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OF

MANCHESTER.

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The Authors of the several papers, contained in this Volume, are themselves accountable for all the statements and reasonings which they have offered. In these particulars the Society must not be considered as in any way responsible.

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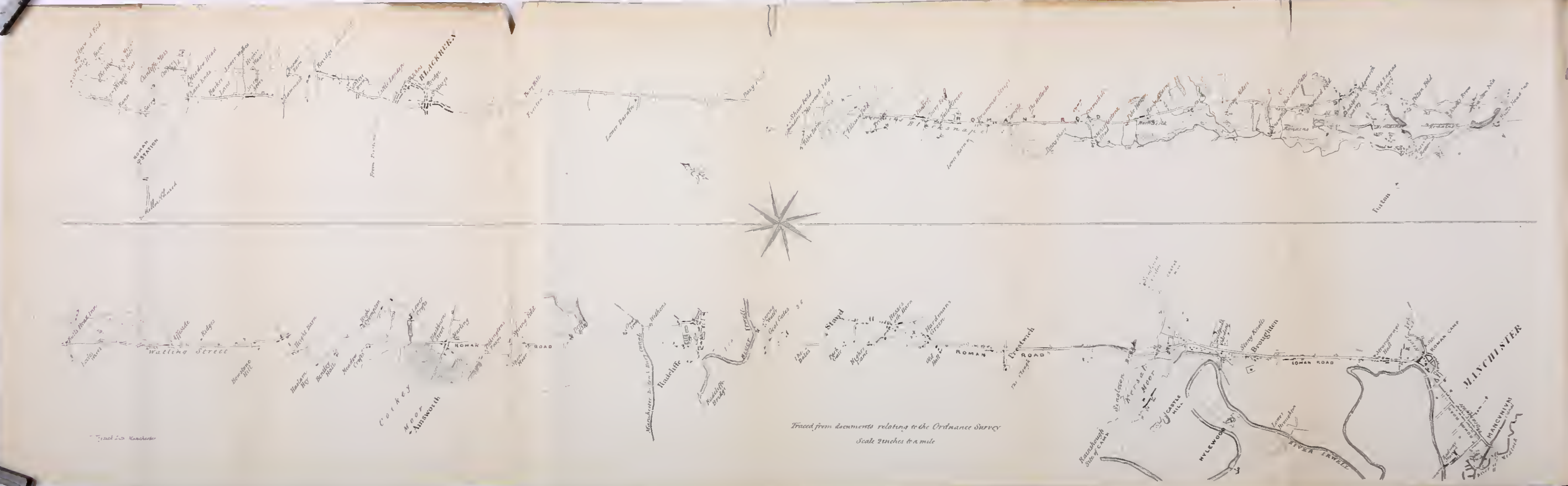
ERRATA.

Page 263, line 18, read "and that," instead of "That."
.. 266, .. 20, .. "Vulpes," instead of "Vulpis."
.. 268, .. 11, .. "Guêtter," instead of "Guêter."
.. 425, .. 19 and 23, read "analyses," instead of "analysis."
.. 433, .. 6, read "Gum," instead of "Potatoe Starch."
.. 434, .. 1, .. "Decandolle," instead of "Decandalle."
.. 435, .. 13, .. "0.7," instead of "9.0."
.. 435, .. 14, .. "The silica in the ashes," instead of "wheat straw."
.. 507, .. 1, .. "becomes," instead of "oecomes."
.. 510, .. 3, .. "$\overset{2}{v+c}$," instead of "v^2+c."
.. 511, .. 1, .. "$-nd\delta$," instead of "$=nd\delta$."
.. 513, .. 22, .. "$\frac{b\nu D}{\mu}$," instead of "$\frac{bD}{\mu}$."
.. 529, .. 23, .. "Stanwix," instead of "Stanwix ;"
.. 529, .. 23, .. "Stan-waegs ;" instead of "Stan-waegs,"
.. 533, .. 20, .. "[,]" after remarkable,
.. 534, .. 20, .. dele "[,]" after and.
.. 536, .. 9, .. "[,]" after asked.
.. 544, .. 8, .. "Rushy," instead of "Bushy."
.. 561, .. 4, .. "have," instead of "has."
.. 576, .. 9, .. "matter," instead of "water."
.. 576, .. 26, .. "covers," instead of "cover."
.. 579, .. 8, .. "eliminated," instead of "eleminated."
.. 584, .. 8, 9, 10, "[,] for a [;]"
.. 584, .. 11, read "have," instead of "havc."
.. 586, .. 13, .. "wanted," instead of "wasted."
.. 588, .. 12, .. "aeration," instead of "areation."
.. 588, .. 22, .. "emerocauses," instead of "enemacauses."
.. 589, .. 1, .. "are," instead of "and."
.. 598, .. 16, .. "straight," instead of "strait."
.. 601, .. 19, .. "radicels," instead of "radicals."
.. 602, .. 15 and 23, do. do.
.. 605, .. 6, read do. do.
.. 606, .. 19, .. "vegetation," instead of "vegitation."
.. 606, .. 20, .. "benefited," instead of "benefitted."
.. 612, .. 24, .. "our," instead of "over."
.. 618, .. 7, .. "more," instead of "most."
.. 618, .. 12, .. "radicels," instead of "radicals."
.. 618, .. 17, .. do. do.
.. 619, .. 6, .. "brairding," instead of "branching."
.. 621, .. 34, .. "aeration," instead of "arcation."



Traced from documents relating to the Ordnance Survey
Scale 2 inches to a mile

ON THE

ROMAN MILITARY ROAD

BETWEEN

Manchester and Ribchester.

By JOHN JUST, Esq.,
Corresponding Member of the Society.

(Read March 22nd, 1842.)

There are two particulars in which Roman Military Roads differ from all other roads. One was mentioned in the paper which I had the honour of reading before the members of this Society nearly three years ago, *viz.*, that they were elevated in ridges, running like ramparts throughout the countries wherein they were laid, and were designed chiefly for the purposes of warfare—the other is, that from point to point, wherever possible, they were laid in perfectly straight lines. This, too, was the result of their long experience in arms with uncivilized nations; for while the elevation of their roads gave them an advantage, in case of an attack, upon their march—the straight line enabled them to see as far as possible before and behind them, lest they should be taken by surprise, and cut off by numbers before they could form into close order and set barbarians at defiance. They were, hence, not roads of communication, but roads of defence. They did not necessarily pass through Roman stations, though the Roman stations were generally in their vicinities, especially such as were selected and fortified immediately after the subjugation of the country. Keeping in view, then, these characteristics, I beg leave to submit to your attention the results of my researches after the present state of the remains of the Roman military road along Antonine's tenth Iter throughout the county of Lancaster, extending them in this paper as far as Ribchester, and offering, as I proceed, such remarks thereon as I conceive either worthy of notice or likely to be interesting.

Whether the line of the Roman military road ran directly through the station Mancunium here at Manchester, or passed forward in the same straight line as that in which it enters Lancashire, and proceeds hitherward through Stretford, it would perhaps now be next to impossible to determine. It might pass through the station, and diverge thence up Deansgate to Hunt's-bank, or it might run on to Hunt's-bank without touching the station. It, however, left Manchester at Hunt's-bank, forming there an angle at the highest point as usual, and corresponding with the present line of road hence to Bury, through Strangeways, as far as that line is perfectly straight. Beyond Strangeways Hall, however, the houses on that side of the road project a little into the line, and here, in front of the farthest houses so projecting, and for a short space in the field beyond, a portion of an elevated straight ridge, directly upon the line, is very evident, which may be remains of the Roman road. The present road at the Toll-bar, or near it, then deviates a little from the line to the left, and continues so to do until we ascend the summit of the hill—while the line of the Roman road continues on, under the site of the houses forming the terrace, to the right ascending the hill, and on account of the remains of the Roman road being discovered in digging the foundations of the houses forming a part of these—we find a portion of them appropriately designated "Roman Road*

* I believe this is the name on the farthest house of the range—"Roman Road Terrace."

Terrace." Continuing hence on the right of the present road, the line of the Roman road crosses the modern road a little beyond Broughton lane end, passes through the lawns in front of the houses on the bank of the Irwell, and at a short distance further again crosses the Bury road into the fields to the right, in which, (a little beyond the Toll-gate at the corner of Kersall Moor,) slight traces again are evident. After crossing another time the Bury road to the left, it coincides with the same, as far as the same keeps a straight line, to Prestwich.

From the Zoological Gardens to Prestwich was an ancient road, destroyed when the present turnpike road was made, probably on the very line of the Roman road, and a portion of the same, slight traces of which ancient road are yet evident near Pilkington Old Hall beyond Prestwich. Across the fields, between the Old Hall and Higher-lane, not a trace of the Roman road is visible. Remains are, however, discernible when we arrive at the houses at Higher-lane, where the line there coincides with the present road past Park Gate, in a straight line, till we come opposite Stand Church, where there is an angle in the present road at the intersection of the Whitefield road to Radcliffe. Beyond this point, the line of the Roman road again falls in with the modern road, near the Dales—passes through the plantation at the Dales, thence into the fields northward, where the elevation is distinctly traceable to the termination of an old lane from Goat's Gate, where it descends the steep banks of the Irwell, and falls, with undeviating accuracy, into the line of the Roman road, as traced out and described in the former paper on this subject.

In addition to the information gathered up during my former investigations of the line of the road from the Irwell to the Bull's Head Inn near Quarlton, may be mentioned what was communicated by the farmer at Spen Moor. He told two gentlemen and myself when examining the line for the Ordnance Survey, now carrying on within this County, that in draining, he frequently had to cut through the substratum of the road. When asked to show us in what direction the line of the road ran, he led us to a field below the farm house, where he pointed out to us across the field where this gravelly substratum was found, the very line of the road ; told us moreover, that the hard gravelly bed was about seven or eight yards broad, about half-a-yard thick, and about a foot below the surface of the soil, just beneath the reach of the plough. He besides pointed out to us the course of this line across other fields belonging to his farm. And when asked, if he ever heard aught about what it was, he instantly replied,—"It was an old road that the devil made, called the devil's way by the old people still." At Starling too, where the Roman road coincides with the modern road from Manchester to Blackburn, that portion is known by the name of Blackburn-street, and forms the boundary between the Parishes of Bury and Radcliffe, which boundary also runs thence to Spen Moor, along the very site of the Roman road, without regard to fences, brook, or any natural demarcation for such a division.

Hitherto no notice has been taken of accounts given by different authors of the line of this road; because in the first place, it has been deemed advisable to allow the remains as pointed out to speak for themselves, by identifying them (should any one think it worth while to do so,) at the several localities. In self-defence, however, I am bound to bring forward all differences, not for the sake of finding fault, but that the public may judge and determine. In Baines' History of Lancashire, vol. III. p. 7, we find the following passage: "The Roman vicinal road from Manchester to Ribchester passes through this parish, (Radcliffe), a portion of which yet retains the name of Blackburn-street." To call the remains of the Roman road from Manchester to Ribchester those of a vicinal road argues, I presume, a total ignorance of the distinction of Roman roads. Vicinal roads were so called "*quia ad vicos ducunt*" because they led to villages. The width of such roads averaged only about eight feet, and so far from being straight, they usually had turns purposely made for wagons to pass each other. The definition of the term, then, and the character of the remains of the Roman road from Manchester to Ribchester, are quite at variance. Besides, Blackburn-street does not pass through the parish of Radcliffe, but forms a part of the boundary between it and the parish of Bury. Again, "the Roman road from Overborough, after passing through Ribchester advances to Manchester, by way of Edgeworth, Bradshaw, and Cockey Moor, and appears at this day near the eastern boundary of Breightmet as a broad paved way of irregular surface." Ibid. vol. III. p. 80. "And farther, the Roman road passes through this township (Turton) and presents the appearance already described in the township of Breightmet." Ibid. vol. III. p. 91.

With the exception of Watling-street, in Offyside forming the boundary between Bradshaw and the Manor of Tottington and Edgeworth, the Roman road approaches none of the localities mentioned; as any one may satisfy himself by walking over it.

We will now again proceed on our way, resuming it where we left off at the Bull's Head Inn, some time ago; and for our encouragement, the first steps we take are over most marked remains. Crossing the new road from Tottington, in front of the out-buildings, is the agger of the road almost unmolested, except where, here and there, it has been dug into for various purposes, just as it were on purpose to allow us a peep into it. A large reservoir of water then intercepts the line, where doubtlessly we all feel disinclined to dive after it, to follow it, as that would be dipping deeper into the subject, than perchance our abilities might warrant us, or damp too much our ardour for proceeding. Immediately after the crossing of the Quarlton road which here deviates to the right, we find marked remains and indications of the road across the fields, until we arrive at the lodge, appertaining to some manufactory, especially at the second fence from the road; where too idle to dig through the road in true country fashion, the fence makers have set the fence across it. Here young Roman road hunters may get hold of a scent which will improve the susceptibility of their organs of sense as long as they live. While, if confirmation be wanting, they may cast off a quarter of a mile or more to the right, where they will find a tumulus in as complete and perfect a specimen as Britain at this day can furnish them. Beyond the lodge no traces of the road are discernible, until we have proceeded further than the immense stone quarries at Edgeworth. In the fields, here again, very striking traces remain, especially near and under a fence which, for a considerable distance runs upon the road. Henceforward, through numerous fields the road has entirely disappeared, until we come near the present public road to Blackburn, where near Round Barn traces again occur ; and at the next house, Pike House, the line falls into the public road, on, and near which, with several traces of it, it continues to the top of the hill at Blacksnape. In looking behind us as we ascend on the road, we cannot but be struck with the exactness with which the straight line of road on which we are travelling, corresponds with the straight line of Watling-street on the high ground at Offyside. And we may be led to enquire into the reason why the modern road should have deviated from the Roman road in the valley, when it has so closely adhered to the line on the heights. If we have at all attended to the nature of the ground in the valley, we may have noticed that it is intersected with numerous brooks, rills, and rivulets, which run down to the main stream at the bottom of the valley in very deep channels, with very steep banks. Now, though these banks might offer no insurmountable obstacles to the hardy and enterprising Roman soldiery, nor to their beasts of burden, when the road along them was well paved and in good repair, they still might be serious impediments to the progress of the Pack horse between Blackburn and Manchester, without such advantages: and therefore the line might be relinquished for one more circuitous, where no such impediments intervened. This seems a very probable reason, when we look at the places where the present road crosses these brooks, rills, and water courses, which are no farther from the line of the Roman road than the very nearest points where the channels offer no obstructions.

From the highest part of the road at Blacksnape, we see the dense smoke of Blackburn in the low ground directly before us. The line of the Roman road here is on the left hand side of the public road, and very distinct remains may be observed as we pass along in a continuous ridge, running between the houses of the village, many of which stand, filthy as some of them are, on classical ground. A house falling to ruins shows here the remarkable feature of having a part of its foundation laid across the very summit of the Roman road, which in its full characters it preserves. Descending from Blacksnape, the line of the Roman road is left for a short distance in the fields where it is scarcely distinguishable, until we come nearly opposite Ellison fold, where it again approaches the public road, and is visible as a slightly elevated ridge and white line, till it crosses the road; and continues as an intercepted white line through the fields, here and there a little elevated, until it passes Harwood fold, below which it falls in with a foot-path and fence, and proceeds along with them nearly to where it again crosses the public road. A small rill of water follows the line of the fence and the road. Cutting off a small corner of the field after crossing the public road, it falls in with the garden and farm-yard of Davy Field, where are very marked remains. Indeed, the most convincing remains are generally about farm steads and houses. For there cultivation cannot be carried on. And I have oftentimes in investigating the remains of the line been glad to see an outbuilding or house upon it, where all traces in the fields had been lost, because, then I anticipated another link to the chain of evidence, and seldom have I been disappointed in not finding one. At Davy Field, the line falls in again with the modern road. We here learned from the farmer, who was very civil and communicative when he understood our business, that the present road was very difficult to keep in repair; that notwithstanding all their attempts to improve it, they had failed; because the old road which we were enquiring about was underneath it, as far as the first turn, and being paved with large rough stones, the broken stones laid upon them were soon ground to pieces by the wheels of the vehicles which passed over the road, and until lately the reason was not known; but being threatened with an indictment, they had dug down to see what the road ailed, and thus had ascertained the fact he mentioned. At the turn in the road, the Roman road keeps to the right, and forms a stony line across the field to the river Darwen. Again, beyond the river it falls in with the modern road, showing traces at the fence, and with slight deviations from the line of the modern road, which here is nearly perfectly straight, it passes through Lower Darwen: where close by the houses as at Blacksnape, its remains are very conspicuous, and continues coinciding with the modern road to the next turn. This straight portion of the modern road is very evident from the high ground at Blacksnape, corresponding exactly with the line, as in the instance before mentioned. At the angle in the road, the Roman road continues straight forward through the fields to the right, showing a moderate elevation, and where drains had been recently cut through it, exhibiting its gravelly substratum in strong contrast with the clayey soil on each side of it. In other fields the levelling was going on at the time of our survey; modern improvements not yet being satisfied with what fourteen centuries have done to obliterate it. The line now crosses the quarries near the Blackburn and Bury road, and intersects Blackburn about forty yards to the east of St. John's church.

Having had a wearisome journey hitherto from Manchester to Blackburn on a line of road, over which though Roman Emperors and Generals formerly deigned to march armies, few persons now-a-days would think it worth while to travel but ourselves, nay, would think us out of our senses for doing what we have done, just by way of breathing awhile, we will for a few moments glance at what others have done in this quarter before us; who may have been foolish enough like ourselves to trouble themselves about such a matter. And first we find Dr. Percival in the Philosophical Transactions, vol. XLVII. p. 228, stating of this Roman road which we have been tracing, that "It goes through Ratcliffe and so to Cockey Moor, and from thence to Offyside, to a place now called Watling-street, and so to Bellthorn Moor above Darwen, and to the east of Blackburn strait to Ribchester." Mr. Whitaker, the historian of this place writes thus,—"After leaving the parish of Manchester, the road must have pushed through Prestwich and Ratcliffe, appears on Cockey Moor, and extends through Watling-street in Offyside over Bellthorn Moor above Darwen, and to the east of Blackburn to the ford, which is a little to the east of Ribchester:" History of Manchester, vol. I. p. 121, merely echoing Dr. Percival's words. However correct or approaching to correctness, the statements of the two authors just quoted, may be in the immediate neighbourhood of this place, and also in the neighbourhood of Bury, they appear both to have been lost when they got so far from home as we are at present. For be it known to all whom it may concern, the Roman road did not pass near Bellthorn Moor above Darwen, but through Lower Darwen itself, where it still may be seen at this day. Nor to the east of Blackburn, but through it, being most satisfactorily to be traced up to Blackburn on this side of it, and as we will now show, from it on the other side.

Leaving Blackburn near St. John's church, and ascending the grounds in the same direction on this side of the town, as we approached it on the other, we soon fall in with the remains of the Roman road, first as a slightly elevated white ridge in the third or fourth field from the town, and then as a bold ridge as it approaches Revidge. Here the first house of the village stands upon the line. The road then crosses Revidge lane, runs along the meadow on the opposite side, but scarcely discernible, passes under some houses at the top of the meadow, and in the corner of the next field appears distinct and almost perfect. Again crossing Revidge lane it descends to Buggart's barn, but no traces here remain. While looking about here, we were much struck by observing through several fields before us, a belt of mole hills extending in a perfectly straight line, in the very direction of the line which we were endeavouring to trace—and singular enough when we came upon this belt, we found it to cover the whole area of the Roman road; which through the meadows here was very evident, the substratum of which furnished a dry road through wet ground for the moles then, as it formerly did for the Romans. We also noticed in these meadows a drain here and there made, which terminated at the margin of the road, either because they found the road dry enough; or as is also probable, because they found it no easy matter to cut through it, since heaps of stones in some places seemed to suggest a relinquishment of labour on account of the obstacles they offered. Crossing the lane to the farm of Higher Waves, the remains are very striking. While passing down the adjoining field which had been just drained, (and the line of Roman road as well also), the drains gave us a plain indication where it ran across the fields. Near Lower Waves, and directly forward up to Ramsgrave, the line is easily traceable. The premises at Ramsgrave stand upon the road, which in the yard and near the outbuildings, is, as usual in such situations, remarkably evident. At this point, the vale of the Ribble bursts upon us, with the whole extent of the line of the Roman road visible through it. We were much struck with our view here, and stood some time gratified to enjoy it. Longridge Fell on the other side of the Ribble, bounded the prospect before us. In an exact line with the spot on which we stood, the white band on the summit of the Fell so distinguishable from the brown heath which it traverses marked the Roman road on the horizon. Exactly beneath this in the same line appeared the whole length of Stony Gate lane. While on this side the Ribble, a series of fences appearing as one continuous fence in the self-same line, brought its course near us. And an occupation road straight as the other lines, and straight with them extended a quarter of a mile and more from our feet, to complete this *coup d'œil* of the road. At the end of this occupation road from Ramsgrave, very decisive remains extend through Cunliffe Moss to Midge Hole. Hence, along another occupation road, or road to a farm, beyond which no traces are observable till we pass Harwood Fold. At Harwood Fold the ordnance survey ends for the present, beyond which point no correct sketch of the line of the Roman road and the remains upon it has been taken. What follows, though an accurate description, depends upon my own authority. Leaving Harwood Fold a few yards to the right, the line of the road continues without any striking evidences through the fields forward, leaving Bank Top to the left, and crossing the Preston new road to Whalley, &c. about 300 yards east of the Royal Oak Inn. Immediately after crossing the road, the line becomes again conspicuous on the headland, close to the fence of the fields, crosses an old lane, and falls in with the road to Stubby Head, which house stands upon it, thence coincides with a deserted lane, which has a narrow paved causeway in the middle, again appears on the headland, close to the fence of the fields at the end of the lane. At last it falls in with another old road, leading to a farm house to the left, then runs close to a fence again. Afterwards through two or three fields which were ploughed at the time this survey was made, and showed a gravelly line throughout, and then disappearing in the last field on the banks of the Ribble, crosses the Ribble about a quarter of a mile to the east of Ribchester on the opposite bank.

Such, gentlemen, is the line of the Roman military road between Manchester and Ribchester, and such the remains at present upon it. It differs in some places widely from the lines given by others—but unlike them, it has evidences to speak for it throughout. It was these differences which, when I came into this neighbourhood, first determined me, if I should have the leisure, to settle them by an actual survey of the line. An authority not yet quoted, viz., that of Dr. Whitaker, the Historian of Whalley, and other celebrated works, gives the line, through the following places:—From Watling-street, the line of the road goes on the high ground between Uglow Pike and Pikelow, then

> "Crossing o'er the Osbaldtwistle waste,
> By Stanhill's height and Knuzden's ample verge,
> The Roman way is dimly seen t' emerge,
> There may be seen its agger broad and deep,
> Through moor and fields in lines majestic sweep."

Now Uglow Pike and Pikelow, are a mile at the least to the east of the Roman road. Oswaldtwistle and Stanhill still more to the east. Knuzden also to the east. Nor does the road cross the Ribble at the Ford, near the bridge at Clayton-le-dale, but much nearer Ribchester. It must, notwithstanding be mentioned, that after Dr. Whitaker resided at Blackburn, he relinquished his notion of the line he published in the History of Whalley, and adopted that part near to Lower Darwen church as a portion of the road, which is really so.

I could bring forward numerous other quotations from other authors of different lines, as portions of this military way, all remote from it and from the truth, but to do so would be a waste of time. It is a pity that in local histories, misstatements of this kind should be found, when the truth might have been so easily ascertained by an actual survey. Hearsay should form no part of history.

At Mellor, near Blackburn, are the remains of a small entrenchment on the heights, in perfect preservation, which is considered a small Roman fort. It consists of a rectangle of 100 paces or yards in perimeter, and therefore, requiring no more than 100 men to man it. It might be an outpost of observation, but has no capacity for, nor character of, defence. It is so situated, that Ribchester church is very evident from it. And Ribchester church is in the area of the Roman station there. And as by removing a few score yards either to the east or to the west of it, Ribchester is completely hidden from the view, there is this probability in favour of its being Roman.

Awaiting some other opportunity, I shall be proud to bring before you an accurate account of the remains of this Roman military way, from Ribchester to Overburrow, thereby completing my survey of it throughout the county of Lancashire.

Chesham Green,
March 19th, 1842.

ON

THE USE AND ORIGIN

OF

SURNAMES.

By the REV. W. JOHNS.

(Read February 23rd, 1841.)

The appropriation of specific names to the various objects with which men are conversant is necessary for the purpose of mutual communication; and the application of proper names to individual men is for the same reason equally requisite.

It is well known that according to the usage of our times, individual men are in general designated by two names, which use authorizes us to call the *christian name* and the *surname*. The christian name is generally chosen from a list of names, which use has consecrated in each community. I think the number of such names in this country—that is, christian names, in pretty general acceptance and currency—amounts to about two hundred.*

It is obvious to remark, that in a numerous society of men, consisting of many thousands or even millions, a list of two or three hundred single names would not be sufficient to distinguish individual men from one another; as the same name must of necessity be attached to many thousand individuals. To obviate the confusion, which in that case would be unavoidable, various ways were adopted, when particular families or communities became too numerous for every individual to be sufficiently distinguished by one name. The Greeks, Romans, and other ancient

* Of course I do not include in my estimate names seldom used, which caprice and fancy lead men sometimes to adopt; nor foreign names; nor names borrowed from the scriptures, scarcely sanctioned by custom, as Obadiah, Ezekiel, &c. It not unfrequently happens, that the surname of the mother's family, or of other relatives or honoured friends, is adopted and used for a christian name. I do not include names of this class.

nations had modes peculiar to themselves; but though I may presently advert to some of these, the space to which this essay is limited, makes it necessary to confine my remarks to the methods adopted in this country since the Saxon times. The principal of these was by surnames. The surnames originated in various circumstances; and they may accordingly be divided into several classes.

The etymological meaning of the word *surname* is *additional name:* a name added to a primary name. It is then by means of a surname —added to what we call the christian name—that we distinguish every particular person from the mass of individuals with whom he is surrounded. This method also serves to distinguish families: it marks the course of descent from father to son, and enables families to trace their genealogy.

I shall now proceed to point out the sources, from which most of the various surnames now in use originated; and I shall distribute them into several classes—an order, as I think, into which they naturally divide themselves. Many indeed are in use which I am not able to explain; and I cannot find how they originated, though in the first instance, no doubt, there existed some reason for adopting them. Owing to the changes which time has introduced into this as well as other kinds of names, either by shortening long words, or by changing the orthography and pronunciation in a great variety of ways, it is in numerous instances exceedingly difficult to discover the original form; and when the true source has been correctly traced, etymological inferences are generally viewed with suspicion, and even sometimes appear ludicrous. As a matter of fact it admits of no doubt, that *Kit* is the legitimate representative of *Christopher*, and *Dick* of *Richard*—with many besides; which, if they were viewed as the speculation of the etymologist, would scarcely be tolerated even as a jest. With respect, however, to the *classes*, which I now proceed to enumerate, there will not, I think, be much room for difference of opinion.

The *first*, and I believe the most *numerous* class of surnames is distinguished by the termination *son*. Thus *Johnson*, *Jackson*, *Williamson*, *Wilson*, with many more, taking the denomination from the father; and also, for particular reasons, sometimes from the mother, as *Nelson*, *Alison*, *Pattison*, *Hanson*, *Moulson*, &c. There are instances also of the termination *son* being joined to surnames, as, *Cookson*, *Swainson*, *Smithson*, &c.

The form of this class is so obvious, and the principle on which it is founded so apparent, that but little is necessary to be added in explanation. So obvious indeed did this mode of designation always appear, that it was very early adopted by various nations in one form or another. In those venerable oracles of remote antiquity, the writings of Moses, we have observed its use not unfrequently. It occurs in the first instance in the second book of Moses, and is applied to persons contemporary with him. A mode of designation was afterwards adopted among the Hebrews, which, though apparently different, corresponded with that just noticed: it was by prefixing the monosyllables *bar* and *ben* to the name of father, as *Benhadad*, *Barjonas*, and a few more.

The like distinction of individuals, by expressing the relation of *son*, was that in common use among the Greeks. Thus I give an instance from Herodotus, in whom the form often occurs: Καμβυσης ὁ Κυρου, Cambyses the son of Cyrus. In instances like this, the word ὑιος *son* is understood in Greek, and is represented only by the article ὁ. In subsequent writers, however, the article ὁ, as well as ὑιος, was omitted. Thus in Thucydides Φαλιος Ερατοκλειδου—literally, Phalius of Eratocleides.*

The usage as to proper names among the Romans differed in general from that among the Greeks; for the usual custom was to designate particular persons by three names—the prænomen, the nomen, and the cognomen, as Caius Julius Cæsar; but the way of pointing out the persons of individuals by a like reference to the father was not unknown. Thus in Cicero we find Caius Caii filius—Caius the son of Caius; Tib. Gracchus Publii filius.† From this we infer, that the

* Wiclif, who translated the new testament into English from the vulgate Latin, expresses himself in the same manner; thus, 'James of Zebede,' and 'James Alfeye,'—[the correct reading was probably 'of Alfeye'.]—See Matt. x. 2.

† The Roman way of introducing the names of strangers, both Greeks and others, is worthy of remark. Thus in Cicero we have Thebanus Epaminondas, Lysias Pythagoræus, Syracusius Dio, Alexander Pheræus, Superior Dionysius, Viriatus Lusitanus—with many more after the same manner. In cases, indeed, where a distinct designation was plainly unnecessary, the bare name was considered sufficient. Thus, Aristides, Socrates, Alexander, Cyrus, &c. In Greek also names occur, in which the personal distinction is marked by

form of proper names in subsequent times adopted by the Irish, Scots, and Welsh, was either borrowed from former usage among more ancient nations, or adopted as that which naturally occurs to the minds of men in all places and times. In these languages the particles *Mac*, *Map*, and *Ap* prefixed to personal names mean *son ;* but *O'*, I imagine, refers in most instances to the place of residence. This last mentioned mode was common in England before the introduction of surnames as now used, *of* performing the same office as the Irish *O' ;* thus Florence of Worcester, William of Malmesbury, John of Gaunt, with many more. It may be here added, that the prefix *fitz*—from the Norman French *fils*—is still used in some instances—chiefly in cases of illegitimacy—to indicate the relation of *son*.

In the *second* class of surnames I place those which have the termination *ton*. This termination, whether occurring in *surnames*, or in the *names of places*, is an abbreviation of the word

a reference to the nation, character, or some circumstance peculiar to the individual; as, Κλειτελης Κορινθιος, Αριστοδημος ὁ Μικρος, Παῤῥασιος ὁ Ζωγραφος. In Latin the cognomen often expresses a characteristic distinction, as Lucius Junius Brutus.

town. The principle then assumed in forming this class of surnames is the appropriation of the *places* or the *towns* where men lived to the purpose of distinguishing their persons: thus John Newton is John who lived in Newton, or John of Newton. The particle *of* before the name of the place was originally used; but it was subsequently omitted by degrees for the sake of dispatch.

The principle here assumed may be extended to all kinds of places and localities—to cities, towns, villages, parishes, townships, dwellings of some consideration, houses with portions of land annexed to them, &c.—the names of the localities being transferred to the persons who lived in them. From this source has been derived a very considerable number of surnames now in general use. Of these the following is a very brief selection—confined chiefly to this district—and it is sufficiently confirmative of the origin to which we have ascribed them: *Manchester*, *Oldham*, *Bury*, *Ashton*, *Hyde*, *Bolton*, *Bowden*, *Cheshire*, *Lancaster*, *Blackburn*, *Eccles*, *Middleton*, *Preston*, &c., &c. Instances, likewise, of the names of localities of a more general denomination, which are used as surnames, are the following: *Hall*, *Parsonage*, *Hill*, *Forest*, *Lodge*, *Parish*, *Town*, *Townend*, *Bridge*, *Heath*, *Moss*, &c. At first, *of* was inserted between the christian name and the locality; but after a time it appeared unnecessary to retain the particle. Thus *Benjamin Manchester* was at first *Ben of Manchester*, *John Hall*, *Jack of the Hall;* and so of the rest.

The source to which I have here referred the formation of a very numerous class of surnames is so obviously apparent, that little needs be said in proof of it; yet in confirmation of the view given, I quote the following passage from Camden's Britannia, in his account of Lancashire. After stating that Richard Fitz Hugh of Hindley had married the eldest daughter and heiress of Gilbert de Culchit, he adds, "he assumed to himself the name of Culcheth, like as his brother Thomas, who wedded the second daughter, was, *of* [or from] the possession, called Holcroft: another also, for the same reason, was named de Peasfalong, and a fourth de Riseley. Which I note," he then adds, "that the reader may understand, how our ancestors, as in other things constant and grave, so in leaving and taking up names *out of their possessions*, were as vain and variable as might be. But even in other parts of

England also, this was in old time a thing of usual practice."*

The *third class*, to which I next proceed, comprehends the surnames which end in *ley*—occurring not unfrequently in the following varieties of orthography, *leigh*, *lea*, *lay*, *lee* and *ly*. The word *ley*, derived from the Saxon *leag*, means a tenement or portion of land used chiefly for pasture, and not for tillage: that being the condition of a much greater proportion of the land formerly than at present. The word *ley* however is still in use; for the expression, 'a ley for cattle,' is often to be observed.

Surnames ending in the syllable *ley* accord in this with the last mentioned class, that they are adopted from the name of a locality; but this class of surnames is so numerous, and so obviously exemplifies, and confirms the principle asserted of deriving surnames from the names of places, that it is thought worthy of a separate comment.

As all farms at present, so formerly the various

* This was written about the beginning of the seventeenth century; whence the reader may infer what is to be understood by the expression *in old time*.

leys throughout the country, were distinguished and known by appropriate names. Those names were of such a character, as to show that in the first instance they were used of places, and could have no significance as proper names of men. They were derived from various circumstances which appeared peculiar to each, and by which, as particular localities, they were obviously observed to be characterized. Some of these characteristics are the following :—situation, produce, the animals pastured on or frequenting them, the peculiar qualities of the leys, the size and position of them, and other circumstances not easy to be classed. It is also to be observed that the number of surnames belonging to this class is very great—not fewer, I think, than five hundred; but the origin and import of a large proportion of them I have not been able to discover.

The following imply a reference to *situation:* *Bottomley*, *Topley*, *Medley* or *Meadley* [perhaps middleley, meadowley] *Edgeley*, *Bleakley*, *Knowsley* [i. e. Knolls'ley] *Hedley*, *Morley* [i. e. near a moor] *Mosley* [i. e. near a moss] *Oversley*, *Westley*, *Hiley*, *Underley*, &c.

The following refer to *produce:*—From this numerous class I select the following—*Cheesley*, *Ashley*, *Alderley*, *Boxley*, *Birchley*, *Fernley*, *Oakley*, *Hayley*, *Wheatley*, *Oatley*, *Reedley*, *Seedley*, *Sedgeley*, *Butterley*, *Nutley* or *Notley*, *Riley*, *Burley*, *Woolley*, with many more.

With reference to *animals* we have the following:—*Birdsley* or *Burdley*, *Beesley*, *Buckley*, *Cowley*, *Crowley*, *Chickley*, *Harley* or *Hareley*, *Horseley*, *Foxley*, *Hindley*, *Hartley*, *Oxley*, *Shepley*, [i. e. Sheepley] *Henley*, *Finchley*, *&c.*

In reference to different *qualities*, *size*, and *other circumstances* we have made a selection of the following:—*Brownley*, *Whiteley*, *Blackley*, *Yellowley*, *Ridley* or *Redley* [all from the colour,] *Eckersley*, [i. e. ley of an acre,] *Weekly*, [i. e. let at a weekly rent,] *Yearsley*, [i. e. let at a yearly rent,] *Bradley*, [corruption of broadley,] *Chalkley*, *Roughley*, *Smalley*, *Worsley*, *Huntley*, [connected in some way with hunting,] *Ditchley*, *Minsterley*, [near, or belonging to a minster,] *Priestley*, *Barnsley*, [with a barn,] *Meanley*, *Rockley*, *Marley*, [producing marl,] *Pooley*, [having a pool,] *Crossley*, *Litley*, [i. e. little ley,] *Netley* [i. e. nettle ley.]

The surnames above enumerated, comprise but a few of the class; but I think the number is sufficient to show, that they were appropriated to the localities and tenements called *leys*, before they were used as surnames. It appears probable however, that in some instances the leys took their name from the owners or tenants, *Kersley*, *Dodsley*, *Willesley*, *&c.* The etymology of the greater number of them is obscure.

There are several terminations used in the composition of surnames, and they were applied to form them upon a similar principle with *ley*. The most frequently occurring are the following: *Field*, *Stead*, *Croft*, *Yard*, *Stow*, *Dale*, *Combe*, *Bourne*, *Shaw*, *Ford*, *Gate*, *Brook*, *Bridge*, *Well*, *Den*. The meaning of others of the same kind is less obvious: *Lowe*, *Hithe*, *Hurst*, *Twisle*, *Hulme* or *Holme*, *Thwaite*, *Clough*, *Halgh*, *By* or *Bye*, *Ney*, *Sey*, *Cross*, *Worth*, *Wick*, *&c. &c.**

I include in the *next class* of surnames those

* *Lowe*, means a rising ground; *Hithe*, a small haven; *Hurst*, a thicket of trees; I think *Thwaite*, signifies to exchange or traffic; *Clough*, a precipitate descent; the others rather uncertain.—Many of the words in the two last periods are used for names singly, as well as in composition.

which are borrowed from natural objects of various kinds, both generic and specific, animate and inanimate. These are so numerous as to admit of an imperfect classification :—*Mountain—Snowdon; Hill—Brownhill, Brickhill; Forest—Dean, Sherwood; Lake—Winder, Derwentwater; Grove*, equivalent to *hurst* and *wood—Musgrove, Hazlehurst, Hazlewood; Rivers—Mersey, Severn, Medlock; Brook—Braybrook; Field—Fairfield, Greenfield; Tree—Ash, Birch, Pine; Flower—Rose, Lilly, Pink; Animal*, (itself not a surname, but)—*Bull, Fox, Hare, Lion, Lamb, Wolfe; Bird—Swallow, Wren, Sparrow, Rook, Crow, Finch*, with many more; *Fish—Salmon, Roach, Pike, Herring, Crabbe, Dace, Chubb, &c.* Most of the following, not inserted here methodically, are appropriated to form surnames, both in a simple and compound state :—*Fountain, Moor, Church, Heath, Kirk, Booth, Bell, Wain, Meadows, Way, Lane, Rock, Stone, Bush, Cave, Clay, Snow, Hay, Den, Horn, Street, Hall, Hill, Butter, Child, Man, Box, Steel, Moon, Winter*, with a great many besides. Why the common names of such objects were chosen as surnames, it is difficult to say, but the choice was evidently made without much fastidiousness; and for that reason we conclude, that they were chiefly appropriated to the "lower orders," who were neither disposed, nor had a right, to be nice. It is not improbable that some of them were given in mockery; and perhaps they had some reference to persons' employment. Of this sort probably are —*Adshead*, *Trotter*, *Cannon*, *Pott*, *Carr*, *Cork*, *Cotton*, *Plant*, *Needle*, *Pool*, *Bridge*, *Gill*, *Parrot*, *Hook*, *Kettle*, *Webb*, *Trowell*, *Coulter*, *Cousin*, *Crew*, *Bell*, *Harrow*, *Tun*, *Spur*, *Silver*, *Mattock*, *Hood*, *Dray*, *Court*, *Cowl*, and a very great number besides.

I now proceed to *another class* of surnames— those which were derived from, or related to, the titles, offices, professions, the trades, handicrafts and employments, and the character and personal qualities of individuals.

The surnames included in this class are exceedingly numerous; but the evidence of their origin is so obvious, that a long enumeration is not deemed necessary. Derived from titles we have *King*, *Prince*, *Lord*, *Baron* &c. From offices are derived *Pope*, *Abbot*, *Priest*, *Monk*, *Bishop*, *Constable*, *Mayor* or *Major*, *Bailiff*, *Chamberlain*. Borrowed from professions we find *Law*, *Physick*, *Clerk*, *Leech*, *Parson*, *Sermon*. From trades are derived *Merchant*, *Draper*, *Hawker*. Numerous handicrafts have been adapted as surnames—*Turner*, *Carpenter*, *Cooper*, *Mason*, *Thatcher*, *Weaver*, *Arrowsmith*, *Cartwright*, *&c.* And the following are to be referred to character and personal qualities—*Meek*, *Cross*, *Merry*, *Wilde*, *Jolly*, *Moody*, *Wiseman*, *Idle*, *Long*, *Short*, *Strong*, *Swift*, *Quick*, *White*, *Brown*, *Scarlet*, *Green*, *Black*, *Armstrong*, *Strongitharm*, *Merriman*, *Sharp*, *Savage*, *Gay*, *&c.*

In reference to the surnames which have passed from trades and employments to those who followed them, it is to be remarked, that those only have been thus appropriated which were known and in practice in comparatively early times; whereas those which had their origin in later times —since about the reign of Elizabeth—will not be found to have come into use as surnames Thus the denominations *Smith*, *Taylor*, *Potter*, *Fuller*, *Arrowsmith*, *&c.* are very numerous; but we have no *Confectioner*, *Banker*, *Barrister*, *Printer*, *&c.* whose origin is of later date. This observation if correct, will furnish some means of discovering the time when surnames came into use in this country.

Another class of surnames originated in the disposition of our countrymen to make merry at the expense of their neighbours; that is, they were *nicknames*, to which some personal deformity, or some outward circumstance in the appearance, history or character gave rise. Thus we observe the not unfrequent occurrence of such names as *Sheepshanks*, *Crookshank*, *Shufflebottom*, *Muddiman*, *Wildgoose*, *Gudgeon*, *Sourbutts*, *Longbottom*, *Rainbow*, *Curson*, *Stringfellow*, *&c. &c.* It is impossible to say, what proportion of the numerous list of surnames at present in use owe their existence to this waggish propensity; but I think the number of them is much greater than is commonly imagined. Why should such designations be adopted as *Wolfe*, *Lion*, *Snow*, *Broadbent*, *Wildbore*, *Ladyman*, *Brownbill*, *Savage*, *Lovelace*, *Jiggs*, *Bray*, *Woodhead*, *Sweetlove*, but for the sake of insinuating some ridiculous or reproachful circumstance, or of conveying some satirical reflection?

As many surnames belonging to this class cannot be etymologically anatomized without some violation of the existing notions of decorum, I take only this general notice of them; though as to their general conventional use, we know that through the potent influence of custom, they are daily pronounced by the most pure lips without any sense of indelicacy.

This mode of designating men by nicknames, in the place of surnames, is still pretty extensively used among the lower ranks in this country, and especially in the county of Lancaster ; and they supersede temporarily the authentic surnames without however acquiring the same permanence, as in days of yore. In some parts of this county, and perhaps indeed in most parts of England, among the common people, persons are scarcely ever distinguished by their right name, and it is often unknown to their nearest neighbours. I believe that the most common analogy which guides them is, uniting the name of the father to that of the son, with the particle *of*, or rather *o'* between them. Thus if the son's name be Thomas and the father's John, the son's name, according to this rustic fashion, will be Tum o' Jack's. I have heard it mentioned, that in some village there was a person whose colloquial name was Dick o' Dick's, that is Richard the son of Richard, and his son, whose christian name was John, obtained the name of Jack o' double Dicks. The practice also of inflicting temporary nicknames on parties misbehaving themselves, or quarrelsome and insulting, especially among school boys, is well known. One of the most common complaints in a school is 'He is calling me names.' Such names however, are in general quickly forgotten, and are seldom delivered down to fame.

It does not appear from the histories which I have had an opportunity of consulting, that surnames in their present form were in use in the Saxon times, nor till a considerable time after the Norman conquest—and that more especially among the commonalty: they were applied probably to few, if any, who had no pretension to the rank of *Gentleman*. The history, therefore, of the first introduction of surnames is uncertain. It resulted from no civil regulation or order; but the use of them derived its origin from the influence of opinion and fashion, and from the progressive improvements and refinements of society: it had, properly speaking, no fixed period of commencement. All, therefore, we can do is, to make such inferences as appear probable from the mode in which our historians have designated the personages spoken of in the respective periods. But even such inferences cannot be wholly relied on, because, I think, the historians have sometimes modernized the manner of recording the names. The oldest records should therefore be consulted, which are accessible only to a few. One thing appears to me pretty evident—Not only was the introduction of surnames as now used gradual, but that form of designation commenced with the aristocracy; it descended from the gentry; and it found its way at length to the common people, but at a late period, and, I believe, after the entire extinction of vassalage.

In the Saxon times, the personages recorded were generally designated by only one name: thus, Alcwyn, Dunstan, Stigand, Harold, &c. When distinction appeared necessary, some obvious circumstance was chosen for the purpose, as Dunstan the monk, Stigand archbishop of Canterbury. Harold indeed had a surname, perhaps a nickname, given him from a personal quality—Harold Harefoot: similar to the Homeric distinction of Achilles—Ποδαρκης διος Αχιλλευς.

After the conquest, and during most of the time the English Crown had dependencies in France, the French mode of using names became very prevalent, and continued so during several reigns; but after that, surnames in the form now used, though for a considerable time far from being so general as at present, came gradually into vogue. The French mode of designation was by connecting the christian name of a person with some locality by the particle *de*. This mode was generally disused in the reign of Henry VII. if not before. I believe, indeed, it was always confined to the rank of Gentlemen—the gentry—and deemed fashionable. The surnames thus formed were never considered properly English and vernacular. The names of John of Gaunt, Geoffry of Monmouth, William of Malmesbury may seem to prove the contrary. But those were not properly surnames, currently used in the life-time of those persons, but subsequently adopted to designate them; or perhaps a translation of the names usually applied to them in Latin—the language most commonly used by our early writers: thus, Gulielmus Malmesburiensis—William of Malmesbury. The English particle *of* is at present used only with some titles of nobility, as Duke of Devonshire; and the particle *de* is affected, as being redolent of ancientness.

Before the period above specified, since which the common people have become distinguished in general by surnames as at present, the personal distinctions devised were commonly as follows :—Jack the Ploughman, Tom the Smith, Dick the Groom, Ned of Lincoln, &c. But after a time these were modified into the present forms, John Plowman, Thomas Smith, Richard Groom, Edward Lincoln.

I observe in confirmation of the remark above made, that in perusing some of our old English writers the classes of surnames ending in *son* and *ton*, at the present time the most numerous, do not often occur before Henry VII. The first time a surname terminating in *ton* occurred to me is in Rich. II. 1387.—*Lockton*, a sergeant-at-law. A few occur in Edward IV.—*Barton*, *Preston*, *Brampton*, *Wharton*. This was in the latter end of the fifteenth century. The first surname I observed ending in *son* is *Curson*, (seemingly a nickname) in Hen. VI. In Edwd. IV. occur *Tomlynson* and *Danson*. In Sir Francis Bacon's History of Henry VII. a surname in *son*, only once occurs—the notorious *Empson*.

For some of the names above enumerated I avail myself of this opportunity to say, that I am indebted to my friend and fellow member of this society, Mr. John Owen; who is far more conversant with old records than I am. And I close this imperfect Essay with the conclusion of his note to me on the subject of it; which agrees in the main, as to the time of the origin of surnames with my remarks.—"It would appear that so early as Henry III. surnames without the *de*, or other similar adjunct, were in use, though not general; and from the lists in Richard II. and Edward IV. the adjunct was then almost disused. The variety is quite as great as at the present day, and some of the names as whimsical."

A SKETCH

OF

THE LIFE & CHARACTER

OF

JOHN EDDOWES BOWMAN, Esq.

F. L. S. & G. S.

BY JOHN JAMES TAYLER, B. A.

Ad hoc proderit nobis inspicere rerum naturam; primo discedemus a sordidis, deinde animum ipsum, quo magno summoque opus est, seducemus a corpore. Deinde in occultis exercitata subtilitas, non erit in aperto deterior.—Seneca, Nat. Quæst. iii.

(Read October 4th, 1842.)

It has been customary in this Society to record some notice of the life and studies of such of its deceased members, as have distinguished themselves by the successful cultivation of any branch of literature or science. The late Mr. Bowman had been associated with it for a very short period; but, during that time, he had acquired so prominent a station among its members, and conferred so much interest on its meetings by the value of his communications, as fully to compensate for the brevity of his connection, and naturally to demand from the Society some tribute of respect to his memory. In compliance with the expressed wish of the Council, the following Memoir has been prepared.

The lives of men devoted to the pursuit of knowledge rarely abound in external incident: their most interesting biography is furnished, where it is attainable, by the history of their minds—of the circumstances which influenced their views, and determined their inquiries, and modified the conception and execution of their different works.

It is fortunate, that in a diary of reflections and observations, which Mr. Bowman began to keep at an early age, and extracts from which have been kindly supplied to me by his family—we possess some interesting data, for tracing the progress of his studies, and the formation of his character, up to the period when he was able to devote himself entirely to science, and became honourably known to the world.

He was born in 1785, at Nantwich in Cheshire, where his father, Mr. Eddowes Bowman, carried on the business of a tobacconist. His education was ordinary and contracted, such as could be obtained in those days from a grammar-school in the neighbourhood; but he very early discovered a love of knowledge, and was distinguished, as a child, for his powers of reflection and his quickness of observation. His holidays were chiefly spent in reading, and his pocket-money was hoarded up for the purchase of books. From his father he derived a taste for horticulture and considerable knowledge of botany.

On quitting school, he was placed, as a matter of course, behind his father's counter: but the employment proved exceedingly distasteful to him; and having urged his father to enlarge the manufacturing department, he was entrusted with the sole management of it. He afterwards travelled for orders; but this occupation and the society into which it threw him, he found equally disagreeable. Amidst these uncongenial engagements, his desire for mental improvement and his habits of observation continued to increase. Suffering from shortness of sight, his sister still remembers the delight which he manifested when a boy, on being presented by his father with a pair of spectacles, which enabled him to see and enjoy distant objects. About fifteen he became very serious, and expressed in writing his solemn purpose to devote the remainder of his life to the pursuit of wisdom and virtue. The spirit of this youthful determination never afterwards forsook him; we may trace its influence in every page of his future diary. During a visit at Sheffield, he acquired a fondness for heraldry and genealogy from his friend Hunter, a gentleman who has since attained to high reputation as a topographer and antiquarian. Whatever he undertook he followed with great ardour; and, in the prosecution of this new object, he would walk for miles over the country, during his intervals of leisure, to visit the churches and copy monumental inscriptions, and collected copious genealogical materials from which he made out the pedigrees and emblazoned the arms of many Cheshire families.

Another example of his industry was a minute analysis of Gibbon's Decline and Fall of the Roman Empire, filling a thick octavo volume. His mind was also much interested in religious questions, and he read as largely as his opportunities would allow on controversial divinity, embracing then, as the result of his inquiries, the opinions which he retained through life. These various pursuits, it must be remembered, were cultivated amidst the distractions and annoyances of a business wholly unsuited to him; but they formed the solace and the charm of his life. They withdrew him almost entirely from the usual recreations and gaieties of the young; and when he was about sixteen, as business required his presence soon after six in the morning, in order to gain more time, he had a small table fitted up at the head of his bed, and rose between three and four, summer and winter, that he might devote himself without interruption to study.

The seriousness of his disposition, and his love of books, gave him a desire to enter the Christian ministry, but in compliance with his father's wishes he relinquished the design. His marriage in 1809 with a cousin, the daughter of Mr. W. Eddowes of Shrewsbury, added greatly to his happiness, by uniting him for life with a companion qualified to appreciate his tastes and to share in his favourite pursuits. From this time the bias of his mind was fixed, future years only more fully unfolding the views and tendencies which became henceforth the ruling influence of his life. Every page of his diary records his attachment to the quiet and simple pleasures of home, and the deepening ardour of his enthusiasm in the study of Nature's works. His morning hours before breakfast were often devoted to the search of botanical specimens, and during these walks he sometimes amused himself with planting in suitable situations a few of the rarer English species, in the hope of seeing them naturalized. In the following extract from his diary, we have a very pleasing picture of his mind at this period of life, and may clearly trace in it the characteristic features which time more amply developed.

"On my return from Altrincham, I was struck with a pretty building in the cottage style, in front and around which, was a beautiful garden, with a profusion of climbing-plants, roses, shrubs, &c., tastefully laid out; a small pond contained the Nymphæa, Menyanthes, Butomus, &c.; a rockery too for mountain plants, and every thing requisite for a perfect garden.—I alighted, and was allowed to walk through this Elysium. The sun shone full upon the scene, and gave to the quivering dew, which hung upon the shrubs and flowers, all the varied tints of Iris; the birds were striving to emulate each other, in sending up their morning hymn of praise; and I should have been at a loss to say, what was wanting to make it an earthly paradise."—"At Northwich, explored the salt mines, in a vast subterranean cavern, of about two acres of surface, the superincumbent earth supported by irregularly placed square pillars of the rock salt. When I had made the scene sufficiently familiar to leave an impression on my mind, I ascended in the bucket, and in a minute and a half we found ourselves on the earth's green carpet, with my feelings more sensibly alive to all the pleasures of a fine summer morning, from the novel contrast of the scene I had just been contemplating. Thus in the course of two hours, was I gratified with a view of the wonderful operations of the Deity, in the most showy and conspicuous, as well as in the most concealed, parts of his works, and both of them, I think, will make a permanent impression on my mind."

In the long evenings of winter he occasionally resumed his genealogical pursuits, and traced back through several generations the pedigree of the family of Eddowes, from which he and his wife were descended. But the simplicity of his mind is conspicuous, in the remarks with which he accompanies in his Diary the record of this employment. "It will be a work of some labour, and fit only for the occupation of leisure time, being of no real value. Virtue, not ancestry, is the mark of true nobility. This pedigree I shall leave as a legacy to my descendants, who, I hope, will consider the examples of so many good and pious ancestors, as so many additional inducements, nay as obligations, to them, to tread in the same steps, and to adopt their moral virtues."

About the year 1813, his father having entered into a Banking concern, he joined it as the junior partner, and experienced much satisfaction in exchanging his former occupation for one, more liberal in itself, and admitting more leisure and repose of mind for indulging his intellectual tastes. The undertaking, through misplaced confidence, proved unfortunate; the property he had accumulated, perished; and in 1816 he found himself thrown upon the world, with only his own exertions to trust to for the support of a rising family. Soon afterwards he removed to Welshpool, where he took the management of a Bank connected with the firm of Messrs. Beck and Co. of Shrewsbury. At Welshpool he resumed the calm tenor of pursuits and enjoyments, which had been interrupted by temporary misfortune. Although he met with little congenial society there, the neighbourhood gratified his love of natural scenery, and afforded him many opportunities of pursuing his favourite studies. Habits of vigilant observation formed a part of his character; and in the course of his daily walks, his eye was ever on the alert to detect something new or curious or beautiful in the phænomena that were casually presented to his view. In the spring of 1820, his family having removed into the country for the sake of health, he walked over to breakfast with them every morning; and on one of these occasions, he was led to examine very minutely the branch-like appearance produced on the bark of the Ash by the larva of a small beetle.

I extract the following interesting passage from his Diary.—"It is the work of the larva of a small beetle. The parent, when about to lay her eggs, passes along the central line which she excavates between the bark and the solid wood, and as she goes along, deposits an egg at the commencement of every lateral branch. As the young weevil increases in size, it of course requires a wider space, which explains the widening of the lateral branches as they recede from the centre, till at the proper period it emerges from its dark labyrinth into open day. What a wonderful and beautiful instance of the economy of nature, as we generally call it, but of nature's God, who has stamped laws necessary for its existence and propagation upon the minutest insect! And this we call instinct. But this is not all; I observe that she excavates her central line in a horizontal direction as the tree grows, or across the texture of the vessels through which the sap passes, and the young larva always bore in the contrary direction, or along the line of the sap vessels, which seems to be a further mark of design; as it is evidently much easier for them to eat their way in this direction, than it would be to cut across the tough ligneous substance of the vessels."

It was during his residence at Welshpool, that the perusal of Cuvier's Theory of the Earth first awakened his interest in geology. With his usual activity of mind he proceeded at once to apply the new views he had acquired, to the objects immediately about him, and began to examine the traces of organic remains in the rocks of Powis Castle park. Geology and mineralogy furnished now the frequent occupations of his leisure hours; but in these new pursuits he was left entirely to his own resources, having neither guide nor associate among his acquaintance at Welshpool. He speaks of the delight which he felt in the accidental visit of two gentlemen, who were making a geological tour in North Wales, and of the fresh impulse imparted to his own studies from this transient intercourse with congenial minds. He possessed, however, too much ardour and perseverance to abandon an object, once deliberately undertaken, for the want of external stimulus. Not finding the Trap rocks in his neighbourhood described in Phillips and Conybeare's work on geology, he collected a series of specimens, and transmitted them to Mr. Phillips with a request that he would give him their names.

In 1824, he became the managing partner of a bank at Wrexham ; an arrangement which occasioned his removal to that place, and eventually, to the Court, a pleasant country residence in its immediate vicinity. In laying out and embellishing the garden attached to his new abode, and in rambles through the beautiful woodland scenery surrounding it, he again found ample opportunities of gratifying his predominant tastes. The romantic woods of Erddig, contiguous to his home, he alludes to with evident satisfaction in his diary, as rich in botanical and entomological treasures. He was often strolling through them in the morning, as early as it was light—sometimes in winter even by moonlight—hunting for specimens, and seldom returned without something curious or interesting to lay on the breakfast-table for the instruction and entertainment of his family.

It was at this period, that he resolved on improving his knowledge of the grasses, a tribe of plants to which he had not before paid much attention. His fondness for minute observation appears from the nature of the objects which he peculiarly selected for study. Cryptogamic botany had remarkable attractions for him; and his interest in it was quickened by a fasciculus of dried specimens from the neighbourhood of Oxford, lent him by the Rev. J. M. Luxmoore. The fungi engaged a large share of his attention. His quick eye discovered a metallic lustre on a very diminutive species of moss, seen only under a particular incidence of the rays of light, amidst the twilight gloom of excavated rocks. The minutest specimens of the insect tribe, the threads of the gossamer spider, and animalculæ—the wonders of nature in her most hidden recesses—invited the scrutiny of his exploring mind; aud in these researches he was constantly aided by the microscope, which became his favourite instrument of investigation. The larger views of geology supplied a pleasing and beneficial variety in the range of his studies and observations. In his rides over the country, he made himself acquainted with the stratification of the rocks, exquisitely enjoying at the same time the charms of romantic scenery. His notices of these excursions in his Diary breathe a healthful spirit of contentment and satisfaction at the good of all kinds which he saw spread around him. "How delightful," he exclaims, "to live in such an age of science and improvements! It gives an air of magic to a country with which one was previously acquainted, to see it so much changed by new roads, bridges and canals, lime and coal works, the road often carried over vallies on artificial terraces, or through a rock which has been cut through to make way for it."—Nature indeed had endowed him with a constitutional aptitude for scientific observation; we never find in him a trace of the fastidious querulousness of the mere man of taste and letters. Elegant literature held a subordinate place in his estimation. At the recommendation of a friend, he read some of the novels of Sir Walter Scott; but while he admitted their merit and evidently felt their beauties, his conclusion was—"after all give me a page of true philosophy or science for a volume of these."

His journeys and seasons of relaxation had always some reference to the studies, which formed more and more the chief interest and occupation of his life. With this view, he visited successively the English Lakes, Scotland, and Switzerland; and returned from all these excursions, enriched with botanical treasures, and furnished with a mass of well observed facts, to guide him in his future inquiries. Of his Scotch tour he wrote out a very full account, which some friends to whom it was submitted, strongly urged him to publish. This he declined doing, on the ground, that it did not contain a sufficiency of new matter, and expressed a hope, that his family after his death would never listen to a similar suggestion. His desire of interesting his children in his own pleasures, induced him to study sketching from nature according to the laws of perspective, that he might practise it with them; but the weakness of his eyes which were never entirely free from pain, compelled him to give up the pursuit.

In 1830, he retired from business, in the execution of a wish which he had long cherished—to spend the remainder of his days in the undisturbed enjoyment of domestic happiness and the cultivation of science. Although it was in the field of contemplation and experiment, that he was by nature fitted to excel—he was distinguished, as a man of business, for prudence, judgment and punctuality, for the strictest honour, and for great energy and decision in cases of difficulty and danger. He possessed in an eminent degree the habit of calm self-reliance. Once convinced that a measure was right, nothing could shake him from his purpose of adhering to it. Owing chiefly to his firm and cautious conduct, the Bank with which he was connected at Wrexham, passed safely through the perilous crisis occasioned by the panic which affected the pecuniary transactions of the country in 1825-6.

His last change of residence was to Manchester, where he took up his abode in 1837. Here he experienced much happiness from constant intercourse with men engaged in kindred pursuits ; a new field of observation was opened before him, and he applied himself to it with his characteristic promptitude and ardour. He entered heartily into the objects of all institutions devoted to the encouragement and extension of knowledge. To the interest and improvement of the meetings of this Society he always largely contributed by his stores of information, and by the readiness and simplicity with which they were imparted. Of the Geological Society of Manchester he may be considered, if not the founder, at least one of the most active organizers and efficient supporters. His papers form a large and a very valuable portion of the first volume of its transactions. Desiring to end his days among friends who had given him so cordial a welcome, and who appreciated and shared his love of science, he had purchased land in the neighbourhood of Manchester, and begun to build himself a house, laid out with an especial view to the reception of his valuable geological and botanical collections, which he hoped to have completely arranged before the approaching meeting of the British Association. But these designs were not destined to be fulfilled. The great exertion and fatigue which his ardour had impelled him to undergo during a geological examination of a part of North Wales, in the summer and autumn preceding his death, occasioned a debility and exhaustion, from which he never entirely recovered; and repeated colds, supervening on his weakened frame, brought on a disorder which terminated his life on Dec. 4th, 1841, in the 57th year of his age.

It was only a year or two before his final release from business, that he began to communicate to the public, in some papers read before the Linnæan Society, the fruits of his long continued observation and reflection. He hailed with delight the appearance of the Magazine of Natural History, as a work fitted to improve the tastes and pursuits of the rising generation, and offered to be an occasional contributor to its pages. Possessed at length of leisure and the power of concentrated attention, he took an active part in the advancement of his favourite sciences, and became a productive member of the Linnæan and Geological Societies, and of the British Scientific Association. Nearly all the papers that now furnish the extant proofs of his knowledge and industry, were written in the tranquil period which embraced the last twelve or thirteen years of his life; and a brief notice of these, in the order in which they were produced, will enable us to trace the progress of his studies, and their relation to the pursuits and feelings of his early life, till the close of his mortal career.

His earliest communication to the Linnæan Society was in the year 1828.* In the damp and shady parts of Erddig Wood, on some decaying branches

* Account of a new Plant of the Gastromycous Order of Fungi. Read Feb. 19.

of decorticated oak that were lying on the ground, and hardly distinguishable from them by its colour, he discovered a most diminutive species of fungus, which owing to its extreme minuteness and its resemblance to other plants of the same natural family, had been overlooked by former botanists. From its peculiar structure, which could only be detected by the compound microscope, he gave this plant the name of *Enerthenema elegans.**

In the following year he made another communication to the same Society in a letter addressed to Robert Brown, Esq., F. R. S., and V. P. L. S.† This paper exhibits in an eminent degree his patience and accuracy in conducting the minutest observations, and his acuteness in making distinctions, and noticing exceptions to general laws. With extreme care he separated the parasite from the root of the ash tree to which it had adhered, and then subjected it to a microscopic examination. In its mode of growth he pointed out a beautiful instance of what Paley has called an interrupted analogy; its stem, contrary to the usual tendency of plants, shooting downwards

* From ἔνερθε and νῆμα, alluding to the insertion of its filaments into the under surface of the pileus.

† On the Parasitical Connexion of Lathræa Squamaria, and the peculiar structure of its subterranean leaves. Read Nov. 3, 1829.

till it meets the roots of the tree, from which it is to derive its nourishment, when it sends out its fibres horizontally in every direction, and fastens itself by the tubercles in which they terminate, on the roots and fibres of the tree. The internal structure of these tubercles, so minute that they had escaped the notice of preceding botanists, he explored by the aid of the microscope, and then traced the course of their fibres to the point of insertion between the imbricated scales on the stem of the parasite, from which it takes its name. These scaly appendages to the subterranean stem of the Lathræa he showed were not *roots*, for which they had been mistaken, but *true leaves*, since the cells imbedded in their succulent substance, to which he also applied the microscope, indicated by their structure, that they were adapted to absorb air rather than extract moisture. The absence of green in these leaves he explained by their performing the office of absorbents under ground.

At the meeting of the British Association, at Bristol, he read a very interesting paper, which was afterwards enlarged, and inserted in London's Magazine of Natural History,—"On the longevity of the Yew, as ascertained from actual sections of its trunk; and on the origin of its frequent occurrence in churchyards." Although the Yew was known to be of slower growth and greater durability than any other European tree, he showed that its actual age could only be approximated by an analysis of sections, and examinations of annual rings. De Candolle had first applied this method to ascertain the age of trees; but by assigning too little space for the rings in young trees, and too much for those in old, he made the young trees too old, and the old too young. Mr. Bowman improved upon and corrected his process. With great accuracy, by the aid of an instrument made for the purpose, he obtained sections of the annual rings of trees, from different sides of their trunk; and by comparing these with each other, marking the proportion between the number of rings and the space occupied by them at different periods of growth, and then striking the average of these separate observations,—he arrived at a general result which led him to conclude, that there were trees still in existence of enormous antiquity,—one in Gresford Churchyard near Wrexham, which these calculations made 1419 years old,—another in Darley in the Dale, 2006; and some yet standing he supposed might have an age of more than 3000 years. The introduction of the yew into churchyards he ascribed to a sentiment of heathen superstition, which regarded it as an emblem of immortality, and which was afterwards, from concession to these natural feelings, adopted by Christianity. In this part of the paper we are reminded of his early fondness for antiquarian lore.

In the summer of 1836, he discovered near Ellesmere, and minutely described, a species of parasite, growing exclusively on flax, which Sir W. Hooker, on specimens being forwarded to him, considered to be a species not before noticed in Britain.* An account of this plant, with some remarks on the general growth of parasites, was read by Mr. Bowman, at the Meeting of the British Association in Birmingham, in August 1839; and afterwards, in an enlarged form, before the Literary and Philosophical Society of Manchester, Nov. 12, 1839.

The subject to which Mr. Bowman devoted his attention almost exclusively during the last years of his life, was geology; and in his papers on this branch of scientific research, we perceive the assistance he derived from his early familiarity with

* On a new British Species of Cuscuta, and on some peculiarities in the structure of the Genus as Parasites.

the botanical department of Natural History, and from the habits of patient and accurate investigation which he had exercised in cultivating it. In 1838 he communicated to the Geological Society through Mr. Murchison, some "Notes on a small patch of Silurian rocks to the West of Abergele, on the northern coast of Denbighshire," which he had visited in the summer of the preceding year.* This paper contains a brief description of the dip, appearance and contents of successive strata, and betrays in a few simple touches the deep feeling of its author, amidst the minutest scientific observation, for the beautiful of natural scenery. Two years afterwards at the request of Mr. Murchison, he undertook an examination of the Silurian rocks in the vale of Llangollen, with a view to ascertain the boundary of the Silurian and Cambrian systems in Montgomeryshire and Denbighshire.† This boundary from the dislocated state of the country, he was wholly unable to trace; but he exhibited in his paper some new geological features which had come to light in the course of his researches.

* Transactions of the Geological Society. Read April 25th, 1838.

† Notice of Upper Silurian Rocks in the vale of Llangollen North Wales, and of a contiguous eruption of Trap and Felspar. Manchester Geological Transactions, Vol. I. Art. x. Read February 25th, *1841*.

On removing to Manchester, in the centre of a district distinguished for its vast mineral wealth, Mr. Bowman, with that admirable good sense which exercises its curiosity on contiguous and accessible objects, and with his characteristic fondness for tracing the prospective subserviency of the great arrangements of nature to the use and convenience of future conditions of existence—applied his mind to the solution of one of the most interesting problems of geological science—the origin of coal. He read a paper on this subject before the Manchester Geological Society, which was afterwards printed in their Transactions.*

* On the Origin of Coal; and the geological conditions under which it was produced. Manchester Geological Transactions, Vol. I. Art. v. Read January 30th, 1840.

It is hardly necessary to remind those who are at all acquainted with the history of geological science, that many of the views adopted by Mr. Bowman in this and the ensuing paper on the Fossil Trees had been already maintained by previous inquirers. His great merit consisted in the luminous statement and combination of these views, and especially in accounting for the origin of the successive seams of coal by applying for the first time to the peat bog theory, the periodical depressions of the earth's surface, as recently observed by Mr. Charles Darwin. For this last remark I am indebted to Mr. E. W. Binney, Secretary of the Manchester Geological Society.

In the first part of this paper, he exhibited the proof of the vegetable origin of coal, showing how peat is formed from the decomposition of vegetable matter, and how it might, after a sufficient lapse of time, under the ancient conditions of the globe, be converted by pressure and exclusion from the atmosphere into coal. In the second part, which proposed to explain through what processes in the immense laboratory of nature this conversion is effected, he assumed in place of the old notion, that the level of the sea had varied, the now generally received theory of the successive elevation and subsidence of the land, proved by effects still in operation; and instead of supposing the vegetable matter ultimately converted into coal, to have been drifted into its present situation, he expressed, as the result of his inquiries, his belief, that the trees and other vegetable matter from which the beds of coal are derived, grew on the identical spots which the latter now occupy. The process—which he illustrated by very intelligible drawings—he conceived in general to be this :—that over an extensive district there were partial subsidences of surface, clothed with a luxuriant vegetation, which was then covered up and pressed down by an accumulation of mud and sand ; and that the inequalities in such subsidences, occurring at long intervals and affording time for a copious deposition of sedimentary matter—and occasionally taking place with a more paroxysmal rapidity—caused the inequalities and interruptions and divisions now observable in the successive seams of coal.

In the midst of these inquiries, and tending to confirm the conclusions which they had led him to embrace—a discovery was made in the spring of 1839 by Mr. John Hawkshaw, F. G. S. of some very remarkable Fossil Trees on the line of the Bolton Railway near Manchester. To these venerable relics of a former world, his attention was at once and eagerly directed, and he communicated an account of them to the Manchester and London Geological Societies.* His paper exhibits, at least in part of it, a very interesting illustration in a particular case opportunely brought before his notice, of the general views which he had expounded in his Essay on the origin of Coal. He ar-

* Observations on the characters of the Fossil Trees lately discovered on the line of the Bolton Railway, near Manchester. Manchester Geological Transactions, Vol. I. Art. vi. Read January 30th, 1841. Read also before the London Geological Society, February 26th, 1841.

gued that these fossil trees must be now on the spot where they grew, since they are perpendicular to the strata of coal on which they rest; for had they been drifted to their present situation, he contended it was in the highest degree improbable, they should all have remained so regularly disposed at a similar angle in the same direction to the horizon. He further endeavoured to show, from an examination of structure, that most of these were hard-wooded trees, which had become hollow through internal decay, in the same way as trees of a similar structure become so in tropical climates at the present day; and that the hollow cylinders thus produced, formed a sort of matrix within the carbonized bark for a mass of sedimentary deposit, which presents in its now exposed state, an exact cast of the original tree. At the close of this paper he threw out two suggestions for the consideration of future inquirers; (1.) Whether from the size of these trees and their probable rate of growth in a tropical climate, it be possible to draw any inference as to the *minimum* of the period which it would require to form a mass of vegetable matter, yielding a coal-bed nine inches thick; and (2.) Whether the amount of shrinkage can be approximated, which such a mass from its compression and consolidation must undergo on its conversion into coal.

The wider views of the Universe opened by geological speculation, did not however wholly withdraw his attention from the minute investigations to which he had been early attached ; indeed, in the chain of his ideas, the minute and the vast were ever placed in an interesting and intelligible relation to each other. In November 1839, he read a paper before the Manchester Geological Society, "On a white Fossil Powder found under a Peat Bog in Lincolnshire." * A quantity of this powder was given to him by Mr. E. W. Binney, who had discovered it, with a request that he would examine it closely. Upon subjecting it to the scrutiny of the microscope, Mr. Bowman found that it consisted of the siliceous remains of fossilized *Confervæ*, standing in the same relation to that class of plants, as the fossil remains discovered by Ehrenberg, to animalculæ. Its magnified particles exhibited the characters of crystallization, which he supposed to mark the earliest stage of

* Manchester Geological Transactions, Vol. I. Art. vii. Prior to his own paper, Mr Bowman had read a Memoir on these Confervæ, by Mr. E. W. Binney, at the British Association in 1839.

transition from inorganic to organic matter. The discovery of their true nature he considered as an important addition to the Fossil Flora.

The last scientific question which exercised his thoughts, and which was deeply interesting him at the time of his death, was that respecting the former existence of glaciers in places where they are no longer found, which had been brought into notice by the original views and bold generalizations of Professor Agassiz. There was indeed a singular coincidence between the results of some of his personal observations and the views propounded by Agassiz. On his return from the scientific meeting at Glasgow, he took the opportunity of visiting some natural terraces on the Eildon hills round Galashiels, in the neighbourhood of Melrose and Abbotsford in Selkirkshire, to which his attention had been called by an article in Chambers's Edinburgh Journal, where they were attributed to the action of water and considered as ancient beaches. On examining the spot, he was convinced that they could not have been produced by such a cause, and stated the grounds of his opinion in an article communicated to the Philosophical Magazine and Journal of Science. Before the article appeared, he saw the announcement by Agassiz of the evidence he had collected of the former existence of glaciers in Scotland; and being soon after favoured with a visit by the Professor himself, he was fully persuaded from conversation with him, that the appearances which he had witnessed on the hills round Galashiels, were to be ascribed to the action of glaciers. This solution of his previous difficulties he at once forwarded to the same journal, accompanied by a brief exposition of the views of Agassiz respecting glaciers, and an application of them to the case of the Eildon hills. It must be in the remembrance of many of us, that the last communications of our regretted friend to this society, were a series of papers explanatory of the glacial theory, and presenting, in a condensed form, the substance of Agassiz's work, *Etudes sur les Glaciers de la Suisse.* Though he was greatly captivated with the new views opened by Agassiz's theory, and undertook a tour into North Wales, in the full expectation of discovering the traces of former glaciers, it is no small proof of the philosophical caution and self-restraint of his mind, that he resisted the seduction of some deceitful appearances, and candidly confessed that the evidence he had looked for, was wanting.—"Believing," he says—in words that well express the governing spirit of his inquiries, and may fitly terminate this very imperfect analysis of his labours—"after deliberate examination, that these appearances have either been produced by other adequate causes, or could not have been due to glacier action, I have felt myself bound honestly to state the conclusions I have arrived at, being satisfied, from some experience, that to allow the observation or the judgment to be warped by preconceived theory however plausible, or to decide on partial and insufficient evidence, must be ultimately injurious to the cause of truth."*

In reviewing the scientific career of Mr. Bowman, we are struck with the remarkable aptitude of his mind, for the observation of the most minute and delicate phenomena. It was in the boundless field of wonders laid open by the microscope, that he was eminently qualified to make discoveries, and increase our acquaintance with the hidden organizations of nature. Mosses and fungi were objects, the unsuspected beauty and marvellousness of whose structure he loved to bring to light. The quickness of his eye was

* On the Question, whether there are any evidences of the former existence of glaciers in North Wales. In the *Philosophical Magazine and Journal of Science*, for Dec. 1841.

indeed surprising. Imbedded in the broken fragments which form the stone-walls of Derbyshire, he discovered a fossil before unknown, which has been named from him,* but so exceedingly diminutive, that, in the specimens containing it, the eye of the ordinary observer had to be guided by narrow slips of white paper pointing towards it. The shortness of his vision might, in this respect, be considered as a sort of optic power with which nature had furnished him for pursuing his peculiar line of research; and his example instructively shows, how a thoughtful, active mind may convert even a natural defect into a means of additional usefulness and enjoyment. It would, however, be greatly wronging his memory to insinuate, that his singular acuteness and accuracy of observation were due even principally to physical causes; these may have contributed to give the original determination to his studies, but the eminent success which attended his cultivation of science, must be ascribed to mental and moral discipline. His early habit of recording the impressions and observations that had occurred in the course of the day, his strict economy of time, and the power which he had acquired of concentrating his whole attention on the object before

* Endothyra Bowmanni.

him—were, doubtless, among the causes of the intellectual excellencies for which he was distinguished; while the simplicity and temperance of his private life, and the tranquillizing influence of his deep devotional spirit, furnish another instance of the connexion which it is always so pleasing to trace, between mental vigour and moral purity. The exact and discriminating observation of minute appearances, which the pursuits of his early life had led him to cultivate, proved of singular advantage, when his mind was directed to the wider views of geology; for the sound maxim of Dr. Samuel Clarke is as applicable to the study of nature as to that of philology,—"ex judicii consuetudine in rebus *minutis* adhibitâ, pendet sæpissime etiam in *maximis* vera atque accurata scientia."

Of the two grand and fundamental types of intellect, to one or other of which Bacon,* in his

* Maximum et velut radicale discrimen ingeniorum, quoad philosophiam et scientias, illud est; quod alia ingenia sint fortiora et aptiora ad notandas rerum differentias; alia ad notandas rerum similitudines. Ingenia enim constantia et acuta figere contemplationes, et morari, et hœrere, in omni subtilitate differentiarum possunt: Ingenia autem sublimia et discursiva, etiam tenuissimas et catholicas rerum similitudinès et agnoscunt et componunt: Utrumque autem ingenium facile

Novum Organum, has referred all the cultivators of science and philosophy—and which a former member of this society—the late lamented Dr. Henry—has eloquently delineated, in some of their highest developments, in his contrast of the philosophical characters of Wollaston and Davy—* that which dwells on distinctions and detects exceptions, and that which perceives similitudes and embraces generalisations—we can hardly be mistaken in classing Mr. Bowman's mind under the former. It was more prone to discriminate than to combine, and more largely endowed with the logical, than with the imaginative, faculty. He rose, indeed, very far above the character of a mere collector of facts. His mind was truly philosophical. It distinctly recognised the general laws and pervading analogies, which the researches of previous inquirers had well established in the several kingdoms of nature ; but its peculiar function consisted in detecting and examining new phenomena under the guidance of those laws and analogies, in extending the range of their application, and pointing out, in particular cases,

labitur in excessum, prensando aut gradus rerum aut umbras. —Lib. I. Aphor. lv.

* In the Preface to the last edition of his Chemistry.—pp. viii–x.

the exceptions to which they were liable. He was more disposed, from the natural caution of his mind, to limit a general principle admitted by others, than to carry it, in his own speculations, a single step beyond the line ascertained and fixed by the observation of phenomena. We have a striking example of philosophical prudence and integrity, in his treatment of the theory of Agassiz respecting glaciers.

These habits of thought, combined with his great knowledge, more especially of the vegetable kingdom of nature, rendered his services as an inquirer peculiarly important in the present state of geology,—a science, which offers so many inducements to rash speculation, and which its own friends and cultivators have charged with precipitate generalisation.—One quality of a true philosopher we may unreservedly claim for him—the love of knowledge for its own sake. No mind was ever more free from all feelings of envy and jealousy, or the craving after that distinction in the world, which high intellectual endowments are supposed to confer, and for which alone some men seem to think they are worth cultivating. Truth was to him a treasure which carried its own recompense with it. In fact, he was too happy in the possession, to care much about the fame, of science. And yet, by loving truth in the first place with an undivided affection, he had secured more of the adventitious honours that sometimes follow in her train, than many of her disloyal and interested servitors. During the comparatively few years that he was known to the scientific world, he had risen rapidly in reputation, and acquired the esteem and confidence of many eminent men. He eminently manifested, in the pursuit of his favourite studies, that self-abandonment and unconsciousness of ulterior objects which a popular writer of the present day, Mr. Carlyle, has declared to be an invariable accompaniment of the higher forms of human character, and without which, it may be safely affirmed, no great proficiency in any branch of knowledge will ever be attained. To look too keenly to the effect of our inquiries on other men's opinions respecting us, and to measure their value by too direct a reference to the utilities and conventional proprieties of the world, is the surest way to stifle all free, independent views of truth, and to strike at the very root of deep thought and thorough attainment. Early in life, the attention of Mr. Bowman was directed to a particular region of the great field of natural science, and he pursued his researches in it with all the ardour which the purest love of knowledge could inspire—his admiration of nature, and his reverence for its Almighty author, increasing with the extent of his acquirements. The object he had thus selected, and others akin to it, he followed with an unswerving attachment. He felt it was the walk in which he was fitted to excel, and he was satisfied with his choice. He never forsook it to court temporary distinction by a weak compliance with the varying temper of popular taste ; and, despising the affectation of universal knowledge, never offered his opinion but on topics which he had well considered and thoroughly understood. In the various situations to which he successively removed, we find him keeping the same objects constantly in view, only studying them under new aspects and in more extended relations. Hence, his whole life was calm and self-consistent. A continuity of thought and purpose pervaded it from beginning to end. In the wider speculations which engaged the concluding years of his life, we trace the natural development of the tastes and studies which had taken deep root in his mind from the commencement of manhood. To this healthful growth and expansion of his intellectual powers—the purity and simplicity of his private character—his aversion from the strife and vanity of the world—largely contributed. As he moved with noiseless step along the 'cool sequestered vale of life,' he fixed his clear eye with a tranquil delight on the glorious spectacle of nature's works, and received back their images on the smooth unsullied mirror of a virtuous mind.

With the modesty which belonged to him, he used sometimes to lament the contracted nature of his early education. It was not easy to perceive the traces of it in his writings or his conversation. His ideas, clear, precise, and well arranged, clothed themselves readily in a simple and unaffected phraseology: and the style of his published papers is admirably adapted to the purposes of instruction;—it is perspicuous and descriptive, and possesses a kind of natural elegance. It may be questioned, however, whether in some departments of human exertion, the absence of a scholastic education be so great a misfortune as has been generally supposed. In the ordinary modes of culture, the mind is often inundated with words which do not clearly convey the ideas of others, and has no opportunity of freely developing its own; its natural appetite for knowledge is anticipated, and its capacity oppressed with a quantity of food which it cannot digest and assimilate; it learns by rote, instead of being incited to observe and reflect; it trusts to authority, instead of relying on itself; and even under the most favourable circumstances, by having the whole field of knowledge conveniently mapped out before it, every object presented in its proper place, with all the requisite means and appliances of instruction—it is deprived of the inestimably valuable discipline of grappling with difficulties and learning by its own efforts to vanquish them, and of all that enthusiasm and energy of purpose which a noble object placed at a distance and beset with obstacles is fitted to inspire. It is of course not meant to be asserted, that the learning of schools and colleges is not of the highest value; there are some branches of knowledge which cannot be cultivated at all without it: but these considerations may be set off as some compensation on the other side. Among the most original thinkers and successful discoverers in the various departments of science and philosophy, the number is not small of those who have really educated themselves; and the inductive system which bestows such value on the accurate observation and arrangement of facts, has opened a wide field to labourers of this description, among whom Mr. Bowman must be allowed to occupy a distinguished place. His example affords the greatest encouragement to all who may fancy themselves excluded by their education or their circumstances, from the privileged walks of science. In science there is no aristocracy; every one whose bosom glows with the pure love of truth, whose knowledge is sound and accurate as far as it extends, who can correctly observe a fact and draw an inference—is already a member of its glorious fraternity which embraces every grade and condition of human society.

