TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

EDINBURGH.

VOLUME XIII.

EDINBURGH:
PUBLISHED BY CHARLES TAIT, AND BELL & BRADFUTE;
AND T. CADELL, LONDON.
MDCCCXXXVI.

PRINTED BY NEILL & CO. OLD FISHMARKET, EDINBURGH.

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PART II.

On the Investigation of Magnetic Intensity by the Oscillations of the Horizontal Needle. By W. Snow Harris, Esq. F. R. SS. Lond. & Ed.

(Read 6th January 1834.)

1. The many irregularities to which the vibrations of the horizontal needle are subject, renders the usual method of investigating the terrestrial magnetic intensity, although precise in theory, occasionally uncertain in practice. It has been my endeavour in the following paper, to consider in some measure the most prominent of the causes of these irregularities, with a view of arriving at certain practical deductions, by means of which they may be either greatly palliated, or otherwise avoided.

2. Of the different causes of disturbance in the state of oscillation of a freely suspended magnet, the following may be considered as the most important:

1st, Variations in the conditions of the surrounding air, in which the oscillations are performed.

2d, Influence of changes in the mechanical conditions, incidental to the mode of suspending the bar; and of other mechanical changes liable to occur in it.

3d, Changes in the disposition and intensity of the magnetism of the bar, from heat and other causes.

4th, To these may be added the influence of the sun's rays, and of atmospheric electricity, both of which have been considered by many distinguished philosophers to exert very sensible effects on the magnetic oscillations.

3. The identity in principle of a needle vibrating by the directive force of the earth's magnetism, with an ordinary compound pendulum, oscillating by the force of gravity, would lead us to infer, that the mere friction and resistance of the air has little or no influence on the time in which a given number of vibrations would be performed, supposing every other condition of the experiment to be the same, as may be seen in Professor Airy's profound paper on the disturbances of pendulums, forming a distinguished portion of the Third Volume of the Cambridge Philosophical Transactions. But although the presence of a resisting medium, such as the atmosphere, may not be thus far prejudicial to the rate of the magnetic oscillation; yet, in consequence of the arc of vibration becoming more or less rapidly diminished, by occasional changes in the density of the air, or otherwise, in consequence of the uniformity of the oscillation being impeded by invisible currents, known to take place in it from various causes, the presence of such a medium is extremely undesirable, more especially when it is considered necessary to vibrate a needle in large arcs.

4. The following experimental illustrations of the differences which may arise in the times and arcs of vibration, in consequence of the presence of a resisting medium; as also of the differences in the rate of vibration, arising solely from differences in the extent of the arc, may not be here out of place.

(a.) A magnetic bar about 6 inches in length, $\frac{1}{4}$th of an inch wide, and $\frac{1}{8}$th of an inch thick, being delicately suspended by a silk filament without torsion, and quite free from the disturbing influence of metallic substances, was deflected from its meridian, and again set free at an angle of 45°. The times and arcs, corresponding to every tenth vibration, were carefully noted, until the arc of vibration was reduced to 10°. This experiment, frequently repeated, both in open space, and when the bar was covered by a glass shade, led to the following result: In a perfectly open space without the glass shade, the terminal arc at 10° corresponded to the 130th vibration; whereas, under the glass, the 180th vibration was completely distinguishable before the arc was reduced to 10°; a slight difference also was observable in the time of 100 vibrations, which in the first case was somewhat less.

The room in which the experiments were conducted, was selected expressly for the purpose, being about 12 feet square, and not liable to sensible currents of air from artificial heat; it had a stone pavement, and was not furnished with a fire-place.

(b.) A magnetic bar, about 5 inches long, and $\frac{1}{4}$th of an inch square, being delicately suspended as before, was deflected and set free under an exhausted receiver, at an angle of 60° from its meridian. The following differences in the rate of vibration were observed as the arcs of vibration became reduced: they were accurately ascertained by means of a valuable chronometer, expressly constructed for taking time, and which could measure the $\frac{1}{60}$th part of a second*.

When the arc was between 50° and 40°, the rate of vibration amounted to 9.5 per minute; between 40° and 30° it was 9.7 per minute; between 30° and 20°, it increased to 9.75 per minute. In an arc of about 10° the rate of vibration had increased to 10 in a minute.

This experiment is not adverted to as comprising any very unexpected result, but merely in illustration of the action of a particular magnet, the state of magnetic inquiries being such as to render any instance of this kind worthy of notice.

5. The great advantage likely to attend a method of experiment which admits of the magnetic oscillations being freed from the disturbing influence of the surrounding air, has led me at various times to the construction of instruments calculated to at-

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* I have to acknowledge the obligation I am under to my friend Colonel Hamilton Smith, for a long use of this valuable instrument.

tain this object *. Plate I, Fig. 2, represents a portable apparatus of this kind, which I have employed with considerable advantage. A brief explanation, with the assistance of Figs. 1, 2, 3, will render it easy to be understood.

Fig. 1. A, represents a strong base, resembling somewhat in form the sector of a circle; it is separable into parts, being held together by a single screw and nut at *a*. The diameter *acb* of this sector is to be directed as nearly as possible in the magnetic meridian. An oblong plane of wood P, Fig. 2, clamped at each end, and carrying the magnetic apparatus and air-pump, is placed upon this base, so as to be moveable for a short distance on a pin *c*, Figs. 1 and 2, concentric with the suspended bar and graduated card; this point *c* is also the centre of the arc *mn*, Fig. 1, which admits of a minute adjustment of the graduated card, in the line of the magnetic meridian, without disturbing the state of the exhausted receiver *r r*, Fig. 2.

6. The magnetic bar *m*, Fig. 3, is suspended in a light frame of wood *ff*, attached to a circular block *q*. The upper and narrow part of this frame *f′*, is separable from the lower portion *ff*, and may be detached if required; it is moveable with friction in a circular hole in the transverse bar which sustains it, and there are sight slits *ff* in the sides of the lower portion of the frame, which serve to adjust with precision, the position of the needle in one direction, by observing the suspension silk through them.

The centre of the block *q* is hollowed to about one half of its diameter, in order to admit of the operation of a sort of forked lever *l l*, Fig. 4, moveable through an air-tight collar; as also for the purpose of securing the block by means of a screw and nut, to a circular plate of finely polished slate, *s s*, Figs. 2, 3, 4. This plate of slate is extremely well adapted to the purposes of an air-pump plate, and is connected with the exhausting barrels by

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* Trans. Royal Society for 1831, p. 69.

means of small air-tight collars of brass, and an intervening pipe, as shewn in Figs. 2 and 4. There is a small gauge-plate *g*, Fig. 2, connected with the pipe leading to the barrels, which may be occasionally employed in particular experiments.

7. By means of the forked lever *l l*, Figs. 2, 3, 4, the suspended bar can be deflected, and steadily arrested at any given angle, or otherwise set free, at any instant of time. This method of arresting the bar has a great advantage; for the forked portions of the lever operating on opposite sides and ends of the bar, preserves it completely in a state of rest; so that, when set free, an extraordinarily steady vibration is obtained, provided the adjustments of the various points are accurately made. To effect this, the forked portions *l l*, Fig. 4, are independent of each other, and are moveable with friction in a tube of brass.

8. The bar is suspended in the usual way, by filaments of unspun silk, attached to a vertical rod of brass, which can be raised or depressed by means of a screw, through certain small distances. A gauge of an extremely simple kind, *k*, Fig. 3, for shewing the state of the exhaustion, is attached to the wood frame *f′*, and there is a thermometer similarly placed on the opposite side, as seen in Fig. 2.

9. The magnetic apparatus is enclosed in appropriate receivers; and when the exhaustion is sufficiently complete, the air-pump, together with the block to which it is fixed, may be removed, if requisite, by unscrewing the connecting joint at *n*, Fig. 2, and the four-milled head-screws which confine the block to the oblong plane P.

10. The arc of vibration is observed by means of graduated cards. There are two of these, *mq*, *np*, Fig. 3, placed one over the other, either of which may be used, according to the nature of the experiment. The lower one, *mq*, is a wide plane ring, having several graduated concentric circles on it; it corresponds to the surface of the wood block *q*, and has a large central hole for the forked lever above described (7) to pass clear. The up-

per card *np* is a narrow ring, about half an inch wide, and admits of the bar vibrating within it. It can be altogether removed if required.

The bar has fine index wires, *ii*, Fig. 4, slightly held in shallow notches at each end. These indicate the precise degree on the graduated cards, so that very great precision may be arrived at, if required, in noting the arc of vibration.

It may be readily perceived, that, in the above mechanical arrangements, there is no considerable mass of metal in the vicinity of the needle, which can disturb its state of oscillation, the parts being constructed, for the most part, of non-metallic bodies, whilst every possible facility is obtained, requisite to carry on the experiments, free from the disturbing influence of the atmosphere.

11. An extensive series of experiments, with magnetic bars and needles, vibrating in air, and in an exhausted receiver, under various conditions, has led me to conclude, that we cannot really depend upon observations embracing extremely minute differences in the rate of vibration, as applicable to terrestrial intensity, until the usual instruments of research have been so perfected, as to place such differences beyond the operation of other causes, foreign to the object of experiment, (a) (b). Indeed, it will be readily admitted, on due reflection, that, considering the vast object to be attained, no less a one than an accurate measurement of the magnetism of the Earth, the same kind of practical perfection is requisite, and should be continually sought in the construction of the magnetic pendulum, as is obtained in that of the ordinary pendulum, oscillating by the force of gravity, without which we can never hope to arrive at results worthy of confidence. Do we require the rate of oscillation, such as to detect extremelysmall differences in terrestrial intensity? A given number of oscillations, which admit of being considered isochronous, is absolutely requisite. Now, this can be readily effected with the instrument above described, Fig. 2, by observing the vi-

brations in a rare medium, and confining them within very small arcs,—as small as possible, so that the arc be consistent with accuracy of observation, (as shewn in Table III, observations on the Shade, p. 22); with a magnet about 5 inches in length, and about $\frac{1}{8}$th of an inch square, suspended from one of its edges, I have succeeded in obtaining between 200 and 300 vibrations, before the arc became reduced from 5° to 3°; the time of 100 vibrations deduced from 200 vibrations, and estimated from the 0th to the 100th vibration, from the 10th to the 110th, and so on, up to the 200th vibration, according to the method pursued by Professor Hansteen, being upon each 100 vibrations exactly the same. Indeed, when such irregularities as those already stated (a) (b) are considered, and which almost always happen to a greater or less extent, when the magnetic oscillations are observed in air, and taken in large arcs, it is not unreasonable to infer that anomalies and slight differences of time may appear on a great number of vibrations, notwithstanding the theoretical corrections proposed to neutralize them. It may not, therefore, be safe to attribute the cause of minute differences in time to variations in the terrestrial intensity, before the instruments of research are made so perfect as to be altogether without the limits of the above mentioned causes of error.

12. The mode of suspension and other mechanical conditions connected with an oscillating bar, are not less worthy of consideration than the disturbing influence of the air.

In all experiments with the horizontal needle, it is evident that we do not measure actually the magnetic intensity of a given place, but only one of its resolved portions, except the place should be directly in the magnetic equator. It is therefore of consequence to the experiment, to preserve the bar in the same relative position. This we suppose to be horizontal, but since every freely suspended magnet inclines more or less in various places, it becomes necessary either to measure accurately the angle of inclination, or otherwise correct it in some way. The latter has been effected, sometimes by a single weight, at others by a shifting point of suspension, and occasionally by mechanical constraint, all of which may cause important differences in the results of experiment.

13. The chances of error with bars whose points of suspension are made to slide on them, are so manifest, that it seems essential to preserve in the bar a fixed point, so as to render this condition invariable. This point, as is evident, should be as nearly as possible in a line passing through the centre of gravity, the centre of motion, and the point of magnetic neutrality. In this case the bar may be conceived to vibrate without error, by the action of the two terrestrial resultants, in the same way as either of its polar halves would oscillate separately, if acted on by one of them alone, since the directive force of the earth may be conceived to constitute in the bar an attractive and repulsive system of parallel forces, each of which is resolvable into a single resultant, whose direction, intensity, and point of application, may be determined according to the elementary principles of mechanics.

14. The following method of preparing and suspending a magnet for vibration, will be found to possess considerable advantage. The bar being at first roughly forged, a coarse hole is drilled through the middle of it. The upper part of this hole is then bushed with a small piece of brass, through which an extremely fine hole is drilled, and which is subsequently intended to pass through the centre of motion, as also the centre of magnetic neutrality. If the bar is to be suspended occasionally from the opposite side, then that side must undergo a similar preparation; only, in this case, a second large hole is drilled at right angles to the former, also through the centre, Fig. 4; the fine holes, if requisite, may be drilled immediately through the steel. When the bar has been regularly worked, but before tempering it, equal distances are accurately measured off on each side the fine central hole above mentioned, so as to make the arms exactly equal, and two shallow slits are cut in the extremities, perpendicu-

lar to the plane in which the vibrations are intended to take place: These are to receive the fine index wires, which are easily retained in the slits after a slight pressure, or tap of a hammer. It should now be nicely poised on a fine suspension-silk attached to a small loop, inserted through the vertical central hole, so as to remain exactly horizontal. The horizontal position may be readily ascertained at any time by a very simple method. Let the bar, Fig. 5, be held near the surface of a reflecting fluid, such as mercury, or, what answers equally well, near the surface of water, in which a little indigo is dissolved, to give it colour; an image of the bar will then be perceived in the fluid. When the lines *a b*, *c d*, formed by the bar and its image, are parallel, then we may be assured that perfect horizontality is obtained. The parallelism of the lines is easily detected by the eye.

15. After the horizontal position has been thus accurately determined, two points are to be impressed on each side of the centre, at an equal distance from the centre and ends. Two small sliders of platinum, *a b*, Fig. 4, of equal weight, are then closely fitted, one on each arm. These sliders have small central holes drilled in them, and bear a very small proportion to the whole mass. They are at first placed so as to bring the central holes immediately over the points impressed on the bar, and in this state the horizontality of the whole is again observed as before.

16. The bar being now properly tempered throughout, is finally polished by filing, and, after being again examined as to position when suspended, is rendered magnetic. It is then exposed for a short time to the temperature of boiling water, and after this to a temperature as low as zero of Fahrenheit's scale. We have then a magnet calculated, as far as possible, to retain for any ordinary temperature an invariable magnetic state. We should now place the bar thus prepared under a piece of strained paper, and having sifted some iron-dust over it, by means of an extremely fine sieve, examine carefully if the centre of the magnetic curves falls about the centre of suspension. Each face of

the bar should be tested in this way. There will be little difficulty in effecting this, provided the bar is accurately made and tempered, and has been equally touched at first with magnets of the same force.

17. For the purpose of an easy suspension, a very small hook is fixed to the small central hole. This is readily effected, by securing the eye of the hook to a fine doubled thread of silk, and then passing the silk either directly through the vertical hole, or otherwise out at the side, through the horizontal hole above mentioned. In either case, a small knot tied in the silk, within the substance of the bar, secures the hook very effectually.

18. The bar being suspended in this way, and the sliders placed at first in their original position, the inclination due to the polarity is corrected, by moving one of the sliders toward, and the other from, the centre, by nearly an equal quantity. The distances through which they require to be moved, in order to correct the inclination, are so little, that we may consider any error arising from change, in the angular inertia of the mass, as being indefinitely small, since the sliders are moved in opposite directions. Should it, however, be considered requisite, in any extreme case, we may find a series of points on each side the centre, in some of which the horizontal position may be obtained, and which would correspond to the same angular inertia with mathematical precision; since we have only to find a series of points, the sums of the squares of the distances of any two of which, from the centre of the bar, taking one on each side, give a constant quantity, the resistance of the sliders to motion being really, as the particles themselves, multiplied into the *squares* of their distances, from the axis of motion.

In order to adjust the slides with accuracy, a fine pair of compasses, and a minutely divided scale, may be employed.

19. In addition to these circumstances, incidental to the mechanical conditions of a vibrating magnet, the silk thread of suspension requires also to be considered. It will be found, on ex-

amination, that comparatively slight differences in the flexibility of the silk, change in some instances the rate of vibration. To avoid this source of inconvenience, it is desirable to prepare a great number of suspensions from the unspun fibre, so as to be without the least torsion, and as nearly alike as possible, and submit each to experiment on the same bar. Their identity as to effect may then be determined. Each silk should be gently drawn through between the fingers, slightly smeared with some extremely fine oil, and finally wound upon a small cork, and laid aside in a box for use.

The bar is easily hung to the silk by means of the hook above described, and a fine loop tied in the extremity of the silk. A similar method is employed, in attaching the opposite end to the suspension point.

20. The possibility of change in the length of a vibrating bar, in consequence of variations in temperature, is very apparent. To arrive at the requisite correction, it seems desirable to make the particular magnet an immediate subject of experiment. This may be managed by means of the instrument above described, Fig. 2, with advantage.

The bar being suspended in the exhausted receiver, and its rate of vibration determined in a very small arc, under a given temperature, the lower portion of the receiver is then enclosed in a small oven of copper *o*, Fig. 6, and a stream of heated air made to circulate about the glass, by means of a lamp placed under the funnel *r*. When the temperature has been raised as many degrees as is considered requisite, the oven is removed, and the rate of vibration again taken as before; and this process may be repeated through a range of temperature sufficient for our purpose, that is to say, up to 200 degrees of Fahrenheit. For the sake of greater accuracy, an additional and very delicate thermometer may be placed near the magnet.

21. I have also employed water with advantage, for the purpose of observing the effects of heat on the needle. An outer

glass vessel *v*, Fig. 7, open at both ends, and having its lower edge ground to the pump plate, is placed round the receiver *r*, Fig. 2. Two brass-tubes *xy*, (Fig. 7,) communicate with the space between the outer and inner receivers, one of them passing out as a syphon, the other being connected with a funnel; and thus a circulation of the fluid, placed round the receiver, may be continued to any degree of temperature required.

22. It is important to remark, that neither of the above methods of research can be relied on, unless the magnetic vibrations be observed in a very rare medium; for currents are produced by the heated glass, the effects of which on the oscillations of the bar, mixed up with the influence of heat on the needle itself, are extremely anomalous. My friend Mr Fox of Falmouth, who has made some interesting experiments on this subject,* informs me, that he made the existence of those currents evident, by means of a little smoke introduced into the receiver. He says, that they were sometimes sufficient to impart motion to light needles, and cause them to vibrate in arcs of from ten to twenty degrees. During the state of vibration of other bars, the arcs became sometimes more rapidly reduced, and in some instances the vibrations were greatly increased in number. Light needles were more disturbed than heavy bars. Fine wires not magnetic, and needles composed of non-metallic bodies, were similarly affected. The effects continued until a perfect equilibrium of temperature took place in the receiver, and were not apparent when it was exhausted of the contained air.

These results, taken in connexion with the experiments above adduced (a), seem to confirm the necessity of a very rigorous method of experiment in deducing the rate of vibration, so as to exclude the disturbing influence of the atmosphere.

23. Of the changes which ordinarily occur in the conditions of

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* Lond. and Edin. Journal of Science for October 1832.

the magnetic pendulum, those induced in the magnetism of the bar itself, from temperature and other causes, may be considered the most important. The effects of heat in the magnetic tension seem to be extremely uncertain, since it tends more freely to a state of neutrality in some magnets than in others. It is therefore requisite also to examine experimentally, in the way above stated, the influence of change of temperature in each individual bar, so as to discover whether it has an invariable magnetic state or not. The influence of heat also, evidently decreases more rapidly than the magnetic intensity, so that bars of weak intensity seem invariable under certain low ranges of temperature. I have hence found it desirable to work with bars of low tension, treated in the way above mentioned (16). The interesting and profound researches of Mr Christie*, however, would lead us to infer that certain variable states may occur in magnetic intensity from heat, without a final loss of force, and for which he has endeavoured to deduce adequate expressions. Although such authority must necessarily be highly appreciated, yet the imperfect state of our knowledge in the sciences of electricity and magnetism requires a very extensive examination of exceptions, deducible by experiment from any particular instance.

24. After a careful inquiry into the effects of temperature on certain small magnetic bars employed to determine terrestrial intensities, I have not been enabled to discover, by *the method* of *vibration*, variable changes in their magnetic state, such, that the tension, being weakened by heat, it could again be restored by cold; nor have I, in the particular instances which came under my notice, been enabled ever to increase the magnetic energy of a bar by reducing its temperature. On the contrary, the exposure of small bars to very low temperature, seemed, as tested by the method of vibrations, to be attended by a *decreased* energy. Thus, a small bar of moderate force was observed to make in va-

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* Transactions of the Royal Society for 1825.

cuo in small arcs, 100 vibrations in 11′ 56″, 6, after exposure for about half an hour to a cold of about 5° of Fahrenheit's scale; it made under similar circumstances 100 vibrations in 11′ 65″; whilst in other cases, similar bars, after being freely exposed to a range of temperature from 0° to 212°, appeared to remain invariable, in respect of certain ranges of temperature taken between these points. These results have received some confirmation since the notice of my paper on this subject, read at the Meeting of the British Association in June last, as appears in a letter to Dr Brewster from Professor Kupffer of the Imperial Academy of St Petersburgh, printed in the London and Edinburgh Journal of Science for August 1832. The coincidence between Mr Kupffer's results and my own is not a little remarkable. I have been hence led to hope, that the effects of heat in disturbing the tension of a vibrating needle, may in certain cases be confined to differences of temperature, without the limits of that to which the needle has been previously exposed. I desire, however, to evince no greater confidence in this opinion, than the results of the experiments above alluded to seem to warrant. Mr Christie's profound and interesting researches in this department of science have undoubted claims to our confidence; and we must therefore endeavour, by further investigation, to discover the source of the apparent anomalies which occasionally present themselves. It may be fairly observed, at least, that whilst such questions as these, and others of no less consequence, remain in any degree doubtful, it is quite impossible to place confidence in results pretending to detect small periodical variations in the terrestrial intensity, which variations, if such exist, may cause differences in time, certainly much smaller than might alone arise from either of the foregoing causes.

25. Beside the changes of tension induced in a bar by heat, we have to consider those which may happen from the manner of preserving the magnets themselves, such as occasional contact with each other, or with masses of iron. The usual method of

retaining the tension of magnets, by placing the dissimilar poles in contact, either with each other or with a mass of soft iron, or both, is certainly very objectionable. A variable magnetic tension is by no means unlikely to ensue. If the force of two small bars be measured by the method of vibration, and be then joined at their dissimilar poles by masses of iron, and again separated, they will not unfrequently shew very great differences when again vibrated as before. It seems, therefore, safer not to give needles intended for experiments on terrestrial intensity a greater degree of tension than they can themselves retain ; and when laid aside for use, to place them with reversed poles out of contact with each other, and at the intervals of some inches apart ; at least I have found bars prepared as above stated (16), and treated in this way, remain very constant.

26. The possibility of change in magnetic tension, renders it desirable that we should be in possession of some means of detecting such change when it occurs. The method occasionally resorted to, of returning again to a given point, and taking the oscillations as before, is not free from objection and inconvenience; since we are ignorant of the changes which may occur in a magnet, in consequence of change of situation, nor are we sure that the directive force is a constant quantity in a given place. We ought, therefore, to be enabled to detect changes on our needles in any place, and after small intervals of time, if required. The following appears a very delicate method of examining the tension of a magnetic bar.

27. It is well known, that if a needle be allowed to vibrate in a ring of copper, it will be more speedily brought to rest, than if allowed to vibrate in an open space, although no sensible differences seem to arise in the number of vibrations performed in a given time*. The effect seems to be in some direct ratio of the magnetic energy. The influence of a ring of copper,

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* Annals of Philosophy, Vol. IX. N. S.

therefore, might be employed to detect changes in tension, at any time and place, provided the force which induced motion in the needle, and that force by which it would be eventually reduced to rest, without the ring, were both constant quantities. But such is not the case. We may, however, reverse the experiment with advantage, and cause the ring *c*, Fig. 8, to oscillate about the bar *m*, which may be conveniently fixed within the ring, so as just to clear it ; and thus a comparative measure of the force of a bar at its poles may be arrived at, by observing the influence of the magnet in reducing the ring to rest. This experiment may be managed in the following way.

28. A ring of copper *c*, Fig. 8, of about 0,4 of an inch wide, and the $\frac{1}{8}$th or the 0,2 of an inch thick, is suspended by two parallel threads of silk fibre over a graduated circle, as shewn in the figure. The threads are fixed in a diameter of the circle, by means of a cross bar of brass *a b*, and at about the $\frac{1}{5}$th of an inch distance on each side of the centre. The ring is to be accurately balanced, and carefully adjusted to parallelism by means of the sliding wires at *w*, from which it is suspended, and its index points, 1, 2, 3, 4, fixed at each of its quadrants, brought exactly over the zeros on the graduated card below. The ring may be suspended by means of the brass plane at *w*, in the frame-work above mentioned, Fig. 3, fixed on the bar f', and hence be substituted occasionally for the magnetic bar.

29. The ring thus circumstanced, is to be deflected by means of the forked lever above mentioned, Fig. 4, from the direction of the parallel threads of suspension ; and again set free at any given angle by means of the forked portions acting on the transverse bar from which the ring is hung. In this case a long and steady state of vibration will be obtained, in consequence of the centre of gravity of the mass constantly rising and falling as the ring swings from one extremity of the arc of vibration to the other. Now, if the number of vibrations made by the ring alone, between given points, on the

graduated card, be divided by the number made between the same points, when put in vibration about the magnet *m*, Fig 8, then the quotient minus 1, may be taken to express the comparative force of the bar *m*, and we shall have a constant quantity for the same degree of tension, whatever extent we take between the first and last arc of vibration. Thus, the ring of copper alone was observed to make 70 vibrations between 40° and 35°, and 164 vibrations between 40° and 30°. When vibrating about the magnet, it made only 30 vibrations between 40° and 35°, and only 70 vibrations between 40° and 30°. It may be here seen that $\frac{70}{30} = \frac{164}{70}$, or very nearly.

30. The formula just given for estimating differences in magnetic tension, seems deducible in the following way: Let the vibrations of the ring alone be called *a*, and the vibrations within the same limits made about the magnet *b*, and call the retarding force by which the ring is brought to rest when vibrating alone *r*, and the retarding force of the magnet *R*, then $r + R$ is the combined or whole retarding force, where the ring vibrates about the magnet; but the number of vibrations between given limits may be assumed to be in an inverse ratio of the retarding forces; hence we have $r : r + R :: b : a$, or $ra = (r + R)\, b$; that is to say, $R = \frac{ra - br}{b}$, or $R = r, \left(\frac{a}{b} - 1\right)$. Now *r* having been taken as the force by which the ring alone is brought to rest, may be considered as unity in every csae; hence the value of *R*, or the force of the magnet, is comparatively expressed by the formula $\frac{a}{b} - 1$, which formula I have found to agree in a remarkable way with the results of experiments. The following instance may serve to illustrate its application in a particular case.

(*c*). The tension of a small magnet was estimated at each pole, by observing the attractive force on a small cylinder of soft iron at a given distance, in the way described in Vol. IX. of the Society's Transactions, p. 278. The magnet was then submitted to further experiment with the vibrating ring of copper, as above described (29). The force, as again estimated at each pole by the magnetimeter, was now doubled, and the retarding force on the copper again observed. In this case the latter was also found to be doubled.

Thefollowing Table comprises the results of this experiment.

TABLE I.

Tension by Attraction. Distance of Iron 0.25.	Tension by Vib. of Ring. Distance of Poles 0,9.
5°	$\frac{100}{55} - 1 = 0.818$
10°	$\frac{100}{38} - 1 = 1.631$

The vibrations of the ring were observed between 40° and 30°, between which limits, when vibrating alone, it completed exactly 100 vibrations; and when caused to vibrate about the magnet, it completed 55 vibrations with the lesser tension, and 38 vibrations when the tension was doubled. In this instance, the respective forces deduced by the formula, correspond as nearly as could be expected with the previously observed attraction on the iron.

This method of detecting changes in the magnetic force of a bar, seems therefore susceptible of precision. It is besides extremely delicate; the only apparent objection is the great accuracy in manipulation requisite for its perfect success. It is nevertheless available at any time, and in any place, since the vibrations of the copper-ring alone, taken within certain limits, may be always reduced to a constant and given number.

31. The experiments of Morichini and Mrs Somerville seem to shew, that certain magnetic properties exist in the solar

rays ; and their views have received much confirmation from the many important inquiries of Mr Christie, and which have at various times appeared in the Transactions of the Royal Society. These researches are of much consequence to experiments with the oscillating needle. If a magnet be caused to vibrate in the shade in air, and subsequently when exposed to the rays of the sun, the time in which the arc of vibration becomes reduced to a given point will in the sunshine be considerably less, whilst the rate of vibration seems upon the whole to be somewhat increased.

In briefly detailing the following experiments made with various bars, both in the shade and in sunshine, I would by no means pretend to have decided the question, whether the solar ray has really magnetic properties or not. My object is merely to shew, that certain differences observed to arise in the arcs and times in vibration, under a given solar influence, may be caused to vanish by placing the needle in an exhausted receiver.

32. On exposing an oscillating magnetic bar to the influence of a bright sunshine, so as to compare the phenomena incidental to the state of vibration with those previously observed in the shade, all the effects so accurately noticed by Mr Christie became strikingly apparent ; but whether the bar be caused to vibrate in a dense, or in a rare medium, the experiment, under an air-tight glass, is quite unmanageable for any accurate purpose. The sources of inconvenience appear to be, 1st, an alteration in the rare or dense medium surrounding the bar, together with a constant circulation of currents ; 2d, a condensation of vapour over the interior of the glass ; 3d, various mechanical and other changes in the conditions of the suspended bar itself. These circumstances rendered the experiments in a midday sun so very precarious, that I was obliged to abandon them.

33. (*d.*) With a view of avoiding the above mentioned sources of inconvenience, I eventually conducted the experiments in the following manner. Having observed the oscillations in the

shade under a closed receiver, and from which the air was not withdrawn, I noted the times and arcs corresponding to every 10th vibration, the needle being liberated at an angle of 45° from the meridian. Some bright beams of sunshine were then thrown into the receiver, by means of plane mirrors, so as to impinge upon the needle, and the same observed again. In this case the heat was not considerable, and the experiment became very manageable.

The bar being allowed to rest for a short time, in order to recover its previous temperature, the same experiments were repeated, after exhausting the receiver, so as to free the oscillations from any disturbing influence produced by the surrounding air. The general results I have collected in the following Table.

TABLE II.

Bar30 .400.	Oscillations taken in Air.		Oscillations taken in Vacuo.	
	Exp. A. Shade, Temp. 62°.	Exp. B. Sunshine, Temp. 70°.	Exp. C. Shade, Temp. 62°.	Exp. D. Sunshine, Temp. 70°.
Mean Time of 200 Vibs.	24′.1″	23′ 56″,6	24′ 10″, +	24′ 15″
Whole Time of 250 Vibs.	29′.2″	28′ 54″,5	30′ 13,5	30′ 24″
Terminal Arc,	14°	9°	22°, 5	22° +

These results, which I confirmed by many similar experiments, would lead us to infer, that, if the oscillations be taken *in vacuo*, the differences in the arcs of vibration in sunshine and in shade, may, under certain conditions, vanish, or nearly so; as also, that the time of a given number of vibrations, is upon the whole, in a void, rather increased than diminished. Hence it may be inferred, in the above instances, 1st, *That the influence of the sun's rays on a magnet oscillating in air, is to reduce more* *rapidly the arc, and to diminish the time of a given number of vibrations;* 2d, *The influence of the sun's rays on a magnet oscillating in a void, is to increase the time of a given number of vibrations, whilst the arc, if the retardation of the rate of vibration be small, is not materially affected.*

34. These phenomena, from the causes already considered (20) (24), seem quite consistent with each other. Thus, the more rapid diminution of the arc in sunshine, may, in great measure, arise from the state of the surrounding air, experiment (*a*) (4); since it is no longer apparent, in a strictly comparative case, when the air about the bar is made very rare; differences in the arc might hence, in a perfect void, become indefinitely small. The differences which thus arise in the arc, necessarily cause corresponding differences in the *rate* of vibration, experiment (*b*) (4); and these will become more or less evident, according as they exceed, or are less than, the effects of other causes, tending to a contrary result.

The diminished rate of vibration under the influence of the sun's rays in a void, may arise from expansion in the bar itself (20), or from a decrease in magnetic tension (24), or from both. If the magnetic state of the bar be at all constant, in regard to the small elevation of temperature to which it is exposed, the former would be the most probable cause of the effect, which, being no longer masked by great differences in the arc of vibration, becomes now very sensible.

35. In the following Table, I have transcribed two series of experiments, carried on *in vacuo,* in which the time of 100 oscillations is deduced from 150 oscillations observed in small arcs. In these the retarding influence of the sun's rays is also evident.

TABLE III.

Oscillations taken in Shade, Temperature 62°.							Oscillations taken in Sunshine, Temperature 70°.						
Vib.	Arc.	Time.	Vib.	Arc.	Time.	Time of 100 Vib.	Vib.	Arc.	Time.	Vib.	Arc.	Time.	Time of 100 Vib.
	°	′ ″		°	′ ″	′ ″		°	′ ″		°	′ ″	′ ″
0	5	24 4	100	4	35 53.5	11 49.5	0	5	2 4	100	4	13 54.5	11 50.5
10		25 14.5	10		37 4	11 49.5	10		3 14.5	10		15 5	11 50.5
20		26 25.5	20		38 15	11 49.5	20		4 25.2	20		16 16	11 50.8
30		27 36.5	30		39 26	11 49.5	30		5 36.5	30		17 27.5	11 51
40		28 47	40		40 36.5	11 49.5	40		6 47.5	40		18 38.5	11 51
50		29 58	50	3.5	41 47.5	11 49.5	50		7 58.5	50	3.5	19 49.5	11 51
60		31 9				11 49.5	60		9 9.5				304,8
70		32 20	Time of 100 Vib.			11 49.5	70		10 21.5	Mean of 100 Vib.			11 50.8
80		33 31.5	Time of 150 Vib.			17 43.5	80		11 32.2	Time of 150 Vib.			17 45.5
90		34 42.5	Term arc,			3°.5	90		12 43.3	Term. arc,			3°.5

Although the differences in time, in the above Table, in the shade and in sunshine, are somewhat small, and might not be considered, upon an insulated series of experiments, of great importance; yet the constancy of the result, on repeating the experiments for many successive days, together with the general agreement with the phenomena in Table II, and the great susceptibility of the bar in indicating such differences when oscillating in a void and in small arcs, are considerations which claim for these researches some degree of confidence.

36. With a view of examining, in some measure, the accuracy of the preceding deductions, I resorted to the following experiments:—

(*e*) A bar of copper, of the same dimensions as the magnetic bar above described, was made to vibrate under a closed receiver in air and in a void, and the vibrations, arcs, and times noted, as before (Table II), both in the sunshine and in the shade: the vibrations were produced in the way already described (29), by means of two parallel threads of suspension. In this instance the same results ensued precisely. In regard to the differences in the arc of vibration, these nearly vanished in a void; the subsequent retardation in the sunshine, however, was not so apparent as with the magnetic bar, although it was still very evident. I do not mean to adduce this experiment as being strictly comparative in regard to the magnetic bar, the rate of vibration having been found different, but merely in evidence of the effects produced by the presence of a dense medium, when acted on by the sun's rays.

(*f*) The times, arcs, and vibrations of a magnetic bar having been noted *in vacuo*, as in the previous experiments, it was completely exposed for twenty-five minutes to the intense light evolved by lime, under the flame of the hydro-oxygen blowpipe, and the oscillations taken again, but no differences whatever were found to ensue in the state of vibration.

The first experiment (*e*) would seem to show, that the phenomena of oscillation observed in the sun's rays, Table II, are not altogether dependent on magnetism; the second (*f*), that mere light is, to a certain extent, incapable of producing them.

37. The electricity pervading a vacuum, seems to have no appreciable effect on a magnet, oscillating under an exhausted receiver. With a view of examining this, I resorted to the following experiment:—

(*g*) A stream of common electricity, generated by a powerful plate-machine of 3 feet in diameter, was caused to pass through a long receiver *s*, Fig. 2, placed within 18 inches of the needle. This receiver was about 6 feet in length, and 4 inches in diameter. The electrical light was continuous for twenty minutes; it seemed to spread over the whole interior surface of the receiver, presenting the appearance of a solid column of white light. No appreciable difference, however, could be discovered, either in the rate or arc of vibration, as compared with the oscillations previously taken, when this artificial aurora was not present.

38. I have been led to offer these inquiries to the attention of the Royal Society of Edinburgh, under an impression that they contain enough of useful matter, to render them not unworthy of its consideration: on this account, alone, I venture to hope, that the Society may be induced to accept them. The measurement of the magnetism of the earth must be at least considered a vast and most important object of physical research. An attempt, therefore, to perfect the ordinary methods of observation, may not be altogether without its use, in the present state of general science.*

Plymouth, *April* 5. 1833.

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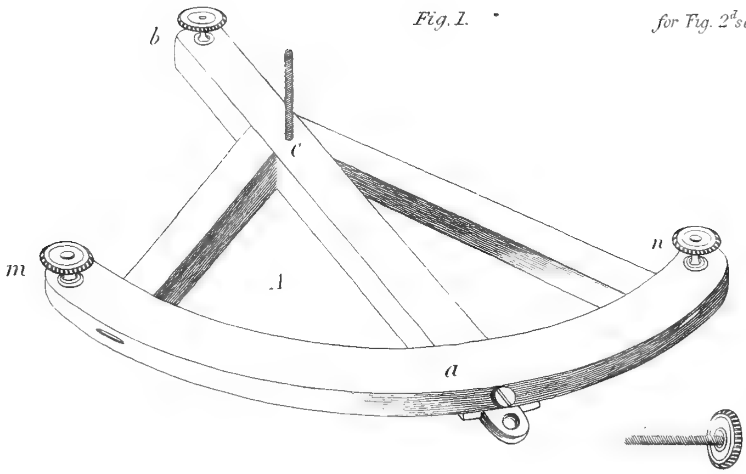
* I am much indebted to Mr Cox, maker of Philosophical apparatus at Devonport, for his valuable assistance in the construction of the Instruments described in this Paper.

Fig. 1.

for Fig. 2d see next Plate



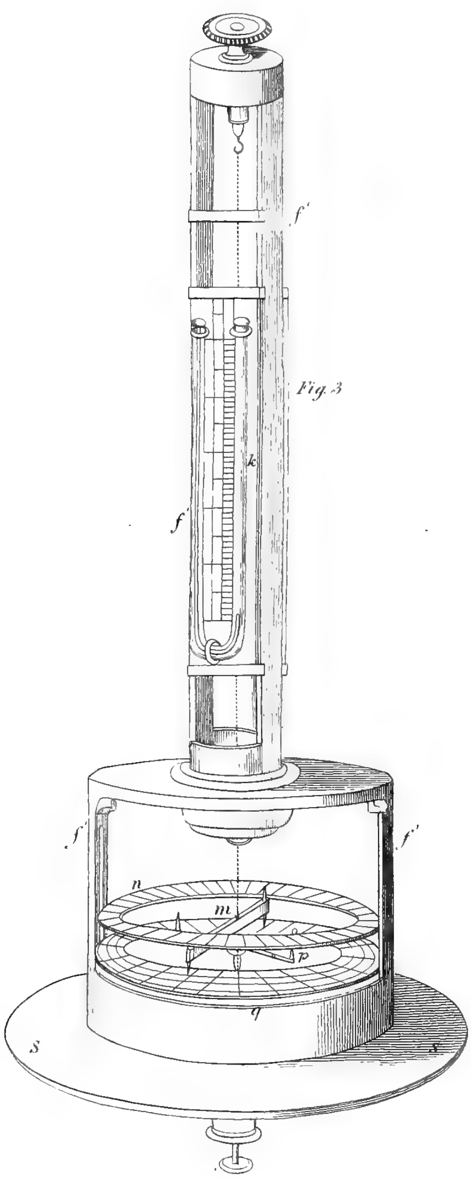


Fig. 3

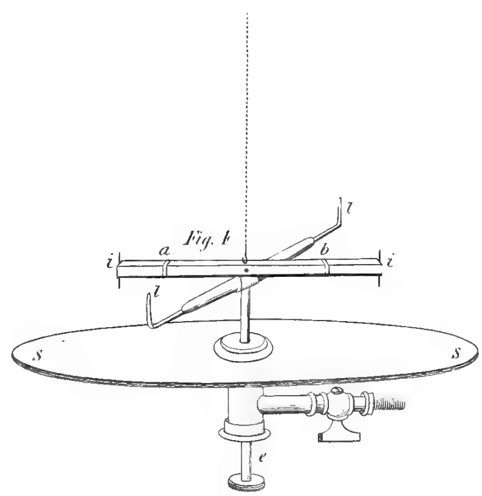


Fig. 4

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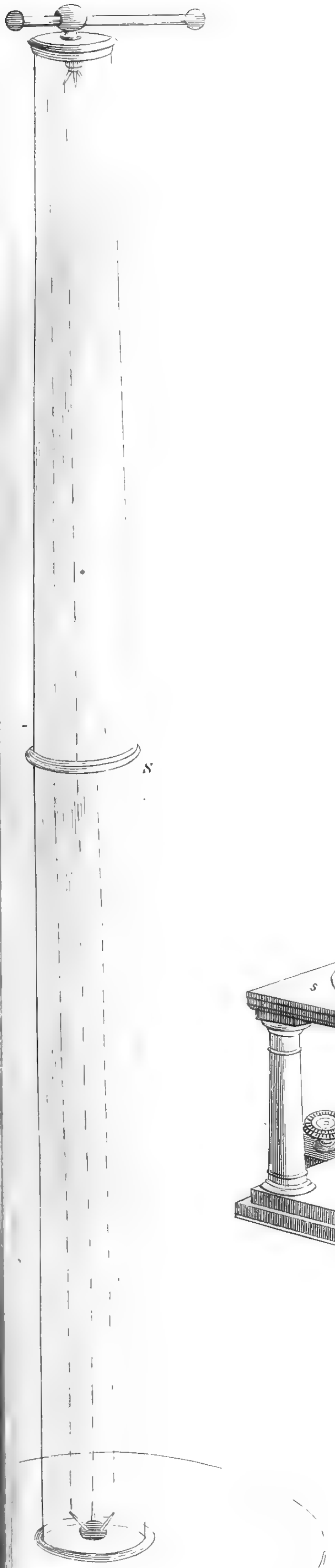
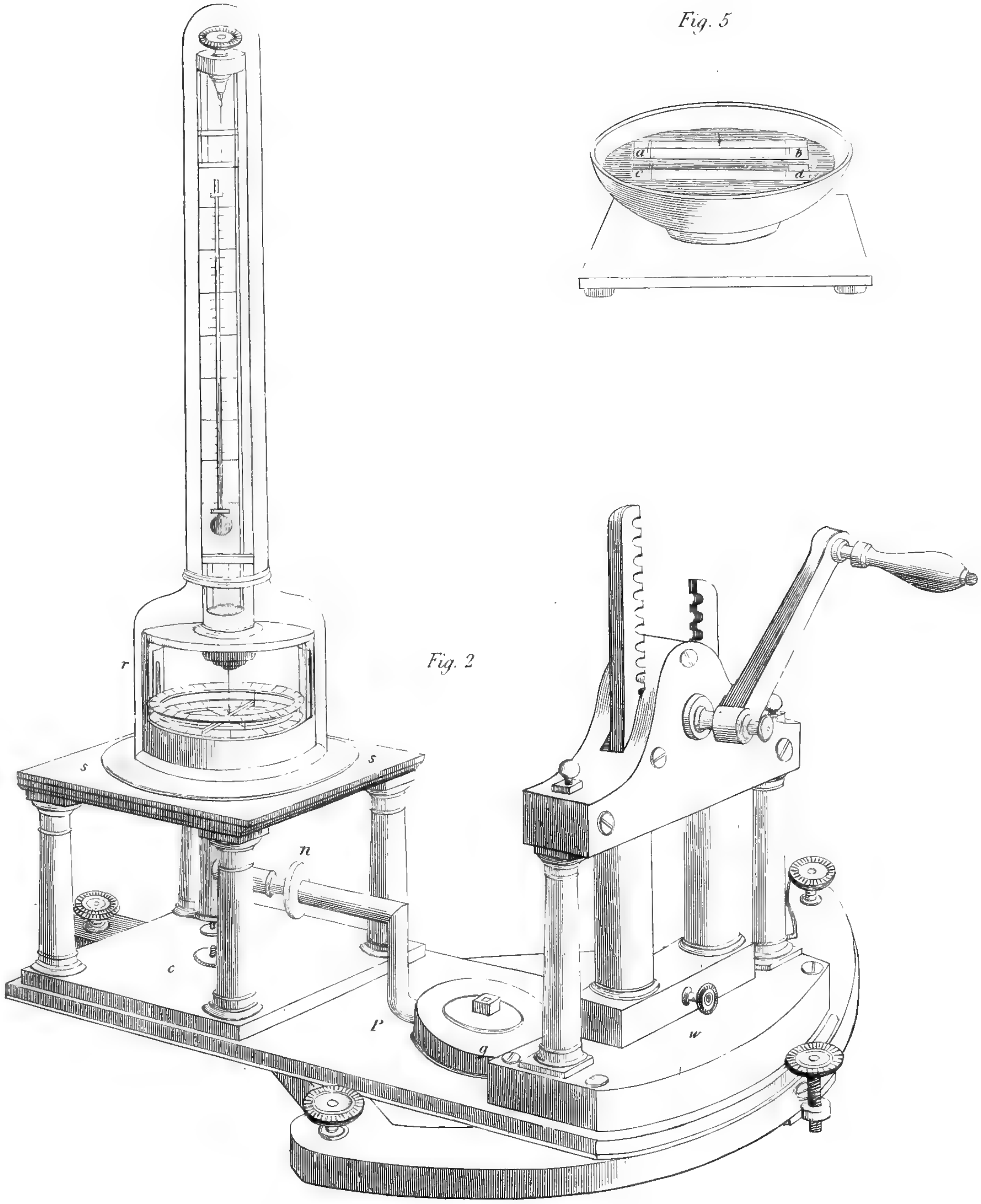


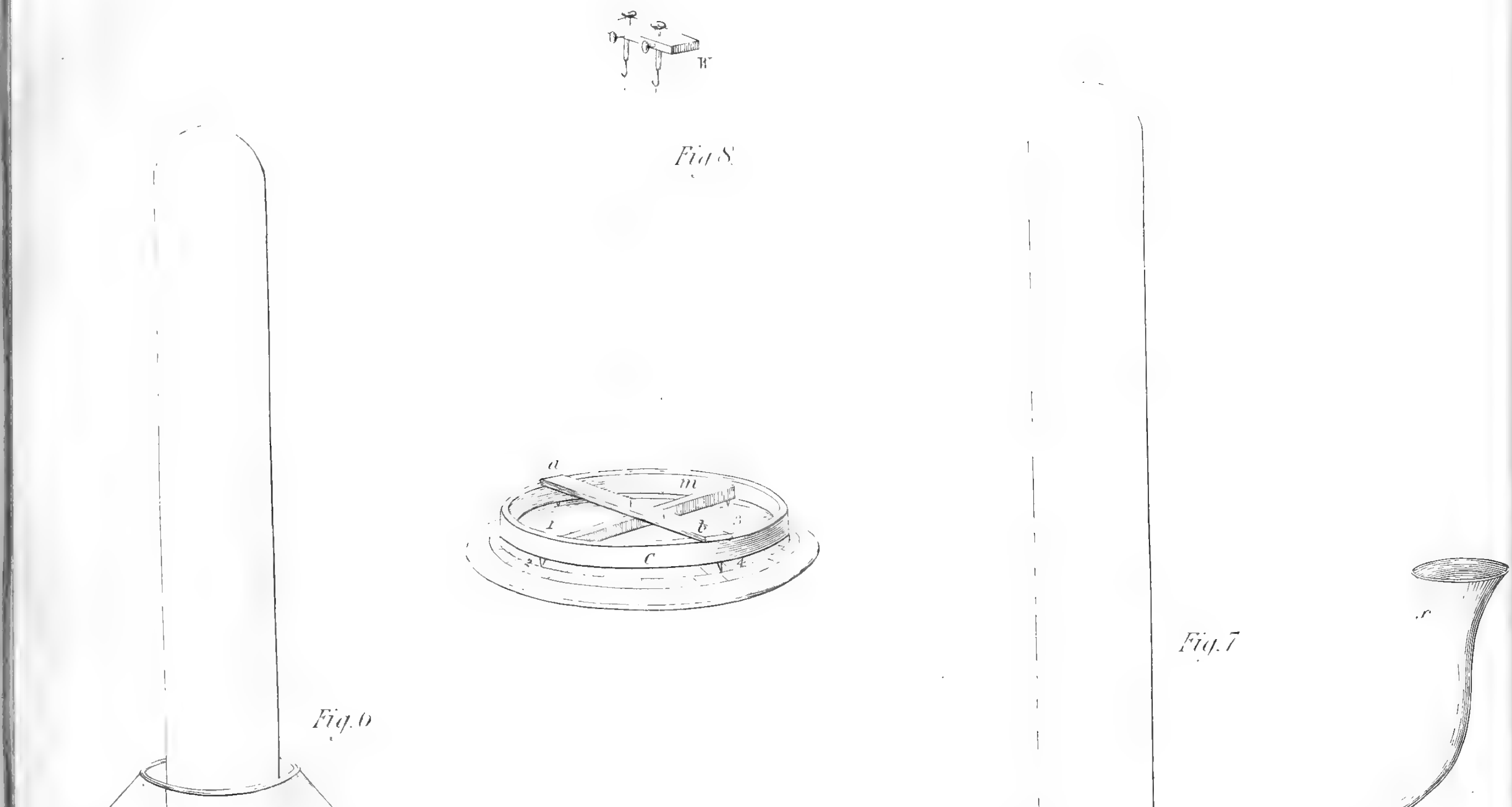
Fig. 5

Fig. 2



Drawn by G. Wightwick Esq.^r

Engraved by W.H. Lizars



G Wightwick Esq.r

Engraved by W H Lizars

An Account of some Experiments on the Electricity of Tourmaline, and other Minerals, when exposed to Heat. By James D. Forbes, Esq. F. R. SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh.

