manner; as well as a small fish-like animal from the same pool. The landshell *Helix nemoralis*, belonging to the phytiphagous family of the *Colimaceæ*, has been observed feeding on dead animal substances: and thus far the anomaly in our shell is supported by a parallel example: but, as the following experiment will show, it proceeds still further and devours living animals.

On the 5th September, 1829, I caught a *Paludina*, and placed it in a tumbler of water. It soon crawled to the surface, and commenced swimming. I observed it with a lens, and found that the mouth was longitudinal, and that it was capable of being expanded to an oval shape, discovering inside, a bunch of prominent, hard, and parallel longitudinal plates, the edges of which were directed outwards: this bunch moved from the top to the lower part of the mouth with great quickness, thereby creating a slight current on the surface of the water, which conveyed the scum into its mouth, where it appeared to be swallowed. It appeared to make attempts to seize a minute living spider which had fallen accidentally into the glass: the spider was, however, too nimble, and succeeded in mounting the foot of the *Paludina*, where it remained in security. The animal made various attempts to reach the spider with its head, but without success; and finally, the spider was forced to the end of the tail, by the action of the muscles, and was floated off. I brought the spider into the current, by blowing it a second and a third time; but the efforts of the shell-fish were equally unsuccessful, and the spider escaped in the same manner as at first. I then procured a jumping mite, with long spider-like legs, which I floated on the water, and blew into the vortex caused by the shell; on reaching it, the *Paludina* seized it, and dismembered it in a trice; and in a very few seconds had swallowed a great part of the animal, rejecting only one or two small fragments. The operation I observed under a lens. I intended to have tried it again; but when I had leisure the weather had become too dry, and the jumping mites were no longer procurable. I have since fed *Paludin*, with musquitoes, newly killed, and floated on the surface of the water: these were soon attacked and carried to the bottom; the body and soft parts being eaten, and the wings and legs rejected. Isinglass also proved a very attractive food.

This little, and apparently insignificant shell-fish, thus appears to perform an useful part in the arena to which it is confined. Inhabiting, in general [1], stagnant pools and ponds, in which it sometimes absolutely swarms: it doubtless devours and assimilates the vast quantites of insects which are drowned in the waters frequented by it, and the bodies of such aquatic animals as are newly dead; thereby contributing to the salubrity of the surrounding air, and forcing upon us the truth of the conclusion so continually recurring to the enquirer in natural science, "that nothing was made in vain." The total extinction of the race in ditches and pools, liable to occasional drought, and therefore the more in want of purifying agents, is effectually and admirably guarded against by its capability of enduring privation of water and food, as exhibited in my first mentioned experiments.

Búndélkhund, February 9th, 1830. W. H. B.

IV.—*On the Climate of the North-Western Mountains.*

To the Editor of Gleanings in Science.

Sir,

Should the accompanying meteorological summary for January, 1829, be adapted to the plan of your work, it will afford me pleasure in transmitting to you similar tables in future, for publication.

The instruments I am in the habit of using, are of the best description, and in every respect as efficient as any in India: the accuracy of my observations may,

[1] Since the date of my communication published in vol. i. page 265, (vide *Paludinæ* C) I have found it adhering to rocks in the river Cén.

therefore, be relied on. With three or four exceptions, not less than ten, and frequently eleven and twelve observations were recorded daily, from sunrise till 10 P. M., every attention being paid to the direction and force of the winds at each hour, and to the particular changes in the atmosphere.

Should the remarks on the state of the weather not be concise enough for your purpose, you can curtail them or leave them out, as may appear to you most adviseable.

It is necessary to mention, that the locality (Bréri) is the name of the spot upon which the house is built. The situation of it, on the declivity of Sílajan or Sílajít ridge, a spot stretching down at right angles from Wártu or Hátu mountain, is delightful, and the prospect on three sides is rather extensive and grand. By the made road, it is about one and quarter mile distant from the post of Kótgerh, and 1100 feet above it; consequently it enjoys a cooler climate during the hottest months in the year. The winter is colder, and snow falls and lies to a greater depth. It is in about latitude 31° 19′ and longitude 77° 30′, and its elevation above sea-level is computed at 7734 feet.

I must not omit to mention that the observations, with Leslie's Hygrometer, are not to be depended upon, though taken and recorded as well as the want of practice and other circumstances would admit. Till the 26th inclusive, they were made in a room where a fire was constantly burning. It was subsequently exposed to the air, as also Kater's, being guided according to the instructions given for the use of the latter. A scientific friend and I made repeated trials with Daniel's much talked of Hygrometer, but without success. It seems defective in some way or other, or is ill suited to the purpose for which it is intended: the many unsuccessful attempts that have been made to obtain any result from this instrument, whether in the plains or in a climate, the mean annual temperature of which is about that of London, prove it to be quite useless in India.

For the sake of convenience during winter, the barometer was always kept in a room with a fire, and in consequence the inside thermometrical observations were taken in a different room, apart from every local circumstance, to indicate correctly the variations of temperature in the house. The detached thermometer was exposed, in the shade, to the air behind a wall having a northern aspect, care being taken that it should be as free from the influence of radiation as possible.

January 31st, 1830.

P. G.

A general statement of the Weather for January 1829.

Clear,	12 days
Fair, but cloudy and partially cloudy,	14 ditto
Rainy, stormy, snow and hail,	5 ditto
Thunder,	2 ditto

Clear, on the 6th, 8th, 13th, 14th, 15th, 19th, 20th, 21st, 22nd, 29th, 30th, and 31st.

Fair, but cloudy and partially cloudy, on the 1st, 4th, 5th, 7th, 9th, 10th, 12th, 16th, 18th, 23rd, 24th, 25th, 26th, and 27th.

Rainy, stormy, snow and hail, on the 2nd, 3rd, 11th, 17th, and 28th.

Thunder, on the 3rd and 18th.

Height of the Barometer.

	Inches	Th.
Mean maximum for the month,	22,860	51,5
Mean minimum,	22,804	45,8
Mean of the daily means,	22,830	48,6
Greatest altitude, on the 17th, at 10 A. M.	22,986	55,5
Least ditto, on the 3rd, at 2 P. M.	22,607	45

Temperature of the air.		*Temperature of the house.*	
Mean maximum,	43,4	Mean maximum,	40,6
Mean minimum,	34,9	Mean minimum,	38,3
Mean of the daily means,	39,1	Mean of the daily means,	39,4
Greatest on the 21st, at 1 P. M.	51,3	Grtst. 20th and 21st, at 3 P.M.	44,8
Least on the 3rd, at P. M.	27,5	Least on the 5th, at sun rise,	33,5
Mean,	29,4	Mean,	39,1

Hygrometrical State of the air.

Leslie's Hygrometer,	greatest ditto, on the 24th, at sun set,		45
Ditto	ditto,	least ditto, on the 29th, at 10 A. M.	— 3
Kater's	ditto,	greatest ditto, on the 28th, at 10 P. M.	615
Ditto	ditto,	least ditto, on the 26th, at 2 P. M.	242

Statement of the Winds, shewing their direction and force during January, 1829.

North-east, on the 7th,	gentle	1 day
East-north-east, on the 1st,	moderate	1 ditto
Ditto, on the 2nd, 6th, 10th, 11th, 12th, 18th, 20th, 22d, 23rd, 24th, 25th, and 26th,	light	12 days
Ditto, on the 4th, 5th, 8th, 21st, 27th and 28th,	gentle	6 ditto
Ditto, on the 9th, 13th, 14th, 15th, 16th, 17th, & 19th,	little	7 ditto
West, on the 3d,	moderate	1 ditto
Ditto, on the 29th,	little	1 ditto
North-west, on the 30th and 31st,	ditto	2 ditto

V.—*On Shading Mountain Land.*

To the Editor of Gleanings in Science.

Sir,

I have read with deep interest the 3d article in your 14th number on the Method of Shading Mountain Land; though I will acknowledge I felt surprised that your intelligent correspondent should have taken the trouble to write on an art so little esteemed or practised in India.

We must cease, however, to wonder at this apathy, when we consider that most of our surveys are confined to the plains, and that even those among the mountains are often simple lines of route, few occasions having yet arisen for examining details out of the more frequented lines of communication.

In offering my remarks on this subject, I trust I shall not be thought disposed to cavil, if I am found to disagree on some points with your very able correspondent; my only object being, like his, to elicit improvements in a neglected but useful art, by the free but fair discussion of every thing connected with it.

I will premise, that I have not had opportunities of studying recent improvements made in Europe in this branch of drawing, thirteen years having elapsed since I concluded my course of instruction at home, since when, I have been left to my own resources for the little real practice that is called for in India.

The German method was made known to me some years ago, and promised such obvious advantages, that I hastened to avail myself of them, and for a time thought the art was rendered thereby perfect. But I have since found that it labours under other defects besides those pointed out by D.

The difficulty of distinguishing the top of the hill from the valley, is not always to be got over, by having regard to the water course or river flowing in the latter. Sometimes in table land there is a river running in a bed many hundred feet above hills within a mile of it. I could adduce several instances of this kind among the hills in this neighbourhood.

Whether in the case of the cone mentioned by D. the same is to be considered a hollow or a projection, cannot be determined by the representation of water in the centre, because there may be a lake or pool on the top of a mountain as well as in the bottom of a valley or hollow. Such instances occur, particularly in volcanic countries; and several may be seen in Merioneth shire.

But the greatest objection yet remains to be noticed; the German method is commonly incapable of representing the acclivities of these narrow ridges or necks, by which great mountain features are often connected; because the breadth of such necks is usually so small, that even when a large scale is employed, there is not room for the introduction of even a few hatches at the required interval from each other.

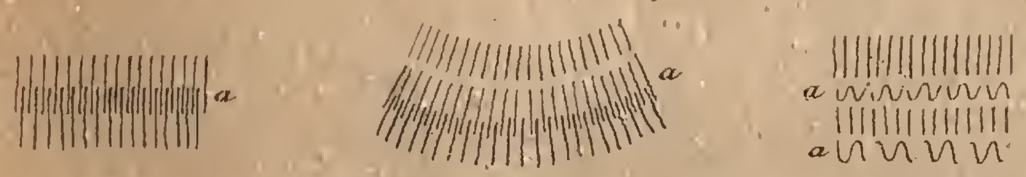
Let it not be supposed that such cases are of rare occurrence. I have met them frequently, and could easily instance one in which the imperfect or erroneous representation of the feature in a military sketch might be attended with serious inconvenience, as the neck affords the only eligible line of road in the ascent of a mountain 4000 feet high; no equally easy communication existing for many miles to the right or left of it. No material aid can be derived in depicting such situations from shades, because the variation in their intensity cannot be rendered evident; and indeed, generally, dependance cannot be placed on the practice of shading as a definite measure of acclivity, for it may be seen experimentally, that we cannot command so many various tones as stated by D., inasmuch as the gradation will be found too nearly imperceptible between any two of them; and I should say, that instead of ten varieties, we have not more than six.

Having detailed my objections to the German method, I will add that I am not the prejudiced advocate of the pictorial mode, the defects of which have been very justly urged by D.; but I think he has not given it the credit due for two qualities, which are never so well expressed by any other method; viz. character and form.

On a full consideration of all the difficulties and objections which have been noticed, I venture to offer the following plan, as calculated to obviate many, though I fear it cannot be thought to include a perfect solution of the whole of them.

I propose to take the pictorial as the basis of my scheme, but to superadd a practice of hatching, which after the German mode, shall furnish a value of the angle of acclivity. Instead, however, of determining this value, by the thickness of the hatches, or the interval between them, I would regulate the *length* of each hatch by a graduated scale, on which each angle should have its length proportionately assigned. The general scale of the map, and the degree of exactness required in representing the acclivities, must of course determine the scale for the hatches; but on maps of two inches to a mile, it will be found that 50 feet for an angle of 80°, and 850 feet for one of 5°, serve well for the extremities of a scale on which the hatches will decrease in length 50 feet for every five degrees of acclivity. The degrees of steepness may thus be easily known; but there is also another advantage resulting from this mode, viz. that the difference of level between any two points may be estimated with considerable accuracy and nearly as much ease as their distance; for it is obvious that each hatch will not only represent an angle, but also the altitude due to that angle on the space occupied by the length of the hatch, a table of which altitudes (as they are constant) should be attached to the map.

It will readily be conceived, that in many cases the length of the hatch (or succession of hatches) will not exactly fill the desired space: I would meet this difficulty by introducing in the intervals of the last line, another series of the same length, but reaching to the extremity required, thus fig. 1. & 2. *a a*:



1 2 3

It is not so easy to provide for another difficulty, which, however, is fortunately of less frequent occurrence; viz. that in which the space to be hatched is too short to receive hatches of the length required. It would, in general, be better to use only a large scale, in order to escape this case altogether; but it may, if unavoidable, be met by coiling up the hatch, thus fig. 3. *a*:

In conclusion, I would suggest that the hatching should, for the sake of distinctness, be performed with the crow pen, and only so much shading allowed (on the pictorial plan) as may be necessary to distinguish the summits of the hills from valleys, and cavities from hollows.

I am, Sir, your obedient servant,

Sylhet, 23*d April*, 1830. F.

VI.—*On the Rent of Land.*

To the Editor of Gleanings in Science.

Sir,

I now beg to be allowed to follow up my letter of November last, (Vol. II. p. 30.) with a few remarks on what I think the most adviseable method of investing capital in agricultural speculations in these parts.

In the first place, I shall address myself to those who have made up their minds to a residence in the country, and who are entitled to hold lands by the regulations of the Government.

Some people who insist that Government take the whole of the surplus produce from the cultivator, leaving him a bare subsistence, will scarcely credit me in advising an investment of money in land subject to this taxation. I shall not enter into the particulars of this question, but confine myself to the fact of its being a very profitable speculation, and none of the speculators having as yet failed, the returns may be considered certain; and to my knowledge the most of them are anxious to increase their property in this line: as, however, my bare assertions may have small weight in establishing what I have stated, it is necessary I should be more particular. The mention of person's names in an anonymous letter, is an invidious task; but in the present instance, where nothing can be stated but what is honorable and creditable to the parties, I hope I shall stand exonerated from any improper feeling. The Thácurs Qázi Nizábat Ali, Callu Chán, Akber Ali Chán, Merdán Ali Chán, and Abdul Rahman Chán, with many others, besides East Indians, are instances of successful investment of property in lands subject to the Government revenue; and if the lands they hold were so much overtaxed, pray where do their lacs of rupees come from; and whence that unquenchable thirst for land? The days of cheap bargains are certainly gone, and the lands of a Rajaship are not procurable for a few thousand rupees as in former times, but it is not long since a *talúqdári* sold in this neighbourhood, which yielded a profit of 40 per cent. per annum, after paying the Government revenue.

There are many places in the Upper Dooab, where capital could be invested with every prospect of success; and I may mention, in particular, a tract of country lying along the foot of the first or low range of hills from the Ganges to the Jumna, about ten miles in breadth; at present it is almost all a jungle, with a number of serviceable trees in it. Sites of deserted villages and ruins of *pacca* buildings are commonly met with, and the soil is very superior. Beautiful flowers and plants are here to be found, which are altogether strangers to the rest of the plains. Wood for building is to be had in abundance, and at the distance of a few miles, firs are procurable for the mere cutting. The vicinity of the hills renders this a most enticing spot for a person who intends to settle himself in India; he may enter his palanqueen and be at Landaúr, in the hills, in eight or ten hours. It is said to be unhealthy in the rains in low places; high spots should be chosen as sites of residence. A considerable trade in wood and bamboos could be carried on down the Ganges and the Jumna; of itself no small inducements, as it has hitherto given good returns.

I may now add a word to those who propose a temporary residence in India, and who are not entitled to hold lands. I see no chance of their succeeding in any thing at present except the manufacture of indigo and sugar: the former a much more difficult process than in Bengal, owing to the coldness of the climate. An indigo vat in Bengal, which takes 6 or 8 hours to come to the proper state of fermentation, takes 12 or 15 hours up here, and the process is altogether rendered less subject to rules of theory, requiring constant attention and application of judgment in the various operations; hence the uncertainty of the produce, and one of the causes of the inferiority of the indigo of the Upper Provinces; and if a person is not well skilled, he had better not hazard a disappointment. To those, however, who are proficients in the art, every prospect of success is afforded, for it has been found perfectly easy to carry on an Indigo concern on a small scale without a single acre of cultivation or advances of money. A capital of 20,000 rupees will set a manufactory going in the small way, viz. 4,000 rupees for outlay in building, and 16,000 rupees for the purchase of plant and other charges; the factory, on this plan, cannot be large, but another may be erected at eight or ten coss distance, as money may be plentiful, and plant procurable. The first thing to be done is to agree with a number of respectable landholders to furnish plant at a certain price, and then to build at any convenient spot:—it is a plan which has entirely succeeded. The value of a *cacha bíga* of plant is 3 rupees, or about 12 maunds of plant. The Government receive four annas per *bíga* for land of this description, and which may partly explain the reason, why holding lands liable to the Government revenue, is, in general, here rather an advantageous thing.

If ever the manufacture of sugar succeed in this country, I am convinced it will be on a plan of this sort; the Native will be the cultivator, and the European

(a) The *cacha bíga* of all superior crops, such as sugar-cane, tobacco, onions, &c., is smaller than a *cacha bíga* of barley, gram, *juár*, &c.

the manufacturer. The sugar mills and boilers should be so constructed as to be easily removed from one place to another. The season for manufacturing in this country continues for five months, viz. November, December, January, February, and March. A moveable mill could take up the cane of many villages: it is purchasable at about 5 rupees per small *cacha bíga* of good cane, or 30 rupees per acre, producing 2037 lbs. of *gúr* by the native process, or little more than a farthing per lb. taking the rupee at the value of two shillings; the difficulty will be to get the people to grow the sugar cane, as this would bring the *tahsíldár* about their ears, at the next *Jama-bandí;* for although the Government, at present, gets only a small portion of the profits of the soil, yet, keeping to the old system, they take as much as they can get, and any thing like improvement this way, or in digging wells, immediately leads to a new pretext for an increase of the demand; and the manner of levying the revenue, and not the actual amount, is the most objectionable part of the Revenue System; but as following the subject further may lead to forbidden ground, I shall here close this communication, and remain your obedient servant,

Upper Dooab, March, 1830. Z.

VII.—*On the Temperature of Wells.*

To the Editor of Gleanings in Science.

Sir,

Having often heard it asserted by natives, in different parts of the country, that the water of deep wells is warm during the cold, and cool during the hot months of the year; and having often remarked myself, while bathing, the high temperature of water, during the cold season, *fresh drawn* from the well, I was induced, last year, to bring the matter to the test of experiment, the result of which, as exhibited in the following table, is at your service, should you think it worthy of a place in your excellent periodical.

The well is 90 yards from the edge of the river Ganges, in lat. 25° 11′ North. The level of the water of the river, on the 23d January, 1829, when the experiments commenced, was sixty one feet below the top of the bank, or general level of the surrounding country. The surface of the water in the well was the same. On the 5th of September the water of the well was 43, and that of the river 37 feet below the same level.

The temperature of the water at the bottom of the well, was ascertained as follows. A thermometer was placed in a bucket and let down into the well, so as nearly to reach the ground at the bottom. After remaining there for about 15 minutes, the bucket was quickly hauled up over a pulley, and the thermometer read off, in the water, before the latter had time to alter its temperature. A second thermometer was placed in a shady spot, close to the well, under some plantain trees.

I am, Sir, your sincere well-wisher,

D. G. J.

1*st May*, 1830.

Date.	Thermometer at the bottom of the well.		Ther. in the open air in the shade.	Remarks.
1829.		°	°	
January 23	At sunrise,	82	49	Wind W.
24	10m. after sunrise,	82	50	,, W.
25	At sunrise,	82	44	,, W.
27	At sunrise,	82	46	,, W.
28	At sunrise,	82	54	,, E.
30	15 ms. after sunrise,	82	54	,, S. W.
31	10 ms. after sunrise,	82	52	,, W.
February 1	At sunrise,	82	52	,, S. W.
	At 3 P. M.	82	77	,, W.
7	At sunrise,	82	47	,, W.
8	At sunrise,	82	45	,, Calm.
May 27	½ hour after sunrise,	82	84	,, E.
	At 3 P. M.	82	102	,, N. E.

1829.		°	°	
May 28	1 hour after sunrise,	82	85	Wind E.
29	At sunrise,	82	85	,, E.
	At 2 P. M.	82	105	,, Variable.
30	$\frac{1}{2}$ hour after sunrise,	82	85	,, E.
31	$\frac{1}{2}$ hour after sunrise,	82	82	,, Calm.
June 1	At sunrise,	82	83	,, E.
2	$\frac{1}{2}$ hour after sunrise,	82	86	,, E. [sun 122°.
	At 2 P. M.	82	105	,, Calm, ther. in the
September 5	At sunrise,	83	84	
6	At sunrise,	83	81	
7	At sunrise,	83	79	
November 27	At sunrise,	82	59	,, S. W.
28	At sunrise,	82	58	,, S. W.

From the above, it is evident that the temperature of the water sixty-one feet below the surface of the ground, continues uniform throughout the year; and that its apparent warmth during the cold, and coolness during the hot months, is merely the effect of contrast with the temperature of the atmosphere.

VIII.—*On the Climate of Bareilly.*

The following abstract of a register of the Thermometer kept at Bareilly, has been handed to us by a correspondent, from whom we are always happy to hear. He has omitted to state what was the situation of his thermometer, whether in or out of doors: from the indications of the thermometer, however, we infer the latter. We would suggest to our correspondent, that the maximum temperature of the 24 hours would be a useful addition to his table; or if he finds it inconvenient to attempt this, then the temperatures of any pair of hours are said to give the mean temperature nearly of the 24. Thus at 8 A. M. and 8 P. M., or 9 A. M. and 9 P. M. or any other pair of similar hours, the above two being the most eligible. It might also be useful to try the truth of this position, as it has been deduced from an examination of a register kept at Leith, every hour, for two years, and the law of climate may be very different in this country.

One more suggestion we will venture to offer, with the view of making such registers more useful. If the observer would use two thermometers, covering the bulb of one of them with cambric muslin, a strip of which should hang down, and dip into a cup of water so as to keep the cloth-covered bulb continually wet;—then the indications of both thermometers (which should be hung near each other) being registered, would afford a much more accurate idea of the nature of the climate as it affects feelings; the mere temperature of the air being not always a key to these.

We may perceive, in looking at this register, one advantage which the climate of Calcutta has over that of the north-western provinces. It is much more equable, the annual range being at the former place 52°, at the latter 83°.

The climate of Calcutta is superior, too, in having less oppressive hot weather. Thus the highest temperature at noon is in April 16° less than at Bareilly. In May 14°, in June 15°, in July 14°, in August 9°, in September 9°, in October 6°, in November 5°. The mean temperature at noon has nearly the same differences. But again Bareilly has cooler mornings. Thus at sunrise the minimum temperature is at Bareilly 5° lower than at Calcutta in September; in October 16½°, in November 23°, in December 21°, in January 20°, in February 9°, in March 24°, in April 14°, in May 7°. We shall take some future opportunity of making a more detailed comparison, but to estimate the comparative value of the two climates, the indications of the moist-bulb thermometer are absolutely necessary.

Register of the Thermometer at Bareilly.

	April, 1829.			May, 1829.			June, 1829.			July, 1829.		
	Hgst.	Lst.	Mn.	Hgst.	Lst.	Mn.	Hgst.	Lst.	Mn.	Hgst.	Lst.	Mn.
Sunrise, ...	78	62	70	85	69	72	87	76	81½	84	78	81
Noon, ...	109	93	101	108½	90	94¼	111	89	100	103	81	92
Sunset, ...	97	81	94	98	88	93	101½	87	94¼	91	80	80½

	August, 1829.			Sept. 1829.			Oct. 1829.			Nov. 1829.		
Sunrise, ...	84	76	80	81	70	75½	74	51½	62$\frac{3}{6}$	60	41	50½
Noon,......	98	79	88½	99	74	86½	96	86	91	89	76	82½
Sunset,.....	89	76	87½	90	83	86½	85	75	80	78	66	
	Dec. 1829.			Jan. 1830.			Feb. 1830.			March, 1830.		
Sunrise, ...	54	31	42½	48	28	38	64	48	56	71	43	57
Noon,......	80	68	74	80	70	75	86	59	72½	92	72	87
Sunset,.....	64	59	62	not observed.			not observed.			not observed.		

IX.—*Miscellaneous Notices.*

1. *Description of a Circular Saw, worked by the Foot.*

This machine is one, the adoption of which in every works-hop, we cannot too strongly recommend. It is fixed on a wooden frame, resembling a turning lathe, and is worked with ease by one person, who presents the wood to be cut to the teeth of the saw, which he, at the same time, turns by the action of his foot on a treadle. The axis on which the saw is fixed, is mounted on two puppets moving in a groove; and it receives its rotatory movement by means of two bevilled wheels with grooves of sizes, one of which is fixed on the axis of the saw, and the other communicates with the treadle, by means of a bent axis and crank, in the usual manner. The saw turns in a trough, which receives the sawdust, and which, when full, is emptied. The upper part of the saw passes through a slit, cut in a piece of wood, which turns by a joint on the back part of the trough, with which it forms an angle, which may be varied at pleasure, by means of a regulating screw, so as to confine the action of the saw to that part which is supposed to be sufficient to cut the wood presented to it on the inclined rest. The dimensions to be given to a piece of wood cut by this machine, are regulated, by a parallelogram of brass attached to this rest or table, and also moving on a hinge, in such a manner that the longer sides of the parallelogram are parallel to the slit in the rest, in which the saw turns. By opening or shutting more or less this parallelogram, which is effected by screws, and by applying the face of the piece of wood to be cut against its side, the desired dimensions are ensured. On the same table or rest is fixed a brass ruler in a groove, carrying a semicircle, divided into degrees like a protractor; this semicircle has at its centre a raised edge, which forms the guide to which the piece of wood is to be applied when it is desirable to cut it at any angle.

With the assistance of this machine we may prepare flat squares, cubes, rectangular pieces, parallelograms of any angle, all of a perfectly regular shape; it is even competent to cut tenons or grooves and tongues. In this case it is only necessary to regulate the small table rest and its parallelogram, which is easily done by the help of the several finger screws.

This method of sawing has a great advantage over the ordinary one, when the pieces to be cut are of suitable dimensions. Time is saved, and an inexpert workman is enabled to produce specimens of as good workmanship as the most practised mechanic could without this assistance.—*Rev. Ency. T.* iii. *p.* 253.

2. *Explanation of Plates VII. and VIII. in Vol. I. of the Gleanings.*

The following explanation of two of our Plates, in the first volume, was mislaid at the time of the title page and index to the volume going to press. It is here printed for the information of those who are interested in the subject.

PLATE VII.

Fig. 1. Unio. B.
Fig. 2 and 3. Fluviatile Arca.
Fig. 4. Anodonta? B.
Fig. 5. Melania, D.
Fig. 6. Melania, C.
Fig. 7. Melania, A.
Fig. 8. Melania, B. [viewed underneath.
Fig. 9. The same with the animal. *a.* the disk or foot and head,

PLATE VIII.

Fig. 1. Ampullaria, A.
Fig. 2. Animal of Ditto, B.

a. outer lip of shell,—*b. b.* tentacula,—*c. c.* lobes of head,—*d.* respiratory canal, —*e. e.* edge of operculum,—*f. f.* pedicellated eyes,—*g.* mouth,—*h.* fold of the contracted foot.

Fig. 3. Paludina vivipara, A.
Fig. 4. Bulimus, B.
Fig. 5. Paludina, B.
Fig. 6. Planorbis corneus, A.
Fig. 7. Pupa, A.
Fig. 8. Lymnæa, B.
Fig. 9. Lymnæa, A.
Fig. 10. Planorbis, B.
Fig. 11. Bulimus, A.
Fig. 12. Achatina A. magnified.
Fig. 13. Paludina, C.
Fig. 14. Cyclostoma, A.

The capital letters refer to the species in the list published in our first volume, p. 264.

3. *Missions subservient to the Progress of Science.*

Perhaps it has not always been sufficiently considered by men of science, how much even literature and general knowledge have been subserved by Missionary Societies. Many languages which existed only as oral and unwritten dialects have, by their agents, been reduced to a systematic form; and in grammars, and lexicons, and elementary works, have received a "local habitation and a name," a mould and pressure which has at once given them durability, rendered them attainable by other nations, and transmissable through all the generations of man.

Numerous tribes of the great family of man, and even nations, the existence of which was scarcely known, have been discovered by them; and their customs and manners, civil rights and religious ceremonies, have been minutely examined and accurately delineated. For much of the authentic information concerning the vast empire of China, containing nearly one third of the human race, the world is indebted to missionaries; first to those sent out by the Propaganda Society of the church of Rome, and since, by the missionaries of various Protestant Societies. The same may be said of the Burman Empire; for a knowledge of the customs and habits, the religion and manners of that people, we are laid under obligations to a very large extent to the labours of those intelligent and indefatigable and devoted men, the American Missionaries.

Nor are geography and natural history wholly unindebted to those Societies. Some of the most captivating and instructive volumes, which on these subjects have issued from the press, have been the productions of men bearing the missionary character. The works more particularly alluded to, are Crantz's History of Greenland, Henderson's Tour through Iceland, Jowet's Researches in the Mediterranean, and Ellis's Narrative of Hawaiee.

4. *Chinese Rice Paper.*

This curious and beautiful substance has been recently determined to be a vegetable production of nature, as had been concluded from microscopic observation in England. Mr. Reeves, of Canton, has forwarded to the Society of Arts, a piece of a branch of the plant from which it is obtained. The pieces being cut into lengths equal to the breadth of the sheet required, are placed upon a thick piece of copper, with two raised edges, as guides to keep them steady. They are then sliced spirally, by means of a large and sharp knife, which is held in the right hand while the piece being turned by the left, the cutting goes on continually from circumference to centre, till the whole piece is thus as it were developed. They are then laid in bundles of 19 or 20 pieces each, which weigh about 23oz. and are sold wholesale for about one dollar a bundle. It is chiefly brought from the Island of Formosa by the Chinese junks.

The Editor of the *Technological Repository*, from whom the preceding is taken, thinks that elder pith is similarly available. We may add that there can be no question, that *Sóla*, cut as it is for manufacturing hats, might be applied to the same use as the Chinese paper. We doubt, however, if it could be got large enough.

5. *Rent of Land.*

To the Editor of Gleanings in Science.

Sir,

Your correspondent H. S. B. in Art. VII. of February last, has deduced from theory, that the proportion taken as rent should be two-fifths, and that two-thirds could not be paid without actual starvation. It may be interesting to him to know the practice by the *Táluqdárs* and others in this neighbourhood, which is in common crops, such as wheat, to take half in all ordinary cases. This season's *Rabi* is considered below an average crop here, and I have known no instance of less than half being taken; while one case occurred in my presence of three-fifths being taken on a very good crop. In superior products, such as indigo, sugar, &c., a much greater proportion is claimed by the *Zemíndár* from the *Cisáns.*

Camp, near Allyghur, May, 1830. I am, Sir, &c. M.

6. *Rain in Calcutta.*

To the Editor of Gleanings in Science.

Mr. Editor,—Can you put me in the way of consulting any Meteorological Registers kept in Calcutta, prior to 1827? From the middle of that year a very complete register has been kept at the Surveyor General's office; but I have not met with any of older date, except what are contained in the two first volumes of the Asiatic Researches, and Mr. Kyd's account of the fall of rain during 16 months of 1821-22. I am anxious to collect information as extensive as possible on the subject of the variations of season, in respect to the number of showers and quantity of rain which falls in different months of the year; and rely upon the ability of some of your subscribers to contribute the result of their own experiments, from their readiness when called upon to do so. C.

7. *Col. Lambton's Tomb.*

In our sketch of this officer's life, published in our last number, we mentioned that it was suggested to Government to erect a monument over his remains, but that we did not know whether the suggestion had been attended to. A correspondent requests us to state, that a suitable structure was erected by order of Mr. Jenkins, the then Resident at Nágpúr; but whether at his own expense or of that of Government, he cannot say. He adds, that it was the intention of the Bengal Officers, then stationed at Nágpúr, to build a monument to his memory, on the Sítabaldi hill; and a plan was drawn out for it: but the relief of the Bengal troops by those of the Madras Presidency, interfered with the execution. Whether the latter officers have ever done any thing, our correspondent is ignorant.

8. *Nepaul Paper.*

In the April number of Brewster's Edinburgh Journal of Science will be found, among the proceedings of the Society of Arts for Scotland, page 365, a notice on the *Nepaul Paper,* mentioned in the 7th number of Gleanings in Science, from which it appears that the paper shewn did not take a good impression from a copper plate. Mention is also made of a paper from *Indian corn,* the impression of which was good. W. H. B.

X.—*Proceedings of Societies.*

1. Medical and Physical Society.

Saturday, 6*th February.*

Dr. T. Stewart, of the Bombay Establishment, was elected a Member.

The Secretary submitted to the meeting an account of the bite of a snake, successfully treated, communicated by Brigadier Wilson, commanding at Naserabád. This account, together with Dr. Wise's observations on intestinal wounds, and Mr. Dempster's case of Tumor in the leg of a Native, were then read, and made the subjects of discussion.

There are several points of scientific interest, upon which we understand the Committee of the Society are anxious to obtain information from resident members; such as—whether suicide by poison is frequently resorted to by the natives, and if so, what poison, (vegetable, animal, or mineral,) and in what quantity? The antidotes, if any, usually resorted to. Well authenticated details concerning the bites of venomous serpents form also a desideratum in our Eastern medical literature. A particular description of the reptile called *Biscopra,* and believed by the natives to be deadly poisonous, would be also desirable—as well as a description, and well authenticated particulars of the *Cobra Manilla,* &c.

Saturday, 3*d April.*

Messrs. C. Mackinnon, senior, and H. Chapman, withdrew their names from the list of contributors; and Mr. T. Clemishaw, Assistant Surgeon, Bengal Establishment, was duly elected a Member.

The following papers were submitted by the Secretary. An account of the treatment of a peculiar disease of the knee joint;—a case of Tumor, and another of Popliteal Aneurism, successfully treated by Mr. Jacob. A description of a disease resembling Land Scurvy, by Mr. James Hutchinson.

A letter was read from Mr. H. M. Parker, Acting Secretary to Government, granting permission to distribute the Fourth Volume to the Non-Resident Members by dák bhangy, free of cost.

A letter was also read from Dr. Carey, of Serampore, in reply to one from the Secretary, respecting a new edition of the *Hortus Bengalensis*, and intimating that he (Dr. Carey) and the other conductors of the Serampore Press, had come to the resolution of publishing the work anew accordingly.

Mr. Swinton presented to the Library the Transactions of the Royal Society of Edinburgh, hitherto published, amounting to ten volumes. Dr. G. Gregory, of London, presented a copy of his work on the Practice of Physic.

Specimens of Extract of *Hyoscyamus, Gundu Feeroza*, and Oil of Turpentine, prepared from this last by Mr. Royle, of Saharunpore, were submitted to the Meeting.

Dr. Webster's case and observations on Strangulated Hernia, and Mr. Hutchinson's account of the disease resembling Land Scurvy, were then read and discussed by the Meeting.

2. Agricultural and Horticultural Society.

At a Meeting of the Society, held at the Town Hall, on the 10th March, the following Gentlemen were duly elected Members; viz.

Brigadier General Knox, Messrs. T. Bush, D. Macnaught Liddell, A. Liddell, A. Johnstone, and —— Earl.

A letter was read from Dr. Strong, transmitting for the inspection of the Society, a specimen of cotton raised in Intally, by Mr. Robt. Kerr, from what that gentleman considered to be the seed of the Brazilian cotton, although not quite certain of the fact. Mr. Kerr states that the cotton separates *very clean* from the seed, without much labour, whereas nearly all the cotton grown in India is very difficult of separation. The Meeting appeared of opinion, that the cotton was rather weak in the staple, though not very short; and that its feel in the hand was not silky, but woolley. The Secretary was requested to send the cotton to Mr. Finlay, of the Gloster Mills, for his inspection and opinion.

Two letters were read from Mr. Tottie to the Secretary, forwarding a Prospectus and Subscription Book, containing proposals for publishing, by subscription, a book to be called *Hortus Orientalis*, or names of the plants and trees growing in this country, arranged under their respective classes, according to the Linnæan system, with the additions which have been introduced into the H. C.'s Botanical Garden since the period of Dr. Roxburgh's departure from the country; and soliciting the encouragement of the Society by a liberal subscription to the work.

Dr. Carey, who was in the Chair, stated, that he was unwilling to offer any opinion upon the proposal of Mr. Tottie, with whom he was unacquainted; but he considered it his duty to inform the Society that the copyright of Dr. Roxburgh's work had been made over to the Serampore Press, and that a reprint of the work, with large additions, was now in progress, under the direction of Dr. Wallich, the only person belonging to this country who was, in his opinion, competent to the task. That he did not, however, intend these observations as an argument against Mr. Tottie, if his intended work contained any matter not contemplated by the publishers of the new edition of Roxburgh's Work, now in progress.

It was resolved to refer Mr. Tottie's letters and prospectus to the Garden Committee for report.

Read a letter from Colonel Wood, commanding at Cyúk-fyú, forwarding to the Society some Oranges from that place, which he considered very superior, and which had been originally introduced from China: also stating that gardening of all kinds was proceeding prosperously at the station; and that the Cinnamon tree in particular, introduced by himself, was thriving.

The Secretary informed the Meeting that he had, in terms of the wishes of the Society, addressed a letter to the Committee of Management of the Town Hall, and had received a letter from the Assistant Secretary to the Committee, informing the Society that the Committee consented to the Society's occupying the S. E. room below stairs, for the purposes of the Society, and with permission to fit up presses and book cases therein, for the papers and books of the Institution.

Upon this, a hope was expressed that Members would contribute agricultural and horticultural books to the Library of the Society.

GLEANINGS

IN

SCIENCE.

No. 17.—*May*, 1830.

I.—*On Sensible Temperature.*

One of the earliest and most remarkable applications of the doctrine of latent heat, was to explain the phenomenon of animal temperature. The caloric necessary to the gaseous existence of the air we inspire, was supposed to be set free at the same time that the base of the gas was fixed in the blood; and thence was inferred the constant production of heat, forming so peculiar a character of animal existence. To this, however, it was plausibly objected, that were this the true explanation, the heat should be greater in the lungs and less at the extremities—a conclusion not agreeable to fact. Crawfurd endeavoured to mend the explanation, by determining, as he thought he did, that the capacity for caloric of the arterial blood is greater than that of the venous blood; and that the caloric set free by the fixing of the respirable gases, was absorbed by the arterial blood, and again given out in its passage through the body, and gradual conversion into venous blood. To this was objected the inaccuracy of his determinations, the capacities being, as nearly as possible, equal; and latterly still more decisively Mr. Brodie's experiment, in which artificial respiration being kept up after death, the body cooled more rapidly than when, as in ordinary cases, the lungs were allowed to collapse. Mr. Brodie advocated the opinion, that the brain and spinal marrow are the source of animal heat, but failed to show in what manner, or by what means they performed this function. There are many facts that render his opinion probable—such as the coldness of a paralytic limit, the heat diffused by receiving food or stimulating liquors, or even cold water into the stomach. Perhaps the heat evolved in topical inflammation may be considered less connected with this source. Dr. Thompson thinks too, that we must admit some connection between the heat developed in the body and the function of respiration; because, as he says, those animals, whose respiratory organs are most perfect, and who respire the greatest quantity of air, have a warmer blood, and *vice versâ*. But this argument is not conclusive; for we know that the perfection of the several functions of the body keep pace with the developement of the nervous system. Thus the warm-blooded animals, which breathe by lungs instead of gills, have also a more fully developed nervous system. As the three facts are co-existent, it is not easy to say, *a priori*, which two should stand in the relation of effects.

Mr. Brodie's opinion has been again recently controverted by Dr. Edwards, who returns to that of Crawfurd. To the usual arguments, he adds the important fact, that Dr. W. Philip and Dr. Le Gallois, in repeating Mr. Brodie's experiment, obtained different results—that is, by keeping up artificial respiration after decapitation they found the temperature decrease less rapidly than if no such operation was performed. Dr. Philip accounts for Mr. Brodie's result in this way. In ordinary circumstances the function of respiration is performed in a very gentle manner. If too much air be forced into the lungs, it has been proved that an animal, in a state of suspended animation, will be irrecoverably killed. But by introducing more air into the lungs of the decapitated animal than was required, and more rapidly—not only was the heat extricated by respiration carried off, but even the cooling process accelerated. By pursuing the opposite course, Dr. Philip found the temperature of a rabbit 100° at the end of an hour; while the animal, treated in Mr. Brodie's way, was only 98°.

The results in Dr. Philip's experiments do not seem either conclusive or satisfactory. In particulur the effect seems always too small, and such as might be accounted for without any theory. And it is worthy of remark, that the very circumstance on which the theory of the production of heat in the lungs is founded,—the absorption or fixing of oxygen by the blood,—is by some experimenters denied altogether. Messrs. Allen and Pepys found that the quantity of oxygen consumed by an animal, is precisely that required to form the carbonic acid he exhales by union with the carbon of the blood. Another opinion which seems to unite both the above in some degree, is that of Dr. Copland, who finds that the ganglionic nerves are the efficient organs in the production of animal heat. He, however, allows that respiration is also necessary to fit the arterial blood for giving out that heat which he supposes the above nerves, in their distribution to the arterial branches, extricate. He, in fact, regards it rather in the light of a vital secretion, than a chemical phenomenon: and though this may be considered as cutting rather than untying the knot, yet, as far as regards the opinion of heat being extricated, in the course of the circulation, from the blood by nervous influence, we must admit that it agrees well with, and explains a number of phenomena. But when we see so much difference of opinion amongst the first physiologists, we shall be satisfied that the subject is not yet understood, and that it requires a thorough re-investigation.

In all attempts to enquire into general causes, the first step should be to ascertain the facts. Before we endeavour to explain any part of the economy of nature, we should have a clear idea of what it is we are to explain. Unless we know the precise amount of the effect, it is not likely we shall come to a right understanding of the cause. Now it is singular enough, that there has never been a single attempt made to estimate the amount of the heat generated in the human body—a fact that must surprise every one; for independent of the curiosity and interest of the question,—and these are very great,—we shall be satisfied, if we reflect a little, of the great importance of the subject, considered in a medical point of view. The temperature of the human body, in a healthy state, is uniformly 98° Fah. or thereabouts—a trifling difference of, perhaps, 1°, or even less, being due to difference of constitution, and about 2 to climate—the inhabitants of warm climates having a temperature about 2° higher than those of cold countries. Now this temperature continues uniform (within the above limits) whatever the temperature or cooling power of the medium in which the body is immersed; whether we are in an atmosphere of 50°, or of 100°; whether in air, or water. The extraordinary fact of a man remaining in an oven till a shoulder of mutton was roasted, gives a still greater range; for we know that to char a substance, a heat of perhaps 500° is required; and to roast it, however gradually, certainly the heat of boiling water is not sufficient.

Now the difference in the quantities of heat taken off under these different circumstances is enormous. When the temperature of the air is 100°, it is actually giving out heat to the bodies immersed in it of temperature 98°. For instance, if we bring a thermometer standing at 98° into a gas, the temperature of which is 100, it is obvious the thermometer must rise. But a thermometer heated to 98°, exposed to the cooling power of a gas of temperature 50°, would sink about 30° or 40° in the first minute. And if we had any means of keeping it steadily at its temperature of 98°, while immersed in the atmosphere at 50°, it is obvious that such a portion of heat would be expended every minute as would be sufficient to raise the thermometer 30° or 40°; yet in both these cases, the animal body remains steadily at the same temperature. But the difference is even greater than this; for not to notice winds, in which the rapidity of the cooling process is still greater, instances are on record of people, in cases of shipwreck, being in the water (and this sometimes in very low temperatures) for hours, without suffering materially in their health. Now the conducting power of water is so greatly beyond that of air, that I suppose it is within bounds to say that it abstracts heat ten times as rapidly. If a thermometer, heated 50° above the temperature of water, be plunged into it, the fall is so rapid, that the time in which the first 30° are lost, is scarcely appreciable. And indeed our feelings tell us this truth, for every one knows he dare not immerse his body in water of a temperature, which, as applied to air, is perfectly innocent. Thus water at 34° or at 170° are intolerable; but air, of those temperatures, might be borne in contact without serious inconvenience for a certain period. We see then, that there is a great variation in the quantities of heat taken from the human body, under different circumstances; and consequently either there must be a great variation in the production of that heat, or there must be some method of carrying off the superfluous heat which would otherwise accumulate and raise the temperature of the blood above the standard temperature of 98°, at which it is always known to remain.

The latter is the general opinion, and that perspiration is the procedure by which the superfluous heat is abstracted, and the temperature preserved at an invariable standard. The abstraction from the blood of a watery secretion, is said to absorb much heat, and the further conversion of that fluid into vapor greatly increases the effect. And we see that in estimating the full amount of the heat produced in the human body, reference must be had to this modification of its action. Now as we know that it would be exceedingly difficult to estimate the amount of the heat disposed of in this way, our object should be to consider that particular limit of the question; where we may be certain that all the excess of heat generated in the body is carried off by the contact of the medium, and that, consequently, there is an equilibrium between the heat generated every instant in the body, and that carried off by the conducting powers of the medium. To ascertain what this particular condition of things may be, requires, however, some previous observations as to the state of our feelings, under different circumstances of temperature and of medium. But as our feelings are fallacious guides, when we wish to compare the intensity of the cause acting on them, it were to be wished, in order to prosecute the inquiry with that precision which the subject demands, that some method were devised of measuring our sensations, or to speak more correctly, of measuring some parallel phenomenon, that we may be certain will, in its relative amount, afford an exact idea of the energy of the exciting cause on which our sensations depend. No such scale has as yet been applied to determine the comparability of our sensations as to heat or cold; and it is perhaps to the want of some such means of accurately estimating them, that we are to attribute the little progress made in the subject of animal heat.

Nor is it easy to devise any such scale, so as to be universally applicable, and yet accurate in its indications. I have, at least for a considerable time past, sought some method of the kind, and have not as yet fully satisfied myself on the subject. Some conclusions of interest, however, I have arrived at, I think: and as the little I have done may afford hints to others, who may be more fortunate than I have been; and as it may at least stimulate those who have sufficient leisure (which I am sorry to say is not my case) to multiply even the imperfect observations I have had recourse to, I shall endeavour here to give some account of them.

The general introduction of the thermometer into our houses has made ordinary people, however unaccustomed to observe and to compare, sensible of its inadequacy, as a guide to the feeling we have of varying temperature. It is a common remark, how fallacious an index the height of the thermometer is to the conclusion we shall form as to the weather being warm, or temperate, or cold—at least within certain limits. Every one who has experienced our hot winds in the north-western provinces, knows that, even with the thermometer at 100°, the feelings are less oppressed than they are in other parts of the country or other seasons of the year, with the thermometer at 86°. This truth is so commonly acknowledged, viz. that a difference of 10°, or even more in temperature may occur, and yet the very reverse terms be applied, which the order of the numbers would authorise, that it is unnecessary to dilate further on the subject.

The reason of this will be evident to those who consider what is the nature of the information which the thermometer gives us, and what is the information we are seeking. The thermometer merely gives us the *temperature* of the medium in which we are immersed, but temperature is only one of the many circumstances which affect rapidity of cooling,—the real question we are enquiring into. For, considering the animal machine as a continual generator of heat, the question is, Under what circumstances is this heat, which would otherwise continually accumulate, carried off or dissipated in the most rapid or effectual manner, and *vice versâ?* The former will be coldest to our feelings, the latter warmest. Now with regard to the cooling of bodies, the following circumstances are found to affect that process universally. 1. Difference of temperature; 2. Specific heat of the medium; 3. Conducting power of the medium; 4. Quantity of the medium applied to the cooling body; 5. State of the surface as to evaporation. It is necessary to estimate the effect of each of these circumstances; while the thermometer, as usually employed, merely gives information regarding one of them.

But there is a method of using the thermometer, that enables us to answer most of these questions, if not all: and it is the object of the present paper to explain it, and to indicate some of the results which the writer has obtained. It is evident, if a thermometer be heated to the temperature of the blood, and be then exposed to the cooling powers of the air; that the direct effect of all these circumstances, except the last, will be indicated by the rapidity with which it loses its excess of temperature, *i. e.* the number of degrees it falls in a unit of time. So that, used in this manner, the thermometer may really become an index to the feelings; and they may, though in their own nature vague and fleeting, be not only measured, but fixed. Thus if in certain circumstances the thermometer lose 10° in a unit of time, we may be always sure that when the thermometer loses 10° in the same unit of time, the circumstances affecting the cooling process will be the same; and that, consequently, the accumulation or deficiency of heat in the human body will be the same; in other words, that we shall feel equally warm or cold. And if we do not, we may infer that the usual action of the calefacient organs, whatever they be, is modified. Whether such modification may be the cause or effect of disease, or neither the one or the other, is one of the many questions which the full prosecution of the enquiry is calculated to throw light on.

The case is, however, not even yet strictly parallel; and this is owing to the absence of the last of the conditions which regulates the cooling process—*i. e.* similar state of surface as to evaporation. There is, in fact, a difficulty in establishing this identity of state, and it is the chief difficulty with which the question is beset. The animal surface remains, if not absolutely dry, yet dry to sense at low temperatures, while in higher temperatures the surface becomes covered with a dew, the evaporation of which must tend greatly to check the accumulation of heat. Indeed we have already seen that it is one of the means used by nature to keep the temperature from ever exceeding a certain height. Now no inanimate substance can be contrived so as to imitate the animal surface in this particular. And, therefore, we shall, I fear, fail to get an accurate answer to our question,—What is the sensible temperature of such a medium, under such and such circumstances?

Yet I would not be understood to despair altogether of an accurate solution to the problem of the sensible temperature. The fact is, that by adopting certain precautions we may obtain what appears very like a tolerable approximation. The naked thermometer raised to the temperature of the blood, and then allowed to cool, shows, by the rapidity or slowness of its fall, the tendency of animal heat to be dissipated or accumulated, as far as depends on the four first conditions of the cooling process. By covering it with cambric muslin, and keeping it moist, we may also have an idea of the proportional effect of evaporation, under certain circumstances, in modifying that effect. It is true that we cannot add this effect to the other, because we know not whether, under all circumstances of the case, (for instance at low temperatures,) the comparison or parallel will hold. Still, as our enquiries into sensible temperature are chiefly interesting with regard to the warmer countries, this contrivance will enable us to obtain some interesting comparative results.

And it may be a question, whether what is called insensible perspiration may not be (at very low temperatures even) so copious as to allow of results being compared at all temperatures. At least it will be interesting to ascertain at what degree of sensible temperature, *i. e.* under what accumulation of heat, this salutary process first manifests a cooling power. And it is evident that till we had a method of investigating and measuring the other circumstances which affect the cooling process, this could not be affected.

A paper was read on this subject some years ago in England, before one of the medical Societies, by a Physician, who, sufficiently aware of the fallacy of the common method of referring to the mere indication of the thermometer, failed yet to take a full and philosophical view of the subject. He proposed, in order to measure the sensible temperature, that a thermometer should be heated 10° above the temperature of the air, and the number of degrees being noted, which it would fall in one minute, when exposed to the cooling influence of the atmosphere, that number he considered would be an expression for the state of the air as capable of acting on the feelings. But independent of his overlooking altogether the importance of attending to the state of the surface, as to moisture, and the effect of evaporation on it, his proposal is founded on an erroneous conception of the laws of cooling. For besides that the cooling influence exerted on a body only 10° above the temperature of the air, could never be a measure of the cooling influence exerted on the human body, which is always the same temperature, *i. e.* 98°, and may consequently be 5° or 6° *under* the temperature of the air or any number from 10° to 100°, or even more *above* it:—besides this obvious objection, there is another of scarcely less importance. It is, that the cooling of bodies does not proceed, supposing other things alike, as the simple difference of temperature, but as some fractional power between the simple difference and its square. The effect of this law is, for instance, such, that a body heated 10° above the air will not cool twice as rapidly as one heated but 5°. The proportion will be something more than twice, though less than four times. These considerations show that such a method of estimating sensible temperature is worse than useless.

But though the law of cooling, as affected by difference of temperature, be such as to interfere with any such method of estimating sensible temperature, it is not so with the size or shape of the bodies to be cooled. These elements of the question follow a simple law, the rapidity of cooling being directly as the surface and inversely as the mass, *i. e.* in similar bodies, directly as the squares, and inversely as the cubes of the homologous sides. And in dissimilar bodies, particularly where one or both are very irregular in shape, though it might be difficult to assign *what* the proportion is, yet we may be sure it will always continue THE SAME as long as the surface and mass of each body continue the same. If, therefore, we can place the thermometer in the same circumstances as the human body, (I mean as far as those circumstances affect the cooling process,) we shall be certain, notwithstanding the great difference as to surface and mass, that the results will always be proportional, and consequently comparable. Thus, if, in certain circumstances, a thermometer is found to lose as much heat as sinks it 1°, while under other circumstances it will lose as much as make it sink 10° and in the same moment of time, it cannot be doubted but that the human body was losing, in these two cases, heat in the same proportion, *i. e.* (supposing the thermometric divisions to represent equal degrees of heating power,) ten times as much in the one case as in the other. For allowing that in the human body there was 1000 times less heat lost than in the thermometer, under the first supposed state of things, it is certain that in the other case it would also lose 1000 times less than the thermometer, because the surface and mass remaining the same, the proportion which depends on them would remain the same; and, therefore, the 1° and 10°, being divided by the same number, their quotients would still be to each other as 1 : 10.

Such being the case, it is only necessary to place the thermometer in precisely the same circumstances as we are ourselves in; and by observing how many degrees it falls in a given moment of time, we have a comparative estimate of our sensations in numbers. That those numbers do not represent the heat lost by the human body, signifies little; for the question we are concerned to resolve is, not how much heat is given off in such and such circumstances, but rather, whether more or less, and in what proportion under two different sets of conditions. Yet, although the absolute quantity is not wanted to render the results comparable, we require that the proportion should be always the same; in other words, that the thermometer, from the indications of which we draw our estimate, should be the same. This is a condition scarcely possible to be fulfilled, for we seldom can find even two thermometers of the same size and shape; but we may obviate the objection by reducing the results made with any thermometer, whatever its size, to a standard one. This may be done two ways; either by measuring accurately the size and surface of the ball of the thermometer, and from these data reducing the indications to the hypothetical or standard instrument, by the application of the theorem already announced, viz. *that the rapidity of cooling is directly as the surface, and inversely as the mass*; or by taking a certain state of the atmosphere, with regard I mean to all the conditions which affect the cooling process; then assuming any convenient sized thermometer as the standard, and observing what are its indications—those of any other thermometer may be reduced to it, by applying it to the same fixed or standard state of the atmosphere, which should be such as to be of frequent occurrence. The scale of the instrument actually used being, so to speak, fixed by either of these proceedings, the other results obtained with them become comparable amongst themselves.

Thus, then, the first point is to establish a good set of standard experiments with which other experimenters might compare their results, and perhaps be enabled to deduce some useful truths. I was desirous of undertaking such a series, but regret that the want of leisure has prevented me; and I must conclude this paper with a few meagre indications of the results I obtained, which I offer more with the view of fully explaining the subject, than as thinking them either useful or even curious. In fact, having neglected to take note of the size of the thermometer, they are rendered of no use whatever as terms of the comparison. Such as they are, they may serve to incite others to take up an enquiry, which is, I am persuaded, well worthy of being prosecuted, if only as a means of throwing light on a very curious physiological question.

The first experiment I tried was when the temperature of the air was 84°, and that of a moist thermometer 74°. In this state of things I took a thermometer, covered the bulb with cambric muslin, and having dipped it into water of the temperature 98°, I observed that it sunk 14°,5′ in one minute. In this experiment, the cooling power of the air, as of a lower temperature than the body; the cooling power of the air in motion, or of the wind; and the cooling power of evaporation from the moistened surface of the skin; all contributed to the result. The feelings were noted as rather oppressive. Soon after they became more bearable, at least while sitting still and making no exertion, being at the same time in a very light undress. The experiment being now repeated, gave 18° as the fall in one minute: the temperature of the air was 81°, of a moist surface 73°. In the evening, when the sensations were quite agreeable, the fall due to one minute was found 21°. In the first of these cases it is evident that heat was generated faster than it could be carried off, while in the last the balance must have been nearly adjusted. The proportion of the heat lost, in these two cases, is nearly as 2 : 3.

The following day having occasion to try again what result I could obtain when the feelings were rather agreeable in a dress of calico, such as we wear in India, in the warm weather, I obtained 23° as the fall in one minute, but observed that during the last 10 or 15 seconds, the thermometer was stationary, having, in fact, reached the temperature of the air. This fact indicates the necessity of shortening the time. The enquiry, in strictness, belongs to a mere momentary decrement; but in practice it is necessary to take a sensible interval, not only to have an appreciable scale of time, but also of the cooling effect. By a well known theorem it is easy, if necessary, to reduce these quantities to the momentary decrements. In the case in question I tried 20 seconds, and found the fall to be 11°, which may, with this thermometer, be said to represent that state of atmosphere in which a calico dress is required, but is yet abundantly cool. The temperature of the air was 78°,5′, and of a moist surface 69°, there was scarcely any wind. In the evening again this thermometer fell 13° in 20 seconds, but as yet the calico dress was not found too cold, nor even afterwards with a fall of 17° in the same time:—perhaps in this case the person was not exposed for a sufficient length of time to make the experiment satisfactory.

On entering the house, the fall due to one minute was found to be 18°. This was the quantity which had been observed the preceding day, but the feelings were certainly not so agreeable as in the present case. The temperatures were, in these cases of the air, 80 and 72; of an evaporating surface, 72 and 67. Though the fall of the moist thermometer was then the same, yet that of the dry one would have been very different. This induces a suspicion that the skin does not part with its heat so rapidly in proportion as a moist thermometer, though more rapidly than a dry one; a conclusion which, on subsequent consideration, I find evident, inasmuch as its moist surface is not exposed *directly* to the cooling power of the air, but *through the interposition* of dry clothes. This very obvious idea did not strike me at the time, and I proposed to get over the difficulty by observing both thermometers, and then combining their results in such a manner as future observation might suggest. The remedy, however, is much simpler: it is only necessary to cover the moist bulb thermometer with a loose envelope of calico; its indications will then form a more accurate register of the feelings. An experiment tried immediately afterwards with the dry thermometer, sufficiently shows the fallacy of the information given by the ordinary indications of this instrument. The temperature of the air, within the house, was within 1° of what it was outside; yet the feelings were so different, that in the one case they were agreeable with a calico dress, in the other they felt absolutely cold, and a woollen dress became desirable. In these cases, the fall within the minute of the dry thermometer, raised to the temperature 98°, was about 8°, in the other 16°; showing that the heat was dissipated twice as rapidly in the latter as in the former case.

The following day another experiment confirmed the preceding view. The moist thermometer, raised to 98°, fell 21° in a minute, yet the feelings were barely tolerable. On trying the dry one, I found it had a depression of but 7°, whereas I found by many subsequent trials that it required 10° at least, with this fall of the moist thermometer, to correspond with any thing like agreeable feelings. I shall insert these for the information of such of your readers as may be curious in such matters.

Thermometer raised to 98°.

Fall in 1 Minute. Dry Thermometer.	Fall in 1 Minute. Moist Thermometer.	Remarks.
10	21	Feelings agreeable.
11	—	Pleasant and cool.
9,5	21	Calico dress just bearable.
4,	13,5	Very oppressive.
6,4	15,5	Oppressive even in undress.
11	27	Woollen clothes bearable.

With these few imperfect and unsatisfactory observations I must conclude. I may, as a recommendation to some of your readers to take up the question, remark, that in a similar manner may be determined the effect of a punkah, of flannel, of woollen clothes, &c. and the state of the feelings accurately fixed when such things become merely bearable or necessary. All this is sufficiently obvious, and I may add that a few results obtained by this method of using the thermometer would give more useful information as to the nature of any climate, as it would be likely to affect the feelings, than the many-columned tables which meteorologists are so fond of drawing up. It appears equally applicable to the purposes of the gardener, for it can scarcely admit of question, that it is the more or less rapid loss of heat that regulates the question, as to any particular vegetable production thriving or being destroyed altogether. And to determine this question, the mere temperature of the air, as I have already shown, affords us little assistance.

D.

II.—*Sketch of the Geology of the Bhartpúr District.*

Into a minute description of the Geology of the Bhartpúr district, I cannot enter; nor would I have troubled you at this time, had I not felt that in the present scarcity of information on the subject of Indian Geology, even a few scattered facts acquire a certain degree of importance. They serve, at least, as landmarks to guide others in their researches; and had we at an earlier period been possessed of that medium of communicating information which your periodical now affords, many facts which are lost would doubtless have been recorded, and these might, ere this, have proved, in the aggregate, a most valuable body of information.

This district may be described as forming a portion of the south-western boundary of the valley of the Ganges and Jumna. Its surface presents a level platform, covered, in most situations, by alluvial and diluvial deposits, and elevated about 60 feet above the level of the Jumna. The country is now in a high state of cultivation, and exhibits a pleasing picture of prosperity and peace, as contrasted with its condition only a few years ago. The rocks which immediately underlie these deposits, in some few situations, occur near the surface, and are quarried for architectural purposes; while strata, probably of an anterior date to these, here and there basset out, forming, especially in the northern portion, small detached hills and collines which are generally topped by a village. To the west again this district is flanked by a belt of older rocks, which extends in a north-easterly direction from Biana, and which is interposed between the newer sandstone and the decidedly primitive formations of the Jépúr and Ajmér territories. The eastern limit of this belt is marked by a low hill range, which is observed a short distance to the west of the city of Bhartpúr. The quarries, which have, for centuries, supplied all this portion of India with materials for building, are situated in the Bhartpúr district; and, as the sandstones of these quarries are important, both in a statistical and geological point of view, I shall, in the first place, communicate to you the little information which I have been enabled to collect relative to their natural history.

Of these rocks there are three varieties. No. 1 is a very close grained argillaceous sandstone, with a schistose, passing into a subschistose texture. It is of a uniform dark red color, is soft so as to be scratched by the knife, and is apparently composed of minute quartzose particles, connected together by an argillaceous or ferrugino-argillaceous basis: minute scales of greyish mica are distributed through the mass, to which circumstance it probably owes its schistose texture. No. 2 is also a close grained argillaceous sandstone. This is a very beautiful variety, its color is dark-red, speckled with white spots, which are generally roundish, and which vary from upwards of an inch in diameter to the size of a pin's head. This rock is less schistose than No. 1, and, when slabs of it are properly cut, it has, at a little distance, exactly the appearance of a fine red porphyry. No. 3 is a rock similar in point of composition, texture, &c. to the last, but is of a salmon color, passing into greyish white. The two last varieties are less micacious than No. 1. The above rocks are all of them employed in architecture, and are remarkably free working stones. Of the two first varieties, the fort Agra, of and most of the buildings of the Agra and neighbouring districts, are constructed; and among these buildings are some of the most splendid edifices of Hindostan. The dark-red color and brick-like aspect of the sandstones, as contrasted with the pure white of the Makrana marble, of which the cupola, pavilions, and trellices of these buildings are generally formed, gives to many, even of the most stupendous monuments of the Mogul power, a fantastic and party-colored appearance, and it is impossible not to regret that the white variety had not come into use at an earlier period. To the general use of the red sandstones there is a still stronger objection. This rock appears to be little capable of resisting the decomposing effects of the atmosphere. Hence it is that so few of the edifices for which Agra was once so famous, now remain entire: and of the few which have escaped, the majority are rapidly falling to decay. The splendid mausoleum of Akber, at Secandra, is likely soon to meet the fate of its fellows, while at every step we take, we see the most stupendous ruins of palaces, and courts, and gateways, which less than two centuries ago were the favorite haunts of the sovereigns of Hindostan. In every climate this is a most serious objection, but in a climate like this it is quite unsurmountable; and, in contemplating the ruins around us, though we may smile at the vanity of Jehángír when, in the pride of his heart, he pronounced his favorite capital to be the "first city of the world," let us not ourselves neglect the lesson which the fate of the, comparatively speaking, modern city of Akberabád has so emphatically taught us.[1]

The above objection is more strongly applicable to the purely red than to the speckled variety. The former, from its slaty texture, in a very short time exhibits signs of decay, and in the course of years it is subjected to a gradual process of exfoliation.

The salmon colored variety, on the other hand, seems less liable than either of the above to such objection. This last is a remarkably fine freestone, and may be had in slabs of any magnitude. It is admirably adapted for paving and hearth stones, and is even capable of being used in the finer kinds of ornamental architecture. I have had an opportunity of seeing a remarkably handsome chimney piece of this rock, which, in the chasteness of its design, and minuteness of sculpture, could scarcely have been surpassed had the finest white marble been employed in its fabrication. On this subject I shall say no more, as you have already been furnished, by a scientific officer of engineers, with a full account of the physical properties of these sandstones, in reference to their employment in architecture; and I shall now proceed to offer a few remarks illustrative of their geological relations.

These sandstones, then, belong to that great formation which occurs so extensively distributed throughout Hindostan, and which is now very generally considered to be identical with the *New red sandstone* formation of the English geologists.

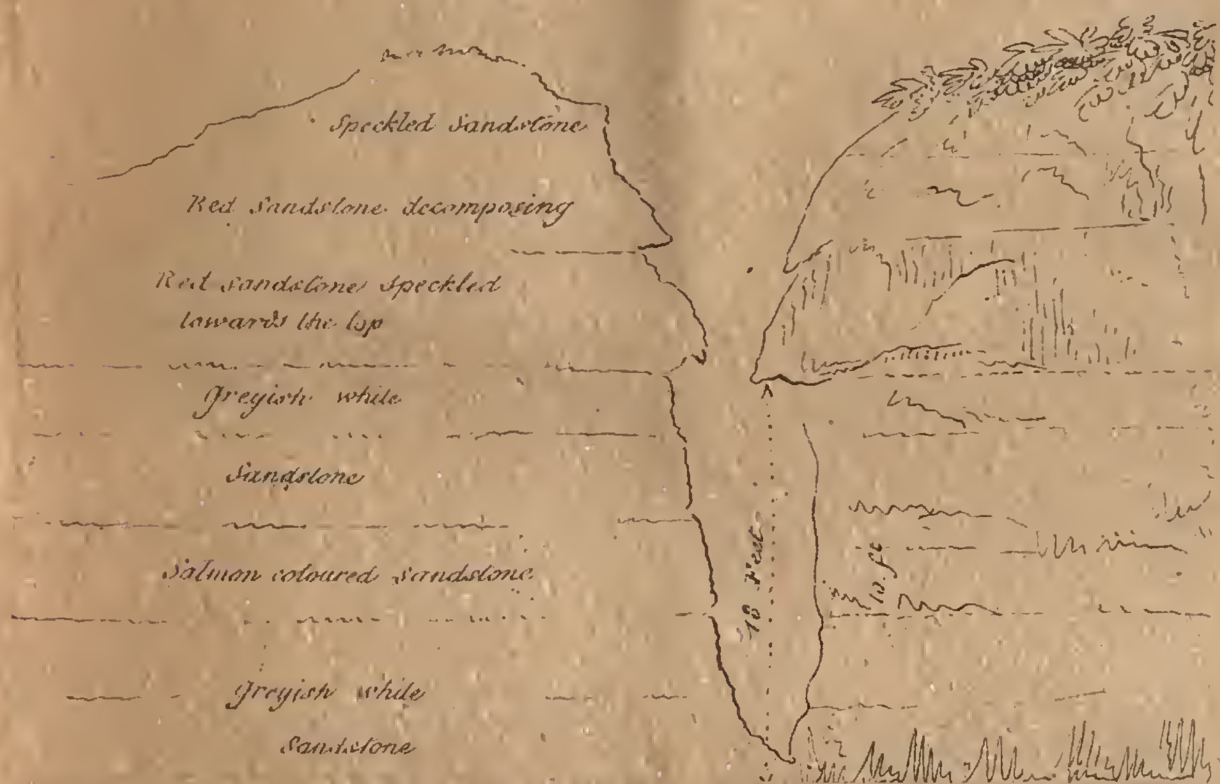
The rocks of this series, which occupy so conspicuous a place in the geology of the southern boundary of the valley of the Ganges and Jumna, appear to be continued on, with but little interruption, into the Bhartpúr district, where they occasionally appear near the surface, though, generally speaking, they are hid from view by the great alluvial and diluvial deposites of the Gangetic Provinces. These deposites, in many cases, seem to occupy extensive internal valleys or basins in the above formation.

In the Bhartpúr district, as in other situations, the rocks of this series are characterised by the horizontal position of the strata, and by the total absence of enclosed organic remains. There are no partings interposed between the strata, and sections of the rocks exhibit a succession of huge rectilinear tables, piled directly the one upon the other. These strata are remarkably free from veins or fissures of any kind, and contain few, if any, imbedded minerals; and, as far as external characters go, they have all the appearance of pure chemical deposits. This last remark, indeed, may, perhaps, be extended to the great majority of the true rock formations of Hindostan; and to this circumstance may probably be attributed the great scarcity of well marked organic remains in the newer marine deposits; as in the *lias*, for instance. It is probable that these are often concealed from view by the compactness of the containing rocks. We have, as yet, discovered but few, comparatively speaking, of the marly, of the loose argillaceous, calcared, argillaceous, and ferruginous beds, which usually accompany the more recent strata in

[1] The marble which was so extensively employed by the Agra architects, during the reigns of Jehángír, Shah Jehán, &c. was all, I believe, brought from Makrana, in the Jaúdpúr district, and appears to be a very durable stone. Unfortunately, however, it was but too often associated in the same building with the more perishable sandstone. Shah Jehán, indeed, encouraged a chaster style of architecture, and the Tāj Mehál, where the ashes of this monarch now mingle with those of his beauteous *Begum*, is still preserved in almost all its pristine splendour. This magnificent marble mausoleum is, indeed, well suited to his gorgeous title of "king of the world."

England, and even our best marked conglomerates, are generally characterised by the compactness of the cementing medium. We have rocks of the new red sandstone formation, assuming the appearance of nearly pure quartz rock; and the old red sandstones, I suspect, occasionally exist in the same form. These remarks, however, require further proof: my own experience has been far too limited to entitle me to draw any conclusions of a general nature, and I have thrown out these hints in the hope of directing the attention of some other of your correspondents to the subject.

The geology of the more recent sandstones of Bhartpúr may be best studied at Rapbás, a town situated about 32 miles, in a north-westerly direction, from Agra. The accompanying section of one of the quarries, (for which I am indebted to the kindness of a friend,) will give you an idea of the mode of occurrence of the rocks in this position. Besides the Rapbás quarries, there are others near the villages of Jugnér and Bussái, &c. all of which places lie within a limited patch of country, which exhibits an undulating surface, and which is marked by several low grounded collines, formed of the sandstones.



As far as I have been able to ascertain the fact, there are no deposits of rock salt or gypsum in the Bhartpúr district. The soil, however, is impregnated to a great depth with saline particles, and a saline efflorescence very generally appears at the surface. The great majority of the wells, too, are brackish. From these sources are manufactured large quantities of a salt called by the natives *çhári nimac*, (a name, by the bye, very indefinitely applied to several saline compounds,) which is extensively used by the poorer classes as a condiment. The salts collected at the surface, together with a certain quantity of the saline soils, are washed with water from the brackish wells; and the solution, thus prepared, is left to evaporate in pits, dug for the purpose, which are lined with a thin coating of lime. The salt is deposited in cubic crystals, many of which are very perfect and colorless: the principal ingredient in its composition is chloride of sodium. On a rough analysis the presence of chloride of sodium in large quantities, sulphate of soda and carbonate of soda in much smaller proportion, and a minute quantity of iron, were detected in the saline water. The salt has a bitterish taste, and the crystals are occasionally tinged with iron.

The wells from which the salt water is drawn, vary in depth, from 42 to 64½ feet; the richest water being that which is procured at a depth of from 57 to 60 feet. This last yields, according to circumstances, from a *chedám* to 1½ pice weight in the seer, (*i. e.* from 1 to 3 per cent. by weight;) and one well, drawn by two bullocks, will produce, in a season, from 100 to 1000 pallahs; a pallah or bullock load being equal to about 3½ maunds, at 90 sonat rupees to the seer. It is said that the water below 65 feet is also saline, but that the salt from this does not form into separate crystals, but is left in a solid cake at the bottom.

The soil through which the wells are sunk is clayey, mixed with scales of mica and fine sand; and beds of sand, exhibiting something of a stratiform appearance, occur at various depths below the surface[1]. The principal salt manufactories are at Bhartpúr, Díg, and Kumbér.

From sources similar to the above, I believe, that salt is manufactured in various other districts in India; and an inquiry into the phenomena attending its occurrence would be extremely interesting.

Are such saline soils found only in connexion with rocks of the new red sandstone formation? Or, in other words, when sea salt is found intermixed with the soils, &c. of the Gangetic Provinces, is there any reason to believe that this circumstance has any connexion with the occurrence of rocks of the above formation beneath such soils, &c.? Or, on the other hand, have these saline particles been transported from a distance, in mixture with the other ingredients of the soils, and by the same causes which operated in forming the alluvial and diluvial deposits? Or are they the produce of some chemical decomposition, which is still going forward in the great laboratory of nature?

The experiments of Sir J. Hall have rendered this a most interesting subject of inquiry; and supposing, for a moment, that his theory, relative to the influence of sea salt, as a consolidating agent, is correct, might we not, also, suppose that the salts, now found intermixed with our soils, had been originally employed in consolidating the strata below? and that, having passed through these strata in a state of vapour, the superjacent soils were thus inpregnated with saline matters without being themselves consolidated?

Since the formation of the new red sandstones the strata have not been subjected to the influence of those violent causes which are supposed to have modified the position, &c. of the older rocks; and it is not impossible, nay, not very improbable, but that future observations may discover that the deposition of some of our Indian diluvial beds was either synchronous with, or anterior to the consolidation of the sandstone strata. There is nothing in the phenomena of sublimation to militate against such a conjecture; and might not the identical agent, (supposing it to have been a body of water,) which was concerned in the deposition of the diluvium, have supplied the requisite pressure, &c. necessary for the conditions of Sir J. Hall's hypothesis? But enough of theory; I have already extended this communication to an undue length, and I shall now conclude with a very short notice of the other rock formations of this district. I have already observed that sandstones, probably of an anterior date to those which have just come under our consideration, basset out in the northern portion of this district. These rise into low hills, and hill ranges, many of which present an abrupt escarpment to the west. The hill of Fattehpúr Sicri, though, strictly speaking, in the modern district of Agra, may be quoted as an example of this formation; and numerous other detached hills of a similar nature may be seen in this portion of the country. These rise abruptly from the alluvial platform, like islands from the bosom of an ocean. The sandstones of these hills are quartzose and gritty:—they occasionally incline to coarse granular, and are very generally ferruginous. They are much fissured and broken, and are, in consequence, not well adapted for building. They are used, however, in the fabrication of native mill stones, and seem well adapted for this purpose. They vary in color, from reddish to grayish white, and are arranged in strata, which are considerably inclined in an easterly direction. I have met with no organic remains from this formation, but specimens have been shown to me as such, which were instances of those beautiful dentritical appearances of metallic origin, which have so often deceived those who are unacquainted with geology. Such appearances are very often met with in these sandstones. With regard to the precise age of this formation, I have not sufficient data to enable me to speak decidedly on the subject: the strata, however, appear to dip under the newer rocks, and they may perhaps be identified with the old red sandstone strata of England.

I have but little to say regarding the belt of rocks which I have mentioned as flanking the Bhartpúr district to the west. About three miles W. S. W. from the city of Bhartpúr we meet with a low hill range, which, as before remarked, forms a portion of the eastern limit of this belt. This range runs in a direction N. E. and S. W.; the hills are very low, (about 150 or 200 feet in height,) and the rocks composing them belong to the *submedial order* of Conybeare and Phillips. On approaching the range from the eastern side, the first rock met with

[1] The soil of the Bhartpúr district is also very generally calcareous.

is a variety of gray wacke or rather transition greenstone. This rock is tough, and difficultly frangible: it has a somewhat shining aspect, and appears to be composed principally of felspar and quartz. Its color is a light, greenish gray. It does not exhibit a conglomerated or re-united appearance, but is occasionally minutely porous; the pores being filled with a soft earthy matter, apparently of a magnesian nature, and occasionally with a red ferruginous substance. When breathed upon, this rock exhales a strong aluminous odour. The whole of the eastern slope of the range is formed of this rock, which we shall distinguish by the letter (*a*). We next come upon a bed of a soft friable slate (*b*), which may be described as a talco-argillaceous schist. It is of a light grayish color; is soft, so as, in many instances, to crumble between the fingers; it has a distinct greasy feel—its texture is fine schistose, and it has a silky, somewhat shining lustre, the crop fracture being earthy. The breadth of the bed is about six feet; we have then three feet of the rock (*a*), succeeded again by two feet of (*b*), and this series is carried on through several alternations, till covered by the soil at the base on the western side. The schist of the first bed is harder, and more gritty than that of the succeeding. The slopes of the hills are strewed by rolled stones, which must have been transported thither from some distance. These consist principally of conglomerates of a ferruginous nature; and several specimens of iron and manganese ores, from the same situation, were shown to me by a friend, to whom I am much indebted for the assistance which he has given me in drawing up this short account. Numerous quartz veins are observed to traverse the rocks of this range: they, generally speaking, dip to the N. E.

To the west of this position the plains are still covered with a deep soil, through which, occasionally, protrude hills and hill ranges, many of which exhibit a bold and craggy outline. The predominating rocks are different modifications of quartz rock, many of which are ferruginous.

Near the ancient city of Biána, which lies 50 miles W. S. W. from A'gra, there occurs a series of alternations of a ferruginous quartz rock, with a peculiar conglomerate, containing imbedded agates, agate jaspers, &c. &c.: these occupy the rugged termination of a hill range, which stretches from this point in the direction of Ajmér. Into a description of the Biána rocks I shall not enter at this time: they form in themselves so interesting a subject, that they are well deserving of a separate consideration:—as surface rocks, at least, they occupy but a small space in the Bhartpúr district; but they may, perhaps, be continued on into the Gwáliar country:—at least agates, agate jaspers, and conglomerates of different kinds would appear to be of common occurrence in that district. I have been told that copper mines were at one time worked somewhere in the left of the *submedial rocks*, described as flanking the Bhartpúr district. The exact locality of these I am not acquainted with. Iron, too, is of abundant occurrence in the same belt, and might, perhaps, be manufactured with advantage.

From the above description it would appear that the Bhartpúr district is situated, geologically speaking, to the east of the Jeipúr branch of the great primitive formation of Central India, and that it is separated from this branch by a belt of transition rocks. The newer sandstones of this district would also appear to belong to a great series of rock formation, which has been traced through a large portion of Hindustán, and which forms, with but little interruption, the north-eastern and southern barriers of the valley of the Ganges and Jumna.

This series is probably continued on, both to the north and south of the primitive branch just alluded to, making a sweep on one side into the Penjáb, and on the other into Haraúti, Málwa, and Méwár, where it takes a turn to the south, and is still seen skirting the primitive strata of the last district:—the older sandstones which have been described as occurring in the Bhartpúr district, have, in other situations, been observed to basset out through the newer sandstones; and future observation may, very probably, discover, in this portion of the country, the out-croppings of other strata, which may complete the series between the old and new red sandstones of the English geologists. Rocks of the *submedial order* would also appear to be very generally interspersed between the newer strata and the primitive formations of Central India, both on the north and south of the Jeypúr basin.——This is a very vague and general description, but I have already trespassed too much on your space and on the time of your readers, and I shall, therefore, conclude for the present. I.

III.—*On the Measure of Temperature, and the Laws which regulate the communication of Heat.* *By* M. M. Dulong *and* Petit.

[From the Journal de l'Ecole Royale Polytechnique, T. xi.]

§ 3. *On the Expansion of Solids.*

If we compare the results of the preceding table[1] with those which we have recorded in Table I.[2], it will be perceived that the suspicions entertained, as to the correctness of the mercurial thermometer, were not without foundation. It will be found that the rate of expansion of the glass tube, and of the fluid contained in it, are very remarkably different in the case of a considerable change of temperature. When the air thermometer indicates 300, mercury, taken singly, would give 314,15; while mercury in glass (the ordinary thermometer) would only mark 307,64.

The results established in the preceding section are the more valuable, inasmuch as they enable us to obtain a very exact measure of the expansion of solid substances. We have only, in fact, to measure the difference of expansion between mercury and each of these substances.

The application of this method to glass is sufficiently simple; the difference in question, being the apparent expansion of mercury in a vessel formed of that material. Although this question has been made the subject of so many experiments, we have thought it necessary to determine it also ourselves, and with every precaution that this sort of inquiry will admit of. A glass tube, about 6 decimetres in length, (23,64 inch,) and capable of holding about 700 grammes (10811, grains) of mercury, was made use of. It was closed at one extremity, and at the other terminated in a capillary tube, the capacity of which, compared with the large one, was so small as to allow the correction for it to be safely disregarded.

The tube being filled with mercury, and carefully purged of air and moisture, by repeated boilings, we determined the weight of mercury, driven out by a change of temperature from the freezing to the boiling point. An idea may be obtained of the accuracy of which this method is susceptible, by considering, that the portion of mercury which is not heated is exceedingly small, while the horizontal position of the tube enables us to apply the correction of the boiling point, dependent on the change of barometric pressure.

Five repetitions of this experiment, using different materials, have given, very nearly, the same results as are expressed in the following table. No sensible difference was observed between tubes formed of different kinds of glass, whatever the size or thickness of their sides.

The values of apparent expansion at 200° and 300°, (cent.) have been deduced from the comparisons previously made between the mercurial and air thermometers.

TABLE 3.

Temperature as shewn by an air thermometer.	Mean apparent expansion of mercury in glass.	Absolute expansion of glass in volume.	Temperature, as shewn by a thermometer; supposing the expansion of glass to be uniform.
100	$\frac{1}{6480}$	$\frac{1}{38700}$	100°
200	$\frac{1}{6378}$	$\frac{1}{36300}$	213,2
300	$\frac{1}{6318}$	$\frac{1}{32900}$	352,9

The first two columns of this table do not require any explanation. The apparent expansion of mercury in glass, between 0° and 100°, will be observed to be a little less than the determination of M. M. Lavoisier and Laplace, which is $\frac{1}{6459}$. We were prepared to expect this difference; for in the work in which they have given an account of their experiments, the authors have particularly stated that they believed this number to be in excess, as they had neglected the precaution of boiling the mercury. The absolute expansion of mercury must have been, for the same reason, in excess; and this is conformable to what we see in Table 2[2]. The third column gives the expansions determined, as above explained. This expansion is not uniform, but increases with the temperature between 0° and 100°; it agrees with the determination of M. M. Lavoisier and Laplace, obtained in a more direct manner. Finally, the last column contains the indications which would be afforded by a thermometer made entirely of glass. We may see by the difference which is found, even at a temperature of 300°, how far glass is from being uniform in its expansion.

By similar means it may be thought we could determine the expansion of iron; namely, by enclosing the mercury in a vessel composed of this metal; but our at-

[1] Vol. i. p. 363. [2] Vol. i. p. 35.

tempts, in this way, not being quite successful, we had recourse to the following method. Into a tube of glass, 18 millimetres in diameter, (,7092 inch) and 6 decimetres (23,64 inch) in length, closed at one of its extremities, we introduced a cylinder of soft iron, fixing it in the axis of the tube by four small pieces, the length of which was about equal to its diameter. After joining on to the extremity of this tube, a second one, having a capillary bore, we filled it with mercury, which we subjected to ebullition during an interval sufficiently long to ensure the complete extrication of air and moisture. By afterwards exposing it to different temperatures, and determining the weight of the mercury driven out, it was easy to deduce the expansion of iron, the volume of the former being manifestly equal to the sum of the expansions of the mercury, and of the metal *minus* that of the glass. To calculate this result, however, we should know what the volumes of the three bodies would be at the freezing point. Now, that of iron is obtained by dividing its weight by its density at the freezing point, the volume of the glass is in the same manner deduced from the weight of the mercury, which it will hold at that temperature; while that of the mercury is evidently the difference between the two.

The arrangement we have just explained, may be applied in the case of other metals, by adopting the simple precaution of oxidating their surface, to prevent the dissolving effect of the mercury. We may easily satisfy ourselves that the layer of oxide is of too great tenuity to effect, in the slightest degree, the accuracy of the results. We have found this method, in fact, answer remarkably well in the case of brass, and we should have decided on employing it with other metals, if a wish to verify our results had not determined us to attempt a method something different.

It is known that the complicated nature of the apparatus required for the measurement of expansion in solid bodies, is occasioned by the necessity of having part of it absolutely fixed and immoveable. But when the true expansion of any one substance is known, we may easily deduce that of others, by observing the changes in a pyrometer formed of two rods, firmly united together at one of their extremities: a condition easily fulfilled.

Instruments of this kind have been employed by Borda and by Deluc. But the first having only observed the effect of low temperatures, (which was sufficient for the object he had in view,) we can draw no conclusions as to the particular question we are here considering. As to the experiments of Deluc, they appear to us to include a source of error sufficient to occasion well founded doubts as to the accuracy of the results he obtained.

His apparatus was composed of two vertical rulers, the one of glass, the other of brass, firmly united by their inferior extremities. The rods were of lengths reciprocally proportional to the respective expansibilities of the two substances. The longer, which was the glass rod, was fixed by its upper extremity; but that of the brass rod was entirely free. It results, therefore, from the relation which the dimensions of the two rods bore to each other, that this end would not suffer any derangement, to whatever temperature the instrument be exposed, *supposing the expansions of the two substances to be proportional.* Now, Deluc observed, that when the compensation for a certain change of temperature was once adjusted, it remained unaffected by any further change, whether great or small.

But it is not difficult to perceive that the water bath, in which the rods were immersed vertically, being of great depth, the inferior strata of the fluid must always have been colder than those above, whatever care might be taken to agitate it. And as the brass rod occupied the lower half of the vessel, whilst that of glass was immersed through the whole depth, we may attribute the increasing expansion which the glass manifested, to the circumstance of the brass rod being always at a lower temperature: this source of error increasing with the temperature. The error might, however, have been got rid of, by performing the experiments with the rods in a horizontal position; or neutralised, by repeating them, and reversing the rods. But Deluc, not being aware of the objections we have stated, made no attempt whatever to verify his results.

A compensation pyrometer, like that of Deluc, offering, as we thought, no particular advantages at low temperatures, while at high temperatures they are exceedingly difficult to manage, we found it requisite to adopt a different arrangement, which we shall now describe.

The rulers which we used are all 12 decimetres (47,28 inches) in length, 25 millimetres (,985 inch) in breadth, and 4 millimetres (,1576 inch) in thickness. To compare their expansibilities, we have only to connect them in a perfectly secure manner at one extremity, by means of a cross piece, secured by strong screws. Each rod carries attached to its other extremity, a small stem of brass, which rises, first vertically, and is then bent into a horizontal position. The horizontal branches of these two pieces are furnished, one with a scale, divided into fifths of a millimetre ($\frac{1}{125}$ inch), and the other with a vernier, which subdivides each of these into 20 parts: this will allow of our estimating hundredths of a millimetre, ($\frac{1}{2500}$ inch) equal to the one hundred and twenty thousandth part of the length of the rods. This fraction answered, in most of the experiments we conducted, to a change of temperature of about one degree; and as it is impossible to err by one part of the vernier, with the least practice in reading the divisions, we may see that each separate determination, even in the highest temperatures, has never been affected by an error of a single degree. Such a degree of exactness appeared to us to be sufficient in researches such as ours. Add to which, that to give greater sensibility to such an instrument, its size must be increased; and in that case the difficulty of establishing a uniform degree of heat, would have occasioned much greater uncertainty, as to the real amount of the expansion.

The two rulers rest on four brass rollers, attached to a bar of iron. The whole apparatus is placed in a trough of copper, 14 decimetres in length (55,16 inch), 15 centimetres (5,91 inch) in depth, and 10 centimetres (3,94 inch) in breadth. In these new experiments, as in those which have been made on glass, we used a fixed oil; and we had recourse to the same means, to render the temperature stationary, during a period of time sufficiently long for the rulers to acquire the temperature of the liquid. There were also on each side of the vessel, arrangements of metallic vanes, which could be set in motion, so as perfectly to mix the different parts of the liquid mass, and to establish, throughout, uniformity of temperature, and this without deranging the rulers. Finally, the copper trough was closed by a cover having four openings, in which were fixed thermometers, by which might be detected any difference of temperature which might have occurred in the different parts of the mass. A thermometer placed horizontally between the bars, indicated the true temperature of the liquid.

For the basis of our comparison we have taken the state of the rulers in the oil-bath, which had been left for several days in a room where the temperature did not sensibly vary. This is preferable to the employment of the freezing point, as a term of comparison; the latter giving a fixed point, only under the precaution of continually agitating the mass, and this particularly when the surrounding air is at 15° or 20° above zero.

The bath being heated to a temperature sufficiently near that which was required, all the openings of the furnace were shut. The heated mass being, in this case, considerable, the maximum of temperature was maintained long enough to allow of the rulers acquiring the temperature of the oil, especially as the agitation continually renewed the surfaces of contact. This conclusion was further confirmed by the steadiness in the indications of the vernier.

Such was the order of our proceeding in all the experiments we made according to this new method. The various precautions which it was necessary to attend to, have prevented us repeating them with many substances; but the results we have obtained, have been determined from experiments sufficiently numerous to warrant every reliance on their accuracy.

In our first series we combined platina with brass. The expansion of the latter substance for every degree of the thermometer being known to us, the observations of the pyrometer enabled us to settle those of platina with the greatest accuracy, equally in the lower as in the higher temperatures. In a second series, we united a glass ruler with one of brass of equal length. This combination of two substances, the expansions of which we had determined, afforded us the means of verifying the operations on which those determinations rested: we, therefore, neglected no precaution that could ensure accuracy in this comparison. We had, however, to overcome a difficulty which does not occur in experiments on metals: it was this. The rulers it was necessary should be firmly connected, which could only be effected by means of screws. But it is well known, that it is nearly impossible to apply pressure to effect the close union of a metallic and glass surface, however carefully they may have been prepared, without fracture of the glass. In our first attempts we interposed paper, exposed previously under strong compression, in a vice to a heat of 300°. But the rulers did not appear to us, on examining them after the experiment, to have been sufficiently well fixed to exclude all suspicion of error. We, therefore, substituted very thin plates of fine silver, and found them to answer the purpose perfectly. The results given by the pyrometer have fully confirmed our previous determinations of the expansion of brass and glass.

We have included, in the following table, the results of all these experiments. There will be found in it the mean expansion of iron, of brass, and of platina, taken between 0° and 100°, and between 0° and 300°. We have not applied ourselves to determine any intermediate expansions, our views having been restricted to the enquiry, how far the different thermometric scales differ amongst themselves. But to give a clearer idea of the results, we have added a column, exhibiting the temperature which each substance would indicate, on the supposition that the expansion is uniform. These temperatures are such as would be indicated by thermometers constructed of each of these substances.

TABLE 4.

Temperature, as shewn by an air thermometer.	Mean expansion of iron.	Temperature, as shewn by a thermometer of iron.	Mean expansion of brass.	Temperature, as shewn by a thermometer of brass.	Mean expansion of platina.	Temperature, as shewn by a thermometer of platina
100°	$\frac{1}{28200}$	100°	$\frac{1}{19400}$	100°	$\frac{1}{37700}$	100
300	$\frac{1}{27700}$	372,6	$\frac{1}{17700}$	328,8	$\frac{1}{36300}$	311,6

These results, compared with those we had previously obtained for glass, prove, contrary to the general opinion, that the expansion of solids, referred to an air thermometer, increases with the temperature, and in a ratio different for each substance.

We think we have attained, in this enquiry, all the accuracy that can be looked for in such delicate operations, and our readers may assure themselves of this, by comparing the numbers we have given for the first 100°, with those obtained by M. M. Lavoisier and Laplace. We will add but one remark, which is, that in attempting to determine the direct expansion of solids, any little uncertainty in the result is tripled in reducing the linear expansion to that of the volume. But our experiments give us, at once, the expansion in volume: whatever the error be, therefore, of our experiments, it is not multiplied in the results.

IV.—*On the Longitude of Benares, as determined by a series of Lunar Transits.*

Of all the methods of finding the Longitude, derived from the Moon's position in her orbit, at a given instant, it must be allowed that the observation of her transit over the meridian, promises, on shore, the best chances of accuracy; both in regard to the steadiness and delicacy of the instruments which may be employed, the almost simultaneous correction of the time, the simplicity of the calculation, and the frequent repetitions which (in this climate at least) may be commanded. For comparative observations, under meridians not far asunder, it affords the further advantage of rendering us independent of the errors of the Lunar Tables. Still, however, there must be always a limit of accuracy in the slow motion of the moon itself: as she performs only a twenty-fifth part of a revolution in a day, any error in the noting of the time of her passage will be magnified twenty-five fold in the resulting longitude; and if, therefore, a quarter of a second in time be assumed as a probable error of observation, the limit of accuracy, in the longitude deduced, will be six seconds in time, or a mile and a half in space.

But this is no great amount; nay,—twice as much would be still considerably within the limits of error of other methods of determination, such as the eclipses of Jupiter's Satellites; where differences of 20 and 30 seconds in time, or seven and eight miles, are by no means avoidable.

In the year 1827, I had, for a short time, an opportunity of judging of the practical facilities of this method; for, with a transit telescope of small power, by no means a superior instrument, I found no difficulty in obtaining results agreeing among themselves within one mile, and seldom more than three miles at variance under unfavorable circumstances.

I was fortunate also in enjoying the co-operation of the Observatory at the Surveyor General's office, which enabled me to check any errors arising from the Lunar Tables. I deferred, however, at the time, the publication of the results, hoping that an opportunity of continuing them would have occurred.

That I should, at this late period, seek to rescue the observations from oblivion, is due to the kindness and patience of a military friend, well known as an indefatigable calculator and astronomer, who has taken the trouble to recalculate, with great care, the whole of the Benares and Calcutta series, for our mutual satisfaction. My friend G. is so well pleased with the results, that he writes in strong recommendation of the method :—

"I think you will agree with me, that two observers, with good transits and clocks or watches, and attending to nothing else but the moon and stars, for the period even of a week or fortnight, would deduce a far better difference of longitude, and much closer observations, than from a whole year's sights of Jupiter's Satellites; for the thirteen obsevrations I have compared, in two instances only, differ half a second from the mean; as you will perceive by deducting the longitude of Benares from that of Calcutta; and the greater part of them do not vary above a single mile in space, which is certainly nearer than Jupiter's Satellites give, even by the best telescopes and observers in the finest weather."

In Table I. I have given the series of observations at Benares and in Calcutta, whence the calculations of Table II. have been made. I worked them out roughly myself, by means of a single formula, finding first the exact Greenwich time of the observed passage of the moon's centre, and then calculating the right ascension of the moon and sun, *for the same moment*, from the Nautical Almanack. I have inserted the differences of the observed and calculated transits, as found by myself, in the second column, by the side of those deduced from the more laborious calculations of G., to which latter the chief reliance must be given, and which alone are used in the subsequent computations of the longitudes in columns 3, and 6.

It is necessary to point out that my observations were entered in mean time, whereas those of Calcutta were given to me, with the apparent time duly worked out: I have not thought it worth while to make the alteration.

Of the Benares series, seventeen are esteemed good, five are indifferent, and three only are rejected: of these, one was marked as uncertain by myself, whereas the others are entered as good observations:—perhaps some error has found its way into the register or the calculation.

Supposing the moon's place, as given by the Nautical Almanac, to be correct, the longitude of my observatory at Benares, from the seventeen best observations marked *a*, will be found, $5^h\ 31^m\ 55^s.2$

from twenty-two, or including the five observations *b*, $5\ \ 31\ \ 57.$

The discrepancies in the first set, extend to about four miles, or half a second in the observations: those of the *b*'s to nearly double that amount. As my observatory was precisely 6^s W. of the Hindú observatory, the latter will stand in Long. $5^h\ 32^m\ 01^s.2$. By Reuben Burrow, it is $5^h\ 31^m\ 59^s$.

The Calcutta Longitudes, calculated also from the Nautical Almanack, and rejecting the observations of the 6th June and 1st November, have a mean of $5^h\ 53^m\ 50^s.9$, which differs 16.9 seconds from the position of the Surveyor General's office, in Park Street, Chowringhee, as estimated from Reuben Burrow's Longitude of Rasapagla $5^h\ 53^m\ 30^s$. The approximate longitude of the office, as found from other sources, up to the period of the lunar transits, was only $5^h\ 53^m\ 16^s.31$.

The mean differences of meridian, rejecting the four observations widest of the truth, is $21^m\ 46^s.6$. The extremes differ 33 seconds in time, "which is less than the cotemporary observations at the Royal observatories of Greenwich and Paris, made under the most favorable circumstances, with large and accurate instruments[1]." By Reuben Burrow, the difference of meridians (corrected for place of observation) would be $21^m\ 41^s$.

In the years 1823-24, I published a series of observations of the Eclipses of Jupiter's Satellites, made also with a view to determine the longitude of Benares, and I feel somewhat at a loss to account for the great discrepancy in the results of the two modes. For the sake of comparison, and to shew how cautious we should be ere we set down a longitude as established, I shall beg to trouble you with the Satellite abstract, reduced to the same meridian as that in which the transits were taken.—(6^s W. of Benares Hindú Observatory.)

	h m s
Mean of 10 Immersions I. Satellite by the *Conn. Tems.*	5 31 37.7
10 Immersions, ditto,	5 31 33.0
Mean of 11 Imm. and Em. II. Satellite, by *Nautical Almanack*,	5 32 13.9
Mean of 5 Imm. and Em. III. ditto,	5 32 28.4
Mean of 15 coincident observations at Madras,	5 31 43.9
Mean of 5 coincident observations at Bushey Heath,	5 31 38.8
Mean of the whole,	5 31 52.63
Mean, rejecting the II. and III. Satellites,	5 31 38.38

The greatest variation from the mean (for the first Satellite) was 28^s—the average deviation 15^s. There was sufficient accordance among these, strengthened by the comparative sights of Madras and Bushey Heath, to warrant

[1] Does not this prove too much?—Ed.

my then assuming that the longitude was very nearly $5^h\,31^m\,40^s$, which I accordingly adopted in the lunar transit calculations :—The present results, however, again throw uncertainty upon the matter, as a difference of 17 seconds, upon the average, is too great to be attributed to mere irregularities.

	h m s.
If, however, we imagine Reuben Burrow's Longitude of Calcutta to be correct ; namely,	5 53 34
and deduct thence the lunar meridl. difference, as found from the obs. viz.	21 46.6
we find the longitude approach much nearer the Satellite result, viz.	5 31 47.4

In this case there should be an error in the Nautical Almanack right ascension of the moon of $\frac{8}{25}$, or about one third of a second in excess ; a point which I am unable to determine, for want of access to the Greenwich observations of 1827.

Leaving, however, the final adjustment of the absolute longitude until we possess more ample data, I think enough has been said to prove, that whoever possesses a transit instrument may turn it to the greatest advantage, by taking every opportunity of registering the passages of the moon. For small differences of meridian, they afford comparative results, almost independent of the errors of the Lunar Tables : but to obviate this last source of inaccuracy, as we are at such a distance from the regular observatories of Europe, I take the liberty of suggesting, that any of your readers who may be engaged in astronomical observations of this or any other nature, would confer a general benefit, by publishing, occasionally, in the Gleanings, the results of his otherwise fruitless labours. In the archives of the Surveyor General's Office, I suspect, from the specimen with which I have been favored, there must be much valuable information in astronomical science collected since 1827, and I hope that the present work may be allowed the honor of rescuing it ere the season of harvest has passed by.

I have omitted to mention in the course of the above note, that the longitude of my observatory at Benares, according to the late Trigonometrical Survey, was found 83° 1′ 47″.5, or in time $5^h\,32^m\,07^s.1$, which is even in excess of any of the above results. When the triangulation shall have reached Calcutta, a good opportunity will be afforded of reconciling all that at present admits of no satisfactory decision in the geographical position of the Holy City.

P.

TABLE I.

Observations of the Moon's Transit.

At Benares.					*At Calcutta.*		
Date.		Mer. Trans. of bright limb in Ben. mean time.	Limb.	Remarks.	Mer. Trans. of bright limb in Cal. appt. time.	Limb.	Remarks.
1827.		h. m. s.			h. m. s.		
June	1	5 00 20.6	1	doubtful.	.. .. ..		
	4	7 19 12.7	1	good.	.. .. ..		
	5	8 09 33.6	1	ditto.	.. .. ..		
	6	9 03 21.1	1	ditto.	9 04 18.32	1	
	8	11 02 15.0	1	ditto.	.. .. ..		
	9	12 05 40 0	1	ditto.	.. .. ..		
		12 08 09.1	2		12 08 25.61	2	
	10	13 11 36.6	2		.. .. ..		
	13	16 04 51.3	2	(3d wire.)	.. .. ..		
	17	19 14 50.0	2	bad.	.. .. ..		
Oct.	23	2 10 05.5	1	very good.	.. .. ..		
	25	4 10 09.7	1	3 wires.	4 24 59.29	1	
	27	6 02 30.7	1	moderate.	6 17 37.44	1	
	28	6 55 23.4	1	good.	7 10 38.22	1	
	30	8 34 50.5	1	good.	8 50 16.93	1	
Nov.	1	10 10 08.6	1	good.	10 25 43.05	1	
	2	10 57 46.0	1	3 wires.	11 13 18.48	1	
	3	11 45 48.2	1	1st limb under			
		11 47 57.1	2	eclipse.	12 03 19.10	2	
	24	4 51 27.0	1	good.	5 03 52.36	1	
	25	5 43 30.7	1	mod.	5 55 40.00	1	
Dec.	1	10 29 18.8	1	good.	10 39 15.27	1	
	2	11 17 34.2	1	mod.	11 27 15.16	1	
	3	12 06 04.5	1	very careful.	.. .. ..		
		12 08 14.6	2		12 15 22.91	2	
	10	17 28 37.7	2	very good.	.. .. ..		
	11	18 13 13.5	2	good.	18 19 00.24	2	
	12	18 59 25.9	2	ditto.	19 04 42.08	2	
	13	19 48 10.2	2	ditto.	19 52 53.80	2	

TABLE II.

LONGITUDES CALCULATED FROM TABLE I.

Benares Assumed Longitude 5h 31m 40s. *Calcutta Assumed Longitude* 5h 53m 30s.

Date.	Observed Trans. + or — of calcd. Mer. pass. moon. by G.	by P.	Longitude deduced.	Remarks. *a* best, *b* mod. *c* bad.	Obsd. Transit + or — calculd. Transit.	Longitude deduced.	Remarks.	Difference of Meridians.	Variation from mean difference.	
1827.	s.	s.	h. m. s.		s.	h. m. s.		m. s.	s.	s.
June 1	+ 0.17	+ 0.5	5 31 34.4	*b* doubtful.						
4	— 0.66	— 0.1	31 59.8	*a* good.						
5	— 0.28	— 0.1	31 48.0	*a*						
6	— 0.48	— 0.0	31 52.8	*a*	— 1.81	5 54 18.3	rejected.	22 25.5	+	
8	— 0.78	— 0.3	31 58.7	*a*						
9	— 0.31	— 0.5	31 47.3	*a*	— 0.86	53 50.3	not same limbs.	22 3	+ 16.4	+ 4.06
10	— 0.14	— 0.7	31 43.4	*a*						
17	+ 0.78	+ 0.4	31 14.4	*c* bad.						
October 23	— 2.26	— 1.8	32 37.2	*c*						
25	— 1.67	— 0.7	32 22.3	*b*	— 1.59	54 10.3		21 48	+ 1.4	+ 0.21
27	— 0.77	— 0.5	32 01.4	*a*	— 0.83	53 53.0		21 51.6	+ 5.0	+ 1.15
28	— 1.06	— 0.4	32 10.7	*a*	— 0.63	53 48.2		21 37.5	— 9.1	— 2.16
30	— 1.54	— 0.1	32 27.4	*b*	— 0.89	53 57.4		21 30	— 16.6	— 4.09
Nov. 1	— 2.77	— 1.1	33 06.5	rejected.	+ 0.15	53 25.3	rejected.	20 18.8	....	
2	— 1.05	— 0.6	32 12.6	*a* 3 wires.	— 0.94	53 59.2		21 46.6	0.	
3	+ 0.15	— 0.1	31 35.4	*b* under eclipse.	— 0.18	53 35.5		22 00.1	+ 13.5	+ 3.22
24	— 1.04	— 0.5	32 08.9	*a*	— 0.84	53 53.3		21 44.4	— 2.2	— 0.33
25	— 0.92	— 0.2	32 07.6	*a*	— 0.98	53 59.4		21 51.8	+ 5.2	+ 1.18
Dec. 1	— 1.34	— 0.7	32 21.9	*b*	— 0.75	53 53.4		21 31.5	— 15.1	— 3.46
2	— 0.09	— 0.1	31 42.8	*a*	— 1.07	54 03.1		22 20.3	+ 33.7	+ 8.25
3	— 0.42	— 0.5	31 52.9	*a* careful.	— 0.25	53 37.7		21 44.8	— 1.8	— 0.27
10	— 0.27	+ 0.1	31 49.1	*a*						
11	— 0.30	— 0.4	31 49.8	*a*	— 0.44	53 44.4		21 54.6	+ 8.0	+ 2.00
12	— 0.38	— 0.6	31 51.9	*a*	— 0.25	53 37.7		21 46	— 0.6	— 0.09
13	— 0.04	— 0.5	31 41.1	*a*	— 0.36	53 40.6		21 59.5	+ 12.9	+ 3.13

V.—*On Raising Water.*

To the Editor of Gleanings in Science.

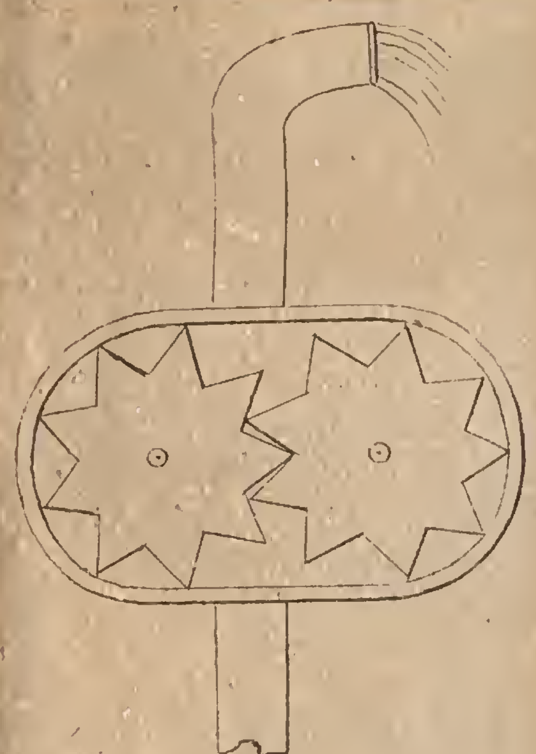
Sir,

As my line of life and situation in the Mofussil, afford opportunities of useful observations upon some of the topics on which your correspondents touch, I send you the following remarks, hoping they may be acceptable to some of your readers.

I shall begin by remarking that your correspondent, who writes on the relative value of lands between India and England, only takes one crop into consideration! He, I suppose, is a resident of the up country, but I beg to remark, that in this quarter the lands average nearly four crops in the year, (giving from two to six,) and *are never allowed to lie fallow.* The profits of all these crops must be calculated before a just comparison can be made.

With respect to the best method of raising water, the China Pump far excels all others as yet in use in this part of the country, for Indigo purposes. I have filled a loaded vat, measuring 22 feet by 22, and 4 feet deep, with 14 men, in 23 minutes, raising the water above 15 feet perpendicular height, by the China Pump. The same number of men on the bucket pump took 32 minutes, while I believe the bullock (bucket pump) of my neighbour took 40. The plant in the vat I suppose occupied one-third of the space, but cannot say sufficiently correct to calculate the quantity of water raised. The English Pump is too subject to derangement, or rather too difficult to repair, and cannot be safely trusted to but in large factories, where, if one goes wrong, it will not stop the work.

A reservoir is always necessary where there are bullock pumps; and where there is a reservoir, bullock pumps are by far the best; as we are then independent of coolies.



In Bengal we consider the bullock pump and reservoir the best, but most expensive way of filling vats; the China Pump the quickest; the basket and lever the cheapest, most simple, and least subject to derangement. The *mót*, in your number for January, will not throw water high enough for indigo purposes, without two lifts; which would be disadvantageous, although one beam would be sufficient to work both. The Cylinder Pump[1] you introduced to our notice is probably far superior to any we have at present in use; should any of your correspondents be acquainted with its powers, he would much oblige us by communicating information on the subject:—even that pump is far from perfect; it has too many cog wheels; the pivots, or paddles, which raise the water, are too far from the fulcrum, thereby wasting power, and a loss is sustained by water escaping through the curvatures along with the pivots; to remedy which, I would propose using two cogs instead of cylinders, as in the accompanying sketch, (No. 1), by which one wheel would be avoided, the power increased, and the size of the outer cog wheels might be much diminished. A tread mill, like that used for the China pump, might be applied to it with advantage.

I shall conclude by enquiring of some of your scientific correspondents, if 8 or 10 inches of a worm screw, contained in a pipe, immersed in water, and turned by a long rod, would not throw up water, and to what height it would raise it?

I remain, Sir, your most obedient servant,

An Indigo Planter.

Bengal, March, 1830.

Remark by the Editor. The screw proposed by our correspondent, if we understand him rightly, would raise water, but at an enormous expense of means. For it is evident that the screw, in turning, would not only have to raise the water, but to support the great weight of the whole column filling the shaft. Instead of

[1] The Rotatory Pump of Mr. Jos. Eve, described in our second number.—Ed.

this screw, we would propose that of Archimedes, one of the oldest mechanical contrivances extant, and one which, we should think, might be used with effect, in many situations in this country. A correspondent has sent us an account of an improvement which has been added in France to this ingenious contrivance. It consists in making the screw double, the one being much shorter, but of a larger diameter than the other, and set on in a reverse direction. A fall of water of a few feet, if sufficiently copious, being admitted to the larger but shorter screw, will, by passing through it, turn the machine, and raise a certain quantity through the longer screw. The proportion is stated to be as high as 60 per cent. We shall give this paper at length in our next.

VI.—*On the Velocity of the Wind.*

To the Editor of Gleanings in Science.

SIR,

Having accidentally, while on a visit to this port, cast my eye upon an article in your last year's collection of GLEANINGS, on the subject of the velocity of the wind, I have felt rekindled in my bosom all the ardour of experimenting, which the dull routine of a maritime life, unsupported by sympathy or encouragement, had well nigh extinguished. It used to be my favorite pastime, during the many spare hours of a noisy life, to store up all the information I could obtain from practice and experiment on the subject of navigation and the construction of ships. It was never in my power to measure the actual velocity of the wind, as compared to that of the vessel; but I used uniformly to register its force in the usual sea phrases, which I have no doubt are, relatively speaking, as good as rough graduations of the scale of wind.

Being anxious to contribute my share of labour in uprearing the vast fabric of experimental science, I shall hasten to procure an anemometer, and to make good use of it in my next voyage; but, in the mean time, I beg to present you with an extract from my former private log, thinking that, with the aid of Smeaton's table, it may, in some measure, answer the enquiries of your correspondent Q.

I trust, Mr. Editor, on some future occasion, to find the pages of the GLEANINGS still flourishing, and ready to receive, at second hand, the few new facts which I myself may be able to glean from the wide field of nature's grandest elements.

W. R. N.

In the following observations I have endeavoured to describe the state of the wind, according to Smeaton's classification and experiments. The want of an anemometer prevents me making any more accurate distinctions.

Velocity.	Force on a square foot.	Classification according to Smeaton.	Classification used by me.
1.47 ft.	.005	Hardly perceptible........	Light airs.
2.93	.020	Light airs.	Light breeze, No. 1.
4.40	.044		Ditto ditto, No. 2.
5.87	.079	Fine breeze.	Moderate breeze, No. 1.
7.33	.123		Ditto ditto, No. 2.
11.00	.246	....	Fresh breeze.
16.67	.492	Brisk gale.	Strong breeze, No. 1.
22.00	1.107		Ditto ditto, No. 2.
29.34	1.968	Fresh gale.	Gale, No. 1.
36.67	3.075		Fresh gale, No. 2.
44.01	4.429	Strong gale.	Strong gale, No. 1.
51.34	6.627		Ditto ditto, No. 2.
58.68	7.873	Hard gale.	Heavy gale, No. 1.
66.01	9.963		Ditto ditto, No. 2.
73.55	12.300	Storm.	Storm, No. 1.
88.02	17.715		Ditto, No. 2.

Date.	State of the Weather.	Sails in Action.	Angle of wind with fore and aft line.	Angle of yards with fore and aft line.	Angle of heeling.	Angle of pitching.	Velocity of ship.	Helm.	Force of wind	What tack.	Angle of leeway.	Remarks.	Supposed velocity of wind as felt in the ship.
1823			° '	° '			knots.		Not known for want of an anemometer.				knots.
Oct. 19	Strong breeze.	C., 2-reefed T. S., J., and Sp.	67.30	45.00	7° to 9°	5°.0	7	0		L.	3°.0		12
20	Light winds, No. 2.	C., 2-reefed T. S., T. G. S., J., and Sp.	90.00	45.00	7°.	3.0	2	0		L.	2.0		3
21	Moderate breeze, No. 2.	C., 1-reefed T. S., T. G. S., R., J. and Sp.	72.37	43.00	6.	3.0	5.6	1 W.		L.			5
22	Moderate breeze, No. 1.	C.,T.S.,T.G.S.,R.,Tr.,T. G. S.S., J., & Sp.	82.10	40.00	4.	2.0	8.0	$\frac{1}{2}$ W.		L.			4
23	Steady breeze, No. 1.	C.,2-rfd.T.S.,T.G.S.,J.,Sp.,Tr.,F.T.G.S.S.	67.30	34.00	3.	2.0	6.6	$4\frac{1}{2}$ W.		S.			8
24	Moderate breeze, No. 2.	C., 1-reefed T.S.,T.G.S., Sp., & F. T. S.	67.30	31.50	$4\frac{1}{4}$.	3.0	4.0	$4\frac{1}{2}$ W.		S.			4
25	Light airs, No. 1.	C., T. S., T. G.S., Sp., F. T. S., J., & F. J.	67.30	33.30	$5\frac{1}{2}$.		2.0	2		L.			1.2
26	Moderate breeze, No. 1.	C., 2-reefedT.S.,F.&M.T.G.S.,J. and Sp.	67.30	34.45	7.	4.0	7.0	6 W.		L.			4
27	Fresh breeze, No. 1.	C., 2-reefed T. S., T. G. S., J. and Sp.	67.30	32.00	7.	6.0	6.0	6 W.		L.		Sea meeting the vessel	8
29	Fresh breeze, No. 1.	C., 2-reefed T. S., T. G. S., R., J., and Sp.	67.30	30.00	5.	3.0	6.6	3 W.		S.		on larbd.	8
30	Moderate breeze, No. 2.	C., 1-reefed T.S.,T. G. S., R.,Sp., J., & F. J.	67.30	32.00	5.		7.0	3 W.		S.			5
Nov. 5	Fresh breeze, No. 1.	C., 3-reefed T. S., Sp., and J.	67.30	30.00	6.	2.0	5.5	1 W.		S.		Water smooth.	8
6	Moderate breeze, No. 1.	2-reefed T. S., Sp., J., & F. T. G. S.	67.30	34.00	5.	2.0	7.0	6 W.		L.			4
7	Light breeze, No. 1.	C., 1-rfd. T.S., T.G.S., J.,Sp., F.J., F.T.G.S. S.	67 30	30.00	$2\frac{1}{2}$.		5.4	3 W.		S.			2
10	Moderate breeze, No. 1.	All sail, wind right aft.	00.00	90.00	7 to 7		7.6	0		=		Time of rolling, 5. 4 & 3s	4
11	Fresh breeze, No. 2.	All sail.	135.	46.00	2.		10.6	3 W.		S.		Time of rolling.	8
18	Strong breeze, No. 2.	C., T. S., J., T. G. S., & Tr.	112.30	54.00	3.		12.2	3 W.		S.		Ob. made during a squall	14
20	Fresh breeze, No. 2.	All sail, wind right aft.	00.00	90.00	12 to 12		12.0	2 W.		=		Time of rolling 6s.	8
21	Strong breeze, No. 1.	All sail.	00.00	90.00	15 to 15		12.6	2 W.		=		Did not ob. time of rolling	12
Feb. 4	Moderate breeze, No. 2.	F. C., T. S., T. G. S., R., Tr., J., & F. J.	67.30	39.00	$4\frac{1}{2}$.		4.4	3 W.		L.		Ob. attentively those	5
5	Fresh breeze, No. 1.	Reefed C., 3-reefed T.S., rfed.Sp., & F.T.S.	67.30	32.00	$6\frac{1}{2}$.	5.0	4.6	6 W.		L		causes which may	8
5 P.M.	Moderate breeze, No. 1.	C., 2-reefed T. S., reefed Sp., J., & F. S. S.	67.30	28.00	3 to 6	3.0	3.6	0		S.	12°.	produce a lee helm.	4
25	Strong breeze, No. 1.	C., 2-reefed T. S., & M. Tr.	67.30	34.00	2 to 12	4.0	6.0	6 W.		L.	9°.	Very wet forward,	12
26	Strong gale, No. 1.	3-reefed Topsails.	135.00	63.00	15 to 15	5.0	11.0	3 W.		S.	7°.	compared with 11 Feb.	30
27	Fresh gale, No. 1.	C., 3-reefed Tr. S., Miz. Tr., & M.St.	112.30	70.00	10 to 25	3.0	13.6	0		L.		Extremely wet.	20
28	Fresh breeze, No. 1.	C., 2-reefed T. S., T. G. S., M. T., & F.T. S.	101.15	55.00	5 to 10		8.0	3 W 31.		L.		Extemely wet.	8
Mar. 2	Strong breeze, No. 1.	Reefed C., 3. reefed F. and M.T.S. F.T.St.	65.00	135.00	13 to 35	6 to 7	10.00	6 W.		L.		Compared with 12th Feb.	11
3	Fresh breeze, No. 1.	2d clue of M.S. up, 2-r.T.S., M.T.G.S., & F.C.	90.00	180.00	15 to 15	4.0	9.2	0 to 9		L.		Time of rolling, 3s.	8

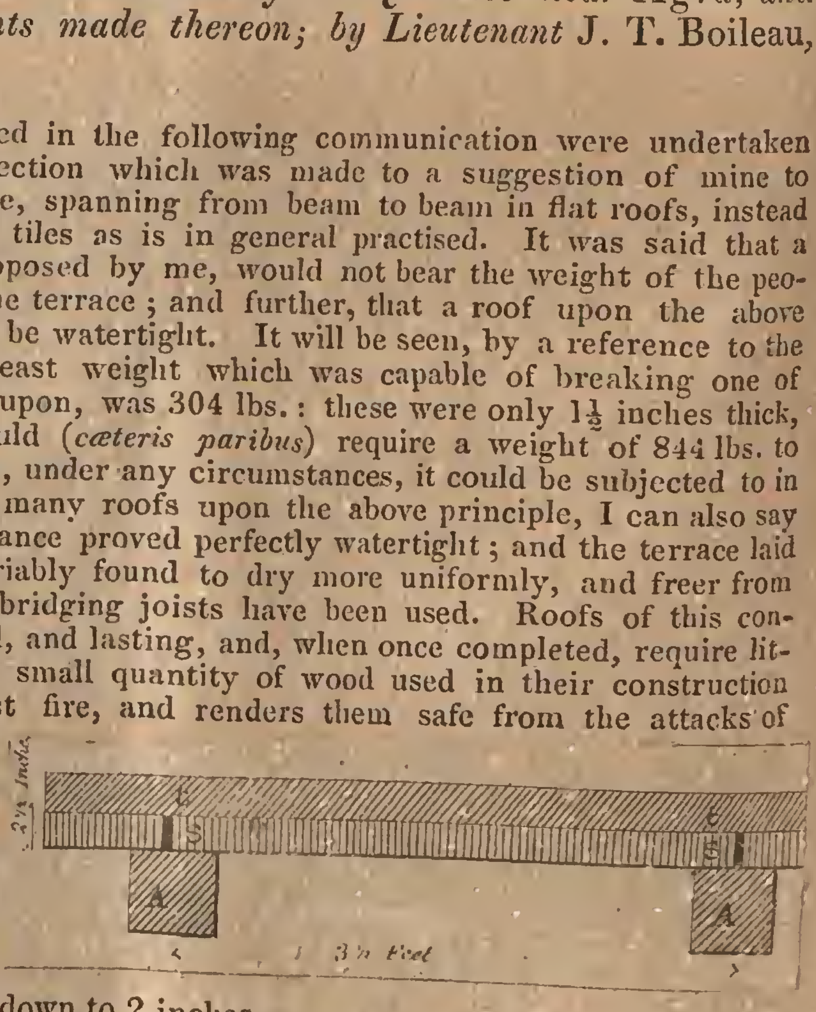
Explanation of the Table. In the above table we have been compelled to substitute some abbreviations, in order to bring it within the compass of the page. C. means courses; M. S. main sail; T. S. topsails; F. T. S. fore-topsail; M. T. S. main topsail; T. G. S. top-gallant sails; J. jib; Sp. spanker; St. staysail; F. J. fore jib; Tr. trysail; R. royals; T. G. S. S., top-gallant studding sails, &c.