In all the relations of private life the character of Mr. Bowman was most amiable and exemplary; his affections pure and warm; his habits healthful and rational; his manners courteous and affable; his tastes elegant yet simple. Of his attachment to domestic pleasures and pursuits I have already spoken; it formed a prominent feature in his character.* When his known acquirements as a man

* His scientific tastes have been inherited by his children. Since this Memoir was read, the Royal Medal has been awarded by the Royal Society of London to his third son, Mr. Wm. Bowman, F.R.S. for his paper, On the Structure and Use of the Malpighian Bodies of the Kidney; published in the Philosophical Transactions for 1842.

of science drew him more into general society, he once expressed his anxiety to a member of his family, that such engagements might not wean him from his love of home and the simple enjoyments of his earlier years. One quality of his mind—such was his abhorrence of everything like parade and affectation—could only be fully known to those who were admitted into the interior of his domestic life—I refer to his strong religious feeling. His devotion was deep, and even intense. It was nourished by the nature of his habitual contemplations, which filled his whole life with the spirit of a sublime and silent adoration. This spirit pervades every page of his Diary, and sometimes breaks forth into direct and fervent prayer. Extracts of such a nature would be obviously a violation of the hallowed secrecy in which his pure mind veiled its deepest thoughts from the eye of the world; but those who have had the privilege of perusing these records of his daily meditations, could not hesitate to say of him, in the beautiful language of Scripture, that he was indeed "a just man who walked with God."

In his politics, he was a decided whig; in religion, a protestant dissenter; and his doctrinal opinions which he had embraced at an early age, were Unitarian. But on all these topics his mind was free from every taint of bigotry; in thinking for himself, he entertained no prejudice against those who thought differently.

In the foregoing sketch of his character, I have attempted to describe simply and faithfully the image left on my own mind by the various impressions received from living intercourse, the statements of his family, and the perusal of his different productions. If it be painful to reflect that our circle has been prematurely deprived of the advantage of his various information and acute understanding, we may at least derive instruction from his example, inviting us, amidst the feverish competitions of a worldly ambition, to a more pure, simple, and rational life, that seeks its happiness in resources accessible to limited means and an humble station, and its distinction in the genuine refinement inseparable from knowledge, taste and virtue.

ON THE

HEAT EVOLVED

DURING THE

ELECTROLYSIS OF WATER.

By JAMES P. JOULE, Esq.

(Read January 24th, 1843.)

In former papers I have endeavoured to prove that the heat evolved by Voltaic Electricity is proportional to the resistance to conduction and the square of the quantity of current,—during electrolysis, and in the cells of the battery, as well as in metallic conductors. The heat, however, which is liberated in cases of electrolysis, was very uniformly found to exceed the product of resistance and the square of the current; and I attributed this to the solution of oxide of zinc and to other actions which are regarded as secondary. The circumstance is evidently of great importance to the whole subject of Voltaic Heat. I have therefore undertaken a series of experiments to ascertain its real character, the results of which are, I hope, of sufficient importance to merit the attention of this society.

It would be quite useless to attempt an inquiry of this kind, without the assistance of a *Galvanometer* of very considerable accuracy. Sensible of this, I have from time to time, endeavoured to improve my own measurers of Voltaism. The instrument used in the present investigation is of large dimensions and great exactness. It is constructed much on the same plan as Pouillet's galvanometer of tangents; the electricity being carried by a thick copper wire, bent into a circle of a foot diameter. The magnetic needle, six inches in length, traverses over a circle of pure brass divided by a machine. Its deflections, appreciable to 5′, are turned into quantities of electric current with the help of a table formed by exact and careful experiments.

Another piece of essential apparatus is a *standard of resistance to conduction*. Mine consists of eight yards of copper wire $\frac{1}{20}$ of an inch in diameter. It is well insulated, bent double to obviate any action on the galvanometer, and arranged into a coil. When used, it is immersed in water, in order, by keeping it cool, to prevent the increase of its resistance.

The *Battery* I used is on Prof. Daniell's plan. Each of its cells is twenty-five inches high, and five and a half inches in diameter. They were charged in the usual way, with acidulated sulphate of copper in contact with the copper; and dilute sulphuric acid, mingled with a variable* quantity of sulphate of zinc, in contact with the amalgamated zinc. *Six* of these cells in series formed the battery used in all the subsequent experiments: but at the same time I may observe that trials were also made with only *three* in series, in order to satisfy myself that the results are independent of the extent of the battery.

The *Electrolytic Apparatus* consisted of a glass jar, containing a pound of liquid, and furnished with electrodes each of which exposed an active surface of about six square inches.

* As I do not throw away my old solution, but content myself with mixing it occasionally with new, the liquid must have contained considerably more free acid at one time than at another. This cannot, however, interfere with my results, because it did not interfere with the *intensity* of the battery.

By ascertaining immediately before and after each experiment, the current which could pass when the battery and connecting wires (including that of the galvanometer) were only in the circuit, and that which could pass when an unit was added to this resistance by means of the standard of resistance; we have, on the principles of Ohm, the equation $ax=b\,(x+1)$, or $x=\frac{b}{a-b}$, where a and b are the above currents, and x is the resistance of the battery and connecting wires.

This being known, we have the means of ascertaining the resistance to conduction of the electrolytic cell. It is obtained from the equation $c\,(x+y)=ax\frac{d-z}{d}$, or $y=(a\left(\frac{d-z}{d}\right)-c)\frac{x}{c}$, where (the other letters remaining as before) c is the current observed during electrolysis, d is the number of cells in the battery, and z is the *resistance to electrolysis* of the electrolytic apparatus, calling (as I intend to do throughout this paper) the intensity of Daniell's cell, unity.

This resistance to electrolysis is obtained by observing the currents which pass through the electrolytic cell when different numbers of the battery cells are employed in propelling them. For this purpose the lowest two numbers adequate to effect electrolysis were chosen; and having reduced the current observed when using the fewest number of cells, on account of the absence in that case of the resistance to conduction of one cell of the battery; the resistance to electrolysis is found by the equation $z = \text{E} - \left(\frac{e}{f-e}\right)$, where E is the first number of cells which can effect decomposition, e is the corrected current observed with it, and f is the current passed when one cell more is added to the battery.

The appended Table contains the particulars of some experiments on Electrolysis in which various electrodes and solutions were used. Each of them is a *double* experiment, or, in other words, is the mean of two experiments, in one of which the needle was deflected towards the right, and in the other towards the left hand of the magentic meridian, so as to neutralize any slight inaccuracy in the position of the galvanometer. The first three numbers of the table, giving the heating power of wire, were obtained, not in immediate succession, but at considerable intervals of time, during which other experiments were tried. By this means I assured myself that the important condition of uniform conductibility of the standard coil had not suffered by its repeated use.

The heat evolved by wire, having been ascertained by these three experiments, furnished the means of calculating the subsequent numbers of column 8, according to the well-established law of resistance to conduction and square of current.

The *time* during which the electrolysis was allowed to proceed, varied from $7\frac{1}{2}'$ to $10'$. The results of the table are however, for the sake of uniformity, invariably reduced to ten minutes of time. The quantities of heat are reduced to the capacity of a pound avoirdupois of water, by making the requisite corrections (deduced from careful experiments) for sp. heat, and the influence of the atmosphere. The *current* is, as in my former papers, expressed in degrees. (1°, passing uniformly during an hour, decomposes a chemical equivalent expressed in grains: as 9 grs. of water, 41 grs. of oxide of zinc, &c.)

By inspecting the Table, it will be observed that the heat actually evolved is in every case of electrolysis greater than that which is due to the product of the resistance to conduction and the square of the current. In a former paper* I ascribed this to the solution of oxygen at the

* Phil. Mag. 1841, Vol. XIX, p. 274, (63.)

positive electrode. Faraday has shown that this occurs sometimes to a considerable extent. In the present experiments I ascertained that the gases were evolved exactly in the right proportions, and this because the small quantity of oxygen dissolved by the solution at the positive electrode was immediately carried by currents to the negative, and there neutralized by hydrogen, as the liquid was not divided by a diaphragm. In this way however, not more than one sixteenth of the mixed gases was re-formed into water. Nor can this account in any degree for the *difference* between the results of columns 7 and 8. For in proportion to the quantity of gas re-dissolved, is the intensity required for electrolysis undoubtedly diminished. And this, entering the equations, has the effect of increasing the calculated resistances to conduction, and, in the same proportion, the results of column 8. We must seek another cause.

And in order to do so with convenience, I have enlarged the table. Column 9 contains the difference of columns 7 and 8, reduced to an equivalent of electrolysis. In other words, the differences of the numbers of columns 7 and 8 would have been equal to those in column 9, had the several experiments been continued just long enough to effect the evolution of a grain of hydrogen. Now I had ascertained, by the very careful experiments given in the first three numbers of the table, that the intensity of a Daniell's cell, such as I used, is equivalent to 6°·129 of heat, per degree of current; and hence I have obtained by simple proportion the numbers of column 10, representing, in terms of the intensity of Daniell's cell, the resistances to electrolysis due to the quantities of heat in column 9. By subtracting these from the numbers of column 5, we obtain those of column 11, which, as we shall presently see, represent the intensity requisite to separate water into its elements and to evolve the constituent gases. Columns 12 and 13, containing new resistances to conduction,* and the heat due to them, are calculated from column 11.

It will be observed that the resistances to electrolysis (contained in column 5,) differ according to the nature of the electrodes. With platinized surfaces the resistance is considerably

* I call these, "resistances to conduction," merely from the want of a more appropriate term to distinguish what is really a mixture of two distinct sources of resistance. Column 6 gives the true resistance to conduction.

1	2		3	4	5	6	7	8	9	10	11	12	13
No.	Electrodes. P.	N.	Liquid.	Current.	Resistance to Electrolysis.	Resistance to Conduction.	Heat Evolved.	Heat due to Resistance to Conduction and Square of Current.	Dif. Cols. 7 & 8 per Equivalent of Electrolysis.	Resistance to Electrolysis Due to Col. 9.	Column 5, Minus Column 10.	New Resistance Conduction.	New Theoretic Results.
1	Coil of Copper Wire.		Water.	7°136	0	0·8876	11°78	11°71					
2	do.		do.	6°770	0	0·8818	10°42	10°47					
3	do.		do.	6°887	0	0·8810	10°80	10°82					
4	Plat.m	Am.d Zinc.	Dilute Sulpc. Acid	3°479	2·809	1·025	11°37	6°426	8°523	1·391	1·418	1·799	11°281
5	Plat.m	Plat.m	do.	3°658	2·475	1·049	11°427	7°271	6°817	1·112	1·363	1·635	11°333
6	P.d Silv.r	P.d Silv.r	do.	6°287	1·748	0·5721	13°929	11°715	2°112	0·345	1·403	0·6773	13°871
7	P.d Plat.m	P.d Plat.m	do.	4°707	1·760	0·922	12°871	10°581	2°919	0·476	1·284	1·120	12°853
8	do.	do.	do.	4°918	1·900	0·7655	12°628	9°591	3°706	0·605	1·295	1·004	12°584
9	do.	do.	Soln. Potash Sp. gr. 1·063	3°111	1·900	1·514	9°370	7°590	3°433	0·560	1·340	1·868	9°366

less than with polished platinum: and the latter, again, is exceeded in this respect when the electrode evolving hydrogen is of amalgamated zinc. Now in all the experiments with dilute acid, the *chemical* effects were exactly the same, consisting of the separation of the gaseous elements of water, and the transfer of quarter* of an equivalent of sulphuric acid from the negative to the positive electrode: and in the experiment with solution of potassa, the same gases were evolved, accompanied, according to Prof. Daniell, by the transfer of the fifth part of an equivalent of alkali from the positive to the negative electrode, which could not fail to require nearly the same intensity as the transfer of acid in the contrary direction. It is quite evident then, that the resistances of column 5 are not *entirely* due to chemical *changes*; though I have no doubt whatever that they arise from the joint action of chemical change and chemical *repulsion*; a repulsion which is principally owing to the presence of electro-positive materials on the negative electrode. If the resistance to electrolysis which is over and above

* Daniell, Phil. Trans. 1840, Part 1, p. 214. I have myself made some rough experiments confirmatory of this remarkable fact.

that due to chemical changes, be unaccounted for elsewhere, it will prove the *annihilation* of part of the power of the circuit, without any corresponding effect. We shall see that this is not the case, but that in the evolution of *heat* where the excess of resistance takes place, an exact equivalent is restored.

For, by inspecting column 9, we see that the excess of heat is greatest when the resistance to electrolysis is greatest; and least also when the latter is least. Now, when this excess of heat is, in the manner I have before described, turned into the resistance to electrolysis of which it is an equivalent, and then subtracted from the compound resistance of column 5, we obtain the numbers of column 11, which represent the resistance due to the separation of water into its gaseous elements alone, and apart not only from the resistance owing to the state of the electrodes, but also from that which is due to the transfer of the fraction of an equivalent of acid or alkali;—for since these are re-mingled with the liquid as fast as they are determined to their respective poles, and the heat evolved by this re-mingling appears in column 7, the equivalents of resistance due to the transfers appear in column 10, and are subtracted from the compound resistances of column 5.

According to these principles, all the numbers in column 11 should be equal; and indeed they do not differ from each other more than might have been anticipated from such complicated experiments. An error of only the fourth part of a degree in column 7 is sufficient to produce the greatest deviation from the mean in column 11: and the results are likewise dependant upon the accuracy with which the resistance of the battery is ascertained.

1·35, the mean of column 11, will very nearly represent the intensity required for the separation of the elements of water, and the assumption by them of the gaseous state. By these means heat becomes *latent*, and a reaction on the intensity of the battery takes place, without the evolution of free heat. It is most interesting to inquire what part of the whole intensity is due to each action. I have endeavoured to ascertain it in the following manner.

I removed the thick copper wire of the galvanometer, and substituted for it a coil of a foot diameter, consisting of 200 turns of well covered copper wire of $\frac{1}{40}$ in. diameter. As the resistance of a voltaic pair, consisting of three inch plates, is seldom more than equal to that of a yard of such wire, the galvanometer could now be depended upon as an indicator of the intensity of different arrangements, particularly by taking care to make their resistances as nearly equal to one another as possible. In this way, calling the intensity of Daniell's cell unity, I found the intensity of Mr. Grove's arrangement, consisting of platinum in nitric acid and amalgamated zinc in dilute sulphuric acid, to be 1·732; and that of Mr. Smee's arrangement of platinized platinum and amalgamated zinc in dilute sulphuric acid (avoiding the effects of immersion*) to be 0·731: also the intensities of similar arrangements, with *iron* as the positive metal, to be respectively 1·14 and 0·149.

Now on account of the extreme facility with which oxygen parts from nitric acid, there can be no doubt that the intensity of the above arrange-

* Phil. Mag. 1842, Vol. 20, p. 105. Becquerel was, I believe, the *first* who referred the great intensity of a pair at the moment of its immersion to the reaction of the air adhering to the negative plate.

ments of Mr. Grove very nearly represent the affinity of the positive metals for oxygen which is not in the gaseous state. For the separation of the hydrogen from the oxygen of the water is simultaneous with the union of hydrogen with the oxygen which may almost be regarded as free at the platinum. And these actions neutralizing each other, it follows, that the intensity of the current very nearly represents the affinity of the positive metals for oxygen.

But with the pairs on Mr Smee's arrangement, there are two things opposed to this affinity. One of them is the separation of the elements of water from each other, whilst the other is the giving hydrogen the gaseous form. Hence we have 1·732−0·731=1·001, and 1·14−0·149=0·991. We shall take 1, (which is very nearly the mean of these,) for the intensity necessary to take hydrogen from oxygen, and to give it the gaseous form.

Again, I placed platinum wires in a solution of sulphate of oxide of zinc, and, connecting them with the long wired galvanometer, I found the currents which passed when 3 and 4 pairs of Daniell were used, to be in the ratio of 160 to

615. Hence 2·648 is the resistance to electrolysis of this solution. Of this, part is due to the separation of zinc from oxygen, and part, to the transfer of sulphuric acid from the oxide of zinc to the water. We must eliminate the latter,—and I know not a better way of doing so, than that of converting 2°·82 (the heat evolved when oxide of zinc is dissolved in dilute sulphuric acid,) into 0·46, (its equivalent of resistance to electrolysis,) and subtracting this from 2·648. Thus we obtain 2·188*, the intensity occupied in separating zinc from oxygen, and giving the latter the gaseous state.

But we have seen that the intensity of the union of non-gaseous oxygen with zinc is represented by 1·732. Therefore 2·188—1·732=0·456, the intensity due to the assumption by oxygen of the gaseous state.

Again, the resistance to electrolysis of solution of sulphate of oxide of copper is 1·702: and

* This number is rather over the truth, (p. 102,) but as I have found by a recent experiment, in which oxywater was substituted for the nitric acid of Grove, that the intensity of Grove's arrangement (from what cause I cannot conceive) is probably greater than is represented by the union of non-gaseous oxygen with zinc, there is probably a compensating error.

though it is difficult to ascertain with precision the quantity of heat evolved by the solution of oxide of copper in dilute sulphuric acid, I am persuaded that it is not less than 3° nor more than 4° per equivalent. Suppose it to be 3°·5 = 0·571 of resistance to electrolysis. Then 1·702 − 0·571 = 1·131,* the intensity due to the separation of oxide of copper into metal and gas. But 0·731 represents the intensity of the union of non-gaseous oxygen with copper: therefore 1·131 − 0·731 = 0·4, a result not widely different from that obtained with zinc. We may, I think, take 0·45 as a near approximation to the intensity due to the gaseous state of oxygen.

As we have already stated that 1 is the intensity necessary to separate oxygen from hydrogen, and to give the latter the gaseous state, 1·45 is, according to the above calculations, the intensity required to electrolyze water. This is not widely different from 1·35, the mean of column 11· − 0·45 of resistance to electrolysis is equal to 2°·76. It would be curious to ascertain whether the same amount of caloric could be evolved by the mechanical condensation of eight grains of oxygen gas.

* See note page 100.

We have given 1·35, 1·131, and 2·188 as the intensities due to the separation of gaseous oxygen from hydrogen, copper, and zinc: and the quantities of heat corresponding to these are respectively 8°·27, 6°·93, and 13°·41, which are therefore the quantities of heat per ℔. of water, which, according to the above data, should be evolved by the combustion of 1 gr. of hydrogen, 31·6 grs. of copper, and 33 grs. of zinc. To the first of these, however, about one sixteenth (p. 93) ought to be added, on account of the dissolution and re-combination of the gases in the electrolytic cell, which has the effect of diminishing the numbers of column 11. On the other hand, the results for copper and zinc are certainly somewhat over-stated, on account of that species of resistance to electrolysis which forms column 10. The error thus arising is not indeed great, and in the case of copper, is, as I have ascertained, no more than 0·13; but nevertheless it ought to be eliminated, as decidedly as we eliminated, in the case of water, the resistances of column 10. After making these corrections, the above quantities of heat are altered to 8°·79, 6°·14, and 12°·62, which approximate pretty closely to 8°·98, 5°·18, and 10°·98, the quantities of heat which Dulong obtained by experiments, the general accuracy of which I have myself ascertained.

Before I conclude, I wish to make a few general observations in connexion with the subject of this paper.

1st. In an electrolytic cell, there are three distinct obstacles to the voltaic current. The first is *resistance to conduction* :—the 2nd is *resistance to electrolysis without chemical change*, arising simply from the presence of chemical repulsion ;—and the 3rd is *resistance to electrolysis accompanied by chemical changes.*

2nd. By the 1st. of these (the resistance to conduction) heat is evolved exactly as it is by a wire, according to the resistance and the square of the current ; and it is thus that a part of the heat belonging to the chemical actions of the battery is evolved. By the 2nd., a reaction on the *intensity* of the battery occurs, and wherever it exists, heat is evolved exactly equivalent to the loss of heating power in the battery arising from its diminished intensity. But the 3rd. resistance differs from the 2nd., inasmuch as the heat due to its reaction is rendered latent and is thus lost by the circuit.

3rd. Hence it is that however we arrange the voltaic apparatus, and whatever cells of electrolysis we include in the circuit, the whole caloric of the circuit is exactly accounted for by the whole of the chemical changes.

4th. As was discovered by Faraday, the *quantity* of current electricity depends upon the number of atoms which suffer electrolysis in each cell: and the *intensity* depends upon the sum of chemical affinities. Now both the mechanical and heating powers of a current are, (per equivalent of electrolysis in any one of the battery cells,) proportional to its intensity. Therefore the mechanical and heating powers of a current are proportional to each other.

5th. The magnetic electrical machine enables us to convert mechanical power into heat, by means of the electric currents which are induced by it. And I have little doubt that by interposing an electro-magnetic engine in the circuit of a battery, a diminution of the heat evolved per equivalent of chemical change would be the consequence, and this in proportion to the mechanical power obtained.*

6th. Electricity may be regarded as a grand agent for carrying, arranging, and converting chemical heat. Suppose two of Daniell's cells in series to be connected, by thick wires, with platinized plates immersed in dilute sulphuric acid. Owing to the near balance of affinities, very little free heat will be evolved, per equivalent of chemical action, in any part of the circuit; and that little will be equivalent to the difference of the intensity of the battery and the intensity due to the electrolysis of water, or to $2-1{\cdot}35=0{\cdot}65$, if we do not regard the heat arising from secondary action in the battery. But then a great transfer of latent heat, equal to $8^{\circ}{\cdot}27$ per equivalent, will take place from the battery to the electrolytic cell; and this by the immediate agency of the current. Again, if a large battery be connected by thick conducting wires with a coil of very thin wire, nearly the whole of the heat due to the chemical changes taking place in the battery will be evolved by that coil, while the battery itself remains cool.

* I am preparing for experiments to test the accuracy of this proposition. Feb. 18th. J. P. J.

7th. Pouillet having deduced from his experiments (repeated with great caution by Becquerel) the general conclusion that, during combustion, oxygen disengages positive, and the combustible, negative electricity ; and Faraday having proved that a constant quantity of electricity is associated with the combining proportions of all bodies ; it only remained to prove that the *intensity* of the electricity passing from the oxygen to the combustible at the moment of their union is just that which is equivalent to the actual heat of combination. This I have attempted, and in so doing have met, I think it will be admitted, with such success, as to put the beautiful electrical theory of chemical heat, first suggested by Davy and Berzelius, beyond all question. I have shown also that the *modus operandi* is resistance to conduction.

APPENDIX.

Since the above paper was read, I have found that the intensity of Mr. Grove's voltaic arrangement may be increased in the ratio 100 : 118, by the use of a mixture of peroxide of lead and sulphuric acid instead of nitric acid. Hence it appears that the intensity of the nitric acid battery is considerably less than the intensity due to the union of the positive metal with oxygen. The calculations at p. 99—101 are therefore built upon erroneous data, and must be laid aside.

I have once or twice made use of the terms "latent heat," "caloric," &c., but I wish it to be understood that those words were only employed because they conveniently expressed the facts brought forward. I was then as strongly attached to the theory which regards heat as motion among the particles of matter as I am now. In this spirit Prop. v., p. 104, was written, the correctness of which I have since established experimentally.

There are many phenomena which cannot be accounted for by the theory which recognises heat as a substance; and there are several, which, though sometimes adduced as triumphant objections to the other theory, tend, when rightly considered, only to confirm it. The heat of fluidity may very naturally be regarded as the momentum or mechanical force necessary to overcome the aggregation of particles in the solid state. The heat of vaporization may be regarded, partly as the mechanical force requisite to overcome the aggregated condition of atoms in the fluid state, and partly as the force requisite to overcome atmospheric pressure. Again, the heat of combination is only the manifestation, in another form, of the mechanical force with which atoms combine: on the other hand, the phenomena of electrolysis by the voltaic battery give us positive proof that the mechanical force of the current requisite to procure the decomposition of an electrolyte is the equivalent of the heat due to the recombination of the elements. Thus it appears that electricity is a grand agent for converting heat and the ordinary forms of mechanical power into one another.

I have lately been at considerable pains in framing a theory of heat more natural and more consistent with facts than the undulatory hypothesis. Setting out with the discovery of Faraday, that every atom is associated with the same absolute quantity of electricity, I assume that these atmospheres of electricity revolve with enormous rapidity round their respective atoms; that the momentum of the atmospheres constistutes "caloric," while the velocity of their exterior circumferences determines what we call temperature. The theory applies very beautifully to the phenomena of conduction, and satisfies rigorously the law of Boyle and Mariotte with respect to elastic fluids. When applied to the doctrine of sp. heat, it demands the extension of the law of Dulong and Petit to all bodies, whether compounds or not, and points out the following general law, applicable to all bodies except perhaps compound gases, viz., *the specific heat of any body is proportional to the number of atoms in combination divided by the atomic weight*, a law which agrees very well with the results of experiment when some atomic weights, as for instance those of hydrogen, nitrogen, and chlorine are halved,—while others, as that of carbon, are doubled. According to this theory, the zero of temperature is only 480° below the freezing point, indicating that the momentum of the revolving atmospheres of electricity in a ℔. of water at the freezing point, is equal to a mechanical force capable of raising a weight of about 400,000 ℔s. to the height of one foot.

February 20, 1844. J. P. J.

A

BIOGRAPHICAL NOTICE

OF THE LATE

PETER EWART, ESQ.

BY

WILLIAM CHARLES HENRY, M.D., F.R.S., & G.S.

It has been the custom of this, as well as of most other learned bodies, to commemorate the services of those, among its deceased members, who have filled its offices of honour, and who have contributed to its reputation and to the advancement of general science. Our volumes are enriched with many such notices of our founders and earliest associates, which thus furnish a continuous record of the labours and achievements of the society itself. These tributes of affectionate reverence for the memory of the dead seem especially becoming and expedient, when lessons of practical wisdom may be gathered from their example for the guidance of the living. It is, on this ground, that it has been judged desirable to present to you some narrative, however imperfect, of the virtuous and honourable career of the venerable friend, whose loss we have so lately had to deplore.

The late Mr. Peter Ewart was born at Troquaire Manse, Dumfries, May 14th, 1767. His father was a Clergyman of the Church of Scotland, and brought up a large family, whose high character and remarkable success in after-life may fairly be received as testimony of the excellence of their early training. Of his six sons, who may all be strictly said to have been the artificers of their own fortunes, the eldest rose to the high station of British Minister at the Court of Berlin; the second, Mr. William Ewart, became one of the most considerable merchants in Liverpool, the friend and political supporter of Mr. Canning; and a third son, Dr. John Ewart, was an eminent physician, at Bath. The youngest son, Mr. Peter Ewart, received his early education at the Free School at Dumfries. He manifested, when very young, a strong predilection for mechanical pursuits. In some brief notes, that have been fortunately preserved, he has recorded, that when nine years old, he was in the habit of passing his leisure hours in the shops of a watchmaker and millwright; and that before he had reached the age of twelve, he had constructed a clock with wooden wheels, which he placed in his bed-room to awaken him in the morning. But it was some time before he succeeded in making its motions regular, and he often lay awake, during the night, thinking of his clock and rising occasionally in the dark to feel if it were still going.

It was probably this strong original impulse that determined his father in the choice of his son's future profession. In 1782, at the age of fifteen, Mr. Ewart was removed from school and placed with Mr. Rennie, of Musselburgh, afterwards so celebrated as a civil engineer, whom he accompanied to London, two years afterwards. At the expiration of his apprenticeship, he constructed, in conjunction with another associate, wooden models for a set of wheels, which Mr. Rennie had to furnish for a distillery in Whitechapel. They agreed to make the models for twelve guineas, and, by working from 4 A.M. to 8 or 9 P.M., they accomplished their task in a fortnight. Mr. Ewart has remarked, that he received his portion of the price, with more lively satisfaction, than any amount of money had subsequently conferred. Indeed, at this period of his life, he appears to have been entirely dependent upon his own strenuous exertions, and to have passed through the stern discipline of hard fare, indifferent lodging, and coarse apparel, for which his early nurture and rank in society could have but ill prepared him. At this very time, his elder brother was Envoy at the Prussian court. In his brief Journal, Mr. Ewart has touchingly observed, "I sometimes lost heart, when I considered the difference of our situations, and the low ebb of my own prospects." But Mr. Ewart was not of a temper of mind to be easily discouraged. He struggled manfully with the difficulties of his position, and his industry, talents, and tried moral worth had already procured him many warm friends in London.

Mr. Ewart went to Soho, in 1788, in the employment of Mr. Rennie, to erect a water wheel and other machinery for Mr. Boulton's mill for rolling copper, silver, and plated metal. He was afterwards employed by Mr. Boulton in the construction of the millwork and machinery of his new mint, where great improvements were introduced in the processes of coining, and were soon after applied to the execution, at Soho, of coinages for the United Kingdom. The perfection of their workmanship, and the just principles adopted, put an end to those counterfeits, which the laws, by frequent executions, had been found insufficient to prevent. Mr. Ewart's services were then transferred to the firm of Boulton and Watt, in their steam-engine business, and he was employed by them in the erection of their engines at Manchester, Leeds, and other places, in 1790 and following years. The sons of Mr. Boulton and of Mr. Watt having been admitted into partnership with their fathers at the end of 1794, they resolved upon a great addition to the manufacturing powers of their establishment, and availed themselves of the assistance and skill of Mr. Ewart in the planning and erecting of new works at Soho foundry, in 1795 and 1796.*

He thus enjoyed the singular advantage of being formed in the best school of practical mechanics and engineering, which this country then possessed. His residence at Soho must have

* For this record, I am indebted to the kindness of Mr. Watt, of Aston Hall.

proved of even greater value to him, at this period of his life, as respects the guidance of his tastes and the developement of his mind, by bringing him into close and friendly relations with Mr. Boulton and Mr. Watt. Communion with minds so surpassingly fraught with varied knowledge, and gifted with such transcendent powers of creative combination, could not fail to enlarge and strengthen his mental vision, and may perhaps have first kindled those lively perceptions of pleasure in the pursuits of abstract science, and those aspirations for a wider and loftier intellectual culture, which Mr. Ewart manifested throughout his entire after-life. Of Mr. Watt he has remarked, "he had a peculiar aversion to pretension of any kind. I never saw him put out with anything else, but he could not bear to see anybody pretending to know more than he really did." For a mind thus constituted, it may readily be imagined, that the fresh and guileless simplicity of heart, the singularly modest or rather humble estimate of his own powers and acquirements, the purity and integrity of feeling and character, which Mr. Ewart preserved unspotted, throughout a long life and a large experience of mankind, must have had an irresistible charm. Of these qualities of heart and character, the following letter addressed to Mr. Watt, furnishes a touching manifestation.

"Apedale, 8th Oct., 1790.

"Dear Sir,—The greatness of the obligations I lie under to you, on account of the very kind proposals from you and Mr. Boulton, I hope will excuse this manner I take of acknowledging them, especially as they came upon me so unexpectedly. I am very sensible, your proposed plan will place me in the most desirable situation in this country for pursuing my business, and under your patronage, influence, and advice, I shall enjoy advantages far superior to anything I ever before had the most distant hopes of, for which I shall never be able to make any suitable return, yet permit me to assure you, that my feeble efforts shall always be exerted to the utmost, when in your service, or where they can in the smallest degree promote your interest.

"Since you have so far testified your kind disposition towards me, I will venture to beg one other favour of you: it is, that whenever you may find me deviating in the least from what is strictly *right*, I may have your reproof, which I hope will always have such an effect upon me as the censure of an invaluable benefactor ought to have. I am very sensible of the risk you run in trusting me at first with an engine by myself, but this risk is an additional spur to my assiduity and vigilance, and whatever be amiss shall be owing to want of ability, not of inclination in me to do right.

"I remain,
"With the greatest respect and gratitude,
"Dear Sir,
"Your most obliged, obedient, & humble servant,
"PETER EWART."

In this early familiar intercourse, and in the deep feelings of mutual regard, to which it naturally gave birth, was laid the foundation of the steady friendship which subsisted between them during the whole of Mr. Watt's subsequent life. It was during his connexion with Messrs. Boulton and Watt, in 1791, that Mr. Ewart visited Leeds, and formed a lasting friendship with another of those high-minded men, who are, one by one, passing away from among us,—the late Mr. Gott.

In the year 1792, Mr. Ewart was induced by the liberal proposals of Mr. Oldknow to relinquish his profession of Civil Engineer, and to enter into partnership with that gentleman, in the Cotton Manufacture. At this period, he became known to the late Dr. Currie, of Liverpool, the illustrious biographer of Burns; and in the following interesting letter to Mr. Wilberforce from Dr. Currie, is placed on record the deliberate judgment, which that sagacious and discriminating observer had formed of Mr. Ewart's talents and character.

"Dr. Currie to W. Wilberforce, Esq.

"Liverpool, 23rd April, 1793.

"Dear Sir,—If in the long letter, which I wrote you two days ago, there appears a good deal of unguarded warmth, the following circumstance will explain, though perhaps not justify it.

"I was sitting in my study, on the evening of Saturday, reflecting on public affairs, when a young man called to drink coffee with me, a manufacturer of Stockport, near Manchester. After giving a picture of the general distress there, he informed me of his own situation in particular, and of the business which brought him to Liverpool.

"He said that the house, of which he is a partner, employed about 1,500 hands, all of whom are now idle, or, as the phrase is, off work. That previous to their being discharged, he and his partner had struggled on from one week to another, in hopes that the times would mend, and a demand, more or less, come for their goods. That, in this hope, they had gone on for the last three weeks, and not having a sufficient quantity of money to pay the people their full weekly wages, they had prevailed on them to accept about a third of the sum, as this, with economy, might suffice for subsistence. In procuring the money for this purpose, he told me, they had been reduced to extraordinary difficulties. Formerly they sold their goods in large quantity, but now they determined to supply the retailers themselves with a single piece, or even less; and, provided they paid them in specie, at almost any price. Accordingly, having goods in their warehouses that suited the home market, they fitted up a light cart and sent a young man with it, full of goods, to supply the retailers in every part of the country, and to bring home the specie every Saturday, whatever might be the loss. The expedient succeeded for about three weeks, but had now failed, and he was come to Liverpool to try if by any possible means he could raise a few hundred guineas, to get over another week and keep his people alive. He told me that he and his partner had been constantly amongst them, and by entering into all their distresses, had prevailed on them to be extremely patient and reasonable. At their last meeting, they had agreed to wait this young gentleman's return from Liverpool, and what money he was able to raise, they had consented should be laid out in oatmeal, which being boiled up with water, potatoes, and some of the coarser pieces of beef, should be shared out in fair proportions among them; and thus in the cheapest manner provide for their subsistence. As the house had many thousands owing them in Liverpool, though he knew there was no hope of any considerable debts being paid, he had no fear of not being able to procure the sum immediately wanted. He had been using every effort for two days, and had actually threatened to arrest two of our principal merchants on the exchange, but he had not been able to raise a single guinea. How he was to face the poor people he knew not, each of whom had four to six weeks' wages due. But he could appeal to Heaven for the anxious exertions, which he had made, to relieve distresses which he could neither foresee nor prevent. As I looked at this young man, I perceived that his countenance seemed actually withered with care and sorrow. He is not a common character ; he was the apprentice of Messrs. Boulton and Watt, and has an extraordinary degree of the most useful knowledge of every kind. He is modest, virtuous, and prudent ; of astonishing application, and, in a word, one of the first young men I ever knew. These qualities recommended him to the notice of the manufacturers, among whom he exercised his profession of a mechanic and engineer. He had offers of partnership from the first houses there, and was actually taken into the house of Mr. Oldknow, of Stockport, about a year ago, at that time perhaps the first establishment in Lancashire. Mr. Oldknow you must have heard of, as the original fabricator of muslin in this country, and a man of first-rate character. He has laid out a property of £50,000 on building and machinery alone. His partner, (the young gentleman I speak of,) is named Ewart, the younger brother of Mr. Ewart, the late Envoy at Berlin. It is men such as these that are reduced to such extremities."

In consequence probably of this fearful depression of commerce, surpassing evidently in extent and severity any subsequent crisis, Mr. Ewart's connexion with Mr. Oldknow was dissolved after the lapse of a year. In 1798, he commenced the business of Cotton Spinning in Manchester, first as a partner of the late Mr. Greg, and he afterwards continued that employment alone till 1835. He then ceased to reside in Manchester, having been appointed, (mainly through the powerful recommendation of Mr. James Watt,) Chief Engineer and Inspector of Machinery in His Majesty's Dockyards. He removed to his official residence in the Royal Dockyard, Woolwich, where he died September 15th, 1842, in consequence of a severe injury, inflicted by the sudden breaking of a chain, while he was superintending the removal of a large boiler.

Mr. Ewart became a member of this Society January, 1798, and was elected to the honorable station of Vice-President in April, 1812, an office he continued to fill to the period of his leaving Manchester.

From the foregoing brief outline of the principal events of Mr. Ewart's life, it will be manifest, that during the largest portion of his career,—his residence of nearly forty years in Manchester,—his time was necessarily in chief measure devoted to strictly commercial pursuits. But his mind was far too active and enterprising to find sufficient occupation in the daily repetition of the same processes; and his early training and practical skill as a mechanician stimulated him to aim at the improvement of these processes by the contrivance of new and more perfect machinery. The first fruits of this revival of his original pursuits was the invention of a loom worked by atmospheric pressure. But this ingenious contrivance was not found susceptible of practical application on a large scale. He continued also to devote much attention to Civil Engineering; and so high was the estimation in which he was held by the most distinguished Civil Engineers, both for skill and integrity, that in 1831 he was unanimously appointed sole referee to determine the amount of compensation to be made by the Eau Brink Commissioners to the Ouze Bank Proprietors. His award in this very complicated question gave great satisfaction to all the parties interested.

Mr. Ewart in 1822 took out a Patent for an Iron Coffer Dam, the value of which is thus strongly attested by Mr. Hartley, the present engineer to the Liverpool docks.

"Liverpool, 13th March, 1843.

"Dear Sir,—By a letter which I have received from my friend Mr. Peter Ewart, it appears you wish me to communicate to you the nature of the Iron Coffer Dam, invented by my late greatly respected friend his father.

"Above I send you a sketch of them in the manner in which they are supposed to be driven.

"The original intention was that they should supersede the usual immense substance of a timber and puddle Coffer Dam, the method being to cast the piles of suitable length and breadth, with dovetailed flanges on each side of them; these being connected by cramp piles having counterpart dovetailed flanges, the whole were driven down to the required depth, and the small triangular interstice, formed betwixt the pile and the cramp filled up with grout made of mortar or clay, renders the whole perfectly water-tight.

"I have used them more or less, and with much satisfaction, since the time he invented them, which is somewhere about 21 or 22 years ago.

"It was he also, who about that time first informed me of the fact of tough iron being rendered short and brittle by the fine hammering of it upon the anvil in a black heat.

"He had a piece of ironwork to make, which required to be neatly done, and to be very strong, for which purpose he carefully selected a *bar*, the best he could select, and put it into the hands of his smith. When the work was finished, he happened to throw it down, or let it fall, when it snapped in two quite short. Being surprised at this, he made a further selection of a bar of iron, and had a similar article made, which was attended by the same unfavourable result. He then began to consider that the nature of the iron must have been changed by the fine hammering of it in a black heat. He therefore had a further trial, by having the same kind of article made from the same bar as the last was made from, taking care that the hammer did not touch it, except in a good *red heat*, by which means the article thus made was found fully to answer his wishes.

"This information has been of very great value to me, as well as to many others to whom I have communicated it.

"This fact has since been also found out by others; and I see a paper has been read on the subject before the society of civil engineers, though known to me through Mr. Ewart upwards of twenty years ago.

"I regret I cannot call to mind at present various other valuable information which I have from time to time received from him."

"I am, Dear Sir,
"Very respectfully,
"Your most obdt.
"JESSE HARTLEY."

"Dr. Henry, Haffield, Ledbury."

In the physical sciences Mr. Ewart's knowledge was at once exact and comprehensive. He had naturally selected as the prominent object of study, from its close affinity with his professional pursuits, Theoretical Mechanics. Without having aspired to the mastery of the higher Calculus, either in its original English form, or in the more refined developement and vastly augmented powers it has attained in France, he was sufficiently conversant with the mathematics to read with facility most works, which were then the standard authorities in mechanics. Of the wide compass of his reading, and of the fullness and precision of his knowledge, his elaborate essay on the "Measure of Moving Force," published in the second volume of our Memoirs, affords the strongest testimony. It is scarcely necessary to affirm of one, who had been trained in the school of Soho, that he was profoundly learned in all that has reference to steam, both as respects the higher philosophy of the constitution and laws of elastic fluids, and their practical application as sources of power. Mr. Ewart was the first to furnish an explanation of the singular phenomenon that the temperature of high pressure steam, escaping from a small aperture falls considerably below 212°; from 290° Fahrenheit to 160.° He suggested (Ann. of Phil., April, 1829) that the particles of elastic fluids have a tendency, when they are forced near to each other, to fly asunder, not only to their original distance, but beyond it. Thus high pressure steam, on suddenly removing all pressure, except that of the atmosphere, is converted into low pressure steam, and its temperature falls in conformity with the general law of the absorption of heat attendant on increase of volume.

From these general conceptions of the nature of steam, or more comprehensively, of matter as subsisting in the state of elastic fluidity, an earnest and enquiring mind could scarcely fail to press onwards to the highest doctrines of the Atomic Philosophy. Mr. Ewart had the merit of recognizing the truth and beauty of this grand generalization, long anterior to its full and universal acceptance ; he had never, it is true, rendered himself familiar with the minute technical details of chemistry,—with the aspect, properties, and habitudes of individual bodies. But the mechanical sciences constituted the best preparatory discipline for the study of the "New System of Chemical Philosophy ;" and in a close friendship of nearly half a century with its illustrious author, permitting the fullest and most unreserved discussion and interchange of thought, he had enjoyed access to the fountain head of these higher doctrines. Mr. Ewart read before this society in September 1812, and afterwards published in the Annals of Philosophy, vol. VI., p. 371, a clear and forcible statement of the main foundations of Dr. Dalton's Theory of Chemical Composition.

Mr. Ewart's range of thought and interest extended beyond the limits of the physical sciences. He possessed, in a large measure, the prevailing taste of his country for metaphysical research and discussion; nor did he shrink from encountering those abstruse speculations, which, in every age and nation, have exercised the ingenuity, but eluded the grasp of thoughtful men. In handling such topics, he was peculiarly fastidious in the choice of language; and most rigid in first marking the precise force of general terms and definitions, and in then exacting a vigilant mindfulness of their pre-admitted value. There was indeed no fault, which he was so keen in detecting, and so energetic in reprehending, in writers on the higher philosophy, whether of mind or matter, as the negligent use of important terms and the disregard of their intrinsic or conventional acceptation. His own metaphysical opinions had been mainly gathered from the works of Reid, Dugald Stewart, and perhaps, but in less measure, from those of Dr. Brown. He was slow in receiving into favour, more modern authorities, and found especially more to condemn than to admire in the eloquent, but, it must be confessed, often declamatory lectures of M. Victor Cousin.

Latterly Mr. Ewart's attention had been fixed, by the publication of Sir James Mackintosh's Dissertation, on the successive Theories of Ethics, which have been powerfully analyzed, in historical sequence, by that profound thinker. He was especially impressed with the value of the Ethical Theory of Bishop Butler; and was even impelled to the careful study of "those deep and sometimes dark dissertations, which he preached at the Chapel of the Rolls, and afterwards published under the name of Sermons." In this branch of inquiry, one of his most favourite authorities was Smith's Theory of Moral Senti ments. It was the work, with which he was occupied, before that melancholy event, which suddenly terminated his useful life; and he has left, in the volume he was reading, numerous references to the passages that had most deeply interested him; references, that thus bring vividly present to us, with a sacred solemnity, his last intellectual acts, his parting moral aspirations. And there is something deeply affecting in contemplating this good and truly venerable man, thus employing almost the last moments of existence, in calmly weighing and analyzing those pure and lofty sentiments, of which his long life had presented so bright and steady a manifestation.

Mr. Ewart's mind was characterized by the healthful and well-adjusted proportion of its various faculties, by strength and symmetry, by general soundness, rectitude and comprehensiveness, rather than by the salient and exclusive preponderance of any one separate endowment. He possessed most remarkably, the faculty of sustained patient thought; and it was by laborious, long continued efforts of reflection, rather than by the suggestive inspiration of genius, that he slowly wrought out his mechanical inventions, and framed his deliberate opinions in exact science, and in the higher Philosophy. His processes of acquiring knowledge, as well as his general habits of thought and apprehension were slow and laboured; but the treasures thus assembled, were strictly methodized and tenaciously retained; and, what is less common, they were at all times readily producible, and were gracefully imparted with inborn gentle courtesy and total forgetfulness of self.

The marked features of Mr. Ewart's moral character, his warmth and steadiness in friendship, his generous and delicate sense of honour, the singular purity and guilelessness of his nature, must be fresh in the memory of many members of this society. He had derived from nature that warm susceptibility of temper, out of which often issue the noblest and most generous impulses, and which, duly chastened and vigilantly controlled, is the parent of the loftiest virtue. In him this warmth of feeling never manifested itself, except in a heightened colour and slight impatience of manner, called forth by what he deemed unsound or irrelevant in argument, false or exaggerated in sentiment or expression. Even this quickness of manner was but momentary in its duration, being at once softened away and subdued by the benevolent courtesy and mild forbearance of his nature.

In conclusion, it may perhaps be permitted to one, who from earliest childhood was honoured by his friendly regard, and who in after professional life stood near to him, in a season of deep sorrow, to bear testimony to the resigned fortitude and serene composure which could be sustained only by the wisest discipline of the affections, and the firmest religious hopes and convictions.

LIST OF PAPERS

READ BY MR. EWART, BEFORE THE SOCIETY.

1.—"On the nature and sources of Property and Value." Read, April 22, 1808.

2.—"On the Measure of Moving Force." Read, November 18, 1808.

3.—"On the Application of Springs to heavy Carriages." Read, January 26, 1810.

4.—"Observations on Mr. Dalton's theory of Chemical Composition." Read, November 13, 1812.

5.—"On Dr. Adam Smith's distinction between Productive and Unproductive Labour." Read, February 7, 1817.

6.—"On the Temperature of Steam, issuing from a Boiler of high pressure, in the act of dilatation." Read, October 18, 1822.

7.—"Experiments and Observations on the phenomena attending the exit of compressed elastic fluids into the atmosphere. Read, November 14, 1828.

All of them, except one which Mr. Ewart withdrew, were voted to be printed in the Society's Memoirs; but I suppose he felt so unwilling to appear in print, that he only allowed his valuable paper "On the Measure of Moving Force," to be printed.

Note by Mr. Clare.

SOME ACCOUNT

OF THE

LATE MR. EWART'S PAPER

ON THE

MEASURE OF MOVING FORCE;

AND OF THE RECENT APPLICATIONS

OF THE

PRINCIPLE OF LIVING FORCES TO ESTIMATE THE EFFECTS OF MACHINES AND MOVERS.

By EATON HODGKINSON, F.R.S., F.G.S.

(Read 30th of April, 1844.)

Having been requested, by several leading members of this society, to give some further account of Mr. Ewart's scientific labours—particularly that on the "Measure of Moving Force," Manchester Memoirs, Second Series, Vol. 2.—than came within the plan of Dr. Henry's elegant "Biographical Notice," I shall confine myself, principally, to the very laborious and ingenious paper above mentioned; giving a short account of the application of the principle which it recommends.

The subject of this paper formerly caused a great controversy among mathematicians, which continued for 30 years or more; and was then dropped, about a century ago. Since which time an idea has been generally entertained that it was only a dispute about terms.

The advocates on one side, including Leibnitz, the Bernoullis (John and Daniel), Hooke, Huygens, Wolfius, Gravesande, Muschenbrook, &c., maintained that the force of bodies in motion ought to be estimated by the quantity of matter multiplied into the square of the velocity; whilst the other side, called Newtonians, and including the names of Maclaurin, Pemberton, Desaguliers, Clark, Jurin, and Robins, contended that the force was as the quantity of matter multiplied simply by the velocity.

To explain the reason of these opposite opinions, Dr. Wollaston, in his Lecture on the Force of Percussion, (Phil. Trans. 1806), proposes the following experiment:

"Suppose a ball of clay to be suspended at rest, having two similar and equal pegs slightly inserted into its opposite sides; and let two other bodies A and B, whose weights are as 2 to 1, strike at the same instant against the opposite pegs, with velocities which are in the proportion of 1 to 2. In this case the ball of clay would not be moved from its place to either side; nevertheless, the peg impelled by the smaller body B, which has the double velocity, would be found to have penetrated twice as far into the clay as the peg impelled by the larger body A."

The results of this experiment were admitted by both parties; but they reasoned upon them differently. The party termed Newtonians asserted that as the clay is not moved, it is a proof that the forces of impact of the two balls were equal; as they would infer from the momenta being equal. Their opponents, on the other hand, maintained with equal confidence that the unequal depths to which the pegs had been driven was a proof that the causes of these different effects were unequal; as might be inferred from considering the forces as proportional to the squares of the velocities. One party drew their conclusions from the fact that equal *momenta* can resist equal pressures during the same *time.* The other party took into consideration the *spaces* through which the same moving force was exerted; and as these were as 2 to 1, or as the product of the weight of each striking body and the square of its velocity, they concluded that the vis viva, to which this is proportional, was the proper measure of the effect of a body in motion.

The main object of Mr. Ewart's paper (in accordance with the conclusions of Smeaton (Phil. Trans. 1776), Dr. Wollaston and others) was to show that if a constant pressure applied by any agent were multiplied by the space through which it acted, the result, being in a given proportion to the vis viva, was the most natural measure of moving force. He urged that if the effects of pressures were estimated with regard to the spaces through which they passed, instead of the velocities for a certain time, they would apply, as a measure of work done, in all the cases of practical mechanics; and would, he contended, remove many inconsistencies and errors, from the reasonings upon questions occurring in them. He gives illustrations of his statements from almost every branch of practical mechanics aud hydraulics; pointing out discrepancies, and solving various problems according to the principles he had assumed. He compared the conclusions from the common theory of fluids with the results of Smeaton's experiments on water wheels, (Phil. Trans. 1759,) arriving at interesting conclusions; but the more recent investigations on this subject by Poncelet and others; with the very important experiments of the committee of the Franklin Institute, upon wheels of 20, 15, 10, and 6 feet diameter,—together with those of M. Morin, on the Turbine of M. Fourneyron,—have placed all others in the shade. Mr. Ewart made many experiments on the reaction of effluent water, and applied the results of his researches on this subject to the solution of the problem of the recoil engine, known by the name of Barker's Mill. This part of the paper was reprinted in 1828, in the Philosophical Magazine, with notes relating to the theory of Barker's mill by the late Mr. Ivory. This great mathematician, in solving the problem, compared the results of Euler's, Bossut's, and Mr. Ewart's solutions, and added other enquiries of his own.

In following the reasonings in Mr. Ewart's paper, though generally sound, the reader has to encounter difficulties from a change of names, and assumptions to which he is not accustomed. For instance, in computing the effects of collisions between non-elastic bodies, Mr. Ewart adopted the language of Smeaton, (Phil. Trans. 1776, &c.) assuming that a large portion of the *moving force* of bodies (which he designated by bv^2, instead of bv, as usual, b being the mass and v the velocity) was lost by changing the figure of the bodies.

Thus, if a non-elastic ball whose mass is A, moving with the velocity v, strike another non-elastic ball whose mass is B, at rest, they will, after collision, move on together with a velocity $\frac{Av}{A+B}$. But the moving force before collision, according to Mr. Ewart's definition, is Av^2; and after collison it is $(A+B)\left(\frac{Av}{A+B}\right)^2=\frac{A^2}{A+B}v^2$. But $Av^2 : \frac{A^2}{A+B}v^2 :: 1 : \frac{A}{A+B}$. The fractional part of the moving force (according to this definition), which is found in the bodies after collision, is therefore $= \frac{A}{A+B}$; hence the part of it which is spent in producing change of figure is $\frac{B}{A+B}$.

If the two bodies were equal, $A=B$, and the part of the moving force lost in changing the figure of the bodies would, according to this reasoning, be one half.

If $A = 2B$, the part of the moving force lost, as here defined, would be one-third.

It must be borne in mind that the moving force here described as lost is the vis viva. The quantity of it is otherwise expressed by Carnot's theorem.*

Mr. Ewart adduces, in his paper, several cases which he conceives not to be explicable according to the principles maintained by the advocates of the opposite measure of moving force to that which he adopted; and if the difference between them had been as great in reality as it was in words, there is little doubt his conclusion would have been just. Both parties seemed to be right, and to obtain correct results, when they reasoned consistently with their hypothesis; but there required a little adjustment between them. The arguments for adopting, under some distinct de-

* Putting u for the common velocity after collision, we have, from Carnot's theorem,

$$Av^2-(A+B)\,u^2=A\,(v-u)^2+Bu^2,$$

the vis viva lost by collision, which we will call L.

From this equation we derive $u=\frac{Av}{A+B}$,

$$\therefore L=Av^2-(A+B)\,\frac{A^2v^2}{(A+B)^2}=\frac{AB}{A+B}\,v^2,$$

which is equal to $\frac{B}{A+B}\times Av^2$, as obtained by Mr. Ewart.

nomination, the product of the quantity of a pressure by the space through which it acted were very strong. It was evident that such a measure of effect produced would have a most extensive application.

This measure had been used by Watt to estimate the effects of steam engines; it had long before been adopted by Smeaton; and writers on mechanics had become prepared, both in this country and on the continent, for the introduction of such a value. It has, therefore, been adopted by the most eminent writers on mechanics, both theoretical and practical; and without making any change in the received definitions of momentum and moving force, which depend on the mass multiplied by the velocity.

PROGRESS OF THE CHANGE.

To give some account of the progress of this change, which has been gradually taking place for a number of years, both in this country and on the continent, I shall begin with the former—mentioning first Mr. Davies Gilbert, who, in 1827, gave a paper, in the Philosophical Transactions, "On the expediency of assigning specific names to all such functions of simple elements as represent definite physical properties; with the suggestion of a *new Term* in mechanics."

Mr. Gilbert observes it has been remarked that neither impetus nor momentum are usually correct measures of the effective action of machines. The criterion of this is the force exerted multiplied by the space through which it acts; and to this function he proposes to give the name *Efficiency*, retaining the word *Duty* for a similar function, indicative of the work performed; and by a comparison of these two functions, viz. the Efficiency expended on, and the Duty performed by any machine, an exact measure of its intrinsic work will be obtained (see Abstracts of Phil. Trans. 1827).

The Rev. Dr. Whewell in his "First Principles of Mechanics," published in 1832, gives a chapter "on the work done by machines," the first section of which is headed Measure of Efficiency, and he adopts the suggestions of Mr. Gilbert, as mentioned above.

Since the vis viva expresses, dynamically, twice the product of any pressure by the space through which it acts, in the direction of that pressure, several recent French writers, as Carnot, Navier, Coriolis, and others have made that principle the subject of their researches, showing its important applications to the science of Movers and Machines. There is, however, no person to whom the public are indebted in so high a degree, for an exposition of this matter, as to M. Poncelet, who, in his "Mécanique Industrielle," published in 1839, has shown the full application of the principle in its most important practical bearings. It has been lately most extensively quoted in the able works of Professor Moseley (Researches on the Theory of Machines, Phil. Trans. 1841, and Mechanical Principles of Engineering and Architecture, 8vo. 1843), and in these the subject has been further extended.

The union of a continued pressure with the space through which it acts has received various names on the continent as well as in this country, as "quantité d'action," "puissance mécanique," "quantité de travaille," &c. The last of which, with its English translation, "quantity of work," or simply "work," has been used by Coriolis, by whom it was first so named, (Journal de l'École Polytechnique, 21e cahier,) and adopted by Poisson, Poncelet, and Moseley, and seems likely to be generally followed.

A unity of work, in the sense here used, is, in this country, represented by the labour of removing one pound through one foot; and, in France, one kilogramme through one metre. With these values any other quantities of work may be compared. As, for instance, if 2240 lbs., or one ton, be raised through ten feet, the work done will be $2240 \times 10 = 22400$ lbs., or ten tons raised through one foot, representing 22400 unities of work.

If p be any constant pressure tending to do work, and s the space through which it has acted, then ps will be the work done; and this quantity of work will be the same if p be increased or decreased in any proportion, provided that s be decreased or increased in the same proportion, since the quantity of work depends upon the product of the two, and not on the value of either.

WORK DONE BY GRAVITY.

To compare the quantity of work ps with the effects of gravity in the falling of bodies, suppose

v=velocity which would be generated in a body p, during its fall through a distance s, and g= the force of gravity (=$32\frac{1}{6}$ feet); then, by the laws of falling bodies, $s=\frac{v^2}{2g}$, and

$$ps=\frac{1}{2}\frac{p}{g}v^2.$$

Here $\frac{p}{g}v^2$ is called the vis viva of the body p, whose velocity is v, and the quotient $\frac{p}{g}$ represents the mass of the body.

If ps be represented by w we have

$$w=\frac{1}{2}\frac{p}{g}v^2,$$

whence the vis viva is equal to double the work done.

It appears that the quantity of work done by gravity, to impress a certain velocity v upon a falling body p, is equal to the half of $\frac{p}{g}v^2$, the vis viva generated in it. And, for the same reason, the quantity of work done by gravity in the taking away of a velocity v from a body p, thrown upwards, will be measured by half the vis viva, as before.

The work done on a body, passing from a velocity v_1 answering to $s=o$, to any other velocity v is

$$w=\pm\tfrac{1}{2}\frac{p}{g}(v^2-v_1^2),$$

where the upper or lower sign is taken according as the velocity is accelerated or retarded.

For important practical examples on this subject, see "Additions et Développements," in the "Mecanique Industrielle," of M. Poncelet; and the Rev. Mr. Moseley's "Mechanics of Engineering and Architecture." The former writer, however, estimating the *work* necessary to overcome the *inertia*, and give the requisite velocity to the body p; and the latter regarding half the *vis viva* as *work accumulated* in the body, through its acquired velocity, and which work it is capable of reproducing upon any body retarding its progress.

WORK DONE BY MACHINES.

In the "Addition," at the end of the second volume of Poisson's "Mécanique," published in 1833, there are given some very profound illustrations of the use of the principle of living forces, (the vis viva) in calculating the effects of machines in motion. M. Poisson observes that the principle of virtual velocities gives, immediately, the conditions of the *equilibrium* of forces applied to machines, whilst that of living forces includes equally the theory of their *motion*, and furnishes the most direct means of calculating the effects of the forces which are applied to them. After giving the most general notions upon the subject, he refers for developments to M. Poncelet and M. Navier.

Machines, he says, may be defined as instruments or systems of solid bodies, adapted to transmit the action of the forces from one part to another of these assemblages.

When a machine is in motion, certain points of it are subjected to the action of given forces, and other parts press upon exterior bodies, or are reciprocally pressed by those bodies which the machine is intended to displace or change the form of. The first description of forces are called *moving forces;* the forces opposed to these are called *resisting forces*.

The connexion of the parts of a machine is such that it can, in general, only take two motions, directly opposite to each other; it follows, therefore, that when the direction of the motion which it actually takes is known, one equation only is required to determine this motion in a complete manner.

To calculate the effect of machines in motion, M. Poisson gives the following equation of living forces:—

$$\tfrac{1}{2}\Sigma mv^2 - \tfrac{1}{2}\Sigma mk^2 = \int \Sigma Pdp - \int \Sigma Qdq.$$

Where v represents the velocity of the mass m at the end of any given time t, k its original velocity answering to $t=o$, P any one of the pressures applied to cause motion in the machine, and Q any one of the resistances to its motion; dp the space passed over by P in the direction of its force during the time dt, and dq the space passed over in the same time by Q, in the direction of its force. The integrals to be taken so as to vanish at the commencement of the motion.

The products Pdp and Qdq of which the sums are subjected to these integrations have, M. Poisson observes, received different denominations; and M. Coriolis proposes to call them *quantities of elementary work*, which name M. Poisson adopts. The sums ΣPdp and ΣQdq will be the quantities of elementary work performed during the same instant by all the moving forces and all the resisting forces; and their integrals $\int \Sigma Pdp$ and $\int \Sigma Qdq$ will express the entire motive work and the entire resisting work, from the commencement of the motion to the time in question.

The equation shows that, in any machine in motion, the increment, during any time whatever, of the half sum of the living forces of all its parts, is always equal to the excess of the motive work over the resisting work during the same time.

If there were no original velocity in the machine, and the object was to consider the effect of friction as a separate retarding force, the following equation is given by M. Poisson for the purpose,

$$\tfrac{1}{2}\Sigma mv^2 = \int \Sigma Pdp - \int \Sigma Qdq - \int \Sigma fNds,$$

where f is the co-efficient of friction, N the mutual pressure of the parts which rub against each other, ds the element of the curve described by their point of contact, and $\int \Sigma Qdq$ the useful work produced by the machine. When the machine has attained to a permanent state, the quantity of work performed during a given time by the moving forces is not wholly represented by $\int \Sigma Q dq$ the effective part of the resisting work; for this is always less than the motive work $\int \Sigma P dp$ by that which is done in overcoming friction, and other resistances. A machine is more perfect according as the effective work approaches to an equality with the motive work. As an example of imperfect machinery, in which the effective work is only a very small fraction of the motive work, M. Poisson adduces the machine formerly used to raise water from the Seine at Marly. For an account of this magnificent collection of injudiciously constructed mechanism, see "Bélidor, Architecture Hydraulique," or, "Gregory's Mechanics, vol. 2."

Professor Moseley, in his "Mechanical Principles of Engineering and Architecture," gives a different interpretation to the vis viva of a moving body, from that of M. Poncelet, before alluded to, feeling that the use of the term vis inertiæ, employed by that writer, and associated with the distinct idea of a force opposed to all change of motion, is attended with difficulties which it would be better to avoid. He has, therefore, given to the principle of vis viva, when applied to the motion of machines, the following new enunciation :—" The difference between the aggregate work done upon a machine, during any time, by those forces which tend to accelerate the motion, and the aggregate work during the same time of those which tend to retard the motion, is equal to the aggregate number of units of work accumulated in the moving parts of the machine during that time, if the former aggregate exceeds the latter, and lost from them during that time if the former aggregate fall short of the latter."—(Preface p. 9.)

Professor Moseley expresses this by the following formula, which is in agreement with those of Poisson:

$$\Sigma U_1 = \Sigma U_2 + \Sigma u + \frac{1}{2g} \Sigma w (v_2^2 - v_1^2)$$

Where ΣU_1 represents the number of units of work done upon the machine by the moving power, ΣU_2 the units of useful work performed, Σu the work consumed in overcoming friction and other resistances, v_1 and v_2 the velocities at the commencement and termination of the period of working, and $\frac{1}{2g} \Sigma w \, (v_2^2 - v_1^2)$ half the aggregate difference of the vires vivæ of the moving elements of the machine, or the work accumulated in them, and reproducible when a falling off occurs in the moving power.

When the motion of the machine has become uniform, the last term of the preceding equation disappears, and we have

$$\Sigma U_1 = \Sigma U_2 + \Sigma u,$$

as might be inferred from the principle of virtual velocites.

The theorems previously given apply to machines, subject to acceleration; but, in actual practice, it fortunately happens that we have seldom need to calculate their effects, except when their motion is uniform, or periodical as in the Steam Engine; and, in these cases, the easy formula of virtual velocities, last given, is sufficient.

There exists in every machine, a direct relation between the following elements:—the work done upon the moving points, that expended upon the working points, that required to overcome the prejudicial resistances, and that accumulated in the moving elements of the machine; Professor

Moseley has sought for this relation in different machines, giving to it the name of the "modulus" of the machine.

In every machine the velocity of the different parts is connected by certain invariable relations, capable of being expressed mathematically, and these relations have been determined, in many instances of complex machinery, in the valuable work of Professor Willis, entitled "The Principles of Mechanism."

In closing this notice of Mr. Ewart's principal scientific effort, I feel desirous of bearing my testimony to the purity and disinterestedness of his love for science, particularly that which had a direct practical application; and to mention that whatever estimation may be given to my own humble experimental efforts, it is probable that none but the earliest of them would ever have been made had it not been for the encouragement given by Mr. Ewart, who first introduced me to my liberal friend Mr. Fairbairn, and afterwards was present through the whole series of my experiments on the strength and best forms of iron beams, the results of which were published in the fifth volume of the memoirs of this society.

OBSERVATIONS

ON THE

RELATION WHICH THE FALL OF RAIN BEARS

TO THE

WATER FLOWING FROM THE GROUND.

BY JOHN FREDERIC BATEMAN, M. INST. C. E.

Read 6th of February, 1844.

In the year 1799 Dr. Dalton brought this subject before the notice of the society, in a valuable paper entitled " Experiments and Observations to determine whether the quantity of Rain and Dew is equal to the quantity of water carried off by rivers, and raised by evaporation; with an inquiry into the Origin of Springs."*

In that paper, after observing that " it is scarcely possible to contemplate without admiration the beautiful system of nature, by which the

* Memoirs of the Literary and Philosophical Society of Manchester, Vol. 5.

surface of the earth is continually supplied with water," and after alluding to the importance of the enquiry to agriculture, and to the arts and manufactures, he enters into a very careful investigation of the whole subject, under the several heads of—

"The quantity of Rain and Dew.
"The quantity of water that flows into the sea.
"The quantity of water raised by evaporation. and
"The origin of springs."

Having first determined, from an extensive series of observations, the probable average quantity of rain falling annually throughout the kingdom, which, taking 31 ins. for rain, and 5 ins. for dew, he considers will be 36 ins., and by multiplying this quantity into the area of the country, ascertained the whole amount of rain, he proceeds to estimate how much will be carried down by the rivers to the sea Grounding his calculations upon an estimate made by Dr. Halley, upon the volume of the Thames at Kingston Bridge, and the extent of country from which the supply of water would be drawn, he goes through a consideration of the quantity of water which must be conveyed to the sea by all the various rivers in the country ; the result is, that he considers 13 ins. of the annual rain are in that way disposed of, and that 23 ins. remain to be accounted for. This brings him to a consideration of the quantity of water raised by evaporation. To ascertain this point, not finding the observations of others sufficient for his purpose, he made various experiments upon the evaporation from soil. From these it seemed that the evaporation amounted to 25 ins. of rain annually, and if 5 ins. for the dew were added, it would give 30 ins. of water raised annually. As this exceeded the medium reserve of 23 ins., he goes on to enquire "whether the rain is adequate, or whether the earth derives a supply of water from some subterraneous reservoir, according to the opinion of some philosophers." Having accounted for this apparent discrepancy, he finally arrives at the conclusion "that the rain and dew of this country are equivalent to the quantity of water carried off by evaporation, and by the rivers."

He finally examines the various opinions which at that time existed upon the origin of springs, concluding that they must be attributed solely to the rain, their variation depending upon the seasons, and upon the quantity of rain which falls.

Any subject which could command so much of Dr. Dalton's scientific attention, would require nothing being said to prove it worthy of the most careful investigation. Meteorology, generally, has been a favourite pursuit with him through life, and the public are indebted for much of the information, and most of the sound views they possess, to the many papers upon this subject which the doctor has contributed.

It is, however, in the application of the knowledge which is acquired in the pursuit of such enquiries to the wants and purposes of life, that their great practical importance consists.

The intimate connexion between the amount of rain falling, and the supply of water in the country, is obvious. On our knowledge of the relation which one bears to the other, under different circumstances, must we depend for rules to guide us in agricultural and engineering improvements, and in the consideration of all cases in which the application of water, or its multifarious effects, form important points.

In irrigation, for instance; in the supply of water to towns, or in its application as a moving power, for manufacturing or other purposes; in navigation or in draining; in estimating its effects upon the country generally, or upon the course and mouths of rivers, and the maintenance of harbours, this knowledge is essential.

It is a matter of surprise therefore, that so little has hitherto been done in the investigation of the subject. Dr. Dalton's paper stands almost alone, and, until recently, no actual observations upon a large scale, appear ever to have been made.

Valuable, however, as the Doctor's reasoning and calculations undoubtedly are, they can only be regarded as mere approximate estimates; but they are grounded upon the proper data, the best he could at that time command, and are well calculated to account in all respects most rationally, for the manner in which the water of rain is disposed of.

To arrive at accurate conclusions, such as may be depended upon with confidence, in applying the results to the purposes of life, extensive observations upon a large scale, for a long series of years, are necessary, and in districts of various physical and geological character.

The quantity of water which may be expected to flow from a flat country well clothed with vegetation, will be very different from that which will pour in torrents down the steep declivities of uncovered mountains ; and again, the water which will pass off from the surface of a limestone district, with its many caverns and perpendicular fissures, will vary considerably from that which will drain off in like manner, from a district, similar perhaps in its external features, but consisting of the dense rock of a primitive formation or the flat beds of the coal measures.—Clay and sand ; morass and cultivated ground ; absorbent chalk and more impervious material, all contribute to produce a varying result.

The rapidity with which rain descends, or the state of saturation in which the ground is found at the commencement of a wet period, will also vary the quantity of water which will flow away in different years as a supply to rivers, although the average or gross amount of rain may be nearly the same.

Perhaps the most serious difficulties which have hitherto existed, consist in the extensive scale on which the observations should be made to be of any real value, and the accuracy required in ascertaining the quantity of water actually discharged from a certain extent of country. The measurement of a single flood may be omitted, and yet that flood may send down as much water in one day as would pass off in two months of ordinary weather.

Of late years works have been constructed in some parts of the country, by using which as a means of observation, these difficulties have been to a great extent removed.

The demand for moving power consequent on the great extension of our manufactures, very early led to the application for that purpose of every important stream in the manufacturing districts. As improvements in the face of the country and in agriculture kept pace with commercial prosperity, the former general regularity of the rivers was found to be considerably disturbed by the effects produced by the making of roads and the better draining and cultivation of the land. The drains and roads form so many conduits by which the rain which falls is rapidly conveyed to the contiguous streams, and the same quantity of water which formerly required several days to make its escape, now passes off in a few hours. Some quiet swamp or bog at the head of a river, which had for centuries contributed to regulate the quantity of water in the stream, by absorbing the superabundance of wet periods and gradually discharging it at other times, becomes allotted off in farms, trenched in all directions and thoroughly drained. The water is then no longer retained to assist the streams in dry periods, but runs off to swell the floods as quickly as the drains can be made to carry it.

This increasing irregularity, combined with a growing demand for further power, has contributed to the construction on many streams, of large reservoirs for the purpose of impounding the surplus water of wet seasons to be applied in supplying the wants of periods of drought, and by such artificial means to regulate the quantity of water in the stream.

Wherever these reservoirs have been constructed of sufficient capacity, opportunities have been created for ascertaining what proportion the water flowing from the ground bears to the rain which falls upon it. The extent of country from the surface of which the water would naturally drain to the reservoir may be easily ascertained, the depth of rain in a given time may be measured by gauges placed in different parts of the drainage ground, and the quantity which is discharged from the reservoir, or which runs to waste in the same period, may, by proper observations, be correctly calculated. Here there is, in a given district, everything which is required, and by a sufficient number of observations, data will be obtained for forming a judgment upon the probable result in a similar locality.

Some of the more important observations, which, under such favourable circumstances, have of late years been made, we will now proceed to notice.

During the construction of the Bann Reservoirs in the north of Ireland, attempts were made to ascertain the quantity of rain which fell in a mountainous district, and how much water could be collected in the basins of the reservoirs.

The Lough Island Reavy Reservoir is situated on the westerly flanks of the Mourne Mountains, at an elevation of about 400 feet above the level of the sea. It derives its supplies from about 3300 statute acres of mountain land, rising in places to abrupt eminences of from two to 3000 feet in height. The direction of the general chain of the mountains is from north-east to south-west, and measuring from Carlingford Bay on the south, to Dundrum Bay at the foot of Slieve Donard, on the north, about 15 miles in length. They form the easterly sea-bord of the island, rising abruptly from the sea, the summit of Slieve Donard 2796 feet in height, the highest in the range, being only one mile and three quarters from the coast. Their average width is about five or six miles, the interior of the country for some distance from their westerly base, being comparatively level, rising to no great elevation above the level of the sea. In these mountains the River Bann and its early tributaries take their rise, within six or seven miles of the sea, from which they run in a westerly direction towards the interior; the river, after passing through Lough Neagh, finally discharges its waters at Coleraine into the North Channel.

The hills are composed of primary and transition strata, many parts uncovered with any kind of earth or vegetation, the declivities for the most part being excessively steep.

Rain gauges were placed at Lough Island Reavy, 400 feet above the sea, and on the summit of the Spelga Mountain 1400 feet above the sea. The gauge at Lough Island Reavy was placed within about 9 ins. or a foot of the level of the ground—that on Spelga, within the same distance of the top of a mound raised about four feet above the general level of the ground, with a trench dug round it to prevent the approach of sheep which fed upon the mountains.

From October 1837 to October 1838 there fell at Lough Island Reavy 72 $\frac{3}{4}$ ins., and on Spelga 74 ins. of rain.

This period was below an average in other places where observations were taken, as were also the succeeding years to 1841. It was difficult in that country to find persons to attend properly to the gauges, but the result of our experience, during those years, was to ascertain that of six feet of rain which fell upon the drainage ground, four feet found its way to, and was impounded in the reservoir, besides allowing a quantity to run past it, sufficient to work some small corn and flax mills.

In the year 1839 gauges were placed in suitable positions across the Penine chain of Hills, pretty nearly along the line of the Manchester and Sheffield Road by way of Glossop, for the purpose of ascertaining the fall of rain at different elevations, over a tract of country which would form a fair average of the mountain drainage of the rivers which take their rise in that district. It was intended to have connected these observations with others upon the quantity of water which flowed from the land as a supply to the rivers, but this has hitherto been found impracticable.

The places chosen for fixing the rain gauges, were 1st, the westerly foot of the hills, in the valley of Hurst Brook near Glossop, about 500 feet above the sea. 2ndly, the head of that valley on the westerly edge of a tract of table land on the summit of the hills, probably 1500 feet above the sea. 3rdly, the head of Ashop-dale upon the flank of Kinder Scout, upon the easterly edge of the summit plain, perhaps 1600 feet above the sea, and fourthly, at the easterly foot of the hills in the valley of the Derwent, at Bamford Mills, near Hathersage. This place may be 300 feet above the sea. Mr. John Kershaw, of Hurst, has kindly superintended the taking of the observations at the two first stations, and Mr. W. Moore, of Bamford, undertook the charge and trouble of looking after the proper registering of the others.

The following table contains the result of these observations to the end of 1842 :—

	At the foot of the hills near Glossop. Probable height above level of sea 500 feet.				Near the summit, at the head of the Hurst Brook Valley. 1500 feet.				Near the summit, on the flank of Kinder Scout, at the head of Ashop Dale. 1600 feet.				In the valley of the Derwent, at Bamford, near Hathersage, 300 feet.			
	1839.	1840.	1841.	1842.	1839.	1840.	1841.	1842.	1839.	1840.	1841.	1842.	1839.	1840.	1841.	1842.
	Ins.	Ins.		Ins.	Ins.	Ins.	Ins.	Ins.	Ins.	Ins.	Ins.		Ins.	Ins.	Ins.	
January						2·6	3·0	1·3		9·4	2·4			5·05	2·6	
February						6·6	3·3	2·7		5·3	2·6			3·60	1·4	
March ...						0·5	3·3	5·9		0·6	4·0			0·65	2·7	
April ...	15·75	21·2			30·5	1·6	6·4	4·0		1·6	5·6			1·30	1·3	
May						7·7	4·6	2·3		5·2	5·4			3·20	4·1	
June						6·5	5·9	2·2		7·3	5·2			3·00	3·1	
July.........	5·50		—	3·50	—	6·3	7·2	3·5		13·4	10·3			4·30	4·1	
August	4·25		—	1·70	3·8	9·5	7·3	5·2		7·0	9 5			4·60	3·6	
September.	7·00		—	4·50	8·0	6·7	6·0			9·3	5·4			2·95	3·15	
October ...	3·50	24·00	—	2·90	4·5	6·5	10·5	12·4	3·5	5·7	11·2		3·20	2·15	6·[illegible]5	
November.	3·70		—	6·60	12·3	7·0	9·0	4·3	6·9	8·0	6·4		5·45	7·10	4·4	
December.	4·80		—	3·30	4·9	1·5	6·1	3·3	6·1	2 0	12·1		2·95	0·9	5·5	
	44·50	45·2		22·50	64·0	63·0	72·6	47·1	16.5	74·8	80·1		11·60	38·8	42·9	
		to 17 Nov. 10½ Mos.		6 Mos.				last 6 Mos 28·7	3 Mos.				3 Mos.			

The mean annual results are—

	Inches.
At the westerly foot of the hills for $2\frac{1}{2}$ years	45
At the westerly edge of the summit plain, 4 years ...	61.7
Mean of 1840 and 1841............	67.8
Easterly edge of do. 1840 and 1841, 2 yrs.	77.45
Easterly foot, in the Valley of the Derwent............ } 2 years, 1840 & 1841 ...	40.85

The gauges were all exactly alike—all sunk into the ground so as to allow the top of the funnel to be about 12 inches above the surface.

One very important fact shown by these observations, is, that a much greater quantity of rain falls on the summit than at the bottom of the hills, and this appears to be uniformly the case, whether taken for a single month or upon the general average.

The years 1839, 1840, and 1841, were all below Dr. Dalton's average of forty-seven years. 1842 only gave three-fourths of that average.

Mr. Thom, of Rothesay, the constructor of the Shaw's water works, near Greenock, and of many other similar works in Scotland, has for many years been an accurate observer, and must have collected a vast amount of information, which it is to be hoped he may some day be induced to bring before the public.

In a communication to the Institution of Civil Engineers some years ago, he gives the particulars of some very important facts, which are the more valuable, as they happen to be from observations made during periods unusually wet and dry, respectively, so that they may be supposed to show extreme results.

The information in his own words, is as follows :—

"*Rain in Bute from* 1*st April*, 1826, *to* 1*st April*, 1827.

	Inches.
Depth of Rain that fell from end of March to October 1st, 1826	12
Of which there found its way to the reservoir	1.5
Taken up by evaporation, vegetation, &c.	10.5
Depth of Rain that fell from October 1st, 1826, to March, 1827	25.2
Of which there found its way to the reservoir	15.3
Taken up by evaporation, absorption, vegetation, &c.	9.9

	Inches.
Depth of Rain that fell in March, 1827......... ..	8.2
of which there found its way to the reservoir...	7.1
Taken up by evaporation, vegetation, &c.	1.1
Depth of Rain from 1st of April, 1826, to April, 1827	45.4
of which there found its way to the reservoir...	23.9
Lost to the reservoir	21.5

NOTE.—The above was from a drainage of more than 4000 Scots acres.

(Signed,) R. THOM."

" *Rain available for Greenock Reservoirs.*

Square feet of surface draining into Greenock reservoirs	217,700,000
Depth of water which fell thereon, year ending September 30th, 1828	6ft.
Cubic feet of Rain which fell on the drainage	1,088,800,000
Cubic feet of Rain which flowed into the reservoirs	744,594,165
Cubic feet evaporated, absorbed by vegetation, &c.	344,205,835

(Signed,) R. THOM."

Now, the year 1826, was the driest year in this country, of which we have any record. It is reasonable to suppose it must have been a dry year also in Scotland; indeed, the returns during the summer months show it to have been so.

In that year, according to Mr. Thom's return, the available rain, or that which was gathered in the reservoir in Bute, was to the total fall as 239 to 454, little more than one half. During the six summer months from the beginning of April to the end of September, only twelve inches of rain fell, being about one-fourth of the whole year's fall. The quantity which drained off was to that which fell, only in the proportion of 15 to 120, just one-eighth. Any calculation upon an average during this period would have been sorely at fault.

The following five months were still, probably, below an average. The rain in Manchester, according to Dr. Dalton's tables,* being only 13.535 inches, while the mean for twenty-two years, including this period, was 15-951 inches, and the quantity draining off would be effected by the previous drought. The soil must have been far

* Vide Memoirs of Literary and Philosophical Society of Manchester, volume VI., new series, p. 575.

short of its usual saturation. The proportion of the water which flowed off the ground, was to the rain which fell upon it, as 153 to 252—three-fifths of the whole. In the month of March, an excessive quantity of rain seems to have fallen, and seven-eighths of it ran off the ground; the quantity for that single month was nearly one half as much as had passed off during the preceding five months, although the winter, and about five times the quantity which the six summer months had yielded.

During the remainder of 1827, the rain which fell, was, according to the Manchester Tables, below an average, the whole year including the wet month of March being only one-thirtieth above it.

The year 1828, Mr. Thom has not given in similar detail, but looking at the Manchester Tables, the excess in the year, (for it is the wettest which those Tables show), appears to have been principally occasioned by extraordinarily heavy rain in July; the remaining months being very near the general average; so that the year might not have been generally so wet, as the gross amount of rain would appear to indicate.

Considering it, however, as a *wet* year, Mr. Thom's return would show, that for such a season, the water flowing from the district above the Greenock reservoirs, would be about two-thirds of the rain which fell. The proportion is as 744 to 1088.

The land draining into the Greenock reservoirs is high and mountainous, principally moorland, having a good deal of similarity to the moorland districts of Lancashire and Yorkshire, but of different geological character. Its formation is principally basaltic. It lies near the sea, on the east of the Firth of Clyde.

Bute is an island, on the west of the Firth of Clyde, about sixteen miles in length, by four and a half in breadth. Where Mr. Thom's experiments were made, it is comparatively a *low country*. It is, I believe, an undulating pastoral district, having no *great elevations*. The average rain for forty years, Mr. Thom states to be forty-eight inches. From its proximity to the sea and high land, it may be a favourable situation for a great fall of rain.

Some useful information may be obtained from observations, which have been made upon the quantity of water discharged from the Turton and Entwistle reservoir, near Bolton. This is a large reservoir, containing about one hundred millions cubic feet of water, constructed about the years 1835 and 6, for the purpose of regulating the supply of water on Bradshaw brook and the river Irwell, by impounding the flood waters.

Mr. Thomas Ashworth, was the engineer employed to construct it. Having fixed an iron gauge below the outlet from the reservoir, all the water discharged was for sometime regularly measured, none being permitted to flow away, otherwise than through the gauge.

In his printed report to the commissioners of the reservoir, dated Sepember 12th, 1836, he states, that the quantity discharged during the first year, amounted to 250,865,600 cubic feet, and that there was then in the reservoir a depth of water of seventy-two feet, which, according to his estimate of the capacity, may be taken at about 50,000,000 cubic feet.

The drainage ground, carefully measured from a map, furnished by the Ordnance office, is 2036 statute acres. On this area, 250,865,600 cubic feet would be equal to a depth of water gathered to the reservoir of thirty-four inches in the year, besides that required for evaporation from the surface of the water. If the fifty millions feet in the reservoir were added to the quantity actually discharged, the depth would be about forty-one inches.

These observations were apparently taken from the summer of 1835, to the summer of 1836, the quantity of rain falling during that period, being rather under a general average. At Bolton, it was three inches less than Mr. Watson's ten years' average of 49.2 inches.*

In a report, dated June 30th, 1837, Mr. Ashworth states, that during the preceding year of 365 days, (probably to midsummer of 1837,) 289,404,000 cubic feet of water had been discharged. This quantity would be equal to a depth of thirty-nine inches, collected in the reservoir.