(*Read 3d January* 1832 *.)

Although the phenomena of the Pyro-Electricity of Minerals, as it has been termed, and those of the Tourmaline in particular, have, after a long period of neglect, been recently studied by more than one philosopher of eminence, there are a sufficient number of undetermined or debatable points, even at the threshold of the inquiry, to yield facts of novelty and interest to those who will take the trouble to look for them.

Having during the past summer been much engaged in studying the relations of bodies to heat and electricity, I was induced, by having in my possession a considerable number of long tourmalines, to repeat and endeavour to verify some recently published experiments with this mineral. These inquiries brought out several new facts; and, with the hope of adding something to our knowledge in this curious field, I have taken this opportunity of communicating to the Society the results of some very recent experiments.

My attention was principally directed to the verification and extension of the views of M. Becquerel, whose ingenious papers published in the *Annales de Chimie* for 1828, give us almost the only information, with the exception of a short paper

* The publication of this paper was delayed partly under the idea of prosecuting the experiments of which it contains an account; but the author having engaged in some other researches, which appear to him of more immediate importance, he merely prints this communication in its original form.—April 1834.

by Dr BREWSTER, published in 1824, which we have gained on this subject since the appearance of the works of HAUY. The undecided state in which several points of the first importance were left by the philosophers of the last century, is not a little remarkable. In fact, the answer to the fundamental question, of whether the tourmaline must be in the act of changing its temperature, in order to the development of electricity, may be considered to rest on the authority of BECQUEREL (who answered it in the affirmative), former authorities being divided upon it. Dr THOMSON, in his work on Heat and Electricity, published in 1830, observes that " when the tourmaline is once excited by heat, it retains its electricity for a long time, if care be taken to place it upon non-conductors. ÆPINUS found it electric after an interval of six hours *." He adds in a foot-note, " These facts, as stated by ÆPINUS, if accurate, seem inconsistent with the statement of CANTON and BECQUEREL, that the electricity is only developed whilst the stone is changing its temperature." A statement of Dr BREWSTER'S might also appear to support the views of ÆPINUS, and by opposing that of BECQUEREL, leave the question still undecided. He mentions †, that a slice of tourmaline cut transversely to the axis of the crystal, and placed on a plate of glass heated to 212°, adhered to it for six or eight hours, even when the glass was uppermost, the electricity of the tourmaline thus supporting its own weight.

The experiment which I am about to describe, will I think set at rest the question, and is in fact capable of shewing within a few minutes, and in a very pleasing manner, the most essential facts of the relation of the electricity to temperature. M. BECQUEREL found that when a crystal of tourmaline was heated to 212°, its electricity was inappreciable so long as the temperature remained stationary ; but that when placed in a cooler medium, the intensity of the electricity was not, as might have been ex-

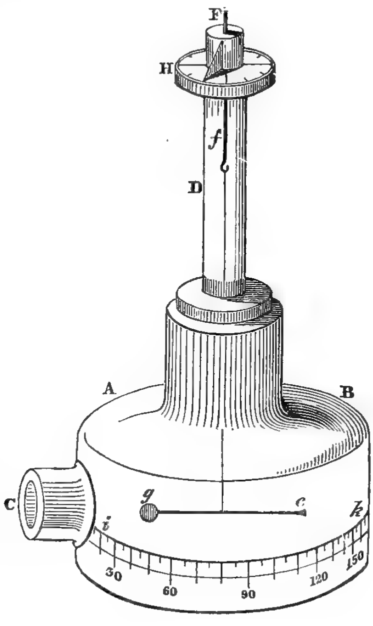
* P. 478.

† Edinburgh Journal of Science, i. 211.

pected, proportional to the rapidity of the change of temperature, which of course would correspond to the period at which the temperature was highest, but on the contrary rose gradually to a maximum, when the tourmaline was about half way cooled to the temperature of the apartment; then gradually diminishing, redescended to zero when it reached that point. This remarkable result M. Becquerel obtained by suspending the crystal horizontally by a fibre of silk under a glass cover, the temperature of the air in which he had the means of regulating; he then applied to the extremities of the crystal, wires from the opposite poles of a dry pile, and, counting the number of oscillations made by the tourmaline, deduced the intensity.

The form of the experiment which I have contrived, and which bears out M. Becquerel's conclusions, gives the same results with great elegance and simplicity, without attempting to indicate the precise temperature of the stone at any period, which, whilst the heat of the medium in which it is placed changes, can only be by M. Becquerel's experiment an approximation, since the interior of the crystal must at any moment have a different temperature from its surface.

I employed a simple form of Coulomb's electrometer, which I constructed for the purpose with little difficulty. A flat shaped bottle, AB, having a wide tubulature at C, was provided. Fitted to the neck is a tube D, plugged at top by a cork F, through which passes a crooked wire *f*, for the purpose of regulating a fibre of raw silk, supporting the needle of gum-lac *e*, one end of which is terminated by the disk *g* of gilt paper. The object to be examined is introduced through the tubulature C, the disk having previously been



charged with vitreous or resinous electricity ; and the repulsions occasioned by the presence of the body under experiment, are measured with sufficient exactness by the deviation of the needle reckoned on the divided circle of paper *i k*. I may add, that the graduated circle H at the top of the glass tube is employed to measure any required torsion of the silk fibre produced by causing the cork F to revolve. By means of this simple apparatus, I obtained results of greater accuracy than my objects generally required *.

Upon presenting to the electrometer, which is quite an insulator, an energetic crystal of tourmaline (which should not be too thin), heated to a considerable temperature, the gilt disk being charged with the same electricity as is acquired in cooling by the pole of the crystal presented, the following results appear :—At first the tourmaline exerts no influence whatever. But after its temperature has begun to fall, the repulsive power is gradually developed, and the gilt disk slowly recedes as the increasing force appears ; the recession becomes very minute, and at length reaches a maximum at which the needle remains for a time stationary. Soon, however, a diminution of repulsion takes place, the disk reapproaches its original point of rest, and, if left long enough, will return to the zero point precisely opposite the crystal, which then as at first produces no action whatever. It is unnecessary to point out how completely this verifies M. BECQUEREL's views, and demonstrates that, as soon as the temperature completely ceases to change, not the minutest vestige of electricity remains, though the insulation throughout should be maintained as completely as possible. This I generally ac-

* It is surprising that COULOMB's instrument has not been more employed in this inquiry. BECQUEREL seems only to have used it once, and Dr BREWSTER had recourse to the laborious and unsatisfactory method of causing pyro-electric crystals to lift fragments of the *Arundo Phragmites*, which can give no comparative results.

complish by heating and handling the crystal in glass test-tubes.

With regard to Dr Brewster's remarkable experiment, it partakes, I suspect, of a partly different class of phenomena. It occurred to me that it might perhaps, if confirmed, be explained thus :—The slice of tourmaline may be considered, in some respect, as an electrical *coating* to the glass. Suppose that the tourmaline and the glass are heated together, and that the side of the slice next the pole of the crystal, assuming vitreous electricity by cooling, is next the glass. Let the other side of the glass (which we shall call the second surface) communicate with the table or any other conductor ; by the law of Induction, then, it will assume resinous electricity, the first surface repelling the vitreous. Conceive the glass plate now to be insulated, we shall then have this state of things :—the surface of the tourmaline *farthest* from the glass, by its even excitation, is resinously electrified, for we have supposed the side which coats the first surface of the glass to be vitreous ; the resinous electricity, which is insulated at the second surface of the glass, is powerfully attracting the opposite electricity of the side of the tourmaline next itself, and prevents the recombination which would otherwise take place with the electricity of the other side or pole.

Having succeeded in repeating Dr Brewster's experiment with thin slices from a large crystal of black tourmaline, I found these hypothetical views confirmed. Having heated a slice cut transversely to the axis of the crystal, I laid it upon a plate of cold glass with the side which became vitreously electric during cooling uppermost ; the adhesion was presently complete, so that the glass could be held with the stone suspended from its under surface. The *other* surface of the glass, behind the tourmaline, being then touched with a minute disk of gilt paper, insulated on a thin stick of gum-lac, and then presented to the electrometer, resinous electricity was found of considerable intensity, shewing that a decomposition of electricity had actually taken place at the second surface of the glass, the resinously electric pole of the tourmaline forming the coating of its first surface, thus attracting the vitreous electricity of the second surface, and disengaging the resinous. Hence it is easy to see, that if the tourmaline remains sufficiently long warm to prevent the recombination of the electricities of its two poles, until the disengaged electricity at the second surface of the glass shall have been carried off by the air or otherwise, that recombination will be prevented, and the electric state will become comparatively permanent.

The use of COULOMB's electrometer, in the manner I have already described, affords an easy and general method of comparing the electric intensities of different crystals. For by measuring the maximum deviation produced by any specimen, we obtain, wholly independent of the exact temperature, a measure of its electric power, a measure independent of time, and, as experience shews, little if at all affected by the precise heat to which the crystal has at first been raised, at least within moderate limits. That the experiment admits, even with the most ordinary attention to collateral circumstances, of considerable accuracy, I have proved by repeating the measures of the intensity of a particular specimen several times in succession. It will at once occur, that a source of fallacy must be guarded against in the loss of the electricity with which the disk of the electrometer is charged, which, as it is constantly diminishing by the contact of air, would give the intensities last measured in a series of comparative experiments too small. In favourable circumstances, and by allowing the disk to remain some time charged before the series is commenced, it is surprising how little this error amounts to; I have always, however, avoided it in practice, by repeating every series of experiments in an inverted order, by which we obtain two observations at equal distances from a mean state of electric tension, the mean of which will give strictly comparable results.

The principal application which I made of this method of observing, was to attempt to discover some relation between the form and dimensions of crystals of tourmaline, and their electric power.

M. Becquerel, in a Second Memoir on the Tourmaline, published in 1828,* announced the rather extraordinary circumstance, that long Tourmalines did not become at all electric by heat, and that their facility of being excited was generally inversely proportional to their length. Dr Thomson, in his work on Heat,† mentions the former assertion, and observes that it is one which he has never had an opportunity of trying. As my experiments have been generally made with black Tourmalines from Van Diemen's Land, some of which are of great length, this point early occurred to me as one deserving of investigation. As these inquiries seem at no period to have excited much attention in this country, and as of late nothing whatever has been done upon them, these observations may prove the more interesting.

The longest Tourmaline employed by M. Becquerel, was six centimetres, or 3.2 English inches in length, with a diameter of about .08 inches. My largest Tourmaline is 3.25 inches, or almost precisely the same, with a diameter little different. Instead of finding this crystal "tout à fait refractoire," as M. Becquerel describes his, it proved uniformly susceptible of powerful excitement, under the very same treatment which I was accustomed to

* Ann. de Chimie. † P. 477.

use towards those of smaller dimensions. The intensity, too, was very great, though more slowly attained than in shorter ones. Various Tourmalines, between two and three inches in length, uniformly shew great activity on being removed from the heat to which they have been exposed, and left to cool, when applied to the electroscope.

This discovery led me to some inquiry into the effect of dimension in modifying electric action. Here it is necessary to draw a distinction between the case of excitation, and the intensity of the effect produced. M. BECQUEREL generally mentions the temperature at which electricity appeared: my inquiries have been directed to the maximum intensity of that electricity when excited, which is in some respects the more satisfactory information of the two. The determination of the temperature, we have already seen, is a point of great uncertainty, since every range of atoms, from the centre to the surface, must have a different temperature. Of course, for the same reason, the maximum effect is the integral of an infinity of variable forces.

Amongst many experiments on different groups of crystals, I may mention the following as the best determined. Six Tourmalines, all 1.3 inches long, whose thicknesses or areas of section were represented by the numbers 14, 11, 7, 6, and 4, had their maximum intensities measured. Two series of experiments in a direct order, and two reversed, all gave the same order of intensity for these specimens, which, instead of bearing any direct proportion to the areas, as might have been expected, where the lengths were equal, gave the following arrangement* in the order of in-

* The best pair of series gave,

No. 1,	Deviation 115°
2,	69.5
3,	0
4,	26
5,	39.5

tensities,—1, 2, 5, 4, 3, the areas following the natural order of the numbers. Other series being taken with sets of crystals, 1.2 and 1.8 inches long, gave similar indications of irregularity,* but the area of section has so far a general influence, that where the differences are considerable, the thickest crystal has almost universally the greatest power. The relative forces are so connected, that we can hardly impute the irregularities to any general law; the differences, as I shall immediately illustrate by reference to another class of experiments, must in all probability be attributed to a variable structure in specimens of the same mineral, as well as in those of different species.

I took a crystal $1\frac{1}{4}$ inches long, and carefully determined the intensity of its electricity, which, by a mean of three experiments, gave 45° of deviation. I immediately broke it at one-fourth of its length from one end, the two portions being then heated and their intensities determined each three times, the mean of the larger portion gave a deviation of 47°, of the smaller 43°, the mean of which gives precisely the original force. As far as intensity goes, the diminution of length would not therefore appear to be favourable to the development of electricity. With a view of procuring through a larger range of dimension, the influence of length alone, I selected a series of tourmalines whose sections were as nearly equal as possible, the diameter being about $\frac{1}{10}$th inch, and one of which was the very long crystal before mentioned. This experiment was made with great care; a

* Nos.	Intensity. 1.2 long.	Intensity. 1·8 long.
1 (thickest)	82°	54°
2	77.5	40°
3	50°	34°
4	57.5	35.5
5	65°	
6 (thinnest)	34°	

direct and reversed series were taken, and several of the determinations independently repeated. The mean deviations of the needle of the electroscope will be given in the following Table:

No.	Length.	Intensity.
1	3.25 inch.	79.°5
2	2.10	82°
3	1.60	60°
4	1.55	60°
5	1.35	89°
6	1.19	68°

We thus see that the long crystal holds a high place among those of equal section with it, and we have at the same time an additional proof of the native irregularities of different crystals.

It is well known that the artificial arrangement which represents best the phenomena of the tourmaline, is that of a series of insulated plates of glass arranged parallel to one another, suitably coated, and with the contiguous coatings connected by tinfoil. If one end of this battery be charged from an electrical source, while the other communicates with the ground, the plates at one extremity will partake of an excess of the electricity communicated, whilst those at the other will have the opposite species in excess, and a large proportion of the range in the centre will exhibit no traces of free electricity: hence, by shortening the pile (supposing the plates very numerous), no change will take place in the intensity of the free electricity, but the intensity will bear a direct relation to the *surface* of the plates, or the section of the pile. So far analogy supports the increase of intensity with the diameter of the tourmaline; but when we come to consider the mode of charging, it fails, and leaves us in great doubt as to whether the length of a crystal, if its structure be perfectly uniform, should have any influence or not. I have found short crystals of a considerable area, and so formed as to have a large surface, perhaps the most energetic.

The unequal temperature of the portion of any section, prevents, as I have already observed, all the parts from giving a maximum intensity at once. This will diminish the total effect, but as all the parts afford the same kind of electricity, the resultant can never be null on this account. Therefore, even if the irregularities of amount did not compel us to admit innate varieties of structure or electric disposition in different specimens, stubborn facts must force us to some such conclusion. In the course of my researches, I have met with a crystal of tourmaline,* possessing no external irregularities of structure, (the terminations, however, of the crystal are not preserved), which has the singular property of presenting in cooling a vitreous pole at *both* ends. Having ascertained this point, I proceeded to examine the electricity of its parts, by means of COULOMB'S *Proof-plane*, by which the electricity of any portion is insulated and examined. As I expected, I found the central portion of the crystal resinously electrified. This remarkable fact is not unexampled. HAUY has recorded the case of a crystal of topaz which had a similar property, which, from its analogy to known facts in the phenomena of magnetism and of double refraction, Dr BREWSTER conceived to be owing to the union of two distinct crystals, with the vitreous poles in contact, as in that case resinous electricity was developed at both ends. Be this as it may, the example of tourmaline which I have cited, proves that the junction of the separate crystals, if such exist, may be imperceptible, and as the probability that such irregularity should exist, however caused, is in proportion to the length of the specimen, this may perhaps explain the want of excitability observed by BECQUEREL in very long crystals.

The phenomena of tourmaline, though entirely electric, bear so strong an affinity to those of magnetism, that the study of

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* It is No. 3. of the Series, at the foot of p. 32.

their relations must be considered extremely important. I have therefore one remark to make upon an experiment which Dr BREWSTER thinks indicative of a "singular breach of analogy between the distribution of the pyro-electrical and magnetical forces." After observing that, in the process of reducing a magnet to powder, the coercive force employed effectually destroys all trace of magnetism, he adds that powder of tourmaline is highly electric when placed on a glass and heated, which is shewn by its adhering in conglomerated masses, exhibiting the appearance of viscidity when stirred. It appears to me that this experiment does not go to shew that tourmaline *in a state of excitation* does not lose its electricity when bruised in a mortar; indeed, such an experiment it would be impossible to perform. A tourmaline, when it is not changing its temperature, is as inert as a bar of iron before it is magnetized; the process of heating or cooling the one, is precisely equivalent to that of conveying magnetism by induction or otherwise to the other. The powder of tourmaline is, therefore, analogous to the filings of iron, both being equally inert, till the native electricity of the former, and the native magnetism of the latter, is decomposed, when the result in both is perfectly identical.

I shall now only very briefly allude to the conclusions to which some experiments on the electricity of other minerals besides tourmaline have led me. I have applied COULOMB's electrometer with perfect success to the examination of topaz, boracite, and mesotype, which have all been long known to possess electrical powers. In the case of these minerals, I have been able to extend BECQUEREL's remarkable law of the intensity of electricity rising to a maximum, when the speed of cooling has become comparatively low, which has not before been demonstrated for any mineral except tourmaline. Topaz possesses the remarkable property of retaining its electricity long after the temperature has ceased to change: probably the decomposition being effected with greater difficulty, the recombination requires more time than in the more excitable minerals. To so great an extent does this take place, that though the maximum deviation of the needle, in one instance amounting 115°, took place within a few minutes after the excited mineral was presented to the electroscope, in 20 minutes it was hardly diminished, in 40 minutes it was still 95°, in an hour 85° *. After a lapse of several hours it was still considerably excited. I obtained similar results with several crystals. Probably in all minerals the difficulty of decomposition and combination increase with the mass; hence, slender crystals are most easily excited, and the effect less permanent. Keeping these facts in view, Æpinus's statement that the tourmaline preserves its electricity, when insulated, for several hours, will admit of easy explanation, by supposing that he worked with large and difficultly excitable crystals, similar to this one of topaz, which had at the same time a very high degree of intensity.

With a large crystal of boracite having about $\frac{1}{3}$ inch for the side of its cube, I obtained very analogous results, when one of its four resinous poles was presented to the electrometer in a warm state, the disk slowly and regularly receded from zero as the cooling advanced, and in about ten minutes reached its maximum duration, which indicated a high degree of intensity. The diminution of electricity was very slow; in three quarters of an hour the disk had receded but a little way. A small crystal of boracite being similarly treated, the maximum was speedily gained, and the needle returned to zero in one experiment in 20 minutes, in another in half an hour. The electricity of the disk in these experiments was extremely steady.

The acicular crystals of mesotype attain with the greatest facility a high degree of electrical excitement, so much so, that

* The disk during this time was of course slowly parting with its charge.

it required some attention to discover that the maximum intensity was not immediately gained. It lasted a short time, and, as in the case of slender tourmalines, the needle rapidly receded, and in a short time returned to zero.

The very satisfactory results which I have obtained from all these minerals, give me great confidence in the aptitude and accuracy of my simple apparatus; and, from the very considerable intensity which I find them all to possess, I expect to be able to estimate much smaller degrees of pyro-electricity in other minerals, and in artificial crystals, than has yet been attempted.

Should my first results have appeared interesting to the Society, I may perhaps at no distant time have the honour of communicating my farther progress in the inquiry.

Greenhill,
2d January 1832.

A General View of the Phenomena displayed in the neighbourhood of Edinburgh by the Igneous Rocks, in their relations with the Secondary Strata; with reference to a more particular description of the Section which has been exposed to view on the south side of the Castle Hill. By Major-General Lord GREENOCK, C. B. F. R. S. Ed.

(*Read* 16*th Dec.* 1833.)

THE country in which Edinburgh is situated has no great elevation above the level of the sea, presenting a gently undulating surface, except where hills of igneous origin, in groups, or perfectly insulated, rise abruptly through the strata, which consist of the sandstones and shales of the coal-formation, with occasional beds of limestone, which they overlie, and this country is more or less covered by old and new alluvial deposits.

The views suggested by these hills to the penetrating genius of HUTTON, who may be justly considered the founder of modern geology, first led to the knowledge of the true nature and origin of the trap-rocks. Their analogy to those produced by existing volcanoes, and the phenomena observed in their relations with the secondary strata, leave no doubt as to their having been poured out from the interior of the earth in a fluid or viscid state, through fissures in the strata occasioned by subterranean convulsions—not, however, in the open air, like currents of lava from recent craters, but in sheets or masses at the bottom of the sea, their cooling and consolidation having evidently been slow and gradual, under great pressure, such as might be produced by a large volume of superincumbent water, as was ably illustrated by the experiments of the late Sir James Hall; or by their having been originally formed as dykes, at considerable depths, either below or among the strata.

The most striking of the phenomena which are to be seen in many of the hills near Edinburgh, are the apparent interstratification of the trap and secondary rocks, owing, in some cases, to the igneous matter having carried up portions of the latter, which are, in this manner, placed above the former on their summits, and in others to its having burst through, and overflowed the surface of the strata, or been injected between them; while, at the same time, it was probably lifting them up from below. Fragments broken away from the stratified rocks, by the passage through them of the igneous fluid, are also frequently seen imbedded in the trap, and the former are very generally observed to be altered in their nature and appearance, when in contact with the latter. Different varieties of trap-rocks are often met with in the same hills, shewing that they must have continued to be erupted at different intervals, and under varying circumstances, during a long period; but, in the absence of every formation superior to the coal measures, there are no data whatever by which their relative ages may be determined.

The igneous rocks almost invariably conform to the same general dip as the stratified rocks with which they are associated, thus affording additional evidence of their having been uplifted together by some common cause. A very distinguished naturalist has pointed out to the author the important fact, that the trap-hills appear in general to surround basins, often of considerable extent, in which the rocks of both descriptions are seen to dip outwards from a common centre, forming, as it were, valleys of elevation on an immense scale, and that the environs of Edinburgh afford an example of one of these basins or valleys; for, if lines be drawn north and south from Burntisland to the Pentland Hills, and east and west from Salisbury Craigs to Corstorphine Hill, they will form the anticlinal lines (or nearly so) from which these rocks will be found to decline in opposite directions.

The researches of several eminent philosophers relative to the interior temperature of the earth, have given rise to the hypothesis now very generally received, which refers the extensive changes of level between the sea and land, that have taken place in all ages, and may probably be going on, although in a less sensible degree, at the present moment, to the effect produced on its surface by the secular refrigeration of the globe, and its inequality in cooling. At so early a period in the history of our planet, as that which we are now considering, the heat being much greater, the process of refrigeration, and the consequent contraction of the crust of the earth, must have been more rapid, and the production of gases, from the superficial consolidation of the interior fluid, more abundant. The force, therefore, which in our days is seen to cause volcanic action, would then have been exerted with greater intensity, so as to have occasioned not only the ejection of a much larger quantity of the igneous matter to the surface, but also elevations of land, on a much more extensive scale than we can form any idea of, from the greatest effects of the most active volcanoes in modern times.

The rock upon which the Castle is situated, consists of greenstone of a hard and compact nature, and of a dark grey colour, approaching to black. It is throughout of the laminar structure, which, in several places, gives it a false appearance of stratification. Some of the lamina may be seen passing into the cuboidal or square prismatic structure, and occasionally to split into imperfectly columnar divisions. Within the Castle walls fragments of sandstone are imbedded in the trap, and near its south-west point, altered rocks, apparently of sandstone and slate-clay, are observed resting upon it in a highly inclined position.

The situation of the section of the Castle Hill, lately made in cutting the new South-west Road, and the relative position of the stratified rocks displayed in it, will be better understood by a reference to the accompanying drawing, which has been very ably and correctly executed by Dr GREVILLE, F. R. S. Ed., than by any written description (See Plate). The first appearance of disturbance commences to the eastward of the limits of the drawing in that direction, and about 200 yards from the point of junction between the secondary rocks and the trap, where the former may be observed to rise at an angle of about 11° or 12° above the level of the road, crossing a footpath that leads up the declivity. Owing, however, to the partial covering of soil and vegetation, several of the strata can be traced only here and there, by their edges cropping out. There appear to be about five or six strata of sandstone in all, varying in thickness from two or three feet to a few inches, the intervals between them being occupied by beds of slate-clay, or perhaps of marl, of a reddish-brown or purple colour; but in their present state, neither the continuity of the one, nor the nature of the other, can be distinguished with any accuracy.

The irregularity and want of conformity in the stratification of these rocks, and the curvatures observed in them, are very remarkable, giving such strong evidence, that, whatever the circumstances might have been which influenced their original deposition, they must subsequently have experienced a considerable derangement, as to leave no doubt in the author's opinion, that the signs of disorder so manifestly visible in this section, are to be attributed to some subterraneous agency. In this view, the only rational explanation that occurs to him is, that they were occasioned by that general disturbing cause, which probably acted upon a large extent of country at the period when it was elevated above the waters, beneath which it had been originally formed. Considering the subject, therefore, in this light, it appears probable that the whole of the sandstone strata, with the intervening beds of slate-clay or marl, had, on the first impulse they received during the process of elevation, a tendency to rise in a uniform direction; but, in consequence of some violent disturbance that must suddenly have interrupted the regularity of this movement, a considerable fault or dislocation was occasioned towards the centre of the section, by which they became bent and distorted in the manner now seen,—their eastern portions having been thrown up, whilst their western portions were cast down, so as to lie unconformably upon the upturned strata. Near the point of junction of these rocks with the trap, the effects of subsidence are very evident, the shattered extremities of the strata having apparently fallen over, being at the same time forced into a more southerly direction, by which they seem to have been brought obliquely into contact with the tabular masses of greenstone, against which they appear to have been precipitated, and upon which the inverted fragments of sandstone, with intervening portions of slate-clay or marl, are now seen to rest.

If the trap had been in a soft state from fusion at the time, some intermixture of the igneous fluid with the stratified rocks must have taken place during this collision; but no imbedded fragments are to be met with, nor any veins passing from the one into the other; neither are there any marks or impressions visible on the smooth surface of the greenstone, except some slight superficial scratches, such as might have been caused by abrasion during the simultaneous passage upwards of these two descriptions of rocks, if both had been consolidated previous to their elevation. The fragments, however, imbedded in the upper part of the rock, as well as the altered nature of some of the contiguous strata, shew that, at some period, this rock must have been in a state of fusion, and capable, by its intense heat, of affecting, in this manner, the stratified rocks in its vicinity.

Although at first sight it might appear difficult to reconcile these opposite and apparently contradictory facts, if we can admit that the stratified rocks had been deposited in seas or lakes, and that the trap had also a subaqueous origin, it must follow that the dry land which they now form, must, at some time or other, have been elevated above the waters, when, in all probability, a considerable modification of their relations with each other must have taken place. We may therefore conclude, that all these trap-rocks have undergone elevation at two distinct periods, —the first when formed in a state of igneous fluidity at the bottom of the sea, the second when, with the whole district in which they are situated, they were lifted up to their present positions in a hard and consolidated state.

The convulsions, which must have been frequent during this latter period, would doubtless have been most violent at those points which had before been the principal foci of volcanic energy; and although there is no appearance in this place of the igneous matter having then reached the surface, an expansion of the interior fluid, while operating these changes of level, may very probably have filled by injection rents or fissures in many parts of the strata below, forming, in this manner, the dykes which are often met with in mining operations, and occasioning many of the faults and dislocations observed in those above, where the cause itself is not perceptible.

Consequently, there appear to be sufficient grounds for considering, that, during the first period, when the greenstone was in a state of fusion, the fragments of sandstone, which are enveloped in it near the summit of the rock, were carried upwards by the erupted fluid, the intense heat of which has probably, at the same time, altered the nature of some of the contiguous stratified rocks. But the derangement of the strata, and all the other signs of disturbance seen in the section of the Castle Hill, are, in the opinion of the author, to be referred to the earthquakes and other commotions of the second period, after the consolidation of these rocks; for, although local circumstances may have contributed, to a certain extent, in producing these appearances, yet, when we can trace the operation of the same influence to extend over a large district of country, determining its features and the inclination of the strata, we may fairly conclude, that this has been due to some more general cause, of which these separate phenomena have only been subordinate effects; and if it be granted that any such elevation as that here supposed has taken place, there seems to be every reason for ascribing this cause to that epoch.

Several years have now elapsed since this interesting section was first laid open, and the public are indebted for the preservation of so instructive a record of the changes and revolutions which have given rise to the present state of the earth, to the consent of the Magistrates of the City then in office, obtained through the active exertions of the distinguished Professor of Natural History in this University, and of his excellent friend the late much lamented Treasurer of this Society, Mr THOMAS ALLAN of Lauriston, whose whole life was spent in the cultivation of science and in contributing to its advancement.

Postscript.—Since the foregoing pages were written, Mr MACGILLIVRAY, of the Royal College of Surgeons, has very obligingly put into the hands of the author, a sketch of the section of the Castle Hill, made by him when it was first laid open in 1829, from which it appears that a mass of trap was then exposed to view, lying beneath the disturbed strata. This igneous rock was so soon covered up in making the road, that it escaped general notice. It is described, however, by Mr MACGILLIVRAY as being totally different, both in appearance and character, from that upon which the Castle is situated, and more nearly resembling the dyke that was met with in excavating for the foundation of the Cowgate Bridge.

Description and Analysis of a Mineral from Faroe, not before examined. By Arthur Connell, Esq. F.R.S.Ed.

(*Read Jan.* 6. 1834.)

Some time ago a mineral was put into my hands by Mr Rose, the intelligent mineral dealer of this city, the nature of which was not known, although it had been conjectured to be a variety of Mesotype. Mr Rose obtained it from Count Vargas Bedemar of Copenhagen, who brought it from the Faroe Islands, and subsequently visited this country some years ago. A short chemical examination soon satisfied me that it differed entirely from mesotype; and having ascertained that it contained silica, lime, water, and potash, and no notable quantity of alumina, I was led to conjecture that it would prove to be a variety of apophyllite, although, in that view, it would have presented very remarkable deviations from the ordinary structure of that mineral.

Some time afterwards, in the course of last autumn, I had an opportunity of recognising, in the possession of Sir David Brewster, a large mass of the mineral in question; and as its nature was quite unknown to Sir David, he thought it would be a matter of some interest to have its chemical nature fully determined, particularly with the view of ascertaining whether it could possibly be of the nature of apophyllite.

I accordingly proceeded to complete an analysis of it; and the result was, that it was found to differ from all other mineral bodies with which I am acquainted. Its external and chemical characters, as well as the steps and result of the analysis, are as follows.

The colour of the mineral is white, with an opalescent tint.

Its lustre is glistening and vitreous. Its hardness is intermediate between that of fluor and that of felspar. It possesses a considerable degree of translucency. Its texture is imperfectly fibrous; but in a few places the fibres appear diverging with considerable regularity, showing some approach to a crystalline structure. Its specific gravity, at 67° Fahrenheit, was found to be 2.3616: at 55°, it was 2.3622; we may take 2.362 as a mean. The most characteristic peculiarity of this mineral is its remarkable toughness and difficult frangibility. To such an extent does this quality extend, that a mass of it cannot be broken into smaller fragments without great difficulty and repeated blows of a heavy hammer.

The chemical characters of the mineral were as follows:—Exposed to heat in a glass-tube, it gave off water in considerable abundance, which did not affect the colour of litmus or turmeric, and did not stain Brazil-wood paper yellow, nor in the slightest degree corrode the tube. Neither was the glass in any degree attacked by the moisture expelled when the mineral was heated in an open tube with previously fused salt of phosphorus. Urged by the blowpipe in the platinum forceps, it did not swell up, but became opaque and white, and fused only on the edge, and with difficulty. Alone, on charcoal, the result was the same, except that the fusion on the edges was scarcely visible. With soda, it melted with effervescence into a semitransparent glass. With salt of phosphorus, it gave a colourless glass, showing silica as an opaque mass, in the midst of a clear globule, which, however, opalesced on cooling, especially after the heat had been continued some time. With borax, it fused into a transparent colourless glass. With soda, on platinum foil, no manganese reaction could usually be observed, although occasionally a feeble tint was visible. With nitrate of cobalt, a fragment of the mineral gave no trace either of alumina or magnesia. In order to ascertain whether it contained any phosphoric acid, a little of the mineral in powder was heated to drive off the water, and then ignited in a tube with potassium ; and after the excess of potassium had been removed by mercury, no trace of the smell of phosphuretted hydrogen could be observed on blowing into the tube.

When the mineral in fine powder was treated with muriatic acid, it gelatinized immediately, with considerable increase of temperature. Even when in coarse powder, there was speedy gelatinization of the finer particles, and the larger were gradually acted on. The external weathered coating of the mineral effervesced slightly with acid, owing to partial decomposition and absorption of carbonic acid from the air ; but the fresh mineral did not show the least trace of effervescence.

In a preliminary examination of a portion of the mineral which was decomposed by muriatic acid, the only constituents found were silica, lime, a little potash, and traces of one or more metallic oxides. Magnesia was sought for, but none found. The presence of water, and absence of fluoric and phosphoric acids, have already appeared.

The steps of the analysis were as follows :—

a, 20.66 grains of the mineral, in small fragments, lost by ignition 3.04 grains, equivalent to 14.714 per cent. of water. The ignited mineral became opaque, and whiter.

b, 24.68 grains were reduced to coarse powder in the steel mortar, and were treated with muriatic acid. Immediate gelatinization of a part took place, with rise of temperature, and the mineral and acid were left in contact with one another in a close vessel till complete decomposition appeared to have taken place. From accidental circumstances, the substances were left in contact in the present instance for several months. It was thought better not to reduce the mineral to fine powder at first in the usual manner in a siliceous mortar, as, from the great tenacity and considerable hardness of the mineral, a good deal of silica would probably have been abraded from the mortar.

c, Water was now added to the mass, and heat applied. Silica was separated by filtration, and, after edulcoration and ignition, it weighed 14.35 grains. It dissolved in boiling potash ley except a residue of .47 of a grain, which shall be afterwards noticed. There was thus left of silica 13.88 grains.

d, The liquid separated from silica was treated with ammonia in some excess. A few flocks only fell, becoming gradually brown. These were separated by filtration, and, after ignition, weighed .14 of a grain.

e, These ignited flocks were digested in muriatic acid, and were dissolved, except a residue of .02 of silica. The solution was boiled with excess of caustic potash. The precipitate remaining undissolved was separated by filtration, and the liquid supersaturated with muriatic acid, and treated with carbonate of ammonia. A slight muddiness ensued; but the quantity or nature of the precipitate was not determined, from its small amount.

f, The residue left by the caustic potash in *e*, was dissolved in muriatic acid, boiled with a little nitric acid, neutralized by ammonia, and precipitated by benzoate of ammonia. The precipitate, ignited with a little nitric acid, yielded .04 of oxide of iron. The residual liquid was boiled with carbonate of soda. A white precipitate fell, which, after ignition, weighed .05. This, when examined, proved to be oxide of manganese, mixed with some lime; but as the proportion of lime could not be determined, the whole may be held as red oxide of manganese, equivalent to .046 of protoxide, although, from the reaction of the mineral on platinum foil with soda, formerly mentioned, the real quantity of that oxide was apparently less.

g, The liquid which had been precipitated by ammonia in *d*, after concentration by heat, was brought nearly to the boiling point, and carbonate of ammonia added. A plentiful white precipitate fell, which, after being collected by filtration, and duly dried, weighed 11.74 grains. This precipitate, which was carbonate of lime, dissolved without residue in diluted muriatic acid. A few drops of hydrosulphuret of ammonia were added to the solution, and a minute quantity of sulphuret of manganese got, which, by treatment with muriatic acid, and precipitation with potash, yielded .01 of red oxide of manganese, or .009 of protoxide; leaving 11.73 of carbonate of lime, equivalent to 6.602 of lime.

h, The liquid separated from carbonate of lime in *g*, was evaporated to dryness, and the ammoniacal salt driven off by heat. A small quantity of a white saline residue remained, which, ignited, weighed .39. This residue, redissolved in water, left .07 of silica, giving .32 for the weight of salt.

i, To the saline solution in *h*, a few drops of carbonate of ammonia were added, by which .02 more of carbonate of lime were got, equivalent to .11 of lime, and to .22 of chloride of calcium, which, subtracted from the saline residue of .32, gave now .298 for the salt. The solution of the salt was not affected by hydrosulphuret of ammonia.

k, After the saline solution had been again evaporated, the residue ignited and redissolved, cubical crystals were obtained by spontaneous evaporation. To the solution of these crystals, chloride of platinum was added, when, after a little, a yellow precipitate formed; and the liquid being crystallized spontaneously, crystals of the double chloride of platinum and sodium were obtained, which were taken up by weak alcohol. The residual double chloride of platinum and potassium, duly dried, weighed .3, equivalent to .0580 of potash, and to .0916 of chloride of potassium, leaving for chloride of sodium .2064, equivalent to .1099 of soda. The double chloride of platinum and sodium decomposed by sulphate of ammonia and heat, yielded efflorescent crystals of sulphate of soda.

l, The residue of .47 left by the potash ley in *c*, was resolved by fusion with carbonated alkalies, and other necessary steps, into .27 of silica, .04 of oxide of iron, and .01 of lime.

We have thus in 24.68 grains of the mineral, exclusive of water,

Silica,	*c*,	13.88	
	e,	2	
	h,	7	
	l,	27	
		———	14.24
Lime,	*g*,	6.602	
	i,	11	
		———	6.613
Soda,	*k*,	.109	.109
Potash,	*k*,	.058	.058
Oxide of Iron,	*f*,	.04	
	l,	.04	
		———	.08
Oxide of Manganese,	*f*,	.046	
	g,	.009	
		———	.55
			21.650

And in 100 parts,

Silica,	57.69
Lime,	26.83
Water,	14.71
Soda,	0.44
Potash,	0.23
Oxide of Iron,	0.32
Oxide of Manganese,	0.22
	100.44

The above composition differs, so far as my information extends, from that of all other known minerals; and shews the mineral under analysis to be essentially a hydrated quatersilicate of lime. Its constitution is expressed by the formula $9\,S^4\,C + 16\,Aq$, which gives

Silica,	58.06
Lime,	26.85
Water,	15.08
	99.99

From table-spar, the mineral obviously differs in containing water as an essential constituent, and in the relative proportions of silica and lime, table-spar being a bisilicate of lime. From those zeolites which do not contain alumina as a necessary ingredient, such as apophyllite, and Dr Thomson's wollastonite, it differs, in respect that the quantity of alkali is too small to be viewed as an essential constituent, as well as in the relative proportions of silica and base.

As soon as I had completed the analysis, I sent an account of the result to Sir David Brewster, who was so kind as to examine a few of the optical properties, and to give me the following notice of his observations:

" My Dear Sir,

" I ought to have thanked you sooner for your letter of the 5th December. There can be no doubt that the mineral is a new one, and I have made various ineffectual attempts to obtain crystals from the surface of the large specimen which I have of it, and which I received from Count Vargas Bedemar. The crystallized faces are perfectly distinct, but they lie so near the general surface, that it is impossible to detach any fragment without pounding it.

" I have, however, ground and polished a piece of the tough mass, and have ascertained that it possesses double refraction. It is also opalescent, reflecting a bluish light, and consequently transmitting a yellowish one. I have also found that its powder possesses no pyro-electricity. I cannot help thinking, that, by a careful examination of different specimens, small crystals will be found in some of the superficial cavities. I am, My Dear Sir, ever faithfully yours,

" D. Brewster."

" *Belleville, Dec.* 26. 1833."

On receiving this letter, I examined the specimens with greater care, to endeavour to discover crystals, and I could observe with a lens, in several places, in one of them faces apparently belonging to minute crystals, and reflecting a fine lustre; but still the portions so observed were too small, and the connection of the faces with one another not sufficiently apparent to enable me to draw any conclusion as to the crystalline form. As I have every reason, however, to hope that larger quantities of the mineral are in the possession of Count BEDEMAR, more light may possibly still be thrown on that matter.

Its other external characters will, I trust, be held as sufficiently distinctive, especially when supported by the chemical analysis; and in case mineralogists shall be willing to receive it as a new species, I should propose to distinguish it by the name of *Dysclasite* *, as expressive of the peculiarity in its external characters formerly mentioned, its remarkable tenacity and difficult frangibility. It will of course be arranged with the Zeolites.

Since this paper was read, I have had a thin slice of the mineral cut and polished by a lapidary; and Sir DAVID BREWSTER, who was so obliging as to examine it, observed the same appearances as before, and found that it polarised light in all directions, shewing that the mineral consists of a congeries of crystals, adhering together in all positions. This structure may serve to explain its great tenacity.

* From δυς and κλαω.

Researches on the Vibration of Pendulums in Fluid Media. By GEORGE GREEN, Esq. Communicated by Sir EDWARD FFRENCH BROMHEAD, Bart. M. A. F. R. SS. Lond. & Ed.

(*Read* 16*th Dec.* 1833.)

PROBABLY no department of Analytical Mechanics presents greater difficulties than that which treats of the motions of fluids; and hitherto the success of mathematicians therein has been comparatively limited. In the theory of the waves, as presented by MM. POISSON and CAUCHY, and in that of sound, their success appears to have been more complete than elsewhere; and if to these investigations we join the researches of LAPLACE concerning the tides, we shall have the principal important applications hitherto made of the general equations upon which the determination of this kind of motion depends. The same equations will serve to resolve completely a particular case of the motion of fluids, which is capable of a useful practical application; and, as I am not aware that it has yet been noticed, I shall endeavour, in the following paper, to consider it as briefly as possible.

In the case just alluded to, it is required to determine the circumstances of the motion of an indefinitely extended non-elastic fluid, when agitated by a solid ellipsoidal body, moving parallel to itself, according to any given law, always supposing the body's excursions very small, compared with its dimensions. From what will be shown in the sequel, the general solution of this problem may very easily be obtained. But as the principal object of our paper is to determine the alteration produced in the motion of a pendulum by the action of the surrounding medium, we have insisted more particularly on the case where the ellipsoid moves in a right line parallel to one of its axes, and have thence proved, that, in order to obtain the correct time of a pendulum's vibration, it will not be sufficient merely to allow for the loss of weight caused by the fluid medium, but that it will likewise be requisite to conceive the density of the body augmented by a quantity proportional to the density of this fluid. The value of the quantity last named, when the body of the pendulum is an oblate spheroid, vibrating in its equatorial plane, has been completely determined, and, when the spheroid becomes a sphere, is precisely equal to half the density of the surrounding fluid. Hence, in this last case, we shall have the true time of the pendulum's vibration, if we suppose it to move *in vacuo*, and then simply conceive its mass augmented by half that of an equal volume of the fluid, whilst the moving force with which it is actuated is diminished by the whole weight of the same volume of fluid.

We will now proceed to consider a particular case of the motion of a non-elastic fluid over a fixed obstacle of ellipsoidal figure, and thence endeavour to find the correction necessary to reduce the observed length of a pendulum vibrating through exceedingly small arcs in any indefinitely extended medium to its true length *in vacuo*, when the body of the pendulum is a solid ellipsoid. For this purpose, we may remark, that the equations of the motion of a homogeneous non-elastic fluid are

$$V-\frac{p}{\rho}=\frac{d\phi}{dt}+\tfrac{1}{2}\left\{\left(\frac{d\phi}{dx}\right)^2+\left(\frac{d\phi}{dy}\right)^2+\left(\frac{d\phi}{dz}\right)^2\right\} \quad \ldots\ldots\ldots\ldots (1.)$$

$$0=\frac{d^2\phi}{dx^2}+\frac{d^2\phi}{dy^2}+\frac{d^2\phi}{dz^2} \quad \ldots\ldots\ldots\ldots (2.)$$

Vide *Mec. Cel.* Liv. iii. Ch. 8. No. 33, where ϕ is such a function of the co-ordinates x, y, z of any particle of the fluid mass, and of the time t that the velocities of this particle in the directions of and tending to increase the co-ordinates x, y and z shall always be represented by $\frac{d\phi}{dx}$, $\frac{d\phi}{dy}$, and $\frac{d\phi}{dz}$ respectively. Moreover, ρ represents the fluid's density, p its pressure, and V a function dependent upon the various exterior forces which act upon the fluid mass.

When the fluid is supposed to move over a fixed solid ellipsoid, the principal difficulty will be so to satisfy the equation (2.) that the particles at the surface of this solid may move along this surface, which may always be effected by making

$$\phi = \left(\lambda + \mu \int_{\infty} \frac{df}{a^3 bc}\right) x \,* ; \quad \ldots\ldots \quad (3.)$$

supposing that the origin of the co-ordinates is at the centre of the ellipsoid: λ and μ being two arbitrary quantities constant with regard to the variables x, y, z; and a, b, c, f being functions of these same variables, determined by the equations

$$a^2 = a'^2 + f, \quad b^2 = b'^2 + f, \quad c^2 = c'^2 + f, \quad \text{and} \quad \frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1; \quad \ldots\ldots \quad (4.)$$

in which a', b', c' are the axes of the given ellipsoid.

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* In my memoir on the Determination of the exterior and interior Attractions of Ellipsoids of Variable Densities, recently communicated to the Cambridge Philosophical Society by Sir Edward Ffrench Bromhead, Baronet, I have given a method by which the general integral of the partial differential equation

$$0 = \frac{d^2 V}{dx_1^2} + \frac{d^2 V}{dx_2^2} + \ldots\ldots + \frac{d^2 V}{dx_s^2} + \frac{d^2 V}{du^2} + \frac{n-s}{u} \frac{dV}{du}$$

may be expanded in a series of a peculiar form, and have thus rendered the determination of these attractions a matter of comparative facility. The same method applied to the equation (2.) of the present paper, has the advantage of giving an expansion of its general integral, every term of which, besides satisfying this equation, may likewise be made to satisfy the condition (6.). The formula (3.) is only an individual term of the expansion in question. But in order to render the present communication independent of every other, it was thought advisable to introduce into the text a demonstration of this particular case.

To prove that the expression (3.) satisfies the equation (2.), it may be remarked, that we readily get, by differentiating (3.)

$$\frac{d^2\phi}{dx^2}+\frac{d^2\phi}{dy^2}+\frac{d^2\phi}{dz^2}=\frac{2\mu}{a^3bc}\frac{df}{dx}+\frac{\mu x}{a^3bc}\left(\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}+\frac{d^2f}{dz^2}\right)-$$
$$-\frac{\mu x}{a^3bc}\left(\frac{3}{2a^2}+\frac{1}{2b^2}+\frac{1}{2c^2}\right)\left\{\left(\frac{df}{dx}\right)^2+\left(\frac{df}{dy}\right)^2+\left(\frac{df}{dz}\right)^2\right\}$$

Moreover, by the same means, the last of the equations (4.) gives

$$\frac{df}{dx}=\frac{\frac{2x}{a^2}}{\frac{x^2}{a^4}+\frac{y^2}{b^4}+\frac{z^2}{c^4}},\qquad \left(\frac{df}{dx}\right)^2+\left(\frac{df}{dy}\right)^2+\left(\frac{df}{dz}\right)^2=\frac{4}{\frac{x^2}{a^4}+\frac{y^2}{b^4}+\frac{z^2}{c^4}}$$

and $$\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}+\frac{d^2f}{dz^2}=\frac{\frac{2}{a^2}+\frac{2}{b^2}+\frac{2}{c^2}}{\frac{x^2}{a^4}+\frac{y^2}{b^4}+\frac{z^2}{c^4}}$$

which values being substituted in the second member of the preceding equation, evidently cause it to vanish, and we thus perceive that the value (3.) satisfies the partial differential equation (2.)

We will now endeavour so to determine the constant quantities λ and μ that the fluid particles may move along the surface of the ellipsoidal body of which the equation is

$$1=\frac{x^2}{a'^2}+\frac{y^2}{b'^2}+\frac{z^2}{c'^2} \quad\ldots\ldots\ldots\ldots\ldots\ldots (5.)$$

But, by differentiation, there results

$$0=\frac{x\,dx}{a'^2}+\frac{y\,dy}{b'^2}+\frac{z\,dz}{c'^2}$$

and as the particles must move along the surface, it is clear that the last equation ought to subsist, when we change the elements dx, dy and dz into their corresponding velocities $\frac{d\phi}{dx}$, $\frac{d\phi}{dy}$ and $\frac{d\phi}{dz}$. Hence, at this surface,

$$0=\frac{x}{a'^2}\frac{d\phi}{dx}+\frac{y}{b'^2}\frac{d\phi}{dy}+\frac{z}{c'^2}\frac{d\phi}{dz} \quad\ldots\ldots\ldots\ldots (6.)$$

But the expression (3.) gives generally

$$\frac{d\phi}{dx} = \lambda + \mu \int_{\infty} \frac{df}{a^3bc} + \frac{\mu x}{a^3bc}\frac{df}{dx}, \quad \frac{d\phi}{dy} = \frac{\mu y}{a^3bc}\frac{df}{dy}, \quad \frac{d\phi}{dz} = \frac{\mu z}{a^3bc}\frac{df}{dz} \quad \ldots\ldots\ldots \; (7.)$$

and, consequently, at the surface in question, where $f = 0$,

$$\frac{d\phi}{dx} = \lambda + \mu \int_{\infty}^{0} \frac{df}{a^3bc} + \frac{\mu x}{a'^3b'c'}\frac{df}{dx}, \quad \frac{d\phi}{dy} = \frac{\mu y}{a'^3b'c'}\frac{df}{dy}, \quad \frac{d\phi}{dz} = \frac{\mu z}{a'^3b'c'}\frac{df}{dz}.$$

These values, substituted in (6.), give, when we replace $\frac{df}{dx}$, $\frac{df}{dy}$, and $\frac{df}{dz}$ with their values at the ellipsoidal surface,

$$0 = \lambda + \mu \int_{\infty}^{0} \frac{df}{a^3bc} + \frac{2\mu}{a'b'c'} \quad \ldots\ldots\ldots \; (8.)$$

which may always be satisfied by a proper determination of one of the constants λ and μ, the other remaining entirely arbitrary.