In the heading of the columns "Angle of heeling" means the inclination, as measured from a longitudinal vertical plane. "Angle of pitching" signifies the inclination, as measured from a transverse vertical plane. In the column "what tack," L. stands for larboard, and S. for starboard.

Remarks by the Editor. We have added to our correspondent's table, in the last column, the apparent velocity of the wind, roughly derived from his hypothetical estimate of its force and denomination. So far as it is to be trusted, the opinion advanced by Q. is fully borne out; for, with the wind on or before the beam, we find, throughout the table, ample evidence of the vessel's attaining superior velocity. One of the most interesting consequences of this fact, now so nearly established, is the exact correction which it will afford of the position of the ship's vane, and a knowledge of the best disposition of the sails. Thus, when the angle of the wind appears in the table 67° 30′ or 22° 30′ *before* the beam, it is really as much or more *abaft* the beam, and the sails (having an angle of 30° with the fore and aft line by the table,) are really situated very nearly at right angles to the direction of the wind, and therefore in the most advantageous position. But we will refrain from enlarging upon these fruitful subjects of speculation until we are favored with more perfect data; and we need not assure W. R. N. that his meritorious labours shall always meet with a ready reception in the pages of our humble repository.

VII.—*Observations on the Sandstones of the Quarries near Agra, and Results of Experiments made thereon; by Lieutenant* J. T. Boileau, *of Engineers.*

The experiments recorded in the following communication were undertaken in consequence of an objection which was made to a suggestion of mine to substitute slabs of sandstone, spanning from beam to beam in flat roofs, instead of the *bergahs*[1] and roofing tiles as is in general practised. It was said that a slab 2½ inches thick, as proposed by me, would not bear the weight of the people employed to beat down the terrace; and further, that a roof upon the above principle would not probably be watertight. It will be seen, by a reference to the annexed tables, that the least weight which was capable of breaking one of the specimens experimented upon, was 304 lbs.: these were only 1½ inches thick, and a slab of 2½ inches would (*cæteris paribus*) require a weight of 844 lbs. to cause a fracture; more than, under any circumstances, it could be subjected to in roofing. Having completed many roofs upon the above principle, I can also say that they have in every instance proved perfectly watertight; and the terrace laid over the stones I have invariably found to dry more uniformly, and freer from cracks, than in roofs when bridging joists have been used. Roofs of this construction are cool, economical, and lasting, and, when once completed, require little attention afterwards: the small quantity of wood used in their construction makes them secure against fire, and renders them safe from the attacks of white ants. The accompanying sketch represents a section across the girders of a roof of this construction, where A. A. are the girders; *s. s.* the slabs of sandstone, rabbeted together, and cemented with fine lime; *t. t.* the terrace laid on 4 inches thick, and beaten down to 2 inches.



The apparatus used in conducting the experiments, consisted of two pillars of bricks and mortar, distant 4 feet in the clear; a scale, and a round bar of iron. The specimens were all 4½ feet long, 12 inches broad, and 1½ inches thick, having a bearing on the pillars of 3 inches at each end. The ends were free to move upwards, on the application of the weights, which were laid on at slight

[1] Bridging joist.

intervals successively: the heaviest (about 20 lbs.) first, and continued until the fracture took place; the lesser weights varied from 5 to 10 lbs.

The first set of experiments was made upon slabs in their dry state; the second after the specimens had been immersed in water until saturated; excepting, however, specimen 12, which was only immersed for two days, and as it gained in weight only $2\frac{3}{4}$lbs., it is probable that, though wet through, it was not completely saturated: its superior strength, compared with that of the other specimens, favors this opinion.

The application of this material in roofing has been very general at and about Agra; and in the old palaces in the fort there are some very curious combinations; descriptions of which, with drawings, I shall communicate to you in another paper. To those who may wish to use sandstone, either as architraves for doors or colonades, or in roofing, the annexed tables, though limited, are sufficiently copious, and furnish every information relative to transverse strength. A more complete set of experiments on the stiffness and direct cohesive force of the same material are in course, and the results shall be forwarded to you as soon as possible; in the mean time, it is to be hoped, that those in whose neighbourhood quarries exist, will be induced to follow up the subject, and publish the results of their experiments in your valuable periodical.

At Allahabad and Chunar there are quarries; and as both these stations are conveniently situated for water carriage, it would be particularly desirable to ascertain the capabilities of their several productions.

A paper on the geology of the sandstone districts having been already prepared by an able geologist and acute observer, I shall pass over this part of the subject, mentioning only that the sandstone in use here is quarried at Rúpbás, Púhárpúr, and other villages lying W. S. W. from Agra, and distant about 35 miles. There are two principal varieties, (geologically three;) the red, which is laminar, and the white or salmon colored, which is compost, and very finely granular: the latter is obtained with difficulty in slabs less than 4 inches thick; the former may be had in plates of less than $\frac{1}{2}$ an inch.

By a reference to the tables it will be seen that the red sandstone does not lose much, if any, of its strength by being saturated with water, which renders it particularly fit for the purposes of roofing; and though, in its dry state, it is far inferior to the salmon colored variety, (as 11 to 17 nearly,) yet when wet, it is something superior to it, (as 11 to 9 nearly,) the latter losing about one half of its strength by immersion in water: for the cause we may probably look to the different arrangement of the component particles of each variety. In the first, they are arranged in parallel and contiguous plates, and derive very little strength from the cementing paste in a lateral direction, as is evident from the readiness with which they yield to cleavage in this direction; in the second, they are entirely dependant upon this paste for their strength, both in a lateral and other directions, the particles being arranged without any apparent regularity, and being retained in position merely by the paste which unites them: when this, therefore, becomes softened, or partially so, by water, the cohesion is lessened or destroyed, and the strength of the material proportionably impaired. A stone architrave lately came under my observation, which had failed, in consequence of being wetted, though it carried its load well enough when dry.

Both varieties of this sandstone have been very largely employed in buildings at and about Agra, for the last two centuries. The fort of Agra, the tomb of the Emperor Akber at Secandra, together with its gateway, the stupendous gateway of the shrine of Selím Chisti at Fattehpúr Sikri, the *jamma musjids* and palaces of Agra and Dehli, the gateways of the *táj* at Agra, and the tombs of Hamaiun and Sefdar Jang, together with the famous *cutub minár* at Dehli, are almost entirely constructed with the red variety. The palaces in the garden at Díg, (among the most beautiful specimens of oriental architecture extant); the new *cachéri* at Bhartpúr; the two famous temples at Mattra, one of which is only just completed; and the beautiful tombs of Súraj Mal and Baldéo Sing, at Gowardhan, are examples of the salmon colored sandstone. The roofs of some of these buildings (particularly the *Gopál Bhówan* at Díg) are very wide, but are, nevertheless, covered in by a series of single slabs spanning from wall to wall.

To the red sandstone, except for flooring or roofing however, there are objections: it decomposes readily by the action of the atmosphere, and is, therefore, useless for outside work or ornament, and its deep brick dust color is unfavorable to its application in buildings of the modern style of architecture. The salmon colored variety has not these objections, and may be classed among the most valuable building materials produced in this country: it is very durable, easily worked, is procurable in any quantity and in blocks of any size, and its color gives to buildings constructed with it a warm hue, which is particularly agreeable to the eye. To those, therefore, who are not so obstinately wedded to custom as to prefer the dazzling glare of white-washed walls to the more pleasing tints of stone fronts, and who are not unwilling, through ill-judged economy, to set aside, for the more permanent monuments of a chaste and dignified art, the ephemeral productions of ill formed taste, the quarries of Agra will be found a valuable and productive resource, affording the means of giving reality to the boldest conceptions of the most fruitful genius: and whether it be for the sculptor's chisel, or the more humble though not less useful purpose of the mason, the sandstone there produced is scarcely to be equalled, and certainly not to be surpassed, by any material in India.

TABLE

shewing the results of experiments made on sandstone slabs, from the quarries near Agra, measuring $4\frac{1}{2}$ feet in length, 12 inches in breadth, and $1\frac{1}{2}$ inches in thickness.

No.	Description.	Specific gravity.	Weight of a cubic foot in lbs.	Weight of the specimen dry.	Weight of the specimen wet.	Weight in lbs. causing fracture.	Deflection in inches at the instant of fracture.	Value of e in the equation $W=\frac{B.\ D^2}{L}.e$
1	Red sandstone slabs, dry,	2275	142	80	..	304	,362	45
2		2346	146	$82\frac{1}{2}$	..	390	,50	57
3		2346	146	$82\frac{1}{2}$	..	482	,75	71
	Means,	2322	$144\frac{2}{3}$	$81\frac{2}{3}$	..	392	,537	57
4	Salmon colored do. dry,	2389	149	84	..	555	,75	82
5		2361	147	83	..	617	,50	91
6		2260	141	$79\frac{1}{2}$	..	555	,50	82
		2336	$145\frac{2}{3}$	$82\frac{1}{6}$	..	575	,58	85
7	Red sandstone do. wet,	2268	141	$79\frac{3}{4}$	$83\frac{3}{4}$	344	,75	50
8		2190	136	77	82	498	,625	75
9		2190	136	77	81	316	,75	46
	Means,	2216	$137\frac{2}{3}$	78[1]	$82\frac{1}{4}$	386	,708	56
10	Salmon colored, do.	2012	125	$70\frac{3}{4}$	$74\frac{3}{4}$	266	,75	39
11		2140	133	$75\frac{1}{4}$	$78\frac{1}{4}$	203	,109	30
12		2131	133	75	$77\frac{3}{4}$	432	.625	64
	Means,	2094	$130\frac{1}{3}$	$73\frac{2}{3}$	77[1]	300	,791	44

VIII.—*On the Art of Taxidermy.*

To the Editor of Gleanings in Science.

SIR,

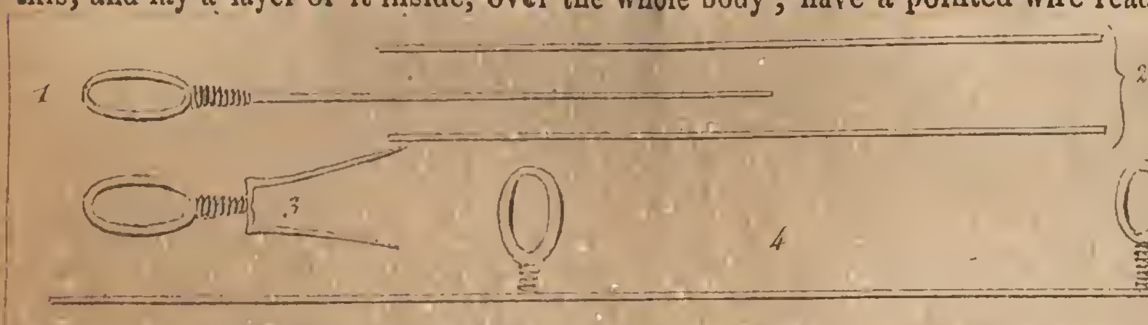
The hill provinces abounding with birds of rare, if not unknown species, and some with the most beautiful plumage, has created a taste in travellers for collecting a set of specimens: and in order to encourage this pursuit, I have thrown together the following memoranda on the art of TAXIDERMY, which are at your service for publication. They may prove interesting to some of your readers.

The bird should be allowed to get cold after it is killed, that the feathers may not easily come off. Some cotton should be put in the mouth to prevent blood

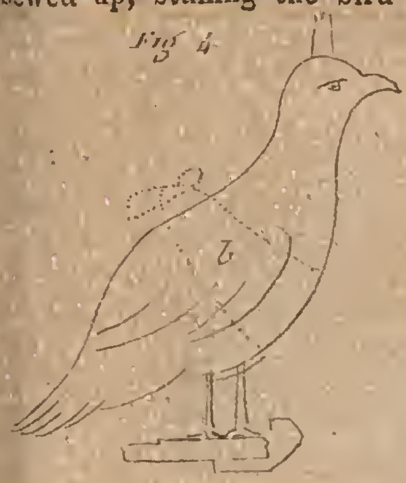
[1] The value of these means is somewhat above the truth, but in so small a degree that it was thought better to reject the fractions and enter whole numbers in their places.

from spoiling the feathers. When it is to be prepared, lay the bird on its back, having previously sown a thread through the nostrils (see fig. 1st.); an incision

through the skin, from the middle of the breast to the rump, as there represented, is then to be made. Skin as far as you can, by introducing the fingers. Press the legs up, and dislocate the bones from the hip; cut the flesh from the thigh bones, and the rump from the body, (it must be left on the skin to keep the tail feathers properly in position;) care must be taken not to cut the skin in this operation, and the following one of skinning the back. Skin the bird as far as you can, by turning it inside out; as you proceed, the wings must be dislocated: cut the neck off close to the head, into which make an opening, (see fig. 2,) and take out the brains; anoint the head and skin of the neck with the preparation intended to be used in preserving the specimen; then with the assistance of the thread through the nostril pull the head to its proper place. Cut the flesh off the wing bones, and tie both together with a thread; anoint the whole skin with the preparation; have ready a quantity of tow well cut into short pieces; stuff the neck very gently with this, and lay a layer of it inside, over the whole body; have a pointed wire ready,



(No. 1); pass it carefully up through the stuffing of the neck, and through the head, the oval will be at the body of the bird. Take a pointed wire, (No. 2,) introduce it up the leg, and fasten it to the oval of No. 1, by twisting it round (a large bird will require to have tow or cloth wrapped round this wire and the thigh bone, and also the wing bone, to represent the thigh and wing as near as possible; the thigh must be made before the wire is fixed); do the same to the other leg. If it is necessary the tail should appear in any particular position, take a piece of wire and form it as represented in No. 3; point the two ends, and pass them through the rump: the oval of this tail piece to be fastened to the oval of No. 1 with thin wire. Take the eyes out, and also the tongue, anointing the sockets and throat with the preparation. As much flesh as possible should be taken from the outer pinions of the wing, by an incision marked *a*, fig. 3, and the parts anointed. The incision first made in the belly is now to be sewed up, stuffing the bird with the



tow well, to give it its natural fulness of appearance; take care none of the feathers are stitched in, and as much stuffing must be laid below the wires in the body as above. Lay hold of the leg wires, and bend them from the inside to an angle of about 45° with the bird, which should now be set up, by drawing the leg wires through two holes made at a proper distance in a board, bending the wires over the board, as shewn in fig. 4; to keep them steady, set the bird to its proper shape, by bending the wires at the knee joints and the head wire, so as to make the bird appear in its natural state, supporting the wings by bandages of cloth till dry, (see *b*, fig. 4.) Should it be wished to give the bird the appearance of flying, or spreading its wings, wires must be introduced through them, and fastened to the oval in the same manner as the legs. When the bird is dry it may be replaced on a proper piece of wood, and all the superfluous wire cut away. Should the head of the bird be loose on first setting up, pass a thread through the head with a needle, fixing it to the wire No. 1, bent for this purpose, as shewn in fig. 4.

A four-footed animal is prepared much in the same way: the head must be skinned as far as the lips, taking off the flesh on the skull, replacing the parts with proper stuffing. Have ready the wire No. 4, put the sharp end through the neck and head, the ovals will be opposite the shoulders and loins; introduce wires up the legs, and fasten to the ovals; the tail should also be skinned, and a wire put through it; the animal is set up in the same manner as the bird. For large animals a bar of wood is necessary from the shoulders to the loins.

As carrying about a large collection, set up in this manner, would be exceedingly inconvenient, it is found better for travellers altogether to omit the wires, merely stuffing the bird partially with the tow; in this manner a number of them can be carried in a box with proper divisions: they should be aired once or twice a week, and kept perfectly dry; if possible, duplicates should be obtained of all the specimens; it is customary to give the duplicates to the artist who arranges and sets them up,—a recompense of much greater estimation than usually rendered in money.

The preparation hitherto used with most success is the French arsenical soap, for which the following is a receipt:—

1st. White soap, 2 pounds.
2nd. Salts of tartar, 12 ounces.
3d. Lime in powder, 4 ounces.
4th. Arsenic in powder, 2 pounds.
5th. Camphor, 5 ounces.

The soap should be slowly melted on the fire, and the ingredients added in the order I have mentioned; when to be used, take a small quantity and dilute it with water, till it have the consistence of broth, with which all the parts of an animal which can be come at, are to be anointed with a brush. I have found it of much use in preserving the bill and legs of a bird, to varnish them with oil of turpentine.

The Munihars make very good glass eyes for birds; sealing wax of the proper color is not a bad substitute: a person, however, fond of the pursuit, should furnish enameled ones of sorts. Where the birds are not set up with the wires, the eyes are not put in, the sockets being filled with tow. A memorandum, however, should be kept, stating the color, with any other remarks; for which purpose the specimens should be all carefully numbered.

The amateur should furnish himself with knives, files, hand-vice, pincers, and hammer, with an assortment of wire: for a bird the size of a partridge, wire $\frac{1}{20}$ of an inch in diameter would be required; and a bird 3 or 4 lbs. weight, double that size, or $\frac{1}{10}$ of an inch; the wire should be softened in the fire before being used, in order that it may easily bend.

In the hills, as in the plains, a native can be hired, who for a trifling sum will bring in a number of specimens daily; the rocket bird and pheasants will attract his particular attention; and if the traveller wishes to add to his amusement, I am inclined to believe he would find his trouble repaid in making a collection of drawings, accompanied with descriptions, of fish, snakes, and a variety of the deer species, some of gigantic size, found at the heads of the Ganges and Jumna.

I remain, Sir, your very obedient servant,

Upper Dooab, February 15, 1830. Z.

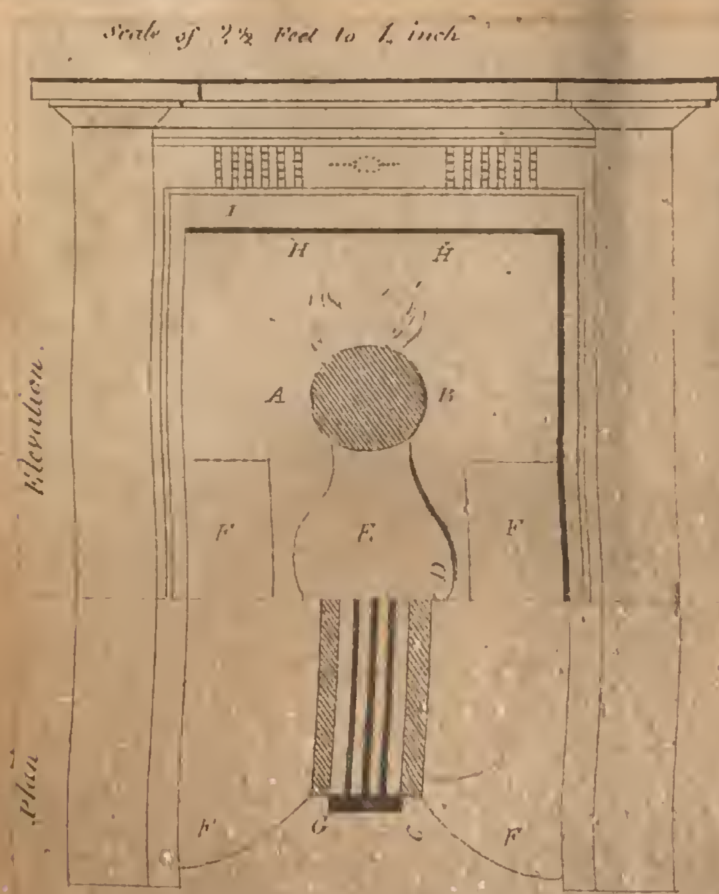
IX.—*Description of the Rumford Grate.*

To the Editor of Gleanings in Science.

SIR,

Although fires are not much required in this country, yet it is by no means uncommon up here, where we have the weather colder than you have in Bengal. As I often hear complaints of the misery attending this luxury, in the shape of smoke, allow me to offer the following account of a fire place, called a Rumford, which appears to be deserving the consideration of those who wish for relief, or about to build, and especially to those who retire to the hills, where fires are almost constantly used.

The plan is by no means new ; yet, as your work proposes to be a republication of interesting matter, it may not be foreign to your purpose.



A.B. Circular grate 9 or 10 inches in diameter.

C.D. 18 inches.

E. The ash pit extending below the grate.

F.F.F.F. The Nob.

G. The chimney in front of the grate, being an opening of 6 inches by 2 into the flue of the usual size ; the whole heat is thrown out, and strongly propels the smoke and flame H.H. with great force up through the small aperture into the flue.

A small quantity of fuel is required (much cannot be used), giving a greater heat in the room than a large common fire.

When no fire is required the small aperture is easily stopped up, preventing any unpleasant smell from the chimney, as is often experienced in common fire places.

The circular grate is merely a round hole in the wall, with three or four grates going along the bottom, to allow the ashes to fall into the ash-pit.

Any common fire place can easily be put on this plan by building up the place usually allotted for the grate quite up to the front of the chimney-piece at I, leaving the small aperture in front, the circular hole, and ash-pit : this masonry should be 8 or 9 inches behind the line of wall of the house.

While I am on this subject I may as well mention a complaint often made by indigo manufacturers, of not being able in wet weather to heat their boilers, which is owing to the circumstance of the native attendant heaping a quantity of wet plant (the fuel used) on the fire, and, as a matter of course, either puts it out, or produces more smoke than heat ; if attention had been paid to the manner the natives heat their sugar boilers in wet weather, the remedy (a very simple one) might have suggested itself,—viz. by adding small quantities at a time : it would be easy to give an order to this effect, but to insure its being obeyed, let the aperture into the fire be made small, a foot in diameter, or even less. The native attending the fire will make objections to the plan, as feeding the fire will require his constant care : it will scarcely be possible for him, however, to put it out, as the small quantity of fuel he can put in, dries instantly, and flames before he can smother it with more.

Obsecro tuum est ? vetus credideram.

Upper Dooab, January, 1830. Z.

X.—*On the Accumulation of Diluvium or Gravel in the Vallies which border the Great Himmalaya System of Formations.* By Captain J. D. Herbert, *D. S. G.*

Along the plainward boundary of the great Mountain System of the Himmalaya, which it is known extends from Cabul to Assam, there are disposed, at intervals, a series of vallies, being, with the exception of those of Cashmír and Nepál, the only vallies connected with that great tract of mountains. They are always bounded towards the south-west by low sandstone hills;—low I mean in comparison with their northern wall, which are generally of the grey wacke formation, with limestone associated. Their surface is either level or moderately undulating, and the rock is seldom visible except in the hills that bound them. As examples of these vallies, I may mention those of Makowál or Rópar, Pinjór, Kyárda, Déhra, Patlí, all between the Setlej and the Gágrah; and again that of Chetaún, which lies to the north of Bettíah. These are all that the writer has seen, but they continue, doubtless, along the whole line at intervals. They are from 20 to 50 miles in length, and of variable breadth, the widest being about 20. A peculiarity of their structure consists in their having two outlets for their drainage, one at each end; their highest level being, somewhere between, though not always exactly, in the middle. These rivers are generally remarkable for the quantity of rounded stones they contain in their beds, which sometimes extend even fifteen miles from the mouth of the valley. These rounded stones comprise every variety of rock that is to be found in the interior of the hills—some of them being of such peculiarity of character as to be traceable, with the greatest certainty, to a distance upward of 25 miles. They are perfectly rounded, though varying in shape from lenticular through every variety of spheroidal to perfect spherical. Their size is most commonly about 3 to 6 or 7 inches in diameter, but they are found both larger and smaller than this.

These boulders, as they have been called, are also found along the foot of the sandstone range that bounds these vallies, and in the beds of the torrents which intersect them, whether on the plain or valley side. They sometimes form small tables or flat-topped hills, separate from the sandstone range, and they appear to be spread out at the foot of this sandstone range to a certain distance. The thickness of some of these deposits is very great. On the road to Bhamaúrí, a well was attempted to be sunk at Tandah; but though the depth of 150 feet was attained, they had not penetrated through the deposite, or come upon any spring of water. At Chilcia, about 60 feet was cut through, with a similar want of success. These facts show the great thickness of these beds.

The soil in the vallies is of various character, but generally it may be described as more or less gravelly. On digging to any depth, gravel is met with more abundantly, though sometimes a reddish brick earth is found instead. The gravel consists of stones more or less rounded, chiefly of quartz, of argillaceous quartz, of limestone both common and magnesian, and of conglomerate. Angular fragments of clay slate are also found. All these latter rocks are in the hills which bound the valley immediately to the north. The deposite, whatever be its nature, is of great depth, as is obvious from the following account of it, for which I am indebted to the Hon. Mr. Shore, who sank a well in the Déhra *dún*, to the depth of 220 feet. The particulars of this operation appear to have been carefully noted by Mr. Shore, and I am happy to place them on record in the pages of the Gleanings.

Particulars of the strata observed in sinking a well shaft, about 1½ mile south of the town of Dehra.

Feet.	Total.	Description.
5	5	Fine black mould, with a few stones.
4	9	Reddish earth, mixed with gravel.
9	18	Loose sand and gravel; large stones, with reddish clay.
2	20	Ditto.
3	23	Stiff reddish clay.
8	31	Stiff yellow clay.
3½	34½	Sand and gravel, mixed with a little red clay.
1½	36	Stiff reddish clay.
2	38	Sand and gravel.
22	60	Stiff red clay.
2	62	Clay, sand, and gravel mixed.
16	78	Sand and gravel.

12	90	Stiff yellow clay, with a little sand.
35	125	Sand and gravel ; a few round stones.
3	128	Ditto ; large blocks of conglomerate.
3	131	Ditto.
13	144	Sand and gravel, with tolerably sized stones.
5	149	Ditto ditto, stones larger.
9	158	Ditto ditto, with pieces of conglomerate.
4	162	Ditto ditto, with enormous stones.
6	168	Conglomerate on 3 sides ; gravel the 4th.
3	171	Sand and gravel, moist occasionally : pieces of conglomerate.
3	174	Conglomerate, blocks of.
$3\frac{3}{4}$	$177\frac{3}{4}$	Layers of sand and gravel ; pieces of conglomerate.
$4\frac{1}{4}$	182	Sand and gravel.
$\frac{1}{3}$	$182\frac{1}{3}$	Conglomerate ; under it water, but scanty.
$2\frac{2}{3}$	185	Sand and clay.
$\frac{1}{3}$	$185\frac{1}{3}$	Conglomerate.
$18\frac{2}{3}$	204	Sand and gravel, rather loose occasionally : pieces of conglomerate, occasionally solid blocks, 160 lbs. in weight.
5	209	Sand and gravel very moist.
$\frac{1}{3}$	209	Conglomerate over half the well spring.
$1\frac{2}{3}$	211	Red clay.
7	218	Sand and gravel, very moist spring.
$3\frac{1}{2}$	$221\frac{1}{2}$	Blackish clay, with angular fragments of clay slate.

XI.—*Miscellaneous Notices.*

1. *Progress of Science in India.*

We have occasionally communicated to our readers phenomena of an interesting description, which have taken place in India, and expressed our regret that so many observations, which might be of use to the advancement of science, and which we were informed had been made in India, should be lost for the want of some channel through which they might be conveyed to the public. Since we first alluded to the subject, various societies have been established in the great eastern portion of the British dominions for the cultivation of physical knowledge. The first volume of the memoirs of the Geological Society of Calcutta has just reached England, containing several papers, not only of local, but of general interest. As the first fruits of an enlightened love for science, we regard this work with excessive pleasure, and doubt not, from the well known zeal of our countrymen in the East, that each succeeding volume will increase in interest.

An enlightened friend to science in all its branches, as well as an efficient patron of it, Sir Edward Ryan, has exerted himself to establish a Scientific Journal as a depôt for all the floating observations which may be made in India. In the present humble form of this small pamphlet, we can perceive the germ of future excellence. An original paper on Indigo, which it contains, would do honour to the first scientific publication in Europe. It is not suited for our pages, but we doubt not it will meet insertion from some journal more exclusively devoted to scientific subjects, and we hope that due acknowledgement may be made of the obligation. Now that a commencement has been made in India, and the example has been set by the first presidency, it is to be expected that Madras and Bombay will not remain behind. The advantages that must result from this are incalculable ; for extensive as our dominion is in India, the natural history of the country is but imperfectly known. In exploring its more remote districts, some travellers have been eminently successful, and the results of their enquiries have been made known to the world ; still there are many provinces which have been rarely trod by the foot of a European, and the notes made concerning them being too hasty or too few to form a volume, have been perused only by the friends of the author. The establishment of a journal in which all such productions may find a place, must form an epoch in the history of British India. As the increase of its contents will necessarily lead to the appearance of articles of the highest interest, we shall always make known such to our readers, to whom we ourselves have frequently suggested, that as the interests of science are greatly advanced by the immediate insertion of observations, we should always feel happy to receive into our scientific varieties any communication, of which the truth of the facts it contains can be properly authenticated.—*Monthly Magazine.*

2.—*China Rice Paper.*

To the Editor of Gleanings in Science.

Sir,

With reference to your notice on the "China Rice Paper," in No. 16, I may mention, on the authority of a friend, that the plant from which it was obtained, is really our *Sola.* In turning over some of Dr. Buchanan's manuscript journal, I find the question nearly put beyond all doubt, as you will perceive by the following description of the plant, which I extract for you.

"The *Sola* is a plant much used both by fishermen, who employ it for floating their nets in place of cork, and by the makers of artificial flowers, who are numerous in Bengal.—Their work is indeed coarse, but the material is excellent, and seems to be the same with that of which the elegant artificial flowers of China are formed.—In fact, nothing can more strongly resemble the structure of the petals of a flower than the pith of this plant, which I am persuaded would be a valuable acquisition to our artists in Europe. It might even be worth while to send some home as a trial.—This plant grows in tanks and marshes. The trunk, which remains under water, is 3 or 4 feet in length, and 3 inches in diameter.—It consists almost entirely of a fine grained very light white pith, which has a considerable adherence of parts, even when cut in very thin slices, and which can be dyed of the brightest colours. For making ornaments, the plant must be cut between the middle of October and that of November: what is procured after the marshes become dry, is only fit for floating nets.—Some confusion seems to have taken place in the *Hortus Malabaricus* concerning this plant, the drawing (Part IV. tab. 18.) seems to have been taken from the *Æschynomene Indica* of Willdenow; while the description, page 31, seems to refer to this plant, which is the *Æschynomene diffusa* of Willdenow."

How Mr. Reeves or the Society of Arts make out that this is a recent discovery of theirs, I am not aware—but if they had taken the trouble to turn over the "*Lettres edifiantes*" of the Catholic Missionaries, (of whom, and by name, you might have said a little more by the way than you have, in your notice No. 3, considering how much we are indebted to them,) or looked into the Abbé Grosier's excellent abridgment of the memoirs of the Missionaries, they would have found that the French at least were well informed on the nature of the substance long ago.—Grosier, vol. 2, p. 237, calls the plant, *ton-tsao*—but does not appear to have been informed of its botanical name. In a subsequent vol.—5th or 6th, I forget which—in his account of the state of the arts, he gives a long description of the process of preparing the paper, and of the artificial flowers made therefrom.

Should the Chinese not be the same species, nothing could be more easily managed than its introduction through the Secretary of the Horticultural Committee, Agricultural and Horticultural Society, who would of course send for the seeds of the plant, if the subject was brought to his notice; and they, or the dried plant, should be sent for at any rate, as it would settle the subject at once, by comparing it with our *Sola.*—I am, Sir, your's obediently, J.

3.—*Introduction of Gas Light into Egypt.*

The viceroy of Egypt is about to make an arrangement with an English Company for lighting Cairo and Alexandria with gas. He has already made an experiment at a palace of his own near Cairo, and is said to have been highly delighted with the effect produced.

4.—*New Esculent Vegetable.*

Mr. Houlton has obtained the silver Ceres medal from the Society for the encouragement of Arts, for introducing a new esculent vegetable, which he calls panace. It is the root of the *Stachys palustri* of Linnæus, the *Ronax coloni* of the older botanists; in English, March-all-heal.

The roots are six to ten inches in length, in flavour very similar to asparagus; they are very tender, having no hard fibres, and require boiling but from ten to fifteen minutes to fit them for the table.

In seasons of scarcity, they been been used to make bread of, after being dried, and ground into flour.—(*Gill's Tech. Rep. Trans. Soc. Arts.*)

5.—*Method of Effacing in Lithography.*

We observe a method has been discovered of effacing any part of a design drawn on stone, so that erasures or corrections may be made without any fear of spoiling the impression. This appears to have been hitherto a desideratum in Lithography. The process consists in applying an alkali to dissolve the fatty and viscous materials by means of which the ink attaches itself to the stone. Another method which is said to be more applicable to writing, is first to remove the traces of the ink by means of spirit of turpentine; a little vinegar is then applied with a hair pencil, after which the stone is wiped with a sponge moistened with water. This stone will now bear retouching.—*Gill's Tech. Rep. (Bull. Sc. d'Encour.)*

XII.—*Proceedings of Societies.*

1. Asiatic Society.

Wednesday, May 5th.

The President in the chair.

The Right Rev. the Lord Bishop of the Diocese, Mr. Lushington, and Major Burney, were admitted members.

A letter was read from the secretary to the Van Dieman's Land Society, proposing to correspond with the Asiatic Society.

The following donations were presented—viz. two cabinets of Minerals, purchased at Benares, by Sir Charles Grey; also specimens obtained from Messrs. Gerard and Royle, or collected by himself on his visit to the upper provinces.

Fossil Minerals from the Himalaya, presented by Mr. Gerard, through Sir C. Metcalfe.

Mineral specimens, and some small figures of Bauddha worship, presented by Captain Mackenzie.

A catalogue of the maps, plans, &c. in the collection of his late Majesty, by the trustees of the British Museum.

The 2d part of the Philosophical Transactions for 1828, and the 1st part for 1830, by the Royal Society.

The 3d part of the 7th vol. of their Transactions, by the Horticultural Society.

The Journal Asiatique, by the Asiatic Society of Paris.

The 1st volume of the Ramayana, by the Editor, Professor Schlegel.

Pentopotamia Indica, by the author, Mr. Lossen.

The 4th and 5th volumes of the History of the Turks, by the author, Von Hammer; also letters on the Library at Turin, and observations on the Byzantine Historians.

History of London, by the author, Mr. Norton.

The Mrichchakati, in the Original Sanscrit; Voet's Commentary on the Pandects; and Report on the External Commerce of Bengal, by Mr. Wilson.

Letters were read from Professor Rafn, presenting Scripta Historica Islandorum, The Krakumal, a Poem, and various Tracts, &c.

The following papers were then read:

Extracts from Mr. Gerard's letters to Sir C. Metcalfe, relating to the Fossils presented by him.

A letter from Mr. Prinsep, forwarding, by desire of Government, Mr. J. Prinsep's Report on the Population of Benares.

Extracts from a letter from Major Burney to Mr. Swinton, giving an account of Dr. Richardson's visit to Laos.

A paper on Boring in the vicinity of Calcutta, by Dr. Strong.

Observations on Ancient Coins found in India, by Mr. Wilson.

XIII.—*Notices of Books.*

The first number of General Hardwicke's Illustrations of Indian Zoology has reached this country, and its execution fully answers the expectations of those who were acquainted with the magnificent collection of drawings and preserved specimens possessed by that zealous cultivator of Indian Zoology. The design of the work cannot be better explained than in the following extract from the prospectus.

"The Natural History of India, which had for a long time been almost entirely neglected, has again, of late years, awakened some share of that attention to which both its richness and extent so justly entitle it. Various useful, and some splendid publications—all published under the fostering care of the East India Company—have contributed to the elucidation of its different branches, and have rescued our country from the stigma of suffering the natural productions of so vast and interesting a portion of its dominions to lie buried in obscurity, while those of other regions, rarely visited by European travellers, are daily becoming better known. Among such publications, those which have been devoted to its Zoology have occupied a conspicuous station; but they have been hitherto confined to particular districts, and have been commonly restricted to certain classes or families of the Animal Kingdom. Such are the magnificent works of Dr. Russell on the Serpents of India, and on the Fishes of the Coromandel Coast; and also the later and less splendid, but not less valuable, publication of Dr. Hamilton, on the Fishes of the Ganges, and Dr. Horsfield's excellent Researches in Java. But a work comprehending the whole of Continental India, and of the Islands which are usually regarded as its appendages, and embracing at the same time all the more perfect departments of Animated Nature, is still a desideratum to Science. To fill up this void, the present publication has been undertaken; the materials for its execution, possessed by General Hardwicke, are, we may confidently assert, unequalled in extent. They consist—

"*Firstly*, of Drawings made upon the spot, and chiefly from living specimens of animals, which General Hardwicke was enabled to procure by the labours of collectors employed for the purpose, and sent on excursions to Nepaul, &c. during a residence of upwards of forty years in various parts of the Indian Empire; executed by English and native Artists, constantly employed for this express purpose, under his own immediate superintendence.

"*Secondly*, of Descriptions and Notes made at the same time with the Drawings, and carefully compared with the originals.

"*Thirdly*, of extensive collections of the Animals themselves, the specimens of which are, for the most part, deposited in the British Museum, the Museum of the Honourable East India Company, and in those of the Linnean and Zoological Societies.

"To these must be added, the Contributions of numerous friends who have liberally added to the riches of the Collection, and those Materials which have been collected by the Editor's personal inspection of various English and Continental Museums.

"From these materials, which have been placed by General Hardwicke at the disposal of Mr. Gray, of the British Museum, under whose superintendence the Work will appear, it is first proposed to make a selection of such Vertebrated and Molluscous Animals, more especially such as have never before been accurately figured, particular care being taken to avoid giving again any of the animals which have been figured in the publications above enumerated."

We are glad to see a work like this, and congratulate the scientific world on its appearance. Taken in conjunction with the equally splendid botanical work now publishing by Dr. Wallich, it will, we hope, redeem the name of India from the imputation of total disregard and indifference to the progress of science, and that aversion to co-operate with the efforts making in other countries, which have hitherto been our reproach. Yet, however effective this late attempt at exertion may be, it will not be the less true or mortifying, that almost all that Continental Europe knows of Indian Zoology, will be associated with the name of Duvaucel and with the glory of France—a country whose sons are not more ambitious of warlike distinction than they are of the purer triumphs of science.

We should not omit to notice, that both these works are publishing under the patronage of our Honorable Masters, who in this, as in many other instances, have shown a princely and munificent spirit worthy of the rulers of a great empire, which, as well as the interest they appear to take in all that concerns the progress of science, contrasts favorably with the character and proceedings of our Indian Governments. We hold it to be one of the surest characteristics of a great and powerful mind to see clearly, where others can but perceive dimly, the value of those labours which contribute in any degree to swell the great sum of human knowledge, and thus to advance our race still farther in the career of improvement, and, as a consequence, of virtue.

GLEANINGS

IN

SCIENCE.

No. 18.—*June,* 1830.

I.—*On the Respiratory Organs and Air Bladder of certain Fishes of the Ganges; by* J. Taylor, *Esq.*

Among the peculiarities of internal structure in the fishes of the Ganges, the most remarkable, in a physiological point of view, are those connected with the organs of respiration and the air-bladder, in certain species of the genera *Ophiocephalus, Bola, Coius, Trichopodus, Macropteronotus, Pimelodus, Silurus, Mystus,* and *Clupanadon.*

The *Macropteronotus Magur, Coius Cobojius, Trichopodus Colisa, Ophiocephalus Gachua,* and *Silurus Singio,* present, in addition to the usual number of gills observed in osseous fishes, certain respiratory organs, which admit, it would seem, from the tenacity of life possessed by these species, of a higher degree of oxygenation of the blood than is effected by means of common branchiæ.

Macropteronotus Magur. This species (the *Silurus Batrachus* of Bloch) possesses, in a deep cavity on each side of the head, two arborescent branchiæ, similar to those discovered by M. Geoffroy, in the *Silurus Anguillaris* of the Nile. These organs supply the place of the superior or cranial limbs of the two middle arches, and consist of two vascular trunks, that spread out into numerous small branches, resembling a tree destitute of foliage, or rather a corroded preparation of the kidney: they are composed of a smooth semi-transparent substance, apparently possessing the properties of the arterial tissue, and are of a deep red colour, their external surface being extensively covered by the minute branches of the branchial artery. The aerated blood appears to be imbibed from the extremities of these branches, through innumerable *villi* in the parietes of the respiratory ramifications into their internal canals, and thence flows into the small vessels that unite to form the aorta. Cuvier assigns to these organs, the double function of affording a surface for the oxygenation of the blood, and of acting as so many hearts for propelling it into the aorta.

Coius Cobojius, and *Trichopodus Colisa.* Each of the branchial arches in these fishes, consists only of one osseous portion or limb, and is provided with very short cartilaginous laminæ; the fourth or posterior arch presenting merely a rudiment of that structure. The supernumerary organ of each side lodged in a cavity, as in the *M. Magur,* and supported upon a broad stalk of cartilage, which is fixed above, by a tendinous process, to the side of the cranium, and below, to the ends of the first and second arches, is concealed from view, by a thin membrane that extends across from the bone corresponding to the clavicle and the superior extremities of the branchial arches, to the internal side of the operculum. It consists of several broad but very thin cartilaginous plates of a convoluted figure, intimately connected at their bases, and so arranged as to present the appearance of a rosette. The branchial artery and aortic vessels are ramified upon a thin membrane, reflected from the laminæ over the surface of this foliated organ, giving it a deep red colour.

Ophiocephalus Gachua. The branchial arches of this species have also very short laminæ. The supernumerary organ of each side is divided into two portions supported upon two broad osseous plates, one projecting from the internal side of the articular bone of the head, and connected with the cornu of the os hyoides; the other, articulated with the anterior branchial arch, the superior limb of which is wanting. The substance composing the organ, is situate upon the edge of these plates, and consists of a thick solid tissue, having a curled margin, not unlike that of the common species of alga, or sea-weed. The branchial artery having given off branches to the laminæ of the arches, runs for a short distance in a canal of the osseous plate, which is connected with the anterior arch, and afterwards proceeds upon its surface, where it divides and subdivides into minute branches, distributed to the tip of the supernumerary organ: the vessels that return the aerated blood unite into one branch, which joins those proceeding from the gills to form the aorta.

Silurus Singio. Besides the eight branchial arches, there are in this fish two respiratory canals, which extend along the back from the cavity of the mouth to near the end of the tail, where they terminate in blind extremities. They are situate below the superior or dorsal portions of the lateral muscles, one on each side of the vertebræ, close to their spinous processes; and are composed of two coats, of which the internal one is smooth, vascular, and of a thin delicate texture, resembling the membrane that covers the branchial laminæ. Each of these canals is provided with a large vessel, which injection shews to be a continuation of that branch of the branchial artery sent to the posterior arch. It runs along the bottom of the canal, giving off, as it proceeds to its posterior extremity, numerous transverse branches that are minutely ramified upon its internal surface. The canals open into the mouth; but the communication between them and that cavity, is in a great measure shut up by the laminæ of the second and third arches, at the junction of their limbs: these laminæ are disposed in a semicircular figure, and are so placed, in relation to each other, as to present, when the two arches are approximated, a laminated surface, of an oval figure, to the mouth of each canal. The air contained in these cavities, is apparently separated from the water, by means of the laminated body: and the blood, thus oxygenated upon their internal surface, is conveyed by small vessels that descend between the transverse processes of the vertebræ, to join the aorta[1].

All the species, possessing these supernumerary organs of respiration, are very tenacious of life; surviving the infliction of severe wounds, and living upon land for a much longer time than other fishes. "In China, the species of the *G. Ophiocephalus*," according to Hamilton, "are often carried alive in pails of water, and slices are cut for sale as wanted, the fish selling dear while it retains life, while what remains after death is considered of little value." The *Coius Cobojius* can be kept alive without water for five or six days, and, according to the same author, is often conveyed in that state to the Calcutta market, from marshes at the distance of one hundred and fifty miles. This species, and the *Ophiocephalus Gachua*, like the *Doras Costata* or hassar[2], possess the power of locomotion on land to a considerable extent; and are the fishes so often met with, after a shower of rain, in fields at a distance from rivers or marshes; and hence supposed to fall from the clouds[3].

The air-bladder of fishes is generally found in the abdomen, adhering to the lower surface of the spine. In the *Ophiocephalus Marulius* and *Gachua*, however, it is not confined in its situation to that cavity, but extends as far as the extremity of the tail, more than two thirds of it being enclosed between the caudal portions of the lateral muscles. It is of a cylindrical shape, and is divided internally into two unequal cavities, by a transverse septum, formed by a reflection of the internal coat; in the centre of which septum there is a small foramen surrounded by a number of short radiated fibres, apparently of a muscular structure, allowing the air to pass from one compartment to the other. The external tunic, at the anterior or abdominal extremity of the organ, is apparently of a muscular texture, and is provided with a large nerve, derived from the eighth pair; but behind it is thin and weak, the powerful lateral muscles by which it is encased supplying its want of muscularity.

The same kind of internal division exists in the air-bladder of the *Macrognathus Armatus*. In both of these fishes, the vascular body which, it is generally supposed, secretes the air of the bladder, is situate on the internal surface of the anterior portion of the organ, and consists of several small kidney-shaped glands, with minute *villi* diverging from them.

In the *Coius Cobojius* it extends also as far as the end of the tail. Behind the bone that forms the boundary of the abdominal cavity, it is divided longitudinally

[1] If a quill, open at both ends, be introduced through an incision, at the side of the spine, into one of these canals, bubbles of air will arise from it simultaneously with those disengaged from under the operculum.

[2] Dr. Hancock's Zoological Journal, No. XIV.

[3] Hamilton's Account of the Fishes of the Ganges, p. 63.

by the spines that support the anal fin, into two cavities: strong tendinous fibres run obliquely between these spines, and fill up the space between them, leaving merely a few irregular openings of communication between the two sides above and below.

In the *Trichopodus Colisa* it differs from that of the former, in having a membranous septum in its abdominal portion; thus dividing the organ into two compartments, from one extremity of it to the other.

The air-bladder varies considerably as to figure, size, and internal structure, among the species of the genera *Pimelodus* and *Silurus*.

In the *P. Aor* it usually consists of two portions of the figure of a heart, placed in a line, and united at their apices.

It is extremely small in the *P. Silondia* in proportion to the bulk of the fish. It lies close to the anterior vertebræ, is of an oval figure, and is divided into two cavities, each of which, in a fish weighing eight pounds, is not larger than a hazel nut.

The *P. Pangasius* has it composed of four or more portions, extending in a line from opposite the pectoral fins, to near the end of the tail. The first is generally oval, the second pyramidal, and the two last, which run between the caudal portions of the lateral muscles, approach to a cylindrical shape. The numerous septa on its internal surface descend from above downwards in the first portion; in the second, they run in a transverse direction; and in the posterior ones, form a number of irregular cells.

In the *Silurus Boalis* it is of the figure of a heart, divided, internally, by a longitudinal septum into two cavities, which have a free communication with each other through a semilunar opening at the anterior part of the septum[1].

It communicates in all of these species with the alimentary canal, by a *ductus pneumaticus*, extending from its lower surface to the esophagus.

The air-bladder, besides possessing the locomotive function generally ascribed to it, appears, from the connection that exists between it and a set of small bones, analogous to the *ossicula anditus*, to exert some influence on the sense of hearing. These bones correspond to the *malleus, incus*, and *stapes* of Mammalia, and are present, according to Professor Weber, by whom they were discovered, in all the osseous fishes in the vicinity of the anterior or cervical vertebra[2]. They exist in the different species of the genera *Silurus, Pimelodus*, and *Cyprinus*, I have had an opportunity of examining; but are apparently wanting in the genera *Ophiocephalus, Coius*, and *Trichopodus:* and also in the *Bola Pama, Mystus Chetala*, and *Clupanadon Ilisha*, in which species their place is supplied by a direct connection between the two organs. The bone corresponding to the *malleus* is considerably larger than either of the other two, and is uniformly situate at the anterior extremity of the air-bladder, extending along the side of the spine to opposite the first vertebra. In the *Silurus Boalis* its anterior half is long, flat, and of a triangular shape, terminating in a blunt point, while the posterior portion is crescentic, having below, a scabrous surface to which the external coat of the air-bladder is firmly attached. By means of a short styloid process, projecting from the side of the latter portion, and received into a pit on the body of one of the vertebræ, this bone enjoys a limited degree of lateral motion, by which its apex can be made to approach or recede from the spine. The *incus*, which is of a short cylindrical shape, is placed between the apex of the *malleus*, and the bone corresponding to the *stapes*, and at a right angle with the former: and is attached to each of these bones by a short round thick tendon, in the centre of which it presents the appearance of being imbedded. The *stapes* may be described as consisting of two parts: one is a thin hollow portion, of an oval figure, somewhat resembling the bowl of a spoon, having the tendon of the *incus* attached to its convex side; the other consists of a small round knob, with a minute spicula projecting from it, and is joined by a neck to the small end of the first portion. It is situate in a large foramen in the first vertebra, and forms one of the sides of a small chamber communicating with the cavity of the cranium. This chamber lies immediately behind the cavity containing the sac and *ossicula*, or calcareous bodies of the internal ear; and within the canal of the first vertebra, its roof being composed of a strong tendinous membrane, that supports the *medulla spinalis* above. The oval portion of the *stapes*, with its concave side presented towards the interior of the chamber, is loosely connected by a membrane, to the edge of the foramen, and admits of being projected into it, to such an extent as almost to touch its fellow of the opposite side, when

[1] This cavity generally contains a number of worms of the *G. Fasciola*.

[2] Blumenbach's Comparative Anatomy, p. 285.

both *mallei* are at the same time gently depressed with the finger, upon the *incudes*. The anterior portion of the *malleus*, and the *incus* with its tendon, as far as its attachment to the *stapes*, are inclosed in a tendinous sheath, containing a quantity of gelatinous fluid: they have no muscles attached to them, and it is probable, therefore, that the slight motion which the *malleus* enjoys, through the medium of its articulation with the vertebra, is entirely regulated by the external coat of the air-bladder.

The air-bladder of the *Pimelodus Gagora*, *Pimelodus Bagharia*, *Silurus Singio*, and *Macropteronotus Magur*, is placed behind the head, close to its articulation with the first vertebra, and in this situation is connected with the *ossicula auditus*, which are conspicuous, and present the same relative position as those of the *Silurus Boalis*.

In the *P. Gagora* there are two air-bladders lodged, one on each side, in an osseous cup, attached by a narrow neck to the body of the first vertebra, close to its junction with the cranium. The mouth of each of these cups is covered over by the common integuments, which are at this part extremely thin, and adhere to the surface of the subjacent bladder, presenting, when the latter is distended with air, an external elastic tumour, of an oval figure. The two air-bladders, which have no communication with each other, or with the alimentary canal, apparently derive their supply of air from a vascular tissue, placed between the two cups where they are attached to the spine: the external coat is of a thin texture, and argentine colour, and has a layer of fine adipose substance interposed between it and the internal surface of the cup.

The *P. Bagharia* has also two air-bladders, which closely resemble the former in the argentine tendinous texture of the external coat, and in having no communication with each other, or with the alimentary canal. They are situate, one on each side of the body, in a deep groove or furrow of the consolidated transverse processes of the cervical vertebræ, and are extremely small, in proportion to the bulk of the fish; each of them, in an individual, weighing ten pounds, not exceeding a large garden pea in size: they are placed at the middle of the grooves, at about an equal distance from the common integuments (immediately behind the pectoral fins) and the vertebral column; the space between each of them and the former being filled up with adipose substance, while that next to the spine is occupied by the *malleus*.

In the *Silurus Singio* and *Macropteronotus Magur*, the air-bladder consists of two small pyriform bags, joined at their pointed extremities by an intermediate canal. It is situate across the spine; each of the pyriform portions being contained in a funnel-shaped case, projecting outwards from the side of the body of the first vertebra, and having its mouth covered over by the common integuments, as in the *P. Bagharia*. The case is formed of bone above, and below by a tendinous membrane that extends across the inferior surface of the first vertebra, thus protecting from the pressure the intermediate canal or isthmus by which the two pyriform portions are united. A communication exists between it and the alimentary canal, by a small *ductus pneumaticus* from the intermediate canal to the esophagus.

In the *Bola Pama*, *Mystus Chetala*, and *Clupanadon Ilisha*, there exists a more direct connection between the air-bladder and organ of hearing, than through the medium of the small bones above referred to.

The air-bladder of the *Bola Pama* is of a pyriform shape, terminating behind in a slender elongated process. From each side of this process an appendix arises by a minute origin, and proceeds by the side of the body of the bladder, gradually enlarging in size, until it reaches its anterior extremity, opposite to which it divides into a number of blind tortuous branches, that spread out upon the lower surface of the kidney. Two of these branches, which are considerably larger than the rest, follow a very singular course: one mounts over the bone corresponding to the clavicle, and appears externally under the thin integuments at the edge of the *apertura branchialis*; while the other winds round the cavity in which the internal ear is lodged, and almost meets its fellow of the opposite side, at its anterior part. This latter branch, (the one connected with the organ of hearing,) is dilated at its extremity into a conical shaped bag, which is of a very thin delicate texture, and is placed in a small pit or fossa on the external surface of the base of the cranium, having merely a thin septum, generally of a membranous nature, interposed between it and the principal *ossiculum*, or calcareous body of the internal ear.

In the *Mystus Chetala* this organ extends in length from the first vertebra to near the extremity of the tail. Anteriorly, it forms several chambers communicating with each other, and behind is divided into two parallel cavities, by a longitudinal septum, which, like that of the *Trichopodus Colisa*, is partly membranous, and partly composed of the spines that sustain the anal fin. These spines are long, hollow, cylindrical bones, and are connected by the internal lining coat of the bladder, reflected on both sides over their whole extent of surface. The lateral muscles forming the walls of the cavity are very thin, and send off from their lower part small slips of muscles that are inserted into the membrane between the spines, by which means the septum can be expanded somewhat in the manner of a fan. A free communication exists between the two sides of the organ, through a number of *foramina* in the membrane connecting the spines, and anteriorly between one of the chambers and the esophagus, by means of a round opening, wide enough to admit the point of the little finger. From the anterior part of the organ a canal proceeds upon the inferior surface of the cranium, as far as the cavity containing the internal ear, opposite to which it terminates in a blind extremity. It consists of a deep furrow on the external surface of the base of the cranium, covered below by an elongation of the external coat of the bladder, (bearing some analogy in its structure to the enstachian tube of the higher orders of animals;) and like the corresponding part in the *Bola Pama,* is separated from the principal *ossiculum* of the internal ear, by a septum so thin and delicate that the slightest touch with a pin is sufficient to rupture it[1].

This air-bladder of the *Clupanadon Ilisha* is long, narrow, and pointed at both extremities, as in the genus *Clupea.* From its anterior extremity two very minute ducts arise, and proceed along the inferior surface of the cervical vertebræ and posterior part of the head, to terminate in two small air-bags, situate one on each side of the cranium. These ducts, when divided with the knife, present open mouths, scarcely large enough to admit the point of a fine bristle: they are semi-transparent, and bear a striking resemblance, both in structure and appearance, to the membranous semi-circular canals of the internal ear. The small bags to which they lead, are lodged in two cavities of a corresponding size in the substance of the osseous walls of the cranium, in which situation they are entirely concealed from view. Each bag may be described as consisting of two portions : the first has a horizontal position, and somewhat resembles a French bean in figure; the second is pyriform, is situate above the first portion, and is united to it at nearly a right angle. These bags have very thin *septa* interposed between them and the *ossicula* of the internal ear, and differ considerably in texture from the organ in the abdomen ; their external tunic being thin, and of a silvery appearance, while that of the latter is thick, and of a dark red colour, presenting a smooth internal surface, like that of a serous membrane.

§ *On the Anatomy of the* Cuchia.

The *Cuchia,* an animal generally regarded as a species of eel, is entirely destitute of fins, and presents, in the structure of its organs of respiration and circulation, some remarkable peculiarities, which would lead us to place it between the class of reptiles and that of fishes. It is common throughout the south-east parts of Bengal, especially in the vicinity of D,haca, where it is generally found lurking in holes and crevices on the muddy banks of marshes and slow-running rivers. Hamilton, viewing its single external spiracle or branchial aperture below the throat as a generic character, has assigned a place to it under the genus *Unibranchapertura* of Lacepede, (the genus *Synbranchus* of Bloch,) and has given an accurate description of it in his excellent work on the fishes of the Ganges[2].

[1] In this species there is a cavity on each side of the head, corresponding to the tympanum of other animals. It is of an oval figure, and is covered externally by the common integuments, which are of a smooth shining appearance and destitute of scales. It is found to contain a quantity of gelatinous fluid, and has a thin cartilaginous plate imbedded in the latter on the one side, and attached to the portion of skin analogous to the *membrana tympani,* by a small hook-like process on the other. It leads to a small foramen on the side of the cranium. This foramen corresponds to the *fenestra rotunda* of Mammalia ; and is filled up by a thick membrane that separates the cavity of the tympanum from the *ossicula,* and the membranous semi-circular canals of the internal ear.

[2] This fish I found in the rivers and ponds of the south-east parts of Bengal. It is said to grow to two feet in length and six inches in circumference : but those that I have seen were shorter and thinner in proportion to their length. Europeans eat the *Cuchia* as an eel : but the natives reject it, and imagine that its bite is fatal to cattle, although less powerful on the human kind; a supposition highly improbable. The whole form of the animal, having no vestige of a fin, resembles strongly a serpent. This fish is cylindrical, devoid of scales, and lubricated with slime. The colour above is dark green, below a dirty pale red : on every part are scattered small round black spots, and short yellowish lines. Two parallel pale lines run forward from the shoulders, and at

Organs of Digestion. The alimentary canal is continued in a straight line from the mouth to the anus, and measures about three-fourths of the whole length of the body. The esophagus is considerably longer than in the eel or fishes in general, and presents, internally, longitudinal folds of a white colour. The stomach, which is slightly dilated at its middle, is distinguished from the former by its more muscular parietes, and the red villous appearance of the internal lining coat: towards the pylorus, where it is separated from the intestine by a valve, it is contracted in diameter, and is of a white colour. The latter part of the canal exhibits upon its internal surface, short villous processes, arranged in a zigzag manner, and is divided by an annular projection of the internal coat into two portions, of which the last (or the part corresponding to the rectum), although shorter, is considerably more capacious, than the anterior or duodenal portion. The liver is of a narrow elongated figure, attached to the side of the stomach, and has several deep transverse furrows upon its inferior surface: the gall bladder, which is of an oval shape, lies near its posterior extremity, on the side next to the stomach, and sends out a long cystic duct, that opens upon the internal surface of the intestine, immediately behind the pylorus. There is a long chain of small quadrangular shaped spleens, covered by a thin reflection of the peritoneum, and joined at their bases to a blood-vessel of considerable size, which extends throughout their length by the side of the intestine.

Urinary Organs. The kidnies, as in fishes, are long narrow glands, of a dark colour, running along the side of the vertebral column, and closely attached to it, as far as the anterior extremity of the abdomen. The bladder is of an elongated shape, and receives the secretion of the kidnies by two ureters opening into it at its fundus.

Organs of Generation. Those of the male are two long ducts of a greyish colour, possessing a number of lateral tortuous branches, that adhere to the external surface of a part of the first portion of the intestine. In the female there is a single pyriform ovarium, containing numerous ova, of various sizes and colours, apparently in different stages of fecundation; and hence it may be inferred that the animal is vivaparous. They terminate along with the urinary organs in a cloaca or common cavity, the external outlet of which, lies immediately behind the anus. *The air-bladder* is wanting.

Bones. The cranium is articulated with the first vertebra by two occipital condyles, one on each side below the *foramen magnum*, and also by a single tubercle of the body of the vertebra, as in the osseous fishes. The bones entering into the formation of the mouth, resemble those of fishes. The superior maxillary one is long, slender, and of a curved figure, having its convex side presented outwards: it is joined above to the malar bone, and below to the coronoid process of the lower jaw. The latter consists of two strong pieces slightly incurvated, and firmly united at their symphisis by cartilage: each piece being formed by two bones, closely wedged together. Both jaws are furnished with teeth, the largest being planted in the palatine arches above; they are fixed in alveolar cavities, are sharp pointed, and of a hook form, but do not possess any structure resembling the tubular fangs of serpents, to confirm the opinion entertained by the natives, of the venemous nature of the bite of the animal. The branchial arches are situate behind the head, at the sides of the four anterior cervical vertebra, and possess a semi-cartilaginous structure, exhibiting, when held up to the light, several detached osseous pieces, of an irregular figure, imbedded in a semi-transparent substance. With the exception of the first or anterior arch, which is fixed to the end of the *cornu* of the *os hyoides*, all the others are articulated below to their fellows of the opposite side. The first and second are composed, each of one portion, and are free at their superior or vertebral extremities, presenting the appearance of being suspended in the membrane of the pharynx; while the third and fourth, or two posterior ones, have very minute superior limbs, which are attached to a small pharyngeal bone, connected through the medium of a long and flat muscle, running between the membrane of the back part of the mouth and the inferior

the eyes join at an acute angle, two others, coming from the sides of the chest. Parallel to the last are two others, one on each side beneath the lateral line. The head is blunt and of a moderate size, but swells out when the animal is irritated. The snout is depressed, short and retuse, projecting a little beyond the under jaw. The nostrils are, near the end of the snout, very small, and each has only one aperture. The eyes are near the upper part of the head, as is also the aperture to the gills. The lateral line forms an arch before, and then runs straight along the middle of the side. The tail ends in a sharp point, is compressed, and has sharp edges above and below.—*Hamilton on Fishes*, p. 16.

surface of the vertebræ, with the base of the cranium. All the arches are smooth on their internal surface. The bones corresponding to the scapulæ, or those which in fishes support the pectoral fins, are long, slightly curved, and united by cartilage at their inferior extremities.

Connected with deglutition and situate below each of the latter, is a small bone, which, together with the pharyngeal bones, is covered on the side next to the gullet, with small teeth. The body of each vertebra is hollowed out posteriorly, forming a hemi-spherical cavity, in which the inner vertebral fluid is contained, and is united through the aedium of this substance, and also by a small triangular process sent off from each side of the cavity of the medulla spinalis, with the bone contiguous to it. The spinous process is bifurcated, or rather consists of two distinct pieces inclining in different directions; and each transverse one, of a short thick spine, grooved on its anterior aspect, with a thin plate of a triangular figure arising from it behind, and occupying the whole side of the body of the bone. The ribs, which are extremely small, are attached to the points of these processes as far as the anus. The cervical and caudal vertebræ are distinguished from the intermediate or dorsal ones: the former by the compressed flattened appearance of their bodies, the latter by possessing spinous processes both above and below.

Organs of sense. That of smelling appears to be the most perfect. The nasal cavity of each side is tubular, and has two apertures, an anterior one at the extremity of the snout, and a posterior between the eyes: and consists also of two parts, one running forwards, the other extending backwards from the anterior opening in a duplicature of the membrane connecting the malar and maxillary bones, as far as the point of union between the latter and the coronoid process of the lower jaw. The cavities of both the sides have a free communication with each other, at their anterior extremities, and may be filled with vermilion injection from one of the apertures. The eyes are very small, and are covered anteriorly by a transparent skin. The *thalami optici* lie behind the cerebrum, and send out two long slender optic nerves, which cross each other without any incorporation of substance.

Organs of Respiration and Circulation. Upon the under surface of the throat there is a semilunar aperture, divided below the integuments in the mesial line of the body into two smallar openings, each of which leads to the gills of the corresponding side. The branchial arches are connected by a strong tendinous membrane, in which are three small openings of an irregular figure for the passage of the water from the mouth, and are covered below, for about one-third of their length, by a thick muscle proceeding from the bone corresponding to the scapula to be inserted into the *os hyoides*. Of all the arches, the second alone possesses laminæ for the purpose of breathing; and these consist merely of a few long fibrils attached to the middle of the arch, and occupying but a very small extent of its surface; the third supports in the place of laminæ, a thick and semi-transparent tissue, which in large individuals of the species presents a fringed or denticulated appearance at its edge, while the first and fourth are bare, having only the membrane that fills up the space between the arches reflected over them. The principal organs of respiration are two small bladders, which the animal has the power of filling with air, immediately derived from the atmosphere. They are placed behind the head, one on each side of the neck, above the superior or vertebral extremities of the branchial arches, and are covered over by the common integuments, presenting externally, when distended with air, two protuberances of a round shape. On dividing the skin, and reflecting it back, each bladder is found to be partially covered, at its anterior part, by a small and extremely thin operculum, joined to the upper part of the articular bone of the head, and connected below with *a membrana branchiostega*, which is supported by six osseous rays. The posterior portion of the organ extending beyond the edge of the operculum, is confined in its situation, by a broad but thin muscular expansion, running across it in an oblique direction, from the tendinous sheath of the spinal muscles above, to the gill membrane below. These bladders, which are smooth, and highly vascular upon their internal surface, do not possess a laminated structure, such as that of the respiratory bags in the lamprey; but present, when separated from their surrounding attachments, and inflated with air, thin semi-transparent membranous parietes resembling the posterior portion of the lungs of serpents. Each communicates with the cavity of the mouth, by a wide semilunar opening between the *os hyoides* and the first branchial arch; its lower margin, which is thick and prominent, is composed of several muscular fibres that extend between the end of the first branchial arch and the side of the spine, constituting a species of constrictor muscle, by means of which the aperture can be contracted to so small a size, as to perform, in some degree, the function of the glottis, and thus produce a slight hissing noise, as may occasionally be heard, when the air is forcibly expelled from the bladder. The heart is situate upon the inferior surface of the esophagus, close to its termination at the stomach; and from the extreme proportional length of that tube, is at a much greater distance from the organs of respiration than is the case in fishes. It consists of two cavities, an auricle, and a ventricle, provided with a thin membranous valve between them. The former receives all the blood of the body by four large vessels, viz. two veins conveying it from the head and anterior parts; one from the liver; and one from the muscles of the posterior portion of the spine, the organs of generation, and the other viscera of the abdomen. The first run close to the spinal column, one on each side, and are joined by a transverse branch opposite to the 12th vertebra: they afterwards separate, and proceed, one in a straight direction, the other across the inferior surface of the esophagus to terminate in the auricle. The posterior *cava*, which is the largest of all, proceeds along the inferior surface of the spine, between the kidnies, to its destination; while the *cava hepatica*, after piercing the pericardium at its posterior part, runs for some distance within the cavity before it terminates in the auricle. The ventricle is of a conical shape, of a reticulated appearance internally, and possesses two sigmoid valves at the origin of the branchial artery. The latter arises from it by an elongated bulb, of an oval figure, and measures from two and a half to three inches in length. It extends along the inferior surface of the esophagus, and divides, opposite to the posterior branchial arches, into three branches of an equal size. Two of these branches run, one on each side, between the fourth or posterior arch and the small bone connected with deglutition, as far as the vertebral extremity of the inferior limb of the former; and being there reflected backwards, unite at an acute angle, opposite to the 10th vertebra, to form the aorta. The third branch, which is the continuation of the artery, proceeds along the under surface of the arches, and gives off, in its course, a branch to the second and third of each side: it then runs forward as far as the *os hyoides*, and is ultimately expended upon the respiratory bladders. The small branches of the second and third arches, after supplying the laminæ of the former and the semi-transparent tissue of the latter with small twigs, are also continued to the bladders; where they are minutely ramified, forming an indistinct net-work of vessels upon their internal surface. Fine vermilion injection, thrown in at the root of the branchial artery, penetrates these minute vessels distributed upon the second and third arches and the internal surface of the bladders, while at the same time it fills the aorta and its branches as far as the tail. The aerated blood is returned by small vessels, forming, at the posterior part of the bladders, two short trunks, which join the branches of the branchial artery before they are reflected backwards, and unite to constitute the aorta.

The *Cuchia*, it is obvious from this account of its anatomical structure, possesses the circulation of reptiles, and the respiration, partly of that class of animals, and partly of fishes. Of the whole volume of blood contained in the branchial artery, one-third passes through the gills and respiratory bladders, while the other two are conveyed directly from the heart to the aorta, without being exposed to the action of the air. This fluid, therefore, undergoes a partial oxygenation, presenting, as may be inferred, a dark purple colour in both divisions of the vascular system, the arteries as well as the veins. Hence the obtuseness of the external senses, and want of activity observable in the Cuchia. It is of a dull and languid nature, exhibiting in all its movements a degree of sluggishness that forms a striking contrast to the vivacity of the eel. A few individuals, which I kept in water for upwards of two months, during the last rainy season, were observed to lie at the bottom of the vessel in a very weak and apparently torpid state, without taking any food: and seldom moved about, except occasionally to rise to the surface for the purpose of inhaling air. The respiratory bladders, although individually of a small size, afford, in conjunction with the branchial structure of the second and third arches, a sufficient extent of surface for the oxygenation of the small portion of the blood transmitted to them. They do not approach either in configuration or texture to any known modification of branchiæ, but on the contrary have a close similitude, in both these particulars, to the posterior portion of the lungs of serpents. The power which the animal possesses of distending them with air, while on land, and the necessity it is under, of rising to the surface of the water for the same purpose, prove beyond a doubt that they perform the function of lungs; and lead us to the conclusion, therefore, that the Cuchia is "amphibious" in the strictest sense of the word,—forming a connecting link, as it were, between the *Ophidian* order of reptiles, and the genus *Synbrauchus* of fishes.

Dacca, 15th June, 1830.

II.—*Some further particulars of the country of Siccim, and of its inhabitants, the Lepchas and Bhótiahs.*

To the Editor of Gleanings in Science.

Sir,

The late account by Captain Herbert, of his visit to Darjíling, which appeared in Nos. 15 and 16, of the Gleanings, induced me to turn to some memoranda I made of an excursion into the same country to Nágri, in the year 1825. Should you deem them interesting or deserving a place in your periodical, I place them at your disposal. I would only premise, that I made the excursion alone, with no object in view but my own diversion, and the satisfaction of my curiosity to see something of a people and country, of which I had heard much.

I am, Sir,

Your obedient servant,

J. T.

Notes of an Excursion to Nágri, in the Siccim country, in the year 1825.

The route to Titalía is so well known as to need no remark, further than a notice of the celebrated ruins of Parwah, with a view to some points on which I would beg leave to differ from the author of the *Visit to Darjíling*. We learn from Stewart's *History of Bengal*, that from about A. H. 750 to 800, Parwah (or more correctly Panduah) was the capital of the Mahomedan kings of Bengal, who rendered themselves independent of the supreme government at Delhi, in the decay of the Affghan dynasty. From the same authority we learn, that the Adínah Mosque, the ruins of which are still an object of great curiosity, was founded by Secander Sháh, in A. H. 763. An Arabic inscription*, in the Sulsee character, over a niche on the exterior of the building, behind the *mimbar*, gives the name of Secander Sháh, and date 776; from which we may conclude that it was completed in the reign of Sief Addeen, the grandson of Secander Sháh. There can be no doubt that it was built from the materials of ancient Hindoo temples, for the fallen steps of the *mimbar* itself discover, on the reverse, Hindoo sculptures; but that the building, in its present form, never was a place of Hindoo worship is clear, not less from the detached and insulated position in which these sculptured stones are found, than from the form and plan of the building. It was moreover the invariable custom of the Mahommedan conquerors of India† to destroy the idol temples, and use the materials for other edifices, but never to convert the desecrated buildings to their own religious purposes. Enough of Parwah, which will well repay the researches of any antiquary, who has resolution to penetrate into a dense mass of jungle, at present the undisputed possession of bears and tigers.

Feb. 22. Rode over to Mundmalla, distant about six miles, and took up my quarters in part of an uninhabited old bungalow belonging to Mr. J. Barnes, of Píprah. Lat. by mer. alt. of the sun 26° 34′ 18″.‡ In the evening received a visit from Rámu Pardhán, agent for the Siccim Rájah in the low countries. He seemed very willing to assist me, should I wish to proceed to the capital. The stages to the capital Gandok he gave as follows: The direct road branches off from the Nágri road at Túmbah-báns. Thence to

Súmdung,
Púbang,
Chongtong,
Choáráni,
Ghók,
Baráh Ringít Colah.

* I have by me two copies of this inscription, both of them incomplete. As far as they go, a translation may not be uninteresting, as a curious specimen of the style assumed by these monarchs. "The erection of this Jami Masjid was ordered in the reign of "the mighty Sultan, the wisest, most just, most perfect and most generous of the Sul- "tans of Arabia and Persia, Secander Sháh; may God prosper his kingdom; in "the month Rajab A. H. 776." Secander Sháh died in 769. The only solution I can offer for the anachronism is that given above, unless we suppose that both copies are wrong, in reading 776 instead of 767.

† The same was the case at Juanpoor, especially in the instance of the Atálah Mosque.

‡ My observations were made with a very excellent sextant, by Troughton, and an artificial horizon of quicksilver. Successive observations at the same place varied so little, that I have no hesitation in vouching for their accuracy.

Lángíláchi—a long stage: no intermediate halting-places.
Tùníh.
Ecgong colah[1]—the residence of a *Lámah.*
Sampho Ghát—Cross the Tístah.
Dác Ghar,
Raiát Laddí,
Rájbárí, *i. e.* the royal residence.

The *Rájah* still resides in the same place to which he had retreated on the Goorkah invasion. It seems that he has given orders for the repair of his ancient capital of Siccim, but that it is not yet completed. This year he is going to Phári, and has ordered some elephants to be sent thither, and Rámu had accordingly sent them by the Lackidwar pass. If this is correct, Turner's supposition relative to this pass is well founded. He thought that the Bhótiyas were afraid of letting him know of this pass into their country, and had, therefore, led him round by the more difficult route of Baxidwar. If elephants can pass through Lackidwar, the difficulties of the road cannot be great.

Feb. 23. In the morning, Rámu, according to promise, sent me over a servant of his, who had been to Phári with elephants, and knew something of the route. He was, however, quite a boy, and it was four years since he had returned, so that much could not be gathered from him. He said that he had taken the elephants by the Bálacót pass, which is on the Torésha Másha. In Arrowsmith's map, Lackidwar is laid down near the Torésha river, and in its vicinity is a place called Bála; this, then, I conjecture, is the pass, though the boy said he left Lackidwar some distance to the east. The only place, the name of which he could remember between Phári and Bálacót, was Páro or Párodong, which he said was on the Torésha Másha. Turner has mentioned a town of that name on the Mába chiú. At 3 P. M. assembled all the coolies, and partly rode and partly walked over to Gosháinpúr, distant about nine miles. Immediately after leaving Mundmalla, you cross the boundary of the Siccim territory. The country is much more open than the Morang, and pretty well cultivated. The road the whole way was along an open plain, with occasional patches of *tarah* jungle. Shortly before reaching Gosháinpúr you come upon the Balásan river, on the east bank of which the village is situated.

Feb. 24. A thick fog prevented our marching before 9. 30 A. M. Our course lay up the Balásan. The distance to Rassadhúra is reckoned five coss, but this must be overrated, as we reached it in about three hours. Found a very comfortable hut standing here, with a stage raised about three feet from the ground on one side, and room for a table and chair besides. Being assured that similar huts would be found at every halting-place, I spread my mattrass on the stage, and sent back from hence the elephant, tent, horse, and bedstead—thus reducing the baggage to very portable dimensions. Numbers of deer visible around, but very shy.

Feb. 25. On the move a little before 9. The distance to Singimári is reckoned four coss, but I think this as usual overrated. The first half of our road was through the forest, at some distance from the river. The trees were by no means very large, but the creepers and parasites very luxuriant. At a place called Munamatti we again emerged on the Balásan, but the scene was now changed. It was no longer slowly meandering on a level surface, between dense forests, but rushing boisterously over a rocky bed, with steep mountains rising on either side. We continued the rest of our march up its bed, to Singimári, which we reached about 11. 15 A. M. Latitude by observed mer. alt. of the sun 26°. 49′. 43″. The road was now becoming interesting. The hills were still covered with wood, from amongst which the bare rock occasionally peeped forth. Singimári is on the western bank of the river, and I had accordingly to cross the bed to reach it. The stream was strong, and the water reached up to the men's middles. In the evening I strolled through the jungle, and accidentally came upon a romantic spot where a little stream precipitated itself over a shelf of bare rock, thirty or forty feet high, with thick forest on either side.

Feb. 26. At 8 A. M. left Singimári. For about the first three miles we travelled along the bed of the Balásan, twice crossing it to avoid projecting rocks. Leaving the river on the left, we then followed the course of a little stream for a short distance, till suddenly turning off we ascended the Jám dhwár mountain. The ascent, though short, was steep, and it was interesting to hear the coolies as they ascended, encouraging each other with shouts, which were echoed from every mountain around. The next ascent was Dharram-dhwár, and after that Cálsabangah; at the summit

[1] The unaccented *o* as in *shot*.

of each ascent were strewed little pinches of cotton (if I may be allowed the term,) acknowledgements to the presiding *debtahs* of having reached the summit in safety with their load. From the top of Cálsabangah, an easy descent led to Dímaligóla, estimated at 3½ coss from Singimári. Arrived just before noon, in time for an observation, which gave the latitude 26° 51′ 50″. This is one of the great places of traffic between the hill-men and the inhabitants of the plain, each bringing their commodities here, and bartering them. The Góla consists of some huts arranged in a square, each side of which may be about 150 yards long. The merchandize is deposited in *changs* or houses raised on stages, to preserve the goods from the effects of the weather. In the centre is a small open *cháng*, round which the trade is conducted ; and on the eastern side of this is a high pole, bearing a flag, and surmounted by a piece of wood, shaped like the end of an oar, for which they appear to have a superstitious reverence. On the western side of the *gólah* rises a steep hill, on which, a little above the huts, is a large house built of bamboos, and said to be occupied by the *Faújdár*, when he visits these parts. For want of a better I took up my residence in it, and fortunately the weather proved mild, as the edifice was airy enough. It was in a new and genuinely Lepcha style of architecture. The stage which formed the flooring was carried off horizontally from the side of a steep hill ; and hence, though touching the ground on one side, it was elevated ten feet above it on the other. The house itself was spacious and well thatched with long grass ; the floor and sides made of bamboo, split in a particular manner. The joint is divided in several places, and then the cylindrical bamboo is opened out and beat into a plain surface, the breadth of which is of course equal to the circumference of the bamboo, which grows here to an enormous size. The flooring and sides thus prepared have a neat appearance when well executed, but the house I occupied was in a most decrepid state ; half of each side was wanting, and the flooring exhibited several chasms and weak spots. Along the front, overhanging the *gólah*, were the remains of a verandah constructed of bamboos, tied together with slips of the rind.

Feb. 27. Marched from Demáli-góla at about 8. 30 A. M. At a mile's distance regained the bed of the Bálasan, and crossed it on a few bamboos, which formed a rude but sufficient bridge. The relics of what is used for the same purpose in the rains hung oscillating over our heads, arguing most forcibly the height of the stream at that season, and the hardihood of the persons who would venture over it on such a contrivance. From the top of a cliff about 20 feet high on one side, to a stage of similar elevation on the other, was swung a rope, or rather a band made of slips of bamboo, bound strongly together, from which depended thinner slips, and to these again the roadway seemed to have been attached on much the same principal as our iron chain-bridges. The Lepcha attendants did not fail to enlarge on the perilous nature of the structure, and the certain death by immersion which would attend a false step. The scenery now became very grand. Few spots can surpass the scenery of Gulgulia-muni. The deep pool of clear water formed by the Bálasan ; the perpendicular cliff of smooth glittering rock towering to an immense height immediately above it, contrasted with the rich and wooded but steep and high mountain on the opposite bank, combined to form a splendid prospect. I here crossed the river on the back of a Lepcha, who though not nearly reaching to my shoulder, took me up with the greatest ease, and singly carried me through a deep and rapid stream, with rather a treacherous bottom. Shortly after we crossed the Rangbang, which here joins the Bálasan from the westward. Pursuing our course a short distance farther, we came to a spot where some half finished huts and *changs* marked the contemplated site of a *góla*. It appeared that the intention had been given up, in consequence of the remonstrances of the Bengali *mahájans* against having to bring their goods so far. We now commenced a very long and deep ascent, which was not accomplished, by those accustomed to the plains of Bengal, without considerable expense of breath and labour. Some short way up, at a place called Túmbahbáns, the direct road to the capital branches off. A walk of about a mile and a half from the summit of this hill brought us to Nágri. There are here four stockades, the centre one of which is the head quarters of a detachment from the Titalía battalion, who are posted here. I was received by the *Súbahdár* and his whole force under arms with military honors, and was compelled to play the soldier with what courtesy and dignity I could master. I then walked with the *Súbahdár* to look at the stockades and select a house for myself, which I was able to effect very comfortably.

Feb. 28, and March 1. Spent at Nágri, in fruitless negociations, and attempts to obtain the means of penetrating farther. Every obstacle was thrown in the way of a farther advance, the most effectual of which was continual delay, so as to exhaust my provisions, of which I had no very great abundance, and time, of which I had still less. I was therefore forced to make up my mind to stopping short and collecting what information I could of the country from the very imperfect sources at hand. A good observation of the meridian altitude of the sun gave me the latitude of the house I occupied 26° 54′ 34″. There are four stockades at Nágri, the centre and western of which are the most elevated, and garrisoned by our sepoys. The southern is held immediately for the *Rájah* by a Bhótiah *súbahdar*, and the eastern is occupied by the *Cázi*. Each of these is situated on the summit of as many eminences, which form the crest of the hill. The house I occupied was a little distant from the rest, on a separate rising ground. It was roofed and floored with bamboos, split and opened out in the way previously described. The sides and partitions were made of mats, and tolerably air tight and comfortable. There were two rooms, a closet, and a neat cheerful verandah to the south. The floor was raised about 1½ foot from the ground—the ceiling black enough from the constant action of a wood fire, the smoke from which had no other channel of escape than the door and windows, and was constantly precipitating flakes of black soot, very much in the most approved London style. The Bhótiah commander of the *Rájah's* stockade, was gone to the Nepalese frontier, to take part in one of the usual negociations regarding refugees, whom the tyranny of the government had driven to take shelter amongst their most inveterate enemies the Goorkhas. The Sepoy *súbahdar* had resided here fifteen months, and was able to give some little information, which could be verified by reference to the few Lepchas present, only one of whom, a lad named Lóha Singh, could read and write. Every Lama resided at a distance, and indeed only once a year does one of that class make his appearance at Nágri, when he comes by the express orders of the *Rájah* to quiet the superstitious fears of the garrison, by appeasing all the *bhút,hs* in the neighbourhood.

The *Rájah's* subjects consist of three tribes, the Lepchas, Limbos, and Bhótiahs. The first of these prevail in the southern parts, to the south of the Rinjít; the Limbos in the north-western, and the Bhotiahs in the north-eastern. They do not, however, very strictly observe these limits, but are considerably intermixed. The Bhotias are held in the highest esteem, the Lepchas in the lowest. These last are a short stout-made race, with very thick and strong legs—their complexion is light, and their features Tartar. Their dress consists of a sort of loose gown, which folds round them, and is secured by a girdle, in which is stuck a broad-pointed knife, about a foot and a half long, called a *bhán*, thrust into a kind of bamboo sheath, open on one side. Their hair is long, black, and very dirty, sometimes tied into a queue behind. Some wear caps occasionally, ornamented with cock's feathers. The cloth of which their gowns are universally made, has once been whitish, with blue perpendicular stripes at long intervals. The cloth appears strong and well made, and they say is their own manufacture; but I had no opportunity of observing the manner in which it is prepared. They are very harmless, good natured, and inquisitive, and used frequently to sit and stare during the taking of an observation or any such process, but went away very readily, if requested to go when they became troublesome. They have no prejudices of caste as to their food, and declare they will eat any thing but a horse or a monkey. The sepoys bandy jokes with them about their readiness to eat Goorkhas, but they declare the only propensity they have in that way is for the flesh of the Jén Pattri, a famous Goorkah *súbahdár*, and their mortal enemy—a joke indicative of no very friendly disposition to their neighbours.

The *Cázis* correspond very much to the lowland *zemíndárs*. They possess a certain number of villages, which descend to their posterity. In time of war they furnish a contingent of soldiers, whom they lead to battle themselves; but there seems to exist no regularly organized army, or gradation of rank. The chief cázi amongst the Lepchas (par excellence *the Cázi*) resides on the Rinjít. The husbandry of the mountaineers is simple in the extreme. They neither use the plough or the spade, but cut down the jungle on the spot they have selected for their field, and, as soon as it is dry, burn it on the spot. In the soil thus formed by the intermixture of the ashes, and slightly scratched, they sow their crop, and as soon as it is reaped move to another spot to pursue the same process. Their chief article of consumption is rice, which they sow in *Assár* and reap in *Cártic*. When cut, they stow it away in baskets, which they place on stages under the ceilings of their houses, where it remains till fit for consumption. They also cultivate *maruah*, from which they make an intoxicating liquor. *Cáncan* and *macai* or Indian corn, *bhángah*, cotton, and *manjít*, are their principal products for export. The latter of these remains standing three years before it has arrived at maturity. The only regular tax which they appear to pay the *Rájah*, is a basket of rice and a rupee annually; but in case of war or any extraordinary demand, the *Rajah* draws upon the *Cázis*, and they again extort from the *raíats* as much as they can. No cattle seem to be kept in this part of the country, but further north they have a very fine breed, much resembling the *Bhaílens* of the Morang and Nepál.

Lóha Sing was my only informant regarding their religion, and his knowledge appeared to be of the lowest order. He represented it as entirely a system of fear. The *bhút,hs* and *débtahs* of the hills are supposed their constant enemies, and the object of their religion to ensure protection from them. Lóha Sing exhibited two pictures he had obtained from a *Láma*. They were very rude, only the face being distinguishable, the rest a mass of flourishes. One of them represented Menjár, and by his favor a traveller is preserved from the attacks of *débtahs;* the other was Tharmáh, whose aid was implored against the demon of sickness. Each god and demon, of whom he enumerated a great number, had his appropriate *mantrah*. Amongst his treasures were some slips of papers about six inches by two, on each of which something was written in the Lepcha language. To one extremity of each was attached a string, so that when all the slips were tied together, the pieces of string hung down from one end of the packet. When a person is ill he takes these slips of paper between his hands, and raising them to his forehead, utters a *mantrah*. Then, with averted eyes, he takes at hazard one of the strings, and the attached slip contains the name of the possessing *débtah*, and the sacrifice by which he is to be propitiated. Another book contained a list of certain articles of food, which should be abstained from on particular days. Such is the superstition of the vulgar; it is impossible to say what are the refinements of the *Lámas*.

The languages in use amongst them, are the Bhótíah and the Lepcha. The former is used in their sacred works, and appears to be the same of which Father Georgius gives us the rudiments, in his *Alphabetum Tibetanum*, and of which a German missionary, who died some years ago at Titalíah, compiled a dictionary, which has since been published by orders of Government. To those who are fond of tracing the analogy of languages, the following list of words may not be uninteresting, as exhibiting those which are well known best to indicate the origin of a dialect.

English.	*Bhótía.*	*Lepcha.*	*English.*	*Bhótía.*	*Lepcha.*
Hand	laktí	kalyok	Water	chúh	úng
Foot	kántí	tonglyok	Fire	míh	míh
Finger	tzúmúh	kajyok			
Nose	nhá	tangnom	One	chí	kát
Teeth	soh	afo	Two	nyí	nyat
Lip	yámchuh	adhúl	Three	súm	sm
Ear	namchok	anyor	Four	ji (French j)	fallí
Eye	mídhóh	amik	Five	ngau	fúngú
Hair	kyah	atsom	Six	trú	torok
Knee	púmúh	takpat	Seven	dheun	kichok
Chin	cukú	tagú	Eight	dyeh	kúkú
Tongue	cheh	alí	Nine	ghúh	kakyot
Mouth	kháh	abong	Ten	chútombáh	kattí
Salt	tsoh	vom			

Loha Singh's skill did not reach so far as the method of arithmetical notation. He pretended to be master of the subject, and wrote me down a long list of figures; but the next day, on being requested to repeat his list, he drew out another perfectly different.

On geographical subjects little was to be learnt. Some Lepchas gave me the names of the villages between Nágri and the Rájbárí; and as they differ from those formerly given me by Rámu Pardhán, it may be as well to mention them.

Pubong.
Chongtong.
Mukdam.
Ghok.
Jimong.
Tanyik.
Nangiláchi.
Tandonglahap.
Tamih.

Rangpo.
Namfok.
Níh.
Sāmphú, (cross the Tístah.)
Kambúl.
Nampong.
Raiat Naddí.
Grido, the residence of the *Rájah,* two short marches west of Gantok.

March 2. A violent thunder storm in the night, accompanied with hail, was succeeded by a beautifully clear morning. It was however very cold when I got up, which I took good care should not be till the sun had shewn me the way; the thermometer stood at 42°. Left Nágri about nine, congratulating myself on the fine day. The march back to Demáli-góla was easy, and did not occupy much above three hours. The road, wherever it leaves the bed of the river, lies through forest consisting chiefly of bamboos. These grow to a great size; many of them can be just grasped with two hands. They are invaluable to the natives, and it is surprising to what a variety of uses they put them. From the stems alone they will construct a very comfortable house. The largest bamboos form the chief support, the lesser complete the frame work. When split and opened out they answer every purpose to which boards are applied, forming the sides and flooring of the house: when thus prepared, cut into lengths, and bent double, they answer for tiles and constitute a very good roof. The house I occupied at Nágri was roofed in this way, and stood a very heavy rain without leaking. Again the small bamboos laid open and scraped thin form excellent mats, whilst slips of the outer rind supply the place of string. But it is not only in domestic architecture that the bamboo is valuable; of the thin strips of the outer rind they make all sorts of basket-work and caps. One of the joints, including the knot as a bottom, serves as well to boil rice, draw water from the well, or hold the store of oil. A bow, arrows, and quiver, may serve to complete the list.

March 3. From the *daffadár* of Demáli-góla I was able to ascertain some little information regarding the trade of Siccim. There are here two *gólas,* the Demáli and Mánji, at the former of which transactions are carried on for the *Rájah, Dewán,* and Dimchurn *Cázi.* On the road to Púhgári, a little removed from the Titalía road, is another small *góla* called Mármá, belonging to Chung *Cázi.* On the Mahanaddi is the Puínjorah, and on the Tistah the Chauwáh; but respecting these I could obtain few particulars. The great article of export is *manjit,* the quantity of which is in proportion to all the other exports—about 5 to 1. *Bhángah,* (raw cotton,) is the next staple commodity, and indeed these may be said to be almost the only articles which are sold at the Gola. The other products of the country, such as the wax, *bhot* or coarse blankets, musk and *charéta,* are procurable only in small quantities, and are generally taken into Bengal for sale. The principal imports consist of salt, earthen-ware, cloth, cloves, nutmegs, black thread, oil, tobacco, rice, and beads, (*mungah*). Almost the whole traffic is conducted by barter; no Lepcha coin is to be seen. An *ad valorem* duty of one anna the rupee is levied on the imports, but the tax on the exports is not so accurately settled, and depends more on the capacity of the *góldárs,* or the patience of the Lepcha traffickers. This tax is levied by the person in whose name the trade is carried on, but I could not clearly discover the way in which the profits at Demali-*góla* were shared between the *Rájah, Dewán,* and *Cází.* It would appear that on every 100 Rs. worth of goods the *Rájah* should obtain a duty on 50 Rs. and the *Dewán* and *Cazí* each on 25, but that this proportion is frequently altered according to the activity of the several *paikars.* The trade at the *góla* begins in December, and terminates at the end of May; so that the trading season had half elapsed at the period of my visit. The accounts kept by the *daffadár,* gave the annexed results for the preceding three months.

At Demali góla.

	Imports.		Duty.		
	Rs.	As.	Rs.	As.	P.
On behalf of the *Rájah,*	1082	8	67	10	6
Dewán,	616	4	38	8	3
Cází,	391	0	24	7	0
	2089	12	130	9	9

At Manji góla.

On behalf of the *Rájah,*	556	4	34	12	3

I got no account of the exact amount sold at Marmah-góla, but it was small, and the *daffadár* assured me not more than 50 Rs. worth of goods had been bartered there during this season. Supposing this correct, we may say in round numbers that the value of the imports at the three *gólas*, during the preceding three months, had been 2,700 Rs. on which 170 Rs. duty had been levied. From all accounts the traffic continues equable during the six months it is carried on. The amount then of the annual imports at the three *gólas* may be obtained by doubling the above sum, Sa. Rs. 5,400, and the duty Sa. Rs. 340. My informants knew little about the other *gólas* of Puinjorah and Chauwah. They are however situated on rivers which afford great facilities for access, and we may suppose them therefore to be not inconsiderable. Demáli is said to be large, whilst Mánji and Mármah are reckoned small *gólas*. We will therefore suppose the traffic of Puïnjorah and Chauwah, equal to that of Demali, Mánji and Marmah, which will give the value of the annual imports from Bengal into Siccim at 10,800 Rs. and the duty levied at 680, of which it is fair to suppose the *Rajah* gets one half, or 340. The computation gives no very high ideas of the resources of Siccim, or the wealth of its sovereign.

After remunerating the *daffadár* and *góldárs* for their information, with some cheroots and a pair of scissars each, (to them an inestimable treasure,) I left them perfectly satisfied at 10 A. M. and marched back to Rassadhúra.

March 4. Walked back to Mundmallah, starting at 9, and reaching my old quarters in the dilapidated bungalow at 2 P. M.

March 5. Returned to Titalíah.

Remark by the Editor.—With the above paper our correspondent sent us a list of observations made with Archdeacon Woollaston's Thermometrical Barometer at Rasa d,húra, Singimári, Dimáli-góla, Nágri, and Gosháinpúr. In using this instrument we are directed by the inventor to observe the difference of its indications at any two places, which being multiplied by a certain co-efficient, gives the difference of elevation. This co-efficient has been lost, but as two barometrical observations were also made, one at Rasa d,húra, the other at Nágri, they would afford the means of recovering it but for the very objectionable peculiarity which the inventor gave the instrument, (in order to increase the scale and yet keep it within the limits of portability,) and which was brought into action intermediately to the observations made at these places.

The peculiarity we allude to, is that of separating, by tapping, a portion of the mercury into the upper bulb—thereby reducing that in the lower, and consequently depressing the indications of the instrument, by a certain number of divisions, according to the quantity of mercury separated. This quantity being entirely arbitrary, the number is uncertain, and in no two cases ever the same; the consequence is, that observations made with this instrument, after and before the operation of *tapping*, are not comparable, unless the amount of change has been observed. We think this a great objection to the instrument.

The observations made at Rasa d,húra and Nágri with an Englefield barometer, of the improved construction, are as follows:

1825 February 25,	4 P. M.	Barometer 29,091.	Thermometer	56°
March 1,	10 A. M.	25,514.		55

Compared with observations made in Calcutta, these give 869 and 8643 feet as the heights of those places respectively.

III.—*Hindustani Synonimes of Plants, arranged alphabetically.*

The following vocabulary, for which we are indebted to a friend, will, we think, be found useful by some of our readers. Imperfect as it confessedly is, it may serve to form the nucleus of a work, which, if properly filled up, and with the Sanscrit names added, would supply a *desideratum* in Indian Botany.

While on this subject, we may be allowed to express our surprise at the length of time which has elapsed since Dr. Roxburgh's death, without the public being yet put in possession of a complete copy of his work on Indian Plants. This work, if published some years ago, would have proved highly acceptable to the student of Botany in India; and we know many to whom the possession of such a work would have proved sufficient inducement to undertake the study of this delightful science. It was, we think, due too to the memory of the late Dr. Roxburgh, to allow no time to be lost in giving the result of his labours to the public. For these and other reasons, we cannot but regret the change of plan of the Reverend Editor, in admitting such bulky additions and interpolations as are found in the first and second volumes of the *Flora Indica*. It would have been consulting the convenience of the public more to have published the work as left by its author, and to have given the additions by Dr. Wallich in a separate form. At present it is by no means easy to form a precise idea of the extent or importance of Dr. Roxburgh's labours. Every page we open contains additions and corrections by N. W., and these letters haunt the reader to such a degree, that he thinks the best part, if not the whole of the work, must be by the same hand. This is surely a fault. But a more serious objection to the course pursued is to be found in the great bulk to which it has swelled the work, and the unheard-of delay that has occurred in its publication; occasioning its completion to be deferred to such a late period, that much of the original interest and value of it have passed away. The more obvious plan of publishing the original matter in the separate form of a text, with the additions as a commentary, would have obviated this and other inconveniences; while it would have given a better idea of what Dr. Roxburgh had done, and of what his successor has been able to add to his labours.

We trust we do not, in these remarks, transgress the bounds of that respect which in common with all who are interested in the progress of science we feel for the justly celebrated editors of this work. We have taken upon ourselves to give utterance to complaints which we have heard repeated from every quarter; but we trust, in doing so, we have not forgotten how much Indian science owes to them. That the public would even yet prefer seeing a complete copy of Roxburgh's work, as left by him, is, we think, certain; but that the present Editor would be justified in giving any weight to our opinion on the subject, we do not pretend to say. Enquiry might, however, furnish more secure grounds of decision.

We perceive by the proceedings of the Horticultural Society, that Mr. Tottie meditates the publication of a *Hortus Orientalis.* This work has evidently been prompted, we think, by the delay in the publication of the *Flora Indica*, of which we have just been complaining. We think that such a work would be well received, notwithstanding the threatened reappearance of the latter work. The public have been so tantalised with regard to the latter, that we apprehend they now take little interest in the announcement, maugre the acknowledged talents and high botanical attainments of its Editors. We recommend Mr. Tottie to persevere in his undertaking, or at least to take the sense of the public on the subject, by circulating his prospectus and subscription book. As far as our limited circulation may enable us, we shall be happy to assist, by publishing any notice of his plan he may think proper to favour us with.

We are happy to see that in consequence of the praiseworthy exertions of the Medical and Physical Society we are to have a new edition of the *Hortus Bengalensis.* It has been hitherto a reproach to our Botanic Garden, and to those under whose management it is placed, that no list of the plants which grow there is accessible to the public, the old edition of the *Hortus* having been long out of print.

The following vocabulary having been drawn up for general use, the orthoepy rather than the orthography of each word has been consulted. For the same reason, provincial, and even vulgar pronunciations are admitted, each having the systematic name assigned, however numerous the synonimes may be, instead of being all referred to one common term, a plan less convenient to the reader, Amongst them will in general be found however the correctly spelled word, and it was intended at one time to distinguish it by a difference of type, but it was found that such an improvement would consume too much time and delay the publication beyond the regular period.

The reader by recollecting the following particulars will easily find a word in this vocabulary. The system of spelling is that hitherto used in the Gleanings, viz. Sir W. Jones's. In this system the vowels sound as follows:

a	á	e	é	i	í	o	ó	u	ú
Americ*a*	b*a*r	b*e*t	th*e*re	f*i*t	s*ee*n	not used	b*o*ne	f*oo*t	f*oo*l

The consonants have all their most usual sounds in English, and the *same* consonant always the *same* sound. *C*, before whatever vowel, sounds like *k*; *ch* like ch in cherry; *c,h* like *kh* in ink-horn; *k* sounds like *ch* in the Scotch word *loch*, or *gh* in the Irish *lough*. *G* is always hard; *gh* the guttural of the Arabs, and equivalent to the Northumbrian burr. The Arabic *cáf* marked in Gilchrist's system as a *q*, has not been been attempted to be discriminated.

The reader will further note that *A* and *A'*, *a* and *á*, are considered distinct sounds, and that in the alphabetic arrangement he must seek the latter *after* not *among* the former. So with the other vowel sounds the accented being divided from the unaccented. So also with *C* and *Ch*, *G* and *Gh*, though *c,h* and *g,h* are to be sought amongst the *cs* and *gs*. The reason of this is, that the manuscript of this article was arranged according to Gilchrist's system, in which these really distinct sounds are expressed by distinct letters. It would have consumed much time to have changed this arrangement; and besides this, it was imagined that there would be a convenience to the reader in the actually adopted one, his guide being the ear rather than the eye.

Hindustani & Eng.	*Systematic Names.*
A.	
Adrac, ginger,	Amomum Zingiber.
Agya ghás, lemon-grass,	Andropogon Schœnanthus.
Agast,	Æschynomene grandiflora.
Ajmúd,	Apium involucratum.
Ajwáyun, lovage,	Ligusticum Ajowan.
Akhrót, walnut,	Juglans regia.
Alsi, linseed,	Linum usitatissimum.
Amaltás,	Cassia fistula.
Ambára, hog-plum,	Spondias mangifera.
Ambúj,	Neelumbium speciosum.
Amra, hog-plum,	Spondias mangifera.
Amrúd, pear,	Pyrus communis.
Amrút, guava,	Psidium pyriferum.
Ananás, pine apple,	Bromelia Ananás.
Anchu, raspberry,	Rubus, sev. species.
Anesun, aniseed,	Pimpinella Anisum.
Angúr, grape,	Vitis vinifera.
Anjír, fig,	Ficus carica.
Anzarút, gum of,	Penœa mucronata.
Aparajíta, crow's-beak,	Clitoria ternata.
Arand,	Ricinus communis.
Arbi,	Arum colocasia.
Arhar,	Cytisus Cajan.
Ark, swallow wort,	Asclepias gigantea.
Arjún,	Terminalia alata-glabra.
Arús,	Justicia Adhatoda.
Aúnla,	Phyllanthus Emblica.
Aútomal,	Asclepias asthmatica.
Arzan,	Panicum pilosum.
Asók,	Jonesia Asoka.
Asterac, storax,	Styrax officinale.
A'.	
A'da, ginger,	Amomum Zingiber.
A'das, lentil,	Cicer arietinum.
A'k, swallow wort,	Asclepias gigantea.
A'cásním,	Epidendrum, sp. of.
A'cáspawan,	Cuscuta reflexa.
A'carcarha, pellitory,	Parietaria indica.
A'l,	Morinda citrifolia.
A'lu,	Arum campanulatum.
A'lu, potatoe,	Solanum tuberosum.
A'lúcha, green gage,	Prunus domestica.
A'mb, mango,	Mangifera indica.
A'mla,	Phyllanthus Emblica.
A'nár, pomegranate,	Punica granatum.
A'nóla,	Pyllanthus Emblica.
A'nís,	Justicia Adhenatoda.
A'lu shakarkand, sweet potatoe,	Convolvulus Batatas.
A'm, mango,	Mangifera indica.
A'nola,	Phyllanthus Emblica.
A'sgand, Italian winter cherry,	Justicia Adhenatoda.
A'sóg,	Uvaria longifolia.

Hindustani & Eng.	*Systematic Names.*
A'sóc,	Jonesia Asoca.
A'saf, caper bush,	Capparis spinosa.
A'san,	Terminalia alata tomentosa.
B.	
Babái, basil,	Ocymum pilosum.
Babúl,	Acacia vera.
Bacáian, beadtree,	Melia sempervirens.
Bacam, sappanwood,	Cæsalpinia Sappan.
Bacchi, saw wort,	Serratula anthelmintica.
Bach, orrice root,	Iris germanica.
Bach,	Acorus Calamus.
Baclatul húmka, purslane,	Portulaca quadrifida
Bacul,	Mimusops Elengi.
Bacumbur, ground-pine,	Ajuga dioticha.
Bagréndi,	Jatropha Curcas.
Bahéra, belleric myrobalan,	Terminalia bellerica.
Bahuár,	Cordia myxa and latifolia.
Baingan, eggplant,	Solanum melongena
Balà,	Sida rhombifolia.
Baltár, toddy tree,	Borassus flabelliformis.
Banda, misletoe,	Epidendrum tesseloides.
Bang, hemp,	Cannabis sativa.
Ban-haldi, turmeric,	Curcuma longa.
Banj, henbane,	Hyosciamus niger.
Bansóe,	Hedysarum Cephalotes.
Bantarái,	Luffa fœtida.
Bantulsa,	Ocymum, species of.
Banafsha, violet,	Viola odorifera.
Bar, bania ntree,	Ficus bengalensis.
Barna, garlic pear,	Cratæva Tapia.
Baral,	Artocarpus Lokutcha
Bariála,	Sida cordifolia.
Bastara,	Callicarpa americana.
Basant,	Linum triginum.
Bat,	Ficus indica.
Bat, húa, goose-foot,	Chenopodium album.
Bàbul,	Acacia vera.
Bàbúna, chamomile,	Anthemis nobilis.
Bàdám, almond,	Amygdalus persicus.
Bàdranjboyu, balm,	Melissa, species of.
Bàdanján, eggplant,	Solanum melongena.
Bàdyán, anise-seed,	Pimpinella Anisum.
Bair, jujube tree,	Zizyphus Jujuba.
Bàjra, millet grass,	Holcus spicatus.
Bàkla, kidney bean,	Phaseolus vulgaris.
Bàkas,	Justicia Adhatoda.
Bàlehar, spikenard,	Valeriana Jatamansi.
Bàlam círa,	Cucumis sativa.
Bán, benzoin,	Styrax Benzoin.
Bán,	Hyperanthera morunga.

Hindustani & Eng.	*Systematic Names.*
Bánj, oak,	Quercus, species of.
Báns, bamboo,	Arundo Bambos.
Bánstara,	Barleria cœrulea.
Bári, cotton,	Gossypium herbaceum.
Bárna, garlicpear,	Cratæva Tapia.
Báwanhatti,	Ovieda verticillata.
Béd, rattan,	Calamus Rotang.
Bedána, mulberry,	Morus, species of.
Bímráj,	Verbesina scandens.
Bél,	Ægle Marmelos.
Béla,	Jasminum Zambac.
Béri,	Zizyphus Jujuba.
Bétmajnu, willow,	Salix Babylonica.
Bhair,	Zizyphus Jujuba.
Bhamber, Egyptian waterlily,	Nymphæa Lotus.
Bhambúl, do.	Nymphæa Lotus.
Bhang, hemp, Bháng,	Cannabis sativa.
Bhangra,	Verbesina prostrata.
Bhant,	Volkameria infortunata.
Bharband,	Argemone mexicana.
Bhatcataid,	Solanum Jacquini.
Bhéláman, or Bhéláwan, marking nut,	Semecarpus Anacardium.
Bhimb, balsam apple,	Momordica monadelpha.
Bhinda tori, hibiscus,	Hibiscus esculentus.
Bhój,[1] birch,	Betula, species of.
Bhringraj,	Verbesina prostrata.
Bhunblai, strawberry,	Fragaria vesca.
Bhuneampa,	Kæmpferia rotunda.
Bhuta, maize,	Zea Mays
Bicháta, nettle,	Urtica interrupta.
Bichua, rattlewort,	Crotolaria juncea.
Bihí, quince,	Cydonia vulgaris.
Bikh, wolfsbane,	Aconitum, species of.
Bilzar,	Murraya exotica.
Birchiu,	Rhamnus Jujuba.
Birnak, asphodel,	Asphodelus species of
Biriála,	Sida cordifolia.
Birti,	Panicum, species of.
Bis, wolfsbane,	Aconitum, species of.
Bisféz, polypody,	Polypodium vulgare.
Bisk, hupra,	Trianthema pentandra.
Bislumba, colocinth,	Cucumis Colocynthis.
Bímraj,	Verbesina scandens.
Ból, myrrh,	Myrrhis odorata.
Bómla, strawberry,	Fragaria vesca.
Bora, Frenchbean,	Dolichos Catiang.
Bút,	Cicer aretinum.
Buráns, rhododendron,	Rhododendron ponticum.

C.

Hindustani & Eng.	*Systematic Names.*
Cabáb-chíni, cubebs,	Piper Cubeba.
Cachari,	Cucumis madraspatanus.
Cacri, cucumber,	Cucumis utilissimus.
Cacronda,	Celsia, species of.
Cachálu,	Arum Colocasia.
Cachnár,	Bauhinia variegata.
Cachú,	Arum Colocasia.
Cachúr,	Curcuma Zerumbet.
Cadú,	Cucurbita lagenaria.
Cadam,	Nauclea orientalis.
Cahwa, coffee,	Coffea arabica.
Cajúr,	Phœnix sylvestris.
Calmi,	Convolvulus repens.
Calapnát,h,	Justicia paniculata.
Calaúnji,	Nigella indica.
Camúd, dye from	Rottlera tinctoria.
Camrha,	Cucurbita Pepo.
Camrakh,	Averhoa Carambola.
Camal, lotus,	Nelumbium speciosum.
Candúri, gourd,	Cucurbita, species of.
Canér,	Nerium odorum.
Cangni,	Panicum italicum.
Cancrél,	Momordica mixta.
Canac,	Datura Metel.
Capás, cotton plant,	Gossypium herbaceum.
Carbi,	Sorghum vulgare.
Caúl,	Nymphæa Nelumbo.
Cana thént,hi,	Clitoria ternata.
Caril,	Capparis spinosa.
Caréla,	Momordica Charantia.
Carna,	Citrus, species of.
Caramcalla, cabbage,	Brassica oleracea.
Caraúnda,	Carissa Carandas.
Casandi,	Cassia Sophera.
Caséru,	Cyperus tuberosus.
Casumba, safflower,	Carthamus tinctorius.
Catára, globe thistle,	Echinops echinatus.
Catcaranja,	Cæsalpinia Bonducella.
Catira, gum from	Sterulia urens.
Cat,h, extract of	Acacia Catechu.
Cat,hal, jackfruit,	Artocarpus integrifolia.
Cawa,	Convolvulus Nil?
Cáisum, southernwood,	Artemisia Abrotanum.
Cáifal, strawberry,	Fragaria vesca.
Cáfúr, camphor,	Dryabalanops Camphora.
Cáhu, lettuce,	Lactuca sativa.
Cáladána, bindweed,	Convolvulus Nil?
Cálajíra,	Nigella indica.
Cálazira,	Nigella indica.
Cálatulsi, basil,	Ocymum sanctum.
Cándli, purslane,	Portulaca, species of.
Cángan, millet, Cángni,	Panicum italicum.
Cás,	Saccharum spontaneum.
Cásni, endive,	Cichorium Endivia.
Cícar,	Acacia, species of.
Céla, plantain,	Musa paradisiaca.
Céora, screwpine,	Pandanus odoratissimus.
Cét,	Feronia elephantum.
Cétgi, screwpine,	Pandanus odoratissimus.

[1] The bark of this tree, which peels off in layers as thin as writing paper, (for which it forms not a bad substitute,) called *Bój-patta*, is used in making hookah snakes.

Hindustani & Eng.	*Systematic Names.*
C,hira, cucumber,	Cucumis utilissimum.
C,hichri, flower of	Zizyphus Jujuba.
C,hirni,	Mimusops Kauki
C,húnsa, asphodel,	Asphodelus, sp. of.
C,húrsa, purslane,	Portulaca oleracea.
C,húrma,	Phœnix dactylifera.
C,haír,	Acacia Catechu.
C,hajúr, date tree,	Phœnix dactylifera.
C,hajúr, wild date,	Phœnix sylvestris.
C,harzahra, rosebay,	Nerium oleander.
C,has-c,has, tatty-grass,	Andropogon muricatum.
Ciráo, field-pea,	Pisum arvense.
Cisári,	Lathyrus sativus.
Cishmish, raisin,	Vitis vinifera.
Cishmish, currant,	Vitis vinifera.
Cishniz, coriander,	Coriandrum sativum.
Ciwarich, cowage,	Dolichos pruriens.

Ch.

Cháh, tea tree,	Camellia[1] Bohea.
Chagalbáti,	Atragene zeylanica.
Chamnsh,	Arum, species of.
Chaplash,	Artocarpus Chaplasha.
Chalgóza, seed of the,	Pinus Neoza.
Chalta,	Dillenia indica.
Chakoúr,	Cassia obtusifolia.
Chakátra, shaddock,	Citrus decumanus.
Chambéli ratta,	Jasminum, species of.
Chambak,	Kæmpferia rotunda.
Champa,	Michelia Chumpa.
Chandan, sandal-wood,	Sirium myrtifolium.
Chansúr,	Lepidum sativum.
Chaúgara,	Scævola Lobelia.
Chaúlál,	Amaranthus polygamum.
Ch,hina, millet,	Panicum italicum.
Chíni,	Melilotus Baumetta.
Chír,	Pinus longifolia.
Chíti,	Plumbago zeylanica.
Ch,huhara,	Phœnix dactylifera.
Chichinda, beet root,	Beta vulgaris.
Chinar, planetree,	Platinus, species of.
Chiréta, chiretta,	Gentiana Cherayta.
Chirchera,	Achyranthes aspera.
Chirónji,	Cheronjia sapida.
Chitcabra,	Hedysarum pictum.
Chitra, berberry,	Berberis vulgaris.
Chitta,	Plumbago zeylanica.
Chokandar, beet root,	Beta vulgaris.
Chorich,	Gorteria, species of.
Chun hatlee,	Abrus præcatorius.
Chúka, sorrel,	Rumex acetosa.
Chúka tipatti,	Marica paludosa.
Chúkandar, beet root,	Beta vulgaris.
Chilgóza, seeds of	Pinus Neoza.
Chambéli,	Jasminum grandiflorum.
Chúnna, gram,	Cicer arietinum.

[1] The tea tree formerly was considered a distinct genus under the name of *Thea*, but it appears to have merged into the genus *Camellia*.

D.

Hindustani & Eng.	*Systematic Names.*
Dám, dragons blood,	Sanguis draconis.
Dám, juice of	Calamus Rotang.
Daúna,	Artemisia, species of.
Dáb, meadowgrass,	Poa cynosuroides.
Dádmardan,	Cassia alata.
Dáudi, oxeyedaisy,	Chrysanthemum indicum.
Dárchini, cinnamon,	Laurus Cinnamomum.
Dárim, pomegranate.	Punica granatum.
Datura, stramonium,	Datura Metel.
Déla,	Jasminum multiflorum.
Deodan,	Andropogon sacharatus.
Déobara,	Uvaria longifolia.
Déodwara,	Pinus Deodara.
Deogandúm, rye,	Secale orientale.
Deokándar, water-cresses,	Nasturtium officinale.
Deona,	Ocynium, species of.
D,handáin,	Æschynomene, sp. of
D,hanga, coriander,	Coriandrum sativum
Dhanattar,	Clitoria ternata.
Dhaúlichampa,	Liriodendron grandiflora.
D,hatúra, thorn apple,	Datura Metel.
D,háé,	Grislea tomentosa.
D,hák,	Butea frondosa.
D,hán, rice,	Oriza sativa.
D,hári,	Grislea tomentosa.
Dub,	Agrostis linearis.
Dúddhi,	Euphorbia hirta.
Dúddhi,	———thymifolia.
Dúddhi,	Asclepias rosea.
Duparia,	Pentapetes phœnicia.

E.

Elwa,	Aloe perfoliata.

F.

Faltár, female of	Borassus flabelliformis.
Faridbúti,	Menispermum hirsutum.
Fálsa,	Grewia asiatica.
Felfildráz, long pepper,	Piper longum.
Fèlfilgird, black pepper,	Piper nigrum.
Firdúk, filbert,	Corylus Avellana.
Fút, melon,	Cucumis momordica.

G.

Gagand-hool,	Pandanus odoratissimus.
Gandbél,	Andropogon Schœnanthus.
Gandhraj,	Gardenia florida.
Gandana, leek,	Allium Porrum.
Ganna, sugarcane,	Saccharum officinarum.
Gáb,	Diospyros Embryopteris.

Hindustani & Eng.	*Systematic Names.*
Gáchmirch, cyenne-pepper,	Capsicum annuum.
Gájar, carrot,	Daucus Carota.
Gándar,[1]	Andropogon muricatum.
Gánjh,	Andropogon muricatum.
Ganja, hemp,	Cannabis sativa.
Gaoshír, gum resin of	Pastinaca Opoponax.
Géhún, wheat,	Triticum hybernum.
Géla,	Mimosa scandens.
Génda, marigold,	Tajetes erecta.
Ghanúr,	Panicum dactylon.
Ghágra,	Xanthum indicum.
Gháricun, agaric,	Agaricus, species of.
Ghícwár, aloe,	Aloe perfoliata.
Ghúnchí,	Abrus præcatorius.
Gilaú, moonseed,	Menispermum glabrum.
Gilaúnda,	Bassia latifolia.
Góbhi,	Cacalia sonchifolia.
Góbarchamp,	Plumeriaacuminata.
Góchru, caltrops,	Ruellia longifolia.
Góndi,	Cordia, species of.
Góndni,	Typha, species of.
Gúgal,	Amyris Agalocha.
Gulab ca p,hul,[2] rose	Rosa, species of.
Guláb jáman, rose apple,	Eugenia Jambos.
Gulachín,	Plumeria alba.
Gul i dáodí,	Chrysanthemum indicum.
Gul i farang,	Vinca rosea.
Gul i jafri,	Tagetes patula.
Gul i cesh, cock's-comb,	Amaranthus cruentus.
Gul i kyru, holly-hock,	Althea rosea.
Gul mihndi,	Impatiens balsamina.
Gul i mukmul, amaranth,	Gomphrena globosa.
Gul i shab-bo, tube-rose,	Polyanthus tuberosa.
Gul i suranján,	Colchicum illyricum
Gul i sosun, lily,	Lilium, species of,
Gul i turah,	Poinciana pulcherrima.
Gul i abbás, marvel of peru or four o'clock,	Mirabilis Jalapa.
Gul i ajáib,	Hibiscus mutabilis.
Gul i ashrafi,	Linum trigynum.
Gulnár, pomegranate,	Punica granatum.
Gúlar, wild fig,	Ficus glomerata.
Gúma,	Pharnaceum Mollugo.
Gurch,	Menispermum glabrum.
Gurcha,	Menispermum cordifolium.

[1] This is the grass the root of which is the *c,has-c,has* of which tatties are made.

[2] *Gul* is the Persian for a rose; hence *Gul-áb*, literally rose water. In Hindustan, however, the people only know the word *guláb*; the rose they universally term *guláb-ca-phul.*

Hindustani & Eng.	*Systematic Names.*
	H.
Haldi, turmeric,	Curcumalonga.
Halíún,	Asparagus, species of
Har, myrobalan,	Terminalia Chebula.
Harfareóri,	Phyllanthus Cheramela.
Harpar,	Cleome viscosa.
Harjóra,	Cissus quadrangularis.
Harkat,	Acanthus ilicifolius.
Harra,	Terminalia citrina.
Harsingar,	Nyctanthes arbor-tristis.
Hálim, cress,	Lepidum sativum.
Hárjóra,	Cissus quadrangularis.
Hát,hjóri,	Lycopodium imbricatum.
Hina,	Lawsonia inermis.
Húrhuría,	Cleome viscosa.
	I.
Ilâchi, cardamum,	Alpinia Cardamomum.
Imli, tamarind,	Tamarindus indica.
Indráyan, colocynth,	Cucumis Colocynthus.
Indurjaú,	Nerium antidysentericum.
Inkri,	Vicia sativa.
Ipár, thyme,	Thymus, species of.
Isármal,	Aristolochia indica.
Isfànáj, spinage,	Spinacia oleracea.
Ishc pécha,	Ipomœa quamoclit.
Iscíl, squill,	Scilla, species of.
Ispaghol, seed of	Plantago Psyllium.
	I'.
I'c,h, sugarcane,	Saccharum officinarum.
	J.
Jadwár, zedoary,	Curcuma Zedoaria.
Jait,	Sesbaniaœ gyptica.
Jalpái,	Elæocarpus serratus.
Jalpái, olive,	Olea europea.
Jamálgota, croton nut	Croton Tiglium.
Janéwa, doob grass,	Agrostis linearis.
Jantéyana, gentian,	Gentiana officinalis.
Jáundri,	Sorgum vulgare.
Jastadhari, cock's comb,	Celosia cristata.
Jatamasi, spikenard,	Valeriana Jatamansi.
Jawa,	Hibiscus rosa sinensis.
Jawási,	Hedysarum Alhagi.
Jáip,hal, nutmeg,	Myristica aromatica.
Jáfari,	Linum trigynum.
Jáci,	Jasminum grandiflorum.
Jál lakri, valerian,	Valeriana officinalis.
Jám, rose-apple,	Eugenia Jambos.
Jáman,	Eugenia Jambulana.
Járul,	Lagerstrœmia flos reginæ.
Játi, jasmine,	Jasminum grandiflorum.