* Memoirs of the Literary and Philosophical Society of Manchester, volume VI., new series, p. 586.

The rain at Bolton for this year was one inch, or 1-49th below the mean, the mean rain being as stated above to the end of 1840—$49\frac{1}{5}$th inches.

No observations having been taken upon the depth of rain falling upon the drainage ground of the reservoir, the information furnished does not enable us to ascertain what proportion of the whole was impounded. Of course the rain must have exceeded the water actually collected and discharged.

During the past year, many observations have been taken upon an extensive tract of land, adjoining the drainage ground of the Turton and Entwistle reservoir, for the express purpose of ascertaining many of the points now under consideration. The facts there observed will probably supply the deficiency, in the information afforded by that reservoir.

On the 13th January last year (1843) a rain gauge was placed on land contiguous to a reservoir of the Bolton Water Works Company, for supplying the town with water. The reservoir is called the Spring Water Lodge, and is situated upon Hampson's pastures, near Belmont. The gauge is placed at an elevation of about 800 feet above the level of the sea, being about 500 feet above the town of Bolton, and perhaps 700 or 800 feet below the highest land in the district. It is sunk in the ground, in an exposed situation on the westerly slope of the valley of the river Eagley, the top of the funnel being about a foot above the level of the ground. The quantity of rain which has fallen during the last twelve months, from the 13th January last year, to the 13th of January this year, is given in the following table, in which I am enabled, by the kindness of Mr. H. H. Watson, of Bolton, to give the fall of rain there, during the same period. The two accounts, and Mr. Watson's average for ten years, as published in the 6th volume, new series, of the Society's Memoirs, are placed together for the facility of comparison.

TABLE.

1843.	Bolton Water Works, Spring Water Reservoir 850 feet above the sea.	Bolton 320 feet above the sea.	Mean at Bolton for 10 years, ending 1840.
	Inches.	Inches.	Inches.
January .... (At the Water Works Reservoir, from the 13th to the 31st.)	3.0	5.28	7.33 (January and February)
February..............	1.9	1.47	
March	2.7	2.31	3.27
April	12.0	7.58	2.50
May.....................	4.4	3.51	2.26
June	5.0	2.81	4.54
July.....................	8.0	5.17	5.59
August	4.5	4.19	4.41
September	1.0	1.05	4.24
October	11.1	8.50	5.05
November	7.4	6.36	5.84
December	2.0	1.17	4.17
1844.	63.0		
January to the 13th, to complete the 12 months	3.2		
	66.2	49.40	49.20
For eleven complete months, omitting January, 1843....	60.0	44.12	

Here, again, the important fact of the greatest fall of rain being upon the high ground is clearly proved.

As to the full quantity of water which has through this period reached the valley, no observations have been taken, but some very careful measurements at particular times, throw a good deal of light upon the rapidity and extent to which the water of heavy rain flows off the ground.*

The months of February and March were dry months, the rain being much below the average. The month of April was remarkably wet, a greater quantity of rain falling in that month than in any other previous April for many years. May was a little above the average. In June the rain for the entire month was below the average, but it all fell in the first ten days; it was nearly continuous rain during eight of them. During this period of the month the following observations were made.

The ground was perfectly saturated at the commencement of the month, the river being

* In the course of the present year some works will be completed for gauging every drop of water flowing from an extensive portion of the district, and from the daily register which will be kept, means will be afforded for ascertaining the results under all circumstances. These observations will extend over a period of five years.

then in a swollen state from the rain at the end of May, and it was therefore to be expected that a large proportion of the water would run off the ground. The fall of rain during the time was five inches. The quantity of water flowing down the river at Dunscar weir, below the Egerton works, from a drainage ground of about 5400 statute acres was measured daily; the rain ceased on the 10th, on which day the stream was flowing at the rate of 157 cubic feet per second; on the day preceding it had been 250 feet per second; on the 11th it was 27 feet per second, and on the 12th it had shrunk to its usual volume, which, with the assistance of the Belmont reservoir, is from 12 to 15 feet per second.

The quantity of rain which had fallen was equal to 99,099,000 cubic feet of water—that which ran off, including one day after the rain had ceased, was upwards of 90,000,000 cubic feet, better than 9-10ths of the whole.

During no part of this period was there a *heavy* flood. It is not at all unusual to have a flood equal to two, three, or four times the quantity which was passing down at that time at its greatest height. The rain was continuous rather than heavy. Half the quantity which then fell in ten days has not unfrequently fallen in one. The greatest volume of the water was about 250 cubic feet per second, the mean $98\frac{2}{3}$.

On the 17th, 22nd, and 27th of the same month, no rain having fallen since the 10th, the river was again carefully measured, as well as every separate stream which flowed into it. There was but little difference in the results. The quantity gradually decreased, but not considerably. The natural stream at Dunscar weir, unassisted by artificial means, was about three cubic feet per second.

The district from which these streams derive their source adjoins the land which supplies the Turton and Entwistle reservoir. It lies to the westward of the valley in which that reservoir is situated, and forms the first trough of any importance in the range of hills, which, commencing at Rivington Pike, and extending inland, is the first to break the clouds borne by the westerly winds, surcharged with moisture, from the Irish Sea. The external features and geological character of both districts are the same. Both are situated on the lower portion of the coal measure formation, which there lies tolerably level bedded. The elevation above the sea is similar, and the declivity of the land and the nature of cultivation very much alike. Both consist of moorland and pasture, the latter forming a somewhat greater proportion of the whole in the Turton and Entwistle district, than in that at Belmont. The Belmont district may be rather more favourable for the deposition or precipitation of rain, as the clouds, after dragging over the first high ground, will rest awhile in the first trough, formed by the Belmont valley, before they are carried over the second summit to the Turton and Entwistle valley.

The general results, however, may be expected to be pretty much the same, and it will therefore be useful, until further observations are made, to connect the different kinds of information which the two valleys now furnish.

We find that in an average year, the water collected and discharged from the Turton and Entwistle reservoir, is equal to nearly forty inches of water upon the whole drainage ground; and during last year, which was very little more than an average one, the depth of rain at Belmont exceeded sixty inches. Now supposing the same depth of rain to have fallen above the Turton and Entwistle reservoir, during the year in which the forty inches were there collected, that quantity would be nearly two-thirds of that which fell. This agrees with the experience of the Bann reservoirs in average years, and with the observations of Mr. Thom, at the Greenock reservoirs in 1828.

This result is twice as much as the proportion Dr. Dalton assumes by his approximate calculations, even with the addition to the depth of rain of five inches for dew. In those, however, there may be two principal sources of error: first, in the quantity of water which flows into the sea, and secondly, in the means of supply.

First, as to the quantity of water flowing into the sea.—

Dr. Dalton grounds his calculations on an assumption of Dr. Halley's, as to the volume of the Thames, at Kingston; and, imagining that

Dr. Halley has overrated the quantity, takes only two-thirds of his result. Now we have seen that the ordinary measure of a river is very far from affording any criterion by which to judge of the volume of the floods, when much the greatest quantity of water which flows from the ground passes down the rivers; therefore the ordinary volume of the river Thames, whether right, as assumed by Dr. Halley, or as corrected to suit the impressions of Dr. Dalton, cannot be taken as a sufficiently accurate basis of calculation. There can be little doubt that the results would be far below the truth, and that the thirteen inches Dr. Dalton assumes as the depth of rain flowing into the sea, would require to be considerably increased.

Secondly, as to the means of supply.—

In endeavouring to ascertain the average quantity of rain, Dr. Dalton takes a mean of the observations which had at that time been kept throughout England. This, no doubt, gives a very correct idea of the rain falling in the generally inhabited parts of the country; but it may be questioned if it forms a correct measure of the rain which forms the principal supply of rivers.

Most of the rivers, in this country, take their rise in mountainous or hilly districts. From the much greater quantity of rain which falls there, and the rapidity with which it flows from the steep declivities of the hills, it is evident that the principal supply to the rivers is there to be looked for.

Dr. Dalton's estimate, therefore, is probably much below the truth.

If, then, both the depth of rain falling as a supply to rivers, and the quantity of water borne to the sea be taken at a higher rate than he has assumed, his calculations upon the evaporation from soil, &c. which must make up the difference, might still be probably correct, but on these, the Doctor himself places no dependance.

It is clear that the best mode of ascertaining the real quantity of water evaporated from land, required for purposes of vegetation or absorbed by the ground, consists in observations similar in magnitude to those which have been attempted to be described. Garden experiments, upon a square box of a few inches, although they may be sufficient to illustrate the operations of nature to a philosophic mind, are not to be depended upon as rules to guide us in the application of our knowledge to the purposes of life.

In the consideration of this question, it is important also, to observe the position of the gauges by which the depth of rain is measured. It is now sufficiently notorious from the number of experiments which have been made, that gauges placed at different elevations, abruptly raised from the ground, indicate a less fall of rain the higher the gauge is placed. For instance, a gauge placed on the top of a house, shows less rain than one placed on the ground immediately below, and another situated at the top of a contiguous steeple, indicates less than that on the top of the house.

From this fact, which seems clearly established, it has been contended, that less rain will fall on *elevated* than on *low* land. The observations which have been detailed in this paper, clearly show that this is not the case, and most other observations within the region of the clouds, where the proper position of the gauge has been attended to, show the same result, although local circumstances vary the depth of rain in particular places amongst the hills themselves.

The material question is, to ascertain the quantity of water which reaches the ground, and though the singular phenomenon of the decrease in a direct line upwards, has given rise to much philosophical speculation, it does not form an important element in the practical consideration of the subject, if it can be proved, that, apart from local influences, the same effect is not produced by different elevations of the land.

REPORT

OF THE

COMMITTEE FOR SUPERINTENDING THE PLACING OF

RAIN GAUGES

ALONG THE LINES OF THE

ROCHDALE, ASHTON-UNDER-LYNE, AND PEAK FOREST CANALS,

WITH

OBSERVATIONS UPON THE RETURNS,

AND OTHER PARTICULARS.

BY JOHN FRED. BATEMAN, M. INST. C.E.

Read 18th of March, 1845.

During the last session of the society a committee was appointed, and a sum of money granted for the purpose of testing the accuracy of the observations upon the fall of rain, which had for many years been made on and near the line of the Rochdale canal; and for some years also on the lines of the Ashton and Peak Forest canals.

The great difference which existed between these observations, as published in the last or sixth volume, new series, of the Society's Memoirs, and others which had been taken in similar localities, occasioned doubts as to the dependance which could be placed upon them as registering correctly the real amount of rain which reached the surface of the earth.

Upon inquiry, it had been ascertained, that all the gauges of the Rochdale Canal Company were placed on the ridging of the roof, of the residences of the lock-keepers, or other persons in whose custody they were; and in some instances the same plan had been adopted on the lines of the Ashton and Peak Forest canals.

This position had, no doubt, been chosen under the impression that from its exposure, it must necessarily catch all the rain which fell, but it was probably not known at the time the gauges were placed, as it generally is now, that it is a fact, which has been ascertained beyond all question, that less rain is received by a gauge on the top of a house or a tower, than is received on the ground itself. Again, it had probably been overlooked, that the slope of the roof would operate in such a way, as considerably to interfere with the quiet fall of rain. If wind accompanied the rain, which it generally does, its course would be deflected by impinging on the sloping roof, and an upward current would be created, which, passing with increased rapidity over the ridging of the house, would carry the falling drops away, and prevent their being deposited within the area of the gauge.

The inference from these considerations, coupled with the greater fall of rain, shown by other gauges placed upon the ground in similar districts, evidently was, that the returns would be under the truth.

The committee, therefore, determined upon placing gauges in the ground as near as possible to the others, so as to be free from the influence of local currents, or other causes of interference. The gauge selected was the one now most approved of and most commonly used, consisting of a hollow cylinder of copper or other metal, about seven or eight inches in diameter, and thirty-six or forty inches in length, with a receiving funnel of the same diameter as the cylinder, and closely fitted to the top. Within the cylinder, a float rises, as it becomes filled with water. It is just so much smaller in diameter as to rise freely, and in the centre is fixed an upright rod, marked in inches and tenths of an inch, which rising through a small hole at the bottom of the funnel, exactly indicates the depth of rain falling in any given time. The surface of the water in the cylinder being completely covered with the float, except the mere annular space of about one-eighth of an inch,—no evaporation takes place. The gauge must be occasionally emptied of the water it contains. It is sunk in the ground, within a strong box or case to prevent injury, and to allow of its being easily taken out; the top of the gauge being left about ten or twelve inches above the ground.

Gauges of this description were accordingly, at the commencement of last year, fixed in the ground, at or near all the places at which observations had hitherto been made by the Rochdale Canal Company, except Ripponden, where they had been discontinued. The gauge at Stubbins was stolen, after remaining a few months, but at all the other places the observations have been continued throughout the year, by the same persons, who had the charge of the Canal Company's gauges.

On the Ashton canal, no gauges remained on the top of the house, but on the Peak Forest canal, there were two; one at Marple, and the other at Combs reservoir. At these places, gauges were placed in the ground, and observations upon the fall of rain, as shown by both the old and new gauges, have been regularly made.

The depth of rain in the Rochdale canal district, as shown by the gauges placed in the ground, which, for convenience, we may call the society's gauges, for the year 1844, is as follows:

	Slattocks or Laneside, near Middleton, about 450 feet above the sea. From January 5th, 1844.	Moss Lock, near Rochdale, about 3 miles from the foot of Blackstone Edge, about 500 feet above the sea. January 4th.	Whiteholme Reservoir, summit of Blackstone Edge, probably 1200 feet above the sea. January 11th.	Toll Bar, Blackstone Edge, Easterly side of summit, 900 or 1000 feet above the sea. January 4th.	Black House, about a mile beyond the Toll Bar, and on higher ground. January 4th.	Sowerby Bridge, in the Valley of the Calder, 300 feet above the sea. January 2nd.
	In. tenths.	In. tenths.	In. tenths.	In. tenths.	In. tenths.	In. tenths.
January......	2·0	2·4	1·0	2·3	2·1	1·6
February	3·0	3·7	2·1	2·4	3·2	2·8
March	3·4	4·1	8·2	8·0	8·8	4·4
April.........	1·0	1·0	0·7	1·1	1·0	0·3
May	0·0	0·2	0·0	0·2	0·0	0·0
June	2·0	1·9	2·2	2·0	2·4	2·0
July	3·7	4·2	4·2	4·2	4·0	1·7
August	3·8	3·8	5·2	5·2	5·2	4·3
September...	4·8	3·3	5·0	3·0	2·9	1·3
October......	1·8	2·4	3·0	3·6	3·3	2·4
November...	2·7	2·7		2·0	2·5	3·0
December ...	0·5	0·6		0·2	0·5	0·0
Total.........	28·8	30·3	31·6 for 9 months & 20 days.	34·2	35·9	23·8

In the case of the gauge at the Whiteholme reservoir, the observations are incomplete, only extending from the 11th January to the end of October, the gauge having then become damaged. Previous to the 11th January, the day on which the gauge was fixed in the ground, there had fallen in the vicinity, at the Toll Bar, on Blackstone Edge, $1\frac{3}{10}$ inches of rain, and if the rain for November and December be supposed the same at Whiteholme as at the Toll Bar, viz. $2\frac{2}{10}$, which, from the close approximation in all the other months, cannot be far from the truth, we should have for the year's fall at the Whiteholme reservoir, $35\frac{1}{10}$ inches.

Unfortunately the observations upon the Canal Company's gauges have not been made exactly at the end of the months, nor on the same days as those of the society, and therefore the difference in the *Monthly* falls, as registered by the two guages cannot be conveniently or accurately compared.

The total fall for the year at each place, as received in the Company's gauges, is shown in the following table, in which is also exhibited the average fall for a number of years, as published in the last volume of the Society's Memoirs, together with the total fall, according to the Society's gauges, with the difference in inches, and the excess per cent over the gauges of the Company.

	According to Rochdale Canal Company's Gauges, placed in the ridges of the Lock-keepers' or other houses.	Average fall according to same Gauges.	Number of years of average.	According to the Society's Gauges put down in open situations nearly on a level with the ground.	Difference in Inches.	Excess per cent. over Company's Gauges.
	In. Dec.	In. Dec.		In. 10th	In. Dec.	In. Dec.
Slattocks, or Lane Side near Middleton	18·14	30·83	8 years	28·8	10·66	58·76
Moss Lock, near Rochdale	20·50	29·10	16 ...	30·3	9·80	47· 8
Whiteholme Reservoir	22·64	34·27	21 ...	35·1	12·46	55·
Toll Bar, Blackstone Edge......	23·45	36·35	10 ...	34·2	10·75	45·84
Blackhouse............	24·89	... ...	... ...	35·9	11·01	44·23
Sowerby Bridge......	16·77	27·61	13 ...	23·8	7·03	41·92

It would be impossible to have clearer proof that the Canal Company's gauges do not show the quantity of rain, which has reached the earth in their several localities.

Much as it is to be regretted, that so many years should have been expended in making observations, which have failed to determine the question for which they were first commenced; it is some satisfaction to find, that an approximate result, probably not far from the truth, may be obtained from the ratio which the relative observations during the past year bear to each other.

The limits within which the variation of this ratio is confined, are much narrower than might have been anticipated. For the summit of the hills and the westerly side, (being that most exposed to those winds which are generally accompanied by rain,) the proportion may be taken as two to three, and for the easterly side of the summit at about six to eight.

Applying these proportions to the average fall of rain, as registered by the Company's gauges, we should be justified in assuming the following quantities, as about the depth which would, in average years, reach the surface of the ground.

At Slattocks, ...	about	46	inches	per annum.
At Moss Lock, ...	„	44	„	„
At Whiteholme Reservoir, ...	„	51	„	„
At Toll Bar, Blackstone Edge ...	„	54	and at	
Sowerby Bridge, ...	„	37	inches	per annum.

In like manner the real fall in any particular year may be pretty nearly arrived at.

On the Peak Forest canal, at Marple Top Lock, and at Comb's Reservoir, the returns for 1844, from the old and new gauges are as follow :

Marple, 531 feet above the sea.	Old Gauge, top of the house.	Society's Gauge, 11 inches above the surface of the ground.	Comb's Reservoir, 850 feet above the sea.	Old Gauge, top of the house.	Society's Gauge, 12 inches above the surface of the ground.
	Inches.	Inches.		Inches.	Inches.
January.........	1.56	2.70	January	2.47	3.60
February	.90	2.50	February	1.48	2.90
March	1.60	3.10	March	3.07	5.0
April	.71	1.10	April	.58	1.50
May	.11	.20	May	.10	.50
June	1.15	1.80	June	1.57	3.20
July	2.80	4.10	July	3.67	5.40
August	1.97	3.20	August	3.83	6.50
September	4.21	5.20	September......	2.26	3.70
October.........	1.62	2.50	October.........	2.82	5.10
November	1.79	2.80	November	2.50	4.90
December......	.22	.20	December	.12	.40
Total......	18.64	29.40	Total......	24.47	42.70

The difference at Marple is 57.7 per cent, and at Comb's reservoir 74½ per cent.

Mr. Meadows, who has furnished these returns, has also sent one of the rain fallen during 1844, at the top of the inclined plane of the Peak Forest railway, near Chapel-le-Frith, at which place the gauge has always been placed on the ground. The account is as follows :—

	Ins.	Tenths.
January	3	6
February	2	9
March	4	5
April	1	1
May	0	3
June	1	9
July	4	4
August	4	3
September	2	7
October	3	4
November	3	7
December	0	2
Total	33 Ins.	

The level of this place, which is far in among the hills, is about 1121 feet above the sea, and the rain in 1840, which was about 1-12th below an average year, was 48½ inches.

It may be interesting to add a few particulars of the rain which has fallen during the past excessively dry year at some other places in hilly districts, where observations have been made.

At Bolton-le-Moors, and in the hills above that town, near Belmont, the rain has been as follows; the fall at Bolton being from the return of Mr. H. H. Watson:—

1844.	Bolton, 320 ft. above the sea.	Belmont Bolton Water Works, 850 ft. above the sea.	At Old Lyons, near Belmont.
	Ins. Dec.	Ins. Dec.	Ins. Dec.
January .... } February ...... }	8·96	6·30 }	
		5·90 }	16·00
March	4·23	4·50 }	
April	1·24	1·80	1·30
May	0·20	1·90	0·40
June	2·13	3·30	3·20
July	3·68	3·70	2·80
August	4·80	10·00	9·70
September	4·70	6·40 }	14·70
October	1·89	3·50 }	
November	2·58	2·40	48·10
December	0·22	0·30	To 8th Nov.
	34·63	50·00	

The gauge at Old Lyons is three miles north of that at the water works, and near the summit of the hill at the head of the valley, draining to the Turton and Entwistle reservoir; probably about 1200 or 1300 above the level of the sea.

At Glossop the depth of rain according to the gauge kept by Mr. Kershaw, at Hurst, at the foot of the hills, was, from the 15th December, 1842, to the 14th December, 1843, $47\frac{2}{10}$ inches, and from the 14th December, 1843, to the 31st December, 1844, $36\frac{2}{10}$ inches. The rain here for 1839, which was about 1-12th below Dr. Dalton's average at Manchester, was $44\frac{1}{2}$ inches; and for $10\frac{1}{2}$ months in 1840, also below an average $45\frac{2}{10}$ inches.

The gauges on the tops of the hills above Glossop, alluded to in a former paper, have not been attended to for the last two years.

By the kindness of William Neild, Esq., I am enabled to add the quantity of rain falling at Sandy Vale Print Works, in Dukinfield, near Stalybridge, at the foot of the high hills which rise immediately behind that town. The gauge stands about 320 feet above the level of the sea at low water, the summits of the hills within two miles of the place being 1300 feet.

	Inches.
January	3·14
February	3·44
March	3·91
April	1·34
May	0·18
June	1·04
July	4·13
August	4·31
September	4·83
October	3·02
November	2·81
December	0·18
	32·33

From a return, obligingly sent by Mr. I. F. Miller, of Whitehaven, the fall of rain at that place, and in the lake district, for 1844, appears to be as follows:

At Whitehaven the depth of rain was 36,723 inches, being 2,030 inches under the fall in 1842, and nearly twelve inches under the average.

At Cleator, from the observations of Mr. T. Ainsworth, the fall was 39·31 inches.

At Ennerdale Lake, 54,626 inches; at Keswick, 40,629; Ambleside, 58,828; and at Grasmere, 65,632.

From the observations of Mr. Marshall, at Kendal, the fall at a few other places in the lake district may be supplied. At Kendal the total quantity for 1844 is 43,012, being less than the average by 12,701 inches, and less than any quantity measured in Kendal during the last thirty-six years, except in 1810, when there were 41,417 inches. In 1826, 43,060 inches were taken. At Rampside, near Morecombe Bay, the fall in 1844 was 30·143 ins. Advancing towards the hills at Cartmel, 42·091. At Troutbeck, 51·886; and proceeding to Esthwaite, 58·592. At Ambleside and Grasmere, the quantities are given above. After passing the mountains, the rain diminishes to 28·234 inches at Carlisle.

ON SOME PECULIARITIES

IN THE

MAGNETISM

OF

FERRUGINOUS BODIES.

BY WILLIAM STURGEON,

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&c. &c.

Communicated by PETER CLARE, F.R.A.S.

(Read Nov. 29th and Dec 27th, 1842, and Feb. 21st, 1843.)

1. Some of the various topics that I shall have to notice in this memoir, have previously been touched on, at different times, by several philosophers eminent in this branch of research, each of whom has produced data conducing more or less to the framing of a true theory of magnetism.

2. From the time that the diurnal vicissitudes of terrestrial magnetic action on the compass needle were made known by Celcius and Graham,

about the year 1722,* the attention of philosophers has been directed to this curious subject; and the observations and experiments of Canton, about the year 1758,† gave rise to the idea that the diurnal variations of the needle have a considerable dependance on the vicissitudes of the temperature of the globe, which, if exteriorly produced, must necessarily be associated with the solar calorific rays.

3. Canton's observations were corroborative of those of Graham, who found that the daily *westward* excursions of the needle commenced between eight and nine in the morning, and arrived at a maximum about two in the afternoon, thence receding till next morning, when the needle recommenced its westward advance.

4. Canton's extensive observations, which were carried on throughout every season of the year, enabled him to discover that these diurnal excursions of the needle were least of all in the winter season, and that they gradually increased through the spring months until the latter part of June, in correspondence with the advances of the sun from the southern to the northern tropic. That

* Phil. Trans. Abridged, vol. vii. 27. † Ibid. vol. xi. 421.

in June the daily variation was a maximum, and that it again diminished with the recession of the sun till the latter part of December.

5. From the data thus collected, Canton was led to infer that *solar heat* is probably the chief agent concerned in the production of the daily variation : and he devised a very ingenious experiment to illustrate his views, which, however, it failed to do strictly, for in that experiment he employed magnets uniformly heated, instead of imitating in them the unequal distribution of heat on the surface of the earth.*

6. At a subsequent period Canton made an extensive series of experiments on magnetic needles, by vibrating them at different degrees of temperature, from which he found that an elevation of temperature invariably produced a diminution of power.

* "Canton placed a compass-needle under the influence of two magnets, one on each side. When boiling water was poured on one magnet, the action of the other predominated on the compass-needle. He also exposed one of the magnets to the rays of the sun, whilst the other was screened, and found that the heated magnet lost a portion of its power."—*Phil. Trans.*

7. Professor Christie arrived at similar results by vibrating magnetic needles, alternately exposed to, and screened from, the sun's rays.*

8. Hence it has become an established fact, that a steel magnet, whether large or small, loses some portion of its power by an elevation of temperature, whether heated by the sun's rays, or by other means.

9. The experiments of Canton, however, have a still higher interest, by showing that the heat of boiling water not only produces a loss of power, but that a portion of that loss is permanent; so that the magnet never recovers its original power by the mere act of cooling to the original temperature.

10. I have repeated Canton's experiments, and have found the same results, and have discovered also, that the loss of power, both transient and permanent, varies considerably under certain circumstances, and have met with other curious results, which will be made known to this society in another communication.

* Philosophical Transactions.

11. From the time of Canton's discoveries of the action of heat on magnetic bodies till the year 1819, philosophers had been led to view the influence of any elevation of temperature as detrimental to the full display of the magnetic powers of ferruginous bodies; and, although the idea amounts to a demonstrable fact, as far as previously magnetised steel and loadstone are concerned, Professor Barlow showed, by some highly interesting experiments conducted with great care, about the period last named, that ferruginous bodies generally, whether in the character of steel or iron, when under the influence of terrestrial magnetic forces only, acquire a greater action on the compass needle, when at a blood-red heat, than at any other temperature; but that at a full white heat the magnetic action entirely disappears.*

12. The discovery of electro-magnetism, by professor Œrsted, in 1819, gave M. Ampère an opportunity of supposing that terrestrial magnetism might possibly be due to electric currents circumflowing the body of the globe; but it was not till the discovery of thermo-electricity, by Dr. Seebeck, in 1822, that any idea could be

* Barlow's Magnetic Attractions; and Phil. Trans.

formed of the *source* of those currents which the hypothesis of Ampère required.*

13. Seebeck's discovery, however, tended to establish a probability, at least, that such currents are in operation, and although that discovery has not thrown any new light on the *direct* influence of heat on magnetic action, but only through the *mediate* agency of electricity, it commenced a new and highly interesting era in terrestrial magnetic inquiries.†

14. In a memoir read to the London Electrical Society, in March, 1838, I stated that I had discovered "some novel facts, which, to me, appeared exceedingly important by their throwing a new light on the action of caloric on magnetism."‡

15. In the discovery of some of these facts, I was anticipated by Professor Kuppfer, who, as I

* Father Beccaria, about sixty years before, stated as his opinion, that terrestrial magnetism was due to electricity.—See his work on electricity.

† Mr. Fox, and Mr. Henwood have shown that electric currents exist in the Cornwall mines.—Annals of Electricity, vol. i.

‡ See my "Second Memoir," Transactions of the London Electrical Society; also Annals of Electricity, vol. iv. p. 46.

afterwards learned from Brewster's Treatise on Magnetism, had heated the extremities of a bar magnet, and thus shifted its poles. My experiments, however, were conducted differently from those of Kuppfer, and led to other results than those which the Russian philosopher had discovered. They form the substance of my third memoir, also read before the London Electrical Society, in the same year.*

16. In that memoir I showed that the action of caloric on the poles of a magnet is subject to a general law, which, as regards their transplacement, *longitudinally*, or along the magnetic axis, is *from the point of heat.* In accordance with this law, the magnetic poles may be shifted either in the *same* direction, or in *opposite* directions; and the magnetic axis joining those poles, may be either elongated, or contracted, at pleasure, according to the situation of the point of heat.

17. I also discovered that, after a few repetitions of an elevation of temperature, (by means of a spirit lamp only) the transplacement of the magnetic poles becomes lessened: and, even-

* Annals of Electricity, &c., vol. iv.

tually, would nearly, if not totally, disappear by the same elevation of temperature; so that the magnet would require to be retouched to display the phenomenon again to advantage.

18. In the concluding paragraph of that memoir, I summed up in the following manner:

There appears to be "a certain determinate action which caloric exercises on the poles of a magnet, viz. That the magnetic poles move *from* the point of heat; or in general, that, *the magnetic poles move in the direction of the calorific current.* Should this law become established by future experiments, and if it could be proved experimentally that a current of the calorific matter will move the magnetic poles *laterally* as well as in the direction of their axis, there would be little difficulty in accounting for the revolutions of the terrestrial magnetic poles in their respective latitudes; and I have no doubt, from the results of some experiments which I have made with flat pieces of steel, that the magnetic poles of the earth are susceptible of a *lateral translation* by the direct action of solar heat alone: and by means of a magnetized steel globe, and a spirit lamp, I can readily suppose that the revolutions of the terrestrial magnetic poles might be very beautifully imitated. But I have not, at present, any spare time to devote to this interesting subject; I must, therefore, content myself, till some more favourable opportunity presents itself, with having called the attention of philosophers to this novel mode of investigation, being perfectly aware that there yet remains a rich harvest for those who may venture on the pursuit. In conclusion, I would beg permission to state, that the expansions and contractions of the magnetic axis, as shown by experiments 23, 24, 25 and 26,* appear to me to afford sufficient data for supposing that the terrestrial magnetic axis suffers similar mutations by the direct action of the sun, and that the phenomena of diurnal variation, and change of intensity on the needle, are probably traceable to these secondary causes.

"Should these conjectures be correct, there is not only a cause of *translation* of the terrestrial magnetic poles, but also a cause of *sustentation.* The sun's heat may, possibly, be the *primitive cause* of both: *directly*, the cause of *translation;* and, through the intermediate agency of electricity, *indirectly* the cause of *sustentation.*"

* Annals of Electricity, Magnetism, and Chemistry, vol. iv. Pp. 152, 153.

19. It appears from the above paragraph, that at the time of writing my third memoir, I considered that the *lateral* translation of the magnetic poles was still wanting to complete the series of investigation which I had undertaken: and I have now the satisfaction of stating to this society, that I have been successful in producing the *lateral* translation of the poles, and of observing that they travel *from* the point of heat, in accordance with the same law as is observed by the *longitudinal* translations, or those in the line of the axis.

20. For the purpose of obtaining the *lateral* transplacement of the magnetic poles, by the influence of heat, it appeared necessary to employ a magnet of more than the usual breadth: and my first experiments, in this inquiry, were made on the blade of a saw, previously magnetized. I was perfectly successful in the first trials, producing a *lateral* transplacement of one of the poles, to a sufficient distance from its original position to cause a deviation of the needle amounting to 2°, by the simple application of the flame of a small spirit lamp to one side of the saw, directly opposite to the pole.

21. When the lamp was removed to the other side of the magnetic pole, the latter again receded *from* the point of heat, as was obviously indicated by the retiring of the needle from its last position, which not only arrived at the meridian again, but passed over it to 1°·5.

22. The lamp was again placed in its first position, (20,) and the needle again indicated the lateral motion of the magnetic pole, *from* the point of heat in the blade of the saw, as in the previous cases, (20, 21,) and by frequently placing the flame of the lamp alternately in the two positions, corresponding motions of the needle uniformly indicated that the pole in the saw blade, which occasioned those motions, travelled *from* the point of heat; though the distance passed over lessened gradually, and with an apparent regularity.

23. Notwithstanding the accuracy with which these first results corresponded with the law of transplacement previously discovered in the direction of the magnetic axis,* it was necessary to ascertain its generality, if such existed, in other

* Third Memoir, "Annals of Electricity," &c., vol. iv.

magnetized bodies. But by extending these experimental inquiries I soon met with some of the most perplexing results; which, for awhile, seemed to overthrow all my previous reasoning on the subject; and, certainly, did at the time, cast such a gloom over those expectations which had been occasioned by the uniform display of the phenomena whilst operating with the saw (20, 21, 22,) that I despaired of reducing the anomalies that presented themselves to any law whatever: I therefore abandoned the experiments.

24. The anomalies in question were the occasional *advances* of the disturbed magnetic poles *towards* the point of heat, which were obviously at variance with the law which governs the transplacement in the direction of the axis; and at the time that I abandoned the inquiry, these occasional movements of the magnetic poles seemed, to me, to be totally inexplicable.

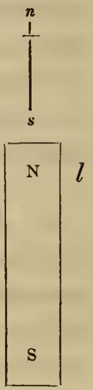
25. Some time afterwards, I ventured on the inquiry again, and by closely pursuing the experiments, through several repetitions, I eventually found that these movements of the magnetic poles *towards* the point of heat were as uniformly dis-

played as those in the opposite direction ; and, what is very remarkable, though the causes of these apparently opposite movements of the poles are in operation during the whole period of the experiment, yet, at certain periods of the heating process, the force which urges the needle in one direction overcomes the force which urges it in the opposite one ; and the needle, obeying the predominating force, moves accordingly.

26. Having now given a general outline of the principal experimental inquiries that have been undertaken with a view of discovering the *direct* influence of caloric on the magnetism of ferruginous bodies, I will proceed somewhat minutely to a detail of those which have, more recently, devolved upon myself.

27. The apparatus employed in these experiments, consisted of magnetized masses of steel of various sizes and figures;—a magnetic compass-needle, two inches long; and a spirit lamp. The needle, *n s* Fig. 1, is first adjusted to the meridian line of the card; and when at rest, the north end of the magnetized steel N S, is placed on the north side of it, with its axis of polarization in the plane of the magnetic meridian; and so adjusted that the pole nearest to the compass keeps the, now inverted, needle also in the magnetic meridian.

Fig. 1.



28. When this arrangement is completed, the flame of the spirit lamp is applied to one side of the magnetized steel plate, N S, as at *l* in the figure. The needle soon removes from the meridian line, indicating a corresponding movement of the adjacent magnetic pole of the plate; which will be *towards* the point of heat, or *from* it, accordingly with the following circumstances.

29. If the magnetized steel plate be thin, say about the thickness of the blade of a small hand-

saw, and not more than two inches wide, the motions of the disturbed pole will, generally, be *from* the point of heat: but as the temper of the metal has much to do with the lateral movements of the poles, a hard temper will sometimes cause a temporary movement *towards* the point of heat, after which the polar transplacements are uniformly *from* the point of heat.

30. When the magnetized plate is hard, more than one-twentieth of an inch thick, and upwards of two inches broad, the first movement of the disturbed pole is almost sure to be *towards* the point of heat; and if the lamp be suddenly removed to the opposite side of that pole, the latter will return to its former position in the steel, pass over that portion, and again approach the point of heat, until the deflected needle indicates a pause.

31. The pole will now move no nearer to the point of heat, but will retrograde a little. If now the spirit lamp be brought to its first position, the needle again indicates a movement of the pole in the steel, which may now be either *towards* the lamp or *from* it, according to the direction of the prevailing force.

32. When the lamp has been shifted from side to side for a few times, that force which urges the needle *towards* the point of heat becomes vanquished; after which the disturbed pole uniformly moves *from* the point of heat, in strict accordance with the law which governs the longitudinal movements.

33. When the magnetized steel plate is of a soft temper, the lateral transplacement of the pole is uniformly *from* the point of heat, in every piece of steel that I have yet operated on; and they have been of various dimensions from one thirtieth, to half an inch in thickness. The latter thickness, however, does not allow of such facilities for polar transplacement as that of one-tenth of an inch and under. I had two pieces of steel made into a peculiar form, by Mr. Dancer, Philosophical Instrument maker, from which I had expected very striking results. Each piece is two and a half inches broad, six and a half inches long, and half an inch thick. They are got up in a very beautiful manner, but the results which I obtained from them were by no means answerable to my expectations. The metal is, in fact, too thick to yield to the influence of the trifling heat of a small spirit lamp, with the necessary rapidity

for the accomplishment of extensive lateral transplacement of the magnetic poles.

34. These are the simple facts which have been developed by an extensive series of experiments; and from which there appears to be the same law in the lateral transplacements (33), as in the longitudinal transplacements (16) when not interrupted by temper of the metal, &c.

35. The other class of facts (30, 31,) which do not accord with the general law, are not the *direct* productions of heat: they are traceable to a *mediate* cause, where laws of action are as uniform as those which govern that class of phenomena which are the *immediate* productions of heat; they at all times interfere with the display of the latter, by urging the needle in the opposite direction, which conceals a portion of the *direct* effects of heat, and sometimes smothers them altogether. The explanation of these counteracting effects being connected with other facts which have come under my notice, and facilitated by means of them, it will be well to defer that explanation until these other facts have been made known.

36. I believe it is generally known that the power of one pole of a magnet is increased by the application of a mass of iron to its other pole; but I am not aware that the other changes which take place in the magnet, by such treatment, have ever been much studied. They are, however, important, and deserve as much attention as any other phenomena displayed by magnetic action. I will here describe some phenomena, of this class, which have come under my own observation, and they will give an idea of the interesting changes which take place in a magnet, by the approach of soft iron.

37. In the first place I may state that, whatever side of a magnet be closely approached by soft iron, a portion of its force is employed on the approaching body; whilst another portion is set free, and ready to operate on other magnetized, or magnetizable matter, properly situated within the sphere of its action. Hence it is, that one pole will suspend more iron, or produce a greater degree of deflection on the compass needle, when a mass of iron approaches the other pole, than when no such iron is present, and when the approaching iron actually comes into contact with

the magnet, the power of the other pole is augmented to the greatest extent.

38. If, by means of a compass needle, we were to ascertain the situation of the unapproached pole, both before and after the application of the iron to the other pole, we should find that the former pole recedes *from* the extremity of the magnet, or retires nearer to the magnet's centre, by the approach of iron to the other pole.

39. Now since the pole of a bar magnet operates more powerfully on a needle by the approach of iron to its other pole, (37), whilst at the same time the former pole recedes from the needle, (38), and consequently operates to disadvantage by an increase of distance, it appears obvious that the absolute gain of deflecting force derived from the approach of iron to the other pole, is much greater than the needle is calculated to indicate. The same reasoning applies to the magnet's lifting power, which, in consequence of an increase of distance of the pole from the iron lifted, does not indicate the extent of power absolutely gained; or rather, of magnetic force liberated by the approach of iron to the other end of the magnet.

40. We next inquire into the changes that take place in that pole of the magnet which the iron approaches; or rather, with which it is in contact: and we find that it advances upon the extremity of the bar. Hence we arrive at another general law, viz. When a mass of iron is brought into contact with a bar magnet, *exterior* to either pole, in the line of the axis, it occasions a transplacement of both poles towards itself.

41. If, instead of placing the iron in contact with that pole of the magnet which is most remote from the needle, we were to place it by the *side* of the nearest pole; the needle would indicate a lateral movement of the latter pole *towards* the iron.

42. With the assistance of these facts developed by the attachment of soft iron to the poles of a magnet, in connexion with the well known fact that the resistance to magnetic excitement in steel is much greater at a hard than at a soft temper, we find a simple and satisfactory explanation of the apparent mystery which the lateral motions of the magnetic poles, *towards* the point of heat, at first involved, (30, 31.)

43. The first applications of the spirit lamp to hard steel, tend to lower its temper, and consequently to lessen the resistance to magnetic excitement on that side of the pole to which the lamp is applied, without producing a corresponding diminution of resistance on the other side. Hence the magnet, under these circumstances, is similarly situated to a magnetic pole approached on one side by a piece of soft iron (41); the polar force will therefore move in that direction, or *towards* the point of heat. But when this resistance becomes so far diminished on every side of the pole, by the frequent application of the lamp warming and softening the steel, as to be overcome by the *direct* calorific force, the latter force prevails in producing the polar displacement; which is invariably *from* the point of heat. When the steel is soft and thin, the resistance of the metal is easily overcome, and the polar transplacement *from* the point of heat, is accomplished even by the first applications of the lamp.

44. One of the peculiarities attending *longitudinal* polar transplacement by heat, is, as I have shown in my third Memoir,* a diminution of space passed over by the disturbed poles on each suc-

* Annals of Electricity, &c. vol. iv. p. 144.

cessive application of the lamp: so that by frequently disturbing the poles by this process, the polar movements are much lessened. They are very conspicuous when the steel has been newly touched, but become much reduced by frequent applications of the lamp, if not occasionally retouched.

45. The *lateral* polar movements, by heat are, also, much lessened by frequent applications of the lamp; and in this respect, appear to be subject to the same law as the longitudinal movements; the greatest range being displayed on a newly touched magnet, and least of all by the last application of the lamp.

46. From the above facts we are led to infer that, if the sun's heat be the *direct* cause of diurnal variation, the excursions of the needle will lessen by time, unless there be some sustaining force in operation; and, therefore, are less now than formerly. This view of the subject presents one of the most important problems in terrestrial magnetism, especially when taken in connexion with the probability of the daily westerly excursions of the needle being greater than the space passed over by the eastward retrograde movements, and thus leaving a daily increment of westward

advance. According to this view, the period of revolution of the magnetic poles, in their respective orbits, would depend upon the magnitude of the diurnal increments; and if these lessen by time, the periodic times of revolution would lengthen in the same ratio. Hence, since accurate chronological tables of the *extent* of the diurnal variation for a long series of years, are the only data that can furnish the necessary information, no available means should be left unemployed to collect data of such essential importance.

47. I hope shortly to submit to the consideration of this society, other novel facts at present in my possession; from which I have every reason to believe, that if there be no sustaining force, the *intensity* of terrestrial magnetic action is considerably less than it was at an early period of the world.

Note added.—In the Report of the 12th Meeting of the British Association for the Advancement of Science, printed in 1843, is the following statement. "It would appear (if earlier observations can be relied on) that the line of least intensity on successive meridians, is travelling rapidly northwards." (P. 2). May not this transplacement of the poles be owing to solar heat?

SECTION II.

(Read December 27th, 1842.)

48. In the first Section of this Memoir I had occasion to allude to the excellent experiments of Professor Barlow, on the magnetic action of heated iron (11). In the year 1827, I repeated those experiments; and, with the exception of a peculiar phenomenon displayed in the Professor's inquiries, I met with similar results.

49. At that time I was also engaged in a series of inquiries in Electro-Magnetism, especially respecting the action of hollow ferruginous magnets;* and as I succeeded in producing the same degree of deflecting power by *hollow* as by solid pieces of iron of the same form and dimensions, when magnetized by electric currents, it occurred to me that the magnetic action of iron tubes, at a high temperature, presented an exceedingly interesting problem, which, till then, had never been noticed.

* Annals of Electricity, &c. Vol. i. p. 472.

50. The only hollow iron I then had at command consisted of a few pieces of an old musket barrel, which had previously been cut for the purpose of forming a voltaic battery; and subsequently employed in the capacity of electro-magnets, in the series of inquiries then carrying on.

51. One of the pieces, about a foot in length, was heated to as high a temperature as an ordinary house fire was capable of raising it to; at which temperature, when placed in the line of the dip, its magnetism entirely disappeared; but returned as the temperature subsided, and eventually displayed a higher degree of magnetic action on a compass-needle than it had shown previously to the heating process.

52. With this fact, however, the inquiry rested for that time, the other proposed experiments being laid aside to give place to the electro-magnetic inquiries then going on; and it was not till arranging the former section of this memoir, that my attention was again directed to the magnetism of ferruginous bodies at high temperatures.

53. As my first repetition of Professor Barlow's experiments had been undertaken on a much smaller

scale than the original ones, I was desirous of repeating them, and also that with the hollow tube, on masses of iron much larger than those I had previously employed. Through the favour of Mr. Richard Roberts, to whom I made known my desire of carrying on these inquiries on a larger scale than I had any opportunity of pursuing them before, I have been enabled to make the necessary experiments under the best possible advantage; and I am happy to say, with the most satisfactory results.

54. The experiments were made at the *Atlas* works, belonging to Messrs. Sharp and Roberts, engineers, on Saturday the 24th instant, in the following manner.

55. The first series of experiments were with an iron tube, two feet long, and two inches in external diameter; the thickness of the metal being about two-tenths of an inch. The tube, when under examination, was placed in the line of magnetic dip; having its lower extremity resting in a cavity, purposely made, in a fire brick, and its upper extremity leaning against a tall brick tile, placed in a vertical position for the purpose. A magnetic compass, whose needle is

two inches long, was placed on the top of the tall tile, nearly on a level with the upper end of the iron tube, and six inches distant on the east side of it.

56. Prior to placing the iron tube in this position, and in the absence of all local magnetic action, the meridian line of the compass card was adjusted to the magnetic meridian of the place where the experiments were carried on.

57. When the needle had come to rest, the iron tube, whilst cold, was placed in its proper position (55), and the deflection of the needle, due to the magnetic action of the upper end, noted. The tube was then inverted, and again the deflection noted. By these means the magnetic action exercised on the needle, by the different ends of the hollow bar, prior to heating it, was ascertained; and the mean action of both ends easily determined.

58. The tube was next heated to whiteness, (not to a welding heat), and in that condition replaced in its former position (55). The needle was carefully watched, and its indications strictly noted. In the first trials the needle was much agitated

during the time that the hot iron tube was being adjusted in its proper place, but soon afterwards came to rest at zero, indicating an entire absence of magnetic action in the iron; which did not reappear until the temperature had subsided so as to display a moderate degree of redness on the surface of the tube.

59. The needle first indicated a magnetic action in the iron tube by a slow movement of its north end towards it, but shortly afterwards its motion became rapid, until it arrived at its maximum of deflection, which occurred when the iron had fallen to a dull red heat. This maximum of deflection continued for a considerable time after the iron had cooled to blackness; and was not seen to lessen whilst the iron remained unmolested.

60. The agitation of the needle during the adjustment of the hot tube in the first experiment (58), was found to be occasioned by the comparatively cold tongs, by which the upper end of the iron was held; and was avoided in all the subsequent experiments by applying the tongs to the middle part of the hot iron: under which circumstances not the slightest movement of the needle

was observable until the heat of the iron had subsided to that stage of redness already described (59).

61. Whilst the experiments with the tube were going on, two solid cylindrical bars of malleable iron, of precisely the same dimensions as the tube, were prepared, for the purpose of making comparative experiments, and ascertaining the difference, if any, of magnetic action of solid, and hollow iron, at high temperatures. The experiments with these solid bars were pursued in precisely the same manner as those with the hollow bars, and the results were almost exactly the same, as is evinced by the following tabulated arrangement of them:—

62. One end of each bar was marked A, and the other end B, for convenience of comparison and explanation.

Experiment 1.

Iron Tube Cold.	End A,	deflection	20°	mean 21°·5.
	End B,	,,	23°	

White Hot ...... zero. Blood-red ...... 35°.
Cooled to Blackness .... 35°.

Experiment 2.

Iron Tube Cold.	End A,	not ascertained.
	End B,	do.

White Hot ...... zero. Blood-red ...... 35°.
Cooled to Blackness .... 35°.

Experiment 3.

No. 1, Solid Iron Bar. Cold	End A, deflection	35°	mean 29°.
	End B, ,,	23°	

White Hot ...... żero. Blood-red ...... 35°.
Cooled to Blackness .... 35°.

Remark.—This bar was neutral nearly four minutes. The needle then moved gently to about 5°. Its subsequent movement was very rapid till it arrived at 35°. The end A, which gave a deflection of 35° whilst cold, had acquired some fixed polarity by the process of being cut by a chisel, from a long bar.

Experiment 4.

No. 2, Solid Iron Bar. Cold	End A, deflection	23°	mean 22°.
	End B, ,,	21°	

White Hot ...... zero.   Blood-red ...... 35°.
Cooled to Blackness .... 35°.

EXPERIMENT 5.

No. 1, Solid Iron Bar Cold.	End A, deflection 22°	22°·25.
	End B, ,, 22°·5	

White Hot ...... zero.   Blood-red ...... 35°.
Cooled to Blackness .... 35°.

63. The general results of these experiments differ in no respect from those obtained by Mr. Barlow, as far as relates to the neutrality of iron when highly heated, and its acquiring a maximum of magnetic action at a certain degree of temperature. But we do not learn from that philosopher's account of his own experiments, that the iron is neutral at a *bright red* heat. The results now detailed, however, have shown that the iron is perfectly neutral even at a moderate degree of redness; and it is not till the temperature falls to a *dull* red heat, that the needle indicates the slightest movement from the meridian. It first moves slowly, till it arrives at about 5° of deflection; stands fixed for a moment, and then travels rapidly to nearly the maximum point.

64. The experiments with the hollow iron, (51—62,) are, I believe, the only ones of the kind hitherto made. They are interesting on two accounts. They show that the magnetism of hollow iron bars at high temperatures, observes the same laws as solid bars of the same dimensions, when similarly treated; and they also show that the *extent* of magnetic action, under these circumstances, is precisely the same both in hollow and solid iron. This latter fact is in strict accordance with the results obtained by the action of cold iron, as shown by another interesting series of Mr. Barlow's experiments.*

65. During the time that one of the solid bars remained neutral, it occurred to me that there still existed a possibility of some slight magnetic action being present which could not be detected by the needle at the distance of six inches. I therefore caused the hot bar to be placed with its upper end at the distance of three inches from the needle, but still no magnetic effect was produced; nor do I think that any magnetic action would be detected even though the needle were placed close to the heated bar.

* Barlow's Magnetic Attractions.

66. I believe that neither Mr. Barlow, nor any other philosopher has hitherto ventured an opinion as to the cause of the *total absence* of magnetic action in ferruginous bodies when at a high temperature. Nor has any one, as far as I know, attempted to explain the cause of the exalted magnetic action which iron displays whilst cooling from that high temperature.

67. It is well known that I have, for several years past, entertained the idea that the electric, the magnetic, and the calorific matter, are perfectly distinct elements of nature; and that each possesses attributes peculiar to itself. Moreover, I am of opinion that I have proved by the clearest experimental evidence, that electricity differs essentially, both from the matter of heat and from magnetism.* And if I do not deceive myself in the views which I take from the results of the experiments detailed in this memoir, they are particularly favourable to the opinion which I have entertained respecting the distinction between magnetism and caloric.

* Transactions of the London Electrical Society. Also my Second and Third Memoirs, "Annals of Electricity." Vol. IV.

68. The grand axiom in all reasonings of this nature is simply this :—*No two particles of matter can occupy the same place at one and the same time.* By keeping this axiom in view we are led easily to understand that either *additions* or *abstractions* of the calorific matter, to or from any body, disturb both the electric and magnetic equilibrium, if such previously prevailed, in that body. It is thus, in fact, that our thermo-electric, and thermo-magnetic phenomena are produced ; and that reciprocally, calorific phenomena as decidedly emanate from magnetic and electric disturbance or excitement.

69. Now, although the display of heat by magnetic excitement, and the display of magnetism by calorific excitement, are, in many cases, entangled by electric agency, which plays an intermediate part in the process ; it is a well established fact that additional portions of calorific matter forced into the substance of a magnet, disturb its powers by direct action ; and it has been shown in the first section of this memoir, that the magnetic poles are absolutely transplaced by the *direct* action of heat, which invariably repels these poles from itself; showing that, as heat is introduced to any particular part of the steel bar, the mag-

netism of it is displaced, and partially removed from that part of the ferruginous mass.

70. Since then, such a notable displacement of the magnetic poles can be accomplished by the heat of a small spirit lamp, it is but reasonable to infer, that, by the introduction of a much larger quantity of the calorific matter, such as would raise the iron to a white heat, a complete and entire expulsion of the magnetic matter naturally belonging to the iron, might be accomplished.

71. Such, indeed, are the theoretical views which I have taken in explanation of the whole of these, properly called, thermo-magnetic phenomena, in which electricity is not an intermediate agent. Whether these views will meet the sanction of philosophers, or not, time alone must determine. But as no previous attempt has been made to show why iron is perfectly neutral at high temperatures, the hypothesis has, at least, the singular advantage of not being contravened by any other.

72. With respect to the iron arriving at its greatest degree of magnetic action at a low red heat, I am far from supposing that this superior

display of magnetic powers is due to the *direct* influence of the calorific matter. I would rather refer it to the molecular movements of the metallic integrants of the bar, whilst contracting, and at a time when the magnetic matter is in the best possible condition for receiving polarization, viz. whilst re-entering the bar, and consequently in a state of motion.

73. That agitation of a ferruginous mass, and consequently of its contained magnetic particles, facilitates its polarization, is a fact long ago established: and to such a degree of importance has this simple fact arisen, that philosophers are now accusing it of being the chief agent in producing disruption of the iron axles of railway carriages, whilst travelling at a great speed.

74. I, however, do not blame any magnetic action as being either directly or indirectly concerned in these unfortunate occurrences: nor can I learn that any experimental results have hitherto been obtained, that could give rise to the idea that there is even a possibility of magnetism weakening the cohesive powers of iron: but on the contrary, from some inquiries that I have made on this topic, I am not without hopes of

proving, that magnetism, properly applied, may be made a powerful auxiliary in giving strength to iron bars.*

75. Although it has long been known that agitation facilitates the polarization of ferruginous bodies when under the influence of a standard magnetic force, such as that of the earth, I am not aware that any experimental inquiries have hitherto been made with a view to ascertain the *extent* of polarization that different kinds of iron and steel are susceptible of receiving by that simple process. But, in order to ascertain how far the views I have taken, respecting the polarization of ferruginous bodies, whilst cooling from high temperatures, accord with facts, I took an opportunity of making a few trials on those bars, both solid and hollow, which had been employed in the experiments already detailed.

* It being well known that electric currents are capable of keeping together two distinct masses of iron, with a force equal to many tons; why, I would ask, should not the same power keep together the particles of a single piece of iron, with an equal force, in addition to the force of cohesion. Some experiments which I have made, seem to sanction this view.

76. When the bars had become quite cold, and after they had lain for a considerable time horizontally on the floor, they were severally held by the hand in the line of the dip; and whilst in that position agitated by blows from a wooden mallet. This operation being performed on an individual bar, it was afterwards placed in the same position as it was whilst hot, in the previous experiments, (54—62); the needle being east, at the distance of six inches, as before. When the magnetic action of the upper end A, of the bar had been indicated by the needle's deflection, it was inverted; and the action of the *now* upper end B, ascertained in like manner.

77. The following are the results of the experiments in which the bars were agitated by a series of blows with a wooden mallet, whilst held loosely with their axis in the line of the magnetic dip. The magnetic action of the *upper ends* of the bars, was alone ascertained.

EXPERIMENT 6.

No. 2, Solid Iron Bar, Cold & unagitated	End A, deflection 26°	mean
	End B, ,, 25°	25°·5.

Same Bar agitated. End B uppermost.	End B, deflection 34°	mean 34°·5.
Inverted and again agitated	End A, deflection 35°	

EXPERIMENT 7.

Hollow Iron Bar, Cold & unagitated	End A, deflection 27°	mean 27°·25.
	End B, „ 27°·5	

Same Bar agitated. End B uppermost	End B, deflection 33°	mean 33°·5.
Inverted, and again agitated	End A, deflection 34°	

78. By comparing the results obtained by the simple process of agitation with those which the iron gave by cooling from a high temperature, the difference will be observed to be so inconsiderable, as to show that the magnetic action due to the latter, is not much superior to that due to the former process; nor indeed at all more so than may be fairly accounted for by the molecular movements within the ferruginous masses employed (72).

79. In pursuing the views I had already taken respecting the complete expulsion of the magnetic matter by means of heat, (66-71) it appeared likely that a bar of iron might be converted into a series of distinct magnets by heating it to a high temperature in several different parts, and permitting the intervening parts to remain comparatively cool, or below that temperature at which magnetic action was complete, in the previously conducted experiments: (55-62), and being desirous of embodying in this memoir all the facts I could collect on this topic, I called on Mr. Fothergill, who had assisted me in all the previous experiments, about one o'clock this afternoon, to request his assistance again in procuring me a few more heats of the iron. This request was readily assented to, and the following experiments were made, at the Atlas Works, after four o'clock this evening.

Experiment 8.

80. The solid iron bar, No. 1, two feet long, was heated to a high degree of redness at one of its ends to nearly the middle of the bar, whilst the other part was kept comparatively cool, and afterwards still further cooled by immersion in

the slake trough. The bar, thus treated, was then placed in the line of the dip, as in former experiments, with its heated end uppermost, having the compass needle at the distance of six inches on the eastern side.

The hot end showed no magnetic action whatever. I now requested the smith who assisted me, to raise the bar till nearly the whole of the heated part was above the compass. In this position the north end of the needle was drawn towards the bar about 25°, indicating the presence of a pole of the opposite kind.

The character and situation of this pole being ascertained, the bar was again gradually raised, till its lower extremity was level with the compass. The deflection of the needle at first declined, and gradually returned to the meridian, as the centre, or nearly the centre, of the cold part was raised to its own level. As the bar ascended higher, the declination of the needle was again shown, but in the contrary direction to the former; and when the lower end of the bar had reached the level of the needle, the south end of the latter was drawn towards it about 27°

81. The results of the above experiment (80) showed, in the most decisive manner, that the cool part of the iron was a perfect magnet, whilst the heated part was as perfectly neutral.

Experiment 9.

82. In this experiment the solid cylindrical iron bar, No. 2, was heated in the middle part, and kept cool at both ends. In this condition the bar, when placed in the magnetic dip, displayed four magnetic poles indicative of two distinct magnets, one at each end of the bar; whilst the intervening heated part was perfectly neutral. The poles exhibited by the cool parts of the iron were of the same character as those which would have been displayed by them had they been two distinct pieces, without the intervention of the heated part, and the heated part entirely removed.

Experiment 10.

83. A long iron bar was now heated, to a bright redness, in two distinct parts, leaving both extremities, and a portion in the middle, quite cool. The bar having been placed in the line of the

dip, displayed three distinct and perfect magnets, at its three cold parts; each having its proper poles as if a distinct and separate piece of iron. The heated parts were perfectly neutral.

84. In the class of experiments last described, (80-83), the poles adjoining the heated parts of the iron, began to advance upon those parts as they gradually cooled: so that in experiment 8, the moveable pole advanced to the extremity of the bar; and in experiments 9 and 10, the moveable poles advanced upon one another; and eventually merged their forces and were lost. Hence just above a black heat, each had become one distinct magnet, as though it had been wholly heated; and thus it remained whilst kept undisturbed.

85. I have already adverted to a peculiar phenomenon occasionally observed by Mr. Barlow, whilst carrying on his experiments on heated iron (48). The phenomenon in question was a deflection of the needle in the opposite direction to that which it would take according to the ordinary magnetic state of the iron. It was observed when the bars were at a bright red heat, and would continue some seconds before the needle returned to the meridian. The usual, or *positive*

magnetic action, would then begin to be manifested, and the needle would travel rapidly to its ultimate position.*

86. I have anxiously watched for the phenomenon alluded to, during the progress of several of the experiments already detailed, but in no instance have I been enabled to detect it. Mr. Fothergill, however, to whom I made known my object, has informed me that he observed it, in an experiment carried on by himself, in my absence.

87. "The only explanation" says Mr. Barlow, "which seems to present itself of the cause of this anomalous action, is that, the bar cooling faster at its extremities than at its centre, one part of it becomes magnetic before the other, and hence gives rise to the irregular action above indicated. It must be acknowledged, however, that this explanation does not meet entirely all the phenomena recorded in the preceding table."†

* Barlow's "Magnetic Attractions," second Edition. 1823. Page 146.

† The table alluded to is a table of the results of an extensive series of experiments; in many of which the anomaly in question presented itself.

88. The experiments last detailed in this section, (80-83), appear to me to be well calculated to give a clue to the cause of the anomalies which Mr. Barlow met with; and, although the idea of the bar's "cooling faster at its extremities than at its centre," did not appear to be a satisfactory explanation to that philosopher; there appears to be a strong probability, at least, that that circumstance was the sole cause; for, by such a cooling process, the bar would be converted into two distinct magnets, as in experiment 9, (82); in which condition it would operate on the needle in correspondence with the apparent anomalies in question, which would thus be traced to the well established laws of magnetic action.

89. Having now placed before this society the experimental results I have arrived at, and the views I have taken in explanation of the phenomena, it only remains that I acknowledge the very handsome manner in which my inquires have been facilitated through the kindness of Mr. Roberts; and the able assistance I received from Mr. Fothergill, in carrying on the experiments.

SECTION III.

(Read February 21st, 1843.)

90. The results which I have arrived at, whilst repeating the interesting experiments on highly heated iron, first instituted by Mr. Barlow, and others already detailed in a former section of this memoir, have shown in the most satisfactory manner, that the magnetic forces of the earth, in this latitude at least, have no power whatever in rendering highly heated iron magnetic; whether that iron be in solid masses, or in the shape of hollow tubes.

91. I have already taken advantage of this important fact, as being favourable to my views, respecting the entire expulsion of the magnetic matter naturally belonging to the iron, by the introduction of so great a quantity of calorific matter as was forced into the metal by the process of heating. But as the polarizing powers of terrestrial magnetism are exceedingly feeble

when compared to those which we have at command by the employment of artificial magnets, and electric currents; and as it seemed possible that, although no polarity of highly heated iron could be detected from the polarizing influence of the earth, yet a much higher polarizing power might develop some trace of magnetic action in the iron, even at the highest degree of temperature at which it could be kept in a solid state; and as no hypothesis ought to be ventured on the philosophical world, which rests on doubtful, or even partial, experimental data, it was easy to perceive the propriety of extending the experiments for the purpose of ascertaining what results could be derived from the employment of a much more powerful polarizing force than that of terrestrial magnetism.

92. In order to carry out these inquiries in the most satisfactory manner possible, I first ascertained the entire neutrality of a heated bar of square iron, six inches long, and 0·6 of an inch broad, when subjected to terrestrial magnetic action in the line of the dip, as in some of the experiments already detailed.

93. As it was necessary to have some idea of

the magnetic forces which the iron bar, whilst cold, and terrestrially magnetized, exercised on the needle, compared with that which it would display whilst under the influence of a powerful system of bar magnets employed in the investigation, I had recourse to the method of vibration.

94. The vibrations of the needle by the force of terrestrial magnetism alone, independently of the presence of the iron, were ascertained to be nearly at the rate of one per second of time ; and, in order to facilitate calculation, they were, by means of a counteracting magnet, reduced to the same standard, when under the influence of the system of bar-magnets placed horizontally in the magnetic north of the needle ; the pivot of the latter being eight inches distant from the vicinal pole. The compass-box was elevated on a brick about four inches and a half above the level of the system of magnets.

95. The iron bar was now placed with one end on the system of magnets, and the other end resting on the edges of the brick, with its axis in the magnetic meridian. The vicinal end of the iron was two inches from the pivot of the needle,

the north pole of which was drawn forcibly towards it.

96. Under these circumstances, the needle was made to vibrate, and the force was such as to cause about thirteen or fourteen vibrations per second. The vibrations were so rapid, however, that it was difficult to count them with precision; but by taking notice of the appearance of the needle on one side of the meridian only, the difficulty of counting is much lessened; and by this means I was enabled to ascertain that the *double* vibrations amounted to more than six per second: sometimes there appeared to be nearly seven per second: but as no great exactness is required in cases of this kind, I have allowed six double or twelve single vibrations per second, which is a little below the real number which the needle performed whilst under the influence of the magnetic force of the iron, when thus in contact with the system of bar-magnets.

97. The magnets being removed, and the needle again at liberty, the iron was placed with its axis in the magnetic dip, having its upper end resting on the brick at the same distance from the needle as in the previous experiment. The

needle was now vibrated under the dominion of both the magnetic force of the terrestrially polarized iron, and that of the earth itself, and was found to perform 1·5 vibrations per second.

98. Having thus obtained the rate of vibration under these three different circumstances, it was easy to ascertain the ratio of the magnetic force exercised by the iron, whilst under the influence of the steel bars, and whilst under the influence of terrestrial magnetism alone. The ratio of the vibrations being as 12 : 1·5, whilst the iron acted in concert with the force of terrestrial magnetism; and the latter alone producing one vibration in the same interval of time; the ratio of the forces of the iron, under the influence of the bar magnets, and of the earth is as $(12^2 - 1^2) : (1 \cdot 5^2 - 1^2,)$ or 114 : 1 nearly.

99. This ratio ascertained, the system of magnets and the counteracting magnet, were replaced, so as to give permission to the needle to swing as if under the force of terrestrial magnetism only. The needle was now drawn 90° from the magnetic meridian, by means of a small magnet, and permitted to rest in that position whilst the iron was heating.

100. When the iron had attained a bright red heat, approaching to whiteness, it was placed in its former position with respect to the system of magnets and the compass-needle; having one end resting on the magnets and the other on the edge of the brick. The needle was carefully watched; but not a movement of it was observed whilst the iron was at a high red heat. As the iron cooled, the needle began to yield to its commencing magnetic action; at first its movement was very slow, but in a short time it advanced rapidly towards the meridian, and finally settled in that plane. In this experiment, as in those in which terrestrial magnetic influence alone was employed, the iron evinced a decided neutrality, whilst at a high temperature: and the general features of the phenomena, under both circumstances, were precisely alike.

101. This experiment was repeated several times with the same results; and although it might appear to be sufficiently demonstrative of the entire absence of magnetic action in highly heated iron, yet as I thought that I perceived the magnetic action appear at a higher degree of temperature in these experiments than in those in which the terrestrial magnetic force alone was

employed, it seemed prudent to augment the polarizing force still further. I therefore nearly doubled the polarizing force last used, in some subsequent experiments, but in no case could magnetic action be detected when the iron assumed a high red heat.

102. I next subjected cylindrical bars of wrought iron, highly heated, to the action of powerful electric currents, from a voltaic battery consisting of eight pairs of cast iron jars and amalgamated zinc plates, arranged in a series of four double pairs, and excited by a strong solution of sulphuric acid. The iron bars were placed in a brass tube enclosed in a spiral conductor, from which it was insulated by intervening stout brown paper. But, although the iron bars, when cold, would display a powerful attractive force by virtue of the electro-magnetic action of similar currents, not a trace of magnetic action on a delicately suspended magnetic needle, beyond that due to the spiral, could be discovered when the iron was at a high red heat. But in these experiments, as in the former, the iron gradually resumed its magnetic functions as it cooled down towards blackness.

103. The results of the several series of experiments, hitherto enumerated, appear to me to be conclusive evidence regarding the entire absence of magnetic powers in ferruginous bodies, when highly charged with calorific matter; and I cannot avoid regarding them in the important capacity of so many experimental demonstrations of the calorific matter, being capable of *expelling* the whole of the magnetic matter from ferruginous bodies; and, consequently, as unexceptionable evidence of the perfect distinction of their physical characters and attributes.

104. Whilst these investigations were going on, others, no less important, presented themselves to my mind, and the experimental results happened to correspond with the views I had taken. They are perfectly novel; and, I believe, were never before thought of by any other experimenter.

105. The facts on which I reasoned were these, 1st. That a bar of iron, as I have shown, (80-83), can be formed into several distinct magnets, by the introduction of calorific matter to intervening portions of the iron; the whole bar being under the feeble polarizing force of terrestrial magnetism. And 2nd. That, as has long been known, every

individual fragment of a broken magnetic bar of steel becomes a perfect magnet; it appeared to me very likely, that instead of employing mechanical violence to break a bar magnet into several smaller ones, the same end might be accomplished by the agency of heat; and that a magnetized steel bar, heated in various places, might be converted into as many distinct magnets as there were parts of it left in a comparatively cool condition.

106. My first experiment, in this new inquiry, was made on a bar magnet about eight inches long, one inch broad, and a quarter of an inch thick. I re-touched this bar previously to heating it, and ascertained the position of its poles, which were found to be nearly a quarter of an inch from its ends.

107. One end of this bar was now placed in the fire, so as to render more than one-third of its length red hot. When the heat seemed to be high enough, the bar was withdrawn from the fire, and placed with its cold part on a brick. In this condition a compass-needle was presented to various parts of it, and indicated that the heated

end was perfectly neutral; and that the cold part was a perfect magnet, having one of its poles at the extreme cold end of the bar, and the other pole distinctly marked where the metal began to be red hot.

108. As the metal cooled the latter pole advanced on the heated part, and eventually assumed a diffused character, similar to that shown by that end of soft iron which is attached to the pole of any bar magnet.

109. My next experiment was on a bar magnet about sixteen inches and a half long, three quarters of an inch broad, and half an inch thick. This bar was heated in the middle part, whilst the two extremities, each about one-third of the whole length, were kept as cool as possible by frequent applications of cold water. When the central part had arrived at a bright redness, the bar was taken from the fire and placed on two bricks, one under each end. Thus situated, it was examined by a compass-needle, and found to be converted into two distinct magnets, one at each end or cool part, and separated from each other by the central heated part, which was perfectly neutral.

110. These two experiments were the only ones made in this inquiry; for, as they had produced the results I had expected, and thus verified the theoretical views I had entertained, it was easy to predict that any bar magnet, of sufficient length, might be made to display as many distinct and perfect magnets as we please, by the influence of heat alone, when applied in a similar manner.

111. The difference in the mechanical, and the calorific modes of subdividing a magnet into several smaller ones, consists in the subdivision of the metal in the former case, and the subdivision of the magnet only, in the latter. By the former process, however, the new poles are permanently fixed in the steel; whereas by the latter, they are transiently displayed, and change their situation as the metal cools. Eventually the heated parts, after cooling, assume a diffused polarity.

ON THE

ANALOGIES AND AFFINITIES

BETWEEN THE

ANCIENT AND MODERN LANGUAGES

OF THE

SOUTH OF EUROPE

AND

THOSE OF THE NORTH.

BY F. E. VEMBERGUE.

(Read the 4th of February, 1845.)

The mathematician who has just solved some difficult problem, the chemist who has just discovered some new property of matter, can alone have an idea of the delight felt by the ethnographer or the etymologist, when he finds out some new analogy between one language and another.

Discoveries may be more or less useful, more or less susceptible of extending the boundaries of human knowledge; but there is no doubt that every step in science or learning must, in various degrees, be conducive to the pleasure and happiness of mankind.

Ethnography and etymology may appear to some men unprofitable pursuits; yet it cannot be denied that they have thrown an invaluable light on the history of man, and that it is only since this light has been applied, that historians have been enabled to plunge boldly and safely into the darkness of antiquity. Thanks to it, we know whence we have sprung up; we know who are our fathers and brothers; we know the various degrees of kindred and relationship which exist between nations, and this knowledge must, in the end, tend to eradicate, or at least to soften, national antipathies and prejudices.

There are two nations in western Europe between which a spirit of rivalry has long prevailed. Political causes, originally arising from the ambition or private quarrels of princes, have, no doubt, partly contributed to this reciprocal jealousy; yet, would these unnatural feelings be so strong, if it were generally known that a close relationship exists between the two people? If England was invaded by the Danes and the Saxons, France was overspread at different periods by Northmen, Goths, Burgundians, and other Teutonic tribes. If Albion received its modern name from the Angli, Gaul owes its present name to the Franks.

I propose in the following researches to show the analogies which exist between the græco-latin languages and those of the Teutonic family; to explain certain permutations which take place in letters, and the various ways in which words are formed from other words.

We must bear in mind that we have received from nature six organs, intended, amongst other functions, to vary the vowels or simple sounds formed by more or less opening the mouth, viz: the lips, the teeth, the palate, the tongue, the throat and the nose; hence consonants are labial, dental, guttural, &c. That when a word passes from one country to another, a letter of one of these organs is frequently substituted for another letter of the same organ, and that articulations or consonants are often added, dropped or transposed. We must also recollect that vowels are not essential parts of a word—consonants alone constitute the basis or root. Two examples will suffice to show what extraordinary liberties are taken in the transformation of words. There is apparently very little resemblance between "Bishop" (Bischof) and "évêque," yet these two words have the same origin, viz: the latin word "episcopus." Again, "head" and "chef" seem totally different; I believe them, however, to be identical, and to be both derived from, or related to, the latin "caput." The Germans, by a process which shall have our attention presently, changed this word into "Haupt;" hence "head." The French, by another almost invariable process exemplified in "cher" from "carus," "cheval" from "cavallus," changed the c into ch, and softened pt into f.*

I have said that consonants are the roots of words. There are certain consonants which serve in most languages to express a certain situation. We find first of all S T, which invariably convey

* The German and English words which are purely latin or French, as amusiren, Nation, science, facility, &c., are, of course, out of the question in the present paper.

In order to avoid confusion, Anglo-saxon, Greek, Spanish, Italian, and French words will only be introduced when the modern German and the latin shall not present the looked-for analogy.

the idea of a fixed state. Sanskrit: stidaha, (to stand); Persian: istad; Greek: istaó, stélé (pillar); German: stehen; Latin: stare, stella; star, style, &c.*

F L. in the Teutonic and græco-latin branches express something fluid: Fluo, Flumen, Fluss, fliessen, to flow.

B R, signify to carry. St. Baradi; Gr.: pheró; L. fero; G: behren, to bear. I have no doubt, bringen, to bring, comes from the same root.

The same consonants B R, followed by a dental convey the idea of brotherhood. Ex: St. Bhratri or Brader, Pn.: Brader; G: Brüder; L: Frater. &c.

The harsh labials F, P and M, with a dental, invariably express other kinds of relationship; St.: Pidra; Pn.: Peder; Gr. and L: Pater; G: Vater, Father; St: Madra; Pn.: Mader; Gr: Métér; L: Mater; G: Mutter, Mother.

B or P R, preceded by a vowel, generally

* Owing to the want of Greek and German types, Greek and German words will be spelled with Roman characters, and the éta will be distinguished from the epsilon, the ómega from the omikron by the accent.

denote superposition. Ex: (צבי) ibr, in chaldean (over); uper, in Greek, über, in German. The Latins have added an S.: super; Fr: sur; su or sovra in Italian.

The T and the R, conjointlyor separately, are universally used to express noise or the effects of noise on the senses; Tonitru, tonare, tonnerre, Donner, thunder; tremor, zittern, to tremble.

The dentals T or D, preceded by a vowel, mean eating: St.: Ada; Gr. edó; L.: edo, I eat; G: essen. The S and T are often substituted to one another in all languages.

It is curious to observe how words, in passing from one language to another, frequently vary the meaning of the root. From the German "tragen" to carry, the French and the English have formed two verbs, tirer and traîner, to draw and to drag. Of the Sanskrit Hasta, (hand) the Latins have made a lance, just as of the latin Arma the Teutons formed the Arm; Vulpis, a fox, is changed into a wolf (Wolf); "Stultus," a fool in latin, becomes "Stolz," a proud man, in German, and a "Stout" one in English. The old French word "Hustins," noise, tumult, has found a refuge in the "Hustings" of England. The latin word "Bulga," purse, "bougette," a little purse, in the old French, now plays a great part among politicians, under the title of Budget; while "Buch," a book, a thing once so rare, always so valuable when good, has undergone the humiliation of becoming in French "un bouquin," a fusty old volume.

Sometimes words are formed from the diminutives of another language:*

Oncle, uncle, comes from	Avunculus,	the diminutive of	Avus,
Nagel, nail, ongle,	„	Unguiculus,	„	Unguis;
Female, fémêle,	„	Feminella,	„	Femina.
Oreille, ear,	„	Auricula,	„	Auris.

The German "Ohr" and the English "ear" keep closer to the latin. The word "oculus" is without doubt related to the German "Auge," as the English "eye" is to the French "œil." The analogy between nasus, Nase, and nose; pes, pedis, Fuss, and foot; labra, Lippe, lip; genu, Knie, and knee, and so forth, is too striking to require any remark.

* See Ampère, Histoire de la Littérature française au moyen âge.

The G of the south often becomes hard in the north: gelidus, kalt, cold; égal, equal. The French G, which is itself a transformation from the latin B and V, is often changed into a W.

Guerre, war, from the latin		Bellum.
Guêpe, wasp,	,,	Vespa.
Gâter, to waste,	,,	Vasto.
Garder, to ward.		
Guichet, wicket.		
Gage, wage.		
Guêter, to watch.		
Garantir, to warrant.		
Gauffre, wafer.		
Guise, wise. G. Weise.		

The Qu of the latin are sometimes transformed into W.

LATIN.	GERMAN.	ENGLISH.
Qui, quod	*Wer, was*	who, what
Quando	*Wann*	when
Qualis	*Welcher*	which

Words may even have a double origin, southern and northern; the Italian "guiderdone," reward, is formed of the Teutonic "wieder," and the latin "donum;" the French adjective "mauvais," bad, from the latin "malus," and the German "Wesen."

The C and G of the latin are often changed into H by the Teutonics and vice versâ.

LATIN.	GERMAN.	ENGLISH.
Hortus,	*Garten,*	garden
Cornu,	*Horn,*	horn
Calamus,	*Halm,*	haulm
Cannalis,	*Hanf,*	hemp
Hostis,	*Gast,*	guest, host
Collis,	*Hügel,*	hill
Casa,	*Hause,*	house
Cor, cordis,	*Herz,*	heart
Cuttis,	*Haut,*	skin
Homo,		ANG.-SAX. guma or gom

The V of the latin almost invariably becomes a W in the Teutonic languages. In German this change is very proper and natural, as the W is sounded V, whereas the V has the sound of F.

Ventus,	*Wind,*	wind
Viscus,	*Wachs,*	wax
Vae,	*Weh,*	woe
Vinum,	*Wein,*	wine
Volo,	*Wollen,*	will
Vannus,	*Wanne,*	van
Valeo,	*Wohl,*	well
Vallum,	*Wall,*	wall
Vidua,	*Witwe,*	widow
Vitis,	*Weide,*	willow
Venus, Venustas,	*Wonne,*	

Sometimes the W is added by the Teutonics.

	LATIN.	GERMAN.	ENGLISH.
	Aether,	*Wetter,*	weather
	Umbo,	*Wampe,*	womb
	Angulus,	*Winkel,*	angle
Gr.	Udór,	*Wasser,*	water
It.	Andare,	*Wandern,*	to wander

The D and L of the latin are occasionally changed into the German Z and English T.

Dens,	*Zahne,*	tooth
Domare	*Zähmen*	to tame
Tempus,	*Zeit,*	time
Lingua,	*Zunge,*	tongue
Lacrima,	*Zähre,*	tear

Sometimes an aspirate or guttural is added in German.

	Lucrum,	*Glück,*	luck
	Viridis,	*Grün,*	green
	Partus,	*Geburt,*	birth
Gr.	Litos, (simple)	*Klein,*	little
L.	Theos, } Deus, }	*Gott,*	God*

* With respect to the word "Gott," Meidinger, a German Lexicographer, says: "At a most remote period of our language, Od was changed into God; it is because the vowel O has in itself a gentle aspiration, (The English word *one* is a remarkable instance of it) and the G was sounded sometimes Ch, sometimes K, as Hlodovig or Clodowig, Hilperich or Chilperich. The G, however never was

Being anxious fully to demonstrate the close affinity between two great families of languages, long considered totally different, I must crave the indulgence and patience of this society, of those members, at least, who do not take a great interest in such researches. This paper would be incomplete, if I did not enter into particulars. I will now introduce all kinds of words, showing that there scarcely is any object or action, the similar expression of which may not be traced both in the Teutonic and græco-latin languages. Let us begin with the verbs, bearing in mind that certain consonants are identical, as

B, P, Ph, F, V;
C, Q, Ch, G;
L, R; L, M, N;
D, Dh, T, Th, S, Ts. Z.

changed into a hard K in the word God, but into a W, the sound of which approaches to that of the G, and this is the origin of the word Wodan. The same writer states that the word "Gott" derives its origin from the root Ot, Od, Eut, which signifies possession, power, and from the gælic or old Slavonian article G. This word means the powerful, the Almighty, the ruler of the universe. Hence also the words Th-eos, Th-eut, D-eus, Z-eus and perhaps also the name of Zeba-oth." For my part, I am inclined to think that "Gott and gut," God and good, are simply derived from the Greek word "Agathos," good or goodness, a word, according to St. Matthew's Gospel, ch. xix. ver. 16, applicable to God alone. I have not the least doubt that Theos, Deus, comes from the Sanskrit "Diva," in Zend "Diu," in the Egyptian "Theos" as in Greek.

LATIN.	GERMAN.	ENGLISH.
Audio, Audire,	*Hören,*	to hear
Aegresco,	*Aechzen,*	to ache
Balo,	*Bellen,*	to bleat
Clango,	*Klingen,*	to clink
Climax,	*Klimmen,*	to climb
Coquo,	*Cochen,*	to cook
Croccio,	*Krähen,*	to crow
Damno,	*Verdammen,*	to condemn
Edo,	*Essen,*	to eat
Erro,	*Irren,*	to err
Foro,	*Bohren,*	to bore
Fornix,	*Brennen,*	to burn
Frigeo,	*Frieren,*	to freeze
Fugio,	*Fliehen,*	to flee
Habeo,	*Haben,*	to have
Invenio, inventus,	*Finden,*	to find
Jocus,	*Jauchzen,*	to shout for joy, to rejoice
Lego,	*Lesen,*	(to read)
Lacesso,	*Locken,*	to intice, to lock in
Lingo,	*Lecken,*	to lick
Meto,	*Mähen,* *Messen,*	to mete, (to measure) to mow
Mulgeo,	*Melken,*	to milk
Nosco,	*Kennen,*	to know
Odi,	*Hassen,*	to hate
Pleo,	*Füllen,*	to fill
Plico,	*Flechten,*	to fold
Possideo,	*Besitzen,*	to possess
Pecto,	*Picken,*	to peck
Pipo,	*Pfeifen,*	to play the fife
Rapio,	*Rauben,*	to rob

LATIN.	GERMAN.	ENGLISH.
Ruo,	*Rennen,*	to run
Rigo,	*Regnen,*	to rain
Sugo,	*Saugen,*	to suck
Scribo,	*Schreiben,*	to write
Sorbeo,	*Saufen,*	to sip
Sedeo,	*Sitzen,*	to sit
Sperno, I despise,	*Spornen,*	
Emo, Sumo,	*Nehmen,*	to take, to assume
Stipo,	*Stopfen,*	to stop, to cork
Specto, Specio,	*Spähen,*	to spy
Scabo,	*Schaben,*	to shave
Sudo,	*Schwitzen,*	to sweat
Satio, (Subs.)	*Sähen,*	to sow
Simul, (Adv.)	*Sammeln,*	to assemble
Tango,	——	to take
Titilo,	*Kitzeln,*	to tickle
Traho,	*Tragen,*	to draw
Trudo, I push,	*Treten,*	to tread
Tollo,	*Stehlen,*	to steal
Ululo,	*Heulen,*	to howl
Vado,	*Waten,*	to wade

Where there is no corresponding latin word, we often find one in Greek, Italian, Spanish or French.