From what precedes, it is clear, that the equation (2.), and condition to which the fluid is subject, may equally well be satisfied by making

$$\phi = \left(\lambda' + \mu' \int_{\infty} \frac{df}{ab^3c}\right) y \text{ and } \phi = \left(\lambda'' + \mu'' \int \frac{df}{abc^3}\right) z;$$

provided we determine the constant quantities therein contained by means of the equations

$$0 = \lambda' + \mu' \int_{\infty}^{0} \frac{df}{ab^3c} + \frac{2\mu'}{a'b'c'} \text{ and } 0 = \lambda'' + \mu'' \int_{\infty}^{0} \frac{df}{abc^3} + \frac{2\mu''}{a'b'c'}$$

respectively. The same may likewise be said of the sum of the three values of ϕ before given. However, in what follows, we shall consider the value (3.) only, since, from the results thus obtained, similar ones relative to the cases just enumerated may be found without the least difficulty.

Instead now of supposing the solid at rest, let every part of the whole system be animated with an additional common velocity — λ in the direction of the co-ordinate x. Then, it is clear, that the equation (2.), and condition to which the fluid is subject, will still remain satisfied. Moreover, if x', y', z' are now referred to three axes fixed in space, we shall have

$$x' = x - \int \lambda dt, \qquad y = y', \qquad z = z'$$

and if X′ represents the co-ordinate of the centre of the ellipsoid referred to the fixed origin, we shall have

$$X' = -\int \lambda \, dt \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (9.)$$

Adding now to ϕ the term $-\lambda x$ due to the additional velocity, the expression (3.) will then become

$$\phi = \mu x \int_{\infty} \frac{df}{a^3 bc}$$

and the velocities of any point of the fluid will be given, by means of the differentials of this last function. But ϕ and its differentials evidently vanish at an infinite distance from the solid, where $f = \infty$; and consequently, the case now under consideration is that of an indefinitely extended fluid, of which the exterior limits are at rest, whilst the parts in the vicinity of the moving body are agitated by its motions.

It will now be requisite to determine the pressure p at any point of the fluid mass. But, by supposing this mass free from all extraneous action $V = 0$, and if the excursions of the solid are always exceedingly small, compared with its dimensions, the last term of the second member of the equation (1.) may evidently be neglected, and thus we shall have, without sensible error,

$$-\frac{p}{\rho} = \frac{d\phi}{dt} \qquad i.e. \qquad p = -\rho \frac{d\phi}{dt}$$

or, by substitution from the last value of ϕ,

$$p = -\frac{d\mu}{dt} \rho x \int_{\infty} \frac{df}{a^3 bc}$$

Having thus ascertained all the circumstances of the fluid's motion, let us now calculate its total action upon the moving solid. Then, the pressure upon any point on its surface will be had by making $f = 0$ in the last expression, and is

$$p_0 = -\frac{d\mu}{dt} \rho x \int_{\infty}^{0} \frac{df}{a^3 bc}.$$

Hence we readily get for the total pressure on the body tending to increase, x

$$P=\int ds\,(p_0'-p_0'')=\frac{d\mu}{dt}\rho\int_{\infty}\frac{df}{a^3bc}\times\int 2x\,ds=\frac{d\mu}{dt}\rho v\int_{\infty}^{0}\frac{df}{a^3bc};$$

v representing the volume of the body, p_0'' the pressure on that side where x is positive, p_0' the pressure on the opposite side, and ds an element of the principal section of the ellipsoid perpendicular to the axis of x.

If now we substitute for μ its value given from (8.), the last expression will become

$$P=\frac{a'b'c'\rho v\int_0^{\infty}\frac{df}{a^3bc}}{2-a'b'c'\int_0^{\infty}\frac{df}{a^3bc}},\ \frac{d\lambda}{dt}\ \ldots\ldots\ldots\ldots\ldots\ldots \quad (10.)$$

Having thus the total pressure exerted upon the moving body by the surrounding medium, it will be easy thence to determine the law of its vibrations when acted upon by an exterior force proportional to the distance of its centre from the point of repose. In fact, let $\rho_,$ be the density of the body, and, consequently, $\rho_,v$ its mass, gX' the exterior force tending to decrease X'. Then, by the principles of dynamics,

$$0=\rho_,v\frac{d^2X'}{d^2t}+gX'-P.$$

If, now, in the formula (10.) we substitute for λ its value drawn from (9.), the last equation will become

$$0=\left(\rho_,+\frac{a'b'c'\int_0^{\infty}\frac{df}{a^3bc}}{2-a'b'c'\int_0^{\infty}\frac{df}{a^3bc}}\rho\right)v\frac{d^2X'}{dt^2}+gX'$$

which is evidently the same as would be obtained by supposing the vibrations to take place *in vacuo*, under the influence of the given exterior force, provided the density of the vibrating body were increased from

$$\rho_{,} \text{ to } \rho_{,} + \frac{a'b'c'\int_0^\infty \frac{df}{a^3bc}}{2 - a'b'c'\int_0^\infty \frac{df}{a^3bc}} \rho \quad \ldots\ldots\ldots\ldots \quad (11.)$$

We thus perceive, that, besides the retardation caused by the loss of weight which the vibrating body sustains in a fluid, there is a farther retardation due to the action of the fluid itself; and this last is precisely the same as would be produced by augmenting the density of the body in the proportion just assigned, the moving force remaining unaltered.

When the body is spherical, we have $a' = b' = c'$, and the proportion immediately preceding becomes very simple, for it will then only be requisite to increase $\rho_{,}$ the density of the body by $\frac{\rho}{2}$, or half the density of the fluid, in order to have the correction in question.

The next case in point of simplicity is where $a' = c'$, for then

$$\int_0^\infty \frac{df}{a^3bc} = \int_0^\infty \frac{df}{a^4b} = 2\int_{b'}^\infty \frac{db}{a^4} \quad \ldots\ldots\ldots\ldots \quad (12.)$$

If $a' > b'$, or the body, is an oblate spheroid vibrating in its equatorial plane, the last quantity properly depends on the circular arcs, and has for value

$$(a'^2 - b'^2)^{-\frac{3}{2}} \left\{ \frac{\pi}{2} - \text{arc} \left(\tan = \frac{b'}{\sqrt{(a'^2 - b'^2)}} \right) \right\} - \frac{b'}{a'^2(a'^2 - b'^2)}$$

If, on the contrary, $a' < b'$, or the spheroid, is oblong, the value of the same integral is

$$\tfrac{1}{2}(b'^2 - a'^2)^{-\frac{3}{2}} \log \frac{b' + \sqrt{(b'^2 - a'^2)}}{b' - \sqrt{(b'^2 - a'^2)}} + \frac{b'}{a'^2(b'^2 - a'^2)}$$

Another very simple case is where $c' = b'$, for then the first of the quantities (12.) becomes, if $a' > b'$

$$(a'^2 - b'^2)^{-\frac{3}{2}} \log \frac{a' + \sqrt{(a'^2 - b'^2)}}{a' - \sqrt{(a'^2 - b'^2)}} - \frac{2}{a'(a'^2 - b'^2)}$$

and if $a' > b'$, the same quantity becomes

$$2(b'^2 - a'^2)^{-\frac{3}{2}} \left\{ \text{arc} \left(\tan = \frac{a'}{\sqrt{(b'^2 - a'^2)}} \right) - \frac{\pi}{2} \right\} + \frac{2}{a'(b'^2 - a'^2)}$$

By employing the first of the four expressions immediately preceding, we readily perceive, that, when an oblate spheroid vibrates in its equatorial plane, the correction now under consideration will be affected by conceiving the density of the body augmented from

$$\rho_{\prime} \text{ to } \rho_{\prime} + \frac{\frac{\pi}{2} a'^2 b' - a'^2 b' \operatorname{arc}\left(\tan = \frac{b'}{\sqrt{(a'^2 - b'^2)}}\right) - b'^2 \sqrt{(a'^2 - b'^2)}}{2(a'^2 - b'^2)^{\frac{3}{2}} - \frac{\pi}{2} a'^2 b' + a'^2 b' \operatorname{arc}\left(\tan = \frac{b'}{\sqrt{(a'^2 - b'^2)}}\right) + b'^2 \sqrt{a'^2 - b'^2}} \rho$$

When b' is very small compared with a', or the spheroid is very flat, we must augment the density

$$\text{from } \rho_{\prime} \text{ to } \rho_{\prime} + \frac{\pi}{4} \frac{b'}{a'} \rho \text{ nearly};$$

and we thus see that the correction in question becomes less in proportion as the spheroid is more oblate.

In what precedes, the excursions of the body of the pendulum are supposed very small, compared with its dimensions. For, if this were not the case, the term of the second degree in the equation (1.) would no longer be negligible, and therefore the foregoing results might thus cease to be correct. Indeed, were we to attend to the term just mentioned, no advantage would even then be obtained; for the actual motion of the fluid, where the vibrations are large, will differ greatly from what would be assigned by the preceding method, although this method consists in satisfying all the equations of the fluid's motion, and likewise the particular conditions to which it is subject. It would be encroaching too much upon the Society's time to enter on the present occasion into an explanation of the cause of this apparent anomaly: it will be sufficient here to have made the remark, and, at the same time, to observe, that when the extent of the vibrations is very small, as we have all along supposed, the preceding theory will give the proper correction to be applied to bodies vibrating in air, or other elastic fluid, since the error to which this theory leads cannot bear a much greater proportion to the correction before assigned, than the pendulum's greatest velocity does to that of sound.

On the Force of the Latin Prefix Væ *or* Ve *in the Composition of Nouns and Adjectives.* By the Rev. Archdeacon Williams, F.R.S. Ed. Rector of the Edinburgh Academy.

(Read 3d March 1834.)

The Society has lately been delighted with the discovery of organic remains in our vicinity, which prove, that, at some distant period of unrecorded time, the fertile plains of Mid-Lothian must have furnished food and habitation to beings very different from those which now draw life and enjoyment from their productions. Yet it must be regretted, that, in the conclusions derived from the existence of such remains (with the exception of a few general principles, leading us to regard with deeper feelings of awe and reverence Him who has made all things so wonderfully and fearfully), we have little with which we can sympathise, and still less that can link our existence with that of those of which we see nothing but these imperishable monuments. I confess, however, with the partiality naturally felt by every man for that study to which he may have principally devoted his time and attention, that I regard with far deeper feelings those fleeting sounds—those επεα πτεροεντα—which, after passing from lip to lip uninjured during the lapse of so many ages, are, when carefully examined, found to have been component parts of those languages, falsely denominated dead, and which can be usefully adduced in illustration of the written records of those mighty spirits who have as yet as far surpassed us in the science of mind, in the purity of their taste, and the perfection of language, as we have surpassed them in the severer sciences—in unfolding the mysterious powers of matter—in discovering the laws according to which they operate —and in subjecting them to the dominion of man. It is with these feelings, and also from a wish to contribute to the great work now so admirably carried on by the German scholars, and by some distinguished fellow-labourers in this country, that of uniting the present and the past, the remote and the near, by proving the consanguinity of the great Caucasian branch of the human family, that the following statement has been drawn up on the force of the Latin particle *ve* or (as it was written by the more ancient Romans) *væ* *.

.... in tenui labor.

Ve seems to have been in common use at an early period of the Roman Republic, as we find it in connection with some of the most ancient part of their mythology, as in *Vedius*, *Vejovis*, and *Veflamen*. Yet when their critics and grammarians, the natural produce of a later period, and of more quiet times, began to inquire into the origin and first meaning both of words in common use and of obsolete expressions, *ve* or *væ* no longer existed, except as a part of compounded words. By comparing, however, in certain cases, the simple word, as *grandis*, with its compounded form, as *vegrandis*, they were enabled to ascertain its original force, without, however, proceeding further, and applying such a discovery to the solution of many etymological difficulties. "*Ve*" (says Festus) "was prefixed by the ancients to a small thing, whence *Vejovis*, little Jupiter †." Soon after, he adds, "they used *ve* instead of *very small* ‡." Aulus Gellius,

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* "Ve, particula quæ in aliis atque aliis vocabulis variatim per has duas literas, cum a litera media immissa dicitur."

† "Ve enim syllabam præponebant parvæ rei, unde Vejovem, parvum Jovem." —Valpy's Delphin. p. 1005. under *Vesculus*.

‡ "Ve pro pusillo utebantur."—Ibid. eod. pag. under *Vescus*.

in his Noctes Atticæ, goes further, and says, "The particle *ve* has a double and even contrary signification, for it has the power both to increase and diminish the meaning *." This royal mode of solving the knot has often been adopted by ignorant persons more anxious to give a ready answer, than to confess their doubts, and thus excite others to explain the difficulty. It is like the answer of the Cambridge man, who, shewing more than a common want of knowledge, was at last ironically asked, if he knew whether it was the sun that turned round the earth or the earth round the sun, and answered, that they did duty alternately; that sometimes the earth turned round the sun, and that at other times the sun turned round the earth. It may be safely affirmed, that no one word (and a particle is a word) ever could have borne two contrary meanings, and that, if such be apparently the case, it can only arise from the fact, that two words, owing to the action of time, have been ground down into the same form. Such was the case with the Greek particle ἀ, called both privative and intensive. The first represents *αν*, the Greek form of the Latin *in*, not the English *un*: the second represents the Greek ἅμα, although at times it loses its aspiration. I shall subsequently shew, that *væ* does not increase the meaning of any Latin word.

Ovid, in the third book of his Fasti, has the following lines on the etymology of Vejovis:

> "Nunc vocor ad verbum, vegrandia Farra coloni
> Quæ male creverunt vescaque parva vocant.
> Vis ea si verbi est, cur non ego Vejovis Ædem,
> Ædem non magni suspicer esse Jovem."

Which may be thus translated: "Now for the meaning of the word, husbandmen call the grain which has not well filled, *ve-*

* "*Ve* enim particula * * * duplicem significatum eundemque inter sese diversum capit. Nam et augendæ rei et minuendæ valet."

grandia, and small grain they call *vesca*. If such be the force of the word, why should I not suspect that the temple of Vejovis is the temple of Jovis, not large," (of the little Jupiter).

Ovid probably derived this information from the works or conversation of Verrius Flaccus, the learned grammarian, to whom Augustus entrusted the education of his grandsons Lucius and Caius Cæsar*. On that occasion, he transferred the whole body of his then existing pupils to the Palatium, on condition that no new member was to be admitted. It was undoubtedly owing to the advantages of his new situation, of his increased and increasing leisure, and the boundless command of books in the Palatine Library, that he was enabled to accomplish his great work "De Verborum Significatione," which can be regarded in no other light than an Encyclopædia, containing an explanation of every word connected with the language, the laws, the religion, and the public and domestic life of the ancient Romans. As he had before him the works of Cato †, of Varro ‡, of Julius Cæsar ‖, of Messala §, of Appius Claudius Pulcher ¶, of Sinnius Capito,**, and of others who had treated of such subjects, and who had all the opportunities of gaining the requisite knowledge, there cannot be a doubt that he had embodied in it all that could be known on such subjects, by men, to whom one great

* "Ab Augusto nepotibus ejus præceptor electus transiit in Palatium cum tota schola, verum ut ne quem amplius posthac discipulum reciperet."—Suet. *De Gram.*

† Cato, his "Origines," and many other dissertations on similar subjects.

‡ Varro, his great work "De Lingua Latina," of which we still have a portion.

‖ Julius Cæsar, his work "On the Analogy of the Latin Language."

§ The eloquent Messala of Horace, called also by him Corvinus. He wrote a book "In Explanatione Auguriorum, &c. duodecim tabularum;" also, "De Dictis involute."

¶ Claudius Pulcher, he wrote a book concerning "Scientia Auguralis."

** Sinnius Capito.

source of their language was unknown, and who looked for a solution of all their difficulties in the only remaining source, namely, the language and literature of the Greeks *.

Had we been fortunate enough to have received this great work in its original state, we should not have, for so long a period, so utterly misunderstood the early history, antiquities, and constitution of Rome. But this, like many other works, more useful than amusing, have miserably suffered under the knife of the abbreviator, that most efficient member of the Society for the Diffusion of Useful Knowledge, who, in his anxiety to diffuse, has often succeeded in draining the real sources.

First, one Festus Pompeius, a man known to us by this bad deed alone, but who is supposed by learned men to have lived under one of the Christian Emperors, undertook to republish the work of Verrius Flaccus on the following principles: "It is my intention, in selecting from the great number of his books, to omit all words already dead and buried, and often, as he confesses himself, of no [present] use and authority, and to reduce all the rest, as briefly as possible, into a very few books †." If this work had reached us, mangled and imperfect as it must have been after such a process, our loss would not have been so great as it really is; for Festus had evidently some knowledge of his subject, and a great wish fairly to represent the sentiments of his principal, without either sneering at what he did not understand, or misrepresenting that which he did not like. But we have only the shadow of the shade. For, in the eighth century, one

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* The Roman who had recourse to the Greek language alone, for a solution of his difficulties, was as helpless as the English scholar, who, neglecting the Anglo-Saxon, expects to find all the necessary knowledge in Latin and French.

† "Cum propositum habeam ex tanto librorum ejus numero intermortua jam et sepulta verba, atque ipso sæpe confitente nullius usus aut auctoritatis, præterire, et reliqua quam brevissime redigere in libros admodum paucos."—Festus, under the word *Profanum*, Delph. ed. Valp. p. 554.

PAULUS, a Lombard monk, justly termed by the great SCALIGER an illiterate barbarian, compelled FESTUS to undergo the same process which VERRIUS FLACCUS had undergone under the pruning hands of FESTUS. In a dedication to no less a person than CHARLES the Great, the restorer of the Western Empire, as the degenerate Italians falsely called him, PAULUS, justly terming himself *ultimus servulus*, thus writes: "Being anxious to add to your library, and having no means of my own, I have been compelled to borrow from another. Now FESTUS POMPEIUS, a man deeply versed in Roman literature, has, in explaining the origin both of obscure expressions, and of certain circumstances, extended his work to twenty prolix volumes. I, therefore, omitting every thing superfluous and unnecessary in his tedious work, thoroughly enucleating with my own pen certain abstruse points, leaving some few things as I found them, have offered this abridgment to the perusal of your Highness *." In the words of ANTONINUS AUGUSTINUS†, the learned bishop of Ilerda (for there have been most learned bishops even in Spain), "this book gave such satisfaction to the unlearned men of the age, that it was substituted for FESTUS in all libraries." "So that in a short time" (adds DACIER‡) "a true copy of FESTUS was not to be found." At a later period, a small fragment of the original FESTUS was discovered in Illyricum, and published by ALDUS. This,

* "Cupiens aliquid vestris bibliothecis addere, quia ex proprio perparum valeo, necessario ex alieno mutuavi. FESTUS denique POMPEIUS, Romanis studiis affatim eruditus, tam sermonum abditorum, quam etiam quarundam causarum origines aperiens, opus suum ad viginti usque prolixa volumina extendit, ex qua ego, prolixitate superflua quæque et minus necessaria prætergrediens, et quædam abstrusa penitus stylo proprio enucleans nonnulla ita ut erat relinquens, hoc vestræ Celsitudini legendum compendium obtuli."

† "Is liber indoctis viris adeo placuit ut pro FESTO in omnibus bibliothecis substitueretur."—VALP. Delph. FEST. p. 12.

‡ "Unde brevi factum est ut verus FESTI liber non amplius apparuerit."—Ibid. p. 7.

when compared with the corresponding portion of PAULUS, enables us to discover the extent of the sins both of omission and commission perpetrated by the monkish barbarian. The mischief caused by omission is of course irreparable; but, under the able guidance of JOSEPH SCALIGER, we may exercise our ingenuity in discovering his counters, and separating them from the more valuable productions of the Roman mint: Yet it is a conjectural art, where it is scarcely possible to persuade another to agree with you in all points; consequently that science, as yet without a name, and of which a small portion only is comprehended under the modern acceptation of the word Philology, has sustained a severe loss, as far as Latin literature is concerned, from the united labours of FESTUS and PAULUS. It must, however, be confessed, that it was principally from this book, maimed, mutilated, and mangled as it is, that the great NIEBUHR drew those materials which enabled him to reconstruct the edifice of the early history of Rome, and infuse life into what had previously been nothing but a confused heap of lifeless bones.

I have thought it right to make these previous observations, in order to account for the many absurd derivations of Latin words commonly found in our dictionaries, many of which (although assuredly not all) are to be fathered upon this miserable PAUL, whose mutilated copy of FESTUS was almost literally transcribed by the first modern lexicographers of the Latin language.

I must here notice a question which has been often asked, Why men like CATO, CÆSAR, VARRO, and MESSALA,—why a VERRIUS FLACCUS or a QUINCTILIAN, should be so anxious about the use and etymology of words, seeing the framers of the language themselves regarded not such trifles, and left them to the researches of future and less active ages? To answer this question fully, and at the same time conclusively, would require a volume. But it may be briefly stated, that a language in its infancy has but few radical terms, and that these are confined to the expression of our common feelings and actions, and to the names and qualities of the substances with which we are conversant. To each word, therefore, a particular idea is attached, and as these roots take various forms, from the addition of simple and well known prefixes and affixes, the shades of meaning gradually multiply, and yet the transition is never so sudden as to prevent the original idea from being still presented to the hearer's mind, as his principal guide, in ascertaining the new meaning. This can proceed only for a limited period. Political convulsions, originating in disgust with the past, and in sanguine hopes for the future, a rage for novelty, and consequently the necessity imposed upon the orators and poets of the day of courting the public favour by new metaphors, new versions of the old, and fresh importations of new words, operate so powerfully, that, in many cases, the original idea entirely disappears, and nothing remains for our guidance but the gratuitous meaning which every body is supposed to attach to a certain term. I need not say, that, in such a case, no two individuals will agree in attaching the same meaning to a word thus let loose from its moorings, and sent forth to wander in the boundless ocean of possible combinations. That hence misapprehensions must arise, with all their inevitable train of strifes, quarrels, and combats.

To prevent the deterioration of language is impossible. To fix it down on *certain* principles, and to guard it from the caprice of fashion, the thirst of notoriety, and the slow but sure attacks of time, is beyond our power. But the evil can be confined within a certain range; the transgressor can be forced to err within certain limits, and the path may be pointed out through which, in better times, a return to first principles may be secured. Thanks to the Great God who made language the main instrument of rational man, its substance (as far as the lapse of ages during which the great Caucasian family of man has existed can lead us to infer) is indestructible. Although apparently the most fleeting, the most evanescent of all the accidents connected with man in his temporal state, it has hitherto proved the most imperishable. Its forms may vary; it may assume as many shapes as the plastic element which the mystic theology of the Greeks represented under the graphic mythos of ὁ Πρωτευς (of him the first, του πρωτου); but when once firmly grasped, when boldly interrogated, and bound by the strict chains which judicious etymology has fabricated for the purpose, it will resume its original form, and enable us to recognize the still existing features—the *disjecta membra* of some well known and yet living dialect.

This can be truly said of the particle *ve*, or rather *væ*, which still exists in the language of the Scottish peasant, under the same form, and, as far as can be ascertained, almost with the same sound, with which it was pronounced more than twenty-five centuries ago in the vicinity and streets of ancient Rome. But I would not wish it to be understood that Scotland alone has retained the word, which, on the contrary, has been, or still is, a useful portion of many other languages, as may be seen from the following table, where the precedence is given to those dialects which have retained the original guttural sound, more or less softened:

Cimbric,	Băch, Văch, softened Vā	*small, little.*
Gallic,	Beag	
Persian,	Bega, Beja	
Teutonic,	Fabe	
Greek,	Βαι, -ος	
Scottish,	Wee	

To prove that the Latin *ve* bore the same meaning, and was in fact cognate with these general forms, it will be necessary to examine every word to which it is prefixed, in its pure state as a long syllable.

Vecors, insane, deficient in sense, literally a man with a small heart *. In Cicero's words, " to others, the heart seems to be

* Κεαρ, κῆρ, cor, cordis, heart.

the mind: hence men are called *vecordes, excordes, concordes;* and Scipio Nasica acquired the surname of *Corculum* from his prudence *." On the same principle, the great wisdom of Solomon is described as " a *large* heart †," the contrary to which would of course be expressed, as in the Latin *vecordia,* by smallness of heart. We have no right to blame the ancients for making the heart the organ of intellect, as we are ourselves, equally unphilosophically, in making that powerful but senseless muscle the source and seat of all our benevolent and malignant emotions: while those among us who pride themselves on assigning the several faculties of the mind to their respective organs, are relapsing into the interminable circle of human errors, and make our sense and folly, our benevolence and malignity, dependant upon the proportional magnitude of the supposed organs.

Vedius, " Pluto, called also Dis ‡," as we are informed by Martianus Capella ||. One of the most difficult tasks that a scholar has to perform, is to acquire a distinct idea of the Latin mythology in its pure state, before it was intermixed, and consequently corrupted, by the wilder and more imaginative inventions of the priests and poets of Greece. In the hands of the latter, Dis, Dios, or Zeus, became exclusively the god of thunder. Among the earlier Latins, we find his duties confined to the operations of light, and that only during the day. " The ancients," says Festus, " called day-lightning Dium fulgur, because they attributed it to Jupiter, as night-lightning was attributed to

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* " Aliis cor animus videtur, ex quo vecordes, excordes, concordesque dicuntur, et Nasica ille prudens Corculum."—Tusc. i. 9.

† " And the Lord gave Solomon largeness of heart."

‡ Διος, dius, dies, day (originally light); hence daze, dazzle, dawn, longer form Dianus, pronounced and written at a later period Janus.

|| " Vedius, id est Pluton, quem etiam Ditem dixere."—Lib. ii. p. 40.

Summanus *," (Pluto, the god of night). Now, *ve* or *væ* is used either adverbially, like the Latin *parum* or *minus*, answering to the English affix *less*, or like the adjective *parvus*, small. As Dius was the god of day, so Vedius, according to the first-mentioned power of *ve*, would signify the god of night, and correspond both in office and name with the same mythological being called by the Greeks ΑϝιδηϚ, the god of darkness, corrupted first to ΑιδηϚ, and lastly into ʽΑδηϚ or Hades.

Vejovis †, parum juvans, or parvus Jovis, either the not aiding or the small Jupiter.

Aulus Gellius, a Roman nobleman and scholar who lived about the middle of the second century of the Christian era, throws much light upon this word as well as the preceding. "In ancient inscriptions we see the names of these gods, Dijovis and Vejovis. Vejovis has even a temple between the arx and the Capitol. The explanation of whose names I find to be this; the ancient Latins named Jove from juvando, and, by adding pater, formed the second name; for Jupiter is formed by contracting and changing certain letters, from the full and entire word Jovispater. Thus combined, we have also Neptunuspater, and Saturnuspater, and Marspater (for such is Marspiter); and Jovis was also called Diespiter, *i. e.* father of day and light. Consequently he was also called Dijovis and Lucetius, words of similar import, because he aided us, and bestowed upon us light and day, our very life, as it were. As, therefore, they named Jovis and Dijovis from juvando, they, on the other hand, called that

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* "Dium fulgur appellabant diurnum quod putabant Jovis, ut nocturnum Summani."—Under *Dium*, p. 227.

† Jovis, from *juvo*, to aid (anciently Jovo, in composition Ju, Cimbric, *Jou*). The Latin deities were originally male and female. Hence, from Dianus and Janus, Diana and Jana; and, as rex, regis, makes regina, so the original female deity of Jovis was first Jovina, shortened into Juno, on the same principle as Jovis became *Ju*.

god who is not capable of aiding, but has the power of hurting, Vejovis *." Whether we admit the explanation of Gellius, or incline to that proposed by Ovid, the meaning of *ve* remains the same. But as the poet describes him as the youthful Jupiter, with youthful look and unarmed hand †, the latter seems the most probable. It is also in unison with the spirit of ancient mythology, to worship the same god under different names with different attributes, and with different ceremonies. Here we see Jupiter worshipped as a child, commonly as *magnus* or *altus*. But not even to Jupiter Latialis himself, the patron of the Latin confederacy, and genius of the Mons Albanus, would the Roman condescend to pay his deepest homage. That was reserved for the tutelar god of the Capitol, the patron of the Eternal City—Jupiter Capitolinus, Jupiter Optimus Maximus. This feeling may be illustrated by the zeal with which, in Catholic countries, the respective merits and powers of our Lady of the Pillar—of our Lady of the Rock—or of our Lady of Loretto—are impugned or defended by their several votaries, who yet allow them all to be one and the same person.

Vepallida, parum pallida, by no means pale, blushing, red-faced, or rather flushed. The original reading in the passage at the close of Horace's second satire, was *vepallida*. The older

* "In antiquis spectionibus nomina hæc Deorum inesse animadvertimus, Dijovis et Vejovis. Est autem etiam ædes Vejovis Romæ, inter arcem et Capitolinum; suorum nominum rationem hanc esse comperi. Jovem Latini veteres a juvando appellavere, eundemque, alio vocabulo juncto *patre* dixerunt. Nam quod est, elisis aut immutatis quibusdam literis, Jupiter, id plenum atque integrum est Jovispater, sic et Neptunuspater, et Saturnuspater, et Marspater (hoc enim est Marspiter); itemque Jovis Diespiter dictus est, et Lucetius. Quum Jovem igitur et Dijovem a juvando nominassent, eum quoque Deum contra, qui non juvandi potestatem, sed vim nocendi haberet, Vejovem appellaverunt,"—*Noctes Atticæ*, lib. v. cap. 12.

† "Jupiter est juvenis juveniles aspice vultus,
Adspice deinde manum fulmina nulla tenet."

commentators, misled by the authority of GELLIUS, interpreted *vepallida* " very pale." BENTLEY fell foul of the word itself, pronounced it naught, and proposed to read " ne pallida." Later commentators and editors read " vel pallida," to the ruin of the sense and spirit of the whole passage, which is subjoined in a note *. The scholar who will read the whole attentively, cannot fail seeing that the clause is not an alternative, but a necessary consequence of " vir rure recurrat." BENTLEY's emendation does not destroy the sense, although it does the spirit of the passage. It retards the almost simultaneous occurrence of the several incidents, and assigns an epithet by no means felicitous to the offending dame. Whoever will compare a passage in the Augustus of SUETONIUS † with JUVENAL's line,

" Vexatasque comas vultusque auresque calentes,"

and with another in the Nero of SUETONIUS ‡, must needs confess, that the " curiosa felicitas" so deservedly ascribed to HORACE, must have sadly deserted him, had he applied " pallida" to a culprit so circumstanced, however great her alarm. The old reading was undoubtedly the right one, and its signification is parum pallida, " flushed," and far from being cool and pale enough to face her husband.

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* " Nec vereor, ne, dum futuo, vir rure recurrat,
Janua frangantur, latret canis, undique magno
Pulsa domus strepitu resonet, vepallida lecto
Desiliat mulier, miseram se conscia clamet."
HOR. *Sat.* lib. i. sat. 2. ver. 126. et seq.

† " MARCUS ANTONINUS objecit fœminam in cubiculum obductam, rursus in convivium, *rubentibus* auriculis, incomtiore capillo reductam."

‡ " Egressus triclinio cum maxime placitam sevocasset, paulo post, recentibus adhuc lasciviæ notis, reversus."

Vesanus, parum sanus *, insane. There is no dispute, nor can any arise, concerning this word.

Vescus and *vesculus* †, having little to eat, half-starved, not well grown. Had not there been two Latin words, both written at present *vescus* (of which *vesculus* is only the diminutive), *vescus* and *vesculus* might have been dismissed as briefly as *vesanus*. But as Gellius has produced *vescus* as illustrative of his principle, that *ve* had an intensive as well as a privative force, we must examine his argument. After advancing the doctrine before alluded to, he adds, " for Lucretius calls the sea *vescum* (corrosive) in one sense; Lucilius uses *vescum* (nice or spare eating) in another sense ‡." In this quotation A. Gellius has shown *that* ignorance of which every philological inquirer who does not examine into the sources of the language under examination must often be convicted. *Esca* must originally have had the digamma, as may be proved from its verb *vescor*, which has nothing intensive in its nature. " Dii" (writes Pliny) " neque escis nec potionibus vescuntur." It was consequently from its original and digammated form that the Lucretian adjec-

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* The different forms under which *sanus* presents itself are very extraordinary. Its oldest Greek form is the Homeric σαος, which must have been a secondary form, as may be inferred from the Teutonic *safe*, verb *save*, which retains the digamma rejected by the Greek. Σαος is also σῶος in Homer; and it is remarkable that this latter form, when used for mental saneness, kept its digammated sound, Σοφος. The Latins introduced a liquid before the *v*, as *salvus*, then dropped the digamma in the noun *salus*, from which, by substituting one liquid for another, they made a new adjective, *sanus*, and a new noun, *sanitas*. It is from the last adjective that the various forms of *sain*, *sano*, *sound*, *gesund*, *sund*, *zond*, in the French, Italian, English, German, Danish, and Dutch languages, have been derived. With *salus* and *salvus* are cognate hail, health, weal, wealth, with their innumerable offspring.

† *Esca*, from *edo*, supine *esum*.

‡ " Aliter enim Lucretius *vescum salem* dicit, ex edendi intentione: aliter Lucilius vescum appellat cum edendi fastidio."

tive *corrosive* was formed, while the Lucilian *vescus* was a compound of *væ* and *esca*, and must originally have differed in orthography and pronunciation from the other.

Ve-sbius *, adjective *vesbinus*, an ancient name for the mountain now called Vesuvius (Monte di Somma), parum extinctus, scarcely extinguished. Raoul Rochette, the laborious historian of the Grecian colonies, assigns the foundation of the Italian Cumæ to the twelfth century before Christ. This may be doubted; but most assuredly the Greek settlements on the shores of the Bay of Naples cannot be referred to a late era. The Chalcidian colony of Cumæ soon spread along the coast, and Dicæarchia (afterwards Puteoli), Parthenope, Palæopolis, Neapolis, Pompeii, Herculaneum, and other names of pure Greek origin, attest the extent of their prosperity. These colonies assigned to the modern Ischia the name of Inarime, because Homer had mentioned that the "couch of Typhoeus" was ειν Αριμοις †; and the modern Procida they called Προχυτα, the "poured forth." Hence, we may infer that the former was an active volcano when the Greeks arrived on this coast, and that the latter emerged from the sea during their residence on these shores. Hence also the name "Phlegræi Campi" applied to the district between Puteoli and Neapolis. It is consequently by no means improbable that the same Greeks bestowed an appropriate name on that mountain, which looked

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* Σβεννυμι, σβεσω (root σβεω), English, *quench* (provincial *squench*). When the Greeks threw the *qu* out of their alphabet, they replaced it in general, although not in all cases, by π or β. Replace the *qu*, and the identity of σβεννυμι with the English *quench* or *squench* becomes immediately visible. I am preparing an essay upon that wonderful reform of the Greek alphabet, by which they threw out all guttural and drawling sounds, and replaced them by the long vowels and double consonants. I shall submit the leading facts in a paper to the Royal Society, before I publish the work.

† Ειν Αριμοις οθι φασι Τυφωεος εμμεναι ευνας. This is the only allusion in the Homeric writings to volcanic action.

down upon and almost overshadowed their settlements of Neapolis, Pompeii, Herculaneum, and Stabiæ. The Greeks were far more accurate observers of nature than the Romans. Pliny the elder, though a dweller in the immediate neighbourhood of Vesuvius, and destined to perish under its renewed activity, makes no further mention of this mountain in his great work, than to say that it looks down upon these cities, and that it was celebrated for a certain grape. But Strabo, although only a passing traveller, has given us a graphic description of its summit, "as being a barren spot, surrounded on all sides by a most fertile district, having a cinderous appearance, and caverns burnt out in the rocks, so that it might be safely inferred that the place had formerly been an active volcano, and contained fiery craters." For such a summit, what could have been a happier epithet than *Ve-sbius*, parum extinctus, which must have particularly suited its state on the first arrival of the Greeks, ten centuries before the famous eruption in A. D. 79? As to the word itself, we find it written Vesevus, Vesuvius, Vesvius, and Vesbius; Greek, Ουεσσουιον ορος, and Βεσβιος. Lucretius seems to have softened the pronunciation into Vesevus, and Virgil followed him. But it is to be remarked, that all the poets who call it Vesbius or Vesvius, wrote after the eruption, when public attention had been called to it, and when its local name was likely to be better ascertained. Nor would I willingly believe, that servile imitators of Virgil, like Silius Italicus * and Statius †, would have ventured on a new form of a word already fixed by the authority of the two great national poets, had it not been the local and original name.

* "Sic ubi vi cæca tandem devictus ad astra
Evomuit pastos per sæcula Vesbius ignes."
Lib. xvii. ver. 597.

† "............................... Chalcidis
Littoribus fractas ubi Vesbius erigit iras."
Silv. lib. iv. carm. 4. ver. 79.

Should my theory be correct, and I firmly believe such to be the case, the trifling particle *væ*, in conjunction with history and tradition, may enable us to form a probable conjecture as to the period of time during which the fires of Vesuvius hushed themselves in "grim repose."

Vesica *, a purse, a bladder, literally a small sack, a "*wee sackie.*" As the vowel *a* in simple Latin words is changed into *i* in composition, as *manus*, *facies*, *caput*, form *eminus*, *superficies*, *occiput*, so the *a* of *saccus* became *i* in *vesica*. The absence of the second *c* only proves the antiquity of the composition; because, as Festus informs us, "the ancient Latins never doubled a letter. It is to Ennius that the change of this custom is ascribed; for he being a Greek, followed the custom of his countrymen, who, both in writing and speaking, doubled mutes, semivowels, and liquids †." *Vesica* (apparently because its origin was unknown) seems to have escaped remodelling, when *saccus* and its derivatives admitted the double mute. The difference of termination between *saccus* and *vesica* is of no consequence, as such is frequently the case even with the same noun, both in Greek and Latin. The learned Varro uses *vesica* in its original sense for *sacellus* (our satchel) or *sacculus*. "Such fish-ponds of the nobility are more for prospect than profit," "magis ad oculos pertinent quam vesicam," "and rather exhaust than fill the owner's purse" (marsupium) ‡. It is from such passages that the

* Σακκος, *saccus*, said to be the most general of all names. The Anglo-Saxon *saec* comes nearest in sound and form to the *sica* of *vesica*. The Greek σακκος originally a hide or skin, took a secondary form, with one sigma, to denote an inflated hide or bladder, Ασκος.

† "Nomen antiquæ consuetudinis per unum *c* enunciari, non est mirum, quia nulla tunc geminabatur litera in scribendo; quam consuetudinem Ennius mutavisse fertur utpote Græcus. Græco more usus, quod illi æque scribentes ac legentes duplicabant mutas semivocales et liquidas."—Festus, under *Solitaurilia*, p. 878.

‡ "Illæ autem piscinæ nobilium magis ad oculos pertinent quam ad vesicam

etymologist is enabled to discover the first meaning of a word, and they are to him what an *experimentum crucis* is to the natural philosopher. This Varronian interpretation will justify us in translating Juvenal's " vetulæ vesica beatæ" a rich old lady's purse.

Vespa, a wasp, literally a small σφηξ or σφαξ *. Aristotle, in his description of Σφηκες, says: " There are two kinds of Σφηκες: of these the wild ones are scarce, larger, longer, and more darkly coloured than the others, also variegated, all having stings, and being very fierce. A wound inflicted by them is more painful than that inflicted by the others, as their sting is proportionally larger." This kind is evidently our hornet, which the Latins called *crabrones*. Aristotle then proceeds to describe the smaller species, which he calls ἡμερωτεροι, more familiar with man, which is undoubtedly our wasp, *vesphax*, softened into *vespa*.

Vesperus, and *vesperugo*, both the evening star and evening-tide, literally the time when objects become indistinct, as the night is coming on †. No etymologist whose works have been read by me, has attempted an explanation of this important word. They have been satisfied with referring us to the Greek ἑσπερος, as if the Greek were not the same word, deprived of the digamma. What could have induced the Greeks to carry their hostility to this and other letters, is a difficult question. But it is certain that they did so, even in cases where it was ne-

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et potius marsupium domini exinaniunt quam implent."—*De Re Rust.* lib. iii. cap. 17.

* Σφηξ, from σφαξ or διασφαξ, cut in two, corresponding with the Latin *Insecta.*—*De Animalibus*, lib. ix. cap. 28.

† *Specio, specere, spes, spero,* old German, *spehan,* English, *spy,* Ital. *spiare,* Span. *espiar,* French, *espier,* and many other forms, all signify *to see.* Even the Scottish *spaewife* corresponds with the old English word *seer.*

cessary (we might think) to the very essence of the word. Thus, the Greek εικοσι, Latin *viginti*, Æol. βικατι, lost the letter which gave it any signification;—as if we were to drop the first two letters of twenty *(twain-tig)*, and pronounce the word "*enty*." A similar accident seems to have befallen the Greek εσπερος. The oldest Latin form appears to have been *vesperugo*, as in Plautus,

"Nec jugulæ, neque Vesperugo, nec vergiliæ occidunt."

And that this was its common name, even in the Augustan age, is evident from the following passage of Vitruvius, an author notorious for his attachment to the vulgar tongue: "The star of Venus appearing in the sky after sunset is called *Vesperugo:* at other times, when it precedes the sun, and rises before day-break, it is called *Lucifer* *." As it was, therefore, when acting as the harbinger of light, called Φωσφορος and Lucifer, it may be fairly inferred that its name, whilst acting as a precursor of darkness, should have something of an appropriate signification. In *ve* and *sperugo* we have such a meaning. For *sperugo* may well be derived from the old verb *specere* to see, used by augurs even in Varro's time, as in the expression "specere avem," to view a bird. *Specio* became obsolete at an early period, being superseded by its own frequentative *specto.* It left, however, innumerable traces of its former existence in all the languages of western Europe. Even *spes* and *spero*, with their numerous progeny, are derived from *specio.* For *spes* signifies a looking forward; nor could Cicero himself give a better definition of *spes*, than by using a compound noun of *specto:* "Si spes sit expectatio boni." "If hope be the looking out for some advantage." Even the difference of the quantity between the *e* of *spero* and that of *vesperus*, presents no difficulty. For (without adverting to the cognate word *prosperus*), in the two words *specula*, a watch-tower, derived immediately from *specio*, and *specula*,

* Lib. ix. cap. 4.

a ray of hope, derived immediately from *specio*, through *spes*, we see the same difference of quantity. It therefore appears evident, that *vesperugo*, and consequently *vesperus*, signifies the time which the Scottish peasant so appropriately calls " the gloaming," known in England under the name of the evening twilight.

Vespices. We find this word only in the work of Festus *, who interprets it " a dense thicket, from its similarity to a garment." This is poor work, and most probably due to Paulus. *Vespices* has evidently the same etymology as *vesperus*, and means an opake thicket. On the same principle, Virgil partly ascribes the capture of Euryalus to the " tenebræ ramorum †."

Vestibulum, a vestibule, an entrance to a house, literally a standing place, " a wee stall or stable."

The favourite etymology of this very familiar word, has hitherto been that propounded by Ovid in the following lines ‡:

> " At focus a flammis, et quod fovet omnia dictus;
> Qui tamen in primis ædibus ante fuit:
> Hinc quoque vestibulum dici reor; unde precando,
> Dicimus, ' O Vesta, quæ loca prima tenes:"

or, as now printed,

> " .... inde precando,
> Affamur Vestam, quæ loca prima tenes."

According to this explanation, *vestibulum* was so denominated from the altar of Vesta, that is, the hearth, fire-place, which, in ancient times, was " in primis ædibus," in Scottish words " the but-end of the house." The authority, however, of Ælius Gallus, a Roman lawyer (and, I may add, that sound, well-taught lawyers are the best authority for the meaning of words), enables me to

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* " Fruteta densa, dicta a similitudine vestis."—Festus, under *Vespices*, p. 1006.

† Æn. ix. ver. 384. ‡ Fasti, lib. vi. ver. 102.

prove that Ovid was wrong, both in his fact and his inference. The whole passage of Aulus Gellius, in which reference is made to Gallus is so judicious, and especially applicable to us in the present day, that I willingly introduce it without mutilation. "There are many words which we commonly use, of which we do not clearly know the true and proper meaning; so that, following common tradition, without examination, we rather imagine that we say what we intend, than say it in reality. This is the case with *vestibulum*, a word in general use, yet not well understood by those who use it so readily. For I have observed, that persons by no means illiterate believe the *vestibulum* to be the lobby (prima domus pars, "the but-end"), commonly called the *atrium*. C. Ælius Gallus, in his second book, 'Concerning the signification of Words connected with Civil Law,' writes, that the '*vestibulum* was not in the mansion, nor a part of it, but a vacant space before the door, through which was the approach from the street to the house * * * the door itself is thrown back, having a vacant space between it and the street *.'" Gellius then proceeds to treat with contempt the numerous although absurd explanations of the word (under which Ovid's must be included), and to give the preference to that of Sulpicius Apollinaris, who derived it from *væ*, with its supposed increasing force, and *stabulum*, on the same principle as *prostibulum* and *naustibulum* were formed: "Because those of old who built large

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* "Pleraque sunt vocabula, quibus vulgo utimur, neque tamen liquido scimus quid ea proprie atque vere significent, sed incompertam et vulgariam traditionem rei non exploratæ secuti, videmur magis dicere quod volumus quam dicimus; sicuti est vestibulum, verbum in sermonibus celebre atque obvium, non omnibus tamen qui illo facile utuntur satis spectatum. Animadverti enim quosdam haudquaquam indoctos viros opinari vestibulum esse partem domus priorem, quam vulgus atrium vocat. C. Ælius Gallus, in libro "De Significatione Verborum quæ ad Jus Civile pertinent" secundo, "vestibulum esse dicit non in ipsis ædibus, neque partem ædium, sed locum ante januam domus vacuam, per quem a via aditus accessusque ad ædes est." * * * "Atque ipsa janua procul a via est, area vacanti inter sita."

houses left a vacant space before the entrance, between the door and the street. There all who came to pay their morning respects to the owner used to remain until they were admitted; so that they neither stood in the street, nor were yet within the house *." Had *vestibulum* been applied only to such mansions, thus magnificently described by Virgil, Georgic second,

> " Si non ingentem foribus domus alta superbis
> Mane salutantum totis vomit ædibus undam,
> Nec varios inhiant pulchra testudine postes ;"

the opinion of Sulpicius would have had some verisimilitude. But we know, on the contrary, that every house had its *vestibulum*. According to Vitruvius, " men of small fortunes need not have magnificent vestibules, because they have to pay their morning respects to others † ;" but small ones of course they must have had. Nay, further, the very small spot of ground by which access from a public road to a sepulchre was secured, was called *vestibulum*. Cicero informs us, that, by a law of the Twelve Tables, the right of way to a tomb, " forum id est vestibulum sepulchri," could not be lost by prescription ‡. Hence, it is evident, that the meaning of the *væ* in *vestibulum* differs not from that attributed to it in the words already examined, and that *vestibulum* originally meant the small space intervening between the street and the entrance into the house.

Vestigium, the track, the footstep, literally the small prick

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* " Qui domos igitur amplas antiquitus faciebant, locum ante januam vacuum relinquebant, qui inter fores domus et viam medius esset. In eo loco qui dominum ejus domus salutatum venerant, priusquam admitterentur consistebant, et neque in via stabant neque intra ædes erant."—Lib. xi. cap. 5.

† " His, qui communi sunt fortuna, non necessaria magnifica vestibula, quod hi aliis officia præstant ambiundo."—Lib. vi. cap. 8.

‡ De Legibus, lib. ii. sect. 24.

made in moist ground by the nails of hares, foxes, &c.; also a small point, "puncto loci aut temporis *."

Nothing can be a stronger proof of the deplorable state in which the science of etymology has so long continued, than that the absurd explanation of this word, first I believe proposed by Vossius, should still be retained in our best dictionaries: "Formerly not only men, but also women, wore long garments, hence the traces not only of their feet, but also of their dress, were left by those who walked. This was the reason why, although the feet made the deepest impression, the name was nevertheless derived from the garment (vestis) †." Becmanus, quoted by Vossius, proposed the true etymology, from *στιζω*, but, according to the radical error introduced by Gellius, wished to assign an intensive force to *ve*. *Vestigium*, as well as the verbs *vestigo* and *investigo*, was borrowed from the hunter's vocabulary, from which many words in every language with which I am acquainted have been derived. In the early ages of every nation, much of the hunter's success depends upon his skill in discovering the small punctures made in moist places (not, however, admitting the compression of the ball of the foot) by the toe-nails of his game. To those versed in field sports, I need not say more; but the uninitiated should be informed, that such a trace is still called a *prick*, and the process by which a skilful eye is enabled to recognize the path of a hare from such small punctures is still called *pricking*. Nor would I ascribe the origin of the name of the Yeomen Prickers connected with the king's hunting establishment to any other source than this, although I am aware that a rider in old English

* *Στιζω, στιξω, στιγμα, stimulus.* The verb *stigo* was itself Latin, as may be seen in the compound *instigo*, to goad on, from which simple verb *stigium* was immediately formed. The word is found in all the Celtic and Teutonic dialects, in the sense of the English *to stick*, i. e. to stab or puncture, Germ. *stechen*, Cimbric, *ystigaw*, the verb; *stich*, Germ. the puncture, Anglo-Saxon *stice*, &c.

† Vossius, Etym. under the word.

was, from the frequent use of his spurs, called a pricker :

"A gentle knight came pricking o'er the lea."