Hindustani & Eng.	*Systematic Names.*
Jéthi mud,h, liquorice,	Glycirrhiza glabra.
Jharbéri, jujub tree,	Zizyphus Jujuba.
J,háu,	Tamarix indica.
J,hinga,	Luffa acutangula.
J,hinjhóra,	Bauhinia parviflora.
Jira, cummin,	Cuminum Cyminum.
Joár, Indian corn,	Sorghum vulgare.
K.	
Kajúr, date tree,	Phœnix dactylifera.
Kúnsa, asphodel,	Asphodelus, species of
Kurfa, purslane,	Portulaca oleracea.
Kurma,	Phœnix dactylifera.
L.	
Labéra, frouit,	Cordia Myxa.
Lashan, garlic,	Allium sativum.
Laharra,	Holcus spicatus.
Lajálu,	Mimosa natans.
Lauca,	Cucurbita lagenaria.
Laúna, fruit of,	Annona reticulata.
Laúng, clove,	Eugenia caryophyllata.
Lapta,	Panicum verticillatum.
Lasóra,	Cordia latifolia.
Lasan, garlic,	Allium sativum.
Ládan, resin of,	Citus creticus.
Lájwanta,	Mimosa adhenanthera.
Lájwanti,	Mimosa natans.
Lálchandan,	Juniperus Cedrus.
Lálság,	Amaranthus gangeticus.
Lisanul calb, dog's tongue,	Cynoglossum, species of.
Lisanus saúr, borage,	Borago officinalis.
Lichi, leechee,	Dimocarpus Litchi.
Limu, lemon,	Citrus medica.
Lóba, kidney bean,	Phaseolus vulgaris.
Lóbán, resin of,	Boswellia serrata.
Lóbiya,	Dolichos sinensis.
Lókát, loquat,	Mespilus japonica.
Lóna, purslane,	Portulaca oleracea.
M.	
Madár, swallow wort,	Asclepias gigantea.
Magréla,	Nigella indica.
Mahua, wowa,	Bassia latifolia.
Mainful,	Vanguera spinosa.
Majit,h,	Rubia Munjeet.
Majnú, willow,	Salix babylonica.
Mac,hana,	Euryale ferox.
Maco,	Solanum nigrum.
Macoe, sarsaparilla,	
Maiúri, dill,	Anethum Sowa.
Mairrua,	Cynosurus cerocanus?
Marórfali, screwtree,	Helicteres Isora.
Marua,	Artemisia vulgaris.
Marua,	Ocymum pilosum.
Marsa,	Amaranthus oleraceus.
Masúr,	Ervum Lens.
Mastaki, gum of,	Pistacea lentiscus.
Matar, pea,	Pisum sativum.

Hindustani & Eng.	*Systematic Names.*
Maúlséri,	Mimusops Elengi.
Mácáo, Ind. gooseberry,	Physalis peruviana.
Málkangni,	Celastrus, species of.
Márchóba,	Asparagus florum.
Mendasinghi,	Nerium grandis.
Méhudi,	Lawsonia inermis.
Mét,ha,	Melilotus nigulosa.
Mét,hee,	Trigonella fænugræcum.
Mirch, blackpepper,	Piper nigrum.
Mircha, chili-pepper,	Capsicum fruesscens.
Mócha,	Musa sapientium.
Mógra,	Jasminum Zambae.
Mótea,	———————
Mót,ha,	Cyperus rotundus.
Múli, radish,	Raphanus sativus.
Mulhatti, liquorice,	Glycirrhiza glabra.
Mund,hi,	Sphæranthus indicus.
Mung, kidney bean,	Phaseolus Mungo.
Mungfalli,	Arachis hypogaea.
N.	
Nal,	Arundo karka.
Nargis, narcissus,	Narcissus tageeta.
Narcat,	Arundo tibialis.
Náfirma, larkspur,	Delphinium ajaccia.
Nágbél, betel,	Piper betel.
Nágdaúna, wormwood,	Artemisia vulgaris.
Nágdaúr,	Asparagus species of
Nágfani, pricklypear,	Cactus indicus.
Nágcésar,	Mesua ferrea.
Nágarmot,ha,	Cyperis pertenuis.
Nárjíl,	Cocos nucifera.
Nárang, orange,	Citrus aurantium.
Nárangi, orange,	Citrus aurantium.
Náshpáti, pear,	Pyrus communis.
Názbo,	Ocymum pilosum.
Názúc,	Zizyphus Jujuba.
Nicchíkri,	Artemisia sternatutoria.
Nigála,	Arundo karka.
Nirbisi,	Curcuma Zedoaria.
Nircha,	Corchorus capsularis.
Nirmillies*,	Strychnos potatorum.
Nisót,	Convolvulus Turpethum.
Níl, indigo,	Indigofera tinctoria.
Nílofar,	Nymphœa Lotus.
Nímb,	Melia Azadirachta.
Nímbu, lemon,	Citrus medica.
Nímda,	Budleia Neemda.
Nóna,	Annona reticulata.
Nonc,ha, sm. purslane	Portulaca oleracea.
O.	
Ol,	Arum campanulatum.
O'sam, gamboge,	Stalagmitis Gambogia.
O'sir,	Andropogon muricatum.

* The seed of this plant is used like alum to clarify muddy water.

Hindustani & Eng.	*Systematic Names.*
	P.
Padam, lotus,	Neelumbium speciosum.
Padam, cherry,	Prunus Cerasus.
Palwal,	Tricosanthes dioica.
Panyala,	Flacourtia catapácta.
Panuár,	Cassia obtusifolia.
Papia, papaw,	Carica Papaya.
Parás,	Butea frondosa.
Parésiyáushán, maiden hair,	Pteris hinulata.
Paruar, gourd,	Tricosanthes dioica.
Pat,harchúr,	Plectranthus aromaticus.
Patua,tou,made from	Hibiscus cannabinus.
Pákar,	Ficus venosa.
Pálak,	Spinacia oleracea.
Pálak júi,	Justicia nasuta, and Ixora undulata.
Pán, betel leaf,	Piper Betel.
Pápra,	Gardenia latifoliá.
Páral,	BignoniaChelonoides
Páras pipal,	Hibiscus populneus.
Pindalu,	Rottleria indica.
Pistu, pistachio nut,	Pistacia officinarum.
Pitpápra,	Odenlandia biflora.
Piál,	Chironjia sapida.
Piaz, onion,	Allium Cepa.
Pílu,	Careya arborea.
Pípal,	Ficus religiosa.
Pípal, long pepper,	Piper longum.
Podina, mint,	Mentha sativa.
Poi,	Basella alba, and rubra.
Post, poppy,	Papaver somniferum.
	R.
Rachan, basil,	Ocymum pilosum.
Rahala,	Cicer arietinum.
Rakat chandan,	Pterocarpus Santalinus.
Rasaút,	Amomum anthorrhizum.
Ratláu, yam,	Dioscorea sativa.
Rawásan,	Dolichus sinensis.
Rái, mustard,	Sinapis ramosa.
Ráhu,	Ocymum pilosum.
Rámturoi,	Hibiscus esculentus.
Rihán,	Ocymum pilosum.
Rit,ha, soap wort,	Sapindus Saponaria.
Ríuásan,	Æschynomene Sesban.
Rút,hni,	Mimosa natans.
	S.
Saclásuhágan,	Hibiscus phœniceus.
Sacmunia, scammony,	Convolvulus Scammonia.
Sadbarg,	Rosa glandulifera.
Safri ám, guava,	Psidium pyriferum.
Sahajna, horse-radish tree,	Hyperanthera Moringa.
Sahunjna,	
Salai,	Boswellia thurifera.
Samálu,	Vitex trifolia.
Sami,	Mimosa albida.
Samandr-sokh,	Convolvulus argenteus.
Samoodr-sokh,	

Hindustani & Eng.	*Sytematic Names.*
San, hemp,	Crotolaria juncea.
Sar,	Saccharum Sara.
Saral,	Pinus longifolia.
Sarmac, orach,	Atriplex, species of.
Sarput,	Saccharum procerum
Saurif, anise-seed,	Pimpenella Anisum.
Sarkara,	Saccharum procerum
Sam, cypress,	Cupressus, species of.
Sarsóri,	Sinapis dichotoma
Ságudána, sago,	
Sagun, teak,	Tictona grandis.
Sál,	Shorea robusta.
Sálbía, sage,	Salvia officinalis.
Sánwa,	Panicum frumentaceum.
Sárang,	Nymphæa Lotus.
Sátar, origanum,	Origanum, species of.
Séb, apple,	Pyrus Malus.
Séhind, spurge,	Euphorbia, species of
Sém,	Dolichos, species of.
Sémal,	Bombax heptaphyllum.
Sémbal,	
Sénd,	Curcuma madraspatanus.
Sénd,hi,	Phœnix sylvestris.
Sént,hi,	Saccharum Sara.
Séo, apple,	Pyrus Malus.
Séora,	Trophis aspera.
Séoti,	Rosa grandulifera.
Shalgam, turnip,	Brassica Rapa.
Shaftálu, peach,	Amygdalus persica.
Sharifa, custard apple,	Annona squamosa.
Shaitul, mulberry,	Morus, species of.
Sháh-álu, cherry,	Prunus Cerasus.
Sháh-tara,	Oderlandia biflora.
Shákul,	Cytisus Cajan.
Sháma,	Panicum colonum.
Shírkisht, manna,	Ornus rotundifolia.
Shísham,	Dalbergia Sissoo.
Sinbalu, chaste tree,	Vitex trifolia.
Singarhar,	Nyctanthes arbortristis.
Singhara,	Trapa natans.
Sirki,	Saccharum procerum
Siris,	Mimosa Siris.
Siwár,	Valisneria spiralis.
Síj,	Euphorbia neriifolia.
Síp,hal,	Egle Marmelos.
Sísu,	Dalbergia Sissoo.
Sítafal, custard apple,	Annona squamosa.
Sítal chíni, allspice,	Pimenta vulgaris.
Sítalpáte,	Phrynum dichotomum.
Sód,	Anethum Sowa.
Sóla,	Æschynomene indica
Sómraj,	Serratula anthelmintica.
Sónth, ginger,	Zingiber officinale.
Sósan, lily,	Lilium, species of.
Sóia,	Anethum Sowa.
Suc,hdarsan,	Crinum asiaticum & zeylanicum.
Súmac, sumach,	Rhus coriaria.
Súnbúl, spikenard,	Valeriana Jatamansi
Súpári,	Areca Catechu.
Súraj muc,hi, sunflower,	Helianthus annuus.
Sút,hri,	Dioscorea fasciculata

Hindustani & Eng.	*Systematic Names.*
Súran,	Arum campanulatum
Súranján,	Colchicum, species of
T.	
Tambácu, tobacco,	Nicotiana Tabacum.
Tamarihindi, tamarind,	Tamarindus indica.
Taj,	Laurus Cassia.
Tara-tézac, garden-cress,	Lepidium sativum.
Tarbúz, water-melon,	Cucurbita Citrullus.
Taj-i-cahrus,	Amaranthus, a species of.
Tal-mac,hána,	Barleria longifolia.
Târ, palm tree,	Borassus flabelliformis.
Téjpát, leaf of,	Laurus Cassia.
Téndu,	Diospyros Ebenum.
T,huhar,	Euphorbia neriifolia.
Tid,hara,	——— antiquorum.
Til, seed of the,	Sesamum orientale.
Tikur, starch from the root of,	Curcuma angustifolia.
Tisi,	Linum usitatissimum.
Tomri, gourd,	Cucurbita lagenaria.
Tulidun,	Solanum nigrum.
Tulsi, basil,	Ocymum sanctum.

Hindustani & Eng.	*Systematic Names.*
Turái,	Cucumis acutangulus.
Turbad, turpith,	Convolvulus Turpethum.
Turmas, lupine,	Lupinus, species of.
Turanj, citron,	Citrus medica.
Tún, toon,	Cedrela Toona.
Túirt, mulberry,	Morus, species of.
Túar,	Cytisus Cajan.
Tuj,	
U.	
Uampi,	Cookia punctata.
Udambar, figtree,	Ficus glomerata.
Uc,h, sugar cane,	Saccharum officinarum.
Ulu,	Saccharum cylindricum.
Untcatára,	Echinops echinatus.
Usfúr, safflower,	Carthamus tinctorius
Z.	
Zaráwand, birthwort,	Aristolochia, species of.
Zard-chób, turmeric,	Curcuma longa.
Zard-álu, apricot,	Prunus armeniaca.
Zíra, cummin,	Cuminum Cyminum.
Zúfa, hyssop,	Hyssopus, species of.

IV.—*Some Account of the Proceedings of H. M. C. M. Corvette* La Chevrette *in the Indian Seas.*

A little more than two years ago, *La Chevrette*, a Corvette of H. M. C. Majesty, commanded by M. Fabré, visited Calcutta in the course of a voyage, the objects of which, if not purely scientific, are not very easy to be guessed at. It was known to those who had the pleasure of making acquaintance with the officers of this vessel, that much of their attention had been given to scientific enquiries, and in particular we may mention a communication of great interest made by one of them, M. De Blossville, to Colonel Hodgson, then Surveyor General, on the inclination and declination of the magnetic needle. This communication, with some additions by Colonel Hodgson, is, we believe, to appear in the forthcoming volume of the physical labours of the Asiatic Society, of which body M. De Blossville is an honorary member.

We have been favoured by the kindness of a friend, with the sight of a small pamphlet published in Paris, by which we perceive that *La Chevrette* had arrived in France, and that her proceedings had occasioned some interest there. The pamphlet in question is entitled *Compte rendu au Ministre de la Marine des operations faites pendant la Campagne de la Chevrette, commandé par M. Fabré;* and it contains a general enumeration of their labours. It is drawn up by M. Le Contre-Amiral De Rossel, Director of the general depôt of the Marine, and is followed by two Reports from the Academy of Sciences, one by MM. Desfontaines, Geoffroy St. Hilaire, Dumeril, and Baron Cuvier, on the additions made to Natural History; the other by MM. De Rossel, Mathieu, and Arago, on the mathematical and physical results. Of the two latter we here present our readers with translations, as additional proofs of the interest taken by the French nation in all that concerns the progress of science. If to the labours of the officers of *La Chevrette* we add those of MM. Diard and Duvaucel, of M. Dussoumier, and of a gentleman well known in Calcutta, now busily employed in investigating the natural history and physical geography of India, we shall be forced to confess, however humiliating the acknowledgment, that France will have done more in the short period of the peace for making India known to the scientific men of Europe, than England has in the whole period during which she has held the country. There is a general spirit of scientific research diffused throughout the French nation, which can only be attributed to the great interest taken in these enquiries, and to the encouragement afforded to their pursuit by the French Government. In England a very different state of things prevails. There, with a few occasional exceptions, every thing is left to private emulation and the promptings of commercial enterprise; so that those branches of science which do not bear so strongly and directly on public utility, as to offer by their cultivation the prospect of fair commercial returns, are left altogether to their own unassisted resources. Yet, even in the vulgar calculation of pounds, shillings and pence, it is a question, whether the policy of the French Government is not superior to ours. It is at all events certain, that their well furnished museums, with the fame of their *savans*, have the effect of attracting numerous visitors to their capital. It is equally certain, that if any one thirst after scientific knowledge he will go to Paris, as being sure to find there such aid, with "all appliances to boot," as can be had in no other city in the world. In fact, if London be considered the capital of the commercial, Paris may be said to be that of the scientific, world, which, though a smaller body, is not without its influence; at least for good, if not for evil. Paris is preeminently the city of the sciences—yet why it should be so, save from the indifference of our influential personages, it would be difficult to say. Education is of course one of the principal sources of the difference between the two countries; but this is included in the preceding. The institution of the Polytechnic school was a grand step towards developing the scientific talent of the French people. There every youth who has shown any talent for any branch of science may finish his education at the public expense. It is truly extraordinary, that with an example of such an institution and its benefits before the eyes of England, she has never attempted any thing of the kind. Yet to a country like England, such a means of discovering and developing talent would be perhaps of incalculable importance. But the Government, who, in measures of this kind, must evidently take the lead and give the tone to the public mind, are indifferent to the progress, and incapable of appreciating the value of science; and their indifference and want of discernment is of course influential in depressing the scientific character of the country, which otherwise would have nothing to fear from a comparison with that of any nation in Europe. But we must return to our subject.

Report made to the Royal Academy of Sciences, by Baron Cuvier, Perpetual Secretary for the Natural Sciences, on the Zoological acquisitions made during the voyage of La Chevrette *in the Indian Seas.*

The minister of marine has required a report from the Academy on the observations and collections made by the officers of the king's corvette *La Chevrette*, during the voyage she has recently made in the Indian seas; and MM. Desfontaines, Geoffroy Saint Hilaire, Dumeril, and I, have been charged with the preparation of this report, as far as Natural History is concerned. We acquit ourselves of this duty with the greater pleasure, inasmuch as it affords us an opportunity of expressing, in the name of naturalists in general, how much we owe these gentlemen for the important and disinterested services they have performed for science.

It was no part of the duties of these gentlemen to make collections, nor to attend in any way to the pursuits of natural history; but their enlightened zeal imposed upon them this task, and they have performed it as effectively as if they had been destined for such employment from the first. M. Reynard, surgeon of the ship, has set the example, and the other officers, encouraged by their commander, M. Fabré, have seconded him with a good-will worthy of imitation. M. De Blossville, the lieutenant, in particular, as also M. Gabert, clerk of the musters, have not only assisted in adding to the collection during the whole of the voyage, but have also employed such leisure hours as their public duty left them in making drawings of interesting animals, when the number procured was greater than M. Reynard could attend to. He in fact was by no means master of his time: having lost the services of his assistant, M. Brossard, (detained at Pondicherry on account of the public service,) the health of the ship's company depended solely on him; but with the spirit of order and with energy we may do every thing, and this young surgeon has given marked proofs of his possessing both these qualities. Nothing has been neglected: the smallest of the mollusca, the frailest of the zoophytes, have been preserved and ticketed in like manner with the fish, the birds, and the mammifera. All those objects, the form or colouring of which was at all likely to be impaired, have been drawn at the moment; and in the accompanying notes or journal, care has always been taken to enter a memorandum of the locality of each specimen, as well as of such properties or qualities as had struck them. This continued attention to method is the more laudable, that (excepting Bourbon and Pondicherry) the ports visited by *La Chevrette* are not much frequented by our ships, and not one of our scientific expeditions has ever visited them. We allude particularly to Ceylon, to the Burmese country, and the river I'rawádi, which waters it.

Sailing from Toulon, the 29th May, 1827, this vessel put in at the Isle of Bourbon the 27th August; anchored at Pondicherry from the 21st September to the 1st October; from the 2nd to the 6th at Madras; and from the 3rd November to the 1st December at Calcutta. She reached Rangoon, a Burmese port on the I'rawádi, the 21st December, and remained till the 9th January 1828. After a second visit to Pondicherry, and one to Karikal, she put in at Trincomalee, on the N. E. coast of the Island of Ceylon—returned to Pondicherry—visited Batavia, where she anchored from the 20th May to the 10th June—traversed the straits of Sunda—and after a fourth visit to Pondicherry, reached the Cape, where she put into False Bay on the 2nd October, and remained at anchor there till the 11th April, when she sailed on her return to France. It is at these several places, and on the passage between them, that the collections and observations have been made. According to the correct lists which have been prepared at the Museum of Natural History by MM. Isidore Geoffroy, Valenciennes, Latreille Audouin, the collections brought home by the *Chevrette*, comprise 6 species of *mammifera*, 236 of birds, 37 of reptiles, 238 of fishes, 271 of *mollusca*, 16 of the *annelides*, 132 of *crustacea*, 590 of insects and *arachnides*, and 161 of *zoophytes*. There are besides 108 species of shells: the number of individuals of each species varies, but in general it is considerable, and the sum total amounts to some thousands. The most valuable part of the collection are the specimens preserved in spirits; several of these, which we had already obtained in a dry state, are now more within the power of the observer, and afford means of studying their interior structure as well as the most minute details of their exterior—this was much wanted, particularly in the class of fishes, mollusca and zoophytes. We have also acquired in this collection a number of specimens hitherto never dissected, but which being so well preserved may be examined under every point of view; besides there are several species which were not to be found in the Royal Cabinet, and others equally numerous, which, never having been published, may be said to be new for science. Amongst these there appear to be 3 of the *mammifera*; 24 of the birds, amongst which is a new genus in the family of the *dentirostres*; 20 reptiles, including a new genus in the family of the *chelonii*, more than 60 of the fishes; 35 of the *mollusca*; 12 of the *annelides*, including 3 undoubted new genera; 95 of the *crustacea*, and at the least 20 amongst the microscopic subjects. Such are the results to zoology, of an expedition which was by no means intended for the advancement of natural history; results in one sense accidental, and attributable solely to the zeal and good understanding which prevailed amongst the officers, and the scientific education which medical officers of the navy are now enabled to acquire in those excellent schools founded by the Minister and directed by the Inspector-General Kérandren. Such has been the general feeling, that M. Brossard, though detached on a specific duty, unwilling to be left behind by his comrades, has also made some very interesting collections, from which he has permitted the professors of the museum to choose such as they think may be useful to the establishment.

The productions of dry land are, as might have been expected in an expedition of this kind, less numerous than those of the sea; and this remark is particularly applicable to the vegetable kingdom.

Nevertheless, among the nine hundred species nearly, which the herbarium that M. Reynard has formed contains, there are found many new ones. The banks of the I'rawádi particularly, which have scarcely been visited by botanists[1], have supplied some curious ones, the principal of which belong to the grasses. Many of the *graminea* and *apocynea* of Ceylon appear new at the Cape, the families of the *synantherea*, the *proteacea*, and the *restiacea*, include a number of interesting species, which have been added to the Royal garden.

We have the honor to exhibit to the Academy three volumes of figures executed by M. Reynaud, and by MM. de Blosseville and Gabert, who so kindly assisted him. Competent judges will immediately appreciate the character of correctness which distinguishes them; while naturalists must be pleased to see representations of so many *medusæ*, of *biphord*, and of other transparent and gelatinous zoophytes, of so many microscopic *crustacea*, which could only be secured to science by the care which our observers always took to design them when alive, and in the very water in which

[1] With the exception of Doctor Wallich, who discovered there that splendid tree *Amherstia nobilis*.

they had been caught. It is by such labours that we are continually reminded how much yet remains to be explored in the vast abysses of the ocean, and how little we are justified in supposing that we have yet filled in all the particulars of the great plan of nature. If, as we hope, the minister of the marine determine that an account of this voyage be published, a selection from these figures will constitute appropriate embellishments, and the work will form one amongst that series of grand works for which science has been so much indebted to the French navy; we mean the voyages of Peron, of Freycinet, of Duperrey; to which may be added that of Urville, for we have little doubt but that brave officer and his learned companions will arrive in a few weeks with their splendid collection, of which our last reports gave us some idea.

It is a new and peculiar character of these recent naval expeditions, the union of details in natural history, with discoveries in geography; it at once distinguishes them from those of other nations; and while it renders the published accounts much more interesting to the general reader, to whom nautical and hydrographical details would appear perhaps a little dry, it completes, by the knowledge it gives us of their productions, the account of countries of which formerly it was the sole object of these voyages to give a mere description of their coasts.

We think that the Academy should express to M. Reynaud and to the other officers of the *Chevrette* the satisfactien which their researches in natural history have afforded them, and that copies of the present report should be addressed to their excellencies the Ministers for the Marine and for the Interior.

(Sd.) Desfontaines, Gfoffroy Saint Hilaire, Dumeril, Le Baron Cuvier, Reporter.

The Academy adopts the conclusions of this report.

Report made to the Academy of Sciences by a commission composed of MM. De Rossel Mathieu and Arago, *on the operations having a relation to the mathematical sciences carried on during the voyage of* La Chevrette : *27th April*, 1829.

His Excellency the Minister for the Marine wrote to the Academy on the 30th January last, and expressed his wish that they should examine the several operations conducted on board the Corvette *La Chevrette* during the voyage from which that vessel has just returned under the command of M. Fabré, lieutenant in the navy. The commission which you have appointed to report on the collections of Natural History has already expressed to you the satisfaction experienced in the contemplation of what science owes to the indefatigable zeal of M. Reynaud, surgeon of that vessel, and to the assistance which the other officers have been so anxious to afford him. It remains for us to notice those researches which have had for their object the improvement of geographical science—of terrestrial magnetism, and of meteorology. These labours will afford a new occasion to appreciate the zeal and ability of which the young officers of our navy have afforded so many proofs of late years.

La Chevrette sailed from Toulon the 29th May 1827, and put in at Saint Denis, in the Isle of Bourbon, after 90 day's sail. From St. Denis she proceeded to Pondicherry; she afterwards visited Madras, Calcutta, and Rangoon in Pegu. On the 27th January 1828, she again returned to Pondicherry. On the 22d of the same month, La Chevrette took her course for Ceylon, where she arrived on the 29th, having touched in her way at Karical. The objects for which she had undertaken the voyage, detained her at Trincomalee 18 days, and in the harbour of Kaits till the 16th March. She returned to Pondicherry, where she remained from the 19th March to the 1st April. On the 2d she sailed for Java, and visited successively Ainer and Batavia. On the 2d July she was again at Pondicherry for the fourth time. After a month's stay in these roads, La Chevrette sailed for France. In the way there she only touched at False Bay. Finally she cast anchor at Havre on the 11th December last, after being in all 368 days at sea and 194 at anchor.

In the course of this long voyage, M. Fabré has fixed, by chronometer, the position of one of the Cape-verd Isles; he has satisfied himself that the islands Saint George, Roquepiz, and The Seven Brothers, are not to be found as laid down in D'Aprés' *Neputiene Orientale*; he has determined the northern extremity of a passage situated in the archipelago of the Maldives, which vessels from Europe, proceeding to the Coromandel Coast, may in future follow with advantage as well as safety; he has had the course of the Irawádi surveyed by M. De Blossville, from Rangoon to Donabieu; the arms of this river between Rangoon and the sea, by M. Jeanneret; and by M. Pacquet the branch on which Pegu is situated, the ancient capital of the kingdom. On the northern coast of Ceylon, M. Blossville has, under his orders, examined the coast from Cape Palmey to the fort Hano-en-hiel; he has also given a plan of the harbour of Kaits and of its vicinage. In the voyage to Batavia the same officer has observed a sufficient number of points to afford several important corrections, both in the maps of the Straits of Sunda and in those of the north of Java. The road at Batavia has also been the scene of his indefatigable zeal.

La Chevrette was furnished with a complete collection of magnetical instruments, for the purpose of making a series of observations on land. These instruments were verified at Paris before her departure; they have been also examined since her return. During the voyage they were always used wherever the ship touched.

The expedition has then made us acquainted with the amount of the declination, inclination, and magnetic intensity of the needle at Toulon, Isle of Bourbon, Pondicherry, Calcutta, Chandernagore, Rangoon, Donabieu, Karical, Trincomalee, Jaffnapatam, Aripo, Changani, Batavia, and Simon's Town. All these observations have been made with the greatest care ; at each place the accordance of the different needles has been quite equal to what an observer in a fixed observatory could hope for. The observations of the horizontal needle will serve to fix the line of no declination : those of the vertically suspended needle will be not less useful, and will enable us to trace the magnetic equator, the position of which, in India, has hitherto rested on certain ancient and rather imperfect observations. The discussion of these valuable results will doubtless confirm what has been already observed as to the gradual shifting of the line of no inclination from east to west: they may even serve to decide a question as yet involved in doubt—that is, whether the movement of this curve is accompanied by any change in its figure.

The attentive examination we have made of the observations for determining the magnetic intensity, has shown us that their discussion will be attended with some difficulty, the needles having in fact, all of them, had their magnetism impaired during the voyage ; but the attention that was given to these observations at Pondicherry during the different visits made to that place, with the series obtained at Paris to be compared with those in the beginning of 1829, will afford data, we hope, for determining the law of the decrease, so as at least to give results that shall be comparable. Your commission cannot pass to the next article of their report without recording here the names of those who have most diligently studied the magnetic phenomena. We may therefore state, that the observations at Paris, made in 1827 and 1829, are by M. de Blossville ; those made at Toulon before the departure of the expedition, by the commander and the young lieutenant we have just named.

The metereological observations made on board *La Chevrette* during her different passages, will be found to constitute one of the most interesting additions made to science for many years back. These observations have been registered with the greatest care, and are contained in four volumes ; the instruments employed had been compared with correct standards before the departure of the expedition ; they have been also verified since its return. The errors arising from the radiation of the vessel have been carefully guarded against. To form some opinion of the extent of these labours, it may be sufficient to be told that the temperature of the atmosphere and that of the ocean have been registered every hour as well during the night as during the day throughout the voyage. The barometer has been observed regularly during thirteen months, generally 12 or 15 times a day, sometimes every half hour, and in a few cases every 10 minutes. So many observations must give us a more correct idea of the mean height of the barometer at the level of the sea, and on the length of the diurnal period of change at a distance from the shore ; that is to say, in such situations as are subject to but small atmospherical vicissitudes in the 24 hours. We shall also be able to determine whether Flinder's remark, made in New Holland, of the difference of effect on the height of the barometer, occasioned by sea and land winds, be equally applicable to the Indian seas. A few series of comparative observations, made at sea with thermometers having their bulbs whitened and blackened, will have the more interest, inasmuch as Captains Parry and Franklin having made some experiments of this kind in their polar voyages, it has been thought to be deducible from them that the solar rays have less force as we approach the equator. Philosophers will also be pleased to learn that our navigators have determined the temperature of the sea at great depths, by means of well constructed self-registering thermometers. These experiments are particularly curious, inasmuch as they assist us in enquiring how the inferior strata of the ocean acquire such much lower temperatures than what is accountable for by radiation from the surface. They have even a greater claim on our attention, inasmuch as it seems to be more than probable (judging from some recent observations) that salt water has not like fresh its maximum density at a temperature above that of its congelation; while, hitherto, this assumption has been made the basis of every attempt to explain the diminishing temperature of the waters of the ocean. For the labours of which we have just given a sketch, we are indebted to M. de Blossville and to MM. Legay and Vidal, the two quarter-masters of *La Chevrette*, who had been instructed by that young officer in the method of taking observations, and whose zeal has never flagged for a moment. We may add, that M. de Blossville had procured, at his own expense, a part of the instruments which he has employed.

To the catalogue of observations which are to be found in the journals of the expedition, we may add those of the tide, the determination of the height, and of the temperature of certain hot springs at Ceylon, and finally even physiological experiments, to which M. Reynaud, surgeon, and M. de Blossville, equally contributed, viz. on the temperature of man and of different species of animals. Selecting from the crew of the *Chevrette* a number of sailors of different ages, constitutions, and countries, these observations have measured the modifications which different climates occasion in the temperature of the blood, and thus have added a few interesting results to those which Mr. John Davy had already published on this subject.

We have confined ourselves so to say to a mere inventory of the various observations with which the *Chevrette* has enriched science—not that we have not ourselves been occasionally tempted to expatiate on some of the conclusions which may be drawn from them, but that the difficulty of such a task being so much below those incurred in collecting such a mass of truths; and this under the burning sun of the Tropics, make it a sort of duty on our part, even at the hazard of rendering our report less interesting than might be desired, to leave to the authors of them the pleasure of being the first to publish the results to which they lead. Your committee will have attained their object if they have satisfied the Academy that the expedition of the *Chevrette*, though not fitted out for scientific research, will hold a distinguished rank amongst those which have most largely contributed to the progress of science. In this case we would propose that the Academy should express suitable acknowledgements to those well informed and zealous officers whose names are introduced in our report, and should also write to their Excellencies the Ministers of the Marine and of the Interior, to state how desirable it is to have these able and useful labours published without delay. We think too that they might add the expression of their wish, that, as far as should be possible, the preparation of each part of the work should be entrusted to those by whose labour the facts were collected during the voyage. Although the journals have been carefully kept, your committee may notice, that in the examination into which they have entered by desire of the Academy, they have remarked the want, in several places, of those little details and explanations which the memory of the observer himself can alone supply, and which it would be a subject of regret not to find in the printed work.

(Sd.) De Rossel, Mathieu, Arago reporter.

The Academy adopts the conclusions of the report.

Certified conformably:

The Perpetual Secretary for the Mathematical Sciences,

(Sd.) Baron Fourrier.

V.—*On the Substitution of the Hindustani for the Persian Language in all public proceedings; with remarks on a meditated change of the current Coin.*

To the Editor of the Gleanings in Science.

Sir,

A report has for some time been prevalent, of an intention, on the part of Government, to adopt the use of the English language, as the medium of conducting public business, instead of the Persian. As both appear to me nearly equally objectionable, and for the same reasons, while another and better plan is obvious, I beg leave to trouble you with a few observations upon the subject. I am far from asserting that no advantage would be derived from the change said to be contemplated; but surely, when we consider the relative numbers of the English and the natives, and the extreme ignorance and incapacity of the latter, a very trifling benefit is likely to be the result. It is impossible to look forward to any period, however remote, at which the English language shall become the vernacular tongue of this country; and, until it does so, if it is to be made the language of business, there must be a continuance of the same fraud on the one side, and distrust on the other, which constitute the evil at present to be got rid of. The plan which I would propose, has, I believe, been more than once suggested before. What the objections to it are, I really cannot conceive. So convinced am I of the beneficial results likely to arise from it, that I am eager to make, before it is too late, the only effort in my power to get the project entertained. I allude to the introduction of the Hindustani language;—not, however, written in the cumbrous and diversified character of the natives, but in our own more fluent and easy form. The advantages in matters of business to be derived from the public use of Hindustani, as being the language spoken by the mass of the inhabitants, are so evident as to render recapitulation unnecessary. And as it is easily learned by Europeans, in the course of a few months' residence, the English is not likely, in many centuries, to be the means of so ready an intercourse between them and the natives. I feel confident that the substitution of the English character would be attended with no difficulty. I know from experience, that a native, who already understands the use of the pen, will learn to write it with very tolerable facility in the course of a few days. There would probably be great difficulty in inducing the world voluntarily to agree in a common mode of orthography. I propose, therefore, that this point should be settled by authority. A committee of competent persons might be appointed by Government to determine the matter, and their decision should be final[1]. Supposing this fixed, let us consider what would be the easiest means of communicating a knowledge of this mode of writing to the natives. In the first place, it would be necessary that an order should be promulgated by authority, requiring, that, from a certain future day, all public business shall be conducted in the Hindustani language, written in the prescribed form and character. At the same time, public schools should be established, at every station, for teaching this mode of writing at as low a cost as will repay the expenses. As I before said, I am persuaded that a few days only would be required to teach it to every native who already understands the use of a pen; while, by those who are learning to read and write for the first time, there can be little doubt, I fancy, that it would be attained with infinitely more facility than any native character. Again, all private persons should be requested to give the system their support, by employing those acquainted with it in preference to others. This, I do not doubt, would be willingly done. Every Englishman in this country understands Hindustani: very few have any knowledge of Persian. There are few men of business, who do not suffer much from having all their documents, their accounts, letters, bonds, receipts, &c. written in a language understood neither by themselves, nor, probably, by any party interested;—an inconvenience, for which the clumsy expedient of keeping a Bengalli translator, also ignorant of Persian, ill compensates. All, I believe, would gladly give two or three rupees more per month to clerks who will write every thing in a language understood by themselves, and likely in a short time to be understood by every one, especially as the triple wages of the discharged Bengalli, thus saved, would more than pay such additional salaries. Fourthly, in order that those, who possess ordinary intelligence and industry, may be able to teach themselves, a book should be published, at as low a price as possible, which may contain every thing necessary to their instruction. In it the corresponding Persian, Hindi, Bengalli, and English characters, should be printed one

[1] The orthography adopted by yourself, in writing the names which occur in your papers, (the same, if I mistake not, as that employed by Shakespear, in his dictionary,) appears to be the most convenient, not only on account of its philosophical accuracy, but for its practical simplicity. I find an objection, however, to giving to the letter "a" the sound which in English is attached to "u" in the words "but," "rum." It appears to me inconsistent with the broad sound, also given to the same vowel. As there would be difficulty in inventing a new letter, I would suggest, as preferable, *leaving out the vowel*, except at the beginning and end of words, when the "a" might be retained. This would be conformable to the practice of the natives; nor do I perceive that it would be attended with any sort of inconvenience. In proper names, or words of rare occurrence, an apostrophe might be used, to mark its place, or, what would be better, a stroke below, after Gilchrist's fashion, as the apostrophe might be confounded with the accent. I think also the mode of writing the guttural consonants, represented by "*gh*" and "*kh*," objectionable. There is no distinction between them and the g and k merely aspirated. I will propose, instead, some distinguishing mark,—an inverted comma for instance,—over g and *k*, to make them gutturals. Thus I would write the Persian title, '*kan*,' instead of '*khan*'; and the Hindustani for a *newspaper*, according to my orthography, would be '*kbr ka kag'z*, or supplying all the points '*k,b,r ka kag',z*.

under the other. All explanation of the use of the capital letters and marks of punctuation should be given, with a sufficient number of examples, to enable the learner to proceed immediately to reading Hindustani books printed[1] in the new form. Such a first book, or primer, as I have described, ought to be accompanied by the publication of a few of the most popular Hindustani works, printed, on the one side in the English character, on the other, some copies in Persian, others in Hindi, the two being word for word alike. Copy books also should be published, showing how, by joining the letters, the printed (if Italic) may be converted into the written form. The means I have proposed would, I believe, be found quite sufficient; and, were they to be followed, I have little doubt that at the end of a twelvemonth, a knowledge of Hindustani, in our character, would be a very general attainment among the natives. As the latter are but imperfectly acquainted even with their own language, a dictionary of Hindustani words, explained in the same tongue, would be a work of great utility. In the construction of such a work, I would earnestly propose, that, wherever the language is found deficient, the vacancy should be supplied by derivatives from the English, the words thus derived being put into a Hindustani form.

It will be needless to detail what appear to be the benefits with which the plan I have suggested would be attended. They may, I believe, be summed up in two words; 1st, the ready communication it will produce between Europeans and natives; 2d, the introduction among the latter of a mode of communication by written signs; so much more easy in its application, than any they at present possess, as almost to confer upon them the arts of writing and printing.

There is another rumoured change, upon which, if I have not already trespassed too long upon your pages, I am desirous of making a few remarks. I mean the abolition of Calcutta Sicca Rupees, and the introduction of Sonats, as the only coin bearing a different impression from that now in use. Whenever a change is made in the currency of a country, it must be attended with a certain degree of inconvenience. When, therefore, it is necessary that this inconvenience should be submitted to, it is desirable that the change should be to as perfect a system as can be contrived. It appears to me that the coinage of this country is peculiarly capable of being made the most perfect in the world. My plan is the following: I would adopt the *cowry* as the standard. The sum of 10,000 cowries amounts very nearly to 2 rupees. I would have a silver coin of this value, which might be called a dollar, or any other convenient name. This piece I would fix as the integer; considering the remaining coins, down to the *cowry*, as its decimal parts[2]. There would thus be a very convenient coin as the integer, with four places of decimals in actual circulation. The coinage then would consist of two denominations nearly, the dollar and the *cowry*. The tenth of the dollar, a piece of 1000 *cowries*, would be a silver coin a little smaller than a 4 anna piece, which, if it be considered too small, might be increased by the addition of a little alloy. The 100th of a dollar, or piece of 100 cowries, would be a copper coin, a little larger than an ordinary pice. And the 10 cowry piece would be a thin copper coin about the size of a six pence. The names of these intermediate coins might be compound words, expressing their respective values. Thus, (in your orthography) they might be called the "*cauri*," the "*dascari*," the "*saucary*" or "*secari*," the *hazárcari* (which might be abbreviated into "*zárkari*,") and the dollar. I can see no difficulty in making this change to any party. There would not be the obstacles, which have occurred in France, in introducing a decimal system; because the basis of the whole is the very coin, with which the lowest classes are already most familiar. The change too in accounts would be of the most simple kind. It would be a mere reduction of the sums, in whatever denomination of coinage they may be kept, to *couries*, which are always known, marking off the 5th figure for the dollars. At the same time it would be attended with all the often repeated advantages which a decimal system affords in calculation.

[1] I should propose as the printed character, not the *Roman*, but the *Italic* letter *only*. I recommend this, because the latter is merely a more precise form of the written character, and an acquaintance with one would be an acquaintance with both. There would then be only the difficulty of learning one set of letters, instead of two; and it is of importance to get rid of any obstacle, however slight it may apparently be.

[2] If it be objected that the value of the *cowry* fluctuates according to the supply in the market, this might be obviated by a store of them being continually kept in the Government Bank, to be issued when they are at a premium, and to be bought up again when they are at a discount. By these means the relative value of the *cowry* and dollar might be maintained with the utmost exactness.

I might descant more largely on the benefits to be derived from this plan, which, I believe, is altogether novel, but I fear I have already occupied more space than perhaps other people may think my suggestions are worth.

I am, Sir, your's, &c. A SUBSCRIBER.

VI.—*Miscellaneous Notices.*

1. *Indian Archery.*

The following is an extract from a letter from a friend in the upper provinces, on the subject of Indian Archery, and may be interesting to some of our readers.

"You know, I suppose, that the Indian bow being made of sinew, horn, and wood, put cunningly together, is, however, liable to the effects of atmosphere. In the rainy season the bow is always unstrung and laid by. In setting up a new bow and an old bow, after its repose in the damp season, the scientific way, you probably know, is, exposing the *parts which bend*, the elastic parts of the bow, over lighted charcoal, very gradually heating the parts, and bending the bow to its proper form, and placing it perfectly in its proper *perpendicular line*, so that when drawn the string fly not off to either side, and always rest on *and prove the perpendicular line* of the bow. Bending a new bow is done in the same manner with the assistance of an iron chain to act as a bow string, the chain gradually lessened in length by a hook or links in the centre; which is attached, to shorten the chain, to links nearer the ends, as the bow is gradually heated (*senkna*) on its *proper elastic parts*, and brought into the form for being used. A new bow cannot be strung but by these means. I dare say, however, there are plenty of *kamángars* in Calcutta, although the bows are not good. I have sent you six old Lahore bows. The older a bow is, without being over used, the better.

"The arrows of the description we call *jangi*, are the best for all uses. I have not sent any of the butt arrows used here, as we don't at home know the art or try our skill in the fashion of this country. The butt here is fine clay, well manipulated, and mixed with oil:—then packed into a pan and pretty well rammed. The bow-man draws an arrow with a long heavy head, the utmost distance generally ten paces for the masters of the art, which art is proved by making what they call *labi mashuq* (love's lips), burying the arrow to the ivory end, the red nitch only remaining to the sight. A young inexperienced or ignorant amateur will break his arrow every time. In proportion with the expertness of the experimenter the arrow penetrates deep or not deep. It would be a fine amusement at home, for bad weather especially, as the practice may be as conveniently carried on in a small room as in the open air, or in a long gallery. In fact, for battle purpose, steadiness of hand is, along with correct drawing of the bow, that alone which will cause the arrow to fly and penetrate as it should. A good hand will pass an arrow through a bullock apparently without exertion. An arrow thrown or shot by one of us would not go above an inch or two inches deep."

2. *Climate of Ava.*

The following is an extract from a letter by Major Burney, our Resident in Ava, dated Amirapúra, 24th June, and with the appended note of the height of the Barometer and Thermometer, will, we doubt not, be read with great interest, being the first authentic notice we have of the metereology of Ava.

"The Thermometer in the day time was often as high as 92°, and at night we seldom had it more than three or four degrees lower. This slight difference of temperature between the night and day was felt by all of us as particularly oppressive. I enclose an account of the state of the Thermometer and Barometer. The barometer which I received from the Surveyor General's department appears to me to be in a very good condition, and I have been careful in registering it every day since I put it up.

The ministers have promised to give me a letter to the Mysoogee of Yevan-gywoon (Petroleum Town), ordering him to dig and search in his neighbourhood, and collect a large quantity of all kinds of those fossil remains for you. The philosophers and learned men here express much satisfaction at Mr. Crawfurd's having discovered these mammoth or mostodon bones; as they maintain that these remains fully establish the truth of the ancient Burman writings, in which it is said that in former times there were ten species of elephants, each species differing in size and power one-tenth from the one next to it:—that nine of the species became extinct, and that the existing species is the smallest and lowest in the scale."

Register of Thermometer.

March.	6 A.M.	Noon.	4 P.M.	9 P.M.	
23	..	86	94¼	78	From Rangoon to Prome.
24	75	88½	96½	80	
25	72	86¾	91¼	84	
26	74	91¾	99	84	
27	73½	85½	96	81¾	
28	71⅛	86	92¼	85	
29	78	86	97	86½	
30	75	88½	92	83	
31	75½	88	96½	84	
April.					
1	76	87	90½	88	Prome.
2	76	88	97	84	
3	75	88½	98½	88	
4	76	90	88	87	
5	77	88	..	..	
6	77	92	100	87	
7	75	83	80½	76	From Prome to Ava.
8	72	87	89	77	
9	67	88	93½	82	
10	68½	87½	93	87	
11	76	90½	98½	80	
12	70½	93¼	94	83	
13	71½	91½	95	84	
14	75⅓	92½	87½	78½	
15	73⅓	87	88½	77	
16	72	82½	91	84	
17	76	87	90	81	
18	77	98	89	84	
19	77	86	89	86	
20	79	88	92	83	
21	78	87½	87½	81	

Register of Thermometer.

April.	6 A.M.	Noon.	4 P.M.	9 P.M.	
22 ..	74½	87	93	82	
23 ..	71½	86	89	84	
24 ..	80	92½	89	..	Ava.
25 ..	84	88	88	86	
26 ..	82	79	77	79½	Earthquake at ½ past 11 A.M.
27 ..	78½	80	91½	84	
28 ..	80	85	88	85½	
29 ..	79	87½	88	85	
30 ..	79	90	95	89	
May.					
1 ..	82	90	95	90½	
2 ..	85	90	92	88	
3 ..	82	94	99	84½	
4 ..	78	91½	91½	87½	
5 ..	77½	90	94	89	
6 ..	79	89	96½	92	
7 ..	79½	87½	93	79½	
8 ..	76	82	87	84½	
9 ..	76	83	87½	86	
10 ..	78	90½	88½	87½	
11 :	81½	89½	81½	80½	
12 ..	77½	88	88	85	
13 ..	78½	89	91	89½	
14 ..	83	91½	92½	82½	
15 ..	77	84	89	87	
16 .	80	84½	90½	88	
17 ..	75	85	86	80	
18 ..	76	84	82	78½	
19 ..	75½	80½	80	81	
20 ..	78	78	83	81½	

May.	6 A.M.	Noon.	4 P.M.	9 P.M.	Barometer. at 10 A.M.	Ther. atthd.	Barometer. at 4 P.M.	Ther. attached.
21	77½	89	88	85½	29°.576	86	29°.466	89
22	79	88	93½	86½	532	87	542	94½
23	80	87	91	80½	664	86½	576	92
24	76	89	92	82	636	88	506	93
25	80	88½	89	80	608	87	546	90
26	79	90	79	78	686	90	584	81
27	80	86½	82½	78	614	87	514	84
28	77	88	90½	80	624	86½	524	91½
29	79	88	91½	86	662	86	554	92½
30	79½	87	90½	87½	696	86½	562	91½
31	81½	91	92	90	678	89½	556	93
June.								
1	81½	90½	88	84½	628	89	512	89
2	80	88	90	87	572	87	416	91
3	..	86	89	79	574	86½	452	90
4	79	86½	84	80	664	84	562	85
5	78	81	86	83½	712	83	590	87
6	79	82½	86½	86	698	82½	576	87½
7	80½	89	92	88½	670	86½	530	93
8	81	90½	92	90	688	90	560	93
9 ...	83½	92	95	80½	704	90	550	96
10	79	88	91	82½	650	87	528	92
11	81	89½	90½	85½	640	86½	516	90½
12	81	88	90	82½	688	85	552	91
13 ...	80½	91½	86	82	652	85	548	87
14	79	86	89	84	672	84	562	90
15	78½	86	89½	83½	672	84½	544	90½
16	80	86	89	84	664	84½	536	90
17	79	87	89½	85½	650	85½	530	90½
18	79	89	92	85½	654	86	514	93
19	81	88½	91½	84½	644	86	500	92½
20	80	86½	87	83	650	86	540	88
21	79½	86	86½	83	654	86	568	87½
22	80½	90	90	82	606	8[illegible]½	484	91
23	80	88½	91½	83	600	86	468	92½
24	80½	90	91½	..	520	86	408	92½

GLEANINGS

IN

SCIENCE.

No. 19.—*July*, 1830.

I.—*On the Salt-Water Lakes in the Vicinity of Calcutta; with suggestions for filling them up by Warping.*

The position of a European city of such importance as Calcutta, upon the very borders of an extensive salt marsh, of which the insalubrity is so generally acknowledged, has often awakened the surprise of intelligent strangers. Nor is it easy to adduce reasons, why the recovery to cultivation of so large a tract has not before the present moment become an object of consideration to an enlightened Government resident on the spot.

The inhabitants of the infant *factory* of Calcutta might with some reason look for security in the proximity of an immense impenetrable morass upon their flank, and as a defence, would endeavour, with jealous care, to preserve it in the same state. But the residents in the CAPITAL of an empire no longer exposed to prædatory inroads, must view it in a different light. Health and appearance have long taken the place of security and defence, as objects to be kept in view in the local improvements of the place; and there is only one way of accounting for the subject having escaped attention, which offers any probability of truth. It would seem that the idea of the lake being necessary to the military defence of the city, has been succeeded by an impression not altogether unfounded, and which appears yet to be rather generally entertained; viz. that the existence of the salt marsh in its present state is indispensable to the perfect drainage of the town.

Upon this point, then, it is necessary to be most particularly informed, before any measures are proposed that would interfere in any way with the present system, whether that system be effective or otherwise. I shall offer my observations upon this point in the first instance, and hope to place the subject in such a light, that its reference to any scheme connected with the city will be at once comprehended.

The river Hoogly, the western boundary of the delta of the Ganges, as well through its connection with the great river as from the streams flowing into it from the west, is subject to an annual rise, technically called the *freshes*, commencing in June and terminating in October. In the central expanse of the delta there are likewise several streams, having connection with the Ganges, which are subject to the same influence, rising at the season named, according to the quantity of influx of fresh water from the Ganges.

The surface of the delta intervening between these, is divided into a perfect labyrinth by tide backwater creeks, which are subject to no other change of level during this season, than what is derived from the vicinity of the great discharge of water from the embouchures of the great river, and the effect of this discharge upon the tides in the upper part of the bay of Bengal; added to the trifling discharge of rain water upon the surface of the country immediately in contact with the creeks.

Calcutta is situated on the Hoogly, below the parallel of latitude to which these tide backwaters are found to run; for in the general slope of the surface of the delta from its upper fork at Jellingi to the sea, there is of course a limit beyond which the tide influence could not extend. And this city built along the crest of the high banks of the river, which are generally above the elevation of its highest known floods, has, immediately behind it, an expanse over which the tides of these backwaters spread, thus dissipating part of their force and elevation.

I may mention here that it is a general principle of these creeks, that they either possess at their upper termination a *jhíl* of this nature, over which they spread and dissipate their force, or else when they are connected with a continued channel, receiving some little water from the great river, the tide, in its run up a long continuous channel, gradually spends itself by the length of its course and friction of the bed.

It is a question of some difficulty in hydraulics to determine the level of the surface of the highest and also of mean tides in these *jhils*, or what may be called the point of dissipation of the creek tides, relatively with the highest level of the tides of the bay from which they are fed; and when I hazard the assertion, that the point of dissipation is below the highest level of the parent tide, even at the distance of 70 miles from the sea, which I believe to be the case universally, I naturally do so with some caution, as the subject has not been considered by any mathematical writer upon the subject of tides.

It is not my purpose to enter more particularly into the reasons that have led me to such a conclusion in this place[1].

The *jhils* above mentioned, and the salt lakes in the present case, from their depression below the general surface of the country, naturally become the receptacles of all its superfluous water; and where this supply is considerable, the general level of the *jhíl*, as also of the creeks which run up into it, is influenced in proportion during the season of such supply. In no case, however, does there exist a rise from such cause at all in proportion to the influence, in parallel latitudes, of the permanent freshes of the great river, when admitted into a branch like the Hoogly. The effect of such supply creates a general slope of the surface of the lake or *jhíl*, proportionate to the expenditure required, and in the creek, which is the outlet of the Salt-water Lake for this expenditure, the summit level is found very little (not 1 foot) above its extreme summit level in the dry months, when only the tide of the sea enters it; and the discharge is then effected by an inequality in the duration of the flood and ebb, in the same way that the freshwater of a river is carried off.

The supply of fresh water which now enters the salt-water lakes is found to raise its mean surface towards the centre or opposite Baliaghát 2 feet. This level is, however—which would not be at first supposed—still below the summit level of the tides of the sea.

I have made the above remarks to explain particularly the nature of these backwater creeks, because in case of the obliteration of the lakes by their being filled up or embanked against the tide, and a canal connected with these creeks being brought in circuit round one half of the city to be drained, it is essential to know what will be the state of the tides under these new circumstances. Indeed the whole question of the expediency of recovering the lake for agriculture, as well as the means to be adopted for the purpose, will turn much upon this point.

The first case to consider is—Supposing the removal of the lakes, and substitution of a closed canal of the above nature to Chitpúr; what would be the effect upon the present tide levels at Tárda, below the confluence of the lake outlets, and Tolly's nulla? Also to assign a position to the summit level of the present Tárda tides upon our general tide-gauge.

Upon the general principle that the extreme level in a creek nearer the parent tide must be above the highest surface in the lake of dissipation when there is no foreign supply of *jhíl* or rain water, and no supply into this lake but direct from the sea by this creek, I have supposed that the present extreme tide at Tárda in the dry season, April and May, is nearly the mark + 1; while the highest level near Baliaghát is at ⊙; and that the extreme levels at Tárda and in the center of the lakes during the rains nearly correspond to + 2. Upon like reasoning it may be shown, that by withdrawing the influence of the lakes, the extreme level at Tárda will be raised a little; but as the tide will have a lengthened canal in which to dissipate part of its force, (while all influence of the river is withheld,) the extreme tide can never exceed what it is at Tárda at present, or + 1 in the dry months, unless the dimensions of the canal are much enlarged. Its highest level in the rains will depend upon the method of draining the town, and the waters of the surrounding country into this outlet, as also upon the question hereafter to be discussed, whether any additional quantity of water is to be thrown in for filling the lake, and where its discharge is to be effected.

The floors of the present Circular-Road drains have an altitude on our guage at the respective places, as follows:

		ft.	i.
Head of Dhermtólah, ...	Standing at	+ 2	1
,, Bowbazár, ...		+ 5	1
,, Matchuabazár, ...		+ 2	4
,, Maniktólah road,..		+ 3	3
,, Sa'mbazár road [2],............		+ 2	10

[1] Vide notes on the Tides, published in former numbers of this journal.

[2] Marhatta Ditch.

These will allow of the drainage remaining without alteration, as long as the water in the canal does not reach above + 3 during the season of rain. It is therefore clear, that the lake's being withdrawn or embanked, will not embarrass the drainage of the town, although the circular canal brings the Sundurbun tides in immediate contact with the drain outlets. It remains now to provide for the maintenance of the same level of + 3 during the rains.

The lake remaining as it is, and no water being admitted from the river, the highest level on the canal, except from the small influence of drain water from the town, will not exceed + 2, and can scarce reach that mark[3]; and this influence of town drain water I consider will never, even in flowing tides, raise the Chitpúr end of the canal more than 1½ foot under any circumstances.

It must now be considered that the drains, with the exception of the Dhermtolah[4], under the new circumstances of being so much nearer their outlet, will have great advantage; and as the lowest spot in Calcutta is much above[5] the highest of these drains, there will be rather an advantage than otherwise in now and then allowing the tide to enter their mouths.

I may therefore safely assume, that any scheme may be adopted which will not raise the waters above + 3 during the rains; and that under the contemplated circumstances, an additional level of a foot may thus be given to the surface of the lake, for the purpose of raising its bed with sediment, without danger to the drainage.

The Salt-water Lake adjoining Calcutta, covers a surface of 18.5 square miles of land. From its lowness in level it is at present the receptacle, during the rains, of the waters of a considerable surface of country, besides the more immediate drainage of the city of Calcutta. The principal influx of water is from the north and west; and in the rains, when this is at a maximum, there is an evident general slope of the surface of the lake proportionate to this expenditure, which has been detected by the several series of levels which have been carried on at different periods.

The surface of this lake is naturally divided into three compartments: two north of the present navigable[6] channel and lake outlet; a third south of that channel. Of the two first, the western, containing 5.5 square miles, at present receives the greatest influx of extraneous water from the north towards Damdama and west from Calcutta; but neither yields so great a depth of water, nor presents to the eye so great an expanse free from vegetation as the neighbouring compartment of 7 square miles, which is separated from it by a natural rise in the bed upon which vegetation has taken root, although the surface is almost at every tide under water.

The surface of the upper end of the first named compartment towards the Sámbazár bridge, is said by Major Schalch to have an elevation of 2 feet above what I have ascertained to be the extreme level near Baliaghát, which would give a slope of 7 inches per mile to the surface. Now as I know this elevation to be very partial, and owing to a certain admission of tide by the Sámbazár ditches[7], I must reject such a slope, as at variance with every other observation I possess relative to its slope, and particularly the velocity of its waters, which certainly never attain to any thing near what would obtain with such fall[8]; and I believe I shall be within the mark in assigning a slope of 3 inches per mile as an extreme to the surface of this compartment during the rains, generally from the Sámbazár enclosures to the Baliaghát *chokis*. The expenditure in this case becomes 11 612 080 cubic feet

[3] A fall of 1 inch, upon 5 square miles of the town will give 11 616 000 cubic feet of water. The canal discharge per hour, when sloping 6 inches per mile, will be 4 376 064 cubic feet. It would never be necessary to drain off an inch of water in less than 3 hours. The slope of the canal being raised to 9 inches per mile, the expenditure will be 6 572 736 cubic feet per hour.

[4] This drain exit has so violent a slope at its mouth, as to do more at present; and an elevation of 1½ of water would be advantageous to it.

[5] Lowest part of Calcutta at the mark + *8ft. 2in.* on the gauge.

[6] Area of the Western, 5.5 square miles. Eastern, 7 ditto. Southern, 6 ditto. Total, 18.5 square miles.

[7] Major S. took this level under the bridge, and neither allowed for the run of one mile thence to the enclosures of the lake, nor the afflux in passing through these enclosures.

[8] The section being 15840 square feet, or $5280 \times 1.5 \times 2 = 15840$.

$\left. \begin{array}{l} r = 2 \\ \frac{1}{s} = \frac{1}{7051} \end{array} \right\}$ rate becomes 1213 feet, or about ¼ mile per hour.

per hour, with a rate of 739.5 feet (about one-seventh of a mile per hour). This is even much beyond what I can believe to be true. The discharge of water from all the compartments of the lake, takes place rather by spilling over the banks of the outlet channel, (which have been raised to a level sufficient to be laid bare in th low tides of the dry season, by the silt left on them by the continued flux and reflux of tide,) rather than through any more defined side outlets; and it therefore becomes difficult to measure the exact quantity of general expenditure from the whole lake in any state, except by an estimate from the dimensions and rate of this outlet, which is subject to a reflux in all cases[9].

That the lakes, if left to themselves, would, in the course of time, fill up of their own accord, I have little doubt; and that such process is now in activity is confirmed by the rapid increase of vegetation upon its surface within my own knowledge. I cannot, however, learn that much ground has been gained to cultivation during the last forty years; and although the amount of yearly filling up must at the present time be much greater than it was forty years ago, another period of this length will probably, unless other means are provided to increase the yearly sediment, witness a very sorry progress towards their final recovery. Unfortunately, during the present state of things, the influx of *jhíl* water, although much of it is originally derived from the colored waters of the river Hoogly, brings little or no sediment; and as the water is quite clear before it reaches the channel of outlet, its discharge checks at this time any silt from the daily tide, and also washes away much that may have formed during the dry months.

The two principles, therefore, upon which I would rely for a more effectual recovery of the lake, and of this compartment in the first instance, are, to prevent the influx of any water, but tide-water, that does not proceed directly from the Hoogly, or from a source whence sediment is certain, as from the drain outlets; and to encourage the growth of vegetation along the banks of the present channel; until, by this means, in the course of a very short time, it will be easy to form a slight embankment at small cost, and over which the water may still occasionally spill.

The quantity of river water to be admitted (and consequently the dimensions of the feeder canal) must then be regulated by the heighth of the waters in the lake, which may at any time be lowered by a slight check to the feeder canal, by which means the discharge will gain upon the supply. I would propose that the waters of this compartment of the lake be kept at an average additional elevation of less than one foot, which would injure no property in the vicinity; or what would perhaps be still more perfect, that the supply be let in during the neaps, and so restrained in the springs, that the lake near Baliaghát shall remain at a constant level, of 6 to 9 inches above its present extreme; it now varies a foot during springs and neap height.

I will now consider whether a feeder of 100 or 200 square feet of section, and excavated in a way that would secure it from accident, or from endangering the vicinity, would ensure a sufficient supply for this purpose, and what quantity of sediment might be looked for during the year with this one feeder.

The surface of this division of 5.5 square miles, at an average depth of 2 feet, will give its contents of water 306 662 400 cubic feet. The contents of the adjoining division must be about double this quantity. Now a feeder canal of 100 feet section, trapezoidal, having the slope of 1 foot per mile, will run at a velocity of 1.2 mile per hour, and supply 634 600 cubic feet only in that time. Increase the fall of the surface of this feeder to 2 feet per mile, and with the additional section gained from its trapezoidal shape, the rate becomes 2.4 miles per hour, and the supply 2 194 614 cubic feet per hour. I should not think it advisable to exceed this velocity.

For the months, July, August, and September, I may safely reckon, from the known state of the tides (and with allowances for closing the feeder when the river is too high, &c.) upon a medium supply of 900 000 c. ft. per hr. for 12 hours of each day. This supply (900 000 × 12 = 10 800 000 cubic feet per day) would furnish a quantity equal to the present contents of this compartment [10] in 30 days, exclusive of water brought from the drains and tide channel, and therefore I might reckon it possible to fill this compartment of the lake in the course of the rainy

[9] Section at Bahminghátà 120 × 24 × $\frac{2}{3}$ = 1920 sq. ft. and 1920 × 5280 × 2 mile per hour = 20 375 200 cubic feet, so that when the lake is full the daily discharge cannot be less than 50 000 000 cubic feet.

[10] Said before to be 306 662 400 cubic feet, and 306 662 400 ÷ 10 800 000 = nearly 30.

season, with four times its present contents of turbid water direct from the Hoogly, by means of a single feeder of the dimensions named.

The proportion of sediment in the waters of the Hoogly is at present unknown. It is, therefore, impossible to say what would be the quantity produced from a depth of 8 feet water[11]. Several inches may be expected; and if this be not thought sufficient, it is always possible to increase the supply, by enlarging the feeder or furnishing a second: for it is quite evident, that as the hourly supply of 2 000 000 cubic feet is so small, compared with what I have estimated as the expenditure along this compartment of the lake (11 612 080 cubic feet per hour), and a daily supply of 10 800 000 cubic feet, so small compared with the daily discharge by the Bahmingháta outlet, that the rise of the surface in consequence of so trifling a supply will not be 6 inches.

It will be said that the method I have now proposed embraces only one third of the whole lake; but it must be remembered that it is the one of most importance to the town, and likely to yield the greatest return. The same feeders, when they have completed this portion, may be turned into the second compartment, and the self-same principle may be applied to the southern division, if thought necessary, by a feeder direct from the high parts of Tolly's Nulla about Kálighát.

The river Hoogly, it must be remembered, is the only source from which turbid water can be drawn, and this only during the four rainy months, 15th June to 15th October. The quantity of sediment in the Hoogly water will bear no proportion with that in the Ganges, and the tide waters of the Sundurbuns are notoriously clear during the rains, as also during the dry season, except in the spring tides, when they have inundated the land.

I have already mentioned that during the rains there is a general slope of the surface of the lake, from the north towards the present outlet. To make this outlet the feeder, it would be necessary, to invert the present slope, or raise the water-level near Baliaghát above what it is at present near Sámbazár, which must unavoidably interfere with the present drains. Besides, that by this means, as the present channel must be at once its supply and place of discharge, the supply, according to every sound principle, cannot be so constant and unremitted as if the supply were separate and at the upper end; and no hope can be formed that the bed will be raised rapidly by any means but a constant introduction, during the rains, of river water.

I have purposely connected the present scheme with the system of canals now carrying on; but upon mature consideration, I can see no good reason why any alteration in those canals should be necessary for this additional object. The discharge canal must necessarily be the best adapted for navigable purposes, from its depth and tendency to deepen rather than fill up; and from its lower level it must be fittest to receive all drain water.

If it is said that no real efficacious method can be adopted until the whole lake is embanked, I need only refer to the very trifling additional rise of the surface of the lake-waters necessary in the present scheme; and to mention that the adoption of such plan will, in the course of a very short time, give the means of embanking at a very trifling expense, where now such a measure would be attended with very great charge and difficulty.

If the circular canal is made the feeder of turbid water to raise the bed of the lake, it must unavoidably be subject to all the disadvantages which I have pointed out in a former paper upon that canal; besides that other means must be provided to carry off the drainage of the town. This last point alone, in expense, would oppose a serious obstacle to such a measure. For as the only low level fit for receiving the drain water will be, in this case, either the lake itself or the series of *nalas* eastward of the lake, tunnels under the canal must be excavated, or a second canal of sufficient dimensions must follow the line of the circular canal, and be continued perhaps to the very eastern boundary of the lake.

By the present scheme, during the dry months, when the feeders no longer serve, the tide would be allowed as before to spread upon the lake, for I would by no means offer any check to this spreading. Here also would the action of silting up be accelerated, by the circular canal into which a higher tide and dirtier water, from its rapid passage through the canal, will flow from the river, and not be allowed to return.

I subjoin a table of the several levels, as referred to the tide guage, and a schedule of the method I would adopt for raising the bed of the lake. I feel some degree of confidence that the principle of this scheme, with perhaps some modification of its details, will, in the end, be acknowledged to offer advantages and economy beyond every other design that could be proposed for the purpose. P. T.

[11] Two feet average depth supplied four times = 8 feet of depth.

Land Levels.	Feet.	Tide Levels.
Highest level, Strand near Old Mint, 20 feet, 6 inches, ...	20	
	19	
	18	
	17	
Roads near Govt. House, 16 ft. 6 in.	16	
	15	
Stone at Chandpál Ghát, 14 ft. 6 in.	14	
	13	13 ft. 2 in. highest rise of Hoogly, in August and Sept.
	12	
	11	
	10	
	9	
Lowest part of Calcutta Muchwa Bazar, 8 ft. 6 in.	8	8 ft. 3 in. Spring floods, in March, April, May.
	7	7 ft. 1 in. Neap floods, Hoogly, in Aug.
The Rice plains north of the Lake, have a surface, sloping gradually towards the Lake from about 7 ft. to 1 ft.	6	
	5	
	4	Lowest neaps in September.
	3	
The floors of the town drains along the Circular Road stand between 5 ft. 1 in. and 2 ft. 1 in.	2	2 ft. highest Lake tide at Baliaghát in Aug. and Sept.; also highest ebb of Hoogly in August.
	1	Lowest neap flood of Hoogly in March.
Zero of Levels.	O	*or average River Level.*
	1	0 ft. 3 in. ordinary high water in Lake and Canal, from Dec. to April.
Average bed of the Salt Lake, 1 ft. 6 in.	2	
	3	2 ft. 9 in. lowest neap ebb tide observed in Canal and Lake.
Lowest bed of Lake, 3 ft. 3 in.	4	
	5	
	6	5 ft. 4 in. Neap tide ebbs in Hoogly in March.
	7	
	8	7 ft. 5 in. lowest spring ebbs in Hoogly, in March and April.
	9	
	10	

Estimate of the expense likely to be incurred in raising the bed of the Lake to a level fit for purposes of cultivation; as also of the probable returns.

1^mo^. A closed drain must be excavated from the Sámbazár bridge to Mohisghát, 3 miles, to convey the present supply of the Damdama *jhil* water into the Eastern division of the Lake section, average 90 square feet, costing about 1200 Rupees per running mile,...................... 3,600

2do. One feeder canal to be excavated from Chitpúr to the Western Lake; the floor at its river mouth excavated to + 2, which will require 16 feet of digging. The floor at the Lake end at ○, or perhaps, +, 1 to be 27 feet or 25 broad at bottom, and trapezoidal, so that 6 feet of water may give 200 square feet average section of such an excavation being 530 square feet, will cost 7,680 Rs. per mile, for length of 3 miles, 23 040

3to. Three bridges will be necessary over this canal, for the several roads; to each bridge I would provide some contrivance for closing the canal, estimating each at 15,000, ... 45 000

4to. Sundry expenses for preserving a channel as fast as the Lake may fill at the place of discharge from the feeder canal; as also in embanking the side towards the discharge canal, when sufficiently raised, also the western side, and watching the place of discharge, ... 15 000

Supervision and contingencies, ... 4 332

5to. Add the compensation for the acknowledged rights of fishery and incomes now enjoyed upon the faith of the rights conferred by the perpetual settlement, (estimated at 1 lac of rupees for the whole lakes,) for the Western compartment, ... 30 000

Total expense and outlay, ... 1 20 972

N.B. The above does not include any charge for purchase of ground for the feeder canal, and provides only a single feeder. It will be possible, I believe, to find ground from that purchased for the circular canal, without any very material sacrifice in the resale of ground from which a certain return is calculated upon.

As *per contra* to the above :—

The contents of the Western compartment of the Lake are 10 630 begas of land, the value of which nett, when fit for cultivation, may be taken at the least 30 rupees per bega, yielding, ... 3 18 900

It would be easy to shew that the recovery of the second compartment, in surface upwards of 12 000 begas, would be still more profitable in return, as it would be free from the great expense of excavation near Chitpúr and bridging the canal. With an additional outlay of 50 000, this second compartment would yield a return of three lacs; but as this part of the undertaking is remote, I wave any further remarks on it in the present paper.

II.—*On Indigo.* *By* Andrew Ure, *M. D., F. R. S.*

[From the Quarterly Journal of Science, No. XIII. N. S.]

Among the vast variety of vegetable products, there is probably none so interesting to Science, by the curious complexity of its nature, and the protean shapes it may be made to assume, as indigo; and certainly, there are few more important to British commerce and enterprise, since it constitutes the most valuable article of export and remittance from Hindostan. At the four quarterly sales appointed by the East India Company, no less than 20 000 chests of this dying drug are, on an average, brought annually into the market. A very considerable quantity of indigo is also imported into Europe from America and Egypt. It is not long since the Caracca and Guatemala indigo held a much higher character, and commanded a much better price than that of India; but the improvements due to the intelligence of our planters in the East, have, within these few years, enabled them to prepare an article very superior to the finest American. The sequel of this paper will present satisfactory proofs of this assertion.

Indigo is procured from many different species of plants, belonging to Tournefort's natural family of *Leguminosa*, included, for the most part, in the genus called *Indigofera* by Linnæus. According to Heyne, the *Indigofera pseudo-tinctoria*, cultivated in the East Indies, produces the best indigo; but others extol the *Indigofera anil, argentea,* and *disperma,* which yields the Guatemala kind, and some the *mexicana.* About sixty species of *Indigofera* are at present known; but those abovenamed are in peculiar esteem. My object in stating these differences here, is chiefly to show that a drug obtained from such a variety of vegetable species, must necessarily vary in composition. The matter which affords the indigo, is confined entirely to the pellicle of the leaves, and exists in largest quantity at the commencement of maturation, while the plant is in flower; at a somewhat later period the indigo product is more beautiful, but less abundant; afterwards much less of it is obtained, and of a worse quality. The plant is remarkable for giving a blue tinge to the urine and milk of cows that feed upon its leaves; a circumstance which accords with the known permanence of the dye. The statement of Mr. Weston, in this Journal, agrees with these observations on the ripening of the blue principle. He shews that the developement of this matter in the *Indigoferæ* goes on in the leaves, even after they are separated from the plant and dried. When packed up for a few weeks, more or less according to their preceding state of ripeness, the leaves assume a light lead colour, which gradually deepens into a blackish hue. The planter studies to seize the period at which the maximum portion of colouring matter is formed, that he may then transfer the leaves to the steeping vat.

Three different processes are employed for extracting the indigo, each of which must modify more or less the nature of the product. In the first and second, the dried leaves are operated on; in the third the recent plant. For the perfect success of the two former processes, the plant should be very speedily deprived of its water of vegetation; hence the *Indigofera* is reaped only in fair weather. An hour and a half before sunset, the plants are cut down, carried off the field in bundles, and immediately spread on a dry floor. Next morning, at six o'clock, the reaping is resumed for an hour and a half before the sun acts too powerfully on vegetation, and the plants are treated in the same way. Both cuttings become sufficiently dry by 3 o'clock, P. M. to permit the leaves to be separated from the stem by threshing. The leaves are now thoroughly dried by exposure to the sun, then coarsely bruised, or rather ground to powder in a mill, and packed up for the use of the manufacturer of indigo.

From these powdered leaves, the dye stuff is extracted, either by simply digesting them in water, heated to 150° or 180° F. in as small a proportion as may be practicable, and subsequently beating the infusion with paddles till the blue indigo granulates, as Roxburgh recommended; or by mashing the ground leaves with twice their bulk of water, at the atmospheric temperature, drawing off the liquor into a vat, where it speedily undergoes fermentation, and is beat as above with paddles or oars, till the blue indigo forms. Some persons prescribe the addition of lime water at this stage of the process; others reject its use.

In operating on the recent plant, it is laid in bundles in the steeping trough, *(trempoir)* which contains sufficient water to stand about two inches above plants slightly pressed down by crossing bars of wood. A brisk fermentation soon begins, with copious extrication of air bubbles. This process is suffered to proceed till the liquor has become green, and casts up a pellicle of a copper-red hue. A sour smell is now perceived, and the blue colouring particles seem ready to separate. This happens commonly at the end of from ten to twenty hours, according to the temperature of the weather. The liquor is then run off into the beating vat, and lime water is added, or not, according to the fancy of the operator. In all cases of fermentation, whether the dried leaves or the recent plant be employed, it is proper to watch the progress of that change with solicitude; because, when too violent, it not only decomposes entirely some of the indigo blue, but introduces much foreign vegetable matter into the precipitate; when too feeble, it is said to leave some indigo unextracted.

From the differences which exist in the nature and culture of the *Indigoferæ*, and of their treatment by the manufacturer, the product, indigo, as found in commerce, differs remarkably in quality and chemical composition. In this respect, it forms a complete contrast to the simple crystalline product sugar. Besides impurities, accidentally present, from a bad season, want of skill or care, the purest commercial indigo consists of no less than five constituents:—1. *Indigo-blue*, a very singular vegetable compound of carbon, hydrogen, and oxygen, with fully 10 *per cent.* of azote.—2. *Indigo-gluten*, a yellow or brownish yellow varnish, which differs from wheat-gluten by its solubility in water. It has the taste of osmazome, or of beef-soup, melts when heated, burns with flame, and affords an empyreumatic oil along with ammonia by distillation.—3. *Indigo-brown;* this constituent is more abundant than the preceding; it is extracted by a concentrated water of potash, made to act on powdered indigo, previously digested in dilute sulphuric acid. Chevreuil's indigo-green seems to have consisted of this substance, mixed with some alkaline matter, and indigo-blue.—4. *Indigo-red;* this is readily dissolved by boiling alcohol, out of indigo previously subjected to the action of an acid or alkaline menstruum; the alcohol acquires a beautiful red-tinge, and leaves, by its evaporation, the red principle in the form of a blackish-brown varnish.—5. *Phosphate of lime.* I have found the bone phosphate in notable quantity in some fine indigo, constituting another feature of resemblance between this vegetable and animal products. Hence, also, the charcoal of indigo is most difficult of incineration, and requires, for perfect combustion, in some cases, the deflagratory powers of nitric acid.

Pure indigo-blue is most easily obtained from the blue vat of the indigo-dyer; the yellow liquid of which being acidulated faintly with muriatic acid, and exposed, with occasional agitation, in a shallow basin, soon deposits the blue precipitate, mixed, however, with a considerable quantity (more or less according to the quantity of indigo used,) of indigo-red. This must be removed from the dried blue powder, by the solvent action of boiling alcohol applied in successive quantities.

In my paper on the *Ultimate Analysis of Vegetable and Animal Substances*, which the Royal Society did me the honour to read at their meeting in June 1822, and to publish in the volume of their Transactions for that year, I gave an analysis of indigo-blue, to which I appended the following remarks:—"I had intended to pursue, at considerable detail, my researches on this azotized product of vegetation, but the subject having been lately taken up by my pupil and friend Mr. Walter Crum, I was induced to leave it in his hands." I then thought it likely that some slight modification might require to be made in the weight of the constituents given by me, for "I did not (then) resume the subject of indigo, after I had become most familiar with the manipulations. I have found since that my mode of analysis was not in fault, but the revived indigo blue, which I employed, had not been entirely purged of the red principle, by sufficient ebullitions with alcohol; for it adheres very tenaciously. Hence that resinous matter introduced a little oxygen and hydrogen, more than absolute indigo-blue contains. But the error will appear inconsiderable, if we compare the result with the analysis previously published by Dr. Thomson[1]. The following is a view of the ultimate constituents of indigo-blue, as given by different chemists:

	Thomson.	Ure.	Crum[2].	Roger and Dumas.
Carbon,	40.384	71.37	73.22	71.71
Oxygen,	46.154	14.25	12.60	12.18
Azote,	13.462	10.00	.11.26	13.45
Hydrogen,	0.000	4.38	2.92	2.66

That pure indigo contains hydrogen, I have recently placed beyond a doubt, by heating a mixture of it and calomel in a green glass tube, the open end of which terminated in an inverted tube, filled with nitrate of silver. Copious fumes of muriatic acid were evolved, and chloride of silver was precipitated in its characteristic curd.

The liquor of the dyer's vat (for calico printing) contains indigo deoxydised by protoxide of iron, and dissolved in lime water. This solution, in its average state

1 Dr. Ure, we conclude, means that, on comparing his and Dr. Thomson's analysis with the other two, his will be found *to differ less* than Dr. T.'s By calling this difference *error*, Dr. Ure must mean, that he considers Mr. Crum's and MM. Roger and Dumas' analysis to be correct. Our opinion, we are aware, will be thought worth little; yet, we will venture to say, that any one who runs his eye over the most recent analyses of organic substances, must be struck with difficulties and anomalies which can only be explained by supposing Analytical Chemistry to be not the perfect art which Dr. Ure would wish us to suppose. Even in the analyses of inorganic substances we see discrepancies, which are sufficient to unsettle our faith in chemistry as one of the exact sciences. When we find Sir Humphrey Davy, and many of the first chemists in Europe, so completely mistaking the nature of Wavellite, as determined by more recent analyses; when we observe Berzelius, after despoiling Thorina of its title to be considered one of the earths, (a title originally conferred by himself, and which it had quietly possessed for so many years,) coming forward with a new Thorina, which he insists is the real Simon Pure; we cannot help a certain degree of incredulity stealing on us in regard to the dogma of the infallibility of analytical chemistry. What, if after all, Dr. Ure, Mr. Crum, and MM. Roger and Dumas, were to be found in error, and Dr. Thomson right? or what if all four analyses were to be found erroneous? then what would the comparison with Dr. Thomson's result prove? That such a supposition does not involve any impossibility, Dr. Ure will not venture to assert. The attempt to regulate prices by the analyses of the chemist, when we see such discrepancies as appear in the list given at the end of the present paper, is visionary; at least till the subject is better understood than it appears to be at present.—E. Gl.

2 Dr. Ure omits to notice Crum's analysis of Cerulin and Phenicin, in which there appears to be 29 and 21 per cent. of oxygen; neither does he affirm whether the "pure indigo" was in the state of sublimed crystals or precipitated vat-fecula.—E.Gl.

of richness, has a specific gravity not appreciably higher than that of distilled water, and affords out of 1000 parts, by weight, not more than 3 parts of indigo-blue, and nearly the same quantity of carbonate of lime, equivalent to about a grain and a half of quicklime in 1000 of the liquid; which is the proportion in common lime water.

If that yellow liquor be introduced into a glass globe, with a graduated stem, previously filled with hydrogen, by plunging the vessel into the vat, we may transfer a portion of deoxydised indigo conveniently to the mercurial pneumatic trough, and measure the quantity of oxygen which a given bulk of it absorbs in becoming blue[3]. The quantity will be proportional to the strength and purity of the vat liquor. I have lately instituted a series of experiments, the results of which will, I hope, prove interesting, in reference to the problem for determining the quality or purity, and strength of different commercial indigoes ; but they are not yet mature enough to meet the public eye. The rigid mode of examining this drug is to eliminate the indigo-blue from the other substances, by the readiest artifices of analysis, and to weigh it apart. It may be objected to the analysis of indigo, that it is too complex and operose a process to be practicable with the dispatch and to the extent which the public quarterly sales of indigo require. But I conceive this to be a mistake. When only one object is pursued, various arrangements may be readily contrived for attaining it. Under this conviction I ventured to state, ten years ago, in the introduction to the first edition of my *Dictionary of Chemistry*, that "the result of numerous researches made with that view, has shewn me the possibility of rendering analysis, in general, a much easier, quicker, and more certain operation, than it seems hitherto to have been in ordinary hands." My experience since has fully justified that statement.

Accordingly, about three years ago, I suggested to the Honourable Court of Directors of the East India Company, the propriety of establishing an assay office for indigo in Calcutta, to guide them in their purchases of that article, and to enlighten the manufacturers in Bengal about the value of their various products and processes ; I again submitted to their consideration, last autumn, a memorial to the same effect, in which I detailed the advantages likely to accrue from such establishment to indigo-planters, dealers or brokers, and consumers. But the Court did not think it expedient at present to make any alteration in their indigo department. How much an office of this kind is wanted in London, in connexion with the quarterly sales of indigo, the following series of analyses, lately executed by myself, will sufficiently demonstrate. The quantity of indigo required for the assay need never exceed ten grains, provided a very delicate assay balance be employed; and by a suitable system of arrangements, the average quality of 500 chests may be accurately determined in the course of a day, by a diligent chemist, with four or six ordinary assistants to follow his directions.

1. *East-Indian Indigoes ; prices as at the last October Sales:*

Price. s.	d.	Description.	Real Indigo in 100 parts.
1	9	Lundy, very low quality,	29
2	9	Low, pale, Oude,	not determined.
2	3	Middling, ordinary Madras,	37.5
2	4	Middling, ordinary Oude,	27.75
3	3	Good, Oude,	54
3	3	Broken, middling, r. viol. dull viol. and lean,	46
3	6	Ditto, a little being coppery viol. and cop.	56.5
3.		Good Madras,	60
3	9	Broken, middling viol. and cop. viol. spotted,	42
4	2	Much broken and very small, very crumbly and limy, soft, good violet,	75
4	3	Large broken, and square, even, middling r. viol.	54.5
4	3	Very fine Madras,	58
4	9	Square and large broken, ½ middling viol. and ½ gd. cop. viol.	60
5	3	Large broken, very good ; paste, a little limy, gd. viol.	70
6	0	Square and large broken, gd. rd. viol.	66.6
6	6	Ditto, soft, fine paste, fine viol.	60
7	0	Ditto, fine purple and blue,	75

[3] In a paper on Indigo, published in our first number, and republished in the preceding number of the Quarterly Journal, this method of measuring the oxygen absorbed was employed, with some necessary precautions however; but the result was at variance with Dr. Ure's analysis.—E. Gl.

2. *American Indigoes—wholesale prices at present.*

s.	d.		Real Indigo in 100 parts.	s.	d.		Real Indigo in 100 parts.
3	2	Guatimala,	.. 19.	5	0	Guatimala,	.. 50
4	6	Ditto,	.. 32.5	5	3	Ditto,	.. 35
4	8	Ditto,	.. 46.	5	4	Ditto,	.. 50
4	8	Ditto,	.. 33.5	5	4	Ditto,	.. 50
5	0	Ditto,	.. 33.5	6	0	Caraca Flor.	.. 54.5

March, 1830.

III.—*Notice of Dr.* Richardson's *recent journey into Northern Laos.*

Between the Burmese territories and northern part of Siam on one side, and those of Tonquin on the other, there is put down, in most of our maps, a district or country called Laos, or Lén sén shán. Of this country scarcely any information is to be obtained in our best and latest geographical works, and the little they do contain is evidently not of the most authentic nature. The following particulars, therefore, of a visit to that country by Dr. Richardson, of the Madras Establishment, attached to the Residency at Múlmén, will, we doubt not, be read with interest.

To the gleanings made from Dr. Richardson's journal we are happy to be allowed to prefix the following letter, by a gentleman well acquainted with those countries, as containing some of the most interesting particulars established by the journey, and related in a more entertaining style than we could hope to give to the mere abstract of a journal. Some few particulars will be found perhaps repeated; though, in general, we have endeavoured to confine our abstract to those points not mentioned in the letter.

"You are already aware of Mr. Maingy having sent Dr. D. Richardson, of the Madras Establishment, on a mission to Northern Laos. As that gentleman has just returned here after an absence of three months, and as I have had a good deal of conversation with him, I think I can give you some account of his mission, which, in the absence of the more detailed account he is preparing for Mr. Maingy, may serve to interest and amuse you for half an hour.

About four months ago a Laos chief sent a party of men to Múlmén with a letter to Mr. Maingy, inviting him to send a British Officer up to the Laos country; and Mr. Maingy availed himself of this opportunity of acquiring some information regarding that territory. He could not have selected a better person for his envoy than Dr. Richardson, who to an intimate acquaintance with the Burmese language and customs, joins an excellent temper, and remarkably conciliatory and prepossessing habits and manners. Dr. Richardson went up the Salaín river for four days, and then travelled in about an E. N. E. direction. He was altogether 44 days on his journey: but of these he was in motion 27 days only. The Laos men whom he accompanied, told him frankly that they could not think of taking him by the easy and direct route to their country, as he might hereafter guide an English army to them; and that, for this reason, they thought it "right to move like an elephant over a difficult road, to feel with the trunk first, and ascertain that it will be safe to move the body forward."

Upon arriving at the residence of the Laos chief, Dr. Richardson immediately discovered that the invitation sent to Mr. Maingy was intended only as an empty compliment, the chief believing firmly that no English officer could or would be able to make a journey to him. The arrival of the *calaphyú*, or white stranger, excited a strong sensation throughout the country; an old prediction being current here, as well as in most of the other Indo-Chinese nations, that they will one day be conquered by white men. Dr. Richardson's arrival excited still more dread, from the circumstance of the Laos country having, during the past year, been subject to great inundation, and when the waters subsided, white fish, a white crow, and several other white animals, had been taken.

I must now tell you the name of the place to which Dr. Richardson went. It is called by the Burmese Labún or La-búng, and is situate about half a day's journey from the capital of Northern Laos, called by the Siamese and Laos men Ch'hiung-mé, by the Burmese Zemi, by the old Portugueze and English travellers, Jangamá, Jamáhé, J,hámé, Chiámé, Zangomé, &c. You may see an account of this place in the 7th vol. of the Modern Universal History, in which it is called Jangomá.

Old Louberé, in his account of Siam, says, that he understood Chiámé to be 15 days above the frontiers of Siam; and in his map he places it far too high—in north latitude 25°. The authors of the Universal History place it in about 20°. 30′; Dalrymple in his map in Symes's Ava, and Mr. Crawfurd in the map accompanying his account of Siam, place it in about Lat. 20°. 15′. In my map of Siam I had placed it still lower—in Lat. 20°. But Dr. Richardson, from some altitudes of the sun, which he was able to observe at Labúng, but upon which he is not disposed to rely implicitly, would place Zemi so low as 19°, or even less. Mendez Pinto gave some account of Zemi in 1545; but the best description of it is given by an English traveller, Fitch, who went there in 1587. He says he was twenty-five days travelling to it from the city of Pegu, shaping his course north-east, and that he passed through many pleasant and fruitful countries, which were very flat and full of rivers. Dr. Richardson found the road difficult and mountainous, and saw few traces of habitations. This country, like every other in this quarter, although by early European travellers described to possess a very dense population, can now only show desolate wastes, with insignificant towns, and a scanty impoverished race of inhabitants. Dr. R. saw only some small villages besides the town of Labúng, the population of which even he does not think exceeds 2500 souls. I take this Labúng to be the same place which the Siamese name Lamphún, but Crawfurd in his map has it down by the name of Labún, and places it correctly with reference to his Chang-mé.

The chief of Labúng has the same title given him by his people as that applied to the king of Siam,—*Cháo-Chivit; i. e.* Lord of life or soul. He described himself as being the chief of Northern Laos, governing not only Labúng, but Zemi, and a large town called by the Burmese Lagwón, and by the Siamese Lac,hwón; the same as Mr. Crawfurd's Dagón; the D in which I take to be a mistake for L.—The *c,h* of the Siamese is pronounced like *g* by the Burmese, according to a well-known rule in their language. The chief and people of Labúng took great pains to assure Dr. R. that they are not tributary to Siam, and that they only occasionally send some teak timber down to Bangcóc, once in six years. But putting aside what I ascertained at Bangcóc, I am satisfied from all Dr. R. states, that this part of Northern Laos is subject to Siam, and that the latter exercises as much controul over the former, as the power or caprice of the one and the weakness of the other may suggest or permit. The moment Dr. R. arrived at Labúng, an express was sent down to Bangcóc, to which also Mr. Maingy's presents were carefully packed up and forwarded; and the utmost anxiety was shown for a reply from Siam. The chiefs of Northern Laos assisted with a force the Siamese invasion of Southern Laos, and destruction of its capital, Lánchang, or rather Vínch,hang, about two years ago; and the Siamese monarch detains at Bangcóc as hostages, several of the sons and relations of the chief of Labúng. Zemi, you may recollect, is said by Dr. Buchanan to have been subject to Ava about the time of Col. Symes's mission in 1795. But not shortly after, the Siamese appear to have recovered it; and as the language of the people is very nearly the same as that of Siam, their subjection to, or dependence on, the latter state, is the more natural condition.

Dr. R. was not allowed to visit Zemi, and he was privately informed, that the Labúng chief was afraid of his learning at Zemi, that some of the captive Burmese, who had been taken during the war from the country near Martabán, and who had been ordered by the king of Siam to be released upon my requisition at Bangcóc, are still detained at Labúng.

Dr. R. describes the country of Labúng to abound in elephants and cattle. He saw no wheat, and observed that the principal grain used by the people is the description of glutinous rice called by the Malays *Palút*, and by the Burmese *caúc-nhi-igen.* He saw no snow or frost, but the thermometer at 8 A. M. was so low as 46°. He could not see the snowy range of mountains to the north, nor does he appear to have observed any very lofty mountains. The language of the people is, as I have before said, the same as that of Siam, with some slight difference of dialect. The appearance of the men did not strike him as being of so robust and large a make as what usually distinguishes the northern race. But the women are eminently handsome and fair, with fine large gazelle eyes, showing no resemblance, which is surprising, to the Tartar or Chinese eye. Dr. R. has reason to believe that a great part of the population consists of a mixed race between the Siamese and Burmese, or Laos and Burmese. He learnt that the cruel and horrible system of border warfare and man-catching, to which our occupation of the Tennasserim provinces has put an end to the southward, still continues in force to the north, between Laos and Ava. The people of Laos are in great dread of the Burmese.

The men of Laos wear larger folds of cloth as turbands on their heads than the Burmese, with their book-muslin *Gúng-búngs.* Their lower garments are the same as the Burmese *pá-tshós,* of silk or blue striped cotton. The old women only cover their bosoms—the young ones keep them bare, but their lower garments are more decent than those worn by the Burmese women, and not open in front like the latter's. The clothes also are worn like petticoats, and not brought between the legs, and tucked up behind like a Hindú's *dhóti,* as all the Siamese women wear them. Dr. R. saw a good deal of English broad cloth, and some specimens of English chintz, of the small sprig pattern, admired at Bangcóc, from whence, of course, they came. The priests are not held in such reverence as in Ava and Siam, and their conduct and manners are not so strict. Dr. R. has seen them often riding about on elephants, which they guide themselves; and the young priesthood he observed flirting with the ladies, and going about at night as gallants.

The coins current are the same as the Siamese *Bat, T,houang,* and *Selung.* Dr. R. saw a good deal of cotton, ivory, and stick lac, and some musk, which he understood are exchanged for articles the produce of China, from whence a caravan, consisting of one or two thousand horses and mules, annually visits this part of Laos. The Chinese caravan had been plundered about three years ago, and it had not visited Labúng for two years; but it was expected this year, and Dr. R. had hoped to have seen the Chinese merchants before he left the country, and to persuade some of them to come on to Múlmén with him. He did not, however, wait to see them. He was told that the Chinese frontier merchants had sent a deputation to the king of Siam, with a present of gold, to solicit his Majesty's protection in future to their annual caravan. Although all the copper brought to Siam is called Laos copper, (*T,háng déng Láo,*) and Fitch considers this metal as one of the native commodities of Zemi, Dr. R. believes that there are no copper mines there; and he was assured that all the metal was brought by the Chinese caravan. There is a great deal of iron ore in the country, and the inhabitants can forge tolerably good musket barrels. He saw a small specimen of lead ore, and he was assured that there is abundance of tin ore even above Zemi, which is much higher than what Crawfurd, I believe, fixes as the extent of the tin formation.

Cattle are very cheap in Labúng,—about 2½ rupees per head. A woman usually sells for about ten head of cattle. Dr. R. has succeeded in bringing down with him to Múlmén sixty head, and about three hundred more were to follow him. This will ensure an immediate benefit from his mission, for a supply of cattle for the use of our European troops at Múlmén was a great *desideratum.* The Laos cattle is small.

Our broad cloths and chintzes and cutlery are much prized in Laos, and I hope before long an useful and extensive commerce may be established between that country and Múlmén. It would be a great point to try and invite the Chinese caravan to come on to Múlmén.

I enclose a sketch of Laos, copied from one made for Dr. R. at Labúng. This letter is already so long that I cannot trouble you more with my ideas of this map. I have let the Siamese names stand as Dr. R., who does not understand that language, has written them. But I have put down the exact Siamese name below his, wherever I could follow him in my Siamese maps. The northern portion of this is remarkable as bringing the great Cambodia river so far to the westward; and it would be curious to try and trace upon it the route of the Chinese merchants who travelled overland from Siam to China, in 1652, and a copy of whose journal is to be seen in Du Halde. Dr. R.'s Wincean is the Siamese Vínch'hang, the same place as Lan-chang, Dalrymple's Sansepura, and the Dutch Winkjan, to which a Dutch embassy went in 1641, an account of which may be seen in Valentyn's great work. This Vínchhang is the city which the Siamese destroyed two years ago. Dr. R. was told that it is due east from Zemi, and distant a month's journey. I cannot believe that it lies in so high a latitude. The authors of the Modern Universal History, in which there is the best account of this Southern Laos, place the capital in Latitude 18°. Crawfurd places it in 15° 40′, and Dalrymple in his map in Symes's Ava, places it a little below 16°.

I hope we shall soon know more regarding these countries. The chief of Matak, or Tak, on the Menam river, has lately sent a friendly deputation to Mr. Maingy. Much confusion arises from the different names given to the same place. The Siamese call it Menang country, or Ban village of Tak. The Burmese corrupt Ban-tak to Bantat. A place immediately under it, the Siamese call Menang

Raheng, and the Burmese Yahaing. You may recollect that it was the governor of Tak, called in Siamese *Phaya Tak,* who came down and drove the Burmese out of the old capital of Siam, after they had had possession of it for some months. This chief was the father of my friend the Rája of Ligór, and is said to have been of Chinese extraction.

Dr. R.'s map improves our knowledge of the geography of Upper Siam, by showing that the Menam river, to the north, consists of several large branches, filling up the space which in all former maps was left by confining the course of the river to one large stream, along which the different towns were placed.

Dr. R. saw no cannon at Labúng, but I think he says that this town, as well as Zemi, as he heard, is surrounded by a brick wall. The chief and people of Labúng expressed great apprehensions of our power and intentions. They were particularly struck with the circumstance of our not being afraid of going in open broad daylight to attack Martaban the other day, although they said that it would have been better to have gone at night, and been able to burn all the inhabitants in bed! When Dr. R. said that we had no desire to interfere with other people if they would leave us alone, and that we are a *straight-forward* race, they answered: "That is the very reason we are so afraid of you. If you advanced *shilly-shally,* in a serpentine line, like a Burmah, we might hope to avoid you; but there is no resisting you when you come but on like the horn of a large animal."

The above are the contents of the letter; and to this we are enabled to add, through the kindness of a gentleman to whom we have been often indebted for similar favours, the following particulars, extracted from Dr. Richardson's journal. The paper, which is a very interesting one, will be published, we understand, at full length in the forthcoming volume of Transactions of the Asiatic Society.

In consequence of several friendly communications between Mr. Maingy, the British resident at Múlmén, and the Zemi chiefs, in which they more than once requested the visit of a European officer, Dr. Richardson of the Madras establishment, was directed to accompany on his return the bearer of one of these letters, and entrusted with an answer and some small presents for Chaú-ché-wít, the chief of Labúng.

Dr. Richardson quitted Múlmén on the 11th Dec. 1829. The first hundred miles of route lay up the Thalaín river, which at starting is confined by limestone rocks. The banks are described as mostly covered with jungle. Occasionally a cleared tract appeared, and the jungle was sometimes varied by rocks. On the 15th they reached the entrance of the hills, which Dr. R. considers to be volcanic, although on the immediate bank of the river he observed some beds of clay slate. The Thalaín is represented as but 300 feet wide at this place. Here the boats were quitted, and the baggage adjusted for coolee loads.

The route now lay through a low mountainous country, the path being generally either the stony bed of a stream, or over steep hills, and so bad, that riding was impossible. Yet the Careens declare that the pass is practicable for bullocks, and that it is the same by which the Burman army entered in 1790. The country appears to be chiefly jungle, though occasional rice grounds were seen, the grain of a particularly fine quality, small, but transparent, and when boiled of the purest white. The teak and the varnish tree, or *tsé-tsé,* were also seen, as also the tree which furnishes the Catechu[1]. Wild animals abound—elephants, tigers, bears, deer, &c.; the rhinoceros being only found in the plains. On the 25th they reached the banks of the Méguén river, from 150 to 160 feet in width. This river joins the Ménlangi, about one day's journey above the confluence of the Tháng-én with it. Here they were met by a party dispatched by the Zemi chief to receive them, to whom a letter was forwarded, reporting the arrival of the English agent. They brought the very acceptable reinforcement of five elephants, the people having suffered much from the nature of the roads travelled. With the elephants, and the assistance of a raft, every thing was crossed on the 26th; and on the 27th Dr. Richardson's party proceeded, accompanied by the deputation. The distance to this river, from the point where they left their boats, was 66 miles.

They continued to follow the course of the river for 40 miles, frequently crossing it, the country being entirely deserted, and covered with jungle. On the 31st they met with two villages, each surrounded by a patch of cultivation. A considerable tract appeared to have been under cultivation the preceding year. The rice of this part of the country was ascertained to be of the glutinous kind, called by the Burmese *Cáchníru.* The Careens who had hitherto accompanied the party

[1] Acacia Catechu?—Ed.

having been relieved regularly at each village from the Thalaín, here left them. They are described by Dr. R. as a fair, well limbed, athletic race, superior in appearance to either Talíns or Burmese, but have been oppressed from time immemorial by Talíns, Burmese, and Shàns.

Cultivation and villages were now of more frequent occurrence, and the path also appeared to improve. On the 1st January they passed the village of Ménlangi, situated on the river of the same name, which was crossed repeatedly. Here the Sháns persuaded Dr. Richardson to halt till an answer should be received to their letter, which had been dispatched when they first met him to notify his approach. He was told that Labúng was still ten day's journey distant.

The country is tolerably open here, this forming the largest valley they had met with since leaving their boats. The width is from 3½ to 4 miles, and the length may be judged of from the circumstance of the hills to the north being barely visible from their encampment, which was at the southern end. The river, which has a course nearly parallel to the Thalaín, rises 15 to 20 days' journey to the northward. After receiving the Máignaú, it falls into the Thalaín about 6 days from Ménlangi The village is small, and the houses not so comfortable as those of the Burmese generally are. There are only seven other villages in the valley, the whole eight bearing the same name, and forming one of the 57 CITIES of the Cháng Mé, or Labúng chief. The collective number of houses is about 200.

Cattle appear to be abundant in the vallies and open country, and about two to eight thousand are annually taken to the country of the Carín-ní, (red or independent Careens,) to be exchanged for slaves and for horses. Their value in their own country is about 1 Rupee Siamese[2] for a cow, and 2½ for the best bullocks. They are exchanged with the Caríns at the rate of seven bullocks for a horse or young-man, and eight or ten for a young-woman. A small return in tin is also received, which is partly the produce of the Carín country, and partly obtained from the Tánthu people. The Sháns also export salt and betle-nut. The former they obtain from Ban-cóc, and the latter from the lower parts of Thalaín.

After being delayed some days, on various frivolous pretexts, Mr. Richardson was again allowed to proceed on the 6th. He here entered a mountainous country, continuing to gain in elevation till the 9th, when he halted at the village of Bó, consisting of sixty or eighty houses, situated in an open plain, of twelve or fifteen miles in length, by five or six in breadth. The distance from his encampment in the valley was forty-six miles. The wild plaintain, *tsé-tsé*, bamboo, all in great luxuriance, the forest more open, and the above trees latterly yielding to the pine, which is described as affording some fine trees, the branches frequently commencing at a height of fifty feet from the ground. The village of Bó is inhabited by blacksmiths, who smelt the neighbouring iron ore, (a red oxyd,) containing 50 per cent. metal.

From this village the road gradually descends to the valley of Mé-ping, which they reached on the 11th, a distance of twenty-six miles from Bó. The whole of the country between the Thalaín and the Mé-ping, with some trifling exceptions, (such as the small valley of the Mén-langi,) is one succession of mountains. They are chiefly of the older formations, the rocks being principally granite, gneiss and limestone; trap rocks are also found. The party encamped at Máng-hát, on the Nan-pa-ping, which, running southward, falls into the sea at Ban-cóc. It is here about two-hundred yards across, and rather rapid; it is, however, fordable by elephants.

On the 12th Mr. R. halted, and proceeding on the 13th, found the road improving, which continued as far as the village of Bón-són-cané and Bén-súp-ta, situated on the Métá, a small stream that joins the Mé-cuáng, a quarter of a mile distant, the distance from Máng-hát being thirty-six miles. Here were found oranges, shaddocks, pineapples, mangoes, cocoa nuts, guavas, &c., all abundant. The orange and the cocoa-nut were the only two in season however. The appearance of the people too was much improved, many of the women and children being nearly as fair as Europeans, and the latter often with light hair. Their eyes, unlike those of the Chinese and other eastern nations, are large and expressive. Here Mr. R. was again detained till the 19th, when the day being pronounced lucky, he advanced to Labúng, six miles, having thus reached the termination of his journey in thirty-eight days; the whole distance travelled, including the water route, being three hundred and fifty three miles.

Labúng is situated in the valley of the Méping, on the right bank of the Mécuáng, which joins the former river about half a mile distant. The Mécuáng is about 30

[2] About 1¼ Rupee Madras.

feet wide, and only 3 feet deep at this season, though at others it is navigable by boats of considerable burthen. The valley of the Mé-ping extends to Ban-cóc, with hills on each side, and is lost at that place in the plain country. The width varies from ten or twelve miles, to sixty or eighty. The soil is a rich sandy loam, and apparently of considerable depth. The hills which bound it are high, though no snow was observable on any. No snow is said to be encountered even in the route to the frontiers of China, represented as about four-hundred miles distant, though from the occasional inundation to which the Mé-ping is subject, Mr. R. thinks its sources must be within the limit of snow. The thermometer in the valley seldom rose above 53° at 7 o'clock.

Zemi, which is of much greater extent than Labúng, is a double walled fort, situated on the Mé-ping, and about half a mile distant. Logán, which is larger also than Labúng, is represented as distant three days journey, being on a small range of hills, on the banks of the Mécuáng river, a feeder of the Mé-ping. The wall round Labúng is built of a red ferruginous sandstone, similar to that common in Ava, finished at the top with bricks. The chief's houses are mostly of wood, and covered with shingles. They are some of them surrounded by a kind of stockade or enclosure made of timber, and eight or ten feet high; within the walls they grow every kind of fruit tree, and in great numbers, so that it has, at a little distance, by no means the look of a city. The fort is in shape an irregular oval, the longer diameter being about 1600 feet, the shorter not more than a third of this. The wall is from fifteen to twenty-three feet high outside, aud from thirteen to eighteen inside. It is not of any strength. It has four gates on the eastern side or face, two on the southern, two on the western, and one on the northern. It has a wet ditch of sixty or seventy feet in width on three sides. On the eastern face the river flows, but at this season is not more than kneedeep. On the eastern bank of the river, opposite the fort, is an old stockade of equal size, with bastions of brick work remaining. There are said to be 30 guns in Labúng, and 40 in Zemi and Logán each, although a Burmese prisoner fixed the total number of the three places as not more than ten. The number of inhabitants was equally exaggerated into 4000[1] for Lobang, and 40 000 to 50 000 for each of the other two towns. Dr. R. thinks the population of all three cannot be rated at higher than 30 000. This gives us a very different idea of these places to what is obtained from the old accounts of the Portuguese and others.

These people appear to be subject to Siam, though they evidently wish to be considered merely as allies. About 45 years ago they were under the Burmese sway; but the rule of the latter was so oppressive, that people could no longer bear with them. The present chief, together with his nephews, having solicited aid from the king of Siam, to whom they had previously married their sister, excited the people to rise on their oppressors, and with the aid furnished from Ban-cóc, they succeeded in driving them out of the country. Two attempts were subsequently made by the Burmese to re-establish themselves, but they proved unsuccessful; and the uncle of the seven brothers, whose title is Chú-ché-wit, (lord of life,) still maintains his independence, with perhaps a nominal subjection to Siam. Two of the brothers govern Zimmé and Logán, which latter place is of recent establishment. Chú-ché-wit is about 68 years of age, mild in his manners, and much respected by his people. Dr. R. appears to have been impressed in his favor, by his general behaviour towards himself and his reception of the mission.

These people appear, like the Siamese, to have been more successful cultivators of music than most other eastern nations. Their music is described by Dr. Richardson as sweet and pleasing, but he does not give any account of the instruments they use. The wives of the chiefs dance before strangers for their amusement. Polygamy is of course extensively practised by those who can afford it. One of the chiefs is described as having 25 wives, every one of which, but one, had been, as he boasted, kidnapped. It is, in fact, by robbery and man-stealing that they have risen to the little importance they are thought to possess; and in these two points they are said to be the most unprincipled set of scoundrels on the face of the earth. They are otherwise a people with much in their character to interest a European; but we must hasten to conclude this unwarrantably long abstract of a paper, which will be published in all its details in another place.

[1] Quære 40000?—Ed.

IV.—*On the Coal field of Pálamú.*

Our readers will remember a notice given in our first volume, p. 178, of a coal mine which had been discovered in the Palamú district; and which had been visited and reported on by Captain Franklin, by desire of Government. The contents of that notice were chiefly taken from a private letter of Captain Franklin's, to a friend in Calcutta, written on first visiting the spot. It was, therefore, so far imperfect; and it gave an inadequate idea of the extent of Captain Franklin's enquiries, as it failed in showing their results with that precision which in so important a subject was desirable. Having lately been put in possession of the whole of that officer's correspondence and proceedings, together with some still more interesting notices on the subject of this coal furnished by Captain Sage, Executive Officer at Dánapúr, we propose resuming the subject, and attempting to give a brief history of the discovery of this coal, as far as our materials will allow; trusting, that should we inadvertently fall into any mistake, those of our readers who are better informed will correct them.

When the project of establishing steam boats on the Ganges was first broached, a very serious difficulty seemed to present itself in the necessity of forming depôts of coal at suitable distances along the line of river. It became, therefore, a question of great interest—was there any probability of discovering supplies of coal so situated as in any degree to diminish this difficulty, and remove the objections founded thereon. The question engaged a good deal of attention in many parts of the country; and amongst others, Mr. A. Prinsep, Register of the Zilla Court of Rámgerh, who was then moving about the district, issued a proclamation, offering a reward to any one who might be successful in discovering the so much sought for mineral. A copy which he had of Rennel's Atlas, afforded some grounds for the expectation on which this step was founded; the words "cole mine" having been found in the map of the Rámgerh district, at a spot not far from Pálamú, and close to the banks of the Amanát,h river. Being encamped at Pálamú, he deputed an intelligent native to search for this "cole mine." The search was successful; specimens of the coal were brought in, and the report of the native forwarded to Calcutta, where it excited much interest.

About the same time some of the members of the Asiatic Society, considering as very desirable the completion of the geological labours of Captain Franklin in Bundélc,hand, (which had been suspended in consequence of the economical measures then in progress all over the country,) had petitioned Government on the part of that officer, that he might be permitted to finish that survey on his own account and that of the Society, he relinquishing any claim to remuneration. Although there did not appear any pressing or immediate occasion for Captain Franklin's services with his regiment, there was still felt to be some difficulty on the part of Government in acceding to this application,—till the newly discovered coal being suggested as a subject of great and public interest, it was deemed a less questionable ground on which to sanction the desired arrangement, than any which could be found in the barren and speculative, though perhaps ingenious views connected with the prosecution of what was supposed to be a mere geological survey. Accordingly Captain Franklin obtained leave of absence for six months, and was furnished with instructions from Government as to the special duty assigned to him of visiting, examining, and reporting on this highly important (as it was supposed) deposite of coal.

Captain Franklin having quitted the Presidency, lost no time in proceeding towards the scene of his labors. His first report is dated 24th April, 1829, and it states that he had, in obedience to the instructions received, visited the coal mines of the district of Pálamú, lately brought to notice by Mr. A. Prinsep. That there are three places in that district where coal had been found, two of which are about 12 miles south-west of Pálamú, in a jungly part of the country, near the spot marked "cole mine" in Arrowsmith's map; the third at the point where the Ammanát,h *naddi* falls into the Cáil river, at the village marked Sidra (properly Singrah) in that map. The position of the latter being most favorable for transport, Captain F. made it the principal subject of his examination. In a section which accompanies his report, he exhibits 5 parallel seams of coal, each of which bassets at the foot of the hills forming the scarps of the small troughs or vallies by which they are intersected; the inclination of the seams being about 1 foot in 30. The surface rock is described as a coarse crumbly gritstone, equivalent, as he thinks, with the *millstone grit* of England; and under each seam is found hard micaceous sandstone, which, occasionally, in the vicinity of the coal, gradually passes into a true shale. The greatest height which this coal attains is 15 feet above the water line of the Ammanát,h river.

Of these five seams, the whole of the coal above the water line appeared to Capt. F. perfectly valueless, possessing none of the properties which distinguish good coal. It is deficient in bitumen, and will not inflame, nor even burn at all without difficulty, and being urged by a blast. Finding all above water line thus bad, he endeavoured to get at one of the inferior seams where it dips below this line. It proved equally bad. The coal indeed was more compact, and had a little more lustre, but yet had none of the properties of good coal. He then endeavoured to sink a shaft, so as to get at a still deeper seam, but want of means and time prevented his completing the experiment; he found the rock lattlery (a micaceous sandstone) much darker, and approaching closely to the aspect of bituminous shale.

From Singrah Captain Franklin proceeded to examine the place (Ganniághát) where the navigation of the Cáil is said to be interrupted. He states that the whole bed of the river is covered by hard gneiss rock, not fragments, but protrusions from a bed *in situ*. A passage has been worn in it by the river, of about 30 yards in width, through which rafts of wood and of bamboos pass every year in September, October, and November. In the height of the rains they do not venture on account of the force of the current; nor can they pass after December for want of water. No boats had ever passed upwards from the Són, with the exception of two brought for the use of ferries; nevertheless it did not appear to Captain Franklin at all impracticable for light boats to pass in the months of September, October, and November; and even if any difficulty should occur, it might be removed by blasting.

Captain F. states in conclusion, that the coal of Singrah resembles closely that which he had met with in the Mahadeo hills; it further agreed with specimens obtained in sinking a well at Jabbalpúr; to which if it be added that shale had been found in Sóhájpúr, he thinks himself warranted in inferring that there is a deposit of coal along the flank of these hills, and intimates his intention to seek for profitable beds in a westerly direction.

On the 5th May Captain Franklin reported his discovery of coal, at a place called Mánpúr, situated 16 miles south of Chér,gerh in the district of Sirgúja. This coal was of a superior quality, being much more bituminous than the Singrah coal, but being situated in a mountainous and jungly country, and the navigability of the adjacent rivers, the Kánhar and the Raír, being doubtful[1], the prospect of the discovery's proving useful was slender. But the occurrence of the mineral here having so far justified his conjecture of the existence of coal along the declivity of the Vindía range, he proposed to search for it in other quarters, along that line possessing greater facilities of transport.

On the 12th July, Captain Franklin reported that a third locality of this coal had been discovered by him at the confluence of the Tipán *naddi* with the Són river, about 30 miles from the source of the latter, and about the same distance south-east of Sóhájpúr. In point of quality this coal is described as superior to that of Mánpúr, and still more to that of Singrah. It contains a considerable proportion of bitumen, is easily ignited, and burns with a bright flame, and bituminous odour; but it neither cakes nor swells like good coal: the fragments retain their original shape after the bitumen is consumed. These particulars mark it as synonimous, he thinks, with the coal of the millstone grit of England, which is always of an indifferent quality. But though inferior to the best coal, Captain F. thinks this coal might be found useful, provided it could be got at; but unfortunately the bed of the Són is obstructed by rocks at two places below this point, and it appears that two attempts which were made to get boats down, had proved both of them unsuccessful. This is, however, a point which ought to be ascertained by a competent examination.

Captain Franklin further states, that when examining the newer sandstone rocks which border the plains of Bundélc,hand, he found traces of coal at Shápúr, in Bejaúr, and also in the glens of Ajígerh and Cálinjer. He has no doubt that coal is to be found there, but perhaps only by sinking to a considerable depth through the rock. The distance from water carriage is, however, a still stronger objection; but should this deposite continue to the eastward as far as the Tóns river, the proximity of water carriage would give it a value superior to that of the best coal

[1] The beds of these rivers being elevated about 1000 feet above the sea, while their confluence with the Són could not well be more than 500, led to the conclusion that there must be considerable falls.

less favorably situated. He offers his services to determine, in the following season, either this point or any other connected with their coal-beds that might appear to Government desirable—waving any claim to remuneration. These enquiries, we need hardly remind our readers, were, and are, full of interest; and in the name of science we may be permitted to regret, that the views of Government did not allow of their sanctioning Captain F.'s proposal. As a mere object of a liberal and enlightened curiosity, the enquiry was well worthy of being prosecuted. As a question of public utility, the result fully proves the expediency of such a course.

Soon after sending in the above report, Captain Franklin finding his health had suffered much, obtained leave to proceed to England.

The preceding particulars, we think, upon the whole, favorable to the question of the discovery of coal of a useful quality, if not the best, somewhere in the tract limited and defined by Captain Franklin; and we doubt not, had Government authorised a proper search to be made, that it would have been successful. Subsequent enquiry has thrown some more light on the question, and shown, that, upon the whole, Captain Franklin's opinion was by no means sanguine, but rather the contrary. It appears, in fact, that the coal at Singrah, though, as described by him, non-bituminous, is not utterly worthless. The difficulties of the navigation of the Cáíl too appear not to be so great as was supposed. On this subject it gives us pleasure to be able to contribute the following additional particulars, furnished by Captain Sage, Executive Officer at Dánapúr.

Captain Sage, desirous of more fully exploring the resources of the district of Pálamú, proceeded on the 13th January, 1830, accompanied by a small party, consisting of his barrack serjeant, a corporal, and three men of H. M. 13th Light Infantry, who had been colliers in England, to Singrah. The surface of the country is very undulating, comparable to nothing better than what we may imagine would be the appearance of the suddenly frozen billows of a stormy sea. It will be recollected by our readers, that Mr. Jones, in his account of the Burdwan coal field, published in our first volume, p. 261, describes the arrangement of the surface at that place in nearly the same terms. The hillocks, or billows, as they may be called, are from forty to sixty feet high, of every extent and form. Their bases are marked by coal bassets in every direction. Their tops are covered with jungle, though the small interlying vallies are cultivated. The Amanát,h river joins the Cáíl, about one hundred yards west of Singrah. In its course it has cut off a portion of a hillock, 1200 feet long, 100 feet broad, and 40 feet high. The following are the particulars of a section:

No. 1.

Section of the Strata at Singrah.

ft.	in.	
11	6	Earth, clay, pebbles, &c.
	11	Shale.
3	7	Pebbles.
1	6	Coal, No. 2.
8	0	Sandstone conglomerate, increasing in compactness as it descends.
4	7	Non-bituminous coal No. 3, or Anthracite.
30	1	Total.

Below the coal is a blue sandstone, readily splitting into tables or large flags. The depth has not been ascertained.

No. 3 was found a good non-bituminous coal, burning without flame, and leaving for residue a white ash. It was tried on rather a large scale, both in the smitheries of the barrack yard and in brick burning. In both it has answered well: in the former superseding the use of charcoal. This bed of coal extends up the Amanát,h to the confluence of the Gángárig, and it was traced up the bed of the latter nearly three miles, to the confluence of two lesser streams which join it from opposite sides.

On the 15th and 16th, Captain Sage proceeded passing through Sháhpúr, Chaínpúr, Chandás, and Chapári. About 5 miles beyond the last named place, he entered a small valley watered by the Gorásan *naddi.* Here he found coal-beds extending more than five miles up the stream. The following are the particulars as observed in the bank opposite where his tent was pitched. This coal is entirely different from that of Singrah, being in fact very bituminous, and burning with a clear bright flame. It makes a most cheerful fire in an open grate.

No. 2.

Section of the Coal Strata in the Gorásan river.

ft.	in.	
8 .	0	Earth and sand, and gravel.
6 .	4	Sandstone.
1 .	0	Shale (Bituminous ?)
2 .	1	Ditto.
1 .	1	Coal.
1 .	0	Shale.
1 .	4	Sandstone.
3 .	9	Coal.
24 .	7	Total.

This section terminates with the water line of the *nallah;* how much deeper the coal may be, is not mentioned.

On the 17th, Captain Sage went in search of Rennel's *Cole Mine*, the site of which he found to be close to the village of Hotár ; although the inhabitants being questioned, appeared ignorant of, or at least denied, the existence of any thing like coal. After looking in vain for bassets of coal amongst the hillocks, he turned up the Barra river, and at a distance of about half a mile, found here also extensive beds of coal on the left bank, the river having laid part of them open. This coal is the same as that of the Gorásan, and dipping in the same direction. Iron-stone is plentiful in the neighbourhood ; at Alyapúr, three miles south, it is worked, but the process of reduction being imperfect and unskilful, the iron is said to be inferior.

The coal being ascertained to be of value, and also in sufficient quantity, the next question was—that of carriage. At what expense could it be conveyed into the Ganges ? To answer this question, it should be recollected, that the three localities are all beds of streams, which at a few miles distance are lost in the Cáil, the latter being a tributary of the Són river. The question then resolves itself into that of the navigability of the Cáil. Captain Sage thinks it is, or at least may be, made at a trifling expense navigable nearly the whole way, *i. e.* during the rainy season. The use of ferry-boats he considers sufficient evidence that the rapidity of the stream is not too great. He traced the river the whole way to its confluence with the Són, opposite Rotásgerh ; and he finds nothing to prevent the navigation of this river in the rains. From Gania to below Manjián several rocks protrude in its bed here and there, but not sufficient to form any serious obstruction to the navigation :—were it otherwise, it would not be a work of either difficulty or expense to remove these rocks by blasting.