Gr.	Brazein,	*Brauen,*	to brew
	Erettein,	*Rudern,*	to row
	Macin, (to wish, to demand)	*Mögen,*	I may

	ITALIAN.	GERMAN.	ENGLISH.
	Costare,	*Kosten,*	to cost (L. consto)
	Rostire,	*Rösten,*	to roast
	Sprizzare,	*Sprützen,*	to spout
	Trottare,	*Traben,*	to trot
	Scurare,	*Scheuern,*	to scour
Fr.	S' Etonner,	*Staunen,*	to be astounded
	Chercher,	*Suchen,*	to seek

Adjectives are not less numerous.

LATIN.	GERMAN.	ENGLISH.
Angustus,	*Enge,*	(narrow)
Alter,	*Ander,*	other
Clarus,	*Klar,*	clear
Castus,	*Keusch,*	chaste
Crassus,	*Gross,*	great
Curtus,	*Kurz,*	short
Cholikos,	*Gallicht,*	full of gall
Communis,	*Gemein,*	common
Falsus,	*Falsch,*	false
Ferox,	*Frech,*	fierce, freakish
Gilvus,	*Gelb,*	yellow
Gelidus,	*Kalt,*	cold
Juvenis,	*Jung,*	young
Lassus,		lazy
Longus,	*Lang,*	long
Novus,	*Neu,*	new
Paratus,	*Bereit,*	ready
Plenus,	*Voll,*	full
Probus,	*Brave,*	brave
Rectus,	*Recht,*	right

	ITALIAN.	GERMAN.	ENGLISH.
	Rutilus, russus,	*Roth,*	red
	Rotundus,	*Rund,*	round
	Securus,	*Sicher,*	sure, secure
	Strenuus,	*Streng,*	strong
	Tenuis,	*Dünn,*	thin
	Tristis,	*Traurig,*	(sad)
	Verus,	*Wahr,*	(true)
	Vir, virtus,	*Werth,*	worth
Gr.	Agathos,	*Gut,*	good
	Phaulos,	*Faul,*	foul
F.& It.	Brun, bruno,	*Braun,*	brown
	Gris, griggio,	*Grau,*	grey
	Fin, fino,	*Fein,*	fine
	Frais, fresco,	*Frisch,*	fresh
	Aucun, alcuno,	*Kein,*	no
	Plat, piatto,	*Flach,*	flat
	Riche, ricco,	*Reich,*	rich
	Risque, rischio,	*Rasch,*	rash
	Sur,	*Sauer,*	sour

The personal pronouns and possessive adjectives present such evident analogies, that I shall not dwell upon them; I will proceed to substantives, which I will divide into three classes, animals, plants and nouns common; admitting old words only and no newly invented technical expressions.

LATIN.		GERMAN.	ENGLISH.
	Asinus,	*Esel,*	ass
	Bibber, fiber,	*Biber,*	beaver
	Balena,	*Wahlfish,*	whale
	B-os,	*Ochse,*	ox
	Cavallus,	*Gaul,*	(horse)
	Carabus,	*Krabbe,*	crab
	Catus, catulus,	*Katze,*	cat
	Cornix,	*Krähe,*	crow
	Cucullus,	*Gukuk,*	cuckoo
	Echinus,	*Igel,*	hedge-hog
	Frigillus,	*Finke,*	finch
	Hircus,	*Hirsch,*	hart
	Leo,	*Löwe,*	lion
	Lupus, Vulpes, }	*Wolf,*	wolf
	Mus,	*Maus,*	mouse
	Natrix,	*Natter,*	adder
	Ostrea,	*Auster*	oyster
	Piscis,	*Fisch,*	fish
	Pulex,	*Floh,*	flec
	Pavo,	*Pfau,*	peacock
	Sus,	*Sau,*	sow
	Storio,	*Stör,*	sturgeon
	Sturnus,	*Stahr,*	starling
	Struthio-camelus,	*Strauss,*	ostrich
	Taurus,	*Stier,*	steer
	Veredus, (post-horse)	*Pferd,* *	
	Vermis,	*Wurm,*	worm
It.	Aringa,	*Häring,*	herring
	Falcone,	*Falke,*	falcon

* "Horse" comes from another German word "Ross."

	ITALIAN.	GERMAN.	ENGLISH.
	Ganza,	*Gans,*	goose
	Ratta,	*Ratte,*	rat
Sp.	Carpa,	*Carpfe,*	carp
	Choto, gasella,	*Geis,*	goat

PLANTS, &c.

	LATIN.	GERMAN.	ENGLISH.
	Arbos,	*Baum,*	tree (from the Gr. drus)
	Aesculus,	*Esche,*	ash
	Bacca,	*Beere,*	berry
	Castanea,	*Kastanie,*	chesnut
	Cerefolium,	*Kerbel,*	chervil
	Caulis,	*Kohl,*	(cabbage)
	Ficus,	*Feige,*	fig
	Granum,	*Kern,*	kernel
	Mespilum,	*Mispel,*	medlar
	Muscus,	*Moos,*	moss
	Oleum,	*Oehle,*	oil
	Oryxa,	*Reis,*	rice
	Rapum,	*Rübe,*	(radish)
	Vitis, (sarment de vigne)	*Weide,*	willow
	Ulma,	*Ulm,*	elm
	Semen,	*Saamen,*	
	Rosa,	*Rose,*	
Gr.	Apion,	*Apfel,*	apple
	Aegulos,	*Eichel,*	acorn
	Grastis,	*Gras,*	grass
	Puanon,	*Bohne,*	bean

Of common nouns the number is endless, I will quote a few.

LATIN.	GERMAN.	ENGLISH.
Ancus,	*Aenkel,*	ankel
Anxietas,	*Angst,*	anguish
Anchora,	*Anker,*	anchor
Ascia,	*Axt,*	hatchet
Acies, (point)	*Ecke,*	edge
Ager,	*Acker,*	acre
Anima,	*Athem,*	Old Goth. ahme
Aestas,	*Hitze,*	heat
Advocatus,	*Vogt,*	provost
Acetus,	*Essig,*	(vinegar)
Balneum,	*Bad,*	bath
Bracha, (langes, braie)		breeches
Butyrum,	*Butter,*	butter
Catena,	*Kette,*	(chain)
Cellarium,	*Keller,*	cellar
Calix,	*Kelch,*	chalice
Caseus,	*Käs,*	cheese
Crux,	*Kreutze,*	cross
Corona,	*Krone,*	crown
Costa,	*Küste,*	coast
Cista,	*Kiste,*	chest
Crusta,	*Kruste,*	crust
Coma,	*Kamm,*	comb
Carcer,	*Kerker,*	(prison)
Corpus,	*Körper,*	corps (body)
Clangor,	*Klang,*	clang
Crypta,	*Grab,*	grave

LATIN.	GERMAN.	ENGLISH.
Caupo,	*Kauf,*	(purchase)
Cremor,	*Rahm,*	cream
Culpa,	*Schuld,*	guilt
Clunis,	*Lende,*	loin
Currus,	*Karren,*	cart
Dies,	*Tag,*	day
Dictum,	*Ding,*	thing, (derived, perhaps, from the G. thun, to do)
Domus, G. Doma,	*Dom,*	roof, dome
Emplastrum,	*Pflaster,*	plaster
Fenestra,	*Fenster,*	(window)
Fel, G. cholé,	*Galle,*	gall
Flo, (I blow,)	*Flöte,*	flute
Fallo,	*Fehler,*	fault
Fuga,	*Flucht,*	flight
Furca,		fork
Flamma,	*Flamme,*	flame
Fax,	*Fackel,*	torch
Fructus,	*Frucht,*	fruit
Genus,	————	kin
Gabulus, (cross)	*Gabel,*	(fork)
Gelu,	*Kälte,*	cold
Gustus,	*Kost,*	(food)
Gula, (a mouth)	*Kehle,*	(throat)
Herus, (master of the house,	*Herr,*	(sir)
Hora,	*Uhr,*	hour
Hepar,	*Leber,*	liver
Homo,	*Mann,*	man, (gom in Ang. S.)
Jugum,	*Joch,*	yoke
Jocus,	*Geck,*	joker
Lux,	*Licht,*	light

LATIN.	GERMAN.	ENGLISH.
Linum	*Linnen,*	linen
Luxus, Voluptas,	*Wollust,*	(voluptuousness)
Laus,	*Lob,*	laud
Magister,	*Meister,*	master
Matta,	*Matte,*	mat
Medium,	*Mitte,*	middle
Modius, (bushel)	*Maas*	measure, mode
Mens,	*Meinung,*	mind
Mola,	*Mühle,*	mill
Moneta,	*Münch,*	mint
Mensis,	*Monat,*	month
Minor,	*Minder,*	(less)
Mare,	*Meer,*	(sea) mere
Murus,	*Mauer,*	(wall)
Margo,	*Marke,*	mark
Mercatus,	*Markt,*	market
Mors,	*Mord,*	murder
Modestus,	*Mädchen,*	(modest) maid
Nix,	*Schnee,*	snow
Nox,	*Nacht,*	night
Nebula,	*Nebel,*	
Necessitas,	*Noth,*	need
Nomen,	*Nahme,*	name
Nidus,	*Nest,*	nest
Nodus,	*Knote,*	knot
Nepos,	*Neffe,*	nephew
Ordo,	*Ordnung,*	order
Os, (opening)	*Hose,*	
Pellis,	*Fell,*	(skin)
Pileus,	*Filz,*	felt

LATIN.	GERMAN.	ENGLISH.
Pullus,	*Füllen,*	filly
Populus,	*Volk,*	(folks) people
Pondus,	*Pfund,*	pound
Pecunia,	*Pfennig,*	penny
Poena,	*Pain,*	pain
Pannus,	*Banner,*	
Puer, puber,	*Bube,*	boy
Palus,	*Pfahl,*	pole
Pelum,	*Pfeil,*	(arrow)
Pila,	*Ballen,*	bale
Poculum,	*Becher,*	(cup)
Pecus,	*Vieh,*	(cattle)
Punctum,	*Punkt,*	point
Puteus,	*Pfütze,*	(a well)
Par,	*Paar,*	pair
Probo,	*Probe,*	proof
Pretium,	*Preis,*	price
Piper,	*Pfeffer,*	pepper
Retor,	*Rede,*	(speech,) hence, orator
Rumor,	*Ruhme,*	(glory)
Regula,	*Regel,*	rule
Ruga,	*Rünzel,*	wrinkle
Sal,	*Salz,*	salt
Summa,	*Summe,*	sum
Similago, Similia, fleur de farine,	*Semmel,*	simnel, (sweet cake)
Sensus,	*Sinn,*	sense
Scappa,	*Schiff,*	ship
Scandalum,	*Shande,*	shame, scandal
Scortea, (leather cloak)	*Schürze,*	skirt, shirt

LATIN.	GERMAN.	ENGLISH.
Scrinium,	*Schrein,*	scrin
Strata,	*Strasse,*	street
Speculum,	*Spiegel,*	mirror
Spatha,	*Spaten,*	spade
Speculum,	*Spitze,*	spit (point)
Sputum,	*Speichel,*	spit (saliva)
Sol,	*Sonne,*	sun
Stramen,	*Stroh,*	straw
Segulum,	*Segel,*	sail
Sigillum,	*Siegel,*	seal
Causa,	*Sache,*	Ang. Sxn. saca
Saccus,	*Sack,*	sac
Soccus,	*Soche,*	sock
Saccharum,	*Zucker,*	sugar
Sapo,	*Seife,*	soap
Seco, I saw,	*Sage,*	saw
Sella,	*Sattel,*	saddle
Tonus,	*Ton,*	din
Tabula,	*Tafel,*	table
Turris,	*Thurm,*	tower
Tectum,	*Dach*	(roof)
Tegula,	*Ziegel,*	tile
Ulna,	*Elle,*	ell
Vulnus,	*Wunde,*	wound
Votum,	*Wünsch,*	wish
Vogo,	*Woge,*	wave
Vehiculum,	*Wagen,*	waggon
Via,	*Weg,*	way
Veritas,	*Wahrheit,*	verity
Verruca,	*Wartz,*	wart
Validitas,	*Gewalt,*	

LATIN.	GERMAN.	ENGLISH.
Vas, vasum,	*Fass,*	vase
Vellus,	*Wolle,*	wool

I will now introduce a few Greek, Italian and Spanish words. French words I leave out, because they are so little disguised, except in the pronounciation, that they are easily recognised.

	GREEK.	GERMAN.	ENGLISH.
	Airu, eminent, Jeros, holy,	*Ehre,*	(honour)
	Ama, (together)	*Heime,*	home
	Axa, axos,	*Asche,*	ashes
	Brotos, (rich, savory)	*Brod,*	bread
	Brun,	*Brunnen,*	(spring)
	Gaia, (district)	*Gau,*	
	Churiachon(temple)	*Kirch,*	kirk
	Chlide,	*Kleid,*	(coat)
	Gennetos,	*Kind,*	child
	Megas, megados,	*Macht,*	might
	Osche,(vine branch)	*Ast,*	
	Pidos,	*Bütte,*	butt
	Stula,	*Stuhl,*	stool
	Theros,	*Dürre,*	dryness
	Thugater,	*Tochter,*	daughter
	Thura,	*Thür,*	door
It.	Bosco,	*Busch,*	bush
	Banda,	*Band,*	bond

ITALIAN.	GERMAN.	ENGLISH.
Barca,	*Barke,*	barque
Baja,	*Bucht,*	bay
Batello,	*Boot,*	boat
Bordo,	*Bord,*	board
Birra,	*Bier,*	beer
Brusca,	*Bürste,*	brush
Brodo,	*Brey,*	broth
Bottega,	*Bude,*	(shop)
Cortina.	*Gardine,*	curtain
Cruccia,	*Krüche,*	crutch
Camera,	*Kammer,*	chamber
Daga,	*Degen,*	dagger
Fiasca,	*Flasche,*	flask
Fante,	*Kind,*	infant
Parco,	*Pferch,*	park
Paura,	*Furcht,*	fear
Pipa,	*Pfeife,*	pipe
Pacco,	*Pack,*	packet
Piliere,	*Pfeiler,*	pillar
Piaga,	*Plage,*	plague
Piazza,	*Pfalz,*	place
Rullo,	*Rolle,*	roller
Roccetto,	*Rock,*	frock
Rocca,	*Rocken,*	(spinning staff)
Stanga,	*Stange,*	(pole)
Schiuma,	*Schaum,*	scum
Schierra,	*Schaar,*	(troop)
Sp. Ganapa, (porter)	*Knecht,*	knave (L. gnavus, diligent
Quilla,	*Kegel,*	(ball)
Mucho,		much

Adverbs and prepositions do not present many analogies; yet it would be easy to find affinities even in these. For instance:

	LATIN.	GERMAN.	ENGLISH.
	Nunc	*Nun,*	now
	Major,	*Mehr,*	more
	Nihil,	*Nicht,*	not
	Hodie,	*Heute,*	to-day
	Sic,	*So,*	so
	Ita,	*Ja,*	yes
	Jeri,	*Gestern,*	yesterday
	Pro,	*Vor,*	before
	Porro,	*Fern,*	far
Gr.	Polus,	*Viel,*	(much)

In a series of papers, which I have had the honour of submitting to this society, I have shown that all the languages of Europe could easily be traced to the Sanskrit, in which are to be found the roots of most Teutonic and Græco-latin words. On the present occasion I have entered into a more minute comparison between these two families, not so much with a literary object in view, as fully to demonstrate that all the nations of Europe are closely related to one another, and ought consequently to entertain towards each other mutual sentiments of love and benevolence only. The thought of arranging and making known this the fruit of many years' research and observation was suggested some months ago, when two of the most powerful and most enlightened nations in Europe seemed to be on the point of renewing all the horrors and miseries of war. Thanks to the wisdom and moderation of the two governments, that danger is now over; but it may recur; and I considered it a duty to contribute my humble share to that great work of modern civilization and philosophy, a work of charity, unity and peace, which is to efface by degrees the supposed originality of races, the difference of traditions, the diversity of habits and customs, the opposition of interests, and, at last, to constitute the great human family on the wrecks of a long divided and ever struggling society.

January 6th, 1845.

ON

THE ICTIS

OF

DIODORUS SICULUS.

BY THE REV. R. WALLACE, F.G.S.

(Read January 23rd, 1844.)

In the "Bibliotheca Historica" of Diodorus Siculus,* who flourished towards the middle of the first century, occurs the following passage, relating to the locality in which the tin trade was carried on, between the natives of Cornwall and those who visited the British coast, for the purposes of traffic.

"Concerning their institutions, and other peculiarities, we will treat separately, when we come

* Lib. V. Cap. xxii. (Ed. Wesseling, 1746. Vol. I. p. 347.)

to Cæsar's expedition into Britain. But we will now speak of the tin produced there. Those who dwell near the promontory of Britain, called Belerium, are remarkably hospitable; and, from their intercourse with foreign merchants, civilized in their mode of life. These prepare the tin, skilfully working the ground which produces it, and which, though rocky, has fissures containing earth; and having worked out what these fissures supply, they wash and purify it, and when they have cast it into regular blocks, they carry it to a certain island, situated opposite to Britain, and called Ictis. For at the ebbings of the tide, the intervening space being left dry, they convey to it in wagons large quantities of tin. Now a remarkable circumstance happens with regard to the neighbouring islands, which lie between Europe and Britain: for at high water, the intermediate space being filled up, they appear islands; but at low water, the sea retiring, and leaving a large extent of dry ground, they are seen to be peninsulas. Hence the merchants buy the tin from the natives, and carry it over into Gaul; and at length, travelling through Gaul on foot about a thirty days' journey, they bring their burdens on horses to the mouth of the river Rhone."

Some have contended, that the island, to which Diodorus here gives the name of "Ictis," is the Isle of Wight, on account of the partial resemblance, which the Greek Ἰκτὶς bears to the Latin *Vectis*. But as Diodorus spent a considerable time at Rome, for the purpose of collecting materials for his work, it seems in the highest degree improbable, that he should have remained unacquainted with the true mode of spelling the Latin name *Vectis*.

In proper names beginning with V, Greek writers are accustomed to express this letter, either by the consonant B, or by the diphthong Ου. Thus, for the Latin *Varro*, the Greeks wrote indifferently Βάῤῥων, or Ουάῤῥων; for *Valerius*, Βαλέριος, or Οὐαλέριος; for *Virgilius*, Βιργίλιος, or Οὐιργίλιος; and for *Nervii*, Νέρβιοι, or Νερουίοι.* Numerous instances of the substitution of the diphthong Ου for the Latin V, in the proper names of places, might be adduced from Ptolemy and Strabo. Thus, to go no further than the name *Vectis*, Ptolemy writes it with an Ου, clearly showing that he did not disregard the initial letter V.† In Strabo

* Jos. Scaligeri *Animadv. in Euseb. Chronicon*; p. 112, a.—Dawesii *Misc. Crit.* Sect. iv.

† In the present text we have Οὐίκτησις, but Wesseling sup-

there is no mention of this island. But that writer has plainly indicated, by his mode of spelling other proper names beginning with a V, that he was not insensible to the force of this letter; and that, if he had been led to make mention of the Isle of Wight, he would as soon have thought of calling it *Ictis*, as we should of leaving out the initial W, and calling it the Isle of *Ight.* The same remark will apply to Diodorus, who calls the *Volsci*, a people of Latium, Οὐόλσκοι;* and *Vesuvius*, the celebrated volcanic mountain in Campania, Οὐεσούϐιος.† It is contrary to all analogy, indeed, to suppose that a proper name, commencing with an I in Greek, should be the representative of one commencing with a V in Latin;‡ and for

poses this to be an error of transcription for Οὐήκτις, because the ancient Latin interpreter has rendered it by *Vectis*. Vide Antonini Augusti *Itinerarium*; p. 509.

* Vol. I. p. 652; Ed. Wesseling.

† Vol. I. p. 267. In the printed text it is Οὐεσούσιος, which is a manifest error for Οὐεσούϐιος; and so it is regarded by Diodorus's learned editor.

‡ There are instances, in which digammated words, whose initial letter is I, on passing into the Latin, have retained the digamma, which is commonly expressed by the letter V, (as *vis* from ἴς,) but Greek authors of the age of Diodorus, when writing a Latin word in Greek characters, pay as much regard to the V as any other letter, and usually express it in the way pointed out above.

this simple reason, if there were no other, we should be justified in concluding, that the *Ictis* of Diodorus is not the *Vectis* of Cæsar, Pliny, and Suetonius.

But the orthographical difficulty is not the only one, which lies in the way of the supposition, that the Ictis of Diodorus is the Isle of Wight. There are geographical difficulties, which render this supposition altogether improbable; and incontestably prove, either that Diodorus was grossly ignorant of the locality of the Isle of Wight, or that his commentators have been mistaken in supposing, that it was his intention to describe that island, under the name *Ictis*. But as the description is remarkably circumstantial, and as this ancient writer could scarcely have erred in so plain a matter, the probability is, that the error rests with his commentators; and when the nature and extent of that error are considered, it is strange that it should ever have been committed by persons, possessing the slightest knowledge of the Isle of Wight, and its position relatively to the tin mines of Cornwall.

Ictis is described as "a certain island opposite to Britain." But Diodorus speaks also of "the neighbouring islands, which lie between Europe and Britain," and says that "at high water, the intermediate space being filled up, they appear islands, but at low water, the sea retiring, and leaving a large extent of dry ground, they are seen to be peninsulas;"—a description which can be applied, in no sense whatever, to the Isle of Wight.

Again, Diodorus not only says that the space between the main land and the island of Ictis was dry at low water, but that the natives "conveyed to it in wagons large quantities of tin." This, on the supposition that *Ictis* is the Isle of Wight, is a statement obviously at variance with all probability. Cornwall, in which the tin was raised, is a long and narrow district; and there is scarcely a spot in the whole county, which is more than fifteen miles distant from some convenient point on the coast, from which the tin might have been shipped. Is it credible, then, that this metal, cast into ingots, should have been carried in wagons through the whole length of Cornwall, Devonshire, Dorsetshire, and a considerable part of Hampshire, much of which must then have been thickly wooded, and the whole destitute of those facilities for transit which exist in our own times; when it might have been conveyed to the trading vessels, on almost any part of the coast of Cornwall, at a twentieth part of the trouble and expense? Even so recently as the former half of the last century, the roads in this country were almost impassable for wagons, and nearly the whole of the traffic was carried on by means of pack-horses. Can we suppose, then, that the ingots of tin, in the times of the ancient Britons, were carted through so long a tract of country, before they were conveyed to a port of embarkation?

But suppose the difficulty of this long and tedious overland journey to be surmounted, and the tin to be conveyed to some convenient place on the present Hampshire coast: how shall we contrive a passage for it, by land, to any part of the Isle of Wight? We are told, that the passage may have been along the shingle bank, which *probably* once connected Hurst Castle with the Isle of Wight; and of which traces, even in the present day, are far from being obliterated. But though, as Mr. Lyell states, the entrance of the channel, called the Solent, is crossed for more than half its width by such a shingle-bank,*

* Lyell's *Principles of Geology;* Book 2. Chap. vi. Vol. I. pp. 425, 426. Ed. 5th.

that eminent geologist has nowhere hazarded the conjecture, that the Isle of Wight, at low water, was ever connected with the main land, during the historical period ; nor has the slightest portion of direct proof ever been adduced, in support of such a supposition. On the contrary, there is a long and unbroken chain of evidence to show, that the Isle of Wight has been separated from the main land, as far back as any written record of it extends.

For these reasons, then, we seem warranted in rejecting, as wholly untenable, the supposition that the Ictis of Diodorus was the Isle of Wight.

Impressed with the difficulty attending this supposition, several literary and scientific men of eminence, among whom are Sir C. Hawkins, Dr. Maton, Dr. Barham, and Mr. Hawkins, have thought that St. Michael's Mount was the island, which forms the subject of the present investigation. This hypothesis is represented as "extremely probable" by Sir Henry T. De la Beche, in his "Report on the Geology of Cornwall, Devon, and West Somerset;" and he remarks, that, "as far as the geographical description extends, there is no other place on the Cornish coast which will answer to it."* In defence of this opinion, it has been asserted, and with perfect truth, that St. Michael's Mount, at high water, is an island; and that, at low water, it is connected, by a narrow isthmus, with the main land. Still there are difficulties of no trifling magnitude to be overcome, before we can give our assent to this hypothesis.

Diodorus speaks of "the neighbouring islands:" but St. Michael's Mount is only a single mass of rock, rising abruptly, in solitary grandeur, from the bosom of the waves; and attracting the eye of the observer, in a peculiar manner, by its very abruptness.

Diodorus also says of these "islands," (still using the plural,) that "they appear islands" only at "high water;" and that, when the tide is out, the intervening space is left dry, and "they are seen to be peninsulas." This he mentions as something peculiarly deserving of the reader's attention: but his commentators have overlooked the important fact, that there were other islands, in the vicinity of the one which he

* Chap. xv. p. 524.

called *Ictis ;* and seem not to have been aware, that it was the circumstance of there being *several tracts of land* in the same locality, appearing to be islands at high water, and peninsulas at low water, which constituted the peculiarity mentioned by him, and dwelt upon as singularly worthy of notice.

Diodorus further says, that "at low water," the sea retires, and leaves dry "a large portion of ground," (πολὺν τόπον). But if the island of Ictis and St. Michael's Mount be one and the same, this "large extent of ground" does not exceed, at furthest, 300 or 400 yards long, by 30 or 40 broad. This isthmus probably served as a passage for the votaries to the shrine of St. Michael, which was much frequented by pilgrims; as the one at Landisfarn, described by Sir Walter Scott, in his "Marmion," did to that of St. Cuthbert.

> "The tide did now the flood-mark gain,
> And girdled in the Saint's domain:
> For, with the flow and ebb, its style
> Varies from continent to isle;
> Dryshod, o'er sands, twice every day,
> The pilgrims to the shrine find way;
> Twice every day the waves efface
> Of staves and sandall'd feet the trace."
>
> CANTO II. STANZA 9.

Nor is it denied, that tin might have been conveyed in wagons along such an isthmus as this, in the time of Diodorus. But how that writer could describe a neck of land of such narrow dimensions, uniting a single peninsula to the main land, as a large extent of ground, forming a number of isthmuses to several distinct peninsulas, we must leave it to the ingenuity of those who advocate this hypothesis to explain.

But the hypothesis has a still more formidable difficulty to contend with, in the fact, that St. Michael's Mount was formerly neither island, nor peninsula; but a portion of the main land, situated at some distance from the sea. It appears from tradition, confirmed by observation, that this mount once stood in a forest, and was called "The hoar rock in the wood." In the charter of the Confessor, it is described as "St. Michael *near* the sea."* Its exact distance from the coast is not mentioned in that document: but Florence of Worcester says, that it was originally enclosed within a very thick wood, distant from the sea *six miles*, affording the finest shelter for wild beasts. The sea, however, has made great encroachments on this part of the coast, within the

* Sanctum Michaëlem qui est *juxta* mare.

historical period. It is stated in Dr. Paris's "Guide to Mount's Bay and the Land's End," that the grandfather of the incumbent of Madron was known to have received tithes from land under the cliff of Penzance; and that, in the memory of many persons living when that book was written, the cricket-players were unable to throw a ball across the Western Green, between Penzance and Newlyn, which is now not many feet in breadth.* Dr. Borlase likewise informs us, that "on the strand of Mount's Bay, midway between the piers of St. Michael's Mount and Penzance, on the 19th of January, 1757, the remains of a wood, which, according to tradition, covered a large tract of ground in Mount's Bay, appeared."† These remains consisted of hazel and alder, with some forest trees, including the elm and the oak. The hazel-nuts were abundant; and even fragments of insects, particularly the elytra and mandibles of the beetle tribe, still displaying the most beautiful, shining colours, but crumbling into dust on exposure to the air, were found amid the vegetable mass.‡ The exact time

* *Guide to Mount's Bay;* pp. 15, 16. (2nd Ed.)

† *Athenæum,* No. 761, (May 28th, 1842,) p. 484.

‡ De la Beche's *Report on the Geology of Cornwall, Devon, and West Somerset;* Chap xiii. pp. 417, 418.

of the catastrophe, to which the submersion of this wood is to be referred, cannot now be determined with certainty. But, from the fact of ripe nuts being found, and the trees not being destitute of leaves, it may be inferred, that the irruption took place in the autumn ; and it is a circumstance deserving of notice, that, in the reign of William Rufus, on the 11th of November, 1099, such an invasion of the ocean did occur, and is thus noticed in the Saxon Chronicle. "In this year (MXCIX) also, on the festival of St. Martin, the waves of the sea made great inroads, and occasioned more loss than any one had ever known them to do before."* Simeon of Durham alludes to the same inroad of the sea, as having taken place " on the third of the nones of November," (which corresponds with the 11th of that month;) and entombing " towns and men in great numbers, and oxen and sheep innumerable."†

* Hoc item anno, in Sancti Martini festo, tantum aucti sunt maris fluctus, tantumque damni maritimis dederunt, quantum nullus meminerit eos unquam antea dedisse. (Gibsoni *Chonicum Saxonicum*, A.D. MXCIX. p. 207.)

† Tertio Non. Novembris mare littus egreditur, et villas et homines quamplures, boves et oves innumeras demersit. (Simeonis Monachi Dunelmensis *Historia de Gestis Regum Anglorum*, apud *Historiæ Anglicanæ Scriptores X.* edente Rogero Twysden; 1652, Fol. Anno Domini 1099. p. 224.)

There seems, then, as little ground for supposing, that the Ictis of Diodorus is the small rocky island, from which St. Michael's Mount rises, as that it is the Isle of Wight; and if the accuracy of the Greek historian's description is to be tested, solely by the present outline of the Cornish coast, the attempt to reconcile that description with existing appearances must be abandoned as hopeless. But no one, who has visited Cornwall with the eye of a geologist, can be a stranger to the fact, that the sea, which washes its coast, presents, in various directions, evidences of recent changes, which have produced an amazing influence on its hydrographical character: and who will venture dogmatically to assert, that, among these changes, none can be found, which shall tend to establish the credibility of Diodorus's narrative? It has appeared to me, while engaged in considering this subject, that the difficulties, presented by the language of Diodorus, are not to be surmounted, by taking into consideration merely the present outlines of the Cornish coast; but by tracing back those changes, which history has recorded, or which present appearances render probable, or of which tradition at least has preserved some notice, among a population remarkably tenacious of the memory of the past. Any one of these modes of solution is preferable to that reckless charge of ignorance, or want of historical accuracy, of which many are so ready to avail themselves, when their efforts to throw light upon the records of antiquity are baffled, and they find themselves unable to interpret the written memorials of former times, or to admit the credibility of facts, to which nothing similar, or analogous, has occurred, within the range of their own limited experience. It is well known that such was once the fate of certain narratives in the history of Herodotus, the accuracy of which has been abundantly confirmed by subsequent investigation; and if this result has been attained in regard to a writer, who is known occasionally to have mingled fable with history, candour requires that we should pause, before we reject as incredible a statement of Diodorus, which not only involves nothing of the nature of a physical impossibility, but bears impressed upon it the strongest internal marks of having been committed to writing, on the testimony of eye witnesses.

Assuming the correctness, therefore, of the geographical description now under consideration, and discarding, as improbable, the supposition, that the Ictis of Diodorus is either the Isle of Wight, or St. Michael's Mount, let us proceed to inquire, to what causes of change the coast of Cornwall has been exposed, and what has been the actual, or the probable effect of these causes, operating through a long series of ages.

The effects of oceanic agency, in bringing about geological changes, are of two kinds. Sometimes the ocean forms accumulations of detritus, which act as barriers against its own encroachments. At other times, it obliterates all traces of what once was solid land; either gradually and slowly undermining it, or battering it down by a succession of attacks, or sweeping it away by a single mighty and resistless effort. As Diodorus, therefore, mentions a certain island, which he calls *Ictis*, situated near a certain promontory, which he calls *Belerium*; and as *Ictis*, and the neighbouring islands, were such only at high water, and presented the appearance of peninsulas at the ebbing of the tide; it may be worth our while to inquire, whether they have lost their insular character, by an accumulation of detritus, or whether they are wholly, or in part, submerged, and their communication with the main land, or with each other, broken off by the inroads of the sea.

The most striking evidences of the accumulation of detritus have been observed on some parts of the Cornish coast. In the Carnon stream-works, to the north of Falmouth, a few years ago, two human skulls were found, imbedded, with other animal remains, in a mass of vegetable matter, at the extraordinary depth of more than fifty feet below the level of the river, and covered by several successive deposits of silt, shells and sand;* and the district around Hayle, extending nearly without interruption from St. Ives to Padstow, is little more than one continued desert of sand, which, in many places, has accumulated to the height of sixty feet, and beneath which, human bones and the remains of ancient buildings have been found. There is no local tradition, relating either to the time or manner, in which these monuments of human existence were entombed: but it has been inferred, from certain ancient records of the Arundel family, that the catastrophe took place about the twelfth century;† and in examining the old Chronicles, for notices of

* *Transactions of the Geological Society of Cornwall,* Vol. IV. p. 58; quoted by Sir Henry T. De la Beche, in his *Report on the Geology of Cornwall,* &c. Chap. xiii. p. 404. Bakewell's *Introd. to Geology,* Chap. i. p. 22. (5th Ed.)

† *Guide to Mount's Bay,* &c. pp. 161, 162.

the more remarkable inundations, with which the shores of England have been visited, I have found, in Radulfus's " Ymagines Historiarum," a description of one, which was particularly destructive at St. Ives, towards the end of the reign of king John. " A sudden and unexpected inundation of waters," says that writer, " took place in many parts of England, whence many *men* were drowned, and *houses* overturned, especially at Exeter and *St. Ives*."*

The author of the " Guide to Mount's Bay and the Land's End"† thus describes the district around Hayle. " The river *Hayle* takes its rise near Crowan, and falls into St. Ives' Bay, although it arrives at the level of the sea three miles before it reaches the northern coast, and winds its way through an area of sand, nearly half a mile wide, and more than two miles long ; this sand, at high water, is generally submerged, so that the traveller who wishes to cross is obliged to take a

* Subita et improvisa aquarum inundatio pluribus in locis per Angliam facta est, unde plures homines submersi sunt, et domus eversæ, maxime apud Excestre, et sanctum Ivonem.—*Historiæ Anglicanæ Scriptores X*, edente Rogero Twysden; 1652, Fol. p. 710.

† Pp. 158, 159.

circuitous route over the bridge at *Saint Erth*; but upon the ebbing of the tide, it soon becomes fordable, and may be passed over even by foot passengers. It is a curious circumstance that at twelve o'clock at noon, and at midnight, it is *always* fordable; this apparent paradox is solved by knowing, that at *Spring tides* it is always low water at these hours, and that the *Neap tides* never rise sufficiently high to impede the passage."

This description, as far as regards the extent of land, alternately flooded and left dry, approaches more nearly to that of Diodorus, than any which has yet come under our observation; for here we have an area of sand half a mile wide, and more than two miles long, which, at high water, is generally submerged, but at the ebbing of the tide becomes fordable, and may be crossed even by foot passengers: whereas, the extent of the isthmus of loose stones and pebbles, which, at low water, connects St. Michael's Mount with the main land, is trifling in comparison, and ill corresponds with the "large space" of which Diodorus speaks. Was the peninsula west of St. Erth, then, the Ictis of that writer? There is one remarkable circumstance, connected with the trading visits of the Phœnicians to the Cornish coast, which may at first view appear to confer upon this supposition some degree of probability: —I mean, the fact of their taking great pains to conceal from the captains of the Roman vessels the part of the coast on which they landed, and to which the tin was brought by the natives, for the purposes of sale and exportation. Strabo tells us, that the master of a Phœnician trading vessel, on its voyage to the Cassiterides for tin, being followed by the captain of a Roman vessel, whose vigilance he was unable to elude, purposely steered into the shallows, and thus caused the destruction of the two vessels; but that, his own life being preserved, he was rewarded by his countrymen for this act of self-devotedness, and patriotism.* It might, therefore, be inferred, if all other circumstances were favourable, that the Phœnicians, for the purpose of keeping in their own hands a monopoly of the trade in tin, instead of sailing directly to some point on the southern part of the Cornish coast, passed the Land's End, and Cape Cornwall, entered the mouth of the Bristol Channel, and landed somewhere on the western coast of the Bay of St. Ives; that either intentionally, or through ignorance, they

* Strabonis *Res Geographicæ;* Lib. iii. Tom. I. pp. 239, 240. (Oxon. 1807. Fol.)

represented the granitic district in the western part of Cornwall as an island; and that this supposed, or pretended island is the Ictis of Diodorus Siculus.

Ortelius, under the article *Cassiterides*, alludes to the opinion of Camden, that these were the Scilly Islands; but adds, " I am almost inclined to believe, that the British Islands themselves are described by the most ancient writers under the name *Cassiterides:* if I am deceived, I would say, with Herodotus, that I am not acquainted with the *Cassiterides*."* Mr. Carne too has conjectured, that the metalliferous district of St. Just constituted the principal portion of what was formerly known under the name of the *Cassiterides:*† and it is a fact worthy of notice, that Pliny, who says, in one place,‡ " opposite to Celtiberia there are many islands, called by the Greeks *Cassiterides*, from the quantity of lead which they yield," in another place seems to dis-

* Abr. Ortelii Antverpiani *Thesaurus Geographicus Recognitus et Auctus; Art.* Cassiterides.—Herodoti *Hist.* Lib. iii. c. 115.

† *Guide to Mount's Bay;* p. 141.

‡ *Nat. Hist.;* Lib. iv. c. 22. Ex adverso Celtiberiæ complures sunt insulæ, *Cassiterides* dictæ Græcis, a fertilitate plumbi.

tinguish one of these from the rest, under the name of *Cassiteris;* for he says, in the latter place, " Midacritus was the first, who carried away lead from the island *Cassiteris*."* Now it seems not improbable, that this supposed island, to which Pliny applies the name *Cassiteris*, was the western part of Cornwall. At a cursory glance, therefore, it might be thought, that the island *Cassiteris* is no other than the Ictis of Diodorus; but Pliny represents Timæus, the historian, as speaking of the island *Mictis*, (an evident mistake of some copyist for *Ictis;)* so that Pliny probably made a distinction between Cassiteris and Ictis. His words are " INSULAM MICTIM,"† which might easily have arisen, by an error of transcription, from " INSULAM ICTIM." Wesseling accordingly regards the " Mictis" of Pliny as the " Ictis" of Diodorus under another form;‡ and Borlase, in his observations on the Scilly Islands, says, " this ICTIS of Diodorus

* Plumbum ex Cassiteride insula primus apportavit Midacritus. Lib. vii. cap. 57.

† Timæus historicus a Britannia introrsus sex dierum navigatione abesse dicit insulam Mictim, in qua candidum plumbum proveniat. Lib. iv. cap. 16. vol. I. p. 233. Ed. Hardouin. Paris, 1723.

‡ Refingens *Ictim*. Diod. Sic. *Bibl. Hist.* Lib. v. cap. 22. vol. I. p. 347. Not. in l. 60.

Siculus is probably the same island, which Pliny, from Timæus, calls Mictis."*

The produce of this island, according to Pliny, was "candidum plumbum," corresponding with the "plumbum album" of Cæsar,† whose editors explain these words by the latin "stannum," and the Greek κασσίτερος, both signifying *tin.* ‡ But Timæus, as quoted by Pliny, also says, that this *Mictis*, or *Ictis*, "was six days' sail inwards from Britain;" which led Hardouin, the editor of Pliny, to remark, that its situation could not be certainly determined.§ Wesseling was also of opinion, that Timæus might have been led into an error as to the distance, by following an uncertain tradition of the common people.|| On this subject, Borlase says, "the distance here

* *Observations on the Ancient and Present State of the Islands of Scilly.* p. 77.

† Nascitur ibi plumbum album in mediterraneis regionibus. Cæs. *De Bello Gall.* Lib. v. c. 12.

‡ Ibid. Ed. Oudendorp. p. 225.

§ Quæ sit hæc Timæi Mictis in Germanico Mari sex dierum navigatione a Britannia dissita, stanti certo non potest. Plinii *Op.* vol. I. p. 223.

|| Fieri tamen potest, ut Timæus, incertum vulgi rumorem secutus, in eo spatio aberraverit. Diod. Sic. *Bibl. Hist.* vol. I. p. 347.

laid down is no objection to Mictis being one of the Scilly isles, for when the ancients reckoned this place six days' sail, they did not mean from the nearest part of Britain, but from the place most known, and frequented by them, (that is, by the Romans and Gauls,) which was that part of Britain nearest to, and in sight of Gaul, from which to the Scilly Islands the distance was indeed six days' usual sail, in the early times of navigation; therefore I am apt to think, that, by Mictis here, Pliny meant the largest of the Scilly isles, as I do not at all doubt but Diodorus Siculus did, in the passage mentioned above."*

But wherever we may finally determine Ictis to have been, it is probable, from what has now been said, and from the additional fact, that Dionysius Periegetes, a writer, who flourished in the Augustan age, and wrote a geographical treatise in Greek hexameters, expressly distinguishes the Cassiterides from the British Isles, that it formed no part of the present main land; and that Diodorus was correct, in representing it as an island, at high water. Nor is it possible to reconcile what he says about its vicinity to the promontory called *Belerium*, with the supposition,

* *Observations on the Islands of Scilly*, &c. pp. 77, 78.

that it was the district between the Land's End and the Hayle Sands.

Among the elements essential to a determination of the point in question, must be reckoned the fact of Ictis, and the neighbouring islands, being situated near this promontory. If we could determine, with absolute certainty, to what promontory Diodorus alluded, under the name Βελέριον, the chief preliminary difficulty in the present inquiry would be overcome. But this still remains a matter of doubt, some thinking that it is the Land's End, others Cape Cornwall, and others Tol Pedn Penwith. Nor will this difference of opinion be deemed surprising, when it is considered, that Cornwall was originally called *Kernaw*, probably from the Phœnician, קרן (Keren,) or the ancient British *Kern*, signifying *a horn*, on account of its numerous promontories. Ptolemy writes the name of this headland, Βολέριον. But he also calls it Ἀντιουέσταιον.* Now the proper names of places, compounded with the preposition ἀντὶ, are generally derived from the names of other places, to which they stand opposite: thus, *Antilibanus* is the mountain opposite to *Libanus*;

* Ptolem. *Geogr.*, Opera P. Bertii; Lib. ii. cap 3. p. 35. Qu. Ἀντιουέσσαιον.?

and *Antiparos* an island opposite to *Paros*. It is reasonable to presume, therefore, that Ἀντιουέσταιον was the name of some promontory, or headland, on the British coast, looking towards some other promontory, or headland, on the opposite coast of Gaul, bearing the same, or a similar name, but without the preposition. If then we take away the ἀντὶ, we have Ουέσταιον, *(Ouessant*, or *Ushant,)* the furthest headland of France to the west, distant about twelve miles from the continent, and on the south side of the English Channel, immediately opposite to the Land's End, which was probably the Βελέριον of Diodorus. Volaterranus says, that this promontory was once called *Helenum*,* but this is evidently nothing more than a various reading of *Belerium;* B having been converted into H, and RI into N, thus;

BELERIUM,
HELENUM.

Near this promontory, then, was the island of Ictis, to which, at low water, the natives conveyed the tin in wagons, for the purpose of selling it to the foreign merchants. But all which now remains, to mark the locality, is a small archipe-

* Gibson's Ed. of Camden's *Britannia;* vol. I. p. 148. Lond. 1772.

lago of barren rocks, called *The Longships.* These are situated about two miles west of the Land's End; and in the time of Diodorus were probably connected together, so as to form one island, which, at low water, was joined by an isthmus to the main land. Nor is this mere conjecture. "The inhabitants," says Camden,* "are of opinion that this promontory did once reach further to the west; which the seamen positively conclude from the rubbish they draw up."

It is well known, that there has been a considerable subsidence of the land on this coast. Of this, the animal and vegetable remains found at the Carnon stream-works, the submarine forest near St. Michael's Mount, and the disappearance of tithable land from beneath the cliffs of Penzance, afford ample proof; and if, as Sir Henry T. De la Beche has well remarked,† the present bed of the sea were raised about thirty feet, in the direction of the ten-fathom line, numerous small portions of dry level country would be produced in some situations, while in others large

* Gibson's Ed. of Camden's *Britannia;* vol. I. p. 148. Lond. 1772.

† *Report on the Geology of Cornwall,* &c.; chap. xiii. p. 421.

tracts of land would appear. The same writer says,* "if true proportional sections be constructed, and many miles from the land be included in them, the plain-like character of the floor of the sea adjoining such coasts as those of this district becomes very striking." The line of forty fathoms includes the whole of the Scilly Islands, and the line of thirty fathoms approaches within about six or seven miles of the Land's End; but between the latter and the main land the depth diminishes more rapidly, so as to render navigation exceedingly dangerous. The passage to the Longships' lighthouse is attended with so much hazard, that the men who have the charge of it are frequently unable to communicate with the land for two or three months together. Here then, in all probabilty, lay the island of Ictis.

Tradition says, moreover, that a considerable tract of country formerly existed, between the Cornish coast and the Scilly Islands, containing not fewer than a hundred and forty parish churches. Gibson, alluding to this tradition, says,† "about the middle way between the Land's-end and Scilly, there are rocks called,

* Chap. i. p. 24.

† Camden's *Britannia;* Ubi supra.

in Cornish, Lethas; by the English Seven-stones; and the Cornish call that place within the stones Tregva, i. e. a dwelling; where it has been reported that windows, and other stuff, have been taken up with hooks, (for that is the best place of fishing.)"

It would seem, that most of the islands on this coast received their names from some fancied resemblance, which they bore to different animals. Thus *Lethas*, (or *Lethowstow)* mentioned by Gibson in the passage just quoted, denotes *The Lioness*. A few miles south-west of the Land's-End there is still a rocky island, called *The Wolf*, which doubtless owes its preservation to the circumstance of its being composed of limestone, a species of rock better adapted to resist atmospherical changes than many kinds of granite; and near the *Lizard*, which probably derives its name from the elongated form which it exhibits, resembling that of a lizard, there is a cluster, known by the name of *The Stags*. *Ictis* itself, which may have been a translation from some old British word, is the Greek for *An Otter*;* and

* The meaning assigned by Scapula to the word Ικτὶς, is "mustela sylvestris." He adds, "Sunt qui viverram interpr." But Aristophanes, according to Brunck, uses it neither for *a*

it requires no great stretch of imagination, to trace an analogy, between the amphibious habits of this animal, and the mutable character of the Ictis of Diodorus.

The names of *The Lioness* and *The Wolf* may have been given, either from the roaring of the waves, and the howling of the storms, which have been known to produce such terrific effects in this sea; or from the danger incurred by the daring, or inexperienced mariner, on too near an approach to them.

> "Hinc exaudiri gemitus, iræque *leonum*,
> Vincla recusantum, et sera sub nocte rudentum,
> Sætigerique sues, atque in præsepibus ursi
> Sævire, ac formæ magnorum ululare *luporum*."
>
> VIRG. ÆN. VII. 15—19.

Allusion has already been made to inroads of the sea on the English coast, in the reigns of William Rufus, and John; and there are records of similar inundations in the times immediately

weasel, nor *a ferret;* but *an otter*. "Ικτῖδας ενυδρως, εγχελεις Κωπαϊδας. (Acharn. 880.) Certo certius est de animali quodam aquatico Bœotum hic loqui. Bene igitur vertit Brunckius 'lutras.' *Anglice*, otters."—*Lexicon Græco-Prosodiacum.* Auctore T. Morell, S. T. P. Edit. Alter. Londin. 1824. *Art.* Ικτὶς; p. 412.

preceding the conquest. Simeon of Durham, in his account of the reign of Ethelred II., says, that, in the year 1014, such an encroachment of the sea took place on the 3rd of the calends of October, when a great many towns, and an innumerable multitude of human beings were submerged;* and John Bromton, in his history of the reign of Canute, (A. D. 1017—1039,) says, "at that time the Lord also added to the ordinary calamities an extraordinary one, for the sea, rising above its usual level, overwhelmed several towns, with people innumerable."† It is probably this latter encroachment of the sea, a traditional account of which has reached us in the well known historical myth, respecting this monarch and his courtiers. They would fain have persuaded him, we are told, that the sea was obedient to his will; and he is represented as having rebuked

* Mare littus egreditur III. Kal. Octobris, et in *Anglia* villas quamplurimas, innumerabilemque populi multitudinem summersit.—Simeon. Dunelm. *Hist. de Gestis Regum Anglorum,* apud "Rogeri Twysden *Historiæ Anglicanæ Scriptores X;*" p. 171.

† Eo tempore eciam addidit Dominus malis solitis malum insolitum, mare namque insolito superius ascendens, villas in *Anglia* nonnullas cum populo innumero submersit.—Johannis Bromton *Chronicon,* ubi supra; p. 892.

their flattery, by commanding the sea to retire, and waiting the result, as though he expected that it would obey his mandate. But the tide advancing, and compelling him to withdraw, he is said to have taken occasion, from this circumstance, to let his base flatterers know, that the titles of LORD and MASTER belong only to Him, whom the land and the sea obey. It is also reported that, from this time, Canute never wore his crown again; but ordered it to be put upon the head of the crucifix in Winchester Cathedral. It is far more probable, however, that this act of humility was occasioned by the loss, which his kingdom had sustained, from the unprecedented encroachments of the sea, than by the mere circumstance of the tide, in its usual course, having presumed to wet his royal person. Bromton mentions another instance, in which the sea burst its boundaries; and did not return within its customary limits, till after a lapse of about two days.* This occurred in the reign of Henry II., A. D. 1176; so that, including the two cases mentioned in a

* Eodem anno mare extra fines in *Anglia* erumpens multos in *Holandia* homines et pecora absorbuit, et quasi post biduum furore sedato in semet ipsum rediit.—*Chronicon*, ut sup. A.D. 1176; p. 1117.

former part of this paper, we have no fewer than five recorded inroads of the sea upon the English coast, within the space of two centuries.

Of the causes, which led to the disappearance of the Ictis of Diodorus, history has preserved no trace. Whether it was the effect of a gradual subsidence of the land, or of the continued action of the waves and tides on a coast unusually exposed, we have no positive means of determining. Each of these may have contributed its share towards the final result. A depression of the land may have taken place in the first instance, and the inroads of the sea may have done the rest. It is well known, that the winds, which blow from the Atlantic on the Cornish coast, frequently bring with them storms of most destructive violence. The effects of these storms, combined with the disintegration of the softer beds of granite, from the ordinary atmospherical influences, are strongly exemplified in the caverns and hollows along this coast. There is something particularly striking in the Funnel Hole, at Tol Pedn Penwith. This hole resembles the hopper of a mill, being in shape like an inverted cone, from ten to fifteen fathoms deep, with an opening into a cavern below, communicating with a small creek, or bay, called Chair-ladder Cove ; and is supposed to have been formed by the decomposition of a portion of the rock. On various parts of the cliffs, too, patches of disintegrated granite are found, which have a sandy, or gravelly appearance, from which it may be inferred, that the ocean has made great inroads, in the lapse of ages, upon the rocks lying along this coast. Nor must it be forgotten, that the granite of the Scilly Islands is peculiarly liable to decomposition; that portions of these islands are rapidly disappearing from this cause alone; and that the surface of water which they displace is constantly diminishing, although their number is increasing, in consequence of their being broken up into smaller islets, by the abrading power of the ocean.

If, as is generally supposed, the Cassiterides of the Greeks were the Scilly Islands, their number in the time of Strabo was only ten.* It is now, probably, as many as *a hundred and fifty ;* and it is a singular fact, that they take their name of the *Scilly Islands* from one of their number, which, in its present attenuated state, is nearly

* Strabonis *Res. Geogr.* Lib. iii. p. 239.

the smallest in the cluster, its whole surface being not more than an acre. On this subject, the following extract, from Borlase's "Observations on the ancient and present state of the islands of Scilly,"* will be found deserving of particular attention. The writer has just been describing a certain eminence which he visited. He then goes on to say, "From this hill I observed the Guêl Hill of Brehar, and the isle of Guêl, stretch away towards the little isle of Scilly, and with it making a curve, of which Scilly is the headland; and from the furthermost hill of Brehar a promontory shoots, at the extreme point of which a vast rocky turret, called the castle of Brehar, *on every side many rocks show themselves above water, and intimate their former connexion with Brehar, and their being reduced to their present nakedness by the fury of the ocean.* From this disposition, therefore, of the rocks and islets on this side, we may answer a question which would otherwise be extremely difficult to solve, viz:—How came all these islands to have their general name from so small and inconsiderable a spot as the Isle of Scilly, whose cliffs hardly anything but birds can mount, and whose barrenness would

* Pp. 57—59.

never suffer anything but sea birds to inhabit there? A due consideration of the shores will answer this question very satisfactorily, and convince us that *what is now a bare rock about a furlong over, and separated from the islands of Guêl and Brehar about half a mile, was formerly joined to them by low necks of land, and that Trescaw, St. Martin's, Brehar, Samson, and the rocks and islets adjoining, made formerly but one island;* nay to these, I believe, I may safely add the eastern islands, and St. Mary's too, there being *great flats* reaching from St. Martin's almost to both, *all uncovered at low water, and having but four feet water in the deepest part.* This (at that time) great island had several creeks, such as New and Old Grynsey, and others, *by the sea's encroachments, or by the dipping of the islands,* since extended into harbours. It had several headlands, of which that now called Scilly was the highest, outermost, and consequently most conspicuous." Portions of the island of St. Mary's, too, which is now the largest in the whole group, covering a space of nearly two thousand acres, and which is one of the nearest to the main land, exhibit signs of rapid and certain decay; and it is calculated, that at no distant period, unless measures are taken to form an artificial barrier against the incursions of the ocean, a channel will be formed, which will divide it into two smaller islands.

Facts like these go far to prove that the Cassiterides of the ancients were no other than the Scilly Islands of our own time ; and that the difference between these islands, in their past, and in their present state, is attributable to subsidence, and to atmospherical and oceanic agency. The process by which so extraordinary a change has been effected, is precisely similar to that which has been in operation along the Cornish coast, and to which the disappearance of the Ictis of Diodorus may not unreasonably be attributed.

We arrive, then, at the conclusion, that such an island once actually existed; that it was neither the Isle of Wight, nor St. Michael's Mount, nor a portion of the present main land; but that the small group of rocky islands, called *the Longships*, are probably the summits of its more elevated parts, the rest being either submerged, or swept away; and that "the neighbouring islands," to which the same peculiarity attached as to

"Ictis," of being islands at high water, and peninsulas at low water, were no other than the Cassiterides, which have undergone a corresponding increase in number, and diminution in superficial contents, and now form two distinct archipelagos, the nearer and smaller one bearing the name of *the Seven Stones*, and the larger and more remote that of *the Scilly Islands*.


E Sibson Del

AN ACCOUNT

OF THE OPENING OF AN ANCIENT BARROW,

CALLED

CASTLE HILL,

NEAR

NEWTON-IN-MAKERFIELD,

IN THE

COUNTY OF LANCASTER.

BY THE REV. EDMUND SIBSON,

Minister of Ashton-in-Makerfield.

(Read October 17th, 1843.)

MR. BAINES' ACCOUNT OF CASTLE HILL.

Mr. Baines, in his History of the County of Lancaster, gives the following description of Castle hill :—

" At the distance of half a mile from, and to " the north of Newton, stands an ancient barrow, " called *Castle hill*. It is romantically situated, on " elevated ground, at the junction of two streams, " whose united waters form the brook, which flows " past the lower part of the town of Newton. The "sides and summit of the barrow are covered with "venerable oaks, which, to all appearance, have "weathered the rude and wintry blasts of centuries. "It is a spot well adapted for the repose of the "ashes of the mighty dead."

The streams which unite, at this barrow, are the Dene and the Sankey.

BRITISH ROAD.

This barrow seems to stand near the old British road, from the town of Haydock to the town of Lowton, both of which were probably British towns. This road runs from the town of Haydock, past Hall-meadow, down the Townfield lane; and, crossing the Wigan turnpike road, near Newton, it points to Castle hill, and the town of Lowton. The Townfield lane is six feet below the level of the adjoining ground, and is properly a British Covered Way; and, it is found, that, where the Roman road from Warrington to Wigan, crosses this old Lane, it slopes down to it on both sides; thus plainly showing that the Townfield lane was made before the Roman road. The British road seems to have crossed the Sankey, close to Castle hill, where two piers have been erected for a bridge.

ROBIN HOOD'S CAVE.

At this ford of the Sankey, there is a cavern, in the red rock, which is called Robin Hood's Cave.

FORM OF CASTLE HILL.

This barrow is bell-shaped, like those on Salisbury Plain; and its circumference is nearly circular. Its circumference, at the bottom, is 320 feet: at the top, 226 feet; and its height is 17 feet. See the drawing of Castle hill.

On the south and west sides of Castle hill, there is a large fosse, about 5 feet deep, and 30 feet wide: this Fosse appears to have been originally 7 feet deep, 2 feet of which have been cut in the rock; but, on the other sides there is no fosse; for, the ground slopes towards the Sankey and the Dene.

The foundation of the barrow has been upon the original green sward; and, the Barrow has been formed by the earth and rock taken out of the fosse.

There is a tradition, that Alfred the Great was buried here, with a crown of gold, in a silver coffin.

This barrow had long been an object of curiosity; and, for more than twenty years, there had been a general wish to explore it.

OPENING OF THE BARROW.

At length, with the consent of Thomas Legh, of Lyme, Esq., the Lord of the Manor of Newton, a number of colliers were employed by the Rev. Peter Legh, M.A., the Incumbent of Newton, Edward Holme, of Manchester, M. D., Mr. William Mercer, of Newton, agent to Thomas Legh, Esq., and by the Rev. E. Sibson; and, excavations were made in this barrow on July 6th, 7th, 10th, 11th, and 14th, 1843.

GENTLEMEN PRESENT AT THE OPENING OF THE BARROW.

The principal part of the barrow was explored on Friday, the 7th of July, in the presence of the Rev. Peter Legh, Edward Holme, of Manchester, M.D., John Roby, Esq., M.R.S.L., author of the Traditions of Lancashire, William Langton, Esq., of Manchester, a descendant of the ancient family of that name, formerly barons of Newton, James Dearden, Esq , lord of the manor of Rochdale, James Fenton, Esq., of Lymm Hall, Joseph Fenton, Esq., of Bamford Hall, William Beamont, Esq., of Warrington, John Robson, Esq., of Warrington, Editor of Three Metrical Romances, from the Blackburne M.S.S., John Green, M.D., of Newton, Samuel Ashton, Esq., of Bury, John Bimson, Esq., of Wigan, John Sharp, Esq., of Warrington, James Allen, Esq., of Newton, Peter Allen, Esq., of Newton, Arthur Potts, Esq., of Newton, and most of the gentlemen in the neighbourhood.

It was expected, that a Kistvaen would be found, containing amber and glass beads, ornaments of bone, and a Celt. It was also supposed, that the Kistvaen would be on the level of the original green sward; and, at this level, about 12 feet below the top of the hill, an opening, 4 feet square, was made on the west side of the hill; and this opening was driven forward horizontally, towards the centre of the hill, till it met a shaft, 6 feet in diameter, which was sunk from the centre of the top of the hill. This work was done by nine colliers, brought by Mr. Mercer, from Haydock. They began to work a little after noon on the 6th of July; and, they laboured, day and night, without intermission, with persevering energy, and hearty good-will, until nine o'clock in the evening of the following day.

DRIFT ON THE WEST SIDE.

The hill was found to be composed of clay and marl, red sand and red sandstone; but, the west side of the barrow was composed principally of stiff red clay.

BURNT CLAY AND CHARCOAL.

In the drift, on the west side of the hill, nearly on the level with the original sward, were found patches of burnt clay and marl, mixed with ashes and pieces of wood-charcoal. The ashes seemed to be coal ashes; for, a piece of unburnt coal was found in the clay; and this proves, that the use of coal was not unknown, when this barrow was made. Several stones were found, on which there were evident marks of the action of fire. The pieces of wood-charcoal were very light: the fibre of the wood was very visible; and, we should have thought, that the wood had been beech, if Cæsar had not said, that there were no beech trees in Britain. A cubical piece of this wood-charcoal, nearly three inches square, is in the possession of the Rev. Peter Legh.

OAK BRANCHES.

Roots, and branches of oak, were found imbedded in the marl, in this drift: the bark remained entire, and retained its original form and colour; but, the wood was entirely absorbed; and the space, once occupied by the wood, was now filled with the fine aluminous red clay, of which the barrow was partially composed. It was thought by Dr. Black, of Manchester, that these branches of oak, were in a state of transition from vegetables to fossils; and, that, in some two thousand years more, this aluminous clay would have become stone, and the bark would have been carbonized. In Fossil Wood, however, the fibres and grain of the wood are visible; but, in this aluminous clay, no trace of the wood was visible.

PERPENDICULAR SHAFT—ANTICIPATED DEPOSIT.

Most of the things found in the shaft, were similar to these discovered in the western drift. In sinking the shaft, however, from the top of the hill, it was observed by Mr. Mercer, that, on the north side, the earth was loose sand; but, that, on the south side, the earth was a compact body of clay and marl; and it was, therefore, thought by Mr. Mercer, that, the deposit contained in this barrow, would be found, under the coats of marl, on the south side of the shaft. Mr. Mercer also observed, that there was a short circular ridge on the south side of the top of the barrow.

The position of the original green sward, was easily seen in the shaft; for, there was almost a perfect ring of tufts of decayed grass round the shaft: these tufts of grass had the appearance of compressed hay; the blades of grass being long and strong. Below the grass, were seen the natural strata of earth, clay, and marl. Two feet below this ring of grass, a large living black beetle was found.

DRIFT FROM THE SHAFT TO THE SOUTH SIDE.

At the suggestion of Mr. Mercer, a tunnel three feet square, was driven horizontally, on the level of the original green sward, from the shaft, into the south side of the barrow.

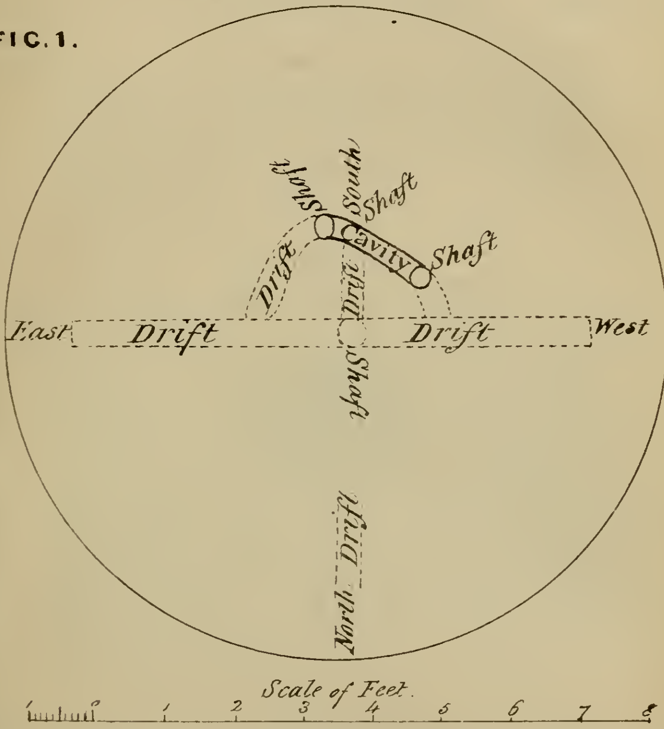
CHAMBER.

And at the distance of about ten feet from the centre of the barrow, on the south side of the shaft, a chamber was discovered, which, it is supposed, contained the original deposit. The base of this chamber was two feet broad, and was curved, as in the annexed figure No. 1: its length was twenty-one feet: its height was two feet; and its roof was a semi-circular arch. It seemed to be constructed of masses of clay, about a foot in diameter, rolled into form, in a moist state, and closely compacted

FIG. 2.

Acorn found in Chamber.

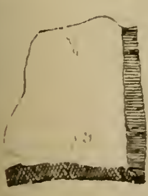
FIG. 1.



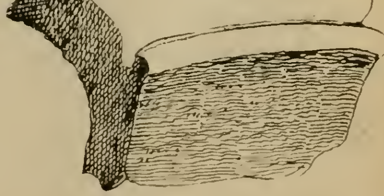
zontal Section of the Cavity & Workings at Castle Hill.

FIG. 4.

FIG. 3.



Whelstone



Fragment of Urn.

by pressure. When the chamber was first opened, the candles were extinguished, and there was great difficulty in breathing. The sides and bottom of the chamber were coated with an impalpable powder of a smoke colour.

ANIMAL MATTER.

The bottom of this chamber was covered with a dark coloured substance, about three inches in thickness. The external surface of this substance was like peat earth, being rough and uneven, and of a black colour. The inside of it, when broken, was close and compact, and somewhat similar to black sealing wax; and, when examined by the microscope, it was found to be closely dotted with particles of lime. And, it was thought to be a mixture of wood-ashes, half-burned animal matter, and calcined bones.

BEETLES.

An immense number of the elytra of small beetles were found imbedded in this cake of carbonized animal matter. The elytra were of a light puce colour, about a quarter of an inch in length. They were striated longitudinally; and, in the channels, were spherical dots, which, when examined by the microscope, had a very beautiful appearance.

It was thought, that these insects belonged to an extinct species, which had its existence, when the temperature of the earth was higher, than it is at present. But, Mr. Westwood, of Hammersmith, an eminent entomologist, says, that these beetles are of the species Cossonas, and are of a genus of Wood-boring beetles; and, that the species is not extinct; and, that he has specimens of the same kind, both from Greece and Egypt.

TRENCH.

This plate of animal matter, seemed to have been placed on the level of the original green sward. And, on the surface of this animal matter, was a covering of loose earth, about two inches in thickness, which seemed to have fallen from the roof, and sides of the chamber. Immediately below this plate of animal matter, a trench had been cut, about fifteen inches in depth; and two tiers of round oak timber had been placed in this trench. The first tier was notched into the green sward; and the second tier was nine inches below it. The horizontal distance of the several pieces, was about eighteen inches; and, the pieces, in the lower tier, were placed exactly opposite to those in the upper one. Several of the pieces were charred; and, many of them had entirely disappeared, leaving black marks in the sides of the trench, where they had formerly been placed. These pieces of oak appeared to have been three or four inches in diameter. In almost all the cases, the wood of these pieces had been absorbed : in some cases, the bark, on the under side of these pieces, was carbonized, and had nearly the appearance of coal ; and, in other cases, the bark, on the under side of these pieces, retained its original form and colour. In one case, however, one of these pieces, in contact with the animal matter, had the appearance of dry, decayed wood. This piece of wood is in the possession of Mr. John Robson, of Warrington.

The trench, below the plate of animal matter, was filled with clay. The oak being sacred to the Druids, it is probable, that the trench was made for the purpose of containing the tiers of oak, so that the bodies might rest upon sacred wood.

There was no appearance of either Kistvaen, or urn ; and neither armour, nor ornaments, were found. From this it may be inferred, that the barrow had been made in a very rude state of society, when the arts were in their infancy.

This chamber containing the animal matter, appeared to have been air-tight; for, its exact termination was discovered on the west side ; and, it was found to be closely sealed up with bundles of grass, a coat of fern, dry roots, and clay. The fern and the roots were placed against a vertical face of clay, on the outside of the chamber ; and they were not carbonized.

It seems probable, that the bodies had been partially burned, with wood and coal, on the green sward, on which the barrow was formed : that the partially burned remains of the bodies had been carefully collected, and deposited on the surface of the circular trench ; and, that then a wall, and a strong arch of moist clay, had been formed over these remains. It would be necessary, that this arch and chamber of moist clay, should become hard, before the barrow was formed ; otherwise, the weight of the barrow would compress, and destroy the chamber. Therefore, that the arch, and chamber, might be hardened by the sun's heat, on the outside, and by the heat of the half-burnt remains, on the inside, it would be necessary, that the two ends of the chamber should remain open, for a considerable time, before the barrow was raised. And, it is probable, that, when the half-burnt remains were cool, and while the two ends of the chamber were open, the beetles would creep in, and burrow in the animal matter. And, when the arch and sides of the cavity became hard and solid, the two ends of the chamber would be closed, and the barrow would then be raised. After the ends of the chamber were closed, the half-burnt animal matter would gradually become hard; and the beetles, imbedded in it, would die.

Pieces of the carbonized animal matter, are in the possession of nearly all the gentlemen, who were present at the opening of the hill.

It is probable, that this chamber contained the original deposit; and, that it had never been opened before.

DEVEREL BARROW.

When the Deverel barrow, in Dorsetshire, was opened, by Mr. Miles, in 1825, it was found, that the bodies had been burned: that most of the ashes had been protected by urns: that the urns were disposed in a circular form: that the most ancient urns were moulded by the hand, without the assistance of the potter's wheel: that these urns were made of earth, which had not been baked, but only dried in the sun; and, that, in this barrow, urns were found of better workmanship, evidently made upon a potter's wheel, in a more advanced state of society; which showed, that this barrow had been used as a place of burial, for a long period. Mr. Miles, in his account of the Deverel barrow, says: "all the urns, except one, "were placed with their mouths upward, which "appears a custom more prevalent in Dorset, than "in Wiltshire barrows; since, Sir R. C. Hoare, in "his introduction to ancient Wilts, observes, that, "the bones when burnt, were collected and placed "within the urn, which was frequently deposited "with its mouth *downward*, in a cist cut out of "the chalk."

The circular chamber, at Castle hill, corresponds to the circular base, on which the urns were placed in the Deverel barrow: the animal remains, placed on the green sward, and the arch of clay above them, correspond to the inverted urns, covering the ashes, described by Sir R. C. Hoare, in the Wiltshire barrows; only, the arch of clay, at Castle hill, seems to indicate a more ancient period, than the inverted urns in the Wiltshire barrows. The arch of clay, at Castle hill, seems to correspond with the passage in the book of Job, chap. xxi, 32—33, "yet shall he be brought to "the grave, and shall remain in the tomb. The "clods of the valley shall be sweet unto him."

As neither urns, nor celts, nor armour of any kind, nor any beads nor ornaments, were found in this chamber, it would appear, that this barrow, at Castle hill, is of very remote antiquity.

PROBABLE AGE OF BARROW.

In the Rev. Mr. Whitaker's history of Manchester, vol. I. pp. 7—8, we are informed, that a colony of Celts settled in Lancashire, 500 years before the Christian era; and that, about 150 years afterwards, this country was invaded by the Belgæ. As the barrow at Castle hill, seems to be one of the most ancient, it is probable, that some of the kings of the Celts, having been defeated, and slain, by the Belgæ, were buried at Castle hill, 2193 years ago.

ACORN.

An acorn was found, in the chamber, on the plate of animal matter. The plumula, and the radicle, were in a state of complete germination; though, it is probable, that this acorn had lain undisturbed, in this cavity, for nearly 2200 years. Both the plumula, and radicle, were of a white colour; and, the radicle was twisted in a spiral form, as in the annexed figure, No. 2.

This acorn is in the possession of Mr. Mercer, of Newton. An acorn, in a similar state of vegetation, was found, in 1810, by the Rev. William Marriott, in a barrow at Ludworth; and it is described by him, in his History of the Antiquities of Lyme.

IMPRESSION OF A HUMAN BODY.

On the roof of the east side of the chamber, Mr. Mercer discovered a very distinct, and remarkable impression of a human body. There was the cavity formed by the back of the head; and this cavity was coated with a very thin shell of carbonized matter. The depression of the back of the neck, the projection of the shoulders, the elevation of the spine, and the protuberance of the posteriors, were distinctly visible. The body had been that of an adult; and, its head lay towards the west.

POSITION OF THE CHAMBER.

There seems something remarkable in the position of the circular chamber, in which the animal matter was found. If the circular arc of this cavity be bisected, the bisecting line will point nearly to the position of the sun, at three o'clock in the afternoon; and, therefore, this line will nearly indicate that time of the day, at the summer solstice, when the heat of the sun is greatest, and when, therefore, it would be supposed, that the sun's influence was greatest. Now, Mr. Whitaker, in his History of Manchester, vol. 2, pages 182 and 184, informs us, that the Druids were Astronomers; and, that the sun was worshipped in Britain. It seems, therefore, that, in the construction of the circular chamber, at Castle hill, particular regard had been paid to the sun's influence.

RIDGE ON THE CREST OF THE HILL.

It is also remarkable, that the exact form, and vertical position, of the circular chamber is indicated by a ridge on the crest of the hill. This was first noticed by Mr. Mercer; and, this was also an additional reason, why the tunnel was driven, from the bottom of the shaft, towards the south.

As these barrows were burial places, for persons of distinction, until about the year 750, when the custom of interment in church yards, was brought over, from Rome into England, by Cuthbert, archbishop of Canterbury, it is probable, that many pilgrimages were made to this shrine of relics ; and, that devotees knelt and wept, on the top of this barrow, and struck their foreheads on this sacred ridge.

It is also known, that religious festivals were observed at these barrows; and, it is probable, that venerable and hoary Druids preached, from the summit of Castle hill, to listening multitudes.

EXCAVATIONS IN THE HILL.

In attempting to make further discoveries, at Castle hill, the shaft was sunk down to the rock, which was found at the distance of about eighteen feet from the top of the hill; and, it was discovered, that the surface of the rock was about four feet higher, in the centre of the hill, than in the centre of the fosse, on the west side of the hill. From this it appears, that the rock had been cut away, in the fosse, on the west side of the hill. In sinking this shaft, nothing was found but the black beetle already mentioned, and a small piece of wood charcoal.

Three shafts were also sunk, in the circular chamber, containing the animal matter; and the position of these shafts is shown, in the figure, already given, of the horizontal section of the hill. These shafts were sunk into the undisturbed strata of solid earth, nearly to the rock; and nothing was found, except the trench, and the two tiers of oak wood already described.

DRIFT ON THE EAST SIDE.

The drift, from the west side of the hill, was continued in a straight line, through the hill, to the east side; and, on the east side of the centre of the barrow, the earth was less compact, and was mixed with sand, and large fragments of red freestone.

WHETSTONE.

In this drift, on the east side of the shaft, and near the centre of the hill, a broken whetstone was found, which is represented in the annexed figure, No. 3. It was of freestone, of a fine grain, of a dull white colour, slightly veined with red; and the surface was finely polished. It was about five inches in length, and three in breadth. There is no stone like it in this neighbourhood. This whetstone evidently belonged to a much later period, than that of the circular chamber, in which the remains of animal matter were deposited. The whetstone is in the possession of the Rev. Peter Legh.

DRIFT ON THE NORTH SIDE.

On the north side of the barrow, about six feet above the bottom, a drift, four feet square, was carried horizontally, about eleven feet towards the centre of the hill; and, because the ground, on the north side of the hill, slopes towards the Dene, this drift was commenced at a lower level, than that on the west side. It was found, that the hill on this side, was formed of clay and red sandstone.

FRAGMENT OF AN URN.

Nothing was found in making this drift; but, when the workmen were filling up the drift, a fragment of unglazed pottery was found in the clay, which is represented in the annexed figure, No. 4. It was made of fine cream-coloured potter's clay, which was very soft and flexible, when the fragment was first found; but, which soon became hard by exposure to the air. It had been made on a potter's wheel; for, there were exactly circular marks on its surface. It did not seem to have been baked in an oven; but, its surface seemed blackened, and discoloured by fire. It had not been broken lately; for, the edges of the fracture were dull and discoloured. It seemed to have been part of an urn, whose diameter was about eight inches. This piece of pottery is in the possession of the Rev. Peter Legh.

As the first burials, in this hill, were made under a rude arch of clay; and, as this piece of pottery seems to have been a fragment of a funeral urn, made of fine clay, and highly finished, in an advanced state of the arts, on a potter's wheel; there must have been a long interval between the time, when the original deposit was made in the chamber, and the time, when this urn was made. And, from this it appears, that Castle hill had been a place of interment, for persons of distinction, for a long period. And, as only a fragment of this urn was found, it is evident, that a part of the barrow had been previously explored.

OAKS.

There are now ten oaks on the top of Castle hill: they may be 300 years old; and, they, probably, were planted by the Langtons, when they were barons of Newton. And, when the oaks were planted, a search would, probably, be made for antiquities, and the urns might then be found.

WHITE LADY OF CASTLE HILL.

It is scarcely necessary to add, that Castle hill is said to be *haunted* by a White Lady, who flits and glides, but never walks; who is sometimes seen at midnight, but never talks. She was, probably, like Sir Walter Scott's White Lady of Avenel, "when there stood the figure of a female clothed in white, within three steps of Halbert Glendinning."

> "I guess, t'was frightful there to see
> A lady richly clad as she—
> Beautiful exceedingly."
>
> *Waverley Novels.—The Monastery.*

COPPER COIN.

We were not a little puzzled by the trick of an artful boy, from Newton, who contrived to thrust an old halfpenny into a piece of clay, at the bottom of our excavations. The halfpenny had been in the fire, was much corroded with rust, and the impression was very indistinct; but, it was found to be a coin of George II. If this little trick had not been discovered, we must have inferred, that the barrow had been opened before.

As the Rev. Peter Legh had given an invitation to his friends to be present, at the opening of Castle hill, on Friday, the 7th of July, we soon found, that it would be necessary for the colliers to work all night, so that the openings might be ready. During the short twilight of a summer's night, there was a sharp, cold breeze, on Castle hill; and, we kindled a fire on the top of the hill. The eddying columns of white smoke rising under the lofty arch of oak boughs; the breeze veering, and whirling, two or three times round, from every point of the compass; the flickering flame throwing its glare of light upon the quivering arch of oak leaves; the dark and dingy faces of the colliers, standing close about the fire; and, the apprehension, that the White Lady of Castle hill, might be looking upon our rash proceedings with displeasure, produced an association of ideas not easily to be expressed.

OBSERVATIONS

ON

GUANO,

AS A

MANURE FOR POTATOES.

BY RICHARD PARR BAMBER, M.R.C.S.

(Read November 26th, 1844.)

"It is the duty of every man to endeavour that something may be added by his industry to the hereditary aggregate of knowledge and happiness. To add much, can indeed be the lot of few, but to add something, however little, every one may hope; and, of every honest endeavour, it is certain, that however unsuccessful, it will be at last rewarded." RAMBLER.

It is not intended to enter into any detailed account of the introduction of Guano into this country, or of its use for centuries in Peru. Its properties are now so well known as to render this unnecessary. From the voyage to South America of Don Jorge Juan, and Don Antonio Ulloa, published in this country in 1760, to the time when Baron Von Humboldt, on his return to Europe brought specimens for analysis, its power as a fertilizer appears to have been only known to a few of the continental chemists and men of letters. It is to the commercial enterprise of later years, that this important addition to our manures has been obtained. To those who feel an interest in Guano, an account of its analyses by Dr. Ure, in the last number of the Pharmaceutical Journal and Transactions, will amply repay them by its perusal.

My attention was more particularly directed to the use of Guano when engaged in some researches on the properties of manures in connexion with agricultural chemistry, and having found that a number of experimental trials had been made with it, in some of which its productive powers had been extolled, in almost unlimited terms of approbation; while, on the contrary, its value had been equally, and perhaps unduly depreciated, from its occasional failure. I was unable to reconcile this discrepancy of opinion.

In order to test its value as a manure, and to ascertain, by experiments made under my own observation, the correctness of the somewhat conflicting statements which had been published, I prepared a portion of land in April, by spade labour. The field is at Barton-upon-Irwell, near the canal leading to Worsley. It is rather low in situation, except at its upper part, having a south-eastern aspect, with a soil rather light and sandy. The lower part is wet, but has been improved by draining, and is always more fertile than the upper part. It was well marled about twenty years since. I have grown potatoes upon it for several years without any rotation of crop, taking care, however, to change the seed every year, and the crop has always been good, and of excellent quality.

It was planted in the beginning of May, although the long continuance of dry weather, and the prevalence of easterly winds were unfavourable. The potatoes were cut into sets of one or two eyes each, and some of their shoots, were from one to three inches in length. These were carefully preserved, as being most vigorous, and were cautiously placed in the ground. One pound of Guano was applied to each drill, being at the rate of 5 cwt. to the statute acre. The Guano was African, and cost in Liverpool £8 per ton.

As the soil was very dry, when two drills were opened, the Guano was sprinkled along the bottom, and on each side. The drills were then freely watered, the sets placed about eight inches apart, and were covered up directly. No rain fell for some weeks afterwards, yet the shoots soon appeared above the surface; those which had spritted, much sooner than the others. The failures were few in number. The vegetation, after rain fell, was most luxuriant, the stems being of a darker green than those planted with common manure. They were longer and of a larger size than usual, and I doubted whether the crop, from this circumstance, would be productive. It was pleasing to observe the small quantity of weeds, as I had been much annoyed with them in former years. Very few of the leaves were curled, but I occasionally noticed a peculiarity in some of the stems, which I had also seen in some varieties of the potatoe in preceding seasons. When the shoot emerged from the ground, a small tuber was formed, and from it another feeble shoot sprang forth. They were always removed, and brocoli planted in their places, with a sprinkling of Guano in each vacancy, which I afterwards regretted, as the plants grew up with such rapidity, that their leaves overshadowed the adjoining potatoes, and lessened their productiveness.

The stems withered sooner, and the potatoes were gathered in, several weeks earlier than usual. They were clean-skinned, free from scabs; which often arises when night-soil or stable manure is used. They were mealy when boiled. Many of the tubers were of large size, firm, and solid, and the quantity of small ones unusually small, scarcely one fourth in amount to that of former years.

I have given the results of each kind in a tabular form, for greater convenience of reference.

THE POTATOES WERE OF THE FOLLOWING KINDS:

No. 1.	Red Champions, ...	12 drills,	14	yards	each.
2.	Lee's Blacks or Sweeps,	18 „	14	„	„
3.	Do. do. do.	19 „	12	„	„
4.	Golden Balls, ...	7 „	12	„	„
5.	Do. do.	14 „	13	„	„

PRODUCE OF EACH.

	lbs.		lbs.	lbs.	yards.
No. 1.	300.	Average per drill,	25,	{ or 252, 1 load, } in	140
2.	600.	„ „ „	33, ...	do.	106
3.	560.	„ „ „	29½, ...	do.	104
4.	180.	„ „ „	26, ...	do.	116
5.	400.	„ „ „	28½, ...	do.	114

AVERAGE PER STATUTE ACRE.

	lbs.				lbs.			tons.
No. 1.	17,380,	...	69	loads of	252	each	....	$7\frac{3}{4}$
2.	23,000,	...	91	”	”		....	$10\frac{1}{4}$
3.	23,450,	...	93	”	”		....	$10\frac{1}{2}$
4.	20,900,	...	83	”	”		....	$9\frac{1}{3}$
5.	21,400,	...	85	”	”		....	$9\frac{1}{2}$

The above weights and estimates are merely approximations, and are only intended to show as nearly as possible, the average produce, excluding the small ones.

No. 1. Was manured with night soil and a slight sprinkling of Guano. Being in the highest part of the field, the manure, when the potatoes were dug up was found to be only partially decomposed. These potatoes are of very shy growth, their skins were occasionally scabbed. Their amount per drill some years since was $19\frac{1}{2}$ lbs.