From the first and technical meaning of *vestigium* other significations arose, of which that of the whole footstep was the most natural. "Hac video socci vestigium in pulvere," says Plautus. Hence also all other traces whatsoever were indicated by *vestigium*. But still there remain in the best writers many passages in which the word is used without the slightest allusion to its secondary meaning, and for the explanation of which we must have recourse to *ve*, little, and *stigium*, a point. These furnish us with the "experimentum crucis" before alluded to, in which, if the facts are not denied, the inference must be allowed. Such was the use of the word by Cæsar, "Eodemque tempore vis magna pulveris cernebatur, et vestigio temporis primum agmen erat in conspectus *." Here we have "vestigio temporis," not in the meaning of vestige, trace, track, &c. but evidently for "puncto temporis," in a moment. Should we entertain any doubts, they will be resolved by the following passage from Cicero's invective against Piso, "Atque eodem in templo, eodem et loci vestigio et temporis †;" "and in the same temple, at the same point both of time and place." Columella employs it in a still more primary sense ‡ (giving to *stigium* its first meaning of a blow), where he writes, that a vine branch injured by the pruning-knife, used *ductim* in opposition to *cæsim*, can be smoothed (allevari) "uno vestigio," "with one slight stroke."

Vegrandis ||. We have already seen, on Ovid's authority, that *vegrandis* signified not well-grown. It remains now to show

* Bell. Civ. lib. ii. c. 26. † Cap. ix. ‡ Lib. iv. c. 25.

|| *Grandis*, a term borrowed from the swelling or rounding of the grains (grana) of corn, afterwards applied to the full growth of other objects. It is peculiar to the Latin language.

that it never means very large, as asserted by AULUS GELLIUS. The only words (with the exception of those already examined) which he quotes as exemplifying the intensive form of *ve*, are *vehemens*, *vetus*, and *vegrandis*. But *vehemens* has the *ve* short, and is derived from the verb *veho*, not compounded of *ve*, aspirated into *vehe*, and *mens*, mind. In *vetus*, he is still more unfortunate; for, had it been compounded of *ve* and *ætas*, no power could ever have shortened the penult. In reality, *vetus* is immediately formed from the Greek ετος (a year), which, in HOMER, has always the digamma, Ϝετος. The Saxon expression, "of yore," corresponds in meaning with the Latin *vetus*. The only remaining word for which the intensive force of *ve* is claimed, is *vegrandis*. For such usage two lines were adduced; the first from LUCILIUS, and to which GELLIUS refers. It is still to be found in the first book of NONIUS, n. § 34.

"Non idcirco extollitur nec vitæ vegrandi datur."

But it is to be remarked, that NONIUS having probably found a better edition of LUCILIUS, quotes the same line in the following manner,

"Non idcirco extollitur vel iræ vel gaudii dator."

The other example was supposed to exist in PERSIUS,

"Ut ramale vetus, vegrandi subere coctum."

But more manuscripts were found to have *prægrandi*, not *vegrandi*: hence it has long ago disappeared.

Hence, my induction is complete, that *ve* never has an intensive power, but has always the signification either of the adverb *parum*, as in *vesanus*, or of the adjective *parvus*, as in *vestigium*.

On Phosphuretted Hydrogen. By Thomas Graham, Esq. F. R. S. Ed., Lecturer on Chemistry in the Andersonian Institution, and V. P. Phil. Soc. Glasgow.

(*Read* 1*st December* 1834.)

Few substances have been made the subject of experimental inquiry more frequently than the compounds of phosphorus and hydrogen, and no subject is so remarkable for the various and conflicting results which it has presented to chemists of the greatest acuteness and practical skill. The obscurity which long hung over the subject has been dispelled, however, in a great measure, by the recent investigations of Henry Rose of Berlin. Although baffled in his early researches, that philosopher returned again and again to the subject, and at last succeeded in determining the chemical functions and true constitution of phosphuretted hydrogen. He has shewn it to be analogous to ammonia in chemical character and composition. But hitherto two compounds of phosphorus and hydrogen had generally been admitted to exist, which were believed to differ in composition, as they do in properties, one being spontaneously inflammable in atmospheric air, and the other not so. Rose establishes beyond all doubt that these gases are essentially of the same composition, and of the same specific gravity; and, indeed, that they are mutually convertible, each into the other, without any addition or subtraction of matter that could be perceived. In explanation of their possessing different properties, under the same composition, allusion is made by Rose to *Isomerism*, or the doctrine that two bodies may exist identical in composition, but differing in properties. Certainly the existence of *two gases*, constituted alike, and yet possessing different properties, if established, would afford a firm basis for this doctrine.

It was the importance of the theoretical results which might be looked for, that induced me to attempt to continue the investigation beyond the point to which it had been carried by Rose.

Holding the general doctrine of Isomerism as problematical, my inquiries were directed to the discovery, in one or other of the gases, of some adventitious matter, to the presence of which the peculiarities of the species might be attributed.

It is to be understood that the spontaneously inflammable gas made use of in my experiments, was prepared by the well-known process of heating phosphorus, lime and water together. This gas is spoken of as "the self-accendible gas," or as "the gas from phosphuret of lime." The other gas, which is *not* spontaneously inflammable, was prepared by heating hydrated phosphorous acid, or by allowing the preceding species, contained in low receivers, to stand over water for twenty-four hours. It is described as "the non-accendible gas," "the gas from phosphorous acid." The ascendibility of the gas was judged of by allowing it to escape in bubbles into the air from the receiver containing it, either over water or mercury. The experiments were all made when the temperature of the atmosphere was between 60° and 70° Fahrenheit.

1. In the process by which the self-accendible gas is procured, free phosphorus distils over, of which a trace, in the state of vapour, may well be supposed to remain in the gas for some time. Hence the idea has generally presented itself, that the free and highly accendible phosphorus present may be the cause of the spontaneous inflammability of the gas. Dr Dalton, who all along maintained the opinion, which has finally been established by Rose, that the two gases are of the same composition, was in the habit of referring the spontaneous inflammability of the one species to this cause. The speedy loss of the property in question, in the case of gas confined over water, seemed to favour this view.

I find, however, that if a small quantity of phosphuretted hydrogen, when not self-accendible, be added to a confined portion of air, sticks of phosphorus introduced into that air do not smoke, that phosphorus has no disposition to combine with oxygen when phosphuretted hydrogen is present. In a transparent mixture of one volume phosphuretted hydrogen with one thousand volumes, or any smaller proportion of air, sticks of phosphorus remain unaffected, but the phosphuretted hydrogen itself always undergoes a slow oxidation. In a mixture of one volume phosphuretted hydrogen and two thousand volumes air, phosphorus smoked strongly for some time; but at a certain period the action ceased, long before the oxygen of the air was exhausted. A minute proportion of phosphuretted hydrogen is, therefore, sufficient to protect phosphorus from oxidation, in which respect this gas resembles the hydrocarburets and essential oils, which have been shown to be equally efficacious in protecting phosphorus from oxidation. All these bodies appear to act in this respect in one way, namely, by taking the precedence of phosphorus in the process of oxygenation. Phosphorus therefore being less oxidable than phosphuretted hydrogen itself, cannot be supposed to take fire and to inflame the gas, or to be the cause of the ascendibility of the gas at low temperatures.

On sending electric sparks through non-accendible phosphuretted hydrogen itself, phosphorus is deposited, but the gas when still cloudy from the phosphorus suspended in it, proved to be non-inflammable on passing it into air.

The loss of accendibility in the case of gas confined over water, is certainly wholly unconnected with the deposition of any free phosphorus from the gas, which may occur, but is due to the rise of *oxygen* from the water into the gas. It was observed that water which had been boiled to deprive it of all air, and which was then passed up to self-accendible gas confined over mercury, did not affect the gas in the course of forty-eight hours. In this case, moreover, the gas was agitated with the water. The gas continues in general spontaneously inflammable over mercury for forty-eight hours, and sometimes for three or four days, but ceases to be so in a very short time, after the admission of a small proportion of air, particularly if the air be added in a gradual manner. Thus, if to the gas be passed up one-twentieth part of its bulk of cork or of dry stucco, containing air in its pores, a white smoke appears in the gas, and it ceases to be spontaneously inflammable in the course of a few minutes. The same mass of stucco, warmed before being passed up into the gas, so as to expel the air it contained, did not produce the same effect. The self-accendible gas always deposits on standing a solid matter containing phosphorus, of a lively yellow colour, but in quantity too minute for analysis. This matter is not acted on by any of the ordinary solvents, such as alcohol, ether, alkalies, or muriatic acid, but is destroyed by chlorine-water, and by nitric acid. The precipitation of this matter is most rapid in the case of gas over water, and is indicative of deterioration of the gas.

2. The self-accendible gas procured from phosphorus, water, and lime, is always mixed with free hydrogen, varying in quantity from 25 to 50 per cent.; while the non-accendible gas from phosphorous acid contains no hydrogen gas, but is pure. Rose concludes that the spontaneous inflammability of the first species cannot depend upon this hydrogen, for the other species is not made self-accendible by the addition to it of any proportion of free hydrogen. On trying the experiment, however, I obtained a different result. A quantity of gas had lost its self-accendibility by standing over water for two or three hours; to my surprise, the addition to this gas of hydrogen, in any proportion from one-third of a volume to three volumes, restored the self-accendibility of the gas. Spontaneous inflammability was likewise communicated, in some cases, to the gas procured from phosphorous acid, merely by adding hydrogen to it. It was early perceived, however, in the course of the investigation, that hydrogen did not uniformly communicate the property in question, and that its in-

fluence depended on something accidental and not essential to the gas. For instance, the hydrogen which comes over almost pure towards the end of the process for phosphuretted hydrogen had none of this property, nor did it appear in hydrogen obtained from the following sources;—from the electric decomposition of water, from the decomposition of steam by iron, from the action of water on amalgam of potassium, or from the action of muriatic, arsenic, or phosphoric acid on zinc. Even in the case of the action of sulphuric acid on zinc or iron, which had first afforded hydrogen possessing the property in question, it turned out that only the hydrogen evolved at an early period of the action is efficient, while the gas evolved after the vivacity of the action is impaired is nearly, and sometimes entirely, destitute of any influence. The activity of the hydrogen was in short traced to a slight impregnation of *nitrous acid vapour*, which it possessed. The sulphuric acid of commerce always contains a small portion of some acid of nitrogen, probably the hyponitrous, from which, I find, it cannot be freed by boiling or concentration continued for any length of time. On quickly mixing sulphuric acid with two or three volumes of water, the presence of nitrous acid is attested by its peculiar odour, and almost certainly by the appearance of brown fumes. That the hydrogen did not owe the property in question to a trace of nitric oxide, which, combining with oxygen, might, by a slight consequent evolution of heat, have an effect in kindling the phosphuretted hydrogen, was proved by the fact, that the property in question could not be imparted to hydrogen by any proportion of nitric oxide; but to this point there will be occasion to recur.

At an earlier stage in the inquiry, some experiments were made upon the effect of other gases than hydrogen upon phosphuretted hydrogen. None, with the exception of sulphuretted hydrogen (evolved by the action of sulphuric acid on sulphuret of iron, and which therefore contains free hydrogen), appeared to favour the accendibility of the gas. On the contrary, the addition of all others, and even of hydrogen and sulphuretted hydrogen themselves above a certain proportion, distinctly impeded or destroyed the accendibility of this gas. Thus, one volume phosphuretted hydrogen ceased to be spontaneously inflammable when mixed with the following proportions of different gases:—

With	5	volumes	hydrogen,
...	2	...	carbonic acid,
...	3	...	nitrogen,
...	1	volume	olefiant gas,
...	$\frac{1}{2}$	...	sulphuretted hydrogen,
...	$\frac{1}{10}$	...	nitric oxide,
...	$\frac{1}{20}$	...	muriatic acid,
...	$\frac{1}{5}$	...	ammoniacal gas.

It is to be remarked, however, in reference to the preceding table, that some specimens of phosphuretted hydrogen appear to be more highly accendible than others, and that there is considerable latitude in the proportion of foreign gas which may be requisite for destroying the spontaneous inflammability of a given specimen. Often a much smaller portion suffices than is stated in the table. I have found half a volume of carbonic acid or of nitrogen to produce the effect. Of course the introduction of any trace of air, with the gases, must be carefully guarded against. Nitrous acid, when present in hydrogen in too small a proportion to enable that gas to communicate spontaneous inflammability to phosphuretted hydrogen, or to be perceived by the smell, may be detected by the effect of the hydrogen upon a prepared mixture of non-accendible phosphuretted hydrogen and air, which mixture may be had quite free from white smoke, and transparent. The addition of hydrogen to this mixture occasions the immediate appearance of a dense white smoke, the oxidation of the phosphorus being partially induced, if even an infinitesimal proportion of nitrous acid exist in the hydrogen. Although the oxidation of the phosphorus takes place at the expense of the air present, and only when air is present, yet the nitrous acid appears to be speedily consumed; the fumes soon ceasing, but appearing again on every subsequent addition of active hydrogen, till several volumes have been added, or till the oxygen of the air present is exhausted.

That the influence of hydrogen was referable to the nitrous impregnation, appeared also from the fact, that phosphuretted hydrogen, which had lost its spontaneous inflammability, was rendered as actively inflammable as ever by passing it, bubble by bubble, into an inverted receiver filled with sulphuric acid, recently diluted with three measures of water and cooled. The gas was now capable of igniting spontaneously, when passed into air, without the intervention of hydrogen. The same diluted acid lost the smell of nitrous acid, by exposure to air in a shallow vessel for a few hours, and thereafter was found unfit for the purpose in question. Phosphuretted hydrogen, which had acquired spontaneous inflammability from a nitrous impregnation, appeared to retain that property as long as the phosphuretted hydrogen, which is spontaneously inflammable as first prepared.

Hydrogen gas, too, which had received a nitrous impregnation by being passed through a diluted sulphuric acid, retained, in one case, after being confined for twenty-four hours over water, the power of rendering phosphuretted hydrogen spontaneously inflammable. From the preceding results and other considerations, it seemed not unlikely that the spontaneous inflammability of phosphuretted hydrogen may be an accidental property, and depend upon the occasional presence of some foreign body in minute quantity. The inquiry suggests itself, is there a *peculiar principle* in the self-accendible gas, and what is it?

3. It was very soon found that a peculiar principle is withdrawn from the gas by *porous absorbents*, such as wood, charcoal, and baked clay, which substances are capable of destroying the inflammability of several hundred times their volume of gas. Thus, in one experiment, to 500 measures of highly accendible phosphuretted hydrogen, one measure of charcoal, recently heat-

ed to redness, and cooled under the surface of mercury, was passed up. In the course of five minutes a contraction of eight or ten measures occurred, without any oxidation of the gas, for no air was introduced with the charcoal. The gas was still spontaneously inflammable, but ceased to be so in the course of half an hour. It was found, in fact, by different experiments, that wood-charcoal can absorb about ten times its volume of phosphuretted hydrogen gas itself; that the phosphuretted hydrogen and the peculiar principle are absorbed indiscriminately at first by the charcoal, but that by-and-bye the peculiar principle comes to be entirely absorbed by the charcoal, without any farther absorption of phosphuretted hydrogen.

When the phosphuretted hydrogen did not exceed fifty or sixty times the bulk of the charcoal, the peculiar principle was entirely withdrawn in five minutes, and the gas ceased to be self-accendible. Charcoal, which had been drenched in water, was without effect upon the gas. On heating the charcoal saturated with gas, in a retort filled with water, phosphuretted hydrogen was given off, which, however, was not self-accendible; and all my attempts failed to isolate the peculiar principle, by separating it from the charcoal. It was quite clear that the peculiar principle formed but a very small proportion of the volume of the phosphuretted hydrogen, evidently much less than one per cent. of the bulk of the gas.

Spongy platinum introduced into the gas did not exercise any sensible absorbent effect, and no quantity of it seemed sufficient to withdraw the peculiar principle from a small bulk of the phosphuretted hydrogen.

Stucco, likewise, was without effect upon the gas, at least when access of air was guarded against at the same time. But both of these substances are known to possess a very low absorbent power.

4. Phosphuretted hydrogen transferred to a receiver over mercury, the inside of which is moistened by a strong solution of

caustic potash, always loses its spontaneous accendibility, although by no means rapidly, several hours being generally required.

5. Certain *acids* appear to have a remarkable power in withdrawing the principle of inflammability from phosphuretted hydrogen.

Let phosphuretted hydrogen be transferred into a jar inverted over mercury, of which jar the inner surface has been moistened with concentrated phosphorous acid. A small quantity of a milk-white matter immediately appears in the acid, where exposed to the gas; and in two or three minutes the gas has ceased to be spontaneously inflammable, without any appreciable diminution of its volume having occurred. This white matter, although very sensible to the eye, exists only in the most minute quantity. It is not crystalline, and perhaps is not even solid. The introduction of concentrated phosphoric acid into the gas, was attended by similar phenomena; and the gas lost its spontaneous inflammability in the course of half an hour.

A strong solution of arsenic acid acts as rapidly in withdrawing the peculiar principle as phosphoric acid does, but the arsenic acid soon begins to react upon the phosphuretted hydrogen itself, a dark copper-coloured incrustation soon forming upon the surface of the gas-receiver, which matter is probably a phosphuret of arsenic. Concentrated sulphuric acid is capable of absorbing phosphuretted hydrogen itself, which the preceding acids are not, but even sulphuric acid appears to absorb the peculiar principle, in the first instance, by a more active affinity than it exerts upon the gas itself. Dilute phosphorous, phosphoric, and arsenic acids, react in the same manner upon phosphuretted hydrogen, but not so rapidly as the concentrated acids do.

6. The following liquids are capable of dissolving the quantity of phosphuretted hydrogen gas placed against their names, at 65° Fahr.

Alcohol (sp. gr. 850),	$\frac{1}{2}$	volume.
Sulphuric Ether,	2	.....
Oil of Turpentine,	$3\frac{1}{4}$	......

The *essential oils* and most of the *hydrocarburets* appear to withdraw, or to negative the peculiar principle in spontaneously inflammable phosphuretted hydrogen in a rapid manner. If a jar be moistened, in the slightest degree, with oil of turpentine, coal-tar naphtha, or by the liquid distilled from caoutchouc, and then be used as a receiver for containing self-accendible gas, either over water or mercury, the gas is found to lose its spontaneous inflammability in a very few minutes. White fumes often appear in the gas at the same time, but these I am satisfied are due to the evolution of some gaseous oxygen from the liquids, and appear in the case of the portion of gas which is first brought into contact with the liquid, but do not occur in the case of subsequent additions of gas, although the liquid remains capable of destroying the spontaneous accendibility of many portions of gas, successively exposed to it. It is not easy to decide whether the vapours destroy irrecoverably the peculiar substance of spontaneous inflammability, or merely negative the action of that principle by their presence.

I am inclined to think, however, that they destroy that principle, for the action is not so rapid as the diffusion of the vapour through the gas, the impregnation appearing to be fully accomplished, and yet the loss of inflammability not occurring sometimes for two or three minutes afterwards, particularly in the case of naphtha, a portion of that pure liquid, in which potassium had been preserved, being used in the experiment. A small addition of ether-vapour also destroys the inflammability of phosphuretted hydrogen, although a distinct interval must elapse before the change occurs, such as a quarter or half of an hour.

The action of alcohol vapour is much slower, generally requiring two or three hours. Pure olefiant gas, containing no air, added in the proportion of 10 or 20 per cent., eventually destroys the spontaneous inflammability, but requires a period of not less than twenty or thirty hours.

Olefiant gas has a negative influence of quite a different character, which has already been alluded to, and which is in action the moment the gases are mixed, but which does not appear unless the proportion of olefiant gas be very considerable. It is probable that ether-vapour and the gaseous hydrocarburets likewise have an influence of the same kind. An astonishingly minute quantity of an essential oil suffices to destroy the inflammability of the gas over mercury, if allowed an hour or two to act. Hence it is very difficult to preserve gas in the inflammable condition, in the mercurial trough, if any portion of the mercury has been soiled by an essential oil.

7. The action of *potassium* on the peculiar principle is equally remarkable. A most minute quantity of this metal, or of its amalgam, destroys the self-accendibility of the gas in a few minutes, without occasioning any sensible reduction of volume that could be measured.

The fact is, potassium, or its amalgam, is without effect upon phosphuretted hydrogen itself, at the temperature of the air, neither absorbing nor decomposing the gas ; but upon the peculiar principle the action of this metal is rapid and certain. One grain of potassium, amalgamated with fifty pounds of mercury, rendered that large quantity of mercury quite unfit for retaining gas over it, in the self-accendible condition, for more than a few minutes. In such experiments the interference of naphtha vapour was perfectly excluded. Zinc and tin, either by themselves or in the state of amalgam, have no sensible effect upon self-accendible gas, at least in a period of five or six hours. Protoxide of mercury speedily withdraws the peculiar principle, but afterwards also reacts slowly upon the gas itself. On the other hand, the peroxide of the same metal is nowise injurious to the self-accendible gas. Arsenious acid in powder acts in the same manner as protoxide of mercury. The solution of protosulphate of iron, if previously boiled to deprive it of air, is without effect upon the gas.

The extraordinary action of potassium, and that also perhaps of the essential oils, seemed to point to the existence of an oxygenated principle, as the cause of the spontaneous inflammability of phosphuretted hydrogen.

It is sufficiently evident that the proportion in which this principle exists to the whole gas, is exceedingly small, too minute to afford any hope of isolating that principle. The nitrous impregnation, too, which was found adequate to render gas spontaneously inflammable, shews to how minute a quantity of matter the spontaneous inflammability of phosphuretted hydrogen may at times be owing. It seemed within the bounds of possibility that the gas might owe its spontaneous inflammability, in ordinary circumstances, if not to nitrous acid, at least to some other principle analogous to that substance. This led to a careful examination of the properties of phosphuretted hydrogen made inflammable by means of nitrous acid; a subject of much interest, as illustrating the effect of a most minute and almost infinitesimal quantity of foreign admixture, in communicating so striking a property as spontaneous inflammability to a chemical body, independently of the light which it may throw upon the constitution of ordinary phosphuretted hydrogen.

8. Phosphuretted hydrogen, which had lost all trace of spontaneous inflammability by standing a day or two over water, or the gas from hydrated phosphorous acid, might be impregnated with nitrous acid, and made spontaneously inflammable in various ways. It was ascertained that the gas obtained, by either process, was affected in the same way. Such gas only, entirely destitute of spontaneous inflammability, was employed in the following experiments:—

(1.) The nitrous acid of Dulong may be added directly to the gas over mercury, a glass spherule, or the bore of a short piece of thermometer tube being filled with the liquid, and passed up to the gas. When *nitric acid* is brought into contact with the gas in this manner, a violent action occurs ; but with *nitrous acid* the evolution of white fumes is very slight. The nitrous acid is absorbed in part by the mercury, but this absorption is slow, provided the quantity of gas be considerable with which the acid vapour is mixed. If the quantity of gas primarily impregnated with nitrous acid, in the manner described, be small, or the impregnation of nitrous acid considerable, the gas exhibits no disposition to smoke or to take fire, when passed into air. It has not become spontaneously accendible. On diluting the gas with a large proportion of unimpregnated phosphuretted hydrogen, no reaction is indicated, but the whole becomes spontaneously accendible in a high degree. In fact, it was discovered that the gas is not accendible when the nitrous acid exceeds a certain proportion, which is by no means considerable.

(2.) Allow a single drop of nitrous acid to fall into a dry glass jar, which may be of small dimensions. Fill the jar with mercury, and invert it without loss of time in the mercurial trough, a bubble of gas will collect in the upper part of the jar, which bubble is chiefly nitrous acid vapour. One cubic inch or so of phosphuretted hydrogen, or of hydrogen itself, may then be added to the gas in the jar, and this is our nitrous impregnating mixture. Suppose this mixture to contain one-twentieth of its bulk of nitrous acid vapour. The addition of it, in any proportion, to phosphuretted hydrogen, is not attended by the slightest production of white fumes ; in fact no reaction appears to take place. But the addition of a single bubble of this mixture, not exceeding one-tenth of an inch in volume, to five or six cubic inches of phosphuretted hydrogen, will render the whole highly accendible, so that every bubble passed into the air will take fire.

(3.) In the above arrangement, a drop of the strongest nitric acid may be substituted for the nitrous acid, in the preparation of the impregnating mixture. The nitric acid acts on the mercury, and nitric oxide, charged with nitrous acid, is collected, which may be diluted with hydrogen as above.

The preceding processes uniformly afford a nitrous impregnating mixture which may be depended upon; but when the experiment is attempted over water, there is not the same certainty of the impregnation being successful. I have often, however, made hydrogen highly suitable for the purpose, by passing it through a column of fluid composed of nitric acid recently diluted with water, provided that the acid had been fuming from the presence of nitrous acid; or by passing hydrogen through recently diluted sulphuric acid, as has already been stated.

In regard to the proper proportion of nitrous acid-vapour to the phosphuretted hydrogen, I am satisfied that the proportion most efficacious, is somewhere between 1 part nitrous acid to 1000, and 1 to 10,000 phosphuretted hydrogen. One volume nitrous acid-vapour to 100 gas, or to less gas, is never accendible, but becomes so on diluting it with enough of phosphuretted hydrogen.

I was anxious to discover how far nitric oxide interferes in the phenomenon. The nitrous acid is never free from, but always accompanied with, a certain proportion of this gas.

9. *Action of Nitric Oxide.*—In a table formerly given, nitric oxide is set down as incompatible with the accendibility of the good gas from phosphuret of lime, when the proportion of the first is so great as one-tenth of the whole mixture.

In fact, the best inflammable gas, when mixed with nitric oxide, in quantity from two volumes to one-tenth of a volume, exhibited no symptoms of spontaneous inflammability. The nitric oxide forms red fumes when the mixture meets the air, but the phosphuretted hydrogen does not even smoke, so that the oxidation of the nitric oxide has not a kindling effect upon the phosphuretted hydrogen, but the very reverse. A mixture of one volume nitric oxide, with twenty volumes good phosphuretted hydrogen (self accendible *per se*), is still self accendible; the bubble, however, does not take fire the instant it bursts in the air, but after rising to a little height, and then explodes with a puff like loose grains of gunpowder, and not with the usual snap, the oxidation of the nitric oxide preceding the oxidation of the phosphuretted hydrogen by a sensible interval. Nitric oxide, in a considerably smaller proportion than one-twentieth volume, exhibits a sensible effect in retarding the combustion of self-accendible gas, but does not altogether prevent it. In the case of phosphuretted hydrogen, which was not self-accendible, small additions of nitric oxide, such as 1 to 100, to 500, to 1000, or to 2000 volumes phosphuretted hydrogen, did not induce self-accendibility, when the nitric oxide employed had been previously washed with caustic alkali. The experiment was tried with three different specimens of washed nitric oxide. But nitric oxide, which had not been washed with alkali, particularly if it resulted from a turbulent action of the nitric acid on copper, and came overcharged with red fumes, and was withal newly collected, was pretty often efficient in making the gas self-accendible. The proper proportion of such nitric oxide for this purpose, was found to be 1 volume to a quantity between 1000 and 2000 volumes of phosphuretted hydrogen. A greater or a less proportion of the nitric oxide failed to produce the desired effect. All these experiments with nitric oxide were made over water.

It is well known that a mixture of phosphuretted hydrogen and nitric oxide may be exploded by a bubble of oxygen gas, a method of firing these gases, first practised, I believe, by Dr THOMSON. But pure nitric oxide was found by Dr DALTON to oxygenate phosphuretted hydrogen in a gradual manner, when the two gases are left together. It is probable, therefore, that it is, by acting itself upon phosphuretted hydrogen, that nitric oxide prevents atmospheric air from acting upon that gas in our experiments. It is conceivable that the oxygenating action of nitric oxide upon phosphuretted hydrogen, like that of air upon the same gas, may be promoted by the presence of nitrous acid, which will explain Dr Thomson's experiments.

The impregnating nitrous mixture of the foregoing experiments was not destitute of nitric oxide, but what proves that the efficiency of the mixture did not depend upon the last mentioned ingredient, is the circumstance, that the mixture lost its virtue by standing over mercury for a week, during which period the acid-vapour was absorbed by the mercury, but the nitric oxide remained, as appeared on admitting air to the gaseous mixture. Hence, we may conclude, that when nitric oxide acts in producing inflammability in phosphuretted hydrogen, it is from the nitrous acid which it occasionally contains.

It is certainly, however, very curious that nitric oxide is not quite equivalent to nitrous acid, in producing the change in question upon phosphuretted hydrogen, seeing that the nitric oxide passes immediately into nitrous acid upon meeting air. Whether the negative influence of nitric oxide upon really accendible gas is sufficient to account for this anomaly, I am doubtful. It may be thought that nitrous acid and phosphuretted hydrogen, when in contact for a short time, react upon each other, with the production of some entirely new and highly accendible body. But this supposition seems not to quadrate with the fact, that the impregnating mixture requires to be diluted by so large a proportion of phosphuretted hydrogen, before the whole becomes spontaneously accendible. Nor is it supported by any visible signs of reaction between the nitrous acid and phosphuretted hydrogen. Indeed, nitrous acid-vapour appears to be compatible with phosphuretted hydrogen, to an extent which could not have been anticipated.

Again, that nitrous acid, or at least some acid compound of nitrogen, continues to exist in what we may now call the *nitrous phosphuretted hydrogen gas*, appears to be corroborated by the properties which this self-accendible gas is found to possess.

10. *Properties of nitrous phosphuretted hydrogen.*

(1.) This gas loses its self-accendibility when kept over mercury, in a period varying from six to twenty-four hours, according to the amount of nitrous impregnation.

It is remarkable that this gas continues, in general, inflammable for a longer time, when confined over water, than over mercury, which is the reverse of what occurs with the gas from phosphuret of lime.

(2.) The factitious gas is deprived of its spontaneous inflammability by charcoal and other porous absorbents, by essential oils and hydrocarburets, and by amalgam of potassium, and quite as rapidly as is its natural prototype.

(3.) Phosphorous acid, and concentrated sulphuric acid, appear likewise to withdraw the nitrous principle, although phosphoric acid does not. The agency of these acids probably exemplifies the disposition of nitrous acid to combine with other acids. The action of potassium and of essential oils upon nitrous acid, requires no explanation. Potassium has, I find, no action upon pure nitric oxide in the cold.

(4.) A cubic inch of this gas, passed up into a receiver, of which the inside was moistened with caustic alkali, had its accendibility sensibly impaired in fifteen minutes, but not completely destroyed in less than an hour.

In conclusion, the statement of the above properties is abundantly sufficient to prove that a strong analogy subsists between our nitrous phosphuretted hydrogen and the self-accendible gas, which has been so long in the hands of chemists. The peculiar principle of the last may therefore possibly be an oxygenated body. That principle cannot be nitrous acid, but it may be a compound of phosphorus and oxygen, $\dddot{P}$, analogous to nitrous acid. In all the reactions by which self-accendible phosphuretted hydrogen is produced, we have the simultaneous formation of compounds of phosphorus and oxygen, such as hypophosphorous and phosphoric acids. The compound P is hypothetical, however, and has not yet been formed directly. Its existence is only surmised from the parallelism which appears to be established between nitrogen and phosphorus, and between their compounds; phosphuretted hydrogen itself corresponding with ammonia, phosphoric, and phosphorous acids, with nitric and hyponitrous acids. The peroxide of chlorine of Davy and Stadion $\ddot{\ddot{C}}$, corresponds with nitrous acid, and with our hypothetical oxide of phosphorus, which we may speak of as the peroxide of phosphorus.

The peroxide of phosphorus would appear to resemble the peroxide of chlorine, in being acted on more slowly by mercury and by alkalies, than is the case with nitrous acid. It is to be admitted, however, that I did not succeed in producing an inflammable phosphuretted hydrogen, by the agency of peroxide of chlorine—that there is no *chlorous* phosphuretted hydrogen. The reason is, that peroxide of chlorine is incompatible with phosphuretted hydrogen, reacting upon that gas the instant of mixture.

As to the mode in which nitrous acid vapour, in a proportion so minute, contributes to the accendibility of phosphuretted hydrogen, I have been able to form no distinct idea. The most likely conjecture is, that the nitrous acid, or resulting hyponitrous acid, combines with some product of the oxygenation of phosphuretted hydrogen, and thereby disposes or promotes the occurrence of that change. The oxygenation of pure hydrogen itself, under the influence of a clean plate of platinum, is not promoted in a sensible degree by any nitrous impregnation. Sulphurous acid and muriatic acid gases, and vapour of acetic acid, appeared to contribute nothing to the accendibility of phosphuretted hydrogen.

It appears, then, that the two phosphuretted hydrogens are not isomeric bodies, but that the peculiarities of the spontaneously inflammable species depend upon the presence of adventitious matter:

That the vapour of some acid of nitrogen, which, in the present state of our knowledge of that class of compounds, seems to be the nitrous acid, is capable of rendering phosphuretted hydrogen spontaneously inflammable, when present to the extent of *one ten-thousandth part* of the volume of the gas:

That the last gas has a general resemblance to phosphuretted hydrogen, as obtained in the spontaneously inflammable state by ordinary processes, which, it is probable, owes its ready accendibility to the presence of an equally minute trace of a volatile compound of *phosphorus and oxygen*, analogous to nitrous acid.

General Remarks on the Coal Formation of the Great Valley of the Scottish Lowlands. By Lord Greenock, C. B., LL. D., V. P. R. S. Ed., F. G. S.

(Read 15th December 1834.)

There is perhaps no geological problem of greater interest than that by which the conditions of the globe, in respect to the distribution of land and water, at the epoch of the coal formation, might be determined, if the necessary data for its solution could be obtained. We possess sufficient evidence to shew that, since that period, most of the continents and islands of the present day have been elevated above the waters, under which they were originally formed; but such proofs are altogether wanting when we endeavour to restore, in imagination, the probable extent of the older formations that might then have existed as dry land, but which now lie buried in the depths of the ocean, or have since been covered by an accumulation of more recent deposits.

If we consider the immense quantity of detrital matter, that must have been required to form beds of such prodigious extent and thickness, as many of those which are met with in the coal-fields of different parts of the world; we shall naturally be led to the conclusion that, in the immediate neighbourhood of these deposits, much more extensive tracts of land must have existed, in connection with each other, than could possibly have been afforded by the older portions of the present countries. For whatever might have been the conditions of the globe, in this respect, at that epoch, we have convincing proofs in the organic remains of plants, which abound in all the members of the carboniferous series, as well as in the acknowledged vegetable origin of the coal itself, of the pre-existence of a sufficient extent of dry land, clothed with a luxuriant tropical vegetation, to have supplied this detritus and these remains, and sufficiently elevated to have given rise to the rivers and torrents, by the action of which this transported matter must have been carried down into the lakes or estuaries, in which, to all appearance, such deposits were formed. Whether this land constituted groups of islands, according to the opinion of many geologists, or continents of greater or less extent, which, by subsequent revolutions, have been either wholly or partially submerged by the existing sea, is not our present object to inquire; but, from the size and position of the fossil trees enveloped in many of the beds of this formation, and other phenomena connected with them, there appear to be strong grounds for inferring that the volume of water discharged by these rivers, and the magnitude of their estuaries, must have been much more considerable than would probably have been the case if they had been situated in small islands; and the occurrence of terrestrial plants, with the remains of fishes, and of mollusca, that had either been the inhabitants of fresh-water or of shallow seas, in the lowest beds of the series, which have afterwards been covered by other deposits more decidedly of a marine character, seems to shew, that, although the former might have been originally deposited in lakes, or on flat shores near the mouths of rivers, frequent alternations in the relative level of the sea and land may probably have taken place since, by which a corresponding succession of deposits, varying accordingly in the character of their organic remains, have been produced.

The intermixture, however, of organic remains of a fluviatile character, with those of marine fishes and shells, is very general throughout the coal-measures, and it is still doubtful whether any well-authenticated proof exists of any beds having yet been discovered in the carboniferous series, which, from their organic contents, could be pronounced to have had exclusively a fresh-water origin, unless an exception should be found in the limestone noticed by Mr Murchison at Pontesbury, and other places in the Shropshire coal district; for the opinion expressed by M. Agassiz, relative to the ichthyolites which have been found in the Scottish coal-fields, has shewn that neither the Burdiehouse limestone, nor any of the other beds of the carboniferous group in this country, with which we are at present acquainted, have any claim to be so considered; nevertheless, there are peculiarities in the character of the limestone at Burdiehouse, both in regard to its organic contents, and to its position in the series, which render it an object of the highest geological interest: and the discovery by Dr Hibbert, of a new species of sauroid fish, the Megalichthys, in that limestone, is one of the most important that has been made in the present times.

In Scotland, the greater part of the secondary formations, including the coal-measures, have been deposited in that extensive district which now forms the great valley of the Scottish Lowlands, and separates the primary country, of which the northernmost extremity of Great Britain is chiefly composed, from the transition chain of the southern border (at that time probably partly submarine), which may be geologically considered as the South Highlands.

According to Williams*, a line drawn from the mouth of the Tay, passing through Stirling to the northern extremity of the Isle of Arran, and another nearly parallel to it from St Abb's Head on the east coast, to Girvan on the west, will include between them the whole of the coal-fields of Scotland, with the exception of the insulated coal basins on the Nith and the Esk in Dumfriesshire, and the seams of coal that have been worked in Roxburghshire and on the coast of Berwickshire. The coal beds of Brora, and those that have been met with in some of the Hebrides, have generally been referred to formations of a more recent period.

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* History of the Mineral Kingdom, vol. ii. p. 302.

These lines are merely noticed here, in order to convey a general idea of the space occupied by the coal districts of the Scottish Lowlands; but they can by no means be considered as defining their precise limits with any degree of accuracy. On the south side of the great valley, these coal-measures follow the sinuosities of the transition hills by which they are bounded in that direction. Towards the north, although secondary strata referable to the lowest members of the carboniferous series are met with, skirting the base of the North Highlands, no workable coal (of the same formation) appears to have been found beyond the Tay, or to the northward of the boundary Mr Williams has indicated.

Although coal may not occur in equal abundance in every part of the district included within the limits specified, the causes by which it was produced having probably been influenced during their operation in a greater or less degree by local circumstances, in consequence of which a more copious deposition of the vegetable and bituminous matter from which this valuable combustible was derived, as well as a greater or less difference in the organic contents, or mineral character, of the associated strata may have taken place in some situations than in others; yet, when we consider that, with a few exceptions, only occasioned by the partial intrusion of rocks of another description, the whole extent of this space is occupied by an alternation of limestones, sandstones, slate clay, bituminous shale, coal, and ironstone, containing all the characteristic indications of their being members of the same formation, we have fair grounds for supposing, that although now separated into different fields or basins, the whole of the coal-measures throughout this extensive district were in all probability originally connected: the strata appearing to have been deposited more or less in a horizontal position, according to the force of the currents by which their materials had been transported, and to the nature of the surface upon which they were thrown down at the bottom of the sea, that must then have covered at least the whole of that portion of the Scottish Lowlands, forming either a strait or channel between two islands, or perhaps a vast estuary into which the rivers of the neighbouring primeval countries discharged their waters. The ripple marks, which are so remarkably displayed on the surface of the different beds of this series, even of those which are now the most highly inclined, likewise afford very strong evidence in favour of this supposition.

If we examine the nature of those portions of the district referred to, which form the exceptions to that regularity and continuity of the beds of the carboniferous series, we have supposed to have prevailed at the period of their deposition, we shall at once see that the rocks of which they are composed; although equally formed beneath the sea, could not like them have been mechanically produced by the transporting action of water, their crystalline character, and the various phenomena exhibited in their relations with the fossiliferous strata, affording sufficient proof of their origin having been due to a different agency, as well as that the ranges or groups of hills of the same description, which are now seen to interrupt or cut off the coal-measures separating them, into the fields or basins in which they are found at the present day, have for the most part been elevated at periods subsequent to the deposition and consolidation of that series.

It is now generally admitted, that these trappean hills and rocks are of igneous origin, the eruptions of porphyry, greenstone, or basalt by which they were produced having taken place from submarine volcanoes, or through cracks or fissures in the previously existing strata which at that period formed the bottom of the sea, over the surface of which the igneous matter appears to have been poured out in sheets of greater or less extent, either continuously, that is to say, one stratum overflowing another in constant succession, so as to form the immense overlying masses which everywhere present themselves to view in the coal measures, enveloping and carrying up in its passage large fragments of the broken strata, and exhibiting all those signs of violence and intrusion which such rocks so frequently display; or they may sometimes have been followed by sufficient intervals of repose, to have admitted of new deposits of sandstones, shales, or limestones, above the produce of each successive eruption, and in this way the interstratification of the trap with these beds, may be accounted for in those cases in which few, or no marks of disturbance are visible.

To the effects of Plutonic action during the carboniferous epoch, which may have been in activity for ages under the pressure of the superincumbent ocean, may be referred, besides the dislocations and undulations by which the strata are broken and distorted, those gradual risings and depressions of the land which it seems necessary to admit, in order to explain the alternations we so frequently meet with in the coal-measures, of beds teeming with the remains of marine animals, with others in which they are totally wanting, or that contain only those of terrestrial plants. In fact, they appear to have been the means of gradually preparing the countries occupied by these deposits, for the purposes which, in after times, they were destined to fulfil, when, by a renewal of the same agency, both descriptions of rocks were simultaneously lifted up above the waters in their consolidated state, to the positions they relatively occupy at the present day.

Nor is the infinite wisdom that has presided over the formation of our planet in this, as well as in every period, apparent only in the changes that have been thus produced in the configuration of its surface; for when we consider the various ways in which these subterranean convulsions may have influenced the chemical changes and combinations of the vegetable, animal, and gaseous matters, through which the coal itself has been brought into its present combustible form, they evidently appear to have been the chief agents employed by the wise design of a beneficent Providence, in the modification of this important deposit, by which it was prospectively adapted to the future necessities of the human race, which was not to be called into existence until after the lapse of a long succession of geological epochs.

The Pentland, Campsie, and Ochill Hills, as well as many others that occur within the limits specified, afford numerous and striking examples of the effects produced by their intrusion among the carboniferous strata, which, indeed, are no where better seen than in the Plutonic group that surrounds Edinburgh.

The Pentland range, which lies in a direction from south-west to north-east, is prolonged to the shores of the Frith of Forth by the trap-hills of Edinburgh, separating the coal deposits of East and Mid-Lothian from those which are situated to the westward of that chain, and form the great coal district in the basin of the Clyde. These hills bear strong internal evidence of their having been elevated at periods subsequent to the consolidation of the carboniferous series. In the midst of the Pentlands, a mass of sandstone has been thrown up, which forms a hill of considerable elevation ; and, although grauwacke-slate, and portions of a conglomerate of the same age, are seen at Habbie's-How, enveloped in the porphyry, it is highly probable that, if many of the rocks of this range, which have hitherto been referred to the transition period, were subjected to chemical analysis, they would be found to consist of the sandstones or the slate-clay of the coal-formation, altered to their present appearance by the effect of the igneous rocks with which they are in contact.

The occurrence of glance-coal, both in veins and disseminated in the porphyry and trap tuffa of the Calton Hill, and, in a similar manner, in most of the other hills of this group, tends also very strongly to confirm this opinion ; for it has most probably been caused by the igneous matter having burst through the coal strata in a fluid state, carrying up fragments of the coal with it, which, being by this process deprived of their bituminous qualities, have been converted into anthracite.

The Campsie Hills particularly, in the neighbourhood of Kilsyth, exhibit several magnificent sections illustrative of the period and manner of their elevation, when viewed in the relations they are there seen to bear to the carboniferous series. In the Bathgate Hills, and in Stirlingshire, coal and limestone are worked beneath columnar trap; and, indeed, the whole country occupied by the Scottish coal-measures displays more or less the effects produced by the influence of such igneous hills, or of the dykes connected with them, some of the former rising into mountain masses, others forming detached hills, either lofty and rugged, or scarcely elevated above the level of the surrounding country; while many of the latter do not penetrate to the surface, but remain at greater or less depths beneath it, producing much disturbance in the strata below, as well as many of the dislocations and elevations that are observed above ground, although the cause itself may not be perceptible.

When viewed collectively, a certain degree of parallelism may be observed in the principal chains formed by the hills of this nature, their general bearing being from the westward of south to the eastward of north, which corresponds with the general strike of the fossiliferous rocks; and they sometimes appear to assume a circular arrangement, encompassing valleys or basins of greater or less extent. This, however, cannot be said to be the case with respect to the smaller groups or insulated masses, many of which seem to have been protruded through the strata at different times, and under varying circumstances, without any apparent order or regularity; and the dykes are found to proceed in every direction from the principal masses, in some cases appearing to radiate from them as centres.

Besides the interruption occasioned to the coal measures by the elevation of the hills, by which the coal-fields are now separated from each other, their connection has in many instances been more or less cut off by rivers, estuaries, or portions of the sea, the beds of which have been formed in these deposits. This is particularly exemplified by the Clyde, the Frith of Forth, and very probably by that part of the Irish Channel which is situated within the prolongation of the lines before adverted to, as they flow through or cover strata of the coal formation, which though now removed by, or concealed beneath their waters, we have every reason to presume had once been continuous with those that are still visible on their opposite shores.

The connection between the Lothian coal-fields and that of Fifeshire, is very apparent, both in the general direction of the strata as shewn by their outcrop on the opposite shores of the Frith of Forth, and in the number and thickness of the beds of coal in each, which exactly correspond. Coal, too, is worked to some extent under the bed of the Frith at the Wemyss colliery in Fifeshire; but, in consequence of the frequent dislocations and other changes that may be attributed to the influence of igneous rocks, it would be difficult to establish the complete identity of each particular stratum on both sides of the water.

The carboniferous strata seen in the Island of Arran, and at Campbellton in Kintyre, appear to be only portions of the bed of the Irish channel, that have either been brought up to their present positions by the elevation of the plutonic rocks with which they are associated, or left in those situations when the disruption took place, by which that channel was formed; for although the coal district does not immediately shew itself on the Irish coast except at Ballycastle, being cut off by the trap of Antrim, there are still sufficient indications of its former existence along that shore, to prove the geological connection in this, as well as in most other respects between the west of Scotland and the north-east of Ireland.

The Scottish coal-measures having hitherto been very imperfectly described, their exact position in the series, as compared with that of the coal formation of England, has never been precisely determined; and although more than forty years have elapsed since the remarkable organic remains which have lately excited so much interest were first noticed in the coal-fields of Scotland, the important conclusions that might have been derived from this valuable addition to the palæontology of that period appear to have been entirely overlooked.

It may, however, be worthy of notice in this place, that although traces of some of the more recent secondary formations, such as the lias and oolite, and even chalk flints, have been observed in the northern parts of the island where primary rocks chiefly prevail; none whatever of a later date than the coal-measures (if we except the alluvial deposits) have been met with in the Great Valley of the Scottish Lowlands. For the red sandstones of that district do not appear in any instance to have been identified with the new red sandstone of England, but they have, apparently on good grounds, been referred by the best geological authorities to the lower part of the carboniferous series.

Professor Sedgwick * and Mr Conybeare † have remarked that some of the inferior beds of the English coal-measures, such as the millstone grit and limestone shale, which are but of little importance in the southern coal-fields, spread out as they proceed northwards, presenting subordinate beds of coal; and as these strata approach the transition chain of the Scottish border, the lower beds of the carboniferous limestone, in like manner, become subdivided, and alternate with coal, sandstone, and shale, the calcareous beds decreasing, and the coal-beds increasing, until they assume the character of a regular coal formation, to which that of Scotland bears so much analogy, as to render it highly probable that the same law has extended beyond these mountains, and that the Scottish coal-measures occupying the great valley of the Lowlands, are equally referable to the lower beds of the carboniferous limestone group.

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* Sedgwick's Address to the Geological Society, 1831.

† Conybeare's Report on Geology, Transactions of the British Association, vol. i.

Mr De La Beche, in a recent work on theoretical geology,* from a consideration of the facts connected with these coal-measures, is led to the conclusion, that, at the period when the carboniferous limestone of the south of England was produced in the sea, there was probably dry land in the part of the European area not far to the northward of the present Tweed, and that a gradual rise of the land was effected, by which means terrestrial vegetation travelled farther to the south, so that its remains became abundantly entombed in that direction, producing the coal now found in Southern England and Wales, as also in Belgium and Northern France, the continuity of the whole being superficially concealed by the more recent secondary and tertiary deposits of those countries.

But, as Mr De la Beche so justly and eloquently adds,† "To trace even the probable positions of dry land over the European area at the carboniferous epoch, would be most difficult, particularly when we recollect that what we term a geological epoch, may include a long series of ages, and that thus land may rise and fall, be degraded and replaced, sometimes by the same, sometimes by greater, intensities of various forces than those the effects of which we daily witness, and yet the whole be included in a geological epoch, or rather one during which a particular group of rocks has been formed."

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* Geological Researches, p. 318. † Ibid. p. 322.

Chemical Examination of the Petroleum of Rangoon. By Robert Christison, M. D. F. R. S. E. Professor of Materia Medica in the University of Edinburgh, &c.

(Read February 7. 1831.)

At the close of the preceding session, the Council of the Society did me the honour of entrusting me with the chemical examination of several articles sent not long ago to the Society by Mr Swinton, Secretary to Government at Calcutta. The articles in question are, 1. Specimens of the *black varnish* used in different parts of Hindostan and the Burmese territories, with specimens of the juices of which these varnishes are said to be compounded; 2. Specimens of *naphtha* from Persia, and of *petroleum* from Rangoon; 3. Specimens of *wood-oil*, a variety of fluid turpentine; 4. Specimens of crude caoutchouc, and of solutions of it in wood-oil.

The only one of these articles which has hitherto yielded results of such interest as to induce me to lay them before the Society, is the petroleum of Rangoon, which appears to contain a compound inflammable principle hitherto unknown.

The petroleum of Rangoon, termed by Mr Swinton Earth-oil, and more generally in the East, Ground-oil, is probably the same with what may be procured in various parts of our eastern dominions, by merely digging a few feet into the soil. In the vicinity of Rangoon it may be obtained in immense quantity for the mere trouble of digging it. It is used in Hindostan as pitch for all manner of wood-work; and is likewise a favourite external remedy for rheumatism, being employed for that purpose in the way of friction.