Of the Singrah coal, which may be obtained at a trifling expense, by running horizontal galleries into the face of the hillocks, at the foot of which the bassets appear, Captain Sage thinks that there is a quantity available, equal at least to the probable demands of the next twenty years ; and his calculation seems founded on unexceptionable data. Of the other two localities, the Gorásan and Barra, the coal seems to be one and the same bed, and it must consequently be extensive. It will, however, be more expensive working this coal. The Singrah coal, transported on the bullocks, cost but 12 anas a maund, and it is expected with water carriage that it will not cost more than 6 anas. Even with land-carriage to Bandar-Ghát, on the Són, and transported thence by boats, in the rains, to Dánapúr, it could not well exceed 7 to 9 anas.

V.—*On Land Revenue.*

To the Editor of Gleanings in Science.

Sir,

To give a detailed account of the regulations affecting the land revenue, would far exceed the limits of a publication such as yours ; I shall, therefore, endeavour to be as brief as possible concerning them.

Regulation VII. of 1822, appears to have been framed principally with a view to protect the cultivators, and at the same time to secure to the Government a proper proportion of the profits of the soil, according to its qualities and products. It has met with some opposition from members of the Civil Service and others, by whom it is said to upset all the former good old methods of assessment, taking away the authority and respectability of the *biswadárs*, and raising animosities among the people; it may not be improper, therefore, to enquire what this good old method was, which this regulation is blamed for removing.

It appears that the proprietors of a village were sent for after a measurement, and enquiry into their lands and property; and having appeared before the collector, he declares they shall pay so much. The proprietors, well taught by long experience, state their inability to pay more than one-fourth that sum; they are then remanded for a few days, when they are again brought before the *hazúr*, who probably has come down a few hundreds in his demand; and the proprietors, to shew that they are also not unreasonable, will advance a hundred on their first offer, which is rejected, and the collector, who can find no one to take the village on his terms, leaves the settlement of the matter to the *tehsíldár;* and those who are at all acquainted with the subject, must know the abuses this authority was put to, in the shape of *jútí-mar*ring; making them stand on sharp wooden pins; suspending them by the hands, their toes just touching the ground; putting cow's bones into their mouths; tying them up to a triangle, and flogging them; making them ride on asses; and baking, by exposing them to a hot sun:—it may be said these abuses are eradicated, that they never now occur; but it is owing to this very regulation: but while I state this fact, I am also prepared to assert, that bribery and corruption among the native *ámla*[1] is gaining ground, which can only be put a stop to in the revenue departments, by the regulation being more generally acted up to; and till the whole country is put under the operation of it, the temptation and opportunities are so great, that it may be impossible to avoid it.

But the effects of the good old system have not been fully exposed. Let us suppose the proprietor has effected a bargain with the collector, through the good offices of his friends, or by good luck; he proceeds to the village, and makes known the terms of the assessment. The lands, which were measured, most likely under a bribe, are now distributed among the cultivators, the *biswadárs* taking care to use a small *bígha;* the cultivator is taxed in various modes to the very utmost he can pay, for these poor people, rather than leave their homes, will submit to great hardships; and he is plundered by the grossest acts of fraud and imposition, which occasionally are brought before the courts, where the judges (who had not formerly been in the revenue line,) are often ill versed in the laws and usages which pervade the village community. Even if the *raíat* succeeds in gain-redress, the proprietor has it in his power amply to punish him: the grazing of his cattle is stopped, and the people seeing him unprotected, his fields are invaded at night by stray cattle, and worried to death, till he is driven from the village. The profits of the village are probably considerable—it is all the same to the *cisán;* and when a hard season comes, he suffers: his cattle either perish of hunger, or are sold to pay his share of the assessment; he borrows cash to purchase others, as well as seed for a crop; and, if fortunate, he may retrieve himself, but his means must always be precarious.

This is an attempt at a short description of an assessment on the old plan, which was handed down to us with the possession of the country; and notwithstanding the boasts of the Aín Acberi and the natives, of the flourishing state of the country, this is the very system which was then the means of raising and adjusting the revenue of the Great and Good Acber.

This practice of cheapening and prigging for a good *jemma*, appears to have been followed by the successive Governments in their turn; and a ridiculous story is told of the Marhattas who, during their short rule, also had their settlements and *bandobasts*. In order to secure a good round sum, for what they thought a fine village, they set out with the demand of a *lác,h*, and, to their astonishment, the proprietors agreed to it immediately, rather than go through with the inconvenient ceremonies I have detailed; but the *zamíndars*' prognostications were true: the Marhatta power did not subsist long enough to give them an opportunity of making the collection: the circumstance, however, has gained the name of *Lác,hú* to the village, to this day.

It remains now to be seen what the regulation purposes: it brings the lord or superior of the soil into contact with the cultivator, registering the property he possesses as a tenant or otherwise, defining and establishing the nature of his rights, and meets him on fair terms: his quota of lands are measured, valued and assessed, at a certain rate, by the collector; and if he agrees to it, he receives his *patti*, and pays neither more or less: the profits and loss are his own, and

[1] The preamble of Regulation I. of 1821, gives a description of the proceedings of these sort of people, which holds good to this day. I wonder what the preamble to the Regulation for admitting them to places of higher trust will say.

it is his interest to improve his property, (according to the duration of his lease, and the prospect of a future assessment on the improvement.) It may so happen that the cultivator will not accept his lands on the terms proposed; the remedy is easy: after hearing his objections it may be raised, or it can be offered to another, or it may be cultivated under the system of *c,hám*. Few cases of this kind occur; it often happens that other *cisáns* eagerly accept of the rejected lease, and at other times it is farmed jointly by the whole village. Ten years ago it is strange how ignorant the Government were of the laws and customs which regulate the internal economy of a village; and to this day various little rights and privileges are discovered, as belonging to some class or another:—the details under which the regulation acted, therefore, laboured under a disadvantage in not meeting with some cases, which naturally will occur in settling such an extensive country. Considering the time at which the regulation was promulgated, it must be allowed to possess the quality of being founded on an extraordinary and extensive knowledge of the matter which it professes to regulate, and which probably no other person in the country can even now be said to possess: and although it has not been found to trespass on any of the rights and privileges of those interested in the soil, yet it has been thought proper to allow some latitude in deviating from certain of the minutiæ of the plan in some districts; and hence it will be found, that this latitude has been encroached upon, or indulged in, so far, that every collector seems to have a different mode of pròceeding on the regulation; so that one shall effect the settlement in an ordinary process and quantity of writing, while another builds statement on statement, paper on paper, till it reaches a *maund* in weight.

While we must admit the propriety and justice of recording every man's claim who has a right in the soil, yet the principle on which the amount of revenue is adjusted, professing to be a share, according to quality of soil and the products, does not seem to be so unobjectionable, as pointing a direct tax on the superior products of the country; this was part and parcel of the system handed down to us, and adhering to it seems to be the only objectionable part of the regulation, unless Government wish to check the production of these articles, which can hardly be the case.

Land, like every thing else in the market, will always be valuable according to the demand for it, and immediately when overtaxed in any particular spot, will find its level, by the cultivators repairing to other portions, not obnoxious to this high taxation; thus we find the lands of villages deserted, that some years ago yielded four or five thousand rupees annually, and the collector glad to get a farmer to take it for half that sum. I have now a memorandum of a village before me, which the Government took from a *jagírdár*, valued at five thousand rupees, and which about four or five years after was farmed for eighteen hundred rupees; the loss was of course made good by the Government to the *jagírdár*. To an individual a few cases of this kind would so press themselves on his notice, that the root of the evil would soon be found out and redressed; while to an extensive Government, lessons of this nature are lost in the consideration, (a false one,) that where they lose in one village they gain in another; but I could instance many villages, and even tracts of country, capable of producing the finest crops, ruined in a similar manner, within the last seven or eight years. Upon a good village the revenue officers, as if envious of its prosperity, go on increasing the assessment, till at last it passes beyond certain bounds, when the people's affections for their lands and home, being outweighed by the pressing demand, they determine on retiring, and the lands are left to be managed by the collector the best way he can. They seldom leave the village, but dissensions are sown among them, and they can no longer act in concert—probably in consequence of the respectable men of the village being held in durance for arrears. The village is farmed; a restlessness on the part of the cultivators probably soon tires out the farmer; the collector takes it into his own hands; a *chaprásí* or two are sent to look after its concerns, but in almost every case where the *zamíndárs* are not the farmers, the Government are the losers, and in a few years a sugar mill, or other sign of prosperity, is not to be found in the village.

This taxing of the land according to its productions, can only suit a country already completely occupied in the culturable lands; but as long as one-third of the lands are still uncalled for, it appears premature. If a village in the neighbourhood of one of our stations was to be assessed, and a gentleman (let us suppose the collector himself), who had his *pacca* house and garden situate on part of the *chálsa* lands, for which he was paying one rupee per *pacca bíga* of rent, came to have his lands measured, and the value estimated under the regulation, what would he say if he had it raised to ten rupees per *pacca bíga*; the value of the house would probably induce him to remain under the new assessment, with a fair share of grumbling at the demand: but on a new settlement raise it to twenty rupees per *pacca biga;* and although at a great inconvenience, and probably loss, his patience being outdone, he would most likely sell his house for the value of the materials, and build upon a neighbouring spot of ground, equally good, but not at present liable to this heavy assessment: the garden would go to ruins, and the Government would be just where they were, if not worse. This is just the case with the *cisán,* who takes it into his head to cultivate a superior produce, when he finds he has been taken in, and obliged to give it up, by the exorbitant taxation; or probably finds that his profits on inferior produce were just as good, or sufficient to keep him comfortable, without the risk[2]. In this way others are deterred, and improvements of every kind are looked upon with suspicion as a *fráib* (pretext) for fresh imposts; and I am very much mistaken if this new tobacco and cotton seed will not be received very cautiously. I perceive some has been sent to Bundél-c,hand; a very proper present, after the screw they got on the old system some ten years ago. Collectors may send down flaming accounts of the thankfulness of the *raíats,* but when they looked at it, their thoughts must have been naturally guided to the amount of assessment on an article of this very superior sort. This is a feeling not peculiar to the natives, but common to human nature. It was a feeling of this sort that some years ago made the Europeans batter the thermandidote with brickbats out of the barracks, as conceiving it some pretext for depriving them of part of their batta[3].

The taxing of the lands on the quantity of soil and products, leaves every opening for bribery and corruption, and embroils the collector with the peasantry. A constant teazing enquiry is going forward throughout the country, and it would be no difficult matter to prove that he is after all grossly deceived in the reports of every village; and in order to put a stop to these evils, the only plan is to reduce the assessments of the revenue to as simple a method as possible, leaving the products altogether out of the question, which is the only way to secure an improved agriculture.

It has been ascertained by measurements of parts of the buildings at Agra, and the distance between the *cós minárs,* that Acber's *gaz* was as near as possible 33 inches; sixty *gaz* is the side of a *pacca bíga,* or fifty-five English yards, giving a *bíga* of 3025 square English yards; this *bíga* of cultivation assessed at one rupee, will be found to be nearly the average revenue of the whole of the Upper Duáb; and if this was made the groundwork of the assessment, the whole would be immediately simplified, and the collector would have nothing to do but to look to the quantity of cultivated land; the tax of the culturable waste might be assessed at 3 *anas* per *pacca bíga,* allowing five *bígas* for the pasturage of each cow or buffaloe, and the value of the produce of a cow or buffaloe being estimated as low as 15 *anas* per year, one-third of which may be taken as the Government revenue, while the fodder from the cultivated lands may be presumed as giving sufficient food for the working cattle employed in agriculture. Land that is not worth $5\frac{1}{3}$ *anas* per *cacha bíga* of 1008 square English yards, (the *zamíndar's bíga* is about 800 yards,) is not worth cultivating, and the only evils which this assessment would ever produce, would be, in case of being over assessed, to drive the cultivators to better lands yet uncultivated; but it may be relied on, that no *cisán* will, under any system, ever put his plough into a soil which is not capable of producing this amount of revenue with advantage to himself; and in no case could any risk be run, of ruining rich and improving villages, which should be every way encouraged and protected by the Government.

An assessment of this nature would of course do away with all allowances or per centages from Government to proprietors, as he would derive the full benefit from the value of his estate, and the capital he might expend on it. Where the village was farmed to another person, the former would be charged with such allowance of *málicana* or *nankar* to the proprietor, as the value of lands might seem to warrant.

These remarks have been hastily drawn up, and I hope the attention of others, more capable, may be attracted to the subject, which is of much interest, and deserving of every consideration, as affecting the condition of the whole population of the country. I remain, Sir, your most obedient servant,

Upper Doab, 1830. Z.

[2] *Gújars* and other indolent tribes, feeling the tax on industry, cultivate little or no spring crop; perfectly satisfied with their autumn produce on a trifling assessment.

[3] Some old used hands in breaking machinery in England, were probably at the bottom of it, and might have taken the uncouth looking Thermandidote for an Indian mule-jenny.

VI.—*On the Animal of* Ampullaria.

To the Editor of Gleanings in Science.

Sir,

The observations of your valuable correspondent W. H. B. having directed attention to the animal of *Ampullaria*, permit me to offer the following remarks through the medium of your pages.

I procured several specimens of each of the species mentioned by W. H. B.; the dark olive and the banded shell; and from experience I am enabled to confirm the leading characters which he has given; also, from as careful a dissection as could be made without the aid of a microscope, I can testify to the internal anatomy of our Eastern species being in some points similar to their congener of the West; as the latter is described by Mr. Guilding in the 4th vol. p. 535, of the Zoological Journal. Our species have the *genæ in lobos duos tentaculiformes expansæ*, the "*os anticum labiis plicatis. Mandibulæ cartilagineæ, musculis validis tumidis*"—the "*oculi pedunculati, ad basi externam tentaculorum*," and the "*tentacula vera duo, longissima*," (though I doubt if *setacea* be not too strong a term,) &c. but as they belong to the new genus *Pachystoma*, of that gentleman, they have calcareous instead of horny opercula; and probably many other variations of structure may be discovered, on a more careful examination than I am enabled to make. The head of the animal of the larger species of our shell, is transversely corrugated, of a dark colour, and thickly covered with small irregular, oblong, vermilion-coloured spots; chiefly placed on the ridges between the corrugations; the upper sides of the foot, of the tentaculæ, and of the tentaculiform cheeks, are marked in the same manner. The eyes have the appearance of a black point, with a light red areola; but in the specimens I have examined, they do not seem to possess the higher degree of sensibility which W. H. B. is inclined to allow them: on the contrary, although the animal is sensible to the slightest touch upon the vessel in which it was contained, it bore undisturbed the approach and recession of a lighted candle; and this when immersed in a large bottle of water, through the medium of which the light was probably largely increased. I may here remark that a wide mouthed white glass bottle, is an excellent vessel for the purpose of examining molluscous animals, for if the water be perfectly clear, every surface of the specimen may be conveniently viewed.

The animal of the smaller, or banded *Ampullaria*, is rightly described by W. H. B. as of a lighter colour than the above: neither has it so much red on the head, foot, and tentaculæ; the foot, indeed, is almost white. It is desirable that the functions of this animal should be accurately observed, that it may be compared with the *Ampullaria* of the West, and the difference of structure, if any, brought to corroborate the propriety of the separation of the genus, founded upon the difference of the operculum and aperture. Though some of Mr. Guildford's separations may arise, perhaps, from a too great fondness for new names, I cannot but think he is in this instance correct; for I do not look upon the circumstances, of the operculum of the one animal being horny, whilst that of the other is testaceous; of the "*Labro crasse marginato sæpius caniculato*" of the one, whilst the other has "*Labro simplice tenui*," as so unimportant as not to warrant a greater separation than one merely of species. It is difficult to imagine so great a difference in the habitations of the two animals, without a material difference in the structure of the bodies that construct and inhabit them. To prevail upon W. H. B., than whom few are more competent to attempt the discovery of this organic variation, by dissection and observation, is a principal object of this note, and I need scarcely suggest the discovery of the functions of the tentaculiform cheeks, as a branch of the physiology of this animal well worthy of being known.

I may add that I have also observed the floating of *Ampullaria* in *jhils*, and in the bottle abovementioned I have seen it fall below the surface, and swim (if I may use the term) in the surrounding fluid, propelling itself, as it would seem, by means of the foot. In this attitude the appearance of the animal is striking. The large broad foot is expanded beneath; the head, with its moveable and moving tentaculiform cheeks, is thrust forward, and turned from side to side, as eager to seize and to devour any thing it can find in its way; whilst the long, slender tentaculæ, are ever searching around, now in the form of waving lines, now doubled backwards upon themselves, and again protruded to their utmost extent, as messengers to explore the way, or aids to the ocular points which are affixed to the peduncles at their base; and over the whole of this, the large, dark, orbicular shell rests upon the operculum, containing and sustaining the animal, who seems to support it.

Midnapore, 1830. I am, Sir, your obedient servant,

J. T. P.

VII.—*Account of a Boring made to obtain an Overflowing Spring.*

To the Editor of Gleanings in Science.

SIR,

As the experiments which are at present in progress under the auspices of the Asiatic Society, for the purpose of ascertaining whether good spring water is procurable in Calcutta, by boring, have attracted attention, it might be of service were I to detail the successful execution of a work of this nature in England, to which I was witness in the year 1824, it having been done in the grounds of a country house, at that time occupied by our family, at Longford, county of Middlesex.

I must premise, that attached to the property is a powerful water mill, at that time applied to the machinery of a calico print work, a business which requires a copious supply of pure water for the "bucking" of the prints. As the greater part of the work is executed in the fall of the year, and during the winter, the impediments caused by the heavy rains, during the above periods, rendering the water of the river turbid, are at times of serious moment in all print works, urged by water power; to remedy this, the idea of boring suggested itself. It may be of service to mention, that the country in that quarter is estimated to be on a high level; it is popularly said, somewhat higher than the dome of St. Paul's, and that it attains still greater elevation on the road to Windsor, on the other side of which town, about 8 miles from Longford, the descent begins into the rich vale of Berks. I know of no other spot in England so situated. In the space of less than ¼ of a mile no less than five natural and artificial branches of the same stream (the Coln) run parallel to each other for a short distance, and then diverge widely, each on its own course: the formation of several rich island meadows has thus been effected. I am thus particular in order that the important consideration of *locality* may be understood: it might naturally be asked by any one unaware of the importance of a copious and constant supply of the purest water in calico works, what was the use of securing additional supplies of water in such a land of rivers? —but the answer is obvious. The river at all times was equal to the task of urging the water wheel, and its attached machinery, but when rendered turbid by the winter floods, impeded the course of business. The surface of the country is a light loam, resting on a bed of gravel of considerable thickness; this point of course was known, but apprehensions were entertained, that, in the course of operations, a stratum of chalk would be encountered;—this, however, was not the case. The spot selected for the boring, was dictated by convenience, being within 4 feet of the river bank, and only a few yards from the mill wheel. As well as I can recollect, a triangle was set up, composed of three spars, one resting in the stream, the other two on shore; midway on this, a platform of boards was placed on smaller spars, lashed to those composing the triangle: an iron wheel, like those used for "*monkies*," was affixed to the head of the triangle; through this wheel was driven a stout rope attached to a barrel and winch. The platform was pierced with a square in which the borer worked;—the borer was like the ordinary iron earth borers, and in pieces of three feet, each screwing on the other, and the whole armed with a large "earth gouge" at the lower end.

At this distance of time, I am unable to say in what manner the bore was worked, but I am inclined to consider that no other power was applied, than that of two strong men turning the handle of the borer in the same manner that an auger is worked into wood. As soon as a few turns of the borer had been made, an iron instrument of large size, but somewhat similar in shape to the head of a claw hammer attached to the rope of the windlass, was hooked to the borer at the part nearest to the earth, a few turns of rope having previously been applied round the borer at that part, forming a kind of knot or stay to which the claw might attach itself. The windlass was then put in motion, the borer drawn up, and the earth cleared out of it: this process was repeated for many days; I believe about a fortnight.

The successive strata cut through were noted, but I have not the means of detailing them. Loam, gravel, sandy clay, then clay (of the usual kind), and sandy earth and clay, alternating in beds of various thickness, continued to the depth of 140 feet, without any indications of water, beyond the occasional cutting through of a small vein, hardly sufficient to moisten the borer, until the instrument came upon a bed of clay of a violet or blackish blue colour, imbedding many small shells, when the spring was found to be touched, the water rising to the surface through the small hole caused by the borer. The boring was continued somewhat further into the clay, when the spring still continuing, it was stopped.

The spring was then suffered to run over for a few days to clear the opening, when *copper tubes*, of about three inches diameter and in lengths, were carefully let down into the opening, to the distance of about 20 feet, and secured at the top. The uppermost tube was then inserted into a wooden cylinder, bored in the manner of a water-pipe, and about the height of an ordinary pump; the water rose up in this to the point where a spout was inserted into the cylinder, which carried the water to the bucking wheels, enabling the prints to be washed at all times. The supply was about four bucket-fulls per minute, and was never observed to vary night or day. The boring took place in the spring, and the winter following was so severe, that even the rapid stream of the Coln was frozen, while the wells and pumps in the neighbourhood were completely locked: yet for all this, the spring continued to flow exhaustless, and invariable: I saw it constantly for full seven months afterwards, when it was flowing, and I doubt not as it will flow to eternity. The water was so "*soft*," as it is termed, as to be used in preference for washing linen, and was sought for by the neighbourhood as the purest drinkig water; to the taste, it was, in reality, *tasteless*.

Allow me to close this paper with a digression. Pope has the well known line,

"Coln, whose *dark* streams his flow'ry islands love."

In the latter part of its course, and where it approaches its *embouchure* into the Thames, above Hampton Court, the Coln is a slow, and dark stream; but from its source beyond St. Albans, and especially at Denham and Uxbridge, and on to Cowley and Drayton, it is rapid and clear. What angler is unacquainted with the fame of the Uxbridge Trout, and what *gourmand* but can distinguish it from the trout of the southern countries[1]: it is known that this fish attains perfection only in clear and rapid streams.

S.

VIII.—*On the Compression of Books.*

The gradual compression of books into a smaller compass forms a not uninteresting feature in the history of typography. Few people are aware of the great difference which exists between books in this feature. Many of our expensive quartos, published by fashionable book-sellers, are as expanded as manuscript, while the small limits within which the contents of some books have been forced, is scarcely credible. There are three different sources of the improvements made in this department—for an improvement it undoubtedly is; at least if we are to believe the Greek proverb, that "a great book is a great evil." The first of these is due to the book-binder, who by substituting a roller press for the old method of hammering the leaves, has brought books into three fourths the former bulk, thereby reducing the cost of carriage, of book cases, and in works of many volumes, the cost of binding one fourth, or 25 per cent. The second source of compression is the thinness of the paper or leaves; and in this respect there is also a great difference. The small pocket bibles are printed on a very beautiful paper, the thickness of which is such, that 418 leaves are equal to one inch. Again, the paper of some of our large quarto works,—Brewster's Encyclopædia for instance,—contain 267 leaves in an inch, or a little less than seven-tenths of the former. The effect of this cause, then, is to reduce books by one third of their bulk. The third source of compression is the size of the type; and to show the great latitude there is in this respect, I shall enter into a little detail. The simplest way of giving an idea of the size of the type is to state the number of letters contained in a square inch, on a mean of the whole page. In this way we shall find, that the number of letters in a square inch of each of the undernamed works, is as follows:

Pickering's edition of Cicero,	612	This is perhaps too small, except for very young eyes.
Ditto of Tasso,	503	Perfectly legible by *good* eyes.
Pocket edition of Old Testament,	400	
New nonpareil type,	270	
Gleanings in Science,	200	
Whittingham's Cabinet Library,	180	A very legible type.
Brewster's Cyclopædia,	131	
Cleveland's Mineralogy,	107	

[1] Trout have been taken at Drayton and Longford of 8 or 10 lbs. weight; I have observed they are of the kind with black spots, and appear to be a kind of salmon-trout.

Brande's Journal,	89
A running hand, wide,	15
Not too small,	60
Difficult to write,	120

Thus we see that the size of type varies from 89 letters in the square inch to 600. No doubt there are books with larger type even than the above, but they are not common, and are generally intended for book fanciers rather than for common purchasers. Confining ourselves to the above sizes, there is evidently such a range in the size of the type as would make a book, according to the type used, 6½ times as large as it need be. This is taking into our account the small diamond type of Corrall, with which the snuff box edition of Shakespeare is printed. This type is, however, too small, and will suit few eyes—sometimes not even the youngest. But restricting ourselves to the type in which the pocket testament is printed, which is certainly legible, we shall have as the proportion 4½, and even between the type of the present work and the largest of the above, there is a ratio of 2¼.

The whole of the above circumstances may be brought into view by comparing the quantity of matter in a book with its bulk. This is best done by computing the number of letters in a *cubic* inch. This number will be found to be for the following works as stated.

Brewster and Rees' Cyclopædias,	90000
New Octavo Edition of Johnson's Dictionary,	250 000
French Edition of Voltaire,	281 250

These estimates are made from the size of the printed page, not from the bound work. The following estimates from the latter include the covers and margins.

Brewster and Rees,	24 600	call 1
Voltaire's Works,	95 800	4
Johnson's Dictionary,	123 000	5
Pocket Bible,	208 000	8½
Pickering's Tasso,	310 000	13

Were Dr Rees's bulky work prepared with the same degree of compression as the last in this list, his 39 vols of type would dwindle to 3 ! ! ! and if all the useless and erroneous matter were omitted, most probably to 2—thus forming a really portable library, instead of a load for a cart.

The French printers have set us an example worthy of imitation in the beautiful, yet compressed editions they have given of their classic writers. Voltaire in 3 vols., Rousseau, Montesquieu, Boileau, Racine, and Moliere, each in one, are models of typographical excellence; nor have we any thing to compare with them in English for the beauty of the paper and clearness of the type. They are even taking up our English works, and have already given us the most desirable editions of Walter Scott's prose works in 6 vols. octavo, and of Moore's, Byron's, and Scott's Poetry, in one each. The cheap and compressed English editions published by Jones and Co. are far inferior to these; nor is there any thing in English worthy of being compared with them, except, perhaps, Lowdon's Encyclopædias of Gardening, of Agriculture, and of Plants, and the above mentioned 8vo. edition of Johnson's Dictionary, with the corresponding one of Ainsworth's, which has recently appeared. These latter are two books which no one ought to be without. It were much to be desired that the new American edition of Johnson, now in course of publication in England, could be given to the public in the same form, and with the same compression as these; which, if the original work were judiciously weeded, might, we think, be effected, and the work kept within the limit of one volume.

F.

IX.—*Proceedings of Societies.*

1. Asiatic Society.

Wednesday, 27th July.

Sir Edward Ryan, Vice-President, in the chair.

Mr Mansel, Dr. Sully, Mr. J. Prinsep, and the Reverend Mr. Everest, were elected Members.

A vacancy in the Committee of Papers having occurred, the Reverend Principal Mill was elected a member of it.

Letters were read from Dr. Stewart, Captain Jenkins, and Maharajah Badynath Roy, withdrawing from the Society.

The following letters were also read: One from Major-General Hardwicke, forwarding a prospectus for the publication of a work on the Zoology of India. One from M. Roux, upon the 'Crustaces de la Mediterranie,' requesting communications from Members of the Society, on subjects of Natural History. One from Mr. Scale, of St. Helena, opening a correspondence on subjects of Natural History. A letter from Cámti, on the rearing of Silk Worms, (with specimen of the silk,) which it was resolved to refer to the Agricultural and Horticultural Society. A letter from Mr. Prinsep, forwarding an extract from the letter of the Honorable the Court of Directors, transmitting the catalogue of the library of the late King, presented to the Society by his present Majesty.

A report, by Dr. Strong and Mr. Ross, on the process and the probable expense of Boring for Water, agreeably to a resolution of the Physical Class, referring the subject to the general meeting, was read; and it was resolved that one thousand rupees be placed at their disposal to provide for the cost of carrying on the necessary operations under their general superintendance, and that they report progress from time to time. The Secretary's observations on the Madáris, or followers of Shékh Madár, were then read.

The following donations were received:—The Banner of the Madári, presented by Baboo Ram Comul Sen. Wood's Zoography, in the name of the Author, by his son, Mr. G. Wood. Avdall's Abridgement of Chamik's Armenian Grammar, by the Author. Von Hammer's Siege of Vienna by the Turks; from the Author. The Pentapotamia Indica, by Mr. Lassen. Numbers of the Journal Asiatique, by the Asiatic Society of Paris. The following proposal was submitted by Captain Herbert—"That the Museum and Library be open early in the morning, for the accommodation of such Members as may find it convenient to visit them at that hour. It appears that the hours at which the house is usually open, (ten to four o'clock,) are any thing but convenient to the generality of Members:—first, inasmuch as, during great part of the year, few people consider it salutary or even safe to venture to any distance at that time of day, at least for many days successively; and secondly, because those hours comprehend that portion of the day in which most of the members are engaged in their official duties." It was accordingly resolved, that the Secretary should consult with the Superintendent of the Museum, and the Librarian, on the practicability of the proposal.

Class of Natural History and Physics.

Tuesday, 30th June, 1830.

The President in the chair.

The following donations were made:—A specimen of the Ornithoryncus Paradoxus, from Van Diemen's Land, in good preservation, by Dr. Adam. Several Stuffed Birds, some Shells, and dried Insects, with an explanatory paper, from Mr. Pearson, Assistant Surgeon, Midnapore. A Cross Bow and Quiver of Arrows, with a specimen of the Poison used for the arrows, from Captain Bellew, who states that such poisoned arrows are used by certain tribes of Hill people in the Morung; but cannot afford any information relative to the tree from which it is taken. It was resolved to transfer the packet of poison to the Medical and Physical Society.

An extract of a letter was read from Mr. Scott, Agent for the Governor General on the N. E. Frontier, to Mr. Swinton, dated Charra Punji, 19th June, in which Mr. Scott describes his visit to the cave in the neighbourhood, and the precise situation of certain curious stalagmitic balls found on the floor thereof, some no larger than a pea, and some as large as a cauliflower; but the most numerous, of the size and shape of a custard apple.

Two letters from Dr. Gerard were transferred from the General Secretary, who stated that a collection of mineral specimens referred to in them, had arrived; when it was resolved, that a committee be appointed to examine and report upon the minerals at the next meeting, to consist of Mr. Calder, Captain Herbert, Captain Jenkins, Dr. Adam, and Mr. Ross.

Dr. Strong's paper on Boring for Fresh-Water Springs, read at the last general meeting of the Society, was transferred by the General Secretary to the Secretary of the Physical Class, and after some discussion on the subject of the borings now carried on in Fort William, it was resolved, that Dr. Strong and Mr. Ross be requested to report to the next general meeting of the Society, the progress made in the recent borings in the Fort, and the probable expense that may attend the prosecution of the work, to the attainment of the desired object.

There were received from Dr. Hardie, of Udayapúr, a paper on the Geology of the country to the eastward of Cluton; Sketch of Section of the Strata between Neemuch and Meintah, near Udayapùr, with explanations; Sketch Map of the northern part of Megmaw, with extracts from an accompanying letter; Supplement to the Sketch of the Geology of Central India, &c. The reading of these was deferred to another Meeting.

X.—*Notices of Books.*

Reflections on the Decline of Science in England, and on some of its causes. By Charles Babbage, Esq., *Lucasian Professor of Mathematics in the University of Cambridge, and Member of several Academies.* London: B. Fellowes. 1830.

Here is a work of a novel, and, we may add, fearless character. The author is the celebrated inventor of the Calculating Machinery which is now under construction at the expense of Government, and which has excited so much of the attention of the scientific world. The subject is interesting and important, though we should think a ticklish one, supposing the author prepared to do his duty. That this author has done so, at least that he has not been prevented doing so by any undue consideration for influential personages, will be abundantly evident to those who peruse his volume; and whatever may be his other faults of execution, he cannot be accused of shrinking from the exposure of the many abuses and cabals which are found in England to check the progress of true science, and to render coteries and jobs triumphant. The picture which is here drawn of the state of English science is melancholy in the extreme, and certainly makes the wonder less that we should be so much below the nations of the continent in the cultivation of every branch of science. In one point, however, we differ from our author: he considers the vile system he has exposed, to be the CAUSE of the decline of science: we, on the contrary, are disposed to think that science must already have been at a miserably low ebb in any country where such things "can be, and not o'ercome us as a summer cloud with special wonder." Where such things could pass as every-day matters, there science must have already declined.

That the low state of science in England is mainly owing to the system of education, is an opinion we had ventured to give before seeing the present work; and we are glad to see this opinion confirmed by one so much more competent to judge. He observes—"A young man passes from our public schools to the universities, ignorant almost of the elements of every branch of useful knowledge; and at these latter establishments, formed originally for instructing those who are intended for the clerical profession, classical and mathematical pursuits are nearly the sole objects proposed to the student's ambition." The pertinacity with which we cling to a system of education established in the dark ages, and for a particular class, is indeed characteristic of our nation—as one jealous of innovation and intolerant of change. Yet the many practical evils of our perseverance in so absurd a course, generally acknowledged as they are, would seem to be more than sufficient warrant for a new experiment. That some improvement must take place sooner or later, seems evident, but in the mean time we have lost ground in the race, and have allowed ourselves to be outstripped by all the world, even by that scion from our stock, America[1].

It is a consequence, it appears to us, of the little attention paid to science in England, that, unlike the other countries of Europe, it has no separate class or profession, devoting itself to the exclusive cultivation of science. All our scientific characters belong to other professions, on which they depend for their provision, their scientific labours being occasional and desultory. One consequence of this state of things must be, that owing to the claims on their time made by their profession, few of the investigations they engage in can be pursued to a satisfactory issue—nor is it scarcely possible that they should ever attain excellence while their attention is thus divided. A second consequence is, that there is wanting that stimulus of professional rivalry which would often prompt to great discoveries, and equally that wholesome supervision exercised by a profession over all its members, which, giving to each man the credit which he is justly entitled to, is as certain to reward merit as it is to mark deficiency. If a man ignorant of law and lawyers wish to employ the first counsel, let him enquire to whom the profession generally look up, and he is not likely to be disappointed in his choice. So also in other professions or distinct classes, in which an *esprit de corps* seems to give every, even the lowest, member a sort of interest and pride in the success and efficiency of the highest. But scientific men in England have no *esprit de corps*—the consequence is, they are divided into coteries, parties, cabals. The interests of science are thus lost in the squabbles and party differences of individuals, and the result is every way mischievous. It is not the least evil of this state of things, that no union of effort, or concert of aim, can be expected from them.

[1] Mr. Babbage mentions, that the *first* translation into our language of that grand work, the *Mecanique Celeste*, has just arrived from America.

Our system of education is undoubtedly the root of the evil: yet it is not sufficient to account altogether for the low estimation into which true science has fallen, or for the fact of its having been almost universally replaced by the contemptible system of tricking and puffery here exposed. An almost equally powerful cause will be found in the want of national encouragement, which is the disgrace of England. In the true spirit of a *nation boutiquiere*, it is said that every commodity, and science amongst the rest, will always, if left to itself, fetch its true value; and that if required by the public, the demand will necessarily occasion the requisite supply. But the maxim thus applied is as erroneous as it is contemptible. Mr. Babbage has well shown, that science consists of two parts, theory and practice—in other words, principles and useful results. The latter are the effect of the application of those principles to the affairs of life. Now the investigation of each class of truths is so distinct, that seldom or ever do the qualities of mind, requisite for their discovery, unite in the same individual. But it is the latter alone that the public know any thing of, and consequently to those who excel in the discovery of practical applications, and to them alone, is the encouragement of the public given. The investigator of the principles which have been applied, though equally entitled to the reward, is altogether lost sight of. The discoverer, for instance, of the principle of latent heat, as it has been called, had no share of the immense reward which fell to his lot, who suggested merely one application of so general a law. Yet had Watt not known of this law, would he have ever stumbled on the capital improvement of condensing the steam in a separate vessel. The inventor of the reflecting quadrant could have done little but for the previously known laws of optics; nor could the astronomer have derived much benefit from the instrument, when invented as applicable to determine the longitude, but for those abstruse researches of the mathematician, which, assisted by a few observations, enables him to predict the exact place of the heavenly bodies. It will, in fact, be found throughout, that theory and practice must go on together, or, to speak more correctly, practice cannot move a step but as she leans on theory. The limits of the latter will be the limits of the former. Now what encouragement has any one, however well qualified by nature, to devote himself to this branch of the subject? If he be independent, he may perhaps, urged by the strong bias implanted by nature, pursue a path where neither profit nor distinction, as emanating from the Government, await him; but if he has to struggle against the *angustas res domi*, he will of course turn his attention and talents to some more promising occupation.

In other countries the case is different; there men of science not only reap distinction and pecuniary reward, but are considered eligible to the first appointments in the state. Mr. Babbage gives a long list of these; amongst which we may mention, as known to all our readers, Laplace, a Marquis, and President of the Conservative Senate; Carnot, Minister of War; Chaptal, a Count, and Minister of the Interior; Cuvier, a Baron, and Minister of Public Instruction: the latter, too, having to struggle with the prejudice against his religion, (the reformed.) The consequence of this difference of system is, that on the continent, and specially in France, science is in the most flourishing and prosperous condition; while in England we are day after day losing all that once made our distinction. Even our mechanical arts connected with science, the encouragement of which can never be trusted to the commercial spirit, are fast losing ground. The achromatic telescopes of Dollond, which once made us a name all over the globe, have been eclipsed by the productions of an establishment at Munich[2], assisted as they were by the successful efforts of an obscure Swiss clock-maker[3], to manufacture glass of a superior quality, and in larger pieces. This is one of the many practical evils attending that total indifference to the scientific character of the country, which is particularly indicative of a Government incapable of appreciating the value of science, and ignorant of the fostering cares required to assist its progress.

We wish our limits would permit us to give a more complete idea of this work, but we can but glance at some of his topics. Amongst other causes of the decline of science, he considers the fallen condition of the Royal Society as not the least influential. To be a Fellow of that Society, is evidently no longer a distinction; a circumstance the more to be regretted in a country where the Government is passive with regard to science. The picture given of the proceedings of this Society is pitiable in the extreme, and must, we should think, from the strong revulsion of feeling which it will occasion in the public, lead to a reform. We cannot afford space to enter into all the statements given on this subject, but one

[2] Frauenhofer's. [3] M. Guinand.

fact is sufficient to enable us to appreciate the authority of the Royal Society, as a learned body, and the degree of distinction conferred by the designation of F. R. S. England, with a population of 22 millions, has 685 members composing her scientific academy, with 50 foreigners: France, with a population of 32 millions, has 75 natives, 8 foreign and 100 corresponding members. It requires little consideration to determine in which country the title of academician is calculated to confer credit. As qualifications for admission into the Royal Society of London, except the fee, nothing seems to be required, save a matter of course certificate from three of the fellows; when, if the candidate, as Mr. B. observes, be so fortunate as to be quite unknown, he is sure to be elected. Even if he be a person of some celebrity, and yet so anxious to become a fellow as to overlook the trifling inconvenience of being black-balled, he is pretty sure, on a second application, or at most, a third, to succeed[4].

One of the most extraordinary parts of this work is the very singular and apparently complete exposure made of the observations given by Captain Sabine, in his work on the Pendulum. This work was printed at the expense of the Board of Longitude, and the Council of the Royal Society spared no pains to stamp the accuracy of them with their testimony. The circumstance of Government providing instruments and means of transport for the purpose of these enquiries, gave Captain Sabine advantages which do not often fall to the lot of the Amateur; while his great industry in observing, enabled him to bring home an immense mass of observations, and from places but seldom visited, at least with such instruments.

The close agreement of these results, though not exactly what those most conversant with the subject expected to see,—seemed yet to others creditable to one who was so new in his career as an observer. Whatever the subject of enquiry, all other observers were left at a distance, and to many it appeared that Captain Sabine had some keenness of vision, or acuteness of touch, that it was hopeless to expect to rival.

Among the instruments employed by Captain Sabine was a repeating circle, of 6 inches diameter, with a telescope of 7 inches focus, and 1 inch aperture, made by order of the Board of Longitude, for the express purpose of ascertaining how far repeating instruments might be diminished in size. Captain Sabine, in his work on the pendulum, unequivocally stamps his approbation upon this small instrument, and gives it as his opinion "that the disadvantage of a smaller image, enabling a less precise contact or bisection, and of an arch of less radius, admitting of a less minute subdivision, may be compensated by the principle of repetition." He does not hesitate even to compare the performance of this minute instrument with that of the circles used in the French Survey, allowance being made "for the extensive experience, and great skill of the distinguished persons who conducted the French Observations[5]. The results at Maranham and Spitzbergen are especially referred to, as obtained in greater number, and with every attention to accuracy.

Mr. Baggage, in two tables, compares the deductions at the former place with those made by the French astronomers at Formentera, by means of a repeating circle of 16 inches diameter, constructed with special care by Forten. In the French series, taking only the inferior passage of Polaris, the results of from 64 to 120 repetitions have an extreme difference of 3", 8. The greatest difference from the mean is 2", 2. In Captain Sabine's Series the results are derived from but 8 to 12 repetitions; the stars are all different, yet the extreme difference amongst 6 results is 2", 5, the greatest difference from the mean 1",3. The natural inference from this comparison is, not that a repeating circle of 6 inches diameter is merely *equal* to one of 16 inches, but that it is decidedly *superior* to it.

"Fortunately for astronomy, as Mr. Babbage observes, long after these observations were made, published, and rewarded,[6] Captain Kater having borrowed the same instrument, discovered that the divisions of its level, which Captain Sabine had considered to be equal to *one-second* each, were, in fact, more nearly equal to *eleven-seconds* each, one being 10," 9"!!!

If now correcting for this strange error, we enquire what would be the results at Maranham, "*which were obtained with especial regard to every circumstance by which their accuracy might be affected,*" we find that instead of the extreme difference from the mean being only 1", 3 "it is 12," while the difference amongst the re-

[4] Mr. Babbage mentions, that during 14 years' experience he has had of these matters, all the candidates rejected were *known* persons.

[5] Vide our number for January, p. 9 *et sequ.*

[6] Captain Sabine obtained one of the medals of the Royal Society, and a 2nd from the French Institute for these observations.

sults instead of being 2″, 5, amounts to 22,″. In another series at Bahia, there is a difference even in the same star, Lyræ, of 28′, though observed within three days. But though this allowance for the error of the level, brings the observations nearer to what might *a priori* have been supposed the powers of such an instrument, there is still this difficulty. In a series of so many observations, how has it happened that the error of the level should have amounted always so precisely to that quantity required to produce a perfect seeming accordance? Against such a compensation, depending on so arbitrary a correction, the chances are, as Mr. B. observes, enormous. It was too, to say the least of it, "a singular oversight not to measure the divisions of the level."

To this *exposé* we must add two facts more, which we think will astonish our readers in India. Mr. Babbage says, that at a visitation he made officially to the Royal Observatory, he was an eye witness of the process of an observation, or otherwise never could have believed what he then ascertained, viz. that at that time no *original* observations made at the transit instrument were ever preserved!!! It appears that the Astronomer Royal most justly appreciates the value of his own observations; of which, though a copy be not obtainable by an Astronomer from the Royal Society, at whose disposal they are placed, yet any number may be purchased as *waste paper.* These observations are printed at the expense of Government, in the most costly style of typographic luxury, on hot-pressed paper, with wide margins, &c. and are then given to the Astronomer Royal as a perquisite, 60 copies being retained by the Royal Society, for distribution to the observatories and learned societies of the continent. Those sold by the Astronomer Royal are said to make *capital Bristol Board.* This does seem the most extraordinary method of making up the deficiency of a public officer's salary that we have ever heard of, and we wonder the Astronomer himself does not suggest some other.

A very interesting section of this work, is that entitled, *On the frauds of observers.* These he ranges under the following heads: 1. Hoaxing; 2. Forging; 3. Trimming; 4. Cooking. The first is, perhaps, the most venial, as ultimatety no fraud is intended; the real object being only to excite a laugh. Mr. Babbage, however, well observes, "that the productions of nature are so various, that mere strangeness is very far from sufficient to render doubtful the existence of any creature for which there is evidence;" so that, except in an extreme case, the hoax "can only be regatded as a deception, without the accompaniment of wit." Of the second, the nature of which is sufficiently obvious, from the name, there are fortunately few instances. One however is mentioned of a Chevalier D'Angos having published observations of a new comet, which have been since proved by M. Enche to have been invented; a mistake the author made in his calculations having led to the detection. "*Trimming* consists in clipping off little bits here and there from those observations which differ most in excess from the mean, and in sticking them on to those which are too small, in order to obtain the character of a very close observer. "The fraud is not so injurious (except to the character of the trimmer,) as cooking," because the average given is still the same, whether trimmed or untrimmed. *Cooking* again is an art, the object of which being to establish the character of the observer for the utmost possible accuracy, he stickles at nothing which he thinks can conduce to this end. Of the many processes which have been resorted to, the following are the most common. He makes a multitude of observations, and out of these, selects for publication those which happen to agree best. Or in calculating his observations, he will use two or more formulæ, so as to insure perfect accordance in the results. He may use different catalogues of stars—different tables of refraction, of specific gravities, of specific heats, &c. &c. so as always to have the desired accordance in the results. Sometimes the constants of a formula, though differing amongst themselves, will not suit the observations in this case; a *cooked* mean value is required, which is easily managed, by a pretended discussion on the weight due to each, to Bessel, to Gauss, to Laplace, &c.; this being properly adjusted, the mean comes out as it should do, and the *cookery* is complete. There is a good deal on the improper influence which other bodies, such as the Royal College of Physicians, the Royal Institution, &c. exercise in the proceedings of the Royal Society, but our space will allow of no more extracts. Mr. Babbage concludes with a very interesting comparative view of the scientific characters of the late Sir H. Davy and Dr. Woollaston. This we shall give at length in our next number.

GLEANINGS

IN

SCIENCE.

No. 20.—*August,* 1830.

I.—*On Value.*

An opinion now prevails, that unless the nature of value be thoroughly understood, the nature of wealth cannot be understood. I do not mean to assert that this opinion is correct, or otherwise; it is sufficient that it exists, and that it influences writers on political economy.

This opinion had its origin, more immediately, in the writings of Mr. Ricardo; as his theory of profits, "the crowning achievement," as it is called, in the science of political economy, rests on certain propositions regarding the value of products. Now as Mr. Ricardo's authority is admitted to be paramount and unquestionable, by the whole of the *liberal* writers of the age; and as the system of legislation, in matters connected with national wealth, recommended by these most indefatigable teachers of the ignorant, is bottomed on the admission of Mr. Ricardo's infallibility; it behoves those, who do not like to take such things on trust, to institute an inquiry into the exact nature of this interesting subject, and to sift to the bottom those pretensions to infallibility, which are so clamorously and perseveringly put forth.

It is clear, that to come at just conclusions on a subject, which has, in these latter days, excited so much attention, we must descend to fundamental principles; if the conclusions of Mr. Ricardo are vitiated by any false step in the progress of that chain of ratiocination, which has been so highly extolled as perfect, and as sound in every link, it is manifest that we can only hope to detect the breaks and flaws, by minutely examining every link in the chain; and if we can go nearer even to the end of the chain than he has done, so much the more will the chances be increased of our detecting latent failures, or instances of seeming dependence, where the successive links are not really conjoined. This it is my intention to do; but enough of preamble; I sincerely believe that fallacies do exist in his arguments; and I flatter myself with the hope of exhibiting them to the full satisfaction of my readers. They must bear, however, with, what will probably appear, my prolixity—they must remember that I am taking up the subject *ab initio,* and that I must carry conviction with me in every successive step I take in the disquisition; else my labour, and their attention, will unquestionably be thrown away. If I am found unconsciously tripping, I shall be happy to be informed of it; for I seek nothing but truth. Another thing I beg of them to keep in mind, (for political bias will obscure the vision of the most determined searcher after abstract truth,) that although I conceive Mr. Ricardo's reasonings to be fallacious, and although I am set in opposition to them, yet it does not follow that I shall ultimately adopt whatever happens to be diametrically opposed to his conclusions. It is the fallacy to which I wish to offer resistance, and to nothing else. A fallacious reasoner, provided he has talent, may readily reach many such conclusions, as a strict logician also arrives at; particularly if these tally with existing appearances; for fallacy is a most supple, and a most subtile auxiliary to an able chief. All I crave of the reader is, what every real enquirer after truth will most freely grant; namely, that wholesome distrust of what has, (I maintain it,) been heretofore taken upon trust, by a very great majority; and that he will shake himself free from those trammels which a great name, and the possession of very great, and very generally admitted talents, are apt to impose upon the mind.

And now let us proceed to the enquiry into the nature

OF VALUE.

Man cannot exist without food—without water—without air. All these bodies are of vital importance to him, as well as many more, to which it is needless particularly to advert; and in the event of his being reduced to extremity, from the want of any one of these, he must be fully disposed to make the greatest possible exertions for the possession of that which is deficient.

Man can have but one opinion regarding the exertion he makes in obtaining any acquisition. It is, in the first instance, a sacrifice of the ease which attends repose; and its continuance amounts to actual pain; the constitution of the human body insures this; for it is only capable of making limited exertions, and of sustaining continued efforts for a very limited time.

Before man voluntarily exerts himself in any acquisition, the gratification the exertion is calculated subsequently to realize, must, in his opinion, be more than sufficient to counterbalance the sacrifice submitted to in obtaining it. He must compare, and balance in his mind, before he proceeds to action, the one against the other; and that which preponderates, he must reckon the most desirable of the two. To the apprehension of man, the voluntary acquisition of certain bodies, possessed of certain qualities, must appear of more importance, than the sacrifice by which they are obtainable. By his physical constitution, and by the circumstances in which he is placed on earth, a necessity is created for his continually making this comparison; and from the same causes, the result of the comparison must, in the great majority of cases, be in favor of the possession; and hence the existence of the idea of a value attaching to such possessions, superior to something with which man, as a moral agent, is conversant—to which a mental reference is readily made; but which is not present to any of his outward senses. Value then is not, strictly speaking, a quality existing in any substance. There is nothing in the world inevitably or inherently possessed of such a quality; to which, as to an outward and visible standard, all else may be referred. But it is an affection of the mind of man, which inevitably must arise, in connection with certain classes of bodies, in all the ordinary circumstances in which he is to be found.

But although, metaphysically treated, there be no such quality in bodies as value; still, as there must ever arise a perception of value, in connection with certain bodies, on which man's existence is dependent; it may not, in ordinary discourse, lead to error, to treat of the inevitable concomitant, as if it really existed in that with which it is ever found conjoined. It may justly be said virtually to exist there; and I propose, therefore, for the purpose of avoiding circumlocution, to substitute the one for the other; and to treat of value, as if inhering in the class of bodies already pointed out.

The objections being thus disposed of, which might be started against treating an affection of the mind of the moral agent, as if it were a property existing in the body which gives rise to the affection in question; we may proceed in the enquiry. From what has been said it must be obvious, that it is the value virtually inhering in the body, which calls forth the exertion to which man voluntarily submits in obtaining that body; and hence we arrive at the following conclusions, directly opposed to all existing theories on the subject; that in place of labour being the cause of the existence of value, it is the previous existence of value, which is the original cause of the exhibition of labour, or of any exertion of what kind soever; and that the original source of value in any body, is not to be sought in the labour which man may be ready to offer in acquiring that body, but in his entire dependence (as God has been pleased to constitute and dispose of him,) on such bodies as we have now had under consideration.

If the necessity for all such bodies as the above be equally great, the scarifices to which man must be willing to submit, for obtaining any one of them, must be equally great also; and as a voluntary sacrifice implies, of necessity, that the thing sought for is held in higher estimation than the thing sacrificed to obtain it; it follows, that the values of all these must be equally great also. The equal value to man, of air, and water, and food, I take, therefore, to be established. But the circumstances under which he is placed, with regard to the last mentioned of these, being widely different from those under which he is placed with regard to the others; it behoves us particularly to attend to this difference; for the one, we find in practice, to constitute the primary basis of all wealth whatsoever; while the others come not under that denomination.

Our Maker, in dooming mankind to a life of exertion and trial, did not, in most cases, (as far we may presume to judge,) deem it necessary to set in operation more than one sufficient cause for producing the effect desired. In the case now under consideration, he, it would appear, supplied in unlimited abundance all substances essential to our existence, with one exception. Of air, water, and I may add heat, light, &c., there was made no present, or pressing deficiency; but of food, the nutriment of the human body, he ordained that there should be a present deficiency; and he so organized that which constitutes our food, that it should be within the scope of man's own powers to increase its quantity, by the exertion of labour. He endowed it with the principle of reproduction, and increase; and placed a little only, originally, at man's disposal. These were his immutable decrees. "Behold, I have given you every herb bearing seed, which is upon the face of all the earth; and every tree, in which is the fruit of a tree yielding seed, to you it shall be for meat." And again: "In the sweat of thy face shalt thou eat bread, till thou return into the ground."

Although, then, all the bodies above mentioned, are of equal value to man with food, still as the necessity was not created, for setting human exertions in the opposite scale to stated quantities of these; and as they were not so constituted as to be periodically furnished in accordance with the recurring wants, and consequent exertions of man, there could exist no index by which he could know, specifically, the value of any given quantity of any of them.

This is not the case, however, with food. Man was doomed to be stimulated to exertion, through the means of hunger and want; and was immediately necessitated to make exertions, whereby he might avert these calamities.

A hungry man cannot fail to be acutely sensible of the existence of value in food. Food, (given on the terms decreed by the Almighty, sparingly, and as a compensation for exertion,) to be available to man's use, must be obtainable in return for some sacrifice, which it is within the power of man to bestow; if it were otherwise, he must perish. Man is under the influence of the procreative principle; his numbers, therefore, particularly in the ages which precede the knowledge of the arts of production, must be pressing against the means of subsistence obtainable.

When man's numbers are pressing against these means of subsistence, the very utmost exertion of every individual must be made in obtaining this subsistence. To obtain, therefore, that food which suffices for a man's support, the amount of the exertion made by each, must be the utmost each can offer. And as the power of making exertion, enjoyed by men, is nearly the same in all, the estimation, in which a sufficiency of food is held, must be determinate, and easily known; being somewhat superior to the estimation in which the sacrifice is held, which is voluntarily made in procuring it, and which is the utmost man can bestow.

The value then of nutriment, unlike the value of air, or water, or heat, is susceptible of specific determination and appreciation, in consequence of the necessity which thus exists for weighing, in one scale, a given quantity of food, equal to the support of a man; and in the other, a given quantity of exertion, regarding the worth of which exertion, all men must be agreed.

Value then is to be treated as an original and independent existence in certain bodies; and it must positively inhere, (virtually, as has been already explained,) in these bodies, whether man exerts his labour or not, in procuring them; and man must be acutely sensible of its existence before he can be tempted to labour in obtaining them: And as it is only in the case of the valuable bodies, constituting food, (the primary description of wealth,) that man has the power of facilitating their increase through the means of his exertions; so it is these alone which are susceptible of specific valuation, or of being estimated at certain quantities of that which is in immediate connexion with the consciousness and perceptions of the moral agents, who are the sole percipients, and appreciators of value.

We may, therefore, pass from the consideration of that value which is not susceptible of appreciation, and which equally inheres virtually in all the different bodies on which man's existence as much depends, as upon food, as not being the subject of our investigation. That appreciable value, however, which exists in food, and which, from the circumstances of its own organization, together with those of man on earth, is capable of exercising an influence on his feelings and actions, is the subject of our investigation; and, therefore, when the word value occurs in this discussion, without any adjunct, it must be understood, as that value which is appreciable, and which appertains exclusively, as we shall hereafter see, to all descriptions of wealth; but originally, and independently to wealth of the primary description.

Against the original and independent existence of value in this primary description of wealth, it may be urged, that as the acuteness of the perception of its value must depend on the necessities of the percipients; the quality, in place of being inherent and permanent, is, on the contrary, accidental and transitory; the same substance being at one time valuable, and at another time scarcely entitled to the name.

But while man, taken collectively, is under the influence of the procreative faculty, and placed on a surface of limited extent; of which extent but little is found spontaneously to yield human nutriment; and while hunger must be of continual recurrence, it will not, I apprehend, be denied, that the disposition to perceive and admit the virtual existence of value in food, may be counted on as a fixed principle; on the continued influence of which we may safely reason.

To the existence then, in any substance, of this appreciable value, which it is our province to study; it is essential, first, that value should, in itself, virtually inhere in that substance; and secondly, that the disposition should exist to make specific sacrifices of labour for its possession.

As the value which is here specified, must positively and independently exist in items of wealth; it behoves me to dispute the arguments which have generally been advanced against the existence of real or positive value; and in so doing, I will first fix the attention upon a consideration sufficiently obvious; but which appears, most unaccountably, to have been heretofore overlooked. It is this; that unless value positively exist in each of any two or more bodies, it is quite impossible, that the relations of the value inhering in each, can be a subject of perception, or of appreciation; or can ever influence the will and conduct of man. I maintain, that to the existence of relative value, which has heretofore been deemed the only value connected with wealth, and which has been, in consequence, the only value which political economists have professed to discuss; it is essentially necessary that positive value should pre-exist, independently in every valuable body, of which the relations may come to be subsequently compared.

The oversight above remarked has not been confined to value alone; but has, by these reasoners, been extended to every sensible quality, wherewith bodies are imbued. Thus extension, solidity, sweetness, and so forth, have come to be considered as only existing in their relative modes; absolute extension, absolute solidity, &c. being treated as creatures of the imagination, of which we can know nothing certainly; and which are incapable of influencing our volition and con duct.

But I apprehend that the following instance must be allowed as proof, that this supposition is not correct. Thus, when a man requires assistance, to enable him more readily to obtain that which is beyond his reach, he naturally looks around for some body in which exists solid extension, more particularly in one direction; and the solid extension existing positively, and independently in the body which he finds fitted for his purpose, is the object of his perception, and the motive for his subsequent actions with regard to it. If there happen to be two or more bodies, each possessed of similar extension in a similar degree, it will be a matter of indifference to him which he employs; still, however, the existence of absolute extension will induce him to use one, although his choice will have nothing to determine which. If again these different bodies possess this quality in different degrees, then the relations of this quality in each will be compared, and that body will be selected which is best suited to his purpose. But it is manifest, that this latter operation must be subsequent to, and wholly dependent on, the pre-existence, independently, and absolutely, of this quality in each of these bodies, and that there could be no perception of differences, and no subsequent operation of the mind in determining the choice, but for the accidental circumstance of this quality being enjoyed in common, although in somewhat different degrees, by two or more bodies.

It is very natural, that the ideas of sensible qualities, most familiar to the minds of men, should be relative; because common qualities are found to exist, sometimes in an equal, sometimes in a greater or less degree, in all the different substances with which we can become acquainted; and because of the consequent necessity under which man must ever labour, of distinguishing and identifying, by their respective differences, whatsoever objects are presented to him.

But it must, notwithstanding this, be remembered, that the ideas of extension, solidity, color, taste, and so forth, when suggested to man, by bodies calculated to give rise to these sensations, are original, simple, and independent ideas; and perfectly capable of exercising an influence on his conduct; and it must not be forgotten, that the ideas of relative extension, relative solidity, and so forth, are later considerations; the subsequent existence of which, depends on the accident of two or more bodies, possessing one common quality, in an equal, or in a greater or less degree.

All that has been stated above, regarding certain qualities, is equally applicable to all qualities whatsoever, whether actually or virtually existing; and amongst the rest as to value; and it is as certain, that value, positively and independently, must virtually exist in each valuable body, as that solid extension, particularly in one direction, must exist positively and independently in each of every one of the sticks, which may be supposed to have been the subjects of perception, or the person whose case has just been given in illustration.

Is there nothing, however, in the quality of appreciable value, as it exists in wealth, peculiar to itself? Originating as it does, not necessarily, in the body itself, which men reckon valuable; but appertaining to it, only under certain circumstances, in its connexion with the moral agents who appreciate its existence. If, for instance, the condition necessary to the creation of this appreciable value be, that valuable bodies shall continually be given in exchange for voluntary exertion; then, as now, the valuation of products, relatively to the exertion they have cost, must be continually made; and, therefore, the only value about which men can be conversant, is still merely relative.

But I would ask, in this case, to what is this value relative; to products, or to men, the percipients of the quality value? Now this is a very material question; for it is to be particularly remarked, that when we reach the human exertion products are found actually to cost, we have come, at last, to that index, whereby may be known the opinions men entertain regarding the real and independent value of the products in question. A value which is relative not to other products; but to the feelings of the actual percipients themselves of this quality. A value which may exist, if there were but one valuable product in the world; and which is capable, under any circumstances, of influencing the conduct of man.

If it be of no importance to distinguish between the relations of products to products, and the relation of products to men; to mark the difference of the connexion subsisting between certain substances, and the connexion of these substances with moral agents; then let this valuation of products, relatively to the voluntary sacrifice they have cost, be called, as has been heretofore usual, relative valuation. But it appears to me of the very utmost importance that this difference should be marked and understood; and I therefore wish to keep separate and distinct, the simple idea suggested to moral agents by the perception of value; and the complicated consideration of two or more values, each already perceived.

II.—*On the best method of procuring a Plentiful Supply of Wholesome Water, in the vicinity of Calcutta.*

The art of sinking overflowing wells—an art of almost modern growth—is one of the many useful results which may be fairly attributed to the operation of scientific views, *i. e.* the reasoning from certain data to establish the truth we are in search of. It seems certain that it was in no way the effect of accidental discovery, but rather the reward of the first intrepid engineer, who, convinced that *water ever seeks its level*, determined to hazard much in pursuit of the valuable consequences which he saw in particular circumstances would result from so simple a truth[1]. The observations of the geologist, showing the great extent as well as inclination of some of the strata, doubtless prepared the way for the more general reception and adoption of the new art, if it may not even have suggested it in the first instance. When we see a stratum impervious to water, (as clay for instance, or limestone, in which may be included chalk,) continue its course unbroken for many miles; and consider also its inclination, (for scarcely a perfectly horizontal bed is to be found,) we are struck with the conviction, that whatever springs of water may be below, but in contact with the higher levels of this stratum, will, if egress through the stratum be afforded them, and they be

[1] Mr. Partington gives the credit of the discovery to Benjamin Vulliamy, Esq. of Norlands.

at the same time prevented escaping laterally amongst the more pervious strata, rise to the same level, at whatever part of the stratum they emerge. Thus supposing a bed of limestone or clay dipping 10 feet in the mile; and supposing that at its basset or outcrop it has been ascertained that plentiful springs exist beneath it, it is obvious, that through whatever part of this bed an opening be made, the water must, if confined in a tube, rise to the level of the first ascertained springs, which, for instance, with such an inclination, would, at the distance of 40 miles, afford an overflowing well 400 feet deep. It must, in fact, be obvious to every one that water will rise to the same height in two tubes, however distant, supposing them to communicate with the same water head or supply. Yet, though few will be found to dispute so obvious a truth, to many it appears little better than a paradox to assert that, in a well 400 feet deep, the water might rise of its own accord to the surface. But this difficulty is owing to common observers not discriminating sufficiently. They see that in ordinary wells, from 20 to 80 feet deep, the water will seldom rise more than a few feet, and they infer the same of deeper wells, without taking into consideration the different levels at which their sources may be situated[2]. This is, in fact, nothing but the aversion to principles, or *theory* as it is called, which marks common men. What they have seen, handled and felt, that they know and believe; but their philosophy, which is eminently practical, goes no further. To take up a principle and pursue it into all the consequences to which it legitimately leads, always puzzles such men. In their ignorance they confound theory with hypothesis, and see in a principle nothing but the words in which it is announced:—to them it is dead and barren of all useful and practical results whatever.

That it is a very great advantage to a well to have this character of overflowing, is obvious, inasmuch as it obviates the trouble of raising the water by manual labour or mechanical contrivances. The supply is generally considerable, as the water must rise with proportionate force as the spring head is higher. At Cambridge, where they are common, they furnish about 12 gallons per minute. Such a supply would afford a very respectable mechanical power for the performance of many domestic operations—besides the great advantage of giving a stream of water always flowing; an advantage that can scarcely be valued too highly. But great as this advantage is, it is not the only one belonging to these deep-seated springs. It appears that the water is almost always of excellent quality, while those nearer the surface are not unfrequently contaminated to an objectionable degree. This renders it often adviseable to bore in search of a deep-seated spring, notwithstanding the expense attending the operation, even when the configuration of the surface, and arrangement of the strata, forbid the expectation that the spring will be an overflowing one. Thus in London, wherever a supply of particularly pure water is a desideratum,—as for instance in many of the breweries and distilleries,—wells have been sunk to a very great depth through the London clay, and the upper or land springs, as they have been singularly enough called, stopped out so as to prevent the contamination of the lower and purer springs. These latter, though not overflowing, generally rise, when first tapped, with such violence as scarcely to allow the well-digger to escape. They furnish a remarkably pure (or soft, as it is commonly called) water. The greater purity of these lower springs also renders them particularly worthy of notice in all situations where, from the proximity of the tide, the surface springs are liable to be influenced by it. Thus at Sheerness, the surface wells furnished a brackish water not fit for consumption, entailing on the government a considerable expense (upwards of £2000 per annum) in furnishing the garrison and dockyard with wholesome water, and, in particular, rendering the place untenable as a military position. The engineer appointed to consider of some method of obviating this latter objection, Sir Thomas Hyde Page, conceived the idea of excavating a well below the lowest level of the adjoining sea water, and of keeping out any infiltration of the tide by proper means. The difficulties he met with appear to have been great, particularly in passing through a quicksand of great thickness, but being supported by the King's approbation of the project, he eventually succeeded perfectly. At a depth of 328 feet, the clay which they had latterly been working in, showed a slight mixture of sand, which, with the moisture, was judged indicative of the spring being at hand. At 330 feet the whole bottom of the well blew up, and it was with some difficulty the workmen saved themselves from the torrents of water, and

[2] It had been supposed, that springs were mere veins of water of limited extent, and partial occurrence; but the many facts that have become known, regarding the influence of distant wells on each other, prove that the supply is much more extensive.

and sand, which rose upon them. In six hours the water rose 189 feet, and in a few days within 8 feet of the top of the well. It was found perfectly good and wholesome, and thus was saved an annual expense of £2000 a year to the government, by this bold and judicious experiment; and what was of more consequence, an important defence was rendered available, which till then had been of no use. The same officer succeeded in a similar undertaking at Harwich fort, where the surface springs were scanty in supply, and bad in quality. By cutting through a stratum of rock, he obtained a plentiful supply of excellent quality. In Alsace again, the want of springs was so remarkable, as to be a bar to the improvement of the country, till the French engineers thought of passing through a bed of clay 80 feet thick, which they supposed kept them from rising. They were rewarded by complete success. At Amsterdam the wells furnished only brackish water, till the same experiment was tried of sinking them to a depth of 232 feet, being far below the level of the Zuyder Zee, from which depth excellent water was procured. It appears from these particulars, that even in the most unpromising situations—even in the immediate vicinity of the sea, fresh and wholesome springs may be found; and this, as far as the experiments yet made enable us to pronounce, within a depth of 400 feet.

This conclusion, which I conceive is warranted by the preceding details, has a particular value for us of the city of palaces, where as none of your readers require to be told, we, *i. e.* the poorer part of us and the natives, labour much under the want of wholesome water. And even those of us who are able to make arrangements for ensuring a supply of rain water for the consumption of our table, would have no objection to a more plentiful supply obtainable without any trouble or arrangement whatsoever. The water of our wells is brackish to a very great degree, more particularly in the hot weather; and excepting a few tanks sparingly distributed through the town, there is no other supply of really wholesome water than what is obtainable from the heavens. Those who cannot afford the time, trouble, or expense of collecting the latter, are entirely dependent on the few tanks, which are said to furnish wholesome water; and the nearest of these may be, and often are, at such a distance, that the poorer classes are obliged to content themselves with the water of the first stagnant pool they meet with. These latter are mostly connected with the surface springs, and are consequently more or less brackish; but independent of this objection, they form such collections of vegetable putrefaction in every stage, and indeed of every abomination, that any one who has ever seen them, must lament that any human being should be compelled to resort to such places for an element of, to these people, indispensible and hourly use.

There is, probably, no other example in the whole world of a city of such importance and wealth as Calcutta being so ill supplied with this necessary of life. Placed on the verge of the tropic, and with every facility for such conveniences, we have neither fountains or baths. Even our puny effort to water about a mile of road, serves but to render us ridiculous for the waste of means in effecting so trifling a good, while it makes the want of such a refreshment in the other parts of the town so much the more palpable. But whatever may be thought of these deficiencies in a city with such a climate as Calcutta, every stranger must be struck with the existence of so important a want as that of good wholesome water for domestic consumption. Even our best tanks—such, for instance, as the *Lál Digi*—cannot be said to furnish pure and wholesome water. It is better, certainly, than the brackish water of our wells, but it is far from pure;—and every stranger who has occasion to use it, considers it, in fact, to be bad; as every one leaving Calcutta is sensible of the improvement of the water as he proceeds up the river.

Had we been a Roman colony, we should have had an aqueduct bringing an ample supply of good water from the nearest point where it is obtainable. It is the more discreditable to us that the resources of science rendering such an expense unnecessary, and placing within our reach a cheap and facile means of obtaining good water in any quantity, we will not avail ourselves of it. The facts connected with the question of deep-seated springs are, as already noticed, sufficently encouraging; but what is more to the purpose, the experiment has been ACTUALLY TRIED, and the result found such as to leave no doubt as to the existence of springs of fresh water at a depth of something more than 128 feet.

The late General Garstin many years ago paid particular attention to this subject; and though the method of obtaining overflowing springs was neither practised or understood at that time, yet it was well known that deep-seated springs furnished generally a purer water than the surface springs, and that the latter might be stopped out so as to prevent their intermixture. From the facts he had collected, he entertained a confident expectation that a supply of good water would be found here as it had been elsewhere, notwithstanding the brackishness of the surface springs. The Government authorised some experiments to be made on the subject, which, though not so conclusive as could have been wished, owing to the imperfect manner in which the art of boring was practised in this country, did yet establish, to the General's satisfaction, the fact of the existence of fresh-water springs at a considerable depth, and covered by a stratum so impervious to water, as to prevent the possibility of their mixing with the brackish surface springs. The facts, I think, fully bear out General Garstin's opinion, and justify the prosecution of a search after them at a time when good boring irons are in store in abundance, and the operation itself is so much better understood than it was at that time.

In December 1804, I believe the first attempt was made to penetrate to the lower springs. The depth attained on the 4th January 1805, was, however, only 75 feet, and the water proved to be brackish[3].

In August the same year they were resumed. In each case, immediately under the surface deposit, stiff tenacious clay was found, with brackish springs at from 60 to 80 feet; and below those, at a depth of 128 feet, hard rock, as was evident from the sharp edge and decided polish given to the augur, an effect never resulting from the friction of clays, however tenacious. The irons being made in Calcutta, and the screws of an inferior quality, they broke at this stage of the experiment, and 41 feet of rods, which could not be extracted, were left fixed in the bore. Sufficient evidence, however, was obtained of the nature of this hard bottom being calcareous. A third attempt being made, the depth attained was only 55 feet; in a fourth 59; and in a fifth 65. In a sixth experiment, continued from December of the same year to February 1806, they penetrated to 127 feet; when, the rods breaking, 91 feet of them remained in the bore. Here also, it was evident, the rock had been attained. The water afterwards rose within five feet of the surface. In a seventh trial, the irons again broke at a depth of 91 feet. The eighth was commenced on the 15th March 1806, and on the 29th of May he had penetrated to a depth of 127½ feet. Here he came upon the usual hard stratum, the augur being sharpened like a knife. Another tool, with a triangular point was applied to the irons, and an attempt made to penetrate the rock. Small bits were brought up, which effervesced with acids, but apparently the method of getting through solid rock[4] was not very well understood at that time, and the operation was at last abandoned as hopeless. Water also rose in this hole and was well tasted.

It is to be remarked, that in all these bores water rose within a few feet of the surface. Was this the effect of the deep seated springs? and if so, how did they penetrate the solid rock. The only way I can account for this curious result is, that this rocky or hard bottom appeared to be more of the nature of *cancar* than of really solid, and continuous rock. Now we know this substance is generally in nodules or kernels imbedded in a stratum of far inferior tenacity to that of even indifferent clay; and that something of this kind was the case, is further evident from the fact of its always being found more or less moist, while the superincumbent clay was always perfectly dry throughout the greater part of its thickness. A still more singular fact is, that the water remained at a considerable height during the hot months, when the neighbouring wells, fed by the surface springs, were at a very low ebb, and on one occasion of a fire, when they were all drawn perfectly dry, the water in the bores maintained a high level. It is, I think, difficult to account for these facts, except on the supposition of their being fed by the deep seated spring rising through the *cancar* stratum, and prevented escaping laterally by the clay which reaches within a few feet of the surface.

General Garstin soon after relinquishing the above attempts, returned to England, but so satisfied was he of the grounds they afforded for a confident expectation of finding water, that he proposed making every exertion when there to obtain efficient means for prosecuting the experiment to a more successful issue. His death soon after his return to this country was to be regretted, inasmuch as there is little doubt that he would have eventually succeeded in demonstrating to the people of Calcutta, the practicability of obtaining ample supplies of good and wholesome water.

His exertions had proved, however, so far useful, that a new and effective ap-

[3] For a more particular detail of these operations, see vol. I. p. 167 *et seq.*

[4] This is done by a tool something like a large chisel attached to the end of the rods, which are suspended in such a manner, that a single workman can give them the proper vibratory motion, by which the rock is gradually chipped away, and afterwards the loose pieces removed by the augur.

paratus had been received from England, which, with the substitution of Europe for country rope, and of superior purchases for managing the rods, occasioned the next operation, or eighth in order from the commencement, to be prosecuted with superior facility. It began on the 19th May, and terminated July 15th, owing to the heavy rains. On the 15th November the work was resumed, and on the 11th January 1815, they had penetrated to a depth of 136 feet, or 8 feet below General Garstin's limit. Beyond this they were unable to proceed, as the gin and ropes both gave way in attempting the removal of the borer, which was, therefore, left in the ground. In 1820 a ninth and tenth attempt were made. In this case also, after penetrating to 122½ and 128 feet, the rods broke and were left in the ground. These two operations complete the history, I believe, of the efforts yet made to obtain fresh springs by boring. The greatest depth attained was 136 feet, and even this depth was not attained without great efforts, and considerable sacrifices. When we compare this result with what has been done at home, where it is not uncommon to hear of depths of 500 and even of 600 feet being attained, we shall be satisfied that the experiment has only failed (if it can be said to have failed) from the non-application of proper means, whether of knowledge or of skill. That with such encouraging appearances nothing should have been done since 1820, up to the present time, to bring the resources of European art to bear on this interesting question, is only one of those circumstances so common in India as to excite little or no surprise.

Recently, however, as is well known to your readers, a gentleman has brought the subject to the notice of the Asiatic Society, and has persuaded them to resume once more the search after deep-seated springs of fresh water. Government has consented to place at their disposal the necessary means, such as boring irons, purchases, &c. The Officer commanding in Fort William has, with the permission of the authorities, permitted the attendance of some men of the 13th, whose employment in England as miners fitted them for giving useful assistance. Several gentlemen of the Society have come forward on this occasion to give their co-operation, and a certain sum has been placed at their disposal by the Society, apparently sufficient at least to establish satisfactory grounds on which a further advance may be made. I think, therefore, there is little doubt of the experiment now succeeding, provided those who have undertaken the management will but persevere. They must have an opinion, however, of their own, and not allow themselves to be frightened by unfounded objections, or their ardour damped by irrational and timid comments. Little of the preceding details is generally known, and still less is the general subject of overflowing or deep-seated springs understood;—the consequence is, that already much has been said in the way of opposition that will not bear examination. Some objectors (for objectors will never be wanting even to the best planned and wisest scheme) say, that as there are no hills near Calcutta, the idea of deep-seated springs is a chimæra. But I would ask such objectors, where were the hills in Alsace, which is a plain tract of considerable extent. Where, again, were they at Amsterdam? And if high ground was to be found in the neighbourhood of Sheerness, still the sea was much nearer. But in truth the whole question is one of levels; and here the difference, 140 feet say at most, is not so very great as to require a very lofty mountain for its supply. The truth is, that a rise of 140 feet will be found in more directions than one at no great distance from Calcutta. Even were it otherwise, there would still be no valid reason to doubt the eventual success of the undertaking; for it no more follows that the spring should be superficial at the effective head, than that it should be so here. This is obvious. What, in fact, is to prevent a spring 140 feet deep, at any moderate distance round Calcutta, from finding its way here under the clay? And if there be no difficulty, what becomes of the objection of there being no hilly ground sufficiently near Calcutta to furnish an adequate supply? It is indeed almost self-evident, that go but deep enough, and you must have springs, let the locality be what it may;—the only question is, to what depth it is worth while to sink in the search after good water. Now this is a question that must always lie determined by circumstances. Those under which the present experiment has been resumed are eminently sufficient to justify the promoters of it:—1st, the certainty, I may say, of finding good water at the inconsiderable depth of 130 or 140 feet; and 2nd, the acknowledged want of so important a necessary as good water. Let us hope then, that none will be so inconsiderate as to throw any obstacles in the way of a project so entirely calculated for the benefit of the public. Whether it succeed or fail, must be determined by the general feeling for or against the project. If supported by the good wishes of the public, whom it is meant to benefit, it must succeed. There is no want in Calcutta of means or of talent to apply them, nor would there be of funds if they were required—I mean, supposing people generally desirous of seeing the thing succeed. But if we allow ourselves, as happens to most of our Indian projects, to be talked into apathy and indifference, of which we have, surely a sufficient quantity already, the usual result will of course follow,—of disgust in those who had undertaken the conduct of the matter, and the consequent abandonment of the undertaking. That this may not be the case, I think most of your readers will join with me in most heartily wishing, as well for our own sakes, as in justice to those public-spirited individuals to whose exertions we owe, that the experiment has been attempted at all. To them the public are at all events much indebted: if they succeed, they may reckon with certainty on the gratitude of their fellow citizens; while even if they fail, they will at least be allowed the credit of having engaged in an undertaking which had every rational prospect of success, requiring but a small expenditure of means for its prosecution, yet calculated, in the event of being successful, eminently to benefit the public. But to my hypothetical "IF THEY FAIL;" they might perhaps well reply in the words of Lady Macbeth,

> "But screw your courage to the sticking point,
> And WE'LL NOT FAIL."

It is certain, that to anticipate failure, and to urge ill considered and frivolous objections, is the way to insure the event professed to be deprecated; while a confiding and generous spirit of encouragement and sympathy, must, on the contrary, command success, if it be possible.

C. J.

III.—*On the Excavation of Vallies, as illustrated by the Volcanic Rocks of Central France. By* Charles Lyell, *and* R. I. Murchison, *Esqs.*

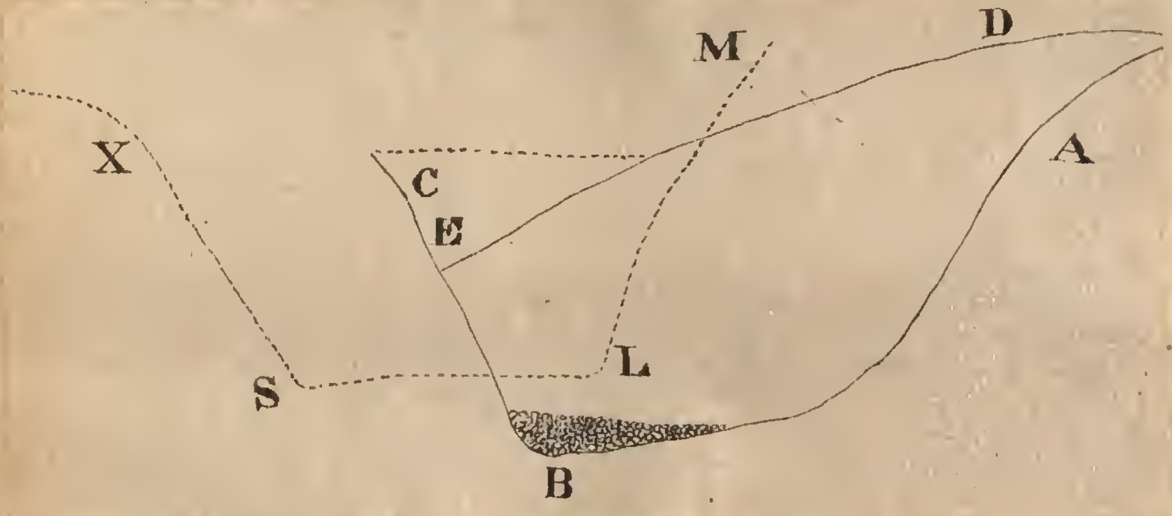
[From the Scotsman Newspaper.]

This is a curious paper, and throws considerable light on some scientific questions which have been the subject of much speculation. It has, however, one material fault. Partly from the intricacy of the matters discussed, and the want of a sufficient number of illustrative drawings, and partly from the inattention of the writers, it is greatly deficient in clearness. This must have been, we think, in part the consequence of inattention, for Mr. Lyell's excellent paper on the Geology of Forfarshire, shews that he can write with great perspicuity on such subjects. The present paper is long, and were we to attempt to follow the authors into their details, we would soon leave our readers behind us. We shall, therefore, confine ourselves to a general view of the conclusions to which the facts seem to lead, so far as we can comprehend them.

There are two opinions respecting the excavation of vallies. One regards them as produced by the erosive action of the streams which we now see flowing in them, continued through a long lapse of ages, and aided by rains, frost, and heat; the other attributes them to the agency of a temporary deluge sweeping over the surface with powerful currents. The first opinion is that which would most readily occur to any observer; and the second has been resorted to, chiefly to escape from the difficulty arising from the prodigious length of time which appears to be necessary, to enable a cause operating so slowly, to work such mighty changes. There are two strong reasons, however, which militate against the supposition, that diluvial currents have hollowed out vallies, at least the vallies in mountainous districts. First, these vallies are generally shut at their upper extremities, which they would not have been had they owed their existence to diluvial currents; and secondly, the vallies in each district, generally speaking, bear a certain proportion in magnitude to the streams flowing in them. Thus the valley of the main trunk of the stream is always larger than that of its primary branches, and the vallies of the primary branches are larger than those of the secondary. The direction too of these branches, which always forms a considerable angle, often a right angle, with that of the main stream, is not easily reconciled with the supposed agency of a deluge. Thus a series of the great waves, or an oceanic current, might groove out the bed of the Garry ; but the same current could not excavate the bed of the Tilt, at right angles to the other. We believe, indeed, that on this subject geologists, with few exceptions, are now disposed to adopt the opinions so beautifully developed by Playfair, in his illustrations of the Huttonian Theory, and which derive no small confirmation from the researches of the two writers now before us.

It is perhaps unknown to some of our readers, that there are about three hundred volcanoes in the south of France, not such volcanoes as Arthur's Seat and North Berwick Law presented to the eye of the fanciful Faujas St. Fond, but real igneous cones, in general with open craters in their summits, surrounded by masses of slags, pumice, and ashes, and with long currents of perfect lava on their sides,—volcanoes, in short, differing in no essential circumstance from Etna or Vesuvias, when in a state of quiescence. History is silent as to their eruptions ; but this only shows that the period of their activity must have been very remote ; for that they were once active, and that a long period elapsed between the first and last of their eruptions, is attested by facts which cannot be mistaken. Having premised these remarks, we shall endeavour to make the reader acquainted with the phenomena which Messrs. Lyell and Murchison describe in detail, and make the groundwork of their conclusions.

Some of the streams of ancient lava in Auvergne have been traced over an extent of thirty miles or more. As the lava in its progress, like any other fluid body, seeks the lowest levels, it necessarily flowed into the beds of the rivers. In this way it sometimes formed barriers across their courses, or filled up their channels for several miles ; a lake was consequently formed, whose waters rose till they sought out some lateral channel, or flowed over the mound of lava which had arrested their progress. There are rivers in Auvergne whose courses have been entirely changed in this way. But it is a curious fact, which is confirmed by what is observed in active volcanoes, that currents of lava do not flow with a level surface like streams of water, but swell up in the middle, and have their sides often sloping at an angle of 30 or 40 degrees. Let us now suppose that the line A B C



represents a transverse section of a cavity cut in the primitive rock (gneiss) by a stream of water, which flows over a bed of gravel at the bottom B. A "coulee" or current of lava flows into this cavity from the right, and fills it up to D E, which represents the curved surface of the lava. The water stopped by the irruption will rise till it escape through the triangular opening at E. formed by the sloping side of the current of lava M E, and the original bank of primitive rock E C. So long as the old part of the river's bed *above* the intruding lava forms a lake, all the sand, gravel, and stones, which are the great engines of attrition, are detained by it, and the limpid water which escapes, makes little or no impression on the rock. But when these travelled materials, having filled up the lake, begin to be rolled over the new barrier, the erosive action commences ; and as it goes on, the lava D E is worn away on the one side, and the gneiss E C on the other, till a new channel M L S X is worked out exactly like the old, with a stratum of water-worn pebbles at the bottom. We may now conceive a second irruption of lava to take place from the same side ; the waters again collect ; a lake is formed, and afterwards filled up ; and a third channel is scooped out, higher and farther to the left than M L S X. Auvergne abounds in examples of such changes as we have now described ; and we are thus enabled to understand how it often happens there, that one side of a ravine or river course consists of volcanic basalt or breccia, and the other of the primitive or tertiary rocks of the district.

We have supposed two, three, or four successive river channels to exist above each other in this way. But the erosive power of a stream varies with the height from which it falls, and the greater hardness or softness of the rock subjected to its action. The volcanic basalt, by filling up a large portion of the channel, may transfer a fall or cascade some miles farther down the stream, to a place perhaps where the gentleness of the slope formerly rendered the erosive action extremely small. A powerful agent thus operating at a new spot, may quarry out the latest bed of the stream to a depth greater than that of the oldest. It is in this way that the secret of these channels beneath channels has been brought to light; the old beds of rolled pebbles and sand which mark the earlier channels, with the gneiss below and the basalt above them, being exposed at some places on the side of the existing river courses, at the height of 60 or 70 feet above the present surface of the water.

Messrs. Lyell and Murchison were able to distinguish four orders or series of river beds of this description, not all in visible connection at one locality, but all sufficiently marked by characteristic circumstances. The distinction was ascertained in more ways than one, but chiefly by the nature of the gravel or rolled pebbles, or bodies inclosed in it, which pointed out the ancient course. Thus one bed of pebbles, 6 or 8 feet thick, was found, consisting entirely of fragments of primitive or tertiary rocks, without a single bit of basalt or breccia, shewing that this very ancient water course had been excavated before the neighbouring cones had commenced their eruptions. Another bed was found, containing some pieces of basalt, but only of that species which is known from its composition to belong to the oldest products of these volcanoes. A third bed contained newer volcanic materials, and was distinguished by including bones of the extinct quadrupeds found elsewhere in the alluvial soil. The fourth bed was that of the present stream.

Now, even the newest of these channels have in some instance, cut their way down 150 or 200 feet beneath the natural surface of the rock. The older ones had, in all probability, produced excavations equally great, though not always at the same spots. We have farther proof of the great lapse of time between one eruption and another, in a fact mentioned by M. Scrope, that the older lava had in some cases acquired a surface of soil, which had been covered with forests when the newer eruptions took place, as attested by the remains of wood found in the *tuff* or agglutinated volcanic ashes.

Let us now consider what are the inferences to which these facts lead. *First*, we might perhaps account for the scooping out of an original valley in the primitive rock, by the agency of the Deluge; but when we find this original valley three times filled up, and three times successively excavated, and have evidence otherwise, that long periods elapsed between each of these operations, it is plainly absurd to suppose that these excavations, divided by long periods of time, were produced by *one* deluge of temporary duration. Mr. Buckland's argument, too, we must remember, if it proves that there was a deluge, is quite as strong to prove that there was but *one*. It is obvious then that the erosive power of streams has cut out vallies of great depth, through rocks of a very durable kind: it has, in short, performed the work which some writers have referred to the agency of a flood, simply because they were unwilling to admit that the streams themselves could effect in any supposable length of time, what we find they have actually accomplished within a period which may be called *extremely limited*, compared with the lapse of ages embraced by the calendar of geology.

But a query occurs, whether those volcanic eruptions and the rocks they produced, are postdiluvian or antediluvian. Now from the lapse of time required to effect the changes we have described, some or all of the eruptions ought to be antediluvian; but what is remarkable, not the slightest trace of diluvial action is to be found upon the volcanic mountains or vallies of Auvergne. So at least say Messrs. Lyell and Murchison, both upon their own authority, and that of other scientific observers. Mr Scrope observes, that the undisturbed and perfect state of the cone of *loose* scoriæ whence the lava of Chaluzet flowed, demonstrates that no denuding wave has passed over the spot since the eruption took place. Messrs. Lyell and Murchison also remark, that the upper part of the lava currents retain their original asperities, and are no where strewed over with sand or pebbles, and that no traces are found on the wide basaltic plateaux of boulders or travelled fragments from the east, the west, or any point of the compass.

But the singular and puzzling fact remains to be mentioned. The bones of the Mastodon, Elephant, Rhinoceros, Hippopotamus, Tapir, Bear, Hyæna, Stag, &c. which have been found in caves, and in the alluvial soil in many parts of Europe and Asia, have been discovered in alluvial gravel, covered by a very ancient Trachitic breccia at Mont Perrier. These animals have disappeared from the earth, (for they are all of extinct species,) and are supposed to have been destroyed by the deluge; yet here we find their bones covered by, or rather inclosed among volcanic rocks which must be regarded as postdiluvian, for the reasons already mentioned. The animals must, of course, have lived in the neighbourhood at the time when these volcanic rocks were deposited on the surface of the country. Shall we then infer that the deluge was only partial, and did not visit Auvergne? Or shall we suppose that these animals lived after it, and were destroyed by some other catastrophe? Shall we also suppose that all the changes we have described have taken place within 5000 years? We do not pretend to see our way at present through these difficulties. Geology presents many problems which cannot be solved at the first glance; but the proofs of a general deluge are so numerous and strong, that we have no doubt of means being ultimately found to reconcile the facts we have mentioned with its existence at or near the period commonly assigned to it.

IV.—*On the Climate of the North-Western Mountains.*

A general statement of the Weather for February, 1829.

Clear,	10 days
Fair, but cloudy and partially cloudy,	5 ditto
Rainy, stormy, snow and hail,	13 ditto
Thunder,	5 ditto

Clear, on the 1st, 3d, 4th, 5th, 6th, 9th, 10th, 15th, 16th, and 28th.

Fair, but cloudy and partially cloudy, on the 7th, 8th, 11th, 18th, and 26th.

Rainy, stormy, snow and hail, on the 2d, 12th, 13th, 14th, 17th, 19th, 20th, 21st, 22d, 23d, 24th, 25th, and 27th.

Thunder, on the 14th, 21st, 23d, 26th, and 27th.

Height of the Barometer.

	Inches	Th.
Mean maximum for the month,	22,828	51,2
Mean minimum,	22,767	45,2
Mean of the daily means,	22,797	48,1
Greatest altitude, on the 28th, at 10 A. M.	22,924	52,5
Least ditto, on the 13th, at 4 P. M.	22,650	45,

Temperature of the air.		*Temperature of the house.*	
Mean maximum,	44,2	Mean maximum,	40,5
Mean minimum,	33,2	Mean minimum,	37,4
Mean of the daily means,	38,6	Mean of the daily means,	38,9
Greatest, on the 26th, at 2 P. M.	51,5	Great. on the 19th, at sunset,	44,8
Least, on the 15th, at sunrise,	26,2	Least, on the 16th, at sunrise,	32,3
Mean,	38,8	Mean,	38,5

Hygrometrical state of the Air.

Leslie's Hygrometer, greatest, on the 9th and 10th: on the former day at 2 P. M., and on the latter at noon,	44
Ditto ditto, least, on the 13th and 24th: on the former day at 3 P. M., and on the latter at 10 A. M.	4
Kater's ditto, greatest, on the 23d, at 1 P. M.	736
Ditto ditto, least, on the 11th, at 4 15 P. M.	197

Statement of the Winds, shewing their direction and force during February, 1829.

West, on the 1st, 4th, 6th, 7th, 10th, 11th, 13th, 15th, 20th, 21st, and 22d,	little	11 days
Ditto, on the 8th, 9th, 14th, and 28th,	gentle	4 ditto

East-north-east, on the 2d, 18th, 19th, and 24th,	light	4 days
Ditto, on the 12th and 26th,	little	2 ditto
Ditto, on the 16th and 17th,	gentle	2 ditto
North-west, on the 3d and 5th,	little	2 ditto
East, on the 23d,	ditto	1 ditto
South-south-west, on the 25th,	light	1 ditto
North-east, on the 27th,	gentle	1 ditto

A general statement of the Weather for March, 1829.

Clear,	18 days
Fair, but cloudy and partially cloudy,	4 ditto
Rainy, stormy, snow and hail,	9 ditto
Thunder,	4 ditto

Clear, on the 3d, 4th, 5th, 8th, 9th, 10th, 11th, 13th, 14th, 15th, 16th, 17th, 18th, 19th, 20th, 26th, 27th, and 28th.

Fair, but cloudy and partially cloudy, on the 7th, 12th, 21st, and 25th.

Rainy, stormy, snow and hail, on the 1st, 2d, 6th, 22d, 23d, 24th, 29th, 30th, and 31st.

Thunder, on the 22d, 23d, 25th, and 31st.

Height of the Barometer.

	Inches.	Th.
Mean maximum for the month,	22,927	58,1
Mean minimum,	22,857	51,2
Mean of the daily means,	22,892	54,6
Greatest altitude, on the 13th, at 10 A. M.	23,046	62,7
Least ditto, on the 2d, at 3 P. M.	22,722	44,0

Temperature of the air.		*Temperature of the house.*	
Mean maximum,	56,7	Mean maximum,	50,6
Mean minimum,	42,9	Mean minimum,	46,0
Mean of the daily means,	49,8	Mean of the daily means,	48,3
Greatest, on the 21st, at 1 P.M.	65,1	Greatest, on the 17th and 20th: on the former day, at 1, 3, and 4, 45 P. M. and the latter day at 4, 45 P. M.	56,7
Least, on the 3rd, at sunrise,	30,8	Least, on the 3d at sunrise, and 8 A.M.	35,3
Mean,	47,9	Mean,	46,

Hygrometrical state of the Air.

Leslie's Hygrometer, greatest altitude, on the 17th, at noon,	96
Ditto ditto, least ditto, on the 4th, at sunrise and 10 P. M.,	3
Kater's ditto, greatest ditto, on the 2nd at sunset,	706
Ditto ditto, least ditto, on the 17th at 3 P. M.,	108

Statement of the Winds, shewing their direction and force during March, 1829.

East-north-east, on the 1st, 3rd, 4th, 7th, 11th, 15th, 16th, 17th, 22d, 23rd, 25th, 28th, and 30th,	gentle	13 days
Ditto, on the 5th, 13th, 24th, and 31st,	little	4 ditto
Ditto, on the 9th and 14th,	moderate	2 ditto
South-west, on the 2nd,	little	1 day
West, on the 6th and 21st,	ditto	2 days
Ditto, on the 8th, 18th, 19th, 20th, 26th, 27th, and 29th,	gentle	7 ditto
Ditto, on the 12th,	moderate	1 day.
North-east, on the 10th,	gentle	1 ditto

P. G.

Remarks by the Editor.—Our correspondent has doubtless observed the discrepancy between Kater's and Leslie's Hygrometers. It is evident that *both* cannot be right, and it is nearly certain that neither are. The error in the graduation of Leslie has already been pointed out in our number for January, p. 24. As to Kater's, it is, we fear, liable to the objections made against every hygrometer formed of an organic substance, irregularity of scale, and eventually impaired action. The moist bulb thermometer is every way preferable to either of them.

V.—*On the Scientific Characters of the late Sir H. Davy and Dr. W. H. Woollaston.*

[From Mr. Babbage's work on the Decline of Science in England.]

In a work on the Decline of Science, at a period when England has so recently lost two of its brightest ornaments, I should hardly be excused if I omitted to devote a few words to the names of Woollaston and of Davy. Until the warm feelings of surviving kindred and admiring friends shall be cold as the grave from which remembrance vainly recalls their cherished forms, invested with all the life and energy of recent existence, the volumes of their biography must be sealed. Their cotemporaries can expect only to read their eloge.

In habits of intercourse with both those distinguished individuals, sufficiently frequent to mark the curiously different structure of their minds, I was yet not on such terms even with him I most esteemed, as to view his great qualities through that medium which is rarely penetrated by the eye of long and very intimate friendship.

Caution and precision were the predominant features of the character of Woollaston, and those who are disposed to reduce the number of principles, would perhaps justly trace the precision which adorned his philosophical to the extreme caution which pervaded his moral character. It may indeed be questioned whether the latter quality will not in all persons of great abilities produce the former.

Ambition constituted a far larger ingredient in the character of Davy, and with the daring hand of genius he grasped even the remotest conclusions to which a theory led him. He seemed to think invention a more common attribute than it really is, and hastened as soon as he was in possession of a new fact or a new principle, to communicate it to the world, doubtful perhaps lest he might not be anticipated; but confident in his own powers, he was content to give to others a chance of reaping some part of that harvest, the largest portion of which he knew must still fall to his share.

Dr. Woollaston, on the other hand, appreciated more truly the rarity of the inventive faculty; and undeterred by the fear of being anticipated when he had contrived a new instrument, or detected a new principle, he brought all the information that he could collect from others, or which arose from his own reflection, to bear upon it for years, before he delivered it to the world.

The most singular characteristic of Woollaston's mind was the plain and distinct line which separated what he knew from what he did not know; and this again arising from his precision, might be traced to caution.

It would, however, have been visible to such an extent in few except himself, for there were very few so perfectly free from vanity and affectation. To this circumstance may be attributed a peculiarity of manner in the mode in which he communicated information to those who sought it from him, which was to many extremely disagreeable. He usually, by a few questions, ascertained precisely how much the enquirer knew upon the subject, or the exact point at which his ignorance commenced; a process not very agreeable to the variety of mankind: taking up the subject at this point, he would then very clearly and shortly explain it.

His acquaintance with mathematics was very limited. Many years since when I was an unsuccessful candidate for a professorship of mathematics, I applied to Dr. W. for a recommendation; he declined it on the ground of its not being his pursuit. I told him I asked it, because I thought it would have weight; to which he replied, it ought to have none whatever. There is no doubt his view was the just one. Yet such is the state of ignorance which exists on these subjects, that I have several times heard him mentioned as one of the greatest mathematicians of the age[1]. But in this as in all other points, the precision with which he comprehended and retained all he had ever learned, especially of the elementary applications of mathematics to physics, was such, that he possessed greater command over those subjects than many of far more extensive knowledge.

In associating with Woollaston you perceived that the predominating principle was to avoid error; in the society of Davy you saw that it was the desire to see and make known truth. Woollaston never could have been a poet; Davy might have been a great one.

[1] This of course could only have happened in England.

A question which I put successively to each of these distinguished philosophers, will show how very differently a subject may be viewed by minds of even the highest order.

About the time Mr. Perkins was making his experiments on the compression of water, I was much struck with the mechanical means he had brought to bear on the subject, and was speculating on other applications of it, which I will presently mention.

Meeting Dr. Woollaston one morning in the shop of a bookseller, I proposed this question: If two volumes of hydrogen and one of oxygen are mixed together in a vessel, and if by mechanical pressure they can be so condensed as to become of the same specific gravity as water, will the gases, under these circumstances, unite (combine) and form water? "What do you think they will do?" said Dr. W. I replied that I should rather expect they would unite. "I see no reason to suppose it," said he. I then enquired whether he thought the experiment worth making. He answered that he did not, for that he should think it would certainly *not* succeed.

A few days after, I proposed the same question to Sir H. Davy. He at once said "they will become water of course;" and on my inquiring whether he thought the experiment worth making, he observed that it was a good experiment, but one which it was hardly necessary to make, as it must succeed.

These were offhand answers, which it might perhaps be hardly fair to have recorded, had they been of a person of less eminent talent; and it adds to the curiosity of the circumstance to mention, that I believe Dr. Woollaston's reason for supposing no union would take place, arose from the nature of the electrical relations of the two gases remaining unchanged; an objection which did not weigh with the philosopher whose discoveries had given birth to it.

[The result of the experiment appeared, and still appears to me, to be of the highest importance; and I will shortly state the views with which it was connected. The next great discovery in chemistry to definite proportions will be to find means of forming all the simple unions of one atom with one, with two, or with more of any other substance; and it occurred to me that the gaseous bodies presented the fairest chance of success; and that, if wishing, for instance, to unite four atoms of substance with one of another, we could, by mechanical means, reduce the mixed gases to the same specific gravity as the substance would possess which resulted from their union; then either that such union would actually take place, or the particles of the two substances would be most favourably situated for the action of caloric, electricity, or other causes, to produce the combination. It would indeed seem to follow, that if combination should take place under such circumstances, then the most probable proportion in which the atoms should unite, should be that which furnished a fluid of the least specific gravity: but until the experiments are made, it is by no means certain, that other combinations might not be produced.]

The singular minuteness of the particles of bodies submitted by Dr. Woollaston to chemical analysis, has excited the admiration of all those who have had the good fortune to witness his experiments; and the methods he employed deserve to be much more widely known.

It appears to me that a great mistake exists on the subject. It has been adduced as one of those facts which prove the extraordinary acuteness of the bodily senses of the individual,—a circumstance which, if it were true, would add but little to his philosophic character; I am however inclined to view it in a far different light, and to see in it one of the natural results of the admirable precision of his knowledge.

During the many opportunities I have enjoyed of seeing his minute experiments, I remember but one[2] instance in which I noticed any remarkable difference in the acuteness of his bodily faculties, either of his hearing, his sight, or of his sense of smell, from those of other persons who possessed them in a good degree. He never showed me an almost microscopic wire, which was visible to his and invisible to my own eye: even in the beautiful experiments he made relative to sounds inaudible to certain ears, he never produced a tone which was unheard by mine, although sensible to his ear; and I believe this will be found to have been the case by most of those whose minds had been much accustomed to experimental enquiries, and who possessed their faculties unimpaired by illness or by age.

It was a much more valuable property on which the success of such inquiries depended. It arose from the perfect attention which he could command, and

[2] This was at Mr. South's observatory and the object.

the minute precision with which he examined every object. A striking illustration of the fact that an object is frequently not seen, *from not knowing how to see it*, rather than from any defect in the organ of vision, occurred to me some years since, when on a visit to Slough. Conversing with Mr. Herschel, on the dark lines seen in the solar spectrum by Franenhofer, he inquired whether I had seen them; and on my replying in the negative, and expressing a great desire to see them, he mentioned the extreme difficulty he had had even with Franenhofer's description in his hand, and the long time which it had cost him in detecting them. My friend then added, I will prepare the apparatus, and put you in such a position that they shall be visible, and yet you shall look for them, and not find them: after which, while you remain in the same position, I will instruct you *how to see them*, and you shall see them, and not merely wonder you did not see them before, but you shall find it impossible to look at the spectrum without seeing them.

On looking as I was directed, notwithstanding the previous warning, I did *not* see them; and after some time I enquired how they might be seen, when the prediction of Mr. Herschel was completely fulfilled.

It was this attention to minute phenomena which Dr. Woollaston applied with such powerful effect to chemistry. In the ordinary cases of precipitation the cloudiness is visible in a single drop as in a gallon of a solution, and in those cases where the cloudiness is so slight as to require a mass of fluid to render it visible, previous evaporation, quickly performed on slips of window glass, rendered the solution more concentrated.

The true value of this minute chemistry arises from its cheapness, and the extreme rapidity with which it can be accomplished: it may, in hands like those of Woollaston, be used for discovery, but not for measure. I have thought it more necessary to place this subject on what I consider its true grounds, for two reasons. In the first place, I feel that injustice has been done to a distinguished philosopher, in attributing to some of his bodily, that excellence which I think is proved to have depended on the admirable training of his, intellectual faculties. And in the next place, if I have established the fact, whilst it affords no better means of judging of such observations as lay claim to an accuracy *more than human*, it also opens to the patient inquirer into truth, a path by which he may acquire powers that he would otherwise have thought were only the gift of nature to a favoured few.

VI.—*On the Strength of Tied Beams.*

Having, in the latter end of 1828, been led to make a few experiments on models of large beams, fitted with iron ties, with a view to ascertain the comparative economy and utility of the system in this country, for buildings of large dimension, where the common trussed roof would probably be inconvenient and more costly, I am induced to offer you the results for publication in the GLEANINGS. It may be scarcely necessary to add, that this is but a simple modification of the suspension principle with iron; and that it has been successfully introduced in Great Britain, both for roofing, and in the construction of bridges.

No. 1. A model of a saul beam, 6¾ feet long, bearing distance 6½ feet, and 1 inch square, having been prepared, it was, in order to ascertain its deflections with different weights, first tried without any artificial tie; and it gave as follows, viz.

lbs.	oz.	inches.
2	11	0,1
8	5	0,4
14	2	0,65
20	15	0,9
26	12	1,175
29	12	1,325

The breaking weight of this specimen, unsupported, about 123 lbs.

This specimen was taken down; and a small chain of about ¼ inch diameter fitted to it, by iron straps at the ends. Three supports or small blocks of wood at about equal intervals were applied between the chain and beam, and the deflections noted as follows.

lbs.	inches.
50	0,05
102	,2

lbs.	inches.
154	,4
206	,7
258	1,0
310	1,4
362	1,8
424	2,65

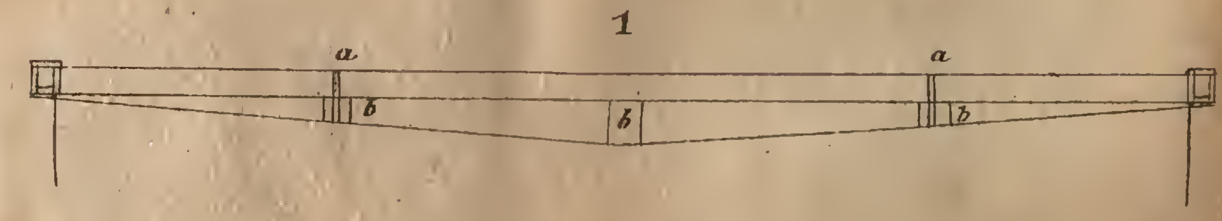
One of the bolts of the chain broke with the last weight; nearly all of them appeared bent by the strain, so much so, as considerably to increase the length of the chain, and thus deprive the beam of its support; otherwise no doubt it would have borne nearly, if not quite, 200 lbs. additional weight.

No. 2. Another saul specimen, 1½ inches square, and 6½ feet long, prepared as a model of a 52 feet beam, 12 inches scantling, scale ⅛th of the full size, gave, without iron, the following deflections:

50 lbs.	,6 inches.
108	1,4

would have broken with about 415 lbs.

1st. A flat bar of iron 1½ × ½ inches was then fitted on, doubled round the ends, secured by small iron straps, and supported as above, by three small blocks of wood (*b b b*) between the beam and iron bar. (Fig. 1.) The following deflections were now observed.



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lbs.	inches.
143	,15
400	,5
700	,95
1000	1,375
1350	2,0
1400	2,2
1457	2,4
1515	3,0

The lateral curvature here began to increase so rapidly, that the blocks of wood were almost forced out of their places; the specimen was, therefore, taken down, in order that they might be properly secured by 2 small straps (*a a*) passing tightly over them, at about ⅓d the length of the beam ; 5 blocks were also applied instead of three, as at first, viz. centre, 2 feet 7 inches ; 2 next, 2 feet 5 inches ; two end ones, 1 foot 5 inches. (Fig. 2.) The deflections were as follows:

lbs.	inches.
150	,125
400	,35
700	,6
1000	,9
1350	1,2
1500	1,4
1600	1,5
1700	1,65
1900	2,0
1961	2,4

Broke near one of the fulcrums.

No. 3. A specimen of similar dimension to the last, but scarfed in the centre, the joint being 6 inches in length, and secured by 2 small straps, was then put to the test, the scarf being placed vertically. This was similarly prepared with tie and blocks as on the last occasion, but 4 straps were intermediately applied instead of 2 between the fulcra, and the specimen was, on the whole, more carefully secured than the entire pieces. (Fig. 3 and 4.)

lbs.	inches.
150	,1
300	,225
450	,375
600	,5
1000	,8
1323	1,05
1500	1,175
1728	1,375
1900	1,525
2135	2,3

Broke at the joint, in consequence of the splitting of the supporting block.

No 4. Prepared similarly to No. 3, broke near the scarf, but clear of it, under a weight of lbs. 2147, deflection 2,15 inches; results so nearly resembling those detailed for No. 3, as to render insertion unnecessary.

In almost all the preceding cases, great nicety in fitting on the parts was not attempted ; doubtless the resistance would, with greater care, have been greatly increased : yet these experiments, so far as they go, tend to show the power of resistance, increased about 400 per cent. by the application of the iron ; a beam which by itself would certainly have yielded under a weight of 415 lbs. having successively sustained 1515 lbs., 1900 lbs., 2130 and 2140 lbs. From the above few experiments, the following conclusions may, I think, be drawn.

1st. That security, as well as simplicity and economy, are likely to result from the adoption of this principle in large roofs ; which, when constructed with a considerable pitch, expose so great a surface to the effects of heavy winds, as to render their stability somewhat precarious, without a heavy expenditure of timber.

2d. That if the iron tie be properly fitted, there should be no deflection whilst it continues perfect, the tendency to bend vertically being prevented by the supporting tie, whilst lateral motion is precluded, by the arrangement of the *burgahs*; hence the peculiar applicability of this system for terraced roofs of large spans, by the security it gives against cracks, and the facility of using small sized timber.

3rd. With beams loaded in the middle, it appears expedient to fill up, with slight materials, the whole of the space between the beam and its tie, in order to render it perfectly stiff. This precaution is obviously less necessary in cases of uniform load.

VII.—*On the Rent and Produce of Land.*

To the Editor of Gleanings in Science.

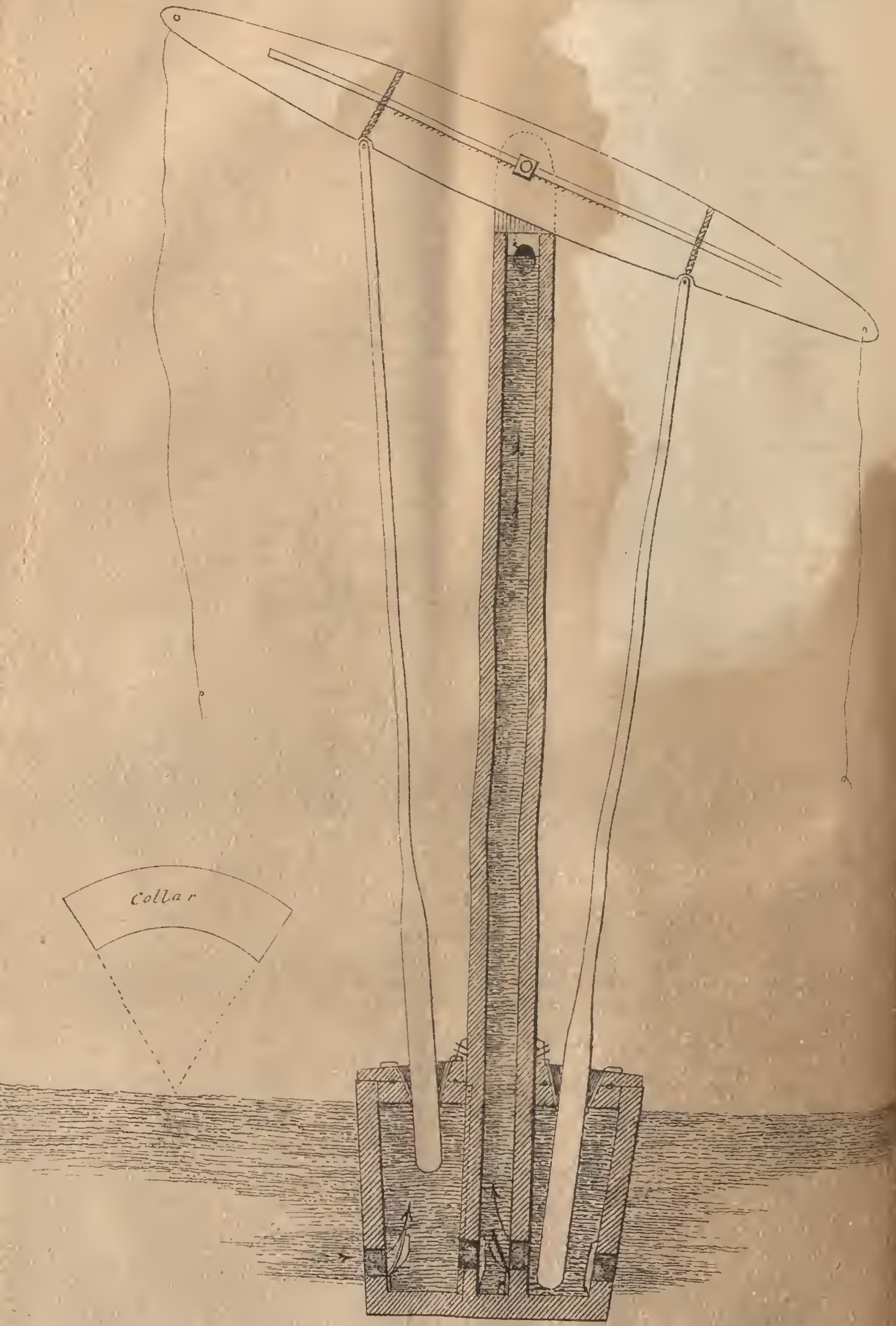
Sir,

A correspondent, under the signature M., has, in the April number of the Gleanings, mentioned, that it is a common practice of the landholders in his neighbourhood, to take half the produce of the land as rent, in all ordinary cases; and that he has known as far as three-fifths having been taken on a good crop. I am well aware that the former proportion, and even more, is very commonly the *nominal* share of the produce expected and bargained for by the landlord. As far as my experience goes, I have never witnessed the actual realization of such a proportion during a course of even a few years, without extensive injury following the exaction, in the shape of extreme distress to the collectors, and consequent deterioration of the estate. Such nominal demand is usually frustrated by the *raíats* stealing (it is hardly fair to apply so odious a term though) from the crop, when in a standing state, or when stacked in the "*callián*," or threshing floor. The universality of this mode of avoiding a ruinous demand, has caused it to pass into a proverb. In cases where the landlord is powerful, and looks keenly after his (supposed) interest, and where he manages either entirely or greatly to stop this practice, I have generally seen the following effects: either the *raíats*, pressed by distress, desert the estate, and flee to where they can procure a sufficiency of food, or advances are continually made to them by the landlord for their maintenance, in order to avoid such an emigration. It is a very usual circumstance to find such landlords produce long and large outstanding accounts against their *raíats*, which of course cannot be pleaded. Neither, indeed, is such often attempted, save either from motives of enmity or revenge against particular *raíats*, or at the expiration of the landlord's own lease from Government, when it becomes, perhaps, a matter of indifference to him whether the *raíats* desert or not; or when, perhaps, it may be an object to him to make them do so, in order to obtain the next lease from Government on lower terms.—These are general remarks, and I do not mean to deny the success to partial exceptions, which local circumstances may induce. But I am convinced that the prosperity of both landlord and tenant would be best advanced by the limitation of the rent to one-third of the produce—that is, that rent should not exceed that proportion.

In Loudon's Encyclopædia of Agriculture, I *think*, it is stated, (I have not the book, and am not very confident of the correctness of my memory,) that in the southern parts of Scotland, the agriculture of which is, perhaps, unrivalled throughout the world, it is considered that the proportion of produce taken as rent, should never exceed one-third; and that more than one-fourth should not be taken, unless the gross produce per acre amount in value to £10. The contrast presented by the northern and southern provinces of France has been often observed. In the northern, fixed money rents on leases prevail: the state of agriculture is flourishing, and the condition of the peasantry comfortable. In the southern, and also in many districts of Italy, the system usually adopted, is the payment of half the produce as rent. The name of a farm, "Metairie," is derived from the practice. The condition of the peasantry, and the state of agriculture are well known to be any thing but flourishing in those parts. Yet *there*, it is part of the contract for the landlords to furnish seed and the instruments of husbandry, (I rather think also both dwellings and bullocks, but am not certain.) This of course greatly reduces the actual amount of the rent from the nominal standard of one half. In this country it is expected that the *raíat* find seed, &c. for himself. That this ultimately and universally falls on the landlord, however, I have above remarked.

If the above are facts, which I believe them to be, I see no reason for supposing that what is destructive to the welfare of the peasantry, and injurious to the agriculture of other countries, should not also be so to this: or rather, I see no reason why we should not at once assume as the chief cause of the bad state of agriculture, and of the depressed condition of the *raíats* in this country, the universally too high rates of rent.

I beg to assure your correspondent an Indigo Planter, in reply to a remark of his, in your May number, that I wished to restrict the data and notices furnished by me to the part of the country of which I wrote. That vast differences must exist in the various parts of so immense a country as India, is sufficiently obvious. To have included, however, the value of the double crops would have produced a scarcely sensible variation in the average value of the produce of land. For the value of the secondary crop (always meaning in that part of the country of which I write) is exceedingly trifling, and seldom yields the landlord more than 2 or 3 annas per *bíga*. The quantity of land so cultivated is also very small. As an average on the whole district, I think $\frac{1}{30}$ of the whole cultivated land of these few estates in which double crops are raised, would be too high an assumption. But again, it is very questionable whether any increased value is realized



Drawn on Stone by J.R. Tassin

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from a double crop: for it is invariably the case, that the primary crop is always an inferior one from land so used; and I am much inclined to think, indeed, that the practice of double cropping is not profitable.—These observations are not intended to apply to the immediate vicinity of towns whence, from the facilities offered to manuring, such practice would be undoubtedly profitable. As a general assertion, it may be truly stated, that manuring, as a practice, does not prevail in my part of the country; the few and limited exceptions bearing the same proportion as a river to the immensity of the ocean. Now no land, of however superior a quality, can be constantly cropped without manure; it must be recruited either by fallowing or manuring. Manuring (save in especial instances) can never obtain in this country till the number of cattle is largely increased; till the *raiats* have more interest in the good cultivation of the land than they now have, by being allowed a larger share of the produce of their industry; and till the dung of animals is less extensively used as fuel. Till manuring is introduced, double cropping can never be practised with advantage. In the outskirts of towns, the *málli* cultivators in this district, raise an uninterrupted succession of crops. These are vegetables chiefly. And I suppose that vegetables also constitute the crops noticed by your correspondent; for it is evident, that neither wheat nor any other grain could be produced at the rate of six, or even three crops per annum. Wheat, with the time occupied in the preparation of the ground, takes seven months to mature; rice five, &c.; and sugar-cane at least fifteen months. The average which I noted was confined to these crops. There is a difference in the rate of rent for sugar-cane of a rupee per *bíga* in this district, when the cane is grown on land on which a *kharif* crop has been previously sown; and this, too, when manure is given in the largest quantities in the power of the cultivator. As about from 4 to 8 annas may be paid for the *kharif* crop, the diminution of the rent of the crop is, say 8 annas per *bíga*, or somewhere about 1½ rupee in the value of the produce. The practice is seldom resorted to, and is considered to be disadvantageous.

August, 1830. H. S. B.

VIII.—*Description of a Plunger Pump.* (With a plate.)

My dear Sir,

Enclosed I have the pleasure of sending you a sketch of the Pump I mentioned to you some days ago, and which I have found to exceed the expectation I had formed of its operation. I will endeavour, however, to explain the mode in which it is constructed, and its powers; and if you think it sufficiently intelligible, pray dress it up a little, and request the Editor of the "Gleanings" to insert it. I have not just now any of the numbers of that work, in which the various modes of raising water are set forth, or I would endeavour to draw a comparison, the only true test of its utility.

The Pump in question is a double stroke "plunger pump," (see Pl. II.) and consists of one square main trunk; with two boxes at its lower extremity, for the plungers to work in, one cross beam (formed of plank) at the top, and two spars, for plungers.