Nos. 2 and 3. The Guano was mixed with twice its weight of fine coal ashes, and Nos. 4 and 5, with sand instead of ashes.

In a portion of Nos. 4 and 5, about a fourth part of the potatoes were rotten. These were of the white skinned kind, while those of a dark colour No. 3, which grew in a parallel direction, were unaffected.

I have endeavoured to ascertain the quantity of potatoes produced in different localities, and in soil of similar quality, in order to discover the relative difference of each, as to productiveness.

No. 6. In a field adjoining mine, 33 linear yards, produced 80lbs. of the same kind of potatoes as Nos. 2 and 3, cow-dung and night-soil used.

No. 7. In my own field, the crop of potatoes called pink-eye farmers, averaged one load in every 72 yards, manure employed—horse and cow-dung, with night-soil.

No. 8. Mr. Bell, the intelligent manager of Mr. Baines's farm on Chat Moss, states, that the average produce of potatoes, whether manured with Guano or night-soil, was 80 loads per statute acre.

No. 9. A farmer on Trafford Moss informs me, that his crop this year, will amount to 1 load for every seven square yards—about 100 loads per acre. Manured with horse-dung, cow-dung, and night-soil.

This, as well as the former, is only an estimate, but both of them may be considered as nearly accurate.

The result of these are placed in tabular forms like the preceding, and I have taken 70 yards by 69, as being the nearest approximation to the statute acre in every instance; allowing 22 inches between each drill in plough, and 18 inches in spade labour. A statute acre comprising 113 drills of the former, and 138 of the latter, of 70 yards each.

	lbs.		loads.		tons.			yards.
No. 6.	23,450	...	93	...	$10\frac{1}{2}$	...	1 load in	85
7.	27,720	...	110	...	$12\frac{1}{2}$	...	„	72
8.	20,000	...	80	...	9	...	„	100
9.	25,000	...	99	...	$11\frac{1}{4}$	...	„	80

There appears not much difference, between the produce of the drills prepared with the spade, and those with the plough, although the former exceeds the latter by 25 drills in the acre.

In a slip of land, on Chat Moss, adjoining the

Railway, about two miles from the Patricroft station, the following was the result of each drill, of fourteen yards in length.—

	lbs.		lbs.		
No. 10. Lee's Blacks	50,	small	3	1 load in	70 yards.
11. Scotch Cups	50.	„	3	Do.	70 do.
12. Bread Fruit	43,	„	5	Do.	84 do.
13. Strangers	57,	„	2	Do.	62 do.

This land was unreclaimed moss, not having been cultivated, but a *drain* running parallel to the Railway, had exhausted its superabundant moisture. It was heavily marled last winter. Manure used, night-soil.

THE PRODUCE PER ACRE.

	lbs.	loads.	tons.
No. 10.	34,770,	138	15½
11.	34,770,	138	15½
12.	29,880,	118½	13⅓
13.	40,350,	160½	18

This is an extraordinary result, when the quality of the land is considered, almost entirely peat. The productiveness is doubtless owing to the heavy marling it had received, and to the consequent addition of the lime, silica, and alumine, the chief constituents of the marl; affording to the peat, those ingredients in which it was deficient. The drills were prepared by spade labour. Had Guano been used, the result would probably have been equal. The experiment is worthy of trial.

In the Gardener's Chronicle, of December 1842, there is a communication from Mr. Ford, of Sheaf House, Sheffield, in which he states the results of an experiment on potatoes, made with Guano, 1lb. to each row of 15 yards; the same quantity of Nitrate of Soda. Horn-shavings, and good stable-dung were also used, and some rows were planted without manure.

The following is the result of these experiments, with the average of each kind of potatoe. They were of three kinds, American natives, Short-topped reds, and Prince regents.

AMERICAN NATIVES.

			lbs.
With Guano,...............	3 pecks	each row	63
" Nitrate of Soda,...	2 "	"	42
" Horn-shavings, ...	2 "	"	42
" Stable-manure, ...	2¾ "	" ......nearly	58
Without manure, rather more than		....	42

SHORT-TOPPED REDS.

		lbs.
With Guano,..............	1 bushel each, ...	84
„ Nitrate of Soda,...	2 pecks and a little more, ...	44
„ Horn-shavings, ...	the same, ...	44
„ Stable-dung, ...	3 pecks each, ...	63
Without manure, the same as Horn-shavings, ...		44

The Prince Regents yielded, as near as possible, in quantity to the Short-topped reds, with all the manures. Those grown with Nitrate of Soda and Horn-shavings, were all extremely scabby, while those grown with Guano and Stable-manure, were beautifully clear skinned, and free from scabs.

This ground was a light loamy soil, with a gentle declivity to the south. It had produced oats the preceding year. The Guano was Peruvian, probably superior in strength to African by 20 or 25 per cent.

AVERAGE PER ACRE, AMERICAN NATIVES.

	lbs.	loads.	tons.	lds. yds.
With Guano, ...	33,220.	132 nearly,	15 nearly,	1 in 60
„ Stable-manure,	30,700.	122.........	13¾nearly,	1 „ 65
„ Nitrate of Soda, Horn-shavings without manure ...	24,900.	99 nly. ab.	11	1 „ 80

SHORT-TOPPED REDS.

	lbs.	loads.	tons.	lds. yds.
With Guano, ...	44,280.	176 nearly,	19¾	1 in 45
„ Stable-manure,	33,220.	132 nearly,	15 nearly	1 „ 60
„ Nitrate of Soda, Horn-shavings without manure ..	23,180.	92.........	10⅓nearly	1 „ 86

These amounts nearly approximate to those produced on Chat Moss, adjoining the Railway.

Mr. Bell, of Villa House, near Listowell, county Kerry, states, that "he watched carefully the progress of the growth of potatoes planted with Guano. In a few weeks the bulb was largely formed, and of the size of a hen-egg before the bulb was visible by the farm yard manure. His crop was very large, and worth £50. per acre, in the ground."

As no mention is made of the quantity of Guano applied, nor whether the produce was that of a statute, or of an Irish acre, I am unable to form any opinion regarding its productiveness. Thirty tons, or 67,200 lbs., rather more than 266 loads, is the greatest recorded crop. 266 loads at 3s. 9d. per load, will amount to nearly the above sum; but from the quantity of potatoes grown in Ireland,

and their consequent cheapness, an estimate of 3s. per load, is nearer their average marketable price. This would require above 333 loads to produce that amount, or 84,000lbs.

Three statute acres, constitute 1 acre, 3R. 16P. Irish. It is most likely the sum above mentioned is for the produce of an Irish acre.

I have been informed, that near Nantwich in Cheshire, in a very favourable season, that a statute acre has produced one load in every 70 linear yards, equal to 28,476lbs. or 113 loads, or, 12¾ tons nearly.

The weight of a potatoe crop, is stated by professor Low (Treatise on Agriculture, 1843,) to be in Great Britain generally, per statute acre, 17,920lbs., or 8 tons. In nine agricultural districts in Ireland, Mr. Wakefield states, in 1812, the average to be 13,669lbs, or about 6 tons; while professor Johnston (Lectures on Agricultural chemistry and Geology) estimates an average crop at 13,440lbs., or 6 tons, and the maximum 67,200lbs. or 30 tons. Dr. Daubeny states it at 13,000lbs. or 6 tons.

From the superior method of cultivation now practised in Ireland, it is probable that the produce is much greater than in Mr. Wakefield's time.

M. Payen, the eminent French Agricultural chemist, has given an account of the produce of seven kinds of potatoes, all grown on the same kind of land.*

THE VARIETIES ARE THE FOLLOWING:

Rohan, ...	14½	tons per	acre.
Large Yellow, ...	9⅓	"	"
Scotch, ...	8	"	"
Slow Island, ...	14	"	"
Legonzac, ...	8	"	"
Siberian, ...	10	"	"
Duvillers, ...	10	"	"

The Rohan and Slow Island may be considered as equal in productiveness to the Farmer, one of our most common potatoes. I planted the Rohan for two years, but discontinued it from its inferiority to the Farmer in its farinaceous properties.

* For some of these statements, I am indebted to a volume lately published, entitled, "The Industrial Resources of Ireland," by Robert Kane, M. D. A valuable statistical work.

The following tabular view is drawn up, not by way of contrast with the preceding, but to exhibit the results obtained from the employment of Guano and the other manures, specifying also the mode of husbandry employed.

FROM GUANO.

No. 1.	Red Champions, ...	$7\frac{3}{4}$ tons per acre	spade.
2.	Lee's Blacks, ...	$10\frac{1}{4}$,, ,,	spade.
3.	Do. do.	$10\frac{1}{2}$,, ,,	spade.
4.	Golden Balls, ...	$9\frac{1}{3}$,, ,,	spade.
5.	Do. do.	9 ,, ,,	spade.
8.	On Chat Moss, ...	9 ,, ,,	plough.

FROM HORSE-MANURE AND NIGHT-SOIL.

No. 6.	Lee's Black, ...	$10\frac{1}{2}$ tons.	plough.
7.	Farmers, ...	$12\frac{1}{2}$,,	plough.
9.	On Trafford Moss,	$11\frac{1}{4}$,,	plough.
10 & 11.	On Chat Moss, each	$15\frac{1}{2}$ tons.	moss land newly marled. spade.
12.	Do. do.	$13\frac{1}{3}$,,	moss land newly marled. spade.
13.	Do. do.	18 ,,	moss land newly marled. spade.

From the foregoing remarks it appears that Guano possesses stimulant powers in the production of potatoe tubers, equal, and in some instances superior to the manure usually employed. That I have not procured an amount of produce corresponding to that mentioned by Mr. Ford, may be owing to the land not having had a rotation of crops. It is, however, much above an average product.

The portability of Guano gives it a decided advantage, as 5 cwt. of African, at 8s. per cwt. is sufficient for an acre of potatoes ; while 40 tons of night-soil will be required for the same purpose. This will cost, at a distance of 8 or 9 miles from Manchester 5s. per ton, while horse-dung is much dearer, and is frequently ineffective in fertilizing power, from imperfect decomposition, and the consequent loss of its volatile principles. It will, however, be necessary to add fresh Guano with every additional crop ; as it does not appear to possess those qualities of permanency when applied to wheat, rye, and the other cereal grains, or to grass land. Juan and Ulloa state "that it is not used for wheat and barley, but when applied to fields sown with maize, and with proper waterings, it is found greatly to fertilize the soil ; a little of it being put close to every stem, and immediately watered."

Horse-manure and night-soil, may have their volatile principles retained by the admixture of chemical substances. Dr. Daubeny states, (Lectures on Agriculture,) "that night-soil may be rendered both inoffensive and portable, by admixture with animal charcoal, obtained from the refuse of the sugar refiners." And, "when we recollect, with what avidity the smallest quantity of manure is collected in countries where it is scarce, it seems hardly possible to calculate the enormous annual loss hitherto sustained by this country, owing to the excrementitious matters derived from a vast metropolis like London, being all deposited uselessly in the Thames."

"Owing to this waste, it is a curious fact, that cattle are slaughtered in South America, often for no other purpose, than that their bones ground into dust should be sent over to manure the turnip fields of England; so that our commerce has been hitherto compelled to supply from a great distance a material, which an application of chemical skill at home now promises to secure to us."

Night-soil possesses the same freedom from weeds as Guano; but this is not the case with horse-manure. Dr. Daubeny also remarks, "when the excrements of the horse or ox are employed, we are obliged to allow of their undergoing a long previous process of fermentation, by which a large portion of their valuable matter is got rid of, in order as much as possible, to destroy the vitality of the seeds, which pass undigested along with the fæces. And after all many still remain, and are thus introduced into the fields when the manure is scattered over them."

"By the use of night-soil we avoid this inconvenience, and hence it is, that in China, where it is exclusively employed, the corn-fields are remarkably exempt from weeds."

In order to obtain uniform success by the use of Guano, or of other manures, a knowledge of the constituent principles of a soil is requisite, and this can only be done when the farmer can obtain an analysis of it by a competent chemist. Guano, however, even when genuine, sometimes fails, is not always equally productive, yet, in some soils and localities, it is superior to most of the substances hitherto employed.

A neglect in using the following precaution, is a frequent cause of failure. It should be well mixed with the soil, either alone, or combined with ashes, charcoal, sand, or any other substance which can be easily procured, so that it does not come into actual contact with the article sown or planted. Its properties are soon developed; but if it be placed in the ground in dry weather, or if it be not watered, it will fail. It is the custom in Peru, when it is applied, to irrigate the land immediately.

Another course of failure is its adulteration by unprincipled dealers. An article has been sold for it, which did not contain 20 per cent of genuine Guano. A deceitful or false analysis has procured the sale of a cargo of it at a high price, and Dr. Ure states that when it came under his cognizance in consequence of having been employed professionally to analyze the identical cargo, he found the Guano to be nearly rotten and effete.

It is probable, from the extensive use now being made of Guano, that at a period not very distant, its supply may be, either exhausted, or much limited in quantity. In the Liverpool Mercury, of November 15th, it is stated, that 400 sail of vessels were then at Ichaboe, and that several had returned home without cargoes. This is worthy the attention of practical chemists, for if artificial manures can be found which will afford the farmer a cheap and permanent supply of their substances, which will give to his fields an increase of fertility, he will be rendered independent of the precarious supply obtained from foreign and distant regions. Should a state of war occur, the difficulty in procuring Guano might be insurmountable.

The formula, now commonly known as Dr. Liebig's compost, for furnishing inorganic matters to wheat, barley, &c., is a step in advance towards the accomplishment of this object. This compost, by admixture with night-soil, will supply the land with the silicate of potass, which is wanting in human excrement; and also in Guano.

Various attempts have been made in England, and on the Continent, to consolidate the night-soil, and prevent its ammoniacal contents from escaping; but Liebig states, that the method adopted for that purpose on the Continent, is defective: this I believe is also the case in this country.

I intended to make some remarks on the farinaceous properties of the potatoe, but as the experiments, now in progress, have become more extensive than I anticipated, their results, with any observations which I may think interesting, will be communicated to the society, before the expiration of the present session.

ON THE

SETEIA AND BELISAMA

OF

PTOLEMY.

BY J. BLACK, M.D., F.G.S.

(Read 2nd April, 1844.)

A cursory survey of the coasts of Lancashire and Cheshire affords many evidences, from their topographical features and geological formations, that the sea-level, both along them and up their estuaries, has undergone several important changes. Prior to the earliest of our historical records, and most probably before the foot of man ever trode these shores, it would appear, from the entombed *turbaria* or peat strata, and the submerged forests having prostrate trunks as well as their stoles yet standing upright, that a great extent of the Fylde country and of West Derby, was once covered with a primeval forest and a dense surface of vegetation; and that these had a much greater latitude towards the sea, than the present extent of the land reaches.

From Mr. Greenough's survey, it seems, that "This subterranean forest extends from near Penwortham, along the coast, to the Mersey, near Liverpool. The inner line of this forest is by Longton Moss and Muchhool, crosses the Douglas, goes by Rufford, in a direct line to Ormskirk, passes on to Litherland and ends in the Mersey opposite Everton. The parishes of Penwortham, Muchhool, Rufford, Altcar, and part of Walton stand upon this forest.

"It takes a line pretty nearly to Crowlands, it extends to St. Michaels, then along the canal line to Lancaster, including the west of the Lune. It goes along the Kendal road to Marton—at Cartmel, it again appears, and extends into Furness for three or four miles, and is a little seen between Milnthorpe and the Sands."*

It further appears, that this once living surface of forest trees and cryptogamic plants became submerged by some cataclysmal inundation, of unknown duration, but having great force, and conveying the superficial detritus of more north-

* Proceedings of the Geological Society, Vol. I., p. 366. Greenough's paper, 1819. Also, Lardner's Cyclopædia, Phillips' Geology, Vol. I., p. 302.

erly countries and of marine exuviæ from a greater or less distance. During this submergence, extensive deposits, both conglomerated and stratified, of gravel, sand, and marly clay, took place, which, on the subsidence of the sea-level, appeared to have completely covered up this land of primeval vegetation. The remains of this once living surface, in subsequent periods, was, and is now at times discovered, at varying depths beneath the soil, by trenching, or by the degradation of the tides, as at Blackpool, Crosby, and Leasowes in Wirral.

The recession of the sea-level during the period of this emergence of the land, appeared not to have been uniformly gradual, but rather intermittent, as the remains of what are called sea-terraces or raised beaches afford very probable evidence. These may be observed at Clifton, round by Kirkham, and on by Poulton in the Fylde, in Walton and Leyland on this side the Ribble, and along the shores of the Dee and Mersey in Wirral and Broxton.

Of late years, at least during the last half century, it would seem that the sea is again slowly encroaching, in some places, on the deposits which it formerly left dry, as the red marl cliffs at Blackpool, and the denudation of the submerged forests at Crosby and Leasowes induce us to infer.

This varying sea level at different epochs, may naturally be supposed to give rise to corresponding differences in the descriptions and maps of geographers living at remote periods of time from each other, and especially, if they were severally ignorant of what their predecessors had observed and recorded. Without this chain of knowledge and collated observation being kept up, the same countries, at different periods, may be so described, as to lose almost their identity; for we may truly say, with an old topographist, *Valans*, that "time worketh many alterations, both by water and land, and where great rivers did sometimes run, now it is utterly drie, and instead of marine waters, we have goodly and flourishing meades."

Leaving, however, and to some other occasion, any further observations, we would wish to make upon the more exclusive geological views, connected with this subject; we shall proceed to what only of a geographical and subordinately geological interest falls within the historical era—with the view of noticing and comparing the several sites and descriptions that have been given of the principal points in the topography of the coasts which have been mentioned.

It is well known to all readers of ancient geography, that Ptolemy was the first author who has left us anything like a chart and description of these parts, in common with his map of the whole kingdom; and that the localities of this coast, as laid down by him, have been a renewed subject of investigation and controversy with subsequent antiquaries, for to reconcile physical appearances at the time, or, the several views of the contraversialist, with the statements of this ancient geographer and astronomer. Ptolemy, in his chart, which I have rudely copied,* but accurately enlarged by the scale, lays down the coasts under review, as having four principal inlets or bays, which he denominates, thus—

	LON.	LAT.
1 Moricambe Æstuarium	17.30	58.20
2 Sistuntiorum Portus ...	17.29	57.48
3 Belisama Æstuarium ...	17.30	57.20
4 Seteia Æstuarium ...	17.0	57.0
Toisobius fluv (Conway)	15.42	56.29

* The map was here exhibited, along with several others down to the Ordnance ones, in illustration.

As this geographer has not distinctly noticed in the map or otherwise, the separate openings of the Mersey and Dee in proximity with each other, nor traced their separated courses interiorly, much uncertainty and dispute have arisen about the appropriation of the estuaries enumerated to their several actual localities; as they have, long since his time, been observed to exist much as they do at the present day.

All antiquaries agree that the *Moricambe Æst.* should be identified with the present bay of that name; and the great majority are agreed that *Seteia Æst:* should be placed at the mouth of the Dee. The chief controversy has been about the two others—whether the *Portus Sistuntiorum* should be the harbour of Lancaster,—the *Alauna*, or the mouth of the Ribble; and consequently, whether the *Belisama* should be the estuary of the Ribble, or that of the Mersey.

We may first observe, that Horsley, in his corrected map of Ptolemy, has laid down the above named estuaries in a manner which neither the map nor the corresponding latitudes and longitudes appear to authorize, as they are stated by the ancient geographer. It may be seen from his corrected map, that Horsley makes the *Setuntiorum Portus* or haven, the mouth of the Ribble, the *Belisama* the Mersey, and *Seteia*, the estuary of the Dee, but as Ptolemy has laid down twenty miles of difference of latitude between the two latter estuaries, we cannot see the reason of this nearer juxta-position of them on such different parallels. Besides he places *Rigodunum* at Warrington, whereas this Roman name is almost universally applied to *Coccium* on the Ribble, and is so marked in Ptolemy's map.

In the map accompanying Antonine's Itinerary, this whole outline of the west coast is depicted with a nearer likeness to modern geography—all the river-mouths are marked open, but without any names attached to the several estuaries. The rivers inland are traced to their main divisions, and there is besides marked a water communication between the Dee and the Mersey, nearly in the line of the present canal, between Whitby and Chester.

In the map of the *Notitia*, which was supposed to be drawn up in the year 435 A. D., the lines of the coast, estuaries, and main branches of the several rivers are also generally laid down, nearly according to their present configuration, but the communication between the two above-mentioned rivers is much wider, as well as the bed of the Gowy is shown to be much larger than in subsequent maps. Names are, however, unfortunately not attached to the several estuaries, which, if they had been, would have enabled future topographists more clearly to have understood Ptolemy.

In Richard of Cirencester's map, the Mersey is named *Seteia fluvius*, while the Dee is marked *Deva ;* the Ribble, the *Belisama ;* and the Lune, *Alauna.*

Cellarius, in his Geography, p. 259, though he notices Cambden's opinion, that the Dee was the ancient *Seteia*, and that the Ribble was *Belisama*, yet says, " *Quod mersejum nunc æstuarium, a Mersey fluvio dictum, videtur Seteia Ptolomæi esse.*"

D'Anville, again, places *Seteia* as the Dee mouth, and *Belisama* the Mersey. He affixes no name to the mouth of the Ribble, but notices Moricambe *Æstuarium* in the right place.

Baxter, in his *Glossarium Antiquitatum Britanniarum*, applies *Belisama* to the mouth of the Ribble. His etymology of this ancient name is, *Bel-is-ama*, from *bel-caput*, and *Amnis* a river, the head or mouth of the river. *Belisama*, he says, was also the name of a goddess. Though this Glossologist makes Lancastrum, the *Caput Segantiorum*, or *Sistuntiorum*, yet he says the *portus* or *haven* of the *Sistuntii* or *Segantii* was Litherpool, sive *pigra palus*—the *Amnis ostium Mersey*. Setting aside any criticism about this author placing the Segantü so far south, as neither in Richard's map, nor in Ptolemy's accounts, are these tribes placed farther south than the Ribble, it is worth noticing the old name of Liverpool, viz. *Litherpool*, which signifies a *standing* or *sluggish* pool, corrupted sometimes into Lyrepool. The original prefix adjective is yet retained in *Litherland*.

Dr. Stukely makes the Ribble the *Belisama*, while the Rev. Mr. Whitaker, in his History of Manchester, is a keen advocate for appropriating *Belisama* to the Mersey, in opposition to the strong reasoning of the former antiquary. A great point in the difference between them was the settling the site of *Rigodunum*, for that was upon the north bank of that River, whose mouth was the *Belisama*.

It appears that Mr. Whitaker had relied too much upon the topography of Richard of Cirencester, for Dr. Whitaker says, (p. 6.) "But Mr. Whitaker had an unfortunate theory to support; he had implicitly addicted himself to the dreams of a monk, before whose unsupported conjectures the contemporary and decisive authorities of Ptolemy and Antonine were equally to give way." Dr. Whitaker then, is opposed to the Manchester historian on these points, for he fixes the *Belisama* to the Ribble, and the *Portus Setuntiorum* at Lancaster. The *Seteia* he places at the estuary of the Dee, as he thinks that Ptolemy overlooked the mouth of the Mersey, if it existed in his days; for Dr. W. says, that there are some natural appearances to support the opinion, that at that time, the Mersey flowed over the low lands of Wirral. I need scarcely mention that Dr. W. moreover differed from his namesake as to the site of *Coccium*, which the historian of Whalley places at Ribchester, called also *Rigodunum*, but which Mr. W. fixes at Blackrode, and *Rerigonium* for *Rigodunum* at Ribchester, while he makes the

Portus Setuntiorum to be Freckleton, at the mouth of the Ribble.

Mr. Baines, in his history of Lancashire, follows Mr. Whitaker, implicitly, in this part of topography; making *Coccium*, Blackrode; *Rerigonium*, Ribchester; the *Belisama*, the Mersey; and *Setuntiorum Portus*, the mouth of the Ribble. The *Seteia* being, both by him and Mr. Whitaker, placed as the estuary of the Dee.

Having stated the principal comparative views that have been taken by historians and antiquaries on these estuaries as first laid down by Ptolemy, who only mentions four, including the two extreme openings in the coast—the *Seteia* and *Moricambe Æstuarium*, about the location of which there has been little dispute, but as there are three intermediate inlets—the mouths of three principal rivers, and only two designations—*Belisama* and the *Setuntiorum Portus*, the difficulty and the principal dispute have been to appropriate them —for one of the three apparently wants a Ptolemean name. Two questions naturally arise from this view of the subject—whether Ptolemy had actually overlooked, in his coasting survey, one of these river mouths, or whether some one of them did not separately exist in his time. Now to bring these questions to any satisfactory solution, it will be better to look at the actual physical geography of the coast, compared with the remaining evidence of what its condition was in former ages.

I cannot, in pursuance of this part of the subject, do better, than make use of some of the very judicious and philosophical observations which Mr. Ormerod has written on this point, in his Introduction to his Historical Survey of Wirral, in his splendid work on Cheshire. He says, "That the waters, before the retiring of the sea from the western coast of Britain, occupied the line of these vales; viz: that of the Gowy and the Dee, will be doubted by no one who has looked down upon the general level of the country, either from the forest hills, or from the ridge of the great natural terrace near Alford or Churton. A tide, a very few yards higher than usual, would now cover them to a considerable extent." He also says, "That a tide much lower than would suffice to cover these levels would fill the smaller valley, which intervenes between Wirral and Broxton, and render the former hundred a complete island, as tradition still maintains it to have been at a remote period." The present Ellesmere canal, it may be remarked, runs through this intervening valley, and as it is without any locks between Whitby on the Mersey and Chester; and its top level only at the height of thirty feet, from two locks, above high water on the Mersey, so the communication between the two rivers might easily be effected by a comparatively slight elevation of the water level in the estuaries.

That the sea-level stood much higher, in even what are the historical epochs of the country, there is every evidence, both from obvious geological changes and from the names of places. In the course of the canal and in the neighbourhood, as at Chorlton and Coghall, are found deposits of sea sand mixed, more or less plentifully, with shells, whole and comminuted, such as are found on our coasts at this day. In the Inee and Frodsham marshes are found, prostrate and embedded in silty clay, forest trees, as oak, of large dimensions, but nowhere is peat found covered up in these levels. That this higher sea-level was not local, is inferred from the tide, during the time of the Romans in this country, flowing up the Ribble to *Coccium*—as boats and anchors have been disentombed at Ribchester—its subsequent name. Even in the days of Leland, the tide flowed further up the Ribble than at present; for he says, "It floweth and ebbeth in Rybyl most communely more than half-way between Preston and Ribchester, and at rages of spring tides further," (vol. iv. p. 1—22. This same laborious Itinerant, in speaking of Thellwal on the Mersey, says, "Thellwaul, sumetime a havenet and little cite, as it appeareth by the King's Recordes. Now fischegarthes mar the haven, and the old towne, now a poore village. It standith a ii miles upward from Warrington."* He also notes, "Wyrall beginneth less than ¼ mile of the city of Chester, and within two bow-shot, Flokar's Broke cometh into Dee Ryver, and there is a Dok, where at a spring tide ships may ly, called Porte Poole." These remarks of Leland as to the rise of the tide at three different places on the several principal rivers on our coast, about three hundred years ago, show that the sea-level was then much higher than it is now; and countenance the further opinion that it stood at a still higher level in more early times.

The names of many places along the estuaries

* "Thellwaul was so called bycause it was walled about with greate Logges or Timber postes." *(Ibid.)*

and valleys of the Mersey and Dee, strongly corroborate the opinion of the former submergence and insulation of these localities, now high and dry, and forsaken even by the highest tides.

We have, on both sides of the course of the Gowy, several places with names ending in the Saxon affix-*ford*, such as *Wimbold's Trafford*, *Bridge Trafford* on the one side of this stream, and *Mickle Trafford* on the other side; while there is *Stamford* and *Stapleford* higher up the valley. There is also *Backford* and *Alford* on the raised terrace of the Wirral on the line of the canal. That insular elevation of new red sandstone, called the Ince, standing at one hundred and seventeen feet above the level of the surrounding marshes, was no doubt an island in the midst of the waters of the *Meersee* in former times. Its name being derived from the Celto-British word *Innis*—an island, further supports the idea.

The name Wallasey, also denotes its original insular condition, signifying the *island* of the *Walli* or *Welch*, as *Anglesey* is the island of the *Angles*. In the Saxon times, and long afterwards, it was written *Walleia—a*, or *ey*, being the affix for an island in the Saxon tongue. That this part, if not Wirral itself, remained an island less than four hundred years back, may be gathered from Harrison, who details his journey in these parts, in the early part of the sixteenth century. I quote from an extract in Baines' Lancashire, he says, "tyll we be past Wyrall, out of *Leirpoole* Haven, and the blacke rocks, that lye upon the north point of the aforesayd island."—p. 92.*

I cannot find that either of the etymons of Wirral, has any signification relating to an insular position: Baxter, derives the word from the British " *Uidr hal*, quod est aqua cerulea salsa." The prefix is derived by some from *Wire*, a wood, in Anglo-Saxon, which at least is more significative of its former condition; for it was, at the earliest times, covered with wood, and at length formed into a forest by the third earl of Chester, but was disafforested in the reign of Edward the third. Birket, Birkenhead, and Woodside, are all names denoting its former wooded condition.

* He further says, in speaking of the district rivers,

"Yrke, Irwell, Medlock, and Tame,
When they meet with the Mersey, lose their name."

And, " Finally, our Mersey goying to Moulton, it falleth into Lierpoole Haven, when it is past Runcorne."

Taking now into view the accounts and inferences which we have derived from ancient historians and topographists, we shall have little hesitation in allowing a much higher height to the sea-level on our coasts than at present exists. If we also take a survey of the present boundaries of the estuaries and the adjoining lands and vallies, it may easily be seen how a rise of the present water-level, even to forty or fifty feet, would produce such great changes on the face, both of land and water, as would materially alter the geographical features of the district.

When we also consider how deeply Wallasey Pool separates even now the north part of the peninsula from the rest of Wirral, and how the line of division is carried on by the Birket Brook almost on a level to near West Kirby, we have no difficulty in seeing that a level rise in the tide, short of thirty, if not of twenty-five feet, would completely insulate the promontory of Wallasey, and would also submerge a great extent of low land towards Leasowes, Great Meoles, and on to Hilbree. The rails at the Birkenhead railway station are but twelve feet above high water, and twenty-five feet below the station at Chester; while the canal between the Mersey and Dee, as we mentioned, is only thirty feet above high water, and without any lock in its course.

When we further consider, that all the flat country, called the Ince Marshes, lies at a level of half-tide, and would be widely overflown, if it were not for the cops along the water's edge; and that at the distance of four and a half miles up the valley of the Gowy, the rise above the sea-level is not more than thirty-eight feet, while several other places above this and on each side do not exceed fifty feet,* a just idea may be formed of the altered geography that would be presented by a submergence to the extent alluded to.

Allowing, therefore, the sea-level to be renewed to the height which has been mentioned, either from the subsidence of the land, or the reaction of the sea, we would find, if not the still water, at least the tide to reach far up the vallies of the Dee, the Gowy, Weaver, and Mersey; while a similar rise in the water-level on the coast to the northward, would again submerge great part of the lower Fylde country, the levels to the north and west of Ormskirk, and again connect Marton Mere with the tidal estuary, as it appears once

* See the Ordnance Survey Maps.

to have been, from the canoes that were found, when this *mere* was drained, above one hundred years ago. Under similar circumstances the peninsula of Wirral would be separated from the main land by one or more aqueous communications between the Dee and the Mersey, and be further constituted into two unequal islands, lying it may be said, at the mouth of one common estuary. Reverting then to the topography of the coast, as laid down by Ptolemy, we shall make some approach towards an explication of the different interpretations that have been made by succeeding antiquaries as to this part of his geography of Britain.

Mr. Ormerod, in his notice of this part of Cheshire, seems fully aware how the difficulties that have arisen on this subject will perfectly vanish, by allowing such a rise in the sea-level to have once taken place, "as," he says, "the two rivers mingling through the channels of Thurstanton and Wallasey, would present conjointly one mouth, broken only by two inconsiderable islands, they would together form the *Seteia Æstuarium*, and leave no impeachment on the accuracy of the informants of Ptolemy." We may, therefore, conclude, that in the days of

Ptolemy, the mean tide-level was at least twenty to thirty feet above what it has been in later periods, and that the mouths of the Dee and Mersey had a common outlet or commingled delta.

In this view, which may be considered well corroborated. The *Seteia Æst.* will include the mouths of both rivers—having an island at their outward confluence, somewhat similar to what is faintly depicted in Ptolemy's map. The *Belisama Æst.* will then be placed at the mouth of the Ribble, and the *Portus Setuntiorum*, will be the port on the *Alauna* or Lune. To further strengthen this last appropriation, it may be remarked, that of the four designations, this is the only one called *Portus*, λιμην, the other three are all termed *Æstuaries*, showing that this one was a port or haven, not a mere outlet of a river. We may further notice, that *Alauna* or the Lune run through the middle of the Sistuntian country, and therefore, it is highly probable that their principal haven would be near the centre of their territory. The location of *Moricambe Æstuarium* has never been disputed.

Though we cannot agree with one or two more speculative writers on this subject, that at the time of Ptolemy, the Mersey was not open at Liverpool, but found its exit over the low lands of Cheshire, nor yet can we subscribe to the opinion of a writer in the "Gentleman's Magazine in 1796, that the submarine forests at Leasowes and at Crosby, were a continuous sub-aerial and living one, with the Mersey, a fresh-water stream, running through this forest in the days of Ptolemy, and therefore unnoticed as an estuary by him; yet there is every reason to suppose, that neither the impetus nor volume of its fresh or tidal waters was near so great as at the present day. The distribution of its waters through so many outlets to the sea, would keep down its degrading effect in any one channel, and preserve the bottoms of all at a higher level, than if only one channel, as of late centuries, had appropriated the whole force and volume of its flowing and ebbing currents. It is, still further, very probable, that the Mersey above Liverpool had formed a very extensive expanse of water, reaching far up beyond Runcorn, and up the valley of the Gowy, and not subject to be drained off at each ebb of the tide. In fact, it might then be considered a sort of inland lake, having several communications with the sea, and giving great countenance to the present name, being derived from its being originally a *Mere-*

sea or *Lake sea*, given to it by the Frisians in the time of the Romans; as I have, in a former paper read before the Society, endeavoured to show. Besides the ancient name of Liverpool being written *Litherpool*, which signifies a *sluggish* or *still* pool, denotes that the waters at this now busy emporium of navigation and trade, did not in those remote days, exhibit the tidal velocity and force which they now so remarkably do.

In conclusion, I fear I have detained the Society too long on the foregoing points of ancient and modern hydrography, and on the comparative observations and opinions of the several writers mentioned, but I have thought the subject, in some manner, worthy of a little critical revision, both in a historical and geographical view.

The lapse of time has produced great changes on the surface of the earth, and very notably on the relative boundaries of land and water, and it is curious to compare the existing geography and geology of many coasts, estuaries, and courses of rivers, with the records of ancient history. Now, these are often very indefinite and obscure, but they are, on that account, no less worthy of study, nor less exciting in interest, so far as they give a stimulus to our investigations, by the light, however faint it may sometimes be, which they afford and reflect upon the existing condition of things.

In lamenting the inexactness and obscurity of the ancient geographical maps, which are, in some instances, nothing more than romance put into diagrams, we, at the present day, have much to congratulate ourselves, and also our posterity, on the perfection to which this branch of science and art has now attained, when every brook, hedge-row, and cottage, of this kingdom, are mapped out to a mathematical exactness in all points; and further, we may say, that almost every coast, bay, and river's mouth, over the habitable globe are *miniatured* in the archives of our admiralty, with a truth and correctness, that can never occasion half the dispute, which Ptolemy's chart of the coasts of Lancashire and Cheshire has done.

A BRIEF HISTORY
OF CERTAIN
ANGLO-SAXON ROOTS
NEARLY OBSOLETE,
OR BECOMING SO,
IN THE ENGLISH LANGUAGE.

BY JOHN JUST, Esq.

(Read April 4th, 1843.)

Times and seasons, their events and vicissitudes, have their histories. They are both influenced by the slow progress of natural processes—by the motion of a constant current of events; which had a beginning, which we cannot know, and that tends to an end which we cannot foresee; nor foretell the results. From this progression nought in creation seems to be free. When we talk of stationary, we can only use the term relatively; absolute rest is not—cannot be. In this onward movement of things—this rising upward towards perfection—or sinking downward towards destruction and oblivion, words, which are the expressions of our thoughts, and the symbols of our ideas, must be subjected to the same changes as men and manners, customs and habits, and have their days of fashionable use and abuse, as well as of neglect and disuetude. And if there be a curiosity in us to look back upon such customs and habits, left behind with the persons who brought them into vogue and were swayed by them, surely the same reflective beings who can find a pleasure in comparing what is gone by, with what is passing before them, cannot consistently be indifferent to the language which was then the means of intercourse, and the medium of all interchange of thought. The history of a language thus becomes intimately connected with the history of a people. The latter informs us of their doings, the former of their manner of discourse, how they spoke in the remote times, ennobled by their deeds.

To us, a subject of this kind offers much to instruct us, as well as to satisfy inquiry. Mingled blood flows in our veins. Mingled words fall from our tongues. We are the offspring of distinct races—children of roving, restless parents—who left their homes to win for us one of the noblest inheritances yet possessed by any of the tribes of the earth. We are sprung from the sea—a sea-girt island is our dwelling-place—and the sea itself our ample dominion, covered throughout its vast extent with our fellow-subjects, in their wooden houses filled with our wealth, which we commit to the winds and the waves to distribute to the extremities of the four quarters of the world. We are, therefore, no common people; nor are they common events which form eras in our history; nor they common revolutions which have differently combined and modified the elements of our speech. And though we have kept no genealogies to record to us from what particular horde of settlers we are sprung—no family chronicles to tell us whether Saxon, Dane, Norse, or Norman owns us as progeny—still our names serve partly to distinguish us, and words themselves thus still remind us of what otherwise would be totally forgotten.

These names show that about two-thirds of us are sprung from Anglo-Saxons; and had our language kept pace only with our blood, we should have had about two-thirds of our modern English of Anglo-Saxon origin. But we have more. Our tongue is hence less mixed than our blood. It will, therefore, be easier to trace out the histories of words than of families. And as many good old words are dwindling away and becoming obsolete—and others, once of common use, now only known as adjuncts, and lingering in compounds—it may not be irrelevant to the purposes of a Literary Society to bring before it the origin, import, and exact radical ideas of a few.

The first specimen which we shall take under our consideration with this view, is the old Anglo-Saxon verb of motion "faran," "færth," "for," to go forth. With the single exception of the colloquial enquiry, "How fares it?" there remains, perhaps, no other simple form of this verb in use at the present day. It was, however, a very common word in the early chronicles, such as "Robert of Gloucester's," "Peter Langloft's," &c., during the transition state of the Anglo-Saxon into modern English. While our very early English Authors, such as Chaucer, Spenser, &c., use, frequently, such phrases as, "so foorth they far'd," where the verb retains its simple original meaning. In this respect, it, to a certain extent, agrees with its cognate term "fahren," in the German language, which either expresses or implies, motion forward, towards an object or place. So with the

Old Norse "ferr," "for," "færi." And the Danish "farer," "fore," "faret," &c. If, however, this verb is reduced in English to a single instance wherein it occurs in its primitive form, it still supplies our tongue with many derivative syllables and adjuncts, besides having furnished, originally, many qualifying and connecting particles which are essential to the perspicuity of the language. Of derivatives and adjuncts, we may notice the following: "way-faring," going away, synonymous with the Norman French, "journeying," "travelling;" "sea-faring," going by sea, equivalent to the Norman French, "taking a voyage." "Welfare," gone well, successfully. Also, "farewell," go well—the parting wish of friendship, fare you well wherever you may be, and whatever may be the business for which you undertake your journey. The last two examples serve to show the peculiar aptness of the Saxon tongue, for the composition of words. "Farewell" is compounded of the same two simple words as "welfare," the position of the two being only changed. "Farewell" expresses all which it is possible for language to do, when we part with those whom we love; and "welfare" includes all we can wish to know when we meet them again, and make our affectionate enquiries after themselves and their affairs. And these comprehensive terms owe their origin to the habits of the people. Restless, roving, like the Nomad tribes of antiquity, they were constantly making their "fyrds," or adventures. Not however, like them did they move with their families, and their flocks, and their herds, to seek pasturage for a season; but they left their families frequently behind them, their near and dear relations in some remote and secluded spot, trusted themselves to the winds and waves, and sought their livelihood on the great deep, and the countries to which it carried them. To "go well" and to "return well" was, therefore, to them all important. All was included in their "farewell" and their "welfare," and they have transmitted to us the two words, to remind us of the perils to which our forefathers were subjected; and, likewise to include for their more fortunate offspring—all it is possible for two words to express—even under our more favoured and fortunate circumstances. For we too, though we have populous cities and a numerous fixed population, inherit the propensities of the race from which we are chiefly sprung. They were restless in body, we are restless in mind. Their brawny right arms, and certain right hands, were to them sure guarantees (generally considered) of success; our superadded vigour of mind and enterprise have hitherto crowned our efforts in the same manner, but to an extent unknown to them or to the most renowned nations of antiquity, equally in arts as in arms, making our power and our skill to be felt and acknowledged over the wide world—so that to "fare well" and to have "well fared" express for us, our most ardent wishes—respecting all our numberless outgoings and incomings, to and from every region on the face of the earth, and for almost every possible purpose. And to make this history of these terms both more apparent and real, we have but just to compare it with similar modes of expression used on similar occasions, by nations sprung from the same stock as the Saxons, and either agreeing or disagreeing with them in their habits of life. Norse and Dane, like themselves, were adventurers on the ocean. Their kings reigned in ships more than on land; we therefore, find them using the same terms on similar occasions, both tongues have their "farvel" and "far vel," and their "vel ferd."

But the Germans, a fixed and inland people, not given to such habits, use not generally such words on such occasions, but those which better agree with their manners. Their common valediction is "leben wohl," live well. This corresponds with the Anglo-Danish parting wish, "good bye," good dwelling to you, which began to prevail when those marauders were so fortunate as to obtain fixed settlements, and mingle with the Saxons. And here it may be a pardonable digression to point out an error which most of us may have heard, respecting the origin of "good bye." It has been considered as a contraction of "God be with you." In this case it could only have been introduced after Christianity. But we have beside what was then introduced into our language, viz. "God bless you:" and in some instances we have still "God be with you," used at the same time. The contraction is therefore useless, and obsolete as far as its meaning goes, provided it be such a contraction. But "bye" is not an obsolete word yet. We have "bye" ways—ways leading to a bye, town, or dwelling. Our term "highway," was originally applied to Roman military roads, and well expresses their character; while "bye road," was similarly applied to Roman vicinal roads, and also equally well expresses their character. "Bye" is a Danish word still common in the names of places in the north of the kingdom where the Danes settled. We have

"Netherby," "Wetherby," "Appleby," "Hornby," &c., all which names indicate the settlement of Danes there, and hence the history of words clearly proves the origin of these places. The valedictions of people thus show much of their leading characters. The "Salem" of the Jew, was an appropriate wish among a nation surrounded by enemies, hating and utterly hated, making constant wars upon them. The "vale" of the Romans, who thought of nothing but conquest and empire, was natural enough among a race with whom valour was the greatest virtue—and virtue itself valour. So among the Gaels. Chieftain and Clansman—Patriarch and descendants, Patriarch and descendants scattered over a barren country, and fighting for a livelihood and existence, might well wish one for another of the same clan—"slan leat," health be with thee—since sickness and infirmity among such tribes could be but certain precursors of destruction. And on we might proceed with such forms of expression, each agreeing with the prevailing habits of the respective nations, were we not digressing too far from our present subject already.

Other derivatives from the root which we have selected—still remaining amongst us, are "Field-fare Fieldgoer," the name of a bird which migrates to our fields in winter, and one well chosen to indicate its vagrant habits; as it visits one field after another during its stay with us—without confining itself to any peculiar locality. "Thoroughfare," a passage or road through, without impediments or interruptions—rare in former days—but now so common that "thoroughfares" are almost the only "fares" in the kingdom, except the "fares" paid for passing over them.

No substantive form, or abstract term from this root remains in the language, with its literal meaning. By Metonymy, however, which is one of the commonest figures of speech, and which puts cause for effect and effect for cause, or otherwise substitutes one part or name for another part or name—we still have in use "fare" as we have just stated, for the fee given for conveying us on a journey, as "what's the fare"—what is the payment due for the passage. We use "fare" also for treatment—diet, or living &c., as good "fare" and bad "fare," good living and bad living—good treatment and bad treatment.

But, though "fare" or "far" to express a journey, is become wholly obsolete, we have still in use a derivative from it in "ford," a shallow place, across a river passable on foot. As qualifying or adjective terms, we have "far," "farther," "farthest." It is, however, curious, that though the synonymes, or variations "further" and "furthest" are indiscriminately used, with "farther" and "farthest," the positive form "fur" (if the positive?) except in such dialects as the Lancashire, is not in use. We may call the man a clown, who in our hearing cries out, "Stan fur Johnny"—yet, if there be any meaning in words, it must be more clownish for us to add, either a comparative or a superlative to his simple vulgar expression. The Lancashire "fur" however, may be a contraction of the Anglo-Saxon "furth," for which we always use one of its dialectic forms "forth"—further and furthest are, therefore, the comparative and superlative of "forth"—furnishing us with an example among many others, of the capricious and lax etymology of the English language in its present state. If "fur" be used in a comparative sense, by the inhabitants of this county, then it is a mere corruption of the Anglo-Saxon "fyrre" "fyr"; and leads us directly to the true source of the ordinal "first," as exactly corresponding with the Lancashire "furst," a contraction of "furrest," which is the exact superlative of the Anglo-Saxon "feor"—the origin of our English far. "First" hence is the farthest one from us—the leading one. And notwithstanding, as first expresses also the nearest, as well as the farthest, we might object to this deduction—still, the cognate dialects of Anglo-Saxon confirm it. The Old Norse has its "fyrsti" directly from "fyrer." And the Danish derives its först, from för—farther having no positive, and perhaps from this Danish word, has been derived the Lancashire "fur" as a comparative, which is probably its legitimate state. The Anglo-Saxon, has a synonyme so far as the latter sense of first is concerned, which, may by being explained, throw some light upon this "lucus a non lucendo"—and this is "ær," "ærer," and "æreste" or "erst" which, if I might be allowed to translate, though almost untranslatable—would in modern ungrammatical English be, "ere," before, œrer, beforer, and "erst" beforest—*i. e.* nearest. Now in German, "erst" is first, in this latter sense. The English "erst" may hence be merged in the English "first," and thence both senses be admissible by our indiscriminately confusing the terms. Our preposition "for" is also deduced from this root, and keeps strict to the literal meaning as exemplified in—"Where are you for?" that is, whither are you going? "This is for you," that is, this is coming to you. Likewise "fore" and "aft," the former part, the part gone from you, and the part behind you of a vessel. "Fore" as an affix is a contraction of "foran" before, and invariably expresses the self-same idea. Yet, it has still by implication the literal idea of the root in its contracted form of "fore."

In the Westmorland dialect, a few remains of this root are occasionally heard, as in the common phrase, "faran ta cove," going to calve; used when a cow is calving. In the Lowlands of Scotland too, a travelling hawker, is sometimes by the lower orders called, a "farandman," a goingman.

Nor is this meaning of the Anglo-Saxon root, peculiar to it and its kindred dialects. It has descended into other branches and tongues. By adhering to the rule discovered by Rask, and established by Grimm, the f in the gothic stock, is convertible into the Greek π. Hence the Anglo-Saxon "far," "for," "fare," is exactly similar to the Greek "πόρος," a passage, a way. And πορεύομαι, compounded of "πόρος" and ἶμι to go, to "faran" our root. In the Latin language "prodeo" is of similar import; as also "proficiscor," both containing in composition, a preposition deducible from the same origin as the root. And here we might apply Rask's rule, and show that the Greek "πέρθω" "πορθέω," and the Latin "perdo," "prœda" contain the convertible mutes of the Anglo-Saxon "fyrd" before mentioned, and, therefore literally express in their primitive meanings similar ideas. Hence the Grecian Argonautic expedition —the Roman border wars with their neighbours connected with these words, seem to imply, that both these nations in their earliest struggles were marauders like Saxons, Danes, and Norwegians; and that out of the same elements of savage valour, then ill-directed and a curse to the nations of the earth—arose by proper cultivation, those civilizations which in their turns have much more than compensated for former injuries, and which show, that the moral government of the world, has a beneficial tendency through the kind superintendence of God's providence, which always effects ultimate good to God's creatures, out of the elements of apparently discordant evils. To pursue the subject further at present, however, would not be as intended, brief.

In order to attempt something like order and connexion, we will now select for our notice the Anglo-Saxon root buan, bude gebún, to dwell, inhabit; because we have referred to the word "bye" already. Bye as we have seen is a place of abode; and though it also occurs in the Anglo-Saxon language, has not been retained in its dialects, so much so as in the Anglo-Danish districts; therefore, we have referred it to the Danish, in which it is still of frequent occurrence. From the Anglo-Saxon form of this root as byàn, byde, gebyden, we still retain a derivative abide, abode. Hence, the abstract noun "abode," from this form is exactly synonymous with the simple form "bye," from which it has been derived. Besides the words already quoted, we have directly from this form—"bye-laws," laws made in courts baron, courts leet; or by companies and corporations, for their special use and protection. There is also "bye word," now used only as a term of reproach, but formerly a common word in a bye, or habitation of a lord or baron among his numerous retainers and dependants, or a common word in a small town—town's talk—now-a-days, dignified with the classical name of scandal, as if it were only the besetting sin of the learned.

Directly from the verb, we have no word as such at present in use in our language. It leads us, however, to the exact import of certain derivatives which we still retain, as for instance, "husband," a dweller in a house, one who has taken a house for himself, and as "it is not good for man to be alone," and a house taken is sure to be followed by a wife also taken; "husband" now is the common name of a married man. "Bondman," one dwelling with you, either as a slave, or a pledge, or hostage. "Bondage," a state of slavery—confined to the house and dwelling—not free. A "husbandman," a man living in the house, a servant of the house, doing its work, or the work connected with it; hence, now signifying a farmer, or one attached to a farm, and constantly employed on its premises. Times, however, are strangely changed. From the palace to the cot, every house is a home, every man among us free. All we keep in "bond" are our merchandise and wares, trafficking in the same way with our goods, as our forefathers did one with another.

"Báuan" exists as a root also in the old Gothic, such phrases of the Greek Testament as, ἐνοικήσω ἐν ἀυτοῖς being translated by Ulphilas. "báua in im," shall dwell in them, 2 Cor. vi. 18. In the kindred dialects we find, as for instance in the Danish, "bor" "boede" "boet." Old Norse, "bua" "bio" "buim," branches from the Gothic, signifying the same as the Anglo-Saxon root. And curious enough our present language has retained the Anglo-Saxon "abidan" rejected its preterite, and borrowed from the Danish "boede" its present imperfect, making a similar kind of mixture of tongues as of persons. Owing to the different habits of the Teutonic branch of the Gothic stock, this root has been modified in its signification. The German "bauen" expresses active pursuits, more than neuter states of being and condition. It therefore lacks such derivatives as we still retain.

The derivatives of this root may serve to throw some light upon the history of our forefathers. Their husbandry from its very name implies, that it was carried on by "bondmen," slaves connected with the soil, bought and sold with it like cattle. We find also, that the Anglo-Saxon estates, were particularly marked out with "bounds" and "boundaries," both derivatives from the root which we are endeavouring to trace out in the terms which it has introduced into our language.

Beyond these limits, the "husbandmen" were not (from expressions still left to us) probably allowed to stray. Hence the expression, a man "out of all bounds," one off his premises wherein his condition legally should confine him. Also, "bounden duty" the special duty of the situation a man fills, the duty of a "bondman." In country districts, we now and then find stones set up called, "bounden stones," these marked the bounds or limits within which the "bondmen" had their range. The number of "bondmen" attached to the soil in this way, was called a "band," and "band" still keeps this its literal meaning, as a band of thieves, a band of soldiers, &c. Also, by referring to the Danish dialect, we deduce from it "boer," the name by which one of these "bondmen" was distinguished. This we retain still as a suffix in the word "neighbour," a "boor," dwelling nigh, one living next in the same "bye;" "neighbour" then like "bye" is more Anglo-Danish than Anglo-Saxon. The Anglo-Saxons, used chiefly "nextan" for neighbour, which referred to all ranks and conditions among them; so did the Danes, but they seem to have had a special term for the special condition of the "bondmen" among them.

A third root which we will now bring under our consideration is "witan," "wot," "wiste," to know; the action of the wit or intellectual faculties, or of the "wits," bodily senses, or perceptions. The simple literal meaning of this word is still retained in the language, as for instance in the infinitive "to wit." "Jeroboam the son of Nebat, caused Israel to sin, and Jehu departed not from them, to wit, the golden calves that were in Beth-el, and that were in Dan." (2 Kings x chap. 29 ver.) Also in the imperfect, as "I wot that through ignorance ye did it, as did also your fathers," &c. We likewise frequently hear such expressions as these, "Such a person has lost his wits." Others have not their "wits about them," &c., which, though not deemed very elegant phrases, are, notwithstanding genuine English; and equivalent to saying, a person has lost his "senses." For "wits" in Anglo-Saxon are synonymous with the Latin "sensus," the origin of our modern word "senses," which we have received through the medium of the Norman French.

We retain in use the abstract, or noun from this root, but we seem to have quite perverted its meaning. "Wit" now-a-days is a word used to express quaintness, humour, quickness of perception. But among the Anglo-Saxons, a witty person was a man endowed with the sterling qualities of sound common sense, and correct judgment, one who could strictly observe what was going on around him, and profit thereby. We, however, consider joking, punning, and saying smart and odd things, as constituting the character of a wit. Such men our forefathers kept for fools—while we are apt to overlook common sense somewhat in the same way—so that by an unaccountable perversion, we regard the wise man of our ancestors as a fool, and they regarded those we hold wise as fit for nothing but amusement and merriment, adorned them in the most grotesque manner, and made "merry Andrews" of them.

From the noun just mentioned comes "witness," the evidence of the senses. As the enjoyment of the bodily senses is a common blessing among mankind, and connected therewith are the endowments of the mind in a greater or less degree; there was hence nothing peculiar in the condition of our Anglo-Saxon fore-elders, or is there in that of ourselves to have introduced or to retain the present root in our language. As the use *of* *the faculties* is common to all, so we may conclude will be the term to express that use. Down from the source of language, a root so essential may be expected to have flowed in most, if not all its channels. Now we know, that the Gothic and Teutonic families have derived their speech from beyond the Indus, and it is there in its treasures that we must seek for the primitive root, the trunk from which the word has branched off. Accordingly in the Sanscrit, the oldest of the Indian tongues we find "véda," in the Hebrew "yadah," in the Gothic "vaitan," in the Greek οἶδα, εἰδέω, ἰδεῖν—from the last, of which in a restricted sense, the Latins obtained by their mode of pronouncing the digamma "video," to see—all which words correspond with our "witan," to know. Then in the German is **wissen,** in the Danish is "veed," in the Icelandic "vita," "vissi," &c.

To connect one word with another as before, we will next select the verb "deman," "demde," "gedemed," to judge, doom, &c. We still use this verb as "to deem." We also have "doom," the result of judgment, the award, punishment, &c. In the Isle of Man, a judge is termed a "dempster." In compound words, "dom" is still common, as "Domboc," the title of the survey of the kingdom, or statute-book of the Anglo-Saxon kings. "Kingdom," the jurisdiction of a king, the country over which a king judged, literally the judgment of a king. "Wisdom," compounded from our last root and the present. The judgment of the senses; the verdict of the "wits." This word, we, who are wiser than our forefathers, abuse very much. We call learned persons, persons of great information, &c., men of wisdom—but in so doing, we pervert the meaning of the word, for a learned man, and a man well-informed, may possess very little wisdom. A wise man is he, who, whether learned or unlearned, makes a proper use of the senses which God has given him, and forms his judgment from the knowledge and experience which he derives from them. "Freedom," free judgment, unbiassed from the authority of another. It is a stronger term than liberty, which merely signifies a state opposite to that of slavery: whereas, freedom does the same, and allows a man to judge for himself, places him on the level with a king, who has only a "dom" like himself, though it may extend to territory and to subjects, while his is solely confined to the mind. "Thraldom," the exact reverse of freedom, a state doomed to perpetual thral. The

Saxon "thræl," was a slave of the lowest caste. He was a slave of the master, not of the premises; he had no "bond," no caste, no fellowship. He was a slave without "bounds"—the lowest of his race, without one privilege or any protection. "Martyrdom," "hallidom," the doom of a martyr, "holy doom," "be me hallidom," by my holy doom, a provincial expression. "Earldom," the dominion of an earl. "Dukedom," the same with respect to a duke, although the term was not in use until the Norman conquest; as the Saxons had no "dukes" among their nobility. "Christendom," the dominion of the church, &c.

In the kindred dialects we have the Gothic "domjan," to give judgment, pronounce a sentence or doom. In Icelandic is "doma;" and in Danish, "dömme," both of the same import as our root. While the German retains the root only in compounds, as Christenthum," Christendom: "heidenthum," heathenism, &c.

In the history of this word there is much implied, and thence thereby much to be learned. It intimates to us the trifling distinction between the early kings of the early Teutons and Scandanavians, and their followers; an "imperium in imperio"—an acknowledged head with limited power—guiding only occasionally the members of the body political, which exercised a power nearly as great as its own, with much more latitude in their sway and at their discretion. And more particularly this was the case with the first settlers from this stock of the human race in our own island. Taking with us the history of their words in studying the history of their nations and customs as left recorded for us, much advantage may be derived therefrom. Respecting the "villani" of the Domboc, difference of opinion has been entertained; some considering them to be free, others slaves. Now applying the information which we receive, from the words in use among them, to the knowledge of the history left us of their times; we may fairly conclude the "villani" were bond slaves—persons attached to the soil: villanus being the latinized term of the Norman French "villain," one of low and servile condition attached to the soil, but having a cottage and land assigned him by his lord, to whom he rendered work and bodily service, but had no right to dispose of himself and services otherwise. "Bordarii" were also similarly situated: they were the property of their lords, and differed from "bondmen" and husbandmen, in being provided for at the table of their master, and having also to provide for his table. Our law terms show us the distinction of "Bordlands" and "Bordwoods," upon which the Bordarii were probably employed to produce food and procure firing for the household purposes of the proprietor. The Servi were the real slaves; the "thrals," those whom the owners could buy and sell independently of the soil—poor wretches without a right—just like slaves in America.

It may not be amiss here, though partly contrary to the avowal with which we set out, to take for our next root "thenian," to serve, to pay homage to, because it bears upon what we have just advanced, with respect to certain expressions in the "Domboc;" although root and branch it has become wholly obsolete. From it is derived the Anglo-Saxon "thane." The whole history of the Anglo-Saxons is mingled up with the names, actions, and transactions, of these personages. We find two orders mentioned, king's thanes, and common thanes. The king's thanes owed him suit and service, for which they held lands under him. He had his "land theyns," his "discthеyns," his "hregel thanes," his "hors theyns," &c. The common thanes were common proprietors in towns and burghs—not owing suit and service to the king, but serving on juries; and liable to be called upon to the "witena gemot," or parliament of the nation. They were free; wore arms, and hence, King Alfred, in his translation of Bede, renders the Latin word milites, by "theyns." They were to possess five hydes of land—and formed the Anglo-Saxon militia; being liable to be called out on every "fyrd," or warlike expedition. They however had no jurisdiction, they had no "dom;" the bishop had his "dom," the abbot had his "dom," as well as the earls already mentioned; though, from the altered habits of society after the conquest, they gradually lost them; but the "thane," had only his "ship" or "gild," his clubs for mutual protection. Under him were the "Ceorls," entitled to helm, mail, and sword; as a proof of the tenure by which they were retained under the thane. These two, in their body, most likely formed the "tenentes," mentioned as a part of the population in the "domboc,"—the "th" of the Anglo-Saxon, being convertible into the Latin t, according to Rask's rule, and thus making admissible into our present paper, what otherwise would be but a digression.

Not to be tedious, we will select but another Anglo-Saxon root for discussion on the present occasion, and that is, Ric, Rica,—rich, reign, &c. This root remains, as far as I know, but in one word in the English language, and that is, "bishopric;" though it formerly was connected with most kinds of rule or sway held in the land. It differed from a "dom," which, as we have seen above, included the power of life and death; in that it only expressed the power and influence which rank, situation, and riches, ever have conferred, and ever will confer among mankind. It expresses a legitimate kind of government, such as must be, and will be exercised among us so long as distinctions prevail—founded upon the power which the superior acquirements of mind and wealth bestow. We hence find the term almost in all languages, where rule by influence of these kinds has prevailed. We see it in the Indian "Rajah,"* in the Gothic "Reiks," in the German "Reich," in the Danish "Rige," in Old Norse and Icelandic "Riki," in Latin "Rex," in Welch "Ren, Pren, Bren, Brenhin;" and in Gaelic "Righ." Among the Greeks, however, where no such influence was felt, where all were heroes, and fit for bearing sway—they repudiated

* Sanscrit. Râgán.

the term, and had their ἄναξ ἀνδρῶν Ἀγαμέμνων and their Βασιλεῖς their Archons, and their Prytanes, &c.—leaders indeed, and directors; but with delegated powers, and exercising no sway, but what was received by their appointments, and not arising from personal influence and station. They were the Americans of the old world—calling kings tyrants, and hating, in a great measure, the very name: but like them, having their slaves, over whom they practised a tyranny, unknown among regal nations, where the people submit to influential sway; but keep themselves, both in relations high and low, truly and nobly free. We have, besides this, to learn somewhat farther yet from this root. It has entered extensively into the names of individuals, before Christianity modified the appellations of mankind, by the ties of kindred, and the relationships of life. And when we consider that the names of individuals were then given from circumstances of birth, and similar considerations; and that the adnomens they afterwards obtained from personal appearance, or otherwise, in the way of epithets or nicknames, were merely fortuitous—we cannot but be struck with the changes effected by the revolutions of ages in our social condition, and the stations of life. According to our views at present, as to the history of words, every man with the suffix of "ric" to his name, among the early Anglo-Saxons, and other allied nations, must have had at that time, a "rica" under him; king, earl, or thane, he may have been, but not one of the bondmen and serfs of the soil. "Frederics," in these days, were rich at least, if not powerful; so were Henries, and Richards, &c.—but alas! the names have survived among us, without the titles, and many with such names, it may be, descended from the rich and powerful of our ancestry, are all but bondmen at present, —paupers—living upon the alms of the nation, and perchance, fixed for life in a poorhouse, within a land which boasts that the very air which wafts over it, if inhaled within its limits, must make the respirer free.

OBSERVATIONS

ON THE

FARINACEOUS PRODUCTS

OF VARIOUS KINDS

OF THE

POTATOE.

BY R. P. BAMBER, M.R.C.S.

(Read March 18th, 1845.)

The vegetable kingdom is so abundantly supplied with starch-bearing plants, that some account of them would form an interesting subject of inquiry, but the limits within which I have restricted myself will prevent me from doing so, and I shall, therefore, confine these observations, with a trifling exception, to an investigation into the farinaceous properties of one which is now so peculiarly adapted, both by habitude and cultivation to this country, that it has become one of the most useful, and important additions to our food.

The following quotation (Dickson's Vegetable Kingdom) is so appropriate, that it will serve as an introduction, better than any farther remarks of my own :—

"The potatoe is not only the source of the purest starch of all, but has many interesting points connected with its history and habitudes. No plant has contributed more to banish those famines which formerly were of frequent occurrence in Europe, and all the dire train of suffering and disease consequent upon them. Yet did it, in many instances, require royal edicts to induce some nations to cultivate what is now regarded as one of the prime blessings of Providence, from nearly one end of the earth to the other; the potatoe being raised from Hammerfest, in Lapland, latitude 71. north, through all Europe, the plains of India, in China, Japan, the South Sea Islands, New Holland, even to New Zealand. What renders it so peculiarly valuable is, that in the seasons when the corn crop fails, that of potatoes is generally more abundant, thus furnishing a substitute for the other, which proves defective from atmospheric considerations, which have little influence over the potatoe, placed as it is under ground, and secure from extremes of temperature."

The quantity of starch in the potatoe is most abundant in the winter months, but varies when grown in different soils, or climates, and by the manures employed. This is also observed in the cereal plants, the wheat grown in Sicily and the Crimea, possessing more gluten than that produced in more northern latitudes; yet, by the use of manure, by better cultivation, and by their untiring energy in tillage, the residents in our less favoured regions, far surpass the inhabitants of more genial climes in the productiveness of their crops.

The gluten of the potatoe, from the nitrogen it contains, renders the tuber of more value as an esculent, while the farinaceous productions of the tropics, although destitute of it, are as equally adapted to their use, as the more stimulating diet required for the sustenance of man in colder climates.

Starch-bearing plants are, therefore, abundant in those regions. In the Indian Archipelago, the palm-like trees yielding sago, *Sagus Rum-*

phii, *Cycas Circinalis*, and *Phœnix Farinifera*, and in South America, the fan palm *Mauritia Flexuosa*, with many others. The Sagus Rumphii, one of the smallest palm trees which seldom exceeds thirty feet in height, has yielded in its fifteenth year, 600 lbs. of sago. Captain Sir Edward Belcher, in his "Narrative of a Voyage Round the World," recently published, states, that the sago tree, which at Amboina, Bouro, Ceram, and adjacent Islands grows most luxuriantly, and attains a large size, eighteen inches in diameter, is calculated to subsist a family for one month, or even six weeks. Mr. Crawford, (Ind. Archip.) expresses himself in similar terms.

The quantity of starch and gluten also vary in the same kind of potatoe. When the tuber is fresh gathered, it is more abundant than when stored up for use. The starch diminishes in spring, when germination commences, being converted into sugar, in order to afford the bud that nutriment which it afterwards derives when more fully grown, from the soil by the agency of its roots, and from the air by its leaves, as they become developed.

The following experiments were made when the tuber was fresh from the field :

		Starch.	Gluten and Fibre.	Water.
Leigh's Blacks, Sweep, or Black Pink Eyes,	per cent. fresh	17.3	8.4	74.3
Ditto..................	stored	16.0	7.7	76.3
Red Champion, ...	fresh	17.5	7.0	75.5
Ditto..................	stored	15.6	6.6	77.8
Radical, ...	fresh	15.4	6.6	78.0
Ditto..................	stored	15.0	7.2	77.8

Dr. Playfair (Lect. Roy. Ag. Soc.) gives the following as contained in 100 lbs. of the fresh root:

Organic Matter 27 lbs. Ashes. 1 lb. Water 72 lbs.

Dr. Kane, (Indust. Res. of Ireland) in a table from Mons. Payen, states the results of seven varieties of the potatoe, grown on soil of a similar quality, as follows:

			100 parts Contained		
VARIETIES.	1 cwt. Seed produced	1 Statute Acre produced	Water	Starch	Gluten and Fibre
Rohan	58 cwt.	$14\frac{1}{2}$ tons	75.2	16.6	8.2
Large Yellow	37 „	$9\frac{1}{3}$ „	68.7	23.3	8.0
Scotch	32 „	8 „	69.8	22.0	8.2
Slow Island..	56 „	14 „	79.4	12.3	8.3
Legonzac ...	32 „	8 „	71.2	20.5	8.3
Siberian	40 „	10 „	77.8	14.0	8.2
Duvillers ...	40 „	10 „	78.3	13.6	8.1

"These results show that the quantity of starch is not largest necessarily in those varieties which yield the greatest weight of tubers. Thus an acre of large yellow potatoes which gives but $9\frac{1}{3}$

tons of tubers, produces 2 tons 3 cwt. of starch, while the acre of Slow Island potatoes, which produces 14 tons, gives only 1 ton 15 cwt. of starch. In cultivating the plant for the purpose of extracting this material, it is therefore of the greatest importance to attend to the existence of these varieties."

The next Table was first drawn up in order to show the relative productiveness of my own crop, and it was afterwards enlarged by the addition of varieties procured from other places. The experiments were made in the months of November, December, January, and February, before any signs of germination had appeared, and while the quantity of starch was at its maximum point.

		1 cwt. seed produced	1 Stat. acre produced	100 parts Produced		
				Water	Starch	Gluten & Fibre
In Soil of similar quality.	Leigh's Blacks ... Sweeps, or Black Pink Eyes mean of 3 analysis	cwt. 10	tons. $10\frac{1}{4}$	76.3	16.0	7.7
	Red Champion mean of 3	$7\frac{3}{4}$	$7\frac{3}{4}$	77.1	16.2	6.7
	Radicals mean of 2 analysis	$7\frac{3}{4}$	$7\frac{3}{4}$	77.9	15.2	6.9
	Kidney	9	$10\frac{1}{2}$	74.8	18.2	7.0
	Golden Ball	9	$9\frac{1}{3}$	76.5	16.4	7.1
	Farmer	$12\frac{1}{2}$	$12\frac{1}{2}$	79.7	14.5	5.8
	Apple (Irish).........	8	8	78.1	15.3	6.6

		1 cwt. seed produced	1 Stat. acre produced	100 parts Produced: Water	Starch	Gluten & Fibre
Chat Moss Peat Soil with Marl.	Leigh's Blacks	15	15½	77.0	16.4	6.6
	Stranger or Cornwall Red......	17	18	80.5	13.5	6.0
	Scotch Cup	15	15½	73.3	19.7	7.0
	Bread Fruit	12½	13⅓	77.8	16.4	5.8
	Pink Eye			76.7	17.3	6.0
Pink Eye (from Irlam)				79.1	15.1	5.8
Ditto do.				74.7	17.9	7.4
Ditto (Culcheth, clayey soil)				74.1	20.0	5.9
Forty-fold...................................				76.6	16.4	7.0
Stranger, (New Hall)				79.7	12.0	8.3
Strawberry Potatoe from Cumberland...				74.3	18.2	7.5
Leigh's Blacks				75.6	16.9	7.5
Ditto				76.9	16.5	6.6

The potatoes were peeled in every instance, and this may be the cause why the proportion of gluten and fibre is less than in the preceding table.

In three varieties, the red colour with which the water was tinged, contained, when evaporated,

In the Radical	2.1	per cent.
Cornwall Red	2.4	,,
Irish Apple....................	4.7	,,

The last named potatoe is very red-skinned, and required extra washing to free the starch from the colour which pervaded its entire mass.

This per-centage has not been added in the table. It disappears in boiling, for the water is not tinged by it. Its taste is acrid, being perhaps that principle which renders the tuber unwholesome when raw, and to which it owes its botanical name, *solanum tuberosum.* It possesses this property in common with many of the starch-bearing plants of the tuberous kind. The Cassada, *Janipha* or *Jatropha Manihot*, called Cassava when made into flour, and from which also the tapioca is prepared, is poisonous in its raw state, the poison being said to consist in a volatile oil. The Arums also, of which the *Arum Macrorhizon*, *Arum Colocasia*, and *Caladium Acre*, are cultivated in the South Sea Islands, and are called Tarro by the natives, possess equally deleterious qualities when fresh ; but upon the application of heat, are deprived of their virulence, supplying a bland and nutritious food to the inhabitants, and are preferred by them to other kinds of food possessing similar qualities.

The following experiments were made with the view of ascertaining whether the starch was equally distributed throughout the potatoe.

		Starch.	Gluten & Fibre.
Leigh's Blacks ...	Nose, or Eye end ...	14.7	6.6
Ditto	Heel, or Root end ...	19.2	8.4
Ditto	Eye end	15.6	6.6
Ditto	Root end	17.3	6.6

It, therefore, appears that the heel, or that portion connected with the stalk, possesses a greater proportion of farina than that part where the eyes most abound; but being prevented from testing their correctness by farther investigation, I am unable to state whether this difference is of casual or of uniform result. It is, however, certain that the root end of some of the long kind (as the kidney,) is more mealy when boiled, and the opposite end more waxy. The correct explanation may be, that the root end, from its direct connexion with the stem, is first ripened.

The following proportions of starch and gluten are given by different writers.—

Dr. Playfair, in 100 parts

Albumen	Unazotized matter	Water
2.0	25.0	73.0

and Professor Johnstone, in a more definite analysis, gives

Water	Husk or Woody Fibre	Starch, Gum, and Sugar	Gluten, Albumen, and Caseine	Fatty Matter	Saline Matter
75.0	5.0	12.0	2.25	0.3	0.8 to 1.

There is a deficiency in this analysis of more than 4.0 per cent.

Boussingault obtained in 100 parts, dry material, 24.1, water, 75.9.

The quantity of starch and gluten per acre is stated by Mr. Karkeek to be, in 12½ tons or 26,880 lbs.

Gluten, Albumen, &c.	600 lbs.	Starch, Gum, Sugar & Fat	3330.	Water 20,250
	or, 2.2 per cent.		12.5	

A deficiency also occurs here of 2700 lbs., or nearly 10 per cent. of which no account is given.

Dr. Kane, in a Table collected from the best authorities, gives the quantity of actual nutritious material usually derived from a statute acre of ground as 9 tons, or 20,160 lbs., producing

	Starch and Sugar	Gluten	Oil	Total.
	3427 lbs.	604	45	4076
or,	17.0 per cent.	3.0	0.2	water 79.8

If the mean of the analysis of Payen be taken, the produce per acre will be rather more than 10½ tons, or 23,946 lbs., consisting of

	Starch	Gluten and Fibre	Total
	3537 lbs.	1652	5189
or,	17.5 per cent.	8.2	water 74.3

In the first 11 varieties in my own Table, the average produce will be nearly 11¾ tons or 24,947 lbs. The quantity of nutriment will be

	Starch	Gluten and Fibre	Total
	3252	1335	4587
or,	16.1 per cent.	6.7	water 77.2

The mean of 20 varieties will be

Starch	Gluten and Fibre	Water
16.4	6.8	76.8

According to Boussingault, the usual crop per statute acre, is 6½ tons or 14,560 lbs. when stored, and when dry 3,509 lbs. and 142 lbs. ashes.

If the per centage previously given by Boussingault be divided in accordance with the mean of M. Payen, and of my own, it will afford a near approximation to the quantity of nutritious aliment of both, and will be,

	Starch.	Gluten, &c.	Water.
Boussingault...............	16.8	7.4	75.8
Payen	17.4	8.2	74.4
My own.....................	16.4	6.8	76.8

The mean of these will afford the probable average of starch, and of gluten, allowing 5.0 per cent. for fibrous matter. Starch 16.9, Gluten 2.5, Fibre, &c. 5.0, Water 75.6.

Mr. Knight states that the quantity of starch varied from 8¼ to 20 per cent., but from the names of those potatoes which produced the smallest proportion, it is likely they had degenerated from long cultivation, and they are not taken into consideration.

The starch and the gluten of the potatoe therefore vary considerably. The lowest proportion of starch being 12.0, and the highest 23.0 per cent., while in the gluten it is from 2.0 to 3.0 per cent. It is probable that the fibrous part contains a considerable portion of nutritious aliment, as "From 2.0 to 3.0 per cent. of fecula always remains with the residual pulp, which it is impossible to obtain."

The tabular view inserted below, exhibits the difference of nutriment in some of the most important articles of food.

			In 100 parts Starch	Gluten
Wheat	(French)	according to Proust ...	74.5	12.8
Do.	(Bavarian)	...... Vogel ...	68.0	24.0
Do.	(Winter)	...... Davy ...	77.0	19.0
Do.	(Spring)	...... Do.	70.0	24.0
Do.	(Sicilian)	...... Do.	75.0	21.0
Do.	(Barbary)	...... Do.	74.0	23.0
Spelt		...... Vogel ...	74.0	22.0
Barley		...... Davy ...	79.0	6.0
Rye		...... Do.	61.0	5.0
Oats		...... Do.	59.0	6.0
Rice	(Carolina)	...... Vogel ...	85.07	3.60
Maize		...... Bizio ...	80.92	0.0

The chemical proportions, by Boussingault, are in 100 parts, of

	Carbon	Hydrogen	Oxygen	Nitrogen	Ashes
Wheat	46.1 ...	5.8 ...	43.4 ...	2.3 ...	2.4
Potatoes	44.0 ...	5.8 ...	44.7 ...	1.5 ...	4.0

Professor Johnstone has a similar analysis, and Dr. Playfair states the following in

	Gluten, from Flour BOUSSINGAULT.	Caseine, from Peas SCHERER.	Albumen, from Eggs JONES.	Ox Blood PLAYFAIR.	Ox Flesh PLAYFAIR.
Carbon	54.2	54.138	55.000	54.35	54.12
Hydrogen	7.5	7.156	7.037	7.50	7.89
Nitrogen	13.9	15.672	15.920	15.76	15.67
Oxygen	24.4	23.034	22.007	22.39	22.32

While Mr. Rigg, (Experimental Researches) gives

	In lean Beef.	The solid fat part of Beef.	Wheat, Seeds, Husks, and Kernel, as carried from the field.
Carbon	14.4	66.95	36.9
Hydrogen ...	8.6	10.06	7.2
Oxygen	72.1	50.44	53.2
Nitrogen ...	4.5	not determinable	1.1
Ashes	0.4	0.15	1.6

Potatoe Starch and Gum

	contains	contains
Carbon	44.250	42.682
Hydrogen	6.674	6.374
Nitrogen	——	——
Oxygen	49.076	50.944

The chemical analyses of the proximate principles of the starch-bearing plants, are, according to Dickson, "mere combinations of water and carbon, (hydro-carbonates or hydrates of carbon,) or compounds of carbon with oxygen and hydrogen in the proportions in which they form water, and consist in 100 parts, of

	Water	Carbon
Gum (pure Gum Arabic) ...	58.6	41.0
Sugar (pure crystallized) ...	57.15	42.85
Starch	56.00	44.00
Lignin	50.00	50.00

"These are so many mutually convertible products, of which Gum may be looked upon as the basis." Decandalle remarks, that, "while gum itself may be considered the nutrient principle of vegetation, diffused freely through the structure of the plant, and constantly in action, starch is apparently the same substance stored up in such a manner as not to be readily soluble in the circulating fluids," thus forming a reservoir of nutritious matter, which is to be consumed, like the fat of animals (which it clearly resembles in structure) in supporting the plant at particular periods. (Carpenter and Prout.)

The identity of lignin with starch and gum may also be stated. "Lignin may be converted into a substance resembling gum, by admixture with strong sulphuric acid, and on boiling the liquid for some time, the gum disappears, and a saccharine principle is generated." (Carp. Phys.) "Both starch and wood can, by different artificial processes, be converted either into sugar or into vinegar." (Prout, Bridg. Treat.) Starch is also converted into sugar during the progress of germination. Hempen cloth may be converted into sugar by the action of sulphuric acid, its chief constituent being lignin.

The constitution of the inorganic portion of the ashes of wheat and potatoes, is, according to Boussingault, in 100 parts.

	Wheat	Potatoes.
Phosphoric Acid..............	47.0	11.3
Sulphuric Acid	1.0	7.1
Carbonic Acid.................	——	13.4
Chlorine	traces	2.7
Lime	2.9	1.8
Magnesia	15.9	5.4
Potash..........................	29.5	51.5
Soda	traces	traces
Silica	1.3	5.6
Alumina, &c.	——	0.5
Moisture and loss	2.4	9.0

The silica in wheat-straw amounts to 67.6 per cent.

I shall now make some remarks upon that portion of M. Payen's table, which has reference to the productiveness of the potatoe.

It is stated, that 1 cwt. of seed of the Rohan variety produced 58 cwt., and that the others varied to 32 cwt., according to the quality planted. It would therefore, only require 5 cwt. or 560 lbs. of seed per acre, in every instance, to produce the crops mentioned in the table.

This amount is so small when compared with the quantity used in general by the farmers in this neighbourhood, that I have inserted a similar column, comprising the results obtained last year from eleven varieties. In the 1st or Leigh's Blacks, 1 cwt. of seed yielded 10 cwt., and it required 2240 lbs. to produce $10\frac{1}{4}$ tons, and nearly the same result was obtained in the other varieties. As my own sets were planted more closely than is the custom of farmers, if 17 cwt. or 1904 lbs. be taken it may afford an estimate of the average quantity of seed employed by them in a statute acre.

An intelligent farmer has informed me, that in a very favourable season, 14 cwt. or 1568 lbs. of seed produced $11\frac{1}{4}$ tons; or 1 cwt. of seed yielded 16 cwt. Another farmer states, that the crop obtained by him, during an average of several years, was 12 tons, from $22\frac{1}{2}$ cwt., or 2520 lbs. of seed; or, 1 cwt. of seed produced 11 cwt.

Mr. Wakefield estimates that in nine agricultural districts in Ireland, the average crop of potatoes, per statute acre, from 1404 lbs. of seed, (about $12\frac{1}{2}$ cwt.) is 13669 lbs. of produce, or rather more than $6\frac{1}{2}$ tons. This gives 1 cwt. of seed to 10 cwt. nearly.

These results exhibit a very different estimate of the quantity of seed required in this country, with that mentioned before, unless some mistake may have occurred in the reduction of French weights to the English.

The remarks subsequently made by Dr. Kane coincide with the results obtained in my own table. The kidney potatoe produced 10½ tons, which yielded above 1 ton 18 cwt. of starch, while the crop of Strangers was 18 tons, which only produced a little more than 2 tons 8 cwt. of starch.

Starch is obtained from the potatoe by a simple process, which has been extensively circulated by Sir John Sinclair and others. Manufactories are established in France and in Scotland for its production, and many families in the latter country, and in the north of Ireland prepare it in small quantities for domestic use, while in this country it has been neglected or unnoticed. The root of white briony will also produce starch by a similar process.

When potatoe starch and arrowroot are examined by the microscope, a difference is perceptible. They are both spheroidal, but the former is longer and more irregular than the latter. Each particle is invested by a membrane, which renders it insoluble in cold water, but which bursts during boiling, or at a temperature of 160 degrees, Fahr. when the substance termed *amidin*, which is soluble in water, is liberated.

In conclusion, I shall briefly relate some of the most important uses to which the potatoe and its starch are peculiarly adapted.

Mr. Jacob, in his Reports, states that in the eastern part of Prussia, potatoes are applied to many useful purposes. They are cultivated to a great extent, and by converting them into starch and treacle, that land is made to yield a profit which might otherwise have produced a loss. Sugar did not answer so well, "but the treacle," says Mr. Jacob, "appeared to me as sweet as any from the tropics, the only perceptible difference between them was, that it had less consistence."

"The starch (Dr. Kane) is not the only material extracted from potatoes, and extensively available in the arts. The potatoe itself, reduced to flour, is at present extensively employed on the Continent, in the preparation of a very wholesome quality of bread, and the starch itself is consumed in making confectionary, jellies, sago, tapioca, in thickening paper, and in a variety of uses, by which such quantities of it are employed as to render its manufacture a really important and extensive department of industry."

"The most remarkable of all the applications of potatoe starch is, however, one to which the excise laws of this country would probably present invincible impediments. It is the preparation of sugar, and of spirits. Under the influence of certain chemical agents, simple, yet peculiar in their action, and to which it would not be my province here to refer in detail, starch is converted into sugar, and this sugar, by fermentation, yields spirit. On the Continent the manufacture of sugar from corn is almost abandoned. Potatoe spirit is almost universally used, and in flavour, it so resembles brandy, that it is well known that a large quantity of the French brandy brought into London, is potatoe spirit from Hamburgh, coloured with burned sugar."