I am not aware that either this, or any of the European petroleums, has been subjected to careful analysis; and I should suppose no such analysis has been made, because no chemist, even with a careless examination, could have failed to observe that it contains a peculiar principle, the discovery of which would have given the analysis publicity.

The petroleum of Rangoon, at ordinary temperatures in this country, is a soft solid, of the consistence of lard. Its specific gravity, at the temperature of 60° Fahr. is 880, water being 1000. At the temperature of 86°, it is of the consistence of thin paste, and at 90° it melts completely, and forms a sluggish liquid, which acquires more fluidity as the temperature rises. Hence in the East, during the hot season, when it is dug for, it must be in the fluid state, and consequently entitled to its vulgar name ground-oil. It has a powerful naphthous odour, different from that of most other petroleums.

It is impossible to analyze this petroleum by means of the ordinary chemical solvents. Most of these solvents, such as the acids and alkalies, have little or no action on it; while alcohol, which acts feebly, and ether and the volatile oils, which act energetically, dissolve all its principles indiscriminately. The only practicable method of analysis, therefore, is the process by distillation.

When six ounces of petroleum were distilled, there was first procured, at a low heat, an ounce of nearly colourless naphtha; then another ounce of straw-yellow naphtha; then, at a higher heat, about another ounce, much more yellow, yet still fluid at 60° Fahr.; next, a considerable quantity of a yellowish liquid, which concreted at 60° into a loose mass, composed of numerous crystalline needles and plates, in a yellow naphthous fluid; and, as the distillation went on, this matter became more and more solid, but even towards the end was not firmer in consistence than lard. The residual matter in the retort, when the heat had been raised to full redness, was a spongy charcoal.

The naphtha, when rectified by a second distillation over a lamp, and then by a third distillation from the vapour-bath, is limpid and colourless, like sulphuric ether, and its density is 779. From the trials I have made, I consider that the Rangoon petroleum, when distilled on the large scale, will yield nearly a third of its volume of this colourless naphtha.

I need scarcely observe, that, in eastern countries, where the fresh juice of the caoutchouc tree cannot be procured, the naphtha from the Rangoon petroleum may prove a useful article. Like other kinds of naphtha it freely dissolves, or rather softens, caoutchouc; which, after the evaporation of the solvent, is recovered with its original properties. When it is to be used for this purpose, however, it must be carefully separated by distillation from the crystalline matter I am presently to describe, which rises as the distillation advances, and gives the naphtha a yellow colour. For, if any material proportion of this impurity be present, the caoutchouc solution dries very slowly, and long retains a greasy surface.

The yellowish, concrete, crystalline matter, like the petroleum itself, is not acted on by the caustic alkalies, or by the strong acids. Alcohol dissolves it very sparingly; ether and the essential oils, freely and entirely. None of these solvents, therefore, is of any use for separating the crystalline matter from the mass. But I have succeeded in procuring it in a state of purity by the following process:

The mass being cooled down to about 40° Fahr. it was spread out on filtering paper, and then subjected to strong pressure between many folds of common blotting paper. In this manner, an oily-like matter was taken up by the paper, and a pale yellowish-white crystalline substance was left, which was subsequently deprived of its remaining colour by repeated solution in boiling ether and recrystallization. Ether dissolved it largely, forming a pale yellow solution, which, on being cooled by immersing the vessel in very cold water, became a soft mass of interwoven crystals. This mass was then taken out, spread quickly on filtering paper, and immediately subjected to strong pressure between folds of blotting paper. The yellow colouring matter, which all remained in solution in the ether after it cooled, was thus, in a great measure, imbibed by the paper; and the crystalline matter was procured in a state of purity by repeating this process twice.

On first procuring this crystalline substance, I considered it as the same with the naphthaline procured not long ago from coal-tar by Mr Kidd, as related in his paper in the Philosophical Transactions for 1821. This opinion I was led to form from the appearance of the crystals, the nature of the substance which yields them, and the process of distillation by which they were procured.

On a careful examination, however, I find that the crystalline principle of petroleum differs materally from that of coal-tar, as well as from every other known body; and I shall therefore beg leave to denominate it *Petroline*, according to the analogy suggested by the name of Mr Kidd's crystalline principle.

As procured by the process described above, petroline forms foliaceous masses of small crystals of snowy whiteness, and bright pearly lustre. It is somewhat unctuous, and has a naphthous odour, which becomes very faint on exposure for some time to the air, and is removed altogether by boiling in alcohol. It fuses at 135° into a transparent, limpid, colourless fluid; but softens ten degrees lower. From a state of fusion it concretes on cooling into a translucent brittle mass, like wax, the density of which is 909 at 60° Fahr. At a temperature intermediate between the boiling point of water and a low red heat, the fluid boils, and distillation takes place. The greater part of the petroline condenses in the form of a fluid, which becomes on cooling a translucent waxy mass, with its original properties. But owing to the elevated temperature required for its distillation, a part is decomposed, a little charcoal is left behind, and a small quantity of inflammable gas passes over with the undecomposed sublimate. When heated in the open air, it catches fire, and burns with a dense white flame and much black smoke.

Petroline is insoluble in water, cold or boiling. Boiling alcohol takes up a small quantity, not more than a 450th of its weight, and on cooling deposites the greater part in minute shining crystals. Boiling ether, its proper solvent, easily takes up a fifth of its weight, which on cooling is in a great measure separated in a congeries of micaceous crystals, so abundant as apparently to convert the ether into a solid mass. Oil of turpentine also dissolves it in large quantity, and so does naphtha.

Caustic potass and caustic ammonia in solution have no visible effect on this substance. When boiled with it, it simply fuses, rises to the surface, and is there found, on cooling, with its usual properties. Concentrated muriatic, nitric, and sulphuric acids are equally without action, even when aided by the heat necessary to boil each. It simply melts and rises to the surface, and, except that it becomes slightly yellow with nitric, and slightly brown with sulphuric acid, no change of property is perceptible. It has no action with acetic or oxalic acid.

With iodine aided by a gentle heat, it quickly unites, forming a violet-coloured fluid, which on cooling becomes a dirty greenish-brown solid, very soluble, like each of its elements, in sulphuric ether.

I have not made any inquiry into the other chemical relations of petroline, my object at present being merely to establish its claims to be considered a new principle, distinct from any other hitherto known. In its properties it resembles naphtaline more than any other substance; but at the same time it differs from that body in very many respects. Naphtaline volatilizes at common atmospherical temperatures; does not fuse under 180° Fahr.; and, when heated a little above 400°, boils and sublimes in fine micaceous crystals. It is heavier than water. It forms a rose-coloured solution with acetic or oxalic acid; and with sulphuric acid it unites to form a peculiar acid, termed the Sulphonaphtalic, which, like other acids, neutralizes bases and forms salts with them. A single glance will satisfy every one how completely this account of the properties of naphtaline differs from the description given above of the properties of petroline.

It remains for me to determine its elementary composition. This I have not hitherto found leisure to accomplish; but I am engaged in the requisite experiments at the present moment, and will soon make them known to the Society. The experiments hitherto made merely enable me to say, that it contains a very large proportion of carbon.

APPENDIX.—*December* 1834.

A few months after the preceding paper was read before the Royal Society, the author observed in *Buchner's* Repertorium, an account of the discovery, in 1830, by *Dr Reichenbach*, of a new crystalline principle in tar, to which that chemist gave the name of Paraffine. As the properties of paraffine seemed from that account to be obviously identical with those of the Petroline of the Rangoon petroleum, and as Dr Reichenbach had ascertained its properties and composition fully, any farther investigation of the crystalline matter of petroleum appeared unnecessary. The original paper is now published, partly because allusions have been made to it in chemical works, and partly to serve as an introduction to the ulterior inquiry of Dr Gregory on the same subject.

The author, soon after laying this paper before the Royal Society, examined by the same process the petroleum of St Catherine's near Edinburgh, of Rochdale in Derbyshire, and of the island of Trinidad; but was unable to detect a similar crystalline principle in any of them.

On the Composition of the Petroleum of Rangoon, with Remarks on Petroleum and Naphtha in general. By WILLIAM GREGORY, M. D., F. R. S. E., Lecturer on Chemistry, Edinburgh, &c.

(Read 15th December 1834.)

IN the month of August 1830, REICHENBACH published his first memoir on the products of the destructive distillation of organic bodies, in which he described a new principle, to which he gave the name of Paraffine, as constantly occurring among those products. Not long after, in 1831, Dr CHRISTISON read before this Society a paper, in which he described a substance contained in the petroleum of Rangoon, to which he affixed the name of Petroline. On comparing the properties of these two substances, it was found that they agreed so nearly, that no doubt could be entertained that they were one and the same. As the priority of discovery rests with Dr REICHENBACH, the name of Paraffine is now generally adopted. The properties of paraffine are as follows:—It is white, tasteless, inodorous, rather tough, lighter than water, fusible at 125° or 130° F., and it distils unchanged at a higher temperature. It resists the action of the strongest acids and alkalies; and, finally, when pure, it burns with a bright flame without smoke. In a second memoir published by REICHENBACH in 1831, he described another of the products of destructive distillation, under the name of Eupione. This body, according to the latest experiments*, is a liquid, colour-

* SCHWEIGGER SCIDEL's Journal für Praktische Chemie, April 1834.

less and tasteless, but possessed of a fragrant odour. It is remarkably fluid, and is the lightest liquid known under the ordinary pressure, having a sp. gr. of only .655. It boils at 110°, distilling unchanged. Like paraffine, it resists the strongest acids and alkalies, and burns with a bright white flame without smoke. In the same memoir, Dr REICHENBACH states, that, proceeding on the usual idea that native naphtha was a product of destructive distillation, he examined it with a view to detect in it the new substance eupione, but could not succeed in obtaining a trace of it. He ascribed his failure to the difficulty of procuring genuine naphtha, and conjectured that oil of turpentine had been employed to adulterate the naphtha examined by him, which possessed in a high degree the peculiar odour and other properties of that oil. He returned to the subject in a third memoir, published in 1833, in which he stated, that he had carefully examined the best naphtha he could procure, but had been unable to discover in it the smallest quantity either of paraffine or of eupione. Struck with this result, he began to think that naphtha might not, after all, be really a product of destructive distillation. He distilled large quantities of brown coal along with water, consequently at 212°, at which temperature no destructive distillation could occur, and obtained considerable quantities of a naphtha, which agreed in all its characters with that which he had previously examined. It contained neither paraffine, eupione, nor any other product of destructive distillation, and, to his great surprise, presented the characteristic odour of oil of turpentine. He then proceeded to compare together the naphtha of commerce, the naphtha prepared by himself, which, of course, had not been adulterated, and pure oil of turpentine; and the result of the comparison was, that these three bodies, in sp. gr., in their boiling point, and in their chemical properties, exactly coincided. From this remarkable coincidence he drew the conclusion, that the naphtha (native) which he had examined was not adulterated, but genuine; and that it was nothing more than the oil of turpentine of the great pine-forests, to which our coal-beds owe their origin, having been separated by a gentle heat, either before the conversion of the wood into coal, or from the coal itself, as in his own experiments. It does not appear distinctly what native naphtha Dr Reichenbach examined ; but, from several passages, it would seem to have been that of Amiano.

On reading the last mentioned memoir of Reichenbach, it seemed obvious that the character given by him of this naphtha was very different from that of Persian naphtha, or from that of the naphtha which may be obtained from the petroleum of Rangoon by distillation ; and when, in addition to this, we consider the existence of paraffine in the latter, it is impossible not to infer, that, if Dr Reichenbach's experiments be correct, or rather if his naphtha were genuine, there must exist, at least, two very different kinds of naphtha. I resolved, therefore, to subject the Rangoon oil to a new examination, in order to ascertain whether or not it contained eupione. Dr Christison having kindly supplied me with a considerable quantity, I proceeded to rectify it, in the first instance ; and I very soon obtained a liquid, possessed, in a very considerable degree, of the properties of eupione, particularly its mobility, low sp. gr., and odour like that of narcissus. This rectified naphtha, although far from pure, has a sp. gr. of only .765, and boils at 200° F. I then subjected it to the action of oil of vitriol, and of a concentrated solution of potash, several times alternately, and also, as recommended by Reichenbach, to distillation with strong sulphuric acid and nitre. It resisted all these agents, for the most part, and, when finally rectified, assumed the appearance of the specimen No. 2., the sp. gr. of which is .744, which boils at 180° F., and which possesses, in a more marked degree than before, the peculiar odour of eupione. This odour, I may add, is exactly that of some impure eupione, which I extracted from coal-tar naphtha, and from the liquid produced by distilling caoutchouc. The smallness of the quantity has alone prevented me from pushing the purification further.

From these experiments, I consider myself entitled to conclude, that the petroleum of Rangoon contains eupione in very considerable quantity; and, as Dr Christison has already shewn that paraffine may be extracted from it, we can no longer doubt that it has been produced at a high temperature, or, in other words, that it is a product of destructive distillation.

Dr Christison, in his original paper on petroline, mentioned that a quantity of oil, sent by Mr Swinton to the Society as pure Persian naphtha, appeared to be pure oil of turpentine, which he concluded to have been substituted for the naphtha. I at first thought from Dr Reichenbach's experiments, that it might have been at once genuine naphtha, and genuine oil of turpentine; only, in this case, it must have been of that species of naphtha, which, like the artificial naphtha distilled from coal at 212° by Reichenbach, is not the product of destructive distillation. But Mr Swinton states that it has not the characters of true Persian naphtha, and although he was persuaded at the time that he had sent genuine naphtha, he is now convinced that a mistake has been committed. The Persian naphtha described and analyzed by Dr Thomson some years ago, of which he was kind enough to send me a small specimen, has again the characteristic odour of the Rangoon naphtha, indicating the presence of eupione. Dr Thomson obtained, by rectification, naphtha of .753, and boiling at 320°; while the eupione first described by Reichenbach boiled almost precisely at the same temperature, and had the sp. gr. .740. Some naphtha sold in Paris for preserving potassium, which is colourless and very free from oxygen, yielded to me by two successive rectifications, a liquid of sp. gr. 755, almost exactly that of Dr Thomson's rectified Persian naphtha. Saussure obtained from the naphtha of Amiano, a liquid of sp. gr. 758, boiling at 186°, thus approaching more nearly to the boiling point of Eupione.

The naphtha examined by Saussure, then, that analyzed by Dr Thomson, that which may be extracted from the Rangoon petroleum, and that now sold in Paris, all agree in yielding a liquid of sp. gr. 0.753 to .765; while that of oil of turpentine is 0.830, and has never been stated lower than 790. Moreover, Dr Thomson's Persian naphtha, that of Rangoon, and the commercial naphtha from Paris, all possess, in a considerable degree, the characteristic odour of eupione. If we consider that the specific gravity of pure eupione is 655, we shall not be surprised that its odour is somewhat disguised, when it is so impure as to have the sp. gr. 750. But we have other means of distinguishing between eupione and oil of turpentine. The first is the action of nitric acid. The violent decomposition produced by it in oil of turpentine is well known. I have tried Persian, Rangoon, and commercial naphtha with the very strongest fuming nitric acid, such as set fire to turpentine, but there was no action except at a high temperature, when a brown colour was produced. In fact, the Rangoon naphtha, during its purification, was distilled with sulphuric acid and nitre without change. The next test was the action of iodine, which produces a slight explosion when introduced into oil of turpentine. In the three varieties of naphtha just mentioned, there was scarcely any action beyond simple solution, and, in all, the solution had a rich violet colour, which is the colour of a solution of iodine in eupione. I am induced to suspect that a small quantity of oil of turpentine may be present, from the slight action manifested by nitric acid, and even by iodine, at a high temperature. But the presence of oil of turpentine is to be looked for in these liquids, if we consider them the products of the destructive distillation of Coniferæ, since the first effect of heat would be to expel the oil of turpentine, which would afterwards mix with the products of the true destructive distillation. Even admitting, therefore, that these varieties of naphtha contain oil of turpentine, still I conceive we must be satisfied that it is not a chief ingredient, far less, as in the oils examined by Dr Reichenbach, the only substance present; and if the characters I have mentioned be sufficient to prove the presence of eupione, these oils must be considered products of destructive distillation. This point, as far as the Rangoon naphtha is concerned, has, I think, been decided by the discovery of paraffine in it; and I think I have been able to ascertain the same fact with regard to the Persian naphtha described by Dr Thomson. Dr Thomson sent me two specimens, one of Persian naphtha, which Mr Swinton considers genuine; the other of petroleum, which Dr Thomson said would yield the same naphtha. The latter, however, turns out to be Trinidad asphaltum sent by mistake. My experiments, therefore, were confined to the naphtha already mentioned. This naphtha, being the most volatile portion of the petroleum, cannot be expected to contain more than a trace of the paraffine, which accumulates on the more fixed portions. Accordingly, on redistilling this naphtha, and collecting separately the last portion distilled, I found that when exposed to cold it became semisolid, just like the latter portions of the distillation of the Rangoon petroleum. I shall take the first opportunity of deciding the question on a larger scale; but in the mean time, from the great resemblance between this Persian naphtha and that of Rangoon, and the indications of paraffine I have obtained from the former, I am inclined to believe that the Persian and Rangoon petrolea are identical in composition.

In conclusion, I would beg to state the chief results which I have endeavoured to establish.

1. That there are some kinds of naphtha which contain paraffine and eupione, and are consequently the results of destructive distillation.

2. That the naphtha examined by Dr Reichenbach, which was oil of turpentine, if genuine, differs materially from that of Rangoon and of Persia, as well as that now sold in Paris, which are decidedly not oil of turpentine.

3. That the fact of oil of turpentine having been obtained by Dr REICHENBACH, by distilling brown coal at 212°, proves that that species of coal had not previously been exposed to a heat sufficient to expel its oil of turpentine; and, *a fortiori*, that it had never been subjected to destructive distillation.

It remains for the geologist to ascertain the sources of those kinds of naphtha which are evidently the products of destructive distillation. The petroleum of coal districts is probably not of this kind, for coal, in its ready decomposition by heat, offers a very strong argument against its having been at any time subjected to a high temperature.

If an easy and economical process could be discovered for extracting the paraffine and eupion out of the oriental petroleum in a state of purity, it seems obvious that the property of burning without smoke possessed by these bodies, would render them capable of an important practical application in the countries where they are found. Paraffine equals wax in the whiteness and purity of its light, while it is much more permanent and indestructible; and there is no liquid used for lamps, at all approaching to eupione in these properties. Moreover, both these substances are entirely free from any disagreeable smell when burnt. I have no doubt that a method of extracting them may be contrived, so cheap as to be applicable in the east; but in order to do this, it is necessary to operate, in the first instance, on much larger quantities than I have hitherto had at my disposal.

I propose, at a future time, to return to this interesting subject.

On the Refraction and Polarization of Heat. By JAMES D. FORBES, Esq., F. R. SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh.

(*Read 5th and 19th January* 1835.)

§ 1. *Some Miscellaneous Experiments with the Thermo-Multiplier.* § 2. *On the Polarization of Heat by Tourmaline.* § 3. *On the Polarization of Heat by Refraction and Reflection.* § 4. *On the Depolarization and Double Refraction of Heat.*

1. THE experiments to be detailed in this paper, which chiefly go to establish properties of heat wholly unlooked for, or only suspected to exist, having been made entirely by means of an instrument of great delicacy—the thermo-multiplier of MM. NOBILI and MELLONI, I shall premise some account of its application to the investigation of some more familiar modes of action.

§ 1. *Miscellaneous Experiments.*

2. We could hardly quote a stronger proof of the rapid and unexpected advances which enlarged theory may produce in practice, than by referring to the employment of thermo-electric action, discovered a few years since by SEEBECK, to the measurement of heat, with a degree of accuracy and facility which, perhaps, no thermometer has ever attained. Such is the principle of the thermo-multiplier of NOBILI and MELLONI. It is well known, that when two metals (and especially bismuth and antimony) are soldered together, and the point of union heated, an electric current is established from the one metal to the other,

which may be carried off by wires, and caused to act upon a delicate galvanometer or multiplier, the needle of which serves as an index; the galvanometer consisting, of course, of a magnetic needle, nearly freed from the influence of the earth's magnetism, and so connected with the wire which transmits the electricity, that the mutual influence of the magnetism and the electricity shall (by the law of Örsted) be a maximum.

3. It will readily be conceived, that, if a series of alternating bars of bismuth and antimony be placed parallel to each other, and the extremities alternately soldered together, when all the extremities facing one way are heated (as by the radiant influence of a lamp), whilst the others remain at the temperature of the apartment, the effects produced in a single pair, such as we first supposed, will be produced at each junction, and that the intensity of the whole effect will be greater, just as in the Voltaic pile. At one time it appeared doubtful how far electricity, of such small tension as is thus produced, could be so reinforced; but the instrument in question seems to prove the practicability of it. About thirty pairs are employed, and so delicately are they made, that the ends which exhibit one set of junctions are contained within a superficial area of four-tenths of a square inch.

4. The wires, from the extremity of the first and last element (just as in the Voltaic battery), convey the electricity to the multiplier, which consists of a flattened coil of silver-wire, covered with silk, the coils of the wire being parallel to the quiescent position of an astatic magnetic needle, which is perpendicular to the magnetic meridian. The deviations are measured in the usual manner, on a divided circle; upon which, with practice, a quarter of a degree may always be observed, and even minuter quantities occasionally estimated. These divisions are not necessarily proportional to the intensities of the currents which produce the corresponding deviations. The coils of wire, extending a long way on each side of zero, prevent the effect from diminishing so rapidly as if they were concentrated there; and M. Mel-

LONI has described, in his paper in the *Annales de Chimie* for May 1833, a simple and satisfactory method of estimating the relative values of degrees, at different points of the scale. He states, however, that, under 20° of deviation, he found them quite uniform. In the following experiments, the deviations were generally under 15°, and in almost no case exceeded 20°. I have therefore assumed the forces to be as the deviations. Besides, no change of importance would take place from a deviation from this law by a small quantity.

5. It will be perceived in the experiments which are to be detailed, that the determination of all the more important facts depends generally on whether one effect be greater or less than another, without much regard to their absolute amount. Now, the confidence which we can place in the uniformity of this instrument, or at least of the small changes capable of affecting it (since it is not liable like thermometers, and especially air-thermometers, to advance by starts) is such, as to admit of almost indefinite subdivision, where the relations of small quantities are alone concerned. I conceived, therefore, that, without impairing its sensibility by lengthening the galvanometer needle, we might advantageously magnify the divisions by optical means. This I proposed to do by observing the motions of the index by means of a small telescope, fixing in front of the object glass a lens whose focus is situated at the part of the scale desired to be magnified. It might also be easy, in order to compare larger quantities, to make this micrometric system revolve so as to be always similarly placed as regards the needle, and thus avoid the effects of parallax, which at present require constant vigilance.

6. The method here indicated, I have put in practice with the greatest success in my later researches ; one-tenth of a degree becomes easily visible, and the constancy of the indications fully justify this method of microscopic examination, which has enabled me to verify the most delicate deductions I had drawn from

simple observation, and to obtain results which otherwise I must have been unable confidently to announce.

7. For the precautions to be employed in the use of the thermo-multiplier, I must refer to the first of M. Melloni's very original papers in the Annales de Chimie (for May 1833), but I may state, once for all, that when habituated to the use of it, I found it more simple, manageable, and comparable, than I could previously have imagined. Notwithstanding its delicacy and the promptitude of its action, a few precautions suffice to prevent any derangement from without. The only inconvenience which I experienced, was in the determination of the zero of the scale, which appears liable to some fluctuations, which may be considered as accidental.* It rarely happened, however, that these affected the results of my experiments, because, as I have said, these were always confined to small variations of temperature (indicated by a deviation generally under 15° on the scale) when such fluctuations did not appear; and the results produced by the same cause under the same circumstances were admirably constant, as well as the position of the zero point.

8. There is one circumstance which gives a degree of delicacy to the indications of the thermo-multiplier, when we wish to ascertain very minute differences of effect, which no other thermometric instrument possesses. When we wish to ascertain the *existence*, not the *measure*, of some cause of heat or cold, if we watch the needle of the multiplier at the instant at which the change of circumstances intended to produce the effect takes place, we shall perceive, in the instantaneous effect on the needle, an evidence of a far more decisive character than the *merely statical*

* This fluctuation appears to have been wholly, or almost wholly, owing to the imperfect communications established between the pile and the galvanometer, which, when I received the instrument from Paris, were in a very rude state, and the want of continuity gave rise to some curious phenomena. Since I had the more important junctions *soldered*, these anomalies disappeared.

deviation (at which, after several oscillations, it is finally to settle) could afford. Not only does the acquired velocity frequently carry it through double the space due to the statical effect; but I have observed that the action of the thermo-electric pile so far resembles that of the voltaic, that we appear to have an excess of effect at the first moment of action, which gives a greater deviation than can be afterwards obtained *. It is therefore to be recollected, that in speaking confidently of effects, which, statically speaking, are exceedingly small, the experimentalist has a species of evidence far stronger than the mere numerical expression of the deviation of the needle, but the degree of which must be taken on the faith of his veracity. Thus I have obtained repeated differences, not exceeding half or even *a quarter of a degree* of the multiplier (observed without a telescope), which, by the promptitude with which the needle was repelled or attracted at the instant that the change of circumstances to be considered was effected, left as little doubt in my mind as if the numerical result had been many times greater.

9. Having satisfied myself, in a variety of ways, of the extreme delicacy and promptitude of action of this instrument, I thought of applying it to detect the heat of the moon's rays in a more unexceptionable manner than, I am persuaded, it has ever been attempted. This curious question had not escaped MM. Nobili and Melloni when they first constructed the instrument, and they mention in their first account of the thermo-mul-

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* This remarkable effect, which may be described as an increase of tension by confinement, seems generally to exist where the conductors of imponderable agents oppose considerable resistance to their passage. It is familiar in Voltaic electricity, and I have often observed it in magnetic electricity. It is similar to the action which I have attempted to demonstrate in the passage of heat from good to bad conductors (see the 12th Volume of these Transactions), where we have the full advantage of the dynamical effect; whilst the existence of statical tension in heat seems likewise to be proved (as we might have anticipated) by the beautiful experiment described by Professor Powell in the Philosophical Transactions for 1834.

tiplier their attempts at its solution *. But, like previous experimenters, they employed a metallic mirror to concentrate the rays of the moon, which, acting in the usual manner of dispersing the heat of the thermometer, produced so great a cooling effect, as completely to neutralize any positive results.

10. It occurred to me, however, from the consideration of M. MELLONI's very decisive experiments as to the permeability of screens of different kinds to heat from various sources, that the moon's heat must, in very great proportion at least, radiate through glass. And this on several grounds; as, 1. because the sun's heat, of which this may be considered as an integral part, does so with scarcely any loss; 2. because heat, accompanied by light, *always* does so, and generally in proportion to the brilliancy and refrangibility of that light; and, 3. because the lunar rays having passed through the whole thickness of the atmosphere must, according to the experiments of DE LA ROCHE, fully confirmed by MELLONI, have parted with the greater part of that species of heat most easily stopped, and hence arrive at the earth in a state comparatively capable of passing through glass and similar substances. If this opinion be correct (nor can I entertain any doubt upon it), and if we substitute a lens for a mirror to concentrate the lunar rays, we shall profit by all, or nearly all, of their heating effect, whilst such a lens, instead of promoting the radiation of the heat of the thermometer to the sky, will *entirely stop it* (because heat of this description does not pass sensibly through the thinnest glass), and thus its disturbing influence will be entirely prevented.

11. I employed, therefore, a polyzonal lens made by SOLEIL of Paris, in my custody, to concentrate the moon's light. The diameter of the lens is 30 inches; its focal distance about 41 inches, whence we may compute the size of the lunar image to be a circle 0.38 inch in diameter. Comparing this with the di-

* Annales de Chimie et de Physique. December 1831.

mensions of the intercepted cylinder of rays, we shall find the concentration to exceed 6000 times. But even if we admit that half the rays are reflected, dispersed and absorbed, we shall have still an effective increase of 3000 times.

12. My experiments were made on the 16th December 1834, between 9 and 11 o'clock, the moon being only 18 hours past full, and (towards the close) less than 2 hours from the meridian. She was also remarkably high, having a declination of 25° north. The thermal pile, which was particularly commodious for the experiment, had one extremity elevated to the proper angle, and being placed accurately in the focus of the mirror, the moon's image was brilliantly thrown on the extremity of the pile. The sky was on the whole very pure, though an occasional milkiness was perceived, but the best observations were made at the clearest moments, because then the air was also most still; for though the instrument was placed in a most sheltered spot, the faintest breeze was indicated by the altered temperature inducing a deflection of the needle, and with such promptitude, that I generally could perceive in this way its approach before I could feel it. The action of the lens was so perfect, that the image was extremely sharp, and the spots clearly defined. The lunar rays were alternately screened and admitted by an assistant passing a sheet of pasteboard across the surface of the lens *next the moon;* for when it was interposed between the lens and the instrument, a sensible disturbance took place. By these and other precautions, the needle was steady beyond my expectation, and during an hour and a quarter that the observation lasted, I had probably at least twenty perfectly unexceptionable comparative observations, free from the influence of wind, and which invariably gave not the faintest indication of warmth. When I got a deviation of the needle at the moment of unscreening the moon's rays, I verified it by screening them instantly, and watching for a return to zero, but I was always disappointed. I feel quite confident that the effect, if there was any, could not amount to *a quarter of a degree*

of the galvanometer; and, owing to the *dynamical* effect which I have described of a first impulse, that it is improbable that it amounted to half that quantity.

13. Hence it becomes an object of interest to form some estimate of the sensibility of the thermo-multiplier, compared to common thermometers. It would be difficult to give a precise measure of the degrees of temperature of the two extremities of the pile,* but we may compare the effect of equal quantities of heat upon this and another instrument. For this purpose I employed two air thermometers of great delicacy; one was the photometer of LESLIE, having one ball covered with lamp black, and exposed to the same source of heat as the pile, whilst the other ball was shaded. The other instrument was a vertical differential thermometer, having a hemispherical reflector, intercepting a cone of rays 2.50 square inches in section. I found it impossible to operate with small degrees of heat, which could not be reckoned accurately on the air thermometers, owing to their tardy action; but, from several experiments, I concluded that the same quantity of heat falling on the photometer ball and on the pile, moved the liquid of the former through 1°, and the needle of the multiplier through 4.°2. The degrees of the photometer being 10ths of 1° cent., one centigrade degree would correspond to 42° of the galvanometer, (assumed of equal value throughout the scale.) The experiment with the differential thermometer, being similarly conducted, gave for the effects of equal quantities of heat, 1° cent. to 62° of the multiplier. If we assume from these experiments that a quantity of heat which raises an air thermometer by one-fiftieth of a centigrade degree, affects the galvanometer by 1°, since *a quarter* of a degree of the latter is a *measurable*

* This might best be done by adapting a differential thermometer of extreme delicacy, so that the balls might be in contact with the two extremities of the pile, and the spaces round them filled up with copper filings, or some such material. But the experiment could hardly be quite decisive.

quantity, and half of that may be estimated as a sensible impression, we may measure an effect of $\frac{1}{200}$ of a centigrade degree, and perceive (by unassisted vision), an effect of $\frac{1}{400}$.

14. In the case of the moon's rays, concentrated 3000 times, we have seen that it is improbable that even the last effect was produced. The whole sensitive extremity of the pile being larger than the moon's image, was not brought into action; but if we compare their relative dimensions,* we shall still find that it is improbable that the direct light of the moon would raise a thermometer *one three-hundred-thousandth part of a centigrade degree*, at least in this climate.

15. The value of the thermo-multiplier consists not so much in the minuteness of its indications, which may easily be equalled by employing large enough thermometers, but in the certainty and rapidity of its action. Air thermometers, such as I compared it with, though the size of the balls was inconsiderable, required so long a time to assume their temperature, that, when exposed simultaneously with the thermal pile to the source of heat, the latter had almost assumed its maximum effect before the others had sensibly moved; and it is obvious that, in delicate experiments, where constancy in the producing cause is presumed, rapidity of execution is essential. In short, with an air thermometer (which requires from 10 to 15 minutes to give a single result), the greater part of the experiments to be described would have been impossible from this cause alone, and the remainder would have been tedious beyond measure. It will therefore be conceived that were thermometers enlarged so as to give as minute indications as the multiplier, they would be utterly unmanageable.

16. Of all the researches of M. Melloni on radiant heat that

* The moon's image contained 0.114 square inches, whilst the area of the pile is about 0.40. Hence little more than a fourth of the pile was brought fully into action; but any dispersed light (for which we have made allowance), would act on the neighbouring parts.

of the refrangibility of non-luminous heat by a prism of rock salt is the most striking. Viewing it in connection with the theory of heat, and its analogies with light, this experiment is even more important than those connected with the very obscure subject of absorption, which has been illustrated by his numerous determinations of the stoppage of radiant heat, by screens or media of different kinds. At the time when I commenced these experiments, in November last, I was not aware that M. MELLONI had published a *second* memoir, which, after many of my experiments were made, I met with in the fifty-fifth volume of the *Annales de Chimie*. It appeared to me a matter of great interest to determine the refrangibility of non-luminous heat by direct experiment; and, in doing this, I was led to verify, in the fullest manner, the published experiments of M. MELLONI on the refraction of heat, not merely derived from brass heated by an alcohol lamp, so as not to have the faintest luminosity in the dark, but also of heat derived simply from water under its boiling point. I found that so admirable was the sensibility of the instrument, that we may determine, with great accuracy, by repeated trials, the angular position of the prism which gives the maximum effect; and, having given the angles made by the incident and emergent rays with the sides of the prism under those circumstances, we may compute the index of refraction for the rock-salt, in regard to rays of heat. Upon making the calculation, it appeared that the direction thus experimentally found, gave nearly the same result as for light, which was an ample proof of the reality and striking nature of the experimental result; but it at the same time appeared that the whole dispersion for the spectrum is so inconsiderable, that, in this way, we could hardly expect to obtain a numerical result for the dispersion of the heating rays. I afterwards found, upon reading M. MELLONI's second memoir, that he had experienced the same difficulties, and that, though he constructed a pile on purpose, he had not succeeded in obtaining numerical results. He found, however, that the refrangibility of the rays diminished with their temperature. I also obtained a slight refraction of non-luminous heat through a glass prism.

17. But if heat be capable of refraction by the ordinary agents, an important question arises, Is the phenomenon of double refraction common to heat and light? Rock-salt, the only substance yet discovered, which transmits dark heat in large quantity, does not possess this power. To attempt it with Iceland spar would certainly be fruitless, from the very small transmitting power which it possesses, besides some other practical difficulties which suggest themselves. It must be by more refined processes that we can detect this property. Such will be stated in the sequel.

§ 2. *On the Polarization of Heat by Tourmaline.*

18. It is well known that two slices of tourmaline cut parallel to the axis of the crystal, as they are looked through with their axes parallel or perpendicular to one another, transmit a great portion of the incident light in the one case, and almost wholly intercept it in the other.

19. It occurred to me as a curious question, at an early period of my researches, whether non-luminous heat would undergo any similar change in similar circumstances. I made a preliminary experiment with heat from an oil-lamp (not an argand), and though, when the axes were crossed, the whole light was stopped, the heat transmitted appeared to be as intense as before. The tourmalines which I employed were mounted on glass, and were kindly lent to me by the Reverend Mr Craig. Struck with the singularity of the result, I repeated the experiment with additional precautions, and I found that some circumstance prevented this statement from being true in all its generality. The quantity of heat transmitted being very small, the lamp, the tourmalines, and the pile were very near to one another; and, as the tourmaline absorbs heat with great rapidity, I found that a minute difference might exist if the experiment was made first with

the axes parallel, and then with the axes crossed, which difference might yet be made up by the secondary radiation from the heated tourmaline, which was constantly becoming more intense. Such at least appeared to be the chief source of error, which I am particular in stating, because I afterwards discovered that M. Melloni had been led to the very same conclusion as I at first was, and had published it.*

20. When I proceeded to verify my results by a series of successive observations, under the two conditions of axes parallel and axes crossed, so as to eliminate any error from a constantly progressive change, I perceived my mistake. As this illustrates the method by which almost all my observations have been reduced, I shall give an example. Two measures of intensity in the position where least light was transmitted, which is marked *Dark*, have their mean taken, which is then compared with the intervening observation in the position of greatest illumination, which is marked *Light*. These tourmalines we may call A and B.

1834, Dec. 4.—*Oil Lamp* † *six inches from Centre of the Pile.*

	Dark	Mean.	Light.	Ratio.
Deviations of Galvanometer.	$4\frac{1}{4}$°			
		4.5°	5.2°	86 : 100
	$4\frac{3}{4}$			
		5.0	6.0	83 : 100
	$5\frac{1}{4}$			
		5.2	6.0	86 : 100
	5			
		5.4	6.5	83 : 100
	$5\frac{3}{4}$			

* In operating with tourmaline, and also with other substances which transmitted directly but little heat, and which, therefore, required to be placed near the source of heat, in order to get distinct results, I have always found that the small differences of effect of which I was in search, became gradually less as the process of *conduction* advanced. The *first* result was generally the best marked. This effect may be compared to the destruction of the phenomena of diffraction in light, by the interference of other undulations than those producing the phenomenon sought. Such interfering waves would, in this case, proceed from the secondary radiation of the interposed bodies. But there seems also some inaptitude in the pile to accommodate itself to reiterated and very slightly different alternations of temperature.

† The oil lamp used when not expressly called "argand," was Locatelli's lamp with a solid square wick, which is what M. Melloni employed.

Another series on a different day gave the following quantities per cent. 91, 82, 94. Mean of the whole 86.4 : 100.

21. Having obtained these decisive results, I proceeded to operate with other sources of heat, and with different tourmalines. Anxious to avoid the interposition of glass, I had a pair of tourmalines of large size cut without any support. But the best kind will not bear this, and they polarized imperfectly. Only fifteen-sixteenths (approximately) of the light in the bright position was stopped in the dark, whilst with the tourmalines A and B every vestige of the brightest gas flame was excluded. With these tourmalines (which may be called C and D) I verified the general conclusions. I was unable to get sufficient effect from non-luminous heat to verify the law in that case.

22. I had two very fine tourmalines cut and mounted on extremely thin glass. These we may call E and F. With them I was enabled to extend and verify the law of polarization even to the case of non-luminous heated brass, (whose temperature when warmed by alcohol, M. Melloni estimates at 390° cent. = 734° Fahr.) And it is worthy of observation that among twenty-nine pairs of comparative observations, made with three sets of tourmalines, and heated from the following sources, argand lamp, simple oil lamp, platinum rendered incandescent by alcohol, and non-luminous hot brass, there was only *one* which did not give positive indications of polarization. The effect, however, with non-luminous heat is extremely feeble, and the percentage very small, because it is with great difficulty that we can obtain results at all with the interposition of two plates of glass, and two of tourmaline (however thin), and a large portion of heat which reaches the pile is derived from conduction, and therefore diminishes the proportion of polarization.

23. It is very important to observe, that in this and all similar cases, *the effect of conduction or the secondary radiation of heat from screens always tends to disguise, and never to produce, the differences of which we are in search;* that is, so long as the *means* of alternate observations are taken in the way we have described.

24. The following are the general results of my experiments on Tourmaline.

Source of Heat.	No. of Comparisons. A and B	E and F	Proportions of Heat polarized by Tourmalines A and B	E and F *
Argand lamp, -		3		16 per cent.
Oil lamp, - -	7	3	14 per cent.	11 ... ...
Incandescent platinum,	4	3	15	12 ... ...
Brass at 700°, -		7 (1 negative)		3 ... ...

I cannot, therefore, entertain any doubt on the polarization of heat by tourmaline, notwithstanding the opposite result which M. MELLONI (and I also at first) obtained.

25. Some very curious considerations arise from the study of these facts. Since 84 per cent. of the heating rays of an Argand lamp pass through the second tourmaline in the case where the light is entirely stopped, we must adopt one of two conclusions: either that the heat which *necessarily* accompanies light is excessively small, or else that radiant light during its instantaneous passage through a medium, is capable of being *converted* into radiant heat. The latter supposition we have no analogies strong enough to warrant us to adopt, though were heat really not polarized by tourmaline, we must have done so. All our experiments point to the first, namely, that heat, though intimately partaking of the nature of light, and accompanying it under certain circumstances (as in refraction and reflection), is capable of almost complete separation from it under others. Thus, almost all the heat is stopped by a plate of alum, which transmits nearly the whole light, whilst a second plate of tourmaline stops the whole light, but transmits a large share of the heat.

26. The tourmaline affords a precious method of investigating the influence of light, since the quantity of matter to be traversed is exactly the same, whatever be the direction of the axes of the crystal. In this it differs from all other modes of absorption.

* It appears that the axes of E and F were not precisely crossed in these experiments.

27. M. Melloni has proved that the more light that accompanies heat, the greater power it has to traverse most media, such as clear glass or alum. I made several experiments on the *quality* of the heat which passed through the tourmalines in their darkest and in their brightest positions, and I always found that the presence of the light materially increased the power of the heat to permeate such screens, though we have seen how little it added to the *quantity*.

28. This fact, namely, that by sifting, as it were, heat separate from light, we give to it the characters of non-luminous heat, or heat of low temperature, and small refrangibility, such as exists beyond the red extremity of the spectrum, seems so far congenial with analogy. But according to Melloni's experiments, this does not hold with other degrees of *sifting* of heat. Thus the absorption of all rays of light, except the blue, the yellow, or the red, by coloured glasses, does not give the peculiar character to the heat which it possesses, when it accompanies light in the process of refraction, namely, that of permeating screens (in general) more readily as the refrangibility is greater. Hence I conceive we must conclude, that heat in the spectrum *accompanies* the light, and has corresponding properties, but that in general these properties are independent of the nature of the accompanying light.

29. The only fact which appeared to militate against this view, so far as coloured media were concerned, was the case of *green* light. It appeared probable that this arose from some peculiarity in the absorptive nature of the material, not from its colour. To investigate this point, I tried the relative transparency (or *diathermancy*, to borrow a word from M. Melloni) of screens for the heat of various coloured flames. I did not find that marked peculiarity in the *green*, which M. Melloni observed in the absorptive action of green glass. The following results are not pretended to be numerically accurate, but they are probably nearly comparable. The flames were obtained from alcohol, combined with the following substances:—for the red, nitrate of

strontia (the muriate is better); the yellow, with muriate of soda; the green, boracic acid; the blue, pure alcohol. The unsteadiness of intensity of an alcohol flame prevents great numerical accuracy.

Colour of Flame.	Number of Rays of Heat out of 100 transmitted by Alum.	Glass.	Rock Salt.
Red,	11	26	85
Yellow,	$11\frac{1}{2}$	28	87
Green,	11	26	84
Blue,	10	30	83

The differences are certainly within the limits of errors of observation.

30. I am disposed to believe, however, that in these experiments, as well as MELLONI's, some effect is probably due to the simple presence of light of a particular quality, though its heating power may be small. This my experiments with tourmalines countenance. We can hardly, however, look for a solution of these difficulties, until some of the most stubborn difficulties in the theory of light, the laws of dispersion and absorption (and especially that peculiar absorptive power which permits the tourmaline only to transmit one polarized pencil) are completely overcome. Meanwhile, we pass with pleasure to the consideration of some of those properties of heat which serve to connect it with the best determined and best explained departments of optics.

§ 3. *On the Polarization of Heat by Refraction and Reflection.*

31. Soon after the discoveries connected with the polarization of light, which illustrated the earlier part of this century, the question of the polarization of heat was taken up by MALUS and BERARD.* In the case of heat accompanying solar light, it was decisively proved, as might have been anticipated; but in the case of heat from terrestrial, and especially non-luminous sources, though M. BERARD considered that he had proved it, he gives

* Memoires d'Arceuil, tom. iii.

no *quantitative* measures which could enable us to judge of the evidence, nor does it appear that subsequent experimenters have been able to verify the assertion.*

32. The importance of the subject will be estimated, when we consider the very definite laws to which the polarization of light is subjected, and the accuracy with which they are represented upon the undulatory hypothesis. If heat, when wholly deprived of light, be subjected to similar modifications, our progress in acquiring a knowledge of the true nature of heat will be greatly advanced by our previous analogical acquaintance with the laws of light.†

33. I had been led to make the experiment with tourmalines, because of the convenience with which all experiments on transmitted heat are made by means of the multiplier. But at the same time it occurred to me, that the transmitted pencil of heat passing through laminæ at the polarizing angle might like-

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* See Professor POWELL's papers in the Edinburgh Journal of Science Second Series, vols. vi. and x.

† The importance of analogies in science has not perhaps been sufficiently insisted on by writers on the methods of philosophizing. A clear perception of *connexion* has been by far the most fertile source of discovery. That of gravitation itself was only an extended analogy. The undulatory theory of light has been preeminently indebted to the co-ordinate science of acoustics, which afforded to Dr YOUNG the most plausible basis of his curious and original investigations; and unless that science had existed, it may be doubted whether such a speculation would ever have been invented, or, if invented, would have been listened to. The penetrating sagacity of M. FRESNEL, in his prosecution of the subject, has led him to draw from mechanical and mathematical analogies, accurate representations of laws which no strict reasoning could have enabled him to arrive at. Of this his marvellous prediction of the circular polarization of light by two total reflections in glass, is the most prominent example, a conclusion which no general acuteness could have foreseen, and which was founded on the mere analogy of certain interpretations of imaginary expressions. The mere reasoner about phenomena could never have arrived at the result,—the mere mathematician would have repudiated a deduction founded upon analogy alone. The cause of the long postponement of the discovery of electro-magnetism was the complete apparent breach of analogy between the modes of action of the electric and magnetic forces, and any others previously known.

wise be adapted to the instrument. I had previously noticed the large proportion of heat transmitted by thin plates of mica, and I thought of applying bundles of mica plates placed at the polarizing angle, and so cut from the plate, that the plane of incidence corresponded with one of the *neutral sections* of the mica plate, (the section used was that *perpendicular* to the principal plane,) so that the transmitted pencil would be polarized exactly similarly to that refracted through glass or any singly refracting medium.

34. I prepared two pairs of bundles of plates of mica of this description, the first (which I called A and B) having a thickness of about one-fiftieth of an inch, and was split into about ten plates, whilst the others (C and D) were only half the thickness, and contained but half as many reflecting surfaces. I found that these plates, placed at the proper angle, polarized light very satisfactorily. On applying them to heat, I had the satisfaction of finding that not only was heat from an oil lamp most decisively polarized, but also that from a brass plate warmed by alcohol, but so as to be quite invisible in the dark, having probably a temperature (as before mentioned) of about 700° Fahr. These experiments were made on the 22d November last, and were afterwards amply confirmed *.

35. It is to this mode of observing that I attribute chiefly the success of my after inquiries. The mode of reflection for polarizing is attended with so much inconvenience where a thermometer is concerned, and especially with the multiplier, as to render the employment of it tedious and incommodious; whereas by having two bundles of mica plates arranged in square tubes, so

* I did not see M. Melloni's second paper till the 10th of December, after I had obtained the chief fundamental results contained in this paper. It does not appear, however, that M. Melloni had thought of applying his instrument to any question of polarization except that of tourmaline, and in a note he alludes to the objections, which had been urged against Berard's conclusions, objections which he does not consider to have been overcome.—*Ann. de Chimie*, lv. 374.

that the one fits the extremity of the thermal pile, and the other slips into the first, and by turning it round we get observations with plates, whose planes of incidence for rays passing along the axis of the tube, are inclined 0°, 90°, 180°, or 270° to one another, the direction of the ray is generally in a single straight line, and the observations are made in the same manner, and with equal facility as in ordinary experiments on transmission. I have little doubt that, in this way, the polarization of heat might be proved without the aid of the thermo-multiplier. The plates were fixed at the polarizing angle *for light*. After what has been said, art. (16), on the refrangibility of heat, it is clear, that the alteration of the polarizing angle, in order to accommodate it to heat, could hardly amount (by Sir David Brewster's law) to a sensible quantity.

36. I fitted up two other bundles of mica-plates, in square pasteboard tubes of the kind described, which were marked E and F, the other plates being occasionally substituted, in order to verify the results, and to shew that no accidental peculiarity of the plates could account for the differences observed. My experiments were usually made thus. The tube E was fixed to the pile; the tube F, containing the other plate, had an index, which pointed to 0°, when the two plates were parallel, to 90° when they were at right angles, &c. Five observations were taken; at 0°, 90°, 180°, 270°, and again at 0°. The mean of the first and last were taken; then the mean of this, and the indication at 180°, and the difference between this and the mean at 90° and 270°, was considered as the polarizing effect. An example will best illustrate this:—

1834, Nov. 26.—*Brass heated by Alcohol: $5\frac{1}{2}$ inches from centre of Pile.*

		Deviation.
Analyzing plate (E) at 0°; polarizing plate (F) at	0°	$6\frac{1}{2}$°
	90	$5\frac{1}{4}$
	180	7
	270	6
	0°	$7\frac{1}{4}$

Mean at 0°...6°.9		
180°...7 .0		
Mean, .	6 .9	Ratio 100 : 81, or 19 per cent. polarized.
Mean at 90° and 270°, .	5 .6	

The general concordance of these experiments will be gathered from the following list of results.

37. With *non-luminous heat* from *brass* about 700°; ratio of effect, when plates E and F were *parallel* and *crossed*, 100 : 78; 100 : 76; 100 : 80 *; 100 : 81 (from five observations each), with plates E and A (from three observations each), 100 : 74; 100 : 59; 100 : 68; 100 : 60; with A and B, ratios 100 : 78; 100 : 72.

38. With *non-luminous heat* from *mercury*, about 500°, plates E and F; 100 : 77; 100 : 90, plates E and A; 100 : 88; with A and B, 100 : 78.