The trunk is formed of three dunnage deals, about 11 inches by 3, and 15 feet long; one plank is cut in two lengths of 5½ by 3, which, inserted between the other two planks, form two sides of the trunk; while the two whole planks form the other two sides, leaving the trunk, in the clear, about 5 inches. The plunger boxes are made of teak, and fitted to the trunk with a rabbit, and secured by two iron hoops, driven on, one near the top, and the other near the bottom, which not only fix the boxes firmly against the trunk, but prevent their ends from yielding to the pressure of the tampions[1], which must be driven in with some force, to render them water tight. The plungers are two fir spars, and the part which works in the collar is about 3 feet long by 5 inches diameter, and are cylindrical; all above this is tapered away, to avoid superfluous weight. To the lower part of this trunk, within two or three inches of the bottom, are fitted two valves, opening *inwards*, opposite to each other. The plunger boxes, which are

[1] These tampions are square pieces, cut to fit the open ends of the plunger boxes, and the lower end of the main trunk, and are driven in tight, with a strip of parcelling round them.

fitted so as to enclose these openings, have each a valve, opening inwards, and opposite the valves in the trunk; the upper tampion of the plunger box has a hole cut through it, of sufficient diameter to admit the plunger and its collar *freely*; say the plunger is 5 inches diameter; which, when the collar is on, is encreased to $5\frac{1}{4}$; then the hole in the upper tampion should be at least $5\frac{1}{2}$. The collar is made of good Europe pump leather, and cut so as to form, when bent round, the frustum of a cone; the smallest diameter of which must be in the clear, just the size of the plunger. The two parts of the leather which meet to form the collar, must be chamfired off so as to overlap each other, but must not be *sewn* together; —the base of the frustum may be hammered out into somewhat of a trumpet shape, to receive the nails which attach it to a block[2], which block is *screwed* to the upper tampion of the plunger box, so as to allow the collar to protrude through the hole before mentioned; round the margin of this hole is placed a ring of loosely twisted hemp, which, when the block is screwed down upon it, prevents the water escaping between them. To work this Pump to the greatest advantage, it should be immersed in the water, to just above the plunger boxes, otherwise the collars, being fitted very slack, (to prevent friction,) would admit the air, when the plunger is drawn up; but when this part is covered with water, this slackness, which is highly advantageous, as reducing friction, is not at all calculated to admit of the escape of any water when the plunger descends; for then the pressure of the fluid shuts the outer valve, opens one of those into the trunk, and at the same time presses the lower part of the collar, (which, it must be recollected, instead of being sewn together, merely overlaps, as before mentioned, with two chamfired edges, was to allow of its expanding and contraction,) closely round the plunger.

As I am but too apt to make mistakes in my calculations, I will give you the data from which I have ascertained the quantity of water the Pump will deliver in a given time. It is placed in a tank, obliquely, the jet pipe leading into a reservoir of masonry; the *perpendicular* height from the surface of the water to the jet pipe is 8 feet 5 inches; the reservoir, which is cylindrical, is 6 feet 3 inches diameter, and 2 feet 3 inches deep. Two men working the Pump will fill this reservoir in *thirteen minutes*. Now, unless I am "out in my reckoning," this reservoir will contain 69 cubic feet, which, at 231 inches to the gallon, is equal to 516 gallons. I have compared this with a common lift pump, (an old ship's,) which is placed in a well of masonry, for raising water into an aqueduct; this well is 5 feet diameter: four men, constantly pumping, lowered the water 2 feet 1 inch in 30 minutes; mean height of the column 11 feet $\frac{3}{4}$ of an inch; although there were four men: I should say that three would be sufficient, but not less.

From the above, I make out that in 30 minutes

Three men, with the Lift Pump, will raise 306 gallons of water.

Two ditto with Plunger Pump, 1190 ditto ditto.

3 dunnage deals at 3,	9	0
1 shinbin,	10	0
7 iron hoops,	10	0
1 carpenter employed 47 days, at 8 Rs. per mensem,	13	1
Europe pump leather,	2	0
Screws and nails,	2	0
Saul plank for beam,	2	0
Two spars for plunger,	2	0
Rs.	50	1

The cost, as exhibited in the margin, might be reduced; as the ends of planks, of 3 feet long, might be substituted for the shinbin; and the man who made it idled away more than half his time, from having other matters to attend to.

Your's faithfully,

Diamond Harbour, *August* 23, 1830. C. Cowles.

Remarks by the Editor.

The friend to whom we are indebted for the above letter, thinking with ourselves, that no "*dressing up*" could improve it, we have given it exactly in the writer's words, who, we hope, may be induced to favour us with some further communications on useful subjects. We have yet failed to draw from practical men the number of communications which we had expected at our outset to see, and which we know they must be capable of furnishing. Some are deterred from offering suggestions, deeming them of little value, and less interest: others from a disinclination or fancied inability to write any thing that shall be worth reading.

[2] These blocks are cut to fit the upper part of the plunger box, over the upper tampions, and the holes in these blocks are larger than those in the tampions, and are cut to fit the trumpet mouth of the collar, which is closely nailed to it.

But the former should consider, that what appears familiar and trivial to them, is to many not known at all; and the more practical it be, the less likely it is to be known to any. To the latter we would say, that if a man have useful or interesting information to communicate, the plainer and simpler his style is, the better.

With regard to Captain Cowles' Pump, we think it very ingenious, and though not exactly the same as Professor Robison's plunger, mentioned in our first volume, in all its details, it is yet on the same principle. But unfortunately—and we would suggest this for the ingenious inventor's consideration—it is altogether different in the manner in which the labour is applied,—the particular feature which constitutes the great excellence of Professor Robison's pump. Thus, in the present machine, the oscillating beam is alternately pulled down on each side by the rope attached, consequently the labourer applies no force, beyond that which is derived from the exercise of the muscles of the arm. Now, in Professor Robison's pump, the beam is worked by the man's weight, the muscles of his body being used, (and this is the most effective exertion,) to raise himself up again, after, by descending, he has carried down with him the descending arm of the beam. Thus there is a *double force* applied, or *an equal one, with half the exertion,* which may consequently last twice as long. But this is not all. For, as before observed, it is precisely this action of the muscles which enables a man to perform most work;—so that whether, as in the present pump, he pull down an oscillating beam by a rope, or turn a winch, or work a pump handle, in none of these operations is the same quantity of work performed, with the same expenditure of animal power, as when a man's exertions are confined to walking along an oscillating beam, or ascending a ladder, and descending in a box fastened to a rope, which passing over a pulley, raises the weight required; or lastly, walking on the internal or external periphery of a large wheel. This fact is well known, having been repeatedly ascertained by experiment; the actual amount of the superiority appearing to be a little doubtful. If we are to take Professor Robison's estimate of its value, it ought at once to supersede every other mode of applying human or animal labour; and so favourable are even the lowest estimates, that we think it well worth the attention of practical men. In the pump above described, a fair opportunity offers of trying it. A bamboo stage could be affixed at a very trifling expense to the beam, and a man employed to walk backwards and forwards; the result, we really think, will be found such as could not have been anticipated.

The particulars given by Captain Cowles enable us to compare the performance of this pump, worked as it is at present, with that of other machines for raising water described in the GLEANINGS[1]. It appears that two men working at it, raise 69 cubic feet of water, 8 feet 5 inches in 13 minutes. This is equal to 22,34 cubic feet, raised 1 foot high in one minute by each man. This is a little more than one-fourth of what Professor Robison's *feeble old man* effected on the walking beam, which was 80,5 cubic feet. It is but a trifle more than ONE-FIFTH of what his "stout young man," loaded with 30lbs. effected. Compared with the several other methods of raising water, the daily produce of which is given in the table, p. 271 of our first volume, we find it about equal to, if not a little superior, to the sucking pump, which gives from about 800 to 810 maunds, raised 10 feet, for the daily work of a man. An accurate comparison cannot be made, as the *daily* work performed with the present pump is not mentioned. As it is so much cheaper, and less liable to go out of repair than the sucking pump, there can be no question as to the value of the invention, although we fully expect that if our suggestion of the walking beam be adopted, the same quantity may be raised with one-third or one-fourth of the exertion at present used.

The form of the present instrument has an advantage over that described by Professor Robison, inasmuch as it is applicable to shallow pieces of water, and will yet raise the water to a considerable height. In the latter, the depth of the water must be equal to the height to which it is required to raise it.

IX.—*On the Rent and Produce of Land.*

To the Editor of Gleanings in Science.

SIR,

When I last addressed you, I had got upon a part of the subject under discussion, which I thought was prudent to avoid; I shall, therefore, retrace my steps,

[1] See Vol. I, p. 268.

to reconsider the prospect of success in employing capital, by investing it in landed property, and agricultural speculations.

This, as has been said, may be done by purchasing the *biswadarí* and *zamíndárí* rights of villages, the lands remaining subject to the Government revenue; or the village may be taken on a lease from Government, which invests the tenant with the necessary authority and power usually exerted by the *biswádars* during the term of the leases; or the lands, or part of the lands of a village, may be subject to a tenant under a lease to Government.

The first of these plans has been adopted by persons who have a right to become proprietors of the soil, and has generally been done, with a view towards providing a suitable estate for children: the latter, where a person wishes to have a temporary interest in the soil, or who is not entitled to hold lands.

Either of those plans are feasible for carrying on general agricultural speculations, which appears to be the most adviseable under present circumstances; and a person so inclined should settle himself in some centrical spot, not far from a navigable river, having within a reach of 10 or 20 miles a country capable of producing indigo, sugar, tobacco, and cotton. By having the lands of a few villages within that circuit at his disposal, he will be able to direct the culture of these crops by the *raíats* as he thinks would be most advantageous, procuring proper seed and plants, and, where necessary, giving instructions in the culture, where a superior mode may be adopted, but by no means interfering any further, which would disturb the industry of the *raíats*, and make him quite indifferent as to the produce, but adhering, as far as possible, to the plan, of purchasing at the fair marketable price. Some lands in all villages will be found proper for either of these crops. Wells may be dug, cuts from rivers, *jhíls*, and canals executed, and irregation improved in all its branches, where the lease of the village will admit of the prospect of a fair return; in general, the crops of wheat, grain, and millet, may be left entirely to the natives, who ought to be encouraged in every possible way; and on purchasing or renting a village, a small grove of *babúl* trees ought to be immediately planted, with a few *sisoo* and *toon* plants on the edges; this would do a great deal to secure their good-will. Mangoe groves are never of any use to the people; they are rented out to the *kunjars*, who watch them night and day, and the villagers never taste of the fruit without payment, except by stealth, and often vexations quarrels arise regarding them; the *babúl* trees are exceedingly useful for agricultural purposes, and they soon grow to a proper size on almost any soil, but are getting very scarce, and few think of planting them. The statistics of Anúpsheher shew how easy it would be to set on foot an improved agriculture, it appearing there are no less than 40 families of *malís* employed there as mere labourers, and, I understand, in general, occupied in dragging wood and bamboos to and from the rafts on the river.

The returns upon villages thus rented, without taking any trouble whatever as to improving them, but merely taking half the produce from the cultivators, after the crop is reaped, or taking it at a valuation while standing, gives good returns, as I shewed in my last letter; and, with a proper direction, the lands of the villages may be turned to a much better account. Even the clearing of the lands from jungle is extremely ill managed: the wood is in general collected together and burnt, while I have known the expenses of clearing an extensive jungle repaid, by floating the wood down a river 100 miles off, to a populous town, where it was sold for firewood. Few *zemíndárs* would think of doing such a thing, and it must be confessed they occasionally meet with obstacles. A respectable landholder told me, that a fine triumphal arch, which was erected at a neighbouring station, for the entré of Lord Amherst, cost him nearly 400 rupees worth of *jhaoo jungle*, which he was in the habit of selling yearly for about that sum; this *jhaoo* grew on the waste lands of a village, for which he probably did not pay more revenue than the value of the *jhaoo* cutting.

The *zemíndárs* of other villages will always be ready to enter into the culture of any produce or mode of cultivation, which will, at the same time, give them proper remuneration, as they have shewn themselves ready to cultivate the best description of indigo plant, and with every success. Encouragement, and non-interference, in a certain measure, seems to be all that is necessary to secure their entering with zeal into measures of this sort.

To the European labourer no encouragement can be held out. I have shewn that it is perfectly possible to obtain for himself a livelihood, as far as subsistence goes; but, as has been stated, in the statistics of Anúpsheher the money rate of labour is so trifling, that it would be ridiculous attempting such a thing, and in a country too, where it appears, and without the smallest reason to doubt it, that in the inhabitable parts, the population is considerably denser than that of England by the census of 1821. A few honest husbandmen would, however, be of much use in the establishment I have proposed, who would act as directors of the indigo, sugar and other works, and take the place of the *mutasaddís*, who plunder alike the *sercár* and the *raíats;* he might have his cottage and 30 or 40 acres of land to manage under the same terms as a *raíat* of the village.

A few establishments of this nature would tend much to the improvement of the country, by raising the value of the products and fitting them to compete in the European market; but if Government wish to give encouragement to improvement of this nature, it appears necessary that the assessment of the revenue of the land should be put on another footing; and in the consideration of this already much disputed and tortured subject, I shall endeavour to muster up courage to address you in my next. Remaining your's very obediently,

Upper Dooab, July, 1830. Z.

Note by the Editor. This letter should have appeared in our last number, but was mislaid by accident. It is a continuation of the subject discussed in our correspondent's letter published in our April number, p. 129.

X.—*Miscellaneous Notices.*

1. *Chinese method of Boring.*

According to M. Imbert, there are, in the vicinity of the town of Ou-thouang khiao, several thousands of these salt wells in a space of ten leagues by five. Every person who is tolerably rich, takes a few associates with him, and digs one or more wells. The expense of digging a well, is from seven to eight thousand francs. These wells are commonly from fifteen to eighteen hundred French feet in depth, while they are only five, or at most six, inches in diameter. They are almost always bored in the solid rock.

The process employed by the Chinese in forming them, although very simple, is not described by M. Imbert so clearly as might be wished; it will be understood, however, on reading what follows. This people accomplishes the most difficult undertakings with time and patience. There is sunk vertically into the bed of the earth, which is commonly met with at the surface, a wooden pipe crowned with a hewn stone, perforated with a hole, which, like the pipe, has the same diameter as it is intended to give the well; that is, five or six inches. In this tube there is made to work a steel head of three or four hundred pounds weight. This steel head, the author says, is notched at the end, and is a little concave above and round beneath. A workman by leaping upon the extremity of a balance or lever, the other extremity of which is attached to the steel head, lifts it to the height of two feet, and lets it fall again by its own weight. Some pails of water are thrown in from time to time, to assist the trituration of the substances. The spur or steel head is suspended by a good *corde de rotin*, of the diameter of the finger, but as strong as a cord of gut. A triangular piece of wood is attached to the cord, and each time that the lever raises the cord, a second workman, seated near the tube, makes the triangle perform a half revolution, that the steel head may fall in a different direction. At noon, the second workman ascends upon the lever to take the place of his companion. At night two other men take their place. When three inches have been bored, the steel head is withdrawn, by means of a pulley, with all the substances with which its upper concavity is loaded. By this mode of boring, there are obtained wells which are perfectly vertical, and the lower surface of which is highly polished. Beds of sand, coal, &c. are frequently met with. The operation then becomes more difficult, and is sometimes entirely frustrated; for these substances no longer offering an equal resistance, it happens that the well loses its verticality; but these cases are of rare occurrence. At other times the iron ring which bears the steel head breaks. When this accident happens at a certain depth, the Chinese know no other means of remedying it than to employ a second steel head to break the first,—an operation which may take several months. When the rock is good, an advance of nearly two feet is made in twenty-four hours; so that it takes about three years to dig a well.

The apparatus for drawing the water is equally simple with that which is employed for boring. A bamboo tube 24 feet long, at the end of which is a valve, is let down into the well. When it has reached the bottom, a workman pulls at the cord which sustains it, giving it strong jerks; each jerk opens the valve, and fills the tube with water. It is then drawn out by means of a kind of vertical capstan, or large windle, fifteen or sixteen feet in diameter, which is put in motion by two, three, or four buffaloes or oxen, and upon which the cord is rolled up.—*Jameson's Journal of Science.*

2. *On the Alligators of the Ganges.*

To the Editor of Gleanings in Science.

Sir,

Last cold weather having been a good deal on the banks of the Ganges in this neighbourhood, and having paid some attention to the Alligators found on its banks, I beg to offer the following information which I received concerning them.

The natives state that there are three different animals of this description; viz. the *Góa*, or long-nosed; the *Nácer*, or snub-nosed; and the *Gariál*, or *Magger*. These three kinds I have minutely observed day after day with a good telescope, as well as examined the bodies of several, which were shot by myself and other gentlemen of the party.

In no instance did I ever see, or find any insects adhering to the internal parts of their mouth, and on no occasion ever saw birds very near the animal: on the contrary, their mouths were clean, and had a white and yellowish colour—in the *Magger* darker, but shewing no appearance of being at any time troubled with insects adhering to the tongue or roof of the mouth. When the sun got warm, they, (particularly the *Góa*,) opened their mouths for the purpose of respiring more freely; on cold cloudy days, however, they kept them shut, and were generally, when cold, unwilling to take to the water, unless much disturbed, which made the natives believe that they were frozen. I was also told that they come several hundred yards in land at night on low grounds, where cattle usually graze, for the purpose of eating cow-dung, and their marks were pointed out by the natives.

The *Góa* were the largest but most inactive, and, I was told, inoffensive kind. The snub-nosed were active and bold; they generally lie with their tail bent ready to strike in case of sudden attack. But the most mischievous and active is the *Gariál*, or *Magger*; although they were the smallest. When struck with a ball, they move with astonishing rapidity into the water; the head is nearly round, and quite a mass of bone; the muscle round the neck is like a muff, and gives enormous force to the jaw. Part of the jaw of one is now before me: it appears that some of the front teeth had been broken, and others are growing in their place; this led me to break some of the complete teeth, and I find each to consist of two and sometimes three, one within the other, so that when the outer one breaks, the inner ones take its place. They retain life in the most astonishing manner. After being shot, brought to the village, and the fat cut out by the natives, they still continued to move and shew signs of life.

I have heard people who have been in the habit of shooting these animals, say, that the most effective place to lodge a ball is the neck. I have found this to be the case, and unless they are shot though the spine there is scarcely a possibility of getting them. I have struck the *Magger* and snub-nosed on the head with a rifle ball, when it sung off as if it had struck a blacksmith's anvil. The head of the *Góa* is easily penetrated.

I am, Sir, your most obedient servant,

Anúpsheher, 10*th June*, 1830. *B.*

3. *Sensible Temperature.*

To the Editor of Gleanings in Science.

Sir,

In your number for May, the author of a paper on Sensible Temperature states, that the subject had been noticed in England by a medical gentleman, who had read a paper on it before one of our medical societies. Perhaps he alludes to the following, which I take from the account of proceedings of the Royal Society, given in the *Annals of Philosophy*, vol. XI. p. 138.

"A paper was read, entitled Observations on the Heat of July, 1825, together with some remarks on sensible cold; by W. Heberden, M. D., F. R. S."

"These observations were made on the author's lawn at Datchet, in Berkshire, by means of thermometers suspended in the shade of trees: the highest temperature observed was 97° Fahrenheit. Dr. Heberden remarks, that the extraordinary weather of this month passed away without rain, lightning, change of wind, or any other obvious *cause* (?) and cites the nearly parallel case of the year 1808, as recorded in the Society's observations, and also by Mr. Cavendish. He gives some observations and experiments on a method of ascertaining the *sensible heat*, which he believes to be much above that indicated by the thermometer, by means of previously raising the thermometer to a high temperature, and then noting its successive decrements in equal times or exposing it to the open air. In the concluding remarks on sensible cold, Dr. H. states his opinion that its chief cause is the loss of heat by the body, effected by the action of the wind, not by the moisture of the surrounding atmosphere."

If this be the matter to which your correspondent refers, it appears that he made a mistake in supposing the paper had been read before one of the medical societies.—I am, Mr. Editor, your's, &c. C.

4. *Boring for Springs of Fresh Water in Egypt.*

In a notice given in the *Calcutta Literary Gazette*, extracted from the *Athenæum* of May 1st, occurs the following very interesting intelligence.

"The miners which a most liberal British spirit brought hither, to bore for water, have had an encouraging success in the neighbourhood of this city; but the main object will be the desert tract between Caïro and Suez, where there is not now a drop of water to be met with."

XI.—*Notices of Books.*

Library of Useful Knowledge. London: Baldwin *and* Craddock. 1829.

In our fifth number (Vol. I. p. 142,) we gave a brief notice of this work, including the titles of the first 28 numbers. Since that time 33 additional numbers have reached this country, the titles of which we here subjoin, for the information of our readers.

29.	Navigation,	
30.	History of Greece,	Part II.
31.	Life of William Caxton,	
32.	History of Greece,	Part III.
33.	Thermometer and Pyrometer,	Part II.
34.	History of Greece,	Part IV.
35.	Geometry,	Part I.
36.	History of Greece,	Part V.
37.	Geometry,	Part II.
38.	History of Greece,	Part VI.
39.	Life of Sir Edward Coke,	
40.	History of Greece,	Part VII.
41.	Galvanism,	
42.	Geometry,	Part III.
43.	History of Greece,	Part VIII.
44.	Animal Mechanics,	Part II.
45.	Life of Mahomet,	
46.	Double Refraction and Polarization of Light,	Part I.
47.	Physical Geography,	Part II.
48.	Life of Niebuhr,	
49.	History of Greece,	Part IX.
50.	Life of Sir Isaac Newton,	
51.	Geometry,	Part IV.
52.	Life of Admiral Blake,	
53.	Double Refraction and Polarization of Light,	Part II.
54.	Glossary and Index, with title page, to the volume of Natural Philosophy.	
55.	Arithmetic and Algebra,	Part III.
56.	Chemistry,	Part I.
57.	Art of Brewing,	Part I.
58.	An Account of Newton's Optics,	Part I.
59.	Animal Physiology,	
60.	Art of Brewing,	Part II.
61.	Geometry,	Part V.

Amongst these 61 numbers will be found 9 of the History of Greece, the ninth being the conclusion; and containing, besides a chronological table, a title page, a table of contents, and an index. The whole subject forms a thin octavo volume of about 300 pages, close print, being equal to 600 pages of our ordinary books, the price of which is 4*s.* 6*d.*, or 6*d.* a number. A volume of natural philosophy is also completed, and is furnished with title page, table of contents, index, and glossaries. This volume contains numbers 6, 7, 11, being the subject of MECHANICS; number 1, or HYDROSTATICS; number 2, or HYDRAULICS; number 3, or PNEUMATICS; numbers 4 and 5, on the subject of HEAT; numbers 12 and 19, or OPTICS; and numbers 46 and 53, or POLARIZATION. It forms a volume of about 470 pages, equal to 1000 ordinary, and is sold for 8*s.* The prices of these books will mark an æra in the history of Bibliopoly—and the Society has done great service in placing within the reach of readers of small means such a mass of useful information as they contain.

But the good effects which the Society is producing do not stop with its own exertions. Let any reader look about him, and say whether we should have had at this moment the many valuable yet cheap works which have succeeded each other so rapidly within the last two years, but for the example set by this Society. Booksellers have been at once frightened and persuaded into doing something towards the reduction of the unreasonable, and we had almost said unfair prices paid in England for books—prices double what they are in every other country. They have learned from this Society that *reduction of price* may bring *increase of profit;* and they have been forced to act on this truth by the conviction, that if they did not, the Society would soon run away with their business. Would Mr. Murray, who has an opinion that a book sells the better for adding a few shillings to its price[1], have ever favoured the public with his FAMILY LIBRARY but for the *vis a tergo* communicated by this Society. Would the London Encyclopædia have ever been published at its most reasonable price, but for the salutary dread of being left behind in the race of profit occasioned by the proceedings of this Society. In like manner we think we can trace Lardner's Cabinet Cyclopædia, apparently the best planned work of this kind, yet projected, and equally good in execution as far as it has yet gone, to the reception these cheap publications have met with. It was stated in our former notice that the sale averaged 10,000 copies, at a very early period: what it is now we do not know.

So jealous were the booksellers of this novel experiment of selling cheap books, that the Society soon after its institution was assailed with all manner of abuse and misrepresentation. The public are now able to appreciate these *tirades* of monopoly and absurdity. This Society have delivered the public out of the hands of a knot of interested chapmen, and have made the meetings and resolutions of the book-selling trade powerless, to shut the door of knowledge If a student of humble means have it now in his power to satisfy his thirst for knowledge, it is owing to the spirited proceeding of this Society. If even the wealthy and idle have caught some glimmering of knowledge, it is owing to the impetus given in the manufacture of popular treatises by the praiseworthy exertions of this Society. If, in fact, there is a stir, a bustle, an activity in the communication and acquisition of knowledge—if science is raising her head where erst she durst not appear, it is mainly owing to the very general interest which has been excited by the institution of this Society. That it has done much good, and will do more, is undeniable, and with the conviction that ignorance is the cause of more than half the evil we suffer in this world, and consequently that knowledge can never become too cheap or be too generally diffused, we cordially desire to see this Society prosper; and in the spirit of such a wish we say, *Esto perpetua.*

Yet while we acknowledge the excellent effects which the silent and unostentatious proceedings of this Society is giving rise to, we cannot but regret that more pains have not been taken to produce works of sterling value. We do not wish our readers to infer by this remark that the treatises in question are not quite as good as any yet given to the public: on the contrary, we think in many respects they are superior, and, in one circumstance particularly, incomparably better adapted to the purposes of the learner—we mean in the adoption of the practice of illustrating subjects by wood cuts introduced amongst the text. But we think the opportunity was favourable for superseding the cumbrous and not seldom erroneous works on which we have hitherto been dependent, and for producing a new series in which all the chaff and rubbish should be swept away, and nothing be found

[1] See his evidence before the House of Commons.

but the solid nutriment of real knowledge. Such a project is manifestly beyond the means or power of any single bookseller or author; but a Society constituted as this is, with some capital and an unheard of sale for their works, might have done much. They might have brought the whole resources of the scientific world to bear upon the subject—they could afford to purchase the co-operation of all those not disposed to labour gratuitously—and by inviting communications to be paid for, if found useful, they might have embodied a mass of information which they could have also commanded the means of properly digesting and reducing to system. Instead of this, what have they done? They appear to have contented themselves with one treatise on each subject, which has been published apparently as received from the author, without any reference to the other parts of the volume or subject of which it formed a part. As a consequence of this want of plan, we have uninteresting repetitions and a general want of connection amongst the facts detailed. For instance, the different water wheels and pumps are twice described in the volume of Natural Philosophy. The common experiment by which the expansion of metals is proved, is twice detailed, and each time with the same cut. This is certainly a blemish, and takes from the unity and systematic character of the treatise. Equally difficult do we find it to agree with their classification of the subjects of Natural Philosophy. If heat be included, why omit magnetism and electricity? They may reply, that these are branches of chemical science; although this can hardly be said of magnetism;—and as for electricity, it is not more closely related than heat to that division of knowledge. Again, why omit Hygrometry, which is as much a branch of Natural Philosophy as Pneumatics, and, in its application to the steam engine, of infinitely more practical value. But, in fact, we object altogether to any such title as Natural Philosophy, which, as being perfectly unmeaning, may be made to include or exclude any thing. The treatises which go to form this volume should have been preceded by a general one on Statics, showing the application of the theory as it regarded solid matter, fluids, or gases. This would have comprised great part of the three treatises of Mechanics, Hydrostatics, and Pneumatics; or at least would have connected well with them. Hydraulics might have followed, and heat, light, magnetism, and electricity, as being imponderable agents, and only knowable in their effects, would have gone well together as a second part. With heat the subject of Hygrometry would connect itself.

We are, however, aware, that the classification of the different branches of science has always been found a difficult task, and that no table of this kind has been yet proposed, to which objections may not be made; nor are we so presumptuous as to suppose that we have attained to a new light in a subject which puzzled Bacon, Locke, and D'Alembert. Still we do not hesitate to say that our arrangement is better than that of the Society, which, indeed, could scarcely be worse. After all, however, had the individual treatises been executed in the style which they might have been, the objections to the arrangement would not have weighed much with us. Had the scientific truths really established been clearly stated, and all useless and erroneous matter been excluded, the merit of such inventions or process as are noticed strictly and impartially discussed, credit given to whom credit is due, and charlatanerie and pretension exposed wherever they appeared, even though in the garb of a Professor, they would have had a merit superior to that of mere arrangement—a merit which we hoped to have seen in them, and which, considering the history of their birth, it is surely extraordinary that they have not. We cannot, in this number, enter into the subject so fully as we could wish; but as an instance of exhuberance that might be lopped off without injury, we may mention the account of Wedgewood's Pyrometer, which, with its appended table of melting points, we consider to be a bright example of what we will venture to call the FUDGE of science. Under the same head we would place Chapter VIII. of the treatise HEAT, being an account "of the absolute *quantity* of heat which any body contains," and "of Dr. Irvine's *ingenious* method of determining the point of ABSOLUTE PRIVATION OF HEAT." The author may well say, that "the results obtained by Lavoisier and Laplace are difficult to reconcile with one another, and with those obtained by other philosophers." The wonder would be were the case otherwise. All the absurdities connected with the question of the zero of temperature are the necessary consequence of the use of those objectionable terms *latent heat* and *capacity for caloric;* for which reason we also object to Chapters VII. and IX. as being very far removed from a philosophical and discriminating account of the subject.

In the account of Leslie's Differential Thermometer, the writer might have omitted half what he has written, and considerably improved the article; for, first, the instrument is of very limited applicability; and, secondly, the graduation of it, as devised by the inventor, is erroneous[2]. In the great variety of objects to which this instrument has been recommended to be applied, we can almost fancy a professor's pen emulating the magic weapon of Harlequin; and we think a very little consideration sufficient to satisfy any one that, as in the instance of the wooden sword, the transformations are more curious than useful. Thus has Professor Leslie made this child of his fancy at once a Differential Thermometer, a Hygrometer, a Photometer, a Pyroscope, and an Ætherioscope. "'Tis every thing by turns and nothing long[3];" but if after considering this versatility of character and list of its performances, we ask *cui bono?* the answer must be, that, excepting to illustrate the subject of radiation of heat, we really know of no use that the instrument can be turned to. It is a bad hygrometer, (even supposing its scale corrected,) a worse photometer, a most lame and inconclusive pyroscope, (the common thermometer much better,) while as an ætherioscope, it is merely a substitute for the latter instrument, with the advantage (if that be one) of giving a little mystification to the subject. But there are deficiencies as well as exhuberances; and our great quarrel with this article is, that the subject of it being HEAT, we have no attempt to put the question of thermometry on a sound basis; and lastly, we have not even an allusion to the curious laws of cooling discovered by MM. Dulong and Petit.

We should be sorry to be misunderstood in these our crude remarks on the proceedings and publications of a Society which we so highly honour. It may perhaps strike the influential members of it, should they ever chance to see these strictures, that faults there must be when even our puny optics can discern them. Nevertheless, as before remarked, our complaint is not that they are not better than other treatises on the subject, but that they are not so good as the Society have the means of making them. On the contrary, let any one compare the treatise on heat, against which we have started so many objections, with any other even our most celebrated, and he will find that it is a far more satisfactory performance. If he wish to be thoroughly mystified on the subject, let him turn to Thompson's Chemistry. The article in Brewster's Encyclopædia is little better. In Ure's Chemical Dictionary, article Caloric, he will find one of the best accounts previously published; yet, in comparing it with the present, the reader will be sensible of its deficiencies. Dr. Ure's style, too, is rather ambitious and figurative for the details of science. Rhetoric is here misplaced, and only serves to dazzle weak minds, and persuade them that the author means more than meets the eye. In Leslie's much talked of treatise, it is quite extraordinary how the author has contrived to shut his eyes to the simple truth which was staring him in the face all the time. This remark is more particularly applicable to his hygrometrical researches; though it is not inapplicable also to those on heat. It is something more to the purpose to say that there is not one of his pretended laws, whether of heat or moisture, which has not been proved to be erroneous.

Upon the whole, therefore, though falling short of what we could wish to see them, and what we believe they could be made, we can safely recommend this series of publications to the student, as affording him at a very moderate price, perhaps better digests of what is known in the various branches of science, than he will be able to find in more expensive works.

[2] See D. B.'s communication on this subject in our 13th number, vol. ii. p. 24. We had once occasion to compare the indications of Leslie's Thermometer with a common one, and were surprised to find they did not correspond, although we failed to perceive the cause of the discrepancy at the time.

[3] Those who have purchased the hygrometers, photometers, pyroscopes, and ætherioscopes of this Professor, will, we apprehend, find the second part of our quotation equally true with the first, and discover to their great mortification that these curious toys with hard names, are only fit for deposit in that museum which is to be found in the house of every experimental philosopher—that limbo of superannuated instruments and crippled apparatus, in which are laid up cracked retorts, broken-necked thermometers, asthmatic air pumps, and barometers that have suffered from wind. We think it was the business of a Society for promoting useful knowledge, to have prevented the disappointment consequent to the purchase of instruments which are of no use save to the maker and inventor, and which answer no earthly purpose save that of transferring money to their pockets from those of the ill advised purchaser. We may add, that were they as useful as they are the contrary, they are so high priced that they would be pronounced unconscionably dear.

GLEANINGS

IN

SCIENCE.

No. 21.—*September*, 1830.

I.—*On Artesian or Overflowing Wells.*

[From the Edinburgh New Philosophical Journal.]

In some districts of France, England, and North America, the want of good spring-water is supplied very successfully, by boring to a considerable depth into the ground; when a great quantity of very pure water rises to the surface, and, in many cases, is even projected to a considerable height above the surface of the earth. Wells of this description are called, in England, Overflowing Wells, and in France *Fontaines jaillissantes, Puits forcés*, or *Puits Artesiens.* The latter name is derived from the circumstance of their having been long in very extensive use in the district of *Artois.* From thence these wells were introduced into other parts of France, yet, in general, much more sparingly than might have been expected from their acknowledged utility, and the peculiarly favourable nature of these districts for their employment. For this reason, for these ten years past, several scientific societies, as the *Societé d' Encouragement pour l'Industrie national*, and the *Societé royale d' Agriculture*, have offered prizes, to diffuse this useful discovery throughout France; and in consequence of these endeavours, several treatises and publications have of late drawn our attention to this interesting and important circumstance. It will not, therefore, be misplaced, to give a short exposition of the scientific information which we may derive from artesian wells; at the same time, it will, perhaps, be in our power to correct some of the erroneous notions upon the mode of origin of subterranean waters, and upon the possibility of discovering them.

We owe the most complete and authentic information on Artesian wells to M. F. Garnier. His work, *De l' Art du Fontainier sondeur et des Puits Artesiens*, which was crowned, in the year 1821, with the prize of 3000 francs, by the *Societé d'Encouragement*, and which has been printed at the expense of the French Government, and of which a second edition has since appeared in 1826[1], contains not only clear directions for boring these wells, with plans of the requisite instruments, but also such sound views regarding the origin of subterranean aqueous reservoirs, and so well founded on facts, that we cannot be far wrong in supposing every where the same, or similar relations, wherever we have hitherto succeeded in conducting to the surface these collections of water. We therefore think that the subject cannot be better introduced to the attention of those who are yet quite unacquainted with it, than by shortly communicating the substance of the above-mentioned Essay, apart from all technicalities.

The observations of M. Garnier were especially directed to the department of the Pas-de-Calais. The constitution of this district, with the exception of some primitive ridges in the vicinity of Boulogne, consists essentially of two portions; of a limestone plateau, called the High Land, intersected by many small valleys,—and of alluvial deposites, which extend in an immense plain as far as Holland, and the north of Germany. The limestone, only very thinly covered with soil, is stratified, full of fissures, and the same with that which forms the basis of Picardy, Normandy, and Champaigne. The line of junction of the limestone and alluvial

[1] A German translation of the first edition, by Waldauf V. Waldenstein, appeared at Vienna, in 1824.

deposites is principally directed from SE. to NW., from between Arras and Lille to Calais: a little to the south of the last of which, the Cap-blanc-nez consists of this limestone.

By far the greater part of the Artesian wells, which are bored in this district, lie to the north of this line, where the newer cover of beds of sand and clay have yet attained no great thickness; and experience teaches us that water is not found till the borer reaches as far as the limestone, or has penetrated into it. Few wells lie to the south of this line, in the limestone formation itself. But the relations of these last are quite the same as those of the others; they are found, for example, in valleys of the formation, the bottoms of which are covered with the same masses which form the greater plain: we even here do not meet with water till the waterproof stratum of clay, resting on the limestone, has been penetrated. When, which is not unfrequently the case, water is met with before this, in the beds of sand and loam, its impurity, and the feebleness of its propulsion upwards, shew that it is derived from quite another source than the pure water of the Artesian wells.

From these relations, which are elucidated in Garnier's work, by the profiles of several boring works, it is sufficiently evident that the water, which ascends through the shafts, is always derived from the deep-lying points of the limestone strata; from the subterranean slope of the mass of the rock. A farther proof of the Artesian wells deriving their supplies only from this source, is derived from the observations made in several places; as, for example, at Lillers and Bethune; that, when one of two adjoining wells, lying in the same line of direction as that of the limestone formation, is rendered muddy by the piston of the pump, the water of the other is simultaneously milky, from the suspension of minute particles of lime. The origin of the Artesian wells can, therefore, hardly be doubted. It is well known how numerous and extensive are the fissures, often miles in length, contained in the limestone of this part of France; how quickly the rain-water is absorbed on the high grounds; and how abundantly it re-appears, in the form of springs, at the foot of these hills[2]. If any proof of this were required from the work of Garnier, it only requires to be mentioned that, among the many streams of water which issue forth with much violence from the fissures in the limestone rock of the steep declivity of Cap-blanc-nez, and which are constantly undermining it, *s* also another proof of the existence of more extensive excavations in this district, and which are continually becoming larger, is in the sinking of the ground, —for example, in the *arrondissement* of S. Paul, being a not unfrequent occurrence. If we now reflect, the limestone strata have a position inclined to the horizon, and that their outgoing often forms the highest point of the district, there can be no doubt that the Artesian wells are only supplied by the atmospheric water,

[2] One of the most instructive instances of the passage of water through subterranean canals in limestone mountains, is certainly that described by Saussure (*Voyages dans les Alpes*, ed. 4. t. i. p. 309), at the *Lac de Joux*. This little lake in the Jura, receives the water of the larger lake of Rouss, and of several rivulets, without its having any other outlet, on account of its being situate in a valley surrounded by heights, than by the numerous crevices between the nearly vertical strata of limestone. On the north-west side, the lake has made for itself a way to them, and has formed a deep hollow, by the bottom of which the water is soon absorbed. The inhabitants of the valley have also formed similar outlets. As it is very important for them that the water maintain a nearly uniform level, they lead the lake, when it overflows, into little reservoirs, which they have dug down to the limestone rock, and are eight or ten feet broad, by fifteen to twenty deep, and which they carefully clear from the mud which collects in it. One could hardly have believed that these reservoirs, or, as they are called there, funnels *(entonnoirs)*, both natural and artificial, gave rise to the springs of Orbe, lying 680 feet lower, and three-fourths of a league from the north end of the lake, if an accidental occurrence, in the year 1776, had not set it beyond a doubt. At that time, the inhabitants, in order to lay dry the little lake, and to clear out its outlets by the *entonnoirs*, dammed up the lake of Rouss, which empties into it; but this lake became at one time so much swollen, that it burst the embankment, and rushed downwards with great violence into the lesser one, which by that means became very turbid. The consequence of this was, that the usually pure spring of Orbe became shortly after dirty and impure. Yet the connexion of the lake of Joux with the springs of Orbe seems to have been suspected from a very early period, as the stream which connects the two lakes above has also the name of Orbe; therefore, it has been clearly marked for a portion of the river which discharges itself into the lake of Neufchatel.

[3] So in orig. The slovenly style in which this paper is *done* into English, is discreditable to the journal in which it appears.—Ed. gl.

which falls on the upper part of the limestone strata, and sinks down through the various canals which they contain: in a word, that they represent the shorter legs of a syphon, the longer of which is buried in the rock. M. Garnier is so convinced of the truth of this principle, that he only advises the boring of wells in the valleys of those districts whose elevations contain the outgoing of a cavernous limestone.

Besides, upon a review of the appearances observed in Artesian wells, it is evidently sufficient, that an inclined stratum of a fissured or porous limestone be included between two water-proof beds of clay, one of which sets a limit to the sinking of the water downwards, and the other keeps it back from above. The existence of such a cover is evinced by all boring works: a water-proof stratum of clay must always be penetrated, before reaching the spring-water. But also, it can easily be conceived, that the undermost layer is never wanting; and although, for the most part, some thinner strata of limestone supply its place, yet the strata, which conduct the water, always contain it in crevices, which are much more numerous on the surface than in the centre of the beds: thus, there is a demonstration as in a boring-work at Blengel, that, even in the limestone itself, beds of clay occur. From these circumstances, it is easily explained how we can never hope to sink Artesian wells in granite, gneiss, porphyry, serpentine, &c. Even in schistose mountains, it would not be advisable to sink these wells, because, even if found, it would be very easily impregnated with sulphuretted hydrogen, from the abundance of pyrites occurring in these strata, and thus be unfitted for many uses. Limestone, on the contrary, which is very insoluble, experience teaches us, yields a very pure water.

Other districts, where water has been bored for, shew a similar geognostic constitution to the Pas-de-Calais. M. Garnier notices this, with regard to Boston, in America, and Sheerness[3], in England. London, where many sugar-works, distilleries, and breweries, have, for a long time, been principally supplied with water from Artesian wells, lies in the middle of a basin-shaped hollow, the fundamental rock of which is a limestone belonging to the chalk formation; which also forms the heights in the vicinity, and which is covered, though at times not immediately, by a water-proof clay. The wells, which are not sunk to this, the *London clay*, give abundance of clear, but mostly very hard, water; while those which penetrate through the London clay, into the subjacent *plastic clay*, a formation immediately covering the chalk, and consisting of alternating beds of sand, clay, and boulders, yields a very soft and pure water[4], which, on piercing this clay, often ascends with such violence, that the workmen have scarcely time to escape[5]. Here the plastic clay seems to be either the conducting medium, or the reservoir of the water yielded by the chalk. Paris is known to be situate in a district whose geognostic relations are almost identical with those of London, and, therefore, we cannot wonder that there, as well as in many other parts of the north and east of France, Artesian wells may everywhere be sunk; nor can we doubt of the extension of this very useful discovery[6]. The soil of Vienna seems also to be well adapted for the purpose, as partly appears from a geognostic description of Prevost[7], and partly from the details given by Popowitch[8] of one of these springs

[3] Very pure and clear water was here found at a depth of 550 feet, under the clay, in a chalky limestone, which sprung at first 344 feet high, then sunk, and now remains 120 feet under the surface of the ground.

[4] It contains some carbonate of soda, about 4 grains per quart.—*Journal of Science*, vol. xiv. p. 145.

[5] Conybeare and Phillips, *Outlines of the Geology of England*, &c. pt. i. p. 34.

[6] Most of those which have been bored in the town and its immediate vicinity, remain under the surface of the ground, although they are often several feet above the surface of the Seine and the common wells. Among a considerable number of those which are enumerated by M. Hericart de Thury, in the *Annal de l'Industrie*, t. ii. p. 58, there are several from which the water is projected, at least at first, with great force, and not without danger, far above the heads of the workmen. This, for instance, was the case with one, which, in the year 1780, was bored in the Vauxhall Gardens, and the level of the water of which has ever since been as high as the surface. This water comes from a depth of forty yards; but on account of the stoney character of the soil, and the consequent expense, they are usually only about half as deep: and this may, perhaps, be one of the causes that permanent spring-wells have not yet been sunk.

[7] *Journ. de Physique*, t. 91. p. 347, & t. 92, p. 428.

[8] *Observations of the Physico-Economical Society of the Palatinate for* 1770, pt. 2. p. 169.

in a suburb of Vienna, if new bores do not lead to unsatisfactory results[9]. In the environs of Modena, Rammazini has already made us acquainted with one of the oldest spring-wells of this kind[10]; and from Shaw, we learn, that, even at Algiers, in the village Wad-Reag, appearances, exactly similar to those in the Comtè d'Artois, are to be sceen[11].

The number of these examples might certainly be increased; only the few which are already adduced, and the frequency of the geognostic relations, which we have seen to be conditions to the boring of Artesian wells, will sufficiently justify the conclusion, that wherever these relations occur, we may calculate on meeting with a spring of water. By no means, however, ought the vain hope to be indulged, which has been published within this short time in a very uncritical essay in the Bibliotheque Universelle, t. xxxix. p. 193 and 204, that, in every part of the earth, where we bore skilfully, a fortunate result may be expected.

Even in a district of the proper constitution, the meeting with these springs depends, in some measure, on accident. Where, for example, we must sink into the limestone itself, the result is naturally dependent on our meeting in time with a vein of water or not. Thus Garnier mentions, that an inhabitant of Bethune, after he had penetrated through 70 feet of alluvium, and 30 feet of limestone, met with a spring which ascended to the surface; while a neighbour, whose shaft almost touched that of the former, met with no water, although he had penetrated 70 feet of sand and clay, and then 105 feet into the limestone, so that he was altogether 75 feet deeper than his neighbour. In the citadel of Calais, they were obliged to carry the shaft to the depth of 110,5 yards before pure water was found; what was met with before this, was saline and brackish. The same is the case in England, where, at least near London, they are not sunk to the chalk; the depth of the stratum which leads the water is very different. Mile-end is 36, Tottenham 70, Epping 340, and Hunter's-hole 410 feet above the level of the Thames; and, in the first place water was found 70, in the second 60, and, in the third, 80 feet above the same level; but in the last situation, 130 feet above it.—(*Conybeare*, n. a. p. 36.) It is not unfrequent, again, to cut across several veins of water with one boring-shaft. This was the case in a well at the brewery of Messrs. Liptrap and Smith, a mile east of London, where, partly by digging, partly by boring, a depth of 370 feet was reached. The first spring was found above the London clay, the three following under it in the plastic clay, and the last in the limestone, 123 feet below its upper margin. The springs which rose from the plastic clay, all ascended to the same height, namely, to high-water-mark on the Thames, which is there 36 feet under the surface of the surrounding country.—(*Conybeare*, n. a. p. 45.)[12] Likewise, in sinking a well in St. Owen (as mentioned in the Globe, No. 54, for this year,) five different veins of water were intersected.

Of the last case, M. Hericart de Thury mentions the curious circumstance, that an already existing Artesian well, in the vicinity of which the new one was sunk, was not at all affected by it[13]. Both together yield about 700 cubic yards of water in 24 hours. A similar case, where two adjoining springs do not appear to have disturbed one another, is mentioned by the same author, in the *Annal de l'Industrie*, t. ii. p. 63. At Epinay, near St. Denis, in one of the highest points of the park of the Countess Grollier, 16,5 yards above the mean level of the Seine, two wells were bored at the distance of a yard from one another, each of which yielded from 35 to 40 cubic yards, or from 38 to 39000 litres of water in 24 hours. The source of the first was at a depth of 54,4 yards, and its surface remained 4,55 yards under the surface of the ground. The same was the case with the second, when it was sunk to an equal depth; but after the boring was carried to 67,3 yards, the water rose 0,83 yards above the surface of the ground. In London, phenomena have even occurred that indicate very distant wells to stand in a certain connexion

[9] Riepl, in an appendix to the German translation of M. Garnier's work, p. 162.

[10] *De Fontium Mutinensium admiranda scaturigine*, of which an abridgment is given in the *Act Erudit.* of 1692, p. 505. Also Leibnitz, in his *Protogæa*, p. 75, expressly speaks of it.

[11] Delametherie, *Thearie de la Terre*, t. iv. p. 246.

[12] The water thus in no way rose 36 feet above the surface of the ground, as stated in the appendix to the German translation of Garnier's work.

[13] It is not strange that, as was here the case, the boring-iron was strongly magnetic. Even rods of iron at rest, in a perpendicular position, become magnetic; how much more must it be the case in an operation, when, in this position, it is subjected to violent shocks? This magnetic property of the boring-iron is a very common appearance.

with one another. Neither is it striking, that on the sea coast, where ordinary springs are often regulated by the ebb and flow of the sea, wells of this description should be subject to a similar disturbance. M. Hericart de Thury mentions this of a well bored to the depth of 17 yards at Noyelle-sur-Mer.—(*Annal de l'Industrie*, t. ii. p. 66.) At time of ebb, its level is 2 yards under ground, while at flood, it is on a level with it: a very ingenious valve has, therefore, been constructed, to maintain the well even during ebb at the higher level. Similar oscillations also occur in the Artesian wells at Abbeville, besides others at Dieppe, Montreuil, Department of Calvades, and the United States.

What extensive fissures the water here and there must fill, is not only demonstrated by the magnitude of many of these springs, but also by a circumstance mentioned by M. Garnier, on the authority of M. Hericart de Thury. In a brewery at Paris, near the barrier towards Fontainbleau, a well, 20 yards deep, ceased to yield any more water. They, therefore, resolved to sink the shaft deeper. But a depth of 19 yards was scarcely reached, when suddenly the borer sunk down into a crevice for more than 7 yards, and would have been inevitably lost, as even then it did not reach the ground, if fortunately a cross bar of wood had not been passed through the eye at the top of the instrument. The boring machine was tossed to and fro, as if it was moved by a large body of water, and when, after much difficulty, it was drawn up to the opening, the water suddenly sprung 10 yards above the heads of the workmen, so that they could scarcely escape quickly enough, and were obliged to leave all their implements in the well. Ever since, the water has stood 12 yards above the circle, which serves as a foundation to the wall of the well.

This irruption of the water, on first piercing these subterranean reservoirs, is often very violent, and is no small proof of the copiousness of many of these wells. Some striking examples of this are quoted from England in the Bibliotheque Universelle, t. xxxix. p. 199. A Mr. Brook had sunk a bore in his garden 360 feet deep, and 4,5 inches in diameter, from which the water was discharged so copiously, that it not only overflowed the whole yard round the house, but also submerged the adjoining cellars. The damage was so great, that the neighbours lodged a complaint, and the police were required to interpose. Two men now tried to close the bore with a wooden peg, but they were constantly driven back by the violence of the water, even when a third came to their assistance. They were equally incapable of restraining the water by an iron-stopper. At last they took the advice of a mason, and planted several tubes of small diameter over the bore, and thus succeeded at last in mastering the water.

At a Mr. Lords's, in Tooting, where a bore had been closed, the water worked with such violence under the ground, that it burst forth in a space 15 yards in circumference, and certainly the walls would have been brought down if free vent had not been given to it. This spring, say the informants, on account of the height of its jet, and the quantity of water (600 litres per minute,) is worthy of being in a public square.

The stream of a well belonging to a neighbour of Mr. Lord, drove a water-wheel of 5 feet in diameter, and this again set a pump in motion, which carried the water to the top of a three-storied house.

Even in the north-east of France these overflowing and springing wells are by no means rare, as is seen from M. Hericart de Thury's notice in the *Annal de l'Industrie.* At Kreutzwald, in the department of the Moselle, one has been sunk 60 metres; at St Quentin, in the department of the Aisne, there are two similar ones which flow over their brinks; further, at Prix, near Mezieres, there is one 143 yards deep, which rises about 0,5 of a yard above the ground. At St. Amand, in the department of the north, were three wells, bored to a depth of 45 yards, the water of which sprang a yard out of the ground, and has never diminished since[14].

[14] A remarkable circumstance, although not immediately connected with Artesian wells, is related by Hericart de Thury, of the sulphureous spring of Bouillon, near St. Amand. In the year 1697, when they began to repair the reservoir of this spring for receiving the fresh water, such a sudden disengagement of sulphuretted hydrogen took place, probably from another direction being given to it by the masonry, that an immense mass of water, mud, and sand was projected. It was curious enough, too, that several coins of different Roman emperors appeared at the surface, and more than 200 images, sculptured in wood. Most of these were much defaced by lying long in the water, yet M. Bottin believes, from his memoir in the *Memoires de la Societé Royal des Antiquaries de France*, that, at the time of the introduction of Christianity into these places, they were thrown into the well from fear of the zeal of the holy Amand, Bishop of Tougres.

At Rieulay, in the valley of Scarpe, towards the end of last century, in searching for coal, they came on a stream of water, which sprang to the height of a yard above the ground, as thick as a man's arm, and yielded enough of water to drive an adjoining mill.

Also, at Gonnehem, near Bethune, in the department of Pas-du-Calais, a mill-wheel, 3 yards in diameter, was driven by the united water of four wells, bored to the depth of 45 yards, and thus 200 kilogrammes of meal were ground in 24 hours. The water of these wells rose 3,57 yards out of the ground.

Equally noted for their abundance, as for their utility, are those at Roubaix, near Arras. This little town was in danger of losing, from want of water, its principal support, the silk-spinning and dye-works, when M. Hallette succeeded, after much difficulty, in boring several very copious wells, one of which even yields 288 cubic yards of water in a day, or double the power of a steam-engine of 20 horse power. The *Societé de l'Encouragement* in Paris has rewarded the meritorious M. Hallette with the prize of 3000 francs.

Lastly, the overflowing or spouting wells, those which have been lately discovered at Amalienbad, near Laugenbruck, in the county of Baden, are worthy of notice. They are bored 58 feet deep, and yet ascend 8 feet above the surface of the ground. Their water, which amounts to 460 tierces a day, is very free from salts, as are the most of the Artesian wells, but is distinguished from them by containing sulphuretted hydrogen, evidently from the bituminous pyritous slaty coal from which this spring seems to rise. The temperature of this artificial natural sulphureous water is 55° to 56°½ F.—(*Berlin Nachricht, v. 9, Oct. of this year.*)

Agreeably to the design of this review, we have hitherto spoken chiefly of those appearances which relate to the boring of fresh water springs. The same phenomena, however, are afforded by salt springs, and often in a very marked manner.

We shall here only notice one of the most striking examples of this description, an event which marked the opening of the salt shaft at Dürrenberg. By the perseverance of the superintendent of the salt works, the Counsellor of Mines Borlach, the shaft had already reached a depth of 113 fathoms, when, on the 15th September, 1763, the salt water suddenly burst through a layer of gypsum 23 inches thick, which formed the floor of the shaft; and notwithstanding the most active working of the machinery, within three hours and a half it had filled the whole depth of the shaft, which was 791 feet, and 5 ells square, and overflowed its margin. One of the workmen was caught by the salt water, and, wonderfully enough, raised 252 feet high in the shaft without being hurt. After more than 40 years, in the years 1802—1805, the salt spring still exerted such a pressure, that, according to the calculation of the Inspector of Salt-works, Bischop, it could rise 5 ells above the highest margin of the shaft. Also at Kösen the salt water reaches the surface, from a depth of 86 fathoms, (516 feet)[15]. Similar overflowing wells have also been lately bored at Nauheim, in the Watterau, at Unna, in Westphalia, and in several other places.

This cannot be the place to prove the advantages of bored wells over dug ones, in an economical point of view, nor in what way they may be most advantageously employed; this must be left to technical treatises; besides, complete information on every thing which is important, in a practical point of view, may be obtained from the work of M. Garnier, which we have so often quoted[16]. Yet a few historical points regarding the boring of fresh water wells still remain to be mentioned. It is unknown who first turned the miner's boring-iron to this use[17]. Ramaz-

[15] Geognostische Arbeiten, v. J. C. Freiesleben. Baud. ii. S. 208.—Bischof in Karsten Archis. Baud. xx. S. 17.

[16] *R. F. Selbman* on the Use of Miner's Boring-irons, Leipzig, 1823, contains a very particular detail of every kind of boring apparatus, as well as an enumeration of the principal works, from which further information may be derived.

[17] Possibly the spontaneous irruption of these waters first attracted attention to overflowing wells. So it happened, in the year 1821, at Bishop Monckton, near Ripon, England, after a rattling noise of the ground, the water burst forth, and immediately excavated a shaft for itself, which, on the evening of the same day, had several feet in circumference, and, on sounding, showed a depth of 58 feet.—(*Jour. of Science*, v. xi. p. 406.) Similar appearances have also occurred in the sandy soil of the Marck of Brandenburg. Thus, for example, in 1756, not far from Ziesar, at the foot of the sandy ridge which lies on the left bank of the Bukan, a spring burst forth with an immense noise, which the old people still remember perfectly. It has since flowed with undiminished violence, and its quantity of water is very great, as is the case with all those of this region. By the continual washing of the loose sand, a large excavation has been made, and the spring itself has retreated considerably, and has formed a basin of more than 500 paces long, which sufficiently shews that the source of the water is very deep in the sandy ridge.

zini's work, which was published in the year 1691, gives ample proof of the art having been practised from the earliest period in the environs of Modena. From thence it spread to France, and, as mentioned in the late programme of the Royal Society of Agriculture of Paris, the merit of their introduction is due to Domenico Cassini, who was invited from Italy to the court of Louis XIV, and was shortly after elected a member of the Academy of Sciences. The earliest information that we possess of any well being bored in the Comté d'Artois, is, perhaps, that given by Belidor, in his *Science de l'Ingenieur*, liv. iv. chap. 12. He saw, in the year 1729, in the church of St. Andre, a well of this description, which gave 20 yards of water in an hour, and rose a yard above the surface of the ground. Near Paris, according to M. Hericart de Thury, the first Artesian well was sunk at Clicky, in the middle of the last century. It reached the depth of 98 feet, and rose four feet above the level of the Seine. In Germany, where the art of boring mines has been known for more than a century, and where Leopold (Schauplatz der Wasserbaukunst, Leipzig, 1724,) has applied them to the boring of fresh water springs, this use has been made of them, but not so much as to the finding of salt springs; yet it may be expected, from the zeal with which the search for Artesian wells is carried on in France, that similar works will be carried on in other countries.

POGGENDORF.

II.—*On Value.*

§ 2.

If from man's entire dependance on food, and from the circumstances, with regard to food, under which it has pleased his Creator to place him on earth, there must be, in all societies, a large proportion of the population in want of the means of subsistence;—if needy men must always be willing to make a sacrifice of their utmost exertion to obtain subsistence; if the utmost exertion of men, on the average, be a certain quantity; and if there can be but one opinion amongst them regarding this exertion; then we have something fixed and determinate to enable us to know the sentiments and feelings of men regarding food. In other words, we have a key to the knowledge of the positive value, of the primary description of wealth, which the mass of the people must entertain; and we have the means of knowing also, how much of labour, or of its products, food will command. When we are told, such and such a product cost so much labour of the poor, we may thence learn its real value; because we can thence ascertain what sacrifice the mass of mankind are agreed in thinking its possession is worth.

Now this appears to me a very different description of knowledge from that which the mere comparison of two products enables us to acquire. This is learning the real value of products; whereas our ascertaining, that one certain valuable product is exchangeable or not for another valuable product, teaches us, not the nature of value in either product, but that value being a quality already existing in both products, it stands either in the relation of equality, or in some other relation within the two different bodies. And yet we are told, that the value of products, relatively to one another, or exchangeable value, is the only value which can exist!

Without an ultimate reference to the feelings of mankind, with regard to products, and without the existence of the means to determine what the opinions of men are, concerning some one class of products in particular, there could have existed no knowledge whatever of appreciable value.

When, as has been usual with those who treat of value, as being merely relative, and as originating solely in labour, we trace the value of products to the labour of which they are the result; and not to that labour as an index to the feelings and opinions, which moral agents, the actual percipients of value, must necessarily entertain regarding an existing quality; what do we effect? We find that labour is a product of labour, and that the value of the labour in the market must be determined by the quantity of labour of which it is the result. If to raise a sufficiency of food to support labour, more and more labour may continually be bestowed in the acquisition, the value of labour must continually rise; and thus the value of labour rising as the food which supports it rises, there will be no end to the ascending process. We are, in thus reasoning, giving, as it appears to me, labour credit for being, at one and the same time, both the cause and the effect;

and in this mode of treating the subject, the nature and causes of value are not rendered a whit more intelligible by our stepping back from the product itself, to the labour which produced it.

But when we consider that certain products have a source of value, distinct from the labour of which they are the result,—a value depending, as it does, on the original constitution of man, and on the nature of those circumstances in which he was placed on earth by his Creator,—we are enabled to come to rational conclusions regarding the process which men pursue in obtaining and appreciating valuable bodies; and we ultimately reach the limit which determines the point beyond which the successful exhibition of labour in obtaining valuable bodies becomes impossible. We learn this also, that products the result of no labour whatsoever might be possessed of appreciable value; and that mankind would work as intensely to obtain them as they now do, when these products are only obtainable in return for the exhibition of some labour. If, for instance, in place of some labour being necessary, as at present, for obtaining, from the earth's surface, those items, constituting primary wealth, on which our subsistence depends, they had been procurable merely for the appropriation; still, as population (doomed, as man is, to increase on a surface of limited extent) must come up to the means of subsistence, those who first possessed themselves of the land, and of the means of making this mere appropriation, would presently enjoy the power of withholding that food which was in excess to their own consumption; and this they would unquestionably do, unless those who subsequently came into being, made it worth their while to bestow this excess upon them. These last being, of necessity, willing to make specific sacrifices for the possession, a sufficiency of this food, although the product of no labour whatever, would then, as now, have become possessed of a determinate value, and would have been acknowledged to be worth, whatever other products of labour those had to offer, who had devoted their utmost exertion to obtain a subsistence, through the means of these other products.

In this case, the whole gross reproduction, except the seed, would have gone to the formation of rent, in place of the proportion only, which, in the present circumstances of the world, goes to the formation of the landlord's share. No part would then, as a portion does at present, have gone to the formation of agricultural wages and profits; all would have been rent.

The mention which has just occurred, of the labour which will inevitably be bestowed upon other descriptions of wealth than food, leads us naturally to the consideration of the nature of the appreciable value inhering in what may be termed the secondary description of wealth.

After productive arts have made some progress, and the means have been secured to man, of obtaining, in abundance, that particular substance, which, alone, of all those equally essential to his existence, had been, in a measure, withheld; and after his knowledge of the productive process has become such, that he is enabled, by his exertions, to secure, not only sufficient of this substance for his own immediate use, while engaged in this important employment, but some greater quantity; then his mind may be turned to the consideration of the means by which gratifications of a more secondary nature may be brought within his reach.

As it is not now necessary to his existence, that he should devote unremitting labour to the production of food, some leisure is enjoyed. But man, subject to minor inconveniences, and imbued with a love of action, esteems this leisure less than the means of guarding himself from the other passing evils, to which he is still exposed. He may, besides, have perceived, that, with the aid of somewhat complex instruments, the great business of agriculture may be vastly facilitated. The leisure which the silent and continuous action of the principle of reproduction, and increase in vegetable products insures, now that this powerful agent is vigorously acting in co-operation with man, is not therefore spent in sloth, but is devoted to the provision of houses, clothing, instruments of agriculture, and other manufactures. At this time these manufactures, although not absolutely essential to his existence, still, as being the articles in the want of which his greatest present inconveniences originate, will come to be held in esteem when obtained, they will certainly be more than equivalent to the sacrifice of ease sunk in their acquisition, or they could not be the voluntary product of man's exertion.

After the primary description of wealth is secured in abundance, and after it has ceased to be the paramount and overwhelming want, to the gratification of which all else must yield, man has leisure to look around, and studying the peculiar characters of the different objects with which he is surrounded, he may perceive properties existing in these, wherewith the smaller evils which are now uppermost in his apprehension may be averted; this, therefore, and the manipulation necessary for fitting natural products for his particular purposes, form his occupation, while the principle of reproduction and increase in food silently performs its peculiar and important office.

But man's numbers, by an immutable law, are destined to increase to the utmost, while the means are procurable of obtaining nourishment. The agriculturalists who have located themselves on, and reclaimed the soil, will, therefore, shortly find numbers ready to offer their utmost exertions to prepare the products of mere manipulation, if they will but give the food in exchange, which it is in their power, by devoting themselves exclusively and unremittingly to agriculture, readily to obtain. That product, therefore, in secondary wealth, the preparation of which occupies the entire time of these poor manufacturers, and for which the agriculturalists willingly give food sufficient for their subsistence, being convertible at will into that primary wealth, for the possession of which, the needy must ever be ready to offer their utmost exertions, will necessarily come to stand in the same general esteem as the primary wealth itself, which it is the means of procuring. Its real value will, therefore, be equal to that of the food which can be obtained for it, in the estimation of all the indigent; and this class, from the influence of the law of population, must, in all ordinary cases, be the vast majority of the society.

But under the supposition that agricultural production had got so greatly the start of the agricultural population, as to leave it at the option of these to labour or not in obtaining the fullest results of reproduction, which the extent of soil in tillage might be made to yield, it may be thought that the value of the primary description of wealth will then suffer deterioration. But it must be remembered, that if we even suppose the original agriculturists to have been brought suddenly into these happy circumstances, it by no means follows that they will take any interest in prosecuting production to such an extent, as considerably to lower the value of food. On the contrary, after a manufacturing class has been established, if the agriculturists find they can obtain all the wrought produce they require, and all the food they require besides, for a less exhibition of exertion than their utmost; in place of spending their spare time in any description of production whatever, they will occupy it in the pursuit of amusements; and if the further pressing of the population overtake this disposition to leave the soil untasked to its utmost ability, those of the rising generation, who cannot live by manufactures, will readily offer to take upon themselves the labour of fully working the soil, on any terms which the original agriculturists may be willing to accept; and hence a landed gentry is created, interested, not in the greatest possible extent of actual reproduction and increase, but in such disposition being made of the soil, as suits most readily their peculiar wishes and fancies. If over-production of food should now cause a glut, and disable, therefore, the tenants from making good their rents, the landlords will inevitably take measures for preventing its frequent recurrence; and thus, whatever the progress of cultivation, and the arts of agriculture, rendering it possible, for the time, to raise more than will support the number of manufacturers, in feeding whom the agriculturalists find an interest; the proprietory right to the soil, and the private views of the parties concerned in cultivation, will, even under these extraordinary circumstances, of food outstripping the progress of population, tend effectually to prevent a permanent reduction of the value of this primary description of wealth.

But we are not, from the experience of the world at large, justified in contemplating the frequent recurrence of such rapid progress in agriculture as the above: except in the case of new, fertile, and unbroken countries, falling into the hands of a people peculiarly well versed in the arts of agriculture, it cannot possibly occur. These events must ever be looked on as exceptions to the ordinary course of production, which this century, and the next, may have to witness; but of which, succeeding generations will know nothing, beyond the mere report. Population must, in all ordinary circumstances, be keeping equal pace with production; and therefore, the full value of food must continually be maintained.

The value then, from what has been written, would appear, in primary wealth, to be original and independent; while the value attaching to the secondary descriptions of wealth, is, like the wealth itself, secondary and contingent. The value attaching to the products of manipulation is secondary; because it is merely on account of the manufacturers, finding the prosecution of their business to be an avenue to the possession of food, that they submit to the drudgery of working; and hence also it is, that their products come to be generally considered, as of equal value, at least to the purchasers, as the food which is given in exchange for them. It is contingent, because it is only while the manufacturers supply that quantity of things exactly, and no more or less, which the agriculturers are eager to obtain, that their products are convertible at will into that primary wealth, for which, particularly, the manufacturers ever offer a demand.

It is important to keep in mind the secondary and contingent nature of the value attaching to the secondary description of wealth; because the neglect of this consideration subjects us to the errors, regarding the impossibility of over-production, which vitiate the reasonings of M. Say, Mr. Mills, and all their followers.

For instance; at the time when agriculturists began to have something to spare beyond what satisfied themselves; and when other needy classes were springing into being, willing to give wrought products for this excess; if these needy classes, knowing no art but that of weaving coarse mats and blankets, continued, when once set in this train, to weave these, without any reference to the wants of the agriculturists; it is certain, that these items of wealth would, with the excess of supply, lose the greater part of their value; and that there would be no sufficient demand to render them all convertible at will into food.—In this case, can we suppose, that the mat makers, and the blanket makers, could, by proceeding with, or increasing their exertions, create a reciprocal vent for their respective wares; and that the value of these wares would be restored, by their mutually bartering their respective products? Or can we suppose, that the agriculturists, already fully supplied with mats and blankets, would, because more mats and blankets were made than they required, set to work, more energetically, to increase their agricultural produce? If they did so, it must be from the mere wish to save the starving manufacturers; a motive upon which we are not, I apprehend, justified in counting. In this case, the mat and blanket makers might double, nay treble, their exertions; and only bring distress more inevitably upon themselves. This, to be sure, is a state of things which could not possibly last; because these poor people must inevitably starve; but while it did last, it would be a case of over-production, and loss of contingent value in secondary wealth; which no efforts directed to the further production of secondary wealth could relieve; and which carried along with it no inevitable inducement, for that extension of production, in primary wealth, which alone could alleviate the existing distress; but which, on the contrary, by enabling the agriculturists to obtain the mats and blankets for less than before, would induce them to produce less than before: and so long as such a state of things held, mats and blankets would continue valueless.

I am very well aware, in the more advanced periods of production, when items of secondary wealth have assumed almost every possible form, and when habits have been engendered, of looking on a vast variety of these as being essential; that after production of all kinds has been adjusted, and after the quantities of primary and secondary wealth have been nicely adapted each to the other, there can be no loss of value in almost any one item of secondary wealth, unaccompanied by a loss of value in the primary wealth presently in hand. But this may be rather considered a sympathetic affection, proceeding from the closeness of the connexion between the two, than from any permanent necessity for the primary wealth losing its value. So long as there exists the same population to consume it, as had been in being before this change was brought about, the value of food must still remain. There would, of course, be a necessity for vast changes in the disposition of society, before a similar population, to that which obtained food in exchange for wrought wares, could obtain it, from the persons raising it, by gift or coercion. Certain it is, however, that secondary wealth might well nigh cease to be formed; and that the value of the primary wealth might be sustained, merely by altering the terms on which the agriculturists shall be allowed to cultivate the soil. In Europe, the land is held by private individuals, who enjoy the power of dictating to others the extent and nature of the culture which shall be pursued; the inducements held out, in that quarter of the world, to tempt landholders to give up the use of their land, that the greatest quantity of primary wealth may be realized for the benefit of society, are the wrought products which, in endless variety, are capable of ministering to every conceivable wish or whim: the full cultivation, and growth of primary wealth, is there effected through the means of secondary wealth.

In Asia it is different; the land belongs to the sovereign, and is given in use to cultivators, who are permitted to spread over the face of the country in such numbers, as shall carry cultivation to that point, at which the largest net reproduction may be realized, wherewith to enable the sovereign to pay the only other classes existing in the society, the army, and the retainers of the court. In both cases the value of the primary wealth is equal; because it equally commands the labour of the vast majority, namely, the poor: and it is possible to conceive, particularly in the more mild and fertile regions of Asia, that, in both cases, the quantity of primary wealth realized may be pretty nearly the same. But the full cultivation of the soil would, in the latter case, be the effect of the necessities of the poor and needy cultivators; and probably of coercion too; and not, as in the former, the effect of a voluntary impulse, created by the prospect of that enjoyment which secondary wealth is calculated to afford.

What I have here been endeavouring to illustrate is, that the value of primary wealth is dependent on nothing but the inevitable demand which must exist, in all ordinary circumstances, for the quantity of food procurable; and that, however the connexion of the two, after society has been constituted on the more complex system, may render temporary losses of value inevitable; there still are circumstances, in which the value of primary wealth may exist, independently of the existence of secondary wealth:—while, on the other hand, the value of secondary wealth could, manifestly, never exist, except in subordination to the existence of primary wealth; and that even its total annihilation might be effected, without necessarily producing any permanent effect on the value of primary wealth.

Provided then, the products, constituting the secondary description of wealth, be accurately accommodated to the wants of those in whose power it is to obtain wealth of the primary description; the value inhering in those articles, when in the market, must be of a quality as positive, as that which inheres in primary wealth: we may, therefore, reason regarding this description of value as existing in secondary wealth, in all ordinary circumstances of supply; with as much justice, as we may reason upon its existence in primary wealth.

All products constituting secondary wealth are really valuable, as they are, in practice convertible, at will, into food, the article regarding which there can be no change in the opinions of men. In practice then, a knowledge of the labour any product has cost, to whatever class of production it belongs, leads us to a knowledge of its value, through the medium of that which is invariable[1] in its value; and it is entirely owing to the chain of dependencies, ending where it does, in the sentiments which must be entertained by man, regarding these first of valuable existences, that value could be known; for had there been no necessity for each person's possessing a specific quantity of any one product, there could have been nothing in existence regarding which all men were agreed. A product might have cost 10 men's labour for a year; or 10,000 men's labour for 10 years; yet as the esteem in which it would ultimately be held, would be uncertain—as all value would, then, depend upon whim and caprice, no two persons agreeing, with regard to the utility or the necessity for the possession of any thing whatever; the knowledge that such and such a quantity of labour had been bestowed upon certain products would have been no guide to a knowledge of its value, or to a knowledge of what the mass of mankind would give to possess it.

What would, in this case, be true of one product, would be true of all products; and nothing could, in consequence, be certain of finding a market. There would exist no steady grounds of inference and calculation; each person, wishing for a wrought product, would be under the necessity of making it for himself; and production would never, therefore, rise beyond that original state, in which it exists amongst the rudest savages.

But when the products brought to market, must, if they are really in demand, be ultimately exchangeable for one thing, regarding which all men are agreed in opinion, the case is altered; and determinate valuation may proceed.

By this last link in the chain of dependencies, connecting the cost of a product, with the sentiments which are entertained regarding it, the positive value, or the relation of the product to man himself, is made known; and as the labour, which will command a sufficiency of food in the market, is fixed, and is consequently

[1] By the invariability of the value inhering in primary wealth, as used throughout the Essay, I do not mean that food may not be affected by passing events; such as changes in the seasons, or changes in the circumstances of society; but I mean that there is a point to which it must continually return, after the influence of passing changes shall have ceased to be felt: just as I would say, that a pendulum invariably settles in the vertical position, whenever the power which maintained its motion ceases to act.

held in the same general esteem in all times and places, the quantity of producing labour being known, we may know, at the same time, the relative values of products, one to the other. But this knowledge, although it will be of use to those engaged in barter, will convey but little information to the political economists; whose aim is to learn, not how products stand to products, but how those products stand to man, which form his wealth and revenue.

The equality or difference in the cost of producing two articles, will, after their real value is known and acknowledged, be the cause of their relative values being equal or different: but the equality or difference is no cause whatever of the existence of their real value. Either of the products might have been possessed of real value, although there had been no possibility that the other should exist; and this, simply, because either the one or the other, even if it were the only product in the world, might be calculated to operate, so steadily in determining the will of man, as to warrant our drawing inferences, and making calculations concerning his subsequent conduct with regard to it.

The cause of real value then, in the primary description of wealth, is not, let me repeat, the quantity of labour of which it may be the result: but the quantity of labour which it must command, will, in practice, be the index, by which its existence is known: for food, it has been seen, might be obtained without labour, and is, in fact, in practice, obtained with much less labour than it will support and command: and yet it is possessed of value, determined by causes quite distinct from the labour bestowed in its production.

If then absolute value be, in reality, but an affection of the mind of man regarding existing products, which must be constant, how can any thing be predicated concerning it, from a study of the mere relations in which products may stand to products? Yet all who refuse to admit the existence of positive value, fall, of necessity, into this gross error of reasoning regarding one subject, on premises drawn from the consideration of another subject, of a nature completely different: and this we have Mr. Malthus's authority for stating, all political economists do: for he declares, in his latest work on Definitions in that science, that no writer, he has met with, "has ever appeared to use the term value, without an intelligible reference, expressed or implied, to something else."

Perhaps it may be said, as I have already remarked, that as all writers, in the expressed or implied references here adverted to, keep in their minds, when comparing the value of products, labour as the ultimate index of valuation; their conclusions on this subject may coincide with mine; labour, in my system, being likewise one of the intermediate steps by which I arrive at my conclusions regarding real value.

But as none of these writers directly admit another source of value than labour—as they, on the contrary, insist, that all descriptions of products must be valuable, merely because of there being so much labour fixed or realized, and not because of their being calculated to produce certain effects on the minds of men—and as they generally admit that, in process of time, more labour may become necessary, for bringing labour into existence, than at former times was necessary, which is, in effect, treating labour as being at one, and the same time, as being both a cause and an effect; I maintain, thati n studying labour as the index to value, under such circumstances, they do no more than study the relations of products to products; for with them, the value of every description of product, and the labour worked up in each, are convertible terms; and looking no further than the products themselves—neglecting, altogether, to come up to the real source of valuation—namely, the thoughts and opinions of mankind, they are continually adrift on a wide sea of uncertainty, without rudder to keep them in their course, or pilot to be their guide.

I shall now devote a few pages to the establishment of the principle, that what has here been premised of the value of primary wealth, in the earlier stages of society, is equally true during all successive periods.

It may, for the sake of keeping the subject clear in the outset, be admitted, as a first principle, that man's personal strength and power of bestowing labour is the same during all stages of production.

With the same view it may be further assumed, that man is, at all times, equally under the influence of the procreative power of nature; and consequently, that his numbers must constantly be making a close approximation to that amount, for which subsistence is obtainable.

If then a man's utmost labour be a fixed quantity, and if he must ever be exerting that utmost labour, it follows, that the produce obtainable with the aid of labour, to which he owes his support, must always be held in determinate esteem, and must be possessed of a positive and certain value, whether there happen to be in existence or not other products besides that which he then obtains.

The quantity of food obtained by the naked savage, struggling for subsistence in the wilderness, whether in the pursuit of game, or in endeavours at the possession of vegetable nourishment, must have been, on the average, merely equal to his most urgent wants. Had it been less, he must have perished; and with the appreciator of the value of food, the value must have gone also.

Had it been more, the means of rearing an increasing offspring must have been enjoyed; and, stimulated as he is by the procreative power, numbers must have continued on the increase, till so much only was again obtainable by each of this increased population, as served for supporting the existence of each;—and, at this time, each individual of the increased population, would be found, inevitably holding the means of his subsistence in the same esteem, and looking upon them as being worthy of the same sacrifice to insure their possession, as those had done who formerly constituted the smaller population.

When capital has come into full employment, and when land has been appropriated, the remuneration of the lower orders, and the esteem in which that is held by which they are supported, must still be the same: for their numbers must still be pressing on the means of subsistence available to their use.

The capitalist may have a great income; the revenue of this class not being like that of the labourers, in a certain proportion to the numbers of the class, but to the extent of the productively employed capital of each. There may, therefore, be in the hands of each a large stock of food and necessaries ready to be distributed amongst the labourers, in return for the produce of their exertions. So also may there be large funds in the hands of landholders. But the capitalist expends his income in feeding only so many labourers, as tend to keep his personal enjoyment at the highest; and the landlord permits the cultivation of his land to such extent only, as leaves the greatest disposable surplus available for his peculiar use; and that in a state too the most readily convertible into items of secondary wealth: and the labourers' numbers pressing, as before, upon that amount of subsistence, which these classes can be tempted to bestow, in exchange for the results of the utmost exertions of these poor people; the estimation in which that is held, which suffices for the support of a human being, must be then as great as it was in the days of pristine rudeness and poverty, and must equally command the utmost exertion which a human being is capable of bestowing.

At either time, when the reward of labour is greater than a mere subsistence, the influence of the procreative principle, in causing increase of numbers, must be felt:—when it is less, numbers must experience reduction. What supports then the lowest individuals of society, whether of savages, or of civilized beings, must ever possess a positive and determinate value—a value independent of the caprices and whims of men; such value, in fact, as those cannot fail to place on it, who must labour their utmost in obtaining it; and which, in consequence, must, when there happens to be other descriptions of products in existence, always be equal to that of any other of these products, on which industry is for an equal term engaged, with the view, in all cases, through the means of the new products, to securing the acquisition of a sufficient subsistence.

But it may be objected, that my present reasonings regarding food, and the positive value it enjoys, are just, only when population has reached its fullest amount—when the world has been fully occupied, which we well know it is not, and never has been: nay we know, that the most populous countries, are still far short of the numbers they may ultimately contain; whereas other writers are employed in the study of what might be passing in a progressive world.

Although I most fully admit, that the world is not now so populous as it will eventually be; and that no country, or province, perhaps, contains the numbers to which it may ultimately yield subsistence; still I maintain, that the immediate influence of a limit to wealth and population must ever have been felt, in every country wherein the increases to population were not so great and so rapid as the unimpeded influence of the procreative principle would have caused.

The term requisite for the doubling of population is supposed, on good grounds, to be less than 25 years. Wherever, then, under the supposition that mankind are under the influence of the principles of production and population alone, a greater length of time has actually been found to have been occupied in the doubling of population, there the increase of mankind has actually been counteracted, by the operation of such causes, as, for the time being, acted as impediments to the progress of wealth; and if the progress of wealth were not such, as to provide for the unchecked progress of population, then there must have existed a proportion of the community, the lower orders, who were born to no possession; whose power of increasing its income was limited; and amongst whom the labour necessary for realizing a maintenance, for such a family as tended to an increase of population, more rapid than was actually taking place, must have been the very utmost the labourers could bestow.

If, then, this be the case, and under the full influence of the principles with which we set out, no other cause could have repressed a more rapid progress of population, it follows, that the food actually supporting a family, must have been of as great an absolute value to those obtaining it, as it would be when population was at a stand, and permitted the rearing of such number of children only, as supplied the place of the parents when dead. Larger families will, in the one case, have been reared, than in the other; but in both cases, the individuals, actually labouring, must have felt the same regard and esteem for what merely sufficed for support of their families; and must, in both cases, have been equally ready to offer their labour for its possession.

If one man's labour, at one time, enables him to obtain as much as suffices for the support of such family as sustaining a fixed population requires; and if, at another time, equal labour suffices to realize sufficient for a family increasing one in number, with every successive generation; then any specific quantity of food obtained, will be of less value, relatively to the labour expended, and consequently to the society, in the latter, than in the former case. But this value must be so very little less, that the difference will be barely perceptible. If, for instance, the average life of man were 60 years; and if each poor couple had the means of rearing three children, population would be doubled in 120 years; and at this rapid rate of increase, the produce of one man's labour would be only so much greater, and the absolute value of specific quantities of food so much less, than when population was stationary, by the trifle which sufficed for the support of one child. Improvements in agriculture then, from improved instruments, and machinery employed in husbandry; and the discovery of new and fertile tracts, to be cultivated with the skill and capital of an old and scientific people; if even calculated, so to stimulate the ratio of increasing population, as to raise it from nothing to a doubling, in 120 years; could have but a trifling effect in lowering the positive value, or in other words, the estimation, in which the food of a man would be held; and would, in consequence, in a very low degree reduce the command of food over labour.

But countries, I apprehend, have, in reality, become progressively more capable of yielding food to increasing numbers, as these additional numbers have sprung to manhood; and of the contingent and ultimate power of supporting increased population, previous indications have, in most cases, been hardly perceptible; the powers of the soil to yield increase, with increased application of labour, being unknown, till that labour had actually been applied; and in the particular manner too, actually found to be attended with success. Increasing numbers must bring with them increasing knowledge; and succeeding generations must be benefitting, not only by the permanent effects of all their predecessors' labour, but by the continually increasing knowledge of facts, handed down from their progenitors, the accumulated information of ages. The labourer of this generation, though he, like his forefathers, succeed to no possession, save this greater knowledge, still adds to this inheritance the results of his own experience; and his successors will, therefore, without the aid of any other possession, be capable of working to more advantage than their forefather.

From this, amongst many other considerations, we may find assurance, that the mere circumstance of tracts of country being still under-peopled, to what they ultimately may be, does not, inevitably, tend to insuring a low value of food; and that population still being scanty, forms no proof, as is assumed, of a low value of the food of the population. If then a nearly constant value be enjoyed by food during the whole of a nation's progress in population and wealth; if cultivation, in the general circumstances of all ordinary countries, must be pressing against the limit, which the existing population, and their existing knowledge of productive arts, define; and if it be also true, that capitalists can no longer employ increasing numbers of labourers, than while, besides these labourer's wages, an increasing profit, also, is realized through them, as I shall hereafter explain more at large; how, I ask, can an increasing price, and value of food be continually created, and obtained; which, by tending to a continual rise of wages, it has now become usual to consider as the ultimate destroyer of profits, and as the sole creator of rents for landlords? Additional labour, when the inevitable condition of its employment was an increased value of the produce, could not, if applied by labourers themselves, be repaid by additional returns; the soil having already been forced to feed the greatest numbers whom the capitalists and landlords were interested in supporting. Let more labour be applied, and in place of obtaining a greater quantity of produce, rendered more valuable by the greater labour of which this last increase of food was the result, the effect produced, will be a mere diminution of the aggregate, going to the use of the capitalist; of which the value, depending, as it does, on other causes than mere labour, must remain unchanged. Additional labour, if applied by capitalists, tending only to their impoverishment, it could never be furnished with the requisite advances from them, if we give them credit for being rational creatures. And if they did submit to the impoverishment, merely that more labourers might be fed, the value of the products, relatively to labour, and to what labour could command, would not thereby be increased; the extra labourers, now at work, having had made good to them, by the capitalists, the difference between what scanty increase of produce was realized by their exertions, and what sufficed for their subsistence. But the case is absurd; it could never occur.

The real value, then, of articles of the primary description of wealth, is determined, not by the quantity of producing labour, but by the quantity of labour which the products will command; and the reasons are shortly these,—that they enjoy the power of raising up their own demanders; and that these demanders must spring up, in consequence of an inevitable, and ever operating law. Man's necessary wants are as so much, say as 10; man's numbers must increase so long as each person, newly coming into being, can obtain 10. But God has ordained, that after man has gained sufficient knowledge in agriculture, each man's labour shall produce, say 15, or 20. The consequence is, that poor men, coming to no inheritance but their naked arms, the vast majority of mankind, esteeming 10 as a sufficient recompense for their labour, will be ready to give for the possesion of 10, their utmost exertion; and for this exertion, although, in agriculture, actually producing 15, or 20, they will only be recompensed by the receipt of 10; and whatever other article of the secondary description of wealth, which is actually in demand, and which is the product of an equal exertion, will be equivalent to 10 also; because *that* will necessarily be the utmost which so much unaided labour can produce.

It is worthy of observation, as a consequence of the above reasonings, that where the principle of reproduction and increase co-operates with industry so powerfully, as to give off, not only what will feed the labourer, but something besides, there must, of necessity, be attached to this produce a greater value, than the products of mere industry can enjoy; and this superior value will be in exact proportion to the greatness of the excess realized, beyond what supports the agricultural labourers. If the labourers required 10, and the net reproduction procurable from the co-operation of their labour were 20, then the produce must be of double the value of any product of equal labour, in which the reproductive principle did not co-operate. If the net production were 30, then the value of the produce would be three times as great, and so on, as that of the mere labour which had been employed, or any of its results in secondary wealth, which happened to be in effectual demand; and if the net reproduction were but 10, then the value of the labour, or of its unaided results, would be as great as that of the products of reproduction. But in this last case there could be no secondary wealth whatever—no products of mere labour; all that was given off, for the support of mankind, being merely sufficient for the subsistence of the agricultural class, there could then be nothing for the support of any other class.

What is available for the support of other classes besides agriculturalists, is great in proportion as the influence of the principle of reproduction and increase after its kind is great in food; and the difference in the value of secondary wealth, the product of mere industry, and of primary wealth, the result of industry in conjunction with the reproductive principle, will be great in the same proportion. Wherever, then, separate classes have grown up in society; and where men have been found enjoying other products than mere food; there, a difference must have existed between the value of food, and of any product of mere labour: and it will, I believe, be allowed, that this must have been the case in every society, which had made any progress whatever in civilization. What then becomes of the modern theories respecting rent,—that it proceeds from the growing value alone of food; which takes place only where population has become dense? On the existence of the constant superiority of value in food, depend not only rents, and profits; but what suffices for the subsistence of all other classes, besides agricultural labourers, depends also upon this permanent superiority of value in food.

It cannot be too frequently repeated, that the study of wealth, and human enrichment, is the study of positive, and not of relative value; all products may remain fixed in their relations one to another; all at one time costing more, and all at one time costing less exertion; while the circumstance of their all costing more or less, which would leave their exchangeable relations unaltered, would be of vital importance to the circumstances of every individual, and to the well being and comfort of society. In the one case, the positive value of each individual product would be increased; each man, seeking to obtain them, would be reduced in his circumstances; would enjoy a smaller income; or, in other words, would be obliged to make a greater sacrifice, for the possession of the articles which constitute his income. In the other case, each would be enriched, or under the necessity of making a smaller sacrifice for the possession of items of wealth. It is therefore very obvious, that those who reason, regarding the incomes and wealth of the society, through the means of the relative, and not the positive value of the products composing these, employ an instrument which may lead to false conclusions; but which cannot, in the nature of things, lead to truth.

III.—*On the Measure of Temperature, by* Messrs. Dulong and Petit.

§ 4. *On the specific heat of Solids at different temperatures.*

We may perceive by the results of the preceding experiments, that in referring the phenomena of heat to a thermometer taken successively amongst the gases, the liquids or the solids, even the most refractory, each instrument leads to the establishment of a particular law. It is not then a matter of indifference, of what substance our thermometer be constructed; supposing we wish to investigate the most simple law of heat, or if the expression be preferred, to represent the phenomena by such a series of measures as shall have the most direct relation to them. But to determine amongst them, it is necessary to ascertain how the capacities of the different bodies vary in each of the thermometric scales which we have investigated.

Since the first establishment, by Black, of the notion of *capacity of caloric*, several philosophers[1] have endeavoured to bring to perfection, the experimental methods necessary to establish this important element of the theory of heat, and to apply these methods to a great number of substances. The labours of Wilk, of Crawfurd, of Meyer, and above all, of MM. Laplace and Lavoisier, are, it is known, the most important of those which have been yet published on this subject. Deluc and Crawfurd, taking for their standard an ideal thermometer, in which the capacity should be uniform, compare its indications with those furnished by a mercurial thermometer, in order to judge of the accuracy of the latter. Their experiments are mostly confined to the mixtures of liquids, the temperatures of which has never exceeded that of boiling water. Their enquiry, in reality is but the reverse of the question, whether the capacity of these liquids remains constant when their temperatures are measured by the indications of the mercurial thermometer? The results obtained by these two philosophers are different: according to the first of them, there is a slight change in the capacity of water within the interval of the first 100 degrees. The second admits, on the contrary, that the capacity is constant. This discordance proves that within the limits in which the experiments have been made, the change of capacity, supposing the latter really to exist, must be very trifling—but such trials are far too confined to authorise Crawfurd's attempt to extend these conclusions to every temperature.

[1] *Physicien.* We have no term in English corresponding exactly to this word. *La Physique* comprehends the inorganic departments of nature, being used in opposition to *L'Histoire-naturelle*; and *Physicien* is used in like manner in opposition to *Naturaliste*. *Natural Philosopher*, which is the nearest in meaning to the French word, is too lumbering and awkward for general use, while our being already in possession of *Physician*, but in another sense, prevents our adopting the French word. *Physicalist* would be in analogy with *Naturalist*.—Ed. Gl.

Mr. Dalton, who has touched on this subject, in the ingenious work we have before cited, supposes that the capacity of the same mass cannot remain constant, because part of the heat is employed in effecting expansion ; but that if we confine ourselves to the consideration of the same volume, it will be constant.

This doctrine of Mr. Dalton's is not founded on any direct experiment, and may be viewed, in fact, as a mere conjecture connected with his other ideas relative to the measure of temperature, and to which we shall return when we have discussed the principles on which his whole theory is based.

We may, in the mean time, apply to the present question the same argument we used with relation to expansion ; viz. that we cannot expect to resolve the problem, except by taking into our consideration a much more considerable portion of the thermometric scale than has yet been done. Accordingly, the experiments we are now about to give an account of, have all been made in an interval of 300° and even 350°.

The season during which this part of our experiments has been conducted not allowing us conveniently to use the temperature of melting ice, we have confined ourselves to the method of mixtures, but with every precaution necessary to ensure the utmost precision.

The substances, the capacities of which we have determined, were necessarily chosen amongst the least fusible of the metals. The homogeneity and perfect conductibility of these bodies render them fitter to be the objects of such experiments as we had undertaken, than any other[2].

One of the greatest difficulties which attends this kind of inquiry, is the exact determination of the temperatures. We have always used boiling water in our experiments, to measure the capacity below that point ; and for temperatures above it, boiling mercury, when the nature of the body would permit. We have used this point, considering it to be equally fixed with the other, and as having determined it with the greatest care, as we before noticed. If however the substance was soluble in mercury, we heated it in an oil bath, which, by the arrangement of the apparatus, could be kept steadily at the same temperature for about a quarter of an hour. Lastly, to avoid the error which might have been occasioned by an inequality of temperature in different parts of the mass—the liquid was stirred continually at the period of attaining the maximum temperature ; and, by a thermometer with a constant volume, the mean temperature, which was of course that of the body, was ascertained. Fixed oils, it is known, acquire a much higher degree of fluidity when heated—the film, therefore, which adheres to bodies plunged into them, is very thin : we have not, however, neglected to allow for the heat derivable from this small addition of matter ; although in a majority of cases this correction has only had reference to very minute quantities[3].

When the body under experiment had been heated to a certain temperature, (which was measured in the way we have just described,) it was plunged, as quickly as possible, into a large mass of water, and the rise of temperature observed of this liquid, as soon as an equilibrium was established. It is in the determination of this element of the inquiry that the greatest possible precision is required, in order to obtain results that can be depended on. Our practice has been, to employ a quantity of water so large, that the rise of temperature could never exceed 5 or 6 centigrade degrees ; and to measure this, a thermometer was used, with a scale, such, that the one hundredth part of a degree could be accurately read off. The water was contained in a vessel of very thin tinned plate, insulated upon a support which touched only at three points. This vessel, in each case, received part of the heat ; but as its weight and specific heat were known exactly, we could, of course, in each case, allow for the effect it produced.

In the greater number of experiments, the water was cooled down previously ; so that when the heated body had imparted its caloric, the whole should be of the temperature of the surrounding medium : in others the heating process commenced from this latter temperature. The first method has appeared to us more exact ; it requires no correction : for the water, immediately after the heated body is plunged into it, acquiring a temperature but little removed from that which must finally

[2] In order to increase the surface as much as possible, the pieces with which we experimented were formed into the shape of very flat rings. They weighed from one to three kilogrammes.

[3] This correction was made to depend upon the weight of oil which adhered to the ring. To determine this, we made in each case a preliminary experiment, by weighing the ring on withdrawing it from the oil bath. At 300° C. the increase of weight never exceeded 3 to 4 decigrammes.

obtain, the influence of the external air must be almost inappreciable. In the other method again, it is necessary to allow for the loss of heat occasioned by the difference of temperature between the mass and surrounding medium, which allowance will also depend on the duration of the experiment. This correction can be determined with sufficient exactness by a subsequent observation, to be made on the rate of cooling of the same mass of water that had been employed in the experiments. Besides, the bulk of the bodies on which we have experimented—the varied circumstances under which each result has been obtained—and the acknowledged accuracy of the thermometer we used—all appear to us to have contributed to the correctness of the results we are about to submit.

The great capacity of iron, (compared with other metals,) and the convenience of plunging it into boiling mercury, have been our inducements for commencing the series of comparisons we proposed to make, with this substance. The following determinations are deduced from a great number of measurements, the differences among which are but small.

Mean capacity of iron from	0°	to	100°[4]	= ,1098
——————	0	to	200	= ,1150
——————	0	to	300	= ,1218
——————	0	to	350	= ,1255

The conclusion to which this increasing series points, will be found to be further confirmed by the following table of the other metals: the determinations made between 0° and 100° and between 0° and 300° only have been inserted.

	Mean capacity between 0 and 100°.	Mean capacity between 0° and 300°.
Mercury,	,0330	,0350
Zinc,	,0927	,1015
Antimony,	,0507	,0549
Silver,	,0557	,0611
Copper,	,0949	,1013
Platina,	,0335	,0355
Glass,	,0177	,0190

It is then equally true of the capacities of solid bodies, as it is of the dilatations, that they increase with the temperature, as measured by a thermometer formed of air: they would even increase contrary to Crawford's opinion, when referred to the indications of the mercurial thermometer. If this result had been deduced from experiments made with a body having a constant volume, there would be no doubt as to the consequences to which such a truth leads. But the gaseous state or condition is the only one which allows of this condition being secured; and in the case of a gas, the experiment would be attended with insurmountable difficulties. However, if the dilatation of solids were uniform, we ought not to attribute the increase of capacities to the quantity of heat which goes to produce the augmentation of volume, because this quantity being in such a case proportional to the temperatures, the relation of the capacities would not be affected. The same consequence does not hold where the dilatations are increasing; it is certain that, in this case, the capacities referred to different points of the thermometric scale, would partake of the irregularities of the dilatations. We cannot form any estimate of the effect due to such a cause; but what renders it probable that it is not altogether inconsiderable, and that the increase of capacity which we have observed, depends, at least in some degree, upon it, is the fact that the metals, the dilatation of which agrees with the most rapidly increasing series, are precisely those of which the capacities increase fastest. But this question can only be decided by observations, which should comprehend an interval of temperature much greater than that in which our experiments have been made. We hope soon to be able to throw light on this subject.

We have shown, when treating of the expansion of solid bodies, that thermometers, constructed of the least fusible of the metals, if adjusted, as in the case of the ordinary thermometer, by the fixed points of boiling water and melting ice, would, under the same circumstances of temperature, indicate very different instrumental values. The same discordance would evidently be found, it appears from what precedes, if we were to estimate temperatures, as several philosophers (*physiciens*) have proposed, by the proportional quantities of heat lost by any

[4] The capacity of water being unity.

given body, in cooling to a certain temperature—for such a scale would be founded on the assumption, that the capacities are constant; or at least that they increase according to the same law in all bodies. Now both these assumptions are equally false. We have given, in the following table, the temperatures which would be obtained in this way, in employing each of the different substances included in the preceding table. We assume that they have been all subjected to the heat of a liquid kept at a temperature of 300°, as indicated by an air thermometer.

Iron,	332,2
Silver,	329,3
Zinc,	328,5
Antimony,	324,8
Glass,	322,1
Copper,	320,0
Mercury,	318,2
Platina,	317,9

General Reflections and Conclusions.

Now, that we have shown, by experiment, within sufficiently wide limits, the laws of those phenomena, which may be employed in the measure of temperature, we have it in our power to decide whether the thermometric scale proposed by Mr. Dalton, possesses really all the advantages he attributes to it. In measuring temperatures on this scale, we shall find, according to this philosopher:—

1mo. That the expansion of mercury and other liquids is proportional to the squares of the temperatures—reckoning from the point of greatest density in each.

2do. That gases expand in geometrical proportion, to temperatures increasing arithmetically.

3to. That reducing to the same volume, the capacities of all bodies are the same.

4to. And lastly, that in the cooling of bodies, the temperatures form a geometrical series, when the times are taken as an arithmetical one.

The manner in which Mr. Dalton has brought forward the principle on which he founds the construction of his scale, sufficiently indicates, that it can only be considered as a hypothesis, with the supposed advantage of connecting together a number of phenomena by very simple relations. Such advantage, if it really existed, would be important enough to induce our adoption of so happy an idea—even though it should fail to be established by direct experiment. We shall not, therefore, stop to discuss the value of the particular observations which appear to have influenced this celebrated philosopher (*physicien*), but confine ourselves to the examination, whether the results established in this memoir agree with the laws in question.

In the comparison we are about to make, we shall consider the air thermometer (the degrees of which are all equal to the 100th part of the difference between the freezing and boiling points,) as an arbitrary scale; and without troubling ourselves, as to the actual quantities of heat which may be represented by the indications of this instrument—we shall use it merely as a common measure to pass from one scale to another.

Instead of enquiring whether, in measuring temperatures by Mr. Dalton's thermometer, the expansion of mercury and of air would really follow the laws he has pointed out, it will be more simple to calculate the indications which would correspond on his scale, with the observed expansions of each of these substances, (taking for granted that his laws are correct,) and then compare the results obtained for each. The following table exhibits the agreement found between some of these terms, calculated in this manner.

Temperatures, as derived from an air thermometer, with uniform scale.	Corresponding temperatures, as they would be indicated by a mercurial thermometer, on Mr. Dalton's hypothesis.	Corresponding temperatures as they would be indicated by an air thermometer, on Mr. Dalton's hypothesis.
— 40°	— 114°,8	— 52°,2
0	0	0
50	57 ,4	53 ,9
100	100	100 ,
200	169 ,1	175 ,7
300	226 ,7	236 ,8
350	251 ,1	263 ,2

In looking over the second and third columns of this table, we see that the temperatures deduced from the expansion of mercury and of air, are far from agreeing, as they ought to do, if Mr Dalton's hypothesis had any foundation. The discordance, nevertheless, in the higher indications does not appear so great as it really is. For as the relative scales of mercury and air have two terms in common, the boiling and freezing points of water; the great discrepancy which appears in the lower part of the scale has no influence on the upper temperatures. It is, in fact, as if the two scales commenced from different points—but in making them commence from the same indication the discordance in the lower terms would be felt throughout the scale. So that even in measuring temperature by Mr. Dalton's new scale, the two first laws we have quoted would be found to represent in no way the actual phenomena.

Again, if we would take into consideration the third law relative to the capacity for heat of bodies, we shall see that Mr. Dalton's thermometer is still farther removed from that perfection which he attributes to it.

In comparing the numbers in the first column of the preceding table, with those corresponding to them, in the 2nd and 3rd, we see that, with the exception of the terms 0 and 100, the accordance of which is assumed by the terms of the question, the indications of the ordinary air thermometer are always higher than the others. But we have shown that, even on that scale, the capacities of solid bodies increase more rapidly than their volumes—*a fortiori* then they would not appear to be constant, when referred to a scale having a less rapid divergence.

Finally, to show in a few words that Mr. Dalton's fourth proposition is also contradicted by experiment, it may be sufficient to say, that the law of cooling in the air is not the same for all bodies; and that, therefore, no thermometric scale can satisfy the condition of occasioning the loss of heat in each body to appear proportional to the excess of the temperature.

Although the propositions we have just been discussing have not that foundation suposed by Mr. Dalton, they at least prove that the insufficiency of the doctrines generally received, had not escaped the penetration of this celebrated philosopher. The greater part of the phenomena of which he had perceived the irregularity, do vary in the sense he has supposed; but he wanted the necessary data to verify his ingenious theory. The researches of which we have given some account, enable us to give a much more precise account of the measure of temperature, and to resolve several difficulties which have been supposed to belong to the subject. It is evident, from what we have said on the variation of capacity, that no thermometric scale can indicate directly the increments of heat, corresponding to a determinate rise of temperature; for—supposing one were found which had this property, considered in relation to one particular substance, it could not be applied to others, as the capacity of different bodies varies according to different laws.

In comparing together, therefore, the different thermometric scales, we may be assured that there is not one in which the expansions of all bodies will be expressed according to simple laws. Add to which, these laws would vary according to the scale adopted. So in taking the air thermometer as the standard, the rate of expansion for all bodies, increases with the temperature—with iron again, it would appear to decrease; while with mercury, freed from the irregularity occasioned by the change of volume in its containing substance, iron and copper would appear to have an increasing rate of expansion; and platina and the gases a decreasing one.

And although, in the present state of the question, we cannot assign any decisive reason for the exclusive adoption of any of these scales, we may yet say, that the generally recognised uniformity of the principal physical properties of the gases, and chiefly the perfect sameness of their rate of expansion, render it probable that the sources of irregularity have in these bodies, perhaps, less influence than in solids and liquids; and, consequently, that changes of volume, produced by heat, have a more direct dependance on the cause of their production. It is then the more likely, that the greater number of the phenomena of heat may present themselves under more simple relations, when referred to the indications of an air thermometer. At least we have been determined, by these considerations, to employ this scale in the prosecution of those researches which will form the subject of the second part of this memoir. And the success which has crowned our enquiries may be offered as an additional argument in favor of the view here taken. We do not, however, mean to say, that the other scales are to be excluded in every possible case. It is, for instance, not impossible that certain phenomena should appear under a more simple form, in referring the temperatures to the thermometric scale deduced from the expansion of the particular body which formed the subject of the experiment; and it was with this idea that we devoted ourselves with so much perseverance to the comparison of the several scales.

Remarks by the Editor.

This paper concludes the first part of the enquiry undertaken by Messrs. Dulong and Petit, as will be observed, by referring back to the introduction prefixed to their first paper, Vol. i. p. 29. The second part comprehends *the Laws of Cooling*, and embraces, still more important results than the first. We hope to be able to conclude the second part in the course of the present volume.

IV.—*On the Climate of the North-Western Mountains.*

A general statement of the Weather for April, 1829.

Clear,	11 days
Fair, but cloudy, and partially cloudy,	10 ditto
Rain, stormy and hail,	9 ditto
Thunder,	6 ditto

Clear, on the 1st, 8th, 9th, 10th, 16th, 17th, 21st, 22d, 24th, 25th, and 26th.

Fair, but cloudy, and partially cloudy, on the 2d, 11th, 15th, 18th, 19th, 20th, 22d, 28th, 29th, and 30th.

Rainy, stormy, and hail, on the 3d, 4th, 5th, 6th, 7th, 12th, 13th, 14th, and 27th.

Thunder, on the 3d, 4th, 6th, 7th, 13th, and 27th.

Height of the Barometer at Bréri, from the 1st *to* 8th *inclusive.*

	Inches	Th.
Mean maximum,	22,873	57,5
Mean minimum,	22,797	50,9
Mean of the daily means,	22,835	54,2
Greatest altitude, on the 3d, at 10 A. M.	22,896	56,7
Least ditto, on the 5th, at 6, 30 A. M.	22,776	48,4

Temperature of the Air.

Mean maximum,	56,2
Mean minimum,	42,6
Mean of the daily means,	49,4
Greatest, on the 2d, at 2 P. M.	62,3
Least, on the 8th, at sun-rise,	41,
Mean,	51,6

Temperature of the House.

Mean maximum,	50,8
Mean minimum,	46,3
Mean of the daily means,	48,5
Grtst. on the 2d, at 1, 2 and 3 P.M.	53,3
Least, on the 4th, at sun-rise & 8 AM.	45,1
Mean,	49,2

Hygrometrical state of the Air.

Leslie's Hygrometer, greatest altitude, on the 2d, at 2 and 3 P. M.	69
Ditto ditto, least ditto, on the 4th, at 9, 30 P. M.	4
Kater's ditto, greatest ditto, on the 4th, at 9, 30 P. M.	673
Ditto ditto, least ditto, on the 2d, at 4, 45 P. M.	239

The following were observed at intermediate places, during the March from Kotgérh to Soobathoo.

Place.	Date.	Barometer.			Thermometer attached.			Ther. detached; or in the air and shade.			Kater's Hygrometer.	
	April.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.	Mean.	Max.	Min.
At Matiána, (1)	9.	92,506	22,454	22,480	67,3	59,8	63,5	70,	51,6	60,8	260	186
Phágu, (2)	10.	22,364	22,330	22,350	62,2	57,5	59,8	65,7	50,7	58,2	177	138
Simla, (3)	11.	23,122	23,046	23,084	62,4	59,2	60,8	72,4	55,7	64,	221	130
Ditto,	12.	23,074	23,018	23,046	61,2	59,6	60,6	70,2	55,7	62,9	221	164
Saíri house, (4)	13.	25,382	25,308	25,345	74,7	69,6	72,1	80,6	2,3	71,4	150	114

Height of the Barometer at Soobathoo, (5) *from the 14th to 30th, inclusive.*

	Inches.	Th.
Mean maximum,	25,848	80,1
Mean minimum,	25,782	3,1
Mean of the daily means,	25,815	6,6
Greatest altitude, on the 16th, at 10, 30 A. M.	25,955	74,7
Least ditto. on the 27th, at 5 P. M,	25,690	78,

Temperature of the air.		*Temperature of the house.*	
Mean maximum,	84,2	Mean maximum,	80,1 (6)
Mean minimum,	65,7	Mean minimum,	73,1
Mean of the daily means,	74,9	Mean of the daily means,	76,6
Greatest, on the 24th, at 4 P.M.	90,7	Greatest on the 29th, at 3 P. M.	87,
Least, on the 17th, at sunrise,	55,5	Least, on the 16th, and 17th, on both days, at sun-rise,	66,
Mean,	73,1	Mean,	76,5

Hygrometrical state of the Air.

Kater's Hygrometer, greatest altitude, on the 15th at sunrise, 237

Ditto ditto, least ditto, on the 24th and 29th, on the former day at sunset, and on the latter, at 2, 3, 4, and 5 P. M., 54

Statement of the Winds, shewing their direction and force, during April, 1829.

West, on the 1st, 2d, 7th, 10th, 17th, and 21st,	gentle,	6 days.
Ditto, on the 9th,	moderate,	1 day.
East-north-east, on the 3d, 4th, and 6th,	gentle,	3 days.
Ditto, on the 5th,	stormy,	1 day.
North-west, on the 8th,	gentle,	1 ditto.
South-west, on the 11th, 13th, 15th, 27th, and 29th,	ditto,	5 days.
Ditto, on the 14th, 16th, 18th, 24th, 25th, and 26th,	moderate,	6 ditto.
Ditto, on the 19th and 20th,	strong,	2 ditto.
South-east, on the 12th,	gentle,	1 day.
West-north-west, on the 22d,	ditto,	1 ditto.
South-west, on the 23d,	moderate,	1 ditto.
Ditto, on the 30th,	gentle,	1 ditto.
North, on the 28th,	ditto,	1 ditto.

P. G.

(1) In North latitude, 31°.12'. East longitude, 77°.25', and elevation above sea level, by Barometrical observation, 7900 feet.

(2)	ditto,	31 .05	ditto,	77 .19	ditto ditto,	8017 ditto.
(3)	ditto,	31 .06	ditto,	77 .11	ditto ditto,	7300 ditto.
(4)	ditto,	31 .04	ditto,	77 .03	ditto ditto,	4730 ditto.
(5)	ditto,	30 .58	ditto,	76 .59	ditto ditto,	4205 ditto.

(6) The attached Thermometer also indicates the temperature of the house.

V.—*Some Remarks on the Mantis Tribe of Insects. By the late* Dr. J. Adam.

The varieties of the insect tribes, met with in India, may be said to be innumerable; and hence opportunities are afforded for the study of entomology, that do not occur in more temperate regions of the globe. The singularity of their appearance, and the striking changes, which many of them undergo, by offering a wider range of enquiry, as respects the economy of the insect itself, or its relation to other animals, combine moreover to confer a greater interest on the science, and allure those to its pursuit, who might otherwise, consider such researches as frivolous and unworthy of their notice. Among the most remarkable insects for their external form, in India, may be reckoned the various individuals of the *Mantis* tribe, of which the accompanying are examples; namely the *M. gongylodes* and *M. siccifolia*. According to the latest classification, this tribe has been divided into the two families of the *Mantida* and *Phasmida*, founded on a difference in the structure of the fore foot or leg; this member in the former, being *raptorious*, or provided with a sharp claw, and a hollow on the leg and thigh, and a double series of spurs, for the better securing its prey;—and in the latter, being simple and destitute of any such peculiarity. The distinction seems to be a very just one, since not only in this essential point of external structure do these two differ, but the latter would appear to subsist entirely on vegetable food, while the former or *Mantis* is well known to be one of the most voraciously carnivorous, of the Mantida that exists. The specimens now before us, I have entitled *gongylodes*, as they appear to correspond closely with the description and figure of that species, in the latest entomological works; but I believe the varieties of this singular insect to be very numerous all over India: and in the two now presented, there may be perceived a considerable difference in the form of the head, and the length of the wings, as well as of the body generally; which, however, may depend merely on difference of sex.

This insect, when alive and fresh, presents a striking resemblance to a blade of grass, differing in color according to the season; being green and succulent in the rains, and in the dry weather imitating so closely a withered straw, that they can with difficulty be distinguished. In Bengal proper, owing to the constancy of the verdure, these creatures are generally caught of the former color; but in the upper provinces, when the surface of the country is parched during the hot winds, they are found as now described. On first beholding this insect I could hardly be convinced that it was not a straw, and part of the same long and dry grass on which it rested; a slight movement of the head, however, like that of the house lizard on the wall, when watching its prey, satisfied me that it was a living object; and on removing grass and all to my tent for examination, we had, in a short time, abundant evidence of its animation; and were both surprised and amused at the extraordinary powers the insect developed, clinging close to the upright straw which was fixed on the table: the animal lay in wait for its prey with no less design than would be exhibited by a cat or a tiger, intent on the same pursuit; and if an unlucky fly happened to alight in his neighbourhood, there was hardly left to it a chance of escape. If situated at a considerable distance from his position, he gradually and cautiously approached, not even stirring the straw on which he moved; and when sufficiently near to be within reach, which the creature seemed deliberately to determine by the eye, he projected rapidly his armed paw, and with unerring aim, transfixing his victim, securely lodged it in the toothed hollow of the thigh, destined for its reception. After having the fly in his power, no time was lost in devouring it; commencing with the trunk, and in a few minutes swallowing the whole body; the head and wings constituting the finishing morsel.

In this manner our "*Death and Dart*" Insect (for so we familiarly termed this new acquaintance,) would destroy, at a meal, five or six large flies, which, in point of bulk, nearly doubled his own body. I afterwards met with many others of the same kind, but have never since seen one so voracious, or remarkable in every respect. On viewing the structure of the fore limb of this insect, it seems impossible to imagine any thing more perfectly contrived for the end in view. The limb itself, so strong and muscular, provided with a claw at its extremity, likewise strong, horny, and sharp as a needle, and the groove in the two last joints, with the double row of teeth, or spurs on the margin, corresponding, and locking closely into each other, like the fangs of the alligator, altogether constitute an apparatus for seizing and securing its prey, which in so small a creature cannot but excite our admiration. By means of these formidable weapons, the insect not only becomes destructive to others, but is employed to attack its own species; and in China, we are told, fighting the *Mantis* forms as much a favorite amusement of boys, who carry them about in cages for the purpose, as cock-fighting in England, or among the inhabitants of the Eastern Islands. It is said, that in these combats, the insect uses his claw in the manner a hussar does his sabre, and with a single stroke not unfrequently cuts off the head of his antagonest; but this rather *oriental* style of description is completely at variance with the structure of the weapon; which, though curved like a scimitar, is provided with no cutting edge; and can only be employed to thrust with, or as a hook or dart in the way above described.

These two specimens of *Mantis* were collected at Midnapore, or in that district. The 3rd specimen, formerly termed *Mantis siccifolia*, but now placed in the *Phasmida* Family, and constituting the genus *Phyllium siccifolium* of Latreille, was sent me very lately from Sylhet, by my friend, Mr. Wardlaw, of that station, who merely states that it is called the "Leaf Insect," and considered a great rarity in that part of the country. Its resemblance to the leaf of a tree will at once be recognized by every one, and is certainly not less striking than that of the former species to the same object, or to a blade of grass, in the state at least in which the insects are now presented. By immersion in spirits, the color of the *leaf-like* portion has become changed to a light yellow, not unlike that of a leaf in England at the fall of the year. I am informed that, when alive, or but recently dead, the hue of the insect is of the liveliest green, which may be preserved unimpaired for any length of time, by simply covering it with cotton wool, excluding the light and air[5]. It appears extraordinary that the *Mantis strumaria*, or Leaf Insect of America, which, in specific characters, would seem to agree with our insect, is yet totally different in the peculiarity of external form from which both derive their names. The *Mantis* or *Phyllium siccifolium* is described as being a native of the Molluccas, and although it is well known, and specimens are not uncommon, I am not aware that it has been found before in *continental* India[6]. Should this be the case it will form one proof more of the similarity, in point of natural productions, both animal and vegetable, of our territories on the eastern frontier, to the interesting islands of the archipelago.

VI.—*On the Mean Temperature of Calcutta; with a table for reducing to the Maximum or Minimum, observations of the Barometer, made at any other time of the atmospheric tide.*

To the Editor of Gleanings in Science.

SIR,

I send you a chart of the temperature of Calcutta for each month of 1829. It is formed, by protracting the temperature at the different hours, as given in the Meteorological Register, (correcting a few typographical errors,) and then drawing through the points, so as to get as nearly as possible the temperature of the whole twenty-four hours. Knowing how little room you can afford, I have endeavoured to condense it as much as possible, so as to be consistent with distinctness, and the size of your page, and shall do the same in my remarks. The mean of 10 o'clock A. M. and P. M. gives 78°,0 while 4 A. M. and P. M. gives 78°,2, the mean of both being 78°,1, which is probably very near the true temperature of the whole year.

The mean of each month may be seen in the annexed table.

January,	66°,2	April,	85°,4	July,	81°,8	October,	79°,2
February,	69 ,8	May,	85 ,7	August,	82 ,0	November,	74 ,2
March,	80 ,0	June,	83 ,7	September,	82 ,8	December,	66 ,6

Comparing the mean temperature of the whole year with that of each month, we find a correction to be applied to each, in order to have the mean of the year.

Thus the mean temperature of the year, is equal to that of :—

Jan+11°,9	Feb. +8°,3	March—1°,9	April—7°,3	May—7°,6	Jun— 5°,6
July—3 ,7	Aug. —3 ,9	Sept. —4 ,7	Oct. —1 ,1	Nov.+3 ,9	Dec.+11 ,5

These numbers may probably vary in different years, yet they give some information which would be increased by farther comparisons :—

Since August is as much above the mean as November is below it, these two months will probably give the mean of the whole year.

By looking at the Chart it will appear that the greatest range of temperature occurs in December and January, when it amounts to about 18°.—The least range is in July, amounting to about 6°.

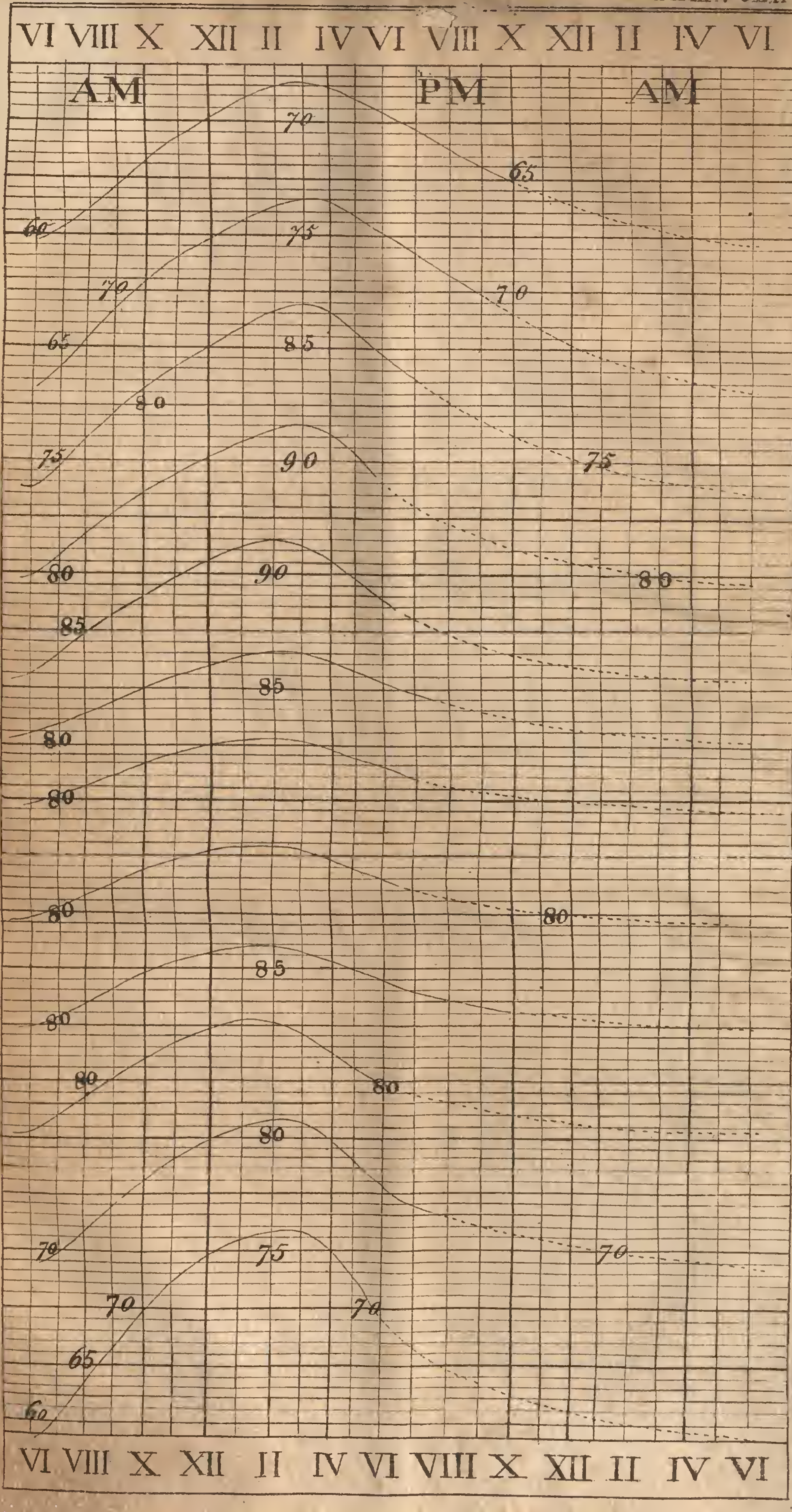
If there had been room, without confusing the Chart, I should have added the range of the wet bulb thermometer.