A DISSERTATION

ON THE

ANGLO-SAXON PATRONYMICS.

BY JOHN JUST.

(Read Nov. 12, 1844.)

There are few persons among us who have devoted much time to the study of the origin and elements of our own tongue; who, before they became thoroughly acquainted with the remains of the Anglo-Saxon literature, and with the kindred dialects which throw so much light upon the structure and expressiveness of that noble language—may not have been frequently at a loss to account satisfactorily for certain suffixes in the names of persons, places, and things, with which they are familiar. Far back into remote antiquity, as the annals of any people run, such annals invariably bring before us patriarchal government over family communities, as the first kind of rule exercised among mankind. The founder of each nation was the father of the people; and the people in one way or another bore his name, either as a Patronymic, like the ancient Greeks; or, as a prefix to the father's name, like certain clans of the Keltic race. Civilization, that great chemist of nations, however has decomposed these distinctions, by blending together numerous communities within the alembic of one vast kingdom or empire; and has combined families and tribes of people in one homogenous mass of population—merely reserving therein such distinctions of wealth, station, and merit, from families of ancient and modern renown—or of no renown at all, but that most honourable renown of all—the renown which individuals earn by their own unaided exertions, as harmonize with the affinities which civilization loves to promote, the affinities for all that is excellent. But this noble synthesis, which civilization has made for the benefit of mankind, has occasioned an awful jumble in the terms, whereby those relationships of primitive life were designated, such as almost defies all patient research, and the most extensive learning and knowledge to reduce them to aught like the appearance of order, according to their primordial elements. And no where on the face of the earth, has this jumble created such a Babel of names and family terms as among ourselves, made up as we are of persons from almost every nation, kindred, tribe, and tongue in the world. It is only, therefore, in the literary remains of the earliest race amongst us, that true meanings can be found for the names of their descendants; and for such terms now in use as were imposed originally by them, for ends and purposes now no more, and such as by their lack, have left the significations of the terms obsolete.

We have scarcely any termination of words in the English language more common than "ing." In present and active participles, it requires neither note nor comment, being merely an inflectional modification of the verb. It is only when it occurs as a suffix in compound names and terms that it requires any explanation—especially in nouns appellative. As for instance, "Wild" is a common family name in some districts of this country. Like the proper names, "White," "Black," "Brown," "Blake," it may be easily understood as having been applied at that very early period of our ancestor's history, when persons were frequently distinguished by epithets, expressive of some quality or circumstance for which they were remarkable. Besides, "Wild," "Wilding," commonly pronounced "Wil-ding," though erroneously, is also a family name. Now, what we want to ascertain is, whether there be any connection between the two names—and what that connection is; or, what is the import of the adjunct "ing;" whether it is an inflectional termination, as in the active participles just mentioned, or a particle in composition, having its own definite meaning. A reference to the very early Anglo-Saxon names so ending, can only determine such enquiries.

Among the ancient Anglo-Saxon genealogies, which have descended to us, we find such lists of names as the following:—"Bældæg, (wæs) Wodening, Woden, Frithowulfing, Frithowulf, Finning, Finn Godwulfing," &c., where the terminations in "ing" are pure patronymics; and the passage quoted means—Woden was the son of Bældæg, Frithowulf the son of Woden; Finn the son of Frithowulf; Godwulf the son of Finn, &c. The termination "ing" in Wilding, may hence be a patronymic; and though it by no means implies, that such Wilding was the son of Wild; it still may be considered as having originally expressed "Wild" the son; though the connection with the father is long ago lost.

The termination then, making this supposition not only possible, but highly probable, we have to examine such grounds as might either obviate or wholly supplant it. "Wilding" is also the name of a tree; and as many family names have been derived from such local circumstances, may not the family name in question have been derived thence? Names of trees and plants have frequently given names to persons. Witness the names of "Crabtree," Cawthorn," "Birch," "Bracken." But such names of persons, apparently so derived, are also names of places. Birch is the name of a place, not far from Manchester. Bracken gives its name to Bracken Park, in Pennington parish, Lonsdale, north of the Sands; to Bracken Hall, in Lambrigg, Westmoreland, &c. So that either family names have been derived from family residences, and the residences have obtained their names from local peculiarities; or family residences have obtained their names from the names of their first proprietors—which names of proprietors have been given them from local peculiarities, which is absurd. Otherwise, we must come to the conclusion, that the names of persons have been assigned them from the names of trees, as distinctive characteristics, which we do not find to be the case.

Therefore, even by mere argument, there is more reason to suppose such names of families to have been derived from the first family residences, than the contrary. We are not, however, left to argument alone, to support such a supposition. Names of places have frequently given names to individuals. The family name of Pilkington, is derived from Pilkington de Pilkington; Middleton, from Middleton de Middleton, &c. And similarly have arisen among us our "Crabtrees, Cawthorns, Birches," &c. We hence perceive, that the family names derived from family residences, or from the names of places; whether Norman French names or otherwise, have arisen from the Norman French fashion which was introduced at the conquest. For though just prior to the conquest we find some Anglo-Saxon names distinguished by their habitations, such as Ælfric at Bertune; we find no Bertune given as a name from the habitation, while on the other hand, all names derived from epithets, have been derived from Anglo-Saxon, Danish, and Norse origin, as already mentioned.

Yet, clearly as we may have established this position, it by no means excludes Wilding from being the name of some place or residence—than it excludes "Crabtree." There is, however, this difference in the two words. "Crabtree" is Anglo-Saxon. Wilding, as far as I at present am aware, is not. And yet Crabtree and Wilding are two English names given to the selfsame indigenous tree, the Pyrus Malus, Linn.—or Wild Apple Tree. Wilding, then, must have been introduced into the English tongue from some other dialect. And far as my researches yet extend, I know of no word in the Scandinavian and Teutonic dialects, except the Netherlandish "wildeling," which has the same signification, and whence it could be imported into English. And this might have been done during the reign of Edward III., when he encouraged over into the kingdom numbers from the Low Countries, either to introduce or improve the manufactory of woollens, and located them in different parts of the country; but particularly A.D. 1338, at Kendal, in Westmoreland, and in the vicinity of this town, as at Bury and Rochdale. And as this was long posterior to the Conquest, when the names of persons began to be derived from their residences; and as these colonists were settled chiefly in towns, and thence had little chance of imposing names upon places—it appears more probable that they introduced the name of the tree alluded to in their several localities—than that they fixed that name upon places whence it could be transferred to the owners. The evidence, therefore, is more in favour of the name Wilding being an Anglo-Saxon patronymic, in accordance with Anglo-Saxon custom—than a later introduction from any other people. And that the different names of trees and plants arose from the different settlers in the country, as presumed in this instance, is strongly corroborated by the fact, that the very tree about which has sprung out all this discussion, is known also by another name in the provincial dialects where the Danes settled in Northumbria, viz., by the name of "Whasset." And from its abundance in the neighbourhood of Beetham, in Westmoreland, a small hamlet in that parish is known by the name of Whasset to this day. By way, then, of a short digression from this wearisome discussion, we may learn this lesson, that it has led us to distinguish the origin of the three names; that Crabtree is due to the Anglo-Saxons; Whasset to the Danes; and Wilding, in all probability, to the Flemings, from the Low Countries; and that by a like research and comparison, we may expect to arrive at similar kinds of discriminations with other names and terms.

But we have not yet done with "ing." It forms a termination to numbers of nouns denoting action, condition, &c., in our language, and as such may be assumed as entering into the composition of names. Anglo-Saxon names, however, were seldom or ever assigned to individuals from nouns of this class; and even if so "ing," except as a patronymic ending, is unknown in genuine Anglo-Saxon. In the English participle first mentioned, "ing" is a corruption and contraction for the Anglo-Saxon "ende." And in the abstract nouns of such termination, the ing in English has been deduced from the Anglo-Saxon "ung." True, a few nouns may be met with in Anglo-Saxon remains ending in "ing," but this has arisen more from the lax spelling of the later Anglo-Saxon authors, than from the Anglo-Saxon orthography; or, perchance, from the influence of the Danish and Old Norse in which "ing" forms a genuine termination. And it may be through this influence that ing has ultimately prevailed; and we, perchance, ought to consider the English termination as a Danish and Norse substitution for the Anglo-Saxon. Summing up, then, all that we have produced on this subject, we may, I think, fairly conclude, the "ing" in the Anglo-Saxon proper names among us as truly patronymic. Let us see, then, what use we can make of it.

First, as in duty bound like all loyal Englishmen, we will, with all due respect to the Queen, examine into the title of King, from whose race for many a generation among us she has so nobly and worthily descended. "King" is derived from the Anglo-Saxon "cyning" of which it is but a contraction. Cyning is compounded of Cyne and "ing." Cyne—the head, founder, of a race or family; or literally, a *kin*—and "ing" the patronymic, meaning a son, or family descendant, the whole word signifying one of the patriarchal stock which held sway over the whole family. The very formation of the word shows us that the early Anglo-Saxon kings were the offspring of the founders of the several Saxon families. The son wielded the power of the father so regularly, that the name became attached to the power, and along with it has descended to our own times. Those early kings ruled over their own kin only, and their subjects thus comprised were called a kindred. Their government was paternal. And the name, now that it has been handed down to a sway over thousands of kindreds, ought ever to remind those who bear it to be fathers of their people; as were those who first established that power, and transmitted it to them in the very title they bear, and by which they hold it.

It will be very easy for us to find numbers of patronymic-names among the families around us; a few of which we will notice before we test the names derived from residence in the same way. Some names keep up the early Anglo-Saxon forms in their primitive purity; such as Cock, Cocking, or Cock the son; Field, Fielding, Hardy, Harding: others, as Fleming, seem to be partly Anglo-Saxon, and partly names of strangers who have settled among us. The Fleemings of some districts appear to be of the Anglo-Saxon stock—from Fleaming—compounded of Fleam and ing. Fleam the son or fugitive.

The Flemings of the Low Countries most likely owe their origin to their being exiles, and thence assumed the plural patronymic form of Fleminga or Flemings. For while the singular noun expressed the descent of the individual, the plural "inga" denoted the whole family race, as in Beowulf the Scyldinga, Brosinga, &c. mean the families or races bearing such names. Besides the Anglo-Saxon patronymic found in Fleming, there may be families of the same name from the Low Countries, which either settled among us at the time already mentioned, or which fled over for refuge during the time their fatherland was oppressed by the Spanish yoke, as well as families of German extraction bearing the same name.

But besides such names as cannot be referred to such circumstances are others, which, though in the patronymic form, are still not of patronymic origin ; such as Pilling, Melling, Walling. These names have arisen from residence at such places like those already alluded to. It would be carrying us beyond our present subject to enquire into the probable origin of such local names ; as also into the introduction of other names of similar terminations : such as Billing, from the old Norse Billingr, Irving, from the Danish, Arving, &c., where, it must be remembered, the termination merely expresses something relative to the person, as in the latter " Arving," meaning an heir. Nor must we confound, in such enquiries, terminations in ing, when a part only of the suffix, as the names Carling, from Car, Harling, from Har—ling in such names being a diminutive. To these we can only just allude to prevent us from confounding one class of proper names with another. At present we only intend to attempt to trace down the Patronymic form through certain changes it has undergone from time, and the introduction and use, after the establishment of Christianity, of different forms of names arising from the consequent alteration in the habits and customs of the Anglo-Saxon race.

If we trace down the Anglo-Saxon names, as they occur from the earliest dawn of their history, we shall find a gradual change. Mixed with the Patronymic form was at first the infantile, either alone or with an epithet or cognomen. Of the infantile kind, given at birth, are the Æthelwulfs, the Eadwulfs, the Ælfreds, and Eadwards. Of the other, are Walfsic, the Blake, Thurkels, the White. Then, more particularly when Christianity had modified manners by its milder influence, we find the father's name frequently connected with that of the son; as Ælmor Ælfrice's suna, Godwine Wolfnothe's suna, till at length epithets, cognomens, and nicknames gave way or were modified into surnames; and we meet with such names as Æthelwerde Stameran, Æthelwerd the Stammerer, and Godwine Dreflan, or the Driveller no longer.

And this form introduced into deeds and documents by Christianity, became the origin of all our surnames ending in son. Yet patronymics descended to this era and received also the selfsame modification, so that they mingle with the rest, and keep their pristine character. Of the fantastical names, given by parents at the birth of their offspring, little needs be said, farther than that they furnished one set of names ending in son, as Edwardson, Edmundson, Richardson, Harrison. Epithets, trades, and occupations furnished another set, as Smithson, Hindson, Swainson. While patronymics supplied a third set, along with names of similar endings, mingling with them from Norse and Danish names and extraction; thus among our family names, we find Dicks: from this we have Dixon and Dickson, and also Dickens, and from Dickens, Dickenson. Dixon with an x, is doubtlessly a false spelling for Dickson, and Dickens so likewise for Dickins. But ins or in is no Anglo-Saxon termination, nor is it Norse or Danish, for the Norse termination in 'in' is feminine, as also is the Danish corresponding one of "inde." It can only be referred to the patronymic ing; and Dickinson is thence, as it were, a double patronymic, and this doubling of forms is not unusual in our language. So many tribes and people have had to modify it and speak it, that it is no wonder that not understanding its essential character, they should sometimes misunderstand it, and apply their own terms to terms expressing the same idea. And what has been said, with respect to the name Dickinson, will also apply to many others, of like origin; spite of all the lax spelling and whims of families, on writing one common family name. The name of Irving, we have already had occasion to refer to; besides its genuine form of Irving, it is met with as Irvin and Irwin. In some names, the 'in' forms a part of the father's name, and then cannot either be taken or mistaken for a patronymic; as for instance, Rob, Robson; Robin, Robinson; Perkin or Parkin, Netherlandish for Peterkin, a diminutive of Peter, Parkinson: but this will be found the exception, not the rule: Tom, Tomson, Tom, Tomlin, Tomlinson, and many other names beside, will readily suggest themselves to the mind of any one who thinks at all on the subject according to this view.

We have already stated, that certain names of persons, have been derived from the names of their first residences. *Vice versa*, we have now to consider whether certain names of places have not been derived from the names of their first owners and occupiers.

Most of the residences of the early Anglo-Saxons, were built of wood. Hence one of their terms used for raising a dwelling, is timbrian, or getimbrian, to timber a house. The residences thus reared, whether to shelter man or beast, were known by the general name of hûs. If a fixed permanent residence, the Anglo-Saxons gave it the name of ham. If in the Anglo-Danish districts, after the Danish dynasty, the name of "bye," was the common term. Where the Anglo-Saxons formed a farming establishment, with its cots, huts, shops, offices, &c., the name of tún was assigned. Off farms belonging to the Anglo-Danish byes,—were named bers. Besides these were Thorps, Worths, Warths, Bolds, Booths, Bôtles, according to their natures, and the localitiesin which they were temporarily, or permanently fixed. If in the ham and the tûn, there was nothing remarkable in the place where it was located, it was named after its owner. If there was anything remarkable in its locality, it was so distinguished ; or if, from the casualty of the times, it was destroyed, and sprung up again after its first designation was lost, then it assumed a new name from new circumstances, or the new possessor. All these might be shown by instances to be facts, would it not waste the time of the reader, and be tiresome beyond all endurance to the hearer; besides being a digression too wide for the purpose of a paper on patronymics.

In the proximity of this town is Cheetham, the ham or home of Cheet, its founder, or the name of the place. Equally near and adjoining is Broughton, the tun named from the brow on which it was first established. Then beyond is Pilkington, the tun of Pilking, the name of its founder, and a Patronymic. Of similar names almost every county furnishes one or more examples. Of hams fewer patronymic forms are found than of tûns; yet Altringham, Addingham, Aldingham, Hensingham show that, though few, they are not altogether very rare. In Westmoreland, I am not certain whether one is to be met with. Two only I have been able to select from Cumberland, and one from Lonsdale north of the Sands. While south of the Mersey they are much more common. This is likewise the case with patronymic forms ending in tun. Ackrington, Whittington, Pennington, Irvington, are north of the Mersey, and in the counties just mentioned. Now as these counties were less under the power of the Anglo-Saxons, than the others, it is to be expected that fewer Anglo-Saxon names of places will occur in them: and therefore, fewer of that kind, with which we are concerned at present. I know that other derivations of some of the names, now mentioned, have been given by authorities much greater than mine. Yet, these derivations are so at variance with the principles and structure of the Anglo-Saxon tongue, that I conceive them altogether untenable. The "ing" which I have been endeavouring to trace up to a patronymic source, has been considered as a mere connecting particle. Every one, even but moderately acquainted with Anglo-Saxon, knows that it was like all early speeches, poor in connecting words, rich in cases and compounds. And, even supposing the ing were such a connecting particle, and that it was a corruption either for in, or en, the only two forms with which it is liable to be confounded, still, this supposition does not mend the matter, there being no such particles as in and en, in that language. And again, were we to suppose them to be but terminations of the former part of these compound names, we still fare no better, in, being before shown not to be any such termination; and en is uncommon except in collective nouns and adjectives, from which class of words none of the names are compounded.

We have already stated, that the old Norse and old Danish languages supply substantives with the termination ing, but that such substantives or nouns so ending, were not patronymics, or at the period referred to, used as patronymics, as the case is in the Anglo-Saxon. And as the Danes, during their Northumbrian sway, and subsequently, formed many settlements in the counties which we have just referred to, Danish names of such settlements, must therein abound. Bye, or as a suffix by, we have already considered as of Danish origin: and if we look over the names of places and districts in Westmoreland and Cumberland, we shall not find them rare but common, almost everywhere, witness: Harraby, Upperby, Moresby, Ponsonby, Netherby, Easby, Lamonby, and scores beside in Cumberland. In Westmoreland, Appleby, Colby, Waitby, Nateby, Soulby, and so on. While in Lancashire, there is Hornby,the strong hold of the Danes, in their occupation of the beautiful vale of the Lune, Crosby, and Formby, on the coast of the county: while in the interior, such names are scarcely, if at all, to be met with. Also in Lancashire, north of the sands, in the mountainous districts of Westmoreland and Cumberland, no such name is to be met with—the Cumbrian Welsh, then holding that region in their possession—and the names of places verifying, by their true derivations, the early history of these districts. And though we have stated that terminations in 'ing' were common in the Old Norse and Danish dialects, we nevertheless, find few, if any, throughout the immense numbers of "bies" in Yorkshire and the counties already quoted, appended to names in 'ing' either as denoting family settlers, or local characters; because, such a termination in these tongues, was generally applied as before mentioned, to personal traits, or animal and vital peculiarities. We thus learn, by our present subject, that though the Danes formed settlements on the sea coast of this county—either fixed, as in the names already given, or temporary, during their predatory incursions—they never gained a general footing in it, this its dialect shows. While the dialects, where Danish names abound, still partake of a Danish character, as perchance, if in accordance with the feelings of the members of this institution, I may take some future opportunity of demonstrating, by comparison and reference to authorities before them.

To complete the design, in bringing before you this Dissertation, one consideration only remains. Allusion has been made to other names, denoting residences, besides those already discussed. Among these, is that of Worth, which it may be worth while to explain, as supplying negative testimony to the general accuracy of the statements made, and the bearing of the facts produced, to support the truth of the novel explanations which have been given on this novel subject. While the Anglo-Saxon "tûn" signifies a proprietor's establishment, on a large scale, the Anglo-Saxon "worth" was the name given to their off farms. On such farms were built a residence or residences for the accommodation of the cultivators, and either the name of the resident given to the "worth" or a designation from any local feature of the place. It is hence, that from the pure Anglo-Saxon character of the surrounding district, we have so many names of the old Anglo-Saxon farms remaining among us, as Ainsworth, Failsworth, Pilsworth, Unsworth, Whitworth. Of these names too, when the first occupier's name is prefixed, that name is never of the patronymic form: and this, because of the secondary character of such place and persons, and the more recent formation of such divisions. For patronymics, seem primarily, to have been restricted to families of ancient caste, and noble origin, to Iclingas, Uffingas, &c.: while freemen, their ceorls bore mere simple designations, and therefore, could only transfer such simple names to their farms and residences when named after them.

We might similarly bring under our review, the other names of residences before mentioned, with like success: but besides being tiresome to do so, it would be superfluous. Doubtless many corruptions, have metamorphosed the old Anglo-Saxon patronymics, through a long lapse of time, and the ignorance and caprice of families, with names so nobly descended. Ings have been contracted into ins, and contrarywise, probably ins from foreign sources, confounded with ings. But this does not materially affect the position maintained throughout this Dissertation, which simply endeavours to show and to prove, that the pure Anglo-Saxon terminations, had not, nor could have had, any other origin.

To the scientific mind, such researches may perchance appear but an innocent kind of trifling, inasmuch as they promise no immediate nor direct advantage to the vast community and hive of industry around us. This may be true, yet the true and thorough philosophical mind will be far from deeming them useless. Names have a philosophy as well as the things which they represent. Mind has to be improved as well as the matter to be considered upon which it experiments, speculates, calculates ; and in order to communicate freely and fully with other minds, it is quite as necessary that it thoroughly comprehends the philosophy of the language through which it makes that communication, as the philosophy of the subjects which it has to communicate. Nor is it that alone which benefits mankind, in its civil capacity, which is to be considered as the legitimate standard of mental excellence. Whatever raises mind in the scale of intelligences—whatever brings before it truth of any name, grade, or degree—only really and permanently, ennobles it. Mind has been formed for truth, and nothing but truth therefore, can satisfy it. And surely, if language be at all worthy of our study—if we spend a considerable portion of our youth, in acquiring a competent knowledge of the Latin and Greek tongues, as keys, to open to us the treasure-houses of the thoughts of antiquity and the reasonings of refinement—our own language ought not less to demand from us some attention in our more mature years—that language which flows constantly from our tongues and our pens, in which we express all our thoughts, and interchange our ideas. He may be, and he is a *great* scholar, who can write Greek and Latin prose, or compose Epics and Lyrics in these languages, almost as well as the authors, who left behind them the models for such imitations: but that man, be he who he may, is the *best* scholar, who speaks his native tongue most purely, and writes it the most chastely and knowingly in all its primitive force, and elegant but eloquent simplicity.

ON THE

SUMMATION OF SERIES,

AND ON

DEFINITE INTEGRATION.

BY ROBERT RAWSON.

(Read November 12th, 1844.)

The summation of x terms of a series, in functions of x, is a proposition, which involves in its solution a considerable portion of the difficulties that are to be met with in the science of pure mathematics. Nor, indeed, is this much to be wondered at, when we consider, that, nearly all the obstacles, which oppose the progress of the investigator, in the application of Analysis to the Physical sciences, may be reduced, finally, to this single proposition.

Mathematicians have not been, hitherto, successful in the investigation of a general formula, which will apply, equally well, to the summation of all kinds of series. And in consequence of this, they have by various methods been enabled to give a solution to this problem, only in some particular classes of series.

One of the most celebrated of these methods, which was, I believe, first given by Newton, is that, which is derived from the calculus of Finite Differences; and, is called the Differential method. Its success may be said to depend upon the circumstance of one and every succeeding order of differences entirely vanishing. Therefore, all series, whose general term is a rational Algebraical Function, may then be summed.

Another method is, by means of a formula, which was first given by the celebrated Euler, and subsequently investigated by the late Rev. Robert Murphy, in his valuable Treatise on the Theory of Algebraical Equations. Mr. Murphy derived his formula from the expansion of the well-known expression $\frac{1}{E^{hz}-1}$, in powers of $h\, z$, by means of the decomposition of rational fractions. This method reduces the difficulties of the summation in question, to the determination of

$\int f(x)\, dx$, and the successive derived functions of $f(x)$, each of which is multiplied by a constant quantity, where $f(x)$ is the general term of the series. The formula, as given by Mr. Murphy, does not, however, appear to me to be very well adapted to practice.

This theorem of Euler, has been investigated also by Professor De Morgan, who has obtained the general form of it, by means of the calculus of Finite Differences, and, has determined the constant multipliers from the well-known expansion of $\frac{1}{E^a - 1}$, in powers of a.—(See Diff. and Integral Calculus, pp. 265, 266.)

It is, then, the object of the following investigation, to determine the solution of this proposition, by a method, which is, I believe, entirely new. Although, it has been justly remarked, by a mathematical writer of great reputation, that "it is dangerous for any one, at the present day, to claim anything as belonging to himself." I wish it, therefore, to be understood, that the method here spoken of, is not to be found in any of the different works on the subject which I have yet had the opportunity of consulting. It consists in the determination of $F(x+1)-F(x)=f\left(\Phi(x+h+1)\right)$, by means of its successive differential equations, and then eliminating from them, by Lagrange's method of multipliers, the functions $\overset{\text{II.}}{F}(x)$, $\overset{\text{III.}}{F}(x)$, $\overset{\text{IV.}}{F}(x)$, &c., &c.

PROB.—To find the sum of x terms of the series whose general term is $f\left\{\Phi(x+h)\right\}$, from the limits $x=1$ to $x=x$, where h is a constant quantity.

Now, if agreeably to the notation which is adopted by the writers on Definite Integral , we denote the sum of the series in question by $\overset{x}{\underset{1}{\Sigma}}.f\left\{\Phi(x+h)\right\}$, which is a function of x, say $F(x)$; then we shall have

$$\overset{x}{\underset{1}{\Sigma}}.f\left\{\Phi(x+h)\right\}=f\left\{\Phi(1+h)\right\}+f\left\{\Phi(2+h)\right\}+f\left\{\Phi(3+h)\right\}$$

$$+\ldots\ldots f\left\{\Phi(x+h)\right\} \qquad =F(x)\ \ldots\ldots(1)$$

$$\overset{x+1}{\underset{1}{\Sigma}}.f\left\{\Phi(x+h)\right\}=f\left\{\Phi(1+h)\right\}+f\left\{\Phi(2+h)\right\}+f\left\{\Phi(3+h)\right\}$$

$$+\ldots f\left\{\Phi(x+h)\right\}+f\left\{\Phi(x+h+1)\right\}=F(x+1)\ldots(2)$$

Subtract equation (1) from equation (2) and there will remain

$$F(x+1)-F(x)=f\left\{\phi(x+h+1)\right\} \quad\ldots\ldots\ldots\ldots\ldots\ldots\quad (3)$$

The solution of equation (3) depends upon the integration of Finite Differences; and by using the notation which is adopted in that science, equation (3) becomes

$\Delta\, F(x)=f\left\{\Phi(x+h+1)\right\}$, and by integrating we have $F(x)=\Sigma_1^x . f\left\{\phi\,(x+h+1)\right\}$ .... (4)

It is, then, the object of this paper to exhibit a solution of the equation (3) in terms of the differential coefficients of the x^{th} term of the series.

If we, now, apply Taylor's theorem * to the

* I shall here avail myself of the opportunity, to notice more particularly, the mode of treatment of the celebrated theorems of Maclaurin, Taylor, Lagrange, and Laplace, by most writers on the Differential and Integral Calculus.

The object of Maclaurin's and Taylor's theorems is, to develope, if possible, the functions $f(x)$ and $f(x+h)$. The first in ascending powers of x, and the second in ascending powers of h.

These two developements, when they are effected by the left-hand side of the equation (3), and Laplace's theorem to the right; then differentiate succes-

methods of Maclaurin and Taylor, are evidently in themselves really identities.

Given $z=y+x\,\mathrm{f}(z)$ to find the value of (not only z) but $\mathrm{F}(z)$, in terms of y, and ascending powers of x.

Given $z=\mathrm{F}_1\left\{y+x\,\mathrm{f}(z)\right\}$ to find the value of (not only z) but $\mathrm{F}(z)$, in terms of y, and ascending powers of x.

The complete solution of these functional equations, conducts us to what are called Lagrange's and Laplace's theorems, which are always obtained by the assistance of the theorems of Maclaurin and Taylor. The solution of the second equation, as given by Lacroix in his large work on the Differential and Integral Calculus, vol. 1, page 279, is remarkable, in consequence of its containing the great principle of the convertibility of independent differentiations.

Now, it is stated, by most of the modern writers on the Differential and Integral Calculus, that the theorems of Maclaurin and Taylor are only particular cases of those of Lagrange and Laplace.

With great respect, however, for the abilities, and opinions of these writers, I must say, that, there does not appear to me, to be any just grounds for making such an inference. I grant, that if in the first equation we make $\mathrm{f}(z)=1$, its solution will contain the developement of $\mathrm{F}(y+x)$. And if in the second equation we make $\mathrm{f}(z)=1$, and $\mathrm{F}(z)=z$, its solution will contain the developement of $\mathrm{F}_1(y+x)$. But what does this lead to, it only shows to any person who will take the trouble to examine the solutions, and then to retrace sively, and multiply the resulting equations by A, B, C, D, &c., &c., respectively, in order to

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their steps from the solution to the original equations, that these developements, themselves, have been obtained by either Maclaurin's or Taylor's theorems.

Therefore, to make such an inference as the one above alluded to, amounts to no more than saying, that x, is only a particular case of x^n: but who would presume to interpret x^n without first having a knowledge of x.

The theorems of Maclaurin and Taylor, are only identities, whereas, those of Lagrange and Laplace, are the solutions of two important functional equations: this distinction between these justly celebrated theorems, will be of greater importance as our knowledge of functional equations becomes more complete.

The late very distinguished mathematician, R. Murphy, obtained the solutions of the above equations in a simple manner, by means of a theorem, which he gave in a memoir, printed in the Cambridge Philosophical Society (See Murphy on the theory of Algebraical Equations, page 77).

Professor De Morgan, a great authority in these matters, in reference to this theorem of Mr. Murphy, remarks, that it "is one of the most general and interesting contributions which analysis has received for many years." (See Diff. and Int. Calculus, page 328).

This may not be an improper place to give a demonstration of Taylor's theorem, without employing the usual supposition of an infinite series.

Let f(x) be the function which we have to develope, when x takes an increment h.

facilitate the elimination of the functions $F^{11}(x)$,

First, $f(x)=f(x)$

Now, if x takes an increment h, the right hand side of the above equation will be increased by some function of x and h, consequently, we shall have

$$f(x+h)=f(x)+F(x,h) \ldots\ldots\ldots\ldots\ldots\ldots (1)$$

All that we know of the function $F(x, h)$ is, that it must vanish when $h=o$, otherwise, $f(x)=f(x)$ + some quantity. Which is absurd. Therefore, we see that $F(x, h)$ must be of the form $h\ F_1(x, h)$, where $F_1(x, h)$ denotes some other unknown function of x and h.

$$\therefore f(x+h)=f(x)+h\ F_1(x,h) \ldots\ldots\ldots\ldots\ldots (2)$$

Now, it is always possible to determine a function of $(x+h)$, which shall be equal to $F_1(x, h)$.

Therefore, we may put, $F_2(x+h)=F_1(x, h)$, where F_2 denotes an unknown function of $(x+h)$.

$$\therefore f(x+h)=f(x)+h\ F_2(x+h) \ldots\ldots\ldots\ldots (3)$$

In a similar manner we have, $F_2(x+h)=F_2(x)+h\ F_3(x+h)$

$$F_3(x+h)=F_3(x)+h\ F_4(x+h)$$

$$F_4(x+h)=F_4(x)+h\ F_5(x+h)$$

&c. &c. &c.

Where $F_2(x)$, $F_3(x)$, $F_4(x)$ &c. &c. are unknown functions of x.

If then, we substitute these values, successively, in equation (3), we shall obtain

$$f(x+h) = f(x) + h.F_2(x) + h^2.F_3(x) + h^3.F_4(x) + h^4.F_5(x) + \text{\&c., \&c., \&c.} + h^{n-1}.F_n(x+h) \ldots\ldots\ldots\ldots (4)$$

It, now, remains for us to determine the unknown functions

$\overset{\text{III}}{F}(x)$, &c., &c.; we shall have, putting $h+1=h'$

$F_2(x)$, $F_3(x)$, &c., &c. To effect this, let us take the successive derived functions, of equation (4), with respect to h.

$$\therefore f'(x+h)=F_2(x)+2h.F_3(x)+3h^2.F_4(x)+4h^3.F_5(x)+\text{\&c. \&c.}$$

$$f''(x+h)=2F_3(x)+2.3h.F_4(x)+3.4.h^2F_5(x)+\text{\&c. \&c.}$$

$$f''(x+h)=2.3.F_4(x)+2.3.4.h.F_5(x)+\text{\&c. \&c.}$$

&c., &c., &c.

These derived functions are true, whatever value be given to h. Take $h=o$, then we have

$$F_2(x) = \overset{\text{I}}{f}(x),\ \ F_3(x) = \frac{\overset{\text{II}}{f}(x)}{2}, F_4(x) = \frac{\overset{\text{III}}{f}(x)}{2\cdot3}\ \text{\&c., \&c.}$$

Substitute these values in equation (4), and we shall have

$$f(x+h)= f(x) + \frac{h}{1}.\ \overset{\text{I}}{f}(x) + \frac{h^2}{1\cdot2}.\ \overset{\text{II}}{f}(x) + \frac{h^3}{1\cdot2\cdot3}.\ \overset{\text{III}}{f}(x) + \ \text{\&c.,}$$

&c., &c. ....(5)

which is Taylor's theorem.

$$\overset{\mathrm{I}}{\mathrm{F}}(x) + \frac{\overset{\mathrm{II}}{\mathrm{F}}(x)}{2} + \frac{\overset{\mathrm{III}}{\mathrm{F}}(x)}{2.3} + \frac{\overset{\mathrm{IV}}{\mathrm{F}}(x)}{2.3.4} + \&c. = \mathrm{f}(\Phi x) + \overset{\mathrm{I}}{\mathrm{f}}(\Phi x)h' + \frac{\overset{\mathrm{II}}{\mathrm{f}}(\Phi x)h'^2}{1.2} + \frac{\overset{\mathrm{III}}{\mathrm{f}}(\Phi x).h'^3}{1.2.3} + \&c.$$

$$\mathrm{A}.\overset{\mathrm{II}}{\mathrm{F}}(x) + \frac{\mathrm{A}.\overset{\mathrm{III}}{\mathrm{F}}(x)}{2} + \frac{\mathrm{A}.\overset{\mathrm{IV}}{\mathrm{F}}(x)}{2.3} + \&c. = \quad + \mathrm{A}.\overset{\mathrm{I}}{\mathrm{f}}(\Phi x) + \mathrm{A}.\overset{\mathrm{II}}{\mathrm{f}}(\Phi x)h' + \frac{\mathrm{A}.\overset{\mathrm{III}}{\mathrm{f}}(\Phi x).h'^2}{1.2} + \&c.$$

$$\mathrm{B}.\overset{\mathrm{III}}{\mathrm{F}}(x) + \frac{\mathrm{B}.\overset{\mathrm{IV}}{\mathrm{F}}(x)}{2} + \&c. = \quad \mathrm{B}.\overset{\mathrm{II}}{\mathrm{f}}(\Phi x) + \mathrm{B}.\overset{\mathrm{III}}{\mathrm{f}}(\Phi x).h' + \&c.$$

$$\mathrm{C}.\overset{\mathrm{IV}}{\mathrm{F}}(x) + \&c. = \quad \mathrm{C}.\overset{\mathrm{III}}{\mathrm{f}}(\Phi x) \quad + \&c.$$

&c. .... (5)

Now by the addition of these equations there will result,

$$\overset{1}{\mathrm{F}}(x)=\mathrm{f}(\Phi x)+(\mathrm{A}+h').\overset{1}{\mathrm{f}}(\Phi x)+\left(\mathrm{B}+\frac{\mathrm{A}.h'}{1}+\frac{h'^2}{1.2}\right).\overset{\mathrm{II}}{\mathrm{f}}(\Phi x)+\left(\mathrm{C}+\frac{\mathrm{B}.h'}{1}+\frac{\mathrm{A}.h'^2}{1.2}+\frac{h'^3}{2.3.}\right).\overset{\mathrm{III}}{\mathrm{f}}(\Phi x)+\left(\mathrm{D}+\frac{\mathrm{C}.h'}{1}+\frac{\mathrm{B}.h'^2}{1.2}+\frac{\mathrm{A}.h'^3}{1.2.3}+\frac{h'^4}{1.2.3.4}\right).\overset{\mathrm{IV}}{\mathrm{f}}(\Phi x)+\left(\mathrm{E}+\frac{\mathrm{D}.h'}{1}+\frac{\mathrm{C}.h'^2}{1.2}+\frac{\mathrm{B}.h'^3}{1.2.3}+\frac{\mathrm{A}h'^4}{1.2.3.4}+\frac{h'^5}{1.2.3.4.5}\right).\overset{\mathrm{V}}{\mathrm{f}}(\Phi x)+\text{\&c., \&c.} \qquad (6)$$

where $\mathrm{A}+\frac{1}{2}=0$; $\mathrm{B}+\frac{\mathrm{A}}{2}+\frac{1}{2.3}=0$; $\mathrm{C}+\frac{\mathrm{B}}{2}+\frac{\mathrm{A}}{2.3}+\frac{1}{2.3.4}=0$; $\mathrm{D}+\frac{\mathrm{C}}{2}+\frac{\mathrm{B}}{2.3}+\frac{\mathrm{A}}{2.3.4}+\frac{1}{2.3.4.5}=0$; $\mathrm{E}+\frac{\mathrm{D}}{2}+\frac{\mathrm{C}}{2.3}+\frac{\mathrm{B}}{2.3.4}+\frac{\mathrm{A}}{2.3.4.5}+\frac{1}{2.3.4.5.6}=0$ &c., &c., &c.

Hence, from these conditional equations, the values of A, B, C, D, &c., &c., may be readily calculated.

$\mathrm{A}=-\frac{1}{2}$; $\mathrm{B}=\frac{1}{6}\cdot\frac{1}{2}=\mathrm{B}_1.\frac{1}{2}$; $\mathrm{C}=0$; $\mathrm{D}=-\frac{1}{30}\cdot\frac{1}{2.3.4}=-\frac{\mathrm{B}_3}{2.3.4}$; $\mathrm{E}=0$; $\mathrm{F}=\frac{1}{42}\cdot\frac{1}{2.3.4.5.6}=\frac{\mathrm{B}_5}{2.3.4.5.6}$;

$\mathrm{G}=0$; $\mathrm{H}=-\frac{1}{30}\cdot\frac{1}{2.3.4.5.6.7.8}=-\frac{\mathrm{B}_7}{2.3.4.5.6.7.8}$ &c.

where B_1, B_2, B_3, B_4, B_5, &c. are Bernoulli's numbers.

Therefore, since C, E, G, &c. are equal to nothing, the equation (6) will become

$$\overset{\mathrm{I}}{F}(x) = f(\phi x) + (A + h')\overset{\mathrm{I}}{f}(\phi x) + \left(B + Ah' + \frac{h'^2}{2}\right) \overset{\mathrm{II}}{f}(\phi x) + \left(Bh' + \frac{Ah'^2}{1.2} + \frac{h'^3}{2.3}\right)\overset{\mathrm{III}}{f}(\phi x) + \left(D + \frac{Bh'^2}{2} + \frac{Ah'^3}{2.3} + \frac{h'^4}{2.3.4}\right)\overset{\mathrm{IV}}{f}(\phi x) + \left(Dh' + \frac{Bh'^3}{2.3} + \frac{Ah'^4}{2.3.4} + \frac{h'^5}{2.3.4.5}\right) \overset{\mathrm{V}}{f}(\phi x) + \&c., \&c. + \ldots\ldots(7)$$

By integrating this equation we have,

$$F(x) = \Sigma_{\mathrm{I}}^{x} . f\left\{\phi(x+h)\right\} = \int f(\phi x)\,dx + (A + h') f\left(\phi(x)\right) + \left(B + Ah' + \frac{h'^2}{2}\right)\overset{\mathrm{I}}{f}(\phi x) + \left(Bh' + \frac{Ah'^2}{2} + \frac{h'^3}{2.3}\right)\overset{\mathrm{II}}{f}(\phi x) + \left(D + \frac{Bh'^2}{2} + \frac{Ah'^3}{2.3} + \frac{h'^4}{2.3.4}\right)\overset{\mathrm{III}}{f}(\phi x) + \left(Dh' + \frac{Bh'^3}{2.3} + \frac{Ah'^4}{2.3.4} + \frac{h'^5}{2.3.4.5}\right)\overset{\mathrm{IV}}{f}(\phi x) + \ldots \&c. + C \ldots(8)$$

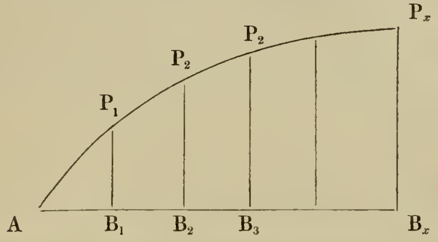
Where the constant C must be determined by taking a particular value for x. Therefore the sum of x terms of the series, whose general term is $\text{f}\left\{\Phi(x+h)\right\}$, is made to depend upon the integration of $\text{f}(\Phi x)dx$, and the derived functions of $\text{f}(\Phi x)$. Again, since equation (8) is true for any value of x, it will be true when $x=y$; then we shall have

$$\Sigma_{\mathrm{I}}^{y} . \text{f}\left\{\Phi(y+h)\right\}=\int \text{f}(\Phi y)\,dy + (\text{A}+h')\,\text{f}(\Phi y) + \left(\text{B}+\text{A}h'+\frac{h'^2}{2}\right)\overset{\mathrm{I}}{\text{f}}(\Phi y)+\left(\text{B}h'+\frac{\text{A}h'^2}{2}+\frac{h'^3}{2.3}\right)\overset{\mathrm{II}}{\text{f}}(\Phi y) + \left(\text{D}+\frac{\text{B}h'^2}{2}+\frac{\text{A}h'^3}{2.3}+\frac{h'^4}{2.3.4}\right)\overset{\mathrm{III}}{\text{f}}(\Phi y)+\left(\text{D}h'+\frac{\text{B}h'^3}{2.3}+\frac{\text{A}h'^4}{2.3.4}+\frac{h'^5}{2.3.4.5.}\right)\overset{\mathrm{IV}}{\text{f}}(\Phi y) + \ldots\ldots \&\text{c.} + \text{C} \ldots\ldots\ldots (9)$$

And if we subtract equation (9) from equation (8), we shall have,

$$\Sigma_{y}^{x} . \text{f}\left\{\Phi(x+h)\right\} = \int_{y}^{x} . \text{f}(\Phi x)\,dx + (\text{A}+h')\left\{\text{f}(\Phi x)-\text{f}(\Phi y)\right\}+\left(\text{B}+\text{A}h'+\frac{h'^2}{2}\right)\left\{\overset{\mathrm{I}}{\text{f}}(\Phi x)-\overset{\mathrm{I}}{\text{f}}(\Phi y)\right\} + \left(\text{B}h'+\frac{\text{A}h'^2}{2}+\frac{h'^3}{2.3}\right)\left\{\overset{\mathrm{II}}{\text{f}}(\Phi x)-\overset{\mathrm{II}}{\text{f}}(\Phi y)\right\}+\left(\text{D}+\frac{\text{B}h'^2}{2}+\frac{\text{A}h'^3}{2.3}+\frac{h'^4}{2.3.4.}\right)\left\{\overset{\mathrm{III}}{\text{f}}(\Phi x)-\overset{\mathrm{III}}{\text{f}}(\Phi y)\right\}+ \&\text{c.} \ldots\ldots\ldots (10)$$

Now, if we describe the curve A P_1 P_2 &c., &c., whose equation is $Y = f(\Phi x)$, x being measured along the line AB_x, and Y perpendicular to it; and at equal intervals AB_1; AB_2 &c., &c.



draw the corresponding ordinates P_1B_1; P_2B_2, &c., &c. Then, from equation (10), we have

$$\Sigma_y^x . f\left\{\Phi(x+h)\right\} = A_1 + (A+h')\left\{f(\Phi x) - f(\Phi y)\right\} + \left(B + Ah' + \frac{h'^2}{2}\right)\left\{\overset{I}{f}(\Phi x) - \overset{I}{f}(\Phi y)\right\} + \left(Bh' + \frac{Ah'^2}{2} + \frac{h'^3}{2.3}\right)$$

$$\left\{\overset{II}{f}(\Phi x) - \overset{II}{f}(\Phi y)\right\} + \left(D + \frac{Bh'^2}{2} + \frac{Ah'^3}{2.3} + \frac{h'^4}{2.3.4}\right)\left\{\overset{III}{f}(\Phi x)\right.$$

$$\left. - \overset{III}{f}(\Phi y)\right\} + \text{\&c., \&c.} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (11)$$

where A_1 is the area of the curve B_y P_y P_x B_x.

Hence it appears, that if we, now, sum the series whose general term is f $\left\{\Phi(x+h)\right\}$, first when x is continuous, that is, when x takes every possible value between o and x; and secondly, when x is discontinuous, that is, when x takes the values 1, 2, 3, &c., respectively, we shall readily see from equation (11) that the difference of the summation of these two series will be expressed by

$$\Psi(x)=\left(A+h'\right)\left\{f(\Phi x)-f(\Phi y)\right\}+\left(B+Ah'+\frac{h'^2}{2}\right)\left\{\overset{I}{f}(\Phi x)-\overset{I}{f}(\Phi y)\right\}+\left(Bh'+\frac{Ah'^2}{2}+\frac{h'^3}{2.3}\right)\left\{\overset{II}{f}(\Phi x)-\overset{II}{f}(\Phi y)\right\}+\left(D+\frac{Bh'^2}{2}+\frac{Ah'^3}{2.3}+\frac{h'^4}{2.3.4}\right)\left\{\overset{III}{f}(\Phi x)-\overset{III}{f}(\Phi y)\right\}+$$

&c., &c. .... (12)

whose general term is $\mathfrak{z}(h')\left\{\overset{n-1}{f}(\Phi x)-\overset{n-1}{f}(\Phi y)\right\}$

$$\therefore \Psi(x)=\Sigma_1^{\infty}.\ \mathfrak{z}(h')\left\{\overset{n-1}{f}(\Phi x)-\overset{n-1}{f}(\Phi y)\right\}.$$

therefore we shall have from equation (10)

$$\int_y^x . f(\Phi x)dx=\Sigma_y^x . f\left\{\Phi(x+h)\right\}-\Sigma_1^{\infty} . \mathfrak{z}(h') . \left\{\overset{n-1}{f}(\Phi x)-\overset{n-1}{f}(\Phi y)\right\} \quad \text{.................................} \ (13)$$

This theorem, which will be found to be of extensive utility in the definite integration of those functions which can be integrated only by means of infinite series, is remarkable, from the circumstance of its expressing the value of the definite integral $\int_y^x . \mathrm{f}(\Phi x)\, dx$ by means of two distinct quantities which are, first, the summation of a definite number of terms of the series whose general term is $\mathrm{f}\left\{\Phi(x+h)\right\}$, and secondly, the summation of an infinite series whose general term is $5\,(h')\left\{\stackrel{n-1}{\mathrm{f}}(\Phi x) - \stackrel{n-1}{\mathrm{f}}(\Phi y)\right\}$, which summation we shall now designate the *complement* of the definite integral $\int_y^x . \mathrm{f}(\Phi x)\, dx$.

Equation (13) will enable us to determine $\Psi(x)$ when $\int_y^x . \mathrm{f}(\Phi x) dx$ and $\Sigma_y^x . \mathrm{f}\left\{\Phi(x+h)\right\}$ are known, and consequently, when any two of the above distinct quantities are given, we can find the third.

Cor. 1. If we take $\Phi(x) = x$, we shall have from equation (13)

$$\int_y^x . \mathrm{f}(x)\, dx = \Sigma_y^x . \mathrm{f}(x+h) - \Sigma_1^\infty . \mathfrak{z}(h') \left\{ \mathrm{f}^{n-1}(x) - \mathrm{f}^{n-1}(y) \right\} \ldots\ldots (14)$$

This theorem, involving an arbitrary quantity h, which may be made to fulfil any condition at pleasure, expresses the definite integral of f (x) dx, between the limits of $x=x$ and $x=y$.

Cor. 2. Take $h=o \therefore h'=1$; and the co-efficients of the even derived functions of f (x) will vanish; this will be the case if h' be taken equal to nothing, consequently we shall have

$$\int_y^x . \mathrm{f}(x)\, dx = \Sigma_y^{x-1} . \mathrm{f}(x) - \Sigma_1^\infty . \frac{(-1)^n B_{2n-3}}{1.2.3 \ldots 2n-2} \left(\mathrm{f}^{2n-3}(x) - \mathrm{f}^{2n-3}(y) \right)$$

$$= \Sigma_y^{x-1} . \mathrm{f}(x) - A\left(\mathrm{f}(x) - \mathrm{f}(y)\right) - B\left(\mathrm{f}^{\mathrm{I}}(x) - \mathrm{f}^{\mathrm{I}}(y)\right)$$
$$- D\left(\mathrm{f}^{\mathrm{III}}(x) - \mathrm{f}^{\mathrm{III}}(y)\right) - \text{\&c., \&c.} \ldots\ldots (15)$$

Scholiam. After having obtained the theorem in equation (15), I discovered, in looking over the late Mr. Murphy's admirable Treatise on the Theory of Algebraical Equations, page 101; and the Differential and Integral Calculus, by Professor De Morgan, page 311, that each of these Mathematicians had investigated a somewhat similar theorem, given originally by Euler, but in a form less practicable than the above, and derived from entirely different principles. Nor do these very eminent Mathematicians appear ever to have thought of applying their theorem to the approximation of definite integrals.

It now remains for me to apply the above general formula, by way of illustration, to a few examples.

Ex. (1.)—Required to approximate to the integral of log. $x\,dx$, between the limits $y=11$ and $x=20$.

Here, we have $\mathrm{f}(x) = \log. x \therefore \overset{\mathrm{I}}{\mathrm{f}}(x) = \frac{1}{x}$, $\overset{\mathrm{III}}{\mathrm{f}}(x) = \frac{2}{x^3}$; $\overset{\mathrm{V}}{\mathrm{f}}(x) = \frac{2.3.4}{x^5}$ &c., &c.

And, if we substitute these values in formula (15), we shall have, by restoring the values of A, B, D, &c., &c.,

$$\int_y^x . \log. x\, dx = \Sigma_y^x \log. x - \frac{1}{2}(\log. x - \log. y) - \frac{1}{12}\left(\frac{1}{x}-\frac{1}{y}\right) + \frac{1}{30.3.4}\left(\frac{1}{x^3}-\frac{1}{y^3}\right) - \frac{1}{42.5.6}\left(\frac{1}{x^5}-\frac{1}{y^5}\right) + \&c., \&c. \quad \ldots\ldots (16)$$

This series is better adapted for computation, and converges much faster, than the series given by Professor De Morgan (see his Diff. and Integral Calculus, pages 313, 314).

If we calculate $\int \log. x\, dx$, between the limits $y=11$, and $x=20$, we shall have as follows:—

$$\int_{11}^{20} . \log. x\, dx = \Sigma_{11}^{20} . \log. x - \frac{1}{2}(\log. 20 - \log. 11) + \frac{1}{12}\left(\frac{1}{11}-\frac{1}{20}\right) - \frac{1}{30.3.4}\left(\frac{1}{11^3}-\frac{1}{20^3}\right) - \&c., \&c.$$

$=24.8333086 - .2989185 + .0034090 - .0000017$
$=24.5377974$. which is true to the seventh place of decimals.

It will be seen that the above near approach to the actual value of $\int_{11}^{20} \log. x dx$ has been obtained, by summing only three terms of the complement of the integral $\int_{11}^{20} .\log. x\, dx$, of which each term is readily computed. I may, however, state that Professor De Morgan, after taking of his series six terms, which are much more difficult to calculate than those in the series above given, makes an approximation which is true only to the sixth place of decimals. The series, which he has given for the purpose of approximating to the value of the definite integral, is expressed in terms of the function and its successive finite differences.

Ex. 2.—Required to approximate to $\int \frac{dx}{x}$, which we know to be log. x.

Here $f(x) = \frac{1}{x} \therefore f^{\text{I}}(x) = -\frac{1}{x^2}$; $f^{\text{III}}(x) = -\frac{2.3}{x^4}$; $f^{\text{V}}(x) = -\frac{2.3.4.5}{x^6}$ &c., &c. If we substitute these values in equation (15), and restore the values of A, B, &c., we shall have

$$\log. x - \log. y = \Sigma_y^x . \frac{1}{x} + \frac{1}{2}\left(\frac{1}{y} - \frac{1}{x}\right) - \frac{1}{12}\left(\frac{1}{y^2} - \frac{1}{x^2}\right)$$

$$+ \frac{1}{30.4}\left(\frac{1}{y^4} - \frac{1}{x^4}\right) - \frac{1}{42.6}\left(\frac{1}{y^6} - \frac{1}{x^6}\right) + \text{ \&c., \&c.} \ldots \quad (17)$$

This series will be found very convenient in calculating the hyperbolic logarithm of x, when x is pretty large, and when y differs from x only by unity.

Cor. If the difference between x and y be unity, the difference of their logarithms will be expressed by $\frac{1}{2}\left(\frac{1}{y}+\frac{1}{x}\right)-\frac{1}{12}\left(\frac{1}{y}-\frac{1}{x^2}\right)$ nearly, which is exceedingly near the truth, if x be a large number.

Ex. 3.—Required the sum of x terms of the series whose general term is $(m+ax)^n$.

Here, we have $f(x)=(m+ax)^n$; $\int f(x)dx = \frac{(m+ax)^{n+1}}{a(n+1)}$; $f^{\text{I}}(x)=an(m+ax)^{n-1}$, $f^{\text{III}}(x)= a^3.n(n-1)(n-2).(m+ax)^{n-3}$; $f^{\text{V}}(x)=a^5.n(n-1),\ldots(n-4).(m+ax)^{n-5}$ &c., &c., &c.

By substituting these values in equation (15), and the values of A, B, C, &c., as previously determined, we shall have

$$\overset{x-1}{\underset{y}{\Sigma}} . \ (m+ax)^{n} = \left\{ \frac{(m+ax)^{n+1} - (m+ay)^{n+1}}{a\ (n+1)} \right\} - *$$

$$\left\{ \frac{(m+ax)^{n} - (m+ay)^{n}}{2} \right\} + \frac{a\ n}{6} . \left\{ \frac{(m+ax)^{n-1} - (m+ay)^{n-1}}{[2]} \right\}$$

$$- \frac{a^3 . n\ (n-1)\ (n-2)}{30 \qquad [4]} . \left\{ (m+ax)^{n-3} - (m+ay)^{n-3} \right\}$$

$$+ \frac{a^5 . n . (n-1) \ldots . (n-4)}{42\ .\ [6]} . \left\{ (m+ax)^{n-5} - (m+ay) \right\}$$

$$- \frac{a^7 . n (n-1) \ldots . (n-6)}{30 \quad [8]} . \left\{ (m+ax)^{n-7} - (m+ay)^{n-7} \right\} +$$

$$\frac{5\, a^9 . n\, (n-1) \ldots . (n-8)}{66 \quad [10]} \left\{ (m+ax)^{n-9} - (m+ay)^{n-9} \right\}$$

$$- \ \&c., \ \&c. \ \ldots\ldots\ldots\ldots \ (18)$$

This series, which will terminate when n is any whole number, was, I believe, first given between the limits $x=1$ and $x=x$, by Simpson, who obtained it by means of a very elaborate investigation (see his Essays, page 101.)

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* By changing this sign from minus to plus, we have $\overset{x}{\underset{y}{\Sigma}} . \ (m+ax)^{n}$, instead of $\overset{x-1}{\underset{y}{\Sigma}} . (m+ax)^{n}$; which is a very singular property.

Cor. If we take $n=1, 2, 3, 4$, &c. successively we shall have

$$\sum_0^x . (m+ax) = \frac{a\,x^2}{2} + \frac{2m+a}{2} . x$$

$$\sum_0^x . (m+ax)^2 = \frac{a^2 x^3}{3} + \left(ma + \frac{a^2}{2}\right) x^2 + \left(m^2 + am + \frac{a^2}{6}\right) . x.$$

$$\sum_0^x . (m+ax)^3 = \frac{a^3 x^4}{4} + \left(ma^2 + \frac{a^3}{2}\right) x^3 + \left(\frac{3m^2 a}{2} + \frac{3ma^2}{2} + \frac{a^3}{4}\right) x^2 + \left(m^3 + \frac{3m^2 a}{2} + \frac{ma^2}{2}\right) x.$$

Ex. 4.—Required the sum of x terms of the series whose general term is

$$a+bx+cx^2+dx^3+ex^4+ \text{ \&c., \&c.}$$

Here we shall have $\mathrm{f}\,(x)=a+bx+cx^2+$ &c.; $\int \mathrm{f}\,(x)\,dx=ax+\frac{bx^2}{2}+\frac{cx^3}{3}+$ &c., &c.; $\overset{\mathrm{I}}{\mathrm{f}}\,(x)=b+2cx+3dx^2+$ &c., &c.; $\overset{\mathrm{III}}{\mathrm{f}}\,(x)=2.3d+2.3.4ex+3.4.5fx^2+$ &c., &c. $\overset{\mathrm{V}}{\mathrm{f}}\,(x)=[5]f+[6]\,gx+[3, 7]\,hx^2+$ &c., &c. Where $[3, 7]=3.4.5.6.7$, and generally $[x,y]=x\,(x+1)\,(x+2)$ to y.

By substituting these values in equation (15), we shall have as follows :—

$$\overset{x}{\underset{y}{\Sigma}}.(a+bx+cx^2+\&c.)=\Big(a(x-y)+\frac{b}{2}(x^2-y^2)+\frac{c}{3}$$

$$(x^3-y^3)+\&c.\Big)+\frac{1}{2}\Big(a+b(x-y)+c(x^2-y^2)+dx^3-$$

$$y^3)+\&c.\Big)+\Big(\frac{b+2c(x-y)+3d(x^2-y^2)+4e(x^3-y^3)}{6.[2]}+\&c.\Big)$$

$$-\frac{[3].d+[4]e(x-y)+[3.5]f(x^2-y^2)+[4,6]g(x^3-y^3)+\&c.}{30.[4]}$$

$$+\ \&c., \&c. \quad\ldots\ldots\ldots\ldots\ldots\ldots \quad (19)$$

If we take $y=o$ we have

$$\overset{x}{\underset{o}{\Sigma}}.(a+bx+cx^3)=\Big(a+\frac{b}{2}+\frac{c}{6}\Big)x+\frac{b+c}{2}.x^2+\frac{cx^3}{3}.$$

$$\overset{x}{\underset{o}{\Sigma}}.(a+bx+cx^2+dx^3)=\Big(a+\frac{b}{2}+\frac{c}{6}\Big)x+\Big(\frac{b}{2}+\frac{c}{2}$$

$$+\frac{d}{4}\Big)x^2+\Big(\frac{c}{3}+\frac{d}{2}\Big)x^3+\frac{dx^4}{4}.$$

$$\overset{x}{\underset{o}{\Sigma}}.(a+bx+cx^2+dx^3+ex^4)=\Big(a+\frac{b}{2}+\frac{c}{6}-\frac{e}{30}\Big)x$$

$$+\Big(\frac{b}{2}+\frac{c}{2}+\frac{d}{4}\Big)x^2+\Big(\frac{c}{3}+\frac{d}{2}+\frac{e}{3}\Big)x^3+\Big(\frac{d}{4}+\frac{e}{2}\Big)$$

$$x^4+\frac{ex^5}{5}.$$

$$\overset{x}{\underset{0}{\Sigma}} \cdot (a+bx+cx^2+dx^3+ex^4+fx^5) = \left(a + \frac{b}{2} + \frac{c}{b} - \frac{e}{30}\right)x + \left(\frac{b}{2} + \frac{c}{2} + \frac{d}{4} - \frac{f}{12}\right)x^2 + \left(\frac{c}{3} + \frac{d}{2} + \frac{e}{3}\right) x^3 + \left(\frac{d}{4} + \frac{e}{2} + \frac{5b}{12}\right)x^4 + \left(\frac{e}{5} + \frac{f}{2}\right)x^5 + \frac{fx^6}{6}.$$

It would not be difficult to extend these cases to terms involving g, h, i, &c., &c., &c.

Ex. 5.—Required the sum of x terms of the series whose general term is $\left(\frac{1}{4x-3} - \frac{1}{4x-1}\right)$.

Here, we have $f(x) = \left(\frac{1}{4x-3} - \frac{1}{4x-1}\right)$;

$$\int f(x)\, dx = \frac{1}{4}\Big(\log.(4x-3) - \log.(4x-1)\Big) = -\frac{1}{4} \log.\frac{4x-1}{4x-3};\ \overset{\text{I}}{f}(x) = -\left(\frac{4}{(4x-3)^2} - \frac{4}{(4x-1)^2}\right);\ \overset{\text{III}}{f}(x) = -\left(\frac{2.3.4^3}{(4x-3)^4} - \frac{2.3.4^3}{(4x-1)^4}\right);\ \overset{\text{V}}{f}(x) = -\left(\frac{2.3.4.5.4^5}{(4x-3)^6} - \frac{2.3.4.5.4^5}{(4x-1)^6}\right);\ \overset{\text{VII}}{f}(x) = -\left(\frac{2.3.4.5.6.7.4^7}{(4x-3)^8} - \frac{2.3.4.5.6.7.4^7}{(3x-1)^8}\right);$$

$$\overset{\text{IX}}{f}(x) = -\left(\frac{2.3\ldots9.4^9}{(4x-3)^{10}} - \frac{2.3\ldots9.4^9}{(4x-1)^{10}}\right) \text{ \&c., \&c.}$$

Hence equation (15) will become

$$\sum_y^x . \left(\frac{1}{4x-3} - \frac{1}{4x-1}\right) = -\frac{1}{4}\left(\log. \frac{4x-1}{4x-3} - \log. \frac{4y-1}{4y-3}\right)$$

$$+ \frac{1}{2}\left(\frac{1}{4x-3} - \frac{1}{4x-1} - \frac{1}{4y-3} + \frac{1}{4y-1}\right) -$$

$$\frac{1}{3}\left(\frac{1}{(4x-3)^2} - \frac{1}{(4x-1)^2} - \frac{1}{(4y-3)^2} + \frac{1}{(4y-1)^2}\right) +$$

$$\frac{8}{15}\left(\frac{1}{(4x-3)^4} - \frac{1}{(4x-1)} - \frac{1}{(4y-3)^4} + \frac{1}{(4y-1)^4}\right) -$$

$$\frac{4^4}{63}\left(\frac{1}{(4x-3)^6} - \frac{1}{(4x-1)^6} - \frac{1}{(4y-3)^6} + \frac{1}{(4y-1)^6}\right) +$$

$$\frac{4^5}{15}\left(\frac{1}{(4x-3)^8} - \frac{1}{(4x-1)^8} - \frac{1}{(4y-3)^8} + \frac{1}{(4y-1)^8}\right) -$$

$$\frac{4^8}{33}\left(\frac{1}{(4x-3)^{10}} - \frac{1}{(4x-1)^{10}} - \frac{1}{(4y-3)^{10}} + \frac{1}{(4y-1)^{10}}\right) + \&c.,$$

&c., &c. .... (20)

The complement of the definite integral $\int_y^x \left(\frac{1}{4x-3} - \frac{1}{4x-1}\right) dx$, converges very rapidly when x and y are large numbers.

If in equation (20) we take $x =$ infinity, we shall have

$$\Sigma_y^{\infty}.\left(\frac{1}{4x-3}-\frac{1}{4x-1}\right)=\frac{1}{4}\log.\left(\frac{4y-1}{4y-3}\right)-\frac{1}{2}\left(\frac{1}{4y-3}-\frac{1}{4y-1}\right)+\frac{1}{3}\left(\frac{1}{(4y-3)^2}-\frac{1}{(4y-1)^2}\right)-\frac{8}{15}\left(\frac{1}{(4y-3)^4}-\frac{1}{(4y-1)^4}\right)+\frac{4^4}{63}\left(\frac{1}{(4y-3)^6}-\frac{1}{(4y-1)^6}\right)-\frac{4^5}{15}\left(\frac{1}{(4y-3)^8}-\frac{1}{(4y-1)^8}\right)+\frac{4^8}{33}\left(\frac{1}{(4y-3)^{10}}-\frac{1}{(4y-1)^{10}}\right)-\&c., \&c. \ldots (21)$$

But $\Sigma_y^{\infty}.\left(\frac{1}{4x-3}-\frac{1}{4x-1}\right)=\Sigma_1^{\infty}.\left(\frac{1}{4x-3}-\frac{1}{4x-1}\right)-\Sigma_1^{y}\left(\frac{1}{4x-3}-\frac{1}{4x-1}\right)$. consequently equation (21) will become,

$$\Sigma_1^{\infty}.\left(\frac{1}{4x-3}-\frac{1}{4x-1}\right)=\Sigma_1^{y}.\left(\frac{1}{4x-3}-\frac{1}{4x-1}\right)+\frac{1}{4}\log.\left(\frac{4y-1}{4y-1}\right)-\frac{1}{2}\left(\frac{1}{4y-3}-\frac{1}{4y-1}\right)+\frac{1}{3}\left(\frac{1}{(4y-3)^2}-\frac{1}{(4y-1)^2}\right)-\frac{8}{15}\left(\frac{1}{(4y-3)^4}-\frac{1}{(4y-1)^4}\right)+\frac{4^4}{63}\left(\frac{1}{(4y-3)^6}-\frac{1}{(4y-1)^6}\right)-\frac{4^5}{15}\left(\frac{1}{(4y-3)^8}-\frac{1}{(4y-1)^8}\right)+\frac{4}{33}\left(\frac{1}{(4y-3)^{10}}-\frac{1}{(4y-1)^{10}}\right)-\&c., \&c.$$

$=\frac{\pi}{4}$, where π is the circumference of a circle, whose radius is unity. (See Lagrange's Calcul des Functions, Leçon septime.)

By referring to equation (17) we shall have

$$\frac{1}{4}\log.\frac{4y-1}{4y-1} = \frac{1}{4}\Big(\log.\,(4y-1) - \log.\,(4y-3)\Big) =$$

$$\frac{1}{4}\,\Sigma_{4y-1}^{4y-1}.\left(\frac{1}{x}\right) + \frac{1}{2.4}\left(\frac{1}{4y-3} - \frac{1}{4y-1}\right) -$$

$$\frac{1}{12.4}\left(\frac{1}{(4y-3)^2} - \frac{1}{(4y-1)^2}\right) + \frac{1}{30.4.4}\left(\frac{1}{(4y-3)^4} - \frac{1}{(4y-1)^4}\right) -$$

$$\frac{1}{42.6.4}\left(\frac{1}{(4y-3)^6} - \frac{1}{(4y-1)^6}\right) + \frac{1}{30.8.4}\left(\frac{1}{(4y-3)^8} - \frac{1}{(4y-1)^8}\right)$$

$$-\frac{5}{66.10.4}\left(\frac{1}{(4y-3)^{10}} - \frac{1}{(4y-1)^{10}}\right) + \&c.$$

And by substituting this value of $\frac{1}{4}$ log. $\left(\frac{4y-1}{4y-3}\right)$ in the above equation, we have, for the quadrature of the circle, whose radius is unity, the following expression :—

$$\frac{\pi}{4} = \Sigma_{1}^{y}.\left(\frac{2}{(4x-3)(4x-1)}\right) + \frac{1}{4}\,\Sigma_{4y-1}^{4y-1}.\left(\frac{1}{x}\right) -$$

$$\frac{3}{2.4}\left(\frac{1}{4y-3} - \frac{1}{4y-1}\right) + \frac{5}{4.4}\left(\frac{1}{(4y-3)^2} - \frac{1}{(4y-1)^2}\right) -$$

$$\frac{51}{6.4.4}\left(\frac{1}{(4y-3)^4} - \frac{1}{(4y-1)^4}\right) + \frac{66}{4.4}\left(\frac{1}{(4y-3)^6} -\right.$$

$$\left.\frac{1}{(4y-1)^6}\right) - \frac{4369}{4.4.4}\left(\frac{1}{(4y-3)^8} - \frac{1}{(4y-1)^8}\right) + \frac{349525}{11.4.4}\left(\frac{1}{(4y-3)^{10}} -\right.$$

$$\left.\frac{1}{(4y-1)^{10}}\right) - \&c., \&c. \ldots\ldots\ldots\ldots\ldots\ldots(22)$$

This remarkable series, which expresses the quadrature of the circle, may be made to have any degree of convergency, by assuming different values for y.

If we take $y=6 \therefore 4y-3=21$ and $4y-1=23$, and consequently equation (22) will become

$$\frac{\pi}{4}=2\,\Sigma_{1}^{6}.\left(\frac{1}{(4x-3)\,(4x-1)}\right)+\frac{1}{4}\,\Sigma_{21}^{23}.\left(\frac{1}{x}\right)-\frac{3}{2.4}$$

$$\left(\frac{1}{21}-\frac{1}{23}\right)+\frac{5}{4.4}\left(\frac{1}{21^2}-\frac{1}{23^2}\right)-\ \&c., \&c.$$

$$=.7646006+.0222309-.0015528+.0001119= .7853989$$

Hence it appears that by taking only two terms of the complement, together with the terms under Σ, we have the quadrature of the circle true to the sixth place of decimals.

Ex. 6.—Let it be required to sum the series $1-\frac{1}{2}+\frac{1}{3}-\frac{1}{4}+\frac{1}{5}-\frac{1}{6}+$ &c., to infinity, which is the hyperbolic logarithm of 2.

The general term of this series will be, evidently, $\left(\frac{1}{2x-1}-\frac{1}{2x}\right)$. consequently, we shall have

$$\mathrm{f}(x) = \left(\frac{1}{2x-1} - \frac{1}{2x}\right); \ \mathrm{f}^{\mathrm{I}}(x) = -\left(\frac{2}{(2x-1)^2} - \frac{2}{(2x)^2}\right);$$

$$\mathrm{f}^{\mathrm{III}}(x) = -\frac{2^4.3}{1}\left(\frac{1}{(2x-1)^4} - \frac{1}{(2x)^4}\right), \ \mathrm{f}^{\mathrm{V}}(x) = -2^6.3.4.5$$

$$\left(\frac{1}{(2x-1)^6} - \frac{1}{(2x)^6}\right), \ \& \int \mathrm{f}(x)\, dx = -\frac{1}{2}\log.\left(\frac{2x}{2x-1}\right).$$

Substitute these values in equation (15) and we shall have

$$\Sigma_y^x.\left(\frac{1}{2x-1} - \frac{1}{2x}\right) = \Sigma_y^x.\left(\frac{1}{(2x-1)2x}\right) = -\frac{1}{2}\left(\log.\frac{2x}{2x-1} - \right.$$

$$\left.\log.\frac{2y}{2y-1}\right) + \frac{1}{2}\left(\frac{1}{2x-1} - \frac{1}{2x} - \frac{1}{2y-1} + \frac{1}{2y}\right) -$$

$$\frac{1}{6}\left(\frac{1}{(2x-1)^2} - \frac{1}{(2x)^2} - \frac{1}{(2y-1)^2} + \frac{1}{(2y)^2}\right) + \frac{1}{15}\left(\frac{1}{(2x-1)^4} - \right.$$

$$\frac{1}{(2x)^4} - \frac{1}{(2y-1)^4} + \left.\frac{1}{(2y)^4}\right) - \frac{8}{63}\left(\frac{1}{(2x-1)^6} - \frac{1}{(2x)^6} + \right.$$

$$\left.\frac{1}{(2y-1)^6} - \frac{1}{(2y)^6}\right) + \&c., \&c.$$

Since $\frac{1}{2}\log.\frac{2y}{2y-1} = \frac{1}{2}\ \Sigma_{2y-1}^{2y}.\left(\frac{1}{x}\right) + \frac{1}{4}\left(\frac{1}{2y-1}\right.$

$$\left.-\frac{1}{2y}\right) - \frac{1}{12.2}\left(\frac{1}{(2y-1)^2} - \frac{1}{(2y)^2}\right) + \frac{1}{30.8}\left(\frac{1}{(2y-1)^4} - \right.$$

$$\left.\frac{1}{(2y)^4}\right) - \frac{1}{42.12}\left(\frac{1}{(2y-1)^6} - \frac{1}{(2y)^6}\right) + \&c., \&c.$$

from equation (17) Ex. 2. And by taking $x =$ infinity, the above equation will become

$$\Sigma_{y}^{\infty}.\left(\frac{1}{(2x-1)2x}\right)=\Sigma_{1}^{\infty}.\left(\frac{1}{(2x-1)2x}\right)-\Sigma_{1}^{y}.\left(\frac{1}{(2x-1)2x}\right)$$

$$=\frac{1}{2}\Sigma_{2y-1}^{2y}.\left(\frac{1}{x}\right)-\frac{1}{4}\left(\frac{1}{4y-1}-\frac{1}{2y}\right)+\ \&c.,\ \&c.$$

therefore $\Sigma_{1}^{\infty}.\left(\frac{1}{(2x-1)2x}\right)=\Sigma_{1}^{y}.\left(\frac{1}{(2x-1)2x}\right)+\frac{1}{2}$

$$\Sigma_{2y-1}^{2y}.\left(\frac{1}{x}\right)-\frac{1}{4}\left(\frac{1}{2y-1}-\frac{1}{2y}\right)+\frac{1}{8}\left(\frac{1}{(2y-1)^2}-\frac{1}{(2y)^2}\right)$$

$$-\frac{1}{16}\left(\frac{1}{(2y-1)^4}-\frac{1}{(2y)^4}\right)+\frac{95}{63.12}\left(\frac{1}{(2y-1)^6}-\frac{1}{(2y)^6}\right)-$$

&c., &c...(23)

If we now take $y = 6$ the above equation will become

$$\Sigma_{1}^{\infty}.\frac{1}{(2x-1)2x}=\Sigma_{1}^{6}.\left(\frac{1}{(2x-1)2x}\right)+\frac{1}{24}-\frac{1}{4}\left(\frac{1}{11}-\frac{1}{12}\right)+\frac{1}{8}\left(\frac{1}{11^2}-\frac{1}{12^2}\right)-\frac{1}{16}\left(\frac{1}{11^4}-\frac{1}{12^4}\right)+\ \&c.$$

$=.6532107+.0416666-.0018939+.0001650-.0000012=.6931472$ which is true to the last place of decimals.

Other series may be obtained from equation (23), even more convergent than the above, by taking y a larger number.

The foregoing general formula may be applied with advantage to determine the continued product of x factors, whose general factor is $\Phi(x)$.

Let us denote the product of x factors, as above described, between the limits of $x=y$ and $x=x$ by $\overset{x}{\underset{y}{[}}\Phi(x)]$, which is a function of x, say $F(x)$. Then, agreeably to this notation, we shall have

$$\overset{x}{\underset{1}{[}}\Phi(x)] = \Phi(1)\times\Phi(2)\times\Phi(3)\times\Phi(4)\times\ldots\ldots\ldots\ldots\Phi(x) = F(x) \ldots\ldots\ldots\ldots\ldots\ldots\ldots (24)$$

and

$$\overset{x+1}{\underset{1}{[}}\Phi(x)] = \Phi(1)\times\Phi(2)\times\Phi(3)\times\Phi(4)\times\ldots\ldots\ldots\Phi(x)\times\Phi(x+1) = F(x+1)\ldots\ldots\ldots\ldots\ldots\ldots\ldots(25)$$

Divide equation (25) by equation (24), and we shall have

$$\frac{F(x+1)}{F(x)} = \Phi(x+1) \ldots\ldots\ldots\ldots\ldots\ldots (26)$$

Hence, it appears, that in order to find the continued product of x factors, we shall have to determine a function of x, such, that its reciprocal when multiplied by its increment, shall be equal to a given function of x. The determination of this function will be, frequently, a matter of considerable difficulty; and consequently, can be accomplished, in finite terms, only in a few cases.

If we denote the direct operation of $\frac{F(x+1)}{F(x)}$ by $\overset{x}{F}(x)$, so that $\frac{F(r+1}{F(x)} = \overset{x}{F}(x) = f(x)$, and the inverse operation of $f(x)$ by $\overset{-x}{f}(x)$, so that $\overset{x-x}{F}(x) = F(x)$; a direct and inverse calculii of the functions denoted by these symbols of operation may be readily established.

The developement of these calculii, (which may, possibly, throw some light upon our present acquaintance with the doctrine of Algebraical equations,) I shall reserve till some future opportunity. Nor will the above idea respecting equations appear to be altogether visionary; when we consider that Algebraical equations are always formed by the continual product of factors.

If we put $\overset{x}{\underset{1}{[}}\,\phi(x)\,] = \phi(1)\times\phi(2)\times\phi(3)\times\phi(4)\,\ldots\ldots$

$$\phi(x) \qquad = a^{F_1(x)} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(27)$$

then $\overset{x+1}{\underset{1}{[\Phi(x)]}} = \phi(1) \times \phi(2) \times \phi(3) \times \phi(4) \ldots\ldots\ldots$

$$\phi(x) \times \phi(x+1) = a^{\mathrm{F}_1(x+1)} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (28)$$

and $a^{\mathrm{F}_1(x+1) - \mathrm{F}_1(x)} = \Phi(x+1)$. Take the logarithm of each side of this equation, and we shall have

$$\mathrm{F}_1(x+1) - \mathrm{F}_1(x) = \frac{\log.\ \Phi(x+1)}{\log.\ a}$$

Now if in equation (13) we put $h = o$ we shall have the following value of $\mathrm{F}_1(x)$ between the limits $x = y$ and $x = x$.

consequently

$$\overset{x}{\underset{y}{[\Phi(x)]}} = a^{\frac{1}{\log.\ a}\left\{\int_y^x .\ \log.\ \phi(x)\,dx + (\mathrm{A}+1)\left\{\log.\ \phi(x) - \log.\ \phi(y)\right\} + \mathrm{B}\left\{\log.\ '\phi(x) - \log.\ '\phi(y)\right\} + \mathrm{D}\left\{\log.\ '''\phi(x) - \log.\ '''\phi(y)\right\} + \&c.,\ \&c.\right\}}$$

Multiply both sides of this equation by $\overset{y}{[\Phi(x)]}$, and we shall have

$$\overset{x}{\underset{1}{[\Phi(x)]}} = \overset{y}{\underset{1}{[\Phi(x)]}} \times a^{\frac{1}{\log.\ a}\left\{\int_y^x .\ \log.\ \phi(x)dx + (\mathrm{A}+1)\left\{\log.\ \phi(x) - \log.\ \phi(y)\right\} + \mathrm{B}\left\{\log.\ '\phi(x) - \log.\ '\phi(y)\right\} + \mathrm{D}\left\{\log.\ '''\phi(x) - \log.\ '''\phi(y)\right\} + \&c.\right\}} \ldots\ldots\ldots \quad (29)$$

Integrate, by parts, and we shall have,

$$\int_y^x . \log. \phi(x)\,dx = x \log. \phi(x) - y \log. \phi(y) -$$

$$\int_y^x x\frac{\Phi'(x)}{\Phi(x)}.\,dx,$$ and, by differentiating, we shall have

$$\log. '\phi(x) = \frac{\Phi^{I}(x)}{\Phi(x)} \text{ and } \log. '\phi(y) = \frac{\Phi^{I}(y)}{\Phi(y)}$$

Put $\frac{\Phi^{I}(x)}{\Phi(x)} = \phi_1(x)$, $\therefore \frac{\Phi^{I}(y)}{\Phi(y)} = \phi_1(y)$. Substitute these values in equation (29) and it will become

$$\overset{x}{\underset{1}{[}}\Phi(x)] = \overset{y}{\underset{1}{[}}\Phi(x)] \times a^{\frac{1}{\log. a}\left\{ - \int_y^x x\,\phi_1(x)dx + (A+x+1)\right.}$$

$$\left.\int \phi_1(x)\,dx - (A+y+1)\int \phi_1(y)\,dy + B\left\{\phi_1(x) -\right.\right.$$

$$\left.\phi_1(y)\right\} + D\left\{\overset{II}{\phi_1}(x) - \overset{II}{\phi_1}(y)\right\} + F\left\{\overset{IV}{\phi_1}(x) - \overset{IV}{\phi_1}(y)\right\} + G$$

$$\left.\left\{\overset{VI}{\phi_1}(x) - \overset{VI}{\phi}(y)\right\} + \&c., \&c.\right\},$$ where a may be any quantity whatever. .... (30)

Since a may be taken equal to any quantity, let us make it successively equal to $\phi(x)$ and $\phi(y)$.

Then, because $\int \Phi_1(x)dx = \log.\ \Phi(x)$ and $\int \Phi_1(y)dy = \log.\ \Phi(y)$, we shall have, putting $A + 1 = \frac{1}{2}$

$$\overset{x}{\underset{1}{[}}\ \Phi(x)\ ] = \overset{y}{\underset{1}{[}}\ \Phi(x)\ ] \times \frac{\Phi(x)^{x+\frac{1}{2}}}{\Phi(y)^{y+\frac{1}{2}}} \times a^{\frac{1}{\log.\, a}\left\{ -\int_y^x .\ x\,\Phi_1(x)\,dx \right.}$$

$$\left. + \ B\left\{\Phi_1(x) - \Phi_1(y)\right\} + D\left\{\overset{\text{II}}{\Phi_1}(x) - \overset{\text{II}}{\Phi_1}(y)\right\} + F \left\{\overset{\text{IV}}{\Phi_1}(x) - \overset{\text{IV}}{\Phi_1}(y)\right\} + \&c., \&c \right\} \ldots\ldots\ldots\ldots\ldots (31)$$

By taking a equal to e, equation (31) will take the following remarkable form :

$$\overset{x}{\underset{1}{[}}\ \Phi(x)\ ] = \overset{y}{\underset{1}{[}}\ \Phi(x)\ ] \times e^{-\int_y^x .\ x\,\Phi_1(x)\,dx} \times \frac{\Phi(x)^{x+\frac{1}{2}}}{\Phi(y)^{y+\frac{1}{2}}} \times$$

$$\frac{\log.^{-1} .\Phi_1(x)^{B} \times \log.^{-1} .\overset{\text{II}}{\Phi_1}(x)^{D} \times \log.^{-1} .\overset{\text{IV}}{\Phi_1}(x)^{F} \times \&c.}{\log.^{-1} .\Phi_1(y)^{B} \times \log.^{-1} .\overset{\text{II}}{\Phi_1}(y)^{D} \times \log.^{-1} .\overset{\text{IV}}{\Phi_1}(y)^{F} \times \&c.} \ldots (32)$$

Ex. 1.—Required the continued product of x factors, whose general factor is x.