39. But even with heat from water below the boiling point, I was able, by the improved method of observing the galvanometer, art. (5), (6), to establish completely the polarizing effect. One series of six comparisons (conducted as in (20),) gave for the proportions of heat transmitted, when the plates E and F were *parallel* and *crossed*, 100 : 93; another of eight comparisons, gave 100 : 96; a third, of eight, 100 : 92. Among these twenty-two comparisons, only *one* gave a result slightly negative.

40. With *platinum rendered incandescent* by alcohol, the effect appears decidedly greater than with any other source of heat I have tried. Plates E and F; ratios of effect when *parallel* and *crossed*, 100 : 59; 100 : 62; 100 : 66; 100 : 54. The brilliancy of the incandescence affects materially the transmission.

41. *Alcohol flame*, as might be anticipated, is less steady; means from sets of five observations, with plates E and F; 100 : 66; 100 : 72; 100 : 79; 100 : 42; 100 : 62.

42. With the simple *oil-lamp* of Locatelli; plates E and F, the ratios are 100 : 76; 100 : 73.5; 100 : 79.

* Plate B was used to polarize in this experiment.

43. With *argand lamp*, and glass chimney; Plates E and F; ratios, 100 : 70 ; 100 : 72; results very steady.

44. When we combine these results*, and compare them with the quantity of light polarized, which was derived from some rude photometrical experiments, which agreed pretty nearly, we get the following approximations to the degrees of polarization, by a given combination, and depending on the source of heat.

SOURCE OF HEAT.	Rays out of 100, polarized by transmission through Mica Plates E. and F.
Argand Lamp (glass chimney),	29
Locatelli Lamp,	24
Alcohol Flame,	36
Incandescent Platinum,	40
Brass, about 700°,	22
Mercury, about 500° (in crucible),	17
Water under 200°,	6
Proportion of *Light* polarized,†	89

45. So completely and satisfactorily made out does the polarization of heat appear by these concurrent experiments, that it was little more than a matter of curiosity to verify it in the case of reflection from surfaces, as well as in that of transmission through plates. This, however, I also established, though not without much more trouble than the other, the change of direction of the ray by reflection presenting a troublesome necessity for making the thermometric instrument, that is, the pile, moveable; at least, this was the most unexceptionable method. I fully esta-

* It should be remarked, that these experiments contain *all* the measures I have made with a view to this determination, except two, which were made the very first day I discovered the fact, and which were not accurate enough to be employed. I mention this, because, in such experiments, it is important to be assured of the constancy and marked nature of a result, which can only be appreciated by keeping back no fairly made observation.

† Though I am not aware of any source of error, I cannot help thinking, that, in this case, and in that of the tourmaline, Art. (21.), the defalcation of light is estimated too high.

blished the fact of comparative non-reflection from a second reflecting plate of mica, the plane of incidence being at right angles to the first; but I had more reason than ever to be satisfied of the value of the simple and effective method of transmission through thin mica plates. In fact, it was only by the aid of that method that I could have advanced to the still more delicate inquiries which, by the constancy of my first results, I was encouraged to undertake.

§ 4. *On the Depolarization and Double Refraction of Heat by Crystals.*

46. The analogies which have hitherto guided us from the laws of light to those of heat, suggest that it is far from improbable that the influence of crystallized bodies upon polarized light, which produces the most splendid and most varied, but, at the same time, amongst the most determinate phenomena of optics, may have a counterpart in the science of heat. The simpler of these, of course, it is our object first to verify; and, to a certain extent, this is all that is necessary, in order to complete the analogy of heat and light in this particular case; for the conditions essential to their production in the case of light, are on all hands admitted to depend on the susceptibility of the principle of light to undergo certain modifications in certain circumstances, extremely limited in number, and which then produce, as necessary consequences, all the subsequent effects. If we find that heat undergoes the same changes under the same circumstances, so far as we can detect them, there is the highest probability in favour of the extended analogy; for if there be a necessary sequence in the one case, it must be inferred also in the other.

47. When polarized light is caused to pass through a crystallized body possessing the power of double refraction, it happens, in a great majority of the conditions under which the experiment may be made, that the light, on emerging from the crystal, has undergone some change. This change may, for in-

stance, render it capable of reflection at a surface inclined to the rays of light at the polarizing angle, which they were incapable of doing before the crystal was interposed, or if before capable of reflection, they may now be partially, or wholly, incapable of it. Such a mode of action may in general terms be called *depolarization*, an expressive term, though not quite correct, or as has more lately been proposed, in conformity with the more accurate views now entertained on the subject, DI-*polarization*, indicating that the action of the interposed crystal is to separate the incident polarized ray into two parts by its doubly refracting energy; which parts are polarized in rectangular planes, and by their union produce the modified effect. But whatever be the explanation which we adopt of the curious and complicated changes which doubly refracting crystals exercise in the case of light, it is clear that the establishment of a correlative fact in regard to heat unaccompanied by light, must force us to admit an identity of the laws which combine, by a singularly refined mechanism, to produce an identical result. The theory of undulations is in fact by far the simplest that we can adopt, and it requires us, if we admit depolarization, to admit the existence of double refraction and of interference. The demonstration, then, of such a property of heat, is one of such importance, as to require the fullest proof.

48. The power of mica to depolarize heat, I discovered on the 16th of December last. If in the case of polarizing light, whether by reflection or refraction, the planes of incidence relatively to the polarizing and analyzing plates be at right angles to one another, the light is wholly (or at least in great part) stopped. The plates remaining in this position, it is well known, that if a film of mica be interposed between them, so as to be perpendicular to the incident light, *that* light will no longer be stopped excepting in two positions, namely, when the Principal Section of the mica plate (or the plane containing the two axes) is parallel or perpendicular to the plane of polarization. In intermediate positions, light reaches the eye. This is true for all thicknesses of the film

of mica only where light of different degrees of refrangibility is combined: with perfectly homogeneous light, at certain thicknesses, no light would in any position reach the eye, that is, it would not be depolarized.

49. The analogous fact, in heat, would of course be indicated by interposing a film of mica between a polarizing and analyzing plate, having their planes of incidence inclined at right angles to one another, and observing whether any difference of heating effect appeared when the Principal Section of the plate was parallel to the plane of Primitive Polarization, or inclined 45° to it.

50. The very first experiments which I tried, seemed decisive on this point. I employed the piles of mica for polarizing by transmission, and interposed successively two plates of mica so arranged that the Principal Section was in the one parallel (or perpendicular), and in the other inclined 45° to the plane of Primitive Polarization. These were cut from the same piece, and precisely of the same thickness; but I afterwards employed one and the same plate, inclined alternately in two positions. By the first experiments with *dark heat* (temperature about 700°) the polarizing mica plates (E and F) being crossed, the ratios of heat transmitted, when the principal section *coincided* with the plane of polarization (when the depolarizing effect was nothing), and when it was *inclined* 45° (when the depolarizing effect ought to be a maximum), were the following:

100:120 100:110 100:122 100:125

With different polarizing and analyzing plates, viz. C and D, the following ratios were obtained also for *dark heat:*

100:118 100:120 100:120 100:113

51. We have seen that the heat of *Incandescent Platinum* is highly polarizable; it is also powerfully depolarized, as the following proportions obtained with polarizing mica plates, and the same interposed films as before, indicate, as the principal section was inclined 0° or 45:

100:126 100:138 100:138

52. There were two distinct interposed plates employed for these experiments; their thickness was such as to transmit the red of the second order of the Newtonian Scale, when viewed by *polarized light,* analyzed at right angles to the plane of polarization. To shew that no appreciable difference existed in their power of stopping *common* or unpolarized heat, and to point out the accuracy of such determinations, I may quote the following experiment on the transmission of unpolarized non-luminous heat through the two plates.

Plate with sides inclined 0° and 90° to Principal Section.	Mean	Plate with sides inclined 45° to Principal Section.
18°¼		
	18°.25	18°¼
18¼		
	17.9	17¾
17½		
	18.0	18
18½		
	18.25	18+
18		

The reduction is performed as in art. 20. These quantities were observed with the naked eye, and may therefore be considered as coinciding in the two columns.

53. In repeating these experiments with a *single* film of mica, which was alternately placed with its axis parallel or inclined 45° to the plane of primitive polarization, similar results were obtained. With incandescent platinum, the result is of the most striking character; under favourable circumstances, the needle moves through from 2° to 3° degrees, (a quantity, it will be recollected, of which a twentieth or a thirtieth part is capable of measurement by the improved method of observation), or even more, commencing the moment that the change in the position of the mica film is effected (which I generally perform with long forceps, so as to avoid the near approach of the hand to the pile). A few of the first experiments gave for the ratio of the effect on the pile in the two positions, with a *single* plate,

	138:100	118:100	116:100
Another series,	130:100	125:100	123:100
A third,	120:100	120:100	
A fourth,*	128:100	123:100	122:100

54. The depolarizing effect of this mica plate (which also gives by polarized light the red of Newton's second order) upon non-luminous heat, was also exceedingly well marked, as I shall presently shew, and amounted generally to between 0°.5 and 1°, as the *statical* effect; but as the source of heat requires to be closer to the mica plates, more is transmitted by conduction, which constantly tends to *diminish* the ratio of the true difference of effect, as observed in (23).

55. It occurred to me, that since thin plates of mica present comparatively little resistance to the passage of heat, that a very thin plate might perhaps depolarize more heat than it stopped, and thus we should have the paradoxical effect of an interposed obstacle increasing the effect, a mode of action which I thought I perceived in a thicker plate. I was at first surprised to find the reverse the case.

56. A film of mica which transmitted a slightly blue white of the first order (by polarized light), and which was capable of polarizing light circularly (nearly), was employed for this experiment. But not only was I unable to detect any increase of effect when it was placed between the polarizing and analyzing plates (E and F) *crossed* so as to give a minimum of transmitted heat, but there was an evident interception when it was interposed. In other words, it stopped more heat than it depolarized. This was true both with non-luminous heat and with that from incandescent platinum. When I proceeded to estimate its depolarizing power by the usual method of placing the Principal Section at 0° or at 45°, I totally failed in obtaining a sensible effect with

* Observed by Dr Traill.

non-luminous heat, and with incandescent platinum it was extremely faint. My subsequent experiments gave for the proportion of the depolarizing effect to the whole heat which reached the pile when the plates E and F were crossed,

Non-luminous Heat.	Incandescent Platinum.	Argand.
.00	.016	.03

But upon performing this experiment with a thicker plate, namely that before alluded to in (53) and (54), I found that where it was interposed between the *crossed* polarizing and analyzing plates, the quantity of heat which reached the pile *was increased by that interposition* by about 0°.5. Hence we have the singular spectacle of the transmission of heat being *greater* when a thick obstacle is interposed, whilst the direct effect is actually *diminished* by the interposition of a thin one. This effect was of the most marked character with heat from incandescent platinum ; with dark heat the result was quite analogous, but within narrower limits. With *unpolarized* dark heat, I found that the thin plate stopped 30 out of 100 rays, whilst the thick one stopped 65, or more than twice as much.

57. The depolarizing effect of mica was tried under every variety of circumstance, and with the most conspicuous and coincident results. The quantity of light accompanying the heat, appeared by no means to regulate the quantity of heat depolarized. The heat emitted from platinum, of a full red, (and therefore not vividly incandescent), was one of the most favourable. Heat from an Argand lamp, with glass chimney, was also employed, and absolutely non-luminous heat from brass about 700°. I also employed mercury in an iron vessel, at about 500°, and found the results admirably marked. Pursuing the experiment as the temperature of the mercury descended, I found the effect still very sensible at 220°, and then thought of trying hot water, which I had not done since I devised the telescopic method of observing the galvanometer, (6). The result was,

that, by most decisive experiments, I found that *heat under* 200° *Fahrenheit, is capable of being depolarized by mica.* Even where I did not measure the amount, the instantaneous motion of the needle in the proper direction, when the Principal Section of the mica plate was parallel, or inclined 45° to the plane of primitive polarization, gave as strong evidence to this fact as to any other I have recorded.

58. It would be quite impracticable to give any detailed account of my experiments on depolarization within moderate compass. It may be satisfactory, however, to mention, that, upon an examination of all the experiments I have recorded, I find that (excluding those on the *thin* plate of mica mentioned in (56),) amongst 157 numerical comparisons, for the purpose of obtaining the depolarizing effect, *only one* gives a negative, and *one* a neutral result; and these exceptions occur in observations made upon heat of the lowest temperatures, namely, from mercury under 500°, and water under 200°. These experiments were made with heat from the various sources mentioned above (57), and with three different mica plates. The comparisons were always made from alternate observations, as in (20) and (52). Of these 157 comparisons, no less than 92 were made with heat wholly unaccompanied by visible light.

59. These conclusions, derived entirely by the use of mica as the depolarizing crystal, I endeavoured to confirm in the case of some others. Selenite, from the thin laminæ into which it may be split, naturally suggested itself, but I found that its interceptive power for heat is so much greater than that of mica, as to render these experiments nearly abortive. With heat from incandescent platinum, however, I got tolerably marked indications of its action.

60. With tourmaline I was more successful. Not only was I able to obtain decisive depolarization when slightly luminous heat was employed, such as that from incandescent platinum, and the

principal section of the tourmaline was alternately parallel, and inclined 45° to the plane of primitive polarization, but also when dark heated brass was used (at 700°). The tourmaline was one of those marked C and D (21), not mounted on glass, and of a pale amber colour.

61. From these experiments, the depolarization, or *Di*-polarization of heat seems unquestionably established, whence admitting that it depends on the same mode of action as the corresponding facts in the case of light, which seems certain, we are bound to admit that heat (even that from warm water), is susceptible of *double refraction*, that the two pencils are *polarized in opposite planes*, and that they become capable of *interfering* by the action of the analyzing plate.*

62. These results we hold to be direct conclusions from the establishment of the existence of a mode of action, of a very complicated character, which nothing but an acquaintance with the corresponding facts with regard to light could have taught us *how* to look for, and which, by coinciding with these, indicate a common mechanism. Hence, too, were our senses or our instruments capable of perceiving them, we should necessarily discover, by the passage of heat along the axes of doubly refracting crystals, all the elegant forms of rings and brushes, defined by heating, instead of luminous rays.

63. But this analogy may be carried still farther. So definite are the experimental results in depolarization, that I thought of comparing the intensities of the effects with those produced in light; and for this purpose, our methods of estimating heat is far more satisfactory than those for estimating the intensity of illumination. The fundamental law, which I felt most anxious to

* I made one attempt to obtain polarizing effects by means of Mr Nicol's very elegant single-image calc-spar prisms, but without success, as I had anticipated, from the great proportion which the thickness of the spar necessarily bears to its aperture.

verify, was the complementary nature of the transmitted heat, when the *plane of analyzation* is parallel, and when it is perpendicular, to the *plane of polarization.*

64. It is well known in the case of light, that when no crystal is interposed between the Polarizing and Analyzing Plates, or when the crystal has its Principal Section parallel or perpendicular to the plane of primitive polarization, the whole of the light is stopped * when the plates are perpendicular or *crossed;* the whole is transmitted when they are *parallel.* If the Principal Section of the crystal be now inclined 45° to the plane of polarization, the depolarizing effect is a maximum, a portion of light now being transmitted to the eye, the plates remaining *crossed,* which was not transmitted before, and, in like manner, a portion of the light which was formerly transmitted when the plates were *parallel* being now stopped. Now these two quantities are equal to one another, and therefore the sum of the intensities of illumination in the two cases (*plates parallel* and *plates crossed*) is a constant quantity. Now these two pencils correspond to the ordinary and extraordinary image in an analyzing prism of calcareous spar. Let us call these intensities O^2 and E^2. Let the whole quantity of polarized light, or the value of O^2, when the principal plane of the crystal coincides with that of polarization, be F^2, and, under the same circumstances, $E^2 = \text{zero}$. Then since the two effects are complementary, whatever be the position of the principal plane, $O^2 + E^2 = \text{const.} = F^2$,

and $$E^2 = F^2 - O^2;$$

or the whole intensity *gained* by the extraordinary pencil (which at first was zero), by the depolarizing influence of the crystal, is equal to that *lost* by the ordinary pencil.

65. That the same law holds in the case of heat, the expe-

* That is, not reflected when the light is analyzed by reflection, or not transmitted when it is analyzed by refraction. In these experiments the latter method was always used.

riments, of which the following is a brief summary, seem to indicate. The coincidence has generally been more perfect, as the steadiness of the source of heat admitted of more accurate comparison. The indications in the same line are alone intended to be compared, as they are expressed in degrees of the multiplier, the absolute amount of which would vary in different experiments. The interposed film of mica No. 1., is that mentioned in (54), as giving a red of the second order when placed between the polarizing and analyzing plates crossed; the film No. 2. gave a plum-red of the first order under the same circumstances.

Source of Heat.	Mica Plate.	Increase of Intensity of Extraordinary Pencil, by the Depolarizing Action of the Interposed Crystal. E^2		Decrease of Intensity of Ordinary Pencil, by the Depolarizing Action of the Interposed Crystal. $F^2—O^2$	
		Number of Comparisons.	Degrees of Multiplier.	Number of Comparisons.	Degrees of Multiplier.
Mercury below 500°,	No. 2	5	0°.23	6	0°.26
Brass about 700°,	No. 1	4	0.46	4	0.32
	No. 1	4	0.35		0.55
	No. 1	4	0.51	4	0.52
	No. 1	4	0.59	5	0.78
	No. 2	4	0.44	5	0.40
	No. 2	7	0.75	7	0.70
	Mean	27 comp.	0.517	29 comp.	0.545
Incandescent Platinum,	No. 1	3	2.12	3	2.14
	No. 1	4	2.22	4	2.52
	No. 1	4	2.01	5	2.13
	No. 2	6	2.38	6	2.50
	Mean	17 comp.	2.18	18 comp.	2.32
Argand Lamp (with chimney.)	No. 1	4	0.97	4	1.00
	No. 2	4	1.90	4	1.74
	Mean	8 comp.	1.43	8 comp.	1.37

66. The table generally points to a coincidence, and that as close as by the nature of the experiments we should perhaps be warranted in expecting. If there be any excess in the second column of results (which the observations with incandescent platinum might lead us to suspect), it is more than probable that it arises from some imperfection in the apparatus employed, such as the incomplete parallelism or perpendicularity of the mica plates employed to polarize, a circumstance which was not minutely attended to.

67. The result, however, is highly satisfactory, as indicating the almost exactly complementary nature of the ordinary and extraordinary pencils, as in light.

68. The somewhat complicated conditions of the variable intensities of the ordinary and extraordinary images (which it is to be recollected correspond to the Parallel and Perpendicular positions of the analyzing plate) in the case of light, are easiest kept in mind by FRESNEL'S formulæ.

$$O^2 = F^2 \left\{ 1 - \sin^2 2\,i \sin^2 \pi \left(\frac{o-e}{\lambda}\right) \right\}^*$$

$$E^2 = F^2 \left\{ \sin^2 2\,i \sin^2 \pi \left(\frac{o-e}{\lambda}\right) \right\}$$

where O^2, E^2, and F^2, have the same signification as in (64), and i represents the angle between the plane of polarization and the principal plane of the crystal: $o-e$ is the difference of the retardations of the ordinary and extraordinary rays within the crystal, and λ the length of an undulation. The sum of the two is always $= F^2$.

69. Now the quantity $o-e$ may always be known by referring to the retardation, which produces the corresponding tint

* This corresponds to the formula $\frac{a^2}{2} \sin^2 2\,\varphi \left\{1 - \cos \frac{2\,\pi\,I}{\lambda}\right\}$ of AIRY'S *Tract on the Undulatory Theory*, Art. 172. Both are only restricted expressions of more general theorems.

in NEWTON's rings, and which is equal to twice the distance between the plates in that experiment. For example, with the thin mica film mentioned in (56), which polarized light circularly, the tint produced (between *crossed* polarizing and analyzing plates) corresponded (by NEWTON's table) to an interval of about five-millionths of an inch between the surfaces of glass, or to a *retardation*, $(o - e)$, of .00001 inch. The film, marked No. 2, which gave plum-red of the first order (65), gives a retardation of .00002. The film No. 1 (65), gives .00004 inch. From these data, then, having the value of E^2 (68), it is clear that we may calculate the value of λ, or the length of an undulation of heat.*

70. In our present case we have always made $i = 45°$; whence $E^2 = F^2 \sin^2 \pi \left(\frac{o - e}{\lambda}\right)$; and of course $O^2 = F^2 - E^2$. But in an experiment we must not use the direct indication of the multiplier, when the polarizing and analyzing planes are parallel, for the total quantity or F^2; for a large proportion of the heat is not completely polarized, and in order to compare the values of E^2 and F^2, we must determine the value of each directly, that is, not only how much is depolarized, but how much is polarized by the mica plates. This I did by ascertaining alternately with the quantities of depolarization, the total intensity of the polarized part of the heat, which reached the pile. This was effected by rendering the polarizing and analyzing plates parallel and perpendicular to one another; whilst the principal section of the interposed mica remained parallel to one or other, so as to exercise no depolarizing influence.

71. To illustrate this mode of investigation, I shall give as an example the very last series of experiments made on this subject,

* Of course this is only true on the supposition that rays of heat and light are equally retarded. This is not demonstrated, but it is probable that they are nearly so, since that part of the heat which accompanies the spectrum is so, and the dispersion in the case of double refraction is inconsiderable.

which, whilst it points out the mode of operating, will exhibit the constancy and considerable magnitude of these effects, amidst the complicated changes of condition to which the heat is subjected. The columns marked "corrected," have a small correction applied for the gradual alteration in the quantity of heat reaching the pile, which corrections are interpolated from the successive observations marked (1), (2), (3), &c. which are made under similar circumstances.

1835. Jan. 16.—*Source of Heat. Incandescent Platinum. Polarizing Mica Plates E and F. Film of Mica interposed, No. I.*

Position of Mica plates.	Principal Section of interposed Mica, at	Multiplier.	Total Polarization.	Depolarization.	Total Polarization Corrected.	Depolarization Corrected.	Ratio.
[E at 0°]	45° (1)	14°.8	....	+ 2°.8			
F at 90°	0	12 .0	6°.0		6°.0	2°.75	100 : 46
F at 0°	0	18 .0					
	45	15 .3	....	− 2 .7			
F at 90°	45 (2)	14 .8	....	+ 2 .2			
	0	12 .6	4 .0		4 .2	2 .1	100 : 50
F at 0°	0	16 .6					
	45	14 .2	....	− 2 .4			
F at 90°	45 (3)	14 .1	....	+ 2 .5			
	0	11 .6	4 .8		4 .9	2 .65	100 : 54
F at 0°	0	16 .4					
	45	13 .4	....	− 3 .0			
F at 90°	45 (4)	13 .8	....	+ 2 .0			
	0	11 .8	4 .5		4 .5	2 .2	100 : 49
F at 0°	0	16 .3					
	45	13 .8	....	− 2 .5			
F at 90°	45 (5)	13 .6	....	+ 2 .4			
	0	11 .2	4 .9		5 .0	2 .2	100 : 44
F at 0°	0	16 .1					
	45	13 .8	....	− 2 .3			
F at 90°	45 (6)	13 .0	....	+ 2 .4			
	0	10 .6	5 .0		5 .1	2 .2	100 : 43
F at 0°	0	15 .6					
	45	13 .5	....	− 2 .1			
						Mean,	100 : 48

When the analyzing plate F is said to be at 0°, it is parallel to the plate E. When the principal section of the interposed film is at 0°, it is parallel to the plane of incidence at the plate

E; at 45° it is inclined 45° to that plane. The signs + and — in the column of "depolarization," indicate whether the effect of the interposed film was to increase or diminish the heat transmitted.

72. The physical meaning of the expression for the intensity of the depolarized light, $E^2 = F^2 \sin^2 \pi \left(\frac{o - e}{\lambda}\right)$ will be found to be this. When the thickness of the interposed film is such as to give a retardation of 0, λ, or any whole multiple of λ, E^2 is equal to nothing, or no light is depolarized, and between those values the amount of E^2, or the intensity of the depolarized light, will gradually increase from the values 0, λ, 2λ, &c. to the values $\frac{\lambda}{2}$, $\frac{3\lambda}{2}$, $\frac{5\lambda}{2}$, &c. and again diminish in the same manner to the next limit. When the retardation is $\frac{\lambda}{4}$, $\frac{3\lambda}{4}$, $\frac{5\lambda}{4}$, &c. half the light exactly is depolarized; it is then circularly polarized; in other cases, it is plane or elliptically polarized.

73. Similar effects might be expected to occur in the case of heat. But we must recollect that it is even more difficult to obtain *homogeneous heat*, than *homogeneous light*, and that we shall have portions of heat differently depolarized by the same plate, (in consequence of the different character of refrangibility, indicating a different length of undulation), exactly as when we operate upon white light. We know that heat of various degrees of refrangibility constitutes the solar heat, and probably all other kinds. Hence, no one plate can completely depolarize all these varieties. As far as my experiments go, made similarly to that of (71), heat unaccompanied by light is generally *less* depolarized by a plate of given thickness than heat vividly luminous. In the case of contrasting heat from an Argand lamp with that from incandescent platinum, and heat quite dark, this is strikingly marked, though not so decisively in comparing the two last kinds. If the inaccuracy be not in the experiments, it may very probably arise from the want of homogeneity in the heat just alluded to. The want of any apparent depolarizing

power for dark heat in the thin mica film mentioned in (56) is now easily explained. Its thickness was such as to polarize (nearly) circularly, the mean luminous rays. Its retardation, or $o - e$ was then $= \frac{\lambda}{4}$ for these rays. But we know from MELLONI's experiments, that the heating rays are *less* refrangible than the luminous rays (I mean in heat from terrestrial sources, as well as that of the solar rays), and that generally in proportion to this obscurity. Therefore, on the undulatory hypothesis, their waves are longer. Hence a retardation of $\frac{\lambda}{4}$ for light, would be a retardation of less than $\frac{\lambda}{4}$, if λ be the length of a wave of heat from an Argand lamp; it would be still less for heat from incandescent platinum, and least of all for dark heat; hence, as the retardation is a smaller fraction of λ or approaches zero, the depolarization or the value of E^2 approaches zero. This perfectly coincides with the experiment of (56).

74. Without attaching much weight to the *numerical* accuracy of the following results, it is worth quoting them as confirming the general fact, that obscure heat has longer undulations than luminous heat. The numbers derived from Plate No. 2, (see 65), are most to be depended upon, and the agreement of the different series made with dark heat is highly satisfactory. The numbers correspond to those of the last column in the example of (71).

MICA PLATE, No. 1. *Retardation for Light,* or $o - e = .00004$ *inch.*	Number of Comparisons.	Ratio of Total Polarization and Depolarization, or $F^2 : E^2$.
Argand Lamp,	4	100 : 80
Incandescent Platinum,	4	100 : 78
Brass about 700°,	4	100 : 69
MICA PLATE, No. 2. *Retardation for Light,* or $o - e = .00002$ *inch.*		
Argand Lamp,	3	100 : 66
Incandescent Platinum,	6	100 : 47
Brass about 700°,	7	100 : 52
Ditto,	4	100 : 51
Mercury about 500°,	5	100 : 52

In discussing these observations, it would be necessary to attend to the remark of (73), respecting the want of homogeneity in the heat.

75. From the last series it appears that a plate of mica which transmits by polarized light (when the polarizing plates are crossed) red of the first order, almost exactly circularly polarizes obscure heat, for it depolarizes half the heat. The characteristic property of circularly polarized light was observed, viz. that little or no difference of result was obtained whilst the mica film was interposed (its principal section being inclined 45° to the plane of polarization), whether the analyzing plate was at 0° or 90°. With incandescent platinum the effect is exceedingly striking; for, if the mica film be at 0°, the polarizing effect on crossing the plates is about 40 per cent. of the whole.

76. It is almost unnecessary to add, that what we have now said, inferring the undulatory theory of light to be true, might be translated into the language of the Newtonian theory of emission.

77. In conclusion, I would recapitulate the chief results at which I have arrived.*

1. Heat, whether luminous or obscure, is capable of polarization by tourmaline.
2. It may be polarized by refraction.
3. It may be polarized by reflection.
4. It may be depolarized by doubly refracting crystals. Hence,
5. It is capable of double refraction, and the two rays are

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* These conclusions were stated nearly in their present form (excepting the 6th), to the Royal Society at their meeting of the 5th January. The whole of the experiments detailed in this paper (excepting only the repetition of M. Melloni's experiment on the refraction of heat (16)), were made between the 22d November and the 16th January, but all the general consequences had been clearly made out before the close of 1834.

polarized. When suitably modified, these rays are capable of interfering like those of light.

6. The characteristic law of depolarization in the case of light holds in that of heat, viz. that the intensities in rectangular positions of the analyzing plate, are complementary to one another.

7. As a necessary consequence of the above, confirmed by experiment, heat is susceptible of circular and elliptic polarization.

8. The undulations of obscure heat are probably longer than those of light. A method is pointed out for deducing their length numerically.

78. Of the evidence for these conclusions I have enabled the reader to judge, by specifying numerical results. But I must farther add, that all the principal conclusions were arrived at by the indications of the galvanometer, observed by the naked eye, including the chief phenomena of depolarization. Since I thought of the method of magnifying the divisions (described in (5),) I had little else to perform than the agreeable task of verifying and defining my first conclusions.

EDINBURGH,
19*th January* 1835.

On the Fresh-water Limestone of Burdiehouse in the neighbourhood of Edinburgh, belonging to the Carboniferous Group of Rocks. With Supplementary Notes on other Fresh-water Limestones. By Samuel Hibbert, M. D., F. R. S. Ed., &c. &c.

(*Read December* 2. 1833, *February* 17, *April* 21, *and December* 1, 1834.)

INTRODUCTION.

I propose in this memoir to methodically connect several desultory notices, which I had occasion to read during the last session of the Royal Society's meetings, relative to the Limestones of fresh-water origin in the neighbourhood of Edinburgh, belonging to the carboniferous group of rocks, and to the organic remains which they contain.

Hitherto, the limestones belonging to this older class of deposits have been considered as exclusively of marine origin. I had long since, however, been prepared to expect that a limestone of a fluviatile or fresh-water origin would, some time or other, be proved to exist. For, in judging from analogy, it would be unreasonable to presume, that, when fresh-water limestones appear in the rocks of every later epoch, they should meet with an exclusion in the carboniferous group. In entertaining, therefore, less confined views, I was not at all surprised to find them confirmed in a limestone near Edinburgh, which lately came under my examination,—I allude to that of Burdiehouse. It enclosed none of the marine shells, corallines, or encrinites to be found in the other limestones of the vicinity, but contained in the place of them, and in the greatest possible abundance, the various plants observable in our coal-fields. I also procured from it specimens of fish allied to such as are obtained from beds associated with coal. These facts indicated to me that I had at length found a fresh-water limestone belonging to the carboniferous group of rocks.

This fresh-water formation, which has formed the object of my research, is referable to the lower series of beds belonging to the carboniferous system. In describing it, the FIRST PART of the present memoir will be confined to a geological description of the limestone of Burdiehouse, in reference solely to the vegetable and animal remains which it encloses;—a SECOND PART will point out the relations of this limestone to both older and newer rocks;—while a SUPPLEMENT will include notices of certain other limestones of a similar description, occurring in the vicinity of Edinburgh.

PART I.

THE FRESH-WATER LIMESTONE OF BURDIEHOUSE CONSIDERED IN REFERENCE TO THE VEGETABLE ANDANIMAL REMAINS WHICH IT ENCLOSES.

No geological description whatever having been hitherto given of this limestone, which, until it had been examined by myself, and its high interest explained to the Royal Society of Edinburgh, had been almost unknown, even by name, I must, upon this account, consider myself as having entered upon a ground perfectly unbeaten, which I tread with diffidence.

This bed of limestone is quarried, with much profit, for the purpose of burning. It is to be seen about four miles to the south of Edinburgh, on the Peebles road, close to the village known by the name of Burdiehouse, which is a corruption of Bourdeaux House.

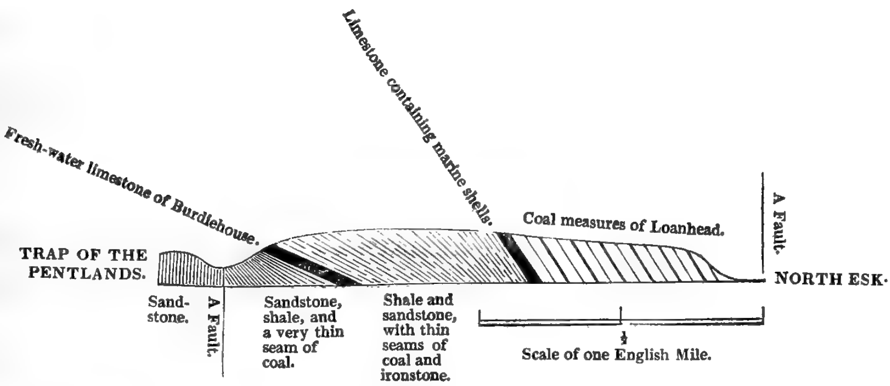
NOTES.

In the Transactions of the Society of Antiquaries of Scotland, vol. i. p. 311, some little account is given of this village by the Rev. Thomas White, in his description of the Parish of Libberton. He states that " north from Straiton is the village of Bordeaux, so called, perhaps, by some of the French who attended Queen Mary in her return home to Scotland in 1561, and who happened to take up their residence here."

It is probable that the name of Bourdeaux *House* was derived from an ancient mansion said to have once stood in the immediate neighbourhood of the village of Bourdeaux, which was built about the time of the unfortunate Mary, and was inhabited by the French who came in her train.

SECTION I.—THE GEOLOGICAL PLACE ASSIGNABLE TO THE LIMESTONE OF BURDIEHOUSE.

The limestone of Burdiehouse may be referred to the lower beds of the carboniferous system, as is evident from the following section:



In judging from the foregoing section, the place which the fresh-water limestone holds in the carboniferous system is sufficiently evident. It maintains a position far beneath a limestone containing marine shells, which is continuous with the well known Gilmerton bed, and still farther beneath the rich coal seams of Loanhead. But I suspend any farther particulars relative to the strata either below or above this bed of limestone, which will be better appreciated after it has been itself described in detail. I may merely remark, at present, that it is a bed in which some of the earliest vegetable and animal tribes incidental to the carboniferous epoch are entombed.

SECTION II.—MINERALOGICAL CHARACTER OF THE LIMESTONE.

The limestone of Burdiehouse admits of various tints. It often acquires its bluish-grey or blackish-grey colour from the bituminous or vegetable matter which is so abundantly diffused through it. Other tints approach to those of a clove-brown, or even of a lavender-purple. In its composition it shews very rarely any crystalline texture, such as is observable in the mountain limestone of neighbouring quarries ; but, on the contrary, while it is of a dull earthy aspect, its consistence is very compact. The hardness of this limestone is considerable. In its fracture it sometimes breaks into a slaty form, particularly when it is alternated with thin striæ of vegetable or bituminous matter, but, when these are absent, it shivers into irregular fragments which have a conchoidal surface. In the quarry the limestone appears in the form of regular inclined strata, severally about four and a half feet in thickness, dipping towards the south-east at angles of 23° to 25°, while its seams of stratification are so regular as to afford, during the process of quarrying, a continuous surface of almost unlimited extent. The joint thickness of the mass, which is intersected by vertical seams, amounts to twenty-seven feet.

The Burdiehouse limestone seems to be tolerably pure, admitting in its composition few extraneous substances except those which have a vegetable or animal origin ; for which reason, it is wrought for the kiln with much advantage. It is traversed in various places by small veins of calcareous spar. It contains some little siliceous matter, and the sulphuret of iron may be occasionally found interposed between its layers.

But this rock is the most remarkable for the vegetable and animal remains contained by it, which concur in awarding to it, not a marine but a fresh-water origin ; and hence, the great difference of character which it will be found to exhibit when compared with the common mountain limestone of geologists.

SECTION III.—THE FOSSIL FLORA OF THE LIMESTONE.

The vegetable matter which is diffused through the limestone of Burdiehouse forms a peculiar feature of its character. In some parts of the bed, more particularly in its higher seams, carbonized matter appears in such an extraordinary quantity, as to impart to the limestone a dark bituminous appearance. In other sites, however, where such matter is less abundant, the rock retains its prevailing bluish-grey or brown colour.

Amidst this carbonaceous diffusion, which must have resulted from vegetable matter in a decayed and pulverulent form, a profusion of fossil plants, analogous to those of tropical climates, appears encased in the rock in the most perfect state of preservation imaginable. It is, however, much to be doubted if this vast assemblage admit of much variety among them, at least in proportion to the immense mass accumulated.

The plant which occurs in the greatest abundance is the fern to which the name of Sphenopteris affinis has been recently given. It is described by Messrs LINDLEY and HUTTON, as having a leaf which is bipinnate, the leaflets being deeply pinnatifid into five segments, each of which is divided into from three to five linear obtuse segments, which are broadest at the upper end, and which are marked with from one to three parallel veins. (*Fossil Flora of Great Britain, plate* 45.)

This beautiful and delicate fern derives its chief interest from prevailing in the deepest seated beds belonging to the carboniferous group of the south of Scotland. In tracing the succession of strata superimposed upon the transition schist of Cove in Berwickshire, the first plant, which, in developing itself, breaks the continuity of a long suite of previously formed sandstones, little varying, and unfossiliferous in their geological character, is the Sphenopteris affinis. I found it here enshrined in a bed of bituminous shale, where it appeared as a sort of harbinger to the rich primeval flora, which was doomed to follow. And in other sites, namely in Linlithgowshire and Ayrshire, the Sphenopteris affinis equally shewed itself as one of the first created plants of the carboniferous epoch.

Much rarer ferns, which in the limestone of Burdiehouse appear associated with this very ancient plant of the coal-fields of Scotland, are the Sphenopteris bifida, the Sphenopteris linearis, and some few others.

The Sphenopteris bifida exhibits a greater delicacy in the divisions of its pinnulæ than a fern which it greatly resembles from a different locality, the Sphenopteris dissecta of M. ADOLPHE BRONGNIART, to which the French botanist has assigned the highest antiquity among the carboniferous group of rocks. Yet there is still so much affinity between the two, that they might be readily confounded one with another. The fern of Burdiehouse, however, may be referred to a bipinnated plant which Messrs LINDLEY and HUTTON have distinguished by the name of Sphenopteris bifida. (See Fossil Flora, plate 53.) It is placed by these authors in the vicinity of Sphenopteris myriophylla, from which it is known by its leaves not having more than three or four primary divisions, and these not radiating from a common centre, and repeatedly dichotomous, but arising from a flexuose axis, and simply bifid. The difference between the Sphenopteris bifida and that of dissecta, as Professor LINDLEY has obligingly remarked to me, is, that the Sphenopteris bifida is a smaller plant with much more slender divisions, and with the principal pinnæ longer and more repeatedly divided.

The third Sphenopteris resembles one which has been hitherto only found at Oldham in Lancashire, having been procured by myself from a deep-seated bed belonging to the great coal-field of Lancashire. A specimen was many years ago placed in the hands of M. ADOLPHE BRONGNIART, who, in describing it under the name of the Sphenopteris linearis, has thus defined it: "S. foliis bipinnatis, pinnis ascendentibus, rapidè decrescentibus, pinnulis obliquis, tri-quadrilobis, lobis bi-trifidis, laciniis truncatis approximatis brevibus multinerviis, nervulis dichotomis."—*Histoire des Vegetaux Fossiles*, tom. i. p. 175.

But this list of ferns might be much increased.

While by the luxuriance of their growth and their consequent numbers, these small ferns appear to have constituted one of the great divisions of the fossil flora of Burdiehouse, another set of plants not less numerous, comprise the doubtful tribe named Lycopodiaceæ, which, by their vegetation and mode of increase, approach in character to the comparatively minute vegetables of recent growth, the Lycopodia, or even to the order of Cycadeæ of far larger dimensions, while by their reproductive organs they have some little affinity to the Coniferæ. In the present instance, those peculiar structures of vegetable organization, which, separately considered, would seem of different character, but which have been conjointly regarded as the dissevered organs of the anomalous Lycopodiaceæ, appear in this limestone under circumstances of as great mystery as in other places, except that the cone, or one of the supposed organs of fructification, is too frequently in approximation, if not in absolute contact with a dichotomous stem, to sanction any other inference than that each is attributable to one and the same plant. Specimens of the Cardiocarpon, or of that species of lenticular and cordiform, or reniform, fruit, equally assigned by M. Adolphe Brongniart to Lycopodiaceæ, have not to my knowledge been hitherto found in the limestone.

With respect to such of the stems of Lycopodiaceæ as have been discovered, some of them may be referred to Lepidodendron selaginoides, (*Plates* 12 *and* 113 *of* Lindley *and* Hutton's *British Flora)*, to L. obovatum, *(plate* 19 *bis of the same work)*, and to L. Steinbergii *(plate* 112). Among the leaves which have been attributed, though doubted by others, to Lycopodiaceæ, are those of Lepidophyllum intermedium, (see *Plate* 43 *of* Lindley *and*

Hutton). The circumstances under which these vegetable relics are found, appear to be the same which had been previously remarked in another and very distant locality. At Burdiehouse, the Lepidophyllum intermedium is found lying along with Cyperites bicarinata. But whether this coincidence ought to stimulate to any fresh inquiry regarding the true character of each, I would not offer any opinion whatever, as it is but too possible that the association might have been purely accidental. At least, " it is curious," as Professor Lindley has remarked to me, " that these two fossils, which have no sort of relation to each other, should have occurred in association with each other, both in the Leebotwood coal-pit and at Burdiehouse." (*Compare plate* 43 *of L. & H.'s Fossil Flora with Fig.* 5 *of Plate VI, given in this memoir.*)

Regarding the cylindrical and imbricated cones supposed to be naturally connected with the Lepidodendron, (a notion to which much countenance is given by the character of the remains discovered at Burdiehouse), two kinds may be identified, viz. the Lepidostrobus variabilis, (*Lindley and Hutton*), *Pl.* 10 and 11), and the L. ornatus (*ibid. Pl.* 26).

No cardiocarpon which can be safely attributed to Lycopodiaceæ has yet, to my knowledge, turned up. I have only observed the Cardiocarpon acutum of Messrs Lindley and Hutton (See Foss. Flora, pl. 76), which is supposed by them to belong to other plants.

These very general remarks must, on the present occasion, be deemed sufficient.

It is thus shewn, that the smaller ferns, referable to Sphenopteris, together with the disjointed organs of Lycopodiaceæ, constitute the great majority of vegetable remains entombed in the limestone of Burdiehouse. Along with these are found, but comparatively in less number or quantity, relics of the Stigmaria ficoides, and of the less common species belonging to Sigillaria, Equisetum, Calamites, and Cyclopteris. But as attention is now directed to the daily disclosures of the quarry, we may expect that, in the course of time, a list of the various genera and species of plants discovered in this interesting locality will be perfected.

NOTES TO SECTION III.

Fossil Botany is a subject of inquiry so beset with difficulties, as to demand an almost exclusive attention to it by the naturalist, if he would pursue it with success. For this reason, I should be unwilling to commit myself by any attempts to treat of the Fossil Flora of Burdiehouse more in detail; nor can such attempts be reasonably expected in a memoir so limited as this must necessarily be. Specimens of the more important plants, among which are some new ones already discovered, have been transmitted, not only from the collection made by the Royal Society of Edinburgh, but likewise from my own cabinet, to Messrs Lindley and Hutton, to be described by them in their important work exclusively devoted to the Fossil Flora of Great Britain. Other specimens, though from my own private collection only, have been sent to M. Adolphe Brongniart, whose acquaintance I had many years ago the happiness of forming in Edinburgh, while he was prosecuting his important researches in Fossil Botany: and thus every opportunity has been afforded for having proper justice rendered to the plants of more peculiar interest which have already turned up; and, in reference to future disclosures from the quarry, other similar transmissions are meditated.

Hitherto, two plants only from the vegetable remains of Burdiehouse have been noticed, which were forwarded to Professor Lindley and Mr Hutton, for their Fossil Flora, by their zealous and very intelligent correspondent Mr Witham. In the botanical description given of the Sphenopteris bifida, it is incidentally stated, (see vol. i. of the work, p. 147), that "the plant was communicated by Mr Witham, from the mountain limestone of the lime-quarries of Burdiehouse, near Edinburgh;" and, upon another occasion, a similar acknowledgment is rendered.

In the absence of any geological description whatever having been hitherto published of this limestone, the very name of Burdiehouse would have been unrecorded, if it had not been for this incidental circumstance.

I am given to understand, that it is intended to devote the whole of a very early number of the British Flora to an elucidation of the plants of Burdiehouse.

In reference to this anticipation, as well as to the chief object of this memoir, which is to illustrate the fresh-water character of the limestone of Burdiehouse, one plate only has been dedicated to its Fossil Flora.

Fig. 1. of Plate VI. is the Sphenopteris bifida.

Fig. 2. is a plant, unfortunately not in the most perfect state, which I am inclined to consider as an undescribed Sphenopteris. But, as I agree with Mr Hutton of Newcastle, in the possibility that the specimen may be

ultimately referred to the Sphenopteris bifida, any farther notice of it will be suspended.

Fig. 3. is the Sphenopteris linearis.

Fig. 4. is the Sphenopteris affinis. In this sketch the Rachis is rather too slenderly drawn.

Fig. 5. is the Lepidophyllum, in connection with Cyperitis bicarinata.

Fig. 6. shews the Lepidostrobus variabilis enclosed in the same specimen with a fish of the genus of Palæoniscus, which will be described in the 7th section of the present memoir.

SECTION IV.—THE MICROSCOPIC ANIMALS CONTAINED IN THE LIMESTONE OF BURDIEHOUSE.

No conchifera, at least of any large size, have hitherto been discovered in the limestone of Burdiehouse.

From having witnessed the great abundance in which fresh water unios are contained within a thin band of limestone and shale, near Old Cumnock, in Ayrshire, and from having read in Mr Ure's History of Rutherglen, that they had been found under similar circumstances near Glasgow, I had long anticipated their discovery in the fresh-water limestone of Burdiehouse. But, if not found in the bed itself, they at least exist in a superincumbent bed of argillaceous shale which encloses similar organic remains.

The animals actually found in the limestone of Burdiehouse merit a particular regard: For there are assuredly no phenomena connected with the deposit of Burdiehouse more remarkable than the Entomostraca with which it abounds.

These minute animals are severally microscopic, ranging from $\frac{1}{12}$th to $\frac{1}{60}$th part of an inch. Various kinds are found among them, all of which, with perhaps the exception of one genus, appear to have been perfectly undescribed. Nor is it the easiest of tasks to submit them to any kind of successful investigation. We see nothing more in these crustaceous animals than their outer and shelly case, while a mineralized state has obscured their internal organization. Accordingly, if we conceive that certain minute oval bodies resemble recent specimens of the Cypris

Faba, or any other of the microscopic animals which are the tenants of stagnant waters, the identity can only be inferred from some external form, not always unambiguous, as for instance, from a bivalve shell possessed in common with conchifera.

I shall endeavour to describe some few of the infinite entomostraca which are found in the limestone of Burdiehouse, all of which are powerfully magnified. The little points inserted below each, shew the natural size of the animal.

Perhaps no minute animal found in this limestone is obtained under circumstances of greater distinctness than the Cypris.

The Cypris, as is well known, is represented as having two antennæ, straight, simple, and like a pencil of hairs at the top. It has a single eye, and it has four claws. A bivalve shell, the only part preserved in a fossil state, encloses the body. The head is concealed.

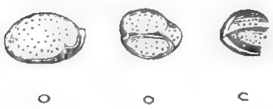
In the present instance, the Cypris of Burdiehouse, which is enclosed in a bivalve of a striking egg-shell complexion and whiteness, differs from the Cypris Faba of geologists in the straightness of its hinge, by which the form of a bean (whence the name of *Cypris Faba*), becomes lost.

In the annexed wood-cut, the greatly magnified, as well as the natural microscopic form of the Cypris, meet with a representation: *a* and *c* represent the general form of the shell, while *b* is a separate and open valve.

This interesting cypris is so characteristic of the deposit of Burdiehouse, that an appellation expressive of its locality would perhaps form its most suitable distinction. The limestone, which is its matrix, occurs close to the village of Bourdeaux, whence, by corruption, Burdie. And hence the name assigned to the locality by the French retainers of Queen Mary is most applicable in the present instance; it suggests the designation of CYPRIS SCOTO-BURDIGALENSIS.

In the next place, if it be allowable from analogy, rather than from any other circumstance, to infer that certain of the minute animals thus described are referable to the Cypris, the prosecution of this research cannot be more safely conducted than in still confining our attention to the recent family of the Cypridea, for the detection, if possible, of other forms belonging to animals presumed to be of common habits. Another form, for instance, rather approaches to that of the Daphnia than the Cypris. But if, instead of an external crust, we could arrive at the knowledge of an internal organization, it is not improbable that the entomostraca thus compared might prove to be different animals altogether.

It is for this reason that I would name the animal represented in the three different views of the annexed wood-cut, by the cautious name of Daphnoidia rather than Daphnia; indicative of an individual approaching to the recent Daphnia.



The shell of the Daphnia, like that of the Daphnoidia of Burdiehouse, is not described as exactly bivalve, but as *subunivalve*, opening longitudinally on one side only of the encased animal. This character may, I think, be plainly detected in the instance of the fossil example. I shall not venture upon assigning to it a specific name, until I am better assured with regard to its proper generic character.

Other minute animals possess shells of extreme tenuity, too many of which appear in a crushed and broken state. In this form they exhibit a sort of spiral organization by no means unlike that of the Planorbis or Spirorbis, to which they were referred by an eminent conchologist, whose opinion regarding them I consulted. But, upon a renewed examination of certain of these remains, which, owing to an infiltration of calcareous matter, have had their forms tolerably well preserved, I am now inclined to place some doubt upon the judgment which had been passed upon their character.

The external form of one animal most resembles that of the

Nautilus, but we are totally precluded from identifying it with

that class of Mollusca, as the internal constitution of its shell shews no septa whatever; but, on the contrary, approaches to that of the Planorbis. It may perhaps be considered as a new genus altogether,—referable to mollusca. The dimensions of this microscopic animal are two or three times those of the Cypris. Its natural size appears in the representation inserted below the magnified form.