I send you also a table for reducing to the Maximum or Minimum, Barometric observations made at any time of the atmospheric tide. It is calculated on the principle, that the amount of reduction is as the square of the time from the nearest maximum or minimum. It will be found, by protracting the variations on a large scale, that they observe a law not sensibly different from this.

The same thing might be deduced from a more general consideration of the subject.

[5] I had one sent from Manipur preserved in this way. It retained i's color, a light green, perfectly.—G. S.

[6] It was sent from Sylhet many years ago, by the late Mr. Smith, to Dr. Roxburgh. I also sent a very fine specimen here to Dr. Brewster, last year. It is also found at the Isle of France.—G.S.


VI VIII X XII II IV VI VIII X XII II IV VI
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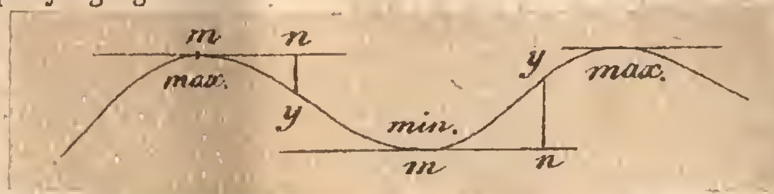
VI VIII X XII II IV VI VIII X XII II IV VI

The mercury, in passing from the maximum to the minimum, proceeds without any sudden start, with a motion gradually accelerated, for the first half of the range, and then it decreases, in like manner, during the remaining half, as may be represented in the accompanying figure.

The intervals of maximum and minimum, being generally six hours, the mean height will occur about the middle of the tide, or about 3 hours from each; now, without enquiring particularly into the nature of the curve, we may find the ordinate *n. y.* corresponding to *m. n.* by a property common to all curves of finite curvature near the point of contact, viz. that the deflection is as the square of the largest, that is, *ny.* is as mn^2. Strictly speaking, this is the equation to the parabola; but the whole variation is so small, that the ordinates would not sensibly differ from those of any curve of the second order.



The table may be useful to those observers, who, in travelling, are frequently obliged to make their observations without waiting for the maximum or minimum.

R. S.

Table of the difference of the Barometer, from the Maximum or Minimum, for every quarter of an hour of the tide.

Variation of tide.	Distance from the time of Maximum or Minimum.												Variation of tide.
	m 15	m 30	m 45	h m 1.0	h m 1.15	h m 1.30	h m 1.45	h m 2.0	h m 2·15	h m 2·30	h m 2·45	hm 3·0	
·01	·0000	·0001	·000	.000	·001	·002	·002	·002	.003	·003	·004	·005	·01
·02	·0001	·0003	·001	·001	·002	·002	·003	·004	.006	·007	·008	·010	·02
·03	·0001	·0004	·001	·002	·003	·004	·005	·007	.008	·010	·013	·015	·03
·04	·0001	·0006	·001	·002	·003	·005	·007	·009	.011	·014	·016	·020	·04
·05	·0002	·0007	·002	·003	·004	·006	·008	·011	.014	·017	·021	·025	·05
·06	·0002	·0008	·002	·003	·005	·007	·010	·013	.017	·021	·025	·030	·06
·07	·0002	·0010	·002	·004	·006	·009	·012	·016	.020	·024	·029	·035	·07
·08	·0003	·0011	·002	·004	·007	·010	·014	·018	.022	·028	·033	·040	·08
·09	·0003	·0012	·003	·005	·008	·011	·015	·020	.025	·031	·038	·045	·09
·10	·0003	·0014	·003	·005	·009	·012	·017	·022	.028	·035	·042	.050	·10
·11	·0004	·0015	·004	·006	·009	·014	·019	·024	·031	.038	·046	·055	·11
·12	·0004	·0017	·004	·007	·010	·015	·020	·027	·034	.042	·050	·060	·12
·13	·0005	·0018	·004	·007	·011	·016	·022	·029	·036	.045	·054	·065	·13
·14	·0005	·0019	·004	·008	·012	·017	·024	·031	·039	.048	·059	·070	·14
·15	·0005	·0021	·005	·008	·013	·019	·026	·033	·042	.052	·063	075	·15
·16	·0006	·0022	·005	·009	·014	·020	·028	·035	·045	·055	·067	·080	·16
·17	·0006	·0024	·005	·009	·015	·021	·029	·038	·048	·059	·071	·085	·17
·18	·0006	·0025	·006	·010	·016	·022	·031	·040	·050	·062	·076	·090	·18
·19	·0007	·0026	·006	·011	·016	·024	·032	·042	·053	·066	·080	·095	·19
·20	·0007	·0028	·006	·011	·017	·025	·034	·044	·056	·069	·084	·100	·20
·21	·0007	·0029	·007	·012	·018	·026	·036	·047	·059	·073	·088	·105	·21
·22	·0008	·0031	·007	·012	·019	·027	·037	·049	·062	·077	·092	·110	·22
·23	·0008	·0032	·007	·013	·020	·029	·039	·051	·065	·080	·097	·115	·23
·24	·0008	·0033	·007	·013	·021	·030	·041	·053	·067	·083	·101	·120	·24
·25	·0009	·0035	·008	·014	·022	·031	·042	·056	·070	·087	·104	·125	·25
·26	·0009	·0036	·008	·014	·022	·032	·044	·058	·073	·090	·109	·130	·26
·27	·0009	·0037	·008	·015	·023	·034	·046	·060	·076	·094	·113	·135	·27
·28	·0010	·0039	·009	·015	·024	·035	·047	·062	·079	·097	·118	·140	·28
·29	·0010	·0040	·009	·016	·025	·036	·049	·064	·081	·101	·122	·145	·29
·30	·0010	·0042	·009	·017	·026	·037	·051	·067	·084	·104	·126	·150	·30
	15	30	45	1.0	1.15	1.30	1.45	2.0	2·15	2·30	2·45	3·0	

[2] In like manner, when the observations are not simultaneous, the thermometric chart may be of use, by shewing the change of temperature, from one hour to another, in the different monhs.

In order to use this table, there is required only the whole amount of the variation, and the time from the nearest *barostice*, if I may be allowed to coin a word, expressive of the stationary state of the barometer.

Enter the Table with the time from the nearest maximum or minimum, *(barostice)* at top, and opposite the oscillation at the side, is the correction to be applied to the observed altitude of the Barometer.

Example.

In May, 1830. Maximum, 29,678 at 9·40
Minimum, ,560 at 4·00

Oscillation, ,118

Required the height at noon, 12—9 ·40=2·20 time from maximum.
Height at maximum, = ,678
Under, 2·20 and opposite. ,118 is ,034

Height by Table, ,644 by observation 647.

VII.—*Account of a curious Insect.*

To the Editor of Gleanings in Science.

SIR,

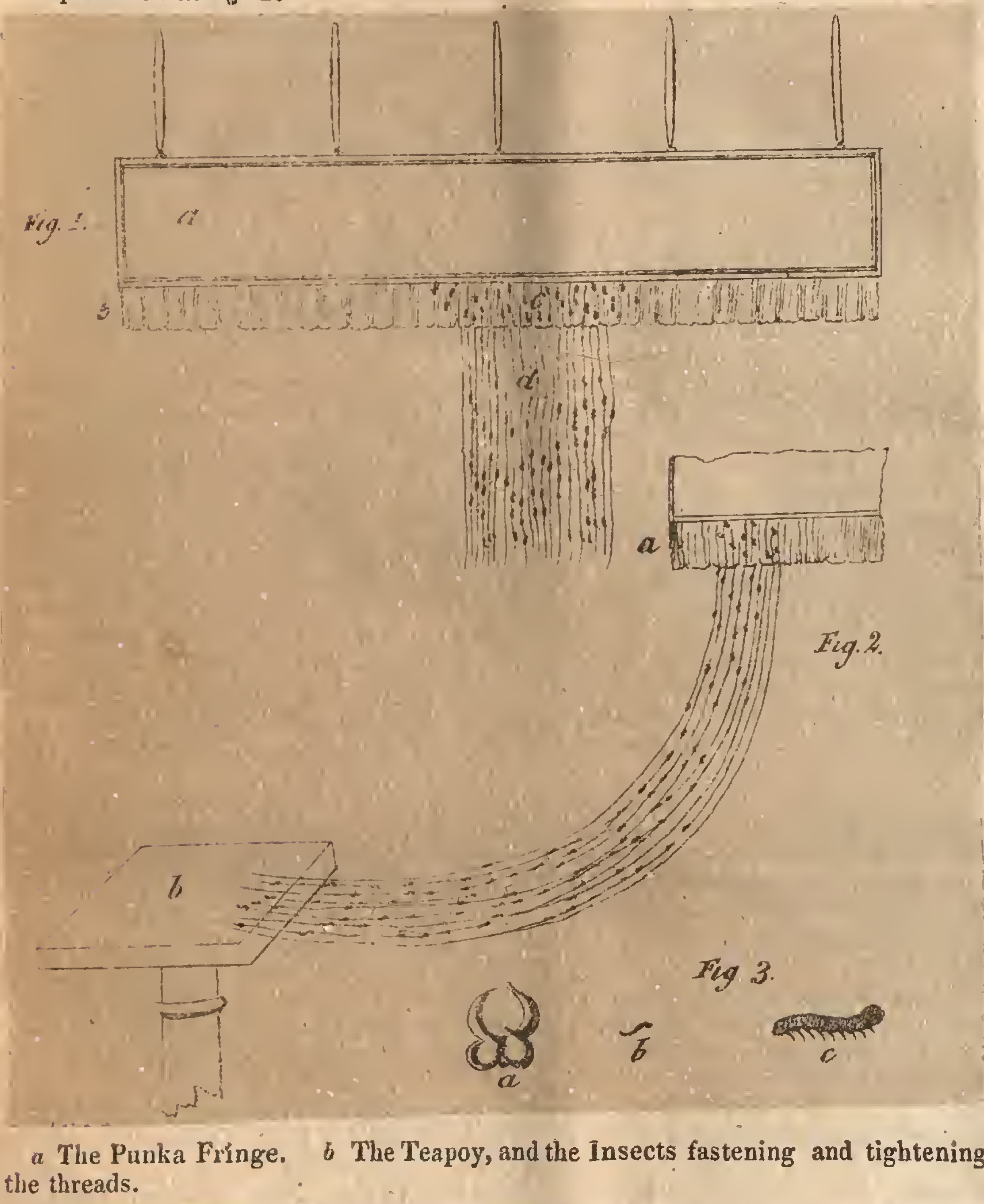
In a recent number of the Lancet, giving an account of a lecture, by Mr. Lawrence, on poisoned wounds, the following observations occur ;—" in the stings of insects there is a small wound inflicted, and there is an acrid matter introduced into the wound. I speak of the stings of the bee, the wasp, the hornet, and the tarantula spider." In another part of the lecture, we have this remark,— " there are certain insects which bite, and which do not sting; that is, they make a small wound, but no venomous or acrid matter is introduced into the wound. This is the case with the flea, bug, gnat, scorpion, &c. the bites of these insects produce slight inflammation." Now with reference to the first quotation, I have my doubts about any of the Aranea having a sting, and the prejudice of the tarantula being so armed, is, I imagine, founded in vulgar error, just as it was common to say, that a snake *stings*. The fact is, I believe, that spiders inject poison into a wound made with fangs—that is to say, from hollow, arched, pointed, and moveable hooks attached to each jaw (*a. fig. 3.*) *: if I am right then, the tarantula* has no sting.

With reference to Mr. Lawrence's other remarks, I am rather surprised that he should fall into the mistake, of classing the *sting* of the scorpion among the *bites* of fleas, bugs, and gnats. I need scarcely dwell on a circumstance so familiar to all of us here, as that the scorpion has a sting, by means of which he inoculates a venom when irritated. Neither am I convinced that Mr. Lawrence is quite correct, when he says, that certain insects, as the gnat and bug, bite, making a small wound, but into which no venomous matter is introduced. I am rather inclined, on the contrary, to conclude that a venomous or acrid matter is *always* introduced—for mosquito bites, for instance, swell, at times, very much, and even ulcerate. This, I believe, is more especially the case with their bites at the period of copulation.

It remains for me next to advert to a circumstance which may be well known to some philosophical observers, but which to me is new. In the month of August, I saw upon the fringe of a punka, that had just been attached to the frame, after returning from the washing, a number of minute insects, weaving delicate gossamer threads. I at first thought they must be minute spiders,—although their working socially together soon made me reflect, that this could not be the case. They looked more like very small, light colored, black headed ants, than any thing else, and several of them were busily and actively engaged on the cloth of the punka fringe—and on the thread they were weaving down from it ; and clinging to which they were visible, like sailors in the rigging of a ship, rapidly going up and down, or slanting out a new thread, in the direction of those already spun. The scattered threads they had thrown down, waved with the slightest breath of air ; and the rapidity with which they worked was surprising ; for all at once new threads shone forth, where none were visible before. The following rude sketch (fig. 1.) may give you an idea of them as they first appeared.

a The Punka. *b* The cloth Fringe. *c* The Insects on the cloth. *d* The Gossamer Threads, and the Insects busy upon them.

At first the threads appeared to wave loosely in the air, but all at once I observed that the whole skein began to give a curve, as if pulled out of the perpendicular; and following the direction with the eye, I now discovered that our tiny work-people were busy on another tack. At some distance, on one side, stood a teapoy, and favoured, I presume, by the action of the air, the insects had fastened the ends of what first appeared to be merely floating threads, to the teapoy—and they tugged so heartily at the end of the threads, that the movement, as they 'hauled taut,' was distinctly visible. The threads then had changed their direction thus, as represented in fig. 2.



a The Punka Fringe. *b* The Teapoy, and the Insects fastening and tightening the threads.

While thus engaged, they made no ceremony of alighting on one's hands and face; and when they did so, the sensation was stinging as if an ant had bitten one—but not so severe. On examining, with a pocket magnifier, they looked exactly like little caterpillars, and I have no doubt but they are the *larvæ* of some insect—but of what kind of insect, I know not. On the cloth of the punka fringe there was a tuft, as it were, of downy moss—and this I suspect was the nest out of which they issued—and which I presume stuck to the cloth when it was spread on the grass, by some tank side, to dry. As it was late in the evening, I had no time to make further observations—even if the presence of the insects, (which was close to a lady's dressing glass,) had not begun to be felt as an annoyance. The more we looked, the more encroaching did they appear to become—until one of the servants, before I was aware of her intention, at one fell swoop, almost annihilated them and their labours. I have managed, however, to preserve a few of them for your inspection. I have already given you specimens of my drawing, which I fear may be considered derogatory to the Gleanings; nevertheless, they may serve my purpose, which is to make description a little more intelligible than it otherwise would be. Fig. 3, letter *b*, represents the natural size of the insect—its black head being the most conspicuous part of the body, and *c* the magnified size, as seen with a small pocket lens. I shall, dear Mr. Editor, feel much obliged to yourself, or any of your able correspondents, for further information respecting this insect. I have the honor, &c.

Sept. 1830. Tyro.

VIII.—*Miscellaneous Notices.*

1. *Climate of the Cásia Hills.*

We have been favored with a sight of a letter from one of the party lately proceeded to the Cásia Hills, from which the following is an extract.

"I have the pleasure to send you Meteorological Observations for three days, taken at our quarters, at Cherra Púnji. The barometer is placed near a door with venetians, freely exposed to the air; the door opens to the west, and the barometer and thermometers are perfectly shaded from the sun: the latter hangs quite free, and close to the former. The place where the former observation was taken with the barometer, and from which the calculation of the height has been made, is situated about one mile and a half to the W. N. W. of our quarters, beyond the village of Cherra, and from guess, I should think about 500 feet above this place. The men suffered greatly in their journey from the boats, which they left at Pandua, to their quarters here; but in spite of the bad weather we have had since our arrival, they have all improved, especially two men, who had very bad attacks of low marsh remittent fever, got at Terriah Ghát, a swampy place, at the foot of the Hills, surrounded with jungle, where we lay in tents for three or four days, until the necessary arrangements could be made for the invalids. I will write again in a few days, and give some account of our journey, and the appearance of this place. I send you also an extract from the Meteorological Table kept by me in my boat, at Pandua, to compare with the other, and see the sudden change you undergo from the plains to the mountain;—of course the heat in a boat would be a little greater than on shore.

Barometer and Thermometer at Pandua, on the 29th September.

Sunrise.	Bar.	29,722	Ther. 82	M. B. Ther.	79,5	Calm.	A clear sky.
10 A. M.		,782	89		83	Light wind from the wt.; cr. sky.	
Noon,		,760	89		83,7	Nearly calm; clear sky.	
2 P. M.		,700	92		84,5	Dead calm; clear sky.	
4 P. M.		,650	92,5		84,5	Nearly calm; ditto.	

There were clouds hanging over the hills, but the sky above us was clear, and the weather very hot.

Barometer and Thermometer at Cherra Púnji, on the 8th of October, 1830.

Sunrise.	Bar.	25,750	Ther. 67	M. B. Ther.	67	Wd. s. w., heavy rain, sky overcast.	
10 A. M.		,800	67		67	Do.	do. thunder.
Noon.		,778	67		66	Do. with heavy squalls and rain.	
2 P. M.		,744	66½		66,5	Do.	do.
4 P. M.		,706	66,5		66,5	Do.	do.

October 9th.

Sunrise.	Bar.	25,750	Ther. 65,5	M.B. Ther.	65,2	Heavy squalls and rain; Wd.S.S.W.	
10 A. M.		,799	67		66,9	Do.	do.
Noon.		,768	66		66	Do.	do.
2 A. M.		,744	66,5		66,5	Do	do.
4 P. M.		,719	66,5		66	Do.	do.

"The Self-Registering Thermometer was, on the night of the 8th, as low as 62°.

"During the two days it was one continual heavy rain, with strong squalls from the south-west and south-south-west, with thunder and occasional lightning. The clouds driving along the hills so, that sometimes you could not see twenty yards. Every thing, both in and out of doors, damp and uncomfortable. Yet the men are all improving, and are most of them out to-day, the 10th, the rain having ceased this morning at 4 A. M.; the sun is out, and the breeze sharp and bracing; but I am suffering from cold, produced by repeated duckings, during the first two or three days.

October 10th.

Sunrise.	Bar. 25,788	Ther. 64,7	M. B. Ther. 64,5	Wd. S.-W.; fine breeze.
10 A. M.	,850	65	65	W.-S.-W.; ditto.
Noon.	,830	67	66	W. by S; ditto.
2 P. M.	,800	67	66	Ditto ditto,
4 P. M.	,766	66,5	64,5	Wy.; fine cold wind.

"During this day, clouds (early in the morning heavy, but after the sun had got a little power, lighter) have been floating about in all directions: the different valleys and mountains giving the wind its course."

2.—*On Hydrophobia.*

To the Editor of Gleanings in Science.

SIR,

Having had several opportunities of observing the facility with which the action of muscular fibre is reduced by the bite of venemous serpents, I have been led to think, that the poison of those animals might be beneficially employed in muscular spasmodic diseases: those to which I more particularly allude, are Tetanus and Hydrophobia.

In many constitutions the effects of all other anti-spasmodics have been found to be feeble or uncertain, and in some perfectly inert. I believe this cannot be said of the substance in question, for no peculiarity of temperament has come to my knowledge, in which the poison of a healthy rattle snake or Cobra de Capella, if applied in sufficient quantities, has not produced an immediate and violent effect.

Naturalists do not appear to have any decided opinion as to the part played in creation, by venomous reptiles: yet, reasoning from analogy, the wonderful mechanism, with which they are furnished, would induce a belief, that, it must be highly important. That they are not created, as some have supposed, for the mere purpose of ridding us of other noxious animals, is, I think, evident, from the circumstances of their being themselves a far greater nuisance, than the most mischievous of the creatures upon which they prey; and in such a case, by an ill adaptation, which is never seen in nature, of the means to the end; for the prey of other serpents, without fangs, does not differ from theirs. Whether the cure of the above two dreadful maladies, be of sufficient importance, to call for a special creation, I leave to others to determine; and for the want of opportunities for experiments, I am compelled to do the same, with the supposition of their being cured by this means at all. However, I may here observe, that venemous reptiles chiefly abound in those countries, in which rabid animals are most frequently met with.

It may be objected that this remedy, if remedy it be, is a violent and dreadful one; such as few would willingly consent to use. It certainly is so; but when the prospect of a lingering and terrible doom be considered, I think there are not many who would hesitate for a moment, in adopting any thing, which promised relief, though even in immediate death. When the old, and, perhaps, humane, custom, of smothering Hydrophobia patients, was in vogue, it was, probably, no difficult matter to obtain the consent of the wretched sufferer.

The method of using this substance must depend upon experiment; but, I conceive, it may be done by inoculation, with a lancet, or by the immediate application of a snake to the body of the patient; modifying the strength of the remedy according to the strength of the malady, by the introduction of a greater or less quantity into the system. When the lancet is used, this may be effected without any difficulty; and managed, in the latter, by irritating the snake to bite some other animal previous to its application.

I wish it had been in my power to have brought forward experiment in support of my opinion; but, believing the matter to be of importance, I do not consider myself justified in withholding it, and, therefore, prefer to offer these crude remarks, in the hope that others may have opportunities of pursuing my idea with success.

I am, Sir, your obedient Servant,

Midnapore, Oct. 7, 1830. W. P.

X.—*Proceedings of Societies.*

1. MEDICAL AND PHYSICAL SOCIETY.

Friday, the 3d July, 1830.

Mr. White, Assistant Surgeon, Bombay Service, and Dr. D. M'Leod, Assistant Surgeon, Bengal Service, were elected Members; and Dr. La Fontane, of

Paris, was elected a Corresponding Member. Mr. Spry's case of Lithotomy, Mr. Chartres' paper on Colica Pictonum, Mr. Raleigh's case of Medullary Sarcoma, and Mr. Twining's account of experiments with the Bengal Extract of Hyosciamus, were then read and discussed by the Meeting.

Mr. Spry's case was that of an Indo-Briton boy. The operation was performed successfully, the patient being discharged well on the twenty-first day afterwards.

2. Agricultural and Horticultural Society of India.

The Governor General in Council having placed at the disposal of this Society the sum of Sicca Rupees 20,000, to be distributed in premiums to the most successful cultivators of *Cotton*, *Tobacco*, *Sugar*, *Silk*, and other articles of raw produce, the growth of this Presidency, the Society is desirous of making known the conditions under which the distribution is to take place.

Two distinct classes of premiums are offered.

First Class; *or Premiums for Fine Samples.*

	Sa. Rs.
Sugar.	
1st.—For the best sample of Raw Sugar, not less than two maunds,	500
2.—For the next best sample of Raw Sugar, not less than two maunds,	250
Silk.	
3d.—For the best sample of Silk, not less than 5 seers,	500
4th.—For the next best sample of Silk, not less than 5 seers,	250
Tobacco.	
5th.—For the best sample of Native Tobacco, fit for the European Market, not less than 1 maund,	250
6th.—For the next best sample of Native Tobacco, fit for the European Market, not less than 1 maund,	125
7th.—For the best sample of any Foreign kind of Tobacco, fit for the European Market, not less than 1 maund,	250
8th.—For the next best sample of any Foreign kind of Tobacco, fit for the European Market, not less than 1 maund,	125
Cotton.	
9th.—For the best sample of Sea Island Cotton, not less than 1 maund,	250
10th.—For the next best sample of Sea Island Cotton, not less than 1 maund,	125
11th.—For the best sample of Upland, or Green Seed Cotton, not less than 1 maund,	250
12th.—For the next best sample of Upland, or Green Seed Cotton, not less than 1 maund,	125

Second Class; *or Premiums for Large Quantities.*

Sugar.

1st.—Rupees 40 per maund, for the first quality of Raw Sugar; *the sample to* be not less than 50, or more than 100 maunds.

2d.—Rupees 20 per maund, for the second best quality of Raw Sugar; the sample to be not less than 50, or more than 100 maunds.

Silk.

3d.—Rupees 40 per seer, for the best Silk; the sample to be not less than 20 or more than 40 seers.

4th.—Rupees 25 per seer, for the next best Silk; the sample to be not less than 20 or more than 30 seers.

Tobacco.

5th.—Rupees 40 per maund, for the finest quality of Tobacco; the sample to be not less than 30, or more than 50 maunds.

6th.—Rupees 10 per maund, for the next best quality of Tobacco; the sample to be not less than 30, or more than 50 maunds.

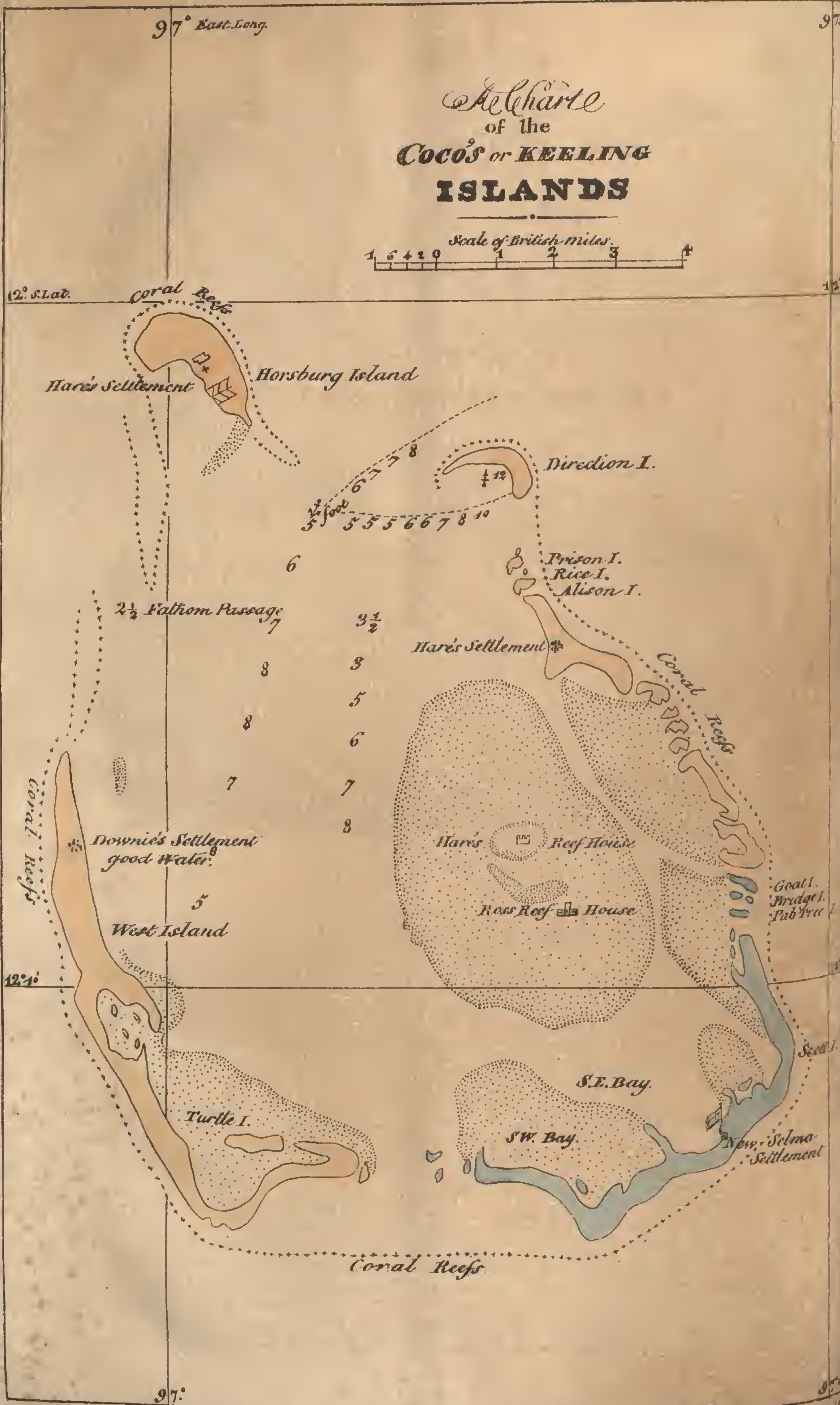
Cotton.

7th.—Rupees 40 per maund, for the best Sea Island or *black* Seed Cotton; the sample to be not less than 30, or more than 50 maunds.

8th.—Rupees 20 per maund, for the next best Sea Island or *black* Seed Cotton; the sample to be not less than 30, or more than 50 maunds.

9th.—Rupees 40 per maund, for the best *green* Seed or Upland Cotton; the sample to be not less than 30, or more than 50 maunds.

10th.—Rupees 20 per maund, for the next best *green* Seed or Upland Cotton; the sample to be not less than 30, or more than 50 maunds.



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J.B. Tassin Del.

GLEANINGS

IN

SCIENCE.

No. 22.—*October,* 1830.

I.—*Some Account of the Cocos or* Keeling *Islands; and of their recent Settlement.*

In the middle of the Indian Ocean, about 500 miles from the coast of Java, in lat. 12° South, long. 97° East, there is a circular cluster of small islands, called the Cocos Islands, or Keeling Islands, from the name of the discoverer, Captain Keeling, who visited them in 1694. They are in general not more than ½ mile in breadth, and they extend, with small intervals between them, in an irregularly circular figure, enclosing, as it were, a basin, the dimensions of which are about 18 miles in length, and 10 in breadth. This circular band of islands owe their origin evidently to the series of actions, so well traced by Captain Krusenstern, in his view of the gradual transformation of a coral reef into a habitable island, to be at last possessed by man. Accordingly they are surrounded by a barrier of coral reéfs, of which they are doubtless nothing but the highest points, on which the usual changes have ended in producing a soil whereon chance seeds have taken root. These rocks or reefs render an approach to the islands difficult, if not impracticable, in every direction but one—on which side there is a narrow passage, also leading into the interior basin, with soundings of from 5 to 8 fathoms. The basin is evidently the hollow part of the reef, having soundings within the same limits, with rocky bottom; and a vessel may ride in safety there, defended effectually by its surrounding barrier from the swell of the Indian Ocean, from whatever quarter the wind may blow.

The situation of this cluster of islets seems particularly convenient for affording facilities to many branches of our Eastern commerce. Fresh water is obtainable from them of a perfectly wholesome quality. Many articles of repair for shipping may be had, besides the convenience of a perfectly safe harbour while refitting. Fresh provisions and vegetables might also be obtained with proper arrangement, sufficient for the wants of any number of ships likely to visit them for many years. They might even serve as a convenient entrepôt for much of the trade in which England, Australasia, Mauritius, India and the Eastern Islands are engaged. To show how particularly fitted for these purposes these islands are, we may give the following as the nearest distances of the several places mentioned from them.

	Miles.	
Batavia.	793	
Swan River.	1828	
Madras.	2070	
Calcutta.	2432	
Port Louis.	2673	
Hobart's Town.	3691	
Sydney.	4450	(By Bass' Straits.)

Exposed to the full operation of the trade winds, with an inconsiderable surface, and distant 500 miles from the nearest land, these islands are blessed with a temperate, and above all, a salubrious climate. No jungles, as in many of the eastern settlements, engender a perpetually renewed stock of febrile miasma—no low and swampy plains are found, where the genius of pestilence delights to dwell, and snare their unwary visitor. Limited in extent, and still more in their vegetable productions, the salutary ocean breeze circulates through every part of them, and drives *malaria*, with all its train of proteau symptoms, far away. Conveniently situated as they are, with regard to Calcutta, we doubt not in fact, that these islands will become objects of attention to us, and that in time they will be a place of resort to our invalids, particularly as a means of escaping our hot weather and rains, which on account of the southern latitude of these islands will correspond with their colder season. Every one who has been obliged to change his climate on account of health, knows that it is not so much the residence at the place as the voyage to and back again that effects the salutary change ; and that the longer the latter is, the more decided the renovation of health. The reason is obvious, it is the greater purity, and consequently salubrity of the air at sea, free as it must be of all pestilential exhalations, or putrid miasms, whether of animal or vegetable decomposition. A residence in these islands would combine all the salutary influences of a sea voyage, with the conveniences and comforts of the land. When to these important considerations is added the proximity to Calcutta, enabling the invalid, should his arrangements require it, to be back at any time within a period of a month, we shall be satisfied that Calcutta ought to take a strong interest in the progress of these islands to a full settlement and occupation.

Up to the year 1825, no attempt had been made to occupy these islands, though offering so many advantages, as a port for refitting, to the ships engaged in the eastern trade. In December of that year, the Borneo, I. C. Ross, Master, a vessel belonging to the house of Hare and Co. of London, and employed in the trade to the Eastern Islands, on her return voyage to England from Padang, touched at them. Mr. Ross it appears[1] had long contemplated a project of settling with his family on this remote and uninhabited spot—convinced, from its situation, that it would become a place of importance to our eastern trade. On the 6th December he landed and took possession in the name of the British Government. During the 6th, 7th, 8th, and 9th he was occupied clearing and planting various useful trees and vegetables. He then sailed for England, determined fully to bring out his family and some settlers, and fix himself in this narrow and remote spot. On his arrival in London, he opened his project to his employers, and obtained their consent. He also communicated to Mr. Horsburgh, Hydrographer to the H. E. I. C., a sketch of the isles, with the anchorage, for publication and general information. He finally embarked with his wife and family, and several settlers and apprentices, in the Borneo, and arrived safe at the Cocos Islands, in Nov. 1827.

On landing he, somewhat to his surprise, found already settled on them, Mr. Alexander Hare, brother of one of the partners of the house in whose employment he had been, and joint owner with them of the Borneo. Mr. Hare had been formerly Resident at Borneo, under Sir Stamford Raffles' government, but had left that place on its being delivered over to the Dutch authorities. He had considerable possessions in Java, but the Batavian government had prohibited his visiting his estates, which happened to be situated in the disturbed districts. He had been consequently a little unsettled in his plans, and for some time was doubtful where to fix his residence. Mr. Hare was attended by a number of followers, including women and children, originally, it appears, slaves, but manumitted in consequence of our abolition laws. It seems that Mr. Hare was anxious to settle these people where they should run no risk of being molested, in order that he might return to Europe on some urgent affairs. In pursuance of this plan, he had visited the Cape, and purchased a farm there, where Mr. Ross had seen him on a former voyage in the Borneo ; and in a conversation with him, understood him to be dissatisfied with his position there, and desirous of changing it. Many places were mentioned—the Eastern Islands were objected to, on the score of jungle and unhealthiness. Mr. Ross says the Christmas Isles were at last fixed on, and he was requested by Mr. Hare to examine them the next opportunity that offered, and let him know the result. Mr. Hare again insists he had instructed him to visit and examine the Cocos Islands, as he had a strong inclination to settle there ; while Mr. Ross declares, that though mentioned amongst the others, he never understood Mr. Hare to mean, by the term he used in his parting note, (" No-man's land)," any other than the Christmas Isles, on which the conversation had chiefly turned. These he had determined, if a fair opportunity offered, to visit on Mr. Hare's account, though

[1] Declaratory Paper drawn up and read on hoisting the British Flag, 4th Nov. 1827.

from the latter having failed to give him the name of the islands, he must have done so at his own risk[2]. However this may be, Mr. Hare did not wait for any report, but shipped himself and followers on the first vessel proceeding in that direction, and was accordingly found in possession of two-thirds of the islands by Mr. Ross when he arrived from England.

The portion of the islands occupied by Mr. Hare comprised West, Horsburgh, and Direction Islands, (see plate iv), the best and most eligibly situated, as they include the passage into the basin, and are also convenient for ships anchoring, having the deep water close to them. Mr. Ross had, however, at that time no idea that Mr. Hare contemplated a permanent residence on these islands—he supposed him merely anxious to obtain a place of refuge for his people, and that having seen them located to their satisfaction, he would proceed to England. With this impression on his mind, he offered to take charge of Mr. Hare's people—but his offer was rejected, and in terms which, combined with other circumstances, induced a conviction that he was regarded as an intruder, though why, he could not very well understand, as there was ample room for both. He, therefore, formally communicated to his fellow settlers, his intention to occupy the remaining islands for his own purposes; and having previously dispatched the ship on her voyage, he, on the 4th Nov. took possession, and on the 5th, as before mentioned, hoisted the British flag, and read a paper before his assembled followers, declaratory of his plans and intentions in settling on these isles. These were, the acquisition, for himself and followers, of the necessary means of subsistence, by affording such facilities to our eastern commerce, as might appear to be within their power. In the prosecution of these views he proposed cultivating such articles of refreshment as the soil and climate might permit, and also providing, by whatever means, materials for the repair of shipping, and for constructing such conveniences as might be requisite for the assistance of vessels in distress, or requiring to put in for the purpose of refitting. These views, as Mr. Ross observes, are praiseworthy, and such as, in the pursuit of, he might safely calculate on the approbation of the civilized world. To England, in particular, the success of the project could never be indifferent, abstracting from all political views as to the eventual value of these islets in case of a war, for it was one every way English-spirited, and full of promise—calculated, if successful, not only to benefit the projector, but to be of extensive public utility.

Mr. Hare's views, it has been seen, were not quite so public spirited. His plans do not appear to have had any reference to the convenience of ships visiting his islets, or indeed to any other useful public purpose, as far, at least, as the facts which have yet transpired, enable us to judge. But his right to settle on an island which he found unoccupied, may be safely conceded; nor can it be denied that he would be entitled to pursue any private plans he may have contemplated, without molestation or hindrance, the same toleration being extended by him to others, and above all, no opposition or resistance being offered to those who have found the means of conciliating their private interests with those of the public. It is, at the same time, to be noted, that Mr. Ross, in his written declaration, distinctly claims whatever rights may attach to the first settlement of these islands; considering his landing, taking possession, clearing, sowing seeds, and digging wells, in December 1825, as the first act of this kind. Coupled as they were, with the declared intention to proceed to England, for the purpose of bringing out his family and other settlers, it cannot be denied that these must be considered acts of settlement; nor does it appear that a person arriving during his absence, and possessing himself of them, could have any right to be considered the first settler.

But whoever may happen to prove the rightful proprietor of these islands, it must, to the public, be a matter of regret that the most eligibly situated of them has fallen into the present possession of one who seems to take no interest in, if he is not even opposed to, the plan of rendering them generally useful. Mr. Hare's possessions, it has been noticed, are situated round the only entrance within their rocky barrier, and are, therefore, much more eligibly placed for affording assistance or refreshment to ships than Mr. Ross', which are at the other extremity. If Mr. Hare's people then were encouraged, by allotting to them small tracts or farms to raise provisions, &c. for ships touching at these islands, the benefits would be obvious, both to the people themselves and the shipping whom

[2] It is known that if the master of a merchant vessel deviates from the direct prosecution of the voyage, he does it at the risk of being answerable for all subsequent losses whatever, to the end of the voyage; and he is not exonerated therefrom, unless regularly warranted by proper *written* instructions.

they might serve. But Mr. Hare is said rather to oppose himself to any thing of this kind, and to be desirous of retaining his full power over his former slaves, though nominally manumitted. He would prevent their acquiring any property of their own, or even their labouring for their own support. He considers the produce of their labour to belong to him, they being entitled to a bare subsistence at his hands. They are said to be even prohibited from taking any of the fish which are in abundance amongst the islands over which they are distributed! Being employed under overseers in preparing the cocoanut oil and coir, which these islands yield in abundance, and which, in Mr. Hare's opinion, constitute their only claim on our attention; they are entirely dependent on his stores of grain (which are regularly imported from Java,) for support. These are kept at West Island, where Mr. Hare himself resides, and where he has built a sort of stockade for his defence, besides keeping the children of these people as hostages for their good behaviour. Their supply of rice is sent every ten days to them from his granary; so that they are completely dependent on him for their food, and as that is withheld, must starve. Such a sate of things is to be regretted in the case of islands, which, if properly settled, in the first instance, would rapidly rise into notice, and become a place of resort to ships in whose path they lie.

The different views with which these two settlers have singularly enough chosen for their abode so remote, and till now neglected a spot, has occasioned the developement of a less cordial spirit than might be desired, between two Englishmen who have cast their lot so far away from the active sympathy and assistance of their countrymen. The diversity of their views has had the further effect of influencing their conduct with regard to the question of the sovereignty of these islands. Mr. Ross, whose plans were connected with the facilitating our commercial proceedings, and who is in every respect an Englishman, looked, of course, to England for protection, and, in taking possession, naturally enough, hoisted the English flag. He soon afterwards addressed a representation to the governor of the Mauritius, pointing out how useful these islands might become to our commerce, and solicited their being taken formally under his protection, and the islands annexed to the Mauritius as a dependence. This step did not, however, appear to be an advisable one to the authorities at that island, and he was left, in consequence, a little longer to his own resources. He has since that addressed a similar petition to the King in Council, but what has been the success of this latter is not yet known. Mr. Hare again, though an English subject, and consequently incompetent to transfer his allegiance; having no designs that should particularly give him a claim to the support of Englishmen, but having extensive estates in Java, access to which he was denied by the Java government; has been, perhaps, as naturally, led into a coquettish sort of correspondence with that government, in which, however, he has been careful to avoid asking them directly to take these islands under their protection, notwithstanding the evident wishes of the latter that he should adopt such a course.

The natural consequence, however, of Mr. Hare's proceedings at length appeared, in the arrival, in October, 1829, of a Dutch commissioner from Batavia, to enquire into all the particulars of the settlement of the islands, and to collect all the information which the Netherlands government might require towards judging of the question to whom these islands by right belonged. Some ill defined expressions in the treaty of 1824, by which Java and the Eastern Settlements had been ceded to that government, appeared to give some color to the claim, which the authorities at Batavia were evidently now desirous of setting up; and as the English government has been always slow to acknowledge the importance of such settlements, they thought a little well-timed bustle might, before the matter was well understood, settle the point in their favour. Fortunately, however, for Mr. Ross, and those whom his exertions are likely to benefit, means have been taken to put the government in England in possession of the real state of the case; so that it is to be hoped these islands, which being without the limits of the Company's charter, can never justly be claimed by the Dutch, may continue in the possession of the spirited individual who has had the merit of first bringing their value to the notice of the public. Being without the limits of the treaty, every circumstance in their history stamps them English. Discovered by, and named after an Englsh navigator; they have been possessed and settled by English subjects; and this, whether we consider Mr. Hare or Mr. Ross the first settler. The British flag is the only one that has ever waved there. How then can any other nation set up a claim to their possession?

On the 28th October, 1829, arrived the Blora transport, Captain Batten, having on board Mynheer Van der Jaht, the Batavian commissioner. His orders were, as he afterwards explained them to Mr. Ross, to enquire into the particulars of the settlement of these islands ; to survey and explore the several islets and the port, and, in short, to make himself acquainted with every particular, for the information of the Netherlands Indian government. On the 3d November he visited Mr. Ross' settlement, at New Selma, where he made minute enquiries as to the stores, the oil press, &c. and took sketches of the house and landing place. A few days after Mr. Ross, who was absent at that time, having arrived in charge of the ship Joanna Maria Wilhelmina, which belonged to his brother—and understanding the commissioner wished to see him, went on board the Blora. There a conversation was carried on through the medium of the Captain, who understood English—of the general bearings of which, the following may give some idea.

Com. It has been reported that you have hoisted the British flag here.

Mr. R. If so—what then?

Com. Oh, I don't know, only we wished to ascertain the fact.

Mr. R. Well then, those who so reported may make the most of it. I do not conceive that if I, a British subject, hoisted the British flag over my own house, that there was any thing criminal in it, or calling for the notice of any foreign government. Do you consider that the Netherlands government mean to take possession of these isles?

Com. I do not know, but I imagine they will ; intimating their intention to the British government. What do you think may be made of these isles?

Mr. R. A tolerable estate for a private person, after they have been cleared, and machinery constructed for manufacturing the produce.

Com. What do you think of our government sending a hundred or so of convicts to clear these isles?

Mr. R. I think it will be equivalent to driving us off.

Com. Have you any objections to recognise the Dutch government?

Mr. R. As an individual I should have no objection to any legitimate government. As a British born subject, however, I have no power to transfer my allegiance to any foreign sovereign.

Com. The Dutch government is doubtless entitled to these isles on account of their proximity to its territories.

Mr. R. It is rather a large proximity ; but be that as it may, I am not aware that proximity is an inviolable rule of decision in such cases. I do not however deem myself competent to enter into the question. It is one for the discussion of government, not of a private individual like myself. My views in forming this settlement are already published by Horsburgh. I think they deserve the encouragement of all who have commercial interests in the Indian Seas. I am here with my family settled on a previously unsettled and unoccupied ground, and I expect that whatever government may exercise the supremacy over it, I shall by it be fairly if not kindly used.

Com. Oh no doubt. The best thing our government could do, would be to appoint you Resident here.

Mr. R. Doubtless whatever government assumes the supremacy over us, will consult its own pleasure in its official appointments.

Com. You can of course inform us particularly about the people Mr. Hare has brought here, how he uses them, and on what footing they are with him.

Mr. R. I have had little or no communication with Mr. Hare for a considerable period. We are not on very friendly terms, and I do not wish to mingle myself with any questions of the kind relating to his affairs.

Com. Oh I have no doubt of your stating what you know in a spirit of fairness for all that.

Mr. R. Excuse me. I must altogether decline entering upon the subject.

The Commissioner after this adverted to reports which he asserted were generally current, that Mr. Hare and Mr. Ross intended engaging in the smuggling trade to Java and the other islands. Mr. Ross easily convinced him of the absurdity of such a rumour, as far he was himself concerned. Even if he had the means, not to say the will, of engaging in such a trade, he showed that he must be worse than mad to attempt it, as he could never possibly hope for the countenance of the British Government, if once engaged in such illegal proceedings. The Commissioner admitted the justice of this observation, and after some other conversation on indifferent subjects, they separated. Mynheer Van der Jaht, before leaving the place, requested Mr. Ross to give him, in writing, all that he had stated to him, but this Mr. Ross declined doing, and the Commissioner a few days after sailed for Batavia.

No further immediate steps were taken by the Batavian government; but the question being once raised, Mr. Ross foresaw that time would elapse before it could be settled; and in the possible event of the islands falling into the hands of the Dutch he feared that he might not be able to fulfil the expectations held out to the settlers and work people brought from England. He, therefore, candidly declared to them the change of circumstances which had occurred, from what he had contemplated when engaging them, and the possibility that he might not be able to fulfil all he had promised them. He gave them, therefore, the option of remaining on these terms, or of leaving him to better their fortune elsewhere. The latter alternative most of them accepted; and of the number of fourteen whom he brought from England, only two remained to take their chance with him. The following is a list of his party as at first constituted.

	Native Country.	*Remarks.*
J. C. Ross, his wife and 5 children, ..	Great Britain, ...	One since born.
Anna Andrews, ...	ditto, ...	Servant to Mrs. Ross, since married to W. C. Lish.
W. C. Lish, ...	.. ....	
Andrew Moody, J. Steevens, C. Steevens, J. B. Grey, } Apprentices, ..	ditto, ...	Left in H. M. Ship Comet.
Joseph Bayley, Carpenter, ...	ditto, ...	Left for England, per Ship Borneo.
Thomas Deely, ditto, ...	ditto, ...	Left for Java, per Ship Tartar.
R. Steevens, Armourer, ...	ditto, ...	Went on a visit to Batavia, per Mulatto, and died there.
G. Brown, Boatman and Tailor, ..	ditto, ...	Left in H. M. Cruiser Comet.
H. Keld, lad from the wrecked Brig, ..	ditto, ...	
J. Munslaw, Boatman, ..	ditto,	Left in H. M. Cruiser Comet.
Juan Antonis, Cook, ..	Portugal, ...	
Kecheel, Boy, ...	Java, ..	

Three Javanese, one cook, one boy, and three women, all servants on wages, were subsequently obtained, and Mr. Ross expected from England three married families as settlers.

The present imperfect sketch of the settlement of these Islands cannot be better concluded than by the following interesting description, written by one who appears well acquainted with them, which, with the Chart annexed, will, it is to be hoped, render them better known, and give a favourable idea of their capabilities.

Description.

The Cocos or Keeling Isles, so far as appears, are entirely of the coralline formation. The sand and fragments dug out of the wells, near the middle of the isles, being altogether of the same material, rounded by attrition in water, similarly to that which at present constitutes the shores and beaches around the isles. The circular form of the group, as of the detached northernmost Keeling Isle, may however, countenance an idea of their being originally based on submarine volcanoes, long extinguished. No traces of the occurrence of earthquakes, or other natural convulsions, are discernible on their shores or surfaces.

The most southern or windward part of the group seems to be the most ancient. Much of the bay is there filled up nearly to the height of low water, neap tides; and the growing state of the coral in the bay, and around the isles, furnishing a continual accession of materials, renders it likely that, in the course of not an extremely long period, the extent of the elevated surface will be considerably increased.

Around the exterior of the isles the shore is heaped up by the surf, from 12 to 21 feet above the level of the sea, high water mark. Interior shores and surfaces are not however, in general, elevated more than from 3 to 6 feet above that level.

The soil is mainly comprised of fine calcareous sand, in some parts inclining to marly, in others strong, (that is rounded pieces of coral and shells,) with a small admixture of vegetable earth, and is from one to two feet deep, lying on a strong platform of aggregated coral shells, &c. Quantities of pumice stone and lava are thrown on shore by the sea; and by decomposition, an addition of earthy matter is being made to the soil.

The general produce of the isles is the coco-nut palm tree; the which, when thinned out to due distance from each other, may be expected to yield as abundantly as in any part of the world.

2nd. A tree producing hard wood, of a dark color, fit for fuel, and for timbers of small craft and vessels up to 300 tons.

3rd. A tree having a leaf resembling, in size and form, the leaf of the box-wood.—The timber hard and heavy, of a reddish color, fit for small parts of machinery, boat's timbers, &c. The bark, having an uncommonly large portion of tan in its composition, is useful for making leather, preserving nets, &c.

4th. A large straight growing tree, which furnishes poles and small spars for rafters, &c.—durable under cover, but subject to rapid decay when exposed to the weather.

5th. A large straight growing tree, leaves and fruit resembling the jack in the box, of the West Indies. The timber is, however, soft, and of little value.

6th. A tree frequently of large dimensions, the wood of which decays even faster than it dries. Its leaves may be used as greens, and are good food for hogs, and with the trunks supply a considerable quantity of vegetable matter to the soil.

7th. The tree called Waroo by the Javanese: it is planted by them in front of their houses, &c. for its shade and flowers. The timber is useful and durable when of large growth; the bark affords a material for making twine and fishing lines.

8th. The Chinkanen or Dadap; a soft wooded green, thorny, barked tree, used in Sumatra for training the pepper vine upon.

9th. A tree, whose fruit, when cut, resembles plum cake, and may be pickled; its root rasped, and infused into a ley of potass, yields a scarlet dye.

10th. A very few small scattered trees and shrubbery plants, and two species of large trees, furnishing literally fine wood, and growing near the shores. One much like the Cripple Boom of the Cape of Good Hope, the other something like the *Woroo:* Also a large tree producing a square nut, of about 6 inches in diameter, rusky on the outside. Many species of creeping plants, one or two highly antiscorbutic, and may be used as salading grass: about four species, all rough and bitterish, not relished by animals.

The whole of these productions are transportable by the sea, in which the seeds and roots of these long retain their germinating power—no countenance is, therefore, afforded from any of these, to the idea or doctrine of spontaneous generation.

Two species of Gannet and the Frigate Bird are particularly abundant on, and around these isles. Many other oceanic birds visit them occasionally. Cranes, blueish, grey and white, in small numbers, Sand Pipers, and a species of Sandrail, are all the birds, not of the web-footed kinds, which are found about these isles. Land crabs, good for food, are plentifully found on them.

Turtle are very numerous,—may be caught without difficulty, and furnish a nutritive food in all seasons.

Fish of many species, nearly all of a good taste and flavour, exist in great abundance all around the isles, and throughout the bay. Ground sharks are not very numerous, but a small species, having the tips of the tail and fins of a black colour, is rather plentiful. No poisonous fish have yet ben found any where within the isles.

No seals, alligators, or other amphibious animals, besides turtle, have been seen, nor any reptiles or snakes.

Since the establishment of the settlement, the following plants, animals, &c. have been introduced, and have succeeded, or are likely to succeed. Fig Tree; Red Mulberry; Shaddock; Custard Apple; Orange; Lime; *Lingsop; Jamboo; Allay;* Tamarind; Pomegranate; *Papaee* or *Paypaya; Mongua; Taijung;* Chillies; Aloe; hedge plants; flowering shrubs, sundry plants from the Mauritius, names, at present, unknown. Lemon grass, and five species of good grass for cattle, the Cotton Plant from Bourbon, sugar cane, two species; Plantain and Banana, seven species; Tobacco; Kluddy, an extremely farinaceous sort of a large sized Pumpkin; Gourds; *Brinjals;* water melons; sundry other India vegetables; sweet Potatoes; common Potatoes, while the sun is in the Northern hemisphere; flag-leaved Leek; Parsley; Celery; Cos Lettuce; Endive; Mustard and Cress. Turnips; Radishes; and Cabbages thrive, but we have not succeeded in obtaining seed from them. Maize, very productive, flourishes throughout the year, in which period successive crops are obtainable. Coffee; Corn, from Cape of Good Hope, rises to near fifteen feet in height. Cattle, Goats, Hogs, Poultry, Ducks, Geese, Turkeys. The Climate, though warm, is perfectly salubrious; range of the thermometer in the warmest season is from 78° to 86°, in the coldest from 72° to 81°: the general winds are from southward to eastward, subject to interruptions from the vicinity of the N. W. monsoon, from January to March. No two seasons have been as yet alike since the formation of the settlement ; but until the present, we have never had more than an occasional squall or breeze of a few hours continuance from the northward or westward. Fresh water, obtained from wells dug on the isles, is good, wholesome, and abundant. The anchorage is safe ; the narrow opening between the Reefs of Horsburg and Direction Isles not affording ingress to any heavy sea, as was experienced during the late northerly gale. The Channel leading into the inner anchorages has only 3¾ fathoms at low water, and is tortuous and narrow for nearly a mile. During the prevalence of the general winds, no vessel drawing more than 12 feet water may safely sail in ; all larger must remain in the outer anchorage. Ships requiring heaving down, &c. may be warped, for that purpose, into the basin inside of Direction Island. No vessel can, however, have occasion for entering the port beyond the outer anchorage, except for safety in time of war ; the intricacy of its entrance is then an advantage ; since enemies' vessels could not reach them to capture them, by making a sudden dash into the harbour, and would not care to run the risk of entangling themselves therein, and thereby running the chance of having their retreat cut off by the arrival of a superior force.

High water occurs at about half past 4 o'clock in the anchorage, on full and change. When the sun is near the equator the two tides are nearly equal, and rise from 3½ to 4 feet ; when he is near the southern tropic, his zenith or evening tide rises to from 5 to 5½ feet ; and the morning tide to 1½ and 3 feet. The contrary happens when he is near the northern solstice ; thence indicating that his attractive force is exerted on the globe in a diagonal direction. The zenith tide being raised in our hemisphere, and the nadir in the other ; and it would be seen, that by keeping this circumstance in view, the occurrence of 12 hours' tides, or one ebb and one flood, in 24 hours, may be accounted for by reference to the moon's position at the time ; assuming that her force is also diagonally exerted, while from the rapidity of her declination it is more or less weakened or merged into the more regular effect produced by the sun.

Report of Shipping, which have anchored in the Port of the Cocos or Keeling *Isles, since the British settlement has been established on them.*

Dates of Arrival.	Ships' Names and Commanders.	Flag.	Burthen.	Whence, & whither bound.	Why Anchored.	Date of Departure.
1827.			Tons			
Feb. 16,	Ship Borneo, J. C. Ross,	B.	428	London, and Cape of Good Hope, Java,	To land party of settlers,	6 March.
Apl. 10,	Ditto ditto, ditto,	do.	do	Batavia, to Sumatra,	Touched to land supplies,	25 April.
May 8,	Do. Hippomenes, R. C. Ross,	do.	336	Sumatra, to England,	To fill up water and wood,	16 May.
Do. 12,	Schooner Rembary, Benot,	D.	120	Java, to Sumatra,	To land supplies,	19 ditto.
Do. 26,	Ship Phœnix, Anderson,	B.	450	Do. to England,	To secure a started butt end, which done, sailed same evening,	26 ditto.
Aug. 18,	Do. Borneo, J. C. Ross,	do.	428	Coast of Sumatra, to ditto,	To be dispatched for England, under command of R. A. Whichelo,	3 Sept.
Nov. 28,	Bark Leda,—Northwood,	do.	280	Sydney, N. S. W. in 35 days, to Calcutta,	For water,	29 ditto.
						1828.
Dec. 2,	Schooner Molucho, J. Clunies,	D.	120	Surabaya, to Sumatra,	To land supplies,	10 Jan.
Do. 11,	Ship Panther, Lloyd Bowers,	N.	380	Java, Cowes, to England,	To stop a leak,	14 Dec.
1828.						
May 22,	Schooner Molucho, J. Clunies,	D.	120	Ditto, to Batavia,	To land supplies,	15 Aug.
Ditto,	Do. Nancy, Bernado,	do.	35	Ditto, to ditto,	Very leaky,	Ditto.
Oct. 31,	Brig Walmouth Castle, G. Sinclair,	B.	290	Glasgow, to Calcutta,	To take in water.	4 Nov.

Dates of Arrival.	Ships' Names and Commanders.	Flag.	Burden	Whence, & whither bound.	Why Anchored.	Date of Departure.
1827.			Tons			
Nov. 15,	Ship Mary, — Green,	D.	355	Java, to Sumatra,	To communicate and convoy Schr. Nancy,	27 Nov.
Ditto,	Schooner Nancy, Seegers,	do.	35	Ditto, to ditto,	On route to Sumatra,	Ditto.
Do. 28,	Ship Hypomenes, R. C. Ross,	B.	336	London, (94 days) to Java,	Letters and stores,	5 Dec.
1829*,						
Mrc. 23,	Do. Borneo, R. M. Whichelo,	do.	428	Van Diemen's Land, to ditto,	To communicate, &c.	31 March.
June 24,	Ditto ditto, ditto,	do.	do.	Java, to England,	To fill up water, catch turtles, &c.	4 July.
July 12,	Johanna Maria Wilhilmina, J. C. Ross.	D.	320	Sumatra, to Bourbon,	Commr. dangerously ill, to be landed,	19 July, in charge of J. C. Ross.
Oct. 28,	Schooner Blora, —Batten,	do	230	Batavia, to Cocos or Keelings,	Bring a N. I. Govt. Comr. to enquire, explore, &c.	16 Nov.
Nov. 7,	Ship J. M. Wilhilmina, J. C. Ross, in charge	do.	320	Bourbon and Mauritius to Sumatra,	To receive her Commander,	14 ditto.
Dec. 20,	H. M. S. Cruizer, Capt. Colpoys,	B.		Madras, to Swan River,	Wood, water, turtle, &c.	23 Dec.
1830.						
Feb. 3,	Schooner Tartar, H. Simmons,	D.	230	Java, to Sumatra,	In distress: channels and chain plates carried away, and water expended,	10 Feb.
Dittó,	Bark Norfolk, Alxr. Greig.	B.	550	Swan River, Madras, &c., to Calcutta,	For turtle, &c,	6 ditto.
Ditto,	H. M. S. Comet, Capt. Saundilands,	do.		Trincomalee,	For the purpose of enquiry.	

* 1829. 4th Jan. Brig Civilian, of Liverpool, from Sydney for Java, landed her boat for supplies, but did not anchor.

A true report up to this date, by me,

New Selma, 13*th March*, 1830. (Signed,) J. C. Ross.

Remarks by the Editor.

The official documents from which the materials of the preceding account have been taken, were furnished to us by the kindness and liberality of the functionary, to whose portfolio they belong, under the authority of Government. Our readers will, on referring to the preceding numbers of the Gleanings, from the first establishment of the work, not fail to discover many other occasions on which we have been equally indebted to official encouragement and patronage. We take the present occasion to make our acknowledgements for this and the other favours of the kind we have received; and we venture to express our hope, that the present valuable and interesting contribution may be followed by many others—and that our pages may be deemed worthy of receiving some of the numerous valuable documents (connected with general science) which it is well known are to be found in the many public offices at this presidency. To rescue such papers from the dust and cobwebs, no less than from the oblivion in which they are immersed, or rather, from the total destruction which the depredations of insects, supposing the climate to spare them, threaten, at no very distant period, would certainly be one of the most useful objects to which our work could be applied. Nor are we insensible of the value which our miscellany would acquire, particularly in Europe, from being made the depository of so much valuable information,—information, which unpublished, is lost altogether to the public; while often, from its very nature, it can scarcely be conceived to be in any way useful even to the Government.

II.—*On Value.*

§ 3.—*Of the Constant Connection between Wealth and Value.*

If mankind had but one want,—namely, for food; and if they were under the unchecked influence of the principle of population and increase; it is evident, from what has been said, that an improvement in the productive processes by which food is obtained, which led to the same quantity of food being obtainable with half the former labour, would ultimately be followed by no loss of value; but, on the contrary, by a great aggregate increase of value. The formerly experienced impossibility of increasing the quantity of food, which alone had repressed the advance of population, being now removed, population would increase of course; and would continue to increase, till, again, the utmost labour which could be bestowed in agriculture, would cease to set free produce sufficient for the support of greater numbers than had then sprung into being. If the country were of the extent of 100 square miles, and if the most every mile gave off, with the former knowledge of productive arts, were a clear surplus, equal to subsistence for 100 individuals; every mile of the country, under the supposition of the labour required in raising food being reduced one-half, would, we may assume, be found capable of supporting 200 souls; and this double production, coming to be appreciated by double the number of individuals, each of whom now, as formerly, laboured his utmost in obtaining the food which supports him, could not fail to be of double the former aggregate value; for each man's share would be of the same positive value that it held before. If again we suppose some wrought wares to be as necessary to the existence of man as food; and that besides what suffices for supplying his food, each person, to be kept in existence, must be supplied with what suffices for the support of another individual, who is to busy himself preparing these essential wrought products; then also, in the event of an improved process in manufactures, which reduced the cost of production one-half, it will be seen that no loss of value could take place on account of the great increase in the means of enjoyment, or of the increased wealth, which would be consequent on the change; for, as in the case where food alone was consumed, an opening would now be made, although in a somewhat different manner, for an increase of population,—for an increase of those who make valuation; and who must be as willing as the smaller population had been before, each to bestow his greatest exertions for what suffices to his support. If what was essential to one man's support between the returns of the harvests, were 20 measures of grain; and if twenty on any given tract of country were the number of labourers by whose exertions the greatest net produce available for man's use, or 400 measures could be obtained; the labour of 10, or of any number less than 20, on the one hand, causing a net reproduction to be evolved, less in amount than 400 measures; while, an the other hand, the employment of 21, or of any number more than 20 labourers tending to the evolution of a net produce, greater perhaps than 400 measures, yet not equal to 420 measures, or to what might be absolutely essential to the support of the additional labourers; in this case, neither more nor fewer than 20 labourers could be found existing on this tract; so long as 20 measures were absolutely essential to the existence of a labourer.

But if it should so happen, that from any sufficient cause, 15, in place of 20 measures, were found to suffice for the subsistence of labourers, then the productive circumstances of every such tract of land would be materially altered; and extension of cultivation, and increase of population, would be the inevitable consequence of such a change. The existing population would require only 300 in place of 400 measures for their actual support, and the present produce is equal nearly to the support of 27 men living at this reduced rate: their numbers will, therefore, indoubtedly increase to this extent, and to such greater extent as the productiveness of the soil, and seed will allow. Suppose that when thirty labourers worked the soil under these new circumstances, for each of whom 15 measures was absolutely essential, 450 measures were the net produce obtained; and that when 29 were employed, 445 measures were the result; 435 being all that 29 absolutely required; while, on the other hand, when forty labourers were set to work, the net produce was 540 measures; 600 being the quantity they required, it is evident that thirty must be the permanent agricultural population of the tract[1].

[1] A measure of seed, on an acre of land, may yield a net return of 100 fold; but it does not follow that two, or four measures, on the same extent of land, will yield a return of double, and quadruple the amount obtained from the single measure: be-

Now the cheapening we have supposed in the wrought wares essential to man's existence, when one-half of his income was expended on these, which reduced the cost of production of the quantity he required, from 10 to 5 measures of corn, is precisely the circumstance adove adverted to. If 20 measures must be raised to support each labourer, one-half forming his own proper consumption of food, and the other half forming that of the person supplying his wrought necessaries; then, if these wrought necessaries be reduced one-half in the cost of production, or, which is the same thing, if they be obtainable through improvements in manufactures with half the former labour, it is evident, by this reduction of the amount of food required, both directly, and indirectly, for the support of men, that cultivation might, with advantage to them, be pushed over lands less, by one fourth in fertility, than those formerly the worst in use, and that high cultivation might be pushed, in the same proportion, over those more fertile tracts already under the plough? The smaller quantity of food now requisite, both directly and indirectly, to maintain labourers, would ultimately, however, be found to be of as great positive value, when actually obtained, as the larger quantity formerly had been; it would still be the result of the greatest exertion the labourers could make, and would be held, in consequence, in similar esteem. The positive value of specific quantities of food would have risen exactly in proportion to what had been lost in the positive value of the cheapened wrought products.

In the above case, what suffices, both directly and indirectly, to support human beings, has become a smaller quantity, but it cannot have become of smaller positive value. There will have been a greater quantity of products obtained; there will have been a greater population supported; and there will have been a greater positive value attached to every portion of that produce which was formerly essential to a man's consumption; and there will have been a greater quantity of products realized to be held in estimation by a greater number of people.

But it may be said, that I have been subverting my own theory of the immutable nature of the value of food, by admitting, that on the occurrence of a cheapening in wrought wares, the food of a man will rise in general esteem. If, at first, it were necessary for each man's existence, that he raised agricultural produce sufficient for himself and for another besides, who prepared his wrought wares; and if, by improved processes in manufactures, it should become essential to his existence, that he should raise food sufficient, not for two men, but for one and a half, then it is evident that the positive value men would place upon the smaller agricultural produce, now necessary for the supply of their food and wrought wares conjointly, would be as great as the positive value they formerly placed on the larger quantity; while it is also evident that the value of the quantity of agricultural produce, both directly and indirectly, necessary for keeping a man in existence, if determined by reference to a specific quan-

cause the greater quantity of seed expended, may be such as to interfere with the full developement and ripening of all the various germs employed.

The smaller quantity of seed is not, however, that, by the employment of which, men are most benefitted, although the proportion return bears to outlay be then the highest obtainable; they must look not to the greatness of the proportion which the return bears to the outlay, but to the greatness of the net aggregate realized; and the greatest aggregate return is, in most cases, found to be obtainable, when other than the highest proportion holds between these too. Thus, from 10 measures of seed on the acre, 200 measures of net increase may be realized; from 20 measures, 240 of net return; and from 50 measures 260; again, from 60 measures of seed no more might possibly be obtained than 260; and from 100 measures, 200 of net increase might not be obtained. The matter to be determined in practice, is the point at which the greatest net aggregate return is obtainable; and the question of the proportions, which may happen at this time to hold, between outlay and return, is one of mere curiosity. What has here been said of seed in agriculture, holds good also in all productive outlay whatsoever: of labour in agriculture, (as in the case given in the text,) or of capital therein, or in the production of secondary wealth. Hence it follows, that when Political Economists conceive it to be the rate of gain realized, whether it be in profits, wages, or rents; or, in other words, when they assume, that it is the proportion existing between outlay and return, which determines the extent of productive expenditure, they run into error; for it must be the aggregate amount of profits, &c. to which producers look, and not to the accidental proportions which happen to subsist between outlay and return, at the time this aggregate is at its highest. This most important, and most obvious principle, has heretofore been overlooked; or if applied at all, it has been so incidentally. Mr. Malthus and Mr. Perronet Thompson, (author of the Catechism of the Corn Laws,) do indeed use it with reference to rent alone; its influence on profits, and on the net returns proceeding from labour, has not been adverted to by any writer with whose works I am familiar.

tity of food, as a standard of value, would be found to have fallen one fourth. But what I contended for was, that the value of the quantity which was actually necessary for the support of a man was immutable; we now find that, in practice, what is virtually necessary for his support, may undergo change. Still, however, I maintain, that at any given time, (passing changes being disregarded,) what is, in practice, found to be requisite to man's support must enjoy a fixed value, whether it be 10 or 20 measures. But admitting that when this quantity experiences increase or diminution (as in the case already given), the value of specific quantities will be altered, is not receding from my former position; that, given the quantity necessary for the support of a man, that quantity, whatever its amount, must be of a determinate value.

It is very true then in practice, as we are told by Lord Lauderdale, and many other writers, that there is nothing which possesses a real, intrinsic, or invariable value; which may, in all cases whatsoever, be referred to as a standard: even determinate quantities of food must, in practice, we see, be susceptible of change in positive value; but this, it must be remarked, is consequent, not on the variability of value, in that which supports a man, but on the accidental circumstance of man's wants, coming in process of time to include other necessaries than mere food. If food alone, at all times, and under all circumstances, sufficed to satisfy the wants of man, then its value must be invariable; and for reasons too which Lord Lauderdale himself points out; "abundance," he says, "of the necessaries of life has a direct tendency to increase population, and by this means to restore the proportion betwixt the demand and the quantity of the increased commodities; thus maintaining their value, notwithstanding their abundance."

From the whole of the above considerations it will appear, that Mr. Ricardo is as far from the truth, in maintaining, that although a million of men may, after the introduction of improvements in production, make double, or treble the amount of riches; they will not thereby have made any increase of value; as Lord Lauderdale is where, in another part of his work, he says, that value might be increased by creating a scarcity. Both leave out of consideration that the existence of value depends on the existence of appreciators of value; that with the real increase of wealth, these also must increase; and that with diminution of wealth these must be diminished also.

Scarcity proceeding from difficulty of production, taken by itself, is not a cause of high value, neither is abundance a cause of the reverse; for if food were so scarce, and so hard to be raised, that the labour required in the acquisition of what would support a man, were beyond what the power of man could bestow, it would be utterly without value; and this simply, because there could not possibly be in existence any men to make an appreciation of its value: and if it were, on the other hand, so abundant, that every foot of the habitable globe were appropriated to its growth, every pound of it would, notwithstanding this vast augmentation of quantity, be possessed of a specific value; because, in proportion to its quantity, so would be the number of persons ready to devote their utmost exertions to obtain a sufficiency to meet their wants, and duly to appreciate its value when obtained. All discussions, therefore, of value, abstracted from the consideration of the moral agents who are to make appreciations of value, must lead to false conclusions.

Value abstracted from the sentiments of men cannot be known; and the only approximation to a standard of value with which we can be practically acquainted, is a specific quantity of that description of wealth which is held in the same esteem by mankind, in all situations wherein society is not under the immediate influence of some passing change; in other words, due allowance being made for differences in prevailing habits, in the lower orders of different countries, and of different times, the quantity of food which must be raised, before one man can obtain a sufficiency for the supply of his wants, is that which approaches most nearly to a standard of valuation.

If the above reasonings be true, it does not appear that Adam Smith was much in error, in estimating wealth, and value indifferently, by the labour which a person has at command, or by the command he holds over the comforts, necessaries, and enjoyments of life; for it has been shewn, that, although at different periods, a specific quantity of labour may produce different quantities of these latter; it has also been shewn, that the same facility which exists for producing items of wealth, extends, nearly as much, towards the production of value also; the principle requiring modification, when applied to societies in which other articles, besides food, have come to be counted as essentially necessary to human support.

In reasoning towards the establishment of a general principle, it is, in most cases, unnecessary to look to any effects proceeding from other causes, than those immediately under consideration: and viewed under this aspect, it appears that value and wealth must proceed hand in hand on the increase, so long as man's only want is food; that which ministers to the one, ministering in like manner to the other. If then we make no allowance for the existence of other wants than food in the lower orders, and for the changes which are likely to be brought about in the modes of thinking of all classes in a society which is making progress in the arts of production, we are justly entitled to calculate on the same exact quantity of products being necessary for human support, both before and after a change is brought about in the means of obtaining products. But in society, as it actually exists, there are various classes; some enjoying, and expending, on their own persons, what would suffice for the support of scores—nay hundreds of their countrymen, and influenced by feelings which would revolt at the prospect of bringing children into the world, to be possessed of less than they themselves consume; and we know that these feelings do not fail, in time, to descend even to the lowest classes. There are, in consequence, various changes in the habits, and wants of all classes, which must be frequently brought about by the very circumstance we now have under consideration, a reduction, namely, in the cost of obtaining products. In a highly civilized, and rapidly advancing country, where a large proportion of persons in easy circumstances exists; and where the procreative influence is counteracted, even amongst the lowest classes, by prevailing modes of thinking; it is highly probable that an increase of the sum of enjoyments, brought within the reach of the inhabitants, may more directly tend to the spread of luxurious habits, and to a permanent increase of the wants of families, amongst all classes, than to an extension of population. In this case, the increase of positive value, in the aggregate, will be checked by that which checks the increase of population; facility of production causing little other effect on society, than an increased consumption by the existing population. But in practice we must calculate, neither an increase of people alone, nor on an improvement in the modes of living exactly corresponding with the additional facility now enjoyed for obtaining that which constitutes man's means of existence. We must always calculate on a mixed effect from the conflicting influence of the prolific power, and from the inevitable progress of luxurious habits; for it is certain, that there never could be produced in society the unmixed effect which is supposed, when we admit the spread of luxurious habits alone, and which, by the bye, is the supposition entertained by Mr. Ricardo, and all who follow his steps, in supposing value does not, of necessity, increase along with riches; for in such a case, there could exist no competent motive to induce producers of wrought wares, to contrive modes for cheapening their respective products. If, for instance, after the discovery of a process in manufactures, by which double the quantity of produce is obtained in an equal time, I, as a manufacturer, should find that I gained only as much as before the introduction of the new process --if, in fact, I gave all men twice the quantity of my wares for the same return in theirs, of what concern would it be to me whether the new process were adopted or not? In as far as I am concerned as a consumer of my own product, this may be of consequence; and in as far as, by the cheapening of the products, the market might be extended to those very indigent classes by whom the product at the former cost was unattainable, so far I might ultimately be benefitted. These are, however, but trifling inducements, and inadequate to the rapid progress of improvements, and extensions of supply consequent thereon, which take place in the world. We may rest assured, therefore, that there is effected no considerable improvement in manufactures and consequent extension of supply, except in societies where not only the wealth, but the value also of the wealth, proceed hand in hand on the increase; and although this progress may not, perhaps, proceed in a manner so clear and marked, as in the illustrations I have given of the principle, in which no check on the principle of population is supposed to exist, and no changes in the habits of the people are contemplated, still in a manner not very different from what is there pointed out.

Knowing full well, as we all do, that in different countries at the same time, and in the same country at different times, very great changes occur in what is esteemed sufficient for the mere subsistence of even the lower orders; that society consists not of one class, but of many; all of which must be likely to increase their numbers on very different estimates of what is sufficient for the maintenance of the individuals of their class; knowing also that the world, or the society on its surface rather, is in a state of continual progression; the knowledge of this day being added to by that of tomorrow; each accession of knowledge widening the field of production, and each facility in production, leading to different sentiments, with regard to what is essential to existence; it cannot, therefore, be supposed, that a perfect standard of value, founded on the basis of the esteem in which men hold any given quantity of the necessaries of life, can possibly exist: for this implies an unanimity of opinion, with regard to what is sufficient for the support of all orders in society; and it implies that a fixed quantity of food shall always suffice for their wants, both directly and indirectly; and under the supposition of the existence of this unanimity of opinion, as it could only be expected to be in full force, for the short intervals which may be supposed to occur, between the fullest enrichment, consequent on the last improvement in productive knowledge, and the introduction of other improved processes—the passing intervals during which society was neither advancing nor receding; it is evident that, in practice, we can possess but a very rough approximation, indeed, to a standard whereby to estimate positive value; a standard liable to alteration with every change of manners, and with every change in the means of obtaining products. We may see, however, the principle upon which such a standard must be formed; and we may be assured, that of value abstracted from the feelings and opinions of moral agents, we can have no knowledge whatever, and we may apprehend the extent of error, and misconception regarding man and his wealth to which we lay ourselves open, when we attempt to elucidate the progress of production and riches through the aid of a standard formed on the relations of products amongst themselves, or indeed any other principles than those which have been pointed out. I trust, also, that, through the means of the approach to a standard of positive value, which I have endeavoured to indicate, this apparent paradox has been explained, and that it has been shewn on somewhat intelligible grounds how it happens, that all increases of wrought as well as unwrought wealth, when brought permanently within the reach of society, should likewise be possessed of increased value; and that too, in a degree bearing some proportion to their quantity; even although the means by which the quantity is increased are those which tend to an immediate reduction of value in each of the individual products of which wealth is composed.

It appears strange that such a discussion should have become necessary, as one to prove, that every increase of wealth—of that in which alone appreciable value can exist,—must be attended with increase of value; but it is certain that Adam Smith, and other writers, by leaving this matter unexplained, did lay themselves open to the objections started by Mr. Ricardo. If it cannot be proved that increasing wealth is inevitably accompanied by increasing value—if it be not shewn that they must wax and wane together, then it never can be admitted, as Adam Smith takes for granted it should be, that the command enjoyed over the necessaries, conveniences, and amusements of life, is the same thing at different times, and under different circumstances of production, as the command of the labour of the persons who provided these means of enjoyment. Now however, that the objections have been started, and it is hoped, removed, men may be satisfied again with the evidence of their senses; and may be assured, that in obtaining a permanently increased command of wealth, they are ensuring to themselves an increased command of value also.

I repeat, however, it must not be supposed, that in the study of highly improved societies, it is at all necessary to the existence of the principle I have been attempting to explain, that the effects of an increased command of wealth should be exhibited in any one direction alone—that the only ultimate consequence should ever, in practice, be the extension of population alone, or the extension of the list of necessaries alone amongst the former amount of population, unaccompanied by an increase of value. On the contrary, there is nothing I would more force upon the mind, than that the principles of production and increase, are subject to constant modification, and counteraction, by the operation of the thousand different moral and political influences, by which man must ever be actuated; and that in no instance whatever, in actual life, are we to count on the unmixed results pointed out as inevitable ultimate consequences, in such abstract discussions as those in which I have been engaged. If, for instance, an improvement should occur in the production of wrought wares in England; so far from anticipating that the increased command of enjoyments thus obtained would tend to a mere increase of population, to a consequent reduction of the quantity of food, directly and indirectly, necessary for supporting men, and to a consequent increase of positive value in what served to support a man, exactly proportioned to the increased command of wealth; I should expect to witness, in such a society as there exists, the spread of luxurious habits amongst all classes; not that more people should spring up, living in exactly the same manner as their predecessors; but that a slightly increased population should ultimately be found in existence, to each individual, of which more products had become essential for what had come to be considered his respectable maintenance:—that if 10 quarters of corn were formerly necessary for the support of the labourer himself, and the manufacturer who supplied his wrought necessaries; and if after a doubling of the productive power of manufacturers 7½ were necessary for yielding him the same sum of enjoyment; the habits and feelings of the labourer would, with the increased facility of obtaining wrought products, undergo a change; and he might perhaps be ultimately found contented to live, and to increase his numbers only when 8 quarters were placed at his disposal, and secured to each of his offspring.

Here, although the command of enjoyments would be increased, the means of extending population would be also enjoyed within the country; and although, the aggregate absolute value now obtained within the country would be increased also, it is clear that it would not have experienced such an increase as would have followed such an extension of population—as might have taken place, if no change had occurred in the habits of the people.

The reasoner who has seen, in the clearest point of view, the fallacy of saying, that wealth and exchangeable value are identical, is Colonel Torrens; and he gives very apt illustrations of the absurdity of the proposition, as propounded by his predecessors. (Torrens on Wealth, page 10—11.) But to what do his reasonings lead? That wealth may exist, which is destitute of value! If he says exchangeable value alone exist, and if all the wealth, supplying human wants, were directly obtained by those who ultimately consume it, then there might be abundance of wealth, and all without value.

But agreeably to my conceptions, it is an essential characteristic of wealth, that it shall, under all circumstances, be possessed of value. Of what value, then, must it be possessed? Either of positive value, or of none. He, treating the only value we know, as being the exchangeable value of Political Economists, deprecates, therefore, the employment of the term value at all; and he defines wealth to consist of articles which possess utility, and which are procured by some portion of voluntary effort. But as value is an affection of the human mind, created by the existence of useful products, which cannot be obtained for nothing; and as products calculated to cause that affection, may be procurable, directly by labour, as well as indirectly by the means of barter—as they may be procurable in fact by voluntary effort; then it follows that they may, and must be valuable, whether barter be effected or not. Exchangeable or relative value is not, most certainly, an essential quality of wealth. But real value is; for the circumstance of value being essential to its existence, is the same thing as labour being essentialt o its existence; for where a sacrifice, such as labouring, is made, there esteem for, and value in the product, must exist.—If the products obtained by voluntary effort were not worth that effort in the opinion of the man obtaining them, the effort would not be made. Where therefore the voluntary effort is made, to obtain products, we may be assured of the existence of value; for value is the esteem in which products are held, after the sacrifice necessary for obtaining them is ascertained; and after a comparison has been made between the present sacrifice and the future gratification which is anticipated from the possession of the product in question.

III.—*On the Velocity of Sound, and Variation of Temperature and Pressure in the Atmosphere.* By John Herapath *Esqr.*

§ 1.—*Velocity of Sound.*

Having communicated the discovery of some theorems, relative to the velocity of sound, and the decrease of temperature and barometric pressure in ascending in the atmosphere, to several scientific friends, I have been prevailed on to give them to the public before the work of which they are intended to form a part.

It is pretty well known in the scientific world, that in pursuing Newton's hints of the cause of gravitation, I have been led to a theory of the nature and constitution of airs very different from that generally embraced. This theory, after unfolding to me the laws of an immense variety of phenomena, I was anxious to apply to solving the celebrated problems of sound and atmospheric temperature and pressure. No difficulty whatever occurred in developing the general laws ; but this was not enough ; if the theory to which I had arrived was right, I felt assured there must be some method of getting at the exact quantities of the phenomena, without drawing on experiments for more than indispensable elements. For instance, in estimating the velocity of sound, I conceived no just theory ought to require more from experiment than the elastic force and specific gravity of the air. The same elements only, I apprehended, ought to be sufficient for determining the exact rate of diminution in temperature and pressure at any elevation. For a long time my efforts were unsuccessful ; at last, however, a very simple idea, which I am surprised should have so long eluded my attempts to reduce the equations of comparison I had previously used to equations of equality, enabled me to solve the hitherto refractory problem of sound, and with it several of much more importance and utility.

What, probably, will appear not the least remarkable is, that this problem, which has obstinately resisted the abilities of Newton, Euler, Lagrange, Laplace, and other eminent mathematicians for 150 years, and the highest powers of analysis, should at last yield to a process scarcely requiring simple equations of Algebra ; and at the same time open solutions to other phenomena, with which I think, I may venture to say, no analyst ever expected it had the remotest connection. But the theorems will speak best for themselves.

Let, as usual, S E D g denote the velocity of sound, elasticity of the air, its density, and the velocity acquired by a falling body at the end of the first second. Then by the theory alluded to,

$$S = \sqrt{\frac{E g \sqrt{2}}{D}} \qquad (1)$$

and if S be the velocity of sound at any elevation x, and P p, the barometric pressures at the lower and higher stations ;

$$\frac{p}{P} = \left(1 - \frac{g x}{3\sqrt{S 2}}\right)^6 \qquad (2)$$

and

$$3\sqrt{2}\,(S^2 - s^2) = g x \qquad (3)$$

For, comparing these formulæ with observation, we have

$$D = \frac{E\, 488}{rh(F + 448)}$$

in which h is the altitude of the barometer at the temperature of water freezing ; $r = 10463$ by Biot, the ratio of the specific gravity of mercury to that of dry air, at 32° Fahr., and barometric pressure $h = ,76$ metres, the metre being 3,28085 English feet, and F the Fahr. temperature. Therefore, since $g = 32\frac{1}{6}$ feet,

$$S = 1089{,}41 \sqrt{\frac{F + 448}{480}} \text{ Eng. feet.} \qquad (4)$$

Now, from a mean of Captain Parry's experiments in the north, at —17°, 72 Fahr. it appears the velocity was 1035,2 feet per second, or, allowing for the difference in the value of g in that high latitude, probably about 1034 feet reduced to our latitude ; our theory gives 1031,5. The French academicians, in 1738, at about 42°,8 Fahr. found it 1103,5 feet ; our theory makes it 1101,6. Dr. Gregory, by the mean of his observations, determined the velocity to be 1107 feet at 48°,62 temp. ; by our theory it should be 1108,1. In 1821 Arago and his colleagues made the mean at 60°,62 to be 1118,43 ; our theory gives 1121,5, and from an article on sound, not yet published, with which the author has kindly favoured me, it appears Moll's late experiments, reduced to dry air at 32°, give 1089,4 feet, the same as our theory. In the same article, some of Goldingham's experiments at Madras, when reduced to 32°, it seems make 1089,9 ; the mean of his other experiments differing as much as 10 feet.

Collecting these observations together, we have,

Observers.	Temp. Fahr.	Observed in Eng. feet.	Computed in Eng. feet.	Diff. feet.
Parry,	—17°,72	1034,	1031,5	—2,5
Academicians,	+12 , 8	1103,5	1101,6	—1,9
Gregory,	48 ,62	1107,	1108,1	+1,1
Arago,	60 ,62	1118,4	1121,5	+3,1
Moll,	32	1089,4	1089,4	+0,0
Goldingham,	32	1089,9	1089,4	—0,5
			Mean difference,	+0,12

I have no more time to devote to this part of the communication, than just to notice the extreme minuteness of the mean difference of our theory, only $1\frac{1}{2}$ inch, compared with that of the old, amounting to 53 metres, or 174 feet. Laplace has, indeed, contrived to reduce this difference to 13 or 14 feet, by an ingenious, but very questionable, hypothetical assumption. The theory from which I have deduced the preceding formula requires no such assistance; nor I believe any thing beyond the simple definition of an air,

2.—*Diminution of Temperature.*

Let us now turn our attention to the other formulæ. Supposing f the Fahr. Temp. at the higher station, F. being that at the lower, and substituting of S. and s their values in (2) and (3), we get,

$$x = \left\{1-\left(\frac{p}{P}\right)^{\frac{1}{6}}\right\}\frac{rh\,(f+448)}{80} = 326{,}1264785\left\{1-\left(\frac{p}{P}\right)^{\frac{1}{6}}\right\}.$$

$$(f+448)^{1}\text{; and } F-f=\frac{80x}{rh}=\frac{x}{326\frac{1}{8}}\text{ nearly.} \qquad (5)$$

From the latter of these theorems it appears that the Fahr. Temp. decreases uniformly at the rate of 1° for every $326\frac{1}{8}$ feet. The difference may, therefore, be easily computed: "Take a $\frac{1}{100}$th of the altitude in yards, substract a $\frac{1}{10}$th of this from itself; and then add $\frac{2}{10}$ths of the part so substracted." Thus, if the altitude was 7600 yards:—

$$\begin{array}{r} 76 \\ -7{,}\ 6 \\ +1{,}52 \\ \hline 69°{,}92 \end{array}$$

And in centigrade degrees—"To a $\frac{1}{100}$th of the Alt. in fathoms, add twice a $\frac{1}{100}$th of itself, and then a $\frac{1}{10}$th of this correction." For example, in the preceding instance:—

$$\begin{array}{r} {,}38 \\ {,}76 \\ {,}076 \\ \hline 38°{,}836 \end{array}$$

Applying this rule to the cases extracted by Mr. Ivory from Ramond's collection of observations, we shall find it agree with the observations, much better than the observations probably by different individuals, at the same place, agree with one another, as the following table shews:

Places.	Heights yards.	Differences of Temp. observed.	Cent. computed.	Differences.	Mean Differences.
Lussac's Ascent,	7630	40°,3	39°,0		—1,3
Chimborazo,	6427	26 ,9	32 ,8		+5,9
Montblanc, Geneva,	4782	31 ,2	24 ,4	—6,8	—5,8
	4782	29 ,2	..	—4,8	
Pic de Teneriffe,	4077	16 ,5	20 ,8		+4,3
Montblanc, Chamouny,	4070	25 ,9	20 ,8	—5,1	—5,5
Ditto ditto,		26 ,6		—5,8	
Etna,	3540	18 ,7	18 ,1		—0,6

[1] These computations, and those that follow, are extracted from a letter in which the metre was reckoned to be 3,281 English feet.

Montperdu, Tarbes,	3408	18°,7	17,4		—1,3
Col du Giant, Geneva,	3346	20 ,4	17,1		—3,3
Maladette,	3174	17 ,4	16,2		—1,2
Pic du Midi, Tarbes,	2858	15 ,9	14,6	—1,3	+1,6
		11 ,0		+3,6	
		11 ,4		+0,2	
		13 ,1		+1,5	
		10 ,7		+3,9	
		15 ,1		+0,5	
		10 ,5		+4,1	
Cal du Giant, Chamouny,	2606	17 ,1	13,3		—3,8
Montperdu, Bareges,	2354	18 ,1	12,0		—6,1
Pic d'Eyre, Tarbes,	2347	10 ,3	11,9		+1,6
Pic de Montaign,	2244	11 ,4	11,4		0,0
Pic du Midi, Bareges,	1808	10 ,3	9,2	—1,1	—3,3
		13 ,9		—4,7	
		13 ,1		—3,3	
		12 ,5		—4,2	
		13 ,4		—1,6	
		10 ,8		—3,9	
		13 ,1		—4,0	
Puy de Dome, Clermont,	1163	6 ,9	5,9	—1,0	—2,1
		7 ,0		—1,1	
		6 ,9		—1,0	
		9 ,6		—3,7	
		9 ,4		—3,5	
Bedat du Bagneres, Tarbes,	611	2 ,9	3,1		+0,2
Pont du Berges, Clermont,	537	3 ,2	2,7		—0,5
La Barrague, ditto,	415	1 ,8	2,1		+0,3

How the altitudes here given were found, I have not read, but probably from barometric observations.

The mean error or difference from observations is 1°,1. This is a degree of coincidence from observations subject to so many causes of error as these are, could scarcely be expected. Were we to allow only 1° for the superior radiation and influence of bodies at the surface of the earth, even this trifling difference would disappear; or were the observations at the same places to be repeated at night instead of in the day, the time at which it is very likely they were made, it is highly probable the difference would be reversed, and be positive instead of negative. For my part, I have no doubt that the apparently more rapid diminution of temperature near the surface, than higher up, is owing to the observations having been made in the day time; and that the contrary would happen were they made in the night, especially if the weather was calm and clear. Dr. Wells's experiments, which in the night manifested an increase of temperature of sometimes 12 or 16 degrees, at the elevation of only a few feet, are a strong confirmation of this.

III.—*Barometric Depression.*

Unluckily I have not a single case by me of an elevation determined trigonometrically and barometrically, so that I am incapable of comparing the other formula with direct experiment. However, as Laplace's empirical formula is said to agree exceedingly with observations, its comparison with ours will afford a tolerably good indirect test.

For the ease of calculation, we may suppose the temperatures of the upper and lower barometer to be the same, and at 32° Fahr. or o cent. with these conditions Laplace's formula (Playfair's Outlines, vol. i. p. 240,) is in Eng. fathoms.

$$x = 10050 \left(1 - \frac{2\,C}{1000}\right) \log. \frac{P}{p}$$

C being the negative cent. temp. of the higher station. And our theorem in fathoms and logs. is,

$$x = \left\{1 - \left(\frac{p}{P}\right)^{\frac{1}{6}}\right\} rh\text{; or log. } x = \text{Log.} \left\{1 - \left(\frac{p}{P}\right)^{\frac{1}{6}}\right\} + 4{,}4164760$$

Now Laplace's formula affording us no assistance in determining the value of C, we have no resource but to compute it from that theorem which we have shewn to agree so well with observation. Assuming therefore, $\frac{p}{p} = \frac{28}{30} = \frac{14}{15}$, our formula gives $x = 298.29$ fathoms, from which C = 3°, and consequently by Laplace x=2993. or 1 fathom above ours. Putting $\frac{p}{P} = \frac{25}{30} = \frac{5}{6}$, ours gives 78.83, and C = 8°, and hence, Laplace's 782.47, or 1.59 above ours. Again, when $\frac{p}{P} = \frac{20}{30} = \frac{2}{3}$, we have from our theorem 1704.8 fathoms, and C = 17°,42, and from Laplace's 1708, 1, or only 3, 3 more, in an altitude of nearly two miles. In Gay Lussac's great ascent, the temp. sunk from 30°,8 Cent. to — 9°,5; the barometer from 1000 to 432; the density of the air from 1 to $\frac{1}{2}$; and we are informed the height ascended, doubtless determined from these data by Laplace's formula, was 7630 yards, or 3815 fathoms from the barometric condition: our formula gives 7600 yards, or 3800 fathoms, that is, 15 fathoms less in a height of $4\frac{1}{3}$ miles. The depression of temp. as we have seen in the table, differs likewise only about $1\frac{1}{2}$°.

It should here be observed, that if Laplace's formula coincided perfectly with observations; and the greater the height the more it must diverge from nature; for by that formula, the atmosphere must be infinite in extent, a palpable absurdity, which Laplace himself acknowledges. However, the differences which we have shewn to exist between the two formula, are much within the limits of error, to which probably the best observations could pretend in such heights.

There is a source of error in barometric admeasurements, which it will be difficult for any theory to estimate or avoid, namely, the unequal distribution of vapour in the atmosphere. This will, in general, tend to depress the lower barometer too much, and consequently to give the altitudes too little. It is probable this may never occasion an error of serious moment, but it will undoubtedly always have some influence.

If we suppose D d to denote the densities of the air corresponding to P p, the combination of our two theorems gives

$$\frac{d}{D} = \left(\frac{p}{P}\right)^{\frac{5}{6}} \qquad (6)$$

or in Lussac's $\frac{d}{D} = (,432)^{\frac{5}{6}} = \frac{1}{2,0126}$ differing from his $\frac{1}{2}$ by only $\frac{1}{315}$th part.

From these instances some idea may be formed of the perfect fidelity with which the theorems I have given represent phenomena; probably it will not be hazarding too much to affirm, that the success of them is greater than could have been anticipated; and that there is scarcely a parallel instance in science, in which investigations begun, and conducted so absolutely independent of experimental aid, have been so thoroughly confirmed by phenomena. Their mathematical analysis will appear in the work already alluded to.

Several important consequences flow from these theorems, besides those we have mentioned; some of which we shall here notice.

1st. The velocities of sound, and the transmission of heat by the air, are the same.

2d. The total altitude of the air is equal to——

$$\frac{rh\,(F+448)}{80}$$

or, at a medium, better than 30 miles; and at other times varies directly as the Fahr. temp. + 448.

3rd. Since r and h are estimated at a common temperature, when air is constant, the other must be constant too in the same air; and therefore the quantity of air has nothing to do with its total altitude. This would be the same, whether there was a half, a third, or a 100 times the quantity.

4th. Other things being alike, the altitude of an atmosphere is reciprocally proportional to the attraction of the body it surrounds at its surface, and the specific gravity of the air under a given pressure and temperature conjointly. If, therefore, our globe was surrounded with hydrogen, its altitude would be about $14\frac{2}{5}$th. times higher than our atmosphere is.

Hence a means of determining the altitude of the atmospheres of any of the celestial bodies; and reciprocally, having the altitudes and the nature of the airs, their attractive forces. And hence, too, a proof of the small attractive forces of comets, which have been found by other methods, with a means of computing them, at least approximatively.

5th. By (1) reduced to (4) it appears that the velocity of sound, at the surface, is independent of the pressure of the atmosphere; and by (2), that the pressure in the higher regions is dependent on this very velocity, and varies with it, being greater or less as this is greater or less; this apparent paradox is eaily explained: for at the surface, the pressure results from the total quantity of air, but at a given altitude, from the total quantity, minus that below, which depends on the temperature at the surface, and thence on the velocity of sound.

6th. Our barometric formula (2) requires no aid from temperatures of the external air. It includes all that is needful within itself, and merely requires that the barometers be of one, or reduced to one, temperature. Even this it might do without. But as I have elsewhere remarked, Laplace's formula in this respect is singularly helpless; it not merely affords no means of finding the difference of temperatures, but cannot do without it.

7th. By the help of the formula here given, the time sound takes to travel over any given space, oblique as well as horizontal, a problem, I believe, that has never been attempted, may easily be determined. For instance, if *a* be the altitude of the generation of sound, *b* that of the auditor, and *q* the distance between the two, the time in seconds is

$$\frac{6\sqrt{2.q}}{g\,(b-a)}\left\{\sqrt{S^2-\frac{g\,a}{3\sqrt{2}}}-\sqrt{S^2-\frac{g\,b}{3\sqrt{2}}}\right\}; \qquad (7)$$

and the time of travelling vertically from the top to bottom of the atmosphere, or the contrary. $\frac{6\sqrt{2}}{g}S$ in which S is the horizontal velocity at the surface. If, therefore, $S = 1089,4$ as we have computed it at 32° Fahr., this time is $4^m 47^s 4$.

I might here observe, by way of conclusion, that should any one feel disposed to compute a table from our barometric theorem for the more easy measuring of heights, it would be advisable to do it for 52° Fahr. The altitude being taken for this temperature, and multiplied by twice the number of degrees which the temperature of the lower barometer may be above or beneath 53°, $\frac{1}{1000}$th of the product will be the only correction required, and is to be added or substracted to the preceding altitude, just as the lower barometer's temperature happens to exceed or fall short of 52° Fahr.

For the centigrade thermometer, the table had better be computed for o temp. or the freezing point.

IV.—*On the Measure of Temperature, and the Communication of Heat.* *By* M. M. Dulong *and* Petit.

PART II.

ON THE LAWS OF COOLING.

The first received views on the communication of heat are to be found in the works of Newton. [1] This great philosopher assumes, *a priori*, that a heated body exposed to a constant cooling process, such as that of a uniform current of air, would lose, in each moment of time, a quantity of heat proportional to the excess of its temperature, over that of the cooling medium; and that consequently these losses of heat, in equal and successive portions of time, would form a decreasing geometrical progression. Kraft, and after him Richmann [2] have endeavoured to verify this law

[1] *Newton Opuscula*, t. ii. p. 423.
[2] Nov. Com. Ac. Petrop. t. i. p. 195.

by direct experiment on the cooling of masses of liquids. These experiments, subsequently repeated by other enquirers, prove, in fact, that for differences of temperature, which do not exceed 40 or 50 degrees, the law of a geometrical progression represents pretty accurately the rate of cooling of any body.

Martin [3] had in a dissertation but little known, but published in 1740, being several years before Kraft and Richmann had made known the result of their researches, shown the incorrectness of the preceding law, and had endeavoured to investigate one which should give a more rapidly converging series of decrements.

Erxleben [4] had also proved, by very accurate experiments, that the errors of the hypothetical law increased with the difference of temperature; and deduced the conclusion, that it could not be safely applied to differences of temperature, much exceeding those at which it had been verified. This well founded remark of Erxleben does not appear to have been much attended to by philosophers; for in every subsequent enquiry the law of Newton is always assumed, not as an approximation, but as a real and demonstrated truth.

In this way Mr. Leslie, [5] in his ingenious researches on heat, has founded several of his determinations on this law, which are, for that reason alone, objectionable, as we shall prove in the sequel.

Some time after the publication of Mr. Leslie's work, M. Dalton made known, in his *Treatise on Chemical Philosophy*, a series of experiments made on the cooling of bodies heated to very high temperatures. The result of these experiments went to show conclusively, that Richmann's law is only approximately true in low temperatures, and becomes very inaccurate, as the temperature increases. Mr. Dalton, instead of endeavouring to investigate a law that should represent his own observations, endeavours to reconcile them with that of Richmann, by substituting for the ordinary thermometric scale, that which he had founded on the considerations we have discussed in the first part of this memoir. But even admitting the soundness of the principles on which he has founded this new scale, it would still be not the less true, that the decrements of temperature are *not* proportional to the excess of the temperature of the body over that of the circumambient air, or in other words, that the establishment of his scale does not necessarily infer the law of Richmann; for in this case it would follow, that the law of cooling must be the same for every body, whereas our experiments have clearly proved the contrary to be the case.

The most recent labours on this subject are those of Laroche, inserted in his treatise—*On Radiating Heat*; amongst other conclusions, he establishes the following:—that the heat which a body yields in a given time, through the means of radiation to a colder body, situated at a distance, follows (all else being equal) a progression more rapid than that which represents the excess of temperature of the one body over those of the other.

This proposition, it may be seen, is much the same as Mr. Dalton's, substituting merely the heat lost by radiation, for the total heat lost by a body immersed in air. Laroche, however, only gives particular results, and does not attempt the investigation of the law on which they depend. We shall see in what follows, that these results include the effects of some particular causes which tend to render the enquiry more complicated; and that it is necessary to separate these last to obtain even the law of cooling in a vacuum, which, it is to be noted again, is not the same as the law of radiation.

The labours then of philosophers have, as far as regards the laws of cooling, been hitherto confined to proving that Newton's law is sufficiently exact, when the question only concerns small excesses of temperature, but that it becomes more erroneous as the differences of temperature increase. If in this cursory view of what has been done in this enquiry, we have omitted to mention the mathematical investigations on the distribution of heat by M. Fourier, it is because his enquiry proceeds upon the basis of Newton's law, assumed as an undoubted truth, whereas our experiments have had for their object, to determine what is the true law which should be substituted for it; however, the very remarkable conclusions to which this profound geometer has been led, will preserve all their value in those cases in which the law of Newton is found sufficiently exact, and to extend them to others it will be sufficient to modify them conformably with the new laws which we are now about to establish.

[3] Dissertation sur la Chaleur, p. 72. et. seq.
[4] Novi Comment. Soc. Gotting. t viii. p. 74.
[5] An Inquiry into the Nature of Heat, p. 265.

§ 1. *On the Cooling Process in General.*

It is known, that when a body is exposed to the cooling process in a vacuum, its excess of heat is dissipated entirely by means of what is called radiation, and that when immersed in any medium gaseous or liquid, the cooling process is more rapid, in as much as the heat lost to the fluid is to be added to that dissipated by radiation. It is, therefore, necessary to distinguish between these two effects, and as they are, to all appearance, the results of two different laws, it is further necessary to study them separately. This is what we propose to do, treating of the cooling process first, in a vacuum ; and secondly, in an elastic medium. But as our method of proceeding is, in each case, founded on the same principles, it will be proper first to establish these.

To obtain *the elementary law* of cooling, that is to say, the law which would be observed by a body, the dimensions of which should be so small, that we might safely assume all its parts to have at each instant the same temperature, it would be needlessly complicating the question, if not indeed rendering it altogether insoluable, to attempt the observation of the phenomena in a solid body ; since in this case an additional element would be introduced, viz. the internal distribution of heat, which is of course a function of the conductibility. Being thus confined to liquids by the very nature of the problem, the mercurial thermometer itself appeared the best suited to such experiments. But as it was necessary, in extending our enquiries to very high temperatures, to employ a volume sufficiently large to prevent the cooling process acquiring a rapidity that should render it difficult to follow it with accuracy, it became essential, first, to determine what effect would be produced on the law of cooling, by the employment of a greater or lesser mass of liquid in the ball of the thermometer. Nor was it of less importance to examine whether this law depends at all upon the nature of the fluid, or on the nature, or even shape, of the vessel in which it is contained ; this preliminary enquiry has been the subject of a series of experiments, of which we shall now proceed to give some account ; first, however, explaining the method of calculation used in reducing our observations to a common term of comparison.

Suppose we have observed at certain intervals of time, equal amongst themselves, (every minute for instance,) the excess of temperature of any body over that of the surrounding medium, and that for the intervals

m	m	m	m	m
0	1	2	3	&c. to *t*

these excesses have been found to be

A	*B*	*C*	*D*	&c. to *T* :

if the law of a geometrical progression were the true one, we should have

$$B = Am\,; \quad C = Am^2 \text{ \&c. to } T = Amt$$

m being a fraction which would be different for different bodies. This expression is not rigorously true, especially if the temperatures are high—but we may conceive the possibility of representing a certain number of terms belonging to the preceding series by an expression of the form $Am^{t} + \beta\, t^2$ determining properly the coefficients m and β. And we may, by the aid of this formula, calculate, with a considerable degree of approximation, the value of the time t corresponding to any excess of temperature T, provided this excess be included in the series which has served for interpolation.

This same expression affords us the means of determining the quickness of the cooling process, corresponding to each excess of temperature, i. e. the number of degrees by which the temperature of the body would be lowered in a minute, supposing the rate of cooling to be uniform during that minute. The expression of this rate will be,

$$\frac{dT}{dt} = T\ (\alpha + 2\beta\, t)\ \log.\ m.$$

this quantity ought, of course, always to exceed the actual loss of heat in that interval, because the rate diminishes continually, and this, however minute the interval is assumed.

Our readers may well suppose, that it is not with any idea of correcting for the minute error just noticed, that we have adopted the course described, but rather with the view, that when a series is thus divided into several parts, each of which is represented by empirical formulæ that answer exactly to the numbers given by observation—the rate of cooling deduced from these formulæ, for different excesses of temperature, is freed from those uncertainties and irregularities which are often found to accompany single results.

Let us now return to the first comparison, of which we have spoken above, and let us enquire how the rate of cooling has varied in the three different series, the calculated results of which are given in the accompanying table.

Excess of Temperature.	Rate of Cooling of Thermometer A. Diam.=2 centim.	Rate of Cooling of Thermometer B. Diam.=4 centim.	Rate of Cooling of Thermometer C. Diam.=7 centim.
100°	18°,99	8,97	5,00
80	14 ,00	6,60	3,67
60	9 ,58	4,56	2,52
40	5 ,93	2,80	1,56
20	2 ,75	1,30	0,73

The first column contains the excess of temperature of the thermometers over that of the surrounding medium ; in the next will be found the corresponding rates of cooling of the thermometer A, the bulb of which is about 2 centimetres (,788 inch) in diameter. These rates have been calculated, according to the method just explained, from actual observation. The third and fourth columns contain the rates of the thermometers B & C, calculated in the same way, and for the excess of temperature in the first Column. The bulb of the thermometer B is nearly 4 centimetres (1,576 inches) in diameter ; that of the thermometer C 7 centimetres (2,758 inches).

A mere inspection of this table will be sufficient to show the inaccuracy of Richman's law ; for we see at once, that the rate of cooling increases faster than the excess of temperature. Now, if we enquire what is the ratio of the corresponding numbers in the second and third columns, we shall find that they are as follow,—beginning with those which correspond with the greatest excess of temperature.

2,11 2,12 2,10 2,12 2,11

These numbers, which differ very little among themselves, and the differences of which appear to be accidental, enable us to infer, that the rate of cooling follows the same law in the two thermometers A & B. In the same way, comparing the numbers in the second and fourth columns, we shall find as their ratios,

3,78 3,81 3,80 3,80 3,77

These numbers being also nearly the same, we see that the law of cooling is still the same for the thermometers A & C ; for the differences presented by the preceding numbers ought to be attributed to the errors inseparable from the most careful experiments, and indeed amount to little more than the hundredth part of a degree on the rates. We are, therefore, justified in concluding, from all that precedes, that the law of cooling, for a mercurial thermometer, has no reference to the size of the bulb, and that it is consequently the elementary law we are seeking ; in other words, the law which the cooling of a material point would follow.

We have not examined the question, how far the rate of cooling would be affected by difference of surface, on account of the difficulty of measuring precisely the surface of a bulb of glass, blown at the extremity of a tube, as well as because this inquiry was distinct from the one we had undertaken. We may however see, even in the approximate measures given of the bulbs, that the rapidity of cooling is nearly in the ratio which would hold with spheres indefinitely small ; that is to say, in the inverse ratio of their diameters.

Let us now attend to the influence which the nature of the liquid of which the thermometer might be constructed would have on the law of cooling. On account of the difficulty of constructing thermometers of any other substance than mercury, a difficulty occasioned by the uncertainty which still envelopes the law of expansion in all other bodies, we determined to observe the cooling of these liquids by enclosing them in a glass matras, in which should be immersed a mercurial thermometer, of great sensibility. We satisfied ourselves, that the position of the thermometer was a matter of indifference, and that at any particular instant the temperature of every part of the mass would be the same; which indeed is a necessary consequence of that internal conduction, the result of the currents formed in liquids, and which is therefore nearly perfect, at least for masses such as those employed in our experiments.

The first of the following tables contains the rates of cooling of mercury and water; the second gives the comparison between mercury and absolute Alcohol; the third between mercury and concentrated sulphuric Acid.

Excess of Temperature.	Rate of Cooling with Mercury.	Rate of Cooling with Water.	Ratio of these Rates.
60°	3,03	1,39	0,458
50	2,47	1,13	0,452
40	1,89	0,85	0,450
30	1,36	0,62	0,456

Excess of Temperature.	Rate of Cooling with Mercury.	Rate of Cooling with Alcohol.	Ratio of these Rates.
40	1,89	1,50	0,798
30	1,36	1,09	0,801
20	0,87	0,69	0,794

Excess of Temperature.	Rate of Cooling with Mercury.	Rate of Cooling with Sulphuric Acid.	Ratio of these Rates.
60	3,03	1,97	0.650
50	2,47	1,59	0,649
40	1,89	1,22	0,646
30	1,36	0,89	0,654

The ratios in the last column of each of these tables show, that the law of cooling is the same for each of these four liquids; the small variations observable, appearing evidently to be the effect of uncertainty of observation; for they might be made to disappear, by altering the rates of cooling only one hundredth of a degree.

Now if liquids so different in their nature, their density, their fluidity, present such perfect similarity in the laws of cooling, we may generalise this result, and say, that a liquid mass, such as we have used, whatever may be its nature, must cool, agreeably to the elementary law we are investigating.

There still remained the shape of the vessel to be examined. We first compared the cooling of two spheres, one of glass, the other of tinned iron, both filled with water. The radius of the latter exceeded, by a small quantity, that of the former,

Excess of Temperature.	Rate of Cooling of the Glass Sphere.	Rate of Cooling of the Tinned Sphere.	Ratio of these Rates.
60	1,39	0,90	1,54
50	1,13	0,73	1,55
40	0,85	0,54	1,57
30	0,62	0,38	1,63
20	0,37	0,21	1,76

Here the ratios inserted in the last column vary always in the same direction, and show that the law of cooling is more rapid for the vessel of tinned plate than for that of glass. Mr. Leslie has arrived at the same conclusion, which he has generalized, by admitting that the law changes with the nature of the body, and that it is more rapid as these bodies radiate less. This proposition is true only in that part of the thermometric scale which Mr. Leslie has confined himself to in his experiments; but by a very remarkable circumstance, a contrary effect is produced in the higher temperatures. So that when we compare the laws of cooling of two bodies, having different surfaces, that law which is the more rapid in the lower part of the scale becomes, on the contrary, the less rapid in the upper part. Thus, in the series above given, the ratios inscribed in the last column diminish, as we consider greater excess of temperature; they would increase again, if the series were continued sufficiently far; and agreeably to the property common to all quantities, the differences of which change their sign, these ratios would continue much the same throughout a sufficiently extensive part of the thermometric scale. This is one of the most important results of the theory of cooling. If we have not deceived ourselves, as to the correctness of the laws we have arrived at, we shall be able to give, in the sequel of this memoir, a very simple explanation of this remarkable fact, which could only have been brought to light, in observing, as we have done, the progress of cooling at very high temperatures.

It is from the want of such experiments that Messrs. Dalton and Leslie have arrived at conclusions so much at variance with truth, on the subject of which we are treating. The first misled by the idea, that the law of Richman was true within the range of his thermometric scale, and having, at the same time, neglected to compare the cooling of different substances within a sufficient range, adopted the conclusion, that the law of cooling for all bodies was the same. Mr. Leslie again, who had remarked, that the law changes with the nature of the surface, but not having included in his experiments sufficiently elevated temperatures, supposed that the difference observed would increase as the temperature increased, which error has involved him in many other inaccurate conclusions, to which we shall have hereafter occasion to refer. We will here content ourselves with one remark, which is, that we are surprised to find Mr. Leslie, who had observed the difference in the law of cooling, occasioned by the different natures of bodies, and had thence concluded rightly, that the law of Richman could not be true, employing notwithstanding that very law pretty generally, in calculating the results of his experiments.

We have concluded these preliminary researches, by observing the cooling of water in three vessels of tinned plate, having the same capacity; the first spherical; the second cylindrical, the height being double the diameter of the base; the third cylindrical also, the height being only half the diameter of the base.

Excess of Temperature.	Rate of Cooling of the sphere.	Rate of Cooling of the Cylinder, No.1.	Rate of Cooling of the Cylinder, No. 2.	Ratio of the 3d column to the 2d.	Ratio of the 4th column to the 2d.
60	0,90	1,11	1,01	1,23	1,12
50	0,73	0,89	0,80	1,22	1,10
40	0,54	0,66	0,60	1,22	1,11
30	0,38	0,47	0,43	1,23	1,13
20	0,21	0,26	0,23	1,24	1,10

The law of cooling is then the same for the three vessels of different shapes, as indicated by the uniformity of value of the ratios in the fourth and fifth columns. The shape of the vessel has, therefore, no sensible influence on the law of cooling; and what confirms this conclusion, is that the ratios between the above rates of cooling, are pretty nearly those of the surfaces of the three vessels, as any one may easily satisfy himself. We may recapitulate the results just established, by saying that the law of cooling of any liquid mass depends upon the nature and condition of the surface of the vesssel in which it is contained, but is altogether independent of the nature of the liquid, and of the shape and size of the vessel. This is the principle we proposed to establish in this introduction, and which must serve as a foundation to the researches we are now about to give an account of.

§ 2.—*Apparatus for the experiments on cooling.*

The bodies, the cooling of which we have observed, have been conformably to the principles just established; thermometers having a volume such, that the depression of temperature could be easily observed with precision. We constructed two, the bulbs of which were about, the one, 6 centimetres in diameter, the other 2; the first, containing about three pounds of mercury, was used in observations on high temperatures; the smaller one being employed for the lower temperatures, in order to shorten the period of the experiments. The results obtained with the latter, were easily reduced to what the other would have given, had the experiment been sufficiently prolonged; to do this, it was sufficient to commence observing with the small one at a higher temperature, than that at which the observations with the large one were to terminate. The ratio of the rates of cooling being determined for a common excess of temperature, gave the number by which the results of the smaller thermometer should be multiplied, to obtain the corresponding rates of the larger.

These two instruments, which were constructed with the utmost possible care, differed from ordinary thermometers only in this, that the tube on which the degrees were marked, was separated from the bulb by an intermediate tube, the calibre of which was very small. The reason of this construction will be presently seen.

The experiments on cooling in a vacuum being those with which we were to begin, required that the thermometer should be quickly disposable in a space sufficiently roomy, in which a vacuum might be produced without delay. It was further desirable, that the case surrounding the thermometer on all sides should be kept at a fixed temperature ; and as the same apparatus was to be used in observing the progress of cooling in air, or any of the gases, it was indispensable that there should be means of readily introducing any of these. These conditions will all be found fulfilled in the following arrangement.

The surrounding case or envelope within which the process of cooling goes on, is a large balloon, formed of very thin sheet copper. (Pl. V. fig. 1. M M′ M″ M‴) The diameter being about 3 decimetres, (11,82 inches,) the projecting neck of this balloon is ground on its upper surface, so as to be an exact plane, which is adjusted to a horizontal position, by means of a level. The balloon is immersed within a short distance of the edge of this neck, in a cylindrical tub of water, and is retained in an invariable position, by means of strong supports R R′ R R′. It will be seen, that the sides of the balloon being extremely thin, and at the same time, excellent conductors, must always be of the same temperature with the water surrounding them ; and being covered inside with lamp black, they reflect exceedingly little of the heat they receive from the thermometer. And as this error, such as it is, is nearly as the loss of heat of the body, it affects proportionally every result. To raise the temperature of the water, it was only necessary to introduce steam into the tub, by means of the curved tube S U V, which descends to the bottom.

The mouth of the balloon is closed, by means of a thick plate of glass, AB, ground with the greatest care, to fit the surface of the neck ; the surfaces in contact have sufficient extent, on account of the thickness of the neck, to allow of the application of a little grease, which renders the connection perfect, and cuts off all communication with the atmosphere.

This flat plate has a circular hole in the centre, in which is fitted tightly a cork, through which passes the stem of the thermometer ; the graduation commences immediately above the cork ; and the connecting tube is of sufficient length to allow the bulb to occupy the centre of the balloon. By making this tube of a very small diameter, the quantity of mercury external to the bulb is diminished ; no current can be established, and the enlargement at the commencement of the scale, allows the insertion of the stem, into the cork, to be made more firmly. The arrangement of the flat plate and thermometer is represented, fig. 2, in which the bulb of the instrument is placed above the furnace, by which it is heated. The screens AA′ are plates of tinned iron, separated from each other, which serve to defend the glass plate AB from the action of the fire.

To return to figure 1. The stem of the thermometer, which is outside the balloon, it is seen, is covered by a hollow tube, the ground edges of which fit exactly to the upper surface of the glass plate. This outer case is terminated above, by a stop cock D, which connects it with a very flexible leaden pipe DEF ; the other extremity of which F is itself connected to the plate HK of an air pump. The tube connecting this plate with the barometer, has another stop-cock T, terminated by a neck, to which is attached a tube full of muriate of lime. The gas contained in the reservoir V escapes through this tube, after passing through the curved one, *m n p r s*. The reservoir being moveable, it is easy to establish an equilibrium between the elasticity of the gas, and the pressure of the atmosphere.

The following was the order of proceeding in each experiment. The water in the tub being brought to the proper temperature, and the thermometer attached to the plate of glass being heated nearly to the boiling point of mercury, it was quickly fixed in the balloon ; the case CT, which had been previously screwed to the leaden tube, was then brought down on the plate ; and while one person was employed bringing the surfaces into contact, an assistant was rapidly working the air pump, for the purpose of making a vacuum. The communication between the balloon and the outer case was effected by means of two holes made in the glass disk, near the central opening.