Here we have $\Phi(x) = x \therefore \Phi^{\text{I}}(x) = 1$, and $\frac{\Phi^{\text{I}}(x)}{\Phi(x)}$

$$= \Phi_1(x) = \frac{1}{x}. \quad \overset{\text{II}}{\Phi_1}(x) = \frac{2}{x^3}, \ \overset{\text{IV}}{\Phi_1}(x) = \frac{2.3.4}{x^5} \text{ \&c., \&c.}$$

Similarly $\Phi_1(y) = \frac{1}{y}$, $\overset{\text{II}}{\Phi_1}(y) = \frac{2}{y^3}$, $\overset{\text{IV}}{\Phi_1}(y) =$

$$\frac{2.3.4}{y^5} \text{ \&c , \&c.} \quad -\int_y^x x\,\Phi_1(x)\,dx = y - x.$$

Substitute these values in equation (31), and we shall have, when $a = e$,

$$\overset{x}{\underset{1}{[x]}} = 1.2.3\ldots x = \overset{y}{\underset{1}{[y]}} \times \frac{x^{x+\frac{1}{2}}}{y^{y+\frac{1}{2}}} \times e^{\,y - x + \frac{1}{12}\left(\frac{1}{x} - \frac{1}{y}\right)}$$

$$-\frac{1}{30.3.4}\left(\frac{1}{x^3} - \frac{1}{y^3}\right) + \frac{1}{42.5.6}\left(\frac{1}{x^5} - \frac{1}{y^5}\right) - \text{\&c.}\ldots(33)$$

This series, which is very convergent, is also very convenient to calculate. The greater we take y, the more rapid will the series converge. I would, however, here suggest, that y be uniformly taken equal to 10, as a convenient number for calculation.

ON THE

PRESSURE OF THE STEAM

WHICH

CAUSED THE EXPLOSION OF THE BOILER

OF THE

IRK, LOCOMOTIVE ENGINE.

BY THE REV. EDMUND SIBSON,
Minister of Ashton-in-Makerfield.

(Read April 15th, 1845.)

1. The boiler of the *Irk*, Locomotive Engine, exploded in the Shed, at Miles Platting, near Manchester, belonging to the Manchester and Leeds Railway Company, about six o'clock on the morning of Tuesday, the 30th day of January, 1845.

2. Mr. Fairbairn, of Manchester, Civil Engineer, in his report of the cause of this explosion, says, that the top of the fire-box was forced down, like a piece of paper, upon the front of the tubes, by

excessive pressure, arising probably, from the accumulated force of the steam having no outlet through the safety valves. And, he calculates, that, if the boiler was sound, the pressure of the steam could not be less than 387 pounds, on the square inch; and that it would require a pressure of at least 939 pounds, on the square inch, to project the engine, by one single impulse of the steam, to the height of 30 feet; the area of the aperture, in the roof of the fire-box being 1619 square inches, and the weight of the engine being $15\frac{1}{2}$ tons.

3. The body of the boiler was a cylinder, 8 feet $5\frac{1}{2}$ inches in length, and 3 feet 6 inches in diameter; and, supposing, that, at the time of the explosion, there was very little water on the top of the fire-box, and neglecting the thickness of the steam pipe, which passes through the steam in the boiler, then the height of the segment of the boiler filled with steam, would be about 1 foot 4 inches; and, therefore, in this part of the boiler, there were nearly 26 cubic feet of steam. The top of the fire-box was 3 feet 5 inches, by 3 feet 3 inches; and this was the base of the cupola of the boiler, the sides of which were 2 feet high, and the top of which was groined in a parabolic curve, the axis of which was 2 feet in height; and, therefore, in

this part of the boiler, immediately over the fire-box, there were nearly 29 cubic feet of steam. Hence, at the time of the explosion, there were 55 cubic feet of steam in the boiler.

4. The whole length of the boiler and fire-box was 12 feet.

5. At the time of the explosion, the Engine was coupled to the Tender, by a bolt passing through two strong plates of iron; and, by bending these plates, the Engine broke loose from the Tender. The Engine, on its motion forwards, broke six tie-beams, in the roof of the Shed, which were 15 feet above the level of the floor, on which the Engine stood; and the Engine was found immediately under the sixth tie-beam, in the next pit, on the south side of the Shed, with its wheels upwards, with the bottom of the boiler nearly vertical, with the fire-box end of the Engine downwards, and turned towards the opposite end of the Shed; so that the Engine, during the time of its motion, must have revolved through an angle of 180° at least. The length of each tie-beam was 39 feet 6 inches: its depth was 12 inches; and its breadth 6 inches. The Engine was thrown horizontally to the distance of 30 feet

from the place where it stood, at the time of the explosion. Hence, therefore, it appears, that, after breaking the last tie-beam, the force of the Engine was exhausted; and that the Engine fell perpendicularly to the ground. It cannot be known to what height the engine was thrown: because, the force which broke the tie-beams would bring down the roof of the shed.

6. The action of the steam, by which the Engine was moved, would be similar to that of the water in Barker's mill; and, therefore, the motive force of the steam, in a direction perpendicular to the top of the fire-box, will be expressed by the area of the aperture, in the roof of the fire-box, multiplied by the pressure of the steam on that area. The motive force of the steam will be diminished, as the density of the steam decreases; and the force of the steam will continue to act upon the Engine, until the pressure of the steam in the boiler becomes equal to the pressure of the atmosphere. As the motive force of the steam is not applied to the Engine, at its centre of gravity, the Engine will revolve about its centre of gravity, in the same manner as if the motive force of the steam was applied to make the Engine revolve about a fixed axis, passing through its centre of

gravity; and its centre of gravity will move in the same manner, as if the whole motive force of the steam was applied at that point. And, as the motive force of the steam always acts in a direction perpendicular to that part of the surface of the boiler which is opposite to the aperture; and as the surface of the boiler, on which the steam thus acts, revolves about the centre of gravity of the Engine, it is evident, that the direction, in which the motive force of the steam acts upon the centre of gravity of the Engine, is continually changing.

7. It is desirable to ascertain the force of the steam, which caused the explosion; but, the motion of the Engine having been so much disturbed by breaking loose from the Tender, and by breaking the six tie-beams, the calculations for this purpose must always be attended with some uncertainty. The only way, however, in which the force of the steam can be ascertained, with any degree of certainty, is to consider and estimate the several effects produced by the explosion, and then to calculate what must be the force of the steam necessary to cause the effects.

When the plane of the top of the fire-box is vertical, the motive force of the steam to raise the centre of gravity of the Engine, will then become nothing; and after the plane of the top of the fire-box, has revolved through more than a right angle, the motive force of the steam will press the centre of gravity of the Engine downwards. The Engine would, therefore, soon begin to descend; and we will, therefore, suppose that the vertical force, with which the Engine impinges upon the tie-beams, is so small, that it may be neglected. Hence, then, we may suppose, that all the tie-beams were broken by the horizontal impact of the Engine; and we may calculate, what must be the force of the steam, so that all its force may be expended in breaking loose from the Tender, and in breaking the six tie-beams.

As the fire-box end of the Engine will begin immediately to revolve about the centre of gravity of the Engine, and as it is supposed that there was very little water on the top of the fire-box, at the time of the explosion, there will not be any immediate discharge of water from the boiler into the fire-box; and the water in the boiler being very hot, and there being nothing to diminish the heat in the fire-box, steam will continue to be generated, until the position of the fire-box oecomes inverted, when the water in the boiler will be discharged immediately, at the top of the fire-box.

From these circumstances, of the motion of the Engine and the force of the steam, the action of the steam would not be instantaneous; for, if it had been instantaneous, the engine would have been thrown upwards; but, it could not have had any *horizontal motion.*

When the fire-box end of the Engine has revolved through 180°, the force of the steam will counteract the first horizontal motion of the Engine, and will impel it horizontally in an opposite direction; but, when the fire-box has revolved through 180°, the water is discharged from the boiler, and no more steam can be generated. The Engine will, therefore, have acquired its greatest rotatory motion when it has revolved through 180°; and as this rotatory motion would be destroyed by impinging on the tie-beams, we may suppose, that the Engine had acquired its greatest horizontal velocity, before it struck the first tie-beam.

8. A cubic foot of water weighs 1000 ounces avoirdupois; and, therefore, a cubic inch of water

weighs $\frac{7}{12}$ of an ounce nearly. But, one ounce of water generates a cubic foot of steam, the elastic force of which is equal to the pressure of the atmosphere; and, therefore, $\frac{7}{12}$ of an ounce is the density of steam, the pressure of which is equal to that of the atmosphere. And, the density of steam is proportional to its pressure.—(LARDNER, *on the Steam Engine*, p. 321.)

9. Given the cubic feet of steam, and its density in the boiler, and the area of the aperture, in the boiler, through which the steam is discharged, it is required to find, in what time the pressure of the steam in the boiler, will become equal to that of the atmosphere, it being supposed, that the density of the steam, in every part of the boiler, at any given time, is the same as that, in the boiler, at the orifice, and that no steam is generated during the time of its discharge, and that the steam moves, in the boiler, and issues at the orifice, in parallel plates.

See n = the cubic feet of steam, in the boiler.

D = the density of the steam, in the boiler, just before the discharge takes place.

δ = that of the steam, at the time t, in every part of the boiler:

The large aperture, through which the steam is rapidly discharged, allowing this supposition

$\Delta = \frac{7}{12}$ = that of steam, the pressure of which is equal to that of the atmosphere.

p = the pressure of the steam, in ounces, on one foot of the surface of a section of the steam, in the boiler, close to the orifice, at the time, t.

v = the velocity with which the steam is moving, in the boiler, close to the orifice, at the time, t.

$a = 11$ = the area of the orifice, in square feet.

$\Pi = 34560$ = the pressure of the atmosphere, in ounces, on a square foot.

$g = 32\frac{1}{6}$ feet.

Then, by Professor Moseley's Hydrostatics and Hydrodynamics, Art. 195, the general expression for the motion of an elastic fluid, is

$$\frac{gdp}{\delta} = d\,\text{P} \mp vdv$$

But, the action of gravity on the steam is so small, when compared with the force arising from the pressure of the steam, that P may be neglected; and when p is diminished, v is diminished also; therefore

$$\frac{gdp}{\delta} = vdv$$

But, because, by Art. 8, the pressure of the steam varies as its density,

$$\Delta : \Pi :: \delta : p, \ \& \ \delta = \frac{\Delta p}{\Pi}, \ \&$$

$$\frac{g\Pi}{\Delta} . \frac{dp}{p} = vdv.$$

By integration $\frac{2g\Pi}{\Delta} \log. p = v + c.$

But, when the pressure of the steam, in the boiler, becomes equal to that of the atmosphere, the steam will cease to issue from the boiler; and, therefore, when $p = \Pi$, $v = o$; and consequently

$$\frac{2g\Pi}{\Delta} . \log. \frac{p}{\Pi} = v^2.$$

$$\& \ v = \sqrt{\frac{2g\Pi}{\Delta}} . \left(\log. \frac{p}{\Pi}\right)^{\frac{1}{2}}$$

Therefore, $av\delta = a . \sqrt{\frac{2g\Pi}{\Delta}} . \left(\log. \frac{p}{\Pi}\right)^{\frac{1}{2}} \delta =$ the ounces of steam, which would be discharged, at the time, t, in one second, with the velocity v; and consequently, $a \sqrt{\frac{2g\Pi}{\Delta}} . \left(\log. \frac{p}{\Pi}\right)^{\frac{1}{2}} \delta \, dt =$ the ounces of steam discharged in the time dt.

But $n(D - \delta) =$ the ounces of steam discharged, in the time, t; hence, therefore,

$$a\sqrt{\frac{2g\Pi}{\Delta}}\left(\log.\frac{p}{\Pi}\right)^{\frac{1}{2}}\delta\,dt = nd\delta;$$

$$\& \frac{a}{n}\sqrt{\frac{2g\Pi}{\Delta}}.\ dt = \frac{-\,d\delta}{\left(\log.\frac{p}{\Pi}\right)^{\frac{1}{2}}\delta} = \frac{-\,d\delta}{\left(\log.\frac{\delta}{\Delta}\right)^{\frac{1}{2}}\delta}$$

Let $\log.\frac{\delta}{\Delta} = x^2$, then $\frac{\delta}{\Delta} = e^{x^2}$, hyp. log. $e = 1$; and let

$\frac{a}{n}\sqrt{\frac{2g\Pi}{\Delta}} = \mu$, and then, $\mu\,dt = -2\,dx$.

By Integration, $\mu t = -2x + c = -2\left(\log.\frac{\delta}{\Delta}\right)^{\frac{1}{2}} + c$
$= \log.\frac{\Delta}{\delta} + c$; but, when $\delta = \mathrm{D}$, $t = o$; therefore,

$$\mu t = \log.\frac{\mathrm{D}}{\delta}$$

$$\& \ e^{\mu t} = \frac{\mathrm{D}}{\delta}$$

$$\& \ \mu t = \frac{\text{com. log.}\left(\frac{\mathrm{D}}{\Delta}\right)}{\cdot 4342} \text{ nearly.}$$

10. When, as in the last article, the steam issues through a given aperture, in the top of the fire-box, and when, as in Barker's mill, the steam, by its re-action, presses upon that part of the top of the boiler, which is opposite to the aperture, and, by this means, causes the Locomotive Engine to revolve about its centre of gravity; it is required to find the angle, through which the Engine will

revolve, in a given time; the weight and dimensions of the Locomotive Engine being given, the pressure of the steam, on the boiler, at the beginning and end of the time, being known, and the pressure of the atmosphere on the top of the fire-box being neglected; and it being supposed that the Engine will continue to revolve about the same axis passing through its center of gravity.

Let $w = 15\frac{1}{2}$ tons $= 555520$ ounces $=$ the weight of the Locomotive Engine.

Let it be supposed, that the whole Locomotive Engine, consisting of boiler, wheels, &c., is a parallelopipedon, whose length is 12 feet, and depth 6 feet; then, the distance of the centre of gyration, from the centre of gravity of the Engine, is $\sqrt{15}$ feet; and if the pressure of the steam, from the aperture, on the top of the fire-box, be supposed to act upon the inside of the top of the boiler, at the mean distance of a foot from the end of the boiler, then $\frac{3w}{5}$ is the weight, which must be placed, at one foot from the end of the boiler, in the line which passes, longitudinally, through the centre of gravity of the Engine, so that, when this weight revolves about the centre of gravity of the Engine, its inertia may be the same as that of the Locomotive Engine.

Let $b = 5$ feet $=$ the perpendicular from the centre of gravity of the Locomotive Engine, upon the direction, in which the pressure of the steam acts upon the inner surface of the top of the boiler.

u= the velocity, with which the point, at the distance of one foot from the end of the boiler, revolves about an axis passing through the centre of gravity of the Engine, at the time, t.

θ= the angle, to radius unity, through which the Engine has revolved about the axis passing through its centre of gravity, in the time, t.

Then, retaining the notation in the last article.

$ap = \frac{a\Pi\delta}{\Delta}$ = the pressure on the inner surface of the top of the boiler; and $\frac{ab\Pi\delta}{\Delta}$ = the moving force of the steam, by which the Locomotive Engine is made to revolve about its centre of gravity; and, therefore, $\frac{5ab\Pi\delta}{3\Delta w}$ = the accelerating force.

Hence $\frac{5gab\Pi\delta}{3\Delta w} \times dt = du$; or, putting $\nu = \frac{5ga\Pi}{3\Delta w} = 62.5$ nearly.

$$b\nu\delta dt = du;$$

but, by Art. 9, $\delta = De^{-\mu t}$; and therefore,

$$b\nu De^{-\mu t}dt = du.$$

By Integration, $\frac{-b\nu De^{-\mu t}}{\mu} = u + c$; and, when $u = o$, $t = o$;

therefore $\frac{bD}{\mu}(1 - e^{-\mu t}) = u = \frac{bd\theta}{dt}$;

$$\text{and } d\theta = \frac{\nu D}{\mu}\left(dt + \frac{e^{-\mu t}}{\mu} \times -\mu dt\right)$$

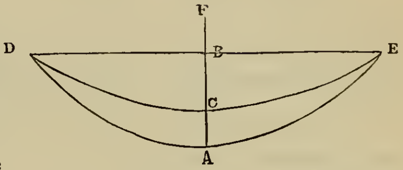
By Integration $\theta = \frac{\nu D}{\mu}\left(t + \frac{e^{-\mu t}}{\mu}\right) + c$; and, when $\theta = o$,

$t = o$; and therefore, $\theta = \frac{\nu D}{\mu}\left\{t + \frac{e^{-\mu t}}{\mu} - \frac{1}{\mu}\right\} = \frac{\nu D}{\mu}\left\{t - \frac{1}{\mu}\right\}$,

as an approximation; for, $\frac{1}{\mu e^{\mu t}}$ is much less than $\frac{1}{\mu}$.

11. A given weight impinges horizontally upon eight beams, of equal dimensions, and of given strength, and breaks them all in succession; it is required to find, with what velocity, the weight must impinge on the first beam, so that all its force may be exhausted, when it has broken the last beam; it being supposed, that the two ends of each beam are immovable, that each beam is perfectly elastic, and that the weight of each beam is so small, when compared with that of the impinging weight, that the weight of the beam may be neglected, and that the point of impact is at the middle of each beam.

Using the 21st Prob. in Sec. 3, of Emerson's Fluxions.



Let D B E be the position, before impact, of the beam, supported at the two ends, D and E.

Let B A be the space, through which the beam is bent, before it breaks; and, let the beam, from the position D B E, be put into the position D A E; and let a weight, c, be laid upon it, at A, which will just break it. Then the weight, w, impinging horizontally, in the direction F B, on the middle of the beam, at B, is to bend the beam through B A, to the point A, where it breaks.

$w=$the impinging weight in pounds avoirdupois.
$c=$ the weight, which suspended at A, will just break the beam.
$b=$ B A$=$the space through which the beam is bent, when it breaks.

Let D C E be the position of the beam, at the time, t.

$x=$B C.
$v=$the velocity with which the point, C, is moving at the time, t.
and let $u=$the required velocity with which w must impinge upon the first beam.

When the beam is bent into the position, D C E, it exerts a force, which is proportional to the distance B C; and, therefore, $b : c \therefore x : \frac{cx}{b}=$the moving force of the point C, in the direction B C A, in consequence of the impact given to the beam, by the weight, w, at B. And, it is evident, that as B C increases, the velocity of the point, C, decreases,

Therefore $\frac{cxdx}{b} = -\frac{wvdv}{g}$.

By Integration $\frac{cx^2}{b} = -\frac{wv^2}{g} + \text{C}$; and when $x=o$, $v=u$;

and, therefore, $\frac{cx^2}{b} = \frac{w}{g}(u^2 - v^2)$.

When $x=b$ $\overset{\text{I}}{v}{}^2 = u^2 - \frac{gcb}{w}$; where $\overset{\text{I}}{v}$ is the velocity, with which the weight, w, will depart from, A, in the direction, B A, after it has broken the first beam; and, therefore, $\overset{\text{I}}{v}$ is the velocity, with which the weight, w, will impinge upon the second beam.

In the same manner, then, it appears, that

$\overset{\text{II}}{v}{}^2 = \overset{\text{I}}{v}{}^2 - \frac{gcb}{w} = u^2 - \frac{2gcb}{w}$; where $\overset{\text{II}}{v}$ is the velocity, with which the weight, w, will depart from A, after it has broken the second beam.

And, in the same manner $\overset{\text{VIII}}{v}{}^2 = u^2 - \frac{8gcb}{w}$; but, it is supposed, that $\overset{\text{VIII}}{v} = o$; and, therefore, $u^2 = \frac{8gbc}{w}$.

But, it is necessary to find both b & c, before u can be known.

To find b, it is necessary to find the equation of the curve, D A E; and, to find c, we must find the weight, which, suspended at A, is just sufficient to break the given beam, D A E.

It is supposed, that the force, by which the Engine was disengaged from the tender, was equal to that which would break two beams.

12. To show in what manner the modulus of elasticity is limited by the modulus of rupture, in the Tables in Mr. Moseley's Engineering, &c.

E = the weight, in pounds, which would be required to extend a beam, whose section is one square inch, and whose length is one foot, to the length of two feet; and,

S = the weight, in pounds, which, at the section of rupture, is just equal to the force of extension of a beam, whose section is one square inch, and whose length is one foot.

In using the modulus of elasticity, E, it is supposed, that the beam, whose section is one square inch, may be extended to twice its length without breaking, and that, during the whole of this extension, its elasticity may remain perfect; but, this supposition is limited by the use of the modulus of rupture, S.

For, let λ = the extension, by the weight, S, of a beam, whose section is one square inch, and whose length is L feet; then, $E : L :: S : \lambda$; or,

$\frac{S}{E}=\frac{\lambda}{L}=\frac{\delta x^1}{\Delta x}=\frac{\rho^1}{R}$, adopting Mr. Moseley's Notation, in his Engineering, &c. Art. 357, and observing, that, in this case, δx^1 is the greatest extension, of which the fibre, whose length is Δx, is capable ; and, in that case also ρ^1 = half the depth of the beam.

13. Given the dimensions of a rectangular beam, which is supported horizontally, on two props, and which is bent by a given weight, suspended from the middle of the beam; it is required to find the equation of the curve, which passes through the *centre of pressure* of every part of the beam, it being supposed, that the two ends of the beam are immovable, that the beam is perfectly elastic, that the deflection of the beam, from the horizontal line, is always small, and that the strength of the beam is known.

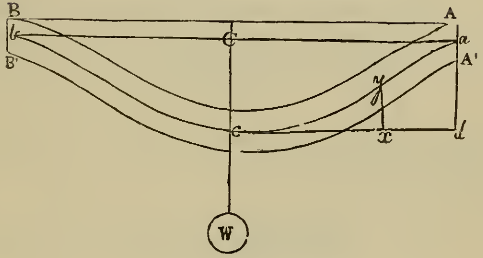
Using the Problem, in Article 371, of Mr. Moseley's Engineering, &c.

Let W = the given weight in pounds.

a = length of the beam, in feet.

b = its breadth, in feet.

c = its depth, in feet.



Let A B B¹ c A¹ represent any lamina of the beam, parallel to its plane of deflexion; and let acb be the line, which passes through the *centre of pressure* of every part of the beam.

Let aCb be the position of acb before the deflection of the beam.

Through c draw cxd parallel to bCa, and though x draw xy perpendicular to cd, meeting the curve $bcya$, in y.

Let c be the origin of the co-ordinates;

and let $x = cx$.

$y = xy$.

R = the radius of curvature of the curve *aycb*, at the point, *y*.

I = the moment of inertia of all the fibres of the beam, about an axis passing through the centre of pressure of the beam, at the point, *y*, perpendicular to the plane of the lamina A A¹ B¹ B.

E = the weight, in pounds, which would be required to extend a beam, whose section is one square inch, to double its length.

Then, substituting in equation (506) of Mr. Moseley's Engineering, &c.

$$\frac{12^2}{R} = \frac{W}{2EI}\left(\frac{a}{2} - x\right)$$

But, $\frac{1}{R} = \frac{dx\, d^2y}{(dx^2+dy^2)^{\frac{3}{2}}} = \frac{d^2y}{dx^2}$, nearly; because, the deflection of the beam being very small, dy^2 is very small when compared with dx^2; and, therefore, $\frac{12^2 d^2y}{dx} = \frac{W}{2EI}\left(\frac{a}{2}dx - xdx\right)$

By Integration $\frac{12^2 dy}{dx} = \frac{W}{4EI}(ax - x^2) + c$; but, it is evident, that *cxd* touches the curve, *cya*, at the point *c*; and, therefore, when $x = o$, the tangent of the angle $ycx = o$, and, consequently, $c = o$.

Again, by Integration, $12^2 y = \frac{W}{4EI}\left(\frac{ax^2}{2} - \frac{x^3}{3}\right)$, which needs no correction.

But, in the rectangular beam, $I=\frac{bc^3}{12}$; and the weight, W, being placed, at the middle of the beam, we have $x=\frac{a}{2}$; and, therefore, $Cc=ad=\frac{Wa^3}{(24)^2 E bc^3}$ (1)

Now, it is necessary to find the value of W, such that, if it be suspended gently from the point c, the beam will just break.

At the point, c, $x=o$; and, therefore, $\frac{1}{R}=\frac{Wa}{(24)^2 E I}$; and, in Art. 12, $\frac{S}{E}=\frac{\rho^1}{R}=\frac{3Wa}{(12)^2 E bc^2}$;

and, $W = 2\frac{(12)^2 Sbc^2}{3a}$, which is the value of c, in Art. 11.

And by substituting for W, in equation (1), we have

$$Cc=\frac{Sa^2}{6\,Ec},$$ which is the value of b, in Art. 11.

Therefore, $u^2=\frac{8gbc}{w}=\frac{4\times 32 g S^2 abc}{Ew}$

$$\& \ u=64\,S\sqrt{\frac{abc}{Ew}} \text{ nearly.}$$

Supposing the tie-beams to be made of Riga fir; and taking the values of E & S, from the tables at the end of Mr. Moseley's Engineering, &c.

$$u=64\times 6648\sqrt{\frac{40}{2\times 1328800\times 34720}}=8.86 \text{ nearly.}$$

14. When the motive force of the steam acts upon the top of the boiler, as in Art. 10, it is required to find the horizontal velocity of the centre of gravity of the Locomotive Engine, at a given time, the pressure of the atmosphere on the top of the boiler being neglected.

Let u=the horizontal velocity of the centre of gravity of the Locomotive Engine, at the time, t.

$$\varpi=3.14159, \&c.$$

Then retaining the notation, in Art. 10.

$$\frac{gap\times sin\theta}{w}dt=du$$

But, $p=\frac{\Pi\delta}{\Delta}$ (by Art. 9) $=\frac{\Pi D}{\Delta}e^{-\mu t}$ (by Art. 9);

$$\& \ \theta=\frac{\nu D}{\mu}\left(t-\frac{1}{\mu}\right) \text{ (by Art. 10);}$$

$$\& \ sin\ \theta=\frac{e^{\theta\sqrt{-1}}-e^{-\theta\sqrt{-1}}}{2\sqrt{-1}}$$

Therefore, by substitution, $\frac{2\sqrt{-1}\times\Delta w}{\Pi Dag}\times du =$

$$e^{\left(\frac{\nu D}{\mu}\sqrt{-1}-\mu\right)t-\frac{\nu D}{\mu^2}\sqrt{-1}}\times dt - e^{-\left(\frac{\nu D}{\mu}\sqrt{-1}+\mu\right)t+\frac{\nu D}{\mu^2}\sqrt{-1}}\times dt$$

By Integration, $\frac{2\sqrt{-1}\times\Delta w}{\Pi Dag}\times u=$

$$=e^{-\mu t}\left\{\frac{e^{\left(\frac{\nu Dt}{\mu}-\frac{\nu D}{\mu^2}\right)\sqrt{-1}}}{\frac{\nu D}{\mu}\sqrt{-1}-\mu}+\frac{e^{-\left(\frac{\nu Dt}{\mu}-\frac{\nu D}{\mu^2}\right)\sqrt{-1}}}{\frac{\nu D}{\mu}\sqrt{-1}+\mu}\right\}+c;$$

$$\text{or} -\frac{\Delta wu}{\Pi Dag\mu^2}\times(\nu^2D^2+\mu^4)=e^{-\mu t}\left\{\frac{\nu D}{\mu}cos\theta+\mu sin\theta\right\}+c$$

But, when $u=o$, $\theta=o$, & $t=o$; &, therefore, $c=-\frac{\nu D}{\mu}$; &

$$\frac{\Delta wu}{\Pi Dag\mu}\times(\nu^2D^2+\mu^4)=\nu D-\mu e^{-\mu t}\left(\frac{\nu D}{\mu}cos\theta+\mu sin\theta\right)$$

By Art. 7. $\theta=\varpi$. As also, by Art. 7, steam will continue to be generated, till the position of the fire-box becomes inverted; and, as during this time, from the escape of steam, at the fire-box, there will be a diminution of temperature, it is evident, that the steam, thus generated, will decrease in density; and it may, therefore, be supposed, with very little error, that the space occupied by the steam, in the boiler, at the time of the explosion, was sufficiently large to contain all the cubic feet of steam, generated between the time of the explosion, and the final discharge of the water, at the top of the fire-box; and that the accumulation of all these cubic feet of steam had the same density, before the explosion, as that of the steam, which caused the explosion; for, on this supposition, the density of the steam will decrease continually, as it issues from the aperture; and, that decreasing density of the steam will correspond nearly with the generation of steam of decreasing density, during the time that the Engine is in motion. On this supposition, the space occupied by the steam, in the boiler, at the time of the explosion, becomes an unknown quantity.

With these assumptions, the last Theorem may be resumed; and we have, from Art. 9,

$n=\frac{a}{\mu}\sqrt{\frac{2g\Pi}{\Delta}}=\frac{21450}{\mu}$, where n becomes an unknown, but determinate, constant quantity.

$$\frac{\Delta wu}{\Pi ag\nu}(\nu^2 D^2+\mu^4)=\mu D^2\left(1+\frac{\Delta}{D}\right)$$

But, by Art. 10. $\mu t=\frac{\mu^2\varpi}{\nu D}+1=\log.\frac{D}{\Delta}$, by Art. 9;

from which $\mu^2 = \frac{\nu D}{\varpi}\left\{\frac{\text{com. log. }\frac{D}{\Delta}}{.4342} - 1\right\}$

By substitution, we have

$$3940 + \frac{\mu^4}{D^2} = \frac{\mu\left(1+\frac{\Delta}{D}\right)}{.00375} \text{ nearly;}$$

$$\& \frac{\mu^2}{D} = 20\left\{\frac{\text{com. log. }\frac{D}{\Delta}}{.4342} - 1\right\} \text{ nearly.}$$

From these equations it is found, that $\frac{D}{\Delta} = 16.34$ nearly; and, that the pressure of the steam, in the boiler, at the time of the explosion was nearly 245 pounds on the square inch, the pressure of the atmosphere being neglected.

If, in Art. 10, the pressure of the atmosphere, and the term $\frac{e^{-\mu t}}{\mu}$, had not been neglected, the value of θ, in Art. 14, would have been

$$\frac{\nu D}{\mu}\left\{t - \frac{1}{\mu} + \frac{e^{-\mu t}}{\mu} - \frac{\mu\Delta t^2}{2D}\right\},$$ by which, the operation would have been rendered very laborious.

Owing to the many assumptions, which have been made, it is evident, that the result of this investigation can give only an *approximate value of the pressure of the steam*, in the boiler, which, when the pressure of the atmosphere is included, will be nearly 260 pounds on the square inch. When the term $\frac{e^{-\mu t}}{\mu}$ was neglected, it was supposed, that $e^{-\mu t}$ was very small, when compared with unity; and, it is found, that the supposition may be made, for $e^{-\mu t}=.061$, nearly.

It was stated, at the time of the inquest, that the *Irk* had been at work four years, and had run 76860 miles. That there were two safety valves; one at the command of the engine-man, and the other placed beyond his control. On examination, after the accident, the former was found in working order; but, the spring-box was broken: the other was fast; but, this might have been the result of a blow.

It was stated by Mr. Joseph Clark, of No. 5, Elm Street, fitter, and repairer of Locomotives, that he had examined the rent in the fire-box; and, that it appeared as if there had been a flaw in the metal.

Mr. Charles Frederick Beyer, also said, that he believed the copper must have been cracked.

It is probable, not only that the top of the fire-box was cracked, but, that there was very little, or no water, upon it; and, that, therefore, it would be red-hot, when, from the peculiar nature of copper and brass, the copper plate *would bend easily, and would also become very brittle.*

Mr. Alfred S. Tayler, Lecturer on chemistry, in Guy's Hospital, says, in his letter to Mr. Chapman, that Engineers do not appear to be sufficiently aware, that the strength of copper is diminished by raising the temperature, in a much greater proportion than the strength of iron. The fact is, that Engineers do not make the copper plate of the fire-box, half thick enough. And, he adds, that, besides this, the *square form* of the fire-box is bad.

AN ACCOUNT

OF A

ROMAN PUBLIC WAY

FROM

Manchester to Wigan.

BY THE REV. EDMUND SIBSON,

Minister of Ashton-in-Makerfield.

CORRESPONDING MEMBER OF THE SOCIETY.

(Read April 15th, 1845.)

On a Roman military way, the soldiers marched from one station to another: on a Roman public way, baggage, and articles of commerce were conveyed.

The military way was seven or eight yards broad, and was nearly in a straight line: the public way was twelve or fourteen yards broad; and, for the convenience of carriage, this road often deviates from the straight line, that it may go on hard and level ground. And it is found


WIGAN
Church
Old Hall
Hindley
ROMAN
ROAD
Rotten
Row

[illegible] Map [illegible] of a Roman Road between Wigan and Manchester

Scale of one inch to a Mile

NOTE

Where the road is marked by a strong black line the remains are evident
Where it is marked by a dotted line the remains are not so distinct

that, where there has been a Roman public way between two towns, the line of this way is generally both shorter and leveller than the road now in use.

As most of the Roman roads have been made nearly eighteen hundred years ago, it is not to be expected, that, in a populous and cultivated district, many traces of such roads should be left. But, it often happens, that, where the traces of the road are nearly obliterated, the names of fields and places still indicate the line of the road.

Because the Roman Road was nearly straight, it is indicated by Street Gate, Street Fold, and Stretton; and, in Scotland, by Strath. Because the Roman Road was hard and dry, it had the appearance of a white track, when it passed over a brown heath, or over a rushy valley; and it is, therefore, indicated by Whitfield, Whitworth, Whitley, White Pits, and Wheat Shaw. Highgate, or He-gate, is the raised road; and it indicates the Roman Road. The Saxon word, Waeg, is way; and, therefore, the Roman Road is indicated by Stanwix; which is Stan-waegs, by Wigan, which is Waeg-on; and by Wigton, which is Waegton. As the Romans had a number of forts on or near the road; so the road is indicated by Caer-common, which is the fort on the common; by Amberswood, which is the fort in the wood, near the water; by Ellenbrough, which is the stone fort near the water; and in Scotland by Borrens. In the same manner, Walton is Vall-ton, or Village-fort.

It often happens, that, in old grassy lanes, on barren heaths, and in corners of fields which have escaped cultivation, very perfect remains of the Roman Road may still be found. Of this there is a remarkable instance, in the Park at Haydock Lodge, where the Roman Road seems, even now, as perfect as when it was first made. It is thus described by the Rev. Mr. Whitaker, in his History of Manchester, book 1, chap. v., sec. 4, page 210, second edition:—"Just beyond the village "of Ashton, and close to the Hall of Haydock, on "a slight deviation of the present road, to the "right, it very plainly appears. Entering the "Paddock, at a large Ash, it continues along it "about six hundred yards; and then regains the "rectified line of the present road. Running "about three hundred, along the edge of the "Paddock, it crosses the back avenue to the "house, and is levelled to admit the plain of it.

" And, about as many yards of it are very perfect; "and a hundred and fifty, in the middle, as com- "plete almost, as they were originally. For this "space, it is very fairly rounded; and has a sharp "slope of nine or ten yards, on either side, from "the crown to the borders." The Roman Road is here eighteen yards broad, and a yard in thickness: it is formed of one foot of earth, on which there is a foot of blocks of freestone; and this is covered with a foot of gravel.

In most places, the freestone and the gravel have been carted away, and the road has been levelled; but, even then, when the ground is turned up by the spade or the plough, the road is indicated by pebbles of brook gravel, and by the loose and peppery nature of the soil. Sometimes. the line of the Roman Road is made arable by a covering of earth; and, in this case, the road is indicated by a belt of earth, about thirty yards in breadth, in the middle of which the road is found, by the spade, to be quite perfect.

To avoid a steep ascent, the Roman Road generally cuts into the slope of a rising ground; and, by this means, it often happens, that on a slope, the Roman Road is found at the depth of four or five feet below the present surface. And, therefore, in crossing the steep declivity to a brook, it often happens, that the hollow track of the Roman Road is still visible.

Mr. Whitaker placed Coccium at Blackrode; and, it was generally thought, that the Roman Road went from Haydock Lodge to Blackrode, in the direction of Strangeways, Lord Street, and Castle Hill, near Hindley. But in Mr. Whitaker's History of Manchester, Book I., Chap. 4, Sec. 3, Note 9, we find, that "Mr. Percival imagines an Iter to be lost, in Antonine, and Richard, that went from Kinderton to five camps, at Warrington, Wigan, Penwortham, Garstang, and Lancaster." In 1831, it was proved, as Mr. Percival had supposed, that the Roman Road found at Haydock Lodge was part of the great Road, passing through Warrington and Wigan, and going up to Standish; and, from thence nearly in the line of the present public road to Walton, near Preston. This being proved, it seemed probable that there would be a Roman road from Wigan to Manchester; and, it was soon found, that there was a Roman road from Wigan, passing over Amberswood Common, Caer Common, Moseley Common, and by Hope Hall to Manchester. An account of these roads, meeting at Wigan, is given in Mr. Baines's History of the County of Lancaster, Division 10, under the head of Wigan Parish. The tracing of the Road from Manchester to Wigan, being not satisfactory for the purposes of the survey of this county by the Board of Ordnance, the tracing of this Road was resumed in 1844.

The road from Manchester to Wigan, appears to be a public way, because it is generally about twelve yards broad: because it often deviates from the straight line, that it may run along hard and level ground; and because, in Worsley Park, at Gadbury Fold, and at a few other places, where the Road has been preserved, it is found to be closely paved.

Very few traces of this Road are now left. A great part of it passes through populous manufacturing districts, and through fields in a high state of cultivation. It is, indeed, remarkable how entirely this Road has disappeared in the last seventy-two years, in the neighbourhood of Hope Hall and Worsley, where it was found by Mr. Whitaker, and minutely described by him in 1773, in his History of Manchester.

Very distinct traces of this Road still remain in Worsley Park, on Moseley Common, near Cleworth Hall, on Little Shakerley Common, at Gadbury Fold, at Caer Common, Lord Street, and on Amberswood Common.

A straight line drawn from the Roman Road at Hope Hall, to the Roman Station at the Castle Field, in Manchester, would cross the Irwell nearly at Woden's Ford. In Mr. Baines's history of this county, division 5, page 165, we find, from Mr. Barritt's collections, that it is said in an old writing, that " Woden's Ford is a paved " causeway across the river Irwell, from Hulme " Field, where the Medlock loses itself in the " aforesaid river, to the opposite bank; but now " lost to the observer, since the Irwell was made " navigable." There is little doubt, that this was the ford across the Irwell, on the Roman Road, from Manchester to Wigan; and, that this Road and Ford were adopted by the Saxons, and, called Woden's Ford.

Instead, however, of taking the Road directly by Woden's Ford, from Mancunium to Coccium, Mr. Whitaker takes it a mile on the road to Kinderton, to the Ford of the Irwell, at Old

Trafford; and, he then brings it back over the Water Meadows, from Old Trafford to Hodge Lane. In the History of Manchester, book I, chap. 4, sec. 3, he says, "The road to Coccium, "or Blackrode, actually began with the way to "Kinderton, and proceeded with it for more than "a mile. Taking the same course to Throstle "Nest, it there turned away to the right, and "forded the Irwell at the shallow, which gave "denomination to Old Trafford. And, having "passed the channel of the river, it then took "its proper direction, and first pointed towards "the Station at Blackrode. It ranged over the "Eyes, or Water Meadows, mounted the little "heights, crossed the high road to Warrington, "and joined the present plain and continued "remains near Hope Hall. But in all this "course, from the river to the Hall, the Road is "wholly invisible." On looking at this line of road, there never seemed to be any sufficient reason for supposing, that the Roman Road should go round by Old Trafford, instead of going directly across the Irwell, at Woden's Ford. But, there appeared to be great difficulty in proving, or disproving, this opinion, on account of the numerous buildings, and gardens, and the high state of cultivation of the ground near

Manchester. And, had it not been from a mere accident, it is probable, that the true course of this Roman Road might have remained undiscovered for a much longer period.

Meeting Mr. Henry Still, of the Ordnance Survey, and Mr. Peter Clare, of Manchester, when men were employed by Lord Francis Egerton in exploring the Roman Station, at Castle Field, Mr. Clare was asked if he knew of any intelligent person, who had turned his attention to the Old Roads in that neighbourhood? Mr. Clare immediately named Mr. Thomas Groome, of Regent Road, in Salford; and said, that Mr. Groome had lived in the neighbourhood more than fifty years, and that he was well acquainted with all the Old Roads. On going to Mr. Groome he immediately showed us the track of the Roman Road, running in a straight line for nearly two miles, and pointing directly across Woden's Ford to the Castle Field. This line did not point exactly either at Hope Hall, or at the Roman Road, through Barton, to Warrington; but, it took an intermediate direction nearly to Gorton Bridge, which crosses the Liverpool and Manchester Railway; and, then this main trunk forked off into two branches, one pointing to

Hope Hall; and, the other running nearly parallel to the Manchester and Liverpool Railway, and pointing past Eccles to Barton.

The annexed trace of the Roman Road, from the Ordnance map, was given by Mr. Henry Still.

The Roman Road, as pointed out by Mr. Groome, was seen very distinctly, with its broad ridge of gravel and stones, on the south side of Regent Road, in the first field on the west side of Ordsall Farm. These remains of the Roman Road were found here by Mr. Groome fifty years ago.

The road was found again, by the gravel, at the west end of Hodge-lane, near the brook. And, this does not differ much from the line of the road given in Mr. Whitaker's History of Manchester, book 1, chap. v., sec. 4, page 210, where he says, that the road to Warrington "issued out of the "road from Manchester to Blackrode, about the "termination of Hodge-lane."

On the north side of the Liverpool and Manchester Railway, close to Gorton Bridge, at the ditch, on the west side of the foot-road, the Roman Road, from Manchester, divides into two branches, as it has been already stated. The branch to Wigan points past Broom House to Hope Hall.

The Roman Road is found again, in the ditch, on the north-west side of the lane, leading from Foster's Wood to the land of Noah; and there is here a large quantity of gravel.

The Road is found again in a grassy lane, leading out of Weast Lane, nearly opposite to Weast Lodge, where the gravel and ridge of the Road are very visible.

By taking this slightly curved direction, the Road skirts along the edge of Hart Hill; and thus keeps nearly on level ground.

In the Lower Broad Hope, which is the second field, on the west side of Old Hope Hall, the Roman Road was found, in a drain, in 1843, by Mr. John Goodwin, the farmer of that estate. This agrees with Mr. Whitaker's account, who says: "It is next discovered in the field which is "beyond the Old Hope Hall, and denominated "the Upper Broad Hope, by the gravel below

" the surface ; and, in the next, or Lower Broad " Hope, by the long ridge above it. The seam " of the gravel is a proof of the road, as the " ground is all naturally clay." Hist. Man., Book I., chap. 4, sec. iii., page 155. The ridge of the Road, in the Lower Broad Hope, has disappeared.

The Roman Road crosses the Gildas Brook, on the northwest side of Brook House Field.

Mr. Whitaker says, that " The Road here " leaving the Hope Hall Estate, and entering the " Heath Fields, the gravel is easily discovered by " the spade, in the first of them, as it crosses the " corner of it. And, in the second, the ridge " appears again, but much greater, and very large, " extending ten or twelve yards in width, and " having a fall on either side. Passing through " an angle of the third, the elevation continues " still evident, but reduced, in the fourth and " fifth : rises to a very considerable height, in " the sixth ; and retains it in Heath Lane, and " the field beyond it. In the last, it appears " equally green and dry, for twelve or thirteen " yards in width, and is skirted by a border of " rushes, in the wet ground of either side. But, "at the extremity of this, in the next, or Toad "Hill Field, and in that beyond both, it entirely "disappears; and the Road is found only by the "spade in the hither end of the second, and at "the further of the third inclosure, and by the "gravel, which it discovers lying upon the natural "soil. And, here the ridge seems to have been "taken away by the farmers, and the materials "have been dispersed with an equal hand over "a part of the sloping ground on the right."—Hist. Man., Book I., chap. 4, sec. iii., pages 155 and 156. Of the Roman Road, which was then so perfect, near Heath Lane, there are now very few traces remaining.

In the next field to Heath Lane, on the Hope Hall side, the Road is still found, below the surface, by the large quantity of gravel, which is turned up by the plough; and, there is no gravel in this field, except on the line of the Roman Road.

In the old grassy lane, called Heath Lane, the Roman Road is easily seen by the broad ridge of earth and gravel, which crosses the lane.

In the Toad Hill Field, which is the second field from Heath Lane, on the Chorlton Fold side, the Roman Road is found, about six inches below the surface, by the gravel, in the drains; and it points exactly upon Chorlton Fold. The Road is not indicated by any ridge, in this field.

It is found again, by the gravel, in the Calf Hey, near Chorlton Fold.

Returning once again to Mr. Whitaker's History of Manchester, we find, that the Road "enters the Westwood fields, and appears again with "a small ridge, ascending the slope of the second "enclosure, and pointing to a large oak upon the "furzy summit of it. And, at that tree, and in the "adjoining field, the gravel is very evident, lying "thick upon the road, and spreading several yards "in width. It crosses the second close of Mr. "Bailey in a plain ridge, and the first of Mr. "Watson in a plainer. Losing its elevation at "this end of Mr. Watson's second field, it reco-"vers it with an addition at the other. But, in the "next, or Mr. Blomiley's, the gravel appears all "along the three closes, lying in a tall ridge of "ten or twelve yards in width. And, in the "adjoining grounds of Brookside Estate, the "Road still appears, though less conspicuous, but "retaining an evident elevation, and pointing im-"mediately by Drywood House to Shaving-lane, "or Shaving-street, about half a mile beyond it.

"The name is preserved by a direct and open "lane, for a mile together, and lost only in that "of Stany-street, a little on this side of Walk-"den Moor. There the Roman becomes the "present way, and passes in a right line by "Street-gate towards the village of Blackrode. "And, here it was discovered, about sixteen "years ago, parallel with the present road, and "at a little distance from it; lying a foot below "the surface, covered with a strong crop of "furzes and briers, and three yards in breadth, "and eight or nine in length."—Hist. Man., Book I, chap. 4, sec. 3, pages 156 and 157. Almost every trace of this road has disappeared in the Westwood fields.

In Elias Chadwick's second field, which was formerly Mr. Watson's second field, it was found by the gravel in the ditch, on the west side of the field, by Mr. James Richardson, of Ashton-in-Makerfield.

The Road is found again by the gravel and boulders, in the corner of one of Mr. Blomiley's fields, at the junction of the Synsley Brook and the Brookside Brook.

And the ridge of the Road is very visible in the next field, called the Clough Croft, pointing to a large ash tree, near the gate, where it was found by one of Lord Francis Egerton's gamekeepers, when he was digging for rabbits.

This line of road does not point, by Drywood House, to Blackrode; but, it continues nearly in the direction of the straight line over the Heath fields, and through Chorlton Fold, and points directly towards Wigan. The Road in Mr. Blomiley's fields was ten or twelve yards wide; but, the road found at Blackrode was only three yards wide. The road to Wigan was, evidently, to an important station, which, most probably, was Coccium; but, the road to Blackrode was only a Vicinal road to the small Fort, in Castle Croft.

The Roman Road is found again by the gravel and stones in the bank of the cutting for the Tram road, on the southwest side of Drywood.

The Road continues in the same straight line, and is found again near the school in Crossfield. It is here a little below the surface, and is closely paved.

In Big Lady Hill Field, on the west side of the Old Hall, at Worsley, the Road was found by Lord Francis Egerton's workmen, when they were making a drain.

The Road is found again, in Bushy Hill Field, on the north side of Randswood,* crossing a line of old oak trees, which have stood in a hedge-row. The cop and the hedge have been lately taken away; but, they had been set, as old hedges generally were, upon the Roman Road; and they had preserved it both from the spade and the plough. The Road is here quite perfect: it is closely paved with large stones; and it has a smooth, flat surface.

On the Map made under the direction of the Board of Ordnance, a straight line was drawn through the Roman Road, in the Heath Fields, and this line was found to pass near the junction of two foot roads, in the Randswood field; and, it is remarkable, that the Roman Road was found

* Rand is edge or boundary.

again, exactly where it was expected to be found, in the ditch near the junction of the foot-roads, on the west side of Randswood Field. The distance of Randswood from Chorlton Fold is nearly three miles; and this close agreement of the Roman Road, with the straight line drawn on the Map, proves the great accuracy of the Ordnance survey.

The Road is found again in an old grassy lane, called Cooke's Lane, on the east side of the brook. The road is here paved with large stones, and is very perfect.

It is found again, by the white Brook-gravel, in the ditch in the Big Woodleys.

The Roman Road appears to have passed through the Wacker's Wood,* and over the little brook, Ellen,† exactly where the declivity of the bank is sloped away, on the line of the present Road through the Wood.

The Roman Road is found again, on Moseley

* Waeker's Wood is probably Waeg-ers Wood, that is, the Great Way through the Wood.

† Ellenbrook, is probably Ellenbrough, that is Al-an-brough, Stone fort on the brook.

Common, not far from the Old Brick House. It was found here in 1831. A short length of the ridge of the Road was then visible; and the grass, on this ridge, was of a pale green. In cutting through this ridge, there was found a foot of yellow sandy gravel: below the gravel was found a bed of blocks of yellow-coloured freestone, six inches in thickness; and below this was the natural black Peat Moss of the Common, from which the Common probably takes its name. As soon as the Peat Moss was cut into, the water boiled up from the bottom; and rose to the level of the Road. The length of Roman Road now visible on Moseley Common, is only about sixty yards: its ridge is nearly levelled away; but, when the turf is cut away, the track of the Road is found to be sprinkled with white gravel.

The track of the Roman Road appears in the hollow slope, by which it has descended into Sheep Lane. And, this shows that Sheep Lane is older than the Roman Road.

The Road is found again, by its ridge and gravel, and pale green grass, in the corner of the

Further Wood Field, near Sheep Lane, on the north side of Sheep Brook.

The Road is found again, in an old lane, called Turncroft Lane, on the east side of Parr Brow. The ridge of the Roman Road crosses the Turncroft Lane; and, the line of the Roman Road is closely paved.

The gravel and stones of the Roman Road, are very visible in the Rough Field, which is part of Cleworth Hall Estate. The name of this field shows, that it had remained, for a long time, in a rough and uncultivated state; and, by this means, the Roman Road, in this field, has partially escaped the ravages of improvement. The line of the Roman Road, from Moseley Common, has been gradually leaving the direction towards Wigan, and inclining northward, that it might avoid the hill on which Cleworth Hall and Tyldesley stand; but, at the Rough Field, the Roman Road returns to the direction of Wigan. From the Rough Field, the Roman Road runs directly across the Cleworth Hall Estate. And, here the Road seems to have been covered with a thick and broad coat of Blue Shale; which has now become a belt of Blue Clay, about a foot in thickness, and twelve yards in breadth. The Farmer at Cleworth Hall says, that, when he has a crop of wheat or clover, he can easily discover the line of the Roman Road; for, on that line, the wheat and the clover grow higher and stronger. This shows, that Blue Shale is a very valuable manure; for, it has remained here nearly eighteen hundred years, and its efficacy is not yet exhausted.

The Road is found again in a field near New Brick Barn.

The Road is still continued in the same straight line; and, it is very visible, where it crosses the narrow slip of Little Shakerley Common. A short length of the ridge and materials of the Road, is here very perfect.

It appears to have gone down into Shakerley Clough, to the Tyldesley Brook, at the point, where the steep declivity is sloped away by a hollow track.

It was found by Charles Gerard, when he lived at Atherton Lodge, in the field called Big Ely; and, it is still visible, by the gravel, at the north-west corner of this field.

It was found, by Mr. James Richardson, of Ashton-in-Makerfield, at the place where it crosses Chanters' Brook, in the next field beyond the Big Ely, on the north-east side of the willow plantation. The Road appears to have descended to the level of the Brook ; and it is indicated by a layer of large stones, of the breadth of the Roman Road, imbedded in the clay, on both sides of the brook. In the same manner, the Roman Road crosses the Synsley Brook, and the Irwell at Woden's Ford. And thus it appears, that, where a Public Way crossed a river, the Romans secured their road from quicksands, by making a paved Causeway across the bed of the river.

The Road is found again in the field, behind Aldred's Cottage, on the east side of Miller's Lane. The field, near the lane, is strewed with the gravel and stones of the Road.

It is found, by the gravel, in the next field, on the west side of Miller's Lane.

It is found, by the gravel and stones, in the field, on the east side of Atherton Parsonage, near the south fence.

The ridge of the Road is very visible, in Beefold Lane, at the Gate, on the northwest side of Atherton Parsonage.

The ridge of the Road is very visible in the curve of the hedge and cop of a field, called Ten Acres, a little on the south side of Guest Fold, near an old Oak Tree, and close to a Water Pit. The cop of the hedge has been set upon the Road; and under the cop the Road is quite perfect.

The Road is found again by the gravel and stones, in the ditch, on the south side of the Turnpike Road, not far from Bridge's Farm.

The remains of the Roman Road are very remarkable, at Hatton Fold. It has gone down to the brook, where the declivity of the hill is sloped away, on the line of the present Road. Here the broad paved Causeway, across the bed of the brook, still remains. And here the ridge and stones of the Roman Road are very visible, near the garden hedge, which is set across that Road.

The ridge and strong pavement of the Roman Road, are very visible, in an old grassy lane, called Cow Lane, opposite to the middle of the orchard, on the east side of Gadbury Fold. It may be remarked, that the line of the Road is indicated by Gadbury, which is Good-fort.

The Road appears to cross the Bolton Railway, near the Whistle Post, at Gadbury Fold.

The Road has deviated a little from the straight line, that it might go up to the Fort, on the high ground, at Gadbury Fold; and, having passed Gadbury Fold, it returns again to the direct line to Wigan.

The gravel of the road is again found, near a Gate, in the hedge of the second field, beyond Gadbury Fold.

The Road appears to cross Lover's Lane, a little below the orchard, at Four Lane-ends, in Atherton.

The Road is again visible where it crosses an old lane, called Small Brook Lane, a little below the New Barn.

The line of the Roman Road is again discovered by the stones and gravel in the bank, in the Brook Meadow, where the Road has sloped down to the Brook, near Dangerous Corner ; and, the hollow track of the Road is very visible, where it has sloped up the hill, from the brook to the public house, at Dangerous Corner.

The Roman Road crosses the present Highway at Dangerous Corner; and the gravel and stones of the Road are very visible, near the hedge, on the north side of the Highway.

Old Thomas Bushel, of Moss House, near Amberswood Common, was told, in 1796, by Anthony Hodgkinson, who was then seventy years of age, and lived at Caer Common, that the gravel and stones of the Roman Road were very visible, in his time, in the southwest corner of the field, where the Road to Caer Common branches off from the present Highway ; and, old Anthony Hodgkinson said, that this old road went over Amberswood Common to Wigan; and that, in old times, it was the High-gate Road from Manchester to Wigan and Preston. The name of Caer Common shows, that there has been a Fort here, on the line of the Roman Road.

The Roman Road was found by Mr. Still, of the Ordnance survey, in the garden, at the Swan Inn, close to the public Highway, about twenty inches below the surface. When Mr. Still was passing along the line of the Road, it happened, that they were trenching the garden ; and, the Road was found here quite perfect, buried in the ground.

From Dangerous Corner to the Swan Inn, the Roman Road runs on the north side of the present Highway ; but, at the Swan Inn, it again crosses the Highway ; and its course is very plainly marked, by the gravel and stones, for a considerable distance, in the waste ground, on the south side of the Highway.

The Roman Road entirely leaves the present Highway, at Ivy House, near Rotten Row.*

The gravel of the Road is found by the spade in the corner of Sudworth's Meadow.

The remains of the Roman Road are very visible at the edge of the brook, in front of the Cheese House.

* Rotten Row probably takes its name from the loose gravelly ridge of the Roman Road, which has run through this district.

The road was found by Mr. Still, of the Ordnance survey, in the ditch, in a field, on the south side of Rotten Row, near Hindley House.

Again, the Road is found by the plough, crossing the bottom of a field, near the Foot road, and close to the brook.

The track of the road is seen, by the gravel, which is strewed across the field, on the south-east side of Hindley Parsonage, just after the Road has crossed the brook.

The ridge and stones of the Road, are very visible in the Old Hand Lane, near Hindley.

The Road is found again, in the Meadow, on the west side of Old Hand Lane, close to the hedge of the next field.

The ridge and gravel of the Road are very visible, at the Watering Pit, in Lord Street, near Hindley. Here Lord Street indicates the line of the Roman Road.

The Road was found by Mr. Richardson, of Ashton-in-Makerfield, in the ditch of the field, on the south side of the brook, near Lord Street, close to the Garden hedge.

The Road was found by Mr. Still, of the Ordnance survey, in the ditch of the second field, beyond Stony Lane, about two feet below the surface.

The gravel and stones of the Roman Road are very visible in the Common Close, which is the next field to Amberswood Common.

The Roman Road having been traced from Warrington, through Ashton-in-Makerfield, to Wigan, and forward to Standish, towards Preston, it seemed probable, that there would be a Roman Road from Wigan to Manchester. The name of Lord Street seemed to indicate the direction of this Road; and, considerable pains were taken to discover some trace of this Road, in the neighbourhood of Lord Street. After many fruitless searches and enquiries, this Road was distinctly pointed out, in 1831, by old Thomas Bushel, of Moss House. He said, that he, and his father before him, had been Common-lookers, for Mr. Walmsley, of Westwood; and, that both of them had frequently cut turf on the line of this old

Road, which runs directly over Amberswood Common, towards Wigan. He said, that, within his own recollection, the line of this Road had been much more visible, than it is at present; and, that much of the ridge and gravel of the Road had been cut away.

The line of the Roman Road is still, however, very visible over Amberswood Common. In many places it is fourteen yards in breadth: the ridge of the Road is broad and round: the grass on the line of the Road is of a paler green; and, wherever this line of Road is cut into, the bright gravel of the Road is found in abundance. At the north end of Amberswood Common, near Common Nook, the high ridge of the Road, and its thick coat of gravel, are very prominent.

There has been another Fort on the line of the Road, on this Common; for Am-ber is Water-fort. The Road is continued, in the same straight line, from Amberswood Common to the west side of Piele, in the Scholes,* where there has been another Fort; for Piele signifies a round tower.

* Scholes is exactly what it is now pronounced, Scoghs, which is Woods.

Beyond Amberswood Common the Road is again seen in the curve of the hedge, close to a Water Pit, where its stones and gravel are very visible in the ditch. Here, as it often happens, the hedge stands across the Road.

The Roman Road is found again, by the gravel, near a gate in Ince Lane.

The Road is found in the ditch of the next field but one to the Leeds and Liverpool canal.

The Roman Road crosses the Leeds and Liverpool canal, near the second Foot bridge, on the west side of the stone bridge.

The ridge of the Road is very visible in the next field, on the north side of the canal.

The stones of the Road are found in a drain, a little on the north side of the Scholes Brook.

The ridge of the Road is very visible, as it ascends the brow, on the south side of the Brick Barn House, in the Scholes.

A full account of the Road and Station at

Wigan, and the reasons for supposing that Wigan is Coccium, are given in Mr. Baines' History of the County of Lancaster. It may be added, that four of King Arthur's battles are supposed to have been fought on the banks of the Douglas, Mr. Baines' Hist. Lan. vol. I. page 30; and, that a piece of high ground, near the Scholes, on the banks of the Douglas, is called King Arthur's camp.

In the VII. Iter of Richard of Cirencester, the route of the Iter is from the port of the Sistuntii, through Rerigonium, to Alpes Peninos, &c. It is generally agreed, that the port of the Sistuntii, was on the Ribble, and that Pendle Hill is one of the Alpes Peninos, the Peninos being echoed in the name Pendle; and, then, the intermediate Station of Rerigonium must be Ribchester; and, therefore, Ribchester cannot be Coccium. See Mr. Whitaker's History of Manchester, Book I. chap. v. sect. 1. page 176.

ON A NEW METHOD

FOR ASCERTAINING THE

SPECIFIC HEAT OF BODIES.

By JAMES P. JOULE, Esq.,

Secy. of the Lity. and Phil. Society, Mem. Chem. Society, London, &c.

(Read December 2nd, 1845.)

Three methods have been employed by natural philosophers, in order to determine the specific heat of bodies. These are, 1st, The method of the calorimeter; 2nd, The method of mixtures; and, 3rd, The method of cooling.

The first of these methods consists, as is well known, in plunging a heated body, the temperature of which has been carefully ascertained, into ice, and observing the quantity of water produced by the cooling of the body to the freezing temperature. Lavoisier and Laplace employed this method in their researches on specific heat; and, by the invention of the *calorimeter*, carried it to the greatest degree of perfection of which it is in all probability capable. In practice, however, this method has been found exceedingly clumsy and tedious, and liable to a variety of errors which have been pointed out by Wedgewood. It has, in consequence, been long ago abandoned by accurate experimenters.

The method of *mixtures* has been employed by Boerhaave, Black, Irvine, Wilcke, Crawford, and others. In this method, the heat lost by one body in cooling, is received by another body which is heated. By knowing the weights of the bodies, and the temperatures gained and lost by them, it is easy to deduce the relation of their specific heats.

The method of *cooling* was employed by Meyer, Leslie, and Dalton, and has been much improved by Dulong and Petit. It consists in ascertaining the rapidity with which different bodies cool, when placed in similar circumstances with respect to the radiation of heat.

The methods of cooling, and especially of mixtures, appear to be founded upon correct theoretical principles. Both of them are, however, exposed to several sources of error which the utmost skill and ingenuity has not hitherto been able to remove. In proof of this I may mention that V. Regnault, one of the most expert experimenters in this department of physics, has found that the two methods give different results when applied to the determination of the capacity of the same body. It is sufficiently obvious, therefore, that although the processes hitherto used are, in skilful hands, capable of giving rough approximations to the truth, they are at the same time wholly unfitted for the determination of specific heat with that extreme degree of accuracy which is so desirable in the present state of science. It will not therefore, I hope, be deemed superfluous to point out a new, and, as I confidently believe, far better method, founded upon those laws of the evolution of heat by voltaic electricity which I have developed in previous memoirs.

I propose in the first place, to point out briefly the principles of the new method, and then to show how it may be applied, in order to determine the specific heat of solids, liquids, and gases.

When any body, capable of conducting the voltaic electricity, is placed in the circuit of a battery, the quantity of heat evolved by it in a given time is proportional to its resistance to conduction and the square of the quantity of transmitted electricity. Consequently, if a wire, traversed by a voltaic current, be made to communicate to any body the heat which it evolves, the capacity for heat of that body and the wire taken together, will be directly proportional to the square of the quantity of electricity transmitted in a given time, to the resistance of the wire, and to the time; and inversely proportional to the increase of temperature of the body. Hence we derive the general equation,

$$y = \frac{Cc^2rt}{h} \qquad \text{.......................} \quad 1$$

where y is put for the capacity, c for the voltaic current, r for the resistance of the wire, t for the time, and h for the increase of temperature.

If we make the time and the resistance of the wire constant, the above equation becomes simplified to

$$y = \frac{Cc^2}{h} \qquad \text{.......................} \quad 2$$

If the current and time are constant, we have

$$y=\frac{Cr}{h} \dots\dots\dots\dots\dots\dots\dots 3$$

I have made several series of experiments, using the same conducting wire as a source of heat, a constant interval of time, and a variable current of electricity. The method adopted in these experiments was, to try the effect of a wire traversed by a current of electricity,—first, when it was immersed in water, and afterwards when it was immersed in another liquid. Hence I obtained two determinations of y in equation 2, one of them for water, the other for the liquid. The relation between these two quantities gave, of course, the capacity of the liquid compared with that of the water.

The results at which I thus arrived, showed by their agreement with one another, that the method just described was susceptible of great accuracy. But it will at once be seen, that it requires very exact determinations of the intensity of the voltaic currents, and thus renders the use of a correct and delicate galvanometer indispensable. Such instruments are in the possession of very few, and, moreover, require many troublesome precautions in using them. It is, therefore, more advantageous to dispense with the measurement of the voltaic currents, by employing equation 3, in which the current is constant and the resistance variable.

I take two pieces of thin wire, the conducting powers of which have been previously ascertained, and immerse one of them in a quantity of water, and the other in the substance whose specific heat is to be determined. I then place the wires in different parts of the same voltaic circuit, so as to cause the same current to traverse both of them consecutively. After the current has passed for a proper length of time, I note the increase of temperature which has occurred in the water and in the substance under examination. Thus I obtain two determinations of y in equation 3, one of them for water, and the other for the substance under trial, from which the specific heat of the latter may be readily deduced.

The method I have just described requires an accurate knowledge of the resistance of the wires by which the heat is evolved. The principal ways of ascertaining the resistances of wires to the passage of electric currents are those of Ohm and Wheatstone. These methods, however, require the use of galvanometers and other delicate instruments, and are not therefore so well adapted for my purpose, as the method of determining the resistances from the calorific powers, to which they are always directly proportional.

Having now given a rough outline of the new method, I may proceed to point out in greater detail the course of experiments which may be advantageously pursued, in order to apply it to the determination of the specific heat of solids, liquids, and gases.

The first business will be to obtain two similar platinum wires, each about four inches long, and $\frac{1}{100}$ of an inch thick.* It will be desirable to employ wires of exactly the same length and diameter; for if perfect equality could be attained in the resistances, equation 3 would become

$$y = \frac{C}{h} \quad \ldots\ldots\ldots\ldots\ldots\ldots 4$$

and the numerical calculations would be much simplified. As it is however, extremely difficult,

* The resistance of the wires ought to be equal to that of the battery, in order to produce the maximum calorific effect.

if not impossible to obtain exactly similar portions, even of the same length of wire, it will be always proper to examine the conducting powers of the wires. For this purpose they must be immersed in similar vessels, containing the same quantity (say a ℔) of water, and then placed in different parts of the same voltaic circuit. If the voltaic battery employed consists of six large Daniell's cells, the copper element of each of which exposes an active surface of two square feet, the heat evolved in the two jars will be at the rate of about one degree per minute. Connexion with the battery must be broken after the current has passed for five or ten minutes, when the increase of temperature must be ascertained to the hundredth part of a degree, by the help of accurate thermometers. The influence of the surrounding atmosphere must be obviated as far as possible by arranging matters, so that the temperature of the water shall be as much above that of the air at the conclusion, as it was below it at the commencement of an experiment. A series of ten experiments will be sufficient to determine the relative heating powers and resistances of the two wires to one thousandth at least; and still greater accuracy may be attained by taking the mean of this and another series, in which the wires shall have changed places in the vessels of water. In this way the effect of any slight difference in the capacity for heat of the vessels will be eliminated.

The next step will be to determine the capacity for heat of the vessels which are to be employed in the experiments. These vessels ought to be as thin as possible, in order that their capacity for heat may be very small in comparison with that of their contents. They may be constructed of any material incapable of being acted upon by the liquids. In order to ascertain the capacity for heat of these vessels, one of them must be filled with water, and the other partly with water and partly with the material of which the vessels are made. A platinum wire, whose resistance or heating power is already known, must be placed in each vessel, and then made to form part of the circuit of a voltaic battery, so that the same current shall traverse both wires. The resistances of the wires and the increase of the temperatures will then determine the relative capacities of the vessels and their contents, from which the specific heat of the material of which the vessels are made may be readily deduced. For calling the quantity of water in the first vessel* a, the weight of the vessel when empty b, the resistance of its wire r, and the increment of temperature h.—the quantity of water in the other vessel a^1, the weight of that vessel along with the quantity of the same material put into it b^1, the resistance r^1, and the increment of temperature h^1: Also calling the specific heat of the material in question x; we obtain from equation 3

$$a + bx : \frac{r}{h} :: a^1 + b^1x : \frac{r^1}{h^1}$$

whence $$x = \frac{ahr^1 - a^1h^1r}{b^1h^1r - bhr^1} \ldots\ldots 5$$

Having thus ascertained the specific heat of the material of which the vessels are made, they may be employed as follows, for determining the specific heat of various kinds of matter.

1.—*Method with Liquids.* One of the vessels must be filled with pure water, and the other with the liquid under examination. We may call the former the standard, and the latter the

* The quantities a and a^1 of course include the weight of water equal in capacity to the thermometers, conducting wires, stirrers, &c.

experimental vessel. The same current of electricity must then be made to traverse two platinum wires immersed in the vessels; and the consequent increments of temperature must be carefully noted. As the capacity for heat of the standard vessel is in this case known, our equation 5 becomes

$$x = \frac{ahr^1 - a^1h^1r}{b^1h^1r} \qquad \ldots\ldots\ldots\ldots\ldots\ldots 6$$

where a is the weight of water in the standard vessel, including the water which is equivalent in capacity for heat to the vessel itself; r is the resistance of its platinum wire; and h the increase of its temperature: a^1 is the weight of water equivalent in capacity to the experimental vessel; b^1 the weight of the liquid under examination in the experimental vessel; r^1 the resistance of its platinum wire; and h^1 the increase of its temperature.

When the specific heat of a fluid, capable of conducting the voltaic current, is to be determined, it will be proper to protect the wire from immediate contact with it. This object may be accomplished by enclosing the platinum wire in a small platinum, or glass vessel, filled with water; or, instead of the platinum wire, a column of mercury, inclosed in a glass tube of narrow bore, may be chosen as the conducting medium.

2.—*Method with Solids.* If the solid body be insoluble in water, it must be broken into small pieces, and placed in the experimental vessel, the remainder of which must then be filled with pure water. In this case, as well as in the last, equation 6 applies; the only difference being that the quantity a^1 includes the water in the experimental vessel, as well as the vessel itself, considered as water.

Soluble bodies must be inclosed in small tubes or bottles, made of tinned iron, platinum, or glass, whose capacity for heat has been previously determined. The bottles must be completely immersed in the water contained in the experimental vessel. It will be necessary, in this case, to take care to let sufficient time elapse between the stoppage of the current and the determination of the temperature, in order to allow the salt to acquire the temperature of the water. Equation 6 applies in this case also, the quantity a^1 including, as in every other case, all the bodies in the experimental vessel, whose capacities are known, considered as water.

3.—*Method with Gases.* The gases must be enclosed in proper vessels, whose capacity for heat has been previously determined, and then the experiments with them may be conducted as in the previous cases, using equation 6. Owing to the great space occupied by the gases, it will be important to employ as little water as possible in the standard vessel, and only as much water in the experimental vessel, as will be sufficient to surround the flask containing the gas.

Of the three kinds of matter to which I have thus shown the applicability of the new method, the solids seem to me to present fewer difficulties to the old methods than the rest. Consequently they are the most fitting to be employed in order to compare the results of the old and new methods together. I have made the following experiments for this purpose.

Five pounds of water were poured into the standard vessel, into which a platinum wire, having a resistance of 100, was then immersed. A bundle of small sticks of lead weighing $11\frac{1}{2}$lbs, was placed in the experimental vessel, along with 4lbs of water, and a platinum conducting wire whose resistance was 106. Each vessel was properly furnished with stirrers and thermometers of known capacity. A current from a battery of six large Daniell's cells was then passed through the platinum wires for 20 minutes; at the end of which time the increase of the temperature of the standard vessel was 3°·575, and of the experimental vessel 4°·35. Applying equation 6, and turning all the weights into grains,* I had

$$x=\frac{35280\times 3{\cdot}575\times 106-28260\times 4{\cdot}35\times 100}{80500\times 4{\cdot}35\times 100}=0{\cdot}03073$$

Another experiment was conducted in the same manner; but in this instance the thermometers and platinum conducting wires were made to change places in the vessels, so as to detect any small error in the values of the scales of the thermometers, or in the resistances of the wires. The result of this second experiment was 0·0299. The mean of the two results is 0·0303. The result of Dulong and Petit is 0·0293; that of Regnault 0·0314.

I made two experiments also on the specific heat of wrought iron. The mean result of them

* The capacities of the vessels, thermometers, &c., were found to be equivalent to those of 280 and 260 grs. of water. Hence in the equation $a=35000+280$, and $a^1=28000+260$.

was 0·10993. The result of Dulong and Petit is 0·11, and that of Regnault 0·114.

Although, in the above experiments, the quantities of water and metal were much larger than desirable, the coincidence of the results, both with themselves, and with those which have been arrived at by the expert use of the best of the old methods, is such as must, I think, inspire confidence in the correctness of the new method. Hitherto, I have not made many experiments with it besides the foregoing, and those which I have incidentally found necessary in the course of other investigations. I have not, however, thought it right to delay the publication of a method which appears to possess eminent advantages over the old processes for the determination of specific heat, and which, in the hands of any pains-taking experimenter, would, I doubt not, lead to the complete elucidation of the laws of a most important branch of physics.

ON THE

PHILOSOPHY OF FARMING.

By JOHN JUST, Esq.,
Corresponding Member of the Society.

(Read Nov. 18, 1845.)

Part I.—*On the Two Fundamentals of Farming.—Draining.— And Aeration of the Soil.*

The Architect designs, and lays down the plan of a building; the Builder rears and completes it. The Architect requires skill, and a thorough knowledge of proportions and uses; the Builder requires practice and material to carry out these proportions fully; and to fit the building for those uses. The one is not perfect without the other. The form of the building depends upon the plan: the full dimensions upon the builder, and the supply of material which he has to work upon.

Be the plan ever so perfect for its intended purposes, if there be a lack of material, it must either remain unfinished, or curtailed in some of its dimensions. If more material be at hand than is needed, either a quantity must be left unused, or the structure must be enlarged, and the dimensions increased. To raise a thoroughly complete building, then,—a plan, a builder, and an adequate supply of material are essential. Man thus proceeds to raise up various kinds of structures for his accommodation and convenience.

Every plant of the field which grows there for man's use, and every flower of the garden which he cultivates for his amusement and for ornament, is a living structure built up of material which it collects from the surrounding media of the air, and the soil. Each species, or sort, is modelled upon the self-same plan of the Divine architect —and the principle of vitality, is the agent, or builder employed by Him to construct the fabrick and to finish it. Yet, though the plan is the same in every individual species, we all know—that such individuals bearing the outline and semblance of that plan, vary exceedingly in size, and proportion of parts. This diversity of developement can arise in no other way, than from a greater or less supply of the requisite material furnished by the air and the soil, for augmenting the whole, or suppressing certain parts. This supply is obtained by plants in their uncultivated state, from the results of natural physical processes. For God in his wisdom has not only appointed vitality as His builder, but He has taxed all inert and mineral water of the earth, to furnish, by their various and continuous decompositions, ratios of the necessary supplies. And as man's good seems to be the ultimate object of all God's visible creation; man's good is provided for by this arrangement. Yet the working out of this good, the full efficiency of such an arrangement for the provision of his species, is put into man's hand almost as much as if it depended upon himself alone. Just, then, according to man's knowledge and skill, will be the nature and properties of the natural products whereby that good itself is secured to him. And the finest fabrick of the loom ever produced by human ingenuity, aided by the physical powers of nature, differs not more in its texture from the rude cloth woven from the fibres of the stems and leaves of plants, which cover the nudity of an Indian, than will differ the produce of the field, when duly and fully cultivated, by means of such an advancement in agriculture, as has taken place in the arts, from the wild heaths of our moorlands, and the rushes and sedges of our undrained swamps and fens—and even from the present crops in grain and grass of our semi-cultivated country.

From these prefatory remarks, we must admit that man stands nearly in the same relation to plants, as the supplier of material does to the builder and the building; and that he can modify plants almost to as great an extent as material, and the builder can do the building. All cultivation consists in bringing to the plant, or placing within its range of action such a supply of material as natural means cannot furnish it with in the situation where it grows. In order to cultivate well, it is therefore as necessary to know what plants want, as for the builder and contractor of material to know what is required for the building. This known, the course to be pursued in farming will be clear and easy; and the necessity of aiding nature in the fundamentals of vegetable economy become as paramount as apparent.

The material, out of which the vegetable structure is reared, consists nearly of aerial and fluid elements and substances alone. The exceeding minute delicacy of the tissues of this structure renders it all but impossible for solid matter to enter the system. The principal kinds of aerial and fluid matter are carbonic acid gas, or air, a little azote, or nitrogen gas, and water; which substances may be said to correspond with the stone, or brick, timber, and mortar of a building. And, singular as it may seem to the unitiated mind, the almost countless kinds of vegetable produce, with their specific properties, are the result of differently arranging and combining these three substances, and the four atomic elements which they contain; and exactly as these elements and substances, under certain conditions of the soil, are present in abundance, or otherwise, when we cultivate plants, the increase of what is profitable for us is promoted, and the decrease of what is noxious to us accelerated, and the noxious qualities almost nullified and rendered void. Hence, as the food of plants consists mainly of fluid and aerial elements, the soil in which plants grow can be but a medium of that growth, or a recipient of the elements and substances of such food or material as the plant requires. To know how the soil acts in this capacity, and how its capabilities so to act may be augmented, must, therefore, be essential, before any just knowledge of its proper treatment for the cultivation of plants can be acquired.

To be a proper medium for the growth of plants, the soil must contain water, or moisture, carbonic acid gas, or carbonaceous substances, from which that gas may be liberated; or azotised matter, from which azote also may be eleminated within the plant. These the soil cannot contain in due proportions, except it be to a certain extent permeable. If the soil be not sufficiently permeable, it will contain an excess of moisture or water, and no plants but aquatics can thrive there. Stagnant water, therefore, destroys all such plants as farmers wish to cultivate, and therefore must be dispensed with; and this destruction of useful plants is occasioned by the displacement of the other elements. Hence the sterility of impermeable clays,—no air, or gaseous elements can penetrate them; and the roots of plants are thence cut off from extending within them; or, if extending, from obtaining therein their complement of elements. This want of a due circulation of air throughout the insterstices of the soil is the cause of the destruction of trees, when they are surrounded with artificial mounds, or their roots otherwise buried beneath the constant accession of this all-important substance. Permeability is not thus necessary to make a soil fertile; but a sufficient and constant filtration, or drainage, in order that the access of one element, or the supply of one portion of the raw material may not interfere with, or prevent the accession of, due supplies of the rest. Whatever moisture exists within the soil, beyond that which the wants of healthy plants require, must be injurious to growth; because it keeps off the accession of other essentials, in the exact proportion of that excess. From these facts we infer that a thorough percolation of water through the soil, and a constant accession of air are conditions absolutely necessary to full growth, and therefore are the two fundamentals of all good farming, inasmuch as no methods of treatment, however good and appropriate in themselves, can fully answer their purposes without them.

Soils may be classed as generally consisting of three distinct kinds—wet, dry, and intermediate. Wet soils arise either from the total, or almost total impermeability of the sub-strata—whether they be stiff clays or impenetrable rocks—or from water which has filtered down from higher grounds collecting in the subsoil, and rising up and swamping the surface. In such soils no cultivation can succeed without a thorough drainage.

No exact practical rule can be laid down for draining; yet one axiom may apply to all methods employed, or which may be employed, and that is, no kind of drainage is perfect which does not carry off the superfluous water as quickly as possible. After a heavy fall of rain, or continued wet weather, main drains should never discharge water beyond four or five days at the utmost; and the less time they continue to flow, the better in all cases. So imperfect, however, is drainage in general, that we see main drains pouring out water week after week in fine weather, and scarcely ever ceasing, unless in continued drought. Whatever methods have been employed, acting so, they are practically useless. Drains, whatever be their kind, ought to be so deep that the water which percolates into them should all fall within the subsoil without having any chance of coming into contact with the soil. For we might as well expect to dry a sponge by lifting it out of water and letting its base touch the surface, as to dry wet ground by allowing the soil to come in contact with the water within the drains. If the substratum be not thoroughly impermeable, depth of drainage is most important ; because the drainage then not only extends farther laterally, but acts more effectually. Drains carried along the sides of slopes or inclining grounds nearly on a complete level, frustrate to a great extent the object for which they are intended; since, instead of conveying away the surface water as soon as possible, they are calculated to detain it long as possible. Besides, through want of a current in the flow of the water, mud soon accumulates at the bottom of such drains from depositions therein from the stagnant water, so as ultimately to fill them up and choke all passage. And even to drain flat surfaces—levels where practicable should be so brought up to them, as to allow the drains being laid deep and a current however slight within allowed. It is essential, therefore, both to be understood and remembered, that those drains which by their depth extend their influence widest, and allow the quickest percolation and the readiest discharge of water from the surface are ever most efficient and desirable.

In some districts, the clay of the subsoil is as impenetrable as the most continuous and unstratified rock. No water can be found within it, when dug to the greatest depth All attempts to dry such soils by drains must therefore fail. The only plan is to dig open channels through the soil, a few inches deep into the clay, that the water may flow away from the surface as readily as possible after every fall of rain, and not stagnate in the furrows. Such open channels ought to take the most direct course for the discharge of the water; and in meadow grounds should be annually freed from the water plants, which may have grown within them.

When water swamps the surface of the ground solely from filtering down from higher grounds into a kind of natural basin of impervious rocks — a single drain tapping the inlet is commonly sufficient to dry the whole. In intermediate soils such as are formed chiefly by alluvial deposits on holm lands, and flats on the banks of rivers, which flow through clayey districts, where the subsoil consists of layers of gravel and fine sand, intermingled with layers of impervious clay, it not unfrequently happens, that after all attempts to dry the surface by ordinary methods have failed— one or two drains cut very deep, and penetrating the stratum in which the water lodges, suffice to dry whole fields of many acres. Successful draining, therefore, requires not the mere practical skill of the persons employed therein—but a good general knowledge of the stratification of the districts in which drainage is required.

And as philosophy takes nothing for granted; as it is a science which consists entirely in a knowledge of facts founded upon natural principles; and therefore submits all that is brought before it to tests; whenever it enters upon a new path; it therefore carries along with it as its guide, such tests as have been furnished by previous experience. Philosophy hence suggests —that when ground has to be drained, where the nature of the subsoil has not been ascertained that that nature be examined as a preparatory step. It gives the following data as guides to the inexperienced. If, when the soil has been carefully removed from an area of a few yards in extent, and the surface of the subsoil has been left to dry, water is found to accumulate within it, when dug into, then that subsoil is drainable; and will draw water from the surface according to the depth dug, and the ground may be made perfectly dry by the usual kinds of drains; provided those drains be laid sufficiently deep, and allowed a free discharge. Whereas, if after the same preparation, the subsoil or clay when dug to a greater or less depth be perfectly dry, then no drainage can be effected therein by ordinary methods, and recourse must be had to opening transit, for the surface water, in open channels, so that the supersaturation of the soil may run off as directly and quickly as possible.

Thorough drainage being thus effected so that soils have been thereby brought into a similar condition to soils naturally dry, due preparation is made for the full developement of the plants to be grown, or cultivated thereon, just as due preparation is made for constructing a building when the foundations are dug to a proper depth for security, and are laid dry. And just as the perfection and duration of a building depends primarily upon the depth and security of the foundations, so is the parallel good, with respect to drainage, we cannot drain too much, we cannot make our lands too dry. And as when we intend to make a noble building, we dig the foundations deep, so if we intend to grow noble plants, we must drain deep too; because thereby we increase space for material to be brought to and used by the plant, similarly, as in the case of the kind of building just alluded to, we make room for more material. This leads us to consider the nature of aeration as the vehicle of the supply of material to the plant within the soil.

Physiology and Chemistry teach us that aerial matter contributes more to the structure of plants than fluid matter does. Fluid matter is required more for furnishing, than building up the fabric —more for storing up properties and carrying forward supplies—than contributing supplies itself. Hence, the absolute necessity of a constant accession of aerial matter, which may be carried into the structure whenever it may be wasted, and to the place where most wanted. Air brings the material from a distance to the plant; and water carries it forward within the plant. The more, then, aerial matter has access to the plant, the more material it will bring to the plant; for water to serve in the construction of the plant and distribute throughout it. Hence, the benefit of draining. When drainage is perfect, it carries away rapidly the superfluity of water within the soil: air immediately occupies the spaces within the soil, previously held by the water; and, therefore, carries material thither, where it was not before. The roots of plants follow this change. A greater space is brought into action—and more material into that extended space. And as the roots take in the material, for the fluid within the plant to carry forward—as they extend and increase in the midst of an extended supply of material—growth ensues apace—just as, when in a building more hands are employed, and more material brought to work upon, the building rises with greater rapidity.