The foregoing best defined forms which have come under my notice, may for the present suffice. A microcosm of entomostraca, and possibly of other classes of animals, invites the attention of the naturalist, among which many undescribed races remain to be detected.

Some of these minute beings have been thus proved to possess a structure in common with that of well known entomostraca, whose habitat is one of stagnant waters rendered turbid by decayed vegetables or roots. When these waters are dried up by the sun, they still linger within a humid basin, and remain there until rains fall anew.

This habitat agrees with every possible circumstance connected with the limestone of Burdiehouse,—with the memorial of some inland fresh-water lake or tank, within the waters of which a calcareous deposit was elaborated, or of some river flowing sluggishly through a tract overrun with Ferns, Lycopodiaceæ, and such aquatic vegetables, as must have flourished among primeval marshes.

The abundance in which these animals appear is the last extraordinary circumstance to be noticed.

Although the remains of these entomostraca occur in much greater quantity in some parts of the rock than in others, there is scarcely a detachable fragment which does not exhibit their presence; and when, during the process of quarrying, a plane of stratification belonging to any portion of the limestone bed has been allowed to remain long exposed to the air, so that some little of its surface has become removed by weathering, the crustaceous or shelly coverings of these minute beings become better defined, and their numbers are more palpably brought to light. In fact, I possess specimens which are almost rendered oolitic by the crowded state of these little animals.

These examples, while they shew to what an extent this primeval lake or river had once teemed with animal life, equally prove the quietness with which its calcareous deposit was conducted, and, consequently, the stillness and stagnation of these marshy waters, in which vegetables, together with entomostraca, have found so extraordinary a diffusion, as well as so extraordinary a conservation of their outward form and character.

NOTES TO SECTION IV.

Mr Ure, in his History of Rutherglen, who wrote forty years ago, has the merit of having first directed the attention of naturalists to the microscopic shells which he found at Lawrieston and Stuartfield; but he does not make us acquainted with the description of beds in which they occurred at these places. He adds, that he also found them in a limestone quarry, about fifteen miles west from Newcastle-upon-Tyne, near the spot where the Roman wall is intersected by Watling Street. One of the specimens, of which he gives a magnified representation, is evidently that of a Cypris. (*See* Ure's *History of Rutherglen*, p. 311.)

SECTION V.—THE DISCOVERY OF OSSEOUS REMAINS IN THE QUARRY OF BURDIEHOUSE.

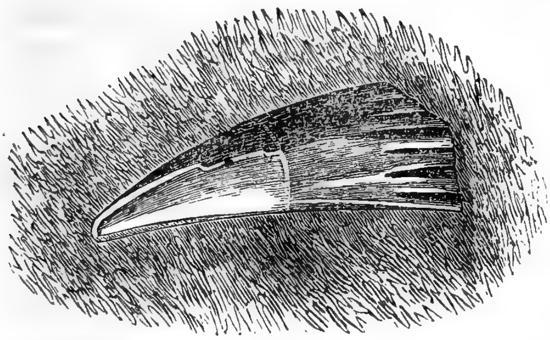
That various kinds of fish were enclosed in the limestone of Burdiehouse, was a fact of which I was apprised upon the first occasion of my visit to the quarry.

My earliest communication to the Royal Society of Edinburgh, relative to the limestone of Burdiehouse, was limited to the information, which I considered as an important one, that, in reference to its organic remains, it had no characters in common with the mountain-limestone of marine origin; but that, on the contrary, the immense abundance of plants which it contained, along with entomostraca and fish allied to such as are found in coal-measures, rather justified our considering it as a fluviatile deposit.

The view which I had thus taken of the Burdiehouse limestone, was made known to the Royal Society on the 2d of December 1833.

A single day, however, had not elapsed subsequent to this communication having been read, when another discovery, perfectly unlooked for, ensued. I had charged the workmen employed at the quarry of Burdiehouse to carefully preserve for me any animal remains which might turn up; and, upon revisiting the place, in company with Mr Witham, one of the labourers brought me a fragment of limestone, enclosing a tooth, two inches and a quarter in length, which was in the most beautiful state of preservation that can well be imagined, possessing an enamel of a nut-brown colour, which shone with all the brilliancy of perfect freshness.

This tooth, which is represented in the annexed wood-cut, I conceived, from its outward form and external structure, to belong to a large animal of the Saurian class. I also procured some other relics, apparently of the same monster; and was led to



remark, that the limestone contained coprolites, referable, from their size and from the undigested scales of small fish diffused through them, to some voracious sovereign of primeval waters.

After this discovery was made public, the workmen were encouraged to look after the relics which might turn up; and I gave them full credit for sincerity when they assured me, to a man, that, in the long course of their quarrying operations, no remains whatever, save those of plants, had ever been the object of their visual organs. But having been now convinced that bones did actually exist in the limestone, and having been excited by the promise of a reward if they should find more such relics, I was presented with an excellent physiological example of the capability which such a stimulus had of increasing, to an almost miraculous degree, the vividness of optical perceptions. The men, to use their own words, now "saw with new eyes." Large scales, large bones, along with the remains of small fish, became palpable, and, as if by the force of a magician's spell, were revealed in a profusion most admirable to behold.

When the quarry had been thus introduced to public notice, a complete dispersion of its fossil treasures seemed inevitable. With the view of preventing a catastrophe so much to be dreaded, I invited the interference of that influence which the Royal Society of Edinburgh possessed as a body. The appeal was not made in vain. To their General Secretary, Mr Robison, the acknowledgment is due of having successfully rendered his invaluable aid in the prevention of a most injurious dispersion of the osseous relics of the quarry, and in the preservation of specimens in such an assembled form, as to render them effective for the requisitions of science.

The very liberal acquiescence of Mr Torrance, the lessee of the quarry of Burdiehouse, with the views of the Royal Society of Edinburgh, merits the highest encomium.

NOTES TO SECTION V.

The following explanation regarding the quarry will not, I trust, be found irrelevant. I owe it no less in justice to certain individuals, than to the Royal Society of Edinburgh.

Very soon after my discovery, the hopes which I had entertained of the extraordinary acquisitions which geology would derive from it, were soon threatened with frustration. A sort of scramble had ensued among scientific individuals (which it must be allowed was a very natural one), to obtain for themselves valuable specimens from the quarry of Burdiehouse: and thus, the great disastrous result which fossil zoology most dreads, namely, the dispersion of its relics, and the difficulty, if not impossibility, of reuniting them, seemed inevitable.

A scramble of this nature, in reference to such a fatal result, not a little resembles the every-day occurrence of workmen who, in the course of digging, have fallen upon some ancient urn which contains the cremated ashes of our Celtic or Teutonic progenitors. The pick-axe is supposed to have been resisted by "a pot of money;"—a tremendous scramble ensues; the urn is broken into a thousand pieces, and the antiquary resigns himself to despair.

Under these circumstances, I took the first occasion in my power to obviate a catastrophe which appeared so imminent.

In addition to recommendations which I drew up for the public journals, urging the patrons of the College Museum, or the Council of our leading scientific institution of Edinburgh, to exert a fostering care over the quarry, and to prevent the dispersion of its relics, I took an early occasion, at one of the meetings of the Royal Society, to make a personal appeal on the subject.

Upon this occasion, I stated, that the great French naturalist, to whom geology has been most indebted, laboured under no difficulty so great as in his endeavours to counteract the dispersion of osseous relics of comparatively little value in a separate state, but most important to science when collected together with a view to the appreciation of the whole in their united condition. This difficulty I illustrated by a few passages from the writings of Cuvier, which shewed the almost incredible labours to which he was obliged to resort, before he could reassemble certain dispersed fragments necessary for him to come to a satisfactory conclusion respecting the fossil saurian animal of Honfleur.

"Having been apprised," he remarks, "by these two lower jaw-bones, that there might exist two species at Honfleur, my first object was to regain the skull and the upper jaw-bone. The collection which I had received from Rouen afforded me some fragments; but the original owner had entertained the unfortunate notion of getting them cut and polished; and he had even distributed a part of them among other cabinets. It is by a series of almost incredible events, that I have collected and been able to reunite six fragments, which had belonged to the same cranium, of which two had been in the possession of the Abbé Bachelet, two had found their way into M. de Drée's museum, while two others had been sent to me from Geneva by the late M. de Jurine, without his ever suspecting their importance in this particular inquiry. By means of these six pieces, I succeeded in reconstructing a considerable portion of the cranium containing the occiput, the greatest part of the upper face, and sides, as far as the snout. It is by similar accidents that I have collected three fragments, which had belonged to one and the same snout, and of which I had only given two in my first edition. These two last were in the museum of the late Abbe Besson; the third was in that of M. Faujas, to whom Besson had given it, without perceiving that, along with the two others, it only formed one entire whole."

The force of this quotation I urged, in connection with the remark, that it would be chimerical for a single individual like myself, perfectly unsupported, to suppose that he had either the means or the influence to counteract such a fatal dispersion; but that the pecuniary means, and, above all, the command of influence possessed by a scientific body like that of the Royal Society of Edinburgh, might, with the greatest prospect of success, be exerted in so important an object.

The Council of the Royal Society, with a liberality of which I cannot speak in too high terms, immediately responded to the representation, and appointed their General Secretary, Mr Robison, with funds at his disposal, to superintend the task, aided by a small Committee.

The task could not have been entrusted to more able hands. Mr Robison immediately addressed himself to Mr Torrance of Meadowbank, the lessee of the quarry, requesting his co-operation with the views of the Royal Society of Edinburgh; and, to the honour of the latter be it added, his assent was immediately obtained. While any dispersion of the relics discovered was strictly forbidden, the labourers were at the same time amply remunerated for all the specimens which they were enabled to collect.

These measures were very soon the means of obviating the dispersion of the osseous fragments of the quarry; which object was still further promoted by a few of the gentlemen who were extensive geological collectors themselves abstaining from any attempt at purchases, and thus setting a laudable example of self-denial.

During the whole of the last winter and some part of the spring, not a week was allowed to intervene without the quarry being visited, either by Mr Robison or myself, always on alternate days; and by this surveillance, aided by the injunctions of Mr Torrance, and the remonstrances of the worthy minister of the Parish, Mr Purdie, a constant check was placed upon any clandestine disposal of the bones, and, perhaps, as few breaches of confidence occurred as might be expected from an assemblage of quarrymen, many of whom were burdened with large families.

Toward the accomplishment of another object, Mr Robison's labours were unassistedly directed. The osseous fragments discovered were firmly embedded in a hard limestone rock, and nice mechanical skill was requisite to detach them from their matrix; which difficulty was again augmented by the very conchoidal fracture assumed by the limestone after a blow of the hammer, and the consequent difficulty in regulating the extent and direction of the cleavage, or of the chippings. Now, in order to overcome these obstacles, Mr Robison laboured with success, and invented a small dressing hammer, which effectually brought under control the destructive fractures into which the limestone was liable to split. This hammer will be soon considered as indispensable to the manual operations of the geologist.

The result of these exertions may be readily anticipated. The enlisting of a gentleman of such well known mechanical science as Mr Robison, in the task of detaching these osseous relics from their stubborn matrix, and of bringing them to light in a state of integrity, was a result truly favourable to the best interests of Geology. Very minute specimens, even those which embraced the almost microscopic fry of the immense questionable animals of Burdiehouse, were developed with a distinctness, which has not a little contributed to solve some important questions which the osseous relics of the quarry have suggested.

While Mr Robison was thus rendering the most valuable assistance in the common object which we had in view, this co-operation was the means of affording me the more leisure to satisfy myself regarding other questions of geological interest suggested by these researches.

SECTION VI.—THE GENERA OF FOSSIL FISH DISCOVERED AT BURDIEHOUSE.

The fish discovered at Burdiehouse will be described after the system of M. Agassiz, Professor of Natural History at Neuchâtel, whose "Recherches sur les Poissons Fossiles," is already familiar to most geologists. They will also be noticed in connection with the judgment passed upon them from personal inspection by this eminent naturalist; and, in availing myself of the kind assistance which he has thus rendered me, I would confess to an obligation for which I am truly grateful.

M. Agassiz's system, as is well known to those who may have but glanced at his work, while it carefully includes the distinctions which prevail in older recognised systems, gives a characteristic pre-eminence to that portion of the structure of the fish, which with geologists ought to claim a leading consideration. The skin of the fish connects the animal with the medium in which he lives. It is the essential character of the skin to form a sort of external skeleton, which protects its surface. Its epidermis may be considered, in the most general point of view, as a membranous layer of horny substance, which covers the whole surface of the animal, which isolates it from the external world, which shelters the more delicate part of its organization, and which, as a bad conductor of heat, preserves to it that degree of internal temperature which is essential to its vitality. While this covering is composed of a number of layers, or folia, superimposed upon and strongly adhering to each other, it is at the same time insensible, and is reproduced with ease.—*Recherches sur les Poissons Fossiles,* tom. i. p. 26, &c.

In reference to this most important portion of the structure of the animal, the great orders of M. Agassiz's system are founded.

The remains of such fish discovered at Burdiehouse as I intend to notice in the present memoir, are referable to two of these orders.

The first of these orders includes Placoidian fish. They are distinguished by the irregularity manifested in the solid parts of their integuments, which consist of materials of enamel often considerable, or sometimes reduced to little points; as, for instance, the tubercles of rays, or the different chagrins of squali, &c.

To this order M. Agassiz has referred the fish to which certain bony rays of gigantic dimensions found at Burdiehouse belong. The animal is supposed to have approached the Cestracion of modern times; which fish is the type of the family of Cestraciontes. He conceives that the remains in question may be referred to a new genus, to which he has given the name of Gyracanthus formosus.

Other relics are referred to fish of a second or Ganoidian order, distinguished by the angular, rhomboidal or polygonal scales, which they possess in common; these scales being at the same time formed of bony plates, and covered over with enamel. This order includes various families, but, in reference to the fossil remains of Burdiehouse, two only of them will have to be considered.

To the family of Lepidoids *(Agass.)*, which are characterised by teeth after the form of a brush disposed in several rows, or in one single row of small obtuse teeth, three genera discovered at Burdiehouse have been assigned, viz. the Palæoniscus, the Amblypterus, and a new genus, to which M. Agassiz has given the name of Eurynotus.

To the family of Sauroids, which are distinguished by conical and pointed teeth, alternating with small teeth *en brosse*, by flat scales, rhomboidal and parallel to the body, which is entirely covered by them, and by an osseous skeleton, M. Agassiz has attributed the remains of two fish which have been found. One of these belongs to his genus Pygopterus, and another to an undescribed and extraordinary genus, the Megalichthys.

This list could no doubt be extended. I am therefore called upon to remark, that, as the entire suite of specimens hitherto collected at Burdiehouse has been submitted to the investigation of M. Agassiz, to be illustrated by him in his "Recherches sur les Poissons Fossiles," it would only burden the volumes of the Royal Society's Transactions, to enter into a description of all the fish discovered in the quarry For this reason, I have proposed to confine myself to such individuals only, as might serve for illustrations of the general character of the limestone, or might involve geological questions of importance.

NOTES TO SECTION VI.

Soon after my discovery of the fossil treasures of the Burdiehouse limestone, I was anxious to obtain M. Agassiz's judgment upon them; and, accordingly, very early during the last spring, I wrote to him, with drawings of three of the best marked specimens which had then been found. Since that period many others have been added to the collection of the Burdiehouse specimens, and when, upon the occasion of the British Association of Science meeting at Edinburgh, I had the satisfaction of cultivating a personal acquaintance with M. Agassiz, he promised to favour me with his opinion regarding all such specimens as I might submit to his consideration.

That I should have consulted M. Agassiz in preference to any other naturalist, however eminent he might have rendered himself in other departments of zoology (and I would add that there are zoologists in Scotland whose memoirs form very valuable portions of the Royal Society's Transactions), cannot create the least degree of surprise among such as are aware of the most imperfect state of our knowledge in fossil ichthyology, and of the great light which is dawning upon this obscure branch connected with geology, by the transcendent researches which are going on under the auspices of the Swiss naturalist.

To those who may happen to be unacquainted with M. Agassiz or his writings, it is sufficient for me to explain, that he appears before us under two great recommendations.

In the first place, he has taken up the elucidation of a branch of natural history, conceded to him as untrod ground by no less an individual than Cuvier himself,—himself a giant in the field of palæontology. This fact is rendered evident by the acknowledgments which M. Agassiz pays to the French naturalist for having committed to his possession all the materials which the Baron had himself collected with a view to similar researches, for which his important occupations in another department, namely, in the extinct land, and amphibious animals of a past state of our globe, had afforded him too little leisure.

A second recommendation of M. Agassiz is, that he comes before us not only in the character of a zoologist, but as one who has studied the character of fish in reference to their geological connections; who has endeavoured to trace in this class of animals the changes of organization which have happened in correspondence with the various revolutions which the earth has undergone. "Fish," as he observes, "being more than all other animals most intimately connected with the incidents of the water, and their organization being besides very high up, they are better calculated than any other class to give us clear ideas regarding the changes which have been going on in such vast seas as have formerly covered over the earth. We shall be enabled to determine if a fish has lived in rivers, in lakes, or in ponds, in the high sea, or upon its shores;—if it was an inhabitant of the surface of the water or of its great depths;—which indications, more or less valuable, will aid us in determining corresponding circumstances with regard to the formation of rocks."

If I had space allotted me in this memoir, I could point out many instances of the tact which M. Agassiz evinced while in Edinburgh, illustrative of the importance of fossil ichthyology, in the assistance which it yields in determining the relative age of rocks, and which would show, at the same time, how inexcusable I should have been if I had not availed myself of a judgment of such value as that which he has evinced.

SECTION VII.—THE FOSSIL FISH REFERABLE TO THE LEPIDOID FAMILY.

For motives of convenience, I shall not describe the fish which I mean to notice in the exact order of classification already explained.

As I have stated, the Lepidoid family is included in the Ganoid order of fish.

To this family the genera of Palæoniscus, Eurynotus, and Amblypterus are referred, of which one species or more of each have been discovered in the quarry of Burdiehouse.

The fish which the limestone entombs in far the greater number is an individual which I had little difficulty in referring to the genus of Palæoniscus. (*The first specimens discovered of this fish were sketched in Plate VII., figs.* 1. *and* 3. *But more perfect ones having been subsequently obtained, they are introduced* *in Plate VI., Figs.* 6. *and* 7., where one of them appears in a specimen which encloses also the vegetable relic named the Lepidostrobus variabilis.)

A doubt at first arose whether the fish formed a new species, and the more particularly as it approached in character to the Palæoniscus angustus of Autun. (*See* "*Recherches sur les Poissons Fossiles,*" *tom. ii. page* 57.)

M. Agassiz has, however, referred the specimen to a perfectly undescribed species; and, in pointing out the small pectoral and the small ventral fins which it possesses, as well as the anterior edge of the dorsal fin being opposite, or nearly so, to the ventrals, he adds, "what principally characterises it, is its elongated form and the thinness of its body, by which it comes nearest to the Palæoniscus angustus of Autun; but, at the same time, it differs from all other species belonging to the genus, in the much more considerable length of the anterior rays of its dorsal and anal fins, and in the considerable size of its tail."

With the name which ought to be given to this new species, M. Agassiz has declined to interfere. He states, that in a letter which I wrote to him, directed to Neuchatel, accompanied with a drawing of the fish, I had, in this instance, successfully attributed it to the genus of Palæoniscus; on which account he would wish its specific name to rest with myself. An opportunity has thus been afforded me, of which I hasten to avail myself, of paying the public acknowledgment which I owe for the unwearied pains taken by Mr Robison, General Secretary of the Royal Society of Edinburgh, not only in preventing the dispersion of osseous relics so important to geology, but in otherwise affording me the greatest assistance in the prosecution of these researches. As a tribute, therefore, of the gratitude which I feel for such services, I would beg to name a new species of fish, which, from its great abundance, may be almost regarded as characteristic of the limestone of Burdiehouse, the Palæoniscus Robisoni.—*See Plate VI., Fig.* 7.

A second fish is considered by M. Agassiz as constituting a new genus of the same Lepidoid family, referable to his *Lepidoides hétérocerques. (See the Plates of the first volume of Recherches sur les Ossemens Fossiles.)* This genus is considered as coming near to that of the Palæoniscus and Platysomus. M. Agassiz writes to me regarding the fish as follows: "I intend to call it Eurynotus. What characterises it the best is the great size of the dorsal fin which occupies the whole back, as in the Platysomus, the anterior rays of which are very much elongated. Fig. 4. of Plate VII. does not give a complete idea of it, the anterior edge of the dorsal fin being much more elongated. M. Jameson has placed in my hands some, almost perfect, specimens of the fish, which, at first, I did not recognise, so remarkable was their aspect. The anal fin is narrow, but its rays are elongated. The general form of the body is rather that of the Amblypterus than of the Palæoniscus, but its double fins, the pectorals and the ventrals, have fewer rays, and these rays are longer. The scales (which, like other parts of the fish, are not very well preserved in the specimen of the Royal Society) are not so long as they are high, and their posterior edge is crenulated and indented; for which reason, I have called this species, hitherto the only one known of the genus, Eurynotus crenatus."

A third fish, referable to the Lepidoid family, is a species of Amblypterus *(Agassiz)*, of which I possess no drawing nor description. It will doubtless be noticed in the "Recherches sur les Poissons Fossiles."

SECTION VIII.—THE SAUROID CHARACTER OF CERTAIN OSSEOUS REMAINS ENCLOSED IN THE LIMESTONE.

A circumstance of no little interest, connected with the organic remains which had been disclosed during the progress of quarrying, was the question which they involved regarding their conceived saurian character.

In the first place, a large tooth was obtained, having no little resemblance to those of the Gavial of the Ganges. (See Plate VIII. fig. 1.) Nor did the internal structure of such teeth as were discovered forbid the supposition. This is shewn in the annexed figure, which represents the section of a tooth, which had been found broken longitudinally:—*a* represents a lower and outer portion of the tooth, while *b* is its reverse side, in which an internal cavity is observable, at present filled up with earthy substance, not unlike the cavity observable in the teeth of large reptiles, which is supplied with a replacing tooth.

a *b* *c*

The larger longitudinal fragment of the same tooth is represented by *c*, in which the counterpart of the same internal cavity is discernible.

It is true that this internal structure was not a decisive mark of the animal having been a reptile, yet, when the immense size of some of the teeth subsequently discovered was also taken into consideration (see Plate IX.), one of which was $3\frac{3}{4}$ inches in length, and when no extinct animal coeval with or earlier than the new red sandstone formation had been hitherto recorded as possessing such immense teeth, saurian reptiles alone excepted, the reference of the teeth to such animals was, at least, the most ready supposition, and justifiable.

In the second place, various scales were collected. These were of two kinds.

The first of these comprised the remarkable structures which are represented in Plate VIII. fig. 2. It is not the first time that these scales have been discovered in coal-fields. They have even been figured as fungi belonging to the vegetable world. In the present instance, it may be supposed that these substances had, in the limestone, met with a better state of preservation, as it was impossible not to be struck with their internal cancellated structure, which forbad any supposition but that they were osseous. Owing to this structure, and to their rounded form, it was conceived that they were the epiphyses of vertebræ; but other specimens having been obtained in which an external lamellar structure might be also remarked, this notion was discountenanced, and the relics in question were adjudged to be large scales. But, with regard to the animal to which these scales might be referred, no supposition was hazarded. Indeed, some little mystery regarding them may possibly subsist at the present moment. (*Other representations of these scales appear in Plate X. and Fig. 2 and 3.*)

A second description of scales possessed great thickness. They were adorned with a brilliant enamel of a nut-brown colour. They were of an angular, rhomboidal, or polygonal form, and most of them exhibited small pits or dots upon their surface, (*see Plate XI. Fig.* 4 *to* 7). Now, it is certain that any opinion which could be formed from these scales was at the best equivocal. Although I at first advocated their exclusively saurian character, yet, when I saw a large specimen of the Lepisosteus, named by M. Agassiz the *Lepidosteus*, which is preserved in the British Museum, I immediately considered the possibility that the scales might belong to some animal of the finny tribe, which suspicion was stated in the memoir that I read at a sectional meeting of the British Association. At the same time, a comparison made with various crocodiles, collected from different countries, not only shewed me scales of the self same form and thickness; but even the very pits or dots by which I at first conceived the scales of the Burdiehouse animal were to be distinguished. So far, then, the saurian evidence was scarcely decisive.

In the third place, a discovery was made of bony rays of extraordinary dimensions and beautifully configurated. (*See Plate XI. fig.* 1, *A and B.* Others also, of a different form, and of less size, were found. Regarding these relics, no question at all could arise. They evidently belonged to fish, and could not by any possibility be confounded with the remains of reptiles.

In the fourth place, very large disjointed bones, generally cranial, were disclosed. These were in such a broken and unconnected state as to throw no light whatever upon the saurian question. Some of them appeared fish-like; and, as it was certain, from the discovery of large Ichthyodorulites, that immense fish must have existed, a reference was easily made. But, on the other hand, certain bones were strikingly reptilian; and, accordingly, it was judged possible that the latter might have belonged to the animal to which the teeth had been referred.

Such was the character of the osseous relics which had been discovered. No important bones were in a state of connection, or integrity, with the exception of a jaw so very minute as to be comparatively microscopic, (*see Plate IX. fig.* 1.) Of this relic, however, an important use was made, as will soon be shewn.

From the foregoing statement it would appear, that, while the scales, and many other bones, yielded rather ambiguous evidence in determining an exclusively saurian question, it was the character of the teeth which afforded the greatest ground of presumption, that saurian reptiles had actually existed during the carboniferous epoch.

But, in order to arrive at the truth, the greatest desideratum was the discovery of some part of the head in which the jaws and teeth would prove to be in a state of connection. It is certain that two or three of such relics had been actually found, the inspection of which was never conceded to myself. M. Agassiz had the opportunity of a momentary glance at one of these remains, when he instantly conceived that it belonged rather to a sauroid fish than to a reptile. This suspicion having been communicated to me, I instantly recollected, that the portion of a very minute jaw, evidently belonging to one of the fry of the questionable animal, was in the possession of the Royal Society of Edinburgh; and, upon placing it under a strong magnifying glass, M. Agassiz informed me that the suspicion which he had entertained was still farther confirmed.

The conclusion eventually drawn from this examination was, that the large teeth, the scales, and many of the bones, did not belong to a saurian reptile, but to an immense sauroid fish.

NOTES TO SECTION VIII.

It had been long known among the workmen of the limestone quarry of Burdiehouse how anxious I had been for the discovery of any bones to which the teeth might be found to actually adhere; which anxiety arose solely from the wish that some additional light might be thrown upon the saurian question. Now, from some cause or other (which I will not seek to explain), relics of such importance as these never came into the possession of the Royal Society, although a price would have been advanced, even on my own account, should the Royal Society have refused to purchase them, fully equal to the sum any other individuals would have paid. Such a value, indeed, did I at one time place upon the discovery of teeth thus found attached, that for such a prize I would have been content that they should have been rated at the highest market price which our best dentists actually receive for teeth belonging to the Homo sapiens of Linnæus.

I had flattered myself that the line of conduct chalked out by myself during this investigation had been sufficiently appreciated. As I had publicly expressed my wish that some individual who had made zoology more a particular object of his study than myself, would undertake the business of describing the osseous remains discovered at Burdiehouse, and, as the specimens in the possession of the Royal Society of Edinburgh were open for consultation to every member of it equally with myself, and, indeed, to every scientific individual capable of forming a judgment upon an intricate question of comparative anatomy, I thus conceived that the possibility of any feeling of rivalry or jealousy being excited, was entirely out of the question.

It was not, however, at Burdiehouse alone, but in other localities near Edinburgh, that sauroid relics had been discovered, of no less importance towards the elucidation of truth. They constituted private property; and, of course, I had no right whatever to make any complaint that I was not permitted to avail myself of the information which they were calculated to impart. I am only entitled to express some little regret that no account of them had ever been published; otherwise, I might have been enabled much sooner to have reconsidered the opinion which I had expressed on the saurian question, and to have acknowledged my obligation for the correction of any error into which I might have fallen;—an acknowledgment whic I have not hesitated to pay to M. Agassiz.

SECTION IX.—THE FISH OF RECENT TIMES CALCULATED TO EXPLAIN THE SAUROID CHARACTER OF THE OSSEOUS REMAINS DISCOVERED AT BURDIEHOUSE.

The conclusion arrived at by M. AGASSIZ could not but be regarded, as portending a splendid accession to our knowledge concerning the immense animals which lived in so early an epoch as that from which our coal-fields date their origin. The monsters which roamed among the more ancient waters of our planet, did not possess for purposes of locomotion, paddles or feet like those of the reptiles of later epochs ;—they were vested with fins, yet still exhibited along with the attributes of fish, a sauroid form of teeth, and a sauroid structure of the larger bones, in connection with the splendent scales of the crocodile, or gavial.

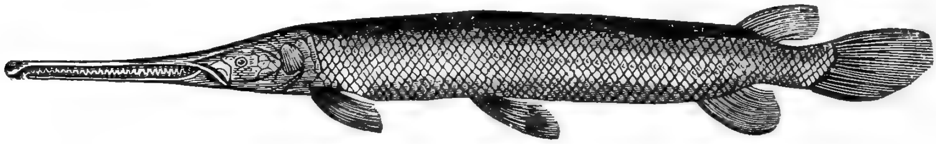
From these considerations a natural question arose,—Does any animal yet exist upon the surface of the globe, with which a monster of so mixed a character can be compared ? A reply has been given in the affirmative.

Among the countless numbers of animated races long since extinct, there would still appear to be some approximating tribes, which linger on the present surface of the globe even during its very altered state. These might have been called into life under local or partial circumstances of subsistence, or habitat, alike common to some condition of a primeval state of our planet. But such concurrent circumstances, whatever they may be, we have not always the means of ascertaining.

It is sufficient to state, that, in reference to fish which possess important characteristics in common with those of extinct tribes, M. AGASSIZ, for purposes of comparison and analogy, has made a very diligent quest, as may be shewn by some striking illustrations which appear among his Ichthyological researches.

This naturalist, in establishing among his ganoid order of fish a sauroid family, had referred, as a type of it, to a recent sauroid fish, the principal species of which dwell among the lakes and rivers of the warm regions of America. To this animal the name of *Lepisosteus*, or, according to M. Agassiz's altered nomenclature, *Lepidosteus* has been given,—a term significant of the hard, bony, and crocodilian character of its scales.

With this sauroid, yet finny inhabitant of the waters, M. Agassiz had already, as far as related to certain comparatively small sauroid fish enclosed in the carboniferous group of rocks, found much analogy. He had, however, still to learn, that sauroid fish of even gigantic dimensions were referable to the self same geological epoch. The knowledge of this important fact he first acquired at Burdiehouse.



The Lepidosteus, of which the foregoing is a representation, admits of three species, the Lepidosteus spatula, the L. Gavial, and the L. Robolo.

The Lepidosteus Gavial is so named from its actual resemblance to the crocodilian animal of the Ganges. "This fish," observes a naturalist, "has the greatest relations of external likeness with the saurian reptile, whence it has derived its name. These traits of resemblance are instantly recalled to the mind of the observer by the form of the head of the animal;—by the very great elongation of its jaws, by their little breadth, and by the furrow longitudinally hollowed out on each side of the upper jaw;—by the irregular osseous pieces, carved, radiated, and strongly articulated with each other, which envelope its head, or which compose its opercules;—by the quantity, the form, and the inequality of the teeth:—by the position of the orifices of the nostrils at the end of the snout;—by the situation of the eyes, which are placed very near the angle of the mouth; —by osseous scales which, being distributed over the whole of the body, form an impenetrable cuirass against the teeth of other inhabitants of the water, and even against the ball of a musket, which makes no impression."—*See the article Lepisosteus (by M. Cloquet) in the Dictionnaire des Sciences Naturelles.*

But I have now to communicate a most important addition to our knowledge relative to this sauroid fish, by which it is even still more closely approximated to animals of a reptilian character.

The cellular structure of the swimming bladder of the Lepidosteus had been long since remarked as curious, but it has been left to M. Agassiz to discover the use of this internal structure, and, along with it, that of the swimming bladder itself,—as this organ is usually named. Having been recently, by the British Museum, allowed the permission of dissecting a specimen of the Lepidosteus, preserved in spirits of wine, he has been so very kind as to favour me with the result of this most important anatomical investigation.

"You know," he observes, "that the nature of the swimming bladder of the fish is still an object of controversy among anatomists; some considering it as a particular organ peculiar to the class of fish, while others (the natural philosophers) suppose that it is analogous to the lungs. In examining this organ, as it is met with in the Lepidosteus, I have not only been enabled to demonstrate that it is a real lung, but I have also found a great approach in its structure to that of the lungs of reptiles. The lung (the swimming bladder) of the Lepidosteus, is not only cellular, as we find it to be in salamanders, and in the reptiles improperly named doubtful reptiles, but it also possesses a trachea *(trachée artere)*, which is extended the whole length of its anterior surface, and which opens at the bottom of the mouth by a glottis surrounded by ligaments which close and shut it;—this apparatus being even more complicated than we find it to be in many reptiles.

"The heart also has not the appearance of the heart of an ordinary fish. It is destitute of that inflation, so characteristic of fish, called Bulbus-aorticus, and has rather the aspect of the heart of a reptile."

Such is the important information transmitted me by M. Agassiz relative to the Lepidosteus.

That the animal should have been found to possess lungs is a circumstance which may be availed of in certain geological speculations, and upon which some few remarks will be made hereafter. In the mean time I may observe, that, if the presence of lungs in their very complicated structure deserve to be considered as a reptilian character, which it is usually supposed to be, the Lepidosteus, instead of being regarded as a sauroid fish, rather deserves the appellation of a finny reptile.

But without insisting upon the propriety of the latter term, which, on account of other points of anatomical difference, particularly the form of the jaw, would be disputed, it may be, lastly, remarked, that the teeth, the scales, and various large fragments of bones which have been discovered at Burdiehouse, will be referred to an animal bearing the greatest affinity to the Lepidosteus, although far larger.

NOTES TO SECTION IX.

M. Agassiz, in his "Recherches sur les Poissons Fossiles," has given a drawing of the Lepidosteus, of which the wood-cut of this Memoir is a reduction. But in the present incipient state of his work, a full description of the animal has not yet appeared.

Soon after my Memoir had been read to the Royal Society of Edinburgh, in which I had supposed that a saurian animal was indicated by the teeth and scales which had been discovered at Burdiehouse, I obtained a glance only at the first number of M. Agassiz's work, in which the Lepidosteus was figured;—(for my own subscription copy had been slow in arriving.) I remember having been struck with the animal's crocodilian appearance, which was the only impression I then felt, as I had not been able to subdue the conviction in my own mind, that the monster which possessed teeth like those which were found at Burdiehouse, must have been as completely a saurian reptile as the Gavial of the Ganges.

The impression produced upon my mind by the drawing was but faintly retained, until I visited the British Museum in London. Upon this occasion, an imperfect specimen of a large fish, with splendent scales, and form of head remarkably gavial-like, immediately arrested my attention. After having taken several notes of the external character of the animal, I added in my memorandum-book, "*This fish I must know.*" In fact, my suspicion was so excited regarding the validity of my exclusively saurian hypothesis, that after having been politely favoured by Mr Gray of the British Museum, with a closer inspection than the highly suspended specimen of the Lepidosteus would permit, of smaller examples of the animal, I came to the conclusion that some of the large scales of Burdiehouse might have belonged to a fish,—an opinion which I afterwards supported. And, most probably, I should have dismissed every notion whatever which I had entertained in favour of an exclusively saurian theory, if it had not been for the remarkable character of the fossil teeth, which, after a consultation of the best works on comparative anatomy, I could not reconcile with the attributes of a fish.

It would be an easy matter for me to shew that I had not been solitary in my judgment, as it was the same which had been expressed by some of the first naturalists in London and Paris, to whom the teeth in question had been submitted. Even M. Agassiz himself, when I accompanied him to the collection of Burdiehouse specimens in the possession of the Royal Society of Edinburgh, with the view that he might select any of them for drawing which might enrich his great work, was so far inclined to countenance my opinion, that he rejected the teeth and many of the large bones collected, as not being objects of his department, considering, like myself, that they had belonged to a reptile.

It has been explained, however, that a single day had not passed over, before M. Agassiz obtained the momentary sight of a jaw of the animal; (for, as I have stated, more than two jaws at the least had been found, which I had never myself the opportunity afforded me to inspect); he then revoked his opinion, and pronounced the animal to be a sauroid fish, rather than an exclusively saurian reptile. Upon this occasion he begged me to accompany him to the College Museum, (my first visit there for many years), where he pointed out to me a very small specimen of the same Lepidosteus which had so commanded my attention in London, to which he now recommended me to direct my particular views of comparison.

SECTION X.—THE SAUROID REMAINS OF BURDIEHOUSE REFERRED TO A NEW GENUS OF SAUROID FISH, THE MEGALICHTHYS.

When M. Agassiz visited Edinburgh, he found the bones of the large sauroid fish of Burdiehouse in so broken a state, among which were fragments of the bones of other large and unknown animals, that they almost defied any attempt of successful connection. In fact this labour is only in progress. The Royal Society of Edinburgh have transmitted to M. Agassiz as many of the specimens from the collection as he wished to examine, in order to be illustrated in his work; and it is to be hoped that more important relics will still turn up during the process of quarrying. The details, therefore, of which this memoir must necessarily be deficient, will no doubt be in the course of time supplied.

From another source, however, M. Agassiz has obtained most important information relative to the sauroid fish of Burdiehouse. After leaving Edinburgh in company with Dr Buckland, these gentlemen visited many of the public museums of Great Britain, among which was that of Leeds, where they found the specimen of an entire head of a fossil animal, obtained from the coal-fields of Yorkshire, which, from the teeth and other bones, was ultimately referred to the same undescribed genus of sauroid fish.

Having obtained access to this great prize, M. Agassiz was then enabled, with advantage, to compare the imperfect relics of Burdiehouse, *first,* with the entire head from the Leeds Museum; and, *secondly,* with a large specimen of the recent Lepidosteus Spatula, preserved in the British Museum.

After M. Agassiz had, by these various comparisons, established the fact, that the teeth, and certain other osseous remains of Burdiehouse, belonged to a sauroid fish rather than to a sauroid reptile, he considered it as a new genus to which he gave the name of Megalichthys; and to the species found at Burdiehouse (the first which he had seen), he added the name of Megalichthys Hibberti.

NOTES TO SECTION X.

Under ordinary circumstances, I might have declined the honour thus rendered me. If I have accepted it, the following reasons may be assigned:

M. Agassiz had expressed some little apprehension for any pain which he might inflict in setting me right upon a question regarding which I had publicly expressed my sentiments. That the apprehension was an unfounded one, I knew but too well, by an appeal to the actual state of my own mind, which admitted no other feeling except admiration for the great talent and discrimination which he had evinced. Under these circumstances, I feared lest my refusal of the compliment which he paid me might be interpreted by him as indicative of any lurking sentiment of impatience under the correction of a misconception, and, consequently, of an unfriendly feeling to the cause of truth and of science.

If I felt any degree of uneasiness whatever, it was in the reflection, that I had, in two instances, been the *sole* cause of others partaking with me in the imperfect view which I had entertained. In the first instance, I was hurt that the misconception had found its way in a work so replete with invaluable facts as that which Mr Lyell has published; and, in the second place, that I should equally have misled another geologist, from whom the most brilliant discoveries in geological science have emanated; I allude to Dr Buckland. But I am convinced that these two individuals possess too enlarged minds not to make a generous allowance for any misapprehension in the case of an animal, which appears, no less in organic structure than in the date of its existence, to have been the very first connecting link between fish and saurian reptiles.

We are, in point of fact, only beginning to be aware, that, in an earlier period of the history of our globe, certain of the largest animals of the waters were endowed with the mixed organization of fish and reptile; that in a later period of the globe, pure reptiles have multiplied; and that the attributes of too many fish and reptiles have been hitherto confounded. M. Agassiz has already removed from the class which had been assigned to them, several animals of preconceived reptilian character. Caithness has lost a trionyx, which now proves to be a fish. The chalk of Sussex has also lost another large reptile, which, in a similar manner, turns out to be a sauroid fish of a genus differing from that of Burdiehouse, and possessing a head, jaws, and teeth as large as those of a crocodile ten feet long. "The teeth," M. Agassiz writes to me, "are as large as those represented in Fig. 8. of Plate IX."

In short, many saurian reptiles, which were supposed to have lorded it over lesser tribes, have been dismissed from their administration; while an interchange of reptilian and finny attributes has afforded the basis for forming a new natural history cabinet.

Such are the original views opened out to us by the recent discoveries of M. Agassiz.

Statements, however, have recently appeared in two consecutive foot-notes appended to the reports of the British Association of Science, and of the Royal Society of Edinburgh, (See the *Edinburgh New Philosophical Journal for October* 1834 *and January* 1835), that M. Agassiz's investigation was not only "confirmatory" of Professor Jameson's previously expressed opinion upon the animal remains of Burdiehouse, but that myself, as well as other geologists, (not even excepting the Swiss Naturalist himself), had " adopted " this opinion. In justice, therefore, to M. Agassiz, I shall distinctly state, that the only opinion to which I ever yielded was that which constituted the great feature of his own peculiar investigation, viz. that the osseous remains in question of Burdiehouse had disclosed a remarkable character possessed by the larger animals of the carboniferous epoch, which appear to have united in their particular organization the character of fish and reptiles. This view had never before been even " dreamt of in our philosophy ;"—it was confirmatory of no other opinion whatever which had been previously expressed ;—it originated with M. Agassiz exclusively ;—it was the only one which had been successfully opposed to my own theory ;—and it was the only one ultimately adopted by myself.

Previous to M. Agassiz's arrival in Edinburgh, my saurian theory was never opposed upon any other ground, than that the teeth of the Burdiehouse animal were those of a squalus ;—which notion, I need not remark, was far more remote from their true sauroid character, than any opinion which I had myself expressed.

As I had distinctly stated at the Sectional meeting of the British Association, my impression that the relics of immense fish existed in the limestone of Burdiehouse, the chief question which remained was with regard to the teeth ; and if I had simply named them *sauroid*, instead of *saurian*, I should have kept within the strict limits of correctness. But notwithstanding this approximation to the truth, it was supposed that my theory had fared worse than was really the case. Granting even the affirmative, I would rather see any theory of mine annihilated upon true scientific principles, than survive a different kind of treatment ;—in which feeling, I partake with the sentiment expressed by one of Moliere's characters, when expatiating upon the systematic rules with which a physician of Paris killed off his patients,—that it would be a less evil to expire under his remedies, than to be cured by those of another person ;—for, as it added, although a fatal event occurs, yet a conviction remains that every thing is conducted in order, and according to rule. " C'est une grande consolation pour un defunct."—" Assurement. On est bien-aise au moins d'être mort méthodiquement."

But, happily, my noble sauroid animal,—not exactly a saurian reptile, but an immense sauroid fish,—still holds his sway over primeval waters, the sovereign of the carboniferous epoch.

Section XI.—Comparison of some of the osseous remains attributable to the Megalichthys, with others found at Leeds, and with the recent Lepidosteus.

As the investigation of the osseous remains of Burdiehouse is in a manner only commenced, I merely asked from M. Agassiz his opinion regarding such important attributes as the teeth, the jaws, or the scales. To this information, which he promised to give me after he had instituted a comparison between the Megalichthys of Leeds and that of Burdiehouse, he has added an account of other parts of the anatomy of the animal, the whole having reference to remarks made upon the recent Lepidosteus.

M. Agassiz's first observations were evidently directed to the dispelling of any possible doubt that the animal of Leeds was a sauroid fish. "The head," he observes, "is most perfect; it is certainly that of a fish, since we see in it the opercular portions, and the branchiostic rays, which only subsist in this class of animals; while behind are the scales of a portion of the trunk. It gives us, then, the greatest possible certainty with regard to the remains in question, as there are in the Leeds specimen many portions in an united state which are only found detached at Burdiehouse."

In reference to the same specimen, a comparison was made with minute bones obtained from Burdiehouse, evidently belonging to one of the fry of the Megalichthys, which had aided in the discovery, that the animal was less a perfect reptile than a sauroid fish. M. Agassiz directs attention to the lamellar bone, which is represented in Plate IX. fig. 1, on the side where the indication, fig. 1, appears. He observes, that the lamellar bone, *(plaque)*, here seen so distinctly, is the folium, which, in the head of Leeds, covers the space comprised between the branches of the inferior jaw, and which takes the place of the anterior branchiostic rays. On the opposite edge, as may be noted, is a row of maxillary teeth, alternately larger and smaller.

The Structure of the Teeth.—With regard to the teeth of the Megalichthys, M. Agassiz states, that in the Leeds specimen they were small, but that upon one of them, which was broken, there was a striated surface characteristic of those of Burdiehouse.

In comparing the teeth collected from Burdiehouse, with those of a specimen of the Lepidosteus, several feet in length, preserved in the British Museum, the latter exhibited teeth of the same size with such as are represented in Plate IX. fig. 6 and 7. They had also the same external structure; they had large plicæ at their base; while the remainder of their surface was otherwise smooth. Some of these teeth, it is added, were sharp at one edge; others were sharp at both edges; while a third description had sharp and projecting edges at their extremity only. As for their interior structure, there was, in all, a conical cavity, more or less elongated, like that of Fig. 7, and it was in the bottom of this cavity that the new teeth were developed upon the old ones falling away.

The Alternation of Large and Small Teeth.—This is the next circumstance to be investigated.

When M. Agassiz visited Edinburgh, he had little knowledge of the distribution of large and small teeth in the Megalichthys, except what was derived from the minute specimen of the jaw of one of the fry of the animal, to which I have already adverted. (*See Plate IX. fig.* 1.) He remarked an evident alternation of larger and smaller teeth, yet it was conceived (as far as such a minute and indistinct specimen could countenance any inference whatever) that there was a greater proportion of large teeth, and that canine teeth were placed upon the whole of the jaw. This notion was again countenanced by the large teeth discovered in an unconnected state, which had been hitherto found more abundantly than smaller ones. An inspection of the Leeds specimen, however, was unfavourable to this notion, inasmuch as there appeared in this instance to be a far greater proportion of small teeth, while the larger canine teeth were chiefly developed in the fore part of the jaw; and hence M. Agassiz at first conceived, that there might be two genera of sauroid animals allied to each other, but differing in the comparative size, number, and distribution of their teeth.

While labouring under this uncertainty, and in the absence of any satisfactory specimen whatever from Burdiehouse, there appeared no other resource left to this indefatigable naturalist, than to inquire whether, in other examples of recent or fossil fish allied to the Megalichthys, the larger or smaller teeth prevailed most in number. If the larger or canine teeth should prove to be fewer in number, and should have a more interior and anterior position among the bones of the mouth, he conceived that a great argument would be afforded for assimilating the specimen of Burdiehouse to that of Leeds, whence their identity as one genus of fish, if not exactly established, would at least be a reasonable presumption.

Such a comparison was accordingly instituted, by which M. Agassiz came to the conclusion, that in the lesser number of larger or canine teeth, or, in other words, in the far greater proportion of smaller teeth, the sauroid genus of Burdiehouse would be ultimately found to identify itself with that of Leeds, and that thus each animal would be referable to the Megalichthys.

This anticipation has been remarkably confirmed in the subsequent discovery of part of a jaw, too late for M. Agassiz to see before he left England. It shews a larger tooth alternating with several of much smaller size. The relic (See Plate X. fig. 1) evidently belonged to a young animal, as it exhibits none of the immense and similarly striated teeth which have been found in a detached state. It, however, not only displays a larger tooth alternating with an excess of smaller ones, but likewise a difference of size between the two kinds,—so extraordinary, indeed, as to afford an illustration of the advantages which were to be derived from a comparison of isolated remains, with the connected ones of other animals, at least approaching to one common family.

But I shall now advert to other investigations of comparison.

In the recent and very remarkable sauroid fish conceived to be a living type of the Megalichthys,—the Lepidosteus spatula,—M. Agassiz observed phenomena nearly similar. He states that teeth of an inch long, alternate with small teeth which are not a line in length. In this species, he adds, the largest teeth are found upon the palatine bones; but in other species of the same animal, they rather occur upon the margin of the maxillary bone, and more especially in their anterior part.

And again, in the new genus of Macropoma *(Agass.)* of which Mr Mantell's Amia Lewesiensis is considered as the type, and of which M. Agassiz professes he is not acquainted with any living species, a similar alternation was observed. Teeth of very different sizes occurred, always smaller on the edge of the jaw bones, and proportionally very large upon the internal bones of the mouth, so that it would have been difficult to conceive that the fragments shewing a series of palatine teeth, or those shewing a series of maxillary teeth, had belonged to the same fish, if they had not been found united in other examples.

These were the instances of analogy by which M. Agassiz, in the first instance, proposed to explain the difference in the size of the teeth which are distributed along the jaws of the Megalichthys, and from which he inferred that a similar disposition might prevail in the Burdiehouse animal. His anticipation, as I have remarked, was singularly confirmed.

It has been at length shewn that in the Megalichthys very large teeth have coexisted with very small ones; but as they severally exceed so much in magnitude those of the recent Lepidosteus, our admiration is greatly excited in contemplating the enormous sauroid fish of a former world.

The larger teeth of Plate IX. fig. 2 and 10, have a breadth at their base of 1½ to 2 inches, and a length of nearly 4 inches

After having explained the teeth and their distribution, I now proceed to the scales of the Megalichthys.

One description of scales exhibits a coating of enamel of a nut-brown colour, and often of the most brilliant lustre imaginable. These scales are of various forms, as may be seen in Plate VIII fig. 3 to 5, and Plate XI. fig. 2 to 8. They are generally angular. A curious character is the punctured surface, which many of these scales display,—a character which I have traced, though in a less prevalent degree, in the scales of the recent crocodile.