If the rate of cooling was to be observed in a vacuum, we stopped when the air pump had reached its limit of exhaustion, and the residuum of air in the balloon

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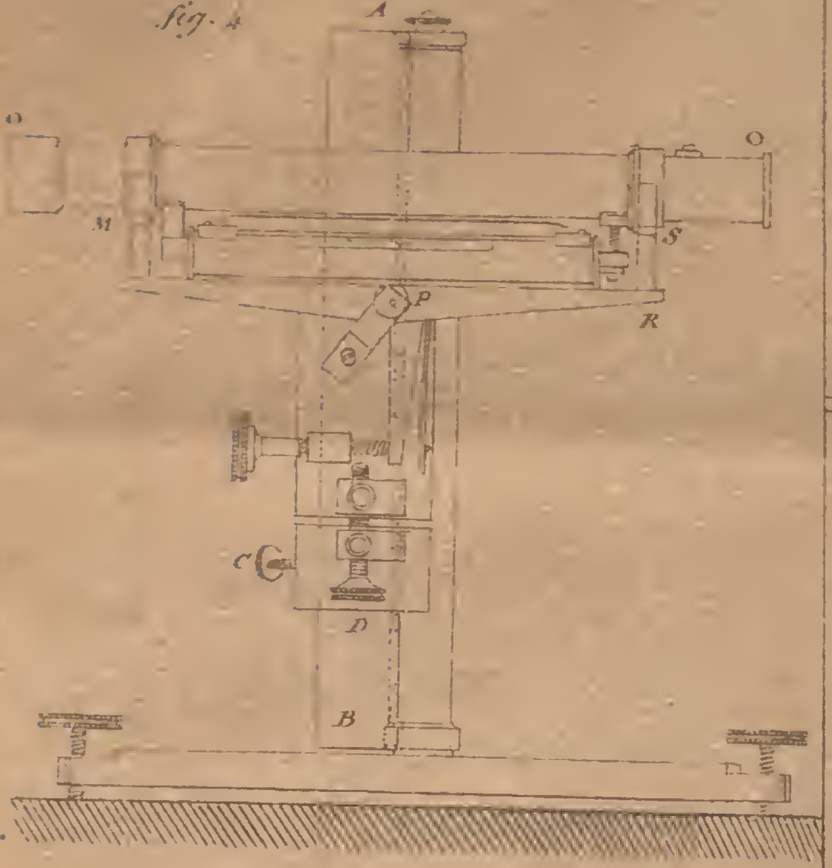

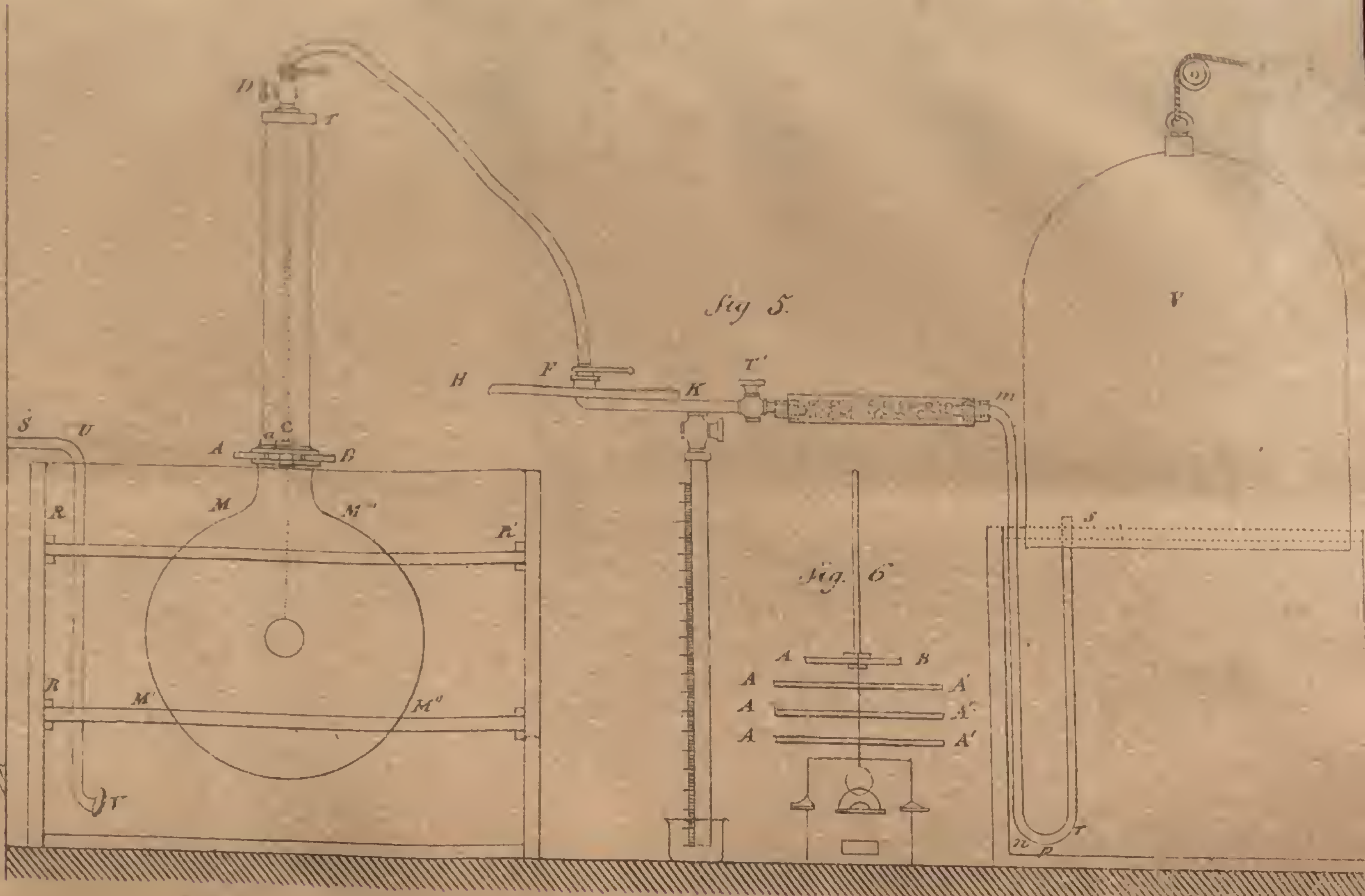

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was immediately measured by the gauge. The stop-cock of the outer tube or case was then shut, and we commenced our observations. When the experiment was to be performed in air, the contents of the balloon were first rarefied, in order to render the junctions more perfect, and the quantity of air which had been abstracted was then re-admitted. Finally, when the experiment was to be performed in a gas, the balloon was first exhausted, a certain quantity of the gas admitted, and the exhaustion made a second time: the full proportion of gas was then admitted, which was thus found contaminated with the smallest possible quantity of air.

We shall terminate this description with observing, that the dimensions of the thermometer had been adjusted, so that the observation of its rate of cooling might commence at 300° (572° F.) in a vacuum. The experiments in air and other gases demanding a little more management, and requiring the fluid to possess an equilibrium of temperature throughout, could not be commenced much above 250°, (482° F.)

Every thing being arranged, as we have described, whether for observations in a vacuum, or in a gaseous medium, there only remained to observe, by means of a watch with seconds-hand, the indications of the thermometer at equal intervals of time; these indications, however, requiring two corrections, which we shall explain. First, it will be observed, that in the arrangement of our apparatus, the stem of the thermometer became always, in a few moments, of the temperature of the air; each indication of the instrument was then too small, by a number of degrees equal to the quantity by which the mercury in the stem would have expanded, had it been raised from the temperature of the air to that of the bulb. This correction there was of course no difficulty in calculating, and we were careful to apply it to each observation. The second correction had for its object, to reduce the indications of the mercurial thermometer to that of air; and for this purpose, we made use of the table given in the first part of this memoir.

When we had, in this way, obtained the correct series of temperatures, we applied the formula of calculation, which we have above explained. The series was then divided into several different parts, each of which was represented by expressions of the form $m\ a\ \alpha^{t} + \beta^{t^2}$, in which t designates the time. These served afterwards to calculate the rate of cooling for different excesses of temperature, which rates, however, required a little diminution easily determinable in each case. To understand in what this correction consists, it may be sufficient to remark, that the cooling of the bulb of the thermometer, proceeding from the loss of heat throughout its surface, is always a little augmented by the descent of the cold mercury from the stem of the instrument. Now the volume of this mercury being known, and also its temperature, this last correction was easily calculated, which, though very small, was not to be neglected.

Such were the methods of observation and calculation followed throughout in all our experiments. We contented ourselves with determining the rates of cooling for each 20 degrees, and fearing to swell this paper unnecessarily, we have omitted all the intermediate steps of calculation by which the results have been established.

We shall now proceed to the detail of our experiments, in the order in which they have been made.

Our preliminary researches having taught us the influence exerted on the law of cooling, by the nature of the surface, it became necessary to study this law for different kinds of surfaces; and it was further necessary that these surfaces should be such, as not to be affected by the heat to which they should be exposed. Those which alone appeared to fulfil this condition, are glass and silver. We have, therefore, made all our experiments on a thermometer, first, with its natural surface, and secondly, having the bulb covered with very thin silver leaf. These two surfaces, it is known, have very different radiating powers; glass being the most powerfully radiating of all bodies, and silver the least so. The laws to which we have been led, in comparing the rates of cooling, as due to these two surfaces, are so simple, as to be beyond all doubt general, in their application to all bodies.

V.—*On the Climate of the North-Western Mountains.*

1. *A general Statement of the Weather at Subathoo, for May*, 1829.

Clear,	16 days.
Fair, but cloudy, and partially cloudy,	9 ditto.

Rainy, stormy, and hail,	6 days.
Thunder,	7 ditto.

Clear, on the 2d, 5th, 6th, 7th, 8th, 10th, 11th, 12th, 13th, 17th, 18th, 21st, 22d, 23d, 24th, and 30th.

Fair, but cloudy, and partially cloudy, on the 3d, 4th, 9th, 20th, 25th, 26th, 27th, 28th, and 29th.

Rainy, stormy, and hail, on the 1st, 14th, 15th, 16th, 19th, and 31st.

Thunder, on the 4th, 14th, 15th, 16th, 19th, 28th, and 31st.

Height of the Barometer.

	Inches	Ther.
Mean maximum for the month,	25,834	85,1
Mean minimum,	25,750	77,7
Mean of the daily means,	25,791	81,4
Greatest altitude on the 1st, at 9 P. M.	25,965	81,
Least ditto on the 29th and 30th, on the former day at 5 P. M. and on the latter at sun-rise, 5 P. M. and sun-set,	25,635	83, on both days.

Temperature of the Air.

Mean maximum,	88,7
Mean minimum,	69,8
Mean of the daily means,	79,2
Greatest on the 29th, at 3 P. M.	94,3
Least, on the 16th, at 9 P. M,	57,5
Mean,	75,9

Temperature of the House.

Mean maximum,	85,1
Mean minimum,	77,7
Mean of the daily means,	81,4
Greatest on the 30th and 31st, on the former day at 5 P. M. and on the latter at 3 P. M.	90,3
Least, on the 17th, at sun-rise,	69,
Mean,	79,6

Hygrometrical State of the Air.

Kater's Hygrometer, greatest altitude on the 16th, at sun-set,	703
Ditto ditto, least ditto, on the 11th, at 4, 45 P. M. and sun-set,	50

Statement of the Winds, shewing their direction and force, during May, 1829.

South-west, on the 1st, 3d, 4th, 5th, 6th, 9th, 10th, 11th, 12th, 13th, 14th, 15th, 16th, 20th, 23d, 24th, 25th, 26th, 27th, 29th, 30th, and 31st,	moderate,	22 days.
Ditto, on the 2d, 7th, 8th, 17th, 21st, and 22d,	gentle,	6 ditto.
Ditto, on the 18th,	little,	1 day.
Ditto, on the 28th,	strong,	1 ditto.
North-east, on the 19th,	gentle,	1 ditto.

2. *A General Statement of the Weather at Subathoo, for June,* 1829.

Clear,	10 days.
Fair, but cloudy, and partially cloudy,	5 ditto.
Rainy and stormy,	15 ditto.
Thunder,	13 ditto.

Clear, on the 2d, 3d, 4th, 5th, 6th, 10th, 12th, 13th, 14th, and 18th.

Fair, but cloudy, and partially cloudy, on the 1st, 19th, 26th, 27th and 30th.

Rainy and stormy, on the 7th, 8th, 9th, 11th, 15th, 16th, 17th, 20th, 21st, 22d, 23d, 24th, 25th, 28th, and 29th.

Thunder, on the 1st, 3d, 7th, 8th, 9th, 15th, 17th, 21st, 22d, 23d, 24th, 28th, and 29th.

Height of the Barometer.

	Inches	Ther.
Mean maximum, for the month,	25,690	83,6.
Mean minimum,	25,613	78,3.
Mean of the daily means,	25,651	80,9.
Greatest altitude, on the 9th, at noon,	25,875	78,5.
Least ditto, on the 4th and 6th, on both days, at sun-rise,	25,490	82,3 and 81,7

Temperature of the Air.

Mean maximum,	84,4
Mean minimum,	72,
Mean of the daily means,	78,2
Greatest, on the 2d, at 5 P. M.	95,8

Temperature of the House.

Mean maximum,	83,6.
Mean minimum,	78,3.
Mean of the daily means,	80,9.
Greatest, on the 4th, at 4 P. M.	91,6.

Least, on the 8th, at 8 A. M.	61,	Least, on the 8th, at 8 A. M.	72,3.
Mean,	78,4	Mean,	81,9.

Hygrometrical State of the Air.

Kater's Hygrometer, greatest altitude, on the 29th, at 8 A. M.	805
Ditto ditto, least ditto, on the 2d, at sun-set,	66

Statement of the Winds, shewing their direction and force, during June, 1829.

South-west, on the 1st, 2d, 5th, 6th, 7th, 13th, 14th, 15th, 18th, 19th, and 27th,	moderate,	11 days.
Ditto, on the 3d and 4th,	brisk,	2 ditto.
Ditto, on the 10th, 20th, 21st, 25th, 26th, and 30th,	gentle,	6 ditto.
North-east, on the 8th, 9th, 22d, 23d, 24th, 28th, and 29th,	little,	7 ditto.
Ditto, on the 16th,	gentle,	1 day.
North-west, on the 11th,	ditto,	1 ditto.
West, on the 12th,	ditto,	1 ditto.
South, on the 17th,	little,	1 ditto.

Remarks.

The rainy season commenced on the 16th, as indicated by the Hygrometer, but did not fairly set in till the 18th.

P. G.

VI.—*Miscellaneous Notices.*

1.—*River Steam Navigation in France.*

We recently had occasion to notice, that a steam boat, on an improved construction, had been making an experimental trip on the river Garonne from Bordeaux to Toulouse, in the south of France, which promised considerable advantages in the navigation of rivers, with rapid currents, and shallow water.

We now learn the following particulars of this expedition, which bids fair to affect such important improvements in the internal communication of France ; for canal navigation in the neighbouring kingdom is still greatly inferior to the state it has attained in this country.

We understand, from our correspondent, that this steam boat is of English manufacture, having been built, on a new model, by Messrs. Bush and Co. of the Regents-Park Basin, for a Steam Navigation Company at Bordeaux. It is stated to possess a remarkably shallow draft of water, in proportion to its tonnage. It is about 80 feet in length, and 18 feet width of beam, with a pair of engines of forty horse power, constructed upon the high pressure principle, similar to most of the steam boats built in the United States.

As far as we can learn, these engines combine all the advantages proposed by Mr. Gurney's patent, in the substitution of tubes of wrought iron, in lieu of boilers for generating the steam, combined with the advantages proposed by Mr. Perkins, in using steam of extraordinary elastic power.—In the first place, all possibility of accident, by explosion, is avoided, by substituting tubes for a boiler in raising high pressure steam, while the mechanical power of the engine is increased, perhaps 50 or 60 per cent, by allowing steam, at a temperature of 400 degrees, or upwards, to expand to double its orginal volume in the working cylinders. We understand these engines are also calculated to condense nearly the whole of the water used for raising the steam ; thus preventing that waste of steam, and loss of fuel, usual in most other kinds of high-pressure engines. This must prove a very valuable improvement in sea-going steam vessels, where salt water is necessarily used : sea-water having a tendency to block up the pipes or boilers with saline incrustations ; and which a few years back produced a dreadful accident at New York, in America. The draft of water of the new steam boat at Bordeaux, is stated, by our correspondent, as only 2 feet 2 inches, when laden with fuel, water, and having 50 passengers on board. The voyage from Bordeaux to Toulouse is estimated (by water) at upwards of two hundred miles ; and the new steamer is stated as having performed this journey in about 14 hours, exclusive of stoppage, or about five miles per hour, against a strong current. On the return voyage to Bordeaux, the steamer performed the distance in 15 hours, notwithstanding many parts of the river were so shallow as scarcely to allow the passage of the boat without grounding. The experiment is stated as having produced a great sensation among the mercantile interests in the south of France, connected with the Mediterranean trade. It cannot fail to have a beneficial effect, both upon French commerce, and English manufactures, for the engines must be made in England to insure good workmanship.

We have just heard from another channel, that a steam boat, upon a similar construction to that built for the river Garonne, was tried a few days since, on a voyage down the Thames, and through the Thames and Medway canal, to Rochester and Maidstone. The voyage is stated as having not taken more than six hours each way, from London to Maidstone, and back, notwithstanding the shoals in the river Medway, above Rochester.

We have always been of opinion, that no more danger could arise from the use of high-pressure engines than low-pressure engines, if proper means were taken to prevent all possibility of explosion. And between the use of steam pipes in lieu of boilers, and having apertures of sufficient size for escape-valves, we cannot see any chance of risk, from the use of high-pressure any more than low-pressure steam; while in point of economy and lightness, the advantages are decidedly in favour of high-pressure engines, for steam navigation.—*Times Newspaper.*

2.—*Royal, and other Societies.*

In the work, of which we gave some account in our last number but one, and which appears to have excited so much attention in England, there is much which, but for our narrow limits, we could have enlarged upon. In the Chapter on the Royal Society, the Author points out the necessity of the members exercising some independence of opinion, and of their speaking out a little more than they do. We have a pretty good idea of the sort of thing which Mr. Babbage describes in this section; and we do not hesitate to say, that a more serious bar to the prosperous and honorable career of any scientific body cannot be found, than this whispering in corners; this deference to names, on points on which every member should consult his own honest and unbiassed judgment. A feeling is generally prevalent, that to call in question the proceedings of any functionary in such a society, is to be guilty of a want of delicacy towards the individual, and, in fact, to make a personal attack on him. This mischievous opinion is well exposed by Mr. Babbage; as is also the indifference generally manifested towards the state of the funds by the members; every thing being left to the management of one or two, who contrive never to give the Society at large any acccount of the expenditure. In this way the Horticultural Society at home got involved in debt to the extent of £19000; and but for the appearance of a determined reformer amongst them, it is very likely that they would have gone on in their mad career to the present moment. There is little doubt that Mr. Babbage's book will occasion much improvement on all these points, in the different societies at home; and especially, that it will produce a very great, if not a *radical,* reform in the Royal Society itself. His statements are so important, being published too now in the face of all Europe, that it is quite impossible to avoid taking up the subject. And if taken up, equally so to avoid applying some remedy to such evils. And though we must, in justice to our Indian Societies, concede that we have nothing which can be compared to the miserable system here detailed, as that by which the Royal Society proceeds, yet do we also stand in need of some reforms—reforms which have been called for, and which must come. And in the conviction that the knowledge of the evil is the first step towards a cure, we recommend to every member of our Indian Societies to read this book, and at each description of any objectionable course, to ask himself how do WE manage these things; may we not take a hint from this, and do better? Above all, we would recommend, did we think our voice likely to have any weight, that every member should take an interest in the proceedings of the Society he belongs to; that he attend its meetings when he conveniently can; and that in voting, he exercise his own judgment, scrutinize the statements made to the meeting, from whatever quarter coming—and that he particularly oppose himself to the slovenly practice, but too common amongst us, of getting rid of a subject brought regularly before the meeting, by private and desultory conversation. We think the few points in which some of our Societies require to be regenerated, might easily be carried, did the members take a little more interest in the proceedings and prosperity of the body to which they belong. And in reality, this is the root of the evil both here and at home, *i. e.* the neglect of their duty by the members; for otherwise, it would be impossible for one man, or set of men, to obtain the influence we see, were he not left the whole field to himself, by the indifference of some, and the timidity of others.

GLEANINGS

IN

SCIENCE.

No. 23.—*November*, 1830.

I.—*On Mr. Ricardo's System of Wealth and Value.*

I have endeavoured to prove, that if it be our desire to learn any thing concerning wealth, we must admit the existence of positive value. I have also endeavoured to show the impossibility of arriving at any just conclusion regarding man, and his circumstances, through the means of relative value; and further to establish the necessity for keeping the two descriptions of value perfectly distinct in all our reasonings. As an example of the consequences of not attending carefully to these principles, I now propose pointing out some of the errors into which the most celebrated of recent writers on Political Economy has fallen.

If it be true, that there are in existence two distinct kinds of value; the real value, which cannot fail to be appreciated by the percipients of valuable properties in the bodies, constituting wealth; and the relative value, by which these percipients may be subsequently guided, in bartering product against product: and if it be further true, that the road to a knowledge of how man is affected by the products, of which wealth is composed, is through real, and not through relative value; then it follows, inevitably, that no talents, however acute, that no intellect, however sound, can save a writer from continually running into fallacies, who conducts his reasonings, regarding wealth, on the false assumption, that relative value alone exists.

But to conduct reasonings regarding wealth on this assumption, is manifestly impossible; wealth and real value being inseparably connected: accordingly, those even who deny the existence of real value, cannot, in discussing wealth, proceed one step even in their career, without unwittingly granting its existence. They are, consequently, continually embarrassed; their minds wandering from real value, to value estimated in commodities; from the relations of products to moral agents, to the relation of products among themselves; and the confusion, hence arising, opens the way to all manner of sophistication. Thus, when it is stated, that price must rise, whenever all products become the result of more labour than before; and when the person advancing the position, is met with this question, "How can price rise, if all the products, in which price can be estimated, must have been equally affected by ther ise?" unless he can point out, that there are two descriptions of price; and that the price which cannot rise, is perfectly different from the price which can rise; that there are two kinds of value or price, positive and relative; a reasoner must either be silenced, or at least so greatly embarrassed, as to feel grateful to any hand which offers to lead him out of the logical difficulties by which he feels himself surrounded. Now Mr. Ricardo having got his reader into this difficulty, undertakes, in a manner of his own, to aid in his extrication. To enable him to do this, however, a necessity, as may be supposed, exists for many manœuvres which just reasoning utterly rejects; for instance, although he lays it down, that wealth and value are perfectly distinct in their nature, he is continually found discussing wealth, and its properties, while he conceives himself to be, and persuades his reader also that he is, engaged with the consideration of relative value alone; and wherever arguments connected with relative value can be made use of to establish the conclusion he is at any time anxious to attain, they are, of course, introduced; and wherever the introduction of arguments connected with wealth, or positive value, can be effected with a view to the same end, they also are dexterously made use of. And this continual shifting of his ground has been, I doubt not, one of the principal causes of the exact nature of his fallacies not having been perceived, and of his system having existed so long without exposure.

To enable us, therefore, to deal with this reasoner, it is necessary continually to keep in mind, that what is predicated with justice, regarding one description of value, may be utterly false with regard to the other. Thus, under the supposition, that coats, shoes, and hats, or the labours of which they were the result, stood to one another, in the relation of 20, 15, and 10; then it is clear, that when we look strictly to the *relative value*, the relations alone of these products amongst themselves, we learn nothing whatever with regard to their real value; whether 200 or 20 men, for a given time, be required to produce the coats; whether 150 or 15 men be required to produce the shoes; and so forth; for, under either circumstances, their relations will be the same, and by the study of the relations alone of products to products, we most assuredly can learn nothing beyond these relations. By the exclusive study of the relative values of these products, I maintain, (and I cannot help stopping to wonder that such reiteration should have become necessary,) that we should make no progress whatever, in ascertaining the circumstances of the persons possessing the commodities;—their wealth or poverty would undoubtedly remain to us for ever unknown, if we continued only looking at the relations of the products, and yet all reasoners will agree that the wealth, or poverty of men, are the subjects of the science of political economy. How then can these relations of commodities be twisted into indications of the wealth of men?

But if in place of studying the relations of these products, or what I may term their commodity value, (in contradiction to their money, which I conceive, for reasons which I am prepared to give, must always correspond with their real value,) we had set about learning the positive value which they enjoyed, and which was inevitably put upon them, by the men who had laboured in obtaining these possessions; if we had ascertained at what sacrifice of bodily ease the people possessing these articles, had succeeded in obtaining each product; if we had passed by, as a matter of minor importance, the accidental relation in which the exertions necessary for obtaining each different product happened to stand, and if we had then bethought ourselves of the esteem in which men must inevitably hold, that which is the product of 10 or 100, of 12 or 120, of 15 or 150 days' voluntary drudgery; and with a view to discover how these people would be affected when a change in the means of obtaining products had taken place, if we had kept our eye steadily fixed on these considerations, and not allowed it to wander to the relations of the commodities amongst themselves; and having seen that it required at one time 20 days labour to obtain a coat, and at another 10; at one time 7½ days labour to produce shoes, and at another time 15; at one time 5 days labour to produce a hat, and at another time 10; we should then have learnt, that at one time the persons obtaining coats, shoes, and hats, had been comparatively in more easy circumstances, than at another; that in fact, they had, at one time, been comparatively wealthy, to what they were at the other; and also that a change had been brought about in the real value of their products; although the relations of these various products, one to another, had undergone no change whatever.

But it may be answered, that Mr. Ricardo also looks to the quantity of labour of which products are the result, with a view to determining originally, their real value. This, however, I maintain, he ought not in consistency to do; for he denies the existence of real, or of any kind of value, except what is relative; and I maintain, that if he had reasoned logically on the subject, and kept his eyes fixed on the relations alone of the commodities amongst themselves, he must have seen at once the utter hopelessness of his undertaking. I am aware, however, that notwithstanding his professions to the contrary, he does continually reason, unwittingly, on the existence of real value; and this is one of the instances. How else could his lucubrations have had any connexion whatever with the subject of wealth? His first step, then, in this science, is a most gross and palpable error, which ought, at once, to have been obvious; his chief aim is to show, in fact—all that is original in his essays, goes to establish this most outrageous fallacy; that whether products be the result of greater, or of less labour, at one time than at another, so long as they are all equally affected by any change, there can be no change in their *real* value. Thus he would write, if coats were now the result of 10 days labour, and had been formerely the result of 20; if shoes were now the result of 7½ days labour, and had formerly been the result of

15; if hats were now the result of 5 days labour, they having previously required 10 for their preparation; as the relations of these various products, one to the other, would now be precisely the same, as they had been before; as they still would stand to each other as 20, 15, and 10, there could, therefore, be no change whatever in their value.

But it must be enquired, would the wealth of the people have remained unchanged, although the relative values of the items composing it had been so? would the society have experienced neither enrichment nor impoverishment, because these products happened to continue in the same relation to each other as before? In answer to this, Mr. Ricardo, now separating wealth and value, and looking only to relative value, answers, the society might have more wealth, but they could have no increase of relative value; and this the only legitimate conclusion to be attained from his premises, ought to have convinced him, that, if he desires to study wealth, the way to effect his object, was not through the means of the relations of products. But no, he would proceed; and he essayed the unatchievable enterprize, of establishing, how invariability in the relative value of products, under certain circumstances, was to be a paramount principle in the theory of wealth, and that upon it was to hinge all the laws which regulate profits, and wages, and rents; the value of food, with the progressive increase of population; and all that is in connexion with taxation, and international commerce.

The following is pretty nearly, I believe, the mode of his proceeding.

Were all products to have become the result of more labour than before, it is not possible that, after so great a change, the incomes of the different classes of society should be found to have undergone no change. The wealth of various classes must have been affected in different degrees, because, in real life, we do find that certain changes are brought about in the incomes of various classes during the progress of wealth[2].

There being only relative value in existence, and there being only commodities wherewith to make payment for commodities; on the occurrence of a general increase of the labours necessary in obtaining products; there cannot possibly be a change of commodity prices, and if there can be no change of price, there cannot possibly be any means of making the rise in the cost of production reach the pockets of consumers. If then the consumers do not pay an increased price, who is to do so? In this dilemma, Mr. Ricardo bethinks him, that in certain stages of society, there are two classes connected with production; those who labour, and those who furnish the advances for setting labourers to work. Having satisfied himself that prices (meaning relative prices) cannot rise, when all things are equally affected by an enhancement of the difficulty of production, he imagines that he has established how price (meaning thereby real price) cannot rise. If then, he concludes, the high real price (mark the fallacy) cannot be paid by consumers, because the *relative prices* are unaltered, the high price *must* be paid by capitalists; and they *must* be subjected to a fall in their profits! Because the relations of products to products must, under certain circumstances, remain fixed, *therefore* the increased real value, when this takes place, the increased difficulty of production; the altered relation between products and men, must fall upon the capitalists! "Oh most lame and impotent conclusion." I trust the rank confusion of ideas, the jumbling of real and relative value or price which pervades the whole of this argument, have been made sufficiently apparent; and that the interminable shifting is seen, from this kind of value, to that which is necessary to bring him to this conclusion, unhappy, and inconsequent as it is. Allowing that there could be no change in the relative value of products under the circumstances supposed, by what manner of logic could it thence be inferred, that an increase of the labour necessary for obtaining all things,—an increase of their real value,—is to fall exclusively on the heads of capitalists? The only logical inference to be drawn from Mr. Ricardo's premises, relative value alone existing, is, that after the general change, affecting all things equally, no effect whatever would be produced on society, except a general diminution of wealth, which had no connexion whatever with relative value; a conclusion which, from his anxiety to find out, and account for other consequences, Mr. Ricardo must have perceived to be shutting him out, while studying relative value alone, from the power of predicating any thing regarding the way in which the incomes of various classes were to be affected in the progress of production: but his object in writing being to account for certain

[2] This sophism is of continual recurrence with the writers of the Ricardo school; such and such consequences must follow, not because of any reason assigned in the argument, but because we find such to be the case in practice.

changes which are known to take place in the incomes of various classes, as society advances in improvement, it behoved him to proceed, and to force a seeming connexion which does not really exist. The invariability of relative value in products establishes, that products, in their relations to each other, will remain precisely as they were before; and not that men will be in the same circumstances with regard to wealth as before: but he wishes to make it appear that no classes of men will be affected by the change except one; and that this one shall suffer impoverishment, while all the others escape! To effect the object for which he was writing, he was necessitated to make all this appear; and that he succeeded, most men have been forward to allow.

All products may equally become the result of more labour, by a general visitation, for instance, whence all labour should be rendered less effective. They will all, therefore, under these circumstances, cost more of the "first price, "the original purchase money of all things;" and will, consequently, be less within the reach of all classes than before: and yet their relative values estimated in gold, if it were merely a commodity; or in hats, or shoes, or coats? might be unchanged. In this case, would the existence of two classes concerned in production, of a class of capitalists, and a class of labourers, be required to shield mankind from general impoverishment? Yet this is, in effect, what Mr. Ricardo lays down as being inevitable under all circumstances. Because relative prices, estimated in products, cannot be altered, when all products are equally affected by any change; *therefore*, when there are two classes concerned in production, the capitalist alone *must* be impoverished, whenever a necessity occurs for going to a greater expense in obtaining all the products requisite for human consumption! To me there appears no possible connexion between the premises, and this conclusion? and that it is such an instance of inconsequent reasoning as is rarely to be met with. I do not mean to say, there are no circumstances in which capitalists may find it to their interest to keep production at its former amount, by taking on themselves the payment of an increase of wages, or any other increase in the necessary cost of production. But I maintain, that if this should happen to be the case, it would not be because of the invariability of relative value; but because the capitalists found their enrichment to be greater, while they kept all their former labourers at work, even at higher wages than before, than if they discharged any number of them; and thus by reducing production, reduced also the number of sales, by the aggregate amount of which, actually effected, they were most greatly benefitted.

In this case relative values might remain unchanged, and the rise of cost in production, from whatever cause proceeding, might be entirely paid by a corresponding fall of profits. But to attribute the fall of profits to the invariability of relative value would be mistaking a collateral effect for a cause.

At any rate such effects could never be produced under the circumstances Mr. Ricardo supposes, when, from the pressing of production, and population against the limits assigned to them, a necessity had arisen for resorting to inferior lands to obtain food sufficient for the growing population; and when production, in all its branches, must be full, and pressing on the limit assigned to it by the physical circumstances of the country, and the existing knowledge of productive arts.

It may be said, that I do not state the argument fairly; that I ought first to explain what Mr. Ricardo takes for granted, namely, that a rise of wages is quite a different thing from any other increase in the cost of production. When the food of the labourer is obtained by a greater expenditure of labour, there is not, he assumes, an increased quantity of labour expended in any subsequent product of that labour, the products are the results of no more labour than before; they are only the result of more highly paid labour; and therefore the increased cost of obtaining products, when a general rise of wages takes place, is very different from any other increased cost of obtaining products. But as I shall hereafter shew, more in detail, this is a mere unwarrantable assumption; and if it can be proved that a general increase in the cost of producing goods, may, in the instance of increased wages, be prevented affecting the consumers, because there can then be no change of relative value; I conceive, that the same arguments must be applicable to every general increase of the cost of production; whether that happen to fall directly on product, as it would, in the event of all men's labour becoming less effective; or whether it fall on products indirectly, as in the case which Mr. Ricardo supposes, where the cost of feeding the labourers has been increased, through whose means the products are obtained. Unless he can establish it as an axiom in his science, applicable to all general rises in the cost of production, that these rises cannot affect consumers, he has done nothing; for we cannot allow him to blow hot, and to blow cold, with the same breath.

Mr. Ricardo states his argument pretty nearly thus. When, to obtain the increasing food, necessary for the support of an increasing population, resort is had to lands of an inferior quality; the product of this unferior land being the result of more labour, must be more valuable; (what value is he discussing now, positive or relative? it will be found that he is now treating of positive value;) the wages of labour must, therefore, rise, to enable the labourer to exist; for this class of men is always to be supposed living from hand to mouth: If on this occurrence the productive capitalist dealing in hats, should say to the capitalist dealing in fish, "give me more fish for my hats, because I have had more than before to pay to the labourers who made the hats:" the answer of the fisherman would be, "my labourers also were paid more wages, and you, in like manner, must give me more hats for my fish:" and thus it would be with all producers whatsoever; with him also who raised the gold from the mine with which money was formed; for Mr. Ricardo always treats money merely as a commodity. There could, therefore, be no rise of price, all products standing in the same relation one to another, as they did before. But in what, I beg to know, does this vary from the statement I have given; except that by the mention of wages, and labourers, he opens the way for introducing the class of capitalists? But if food be necessary for the support of labourers, and if all labourers come to be supported on food on which more labour than before had been expended, and if again, all products are the result of labour, it follows that on the occurrence of this necessity, for applying more labour to obtaining food, all labourers are the result of more labour than they were before; and all the subsequent products, obtained through the means of these labourers, are the result of more labour than they were before. When we talk, then, of a rise of wages, such as the above, we talk, in effect, of an enhancement of the difficulty of obtaining every product; of a rise of their real price, and positive value; and when we say, that a rise of wages must be paid by capitalists, we say, in reality, that every general increase in the necessary labour of production shall fall, not upon all classes, as it would be natural to suppose, when we found that every one's means of obtaining products were diminished; but upon one unfortunate class, which it has pleased Mr. Ricardo to thrust forward for this particular purpose. If such a thing should happen, as a necessity for resorting to worse lands for the increasing food of the society, before there was in existence a class of capitalists, who then would pay the increased real price of food? Consumers could not, in this case, for the same reason, why they could not pay before; if their purses can only be reached through the means of a commodity, their pockets could not be touched by an alteration in the relations of any commodities under any circumstances; and capitalists there are none on whom to shove the charge! What then, would the wealth of the various classes in society be in no way whatever affected by so great a charge as the above? It must be answered, that, if there be no such thing as real value, society could not be affected at all; and the study of relative value, as alone existing, would tell us, that no change whatever would take place amongst consumers in these altered circumstances; because values, estimated in commodities, had undergone no change.

But Mr. Ricardo, as I have already mentioned, always unwittingly reasons as if real value did exist in some classes of products; he always reckons upon the price of food as rising, when it is the result of more labour than before. I apprehend, however, that if his argument against a general rise of price, on the occurrence of a necessity for increased labour in raising food, be good for the purpose to which he applies it, it will be good to prevent a rise of price in food also, as well as a rise of price in commodities.

If, as we have just seen, labourers are only so much food, it follows that when the labouring hatter, the labouring shoemaker, and the labouring clothier, are fed upon what it has required more labour to acquire, then each stands for more food than before; and all the subsequent products of their labour must be the result of proportionately more food; and if the farmer says to the hatter, "give me more hats for my corn, it being the product of more labour," will not the hatter be justified in the reply, that his hats also being the product of more labour, the farmer must pay him more corn for the hats? and will not this answer be just in the mouths of all classes of producers, in that of him who produced the gold, as well as in that of any other? In what way, then, will the price of food be enabled to rise? In what commodity can it be estimated and shown to have risen in value? The relations of all products, one to another, will, in this case as in the other, be just as they were before; and food must remain unchanged in price. If the rise of price in food be necessary to the rise of wages which is to lower profits, Mr. Ricardo, before he can demand assent to his doctrines, must first shew us in what kind of price this rise is to take place: it cannot take place in commodities; and he does not admit the existence of any except commodity price; in what else, then, can he suppose the rise to take place? The study of production, through the relative values of products, will help him to no answer to this query: not even the adroitness in turning all arguments to his purpose, which distinguishes Mr. Ricardo, will serve to extricate him from the horns of the dilemma, between which he has thus got enclosed. He maintains, on the one side, that value must always be great, when producing labour is great; while he maintains, on the other, that value cannot rise, even when the producing labour is increased, if it should so happen, that all things are subjected, at once, to the same change in the means of production. Professing to deny the existence of positive value, he does not perceive that he has already admitted its existence, in the mode in which he makes it act on wages, and through them upon profits; and also in the fundamental principles which he lays down in his chapter on value. "The real price of every thing," what every thing really costs, is the toil and trouble of acquiring it. "Labour was the first price, the original purchase money of all things." In these axioms of Dr. Smith, which he quotes with approbation, he admits the existence of real price; and as price is only a species of the genus value, price being of necessity offered only for what has value, he admits the existence of real value also. Of what value does he treat, when he lays it down, that "value depends on the difficulty, or facility of production?" It must be positive value; for he here has reference to nothing, save the moral agent perceiving the existence of value; and he must admit its existence here, even if there were no other than one description of wealth in being; and when, accordingly, there could be no relative value whatever. But although the attempt is made to deny the existence of real value, he reasons, as I have said, upon its existence, whenever he treats of the rise which is to take place in the price or value of food: when he asserts, that if any one commodity could be found which now, and at all future times, required precisely the same quantity of labour to produce it, that commodity would be of an unvarying value, he treats, of necessity, of its real value; for here it is the moral agent alone, and his perceptions, with which he is concerned. But when he asserts, that on a general and equal change in the labour necessary for producing all things, there could be no change in their respective values, he is treating of relative value; for here he has quitted the consideration of the moral agent altogether, and is treating of the relations of products among themselves. His theory of profits then, is founded on these too conflicting suppositions; that real value can and that real value cannot exist. He looks to a rise in the real value of food, which, having served his purpose of raising wages, is then cast aside; and afterwards working with the relative value of the products, he concocts that scheme of invariability in the value of products, by which the class of capitalists is continually to suffer impoverishment.

But perhaps the doctrine, that we must, of necessity, run back beyond the labour directly employed on any product, even to that which is sunk in rearing and feeding the living instruments of production, in determining of how much labour products are actually the result, may require some further elucidation; Mr. Ricardo making a distinction, so very essential to his theory, between products which are the result of more labour, and those which are the result of more highly priced labour.

It will not, I believe, be denied, that without the co-operation of labour, the labourer himself could not be produced in the market; that in bringing him to market, of full age, and ready to apply himself to such work as offers, a certain quantity of labour has been sunk in feeding, housing, and clothing him. Nor will it be denied, that when he is in the market, a similar expenditure of direct and indirect labour will be necessarily made in the preservation of his existence. If this be so, then it follows, that the necessary wages of labour must be such as to cover all these various descriptions of expenditure, otherwise the supply of labourers must cease. If this be granted, then it follows, to my apprehension at least, that products are the result, not only of the labour which is directly applied, and of the labour sunk in dead implements of industry, the buildings, machinery, and so forth; but of what is sunk in the live instruments also; without which all the others are as nothing. On what ground, then, can it be denied, that the labour which brings labourers to market, and which determines the amount of wages, is a part of that labour, of which a product is directly and indirectly the result? I find no answer to this question in Mr. Ricardo's work. But I meet with this opposite assumption, that if the wages of labour should rise, that is, if it should become necessary to expend more labour in producing labourers, (and consequently in producing all the subsequent results of labour,) this additional labour, of which all products would be indirectly the result, will not be additional labour expended on their production; but would be a something or other, the expense of which it would behove capitalists to take upon themselves. If spades become the result of more labour, because the iron, for instance, was now obtained with more difficulty from the exhausted mine: or because the timber, of which the handle was formed, had been grown at a greater distance than the wood formerly used in these instruments; it would be admitted at once, that the subsequent product of the earth tilled with this spade, was the result of more labour than before. But if this product of the earth were the food of labourers, and this more laboriously obtained food were expended in bringing a more laboriously reared labourer to market, it would be denied by Mr. Ricardo, that the subsequent results of this man's labour were the result of more labour than before; it would be assumed, for what reason I cannot tell, that they were the product of more costly—more highly paid labour, but not of a greater quantity of labour than before. Now to my apprehension, more costly labour means labour which it has cost more labour than the former quantity of requisite labour to produce; and as we all grant that products are the result, not only of the immediate, but of the mediate labour employed upon them, when we say an article is the product of more costly labour, we say, in reality, that it is the product of a greater quantity of labour: and I consequently infer, that whatever may be predicated of products, the result of more labour; may with truth be predicated of products which are the result of labour, which it had cost more labour than before to produce.

But I am apprehensive lest those who are not conversant with the works of this distinguished writer, may conceive that I am not treating the author fairly, in thus submitting his arguments in my own language, and in a manner calculated, in my opinion, to betray their fallacy. Here then follow all the passages which I can find in his main work, bearing upon the points at issue, and tending to establish his theory of value, and that of profits which is entirely founded thereon.

In Mr. Ricardo's chapter on value, we meet with these passages.—"If among a nation of hunters it usually costs twice the labour to kill a beaver which it does to kill a deer, one beaver would naturally exchange for or be worth, two deer." "Without some weapon neither the beaver nor the deer could be destroyed, and therefore the value of these animals would be regulated, not solely by the time and labour necessary to their destruction, but also by the time and labour necessary for providing the hunter's capital, the weapon by the aid of which their destruction was effected." "All the implements necessary to kill the beaver and deer might belong to one class of men, and the labour employed in their destruction might be furnished by another class; still their comparative prices would be in proportion to the actual labour bestowed, both on the formation of the capital, and on destruction of the animals."

Now however interesting this may be to those who are occupied with the determination of the relations of products alone; it cannot, I should think, throw much light on the enquiries of those whose aim is to learn how the wealth of the society might be affected.

Yet I do not find, in this part of his work, any thing further touching upon the question of how relative value is to affect wealth. Mr. Ricardo, however, must have understood that he was all this time writing, about wealth; for he thus proceeds, "Under different circumstances of plenty or scarcity of capital as compared with labour, under different circumstances of plenty or scarcity of food and necessaries essential to the support of men, those who furnished an equal value of capital for either one employment or the other, might have a half, a fourth, or an eighth of the produce obtained; the remainder being paid as wages to those who furnished the labour: Yet this division could not affect the value of commodities, since whether the profits were greater or less, whether they were 50, 20, or 10 per cent, or whether wages were high or low, they would operate equally on both employments."

Here the division of the produce between two parties, in different proportions, according to the varying circumstances of production and accumulation, is stated to have no effect on relative value; and the circumstance of one party's share being small when the other is great, and vice versa, is only incidentally mentioned; and no conclusion is here drawn that one party's share *shall* be small, and another's great, in consequence of this invariability of value : yet, I believe, we shall find, that this incidental notice, paving the way to similar propositions more specifically made, is all on which is grounded that which is subsequently treated as having been proved, namely, that because prices, estimated in the commodity gold, cannot vary on a rise taking place in wages, therefore these rises of wages must, under all circumstances, be made good by reductions in the rate of profits. For instance, in a few pages after, we meet with this proposition ; " The proportion which might be paid for wages, is of the utmost importance in the question of profits ; for it must at once be seen that profits would be high or low, exactly in proportion as wages were low or high. But it could not, in the least, affect the relative value of products, as wages would be high or low in all occupations. If the hunter urged the plea of his paying a large proportion, or the value of a large proportion of his game for wages, as an inducement to the fisherman to give him more fish in exchange for his game, the latter would state, that he was equally affected by the same cause." " No alteration in the wages of labour could produce any alteration in the relative value of these commodities ; for suppose them to rise, *no greater* quantity of labour would be required in any of these occupations ; but it would be paid for at a higher price ; and the same reasons which should make the hunter and fisherman endeavour to raise the value of their game and fish, would cause the owner of the mine to raise the value of his gold.—This inducement, acting with the same force on all these three occupations, and the relative situation of those engaged in them being the same before and after the rise of wages, the relative value of game, and fish, and gold, would continue unaltered.—Wages might rise 20 per cent. and profits consequently fall in a greater or less proportion, without occasioning the least alteration in these commodities."

Here may be observed the growing confidence with which the proposition is now laid down, that as relative value, which, as he knows it, is price estimated in the commodity gold, cannot be altered ; and as thus, relative value, or price, remaining the same, whether wages rise or not, consumers, who have to pay gold price for all products, cannot be affected by the rise of wages ; which being the case, rises of wages, he concludes, *must* be paid by capitalists in a reduction of their profits.

At page 31 we find the original incidental notice, on which his conclusions are grounded, swelled into a broad assertion, " that there can be no rise in the value of labour without a fall of profits ;" and that, as the corn must be divided between the farmer, and the labourer, the greater the proportion given to the latter, the less must remain for the former ; while, at page 45, it is announced, as having been clearly proved, that prices cannot rise, on a rise in the cost of producing food, and on a consequent rise of wages, as maintained by Adam Smith, and others ; and that he has shewn how there are no grounds for such an opinion : and again, in his chapter on profits, where we had a right to expect a full explanation, of how invariability in the relations of products to products, or fixedness of relative value, could produce so inconsequent a consequence as making one class of men poor, or possessed of little value, exactly as it put another class in possession of much value or wealth, (wealth and value being totally distinct, and proceeding from different sources, by his own theory,) we find it has been taken for granted, that these matters have already been satisfactorily proved ; " that corn, and manufactured goods always sell at the same price, whether wages are high or low ; and that profits are, in consequence, always high, when wages happen to be low, and vice versa ; and after a considerable number of ingenious arithmetical illustrations, grounded on the presumed admission of all he requires, namely, that if it so happens that fixedness in the relations of products to products exists after all things have become more expensive in an equal degree, then the consequence must be the causing of one class of producers to be impoverished, and the rest of the society to be left in the former circumstances ; which illustrations, under this assumption, looking to him like actual demonstration, he triumphantly exclaims, " If the farmer gets no additional value for the corn which remains after paying rent ; if the manufacturer gets no additional value for the goods which he manufactures ; and if both are obliged to pay a greater value in wages, can any point be more clearly established, than that profits must fall with a rise of wages [1]?"

[1] Mr. Mills protests "there is not a demonstration in Euclid, in which the links are more indissoluble" than in Mr. Ricardo's Theory of Profits. Article Colony. *Sup. Enc. Brit.* And the Opium Eater exclaims, that Mr. Ricardo " had deduced *a priori*, from the understanding itself, laws which first gave a ray of light into the unwieldy chaos of materials, and had constructed what had been but a collection of tentative discussions into a science of regular proportions, now first standing on an external basis."

Relative value, says Mr. Ricardo, is price. I take leave to differ with him in this. Relative value is the relation of products to products; and price is that by which positive value is designated and known; it is the mark of estimation in which is held the produce of that exertion by which alone man obtains wealth. Price then is not relative, but positive value. Mr. Ricardo proceeds: "It is only through price that consumers can be affected." This I grant, if he means real price; but he does not, he means by price, the relative values of the commodity gold, and other products; and consequently I deny that it is through the means of such price that consumers are affected.

But if what appeared to Mr. Ricardo, in his chapter on profits, the demonstration of the truth of his principles be not sufficient for their establishment, what answer, it may be asked, can be given to the concluding paragraph of the same chapter, in which it is shewn, that whether prices rise or not, the effect will still be the same, a reduction of the capitalists profits? He writes as follows. "In the chapter on wages, we have endeavoured to shew that the money price of commodities would not be raised by a rise of wages, either on the supposition that gold, the standard of money, was the produce of this country, or that it was imported from abroad. But if it were otherwise, if the prices of commodities were permanently raised by high wages, the proposition would not be the less true, which asserts that high wages invariably affect the employers of capital, by depriving them of their real profits. Supposing the hatter, the hosier, and the shoemaker each paid 10£ more wages in the manufacture of a particular quantity of their commodities, and that the price of hats, stockings, and shoes rose by a sum sufficient to repay the manufacturer £10, their situation would be no better than if no such rise took place. If the hosier sold his stockings for £110, instead of £100, his profits would be precisely the same money amount as before; but as he would obtain in exchange for this equal sum, one tenth less of hats, shoes, and every other commodity, and as he could with his former amount of savings employ fewer labourers at the increased prices, he would be in no better situation than if his money profits had been really diminished in amount, and every thing had remained at its former price. Thus then I have endeavoured to shew, first, that a rise of wages would not raise the price of commodities, but would lower profits; and secondly, that if the price of all commodities could be raised, still the effect on profits would be the same." (Page 130.) Now this looks very imposing, and seems to prove beyond all doubt the correctness of his reasoning: for it can first shew itself to be right, not only on the grounds originally taken up; but can be proved to be correct, even after conceding to the adversary the very point for which before he was contending. What, however, has Mr. Ricardo told us here? Simply this; that products, being now the result of a greater capital than before, if you increase the price of products, only so much as serves to cover the extra expense incurred on labour in their production, and do not increase the profits also in proportion, you reduce the capitalists rate of profits. Now this, I conceive, is begging the question; for it is assuming the very point at issue, namely, that wages alone will be repaid in the increased price of wares, and that an increase of profits, corresponding to the increase of capital, will not be made. If we increase the capital with which products are obtained, and do not increase profits in a corresponding degree, it is very clear that the rate of profits will be suffering reduction; that all classes will be advancing while capitalists are standing still: if we raise the value of the produce going to the use of every class except the class of capitalists, nothing can be more certain, than that the capitalists will be rendered comparatively poorer; but the question is, have any sufficient reasons been given why this impoverishment of the capitalists should take place?

Mr. Ricardo establishes, to his own satisfaction, that all who admit the possibility of a general rise of price, on the occurrence of a general rise of wages, or on any other sufficient occasion, run into a most gross and palpable contradiction; and this, whether money be the produce of the country under discussion, or whether it be imported from abroad. "Thus, under the supposition that money is a commodity, and like all other products influenced by a rise of wages, he says, in admitting a general rise of price we pass unnoticed that, when money is a home product, the same reasons which induce the hunter and fisherman to raise the value of their game and fish, cause the owner of the mine to raise also the value of his gold; and when money is a foreign commodity we forget that gold must be influenced by the new demand for it, which must be inevitable for increased circulation, when all the prices of an increased quantity of goods are to be raised. When there is a demand for more products, there is more capital in existence to employ increased quantities of labour. Corn and wages rise because an increased population is to be fed; to circulate more commodities more money must be produced. We have first an increased demand for gold which must raise its price relatively to all other products; and we, at the same time, are contending for a general rise of price, or a rise of all things relatively to gold." This latter argument, he applies only to countries importing gold from abroad; but it holds with equal force in the case of countries containing mines, and requiring more gold of home production, to circulate increased quantities of goods; it cuts two ways, although Mr. Ricardo does not perceive it. But of what price, I ask, is Mr. Ricardo talking, when he makes all these accusations, of people maintaining positive contradiction? It will be answered that he is thinking of gold price alone. This, however, I beg leave to deny; for in the case of corn, or the food of labourers, he always, as already observed, studies its real price, and its positive value; the real cost in labour of its production,—or in other words, its relation to man, the moral agent; while in the case of all other products he views them through the medium of price estimated in the commodity gold, or through their relative value. On what grounds, I ask, can this be justified?

On the occurrence of a necessity for resorting to worse lands for the food of an increasing population, the price of food is to rise, and why? Mr. Ricardo, looking to real price, answers, because it is the result of increased quantities of labour; because its real price, its positive value, will have risen. On the occurrence of the same event, if Mr. Ricardo be asked the same question, he now looking to gold price, or relative value, must answer, there can be no rise of price unless the quantity of gold be increased, relatively to the goods to be circulated through its means; for to say that the price of corn rises, is to say, that the price of gold relatively to corn falls; and the necessity for resorting to worse lands for food, creates no necessity for such an increased production of gold as will occasion this fall: more gold, on the contrary, will be necessary to circulate all the food obtained both by the former extent of cultivation and the new cultivation; therefore, money price will fall. Mr. Ricardo must conclude, then, that with the new demand for gold, the price of food would be more likely to fall than to rise, even though it were the result of more labour than before; where then is this rise of the price of food on which so many of his conclusions turn? There can be no better illustration than the above, of the consequences of attending only to relative value, when we wish to learn the progress of wealth.

All his arguments, then, against the impossibility of a general rise of price, (and it is upon this impossibility that his whole system is grounded,) are thrown to the ground by his own hand, when he admits that the price of products may rise, without an increase of gold, greater in proportion, than the increase of the products in question. We see now that it is only those who blind themselves to all knowledge of the nature and progress of wealth, or of positive values, by studying relative values, or price in gold as a commodity, who can with truth be accused of admitting contradictions; for they actually do maintain, that a rise of prices cannot take place, and, at the same time, insist that it can take place! Adam Smith, and all who have followed him, treated, although perhaps not very clearly, of the original first price of all things, the actual cost of production in labour; and they treated money price as being the index of real value, not as being estimated in gold as a mere commodity; and they saw clearly that this may undergo change; that at one time man may have a small sacrifice to make for his products, and at another a great; and that each of such changes would be a real rise, or fall of prices, whatever changes, commodities being adopted as standards of exchangable value, might happen to indicate. Mr. Ricardo, in supposing a rise of price with the necessity for expending more labour in procuring food, takes his first step in the right direction. But dazzled afterwards by the strange appearances presented, while viewing wealth through the distorting medium of relative values and commodity price, he was struck with the novelty of the exhibition; and proceeded, ever after treating the mirage thus produced, as if it were a reality.

II.—*On the Measure of Temperature, and the Communication of Heat.* *By* Messrs. Dulong *and* Petit.

PART II.

On the Laws of Cooling.

§ 3. *On Cooling in a Vacuum.*

The observations on cooling in a vacuum, calculated as we have just explained, are all affected by an error; small it is true, but which it is indispensible to correct. This error is occasioned by the small quantity of air which remains in the balloon, the elasticity of which, in the greater number of experiments, was less than would have supported a column of 3 millimetres, (,12 inch.)

It is not to the series of temperatures derived immediately from observation that this correction can be applied, but to the results deduced from calculation. It is, in fact, only necessary to diminish the latter by a quantity corresponding to the loss of heat occasioned by the residuum of air in the balloon.

To determine the amount of this correction in each case, we observed the rate of cooling of our thermometer in the balloon, as filled with air of different degrees of density, and we have calculated, for different excesses of temperature, the rate of cooling corresponding to each density. Subtracting again from these rates the values determined in a vacuum, we should have an exact measure of the quantities of heat carried off by the air in its different stages of rarefaction. We shall have, in fact, sufficiently exact determinations of these quantities, if for the rates in a vacuum we substitute the approximate rates obtained in the balloon, though containing a very minute portion of gas.

Having thus obtained the quantity of heat carried off by the air for each excess of temperature, we perceived that these quantities followed a simple law, by the aid of which we have determined, with sufficient precision, the corrections to be applied to the calculated rates. The numbers, therefore, which we shall present in the course of this section, may be considered as being exceedingly near what would be obtained from observations made in a perfect vacuum.

Let us now, then, turn to the consideration of the several calculated and corrected series; and to begin, let us take that in which the balloon was surrounded by ice at 0°, the thermometer with its natural glassy surface:

Excess of temperature over that of the surrounding mass.	Corresponding rates of cooling.
240°	10°,69
220	8 ,81
200	7 ,40
180	6 ,10
160	4 ,89
140	3 ,83
120	3 ,02
100	2 ,30
80	1 ,74

The first column contains the excess of temperature of the thermometer over the surrounding mass; the second, the corresponding rates of cooling, calculated and corrected as above explained. These rates, as we have had occasion to mention before, are the number of degrees, by which the temperature would be lowered in a single minute, supposing the rate of cooling to be uniform during that minute.

This first series is sufficient to show the inaccuracy of the law of Richmann; for by that law, the rate at 200 should be double what it is at 100°, and we see it is more than treble. In comparing again the loss of heat for an excess of 240 and one of 80°, the first appears to be about six times greater, whereas by the law of Richmann it ought only to be treble.

It would be easy enough to represent by a formula, consisting of two or three terms, the results contained in the preceding table, and thus empirically to determine the relation subsisting between the temperatures of bodies and the corresponding rates of cooling. But formulæ of this description, though useful for calculating intermediate results in the series which has furnished the law of interpolation; become almost always erroneous, when extended to cases without the limits of that series, on which they had been founded, and they ought, therefore, never to be considered as the expression of the true law of the phenomenon.

We have therefore deemed it obligatory on us, previously to seeking for any law, to vary our experiments as much as the nature of them would permit. The following consideration, which does not appear to have suggested itself to the mind of any enquirer, has happily directed us in the choice of circumstances calculated to exhibit the true nature of the problem.

In the theory generally adopted, of the distribution of heat, the cooling of any body in a vacuum is but the excess of its radiation over that of the surrounding bodies. Thus if we put t for the temperature of the surrounding matter within which any body is subjected to the cooling process, and $t + \mathrm{t}$ the temperature of this body, we shall have, generally, for an expression of the rate or velocity of cooling V (it being observed that this rate is 0 when $t = 0$.)

$$V = F(t + \mathrm{t}) - F(\mathrm{t})$$

F being used to designate the unknown function of the temperature which answers to the law of radiation.

If the functions $F(t + \mathrm{t})$ and $F(\mathrm{t})$ were proportional to their variables, that is to say, if they were of the form $m(t + \mathrm{t})$ and $m(\mathrm{t})$, m being a constant, the rate of cooling would be found equal to mt; and we should thus have the law of Richmann, inasmuch as the rates of cooling would be proportional to the excesses of temperature, at the same time that they were independent of the temperatures themselves, as indeed has been hitherto supposed. But if the function F be not proportional to its variables, the expression

$$F(t + \mathrm{t}) - F(\mathrm{t})$$

which represents the rate of cooling, will depend at once upon the excess of temperature t, and on the absolute temperature of the surrounding matter.

To verify this conclusion, we have observed the rate of cooling in a vacuum, by bringing successively the water in the tub, in which the balloon is immersed, to the temperatures 20°, 40°, 60°, and 80°. The following table presents, at one view, all the results of each of these series of observations, which have, besides, been repeated several times.

Excess of Temperature.	Rate of cooling: the surrounding matter being at 0.	Rate of cooling: the surrounding matter being at 20°.	Rate of cooling: the surrounding matter being at 40°.	Rate of cooling: the surrounding matter being at 60°.	Rate of cooling: the surrounding matter being at 80°.
240°	10°,69	12°,40	14°,35	......	......
220	8 ,81	10 ,41	11 ,98	......	......
200	7 ,40	8 ,58	10 ,01	11°,64	13° ,45
180	6 ,10	7 ,04	8 ,20	9 ,55	11 ,05
160	4 ,89	5 ,67	6 ,61	7 ,68	8 ,95
140	3 ,88	4 ,57	5 ,32	6 ,14	7 ,19
120	3 ,02	3 ,56	4 ,15	4 ,84	5 ,64
100	2 ,30	2 ,74	3 ,16	3 ,68	4 ,29
80	1 ,74	1 ,99	2 ,30	2 ,73	3 ,18
60	...	1 ,40	1 ,62	1 ,88	2 ,17

This table, which requires no explanation, confirms, as may be seen, the principle we have just attempted to establish. The results contained in it suggest a very simple relation, which has led us to the discovery of the law of cooling in a vacuum. If the corresponding numbers in the 2nd and 3rd columns are compared, that is to say, the rates of cooling for the same excesses of temperature, the surrounding mass being successively at Zero, and at 20°, the ratios of these rates will be found as follows :—

1,16 1,18 1,16 1,15 1,16 1,17 1,17 1,18 1,15.

These numbers, which differ but little amongst one another, and the differences of which do not appear regular, would not require a greater change in some of the rates, to become perfectly uniform, than about one-hundreth part.

Let us compare, in the same way, the rates of cooling, when the surrounding mass is at 20° and 40°. The following will be found to be the ratios of these rates.

1,16 1,15 1,16 1,16 1,17 1,16 1,17 1,15 1,16 1,16.

Again, let us compare the ratios of cooling, when the surrounding mass is at 40 and when it is at 60, we shall have

1,15 1,16 1,16 1,15 1,17 1,16 1,18 1,16.

Finally, for the relations of the rates, when the surrounding mass is at 60 and at 80°, we have

1,15 1,15 1,16 1,17 1,16 1,17 1,17 1,15.

The three last comparisons lead us to the same result as the first, and further instruct us, that the ratio between any two consecutive series is the same, whether the surrounding matter be at 0° and 20°; at 20 and 40; at 40 and 60; or lastly, at 60 and 80°. The preceding experiments may then be considered to establish the following law:—

The rate of cooling of a Thermometer in a vacuum, for a certain excess of temperature, increases in a geometrical progression, when the temperature of the matter which surrounds it, increases in an arithmetical progression. The ratio of this geometric progression is the same, whatever the excess of temperature.

This first law, which only considers a change in the temperature of the surrounding matter, will allow of our putting the formula, previously found for the rate of cooling, in a vacuum

$$F(t + \text{t}) - F(\text{t})$$

under the form,

$$\phi(t) \times a^{\text{t}}$$

a being a constant number, and $\phi(t)$ a function of the variable t singly which it is our object to discover.

The two expressions of the rate of cooling being equal, we have

$$\frac{F(t + \text{t}) - F(\text{t})}{a^{\text{t}}} = (t)$$

which being expanded into series, gives

$$\phi(t) = t\frac{F'(\text{t})}{a^{\text{t}}} + \frac{t^2}{2}\frac{F''(\text{t})}{a^{\text{t}}} + \frac{t^3}{2.3}\frac{F'''(\text{t})}{a^{\text{t}}} \&c.$$

and as this equation must hold good whatever the value of t, it requires, that

$$F(\text{t}) = m\, a^{\text{t}}$$

m being a number to be determined. Hence we deduce

$$F(\text{t}) = m\, a^{\text{t}} + \text{constant},$$

and consequently

$$F(t + \text{t}) = m\, a^{\text{t}} + \text{constant}.$$

Finally, therefore, we obtain for the velocity of cooling

$$V = m\, a^{t}(a^{t} - 1)$$

an equation which represents the law of cooling *in vacuo*.

If we suppose t to be constant, the coefficient $m\, a^{\text{t}}$ will be so likewise, and the preceding law might be expressed as follows:—

When a body cools in a vacuum, the surrounding matter being kept at a uniform temperature, the rate of cooling for excesses of temperature in arithmetical progression increases as the terms of a geometrical progression diminished by a constant number.

The ratio a of this progression is easy to determine, in the case of the thermometer, the cooling of which has been just observed; for when t increases by 20°, t being the same, the rate of cooling requires to be multiplied by 1,165, being the mean of all the preceding determinations. We shall then have

$$a = \sqrt[20]{(1{,}165)} = 1{,}0077^{1}$$

[1] A coincidence sufficiently curious to be remarked, although we have no intention of drawing any conclusion therefrom is, that the above number is very nearly the square of the coefficient of the expansion of a gas.

It now only remains, in order to verify the accuracy of the preceding law, to compare it with the different series included in the table we have given above. Let us begin with that in which the surrounding matter was at 0° ; in this case we must put $m = 2{,}037$, and we shall have

$$V = 2{,}073\ (a^t - 1)$$

in which $a = 1{,}0077$

Excess of temp. of thermometer, or values of t.	Observed values of V.	Calculated values of V.
240°	10°,69	10°,68
220	8 ,81	8 ,89
200	7 ,40	7 ,34
180	6 ,10	6 ,03
160	4 ,89	4 ,87
140	3 ,88	3 ,89
120	3 ,02	3 ,05
100	2 ,30	2 ,33
80	1 ,74	1 ,72

Let us now take the series observed when the surrounding mass was at 20°, the preceding coefficient of $(a^t - 1)$ must be multiplied by $a^{20} = 1{,}165$, we shall then have

$$V = 2{,}374\ (a^t - 1)$$

Excess of temp. or values of t.	Observed values of V.	Calculated values of V.
240°	12°,40	12°,46
220	10 ,41	10 ,36
200	8 ,58	8 ,56
180	7 ,04	7 ,01
160	5 ,67	5 ,68
140	4 ,57	4 ,54
120	3 ,56	3 ,56
100	2 ,74	2 ,72
80	1 ,99	2 ,00
60	1 ,40	1 ,38
40	0 ,86	0 ,85
20	0 ,39	0 ,39

We may now proceed to the series in which the surrounding matter was at 40°; the preceding coefficient of $(a^t - 1)$ must again be multiplied by $a^{20} = 1{,}165$, thus,

$$V = 2{,}766\ (a^t - 1)$$

Excess of temp. or values of t.	Observed values of V.	Calculated values of V.
240°	14°,35	114°44
220	11 ,98	2 ,06
200	10 ,01	9 ,97
180	8 ,20	8 ,17
160	6 ,61	6 ,62
140	5 ,32	5 ,29
120	4 ,15	4 ,14
100	3 ,16	3 ,17
80	2 ,30	2 ,33
60	1 ,62	1 ,61

For the series in which the surrounding mass is at 60°, we shall have

$$v = 3{,}222\ (a^t - 1)$$

Excess of temp. or values of t.	Observed values of V.	Calculated values of V.
200°	11°,64	11°,61
180	9 ,55	9 ,52
160	7 ,68	7 ,71
140	6 ,14	6 ,16
120	4 ,84	4 ,82
100	3 ,68	3 ,69
80	2 ,73	2 ,71
60	1 ,88	1 ,87

Finally, when the surrounding matter is at 80°, we have

$$v = 3{,}754\ (a^t - 1)$$

Excess of temp. or values of t.	Observed values of V.	Calculated values of V.
200°	13°,45	13°,52
180	11 ,05	11 ,09
160	8 ,95	8 ,98
140	7 ,19	7 ,18
120	5 ,64	5 ,61
100	4 ,29	4 ,30
80	3 ,18	3 ,16
60	2 ,17	2 ,18

The remarkable agreement of these results of observation and calculation will not allow of our doubting the truth of the law to which we have been led. Without stopping to trace all the consequences which may be deduced from it, let us proceed to examine the series made with the silvered bulb. When these series were calculated, we did not fail to perceive, in comparing them with the analogous series of the naked thermometer, that the rates of cooling of this latter, for the same temperature of the surrounding matter, and for the same excess of temperature, were proportional to the corresponding rates of cooling of the thermometer with silvered bulb; the formula, then, will equally apply to this sort of surface, preserving for a the same value, and merely diminishing m.

Our first observation on the cooling of the silvered thermometer was made when t was equal to 20°. We found that in this case $m = 0{,}357$, and consequently $m\,a^t = 0{,}416$, whence

$$V = 0{,}416\ (a^t - 1)$$

Excess of temp. or values of t.	Observed values of V.	Calculated values of V.
280°	3°,05	3°,11
260	2 ,59	2 ,61
240	2 ,18	2 ,18
220	1 ,83	1 ,81
200	1 ,53	1 ,50
180	1 ,26	1 ,23
160	1 ,02	1 ,00
140	0 ,81	0 ,80
120	0 ,62	0 ,62
100	0 ,47	0 ,48
80	0 ,34	0 ,35
60	0 ,24	0 ,24
40	0 ,15	0 ,15
20	0 ,07	0 ,07

So extensive a series as the preceding ought to be sufficient proof, that the formula which represents the cooling of the glass bulb, is equally applicable to the silvered bulb, and that a has in each case the same value. However, not to neglect any means of verification in our power, we have varied the temperature of the surrounding mass, and eventually carried it to 80°. The coefficient of $(a^t - 1)$ must then be multiplied by a^{60} which gives,

$$V = 0{,}658\ (a^t - 1)$$

Excess of temp. or values of t.	Observed values of V.	Calculated values of V.
240°	3 ,40	3°,44
220	2 ,87	2 ,86
200	2 ,35	2 ,37
180	1 ,99	1 ,94
160	1 ,56	1 ,58
140	1 ,27	1 ,26
120	0 ,99	0 ,98
100	0 ,75	0 ,76
80	0 ,56	0 ,55

The simplicity and the generality of the law just established—the accuracy with which observation confirms it, in an extent of nearly 300° of the thermometric scale, induces the belief, that it will strictly represent the progress of cooling *in vacuo* at every temperature, and for every substance.

Let us now return to the investigation which has led to the discovery of this law.

The radiation of the surrounding mass is represented by F (t), and we find its value to be

$$m\ at + \text{constant.}$$

Now the point from which the temperature t is reckoned, being arbitrary, may be so chosen that the constant shall $= 0$, which would reduce the preceding expression to the form $m\ a^t$. We may then conclude that if it were possible to observe the *absolute* cooling of a body *in vacuo*, that is to say, the loss of heat of that body without its receiving any from surrounding bodies, this cooling would follow a law in which the rate would decrease in geometrical progression, while the temperature decreased in arithmetical progression; and further, that the ratio of this progression would be the same for all bodies, whatever the state of their surfaces.

From this very simple law, it is easy to deduce that of the actual cooling of bodies *in vacuo*. In fact, to pass from one to the other, it is only necessary to allow for the heat parted with at each moment by the surrounding matter; this quantity of heat will be constant if the temperature does not vary; from which it follows, that the actual rate of cooling of a body in *vacuo*, for excesses of temperature in arithmetical progression, must increase as the terms of a geometrical progression diminished by a constant number. This number must itself vary in geometrical progression when the temperature of the surrounding mass (of which, in fact, it represents the absolute radiation,) varies in arithmetical progression. These different results are all clearly expressed in the equation just obtained. By putting $m\ a^t = M$, we have

$$V = M\,(a^t - 1)$$

M is the number to be subtracted from the different terms of the geometric progression expressed by $M\ a^t$, and we see, besides, that M depends upon t in the manner above explained.

Since the value of a is independent of the nature of the surface, it results that the law of cooling *in vacuo* is the same for all bodies; so that the radiating powers of different substances preserve the same relation at all temperatures. We have found this relation, or ratio, to be 5,7 in the case of glass and silver. This result is a little less than Mr. Leslie's, but the difference has been occasioned most probably by our silver having a dull surface, while his was polished.

We may also see, in supposing the law of absolute radiation to be expressed by the formula ma^t, that we must put $t = \infty$ to have the rate $= 0$, which would fix the absolute zero at infinity. This opinion, rejected by a number of enquirers, simply because it would lead to the conclusion, that the quantity of heat in bodies was infinite (their capacity being supposed constant)—becomes on the contrary more probable, now that we know that the specific heat diminishes with the temperature: for the law of the diminution may be such, that the integral of the quantities of heat, reckoned down to a temperature infinitely low, may yet have a finite value.

The law of cooling, such as we have just presented, and such as can be observed *in vacuo*, has reference to rates of cooling which are estimated from the depressions of the air thermometer. We may see from the comparisons of the other thermometric scales, which have been given in the preceding part of this enquiry, that in using any other sort of thermometer, the relations which we have discovered between the temperatures and the rates of cooling, would lose that character of simplicity and generality, which is ordinarily found to attach to the laws of nature.

If the capacity of bodies for heat were constant, as measured on the scale of the air thermometer, the preceding law would give the expression of the quantities of heat lost in function of the corresponding temperatures. But as we have proved that the specific heat of bodies is not constant on any thermometric scale, we see, that to obtain the real loss of heat, it is necessary to introduce one more element, that is to say, the change in capacity of the body submitted to experiment. In considering the question under this point of view, it would be necessary to determine the law which regulates the variation of capacity of a certain body, and afterwards to determine, by direct experiment, the quantities of heat lost by the same body at fixed points of temperature, as measured by the air thermometer. Then multiplying the rates of cooling deduced by the aid of the preceding law into the corresponding capacities, we should obtain the actual loss of heat. It is not, of course, in the interval of the first 200 or 300 degrees on the centigrade scale, that we could hope to verify these conclusions. As the variation in capacity only begins to be sensible beyond this limit, it would be necessary to push our observations as far as temperatures of 500 or 600°. We may easily conceive the difficulties of such an enquiry. We have, however, succeeded in constructing an apparatus which answers all the requisite conditions; and we have even made a great many experiments on the subject. But as our results do not as yet exhibit all the regularity which we hope to give to them, we shall defer the publication of them for the present.

The means which Mr. Leslie has employed to measure the radiating powers of different surfaces, are well suited to determine the quantities of radiating heat lost at different temperatures. It is known that this method consists in estimating the effect of radiation in any body, by the heating of an air or mercurial thermometer, placed at a certain distance from the hot body, and (in order to render the effect more sensible) in the focus of a reflector.

It was by means of such an apparatus that Laroche obtained the result we have before referred to. Amongst the series of observations then made, there is one which, in fact, includes very high temperatures, but which cannot be turned to any account, because the temperatures were determined by a procedure founded on the hypothesis of the capacities being constant. The numbers representing the loss of heat are affected by another error which proceeds from this; that the rise of the focal thermometer was already very sensibly too great, not to show the error of the law of Newton.[1] But to show that our law will even represent observations of this kind, when freed from the errors just noticed, we will apply it to the series reported in the same memoir, and which are within those limits between which the variation of capacity has but little influence. These series were made with a vessel of iron, full of mercury. In these the temperature not having exceeded 200°, we may suppose the specific heat constant. In like manner, we may neglect the correction which should be applied to the indications of the mercurial thermometer, in order to reduce them to those of the air thermometer, because in Laroche's experiments, the stem of the thermometer, not being immersed in the liquid, the observed temperatures must have been affected by an opposite error, equal, at least, to the above correction.

Instead of taking each series separately, as reported by this experimenter, we have used what may be termed mean results, being deduced from the formula by which Mr. Biot has expressed these observations, which will be found in the 4th

[1] This passage is obscure. The original is as follows: "Les nombres qui représentent les pertes de chaleur sont d'ailleurs affectés d'un autre erreur qui provient de ce que le réchauffment de son thermometre focal etait trop grand, pour que déja l'inexactitude de la loi de Newton ne fut tres sensible."

volume, p. 634, of his Treatise on Physics. The numbers which we present as the result of observation are, in reality, deduced from the formula of M. Biot. To express them according to our law, we must make V, which here represents the radiation, equal to

$$4,24\ (at - 1)$$

t being the excess of temperature of the vessel, and a a constant, the value of which we have determined to be exactly 1,0077.

Value of t.	Observed value of V.	Calculated value of V.
200°	15°,33	15°,29
180	12 ,51	12 ,52
160	10 ,09	10 ,15
140	8 ,04	8 ,11
120	6 ,30	6 ,36
100	4 ,84	4 ,86
80	3 ,60	3 ,58
60	2 ,54	2 ,47

The remarkable agreement found here also between calculation and observation, furnishes a new proof, that the number a depends neither on the mass, nor on the state of the body as to surface, since we again find, and under circumstances so very different, the same value, as in our experiments on cooling *in vacuo* of vitreous and silvered surfaces.

We may easily deduce from the expression for the rate of cooling *in vacuo*, the relation which connects the temperatures with the times. In fact, in designating the time by x, we shall have

$$V = V - \frac{dt}{dx} = M\ (a^t - 1)$$

M being a constant coefficient, which depends solely on the temperature of the surrounding mass. We may deduce then

$$d\,x = \frac{-\,d\,t}{M\ (a^t - 1)}$$

and $$x = \frac{1}{M\ \log.\ a}\left(\log.\ \frac{a^t - 1}{a^t}\right) + \text{constant.}$$

The arbitrary constant, and the number M, will be determined for each particular case, after the values of t are obtained, corresponding to two known values of the time x.

If we suppose t so small, that considering the value of the logarithm of a, we may confine ourselves to terms of the first power in the developement of a^t, we shall again have the law of Newton.

III.—*Some Account of Mandu, an Ancient Hindu City.*

To the Editor of Gleanings in Science.

Sir,

Mandu, a ruined city of prodigious extent, near Indore, and the former capital of Malwa, has never, I fancy, been so accurately described, as its truly magnificent remains would appear to deserve. Situated in a remote province, and distant from the road of the passing traveller, few, beyond those resident in the neighbouring cantonment of Mhow, have the opportunity of witnessing as melancholy a specimen, of what that great destroyer, time, has effected, as the face of the wide, habitable globe perhaps presents. Whilst far less interesting spots are almost daily receiving notice in some publication or other, the desolate state, and tenantless streets and Bazars of Mandu, once busy with the hum of trade and business, seem destined to continue in that condition, fate has ordained they should attain; and oblivion is desirous of casting over it its mysterious lot. Decreed to sink from its former celebrity, into nothing, will no adventurer place on record what it is? The moralizer might be led to sigh for the vanity of human wishes, and the ambitious learn a tale sufficiently full of woe and wretchedness to prove the folly of *his* pursuit. Its almost boundless walls, thirty-six miles in circumference, commanding situation, decaying palaces, mouldering mosques of marble, noble tanks, and broad endless causeways, overgrown with jungle, and difficult to trace, cross the visitor, at every turn; and force on his attention, what this place must have been,—its past greatness, and present desolation.—Uninhabited by one single human being from choice, its proud fortifications shelter none, but the untamable Bhíl; who, when pursued for acts of murder and lawless robbery, finds perfect protection within the numerous recesses of its fast decaying strength. The tiger and hyena range undisturbed within the very dwellings of those, formerly in contentment and affluence, yet now with their descendants, swept from off the earth; as if a plague had possessed the city, and left not a soul behind. This, surely is a spot on which the traveller might pause, and furnish for the curious, pages of matter, acceptable even to those not generally forward in encouraging labours of the kind. Once, though but for a few days, and that many years ago, I was a dweller within its vast precincts; and I feel much desire to excite some of the many now near it, (Mhow is about 20 miles off,) to supply your columns with an account of this remarkable and unfortunate town.—Such an article would not, I believe, be without the pale, to which your publication confines its labours; and I trust this endeavour to procure one, will succeed. I am hopeful too, that an enquirer could easily obtain sight of some manuscript notes, the work of Sir John Malcolm; which were the property of the Mhow Library, and very likely, still belong to it. Sir John resided for a considerable period in the neighbourhood at a village called Checaldra, and was a frequent pilgrim to its shrines. I remember his charming retreat particularly well. Within sight of a spacious lake, artificially constructed and embanked with enormous blocks of granite, did Sir John fix his home. Nothing could be more picturesque. The dwelling itself was a portion of a tomb. The centre apartment was made to contain a billiard table, and the surrounding arcades and verandahs, were ingeniously transformed into offices and retiring rooms. The modern alterations contrasted strangely with the Mahomedan style of architecture, and were not altogether pleasing to the eye, of course. A garden had originally surrounded the whole, of which only time had spared the walls, an ornamental building or two, and a few aged orange trees. On the flat roof of the house inhabited by Sir John, access to which was had by a narrow staircase from the outside, a small double roomed Bungalow had been erected for his sleeping in; and commanding from its elevation, more extensive views than could be obtained from the floor beneath. It was a good thought that placed this Bungalow where it stood, as far as coolness went, though it presented from without an appearance strangely out of character with the Mausoleum on which it was erected!

While making mention of this hot weather resort of the present Governor of Bombay, I must not forget to record that a tigress and two cubs, are said by the natives, to have been dislodged from this very abode, on the gallant General determining from choice to become its occupant. After serving for a series of unknown years to preserve from spoliation, the ashes it may be of some favorite Begum, the terror striking monarch of the neighbouring jungle seeks its solitude for shelter; when subsequently the aggrandizing European arrives, and disputes with the recent savage possessor that claim to its protection, which in reality was the right of neither.. Such are the rude uses—unforeseen base purposes and changes, things in this world are destined to serve and undergo. The lake itself, however, is necessarily the chief object on which the eye of the visitor will rest, while a sojourner at Checaldra. A certain kind of interest will even attach to the remains of past magnificence, whether the actual object under the beholder is abstractly beautiful or not. But while feasting the senses on this really lovely spot, no imaginary feeling of this kind is in the least requisite, to aid the pleasing impression it is sure of producing. The noble dimensions of the tank—its abundant, overflowing, clear transparent glassy waters, confined within their present ample limits, by bunds of massive workmanship; defying age even to shake their power; prove the lavish expenditure bestowed to create it, and suggests the probability of royalty itself even having once enjoyed its refreshing beauties. A small Island, almost floating, so light and buoyant does it rest among the waters, covered with ruins, and interspersed with trees, occupies the centre, and serves to break the view, and give variety to a scene which might otherwise have had a somewhat monotonous effect. The fallen arches of the bridge, by which the Island was reached, are now too broken to permit of traversing it, though the piers yet show themselves above the surface to mark the taste, which the architect of this fairy-scene possessed. Near to the entrance of this passage to the Island, numerous decaying mosques and tombs cover the ground, and some of these, the staff of Sir John altered into places of habitation for themselves, like that selected by himself, though the preparations to render them comfortable, were less visible than in his own. The humble village adjacent, which at present gives the traveller and his followers all the usual supplies requisite in an Indian camp, is built of stone; and holds a *hát,h* occasionally, when the Bhíl tribes descend from their lurking places in the hills to make purchases of cloth and some few other commodities; which simple as their wants are, still require the assistance of this little primitive market to satisfy. From one of these miserable naked, dark coloured beings, I obtained, by the offer of a single rupee, a bow and arrows, which few or any of them were unarmed with. In make, and partly in material, these weapons differed from those I have seen in the plains of Hindoostan, in having that portion elsewhere usually constructed of catgut, formed of a tough strip of bamboo or cane, while the arrows, instead of being pointed with three or four edged pieces of blunt iron, were armed with a flat blade, of about three inches long, by one and a half broad, thin and well polished, and certainly capable of inflicting a severe wound. There was less of ornament, than this description of arms are generally embellished with; yet they were neatly finished, very light, and ready at a moment's warning, to serve the purposes of those that use them. In calling to recollection the many beauties of Checaldra, I am induced to say, that Sasserám, on the new road to Benares, is the only place I was ever at, which bore any resemblance to it—ridiculously inferior of course, yet reminding me, I confess, of what I felt when viewing the former. I allude to a spot on the right hand side of the road, and not immediately within sight of it, a little beyond the town. In the opposite direction, but somewhat more out of the way, a prettier scene will reward the lover of the picturesque, though of a character more formal, and therefore very different to that which gave me such unalloyed delight in the wilds of Malwa. Ere the idler bids adieu to Checaldra, he will do well to enquire after a couple of guides, resident within its seldom disturbed repose, who for the promise of a trifling consideration, will gladly accompany him to Mandu, and prove of infinite use. These two men, formerly held recommendatory chits from their late patron, Sir John Malcolm, as intelligent active individuals, possessing great knowledge of the neighbouring localities, and capable of pointing out every thing worthy of observation. They will also, at the same time, prove entertaining companions, investing their descriptions with marvellous tales of better days; traditions handed down from ages long since, yet still current in the vicinity, and relating to that far distant period, when the deserted city swarmed with inhabitants; and in glory, riches, and pretensions, stood second to none in Hindoostan. As the "City of the Seven Kings," is approached from this quarter, to which, by the way, it must have been a kind of suburbs, the whole length of the road on either side is strewn with the scattered relics of fallen affluence, and appropriately enough, serves as an introduction for the spectator, to that far greater scene of devastation that lies before him. The mind is thus prepared for witnessing the complete display of utter ruin, which I have briefly alluded to in the preceding page, and of which I hope that some more intelligent scribbler than myself will soon give a fuller account than I can supply from an imperfect memory. When about two or three miles from the first gateway, attention is called to an amazing deep wide rent or chasm in the earth, on the left hand side of the path, down which, during the wet season, a waterfall of very considerable magnitude and volume, must descend. Carrying the eye along the dark shade of this dell, a peep can be procured of the country beyond: a valley, through which, the rocky, shallow, but clear and brisk bubbling Nerbaddah runs. While gazing on this distant view, the guides will entertain the time, by relating how a famous juggler, of great dexterity, once amused a king, by stretching across the widest part of this fearful deep, a rope, on which he danced with unconcern, to the wonder and delight of the thousands attracted to witness this daring feat. When in the centre, and over the most giddy height, the rash adventurer claimed reward—one of considerable magnitude was offered. "'Tis insufficient," was the answer; on being asked the amount of his expectations, "Your kingdom," was the reply; when the insulted monarch, enraged at the presumption this hold demand exhibited, drew his sabre, and dividing the rope at one desperate cut, dashed the vain, foolish mummer to the unknown bottom of the pit!

To give an idea of the ground on which Mandu stands, it will be sufficient to explain, that it is built on a point of land, separated almost entirely from the range (the extreme termination of which it is, in fact) forming one boundary to the valley of the Nerbaddah, and which is, except by a narrow strip, completely isolated. The position is very elevated; naturally of great strength, except in one part; and therefore enjoying almost every advantage an inland town can well possess.—It was along this narrow neck that I entered it. Of an altitude considerably less than the main land, you have to make a steep descent, somewhat diminished, however, by a heavy stone causeway or bridge of large dimensions, ending in a handsome gateway, strongly fortified and flanked, through which I passed, and found myself on the commencement of a broad road, gently ascending to the town above, and cut out of the hill, on the surface of which it is built. This road is paved, and the stones worn with ruts, tell what a bustling, busy, thronged thoroughfare it must have been. It is also bounded on the outside by a parapet, commanding the passage below, and having iron cannon, of rude construction, still lying in the embrasures.

On searching the summit, a sharp turn to the right placed me before another gate, which gives entrance to the place. Whether on this, or the former, I cannot just now call to recollection, but on one of them, I observed a slab of white marble, engraved with the date of a visit made by the Emperor Acber. It was very beautifully executed in the Persian character, and unlike what is usually seen in India—in *alto relievo.* I was told, by my guide, that the same curiosity hunter had removed this inscription some few months before, but was afterwards obliged to replace it by order of Sir David Ochterlony, who justly considered it an act of idle spoliation. I shall hereafter have occasion, however, to remark, that Sir David's own presence in Mandu was not entirely unaccompanied by a robbery of the same sort, or if different, more inexcusable than that I have alluded to. Returning to the last mentioned gate, a regular street commences, confined in breadth, but bordered with dwellings far less dilapidated than I could have thought. Bending my steps onwards, nothing unusual invites the stranger to pause, until a noble arch, directly across the street, commands a halt.—I think I never saw one of greater altitude, or more graceful curve. The light slender fluted columns, springing up from earth to heaven, and meeting above, exceeded all that I remember of the Gothic in my own country. After indulging my taste here for architectural beauties until the day was too far advanced to allow of farther investigation or delay, it became desirable to adjourn, where protection from the sun might be had; and accepting the advice of the guides, an airy abode was quickly selected, not very distant, and the common resort, I understood, of all the *Sáhib Lóg* frequenting Mandu. After ascending a long flight of broken steps to a handsome door way, lined with marble, and deeply cut with a border of extracts from the Koran, I discovered myself in a vaulted chamber, of an octangular shape, with windows of fret work, ingeniously carved. A cooler, or more agreeable residence could not have been chosen. In the absence of recollection regarding measurement, &c. I feel diffident in giving any statement of its real size. A lofty dome formed the roof, and the building itself was only a part of a quadrangle, the centre of the front side of which it was. That portion of it used for the holy offices of prayer, &c. was opposite, and formed the rear face. It was connected with the principal range (which I inhabited) by long and rather low arched colonnades. It was as if a series of verandahs had been erected parallel to, and joining one another, and supported on pillars, disposed apart, at equal distances. On two of the corners, high domed towers rested; and though every part of this edifice was more or less dilapidated, only a few yards of it were actually level with the earth. A richly traced pulpit, of pure white marble, may be seen in a neglected state, in the cloisters devoted to religious purposes. Flights of wild pigeons, alarmed by my presence, hastily quitted their nests; until again left to retake that possession of their choice, in which I unwillingly disturbed them. Around the apartment which I had made my home, just from the point where the dome commenced, a curious ornamental cornice of inlaid work encircled it, of a variegated coloured pattern: the distance was too great to allow of my speaking positively as to the material employed. Just outside my dormitory, a little to the left, and opposite, a long pole of cast iron protruding above the ground, some ten or twelve feet, was pointed out as having resisted every effort hitherto made to release it from confinement below. The purpose it was intended to serve, is as difficult to conjecture as its perfect immoveability (which the guides solemnly assured me was the case) is ridiculously untrue. Held by some invisible power, the impossibility of taking it away, though quite loose in the earth, was dwelt on as a proof of its magic origin, among my servants; who with the usual shout of *wah wah!* each shook the iron staff, and seemed perfectly convinced. It is always prudent to give up to the belief, on these occasions, which the ignorant about you so readily grant to fables of the kind. I have often indeed thought the loss which the better informed suffer by not being able to yield credence to such tales is frequently a source of chagrin and disappointment. To have disputed with my Ciceroni, the truth of their absurd assertions in this instance, would only have prevented them from showing me perhaps some other objects more worthy of my curiosity, than this resisting iron pole, thought by them to be of such wonderful attraction. On the first morning after my arrival, and when on the point of sitting down to partake of the only meal I ever relish in India—breakfast—my ears were somewhat very unexpectedly greeted by the mellow, musical sounds of a bell; such as the votaries of Hinduism use in worshipping their gods. Nor was the piercing shrill conch absent on this occasion to increase my surprise, in a spot where I had erroneously enough supposed no followers of that or any other religion dwelt. My guides, however, ever ready to furnish information, quickly satisfied me on this head, by pointing to a square brick building, not very far off; from the centre of which arose a couple of the usual whitewashed tapering temples, plainly of recent erection, and, to all appearance, indebted to some more modern artist, than any other thing about them could boast of having been built by. Their history was as follows:—The Rajah of a neighbouring province (Dhar) had in his household, as is usual, a family priest of great acquirements, who dreamt that Mandu was of Hindu foundation, though subsequently of Mahomedan conquest, and that an idol, much venerated by those of the former persuasion, was lying somewhere buried within its walls. To rescue the image from obscurity, was certainly an object; and while I don't exactly see how the founding of a college for the education of Pundits could tend towards discovery, such, at any rate, proved the consequence of the dream; and to this establishment, containing some ten or a dozen pupils, did I owe my interruption. It appeared the worthy dreamer himself was principal, and the expense attending his delusion was not spoken of as of trifling amount, to its pious, but far from affluent supporter; owing to provisions, &c. having to be carried to it every day from a Bazar, as distant as Checaldra. The only other temple for Hindu worship at present to be found within the extensive space these ruins cover, was showed us down a steep declivity, formed out of the almost perpendicular rock, which acts as a natural defence to the town. It consisted of two or three low roofed cells, out of which a spring of the purest water arose; various statues of Vishnu, &c. surrounded the platform immediately outside the entrance, which was itself laboriously carved in the usual style of Hindu embellishment. Beyond this nothing remarkable was to be seen here, though the excessive coolness of the atmosphere was too inviting, not to continue longer within its sacred precincts, than strictly speaking, the little it contained justified. The fact of the existence of this ancient shrine, dedicated to deities so hateful to those of the Mussulman belief, is a pleasing proof, that numerous sect, with all their bigotry and love of proselytism, were yet tolerant enough to permit an establishment, for the celebration of the rites appertaining to so idolatrous a creed, as that of Brahma.

During the heat of the day, when unable to range with any comfort far from my retreat, I paid a visit to an extensive mausoleum directly behind where I lodged. —It was entirely composed of marble, with its dome and windows in no way differing from the common plan of Mahomedan sepulchral buildings. In the middle, marked by two tastefully designed and sculptured tombs, repose the bodies of those to whose memory the whole had been consecrated. For about three or four feet round them, ran a singularly beautiful tesselated pavement of variously coloured marble; and one of these handsome stones it was, which had been torn up and taken away, that I regretfully referred to, when speaking of Sir David's presence here having been commemorated by an act, similar to what he himself strongly condemned in another case. It was particularly wanton to deface that which belonged to the dead; on whose perishing bones, so much care had been bestowed, by friends, to preserve them from oblivion. It was injuring a costly structure, until then quite perfect; and hastening the arrival of that decay, which had scarcely spared another edifice of a city confined by thirty-six miles in circumference, and exhibiting on its wide surface such another wreck of worldly vanity, as a terrible earthquake would leave, in a place, where so horrible a calamity had recently occurred. The poor ignorant guides, who had long observed how much this ornamental work was admired, (even if they were entirely ignorant of its beauties themselves,) detailed in their simple language by whom the spoliation had been committed, and seemed to lament it as I did. The hand of man was not indeed required to aid the unrelenting destroyer—time. Still further, with a view, no doubt, to preserve this massive pile from destruction, the whole was enclosed by a high wall, and the court yard within paved throughout, and planted here and there with a few stately trees.—There were also commodious colonnades for sheltering the resident Mulvis appointed to pray for the souls of the departed, and the open marble fret work of the gate and balustrades appeared to me unusually light and pretty, and contrasting with and therefore adding to the gloomy air and solemn purpose of the structure. Our Mahomedan predecessors in India, I think, have left behind them such specimens of their architectural style in this respect, as ought to give us a higher opinion of their taste than generally prevails.—If it be wanting in variety, and I acknowledge there is too much of sameness, their designs are always appropriate in themselves, and seldom fail to impress the spectator with an idea always in unison with that they are intended to create. During the half hour or so I loitered here, three wandering *Faqírs* entered, and with that unconcern, and I may say, sincerity of purpose, ever apparent in their actions, proceeded forthwith, and without regarding the presence of an intruder, to perform their several devotions. No *salâms* were spared—a light was quickly brought, the frankincense burnt fragrantly and sent its fumes high up, accompanied with prayers, when these self-elected ministers packed up the few trifling articles of apparel, which, on first entering, had been laid down without, put on their slippers, and once more left me to solitude and contemplation!

The short time circumstances allowed me to remain, did not allow of my continuing long enough at Mandu, to permit of visiting one half of the remains thickly scattered over its expanse. The palace of the dancing girls, its fort, tanks, and I may call them, lakes, though hurriedly seen, were sufficient to create in me a high idea of the luxury which must have prevailed among the inhabitants of a city, now possessed but by disciples of Hinduism, whose compulsory residence alone causes even them to continue in it. I would have gladly occupied weeks in searching out the curious midst this unexampled chaos of ruins, and have feasted on the many landscapes that may be seen from its walls, with all that enthusiasm I feel for a beautiful prospect: their extent and variety can hardly, I think, be exceeded any where. The eye can scarce survey the distance before him, or reach the bounds nature has placed to intercept his gaze!

IV.—*On Artificial Fountains. By* B. Bevan, *Civil Engineer, Leighton Buzzard.*

[*From the Technical Repository, vol.* iv. *p.* 245.]

To the Editor of the Technical Repository.

Dear Sir,

Observing in your useful Repository, No. 20, p. 140, some queries respecting the present mode of boring for water, and finding none of your correspondents have noticed the subject in the last No. I take the liberty of sending a few observations, in reply to the said queries, in case you may not be already furnished with more useful particulars.

From the manner in which the art of obtaining water has been often mentioned, it might be supposed that it might be practised with success in any situation; whereas the possibility of succeeding is confined to certain districts, and depends upon the succession of the strata and the elevation of the surface; requiring an impervious stratum of moderate thickness, resting upon an open stratum charged with water, from a source of greater elevation than the surface of the spot to be bored.

If the substratum is altogether porous, and open to the passage of water, it will be in vain to look for a supply above the natural outlet of the springs in the vicinity: and if the relative height of the situation, where water is required, to the out crop of the underlying porous stratum, should not be favourable, it will also be in vain to bore: but should the surface of the district below, have a substratum of clay or marl to a moderate depth, an aperture made through the clay, to the confined water below, will allow it to rise to the surface, and, in some instances, to rise many feet above the surface, up pipes properly fixed for that purpose[1].

At Cambridge, where this practice is very common, the substratum, after passing through the alluvial gravel and soil, is the chalk marl, or what is commonly called, in that place, *Gault*, and is about 120 to 130 feet in thickness, resting upon a stratum of sand saturated with water, the out crop of which is at a considerable distance, and much above the level of Cambridge: any opening, therefore, through this marl or *Gault*, will allow the confined water to rise; but if no further precaution was used, it would reach the surface indeed, but coming to the alluvial ground, would be diffused in that reservoir of water constituting the more superficial springs, which are often, without any good reason, called land springs: therefore, to secure a supply at the surface, it is essential to cut off all communication with these higher springs; and this is usually done by inserting an iron pipe several feet into the water-tight stratum, and puddling or ramming strong clay round the lower pipe, and continuing the pipe, by additions, as may be required, until it reaches the intended place of delivery. The hole is then bored through the clay, with an auger of about four inches in diameter; and when cleared of the mud and softened clay, produced by the operation of boring, a tin tube, as large as the hole will admit, is passed from the top to the bottom, to keep open the aperture first made, and afterwards the fountain head is fixed upon the iron pipe.

The boring rods are generally about one inch square, of bar iron, screwed together in lengths of from 6 to 12 feet.—Sets of joints may be obtained at any general iron factory, and welded to bars of any length.—The cost of rods and joints, ready for use, will be about 5*s.* a yard; and the total expense of the operation, including the use of all the implements, and iron and tin pipes, to holes of 130 feet deep, is about £25.

It occasionally happens, that thin beds of rock are formed, in boring, too hard for the auger; these are cut through by a piece substituted for the auger, in the shape of a chisel; worked continually round, in a stamping motion, until the rock is perforated: this tedious process sometimes requires 2 or 3 days to pass a rock of 8 or 9 inches in thickness.

The quantity of water produced at one of these apertures, lately made at Cambridge, is regularly 12 gallons per minute; and another finished this week, full 11 gallons per minute.

I am, Sir, &c.
B. B.

V.—*On the* Chirù *or Antilope* Hodgsonii.

To the Editor of Gleanings in Science.

SIR,

My attention having been recently called to a notice of the *Antilope* Hodgsonii (the *Chirù* of Tibet) which appeared, in the 5th number of the Gleanings, so long ago as April 1829, and the person who was kind enough to direct me to this notice, having expressed a doubt as to the forthcomingness of the description and skin of that most rare and beautiful animal, which were furnished several years back to Dr. Clarke Abel[2], I have been induced to search for the rough notes whence the description given to Dr. Abel, was taken, and to put those notes into shape again, for your Journal, in case you will do me the favor to publish them.

So long ago as 1816, I think, Captain Latter, of Titalía, surprized the world with the announcement, that the "Unicorn" of Scripture existed in Tibet, where it was

[1] It is worth remarking, that this is the exact state of the case in Calcutta, where a bed of clay 60 or 70 feet in thickness lies below the upper springs—while the level of the place is lower than any part of the country.—ED. GL.

[2] Dr. C. Abel was at that time Secretary to the Physical Class of the Asiatic Society. Sometime before his death, he had nearly ready for publication, descriptions of a great number of birds and animals. What is become of Dr. Abel's papers?

ANTILOPE *HODGSONII.*

Drawn and printed on Stone by J.B. Tassin. 99 Durrumtollah. Calcutta

known by the name of *Chíru.* The Residency had not been long established at Cát,hmándu when endeavours were made to ascertain the truth of this story: but the ludicrous game that was sometimes started during this quest, (a small horse with a corneous prominence in the middle of its forehead, caused by disease, was once brought in, from a distance of some 400 miles,) the experienced recklessness of Bhóteah asseveration, together with the continued non-appearance of the real object of enquiry, at length caused the abandonment of the search. In 1822, however, or thereabouts, the Bhóteahs, to revive our curiosity, produced a singular horn, which they avowed was that of the *Chíru;* and added, dauntlessly, that the *Chíru* was a unicorn of the deer kind. Unluckily for them, the bend of the horn proved it evidently to be one of a pair; the core also, at the base of the horn, demonstrated it to belong to an antelope; and Mr. Du Vaucel, to whom I sent, some short while afterwards, several of these horns, very justly preferred the analogy of nature to Bhóteah assertion, and laughed at the idea of this noble weapon resting on the suture of the skull.

Mr. Du Vaucel, however, agreed with me, that the horn produced was a novelty, and probably belonged to some very noble antelope. I therefore persevered in my endeavours to obtain it; and at length, after great pains, was only able to realize my hopes, through the kindness of the first minister of this state. In 1824 or 25, (as far as I remember) General Bhím Sén sent a live *Chíru* to the Residency, which he informed us he procured from the Lama of Digerchi, whose pet the *Chíru* had been from youth upwards: and it may serve to show the difficulty attending the obtaining of this species, to add, that I have never since been able to get another alive or dead. Yet, as I am led to believe, this rare, new, and beautiful animal, though carefully and fully described by Dr. Abel and myself, is still, after a lapse of 5 years, unknown to Natural History—both specimen and descriptions having probably been wrecked by the untimely death of my friend Dr. Abel. After these prefatory remarks, which were necessary to explain my reasons for recurring to the subject, I proceed to describe the *Chíru.*

ANTILOPE HODGSONII.

The *Chíru* Antelope.

Habitat Tibet.

Specific Character.—Antelope, with very long, compressed, tapering, suberect [1 sublyrated ?] horns, having a slight concave arcuation forwards, and blunt annulations (prominently ridged on the frontal surface) except near the tips: a double coat, throughout, grayish blue internally, but superficially, fawn colored above, and white below: a black forehead, and stripes down the legs; and a tumour or tuft above either nostril.

This most rare and beautiful animal measures, in extreme length, about five feet; and rises, to a height, at the shoulder, of from 2½, to 3 feet. He belongs to the more elegant section of the graceful genus Antelope—or that which more resembles, in figure and form, the Cervine than the Goat kind. In his well proportioned frame are exhibited every requisite for extraordinary exertions of speed, and for not inconsiderable ones of strength. His limbs are long and fine, but not weak: his neck, rather elongated and slender: his head, well formed and illustrated with the matchless eye of the Gazelle, but somewhat deficient in tapering downwards, owing to the nasal tufts, and to a rather unusual quantity of hair and bristles about the mouth and nose. In his ordinary attitude, the line of his back is nearly horizontal; the neck is bowed outwards and downwards, so that the head is carried not much above the line of the back; and there is a stoop in the hind legs whereby, though they are rather longer (and stouter too) than the fore legs, the hind quarters are not perceptibly raised.

The ears and tail are moderate, well formed, and devoid of any peculiarity: so likewise are the suborbital sinuses. The horns are exceedingly long; measuring, in some individuals, nearly two feet and a half. They are placed very forward on the head, and may be popularly said to be erect and straight, though a reference to the specific character will show that they are not strictly one or the other. Full as the specific character is on this point, a few additional words may be well devoted to it.

1 This word is bracketed, and followed by a note of interrogation, because I am doubtful of its correct applicability?

The general surface of the horns is smooth and polished; but its uniformity is broken by a series of from 15 to 20 rings, extending from the base, to within 6 inches of the tip of each horn. Upon the lateral and dorsal surfaces of the horns these rings are little elevated, and present a wavy rather than a ridgy appearance: but upon the frontal surface, the rings exhibit a succession of heavy, large ridges, with furrows between. The annulation is no where acutely edged. The horns have a very considerable lateral compression towards the base, where their extent, fore and aft, is nearly double of that from side to side. Upwards, from the base, the lateral compression becomes gradually less, and towards their tips the horns are nearly rounded. Compared with their length the thickness of the horns is as nothing—in other words, they are slender, but not, therefore, by any means weak. The tips are acute rather than otherwise. The divergency at the points is from one-third to one-half of the length. At the base a finger can hardly be passed between the horns. Throughout five-sixths of their length from the base, the horns describe an uniform slight inward curve, and at the top angle of the curve, where it returns towards[2] the perpendicular of the base, they turn inwards again, more suddenly, but still slightly; the points of the horns being thus directed inward, and the two horns exhibiting, when seen from the front, the outline delineated in the accompanying sketch. The lateral view of the horns shews a considerable concave arcuation forwards, but chiefly derived from the upper part of the horns. The *genitalia* of the male are exceedingly small, and not at all pendant. In size the testicles exceed not a pigeon's egg: they are perfectly hairless, and black.

Close to the outer margin of either nostril is a singular, soft, fleshy, or rather skinny, tumour or tuft, about the size and shape of the half of a domestic fowl's egg. The purpose of these tufts I cannot discover: but the above is an accurate description of their appearance and feel; and as they would seem to be peculiar to this species, the mention of them may serve to characterise it.

The mouth and nostrils (as already noted) are unusually hairy and bristly, and from the lower lip depend a few hairs, but not in the fashion of a beard. There are, (of course,) 8 front teeth in the lower jaw.—The lateral ones, in the specimen I examined, were worse formed than the rest, and leaned inwards against them. The coat or covering of the *Chìrù* is very peculiar, and like that of all the other Trans-himaláyan animals I have seen, distinctly indicative of the extreme coldness of his habitat. It consists of two parts—an external or hairy (so to speak) and an internal or woolly, substance. The hair is about 2 inches long, and so thickly set on as to present to the touch an impression of solidity. It is straight, nearly erect from its close set, rather harsh, and feeble, being for the most part hollow, like a quill. Beneath this hairy coat lies, closely applied to the skin, a spare fleece of the softest and finest wool—a remarkable provision against the cold with which nature has furnished all the hairy animals of Tibet, without exception, not merely its goats and sheep, but its dogs, horses and kine.

The *Chíru* Antelope is one of the most gregarious species of the genus—being usually found in herds of several scores, and even hundreds. It inhabits, I am informed, the plains of Tibet generally—but, as others acquaint me, only such plains as are within sight of mountains, and preferentially of the Himachal mountains. It is extremely addicted to the use of salt in the summer months, and at that season vast herds of *Chírus* are often seen at some of the rock-salt beds, which so much abound in Tibet, licking the salt. The *Chírus* are said to come to the salt-beds under the conduct of a leader, and to post sentinels, ere they advance to feed, all around the beds; which sentinels, upon the least unusual appearance, give the alarm, when the herd immediately retires, as it had advanced, under the master buck.

The *Chíru* is universally said to be in the highest degree wild, and unapproachable by man—shy to the last degree, but not therefore timid. To avoid man he is wont to rely chiefly on his wariness and speed; but if these fail him, he meets danger with a gallant bearing; and but few indeed of the inhabitants of Tibet have either courage enough to push him to extremity, or weapons adequate to slay him at safe advantage. The one we had alive at the Residency, though tamed in infancy, was thoroughly fearless, and could only be approached with caution. The male and female of the species are said to resemble each other almost entirely. Ours was a well grown young male, and I have not myself seen a female. This species

[2] Not to, but only towards the perpendicular; the divergency of the horns preventing a return to the true vertical line.

cannot endure even the moderate heats of the valley of Nepâl, and the Lama of Digerchi's pet, though familiar with confinement, and most comfortably lodged and fed, died at the very commencement of the hot season, before he had been with us above a month, and while the *maximum* of Fahrenheit, in the stable he tenanted, was only 80° ; a temperature, besides, seldom reached for 2 hours a day, or for 2 days of that month, (March.)

I shall conclude this account with a detail of the dimensions and colours of the individual above noted, which I examined alive, and after death forwarded its skin to Dr. Abel, who added some technical particulars to my account, and did me the honor to name the species after me. Grey blue is the general colour of the hair throughout nine-tenths of its extent, from root to tip, as well as exclusively so of the wool beneath the hair. This radical and prevalent color is, however, but dimly seen through the external or superficial hues with which it is overlaid ; and which hues, upon the superior parts of the animal, are, pale fawn red ; upon the inferior parts and insides of the limbs, and ears, white. The shoulders are faintly marked by a tracing of color lighter than that of the surrounding parts. Down the fronts of all the 4 legs, runs a black line, reaching from the body to the hoofs in the case of the two forelegs, but to the knees only in regard to the two hind ones.[3]

The forehead is perfectly black, and a fringe of the same hue proceeding from the bottom of the frontal skin passes round the outsides of those tufts which I have mentioned as being placed beside the nostrils. These tufts, as well as the rim surrounding them, are black, so also are the bristles of the mouth and lips : but those few depending from the lower lip are white. The lips themselves (upper as well as lower one) are white. The roof of the mouth is colourless : the horns and hoofs, full black : the *irides* dark brown : the genitals black.

Dimensions.

	feet.	inches.
Entire length,	4	11
Length, minus tail,	4	2½
Ditto, minus head and tail,	3	6½
Ditto of the neck,	1	1
Utmost girth of the body,	3	3
Depth of the chest,	1	0
Height of animal, (shoulder,)	2	8
Ditto of fore leg, (to line of belly,)	1	8
Ditto of hind ditto, (ditto ditto,)	1	9
Head, length of,	0	11
Ditto, utmost girth of,	1	6
Length of horns,	2	0½
Basal, depth of ditto, (fore and aft,)	0	2⅛
Ditto width of ditto, (side to side,)	0	1¾
Length of ears,	0	5½
Ditto of tail, (end of hair,)	0	9
Ditto of hair or coat,	0	2

VI.—*Interesting Experiments with Canal-Boats.*

[From the SCOTSMAN Newspaper.]

We regard the experiments described below as extremely important. If the result is correctly stated, and if no counteracting disadvantage has escaped notice, we think these experiments may be said to have added a million sterling to the value of canal property in Great Britain, since they must, at no distant period, add fifty or a hundred thousand pounds to the annual dividends. Nothing can be more paradoxical or startling in appearance than this result ; and yet our knowledge of the many unexpected truths in mechanical science which experiment has brought to light, will not permit us to reject it as incredible. It is this ; *that the surge generated in a canal, by the motion of a boat, and which is so destructive to the banks, in*

[3] Since writing the above, I have procured another skin of a very old male. The ruddy tint of the upper surface is in this specimen nearly merged in hoary grey upon the neck ; hind head and buttocks behind, and the black lines of the legs reach to the hoofs in all 4 legs. The white of the insides of the ears and belly not white, but whitish only, with a subtint of buff.

moderately rapid motions (such as 4 or 5 miles an hour), *ceases altogether when a high velocity is employed.* It is true the vessels were of a particular construction, but this is immaterial. A boat 60 feet long and 5 feet wide, is capable of being extremely serviceable, both for the conveyance of goods and passengers ; and if such a boat can be safely and conveniently dragged at the rate of 9 or 10 miles an hour upon our canals, passengers, by this species of conveyance, will be upon a level as to speed with those who travel per mail. The great recommendations of canal carriage at present, are its cheapness, and the liberty of locomotion which passengers enjoy. Its leading disadvantage is its slowness ; and this is felt now more and more ; when our stage-coaches are touching a speed of ten miles an hour, which will soon be doubled on our railways. We have not technical skill enough to know what a gig-boat is ; but we infer from the other particulars stated, that it must be flat bottomed in the cross section, pretty well curved upwards at stem and stern, and very light. With this form, the quicker it is moved, the less water it will draw. At a very high velocity, it will merely skim the surface as it were ; the displacement of the fluid will reach only a few inches down ; and this circumstance, with the quick motion of the boat, causing a readjustment of the equilibrium of the water equally rapid, the necessary time will be wanting for the motion to propagate itself beyond the narrow zone of water which immediately encompasses the boat. Such is our hypothesis, supposing the fact to be as stated. We have a strong impression, however, that the result depends chiefly on the form of the boat, and that a much greater breadth than five feet will be no material disadvantage, except where the canal is extremely narrow.

Some months ago, by the suggestion of Mr. William Houston, of Johnstone, the Committee of Management of the Ardrossan and Paisley Canal were induced to make certain experiments for ascertaining the rate of velocity at which a light gig-boat might be propelled along that Canal. The experiments were made with a gig rowing boat, of about 30 feet in length, constructed by Mr. Hunter, boat builder, Brown Street, Glasgow ; and this boat, with 10 men on board, was drawn two miles along the Ardrossan or Paisley Canal, in the space of less than 10 minutes, without raising any surge or commotion on the water—the force employed being one horse, rode by a canal driver. No account of this trial has ever been given to the public ; but it was so satisfactory as to induce the Committee of the Ardrossan Canal to contract with Mr. Wood, of Port-Glasgow, for a gig-shaped passage boat, 60 feet in length, and 5 feet in breadth, fitted to carry from 36 to 40 passengers. In the month of April last, a number of experiments were made in the Forth and Clyde Canal with two gig-boats fixed together, constructed by Mr. Hunter, and thus forming what is called a twin-boat. The object of these trials was to ascertain the rate of speed at which vessels might be propelled along that Canal, and the effect of a light, double or twin-boat, in giving that degree of steadiness which it was apprehended would be so much wanting in a light single boat. A statement of these experiments on the Forth and Clyde Canal, has already appeared in the newspapers, and the only fact therein mentioned, which it seems necessary to repeat here, is the remarkable circumstance, that the quicker the boats were propelled through the water, the less appearance there was of a surge or wave on the sides of the Canal. The result of the experiment was so satisfactory, that a twin-boat, of a gig shape, sixty feet in length, and nine feet broad, is at present building by Mr. Hunter, Brown Street, Glasgow, and will be launched in the Forth and Clyde Canal in the course of the present month.

The single gig-shaped passage boat, contracted for by the Ardrossan Canal Committee, was launched at Port Glasgow on Wednesday se'ennight, the 2d of June, and she was towed up to the Broomielaw, and thence carried to Port Eglinton the day following ; and on Friday, the 4th June, a trial, of which the following is an account, took place. The boat is sixty feet long, four feet six inches breadth of beam, and drew on an average, including a deep keel, ten inches when light :—

From the great hurry in which this trial was made, it was done under many disadvantages. The boat started from Port Eglinton for Paisley a few minutes after one o'clock, with 20 persons on board ; and the distance from Port Eglinton to Paisley being 7 miles, was accomplished in one hour and seven minutes. The rider was ordered to start and proceed the first mile or so at a very moderate pace ; but even at this moderate pace the wave raised in front of the boat was very considerable. A high wave was seen on the canal preceding the boat, about eighty or ninety feet in front, and in some cases farther, and causing an overflow at the bridges, and in the narrow parts of the canal. The surge or cutting wave behind the boat was, however, comparatively slight, and except at the curves, would not have caused much injury to the canal banks. The horse was very much exhausted when he got to Paisley, though by no means so exhausted as he was about the middle of the journey, having sensibly recovered after the first four or five miles.

Two post horses were hired there; and lighter towing lines being attached to the boat, it started again, on its return to Glasgow, with twenty-four persons on board, four of whom were boys, and arrived at Glasgow, a distance of 7 miles, in forty-five minutes. The greatest speed attained during the journey was two miles in eleven minutes. During this voyage the surge behind was entirely got quit of, even at the curves, where it was reduced to nothing; and there was no front wave, except at the bridges. It appeared only at the bridges, and just as the boat was about to enter under the bridge, and gradually disappeared as the stern of the boat cleared the bridge. *The quicker the boat went, the more entire was the disappearance of all wave and surge*, except where the water escaped in the centre of the canal, and met in two very noisy and rapid currents from each side of the boat at the rudder. This noise and rush of water was so great behind, as to induce persons on board to look round, expecting to see a great wave or surge on the bank of the canal, but on the banks there was hardly a ripple. The two rapid noisy currents seemed to be completely spent and exhausted by the shock of their concourse behind the boat. Here, therefore, there was no room to doubt of the correctness of the reports of the Forth and Clyde Canal experiments. It was not merely to be said that the greater the speed the less the surge or wave, but it was demonstrated that, *at a high rate of speed, surge and wave were done away with altogether.* Unluckily, there was no Dynamometer attached to the rope, so as to ascertain whether, contrary to all theory, the strain or pull was not equally diminished with the wave, and the tugging labour of the two horses lessened instead of increased, by the accelerated rate at which they drew the boat. There can be no doubt, however, that with one trained horse, properly attached, the distance could be done in a period under 40 minutes. Contrary to expectation, Mr. Wood's boat was quite steady in the water, and by no means crank. It may be proper to mention that the Ardrossan Canal is throughout very narrow; at the bridges and many other places it is only nine feet broad. It has a great number of turns, and many of them very sudden.

VII.—*On determining the Height of a place from Observations of the Depression of the Horizon.* *By Lieut.* R. Shortreed, *Bom. Est.*

There are various well known methods of finding the height of a place above the level of the sea. The following method has not yet been generally put in practice; but from several trials I am disposed to consider it capable of considerable correctness. Having a number of stations within sight of the sea, it occurred to me that their elevation might be found very simply, and independent of other observers, by measuring the depression of the sea.

I resolved, therefore, to try it along with the other methods by barometer, and by a series of elevations and depressions, from station to station, and for this purpose calculated the following table.

Let be the radius of the earth $= r$, and in order to connect the height $= h$, with the angle of depression $= d$, we have $\cos. d = \frac{r}{r + h}$ whence $r \cos. d + h \cos. d = r$ and $h \cos. d = r - r \cos. d = r\ (1 - \cos. d)$ and dividing, $h = \frac{r\ (1 - \cos. d)}{\cos. d}$

$$= r \frac{2 \sin.^2 \frac{1}{2} d}{\cos. d} = r\ \frac{2 \sin. \frac{1}{2} d \sin. \frac{1}{2} d}{\cos. d} = r\ \frac{\sin. d \sin. \frac{1}{2} d}{\cos. \frac{1}{2} d \cos. d} = r \tan d \tan \tfrac{1}{2} d$$

by which formula the table is calculated. I have taken $r = 20921850$ feet as given in the 1st Vol. of the Trig. Survey of England and Wales—the logarithm of which $= 7{,}32060.00$. When the depression does not exceed $10'$ or $15'$ with a tolerable instrument, this is probably one of the easiest and most correct modes of finding the height of a place. When the depression exceeds 1 degree, the probability of error is much encreased by the uncertainty of refraction—which, however, is the only real difficulty in this method. I may mention that I have found the refraction vary from $\frac{1}{7}$ to $\frac{1}{20}$ of the contained arc.

Shortly after my attention was turned to this subject, I found a paper on it in the Edin. Phil. Journal, Vol. 2, by Mr. Scoresby, from which it appears that he had not clear views of the mode of its application. He proposes to have a table calculated to 20000 or even 30000 feet, which being far above the highest peak of the Himalaya, is practically useless. With a given height he finds what ought to be the depression, but does not attempt to solve the converse of the problem, which, however, is the only one practically useful.

I have carried the table to every minute of the first 2 degrees, which is as high as it is ever likely to be used. Humboldt, from the Peak of Teneriffe, found the depression to be nearly 2 degrees all round. The barometrical height 12,358 feet corresponds to a depression of 1°58′ 05″.

Table of Elevation in feet for depression of the Sea, for each minute of the first 2 degrees.

Depr. of Sea.	Elevation in feet.	Diff. for 1′.	Depr. of Sea.	Elevation in feet.	Diff. for 1′.	Depr. of Sea.	Elevation in feet.	Diff. for 1′.	Depr. of Sea.	Elevation in feet.	Diff. for 1′.
′			′			° ′			° ′		
0	0,000		30	796,7		1.00	318,7		1.30	717,2	
1	0,885	8,885	31	550,7	54,0	01	329,4	10,7	31	733,2	16,0
2	3,501	2,656	32	906,4	55,7	02	340,3	10,9	32	749,4	16,2
3	7,966	4,425	33	994,0	57,6	03	351,4	11,1	33	765,8	16,4
4	14,163	6,197	34	1023,3	59,3	04	362,6	11,2	34	782,4	16,6
5	22,129	7,966	35	1084,4	61,1	05	374,0	11,4	35	799,1	16,7
6	31,867	9,738	36	1147,2	62,8	06	385,6	11,6	36	816,0	16,9
7	43,073	11,506	37	1211,8	64,6	07	397,4	11,8	37	833,1	17,1
8	56,650	13,277	38	1278,2	66,4	08	409,4	12,0	38	850,4	17,3
9	71,698	15,048	39	1346,4	68,2	09	421,5	12,1	39	867,8	17,4
10	88,516	16,828	40	1416,3	69,9	10	433,8	12,3	40	885,5	17,7
11	107,10	18,589	41	1488,0	71,7	11	446,3	12,5	41	903,3	17,8
12	127,46	20,36	42	1561,5	73,5	12	459,0	12,7	42	921,3	18,0
13	149,59	22,13	43	1636,8	75,3	13	471,8	12,8	43	939,4	18,1
14	173,49	23,90	44	1713,8	77,0	14	484,8	13,0	44	957,8	18,4
15	199,16	25,67	45	1792,6	78,8	15	498,0	13,2	45	976,3	18,5
16	226,6	25,44	46	1873,2	80,6	16	511,4	13,4	46	995,0	18,7
17	255,8	29,2	47	1955,5	82,3	17	524,9	13,5	47	1013,8	18,8
18	286,8	31,0	48	2039,6	84,1	18	538,6	13,7	48	1032,9	19,1
19	319,5	32,7	49	2125,5	85,9	19	532,5	13,9	49	1052,1	19,2
20	354,1	34,6	50	2213,1	87,6	20	560,6	14,1	50	1071,5	19,4
21	390,4	36,3	51	2302,5	89,4	21	580,9	14,3	51	1091,1	19,6
22	428,4	38,0	52	2393,7	91,2	22	595,3	14,4	52	1110,8	19,7
23	468,3	39,9	53	2486,7	93,0	23	609,9	14,6	53	1130,8	20,0
24	509,9	41,6	54	2581,4	94,7	24	624,7	14,8	54	8150,9	20,1
25	553,2	43,3	55	2677,9	96,5	25	639,7	15,0	55	1171,2	20,3
26	598,4	45,2	56	2776,2	98,3	26	654,8	15,1	56	1191,6	20,4
27	645,3	46,9	57	2876,2	100,0	27	670,2	15,4	57	1212,3	20,7
28	694,0	48,7	58	2978,0	101,8	28	685,7	15,5	58	1233,1	20,8
29	744,4	50,4	59	3081,6	103,6	29	701,3	15,6	59	1254,1	21,0
30	796,7	52,8	1.00	3187,0	105,4	1.30	717,2	15,9	2.00	1275,3	21,2

Use of the table. Correct the observed depression of the sea for refraction, and with this enter the table, and you have the height corresponding to the nearest minute. The 3d column contains the differences for one minute of depression, by which the height may be corrected for the odd seconds, if any.