This great physiological truth in vegetation is substantiated by every fact which we observe in the economy of growth. In gravelly and sandy subsoils trees thrive luxuriantly. The same kinds are stunted and meagre, on stiff clayey substrata, and impervious subsoils. In the former, the roots are found to extend deep and wide. In the latter, downwards they cannot get, and, therefore, are checked and restrained in their growth. Trees near farm buildings and sewers, in stiff clayey soils, also thrive better than in hedge-rows, because their roots can extend along the sides of the foundations of the buildings, and along the sewers, where air circulates, and thence obtain larger supplies. And the remarkable changes which always follow drainage on lands which have been swamped—not only in the quantity, but in the quality of the grasses and herbage thereon, proceeds from the simple effect of this aeration of the soil, in all instances where no tillage besides could be afforded still more to aid nature in bettering the produce. And much of the beneficial effects of falls of rains and showers, is occasioned by the displacement of air within the soil, already acted upon by the roots of plants, and the consequent replacement of fresh air from above, when that rain percolates and filters down into the subsoil, or flows into drains.

But the advantages of as full an areation of the soil as possible to be derived from the constant accession of nutritive elements to the roots of plants contained in the air, are few, compared with those which are effected within the soil itself by the agency of the atmosphere. Complete dryness prevents the decay of defunct organised structures. Supersaturation of moisture restrains the process of such decay. While the joint action of a modicum of moisture and of air, accelerates that process. Indeed, all enemacauses, whatever, depend upon the presence of the air for their action. Out of air spring alike life and destruction ; and the result of the one, is but the counter action of the other. All organized remains then in the soil, and acted upon by the aeration of the soil, and are thereby speedily reduced to their primordial elements; and these elements being in immediate contact with the roots of plants, are immediately absorbed into the plants, and carried like stones, ready hewn, when one structure is pulled down to build up another, to construct the solid portions of the living fabrick. Nor is it that aeration of soils is thus serviceable in promoting the decay of organised matter in soils; but it likewise slowly acts in the reduction of mineral matter, so as to render it soluble in the moisture of the soil, that it also may thence be carried into such vegetable systems as require it, so that aeration accomplishes every thing which plants may want in the medium of the soil for growth and sustenance.

But this action of aeration within soils must soon expend the organised and mineral stores within the sphere of its action. Plants, therefore, which grew and prospered most luxuriantly upon its first introduction, must by and by begin to languish as they reduce their supplies. When nature has thus exhausted her means by her own energies, then art comes forward to her assistance. The produce of the field being carried off, or consumed by man, or for his benefit, man makes a return to the ground in various kinds of manures. These spread over the surface of the soil in different stages of decay—have their decomposition accelerated, both from the solution of the soluble portions by the rains which fall, and by the action of the atmosphere. The rain washes what it dissolves into the ground, and the air following the rain water as it filters off, acts also upon that matter within the soil, and thus carries food of the strongest kind to the roots of the plants, which are thereby restored to vigorous growth. So that manures depend upon the accession of the air into the soil for their full efficiency in increasing growth. Manuring wet grounds is a waste of the greatest portion of the material, as it cannot promote growth, for lack of aerial matter around the roots of the plants within the soil.

If thorough aeration of the soil be thus indispensable for a full growth in meadow and pasture grounds, according to the quality and condition of the soil, it is equally so in arable and ploughed grounds; and as aeration is aided by pulverisation, the great object in ploughing ought to be to bring as much soil as possible into this condition. Deep ploughing is hence as requisite as deep draining.

No crop can be thoroughly good without it; yet we see furrows cut only a few inches deep, and nothing but sward turned up to be pulverized; the most friable portion of the soil being left untouched, and therefore comparatively inert. I have seen good crops obtained from exhausted ground simply by deep ploughing, though such treatment ultimately ended in exhausting the soil still more, through want of a due return in manure *to the soil.* And not only is deep ploughing the foundation of good crops, but it ought to be followed out with complete harrowings and breaking up of clods, and division of the particles of the soil. Such a method allows a freer accession of air, and distributes throughout the soil a larger portion, encouraging thence a greater extension of roots and more successful action.

And herein lies the secret of the great success of subsoil ploughing and the subsoil trenching of spade husbandry. Aeration is thereby increased, and the plants grown are afforded more ample space for the extension of their roots, and come in contact with an extraordinary quantity of alimentary particles. Nor is this beneficial effect of subsoil culture confined to one crop, but continues until the filtration of particles of soil from above has choked up the air passages. Then the process requires to be renewed; and this renewal is also attended with its advantages. A second filtration of particles of the soil into the subsoil, and the renewed action of the atmosphere within it tend to convert the whole into soil by mixing with it decaying animal and vegetable matter. Thus the extent of soil will ultimately be doubled. And husbandry of this kind not only secures profitable returns to the present cultivator, but benefits posterity by leaving to them increased capabilities within the soil; so that while a farmer thus becomes his own benefactor, he becomes also the benefactor of all mankind. For he who, by skill and management, can make two bushels of grain grow on ground where his fore-elders could only grow one, and leave that ground in a condition to continue the produce and increase it by a continuance of the management, may console himself with the thought that, whatever others may have been, he has not been useless in his day and generation. He may leave neither fame nor fortune behind him; yet he bequeaths untold blessings to untold generations of his fellows who have to follow him to the end of time.

The thinner and poorer the soil the greater the advantage of subsoiling. You gain space for the air to bring its store of material to where it is most wanted, and you let in new agents of fertility into the soil. Such soils, however, must be thoroughly dry :—another reason why drains should ever be deep to be out of the way of the spade and the plough. Subsoiling assists drainage in facilitating the descent and escape of the surface water. It is therefore every way advantageous, and should everywhere be encouraged and extended, *where practicable ;* and it appears to be practicable everywhere where the subsoil is pervious and admits of drainage.

We may then fairly consider perfect drainage and aeration of the soil as the fundamentals of all cultivation: nothing can prosper without them. They are the engines and railways of all growth ; though they may not add speed, but only space to produce—they are atmospheric lines—calculated not for short, but all distances and places. The heavens are high above us, and the earth deep below us. Upwards and downwards, then, we may go, adding in height and in depth to the stores of food for our sustenance. The times in which we live are marvellous. They abound in what are natural wonders—in unfolding the stupendous powers and principles of nature. What we have seen yet is but a prelude to what man shall see. Physical laws are mighty and manifold—still more so are vital ones ; because the former are but the servants of the latter. And when these laws become known—when their operations can be seconded, like as are physical operations, and directed to the purposes of mankind, then they will become as much man's devoted and never tiring servants as any physical agents whatever, and will cause the seed sown for the produce of human consumption to bring forth the mightiest results—" some twenty, some thirty, some sixty, and some a hundredfold."

Part II.—*On the Physiology of Sowing Seeds, and Growing Plants for Farming Produce.*

The ground being dug, and cleared duly and properly for a building, the foundations of the building are then laid. If the building intended be of a noble and extensive kind, then the laying of the foundation is a most important matter, and requires the utmost attention. A mistake made there cannot be remedied by any future caution or care. So, when ground has been thoroughly drained, and aeration secured to the utmost extent, preparation has only been *thereby* made for the laying of the foundation of the structure of the future plant—for sowing the seed which contains the germ of all. It would not do to build a house at random. No more will it do to sow seed at random.

Scattering seeds indiscriminately over the surface of the ground previously prepared for their reception, is no more sowing them than tumbling in stones into trenches properly dug for the foundation of a building, is laying those foundations. The seed is the foundation of the structure of the future plant, and the germination of that seed is laying the foundation of the same. It is the first act in the process of the economy of vegetation; and therefore, like all first acts or steps—most essentially important. We must therefore, first inquire into the nature of germination, and the conditions required to aid and promote it, before we enter upon other considerations.

Physiology teaches us that germination is the principle which calls forth the dormant vitality of a seed, and effects all those changes within it which are necessary for the purposes of the growth and developement of the seedling plant. Like an egg, the seed contains a vital germ within it, and a store of nutritive matter around that germ, to furnish it with all its wants till fully matured, and as it were, hatched, and introduced into independent existence. The requisite external conditions to enable a seed to go properly through the process of germination are a proper degree of temperature, a sufficiency of moisture, and a free access of air, with exclusion from the direct action of light. If air be wanting all other conditions are of no avail, since the primary change whereby the germ is enabled to exert its vital energy upon the matter around it, is prevented from taking place. Hence seeds at the bottom of stagnant waters, hence seeds in swampy soils, hence seeds deep buried below the surface of the ground, or shut up in hard clods, being cut off from the action of the air, remain dormant. The great supporter of all life is absent; and, therefore, proper material and proper conditions for aiding and maintaining vital action are vain and useless.

The healthy germination of all seeds depends then, upon a due and adequate accession of air. In order to secure this accession of air, as complete a pulverisation of the soil, as possible, becomes necessary; and also as slight a covering of the seed, within the soil, as is consistent with keeping the seed, during germination, from the retarding influence of light. Light checks the vital chemical action within the seed, whereby the farinaceous substances are converted, from an inert mass, into living, growing tissues. And deep burying the seed causes the blade sent up by germination to the surface of the soil, to become so attenuated by an expenditure of the material of the seed to enable the blade to reach the surface as to weaken the vegetation which follows. Attention ought, therefore, to be paid to the ploughing of the ground for this purpose. Not only is deep ploughing indispensable for bringing up a sufficiency of soil to be pulverized; but as compact close laid furrows as can be accomplished, to prevent portions of the seed from falling down between the furrows, and being buried too deep for active germination, and vigorous vegetation afterwards. Excellence in ploughing should everywhere be patronised and encouraged. It is not to please the eye only, that the ploughmen of Westmorland, Cumberland, &c. and other well cultivated counties, take so much pains in drawing their deep furrows as strait as a line can make them, and laying them so compact, that not a crevice between them can be found in fields of many acres; but it is to favour this grand and fundamental principle of growth, though, perhaps, in few instances this service may neither be known, nor appreciated by them. To encourage, and spread wide as possible such masterly performances as are there being year after year exhibited, is not only a bonus to skill, such as skill ever deserves, but the establishment of a philosophical element in agriculture, without which it can never prosper as fully as it ought to do.

A healthy and vigorous germination of seed, is as indispensable in farming, as a healthy and vigorous offspring in breeding and rearing cattle. No favourable condition ought to be overlooked. The seed ought to be lightly covered, so that it may be under the full influence of external conditions. The soil should be fully pulverised by a thorough harrowing, as well as a deep compact ploughing. And the seeds ought all to germinate simultaneously, and come up full and vigorously.

The full importance of an active germination of seeds, will become more apparent by examining a little farther into its physiological nature. Seeds have been stated to be analogous to eggs, or the ova of animals, and germination to the hatching of such ova. As the hatching of ova, or eggs introduces independent individuals of their kind into existence, so does germination introduce individual plants into existence. And as the greatest portion of the substance of the egg is mere nutritive matter, expended in the formation of the living structure to be reared; so is the greatest portion of the seedling plant developed by germination, mere nutritive matter to be expended in the formation of the living structure of the plant. With the exception of the very small portion of the tissue, which is bound up with the gradual growth of the embryo of the seedling plants, the greater part of the seedling is a nutritive store, forming no portion of the real plant. It merely nurses the nascent plant, until it can support itself. The germinative portions are then commonly absorbed; and either wither away and fall, or they become inactive and inert. Since the germinative parts of seedling plants are intended for this special purpose, it is evident that they cannot be too full and vigorous, as they have to furnish all the supplies to the first stage of vegetative growth; and thus lay the very foundations of the future structure.

This physiological fact may be new to the practical farmer; yet, like every fact in nature, it is easy of demonstration—inviting the eyes of observers everywhere to it, by a variety of instances. Waiving all such instances at present, but such as are connected with our subject, we will briefly follow the seeds sown through the stage of their germination. First we observe four or five radicals protrude from the base of each seed, and elongate downwards into the soil. Next, the blade appears bursting through the summit, and rising upward into the air. Growth is now rapid, but in a few days it seems to become stationary. It is then, that germination ceases. All the material of the seed has been expended in the formation of the seedling plant, and in storing its tissues with nutritive fluid. Yet, though apparently stationary, the plant is not inactive, other roots burst out from the bottom of the seedling plant; and from the centre of the seedling blade springs up another blade. Vegetation commences—and the vigour of its action now depends upon the external conditions within the soil. But of this vegetative growth we see no portion of the germinative, or seedling plant partake; the first blade, or what Botanists call the cotyledon, soon becomes yellow, and has all its fluid absorbed, and then withers away. The same takes place with the radicals—so that we ultimately find, that the germinative portions form no part of the real vegetating structure. And if in this our examination of the seedling plants, any seeds have been buried too deep, and the germinative blade has had to elongate itself to reach the surface, then, we perceive above the seed, and near the surface of the soil, at the place of the first nodus, or joint, roots are thrown out, and the parts which have germinated beneath thenceforward lose their action; a proof that such seeds have been put into an unnatural situation, and that the resources of nature have been drawn upon to meet this, and modify her course, to the contingency. And not solely such an ordinary process in nature does the physiology of germination explain, but other phenomena of extraordinary kinds. Years ago, I recollect an instance, wherein a gentleman applied to me for an explanation of a very singular and rare occurrence. He told me, that in breaking up a field, a portion of which had previously been tilled with the refuse of a glue manufactory; the oats, sown thereon, sprung up without any roots or radicals, and that he felt apprehensive he should have to sow the ground over again, as he imagined the oats could not possibly do any good. However, when the second blade began to appear, roots shot rapidly out from the base of each grain, and the plants thenceforward grew most vigorously, and produced a most abundant crop. A striking proof this, that the radicals produced by the germination of seeds are not essential, inasmuch as plants in favourable circumstances can do without them, and that the germination of seeds is intended by nature for the purpose already assigned, viz:—

that of calling the nascent plant into existence, and nursing and feeding it until it can cater for itself.

The exact value and nature of germination in the vegetable economy being thus made known, no doubt can exist of the great necessity of encouraging the process to the utmost, by a due regard being had to the special conditions. And, as the ultimate object of growing grain is the reaping of a profitable crop, dependant both upon quantity and quality, a simultaneous germination of the seed, and fully vigorous seedling plants alone can ensure such a produce. And if the maltster be so very careful in frequently turning over his steepings of barley, in order that an accession of air and an equal temperature may cause each grain to germinate simultaneously, and his malt may be of first-rate quality; and that thence a more intoxicating beverage may be brewed (for the bane of thousands) —ought, I ask, the farmer to be less attentive that the self-same process, carried on within the soil, should be as fully encouraged, on purpose that an ample supply of bread may be baked for the boon of millions? To such a question any one endowed with the common principles of humanity would feel too indignant to reply.

We can now understand, even for the purposes of germination, why soils should be ploughed as deep as is possible, or as they will permit—why the furrows should be laid compact—and why the soil so turned-up should be pulverized to the utmost extent that the season, and the implements of husbandry will allow, or can accomplish. Yet the introduction of the fact, to illustrate the position which we have endeavoured to occupy and maintain, needs itself to be explained before we proceed directly onward with our subject, inasmuch as we may have occasion to revert to it again; and it may serve us in endeavouring to point out another physiological distinction useful to be known, and that is—germination and vegetation are two separate principles; the former primary and essential—the other secondary and accessory, springing out of the former, but totally different in its action and the sphere of that action. The chemical and vital change which germination effects within the seed, throws off a portion of what the vegetative growth requires; while what favours vegetation most, injures germination. Bee-bread and honey differ not more in their nature and qualities, than the materials out of which the two structures reared by germination and vegetation, differ; and the grub could as well thrive and go through its changes, if supplied with honey (the food of the bee), as germination thrive with the nutriment which best supports vegetation. Hence the highly azotised condition of the soil on the manured portion of the field, in the crop of oats already mentioned, prevented the emission of the radicals in the germination of the seeds, while that condition was most favourable to the vegetation of the plants afterward, as was evinced by the superabundance of the crop. And, as we must allow no fact to pass by us unimproved, we may hence learn the reason why highly azotised manures sometimes destroy the germination of seeds altogether. Guano sown along with the seeds of turnips prevents their germination; whereas, when scattered over the soil, or buried in the drills beneath the seeds, it promotes the vegetation of the plants to a very great extent afterwards. The same is the case when liquid manure, from tanks in farm-yards is applied to soils previously to sowing the seeds. I have known turnips sown on ground so treated fail to germinate entirely; and by injudicious application of night-soil as a dressing for crops of barley I have seen numbers of the grain totally destroyed by contact with it, and those which escaped pushed on to such a rank vegetation after this destruction that they could neither fructify properly nor ripen.

Thus, by collating facts and comparing them with the principles which physiology teaches us, we arrive at truths, as results upon which we may depend in farming, as in all other departments of natural science which are founded upon sound physiology.

It appears, then, that the practice so common of manuring ground for crops, previously to sowing the seed, though convenient, is not a consistent practice. There is thereby a check and restriction upon a free and full germination of the seed. And though the injury thus occasioned, where a judicious application of such manures as is only calculated for the maintenance of one crop, cannot be great, or very apparent, still the application thus, is an infringement of one of the laws of nature, and cannot but be to some extent detrimental. If the same application of the manure could be made at the time vegitation commences, there is no doubt but the crop would be benefitted thereby.

If aeration is essential to germination, it is equally so to vegetation. Seeds, however, can germinate in masses—but plants cannot vegetate in masses. Then, they require room for their growth and expansion. A question hence arises, what quantity of plants on a given area will be productive of the greatest quantity of grain of the best quality? No special rule can be given as an answer to this question—because no special rule can apply to the quality and condition of all soils. It may, notwithstanding, be depended upon as a general axiom—that be the quality and condition of the soil what it may, no more seed ought to be sown than will suffice by leaving space for stooling, to fill up the whole surface to the extent of the full action of light and aeration. I have known ten measures of seed-oats sown on a Lancashire acre produce more and heavier grain than fifteen measures per acre sown at the same time, and in the same field. And, as economy in farming is the basis of all prosperity therein (as in every thing else), almost everywhere throughout this country, whether the farming in other respects be good or bad, a saving of one-fifth to one-fourth of the seed sown might be made to the immediate profit of the farm, as well as of the future produce. I know that it is urged as an argument for thick-sowing—that a protection to the plants against the growth of grasses and annual weeds, as well as against the attacks of grubs and wireworm, is thereby afforded to the crop. But such an argument is altogether futile; for, in the first case, the grasses and weeds arise from nothing else than previous bad management; and in the latter, the attacks of grubs and wireworm, can neither be prevented, nor the injuries inflicted be compensated for by any kind of sowing whatever. The custom of thick sowing, like other foolish customs, has arisen from downright ignorance; and the idea that numbers can make up for that vigour, *where the sources of vigour are unknown.*

In the ordinary mode of sowing the seeds of grain, a great waste of seed arises from too much falling between the furrows, and making the seams between the furrows too thick, even when not buried by falling through crevices below them. This useless expenditure of seed cannot well be obviated, unless by the adoption of drill husbandry, or the employment of sowing machines, similar to such as are used for sowing Italian Ryegrass, &c. Something, however, may at present be done; a light harrow might be drawn once over the ground before the seeds are sown. By this means not only would the ridges of the furrows be reduced, and the seams between them partly filled up, so as to prevent too great a proportion of seed falling into those seams; but any void spaces left between the furrows would also be filled up, and the seed be thereby hindered from being buried beneath them.

What has been stated to be requisite for promoting fully the germination of grain, is true of all seeds sown for farming purposes. The principle which governs them all is one; and the laws by which that principle operates are the same. The same holds good too with respect to planting tubers; or multiplying individuals of any species, by means of buds; for buds have many of the characteristics of seeds, and germinate first, before they vegetate. And it is for the support of this germination that tubers are furnished with stores of amylaceous matter; and like seeds, are articles of food in consequence. The cuttings of potatoes, or the whole tubers which we plant, have to undergo a similar change in spritting, as seeds undergo in germination, and require similar conditions to favour that change, and aid germination. Yet, in our treatment of this most valuable and accommodating of all plants given to man for food, we err more against nature, than in all others put together. Patient of every climate under the sun, we forget that it can be subject to any wrong, or require any of our care or concern for its welfare. Prolific, beyond our wants, we have glutted our domestic animals with it; and employed it largely in the arts and distillery, to contribute to our luxuries. Yet there is a limit to all things, and we are approaching the limits of the abuse which we can unrequitedly heap upon it. Something is wrong already, both in the field and in the store; already it has partially failed in its germination during the spring; already, it has become the prey of disease in its vegetation and maturation in the autumn. Nature is vindicating her right to be obeyed, and since we have neglected to learn from her by lessons of examples which she has offered, she seems determined to make us wise by dear bought experience—to make us feel, that we may remember.

The first law of nature against which we transgress with regard to the potatoe, is in our total neglect of the due preservation of our seed potatoes. If they are only good for food, we never enquire whether they are fit for planting. Yet, were we but to reflect one moment, we should soon see how unnaturally we treat them. Nature, when she alone takes care of them, keeps them within the soil—like all other subterranean buds—during their season of repose; and because in the warm climates where they are indigenous they cannot easily be cut off from a due temperature for their germination,—she checks it by keeping them dry in the soil. We, on the other hand, dig them up from the ground, because we fear, and properly, the effect of frost upon them; but instead of keeping them dry, we heap them up wet in immense quantities on the ground, and cover them over there to keep them so, *with soil*, thereby furnishing them, if they do not rot, with one requisite condition for germination; while the masses themselves raise and keep up the temperature to supply them with another, so that germination has not only commenced, but proceeded considerably, when we dig them up again for planting. Then calculating upon the extraordinary degree of vitality, with which nature has endowed the tubers, we pull off the sprits, cut up the potatoes, and endeavour to reduce that vitality to as low an ebb as possible before we plant them.

Within certain limits plants correspond with animals, in being able to transmit a portion of their vigour and qualities to their offspring. If we wish to cultivate a healthy race, we must plant from a vigorous stock. If the vital energies of that stock have been by any means impaired, the descendant plant will participate in the loss. This fact is obvious in the destruction of the leaves of plants by insects, which so weakens the buds, which the leaves have to maintain, that the shoots next season are weak and sickly; nay, in some instances the whole vitality of the plant is thereby destroyed and it perishes. Weakening the bud ever weakens the future shoot, be the conditions externally as favourable as they may. Allowing seed potatoes to sprit, and then pulling off the sprits, is an infringement of this law. The vital energy of the bud which succeeds is lessened, and a less vigorous shoot is the consequence, and not only so, but less vitality is communicated to the buds of the tubers, which are to continue the individuals of the species, or variety, for the succeeding season. So that even by a perseverance in this infringement of a natural law, we may, whether ignorantly or otherwise is of no consequence, ultimately reduce the vital energy of the buds so low, that it cannot overcome the resistances which surround it; and over planted tubers, or sets of potatoes, may fail to germinate entirely; as in the instance of plants perishing by the reduction in the vital energy of the buds from insects eating the leaves, just alluded to. Besides, if by the spritting of potatoes, the whole of the diastase situated just below the bud, as it is just below the embryo in seeds, be expended, then there is no provision left for the conversion of fecula into saccharine matter for the formation of the first tissues of germination, and germination must, therefore, fail. The corn weevil first destroys this substance in the grains, which it attacks in order that it may have the residue for its future consumption, when the season of germination arrives. Frost liberates it in potatoes, and as its immediate action is the conversion of many hundred times its own volume of fecula into sugar; frosted potatoes are sweet in consequence. Indeed, in no case can germination ensue, without saccharine matter. The sap of trees is therefore highly charged with it in spring, before the expansion of the buds, as a provision to enable the buds to germinate.

Keeping, then, these statements in mind, and considering that the cuttings of potatoes after all, are either planted upon, or under farm yard manure, in a highly azotised state, and frequently warm with fermentation, we need not be surprised that the germination of some of the cuttings sometimes fails; but that after such treatment any cuttings at all can germinate. I know that partial failures in the germination of potatoe crops, have been assigned among a multiplicity of absurd surmises, to atmospheric influences; and I deny not that these may operate at times against it; for I have known portions of a field planted in the morning partially fail, when the afternoon's planting has fully succeeded; and vice versa, all in the same field—all taken from the same heap of potatoes—and all manured from the same dunghill; still, I conceive we deserve a punishment, from laying ourselves open to it; for if we do not kill our seed potatoes outright by ill treatment, we leave them so nearly void of the germinative principle, that the slightest adverse condition only finishes what we have begun.

That it is chiefly the power of germination that is destroyed by the course pursued, as just mentioned, the following facts will render highly probable. Years ago, when my attention was first directed to the failures of germination among the potatoe crops, I was induced to open the drills, where the plants had failed, to see in what state the cuttings were, and was surprised to find them generally sound, and free from symptoms of decay. And not only did the cuttings appear in most instances quite sound, but in July and August, I observed many of them had emitted from the eye or bud, a funiculus, with a small tuber at the end, just as if a weak vegetation had been going on. These small tubers, however, soon ceased to increase, as the matter within the cuttings which supplied them with nutriment, was soon exhausted. This production of small potatoes from tubers without any other vegetating organs is frequently met with in the centre of potatoe stores when these are opened in the spring, such tubers as have produced them, being void of sprits. And when potatoes have been placed in dark cellars where no light nor much air can penetrate, and forgotten there, and left to themselves during the summer, they frequently exhibit in autumn the singular appearance of a crop of tubers, without either root or stem. It appears, then, that the power of germinating may be lost in the tubers of potatoes, without the total cessation of vital action; just as in other tuberous roots, the tubers retain the power of emitting roots (the Dahlia for instance), when separated from the collet of the plant, and thereby precluded from forming buds and vegetating. And it would be a highly interesting experiment for those who have the opportunity of trying it, to ascertain whether the small potatoes produced by vital action, after germination has been extinct, are themselves endowed with the power of germination, and can thence produce vegetating plants.

The method into which we have been led in so far discussing our subject, seems to require from us here the introduction of an inference which must have already crossed the minds of most of us, viz: that as sowing and planting on manure, or manuring at the same time as sowing and planting, are directly contrary to the physiological and philosophical inductions which ought to govern us in the science of agriculture; the case is more particularly so in the planting of potatoes, wherein the manure is not only put where its first action must be injurious, but where its action afterwards is much limited, by being in the exact position *from* which the vegetating roots of the potatoe plants shoot out into the surrounding soil, instead of there where these roots might shoot *to* it, and reap the full benefit of it.

Among the expedients which have been resorted to, to prevent failures in the germination of potatoe crops, may be mentioned that of autumn planting, which, so far as tried, has fully answered expectation. Indeed, from what we have already stated, the result could not be otherwise, inasmuch as by such a plan we follow the course of nature. Tubers left in the ground by being overlooked in the gathering of the previous crop, never fail to germinate. But as such a plan cannot, in all cases, be carried out conveniently, if we only select our potatoes for seed at the time of gathering, and keep them dry in a dark place, and in such small quantities that they cannot sprit, then we may reasonably expect as we answer the object of nature, though for artificial purposes, she will make a due return for the attention paid to the principles of her economy in favouring us with a full and continuous germination of all our potatoe plants. For being so explicit in treating of the planting of potatoes, and the physiology thereof, the vast importance at present of the subject for the future well-being of us all, will, I trust, be accepted, as an ample apology.

When vegetation commences, the habits of the plant are totaly changed. Beforetime it had all it wanted within itself, food which germination cooked and supplied; now it has its own provisions to seek abroad, and take them in as they are, and digest them for itself. It has been regularly weaned, and requires more solid food, and has a more solid structure to rear for more lasting purposes. If we watch the progress of its growth, we soon observe that the roots which first shoot forth after germination has ceased, are quite different in structure and appearance from those of the radicals already in the soil. Instead of being simply fibrous and smooth, they are covered over with numerous fibrils, and are more or less ramified, that they push rapidly downwards into the soil, far beyond the extent to which the stationary radicals have reached; and that upwards into the air blades successively spring, borne aloft by an axis of growth or culm which appertained in no way to the seedling plant. And just also as the soil abounds more or less in decaying vegetable and animal matter, but particularly in azotised remains, the strength and vigour of the culm and blades bear a due proportion, more culms shoot out laterally from the collet, or life knot, as it has been termed, and a fresh number of lateral roots strike off into the soil; so that ere long, there is a tuft of blades and culms upward in the air, with a corresponding tuft of roots downward into the soil. A period, however, soon arrives when this increase in the number of culms and of roots ceases; stooling, or "branching," is over, and the plant rises rapidly into blade and straw, preparatory to the last stage of its growth, fructification and maturation. The healthy and vigorous condition of this growth, is alway indicated by a full and deep green in the blades; where this is not the case, either there are too many plants on the ground, or the soil lacks a sufficiency of azotised matter, to enable the plants to thrive as fully as they ought. This vigour of growth continues until the plants shoot out into the ear, and then growth entirely ceases.

A comparative examination of vigorous plants at this period when vegetation has attained its ultimate object, and built and reared the whole fabrick of the plant's system, shows us one of the two following results. If the roots have but a moderate developement, and there be but a moderate quantity of loose soil in which they can extend themselves, then the vigour of the growth has arisen from the great quantity of nutritive matter within the soil. But if there be a great extent of root, this vigour has been occasioned by their greater range of action. In the former instance, the material has been near at hand, and fewer labourers were therefore required to build the structure. In the latter the material had to be brought from a distance, and, therefore, more hands had to be employed. But were the two soils brought into action to their full limits, then a much nobler structure might be raised by the first than by the second. Much, then, depends upon culture. The capacities of soils have to be brought out, in order that vegetation may flourish to the utmost; and this can only be done by what has been urged again and again as essential, a thorough loosening and pulverising, and consequent aeration of as much soil as exists, or as the implements of husbandry are competent for.

The natural tendency of subterraneous roots, is to shun the light and direct themselves downward into the soil; their natural function is, as they thus descend into the soil, to absorb from it by the extreme tips of the fibrils, or spongioles, the fluid around them with the gaseous and other matter it may contain, and transmit it upward to the parts of the plant above the soil. If the soil be friable and porous, as they descend the more numerous are the fibrils which they emit among these pores, and the more spongioles thence brought into action, and greater supplies sent up to the plant. The vigour of the plant, therefore, proportionably increases, the root participates in this vigour, pushes downward more rapidly, and sends off laterally more ramifications in its course, so that such a supply of material is sent up to the building, that the first foundation does not require all; wings are added, in offsets, or the stooling of new stems, these send down their roots in a similar manner, with similar action, and similar results; a repetition of the same goes on until the space allowed the plant to grow in is filled up, or the stores in the soil drawn upon to the full amount. And whatever interferes with this stooling, whether weeds and grasses, or even grain plants, or too great a number *themselves*, checks vegetation; and consequently diminishes produce.

But if a full aeration be essential *in* the soil, for the multiplication of roots a full areation *on* the soil is equally essential. Upon a free circulation of air around the blades and culms of the plants, and the direct influence of light, depend the digestion and appropriation of the food sent up from the roots. When material is brought to the building it has to be adjusted and built; without this, the other conditions are useless. And so clever an architect is nature, that when she has her own unrestrained course, multiply culms to whatever extent she may, she ever disposes of and arranges them, so that these great and primary functions can be fully discharged. But if man in his ignorance sows his seeds so thick, that this thorough aeration, and this full action of direct light, or sun shine, cannot take place, then the functions of the plants are deranged, the food is not fully digested, and imperfectly appropriated; the plant becomes sickly, structure increases slowly, and nature's plans are all checked, and but partially finished. Weeds have the same effect; they rob the grain plants of air and sunshine, and thereby proportionally impoverish them.

On the physiology of growth, or vegetation, we have but one more subject to mention, and that is, wherever practicable, to encourage growth, by stirring as often as is advisable the soil around plants during the early stages of their growth. Not only does this destroy all the weeds which may have vegetated with the grain, or with the plants cultivated, but it increases the growth considerably, by multiplying the extent of the fibrils of the root. For it is a fact which cannot be too well remembered, that the more you cut off the fibrils of the roots, by stirring and aerating more the soil around them, the more you increase their tendency to multiply. This advantage is especially seen, in harrowing crops of wheat in the spring, after the frosts of winter are ended, and growth has begun. In drill husbandry, however, are the vast advantages of such a course best seen. There you can hoe among your plants, and loosen the surface soil between the drills, without injuring the plants, and the increased vigour after such hoeings is very soon manifest. But it is among the potatoe and and turnip crops where the stirring of the soil is the most marked in its results. There you can use drill harrows to run between the drills to destroy all the weeds and loosen the soil; there you can plough between the drills, once or twice, before the plants are sufficiently grown to be injured by the process. And the result of this is not merely to destroy all the weeds, as was the intention by having recourse to this method, but the increase of roots to an almost unlimited extent, and that too, near the surface, where the action is always most favourable to the cultivator, because it draws most upon the contributions from the air, and thence leaves those within the soil less necessary.

In fine, then, whatever we plant, or whatever we sow, for produce on our farms, let us draw upon the resources of the air to the fullest extent, both within the soil, and out of it; remembering, that our manures, our ploughings, harrowings, and all kinds of labour which we may employ, are dependent upon its action for answering their purposes. All life is maintained by it, and all vigour in every department of animation. It is constantly consuming one thing to form out of its elements another. And we have only to know how to employ it, to compass fully the ends it was intended to answer for us; not merely in supporting the flame of life within us, but in furnishing that flame constantly with supplies of the very best fuel, that life may ever burn brightly, and be in itself, to us, a blessing and an enjoyment. Aerate, aerate, aerate your seeds that they may germinate well. Aerate, aerate, aerate your plants that they may grow well.

AN EXPERIMENTAL INVESTIGATION

OF THE

MAGNETIC CHARACTERS

OF

SIMPLE METALS, METALLIC ALLOYS,

AND

METALLIC SALTS.

BY WILLIAM STURGEON, ESQ.,

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(Read April 7th, 1846.)

SECTION I.

1. THE subject to which this memoir is directed is one that has been already explored at different periods by many eminent experimental philosophers, who have recorded the results of their labours in various works on science; and as several interesting and well-attested facts have been discovered by those investigations, but little would appear to be wanting at this time to accomplish all the desirable information respecting

the magnetic actions of bodies, whether of a ferruginous or a non-ferruginous character. Under these circumstances, any further investigations of mine, or of any other experimental enquirer, could be productive of nothing more than the developement of a few novel facts in addition to those already recorded, or the application of some of them to novel and useful purposes.

2. It frequently happens, however, in experimental inquiries of this kind, that the different modes of investigation resorted to by different philosophers, are not only productive of new facts, but are the means of developing new laws, and of leading to theoretical views differing considerably from those previously entertained for the explanation of phenomena which had long been grafted into the history of science. And it must be acknowledged that, whether new facts be added to the old stock—the application of any of them to useful purposes discovered—or that novel and more exact views for the explanation of those facts be developed, an additional step in the advancement of science would thus be securely established; and it is solely from a hope that by *some* of these means the present memoir will

contribute to the progress of scientific knowledge, that I have ventured to offer it to the consideration of this Society.

3. The superlative degree of magnetic action displayed by metallic iron, above that of all other known bodies, has been a theme of almost continuous contemplation and philosophical speculation from remote periods in the history of science till the present day, and continues to be a subject of interest and admiration throughout every part of the scientific world. The ordinary laws of the magnetic action of metallic iron, especially when in masses, are, however, now so satisfactorily established, and the phenomena so well known, that any further notice of them in this place would be foreign to the object of this memoir; more especially as it alludes to the simple magnetic attractions only, whilst illustrating the novel facts it contains, and the mode by which they were developed.

4. Nickel is a metallic body which, next to iron, stands most distinguished for the display of magnetic action: and, indeed, notwithstanding the number of inquiries that have been made respecting the magnetic action of other bodies, and

the talent and expedients that have been employed in the pursuit, little or nothing has been satisfactorily ascertained beyond that which is so conspicuously displayed by those two metals,—iron and nickel. (7—13.)

5. There is something very remarkable, however, respecting the magnetism of these two distinguished metals when in combination with other bodies. Nickel, for instance, is said to lose all its magnetic action when combined with even a small dose of arsenic, and iron has long been understood to suffer the same fate when alloyed with antimony. Beyond these two alloys of nickel and iron, I am not aware that any other have been magnetically investigated, although, as will appear in the sequel, some of the most extraordinary facts that have hitherto appeared in the magnetism of metallic bodies, are displayed by alloys of iron with other metals.*

6. Tiberius Cavallo was amongst the earliest inquirers into the magnetic action of non-ferru-

* At the time this first part of the Memoir was read, I was not aware of these curious and interesting facts. They were subsequently discovered, and are described in the second part.

ginous metals; but the principal part of his experiments were limited to copper and brass, specimens of both of which he found to be magnetic; and especially after they had suffered the operation of hammering. The investigating apparatus of this philosopher, like that of many subsequent inquirers, consisted of a delicate magnetic needle, to the poles of which the specimens under examination, to prevent commotion in the air, were slowly and dexterously presented.*

* The following are the conclusions at which Cavallo arrived respecting the magnetism of brass:—

"1st.—Most brass becomes magnetic by hammering, and loses its magnetism by annealing or softening in the fire, or at least its magnetism is so far weakened by it, as afterwards to be only discoverable when set to float in quicksilver.

"2nd.—The acquired magnetism is not owing to particles of iron or steel imparted to the brass by the tools employed, or naturally mixed with the brass.

"3rd.—Those pieces of brass which have that property, retain it without any diminution after a great number of repeated trials, viz., after having been repeatedly hardened and softened.

"4th.—A large piece of brass has generally a magnetic power somewhat stronger than a smaller piece, and the flat surface of the piece draws the needle more forcibly than the edge or corners of it.

"5th.—If only one end of a large piece of brass be ham-

7. As brass in an alloy so extensively employed in the construction of magnetic compass-boxes,

mered, then that end alone will disturb the magnetic needle, and not the rest.

"6th.—The magnetic power which brass acquires by hammering has a certain limit, beyond which it cannot be increased by further hammering. This limit is various in pieces of brass of different thicknesses, and likewise of different qualities.

"7th.—Though there are some pieces of brass which have not the power of being rendered magnetic by hammering, yet all the pieces of magnetic brass that I have tried lose their magnetism, so as no longer to affect the magnetic needle, by being made red hot, excepting, indeed, when some pieces of iron are concealed in them, which sometimes occurs; but in this case the piece of brass, after having been made red hot and cooled, will attract the needle more forcibly with one part of its surface than with the rest of it; and hence, by turning the piece of brass about, and presenting every part of it successively to the suspended magnetic needle, one may easily discover in what part of it the iron is lodged.

"8th.—In the course of my experiments on the magnetism of brass, I have twice observed the following remarkable circumstance.—A piece of brass, which had the property of becoming magnetic by hammering and of losing the magnetism by softening, having been left in the fire till it was partially melted, I found upon trial that it had lost the property of becoming magnetic by hammering; but having been afterwards fairly fused in a crucible, it thereby acquired the property it had originally, viz., that of becoming magnetic by hammering.

its magnetic or non-magnetic condition is an important scientific inquiry, which, though for many years in the hands of philosophers, remains at this day as undetermined as when first undertaken. That certain pieces of brass have displayed unequivocal magnetic action is a fact which cannot be questioned, but whether that action was due to the alloy of pure copper and zinc alone, or to portions of iron accidentally present in the metal, different opinions have been entertained.*

"9th.—I have likewise often observed, that a long continuance of a fire so strong as to be little short of melting hot, generally diminishes, and sometimes quite destroys, the property of becoming magnetic in brass. At the same time the texture of the metal is considerably altered, becoming what some workmen call *rotten*. From this it appears that the property of becoming magnetic in brass by hammering, is rather owing to some particular configuration of its parts than to the admixture of any iron; which is confirmed still further by observing that Dutch plate brass (which is made, not by melting the copper, but by keeping it in a strong degree of heat whilst surrounded by *lapis calaminaries)* also possesses that property."

* During the interesting series of experiments carried on by Professor P. Barlow, on the magnetism of ferruginous bodies, the brass compass-box, and several brass screws, belonging to one of the finest looking instruments employed, were found, by that philosopher, to be highly magnetic.—*Barlow's Magnetic Attractions*,—Second Edition, P. 17.

8. It has been supposed by Cavallo and other philosophers, that all bodies, whether metallic or otherwise, are endowed with magnetic powers, which vary considerably in degrees of energy, whilst under the influence of, or operating on, the magnetic needle. But when we meet with such conflicting opinions as those that appear in the writings of philosophers so eminent in this department of physics as Cavallo, Coulomb, Bennet, Haüy, Biot, Becquerel, and others who have entered this field of research, we are necessarily led to infer that the subject has not yet been accurately and satisfactorily determined.

9. The beautiful experiments of M. Arago, and the final developement of magnetic electricity by Dr. Faraday, afford an ample explanation of nearly all those experiments in which vibrations of the magnetic needle, near the bodies under examination, were taken as evidence of their magnetic actions; as well as in all those cases in which light needles of the bodies examined were vibrated under the influence of powerful magnets. It is reasonable to suppose, also, that thermo-electric currents would influence the results of those experiments which were made previous to the discovery of that branch of electricity by Dr.

Seebeck, especially in those cases in which the bodies under examination were held in the hand whilst presented to the magnetic needle.

10. There are, however, some phenomena on record the explanations of which do not appear to fall within the range of the laws either of magnetic-electricity or thermo-electricity; and, therefore, the cause of their developement is necessarily located in some other source. For instance, when Coulomb employed light, delicately suspended needles of gold, silver, glass, wood, and other substances, both organic and inorganic, he found them obey the polar forces of a magnet in precisely the same manner as needles of iron would do; for after the vibrations had ceased, those needles became arranged, between the north and south poles of powerful magnets, in such manner, that their axis rested in the line of magnetic force, or in a right line joining the magnetic poles employed.

11. It is somewhat remarkable that, when similar experiments were made by M. Becquerel, the results were very different. By employing needles of wood, lac, and some other substances, this philosopher found that the positions they

assumed when at rest directly between the north and south poles of powerful magnets, were invariably at right angles to a right line joining those poles; and, consequently, at right angles, relative to the magnetic forces, or to the position in which the needles of Coulomb rested. From the results afforded by the experiments of M. Becquerel, that philosopher has been led to consider that the effects produced by a strong magnet on a magnetic needle, or on soft iron, differ essentially from those which take place in all bodies whose original magnetism is very weak. In the former the magnetic axis of each is arranged in its *length;* but in the latter class of bodies the magnetic axis becomes arranged transversely. M. Becquerel shows, however, that wooden needles assume different positions, with respect to the magnetic poles, according to the distance at which they are placed from them.*

12. In a paper by Dr. Faraday, read before the Royal Society of London, in January last, it is stated, that a variety of bodies, bismuth being the most eminent in this respect, arrange themselves, with regard to powerful magnetic poles,

* Traité Experimental de l' Electricité et du Magnetisme. Tom. II, p. 387.

in precisely the same manner as described by Becquerel; that is, with their longest axis at right angles to the line of magnetic force.

13. Dr. Faraday has attempted a classification of a great number of bodies under the two following heads:—*Magnetics* and *Diamagnetics*. The former class, of which iron is the grand type, become arranged, whilst under magnetic influence, with their longest axis in the magnetic line of force; and the latter class, of which bismuth is the type, become arranged at right angles to the magnetic line of force. In the magnetic class, Dr. Faraday places some of those bodies which, according to M. Becquerel's nomenclature, would be placed in the other class, or amongst those which become arranged at right angles to the line of magnetic force. Such, however, is the condition of this interesting inquiry at the present time, no two of those hitherto engaged in it having arrived at similar results in any series of experiments that have been undertaken.

14. The inquiries that I have made in this department of magnetics have been conducted partly by the employment of magnetic needles, partly by permanent steel magnets, and partly by

electro-magnets, which have afforded different modes of assailing those substances that were the objects of investigation.

15. In all cases where delicate magnetic needles are employed, especially when the suspension is by means of a fibre, and the system astatic, the experiments are exceedingly tedious, and much time is required to allow of the system's repose from its agitations before an attempt can be made to approach it with the specimen to be examined. When, however, a single needle is employed, whose support is a finely pointed pivot, the experiments are less subject to delay than by the other mode, though much caution and some dexterity are still required to enable the experimenter to arrive at satisfactory results. But in whichever way the magnetic needle may be suspended in these delicate investigations, the bodies under examination must either be held immediately in the hand, or indirectly, by means of some other body previously ascertained to have no influence on the needle. If held in the hand, thermo-electric currents are to be suspected; and if attached to the end of a wooden rod, by means of sealing wax, or resinous cement, other electric actions may interfere with the results; or may,

indeed, be the sole cause of any motions that may happen to be observed. Moreover, a delicate magnetic needle does not possess a sufficient degree of power to bring into play the minute portions of magnetism that lie dormant in many bodies. These exiguous sleeping forces can never be roused into a state of activity, and, consequently, can never be discovered by merely presenting the bodies in which they reside to the pole of a feeble magnetic needle. To accomplish their discovery a comparatively powerful magnetic action is absolutely required; for, when thus assailed, their polarization is more easily enforced, and their detection almost certain. The magnetic needle, however, may be usefully employed in cases where the suspected magnetism of a body is of some easily detected amount; and it may be resorted to with advantage, in preliminary trials, under all circumstances, because of the possibility of the specimen under examination possessing a sufficient amount of magnetism to be detected by it, and the more tedious modes of inquiry being thus rendered unnecessary.

16. I have found that a convenient and efficacious mode of examining bodies the magnetic actions of which are very feeble, and others

in which magnetism has but a questionable existence, is by means of an apparatus represented by the accompanying figures.

FIG. 1.

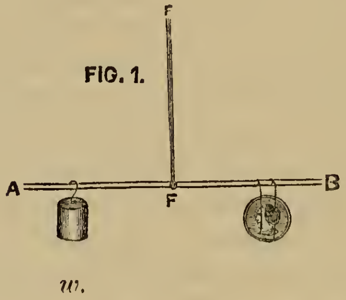


FIG. 2.

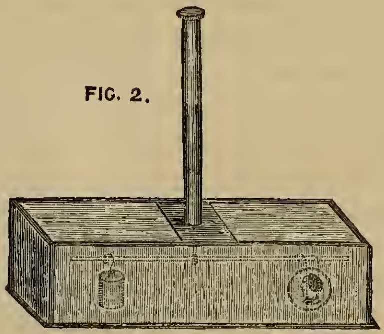


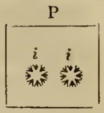
Fig. 1 is that part of the apparatus in which the specimens to be examined are placed. It consists of a light cylindrical wooden rod A B, about twelve inches long, and suspended by a few parallel fibres of silk F F, from the cocoon. The end B is furnished with a light slip of card paper and two loops of horse-hair, for the purpose of holding the specimen; say a half-crown, for instance, as represented in the figure, which is counterbalanced at the other end of the lever by a sliding weight *w*. This part of the apparatus is enclosed in a rectangular box, Fig. 2, whose ends, top, and one of its sides, are of glass, and a brass tube rises from the middle of the top, in which hangs the silken fibres. The head of this tube sustains the fibres and their appendages, and can be turned in any horizontal direction for the adjustment of the lever to a parallelism with the sides of the box.

The glass parts of the box are sustained by a light mahogany frame, with a bottom of the same kind of wood. The ends and sides are fixed, but the top, which consists of two sliding parts, can be removed at pleasure, for the purpose of introducing the hands for the adjustment of the apparatus within, and replaced when the specimen has been accurately counterpoised, a process which is still further facilitated by the introduction of the hand at one side of the box, which is opened for that purpose, and afterwards closed by a sliding mahogany door.

When the agitations of the lever have subsided, the sliding door is partially opened for the introduction of the poles of a powerful steel horse-shoe magnet, which is made of a long and narrow shape for the purpose. This magnet is placed on a sliding carriage, by means of which it is made to approach the specimen, or recede from it, with great facility and in the most gentle manner. In consequence of finding decided polarity in some specimens in which no magnetism was previously known to exist, I have been led to the employment of a powerful bar magnet, which I find convenient in those cases where such polarity is suspected. I have also employed electro-magnets, both straight and of the horse-shoe form; but having found that these are troublesome and inconvenient, I have abandoned the use of them altogether in these inquiries.

In cases where extreme nicety is required, I have found the following mode exceedingly useful.

Besides the sliding door which closes one side of the box, I have another sliding piece P, which fits into the same grooves in the side of the box. Through this



piece pass two cylindrical rods of soft iron, *i i*, about two inches in length. They are firmly fixed in the wooden slider at their middle parts, and parallel to each other. When this piece is in its place the iron rods are in the same horizontal plane, having one half within and the other half outside the box, and their inner ends presented to the specimen suspended on the lever. When the remaining portion of that side of the box is closed by the sliding door, the specimen is nicely adjusted to the ends of the iron rods by turning the top piece of the tube, until the most trifling space is perceptible between them. When all is at rest the poles of the horse-shoe magnet are made to approach the outer ends of the iron rods, bring them into magnetic action, and thus detect the magnetism of the specimen if any exist in its structure.

17. By the assistance of this apparatus, to which I give the name *Torsion Magnetoscope*, I have examined gold, silver, copper, platinum,

tin, antimony, lead, zinc, bismuth, and mercury; and also some of their alloys and salts. In none of these metals, when in a state of purity, have I been able to discover the slightest trace of magnetic action, though in several specimens of some of them, as they appear in a commercial state, magnetic action is strongly developed.

18. A bar of bismuth, for instance, cast from a mass fused in an earthenware crucible, was found to be highly magnetic, and, for a while, was considered as a good specimen of the magnetic action of that metal; but on examining another bar cast from the remaining portion in the crucible, and finding it still more powerfully magnetic than the former, a suspicion was aroused that either their crystalline structures were different to each other or that the metal was not pure. The experimental inquiries which this suspicion occasioned, led to the detection of localities in the two bars in which the magnetic actions were more powerful than in other parts of them, which gave rise to the determination of *sweating* one of the bars at a low heat, and running out of the crucible the most easily fused portions, before the rest became fluid, which is an excellent process for freeing

pure bismuth from some of the impurities with which it is frequently contaminated in the mercantile state.

19. This purified bismuth having been cast into a bar, was afterwards broken into convenient fragments and tested by the *Torsion Magnetoscope*, previously described (16), but not the slightest trace of magnetism could be detected in any of the pieces.

20. Having satisfied myself that no magnetic action resided in the pure bismuth, the dross left in the crucible was softened by heat and poured on a stone slab, and on being tested developed high magnetic powers. It now became obvious that the whole of the magnetism displayed by the bismuth, when in its first state (17), was due to that portion only which was left as dross in the crucible after the pure metal had been run out.

21. From the results thus arrived at I was induced to fuse other portions of mercantile bismuth, and run out the purest portions of the metal at the lowest degree of fusible heat, by which means I have been enabled to separate the magnetic from the un-magnetic portions, and

thus to arrive at the conclusion that pure bismuth is not susceptible of any *direct* magnetic action by the mere approach of the poles of a powerful magnet, which I consider a test of far greater certainty and exactness than that of a feeble magnetic needle, whatever may be the delicacy of its suspension ; and as a peculiar class of phenomena become displayed by a sudden developement of the powers of an electro-magnet, much misconception might rise from its employment.

22. The most usual impurities of bismuth of commerce are sulphur and arsenic, and, occasionally, a small portion of silver and iron. On subjecting the drossy part (19) to dilute sulphuric acid a portion was dissolved ; after which the liquid was reduced almost to dryness by evaporation. The residue being diluted with water, and a solution of ferrocyanuret of potassium being added, it assumed a blue colour, which indicated that a portion of iron had been dissolved from the mass. On dissolving another portion of the dross (19) in dilute sulphuric acid, the presence of iron was again indicated by the addition of bruised gall nut. Hence it was fair to infer that the whole of the magnetic action displayed by the mass was due to the iron it contained.

23. I have examined antimony in the same way, and have found some specimens magnetic, and many others in which magnetic action could not be detected. By thus operating on antimony, however, it would be impossible to form a correct idea of the magnetic, or unmagnetic state of that metal; because, as will appear in the sequel, of its masking, to a considerable extent, the magnetism, of even considerable proportions, of iron, when the two metals form a perfect alloy. Hence, in order to test antimony magnetically, it becomes necessary to ascertain, by chemical processes, that it is perfectly pure, and especially that it is free from iron; for, although antimony will mask the magnetism of iron when in perfect union with that metal, a very trifling proportion of uncombined iron will render the whole mass *apparently* magnetic. By attending to these particulars, pure antimony will not be found to display any magnetic action. (17).

24. It is generally understood, principally I believe, upon the authority of Dr. Seebeck, of Berlin, that iron becomes "completely destitute of magnetic action" when alloyed with four times its weight of antimony.* This, however, does

* Brewster's Magnetism, page 102.

not appear to be correct, for I have formed very perfect alloys of these two metals, in a great variety of proportions, and find that when the iron does not form even a twentieth part of the mass, it is still magnetic, though in a very low degree. When the alloy is of equal parts of iron and antimony, it is highly magnetic. This alloy, when broken, exhibits a dark grey fracture, somewhat glittering. It is easily reduced into powder by the operation of a file, or by pounding in a mortar; and what is very remarkable, it yields an abundance of deep crimson sparks when struck against hard steel.

25. It has already been stated (17), that pure copper is not magnetic; and I must now add that, in a very few cases only, have I detected magnetic action in the copper of commerce, although I have tested a great number of specimens both in the state of sheet and of wire.

26. In the copper coinage of this country, I have never yet met with magnetic action, notwithstanding the number of experiments I have made on the various copper coins that have been struck in the reigns of several Sovereigns.

27. In the gold and silver coinage, however, in which copper forms a constituent part, the case is very different. These alloys are nearly all of them decidedly magnetic, and, probably, none of them entirely free from magnetism. The gold coinage, however, displays much feebler magnetic action than the silver coinage; indeed in many gold coins the existence of magnetism may be considered as questionable, whilst in others, and especially in those of 1844, magnetic action is prominently displayed.

28. The silver coinage, although in some specimens scarcely any magnetic action can be detected, is generally magnetic in a very eminent degree; and I have found that, when any one piece of a particular coinage displays considerable magnetic action, the whole of that coinage, as far as I have examined it, is similarly magnetic. And, on the other hand, when a silver coin has been found to be but very slightly magnetic, I have but rarely met with one of that particular coinage in which any considerable degree of magnetism could be detected.

29. Of the silver coins that have come under my notice, a half-crown of William and Mary,

dated 1691, is the most eminently magnetic. The next, in point of magnetic action, are the half-crowns of 1844 and 1845; then one of George IVth, the date of which I have not noted. The half-crowns of George III., of 1819 and 1820, are more slightly magnetic than those last named, and the half-crowns of both coinages in 1817 are still less magnetic than those of 1819 and 1820. Shillings, also, of certain coinages, are magnetic in an eminent degree, and there are but few, if any, that I have examined, that are entirely neutral to the high magnetic powers with which they have been assailed.

30. Silver articles for domestic purposes, such as spoons, prongs, fruit knives, &c., were, in many specimens, found to be much more magnetic than any of thesilver coins that I have examined. I have borrowed several sets of silver tea-spoons from neighbouring families, and, with the exception of one half-dozen of Scotch spoons, of a very old date, all have displayed high magnetic powers, though of very different degrees in different sets. But what is very remarkable, if one individual spoon was found to be highly magnetic, the whole of that particular set, whether it consisted of half a dozen or a dozen spoons,

were highly magnetic also. And, generally, whatever might be the magnetic condition of any individual spoon, the whole number of the set to which that spoon belonged were magnetic alike, or very nearly so. Hence, if the quantity of magnetic action of any individual spoon were to be denoted by q, and the number of spoons in the set denoted by n, the sum total of magnetic action in that set of spoons would be $n\ q$, nearly. Of course, this reasoning applies only to individual sets of spoons which are of uniform make, composition, and structure of metal. It appears, also, as far as my experience has extended, that the same mode of reasoning would give the sum total of all the magnetic action that any individual coinage would display. Suppose, for instance, the magnetism displayed by a half-crown piece were to be taken as the unit of quantity equal q, then the number of pieces being n, the sum total of magnetism which the whole of that coinage would display would be $n\ q$, nearly; and, similarly, for any other coinage of silver.

31. The difference of magnetic action displayed in the silver coinage and domestic articles of that metal (29,30), led to the supposition that minute portions of iron might accidentally have got

introduced to the alloys whilst in a state of fusion, which had some probability in its favour, from the fact that the metal for silver coinage is fused in cast iron pots,* and, therefore, liable to take up a portion of those vessels. But, on the other hand, if that were always the practice, it would lead to the inference that in all the silver coins the iron would be nearly in the same proportion, and the extent of magnetic action almost the same in all. Whereas, by the tests already described, this is not the case.

32. The current silver coinage of William and Mary became so base, that in the year 1694 it was all called in, and a new coinage issued. From this fact it occurred to me that there was a possibility, at least, that the high degree of magnetic action displayed by the half-crown of 1691 (29), was owing to an undue proportion of copper, or of some other inferior metal. This idea led to the selection of a shilling, in which scarcely a trace of magnetism could be detected, for fusion with an additional portion of copper, also non-magnetic, having been obtained by the electro-type process. These, together with a

* Ure's Dictionary of Arts and Manufactures. Brande's Chemistry, page 1037.

piece of pure silver, were fused in an earthenware crucible, and run out upon a sheet of copper. The copper in this alloy was about one to five of silver, which is more than twice the proportion of that in the standard coinage. On subjecting this mass to the *torsion magneto-scope*, it was found to be more highly magnetic than the old half-crown of William and Mary (29.)

33. This singular result has cost me much thought and a great deal of trouble. The crucible employed was quite clean, having never been used before; and its contents during the time it was in the fire were the silver and copper, and a mixture of pulverized charcoal and common salt. Similar pieces of charcoal and slices of the same quality of salt have been tested, but no magnetic action could be detected in either. Whence, then, this almost unexpected magnetism in the metallic alloy? Fragments of a broken crucible similar to that used were found to be slightly magnetic, probably from a portion of iron in the clay of which it was formed: but the magnetic action in the fragment of the crucible was not nearly so great as that displayed by the alloy. Moreover, the pure portion of the silver that entered the alloy had previously been fused in a

similar crucible (one of the same nest,) and with a similar mixture of pulverized charcoal and salt, and yet showed no trace whatever of magnetic action. Hence, it could hardly be imagined that the magnetism displayed by the alloy was due to iron derived from the crucible.

34. After subjecting a portion of the alloy (32) to dilute nitric acid, and finding no iron in the solution, the surface of the metal was washed in clean water and thrown into dilute sulphuric acid, which, upon the principles of electro-chemistry, took up a portion of the copper ; and had iron been present, would have taken it up also. But no indication of that metal appeared by the test of gall nut, nor by that of ferrocyanuret of potassium ; but the formation of ferrocyanuret of copper was manifested in an eminent degree, though previously to the introduction of the ferro-cyanuret of potassium, scarcely any colour was perceptible in the liquor.*

35. From the facts above described (32, 33, 34), and, at present, I rest on no other data, I

* Another portion of the alloy has since been analysed, with similar results.

am inclined to think that if any iron could possibly have entered the alloy, its quantity must have been too small to cause the high degree of magnetic action which the specimen exhibited.

36. On comparing this alloy of silver and copper, with the alloy of iron and antimony, in which the weight of the latter metal is only about twenty times that of the iron (24), some very remarkable circumstances present themselves. In the former alloy, where no iron can be detected by the usual chemical tests, we have a metal whose magnetic action is, at least, twice as powerful as that displayed by the alloy of iron and antimony, an alloy of which iron constitutes a very considerable proportion, and whose presence, had it not been previously known, could have been detected by the humblest test for ferruginous matter. These parallel experiments tend to show either that an alloy of pure silver and pure copper is magnetic, or that the magnet is a better test for the presence of minute portions of iron, in such alloys, than any hitherto known in chemical manipulations.

37. I regret that another piece of unmagnetic silver has not yet fallen in my way, to enable me to make further investigations on this curious

subject. But I am in hopes of obtaining unequivocal results before the second part of this memoir is brought before the Society.

38. With respect to brass, one of our most important alloys, I have found it to be highly magnetic in a great number of cases; viz., in all the various states of newly cast brass, brass wire, and sheet brass; as well as in several articles of brass manufacture. But, from the very great difference in the degrees of magnetic action displayed by different specimens of this beautiful alloy, and the total absence of that action in others, there has appeared to me a high degree of probability that the magnetism displayed by brass is due to accidental portions of iron in the alloy.

39. Cavallo discovered that in magnetic brass the action was more powerful in large pieces than in small ones. This fact I have also observed in several specimens that I have examined. The reason seems to be that in the larger pieces a greater quantity of ferruginous matter (if present), is brought into operation than in the small pieces.

40. Cavallo also states, that hammering an un-

magnetic piece of brass will cause it to become magnetic. An instance of this kind I have never yet met with: but I have found that when an unhammered piece has been so slightly magnetic as to have that character but just discernible, hammering it so as to compress its two sides closer together, gives it an increased magnetic action; which may possibly be a consequence of bringing the whole of its magnetic particles more completely within the range of the testing magnetic influence; and I am inclined to believe that, had Cavallo's test been more powerful than a magnetic needle, he would have found that those pieces whose magnetism he thought was due to compression alone, were slightly magnetic previously. Still, however, there is a possibility that magnetism might be detected in compressed brass, in which that power is too feeble to be detected whilst the metal is in an uncompressed state, even by powerful magnetic tests.

41. In addition to the advice given by Cavallo, respecting the necessary caution in employing brass in the construction of compass boxes (note to 6,) I should advise the makers of those useful instruments to test every piece of brass, intended to be employed in their construction, by a powerful

magnet, instead of testing them in the usual way by means of a delicate magnetic needle. And if this test be accurately performed when the metal first arrives from the foundry, the detection of any concealed magnetism, even if very feeble, will be almost certain; and much of that labour and uncertainty, which must always attend examinations by the needle, would be avoided. Unfortunately, however, too much reliance is usually placed on the mere appearance of the brass, or on the character of the foundry whence it is procured, and the consequence is, that but very few brass compass boxes that are in common use are entirely free from magnetic action.

42. The next alloy of importance that I have examined is German silver, in which nickel is one of the principal constituents. In the best kind of German silver, (constituted of copper eight parts, nickel six, and zinc three), a slight magnetic action has been detected, but in the inferior kinds of German silver, into which only about three parts of nickel enter, I have not detected any magnetic action whatever. Hence the magnetism of that portion of nickel is obviously neutralized in that particular alloy.

43. The metallic salts that I have examined, are some of those most frequent in common use. The salts of iron were the sulphate, the yellow and the red ferrocyanuret of potassium, also Prussian blue. These, with the exception of the yellow prussiate, are magnetic; the sulphate of iron in the highest degree of any of them. It is somewhat remarkable that the two kinds of prussiate of potash, where the proportions of iron are so nearly alike, (yellow 15 per cent, red 16 per cent,) should display such a material difference in their magnetic characters. And it is still more singular that Prussian blue, which contains more than 45 per cent of iron, is less magnetic than the red prussiate of potash. The sulphate contains about 33 per cent of iron, and is the most magnetic of the whole.

44. Pure sulphate of copper shows no magnetic action, but that of commerce is highly magnetic, being, as I have ascertained, adulterated with sulphate of iron. Hence, the magnet would be a good and speedy test for the quality of the commercial salt.

45. The following salts appear to be perfectly neutral to magnetic forces:—Common salt, salt-

petre, borax, sulphate of magnesia, sulphate of soda, sulphate of potash, carbonate of potash, and carbonate of soda.

46. Thus far I have attempted to contribute to the list of facts previously known, and to correct some errors which, probably on account of the inefficient modes of investigation, have crept into this particular branch of science. It is possible also, I think, that some of the facts which have now been pointed out, may be an inducement to employ the magnet more extensively than hitherto, in the laboratory of the chemist.

SECTION II.

(Read May 5th, 1846.)

47. In addition to the facts enumerated in the first section of this memoir, further investigations have led to the discovery of others of a no less interesting character. By extending the examination of British silver coins, I find that the whole of them are more or less magnetic. The half-crowns of the present reign, that have come under my observation, are certainly all magnetic, and some of them display considerably high magnetic powers, more especially those of the years 1842, 1844, and 1845; and a Victoria shilling, coined in the year 1842, is still more magnetic than any of the half-crowns.

48. The half-crowns of George the Fourth are, in general, highly magnetic, though but very few of them display such high magnetic powers as some of those of William the Fourth. There is something remarkable in the following fact:—I have not met with any of the half-crowns of

George the Third that are so powerfully magnetic as those of subsequent coinage.

49. With respect to silver ornaments, silver medallions, and silver articles for domestic purposes, they differ materially in their magnetic characters. But, generally, they are more highly magnetic than the British silver coins. (30.)

50. The large silver medal of the Society of Arts, for the year 1825, weighs nearly two and a half ounces; yet it does not display even the slightest degree of magnetic action, by the most severe test to which I have subjected it; whilst a small silver medallion of the Commonwealth, representing Lord Essex on one side, and the two houses of parliament on the other, was found to be more highly magnetic than any of the previously named coins. A small silver medallion of Charles the Second, exhibited a slight degree of magnetic action.* An Indian rupee that I examined, showed no magnetic action whatever.

51. The present gold coinage of this country is, in general, but feebly magnetic. I have met

* For the use of these medallions, and the Indian coin, I am indebted to my friend G. Wareing Ormerod, Esq.

with a few sovereigns of the present reign which are more magnetic than any others that have come under my notice.

52. With respect to jewellery, it is generally more highly magnetic than those articles of silver that have come under my examination. Wedding rings, which contain but a small proportion of copper, have so slight a degree of magnetic action as almost to elude the detection of it: whilst ornamental rings, keepers, &c. which contain a much greater proportion of copper, are, generally, highly magnetic. Some ear-rings that I have examined, are still more magnetic than the finger-rings. Gold watch chains are generally magnetic, especially those containing much copper: also gold spectacle frames, unless they be of what is called fine gold, are magnetic to a considerable extent. In all cases where steel or iron screws, or nails, have been found in the gold articles examined, those parts have been carefully removed previously to the magnetic test being applied.

53. We next come to the consideration of metallic alloys, of which either iron or nickel form no inconsiderable proportions. It has already been shown in the first section of this memoir

(24), that antimony, when alloyed with iron, counteracts the magnetic action of the latter metal in a very eminent degree, rendering it almost undetectable when the iron amounts to little less than a twentieth part of the mass: and I find that when the ferruginous metal amounts to no more than about one fortieth of the mass, its magnetic powers entirely disappear.

54. There are several other metals, besides a number of other bodies, which either partially or wholly neutralize the magnetic actions of iron and nickel. The most eminent of the metals in this capacity is zinc. This metal, which, till these researches were undertaken, was not known to affect the magnetism of iron, neutralizes nearly the whole of that power when alloyed with an equal proportion of the ferruginous metal. And although from an accident with the melting pot I have not yet arrived at the fact, I have no doubt whatever that, when the iron amounts to no more than one quarter of the mass, its alloy with zinc will be perfectly neutral to the magnet.*

* Since this paper was read I have ascertained that an alloy of iron and zinc in the proportion of 1 to 7 respectively, is quite destitute of magnetic action.

55. The neutralization of magnetism in iron by alloying it with zinc is a fact of high importance in the contemplation of metallic magnetism: and especially the magnetism of brass, and other alloys, in which zinc forms a considerable proportion. For it is highly probable, that since zinc smothers the magnetic influence of large proportions of iron, a considerable quantity of the latter metal might enter the composition of brass, without rendering it palpably magnetic. Such in fact, would absolutely be the case, provided the alloy were perfect, and that the copper had no influence on the magnetic condition of the combined iron and zinc.

56. To satisfy myself on this point, I have subjected to chemical analysis, some of those specimens of brass which had been found to be highly magnetic: and, as far as I have proceeded, there appears no reason to suppose that the magnetic powers they displayed were due to iron in their composition. Indeed, I am now inclined to embrace the opinion of Cavallo: (6, note) that the magnetism of brass is not due to ferruginous matter: but depends upon a suitable arrangement of the particles of its proper constituent metals, copper and zinc. Nor do I believe that brass

generally, as it leaves the foundry, contains any notable quantity of iron. I have analysed many specimens, both magnetic and unmagnetic, and the traces of iron, where any were discoverable, were very minute, and as frequent in the one kind as in the other. It is true that some specimens of brass contain more than an average proportion of iron; but it is a curious fact that these specimens are not those which display the greatest magnetic powers.

57. We learn, also, from these facts, that the demagnetizing powers of antimony and zinc will necessarily prevent the detection of small proportions of iron in those metals, even by the aid of powerful magnetic forces, and leave us in uncertainty regarding their purity when examined by this test alone.

58. Lead and iron do not easily unite into a perfect alloy, excepting when the ferruginous metal is in very small proportions; but when thus combined, the iron loses a great part of its natural magnetic qualities.

59. Silver and copper unite very sparingly with iron; but whether the magnetic powers of

the latter are affected by its union with those metals, or not, is not yet known.

60. In order to ascertain the exact quantity of pure metallic iron, that would render a neutral half crown* *apparently* magnetic, to the same extent as another half-crown, was *absolutely* magnetic, I attached to the former, by means of softened gum, new iron filings; and after many trials ascertained that the requisite quantity of filings amounted to about a ten thousandth part of the mass. And on changing the pure iron for the peroxide of iron, a 480th part of the mass was required to render it equally magnetic with the standard half-crown. Now, as more than two-thirds of this oxide is iron, it follows that, in this state, the iron loses a considerable portion of its magnetic powers; and that the proportion of iron required in this case, to produce the standard degree of magnetism to the mass, was little short of a seven hundredth part.

61. Now, if any iron existed in the standard magnetic half-crown, uncombined with the other

* This piece of coin was not entirely devoid of magnetic action, but it approached nearer to a state of neutrality than any other I then had. Its action was very feeble indeed.

metals, it must have been in a state of peroxide,* and that more than an eight-hundredth part of the half-crown must have consisted of iron, if its magnetism were due to the presence of that metal.

62. Again: the magnetic action of this half-crown was considerably more feeble than that of the alloy which has been chemically examined; and in which, if iron were present at all, that metal was in a less proportion than a twenty-thousandth part of the mass, which proportion, in a state of peroxide, and divided as it necessarily must have been through the whole alloy, would scarcely yield the slightest perceptible magnetic action. Moreover, if iron to that amount were even pure or uncombined, its quantity was far too small for the display of those high magnetic powers of which it was obviously possessed. And, as there is a probability, at least, that the magnetic powers of iron become deteriorated by an alloy of that metal with silver or with copper, or both, there is not the slightest reason for supposing that the magnetism of the alloy in question (32) was due to any iron that it could possibly contain. Nor do I believe

* It is possible that the iron, if any, might be in the state of a carburet; but even in this condition much of its magnetic powers would be neutralized.

that the magnetic actions displayed by the coinage are traceable to the presence of iron.

63. It has already been stated (42) that the magnetic action of nickel is considerably neutralized when combined with zinc and copper in the alloy constituting German silver. Since that part of this memoir was read before this society, I have had an opportunity of alloying nickel with zinc alone, and have ascertained than when the zinc is about eight or ten times the quantity of nickel, the alloy is perfectly neutral to the magnet. This alloy has a zinc-coloured fracture, and partially crystallized in the manner of zinc; but it is extremely brittle and easily pulverized in a mortar.

64. Nickel and antimony combine with facility and in an extraordinary manner. If two pieces of the metals, one of each, be placed side by side in the crucible, so as to touch one another, especially at their upper ends, the moment the antimony assumes a dull red heat, even a lower heat than that which commences its fusion when alone, the nickel bursts out into a fine scarlet glow, fuses and spreads over the antimony in a beautiful fluid state, and insinuates itself into the pores of that metal, rendering the whole mass soft like paste or butter. If, whilst in this state, the crucible be

removed from the fire and permitted to cool gradually, the fracture of the button of this alloy, when broken, is of a much lighter colour than that of antimony. It is of a light grey, and very imperfectly crystallized. It is not so brittle as antimony, though still pulverable in a mortar. When one-fourth of the mass is nickel, the fracture is very compact, and not unlike that of fine steel, but of a lighter colour. With these proportions the alloy is somewhat malleable, and can be cut by a cold chisel.

65. From a retrospection of the facts developed by these researches in connection with those previously known, we are led to observe a material difference in the magnetic characters of bodies when in their simplest or natural conditions; and that these natural magnetic characters become considerably modified when the simple or elementary bodies are variously combined; some simple bodies losing their natural magnetic properties, and others displaying a new magnetic action of which, before combination, they appeared to be destitute. Under these circumstances it would be difficult to ascertain the line of demarcation between those bodies that are naturally and separately magnetic and those that are not. Probably the safest way would

be to allow all bodies to possess, more or less, of the magnetic character; and to classify them into those that are *palpably* magnetic, like iron and nickel: and those that are but obscurely magnetic, or whose magnetism is not detectable in their individual states, but which become magnetic by combination.

66. Provisionally, therefore, we might venture to call the former class *Sapho* magnetics*, and the latter class *Asapho† magnetics*.

67. *Sapho-magnetics* might be conveniently subdivided into *Mono‡ magnetics* and *Suno§ magnetics*, accordingly as they consist of individual or of compound bodies. Then, as we have many bodies which counteract the highest magnetic powers of simple bodies, these might be called *Kato-magnetics*,‖ because many of them, if not all, have the power of completely neutralizing the magnetic actions of other bodies.

* Σαφα. Clearly, manifestly.

† Ασαφῶς. Indistinctly, without clear evidence or marks.

‡ Μονος. Alone, single.

§ Συν. Together, or, Συναιρω. To co-operate.

‖ Κατα. Opposite to, to make disappear.

68. The *Mono-magnetics* at present known are but few in number, iron being the grand type. Next to iron is nickel. Cobalt is also a mono-magnetic body, and, at present, completes the list of this class of magnetics.

69. In the *Suno-magnetic* class I place alloys of copper and silver, copper and gold, and copper and zinc; and, although these three are the only ones with which we are yet acquainted, I have no doubt that many more alloys will soon find a place among suno-magnetics.

70. The *Kato-magnetics* are very numerous, as this class includes all bodies which, by combination, impair the magnetism of other bodies. Amongst the metallic *Kato-magnetics*, zinc is the most powerful hitherto ascertained. Next to zinc is antimony. Then lead and tin. Arsenic, probably, stands very high in this class, but I have had no opportunity of ascertaining its proper place. The non-metallic *Kato-magnetics* are sulphur, oxygen, cyanogen, chlorine, carbon, and the generality of those bodies which combine with the metals.

71. In proposing this classification of magnetics

I have aimed at nothing further than an abstract of that which absolutely takes place in nature. The whole rests upon facts, most of which have their analogies in electricity. All bodies are known to possess electric properties, but differing in degrees of power, and the compounds display very different electric powers to those of the simple constituents. The electro-magnetic powers differ in different bodies, both simple and compound, as decidedly as the powers which are purely electric. Therefore this classification may be considered as supplying a small portion of an extensive nomenclature that has long been wanting in this region of science.

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THOMAS HOPKINS.

WILLIAM FAIRBAIRN, M. INST. C.E.

AN

ALPHABETICAL LIST

OF THE

ORDINARY MEMBERS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY

OF MANCHESTER,

OCTOBER 10th, 1846.

	Date of Election.
James Ainsworth	January 25th, 1805
Ralph F. Ainsworth, M.D.	April 30th, 1839
Thomas Ashton, M.D.	October 29th, 1824
Thomas Ashton	August 11th, 1837
Thomas Atkinson	January 22nd, 1813
John Atkinson	January 27th, 1846
Richard Parr Bamber	October 19th, 1821
Henry Bannerman	April 30th, 1824
Robert Barbour	January 23rd, 1824
James Barratt, jun.	January 24th, 1834
Joseph Barratt	April 19th, 1842
John Frederic Bateman, M. Inst. C.E.	Jan. 21st, 1840
James Bevan	January 23rd, 1844
Edward William Binney	January 25th, 1842
Alfred Binyon	January 26th, 1838

	Date of Election.
Richard Birley	April, 18th, 1834
James Black, M.D. F.G.S	April 30th, 1839
John Blackwall, F.L.S	January 26th, 1821
George Blake, M.A	January 25th, 1842
Henry Bowman	October 29th, 1839
Edward Brooke	April 30th, 1824
W. C. Brooks, B.A.	January 23rd, 1844
Henry Browne, M.B	January 27th, 1846
Laurence Buchan	November 1st, 1810
John Burd, jun.	January 27th, 1846
Henry Cadogan Campbell	January 23rd, 1835
John Young Caw	April 15th, 1841
Henry Charlewood	January 24th, 1832
John Chippendall	April 24th, 1812
Peter Clare, F.R.A.S.	April 27th, 1810
Charles Clay, M.D.	April 15th, 1841
Richard Cobden, M.P.	April 29th, 1836
Thomas Cooke, jun.	April 12th, 1838
Samuel Elsworth Cottam, F.R.A.S.	October 20th, 1837
James Crossley	January 22nd, 1839
Charles Cumber	November 1st, 1833
Matthew Curtis	April 18th, 1843
John Benjamin Dancer	April 19th, 1842
Samuel Dukenfield Darbishire	January 25th, 1822
John Davies, M.W.S.	November 1st, 1816
James Joseph Dean	November 15th, 1842
Joseph Cheeseborough Dyer	April 24th, 1818

	Date of Election.
Rt. Hon. The Earl of Ellesmere, F.G.S.	April 15th, 1841
Edward Ethelston	April 28th, 1840
William Fairbairn, M. Inst. C.E.	October 29th, 1824
David Gibson Fleming	January 25th, 1842
William Fleming, M.D.	April 18th, 1828
Richard Flint	October 31st, 1818
James William Fraser	January 22nd, 1839
Rev. William Gaskell, M.A.	January 21st, 1840
Samuel Giles	April 20th, 1836
Thomas Glover	January 21st, 1831
John Goodman	January 25th, 1842
John Graham	August 11th, 1837
Robert Hyde Greg, F.G.S.	January 24th, 1817
William Rathbone Greg	April 26th, 1833
Edward Hacking	January 27th, 1846
William Hadfield	April 20th, 1827
Rev. Robert Halley, D.D.	April 29th, 1845
Richard Hampson	January 23rd, 1844
Robert Hardy, M.D.	January 27th, 1837
John Hawkshaw, F.G.S. & M. Inst. C.E.	Jan. 22nd, 1839
William Charles Henry, M.D. F.R.S.	October 31st, 1828
Charles Henfrey	November 15th, 1842
Sir Benjamin Heywood, Bart. F.R.S.	January 27th, 1815
James Heywood, F.R.S. & G.S.	April 26th, 1833
James Higgins	April 29th, 1845
John Hobson	January 22nd, 1839
Eaton Hodgkinson, F.R.S. & G.S.	January 21st, 1820

Date of Election.

James Platt Holden ....January 27th, 1846
Edward Holme, M.D. F.L.S. President of the Natural History Society of Manchester; Fellow of the Imperial Society of Naturalists of Moscow; and Honorary Member of the Royal Institution of Cornwall ....April 25th, 1794
Thomas Hopkins ....January 18th, 1823
Henry Houldsworth ....January 23rd, 1824
Richard Baron Howard, M.D..........January 25th, 1842

Paul Moon James ....January 27th, 1837
John Jesse, F.R.S. R.A.S. & L.S. ...January 24th, 1823
Rev. Henry Halford Jones..................April 21st, 1846
Joseph Jordan ....October 19th, 1821
James Prescott Joule, M.C.S............January 25th, 1842
William Joynson ....January 27th, 1846

Alexander Kay ....October 30th, 1818
Samuel Kay ....November 1st, 1799
Samuel Kay, jun. ....January 24th, 1843
John Kennedy ....April 29th, 1803

Henry Charles Lacy ....January 29th, 1827
Richard Lane ....April 26th, 1822
William Langton..............................April 30th, 1830
James Lillie.................................October 29th, 1830
Thomas Littler ....January 27th, 1825
John Lockett ....January 25th, 1842
Joseph Lockett ....October 29th, 1839
Benjamin Love ....April 19th, 1842

	Date of Election.
James M'Connel	October 30th, 1829
William M'Connel	April 17th, 1838
Alexander McDougal	April 30th, 1844
John Macfarlane	January 24th, 1823
William Makinson	January 24th, 1823
Robert Mann	January 22nd, 1839
Robert Manners Mann	January 27th, 1846
Thomas Lechmere Marriott	January 22nd, 1839
James Meadows	April 30th, 1830
Thomas Mellor	January 25th, 1842
William Mellor	January 27th, 1837
John Moore, F.L.S.	January 27th, 1815
L. A. J. Mordacque	October 29th, 1830
George Murray	January 27th, 1815
William Neild	April 26th, 1822
Henry Newbery	April 30th, 1839
John Ashton Nicholls	January 21st, 1845
William Nicholson	January 26th, 1827
Daniel Noble	October 21st, 1836
Geo. Wareing Ormerod, M.A. F.G.S.	Jan 26th, 1841
Henry Mere Ormerod	April 30th, 1844
John Owen	April 30th, 1839
George Parr	April 30th, 1844
John Parry	April 26th, 1833
George Clarke Pauling	January 25th, 1842
George Peel, M. Inst. C.E.	April 15th, 1841
Sir George Philips, Bart.	April 13th, 1785

Date of Election.

Archibald Prentice........................January 22nd, 1819

Joseph Radford ....January 22nd, 1836
Robert Rawson ....January 21st, 1845
Joseph Atkinson Ransome, F.R.C.S. ...April 29th, 1836
Rev. William Read, B.A................January 23rd, 1824
Rev. John Gooch Robberds ....April 26th, 1811
John Roberton ....April 20th, 1827
Richard Roberts, M. Inst. C.E.........January 18th, 1823
Samuel Robinson ....January 25th, 1822
Alan Royle................................January 25th, 1842

Thomas Seddon Scholes ....April 20th, 1831
John Sharp........................... ...October 29th, 1824
Edward Schunck, Ph. D................January 25th, 1842
John Shuttleworth.......................October 30th, 1835
Geo. S. Fereday Smith, M.A. F.G.S. ...Jan. 26th, 1838
Robert Smith, Ph. D. ....April 29th, 1845
Charles Souchay..........................October 21st, 1825
John Spear................................January 22nd, 1839
Edward Stephens, M.D. ....January 24th, 1834
Robert Stuart.............................January 21st, 1814
William Sturgeon ....October 29th, 1844

Rev. John James Tayler, B.A.........January 26th, 1821
Edmund Peel Thomson..................January 21st, 1820
Robert Thorpe...........................November 3rd, 1815
John Thom................................January 27th, 1846
James Aspinal Turner ....April 29th, 1836
Thomas Turner ....April 19th, 1821

	Date of Election.
F. Eugene Vembergue	January 27th, 1832
Absalom Watkin	January 24th, 1823
Joseph Whitworth	January 22nd, 1839
Matthew A. Eason Wilkinson, M.D.	January 26th, 1841
William James Wilson, F.R.C.S.	April 29th, 1814
Gilbert Winter	November 2nd, 1810
William Rayner Wood	January 22nd, 1836
Bennett Woodcroft	January 26th, 1841
George Woodhead	April 21st, 1846
Edward Woods	April 30th, 1839
Robert Worthington	April 28th, 1840
James Woolley	November 15th, 1842
Joseph St. John Yates	January 26th, 1841

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