I have added another table[1] for every 10 seconds, as far as 30 minutes, which may probably be of use to those who have occasion to measure moderate eleva-

[1] This table has been by accident mislaid; should we succeed in finding it, we shall give its contents in another number. Ed. Gl.

tions on the coast. These tables may be copied into a blank leaf of Lalande's small Longitude Tables, and will be found very convenient for reference in that form.

The square of the observed depression in minutes gives the height in feet when refraction is $\frac{1}{17}$. But if refraction be different, the number so found requires a correction: when

$$\text{ref.} = \tfrac{1}{7};\ d^2 + \frac{d^2}{5} = h:\quad \text{ref.} = \tfrac{1}{10};\ d^2 + \frac{d^2}{11} = h:\quad \text{ref.} = \tfrac{1}{13};\ d^2 + \frac{d^2}{25} = h:$$

$$\text{ref.} = \tfrac{1}{8};\ d^2 + \frac{d^2}{6} = h:\quad \tfrac{1}{11};\ d^2 + \frac{d^2}{14} = h:\quad \tfrac{1}{14};\ d^2 + \frac{d^2}{37} = h:$$

$$\text{ref.} = \tfrac{1}{9};\ d^2 + \frac{d^2}{8} = h:\quad \tfrac{1}{12};\ d^2 + \frac{d^2}{20} = h:\quad \tfrac{1}{15};\ d^2 + \frac{d^2}{60} = h:$$

$$\text{ref.} = \tfrac{1}{16};\ d^2 + \frac{d^2}{123} = h:$$

The results so obtained seldom differ more than a few feet from the truth.

If you think the foregoing worthy a place in your Journal, I shall probably send you a few more papers. Some of them on Meteorology, and on the mode of finding the dew point from a wet and dry thermometer, which, notwithstanding the valuable papers in Nos. 2, 3, and 7, I do not think is yet exhausted.

Prime numbers have long been an object of interest to Algebraists. I discovered the following theorem about ten years ago, and shall feel obliged if any of your correspondents can furnish a demonstration.—n being any prime number $2^n - 1$ is a prime. The highest prime generally known at present is $2147483647 = 2^{31} - 1$ discovered by Euler, and is only a particular case of my formula—$137438953471 = 2^{37} - 1$ is a prime much higher than Euler's; and in this way primes may be found to any extent.

Poona, 18th Sept. 1830.

VIII.—*Proceedings of Societies.*

1. Asiatic Society.

Wednesday, the 1st September.

Sir Charles Grey, President, in the Chair.

Captain Benson was elected a Member. A letter was read from the Secretary to the Society of Arts, &c. acknowledging the receipt of the 16th volume of the Researches, and presenting the 47th volume of their Transactions. The following donations were received; viz. The leaf and fruit of the Coco de mer, from Mr. Morton. A collection of Fossils, presented through Sir Charles Metcalfe, by Dr. Gerard. A work on the Ear, in Hindoostanee, by Mr. Breton. Some Burmese Manuscripts, presented on the part of Government. The Pooroos-Purikhya and Moral Maxims, translated into English, by Rajah Kalee Krishun, presented by the Author. Two pamphlets, presented by the Societé d'Agriculture, &c. of Caen. A Grammar of Sanscrit, presented by the Count Malakowski.

A paper was submitted by Miss Davy, on criminal punishments, &c. in China. A letter was read from Mr. Hodgson, forwarding Catalogues of the Kangyur and Tangyur, with remarks, and also one from General Ventura (forwarded by Mr. Young) transmitting an account of excavations made by him at Manikyala.

2. Medical and Physical Society.

Saturday 7th September.

A very full meeting of the Medical and Physical Society took place on Saturday evening, it being understood that a proposal would be submitted, as to the best mode by which the Society could testify their regret for his death, and their respect to the memory, of their lamented Secretary and fellow, Dr. Adam. The Vice-President, Mr. H. H. Wilson, addressed the meeting upon the subject; and the following we believe, is a pretty correct sketch of what he said:—

Before we proceed to the regular business of the evening, I beg to call the attention of the meeting to the loss which the Society has sustained in the death of its Secretary, the late Dr. John Adam.—It must be quite unnecessary for me to dwell upon the merits of our late Secretary—they must be well known to most of the Members present, and will be readily admitted to constitute a strong claim upon the grateful recollections of the Society.

There is no doubt that the very existence of the Society originated with Dr. Adam, and that a sense of its advantages induced him to propose its institution to a man whose equal zeal for the profession, added, at that time, greater influence from his standing in the service. From that time we all know that Dr. Hare and Dr. Adam co-operated actively in the formation of the Society, and as we have already recorded our obligations to the former, it is incumbent upon us to pay a like tribute to the equal claims of the latter.

But the institution of the Society was one of the least of our late Secretary's merits; and he has other and higher claims upon our regard. The same zeal for the credit of the Profession and the promotion of professional knowledge, which had prompted him to propose the formation of the Society, inspired him to the last, and induced him to discharge the offices of his situation with unwearied diligence and interest. I believe that on no one occasion was he ever absent from his post. I can speak from my own knowledge to a fair proportion of our meetings; and where my testimony is wanting, there are others present who can supply the deficiency. On all occasions too, many of us can vouch that he never failed to conduct the business of the evening as if it was a labour of love—or to take part in the amicable discussions which our meetings are accustomed to witness.

It was not only at our meetings, however, that Dr. Adam's warm interest in the prosperity of the Society was evinced, (and his labours were cheerfully and successfully devoted to it at other seasons)—a variety of little details must always devolve upon the Secretary to an Institution, like ours, which though comparatively unimportant, are not the less troublesome. The correction of the Press, too, devolved upon him, and was performed with remarkable accuracy—but the most troublesome part of his extra-official duty was the correspondence he had to maintain with medical men throughout India, in furtherance of the views of the Society. The manner in which he executed this part of his function, was, no doubt, eminently successful—not only has no complaint of delay or inattention failed to reach us, but it has been evident, from the tone of such letters as were laid before the meeting, that his correspondents were highly satisfied; that from being personal strangers, they learned to write to him as familiar friends, and that the interest they took in the Society, was mainly owing to the manner in which the Secretary invited and encouraged their assistance.

Of Dr. Adam's personal claims to our regret, and of the estimation in which his character was held by all who knew him, this is not the place to speak.—We are now only to consider the claims his memory has upon the Society; and those you will, no doubt, admit to have been such as to demand a suitable acknowledgment. Before we engage in any other business this evening, I propose that we consider how we shall best express the sense we entertain of the services of our late Secretary, and our sorrow for his loss.

After which, Resolutions were passed to the following effect:—

That the Medical and Physical Society of Bengal was originally projected by their late Secretary, Dr. J. Adam, and owed its institution, in a great degree, to his exertions.

That the success which attended the foundation of the Society, and the prosperity it has since enjoyed, are mainly attributable to his assiduity, abilities, and zeal.

That the Society feel it therefore incumbent upon them to record their high sense of his services, and their regret for his loss.

That further, to mark the sentiments they entertain, they erect a plain Monument over his tomb, with a suitable inscription, and obtain, if procurable, a Portrait, to be hung up in the apartment where they may assemble.

GLEANINGS

IN

SCIENCE.

No. 24.—*December*, 1830.

I.—*Directions for the Guidance of those desirous of making Geological and Mineralogical Observations.*

[Continued from p. 48.]

The above divisions, as before mentioned, are to be considered merely conventional, as in the series of rocks, no defined limits can be marked, where the primary transition [1], &c. commence and terminate. They refer to the relative ages of the rocks as to a series of successive formations, supposed to have taken place by the Wernerian theory, which considered granite as the nucleus first formed, on which the others were successively deposited from a fluid which held the matter in solution. The discovery of a mass of granite, resting on a bed of shell limestone, at Christiana, in Norway, was the first important observation, tending to shew the recent date of some of the granitic masses. The first deposits were supposed to have taken place in an inclined plane, on the slope of the granite; these formed the transition class; the next deposits, more nearly horizontal, formed the secondary; after these beds were deposited, the water or fluid is supposed to have been withdrawn. Many difficulties occur in conceiving this mode of formation, particularly in the trap rocks which lie over many of the secondary beds, but are still equally crystalline as the primitive, and nearly of the same composition. To account for the formation of these rocks, required the supposition of a second temporary inundation similar to the first. A summary of the various theories or hypothesis which have been formed to account for the mode in which the formation of the crust of the earth has taken place, will be found in the notes to D'Aubuisson's, or in other works on Geology. Hutton's theory, as illustrated by Professor Playfair, was the most generally received in this country, and admired for the science and elegance with which the reasoning is given and the conclusions are drawn. Contrary to Werner, it supposes the granite to have been the last formed, or at least to have appeared on the surface more recently than many of the stratified rocks, by its having, when in a state of fusion, broken through the crust of stratified rocks, which it has therefore elevated or displaced from the original horizontal position which they occupied. All the unstratified rocks, as the porphyries, greenstones, basalt, obsidian, pitch-stone, and lava, are considered as of a similar origin with granite; and hence all the dykes of these rocks, which intersect the strata, accompanied frequently with shifts, slips and alterations in the rocks or strata with which they came in contact, are explained as a consequence of the violence, and the state of fusion in which they were injected. These dykes, certainly, are of various dates, since they must be considered as more recent than the rocks through which they pass, and some of them penetrate the secondary strata. A whin dyke in England passes through the lias, and another through the oolite, which is the highest bed where such dykes have been found. It will be observed, (from fig. 4 & 7) that a dyke, after passing through the coal beds, terminates on reaching the Magnesian limestone, from which it must be considered as formed anterior to that bed. Many

[1] This may be true in the case of the primary and transition rocks, but with regard to the secondary class, it has been pretty generally, if not universally, observed to be distinctly marked by the unconformable position of the strata.—Ed. Gl.

of these dykes intersect each other, causing shifts (fig. 2) and slips as they do in the strata, which shows their being of different dates with regard to each other. In the coal fields of Newcastle the slip accompanying one dyke is so great, that the beds on one side of it are 90 fathoms below the same beds on the other side. In the section fig. 4, it will be seen, that the beds of the coal measured on one side of the dyke, cease to have their planes parallel to the corresponding beds on the other side; from which it must be inferred that the dyke came to occupy the fissure, accompanied by some convulsion, as a subsidence of the surface of the right. The Wernerian theory considers these veins to have been fissures in the rocks, filled from above; whilst the Huttonian theory considers them to have been filled from below, as above stated. The rocks immediately in contact with them are frequently changed, as in the coal at Newcastle, which is at, and near the point of contact, without bitumen; and the clay has become so hard as to be used for whetstone; the limestone is also altered, but not decomposed, which was considered as conclusive against their having been filled by matter in a state of fusion, until the experiment of Sir James Hall, of fusing or melting the carbonate of lime (limestone) without driving off the carbonic acid, by retaining the whole hermetically sealed whilst in the furnace; from which it is inferred that limestone may be submitted to great heat without altering its nature, if under sufficient pressure, as it would be when at the bottom of the ocean.

In the metallic veins the same changes of the rocks in contact with them, does not appear to be found; such veins are, however, more confined to the primitive rocks, particularly gneis and mica slate, though great part of the lead in England is found in veins in the mountain limestone No metallic veins have been found in strata superior to the red marl; these metalliferous veins are, however, sometimes accompanied by shifts and slips in the strata through which they pass, similar to what has been mentioned of some of the whin dykes; but others, and veins of quartz, are considered as bearing evidence of being of contemporaneous formation with the rocks in which they are found. Others again may be supposed to have been some trifling fissure not extending to the surface, which has been filled by infiltration of matter through the rocks forming its walls. There are other fissures which bear evidence of having been filled from the surface, since they contain fragments of earth, bone, &c. &c. The rock of Gibraltar, which is of mountain limestone, is intersected with such veins, containing bones, cemented in calcareous spar, or in stalactite. Other fissures, or rather openings or holes in the rocks, particularly in limestone, have been found at different places, as in the Hartz, Bayreuth, and lately in Yorkshire, which led to caves containing a great accumulation of bones of various animals, particularly of hyænas and many graminivorous animals, which have caused a considerable interest and discussion, in the endeavour to assign some probable cause which could be supposed to have brought them together in such a situation. It is not easy to conceive in what manner the fissures or veins in the rock of Gibraltar could be filled with bones, but it is considered as pretty satisfactorily established, that the caves of Bayreuth and Kirkdale have been the habitations of hyænas that had transported bones to them for their food; merely the hard portions of the bones being found remaining, and these exhibiting the marks of teeth. The papers descriptive of the above caves will be found in the Transactions of the Geological Society, or in the work of Professor Buckland, termed *Reliquiæ Diluvianæ*. These bones are considered diluvial, or to have belonged to animals existing previously to the present state of things. Professor Buckland gives, as a distinguishing character of diluvial from alluvial bones, the adhesion of the former when touched by the tongue.

The different theories above alluded to, led the attention of former Geologists more particularly to the investigation of the crystalline unstratified rocks, and the mode in which they were connected with the stratified at their points of junction, in the hopes of arriving at a knowledge of the mode in which that junction had happened; whether by a quiet deposition on the nucleus of granite previously formed, as supposed in the one theory, or by having broken through, and torn up the strata, as supposed in the other. The spirit with which these inquiries were pursued led to a knowledge of many important facts, but it may be said that none were found to be conclusive in favour of either theory, or at least other observations were made which could not be included under the same explanation, and many geologists considered the granite, gneis and slate, as all being of the same mode of formation, only differing in structure, in consequence of a variation in the proportion of mica, since their chemical composition is nearly similar. One great object with the supporters of the Huttonian theory was, to ascertain whether dykes of granite and greenstone run into the mass of the mountain, losing themselves, in and forming a part of the mass, which they were supposed to do, according to that theory. This idea of their origin was certainly that generally received, as the most probable of the two, by the geologists of this country, but still no very conclusive fact was obtained; some of the observations were contradictory, and many were questioned, as resting on doubtful authority, or as incorrectly made, from the difficulty which often attends the making geological observations; and also, perhaps, from the supporters of each theory, in the mode they sawthings, reconciling them, in some measure, to their previously formed ideas of formation.

Attention has been lately directed chiefly to the secondary and tertiary strata, particularly in the neighbourhood of Paris, the Isle of Wight, and the beds round London. The shells which are found in the different beds in these strata, possess distinct characters; some being of fresh water, and some of marine origin*; it is hence inferred, that these portions of the surfaces have been alternately submerged under a lake, and under the sea, and that several of such changes have happened. The remains of land animals have been also found in these strata, washed in, probably, by rivers or by other causes. The comparative rarity of the remains of land animals may be accounted for, from the land of that period being probably now under the sea; and also, the remains of animals dying on the land are less likely to be preserved than those dying in the water, which latter would become immediately imbedded or buried in the deposit of new strata then forming. It may here be observed, that all regular strata or beds are considered to have been formed by deposition at the bottom of a sea, lake, or estuary, from the *detritus* or decomposition of rocks previously existing. The quantity of shells and impressions of fish establish this in the beds in which they are found, and hence the same is inferred of others; but there are beds or strata, as gneis, mica slate, clay slate, which are of a doubtful origin; since their stratified structure might be given by an excess of mica or other cause; and from the uninterrupted transition which, as has been mentioned, is found in all species of rocks.

Besides the beds of diluvium, gravel, and pebbles, it has been mentioned, that boulders or round masses of granite, greenstone, &c. are frequently found on the surface, at great distances from any mass of similar rock, from which they may be supposed to be derived.—The mode in which this may have happened is not well accounted for; by some geologists they are considered as having been brought to their present position by a great torrent, and they have traced them to lie in a particular direction from the mountains from which they suppose them to have been detached; by other geologists they are considered to be the more durable portions of a rock which has covered the surface.

Meteoric stones are also found lying on the surface, and, as has been mentioned, often resemble greenstone; many explanations have been offered, attempting to account for them. That most generally received, is that they are small bodies, wandering in space, and hence attracted by all the larger bodies or planets. If the ellipse in which they move be so excentric, that the curve falls within the limit of the crust of the plane, which is one of its foci, these small bodies will come in contact with, or fall on its surface: on the other hand, should the distance from the focus to the curve be greater than the radius of the planet but only by a small quantity, they will be carried through the upper atmosphere with great velocity, and give the appearance of balls of fire. Another idea, is their being formed in the atmosphere, from the various gases which may be supposed to be accumulated in it, from volcanoes, &c. &c. suddenly combining by means of the electric fluid. But were this the case, the stone, in falling, would be influenced by the earth's rotatory motion.

The flints, which are accumulated in great quantities in some parts of England, and in other chalk countries, are considered to have been derived from beds of chalk, which have disintegrated, and disappeared; this is sufficiently established, since these flints are quite similar to those found in chalk; but the nodules of silex which are found in beds lying above the chalk, cannot well be supposed to have such a derivation; and some geologists consider them as probably formed in the same manner as siliceous *tufa*, from springs having silex in solution. These nodules or pebbles are found in beds of gravel or clay, and are generally coloured to some depth from their surface in concentric rings, probably from decomposition going

* The general characters given distinguishing these shells, is the fresh water being much thinner, and more delicate, than those considered salt water shells, as is found to be the case in the shells of the present lakes and seas; other beds containing shells of intermediate character, are considered to have been formed in some Estuary at the mouth of a great river. That the character of the shells were sufficiently defined to warrant the deduction mentioned, was at first much questioned and disputed; but since Conchology has been more studied, it appears to be admitted.

forward, and the penetration of water containing oxide of iron. It is generally remarked, that in rising from the primitive to the tertiary rocks, quartz becomes a less prevalent ingredient in their composition. Quartz or silex is, therefore, considered indicative of older rocks or formations; but the chalcedonic beds, in the upper fresh water formation at Paris, is an exception to this, since from its situation it must be referred to a very recent period of formation.

The occurrence of a conglomerate breccia or puddingstone is considered indicative of the epoch of a new order or class of rocks or formation, or when the upper beds are unconformable to those on which they rest. It is from the organic remains, particularly the shells, that the different epochs of geology are distinguished The red marl beds which lie between the coal and the lias are considered a marked point in the series; the shells found in the beds above being different from those found in the beds below. Some of the shells, however, as the Nautilus and Ammonites, are found through a great range, and the remains of a land animal have been lately found in the oolite beds, in the stonesfield slate, (fig. 6,) previously to which no such remains were considered to exist lower than the chalk. It is only when in beds at different points on the earth's surface, similar shells are found in considerable quantity, that these beds can be considered as belonging to the same period of formation; thus in the lower marine formation of the Paris basin (the calcaire grossiere, fig. 8) and the London clay, there are about one hundred shells common to both.

It will be seen that beds may be considered in this point of view as geologically the same, although different in their mineralogical composition; and it may here be remarked, that besides the unsatisfactory definition of several of the species of rocks which was given in their description, there are many beds in the secondary and tertiary portions of the series, which are so mixed and indistinct, as scarcely to admit of any description of their mineralogical characters. These beds are traced and recognized at distant points, chiefly from their geological connexion, or their place in the series in relation to each other, and to well established points in it, or from the shells which they contain, as also from their own internal character, and that of their accompanying beds.

From such indistinct characters, many of these beds have been traced over a great portion of England, as will be seen in the papers and map of the Geological Society. The lias and chalk are considered the best established points in the secondary series, to which therefore the other beds are referred.

The red marl above mentioned contains the deposits of salt and gypsum, and these are always included in it, wherever it has been found on the earth's surface. It is this, and similar uniformity being found to exist, which gives to geology its great interest, and points out the advantages to be derived from it in directing future researches: for the positions which the strata occupy in the series are always found to be the same, at least they are never reversed, though some of the members of the series may be wanting. Thus, in Sutherland, an instance is mentioned of coal being separated from granite by only a thin stratum of conglomerate; and in France and Boulogne the oolite is found resting on the mountain limestone. In Ireland also, the chalk is stated to be found resting immediately on the sandstone which covers the coal measures; all the oolite and lias beds which are found to intervene in the English strata (as seen in the section, fig. 6. 7) being wanting; another situation is mentioned near Fairhead, where the basalt comes immediately in contact with the coal measures. The detail and description of the observations above alluded to, will be found in the 3d vol. of the Geological Transactions, in a paper by Dr. Berger and Mr. Conybeare. In that paper it is mentioned, that the bed corresponding in its fossils to the English greensand, is found in its place in Ireland, namely, under the chalk, where it is denominated mulatto stone: it is described as a calcareous sandstone, mixed with specks of green earth and oxide of iron. The lias limestone is also said, in the same paper, to be found at different points in the north of Ireland, alternating with clay slate: it is described as a blue argillaceous limestone, similar in appearance to the English, and containing a great many shells of Ammonites, Gryphites, and Pentacrinus. The red marl has likewise been found, and with its constant subordinate portions of gypsum and salt; but the oolite beds appear, as far as these observations have been made, to be wanting in Ireland. The Geologists who have visited the north of Ireland, have, in particular, directed their attention to the whin dykes, and to the junction of the chalk and basalt, as will be seen in the plates to the paper above mentioned, and also in a paper by Dr. Richardson, published in the Irish Transactions, vol. 9. There are many observations given, which can only be explained from the force and convulsion attendant on the formation of the basalt. In some places, masses of chalk, completely detached and imbedded in it; the chalk in contact with the basalt or to the distance of 8 or 10 feet, is also found to be altered, so as to be crystalline like marble, and to become phosphorescent when heated. Beds of columnar basalt, alternate with beds of amorphous basalt, or are separated by a thin seam of bole or red ochre. These amorphous or compact beds of basalt are described, however, frequently to contain a structure of small basaltic columns, radiating from a centre. It may here be observed, that the prismatic figure of basalt is quite different from the regular forms of crystallization in simple minerals; the measure of the angles is not constant, or subject to certain laws, as in simple minerals: these prismatic fissures or divisions appear, therefore, to be merely a mechanical effort, probably in cooling, and the basis of the columns also appear to be nearly always directed to that surface at which the cooling may be supposed to have commenced. Thus, in veins or dykes of basalt, the columns are found to be transversely situated.

In the section of England, it will be observed, that a bed of chalk marl forms the lower portion of the chalk strata: this has also been found in Ireland, and appears to be general; it contains a portion of alumina, about 10 per cent. which renders it valuable as a manure, from its decomposing easily when exposed to the weather; and it is more particularly sought after, for a light, sandy soil, to which the alumina is also advantageous. This may lead to some mention of the different soils, since they are formed by the decomposition and disintegration of rocks; their qualities may in some degree be inferred from the nature of the rocks from which they have been produced, and from which, in some situations, they still continue to receive a supply. Granite, when unconnected with beds of other rocks, appears to give a barren sand, or pebble soil; particularly when the argile of the feldspar is washed away. Gneis, mica slate, and clay slate, give a light soil, but when beds of lime form a portion of the whole mass, the soil is always productive. Magnesian limestone, is, however, an exception; for wherever magnesian earth prevails, it appears to render the soil sterile, and unfavourable to vegetation; when alumina forms the principal ingredient, as in the rocks in which feldspar predominates, the soil is a stiff clay. Granite without mica (pegmatite) is found to give by its decomposition the best kaolin or porcelain earth.

Some of the basalts give a very productive soil, others appear entirely to resist decomposition; the lavas much the same. Those in Auvergne, though of a date anterior to all tradition, are still fresh and uncovered by soil; whilst two of the streams from Etna, which are known to have been formed by the eruptions of 1187 and 1329, have already 12 and 8 inches of vegetable earth respectively. The progress of decomposition in the basalt and lavas, appears to depend, in some degree, on the quantity of iron, or of alkali, which they contain.

The decay of rocks, however, and their deposit in the vallies and in the sea, is so slow, as to be little more than observable that such an action is continually in progress, and that the ultimate effort would be to transport the whole of the land under the level of the ocean.

Professor Playfair, in his illustration of the Huttonian theory, enters into a calculation to shew, that from the quantity of mud floated down by rivers in floods, the amount, in a century carried into the sea, may be equal in bulk to Mount Blanc. In the same work will be found reasons given for believing that the surface of the earth has at different periods changed its level, and that such changes may be constantly in progress, though so gradual as scarcely to be noticed.

In conducting geological investigations, as will have been seen, many branches of science are requisite. Zoology or comparative anatomy, so as to be able to determine the nature of the bones found, and their analogy to existing species, has been employed, particularly by Mr. Cuvier, so as to deduce very important results, respecting the changes of those animals which have successively lived upon the surface of our globe; it establishes that there was much similarity with the present order of things, but still scarcely a species exactly the same, but that they assimilate nearer to those now existing, in proportion as they are found in higher or more recently formed beds. His attention has been particularly directed to the bones found in the chalk beds near Paris, and to those of the animals found in the diluvial beds, as of the Elephant and Hippopotamus.

Conchology is of the same importance in the determination of the shells. No individual can fully embrace the study of all these branches, but a general acquaintance with those fossil shells which most commonly occur, is certainly desirable. This may be obtained from the plates and descriptions in Parkinson's Organic Remains, and by examining such collections as may be met with.

The endeavours of botanists to determine the impressions of plants found in the shale, in the coal measures, and in other strata, have been less successful, as they appear to be quite different from any now existing. They are considered to have been gigantic grasses or reeds, and some palms, and may be supposed to have required a greater degree of heat and moisture for their production than what exists in our present atmosphere. This has led some to imagine that the earth has been continually, but gradually, falling in temperature. This observation, and the opinions mentioned as held by the supporters of one of the theories of the formation of the earth, which supposed an internal or central heat, independent of that which is communicated by the sun, led to the investigation whether a difference of temperature could be observed in descending into mines. The observations were made at different depths, in several mines, and the thermometers were let into the rock; the conclusions from them rather indicate an increase of temperature in descending. The thermometers, however, cannot well be supposed to have been kept entirely free from the influence of the miners' light, blasting, &c. &c. and also the greatest depth which has been reached below the level of the sea is only about 1200 feet, or nearly $\frac{1}{18000}$ of the earth's semi-diameter. Mines in mountain districts, are, however, much deeper, some double that depth from the surface, or, if the greatest height and lowest level which has been examined be taken for the fraction, it gives about $\frac{1}{1000}$ part of the earth's radius, which is therefore the portion known, or rather partially examined by geologists. The experiments, however, of Dr. Maskelyne and Mr. Cavendish, (Philosophic Transactions,) give the mean specific gravity of the earth at nearly double that of the rocks composing its crust, or equal to five times that of water; and from the more recent investigations made by Laplace, he considers the layers of different specific gravity to be concentric to the centre of gravity of the earth, their density increasing as they are situated nearer to the centre. The same philosopher has inferred, from the effect of the tides on the the earth's surface, that the mean depth of the ocean is about 11 English miles.

Mineralogy is the branch which is most requisite to the Geologist, since a knowledge of the simple minerals which enter into the composition of rocks, is essential. The manner in which some knowledge of this branch may be acquired, has already been mentioned. In the description of the rocks it has been seen, that the different species pass into each other: no such transition can take place between the species in mineralogy; but to be capable of being determined, the minerals must be in a sound unaltered state, and in many species crystallized, or have faces of cleavage, and of a sufficient size to observe the characters; otherwise they can only be determined indirectly, by inference from a perfect specimen, or suite of specimens, establishing the identity or agreement of character, into the smaller and less distinct.

The minerals composing rocks are frequently so minute as only to be made out in this manner; but when that is once done, the eye acquires the habit of judging in similar cases. Many of the greenstones, the base of porphyry, &c. are of this nature. In these pastes, feldspar appears to be nearly always the predominant ingredient.

Mechanical analysis is a mode which has been resorted to in such cases; namely, by reducing the ore to almost a powder, and by a microscope or lens, observing the appearance of the parts, as to the cleavage, &c. &c. on these being agitated in a vessel of water, they will separate, according to their specific gravity. Monsr. Cordier mentions having pounded lava, and also hornblende and feldspar, and mixed the two latter, until under the microscope they gave the same appearance as the former, so as to be able to judge or estimate the relative quantities of each. Chemical analysis cannot well be applied in such cases, since from the different minerals which compose the rocks, frequently changing in their relative quantities, the result would not be satisfactory.

In the system of Mineralogy by Mohs, no chemical properties are employed to distinguish the minerals, since such cannot be observed without changing or destroying them; it is entirely founded on external character. The species so determined, however, and the chemical species, agree with very few exceptions: that is, the same component parts combining in the same proportions give the same form, hardness and weight. This was assumed by Hauy in the definition of his species; but lately it has been proved by Mr. Mitcherluh, of Berlin, that with some bases, different substances would combine, giving exactly the same crystalline form, and hence the possibility of their combining with the base in variable proportions, which accounts for some discrepancies in the analysis of Garnet and other minerals. Some chemical tests are useful, as the readiest mode of arriving at the knowledge of the mineral or rock, as for instance acid for lime. Muriatic acid, diluted with an equal quantity of water, is the most generally used. Other carbonates equally effervesce, but being rare, compared to lime, cannot well lead to error, besides which, by their specific gravity and hardness, they are easily distinguished.

The foregoing remarks have been offered, in the hopes of giving some general idea of the objects which may be attended to in making geological observations, or which are included in the extensive scope of matter which geology embraces within its researches. To recur, however, to what is the chief object, namely, to collect data for giving a geological and mineralogical map, sections and descriptions of the country, the manner in which specimens are to be taken, and the direction and dimensions of the beds marked for this purpose, has been specified in the first pages. The labels on the specimens must be securely affixed, and the reference to their position in the plan carefully marked, to avoid the chance of error. The geological plans should be coloured, distinguishing the different rocks, as they are traced on the surface, and in the section. To induce uniformity in this respect, a table of colours is given for the principal rocks, which is nearly the same as that in the Geological Map of England. It has been found convenient, in addition to the colours, to have letters of reference, as is shown in the sketch: any remarks, as of simple minerals found in the beds, can be made to the same letters.

II.—*On the Measure of Temperature, and on the Laws which regulate the Communication of Heat. By* M. M. Dulong *and* Petit.

[From the Journal Polytechnique, T. IX.]

§ 4. *On Cooling in Air and in the Gases.*

The laws of cooling *in vacuo* being known, it is easy enough to separate from the total cooling of a body, surrounded by air or any other gas, the portion of the effect due to the contact of the fluid. It is sufficient evidently to deduct from the actual rate of cooling that which would be found, if (all else being equal) the body had been placed *in vacuo*. This correction can always be made, now that we have a formula to represent this rate with every degree of accuracy, and for every possible case. We can then determine the energy of the cooling process as due solely to the contact of fluids, and such as they would be observed could we deprive substances of the power of radiation. This part of our enquiry required a great many experiments to be made, since the laws which we were desirous of investigating were to be studied with different gases, and with each of these again for different pressures and temperatures. Each experiment has been conducted, and the result calculated in the manner previously explained: we shall therefore confine ourselves to giving the mean results of calculation from all these observations.

The first question which it was incumbent to enquire into was, Whether the surface of the body, which so powerfully affects radiation, would effect any change in the loss of heat occasioned by the contact of fluids? To determine this point, it was sufficient to observe the cooling of our thermometer in a gas of a determinate elasticity and temperature, first with the natural surface of the glass, and afterwards as covered with a leaf of silver.

Of all the experiments which we made with this question in view, we shall here give but the two following:—

In the first we observed the cooling of the large thermometer in the balloon, containing air of the density ,72 metres, and of the temperature 20°,.

First Case.

The Thermometer with its natural surface.

Excess of temp. of the thermometer.	Totalrate of cooling of the thermometer.	Rate of cooling *in vacuo*.	Difference, or rate of cooling due to the air.
200°	14°,04	8°,56	5°,48
180	11 ,76	7 ,01	4 ,75
160	9 ,85	5 ,68	4 ,17
140	8 ,05	4 ,54	3 ,51
120	6 ,46	3 ,56	2 ,90
100	4 ,99	2 ,72	2 ,27

SECOND CASE.

The Thermometer having its bulb covered with silver leaf.

Excess of temp. of the therm.	Total rate of cooling.	Rate of cooling. *in vacuo.*	Rate of cooling due to the air.
200°	6°,93	1°,50	5°,43
180	6 ,02	1 ,23	4 ,79
160	5 ,19	1 ,00	4 ,19
140	4 ,32	0 ,80	3 ,52
120	3,50	0 ,62	2 ,88
100	2 ,80	0 ,48	2 ,32

We see in comparing the last column in each of these tables, that the corresponding members differ by very small quantities, which may be fairly attributed to the errors of the experiment. Air carries off then, all else being equal, the same quantity of heat from surfaces, whether of glass or metal.

The two following tables contain the particulars of a similar comparison made with hydrogen gas, the small theremometer being substituted for the large one.

The experiment was made at a temperature of 20°, the density being ,74 metres.

FIRST CASE.

The Thermometer with its natural surface.

Excess of temp. of the therm.	Total rate of cooling.	Rate of cooling *in vacuo.*	Rate of cooling. due to the contact of hydrogen.
80°	22°,96	5°,03	17,93
60	16 ,14	3 ,54	12,60
40	9 ,87	2 ,18	7,69
20	4 ,28	0 ,95	3,33

SECOND CASE.

The Thermometer having its bulb covered with silver leaf.

Excess of temp. of the therm.	Total rate of cooling.	Rate of cooling *in vacuo.*	Rate of cooling due to hydrogen.
80°	19°,59	1°,77	17,82
60	13 ,97	1 ,29	12,68
40	8 ,62	0 ,87	7.75
20	3 ,74	0 ,37	3,37

This comparison gives for hydrogen a similar result as for air. The similarity of action being then established, in the case of surfaces which differ so much as glass and silver in their radiating properties, and for gases differing so much as air and hydrogen, it is natural we should generalise this result, and deduce from it the following law:

The loss of heat occasioned by the contact of a gas is (all else being equal) independent of the state of the surface of the body which is subjected to the cooling process.

This remarkable law in the communication of heat, has been already admitted by Mr. Leslie; but this able philosopher has only advanced it as a conclusion rendered probable by the result of two indirect experiments, from which it appears that the state of the surface has only a feeble influence on the total cooling effect, when the circumstances are such as leave but a small share in the loss of heat attributable to radiation. As for example, when a heated body is exposed to a violent wind, or when it is immersed in a liquid. But however ingenious such experiments may appear, they can never supply the place of the direct investigation, for in the case in question, where is the difficulty of supposing that air in motion may have some property or quality which may not be predicable of still air? Such an objection would appear still stronger if we admit, as Mr. Leslie supposes, that still air receives heat from bodies by two different means, that is to say, by conduction as in the case of solid bodies, and by the continued displacement of the fluid, owing to the ascending current. Our method of proceeding, by allowing us to verify the law in gases so different in their nature, obviates every doubt which the experiments of Mr. Leslie may appear to have left. It is one of those occasions on which some opinion may be formed as to the advantages of our method of experimenting.

The principle we have just established, having been thus verified, we have confined ourselves in our subsequent experiments, to observing the cooling of the thermometer with the naked bulb in air, and in different gases. Henceforward, therefore, we shall only tabulate the effects due to the contact of the gas; they have been always calculated, as we previously remarked, by subtracting from the total rate of cooling that which would have been observed if the thermometer had been cooled in a vacuum.

We shall now enter upon an examination of the various circumstances which might be supposed to modify the energy of elastic fluids in the production of the phenomenon we are treating of. We propose to study the influence of each of these causes; first, in the case of common air, afterwards, in those of hydrogen, of carbonic acid, and of olefiant gas; the first two have been selected on account of the great difference which they present in some of their physical properties. Air and olefiant gas again are gases of nearly equal density, but of very different capacities.

The influence exercised on the cooling process, by the greater or less temperature of the surrounding mass, has naturally suggested to us the enquiry, Whether the temperature of the gas might not produce some analogous effect on the loss of heat occasioned by its contact? It is scarcely necessary to say that such experiments have never yet been attempted; those enquirers who have occupied themselves with such questions, having supposed that the rate of cooling entirely depended on excess of temperature.

We shall not stop to give the details of our first attempts, but at once give tables of results in which the law is itself manifest. In the experiments which we made, we effected the required change of temperature of the gas, by heating the water which surrounded the balloon. But the gas was, at the same time, allowed to expand freely, so that the pressure was always the same. The following table contains the particulars of such a series made with air.

Excess of temp. of the therm over that of the surrounding air.	Rates of cooling due solely to the contact of air.			
	Pressure $0^m,72$. Temperature 20^0.	Pressure $0^m,72$. Temperature 40^0.	Pressure $0^m,72$. Temperature 60^0.	Pressure $0^m,72$. Temperature 80^0.
200°	5°,48	5°,46	....	....
180	4 ,75	4 ,70	4°,79	....
160	4 ,17	4 ,16	4 ,20	4°,13
140	3 ,51	3 ,55	3 ,55	3 ,49
120	2 ,90	2 ,93	2 ,94	2 ,88
100	2 ,27	2 ,28	2 ,24	2 ,25
80	1 ,77	1 ,73	1 ,70	1 ,78
60	1 ,23	1 ,19	1 ,18	1 ,20

A simple inspection of this table is sufficient to show, that the rate of cooling is the same in each of the four series; the excess of temperature being the same. This law is too important not to require being verified on other elastic fluids. The following table contains the results of a similar comparison made with hydrogen gas, which has been raised, successively, to the temperatures of 20^0, 40^0, 60^0, and 80^0. The pressure has been in all these experiments $0^m,72$.

Excess of temp. of the therm. over that of the air.	Rates of cooling due solely to the contact of the gas.			
	Pressure $0^m,72$. Temp. 20^0.	Pressure $0^m,72$. Temp. 40^0.	Pressure $0^m,72$. Temp. 60^0.	Pressure $0^m,72$. Temp. 80^0.
160°	14°,26	14°,08	14°,18	....
140	12 ,11	12 ,16	12 ,12	12°,08
120	10 ,10	10 ,13	10 ,20	10 ,19
100	7 ,98	7 ,83	8 ,03	8 ,05
80	6 ,06	5 ,97	6 ,01	6 ,00
60	4 ,21	4 ,17	4 ,18	4 ,20

This table evidently leads to the same truth as the preceding. To show that it extends to all the gases, whatever may be their nature and their density, we shall here give the results of experiments made on carbonic acid, under a pressure of $0^m,72$, and on common air, rarefied to the pressure $0^m,36$.

1. *With Carbonic Acid Gas.*

Excess of temp. of the therm. over the surrounding air.	Rates of cooling due solely to the contact of the gas.			
	Pressure $0^m,72$. Temp. 20°.	Pressure $0^m,72$. Temp. 40°.	Pressure $0^m,72$ Temp. 60°.	Pressure $0^m,72$ Temp. 80°.
200°	5°,25	5°,17	....	....
180	4 ,57	4 ,63	4°,52	....
160	4 ,04	4 ,06	3 ,97	4,10
140	3 ,39	3 ,39	3 ,34	3,43
120	2 ,82	2 ,80	2 ,79	2,83
100	2 ,22	2 ,18	2 ,21	2,20

2. *With Common Air rarefied.*

Excess of temp. of therm. over that of surrounding air.	Rates of cooling due solely to the contact of the gas.			
	Pressure $0^m,36$. Temp. 20°.	Pressure $0^m,36$. Temp. 40°.	Pressure $0^m,36$. Temp. 60°.	Pressure $0^m,36$ Temp. 80°.
20	4°,01	4°,10	....	....
180	3 ,52	3 ,50	3°,55	....
160	3 ,03	2 ,99	3 ,04	3°,09
140	2 ,62	2 ,57	2 ,62	2 ,66
120	2 ,12	2 ,16	2 ,14	2 ,15
100	1 ,69	1 ,71	1 ,67	1 ,73

From all these comparisons, we may deduce the following law :—

The rate of cooling of any body, as due solely to the contact of a gas, depends for the same excess of temperature, on the density and temperature of the fluid; but this dependence is such, that the rate of cooling remains the same if the density and the temperature of the gas change, so that the elasticity remains constant.

In our researches, then, on the law of cooling in gases, we have only occasion to consider elasticity. It is then the influence of this property which it is necessary to determine.

To obtain it, we have determined successively, for each gas, and at different elasticities, the rates of cooling, corresponding to the same excess of temperature. We shall only give such account of these experiments as may be sufficient to establish the law to which we have been led.

Let us begin with air.

The following table contains the rates of cooling, due solely to the contact of air under the pressures $0^m,72$ $0^m,36$ $0^m,18$ $0^m,09$ $0^m,045$, that is to say, under pressures which are as the numbers

$$1, \tfrac{1}{2}, \tfrac{1}{4}, \tfrac{1}{8}, \tfrac{1}{16}.$$

Excess of the therm. over the temp. of the surrounding air.	Rates of cooling due solely to the contact of the gas.				
	Pressure $0^m,72$.	Pressure $0^m,36$.	Pressure $0^m,18$.	Pressure $0^m,09$.	Pressure $0^m,045$.
200°	5°,48	4°,01	2°,95	2°,20	1°,59
180	4 ,75	3 ,52	2 ,61	1 ,90	1 ,37
160	4 ,17	3 ,03	2 ,21	1 ,62	1 ,20
140	3 ,51	2 ,62	1 ,91	1 ,40	1 ,02
120	2 ,90	2 ,12	1 ,57	1 ,15	0 ,84
100	2 ,27	1 ,69	1 ,23	0 ,90	0 ,65
80	1 ,77	1 ,29	0 ,96	0 ,70	0 ,52
60	1 ,23	0 ,90	0 ,65	0 ,48	0 ,35
40	0 ,75	....	....	....	....
20	0 ,32	....	....	....	....

If we take the ratios of the corresponding numbers of the second and third columns, we shall find that their values are, commencing at the top,

1,37 1,35 1,37 1,34 1,37 1,34 1,37 1,36.

In the same manner, we have for the ratios between the numbers of the 3rd and 4th columns,

1,36 1,35 1,37 1,37 1,35 1,37 1,34 1,37.

For the ratios of the numbers in the 4th and 5th columns,

1,34 1,37 1,36 1,36 1,37 1,36 1,37 1,35.

Finally, we shall have, in dividing the numbers of the 5th column by those of the 6th,

1,38 1,38 1,35 1,37 1,36 1,37 1,35 1,37.

These ratios differ only as the results of the most careful observations must differ; and we are entitled to draw from them the following conclusions :—

1mo. *Whatever the elasticity of the air, the rate of cooling, as affected by the sole contact of the fluid, varies after the same law, provided the excess of temperature remains constant.*

2do. *The elasticity, or tension of the air, varying in geometric progression, its cooling power varies also in geometric progression : but in such wise, that when the ratio of the terms in the first series is* 2, *that of the second is* 1,366, (*the mean of all the preceding numbers.*)

If will be conceded that these laws could not have been established without many trials : but once verified in the case of common air, it was natural to enquire if they held good, with regard to the other gases. We shall proceed to give an account of our observations with regard to each of them.

To begin with hydrogen.

Excess of temp. of the therm. over that of the gas.	Rates of cooling solely due to the contact of the gas.				
	Pressure 0m,72.	Pressure 0m,36.	Pressure 0m,18.	Pressure 0m,09.	Pressure 0m,045.
180°	16°,59	12°,86	9°,82	7,49	5°,81
160	14 ,26	10 ,97	8 ,37	6 ,49	4 ,95
140	12 ,11	9 ,24	7 ,11	5 ,47	4 ,24
120	10 ,10	7 ,83	5 ,99	4 ,64	3 ,51
100	7 ,98	6 ,23	4 ,72	3 ,63	2 ,80
80	6 ,06	4 ,62	3 ,58	2 ,77	2 ,09
90	4 ,21	3 ,21	2 ,48	1 ,83	1 ,46

The ratios between the numbers of the 2nd and 3rd columns are ;

1,29 1,30 1,31 1,29 1,28 1,31 1,31.

The ratios between the numbers of the 3rd and 4th columns are ;

1,31 1,31 1,30 1,31 1,32 1,29 1,29.

The ratios of the numbers in the 4th and 5th columns are ;

1,31 1,29 1,30 1,29 1,30 1,29 1,32.

The ratios of the numbers in the 5th and 6th columns are ;

1,29 1,31 1,29 1,32 1,30 1,32 1,29.

The almost perfect equality of these numbers furnishes us with a result analogous to that which determined for air ; thus :—

1mo. *Whatever the elasticity of hydrogen gas, the intensity of its cooling power is represented by the same function of the difference of the temperatures ;*

2do. *The cooling power of hydrogen gas decreases in a geometric progression, the ratio of which is* 1,301, *when its elasticity is diminished according to a geometric progression, the ratio of which is* 2.

We have found the same thing, with regard to carbonic acid and olefiant gas ; the truth may be verified by examining the two following tables, arranged for these gases in the same form as the preceding two, for common air and hydrogen gas.

1. *Carbonic Acids.*

Excess of temp. of the therm. over that of the gas.	Rates of cooling due solely to the contact of the gas.				
	Pressure 0m,72.	Pressure 0m,36.	Pressure 0m,18.	Pressure 0m,09.	Pressure 0m,045.
200°	5°,25	3°,64	2°,56	1°,79	1°,25
180	4 ,57	3 ,22	2 ,25	1 ,56	1 ,09
160	4 ,04	2 ,80	1 ,97	1 ,37	0 ,95
140	3 ,39	2 ,38	1 ,65	1 ,17	0 ,80
120	2 ,82	1 ,97	1 ,36	0 ,95	0 ,67
100	2 ,22	1 ,55	1 ,08	0 ,76	0 ,52
80	1 ,69	1 ,17	0 ,82	0 ,57	0 ,40
60	1 ,18	0 ,82	0 ,57	0 ,40	0 ,28

2. *Olefiant Gas.*

Excess of temp. of the therm. over that of the gas.	Rates of cooling due solely to the contact of the gas.				
	Pressure 0m,72.	Pressure 0m,36.	Pressure 0m,18.	Pressure 0m,09.	Pressure 0m,045.
200°	7 ,41	5°,18	3°,44	2°,58	1°,84
180	6 ,45	4 ,57	3 ,17	2 ,22	1 ,59
160	5 ,41	3 ,86	2 ,72	1 ,89	1 ,34
140	4 ,70	3 ,31	2 ,35	1 ,63	1 ,13
120	3 ,84	2 ,76	1 ,92	1 ,35	0 ,96
100	3 ,12	2 ,21	1 ,55	1 ,08	0 ,78
80	2 ,34	1 ,62	1 ,15	0 ,79	0 ,62

Mean value of all the ratios :

For carbonic acid gas, 1,431.
For olefiant gas, 1,415.

We may now, then, from all that precedes, draw the following conclusions :—

1*mo. The rate of cooling due to the sole contact of a gas increases as a certain function of the difference of temperature, whatever the elasticity of the gas.*

2*do. For the same difference of temperature the cooling power of a gas varies in geometrical progression when the tension varies in geometrical progression; and if we suppose the ratio of the second progression to be* 2, *then the ratio of the* 1*st will be* 1,366 *for air ;* 1,301 *for hydrogen ;* 1,431 *for carbonic acid, and* 1,415 *for olefiant gas.*

This result may be exhibited in another and more simple form, by the aid of the following transformations:

Let P represent the cooling power of the air, having its tension $= p$. This power will become P (1,366), with a tension p 2; $P\ (1{,}366)^2$ with a tension $p\ 2^2$; and finally with a tension $p\ 2^n$ it will be $P\ (1{,}366)^n$. Now putting $p\ 2^n = p'$ and $P\ (1{,}366)^n = P'$
we shall have, by eliminating n :

$$\frac{\text{Log. } P' - \text{log. } P}{\text{Log. } (1,\ 366)} = \frac{\text{Log. } p' - \text{log. } p}{\text{Log. } 2}$$

whence, returning to numbers,

$$\frac{P'}{p} = \left(\frac{p'}{p}\right)^{0,45}$$

In the same way for hydrogen we shall find

$$\frac{P'}{P} = \left(\frac{p'}{p}\right)^{0,38}$$

for carbonic acid the exponent will be 0,517, and for olefiant gas 0,501.

Hence, we may infer, that the cooling power of a gas is, all else being equal, proportional to a certain power of its elasticity ; but the exponent of this power varies with the gas. It is 0,38 for hydrogen, 0,45 for air, 0,517 for carbonic acid, and 0,501 for olefiant gas. These three last numbers differ so little from 0,5, that we may say their cooling power varies nearly as the square root of their elasticity.

If we now compare the law which we have announced, to the approximations proposed on the same subject, by Messrs. Leslie and Dalton, we may judge of the error into which they have been led, by the erroneous suppositions on which all their calculations have been founded, and by the want of the necessary precision attributable to the several methods they have employed. In fact, the former of these gentlemen[1] finds, by photometrical experiments, calculated after the law of Newton, that the cooling power of the air is as the 5th root of the density, (tension ?) and Mr. Dalton[2] finds it proportional to the cube-root, assuming a certain invariable law as representing the total cooling effect for every body, and in every gas.

Knowing the influence exerted on the cooling process, by the temperature and density of the gas in which it is going on, it remains to discover in what manner, for a given condition of the fluid, that process may be found to depend on the difference of temperature.

We have already remarked, that the law which expresses this dependence is the same for each gas, though its elasticity vary. Let us see what happens when we pass from one gas to another, and for this purpose let us refer in the preceding tables to the rates of cooling due to the sole contact of air, of hydrogen, of carbonic acid, and of olefiant gas, these four gases having a tension of $0^m,72$.

Excess of temp. of the therm. over that of the gas.	Rates of cooling due solely to the contact of the gas.			
	Common air pressure, $0^m,72$.	Hydrogen gas pressure, $0^m,72$.	Carbonic acid pressure, $0^m,72$.	Olefiant gas pressure $0^m,72$
200	5°,48	....	5°,25	7°,41
180	4 ,75	16°,59	4 ,57	6 ,45
160	4 ,17	14 ,26	4 ,04	5 ,41
140	3 ,51	12 ,11	3 ,39	4 ,70
120	2 ,90	10 ,10	2 ,82	3 ,84
100	2 ,27	7 ,98	2 ,22	3 ,12
80	1 ,77	6 ,06	1 ,69	2 ,34

Dividing the numbers in the third column by those of the second, we find as the ratio of the loss of heat, occasioned by common air and by hydrogen gas,

3,49 3,42 3,45 3,48 3,51 3,43.

And as to make these perfectly alike, it would only be necessary to alter the rates from which they are derived by quantities, far within the limits of those errors to which every observation is subject ; we may safely conclude that the law we are in search of, is the same for hydrogen as for common air.

The same consequence is derivable, with regard to the two other gases, by taking the ratios of the rates of cooling, as given in the second column, with the rates as found in each of their columns. Thus for carbonic acid, we shall have

,958 ,962 ,968 ,965 ,972 ,977 ,955 ;

and for olefiant gas

1,35 1,36 1,30 1,33 1,32 1,37 1,32.

The law of cooling, then, as far as that depends on the contact of a gas, is independent of the nature and of the density of the gas. And it will be obvious, on comparing any of the above series with an analogous one, of the rate of cooling in a vacuum, that the law we are seeking is different from that of radiation.

After a great many trials, of which it would be useless here giving any account, we found that the rate of cooling, due to the sole contact of a gas, varies with the excess of temperature of the body, according to a law which has some analogy with that connecting the cooling power of a gas with its elasticity ; that is to say,

[1] *An Experimental Enquiry into the Nature and Propagation of Heat*, p. 486.
[2] *A New System of Chemical Philosophy*, part 1. p. 121.

that the quantities of heat[8] carried off by a gas from any body, form a geometric progression, the excesses of temperature being also in geometric progression. The ratio of the latter being 2, that of the former will be 2,35, and by a similar transformation to that before given, this truth resolves itself into the following more general one, that is to say, that *the loss of heat, due to the contact of a gas, is as the excess of temperature raised to the* 1,233 *power.*

To give an idea how well this law represents the phenomena, we give, in the following table, the cooling effect of common air, under a pressure of $0^m,72$, the second column containing the observed results, and the third the calculated ones derived from above the law.

Excess of temperature.	Rates of cooling observed.	Rate of cooling calculated.
200°	5°,48	5°,45
180	4 ,75	4 ,78
160	4 ,17	4 ,14
140	3 ,51	3 ,51
120	2 ,90	2 ,91
100	2 ,27	2 ,31
80	1 ,77	1 ,76
60	1 ,23	1 ,24
40	0 ,77	0 ,75
20	0 ,33	0 ,32

It would be useless multiplying these comparisons, as derived from observations made with the other gases, and at various pressures, for we have already shown that each gas follows exactly the same law, and this whatever the pressure. And the comparisons to which we allude, have given us equally satisfactory results with the preceding, a fact which may be easily verified on any of the series we have registered.

To have a general expression for the rate of cooling, as affected by the contact of a gas, it is necessary to collect together the several particular laws which we have established. The first teaches us, that the state of the surface of the body has no influence on the quantity of heat abstracted by the gas; the second shows, that the density and temperature only affect the cooling process, inasmuch as they alter the pressure; so that the cooling power of this fluid depends simply on its elasticity. This elasticity, and the excess of temperature, then, are the only elements by which the rate of cooling is affected. If the first be expressed by p, and the second by t, we shall have, for the expression of V = rate of cooling

$$V = m\ p^c\ t^b$$

b being for all bodies and all gases = 1,233; c being constant for all bodies, but varying with the gas; and m depending for its value not only on the nature of the gas, but also on the dimensions of the cooling body. The values of c are, as we have already found, 0,45 for air, 0,38 for hydrogen, 0,517 for carbonic acid, and 0,501 for olefiant gas. The values of m depend, as we have already said, on the dimensions of the cooling body, and on the nature of the gas. For the thermometer we used, m is equal to 0,00919 in air, 0,0318 in hydrogen, 0,00887 in carbonic acid, and to 0,01227 in olefiant gas. These values of m, suppose p to be expressed in metres, and t in degrees of the centigrade scale. We may, by means of this expression for V calculate the ratios between the cooling powers of the several gases, and for each pressure. Thus, in taking unity to represent the cooling power of air, and supposing the pressure = $0^m,76$, we have for the cooling power of hydrogen 3,45, and for that of carbonic acid 0,965. These numbers would change with the elasticity of each of the gases—a fact not perceived by Messrs. Dalton and Leslie, though easily deducible from our formula; their determination, however, for the tension $0^m,76$, differs but little from those we have just calculated. We shall return presently to this accidental agreement between their experiments and ours.

The simplicity of the general law we have just explained made us very desirous of verifying it on higher temperatures than any we could command in the

[8] By quantities of heat, is meant, loss of temperature, i. e. depression of the thermometer. The expression, however, is objectionable, and eminently unphilosophical. —Ed. Gl.

preceding experiments. We have effected our object by means of a very simple arrangement, the idea of which is due to Mr. Leslie.

When the thermometer, with naked bulb, cools in air, the total rate of cooling is made up of the rates due separately to contact of air and radiation. If these rates be put $= \nu$ and ν' the total rate will be $\nu + \nu'$. If again the thermometer have its bulb covered with silver leaf, the rate ν, due to the contact of air, remains the same for the same excess of temperature, and ν' becomes $\frac{\nu'}{5{,}707}$ since the ratio of the radiating powers of glass and silver is 5,707. The total effect of the cooling process, in the case of the silvered thermometer, will then be $\nu + \frac{\nu'}{5{,}707}$. Hence it is easy to see, that to determine the loss of heat at every temperature, produced by contact of air, it is sufficient to determine the total rates of cooling for the thermometer; first, with its natural surface, and then with a silvered one. These rates of cooling being represented by a and b, we shall have

$$a = \nu + \nu'; \; b = \nu + \frac{\nu'}{5{,}707}$$

whence

$$\nu = (b - a) \frac{5{,}707}{4{,}707}$$

Let us apply this formula to the results contained in the accompanying table:

Excess of temp. of the therm.	Total rates of cooling. glass bulb.	Total rates of cooling. silvered bulb.	Values of ν.
260°	24°,42	10°,96	8,10
240	21 ,12	9 ,82	7,41
220	17 ,92	8 ,59	6,61
200	15 ,30	7 ,57	5,92
180	13 ,04	6 ,57	5,19
160	10 ,70	5 ,59	4,50
140	8 ,75	4 ,61	3,73
120	6 ,82	3 ,80	3,11
100	5 ,57	3 ,06	2,53
80	4 ,15	2 ,32	1,93

The second and third columns contain the total effects of cooling of thermometers, with naked and with silvered bulb, for excesses of temperature, as contained in the first. The last column contains the corresponding values of ν deduced from the above formula; that is to say, the loss of heat which contact of air alone would occasion in each of the thermometers. Now, according to the preceding part of this paper, the law which loss of heat, proceeding from this cause follows, is expressed by the equation

$$\nu = nt^{1,233}$$

in which n must always be determined for each particular case. In that we are considering $n = 0{,}0057$. If for t be successively substituted the numbers from 80° to 260° we shall have corresponding values of ν, which will not be found to differ much from those determined by experiment. This may be seen by comparing the second and third columns of the following table:

Excess of temperature.	Value of ν from obsn.	Value of ν as calculated.
260°	8°,10	8°,14
240	7 ,41	7 ,38
220	6 ,61	6 ,63
200	5 ,92	5 ,87
180	5 ,19	5 ,17
160	4 ,50	4 ,47
140	3 ,73	3 ,79
120	3 ,11	3 ,14
100	2 ,53	2 ,50
80	1 ,93	1 ,90

Thus the law we have announced, as representing the loss of heat, occasioned by the contact of air, is confirmed, when we extend our researches to greater excesses of temperature. The results already tabulated, will furnish even the means of verifying the law of cooling *in vacuo.* It is sufficient for this purpose to deduct from the total effect in the air, the rate which would be occasioned by the sole contact of air, that is to say, the successive values of v. The several remainders will evidently be the rates of cooling as due to radiation, or which comes to the same thing, those which would be observed *in vacuo*.

We shall here give the numbers determined in this way for the thermometer, with naked bulb, and also the rates calculated from the law of cooling *in vacuo.* These rates are, we know, expressed by the formula

$$m\ (a^t - 1.)$$

t being the excess of temperature of the body; m a constant coefficient, which must be determined for each case, and which here is equal to 2,61; and finally a being the exponent 1,0077 common to all bodies.

Excess of temperature.	Rates of cooling *in vacuo*, deduced from observations in the air.	Rate of cooling *in vacuo*, as calculated.
260°	16°,32	16°,40
240	13 ,71	13 ,71
220	11 ,31	11 ,40
200	9 ,38	9 ,42
180	7 ,85	7 ,71
160	6 ,20	6 ,25
140	5 ,02	4 ,99
120	3 ,93	3 ,92
100	3 ,04	2 ,99
80	2 ,22	2 ,20

It will be seen by the example just given, that we may, by observing directly the cooling process in the air, determine separately the loss of heat due to contact of the air and to radiation, and that to effect this purpose we must observe the cooling of the same body with different surfaces.

But this method of calculation depends, in the first place, on the supposition, that the quantity of heat taken up by the air is independent of the nature of the surface of the body; and in the second place, on this principle, that bodies of different surfaces, preserve at every temperature, the same ratio between their radiating powers. These two propositions are rigorously true, but they could not possibly be established except by direct experiments; such, in fact, as we have given an account of in this paper. And though Mr. Leslie has adopted them in the use he has made of the principle we have above explained, his results are not the less inaccurate, as he has always calculated the rate of cooling after the law of Newton.

The laws relative to each of the two effects which are combined in the cooling of a body plunged in a fluid, having been separately established, it is sufficient to combine them to have the total cooling effect.

The rate v then of this cooling, for an excess of temperature, t will be expressed by the formula

$$m\,(a^t - 1) + nt^b.$$

The quantities a and b will, for all bodies, and in every fluid, be equal to 1,0077 and 1,233. The co-efficient m will depend on the size of the body and nature of its surface, as well as on the temperature of the surrounding matter. The co-efficient n, independent of this temperature as of the nature of the surface, will vary with the tension as well as with the kind of gas in which the body will be immersed, and these variations will follow the laws we have already explained.

This formula shows us, in the first place, as we have stated in the beginning of this memoir, that the law of cooling in elastic fluids changes with the nature of the surface. In fact, when this change takes place, the quantities a, b, and n, preserve their values, but the co-efficient m varies proportionally to the radiating power of the surface. If the new value be represented by m' the rate of cooling will become

$$m' (a^t - 1) + nt^b.$$

a quantity which is no longer proportional to

$$m (a^t - 1) + nt^b.$$

when t changes.

Let us inquire how the ratio of these rates of cooling varies ; and suppose, for clearness, that m is greater than m', that is to say, that it refers to the body, the radiation of which is most powerful.

We may easily satisfy ourselves, by the rules of the differential calculus, that the fraction

$$\frac{m (a^t - 1) + nt^b.}{m' (a^t - 1) + nt^b.}$$

becomes equal to $\frac{m}{m'}$ when $t = 0$ or ∞

If we suppose t very small, the quantity $a^t - 1$ becomes equal to t log. a, and the preceding ratio, dividing by t log. a

$$\frac{m + \frac{n}{\log. a} t (b-1)}{m' + \frac{n}{\log. a} t (b-1)}$$

Under this form, it is evident that the ratio ought to diminish as t increases, b being greater than 1. But after having diminished, this ratio will augment, because it ought to have the same value when t is infinite as when it $= 0$. Hence the conclusion is obvious, as stated at the commencement of this memoir, that when we compare the law of cooling in bodies of different surfaces, the law is most rapid in the low temperatures for those bodies which radiate least, and least rapid, on the contrary, in elevated temperatures.

This truth may be easily verified in the following table, in which have been inserted the rates of cooling, both of the thermometer with naked and with silvered bulb ; as also the ratios between them :

Excess of temp. of the thermometer.	Rate of cooling		Ratios.
	Of the therm. with naked bulb.	Of the therm. with silvered bulb.	
260°	24°,42	10°,96	2°,23
240	21 ,12	9 ,82	2 ,15
220	17 ,92	8 ,59	2 ,09
200	15 ,30	7 ,57	2 ,02
180	13 ,04	6 ,57	1 ,98
160	10 ,70	5 ,59	1 ,91
140	8 ,75	4 ,61	1 ,89
120	6 ,82	3 ,80	1 ,80
100	5 ,56	3 ,06	1 ,81
80	4 ,15	2 ,32	1 ,78
60	2 ,86	1 ,60	1 ,79
40	1 ,74	0 ,96	1 ,81
20	0 ,77	0 ,42	1 ,85
10	0 ,37	0 ,19	1 ,90

The mere inspection of the numbers contained in the last column, fully confirms the fact above announced. We see also that the ratios of the rate of cooling of the two thermometers remain much the same for excesses of temperature comprised between 40° and 120°. This circumstance, which results from this, that after diminishing, the ratios augment, has probably contributed to persuade Mr. Dalton, that the law of cooling in the air is the same for all bodies. If this series were prolonged, we should find that the ratio, which is already 2,23 for an excess of 260°

increases rapidly as this excess augments; and that it continually approaches the number 5,707 which is the limit, this being the value of the fraction $\frac{m}{m'}$ in the case of glass compared with silver. This examination offers a new proof of the necessity of extending, to a very large interval of temperature, our inquiries with regard to certain phenomena of heat; as it also well explains how Mr. Leslie has been led to results so different from those we have found. In fact, this celebrated philosopher, beginning with observations made at low temperatures, has thought that the ratio in question would continue to diminish, until finally it would become unity; so that, according to him, the total loss of heat in very elevated temperatures, would be nearly independent of surface. But indeed the laws proposed by Mr. Leslie, by Mr. Dalton, or still earlier by Martin, may all be refuted by a single argument; for all these laws make the rate of cooling depend solely on the excess of temperature of the body over that of the surrounding medium; whereas experiment shows that this rate changes, in a marked degree, with the temperature of the fluid which surrounds the body.

It is, therefore, useless to enter into any discussion on the subject; for admitting even that the laws imagined by them did represent the results of experiment, within the limits of their inquiries, it is certain, from all that precedes, that, in extending them beyond these limits, we should obtain results very far removed from the truth.

We may, by considerations analogous to those of which we have previously made use, determine in what way the law of the total cooling effect changes in the case of any given body for a change in the nature or in the density of the gas.

The total effect of cooling is expressed by the formula

$$m\,(a^t - 1) + nt^b.$$

If we suppose another gas, or even the same gas, under another pressure, the rate of cooling for the same body will be

$$m\,(a^t - 1) + n'\,t^b.$$

for the co-efficient, n is the only one which, in this case, would change.

Comparing these two expressions, we shall find, that their ratio becomes equal to unity, whether we make $t = 0$ or $t = \infty$. Thus the total effect of cooling in different gases, becomes at high temperatures nearly the same; while again, in the lower part of the scale, the rates may be very different. This fact suffices to shew the inaccuracy of the proceedings by which Messrs. Dalton and Leslie attempted to compare the loss of heat, occasioned by the contact of various elastic fluids; for these proceedings are founded on the supposition, that the total rates of cooling, as observed in different gases, preserve the same ratio, whatever the temperature. By a singular coincidence, however, which it is unnecessary to dwell on, the particular temperature at which they operated, renders their error of less consequence; their determinations are, therefore, as we have before observed, sufficiently approximate, provided they are not extended to circumstances different from those in which they were made.

The necessity of separately determining the influence of each of the causes which affect the progress of cooling in any body, not having allowed us to give a connected view of the different laws to which we have been led—we have thought that it might be useful to give a short recapitulation of them, in which the natural order of their sequence, often broken by accounts of experiments or discussions, and of results, might be attended to.

Distinguishing, as we have done, the losses of heat due to the separate causes of conduction, by contact of fluids and radiation, it is soon perceived, that each of these effects has its own law. These laws must express the relations which are found between the temperature of the body, and its rate of cooling for every possible circumstance. It is to be recollected, that by rate of cooling, we mean the number of degrees by which the temperature of the body would be depressed, during a certain indefinitely small interval of time.

1st Law. If it were possible to observe the cooling of a body placed in a vacuum, bounded by non-radiating matter, the rate of cooling would diminish in geometric progression, while the temperature diminished in arithmetical progression.

2nd Law. For the same temperature of the matter encircling the vacuum in which the cooling body is placed, its rate of cooling, for excesses of temperature in arithmetical progression, would decrease as the terms of a geometrical progression diminished by a constant number. The ratio of this progression is the same for all bodies. and is equal to 1,0077.

3rd Law. The rate of cooling *in vacuo*, for the same excess of temperature, increases according to a geometric progression, the temperature of the surrounding matter increasing in arithmetical progression. The ratio of this progression is also 1,0077 for all bodies.

4th Law. The rate of cooling, as affected by the contact of a gas, is entirely independent of the surface of the body.

5th Law. The rate of cooling, as due to the contact of a gas, varies in geometric progression, the excesses of temperature forming also a geometric progression. If the ratio of this second progression be 2, that of the first will be 2,35, whatever the nature of the gas, or whatever be its tension.

This law may also be stated, by saying, that the heat abstracted by the contact of a gas is, in all cases, proportional to the excess of temperature of the body raised to the power 1,233.

6th Law. The cooling power of an elastic fluid diminishes in geometric progression, when its tension diminishes in a geometric progression. If the ratio of this second progression is 2, the ratio of the first is 1,366 for air; 1,301 for hydrogen; 1,431 for carbonic acid; and 1,415 for olefiant gas. Or we may state this law as follows: The cooling power of a gas is (all things else being equal) as a certain power of the tension. The exponent of this power, which depends on the nature of the gas, is for air ,45; for hydrogen ,315; for carbonic acid ,517; and for olefiant gas ,501.

7th Law. The cooling power of a gas varies with its temperature, but in such manner, that if the gas can dilate itself, so as to continue under the same pressure, or preserve the same elastic tension, the cooling power will be as much diminished by its rarefaction as it is increased by its rise of temperature, so that actually the cooling power depends only on the tension.

It may be seen, by the statement of each of these propositions, that the total law of cooling, which must be made up of a combination of all these particular laws, must be very complicated; we shall not, therefore, attempt to express it in ordinary language. We have given it under a mathematical form in the course of this memoir, which will allow a discussion, as to all the consequences to which it may lead.

We shall be satisfied to remark here, that it is, without doubt, to the extreme complication of this law, considered as a whole, that we are to attribute the little success which has hitherto attended the several efforts made to discover it. It was evidently impossible to effect the solution of the question without studying, one by one, the several causes which combine to give the total effect.

III.—*Examination of the Minerals collected by* E. Stirling, *Esq. at the Turquoise Mines, near Nishapúr in Persia. By* J. Prinsep, *Esq. F. R. S. Sec. Phys. Cl. As. Soc.*

[Read at a Meeting of the Physical Class of the Asiatic Society, 27th Oct. 1830.]

An interesting account of these celebrated mines will be found in Fraser's late travels in Korasán, page 408—20. As, however, that work may not be at hand for immediate reference, I shall take the liberty of repeating a few of the author's local observations, before proceeding to a description of the minerals brought thence by Mr. Stirling.

The hill, in which the mines are situated, lies about 40 miles N. W. of Nishapúr, and is connected with those that separate its plain from Curdistán, being a branch from the Elberz range: the latter are of a primitive character, but the hill of Madán totally differs from them in appearance and formation.

The approach from the eastward is by a long glen, in which there is a quantity of conglomerate, composed of various sized pebbles. The hillocks are generally of an ocureous earth under decomposition, with occasional patches of a substance resembling red chalk.

The whole range is evidently much tinged with iron: in some places the metal may be seen in the form of specular ore, in veins and masses.

The turquoise, or *fírózah*, as it is called by the natives, is found only in the principal hill of the cluster; that is to say, no mine has hitherto been discovered, or, perhaps, sought for, in the others; it is found in most, if not in all, of the substances of which the hill is composed, but principally in four: in a dead grey earth; in the heavy hard brown rock; in the soft yellow stone; and in the specular iron ore. There are five principal mines or pits from whence the gem is taken. The first to which we were led, is called the "Kerúch," which signifies, "that which has made its appearance;" this is by no means so old as the others: the stones are here found in loose soil, of a whitish grey colour, attached to pieces of the matrix, dug from small pits, on the side of a hill: this earth, and the small stones it contains, the workmen turn over and over, and find fragments of the gem as if by chance.

"We wound along the side of the hill, rising, to a great mass of dark brown stones, the accumulated refuse thrown out of several pits dug under an overhanging rock, upon the hill side. Among this debris we found several pieces, with fragments of the gem sticking to them in various forms; some in that of small round pimples, of an exquisite blue, budding, as it were, from the dark brown stone. In the excavations there lay a quantity of the fragments hewn from above, to be broken up for their contents; and upon examination, both above and below, the rock was discovered to be full of little veins containing the blue matter of the turquoise. These fissures shewed no signs of stratification: beneath the solid rock, and in the crevices, there was a congeries of pebbles like the aggregates before mentioned—yellow, red, grey, and brown; and in this mass pieces of the gem are frequently found. In one of the excavations, a white efflorescence was observed, which the natives said was *zeng* (alum), and which, they informed us, abounds hereabouts. On the roof, too, there were several patches of a fine verdigris blue colour, that led me to suspect the presence of copper, although the natives denied having ever heard of copper in the hill, and attributed this appearance also to the presence of the *zeng*. Could this have been the colouring matter of the turquoise, laying hold of some other substance adapted to receive it, though calculated to vary its shade?"

"In another mine the turquoise matter did not appear to run so much in veins: on breaking some promising pieces of rock, a quantity of white or light green stuff was found, which might have been taken for turquoises in an imperfect state. Others there were of sufficient hardness, but of a dead pale hue: had these been possessd of a full degree of colour, they would have been worth an immense price; as it was, they were worthless. In some instances, the stone was found adhering to the hard rock; in others, dispersed in spots through its softer and more decayed parts. Thus there was no general inference to be deduced, from what was seen, that would account for its mode of formation; for although the finer specimens are very sparingly sprinkled, the turquoise matter *seems to pervade the whole hill* in greater or lesser degree.

"The mines, being the property of the crown, are farmed out to any one who chooses to give the rent required: which was, in 1821, two thousand *tománs*. The inhabitants of the two neighbouring villages have, time out of mind, had a monopoly of the mining operations. The manner in which they conduct the business, is as follows: one hundred villagers take the whole mines, and work in parties of from five to ten, dividing the produce of their labour collectively. The produce is either sold to the merchants who resort here to trade, or it is sent to Mèshed.

"The turquoise is sold in three shapes; 1. As single stones, freed from the matrix, and ground so far as to expose the size, shape, and colour of the gem, but not polished: in this state, by wetting it, any flaws it may contain can be perceived, and the true value ascertained. 2. Separated from the rock before grinding, and covered with the impure crust that envelopes it in the vein. 3. In lumps, with the matrix rock very loosely knocked off, at variable prices, from 3 to 30 *tománs* for a *marcn*, ($7\frac{1}{4}$ lbs.)

"Four miles down the glen, are situated some mines of finely crystallized salt."

The series of specimens put into my hands, by Mr. E. Stirling, comprehend all the varieties alluded to in the foregoing description:—the detached rounded pieces, the botryoidal concretions, and the disseminated veins; the colour of them likewise varying from white earthy, to dead pale blue, genuine turquoise blue, greenish, and full green, passing finally into ochreous pyrites.

The matrix may be called, generally, clay iron ore; with abundance of pyrites, lithomarge, jaspers, specular iron, crystallized carbonate and sulphate of lime. Hornblende, augite, amethystine quartz, lithomarge and felspar, occur in the mines themselves, or in the immediate neighbourhood; proving the hills to belong, in all probability, to the volcanic formation.

A full description of this mineral, as distinguished from the bone-turquoise, has been given by Mr. President Fischer, in a pamphlet published at Moscow, in 1818;

he restores in calaite the name under which it was known to Pliny (*calais,*)—and distinguishes a "blue conchoidal" and a "greenish quartzy variety" to which he assigns the names of agaphite and Johnite, with a passion for multiplying names too often to be found among mineralogists. The genuine turquoise was analyzed by professor John, of Berlin, who found its specific gravity 2,860, and composition as follows :

Argil,	73,
Oxyde of copper,	4,5
Water,	18,
Oxyde of iron,	4,
Lead and loss,	0,5
	100

The other varieties have not, it appears, been examined ; but the specific gravity of the agaphite has been set down as from 3. to 3,25, and the colouring matter was supposed by Gahn to be *arseniate of iron.* But the specific gravity of the *odontolite,* or tooth-turquoise, is 3,1, and the colouring matter has been ascertained to be phosphate of iron. Probably, therefore, the specimen examined by Gahn was of this species. Fischer suggests that the Johnite may contain much silex, from its quartzy appearance and hardness.

The specimens now before me, confirmed by Fraser's account, prove that all the varieties, white, blue, and green, are but gradations of the same mineral : whether such a conclusion is borne out by chemical analysis, remains to be examined, as well as what may be the cause of the observed differences of colour. The hardness of the green and blue stones is equal ; they are not scratched by quartz. Their specific gravity is 2,76 at 70° Far.

Having to work upon very small portions of the mineral in the following examination, I do not give the quantities as rigidly correct ; the operations were hardly calculated for more than a test or trial analysis.

A. The first process was to appreciate the quantity of water, by heating the pounded mineral red hot, in a glass tube, with filtering paper to absorb the disengaged vapour, and strips of turmeric and litmus paper, to recognize the presence of freed alkali or acid.

	Mineral.	*Water, per cent.*	*Acid.*
No. 1.	White friable turquoise,	19,	trace of acid.
2.	Precious blue ditto,	18,62	perfectly pure water.
3.	Bright green variety, encrusted with pyrites,	18,	litmus hardly reddened when the specimen was clean ; when otherwise, much sulphuric acid was given off.
4.	Decomposing white feldspar,	4,	reddening litmus slightly.

From the above trials, it seems that the water was a definite ingredient in the three first minerals, being in amount (as in John's analysis) about 18¼ per cent. It is curious, also, that the water in the feldspar bears the same proportion to the alumina of that mineral, as 18½ does to the alumina of the turquoise, or 80 : 18,5 : : 17 : 4.

These proportions nearly correspond with an atomic compound of 2 parts water, and 3 alumina, making the compound a sesqui-hydrate.

B. The second process was, to ascertain the nature of the metallic colouring matter by means of the blowpipe.

All of the specimens behaved before the oxydiging flame, like the generality of minerals whose chief ingredient is alumina ; dissolving, gradually, with little or no effervescence, and forming clear glasses with borax. The colours varied as follows :—

No. 1. Turquoise, white—very faint blue glass.
2. Ditto, blue—light blue glass, becoming emerald green when any of the outer crust was taken up.
5. Ditto, light green blue—emerald green glass.
3. Ditto, bright green—dirty yellow green or brown glass.
6. Yellow ochreous earth —brown or amber glass, with a slight cast of green.
4. Feldspar—pale yellowish tinge.

The first impression upon these experiments was, that the blue beads were coloured by a minute portion of cobalt, the green by copper, and the brown by iron; as such are the colours ascribed to the oxydes of those metals in all treatises upon the blowpipe. However, upon making a series of comparative assays with the pure oxydes themselves, on a flux of borax, I obtained a satisfactory explanation of the difficulty which had arisen from placing implicit confidence in book authorities.

Pure oxyde of copper yielded invariably a *blue glass*, varying in depth, according to the quantity present in the assay; while hot, it was at first red, then emerald green, but it always became blue on cooling.

Pure oxyde of iron gave an *umber* coloured glass, hardly visible with minute portions, and increasing to a deep red brown, with more of the oxyde.

The mixed oxydes of copper and iron yielded every variety of green, as might have been expected, from the former results: with minute portions of the two, a neutralization of the colours, (*umber and blue* being nearly complementary to one another) seemed to take place; and the glass was less coloured than with either of the oxydes alone; the depth of shade, therefore, is not a perfect test of the quantity of each metal present: yet, with a standard set of prepared glasses, the blowpipe examination might be rendered capable of affording quantitative results with greater accuracy than any other method, considering the minuteness of the specimen operated upon.

The genuine, or precious turquoise appears, then, to be coloured wholly by oxyde of copper; and as the mineral becomes tinged with green, it is in consequence of contamination with oxyde of iron, until in the bright green variety the latter metal greatly predominates. It may be remarked how perfectly this result corroborates the observation of the native miners, that the green colour is caused by the impregnation of *zeng*, which rather signifies vitriol, or sulphate of iron, than alum, as it is translated by Mr. Fraser. The miners are also correct, as to the fact of there being no considerable ore of copper among the mineral products of the neighbourhood: the blowpipe does barely detect a trace of this metal in the earthy sulphuret of iron or decomposing pyrites, which may, probably, be the source whence it has been acquired by the turquoise.

C. The third process of analysis was directed towards the separation and estimation of the constituent earths and oxydes of the turquoise.

In its natural state, the mineral is not affected by the acids; but after calcination it becomes, for the greater part, soluble in them. The residue in four experiments varied from 5 to 6 per cent. and was found to consist of alumina and red oxyde of iron, with a small portion of silex. As it was impossible to free the surface of the stones, in the first instance, entirely from their ferruginous crust, I am inclined to regard the whole of the insoluble matter as extraneous; more so, as it was evidently smaller in amount in proportion as more care was taken in selecting the specimens for the mortar.

The nitric solution, of a green tinge, being evaporated to incipient dryness, and redigested in boiling water, left the greater part of its earthy deposit undissolved, while the liquid itself remained turbid, as if holding the matter taken up, merely in suspension. At first it was imagined that the deposit must be chiefly siliceous, since the above is the formula provided for the separation of that earth in the humid analysis: either nitric or sulphuric acid, however, dissolved the whole without residue; and when a proper portion of potash or ammonia was added to the solution in the latter acid, the whole was converted into pure crystallized octohedral alum.

It is worth while to draw the attention of the inexperienced to this property of the earth of alumina, which might otherwise occasion frequent errors in the analysis of minerals, where the usual preliminary process of fusion with potash or soda, is not resorted to. The fact seems to be as follows:—When a fixed alkali or alkaline salt is present in the magma, the alumina will be redissolved on the addition of water; but the earth alone (for the nitrate seems to be decomposed by simple drying) is by no means re-soluble in water.

Such part of the alumina as was still suspended in the liquid, being thrown down by ammonia, and the gelatinous precipitate (which was tinged with oxyde of copper) collected in the usual manner, dried, and added to the first deposit; the alumina was found to weigh 7 grains, or 70 per cent.

D. The blue ammoniacal solution evaporated to dryness, yielded dark green oxyde of copper, with a slight glaziness, from a remnant of alumina, or perhaps of alkali, weighing altogether 5,3 per cent.

A previous test analysis had proved the total absence of glucina, magnesia, and, manganese, and only a minute, if any, contamination of lime. Some iron, as well as copper, generally accompanied the ammonio-aluminous precipitate, but this was attributed to the foreign matter above mentioned.

Limiting, therefore, the ingredients of genuine turquoise, to the matter soluble after calcination, the composition, according to this analysis, will be found, by increasing all the results 6 per cent. which will give as follows :—

Alumina,	74,2
Water,	19,7
Oxyde of copper,	5,5
	99.4

This composition does not differ much from the result of Professor John's analysis, deduction being made in his of the oxyde of iron: the oxydes of copper and iron, I have already argued to be variable in quantity, and bestowing as variable a colour upon the mineral.

The precious variety of turquoise may, however, be safely regarded as a *sub-hydrate of alumina and copper.*

E. The bright green variety was analyzed in a similar manner to the preceding, substituting nitro-muriatic for nitric acid, so as to dissolve the oxyde of iron: the quantity used weighed but 3½ grains, so that much accuracy cannot be expected; the composition appeared to be as follows :—

Alumina, (with trace of sulphuric acid,)	62
Water,	18
Protoxyde of iron,	18
Protoxyde of copper,	2
	100

F. Two grains of the white earthy turquoise, analyzed in the same way, although previously calcined, by which operation it lost 19 per cent. of water, left a residue of about 30 per cent. insoluble in nitric acid, which was, probably, silex or silicate of alumina. The carbonate of ammonia also precipitated only 30 per cent. of alumina from the nitric solution :—the copper was small in quantity, and no iron was recognized by its proper tests ;—a deficiency of 20 per cent. therefore, remained unaccounted for ; but the desire of preserving the rest of the specimen, prevented my pursuing the inquiry further than to ascertain that there was barely a perceptible quantity of sulphuric acid present. This variety is, of the three, the least soluble ; contrary to what would have been expected from its friable decomposed state. It is, perhaps, improper to call it *decomposed,* for this is more likely to be the nascent form of the turquoise, after the disintegration of the felspar, by the action of the pyrites contained in it, and the lodgement of a part of the ingredients, the hydrate of alumina in particular, in the circular cavities or air bubbles of the iron stone.

IV.—*Observations on Shooting Apparatus; with directions by which every Sportsman or his servant may, with very little expense or trouble, make a new sort of Charges or Cartridges, on a safe and simple principle. By Mr.* J. Jenour.

[From the Mechanics Magazine.]

Though much has been written, and the most contradictory notions have prevailed on the present subject, it appears reducible to a few simple facts.

The sportsman's principal object, is to combine safety to himself and others, with every possible convenience and quickness of loading, and the utmost possible effect of his gun ; that is to say, *to make surety of any single head of game (without being liable to mangle it,) at any point between the nearest and the greatest distance practicable.* In this view (admitting the gun to be of the best materials and workmanship), the subjects for consideration appear to be—1. Size of the bore. 2. Deviation from a cylinder. 3. Length of barrel. 4. Size of shot and proportions of charge. 5. Wadding. 6. Leading and foulness of barrel. 7. Clubbing or balling, either with loose shot or cartridges of different sorts. 8. The spread or disk of the shot, by different ways of loading, &c.

First, as to the size of the bore. Guns of small bore shoot sharper, or, in other words, throw their shot stronger than those of large calibre, proportionably ; thus, we may take as a maximum, the difference of a 16-gauge light gun, with an ounce and half of No. 6 shot ; and a duck gun, with a bore about the size of a half-crown piece, carrying a pound of No. 1 shot : in which case, the light gun would perform as well, or better, for a single bird, at 40 or 50 yards' distance, than the other would at 100 yards; the advantage of very large bored guns being confined to firing at numbers of fowl together. Hence we may conclude, that the smaller the bore the better, so that it will carry a sufficient charge of shot without too much recoil; but a small calibre leads and gets foul much sooner, and is more liable to burst than a large one, in the common way of loading: objections, however, which are entirely obviated by the new charges here recommended, which the writer considers as a peculiar advantage.

Secondly. Whether there should be any deviation from a cylinder is still doubtful in some respects; though the majority of gun-makers and others have approved of the slightest possible, and a very gradual enlargement from about the middle of the barrel to the breech, and a similar one from the middle to the muzzle, liable to some little variation, according to different opinions as to the part most proper for the contraction thus occasioned, and to the exact degree of opening or relief; but as this variation from the true cylinder is in general scarcely perceptible, and as there are many who prefer the cylinder, we may safely conclude the difference to be of not much importance in respect of the average shooting of the gun: for so infinitely varied are the results of trials at a mark, with even the same gun, and the same charge, that it seems almost impossible to arrive at any accurate conclusions, as to the difference in this or that system of boring, &c. But, in regard to safety, it is obvious, that should the contraction be too great, it might occasion a liability of bursting the barrel; and that any kind of opening in the lower part, is also more liable to occasion the same accident than a cylinder, because either a wadding or a cartridge, after passing the tighter part, cannot be so secure of fitting close when down, as if it had not to pass through any smaller part than where it is to remain: for, if it would expand by ramming well, even that trifling operation might happen to be omitted in the hurry of sport.

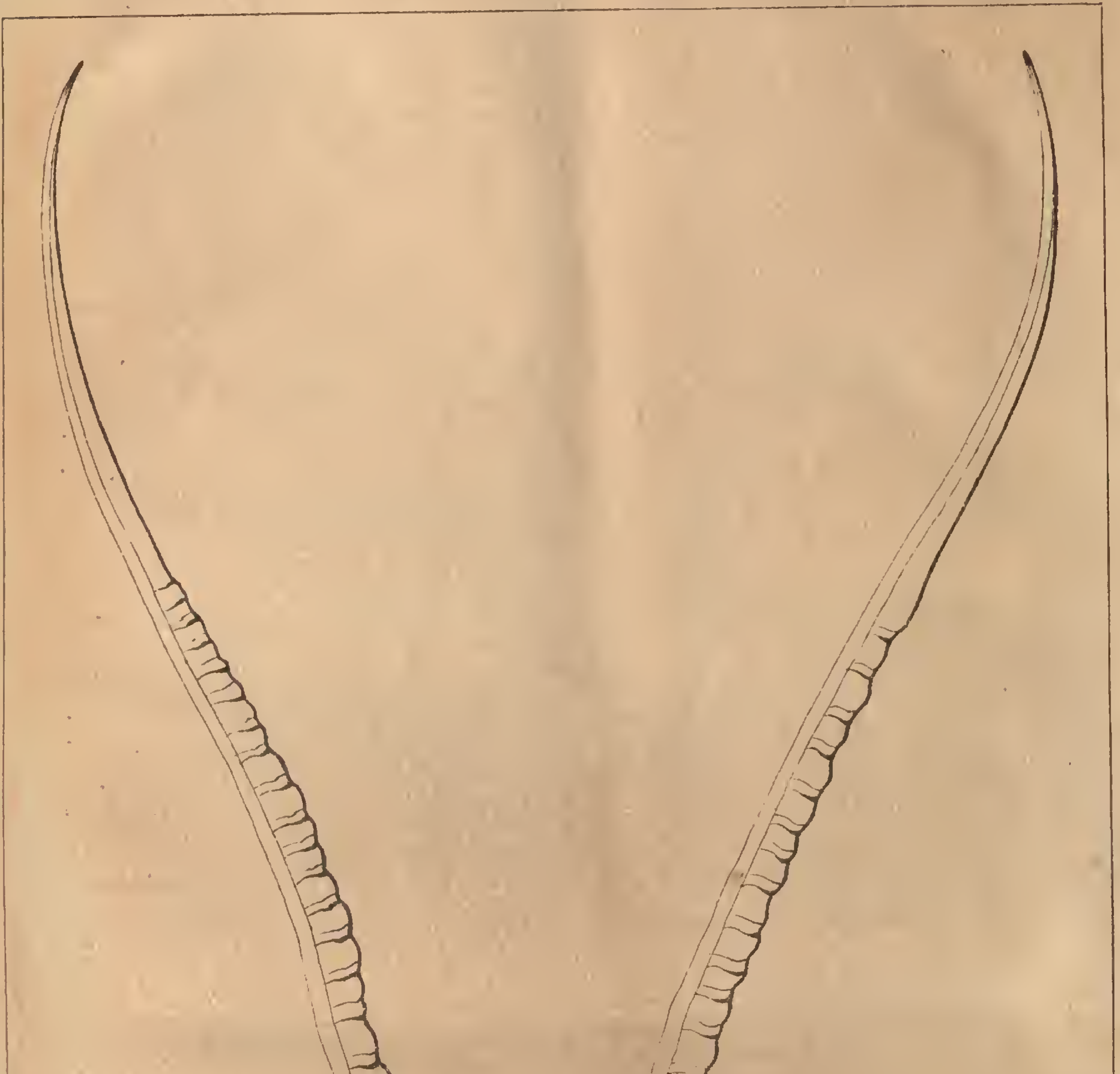
Thirdly. Length of barrel unquestionably increases the range of the shot, but in so trifling a degree, from a barrel of two feet to one of three or four feet, that, after all, it seems a mere matter of choice, and principally relates to the proportionable appearance of the gun: as to any length between the two extremes, length of barrel may, perhaps, afford some little advantage to a show shooter, but for the modern great quickness of action it must prove rather disadvantageous than otherwise.

Fourthly. With respect to the size of shot, the writer, having plucked and examined many pigeons *killed at measured distances, and made a table of particulars, can confidently say, that No. 8 has (and of course will) with the wire-cartridge,* in many instances, broken the *thick bone of the wing and leg*, or gone entirely through the thickest part of the body at 55 yards; and is thence led to conclude, that No. 8 is sufficient for any game, with equal measures of powder and shot, the shot being made into the cartridges, which will be presently described: but No. 6, which seems to be generally adopted, cannot perhaps be improved upon. As to the proportions, equal measures may be considered as the proper standard; and any material deviations therefrom is merely fanciful; the measure being regulated by the recoil, so that it is just felt without being disagreeably sharp.

Fifthly. Of wadding there is so great a variety as to puzzle the choice by the various recommendations; the common mill-board, however, certainly answers every purpose wadding can effect as to the shooting of a gun; but if any other sort does, as stated, either keep the charge more securely in its place, or prevent the leading and fouling of the barrel; it must be a very great advantage with loose shot; but as the cartridges to be described here effect every thing possibly required in these respects, the writer considers it needless to add any thing under this head.

Sixthly. The fouling of the barrel is perhaps one of the most important considerations belonging to shooting; it must obviously alter the form of the bore, and is altogether highly disadvantageous and dangerous. It, therefore, appears a most desirable object to load in a manner to do away with leading and fouling altogether, if practicable, by any convenient means calculated for general adoption and utility. This is the writer's object in the new cartridge, the stated merits of which, as it may be so easily tried, will probably be speedily either generally known and acknowledged, or disproved.

Seventhly. Clubbing or balling is supposed, by many, to occur only with cartridges, but this is a most dangerous mistake; and those who consider that they may fire confidently without any regard to the position of their friends at any considerable distance with loose shot are in danger—certainly a very remote one we



J. B. Tassin Lith.

HORNS of the CHÍRU Antelope.

may say—but still they are in such-of finding out their mistake by some irreparable accident; for all guns are liable to club or cluster, (which latter is the most likely to do mischief, as it is similar to firing several bullets or slugs,) and if but very rarely, still the bare possibility of the circumstance must leave something of an unsatisfactory feeling in the mind; so that the writer, under these impressions, considers, that he shall render a public service, if his new plan becomes generally adopted, and, therefore, trusts it will have a full and fair trial.

Eighthly. The spread, disc, or circular space covered by the shot, at any given distance, is, perhaps, on an average, not materially different beyond thirty or forty yards, either with different guns or charges, (except when a very large or very small proportion of powder is used;) nor even with the wire cartridge, as regards that circumference which takes in the greater portion of the charge, merely excluding a few very wide straggling pellets; but there is a great difference as to the manner in which the shots are dispersed over such disc: thus, as the wire-case retards the dispersion, of course there must always be a greater number of pellets about the central part of the disc than with loose shot, or with any other cartridges that do not, by some other means, retard the dispersion. And when the common charge clubs more or less, it sometimes, by gradually dispersing, thus acts somewhat as a wire cartridge. But though there is not much variation in the space covered by different guns and charges, there is, as before observed, a great difference in different parts of such circle, and therefore there must, of course, be a similar variation, as to the number of pellets put into a sheet of paper; but, as before noticed, not a much greater difference than occurs with the same gun and chargein different trials. These circumstances are attributable, no doubt, to the very uncertain manner in which the shots dispose themselves when separating. The penetrating force (strength, as it is termed,) of the pellets also varies extremely; and no doubt, the cause of many of the weak pellets, with loose shot, when no substance is employed to fill the interstices, is, that all those which come in contact with the barrel, are nearly flattened in passing through it, by the violent compression and friction which occurs, but which the dust, &c. by filling up the spaces between the pellets, completely prevents. Having thus far, and in the most concise manner he was able, stated his ideas, the result of numerous experiments, and long attention to the subject, the writer submits them with deference to those conversant with it; and now proceeds, in as few words as possible, to explain the principle of his new cartridges, and every thing requisite to enable any person to make them.

The principle of the new cartridge (or *brittle charge*, which the inventor considers a more appropriate appellation,) consists in the charge of shot being held together simply by a uniform *brittle* substance occupying the interstices, instead of any sort of paper or other case or covering which all other cartridges have, and which cases, by failing to rupture sufficiently for the egress of the shot, occasion thereby the circumstance of balling, an accident which cannot possibly occur with a charge or cartridge cemented together into its proper form by a brittle substance only: for the firing of the powder, by acting similarly as a blow of a solid body, as *infallibly breaks the whole of such brittle substance into the smallest particles, as a violent blow of a hammer breaks a piece of glass.*

There are several other very material advantages attending this principle of forming the charge of shot ready for loading with; besides this most important one of impossibility of balling, which being set down as the first, we may add—second, facility and cheapness of making; third, exactness of sizes, being, in this respect, similar to bullets cast in one mould; so that when once the proper fit for a gun is obtained, it will always be preserved in any number of charges required; and from the solidity of conformation, will not in the least alter in form by carrying about, which all other cartridges are liable to do. Fourth, the charges of one making, or batch, like a batch of bread or biscuits, must be all the same in every respect. Fifth, roundness and strength of the pellets, and a uniform regularity of spread over the disc, that is to say, not tending so much to *clusters* as the shot in the usual way. Sixth, quickness in loading, as wadding is rendered *entirely useless* if the charges are made to fit with nicety. Seventh, a perfectly clean barrel (above the breech) for any number of shots.

Directions for making the Brittle Charge. Invented by Mr. J. Jenour.

According to the weight of shot to be made into charges, take a proportionate quantity of common bread flour (inferior meal af several sorts would no doubt answer, but the inventor has not tried any thing except common flour), and knead it into as stiff a paste as possible, with water and a small proportion of lard or grease ; but the latter is not essential, though advantageous. Having thoroughly kneaded the paste, and cut it through repeatedly with a knife, to ascertain that the materials be perfectly blended, spread it out thinly on a firm table or clean floor, &c. and spreading the shot over it, mix them up together perfectly with the hands or a smooth piece of wood (but not any thing to batter the shot) until the whole becomes perfectly well mixed, so that the shot come in contact with each other, as if not mixed with the paste, which should merely fill up the otherwise vacant spaces.

The next operation is to mould the composition close and tight into tin or other tubes of a size according to the gun they are intended for, and of a length according to the weight of the charge, which will be better ascertained by a few judicious trials than by any particular rules that could be set down. One tube (which may be a piece of gun-barrel) only may be used; in that case, the moulded charges must be put *gently* into a Dutch-oven, or on the hob of a fire place, or any where near a fire, or in a slow oven, so that they may be slowly baked, scarcely to discolour them, but so that they become perfectly dry and hard throughout. They may then be slightly brushed over with gum-water, and when the gum is sticky, rolled in bran, pollard, or flour, which will make them fit with the utmost nicety*, and go down the gun pleasantly, and will more effectually prevent any of the lead coming in contact with the barrel.

To add more particular directions would be useless, as the principle of manufacture being clearly understood (which the writer trusts it now must be), something must always be left to the judgment and cleverness of the manufacturer.

The writer does not consider that he has in the least exaggerated the advantages stated, of which he fully satisfied himself by repeated trials last year, and would have taken a patent had not disappointments and misfortunes at length put it totally out of his power; which now induces him to give publicity to the plan, rather than it should remain unknown ; and in the hope of seeing it adopted, though with little prospect of any substantial benefit to himself, unless through the spontaneous liberality of gentlemen who may approve of this and the writer's other inventions, and may consider his exertions and misfortunes altogether meriting some assistance.

V.—*On Brick-making.*

The *Bulletin Universal* of February, 1829, contains an abstract of a work by M. J. F. Clere, on the art of Brick-making, which makes known several useful particulars, in regard to the nature of the material, the mode of fabrication, and above all, the manner of loading and firing the kilns with sea coal.

As this fuel has been recently introduced, with success, in Brick-making, both at Patna, and in Calcutta, we think a translation of the article may prove interesting to such of our readers as are engaged in public works, in India, where the builder is, in most cases, also the Brick-maker, and where, in consequence, there is not the same chance of the art arriving at perfection, as in countries where it forms an exclusive trade.

It, indeed, surprises us not a little, to find, from the preliminary statement of Mr. Clere, that the price of Bricks, in Flanders, is only ¾ths of the net cost of them here, notwithstanding the advantages we should enjoy in the cheapness of labour, and fuel.

The work is divided into 4 sections :

1st. On choosing and preparing the earth.
2nd. On mixing the ingredients.
3rd. On drying the bricks, and
4th. On firing the kilns.

We shall pursue the subject in the same order.

1. A good brick earth should contain neither too much alumina, nor too much silica.

The effect of an excess of alumina is to produce an uneven surface in the bricks, which thus lose their shape, and crack in every direction; whereas too

* It is of great importance that a cartridge or wadding should fit tight *when down*. See Obs.

much silica renders them difficult to mould, and increases the danger of their vitrifying, and adhering together in the kiln. The proportion may, however, vary considerably; and the best mode of ascertaining the fitness of any clay, is to make a few bricks, and bake them in a small kiln or oven.

Having ascertained the earth to be of a suitable nature, the preparation of it will occupy, from November to January.

The upper surface of the ground is first removed, if it consists of vegetable soil unadapted to the purpose: a trench is then dug from 3 to 5 feet in width, and about the same in depth: the earth of excavation is heaped along the edge of the trench:—close alongside, a second trench is dug, the clay from which is thrown into the first trench, and so on, for a convenient space, taking care to leave the clay in loose lumps, that it may be permeable to water and frost: unless this precaution be taken, of leaving the crude clay exposed during the winter, the bricks will be apt to split and break in drying; especially if the clay be of a rich quality.

Towards April or May, the work is resumed in the following manner:—

A labourer, called in French the *demileur*, throws several buckets of water into the last cut trench, which has remained empty, against the heap of clods; he breaks these up with the spade, and piles them in layers in a circle around him, of 10 feet diameter, watering the layers until they have reached a height of 1½ foot:—he then works the mixture with his feet, and with a hoe, draws it to the centre, removes all stony particles, replaces it in heaps, smooths the surface, and sometimes even covers it with straw, to prevent the drying action of the air.

It is convenient to have a trough of water hard by, to clean the tools used; also a small wooden scraper.

Each heap should contain clay enough for 2,000 bricks, or about a cube of 12 feet.

A portion of the clay is now conveyed in barrows to the table of the moulder, who is provided with several wooden brick-moulds, and a *stribe*, or flat lath, to pass over the surface of the moulds.—On the table should also be placed a small trough of water, to hold the *stribe*, a small knife to clean the moulds, and a supply of dry sand. The French bricks are commonly 9 inches long, 4½ broad, and 2¼ thick, and they contract a tenth in drying.

The barrow-man, and his assistant, place upon the table, previously sanded, a quantity of clay sufficient for 200 bricks.

A boy dips one of the moulds first in water, and immediately after, into the dry sand, and hands it to the moulder, who fills it with clay, squares it, slides it to the edge of the table, and hands it, held sideways, to an assistant, who carries it to the drying ground, suddenly turns it, and raises the mould perpendicularly, so as not to deform the brick. Proceeding in this way, the moulder ought to make from 6 to 9,000 bricks per diem, if the clay be very soft; but it is better to use a harder material, though the produce be reduced thereby to 2 or 3,000.

The drying ground should be well levelled and sprinkled with sand; it should be provided with drains in case of rainy weather. The bricks remain on their flat sides for about 24 hours, when, if sufficiently solid, they are set on edge, and when capable of bearing it, they are stacked with interstices, to admit a circulation of air throughout. Should rain be expected, the stacks should be protected with thatch. About 20 days will be required to dry the bricks fit for kilning.

We now come to the operation of kilning, which requires some nicety, as the heat at which a brick will be well baked, falls little short of that which will vitrify, and spoil it for purposes of building with mortar cement.

Kilns, of peculiar construction, are required for burning bricks with wood; but where coal is used, nothing further is required but to build up a solid pile, with alternate layers of brick and coal.

The first point is to level the ground, lay one or two courses of waste bricks, as a substratum, and entrench the whole with a ditch by way of drain.

The dimensions of the base depend on the quantity of bricks to be burned at a time. The larger and higher the kiln the less fuel will be used in proportion. At Lille they sometimes carry the structure to 75 or 100 courses, but at Valenciennes, where coal is cheaper, 30 bricks is the average height:—the upper tiers are always the most evenly baked, and the Flemish Engineers make a point of rejecting the 20 lowermost layers.

M. Clere regulates the length and breadth of the parallelogram in the ratio of

2 to 3. But the square seems preferable, both in point of solidity, and from its exposing a smaller exterior surface.

The construction of the kiln is as follows : 40 bricks are placed on their edges, end to end, along the short side of the parallelogram, and dressed in a line by means of a string : 236 lines of bricks, ranged in a similar position, serve to cover the whole area of the kiln. The second layer is likewise disposed edgewise, but transversely to the former, and at such distance apart, that two bricks shall stand upon the length of one of the first row.—With this layer commences the construction of the flues, which are parallel channels of the whole length of the kiln, one brick wide, two high, and four bricks asunder: the walls between the flues are thus composed of 8 bricks on their sides, standing on the 4 bricks below, and running the whole length of the kiln, for the *first* tier ; the same for the *second* and *third* tiers, only the bricks are alternately ranged lengthwise and crosswise. The interstices of these 3 tiers are then filled with middling sized coal ; to the *fourth* tier, of which the bricks are placed in close order, the flues preserve the same dimensions ; and at this period, they are filled with straw, faggots, and logs of wood, with coal on the top, over which is spread a coat of sifted small coal. The *fifth* tier of bricks projects half way on either side over the flues ; this space also is filled up with coal. The *sixth* layer covers the flues completely, having only 2 or 3 chimney holes of one brick square in the whole length of each flue. It is through these that lighted conbustibles are introduced, to set fire to the faggots and straw below. Having completed this row, small coal is thrown over the whole surface, excepting directly above the flues. A *seventh* layer is then laid in a similar manner, preserving only the chimney holes open.

The foundation of the kiln being thus completed, a large wood fire is made in the centre, and covered with coarse coal: when this is thoroughly kindled, the kilnman sets fire to the extremities of the flues, on the side not exposed to the wind ; he then does the same at the other end of the kiln : during this time, firemen are employed strowing red hot coal down the chimneys, while other workmen are busy raising screens of straw to keep off the wind. After 18 or 20 hours, when the fuel is in full ignition, the chimneys are carefully closed with bricks as well as the extremities of the flues.

The exterior is also plastered with straw and mud, to shut out the air effectually.

Before building the *eighth* tier, the kilnman spreads small coal, or " breeze," over such parts of the seventh tier as cover the flues ; he then proceeds to lay all round the kiln, and a little drawn back from the exterior, a line of bricks, called *boutiches*, or having the butt end pointed outwards ; behind this row, three more are placed in the same direction, and the middle space is filled with bricks crossing those of the layer beneath. The distance to be kept between the bricks is such, that 7 bricks crossed edgeways, should occuby the space of 2 bricks-length. One layer should contain about 7 or 8,000, but the layer immediately under the coal, may be packed a little closer than that above it.

In the *ninth* course, the exterior bricks must be as before, *en boutiches*, 4 rows in depth completely round the parallelogram, and the bricks within merely crossed with the layer beneath. Over this again is spread a coat of small coal.

The *tenth* course presents, externally, a line of bricks on edge, broad face outwards, so as to allow of a decrease in the pyramid of a quarter brick : within follow 4 rows of *boutiches*, and cross bricks inside as before. The *eleventh* has the same arrangement as the 9th, and so on alternately to the top ; after the 9th course, layers of coal are only given to every other tier.

The quantity of coal used, varies naturally with the quality of the clay : however, it should never exceed a thickness of one inch, and should be regulated according to the heat in different parts of the kiln, diminishing also with the height.

If the fire rises too rapidly, the bricks above are stacked closer, and if it is too slack, larger interstices are left. If the heat becomes too strong throughout, sand is spread over the surface.

Having reached the *ninth* course, the whole exterior surface is plastered over with well mixed clay, so as to form a firm crust.

The loading of the kiln is resumed always at break of day. Should the fire, during the night, have reached, and much heated the upper stratum, the bricklayer hastens to place another layer, so as to confine the heat ; and he must attend particularly to this point, by providing so many tiers the night before, that there may be no fear of too rapid an ignition.

As the kiln advances in elevation, the screen of mat or straw must also be raised to windward. Upon the top layer of bricks a thin stratum of coal dust is strewed, and plastered over with clay.

The pile being now completed, and the fire burning briskly, the screens may be removed. It takes 8 days to build a kiln of 200000 bricks. At Lille it occupies 20 to finish one of 600000, with a height of from 75 to 90 courses.

As yet no mention has been made of the species of coal employed, but in reality much of the success of the operation depends upon a proper solution of this ingredient.

For the flues, and for the interstices of the bricks, a bituminous caking coal, swelling and giving much flame, is preferred: on the other hand, a dull coal, burning without flame, is most suitable for the coatings between the bricks:—another sort of coal intermediate between the two, may, however, be used in both situations, without prejudice. When the rich coal is used alone, care must be taken to leave greater space between the bricks, and not to lay fresh tiers until full ignition shall be established up to the second bricks, under the feet of the workmen. The dull coaky coal is much less troublesome to use, merely requiring the mouths of the flues to be reduced in size; for too much air extinguishes this coal, instead of increasing its combustion.

At Valenciennes, about 115 *kilogrammas* (4 maunds) of coal, are allowed to 1,000 bricks, besides 60 faggots, and 7 or 8 cubic yards of logs. The cost per 1,000 bricks may be thus estimated.

	Francs.
Table moulding by contract,	3,75
1 *hectolitre* of coal (4 mds.)	1,40
Wood for the flues, and other fuel,	2,10
Wastage in spoiled bricks, &c.	0,75
Expense of loading the kiln,	0,60
Rent of the ground,	0,40
Total,	9.00, or 4 Rupees.

The above, assumed as the highest rate of charges, so that, notwithstanding all the advantages of cheap labour, cheap fuel, and low ground rent, which we enjoy here, our Indian ground-made and misshapen bricks, still cost fully as much as the best table-made bricks of Flanders or Valenciennes. P.

VI.—*Proceedings of Societies.*

1. Asiatic Society.

Wednesday, 3d November.

The President, in the Chair.

Mr. Hunter was elected a Member of the Society.

A letter was read from the Secretary to Government, in the General Department, forwarding one from the Secretary to the Government of Bombay—presenting two gold coins, one of Toghleh Shah, the other of Mahmood Shah, found in the Konkan.

A letter was read from Captain Twemlow, on the site of an ancient city at Ellora, and presenting a copper coin of Toghleh Shah found there.

Various donations of books were laid on the table.

A letter was read from Monsr. Le General Ventura, forwarding, by M. Meyeffreds, impressions of three coins, with a report of his operations at Manikyala Read; also extracts from a Tour along the southern frontier of Ladac, by Dr. Gerard.

Class of Natural History and Physics.

Thursday, 19th August.

Sir Edward Ryan, in the Chair.

A report of the progress of the Boring in the Fort, was presented by Messrs. Strong and Ross, which being read, it was resolved—that a Sub-Committee of the following gentlemen be appointed to investigate and report upon the Boring now going on in Fort William, and that they meet there at three o'clock P. M. on Tuesday next—viz. Mr. James Prinsep, Mr. Kyd, Mr. Hurry, Mr. Calder, and Captain Forbes.

A series of stalagmitic balls, with some animal remains, were presented by Mr. Swinton, on the part of Mr. Scott, with a short notice of the same. These stalagmitic balls were found in the cave in the Cossyah hills, already alluded to at a former Meeting.

A letter was read from the Secretary, stating his inability, from the pressure of public business, to attend to the duties of the office, and begging to be allowed to resign. This request was acceded to, and the thanks of the Meeting were voted to Mr. Ross, for his past conduct in the office of Secretary. It was then moved, and unanimously carried, that Mr. James Prinsep be requested to accept the office for the future. Mr. Prinsep being present, expressed his acceptance of the same.

Wednesday, the 27th October.

Read a letter from the Resident at Nagpoor, requesting information on the subject of the experiment of boring for water.

A letter from the Russian Resident Minister at Hamburgh, Baron Struve, communicated through Mr. H. Velthusen, presenting to the Society a selection of rare Norwegian Minerals, and requesting, in return, a series of Oriental specimens from the Society's Cabinet.

A letter from the Secretary to the Royal Society, acknowledging the receipt of the First Part of the Asiatic Researches, Physical Class, for 1829.

A letter from Dr. Gerard, of Soobathoo, transferred from the General Secretary, descriptive of some fossil remains of shells, discovered by him in the Himmalayan range.

The following contributions to the Museum were laid on the table :

Specimens of Anthrocite Coal, from the Carapeúr hills, near Baghulpúr, by the Secretary.

Bituminous Coal, from the banks of the Warda river, near Chanda, by the Secretary.

Specimens of the Coal from the coal-field at Palam, recently opened by Captain Sage, Barrack-master at Danapúr, whose report on the subject, together with the previous official correspondence of Captain Franklin, during his visit thither, in 1829, formed the subject of a Note drawn up by Captain Herbert, and read to the Society.

Specimens of the Garnet Sand, from Cape Comorin, and some Fossil Seeds from a stratum of brown coal, at Wurkelly, in Travancore, from Colonel W. Morison.

Specimens of the Turquois, and of the Rocks whence it is derived, collected at the mines near Nishapúr, in Persia, by E. Stirling, Esq.

Specimen of sandstone from Agra, exhibiting a striking picture of ferruginous arborescence, presented by Major Jos. Stewart.

The progress of the Experimental Boring in the Fort was explained to the meeting, and specimens were produced, of the several strata of clay, down to the depth of 109 feet. No accident has hitherto occurred to impede the works, owing to the effective apparatus fitted up by Mr. Kyd, for lifting the rods.

An examination of several bottles of water, from the Hot Springs on the Arracan Coast, was communicated by the Secretary.

Some discussions took place on the enlargement of the Mineralogical Department of the Society's Cabinet; and a resolution was passed, that steps should be taken to procure specimens of the crystalline gems of Ceylon, and the South of India.

2. Medical and Physical Society.

Saturday, 5th June.

Notice of a kind of Manna, the produce of an insect, with a specimen of the article, by Mr. W. Bell.

A paper, descriptive of *Colica Pictonum* in the Natives, by Dr. Chartres.

A case of Lithotomy in a Native Child, by Mr. Spry.

Medical remarks on the cases treated at the Convalescent Depôt, Landaur, in 1829, by Mr. Lovell.

A new formula for the preparation of Sulphate of Rohena Bark, with a specimen of the salt in large crystals, by Mr. Piddington : being in continuation of his former researches and observations on the same subject.

A case of malignant fungus of the Eye, succeeded by Tetanus, with drawings of the tumour, by Mr. Raleigh.

The following donations were made:

A copy of a work on Tuberculous Diseases, by Dr. Baron, of Gloucester, presented by the author ; a copy of James on Inflammation, by Mr. Piddington, and the two first volumes of the Glasgow Medical Journal, by Dr. Waddell.

The notice on Manna, and Mr. Jacob's papers on diseases of the knee-joint, popliteal aneurism, and case of tumour, were then read and discussed.

Saturday, the 4th September.

Sir Gilbert Blane was elected an Honorary Member of the Society.

A letter was read from Mr. Royle, submitting an account of Senna produced at the Saharunpore gardens, as well as a specimen of the plant.

A letter was read from the Secretary to the Physical Committee of the Asiatic Society, accompanying a specimen of the Morung poison, and requesting that its properties should be investigated.

Mr. Hutchinson's paper, on alvine fluxes of the natives of Hindoostan, was then read and discussed.

Saturday, 6th *November*.

Mr. Allingham was elected a Member of the Society, and Mr. C. C. Egerton, Assistant Secretary.

The following communications, received since the last meeting, were laid before the Society :—Notes of a case of lithotomy, with remarks, by Mr. Lindsay, Assistant Surgeon, Kamaoun. Two cases of abscess of the liver, communicated by the Medical Board.

A letter from the Members of the Medical Society, belonging to the Bombay Presidency, expressing their warm approbation of the proceedings of the Society on the occasion of the death of the late Dr. Adam.

Mr. Burnard's account of amputation of the hip joint, and remarks on lithotomy ; Mr. Thomson's case of wounded abdomen ; Mr. Hutchinson's communication on laceration of the small intestine, from external violence ; Rajah Kaleekissen's letter on the medical purposes to which *docata* is applied by the Natives of Bengal ; and Mr. Lindsay's notes on a case of lithotomy, were then read and discussed by the meeting.

3. Agricultural and Horticultural Society.

Wednesday, the 8*th September*.

President, Sir Edward Ryan, in the Chair.

Mr. Boyd, of Kishnaghur, and Mr. Bagshaw, of Calcutta, were elected Members.

Letters were read from the Secretaries of the Royal Society of Edinburgh, of the Horticultural Society of London, of the Geological Society of London, and of the Royal Asiatic Society, acknowledging receipt of the first volume of the Transactions.

A letter was read from H. H. Wilson, Esq. Secretary to the Asiatic Society, referring to the Agricultural and Horticultural Society, a letter on the rearing of silkworms, and a specimen of silk which had been transmitted to that Society, by "*A Friend to Industry*," at Kamptee, near Nagpore ; and also one from a Lady to the Secretary of the Agricultural and Horticultural Society, giving her real name, and stating herself to be the "*Friend to Industry*,"—and soliciting a pecuniary loan, and a donation of silkworms. A communication, it was determined, should be sent to the writer, stating, that some silkworms should be sent, but expressing the inability of the Society to make any pecuniary advances.

A letter was read from R. S. Græme, Esq. Resident at Nagpore, recommending Nagpore as a fit place for the rearing of fruit trees and exotics, and offering to bestow attention on any which the Society might be willing to send there for cultivation. Also requesting a supply of American cotton and tobacco seeds. The Secretary was requested to reply to Mr. Græme, and to express the anxiety of the Society to comply with his wishes.

The Secretary submitted a list of applications for garden seeds, which had been complied with, including packages, sent by the Society, on a large scale, to Dinapore, Poosah, Saharunpore, Nipal, Almorah, Simlah, Sylhet, and Moulmien, amounting to ninety-three packages, and nearly exhausting the stock in hand. A letter was read from Mr. Calder, offering to the Society, at prime cost, a quantity of garden and flower seeds, also of marrow-fat peas, grapes, and oats, just arrived from Aberdeen, and supplied by Mr. Gibbon, formerly an indigo planter at Tirhoot—Resolved, that the seeds be taken on the terms proposed, and that they be made over to the Garden Committee, with instructions to dispose of the flower seeds, grapes, and oats, and to retain the garden seeds for further distribution to Members of the Society and native *malis*.

The following donation of books was received from Mr. Robison ; *Deewan Pusind*, a treatise on Agriculture, translated by Mr. Lewis Da Costa ; a *Treatise on the cultivation of Sugar, Indigo, &c.* by Mr. Fitzmaurice ; *American Gardener's Calendar*, by Bernard MacMahon ; Speechly *On the Vine, and the Pine-apple* ; Ditto *On Rural Economy*.

A letter was read from Rajah Kalee Kissen Bahadur, submitting a treatise by him *On the cultivation of Tobacco*.

A letter was read from Mr. Hill, of Madras, transmitting a small quantity of the seeds of the Umbrella tree, which had lately been introduced there.

Sir Robert Colquhoun informed the meeting, that Mr. Patullo, of Pinang, had just brought with him from that place, and presented to the Society's Garden, a number of mangosteen trees, orange, *dooreans, nam-nam* and variegated pine-apple plants.

Mr. Abbot presented six boxes of Virginian tobacco, grown in the Society's Garden, and made up after the fashion of Havannah Cigars, by Mr. Van Zandyk, of Chinsurah. The Secretary was requested to transmit four of these boxes to the Honorable the Court of Directors, with a letter, explanatory of their history.

It was resolved, that Mr. Patrick be invited to make trial of the cotton saw gin at Gloucester works, for a month, and be requested to report the result of the trial to the Secretary.

A list was submitted by the Secretary of eighty-five applicants for the American cotton and tobacco seeds, lately furnished by Government, shewing delivery and transmission to almost every part of this Presidency.

Monday, the 13th September.
Sir Edward Ryan in the Chair.

Mr. W. Hickey, of Tirhoot, and Mr. Henley, were elected members.

A letter was read from the Secretaries to the Royal Society, and to the Linnean Society of London, acknowledging receipt of the first vol. of the Society's Transactions.

A letter was read from Mr. W. H. Macnaghten, Deputy Secretary to Government, dated 7th Instant, acknowledging receipt of the Secretary's letter of the 26th August, and stating that, under the circumstances therein mentioned, the Governor General in Council approved of the suggestions of the Society, respecting an experimental plantation to be conducted by the Society; and authorised the acceptance of Mr. Myers' offer of 500 biggahs of land at Akrah, at the rate of Rs. 3-8 per biggah, for three years,—the Society reserving the right of continuing to occupy the ground from year to year thereafter, on the same terms; and that Government had further sanctioned, for the same period, an annual disbursement of 10,000 Rupees for all charges of cultivation and superintendence, together with the sum of 4,500 Rupees for the erection of buildings, and the provision of stock suitable to each farm. It was resolved, that the Society be requested to reply to the letter of Mr. Macnaghten to the Society, and express their grateful acknowledgment of the liberality with which Government have complied with their suggestions—and to assure the Right Honorable the Governor General in Council of the earnest and anxious desire of the Society, by every means in their power, to further the objects in view, and for which Government have assisted them in making experiments in the cultivation of cotton and other articles of raw produce.

A letter was read from Mr. Smoult, forwarding an account of the expense incurred by him, since last December, in forming a cotton and tobacco plantation at Akrah, of between 60 and 70 biggahs, (amounting to 566 rupees, including rent,) and which plantation he was willing to hand over to the Society, as it now stood, upon being reimbursed his outlay: also offering for the acceptance of the Society a machine for cleaning cotton, sent to him from the Isle of France, by Mr. Telfier:—a specimen of Mr. Smoult's tobacco was submitted. It was resolved, that as the abovementioned plantation forms a portion of the ground which the Society wished to rent from Mr. Myers, Mr. Smoult's offer be accepted, on the terms stated by him—the management of the plantation to be placed in the hands of the Committee.

Resolved, that Mr. C. F. Hunter be added to the Agricultural Committee.

A letter was read from Mr. Biscoe, of Chuprah, presenting three varieties of pears, reared at the station, and varying from 25⅛ Sicca Weight to 12, and measuring, some of them, 10¾ inches in circumference.

A letter was read from Mr. Blacquiere, presenting a sample of nankeen cotton, and a piece of cloth made from it, of a beautiful texture, and great strength. A few years ago, Dr. Wallich supplied Mr. Blacquiere with a few plants, among which was one said to be the plant which produced the cotton from which Nankeen had its origin. The plant is the *Gossypium religiosum* of Roxburgh. In due time it produced pods in a considerable number, and cotton, in a fair quantity, with reference to their size. At length from repeated sowings, the cotton accumulated to a sufficient quantity to encourage an experiment of manufacturing it. It was spun into thread of different degrees of fineness, out of which pieces of cloth, of different widths, were woven, which looked like dark nankeen. Four of the pieces of cloth have been washed and found to be durable and pleasant, and to retain the colour under constant and repeated washings.

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