It is remarkable that M. Agassiz, from his observation on the sauroid fish of Leeds, unites the character of certain large angular scales, which present a punctured appearance, with the character displayed by certain bones of the head, which, in an isolated state, possess very irregular forms, but which have a surface of the same kind as that exhibited by the scales. To such bones he refers all the elongated portions collected at Burdiehouse possessing such a character; and, in recommending that the Royal Society of Edinburgh apply for a cast of the head of the Megalichthys preserved in the Leeds Museum, he adds,—" You will see in the largest of the Leeds specimens, many portions united which are only found detached at Burdiehouse. The surface of these bones is enamelled and punctured in the same manner as in the larger scales of Burdiehouse, and this trait is again developed in the bones of the cranium, of the face, of the thoracic cincture, and of the branchiostegal lamellæ, exactly in the same manner as is shewn in the plates of scales, as well as in all the detached maxillary and thoracic bones found at Burdiehouse. It can therefore no longer be doubted, that all these portions are referable to the same animal."

These remarks upon the scales of the Megalichthys form the communication which I have received upon the subject from M. Agassiz.

A chemical estimate has been recently made of these relics in reference to the scales of the Lepidosteus.

The scales of the Lepidosteus, which struck me as being so like those of the Burdiehouse animal, had not suggested any particular observations of comparison; their mutual similarity, except perhaps in some few circumstances of form, being so evident. As far, however, as relates to analysis, a gentleman of profound chemical knowledge, Mr Arthur Connell of Edinburgh, has had his views directed to this subject, as, indeed, to other comparative inquiries of the same nature, relative to the change which animal remains of so early a geological date as those of Burdiehouse have experienced. His valuable memoir on this subject will appear in the present volume of the Transactions of the Royal Society of Edinburgh.

I have been favoured by Mr Connell with the result of his examination of the scales of the Megalichthys, which he has subsequently compared with an analysis given by Chevreul of the scales of the recent Lepidosteus. These two results, with the view of comparison, I have arranged in a tabular form.

M. Chevreul's Analysis of the Scales of the recent Lepidosteus.		Analysis of the Scales of the Fossil Megalichthys, by A. Connell, Esq.		
Phosphate of Lime,	46.20	Phosphate of Lime, with a little Fluoride of Calcium,		50.94
Carbonate of Lime,	10.	Carbonate of Lime,		11.91
Phosphate of Magnesia,	2.02	Phosphate of Magnesia, *trace*		
Carbonate of Soda,	.10	Potash and Soda,		.47
Gelatinous Animal Matter,	41.10	Animal Matter, *trace*		
Fatty Matter,	.40			
		Siliceous Matter,	33.10	
		Water,	3.48	
				36.58
		Bituminous Matter,		.12
	99.82			100.02

Upon the assumption that the scales of the recent Lepidosteus and those of the Megalichthys identify themselves with each other, as far as external character and consistence are concerned, the foregoing Table of chemical comparison is a most important and instructive one.

With regard to the earthy ingredients of the phosphate of lime and carbonate of lime, it will be seen at first view, that, in both the fossil and recent animals, they constitute about three-fifths of the solid matter of the scales. In the fossil animal, they have remained in a proportion, most probably, unalterable.

In Mr Connell's analysis, a little fluoride of calcium appears.

The phosphate of magnesia, along with potash and soda, or the carbonate of soda, appear in very small proportions in both the fossil and recent animals.

Gelatinous substance, with a very trifling quantity of fatty matter, amounting together to somewhat less than one-half of the materials contained in the scales of the recent Lepidosteus, has almost entirely disappeared. In the fossil animal, Mr Connell could detect nothing more than a trace of animal matter.

But the place of the animal substance has been supplied in the scales of the fossil fish, by nearly thirty-seven parts in a hundred of siliceous matter, in which is included a little combined water. These foreign ingredients have no doubt been derived from the matter in which the scales have been entombed. In the present instance, they evince the solubility of silex, under certain circumstances, and its important geological character as a replacing substance.

From this interesting comparison, I now pass on to the rounded scales of the Megalichthys.

The Rounded Scales.—M. Agassiz has conceived that no doubt whatever can be supposed to hang over any of the bones which he has hitherto considered. Regarding one class of relics, however, perhaps some little degree of obscurity may remain. The rounded scales of the Megalichthys are indicated by an internal cancellated, and by an external lamellar structure, as well as by an absence of the shining enamel so distinctive of the angular scales. A representation is given of them in Plate VIII. Fig. 2.

Some of the rounded scales appear to be finely fringed at their edges. (See Plate X. Fig. 2.)

In Plate X. Fig. 3, a representation is given of these scales in a state of contiguity. M. Agassiz, from an examination of them, conceives that they are imbricated.

The rounded scales are of various sizes. I have seen specimens of them attaining the diameter of five inches; but, in proportion to their great magnitude, they appear to grow thinner.

It is supposed by M. Agassiz, that these relics will meet with an explanation in the Plate of scales given of the Lepidosteus in the first volume of his "Recherches sur les Poissons Fossiles;" and that they are referable to such dorsal scales as are to be found at the top of the long series which traverse the Plate, given by him in Vol. ii, Tab. B.

With these observations, the communication with which I have been favoured by M. Agassiz relative to the sauroid remains of Burdiehouse, is concluded. Some few additional observations from the same eminent naturalist, (See the Edinburgh New Philosophical Journal for January 1835), on the comparative character of the large sauroid fish of early and more recent formations, will properly close the present inquiry.

He states, that it is in the deposits below the lias that we begin to find the largest of those enormous sauroid fish, "the osteology of which recalls in many respects the skeletons of saurians, both by the closer sutures of the bones of the skull, by their large conical teeth striated longitudinally, and by the manner in which the spinous processes are articulated with the body of the vertebræ, and the ribs at the extremity of the spinous processes."

It is likewise remarked, that "we do not find fish decidedly carnivorous before the carboniferous series; that is to say, provided with large conical and pointed teeth. The other fish of the secondary series before the chalk appear to have been omnivorous; their teeth being either rounded, or in obtuse cones like a brush."

I cannot sum up this portion of the investigation, without adverting to the geological importance of selecting for purposes of comparison and analogy, of which the Lepidosteus affords a splendid example, animals subsisting in recent times, which may be supposed to have the nearest relationship to races long since extinct. In the present instance, it has been reserved to the talents and discrimination of an AGASSIZ alone, to rescue from a kind of obscurity a sauroid fish, (I was nearly repeating the term of a *finny reptile)* dwelling among the lakes and rivers of the most thermal regions of America, and to render it elucidative of one of the earliest states of our globe, when, in the language of this naturalist, fish combined in their peculiar organization the character of reptiles;—of a class of animals, which only appeared in great number during a later period of the history of our planet.

NOTES TO SECTION XI.

In closing M. AGASSIZ's account of the Megalichthys, I feel it my duty to express the deep sense of obligation I am under to him for correcting the imperfect views which I had entertained regarding this animal. But it is not the gratitude of an individual like myself that will form the meed which awaits this naturalist. The general cause of geology is interested in the light which he has thrown upon the subject of Palæontology.

Considering the very disjointed state of the specimens hitherto collected at Burdiehouse, no feeling can possibly remain, except one of admiration that M. AGASSIZ has been enabled to make so much of them. It is greatly to be lamented, that this naturalist had not, before he returned to Switzerland, the opportunity of studying some jaws of the Megalichthys which had assuredly turned up, but the sight of which was never allowed to extend to the Royal Society, or to myself.

It was in the eleventh hour that I came into possession of part of a jaw, which was, I believe conceded to me, from motives of compassion. It was considered hard, that he who had first pointed out the treasures of the quarry should not be permitted to avail himself of one of the relics which he most coveted. I am sorry that it had arrived too late for a drawing of it to be forwarded to M. AGASSIZ, in time for his remarks upon its striking osteological character.

SECTION XII.—OTHER LOCALITIES IN WHICH THE REMAINS OF THE MEGALICHTHYS HAVE BEEN FOUND.

It becomes an interesting object of inquiry if the remains of the Megalichthys have been found in other localities.

The result of the information that I have been able to obtain is, that such remains have been discovered in the coal-fields of the neighbourhood of Glasgow, and in the districts of Cupar, Errol in Perthshire, East Calder, and Musselburgh; also in the coal-fields of Northumberland, of Yorkshire, and of North Wales. It amounts only to a probability, that remains of the Megalichthys have been found in certain limestones of Ashford, in Derbyshire, and of Northumberland. The details of these discoveries are given in the note appended to this section.

M. AGASSIZ has stated his opinion, that there exists in Scotland another species of Megalichthys, of which some relics were placed in his hands by Mr ROBISON, who had procured them from Greenside near Glasgow. He conceives that the animal to which they may be referred, is to be distinguished from that of Burdiehouse by the form of the teeth, which is more compressed and very sharp at the edges. He proposes to call it the MEGALICHTHYS FALCATUS.

NOTES TO SECTION XII.

If it be allowable to suspect that the scales of the Megalichthys have been mistaken for those of saurian reptiles, as was the case in the first instance at Burdiehouse, the evidence with regard to other localities in which the remains of the Megalichthys have been found, becomes multiplied.

The black limestone, as it is called, of Ashford in Derbyshire, so named from the bituminous matter which is diffused through a great part of it, rather affords evidence of its having been the deposit of an estuary than of a fresh water river, or lake, such as the limestone of Burdiehouse is supposed to have been. This fact I shall endeavour to substantiate hereafter. It is sufficient at present to state, that the limestone of Ashford contains, along with marine shells and corallines, various remains of the plants common in coal-fields.

In this limestone Mr WHITEHURST, who wrote his well known Theory of the globe in the year 1778, found, as he stated, the impression of a crocodile. Mr WHITE WATSON of Bakewell conceives that Mr WHITEHURST mistook for these remains a large orthoceratite; but, with all due deference to my old acquaintance, I am inclined to doubt the legitimacy of this explanation, from having myself lately ascertained that remains of very large fish existed in this limestone, which, if not of the Megalichthys, assimilated themselves to other remains found at Burdiehouse;—this being a fact which had been previously unknown. It seems then at least a plausible conjecture, that some remains of a large sauroid fish might have turned up in this locality.

This suspicion meets with additional support from a passage which we find in the "View of the present state of Derbyshire" (vol. i. p. 200), written by Pilkington in the year 1789. The writer not only affirms that a small alligator was found entire in the black marble of Ashford, but he also adds that another specimen, the tail and back of a crocodile, had been discovered in the same locality, and had met with preservation in a cabinet at Brussels.

But it is time to advert to statements better verified.

In the year 1793, the Rev. David Ure wrote his History of Rutherglen (a most interesting work), in which it is certain that he discovered part of a jaw and teeth, which, from the drawing given of them (plate 19 of his work), are referable to the Megalichthys. They are said to have been found among schist in the quarries of of Philipshill, and in the till above coal at Stonelaw. (*See p. 330 of the work*). Other remarkable teeth will also be found described in his work.

In the year 1830, Dr Fleming published an account of scales, and of a tooth which evidently belonged to the Megalichthys. They were found in the yellow sandstone of Drumdryan quarry to the south of Cupar, and in the red sandstone of Clashbinnie near Errol in Perthshire (*Edinburgh Journal of Natural and Geographical Science*, vol. iii. p. 81.)

During the same year, viz. in A. D. 1830, Mr Lyell, in his Principles of Geology, states, that the Rev. Vernon Harcourt discovered in the mountain limestone of Northumberland a saurian vertebra. Whether this relic may be referred, or not, to the Megalichthys, remains to be determined.

In December 1833 my discovery took place of the Megalichthys of Burdiehouse.

Also in 1833, there was figured in the Fossil Flora of Professor Lindley and Mr Hutton some remarkable relics, which, under an impression, acknowledged at the time to be a very dubious one, that they might be identified with some fungus, suggested the name of Polyporites Bowmanni. They were discovered by J. E. Bowman, Esq., of the Court near Wrexham, among the ejected shale of a coal-pit near the entrance of the vale of Llangollen in the County of Denbigh (*Fossil Flora*, plate 65). Although the writers of the Fossil Flora have proposed the name of Polyporites Bowmanni, indicative of a vegetable fungus, it is with proper caution remarked, that "it is a matter of great doubt whether these relics actually belong to the vegetable kingdom;" and they admit with Mr Bowman, that one of his specimens "might be taken for the scale of a fish, or of some great saurian reptile." Now I have little or no doubt whatever in my mind, that these relics are the round scales of the Megalichthys. It is stated at the close of the communication, that it may be worth considering "whether the Carpolithes umbonatus of Sternberg, referred with doubt to Cyclopteris by Adolphe Brongniart, may not also be something of a similar nature."

In 1834, Mr Witham of Lartington obtained from the limestone of East Calder some scales of the Megalichthys. Mr Robison procured some teeth from Greenside, near Glasgow, of the Megalichthys falcatus (Agass.), and Lord Greenock made the highly interesting discovery, that the remains of the Megalichthys were entombed in a seam of coal and bituminous shale at Stoneyhill, near Musselburgh, along with various other relics.

During the same year (1834) scales of the Megalichthys were shewn to me by W. C. TREVELYAN, Esq. of Wallington, which he had obtained from the coal-fields of Northumberland.

As these remarks conclude my account of the Megalichthys, I may here mention the information which M. AGASSIZ communicates to me, that in the much newer formation of Whitby and Scarborough, he has found the fragments of a fish which even exceed in size those of the Burdiehouse animal; some of the portions of the cranium being nearly two feet in diameter. In announcing to me the discovery of this monster, he adds, "After having seen so many extraordinary and surprising things in the monster of Whitby, which exceeds the largest Ichthyosaurus, I perceive that I am still only commencing the preface of a book, which future years will supply with the wonders of the sea. And I shall think myself happy if I can only have excited interest for a study, which is still so difficult for want of terms of comparison sufficiently numerous."

SECTION XIII.—THE GENUS OF PYGOPTERUS BELONGING TO THE SAUROID FAMILY.

While the primeval waters of the carboniferous epoch had their larger sauroid monsters, they had also lesser sauroid tributaries, one of which was in the form of the Pygopterus, belonging to the same family as that to which the Megalichthys has been referred.

M. AGASSIZ has favoured me with the following description of the Pygopterus of Burdiehouse. Unfortunately, however, it only applies to a fragment of the fossil fish, which is represented in Plate VII. fig. 2.

The proportions of the rigid tail of the animal, the relative position of the dorsal and of the anal fins, the form of the fins, which are horizontally and perpendicularly elongated, as well as the considerable number of rays which compose them, serve to point out the posterior part of a fish of the genus Pygopterus, which belongs to the "Sauroides Heterocerques," (AGASS.) Amidst the numerous fragments of bones of the head, as well as of the thoracic cincture, which have been found along with them, it will remain for a detailed description to explain, by the system of exclusions, what might have belonged to the animal. M. AGASSIZ then conceives, from his having seen so many heads of the Pygopterus, as well as the head of the Megalichthys of Leeds, that the identification will not be insurmountable.

This relic was the first which I discovered at Burdiehouse; for which reason I have requested M. Agassiz, that, in its specific name, I may be allowed to dedicate it to a geologist for whom I possess the greatest esteem. He has accordingly obliged me by sanctioning the name of Pygopterus Bucklandi.

M. Agassiz conceives that the best specific character of the Pygopterus Bucklandi is the relative smallness of its scales.

SECTION XIV.—THE REMAINS OF THE FISH OF THE PLACOIDIAN ORDER DISCOVERED AT BURDIEHOUSE.

It has been explained, that Placoidian fish are easily known on account of the irregularity manifested by the solid parts of their integuments, which consist of materials of enamel sometimes considerable, and sometimes reduced to little points; as is shewn in the tubercles of Rays, or in the different chagrins of Squali, &c.

The same naturalist, at the commencement of his great work, has expressed his imperfect knowledge of the fossil fish of this order. But I believe that since his recent visit to Great Britain, he has been enabled to examine some few specimens in comparatively a better state of preservation. He conceives, that, in correspondence with differences of organization, such remains of fish as are found in beds anterior to the carboniferous group, would have to be referred to the order of Placoids.

M. Agassiz has recently stated, that, among the Placoids, those above all predominate which have their teeth furrowed in both the external and internal surface, and have large thorny rays.

These large rays are now considered as the supports of the dorsal fins of several genera, which, from their approach to the Cestracion of New Holland, M. Agassiz has formed into a distinct family, under the name of Cestraciontes, to which family six fossil genera, approaching to the Squalus, with rays differing considerably from each other, have been already assigned.

M. Agassiz remarks of this family, that it only comprehends one genus of actual creation, the genus Cestracion ; the others being fossil ; that the Hybodontes (Agass.) are equally fossil. Then follow the Squali, the Rays, and the Cyclostomæ.

The limestone of Burdiehouse entombs thorny rays of immense magnitude, and most beautifully configurated, which it is impossible to contemplate without amazement. (See Plate XI, fig. 1, A, B, and C.) The drawings of them given in Plate XI. are upon a scale of one-half only of their original dimensions. They are treble the size of a recent dorsal ray, obligingly shewn me by Mr Clift of the Museum of the Royal College of Surgeons, which was supposed by M. Blainville to be the dorsal bony ray of a large Silurus of the Ganges.

M. Agassiz confesses that he knows of no other relics found elsewhere to which they bear any resemblance ;—that, while the mountain limestone, the lias, the oolite, and the chalk formation, afford some very striking and decided genera, none of them resemble the rays found at Burdiehouse, which he thinks ought to be referred to a separate genus of the family of Cestraciontes. He proposes to name the fish to which these rays belong the Gyracanthus formosus.

It is most unfortunate that nothing is known of the Gyracanthus formosus more than is indicated by these relics, which give the promise of a fossil monster some time or other turning up, equalling, if not surpassing in interest, the Megalichthys.

The teeth, even of the Gyracanthus, are unrecognised. "It is not impossible," says M. Agassiz, "that the teeth of so remark-

able an appearance, which have been discovered, may belong to this animal. But this is a mere supposition, solely founded upon the analogy of what I have seen concerning the rays of other formations."—I may here observe, that the teeth alluded to were not found at Burdiehouse, but in the important fossiliferous coal-seam of Stoneyhill near Musselburgh, first brought to notice by the researches of Lord Greenock.

Besides the rays which have been assigned to the Gyracanthus, there exists a peculiar kind of Ichthyodorulite, comparatively small, and of an elegant form. But as so much more information is demanded relative to the placoidian animals to which they belong, any drawings of them may be properly suspended until further remains turn up.

I have now to state a very important analysis, undertaken by Mr A. Connell, of the bony rays of the Gyracanthus formosus:—Phosphate of Lime with a little Fluoride of Calcium, 53.87; Carbonate of Lime 33.86; Siliceous matter 10.22; Potash and Soda, partly as chlorides, 0.71; Bituminous matter 0.54; Phosphate of Magnesia, a trace; Animal matter, a trace: Total 99.20.

NOTES TO SECTION XIV.

Mr Connell has compared the analysis which he has given of the bony rays of the Gyracanthus formosus with one by M. Dumenil of the bones of the recent pike. I shall throw these two analyses into a tabular form, for the sake of a more ready comparison.

Analysis of the Bones of the Pike, by Dumenil.		Analysis of a Thorny Ray of the Gyracanthus formosus, by Arthur Connell, Esq.	
Phosphate of Lime,	55.26	Phosphate of Lime, with a little Fluoride of Calcium,	53.87
		Phosphate of Magnesia,	*trace*
Traces of Soda, and loss,	1.32	Potash and Soda, partly as Chlorides,	.71
Animal Matter,	37.36	Animal Matter,	*trace*
Carbonate of Lime,	6.16	Carbonate of Lime,	33.86
		Siliceous Matter,	10.22
		Bituminous Matter,	.54
	100.10		99.20

If it be allowable to suppose that there was any original approximation in the quality and quantity of chemical ingredients severally possessed by the bones of the pike and the fossil thorny rays of the Gyracanthus, which is rendered very probable by the proportion of phosphate of lime in each so nearly agreeing, the one analysis yielding 55.26 parts, and the other 53.87 parts in a hundred,—the following observations occur.

The fossil thorny rays contain a trace only of phosphate of magnesia, which has not been detected in the bones of the pike.

There is a small proportion of soda in the recent bones, and perhaps a less proportion of potash and soda existing partly as chlorides in the Gyracanthus.

The proportion of animal matter in the bones of the pike amounts to as much as 37.36 parts in a hundred, of which a trace only appears in the Gyracanthus formosus.

Having stated these differences, I shall first make some remarks on the great quantity of phosphate of lime possessed by the fossil ray.

Mr Connell has very properly remarked to me, in reference to the Gyracanthus having been assigned to the family of the Cestraciontes, that the Cestracion is considered as a cartilaginous fish, whilst the great quantity of bone-earth contained in the dorsal ray of the fossil animal, amounting to 54 per cent., is incompatible with the notion of its being any thing but bone. Now, I certainly find it remarked of cartilaginous fish, that, while the skeleton of these animals remains constantly cartilaginous, in rare cases only it contains osseous fibres; the calcareous matter being in such instances deposited simply in the form of small grains. There are, at the same time, certain fish of this class, though appearing to separate themselves from it, as, for instance, the orders of Chismopnea and Teleobranchia, among which the skeleton is fibrous, and becomes with age perfectly osseous.

To such a structure, therefore, I would refer the fossil dorsal rays of the Gyracanthus, which appear to have been formed of osseous fibres, disposed in a longitudinal manner (as can even be traced in their mineralized structure), the intervals or pores having been originally filled with animal matter. This arrangement of structure is indeed remarkably shewn in some fossil specimens, where elongated pores, or tubes, larger than common, are filled with siliceous matter.

Another remark is suggested by the mode in which the loss of animal matter has been supplied in the fossil rays of the Gyracanthus. It would here seem, that the bones of the pike contain, in addition to the phosphate of lime, about six parts in a hundred of the carbonate of lime. Now, it is remarkable that the fossil thorny ray contains as much as 34 parts nearly of the carbonate of lime, along with only 10 parts of siliceous matter, by which it appears that the loss of animal matter has been very readily supplied by infiltrations of calcareous matter, derived no doubt from a calcareous matrix, but comparatively little with siliceous matter. Ought this circumstance to throw any light upon the original organization of these rays?

There is, at least with myself, much difficulty in explaining why the loss of animal matter is chiefly supplied in these fossil rays by carbonate of lime, which is exactly the reverse of what appears to have taken place in fossil scales. Whether, in this instance, the animal matter has been sooner released from the substance of the bone, and, as a consequence, its place more directly supplied by infiltrations of calcareous matter from the matrix of these relics, I will not pretend to determine. In the dense and compact structure of the bony scales of the Megalichthys, the mineralization might have gone on far more slowly; and hence the substitution of a great excess of siliceous matter.

The two different characters of mineralization are exhibited in the following Table.

Fossil Scales of the Megalichthys contain		*Thorny Rays of the Gyracanthus contain*	
Carbonate of Lime, . .	11.91	Carbonate of Lime, . .	33.86
Hydrated Siliceous Matter,	36.58	Siliceous Matter, . .	10.22
	48.49		44.80

These observations have been suggested by the important analysis of Mr Connell. But he is far better qualified than myself to enter into questions relative to the chemistry of geology.

I shall, lastly, make some few remarks upon these large rays, as they are found in other localities.

It would appear that the large bony rays of the Gyracanthus are not confined to the limestone of Burdiehouse, as I have seen specimens of them collected from the argillaceous shale of Fifeshire. Similar relics have also been discovered in Northumberland.

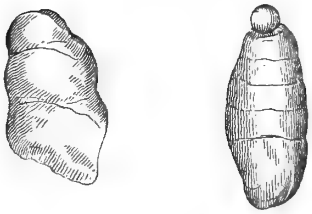
In the next place, large bony rays, referable to other placoidian fish, appear in the shale of the coal-fields of Glasgow; they are likewise found in limestones of marine origin, as in the mountain-limestone of Ashford, from which locality I procured them during the course of the last summer. Dr Simpson of Bathgate found a silicefied organic substance in a mountain-limestone of marine origin, which, if it had not been in so old a deposit, I should have taken for a belemnite. It was obtained from Kate Shield's or Crawford's quarry, near Bathgate. M. Agassiz thinks that it indicate an interior cavity (from which the organic matter must have been removed) of one of these large rays.

SECTION XV.—THE COPROLITES OF THE LIMESTONE OF BURDIEHOUSE.

From the numerous relics of small and immense finny animals discovered in the quarry, it cannot surprise us to find that fœcal remains should occur in an abundance, rivalling perhaps in this respect the particular locality which very early excited the attention of Dr Buckland, and to which he has given the name of the Cloaca maxima of Gloucestershire.

In inspecting the very exact figures of the coprolites which have been published in the excellent memoirs of Dr Buckland on this subject, it might be expected that we should find these shapes abundantly developed in the limestone of Burdiehouse; but the contrary is the fact. The peculiar forms of the fœcal contents of the intestinal canal must, from their aqueous submersion, have been frequently lost. Although coprolites containing the indigested scales of fish are most abundantly diffused through the limestone, they seldom preserve any decisive form.

An exception to this rule certainly occurs in the argillaceous shale lying above the limestone, indicative of a difference of circumstances by which the form of the coprolites has been preserved. This deposit was no doubt originally a turbid one, calculated to obviate the washing away of any fœcal shape.



Two representations of the smaller coprolites are here given. Their form is far better preserved than that of larger specimens.

The larger coprolites attain a great size. I have seen them occasionally diffused over a surface of limestone to the extent of nearly a foot.

These fœcal remains are of a pale yellow colour, and have a very dull earthy aspect. In the shale they acquire a darker tint.

Mr Connell has undertaken an analysis of these coprolites, which he has accomplished with his usual skill. Two of these I shall subjoin.

	First contains	Second contains	
Phosphate of Lime, with a little Fluoride of Calcium, ...	85.08	83.13	
Carbonate of Lime, ...	10.78	15.11	
Silica, ...	.39	.29	
Potash and Soda, ...	.59		
Bituminous matter, ...	3.95	1.47	{ with alkaline matter.
Phosphate of Magnesia, a trace.			
Animal Matter, a trace.			
	100.79	100.	

Another analysis of a Burdiehouse coprolite has been recently published by Dr William Gregory and Mr R. Walker; but as the specimen was unfortunately mixed with foreign matter, as, for instance, with the sulphuret of iron, the result would rather mislead. These gentlemen have, however, conducted a very satisfactory analysis of a coprolite found in Fifeshire. (See Edinburgh New Philosophical Journal for January 1835.)

The most important information yielded by these coprolites, points to the means of support and habits of the animals which lived during such a remote epoch.

In the smallest description of coprolites, we see, with few exceptions, little more than a homogeneous mass. A question then arises with regard to the support of such immense shoals of smaller fish as appear to have frequented this locality. These must have formed objects of pursuit for larger monsters whose remains are here collected.

In this inquiry I shall recur to the Entomostraca which formed the subject of our early inquiry.

That calcareous springs, such as must evidently have been in play when the deposit of Burdiehouse was formed, should have been favourable to the growth of the myriads of microscopic minute animals which occupied the waters of this ancient river, or lake, I need not remark, as the nature of their testaceous coverings points to the medium in which they subsisted.

The inquiry, then, which is suggested, has a reference to the existence of these Entomostraca as the food of the smaller finny inhabitants of ancient waters.

The existence of these microscopic races assuredly finds some analogy with what has been recorded by an excellent naturalist regarding the modern waters of Lochmaben in Dumfriesshire. Entomostraca abound in this fresh-water lake about seven-twelfths of a line in length, which are supposed to approach nearest to the Lynceus lamellatus and Trigonattus of MULLER. They breed very frequently throughout the year, and carry the ova about with them, as is the habit of most crustaceous animals. (*See Dr Knox's Memoir in the Transactions of the Royal Society of Edinburgh*, vol. xii. page 505.)

Until the appearance of Dr KNOX's instructive paper on the habits of the Vendace (the Corrigenus of systematic writers), which was afterwards extended to those of the herring and salmon, the importance of entomostraca, as an object of the food of many fish, was either unknown or lost sight of; and the popular opinion regarding the vendace of Lochmaben, long entertained, was, that, "cameleon like," they derived support from the medium in which they existed. But Dr Knox shewed that more solid matter was demanded for the food of these fish, and that the microscopic entomostraca diffused through the waters of Lochmaben served as the food of the vendace, affording them a rich, and, at the same time, a most delicate bait, for which they refused every other kind of food, and, consequently, were not to be captured by the line.

In judging, then, from analogy, it is not an unreasonable supposition, that the abundance of microscopic animals contained in the calcareous deposit of Burdiehouse might, like the modern Entomostraca of Lochmaben, have stood in a similar relation to certain of the smaller fossil fish which have been described, in having afforded them the abundant means of sustenance.

But if this analogy may reasonably explain the kind of support afforded to some of the smaller fish, it fails of application when we come to larger kinds.

In proportion as coprolites increase in size, we find that they contain the scales of fish, shewing that the larger fish to which these fœcal remains are referred, must have frequented the ancient river or lake, indicated by the limestone of Burdiehouse, in quest of their prey. Many of the indigested scales are of smaller fish, but in some of them I have found larger scales, as well as fragments of larger bones, shewing that the Megalichthys might have even lived upon its own progeny.

But, as a contemporaneous fish of extraordinary magnitude, the Gyracanthus formosus, must have likewise frequented these ancient waters, it is not always easy to make very accurate coprolitic distinctions.

Another kind of food afforded to the lesser finny inhabitants of primeval rivers or lakes, must of necessity have consisted in the droppings from larger animals, or even in the putrid carcasses of such fish as died in the waters. In these cases, numerous little scavengers (as Dr Buckland calls them) would not allow putrid and insalubrious matter to remain long undevoured.

It would thus appear, that while the impurity of ancient waters was thus obviated, an excess of increase was checked by a mutual system of voracity.

NOTES TO SECTION XV.

M. Agassiz writes to me, that he has been making observations on the organs of digestion possessed by the large monsters belonging to later formations. He found that a very singular body, which Mr Mantell had taken for the swimming bladder of the Macropoma, was in reality its stomach, the different membranes of which were well preserved, yet were liable to exfoliation when long in contact with the air. At the posterior extremity of the alimentary tube, coprolites might be detected, round, like those of the reptiles of Lyme Regis, and containing scales of the Zeus Lewesiensis. " Who would have formerly presumed," he asks, " to one day find in the fossil state organs of digestion with all their forms preserved ?"

SECTION XVI.—THE MODE IN WHICH VEGETABLE AND ANIMAL REMAINS ARE DIFFUSED THROUGH THE LIMESTONE OF BURDIEHOUSE.

The sequel of this portion of the present memoir will be devoted to a few remarks on the mode in which the vegetable and animal remains of Burdiehouse are diffused through the limestone.

In this diffusion, little or no order is preserved. Vegetable and animal remains are not confined to particular seams of the rock, but may occur in any part of it. Nor are they confined to the limestone itself, since they have been found in argillaceous and bituminous shale, both above and below the bed. And if they are discovered in much greater number in the limestone, the excess may, in some little degree, be owing to the far better state of preservation in which organic remains are preserved in a calcareous matrix. At the same time, the circumstance must not be lost sight of, that ancient waters, in which calcareous matter was elaborated, seem to have been more favourable to the development of animal life than any other medium.

One observation, however, I have not unfrequently made, which is this : that in portions of the limestone remarkable both for the quantity of entomostraca, as well as diffused vegetable matter which they contain, numerous remains of fish are not unfrequently found. In Plate VII. fig. 6., for instance, is a fragment which includes, along with microscopic entomostraca, the vegetable remains of the Lepidostrobus, and the fish to which the name has been assigned of Palæoniscus Robisoni.

SECTION XVII.—THE ACQUISITION OF ORGANIC REMAINS WHICH MAY BE EXPECTED DURING THE PROCESS OF QUARRYING.

I have at length described the mineral character, and the various organic remains enclosed in the freshwater limestone of Burdiehouse ; its plants ; its entomostraca ; its smaller fish ; the entombed relics of its larger monsters, the scaly Megalichthys, and the Gyracanthus formosus, the latter armed with immense and beautifully configurated rays.

An investigation of the rich treasures of the quarry is only at its commencement. From the multitude of vegetable remains which every explosion of the quarry brings to light, the limestone appears as if it had been destined to enclose a whole grove which had existed of Lycopodiaceæ, and other kindred plants, beset with a dense undergrowth of smaller ferns. Under these circumstances, a rich field must surely await the fossil botanist.

Nor is the field of research less promising to the entomologist who has leisure to study the countless entomostraca with which the deposit, in its original state, must have almost seemed alive. Although many genera or species may be detected, only three have been yet noticed, chiefly on account of the less complex character which they display.

But the great treasures of the quarry consist in its vertebral animals. Upon the valuable additions to fossil ichthyology, which, from this source, may be expected, I will not enlarge. Its fish, though numerous, are known to us by very few specimens; and, as for its gigantic monsters, we are hitherto only enabled to judge of them by broken relics: Ex pede Herculem.

Great, however, as is the satisfaction from contemplating the riches of the deposit of Burdiehouse, it is impossible that this feeling should not be alloyed, in some degree, by the recollection that these limestone quarries had been worked for perhaps half a century, or more, at no greater distance from Edinburgh than four short miles; and that, during this period, countless bones, to the irreparable injury of science, must have been sacrificed on the fires of the adjacent limekiln.

If we would form some little, yet at the best a very inadequate, notion of the extent of this continued destruction of osseous fragments which must have been going on throughout a very prolonged period of time, we must descend into the caverned chambers which constitute the old workings of the quarry.

The old line of Burdiehouse quarries was, in the earliest stage of its sinkings, so wrought as to present to view a deep perpendicular escarpment; which is the state of the newer quarry at present. The strata of limestone, which are conjointly twenty-seven feet in thickness, dip towards the south-east, at an angle of 25°. Now, in order to extend these workings by deep excavations, immense square pillars, wrought out of the rock, have been allowed to remain, as the supports of a roof formed by the upper stratum of the limestone, which roof, in its turn, sustains a great thickness of superincumbent shale.

These excavations are of considerable extent, greater even than is rendered manifest. The quarry, which was originally laid dry by an engine, became converted, upon its abandonment, into a reservoir intended to contain the drainage-water, conducted by subterranean channels, from a more elevated quarry of freestone. Consequently, long-extended chambers lie concealed beneath the deep waters, which pervade the intricate recesses of this spacious grotto.

From this vast submerged labyrinth, formed during the process of quarrying, myriads of organic remains have, for fifty years or more, been extracted,—only to be devoted to the kiln!

But this destruction is not without a precedent.

When I accompanied a savant to the older excavations of Burdiehouse, he pronounced the quarry, as well in reference to its picturesque character as to the associations which it instantly inspired,—A SECOND MONTMARTRE.—(*See Plate V.*)

The famous Gypsum quarry of Paris certainly exhibits an analogous instance of a prolonged destruction of organic remains, which was not effectually resisted, until a CUVIER, supported by all the aid which a government friendly to science could impart, interfered, and, with the arm of power, stopped a ruthless annihilation of organic remains fatal to our knowledge of the tertiary history of our planet.

In the present instance, the merit of a similar interposition, which Geology will ever commemorate with gratitude, is due to THE ROYAL SOCIETY OF EDINBURGH.

NOTES TO SECTION XVII.

In admiring the picturesque character of the old quarry grotto of Burdiehouse, the imagination is liable to be carried beyond the precincts of sober philosophical meditation, and, in reference to the osseous relics enshrined within its solid walls, to even indulge in the phantasies which geology can but too readily conjure up. Nothing seems wanting to complete the illusions which the grotto is calculated to excite, except to connect them with an imaginary emergence from beneath its dark watery recesses of the GENIUS OF FOSSIL HISTORY,—the same Genius who has given inspiration to a WOODWARD, a CUVIER, a BUCKLAND, or an AGASSIZ, to whom, in a visionary mood, we may assign local attributes of personification, and whom we may array in the costume of the carboniferous epoch, so well indicated by the fossil relics of Burdiehouse. She may be armed, *cap-a-pée*, with a resplendent coat of mail wrought from the hard and enamelled scales of the Megalichthys; crowned with a wreath of ferns, in which many fantastic species of the Sphenopteris are entwined; while in her grasp may be placed a magic wand, wrought from a jointed stem of the

Calamites, at the waving of which, countless bones of fish, including the dazzling scales, the elongated bony rays, and the cranial fragments of vast finny tyrants, burst from their marble prisonhouse amidst a classical shower of coprolitic relics. We may next suppose, that, in obedience to another fiat, various bones reunite and distribute themselves into their respective genera, species, and varieties; the mind being lost in the grotesque appearance of larger monsters, developed in their mixed ichthyoid and sauroid character.

If it be occasionally allowable, in a disquisition dedicated to Philosophy and Truth, to let the imagination for an instant run wild, Truth would at least stipulate that it be strongly pervaded with a philosophical moral. The Genius of Fossil Zoology may therefore rise again to our fancy;—she may point to contiguous limekilns, whence issue dense clouds of vapour; she may demand, with scowling looks, why to these destructive fires, which have blazed for half a century, countless bones have been devoted; and why cremations, in honour of her destructive opponent Cybele, have so long been rendered; or why no attempt has been made to extricate the treasury of osseous relics enclosed by them, which may have even aided in the composition of the very mortar with which the walls of Edina's geological museums have been cemented.

I will not, in my own defence, venture upon the task of inquiry, Why the limestone of Burdiehouse, situated so near the city of Edinburgh, had not been before examined? or why, in reference to my own private repose, so long as I remained in Edinburgh, I had thought it necessary to exclude Scottish rocks from my researches?

I have not, however, on this account, been the less industrious, although labouring under the inconvenient necessity, either of limiting the object of my tours to the elucidation of the archæology of Scotland, or of selecting any country as a field for geological exertion except that in which I had long resided, and to which I have been long attached by many ties of near connexion and friendship.

Many years had consequently elapsed since my retirement from the study of Scottish rocks, nor did I think that I should ever re-enter that rich, yet almost forbidden, field of Geological observation, however much I valued its high interest. This reluctance was at length overcome by a visit, purely accidental, which I paid on my return from an excursion to Roslin to the limestone quarry of Burdiehouse. My impression upon the first sight of this quarry, to which the individuals who accompanied me will remember I gave utterance, was, that I had found a geological deposit which I had never seen before, but which I had long hoped to discover. It was a freshwater formation belonging to the carboniferous group of rocks.

The result attributable to this occurrence has not, I trust, been overrated.

Upon the occasion of the meeting of the British Association of Science at Edinburgh, the osseous relics in the possession of the Royal Society were submitted to M. Agassiz, who considered them as forming some of the most valuable acquisitions which had ever been made in Fossil Ichthyology, and, in conjunction with Dr Buckland, a request was made to the Council of the Royal Society, to continue their important exertions in preventing the dispersion of the fossil treasures of Burdiehouse.

The following letter, addressed by M. Agassiz to Dr Buckland, I beg leave to subjoin:—

"Mon cher Monsieur Buckland,—Vos relations avec les membres de la Société Royale, vous permettent mieux qu' à moi de faire une démarche que je crois très-importante. La découverte d'animaux vertébrés dans les carrières de Burdiehouse, a suscité des discussions du plus grand intérêt sur les caractères des êtres de cette époque antique. Ce serait une nouvelle ère pour la paléontologie, s'il pourrait etre démontré, comme je le crois, que les poissons de cet âge réunissent à leur organisation particulière les caractères des reptiles d'une classe d'animaux qui n'apparaissent que plus tard en grand nombre. Il faudrait donc poursuivre avec ardeur les fouilles dans ces carrières, et prendre les mesures les plus sûres pour empêcher la dispersion des échantillons, et conserver à la science des documens qui pourront constater une découverte aussi importante.

"Si vous pourriez contribuer à faire prendre ces précautions, vous auriez rendu un grand service aux recherches sur les fossiles.

"Agréez l'assurance de ma considération tres-distinguée, et recevez à l'avance mes remercimens pour tout ce que vous voudrez bien faire dans cette circonstance. Votre tout dévoué,

L. Agassiz."

"Edinbourg, *le* 15 *Sep.* 1834."

PART II.

THE GEOLOGICAL RELATIONS OF THE FRESHWATER LIMESTONE OF BURDIEHOUSE.

INTRODUCTION.

Having in Part First confined myself to the description of the limestone of Burdiehouse, considered as an individual bed belonging to the carboniferous group, I shall now point out its geological relations to other strata, both of an older and newer date.

The carboniferous strata, of which the limestone of Burdiehouse forms a very subordinate bed, appear in the great Lowland valley of Scotland, which is watered by the Tay, the Forth, and the Clyde. Some of these strata, particularly such as are in contiguity with the limestone of Burdiehouse, I propose to consider very generally; first, in regard to their fractures, and, secondly, in regard to the remarkable alternations which they exhibit; and after these considerations, some important inferences may be drawn concerning the geological relations of the freshwater bed, which forms the proper subject of this memoir.

NOTES TO THE INTRODUCTION.

Preparatory to entering upon any investigation of carboniferous strata, a few remarks on the older rocks to which the coal-fields of the south of Scotland have succeeded, may perhaps aid the inquiry. These I shall throw into the form of a note.

No rocks are perhaps better calculated than those of Scotland to aid the inquiry which has been undertaken by various geologists, with the view of determining the extent of those convulsions of different periods, which have been attended with corresponding changes of level affecting the surface of our planet. Sometimes these changes appear to have been the result of sudden violence, and, at other times, of a gradual process of elevation or depression, carried on perhaps during the whole persistence of a geological age, or system. An early elevation of the Grampian ridge, for instance, appears to date from a period immediately subsequent to the formation of such primary rocks as form the ingredients of the Scottish Alps, namely, talcose schist, or primary clay-slate, mica-slate, gneiss, and their accompanying granites, serpentines, or primary trap.

At the foot of the Grampians, in the geographical line which extends from Stonehaven to Bute, transition strata of grauwacke or argillaceous schist appear in succession, which seem to have been deposited in a southerly direction over a considerable tract of space, though at present concealed beneath newer formations. This continuity I have inferred from the fragments of grauwacke schist, which I have found entangled in such trap-rocks of Angus, of the Lothians, of Ayrshire, or of Berwickshire, as had forced their way through superimposed and newer deposits. In the ridge of hills extending from Berwickshire to Galloway, grauwacke schist is seen to emerge.

The grauwacke schist has, generally speaking, a line of bearing from about S. 60° W. to N. 60° E., and dips to the west at very considerable angles, seldom less than 50° or 60°. It is of a dark grey colour, and is either compact or very finely granular. In some places it is highly laminar, and is successfully quarried for roofing slate. It frequently passes, particularly in its upper beds, into a substance of a very arenaceous and mechanical structure, which may be fairly regarded as a harder sort of sandstone. The colour of the sandstone, or schist, thus induced, is more inclined to a light grey, greenish-grey, or yellowish-brown colour than to one of red; though to this rule I have seen exceptions. It is also remarkable for the quantity of mica which it contains. In Ayrshire, beds of granular limestone alternate with the grauwacke schist.

That these beds of grauwacke schist will be found to contain organic remains, particularly in their upper strata, I have strong reason to suspect.

Subsequently to this formation of transition strata, the range of the Grampians appears to have undergone a sudden elevation, which may explain the presence of such lower beds of coarse conglomerate strata as were deposited at the foot of this chain of mountains. I also suspect that about the same period commenced another elevation of the ridge of grauwacke schist which extends from Berwickshire to the Mull of Galloway. But if this southerly elevation may be supposed to have actually commenced about the same period, I conceive that it was under circumstances of far less violence, and that it was a gradual, rather than a sudden process.

But whatever may be the exact nature of these disturbances or convulsions, it is certain that, from the cessation of the grauwacke deposit may be dated the incipient conversion of the considerable track intervening between the chain of the Grampians and the lowland confines, into a sort of basin, which, in the first instance, would no doubt have its continuity much broken by irregularities of surface. This I infer from various insulated and very limited deposits, which appear to me to have a date of formation intermediate to grauwacke schist, and to carboniferous strata. The remark, however, fails in applying to the district north of the Tay, where a continuous depression appears to have been formed, within which was deposited very considerable beds.

The oldest beds which have succeeded to the grauwacke schist, and which repose upon it in an unconformable position, consist of the hard, yet fissile and micaceous sandstone north of the Tay, known under the name of the Arbroath Pavement, which, in its upper beds more particularly, passes into sandstone, often very red, but sometimes of a paler reddish-brown or buff colour, and identifies itself with the old red sandstone of geologists. It is now ascertained that the remains of fish are enclosed in this very early formation of sandstone, and I have also observed in it indications of plants.

The termination of this deposit carries us to the dawn of the era when the carboniferous group of rocks was deposited, preparatory to which I shall make some few remarks connected with the formation of the great lowland basin.

Although I believe that the chain of the Grampians dates, at least, two of its elevations from periods antecedent to the deposit of such strata as are known by the name of the Arbroath Pavement, yet I must consider that its greatest elevation took place immediately before the deposit of the carboniferous group of rocks. That this convulsion was a sudden one, and attended by circumstances of great violence, is evinced by the immense mass of conglomerate strata, which, in being deposited at the foot of the Grampians, repose upon prior formations of the Arbroath pavement, and other analogous strata. It would also appear, that the whole district intermediate to the Grampians and the Tay, and extending SW. by W. to Bute, had likewise been subjected to a similar process of elevation, though under an agency of far less suddenness or violence than that which had thrown up the Grampian chain.

Along with this disturbance, a sort of nearly parallel movement appears to have taken place in the elevated ridge of grauwacke schist extending in a direction of ENE to WSW. from St Abb's Head to Ayrshire and Galloway, which upon this occasion received its great elevation.

Owing to these convulsions, the great chain of the Grampians was elevated considerably above the level of the ocean, as may be shewn by the relative differences of height which appear among the primary strata of the Scottish Highlands, and such limestones of marine origin as may be found in the great valley of the Lowlands. In comparing these levels, and even in allowing for any sources of error arising from comparatively late disturbances of strata, no inference can possibly arise, but that at the time when the coal-measures began to be formed, the Grampians must have sustained a very considerable elevation above the level of the ocean which then subsisted.

The same remark, though in a less degree, applies to the ridge of grauwacke schist which extends from St Abb's Head to Ayrshire, and which towers greatly above the carboniferous strata which repose at the foot of this lofty chain, although it is, at the same time, of much less elevation than the Grampian Alps.

It would thus appear that, intermediate to the Grampian chain, and to the chain of hills, which on the south, bounds the present Lowland valley, a deep basin must have subsisted.

Within this basin the carboniferous strata of the south of Scotland were deposited.

SECTION I.—THE FRACTURED STATE OF THE STRATA IN THE VICINITY OF THE LIMESTONE OF BURDIEHOUSE.

The strata with which the limestone of Burdiehouse are more or less connected form the great Mid-Lothian coal-district, of which the neighbourhood of Dalkeith may be considered as the centre.

Mr De la Beche, in his excellent little system of theoretical Geology, has remarked of the Earth's crust, that there is scarcely an area of eight or ten miles which has not been more or less fractured; which remark cannot be better illustrated than in the Scottish coal-fields, which may be considered as exhibiting a congeries of fractured areas, separated from each other by long lines of fissure.

The rationale of this fractured state is now beginning to be understood, for which we are indebted to an eminent geologist, M. de Beaumont.

The theory hinges upon the following circumstances:—The interior of our globe has had its temperature diminished sensibly by the reduction of heat, and it has consequently contracted. The earth's crust, however, has preserved, at the same time, a nearly rigorous constancy of temperature, its refrigeration having been almost insensible, and hence little contraction has ensued. Keeping, then, in view these inequalities of contraction, it would follow that, as the interior of the globe must, in length of time, have had its temperature lowered far more than its exterior crust, its capacity must have undergone a proportional excess of diminution. In this case, the external solid crust, in striving to conform itself to the fluid or viscid surface beneath, and to embrace it more closely, would undergo considerable dislocations;—and, from a cause so generally operating, fractures would be induced, which would have a greater or less tendency to a parallel direction.

In the Mid-Lothian coal district decided lines of fracture from these causes must have very early taken place. They have, for the most part, a direction between the limits of SS.W. to NN.E., and S. W. to N. E.; and, in this respect, remarkably confirm the theory of M. DE BEAUMONT.

The first line of fracture may be traced from the vicinity of Burdiehouse, and not far from the trap of the Pentlands, in a direction from SS.W. to NN.E. as far as Joppa on the Frith of Forth. This line of fissure passes to the west of, and close to, the limestone of Burdiehouse.

A second line of fracture nearly follows the line of the North Esk, from the vicinity of Penicuik to Bilston Burn near Loanhead, whence it is continued to the west of Musselburgh, in a direction from S.W. by S. to N.E. by N. This great line of fissure was first traced by WILLIAMS, the mineral surveyor, who has written so ably on the coal-fields of Scotland.

A third line of fracture appears to nearly follow the course of the South Esk, where it may be traced at intervals from the vicinity of Dalhousie to the east of Musselburgh, in a direction from SS.W. to NN.E. This line of fissure is not so evident, owing to the comparatively little difference of inclination subsisting among the strata upon each side of it, as well as the covered state of the ground.

A fourth line of fracture, owing to the covered state of the ground, is not easily traced. It probably has its rise from the vicinity of the Moorfoot Hills. Its course is assuredly along the Roman mount, east of Dalkeith, where it is possibly implicated with an intrusion of trap, which may have not found its way to the surface. The direction of the fissure is not far from SS.W. to NN.E.

There are, however, other fissures varying from these, which do not appear to be of any great extent. Among these, I do not include the numerous subordinate faults or troubles which are so well known to miners. One near Dalkeith, perhaps larger than common, is said to run in a transverse direction, nearly from N. W. to S. E.

Fractures among the coal strata, more or less inclined to parallelism, having been thus induced, a subsequent effect produced by the struggle of disjointed portions of the earth's surface to conform themselves to the diminished capacity of the internal nucleus of the globe, would be manifested in the edges of these fractures being pressed forcibly against each other, by which some of the strata bounding the edge of a line of fracture would give way to this mutual compression, and would consequently be thrust upwards in the form of mountain ridges.

Accordingly, these mountain ridges observe lines of direction which are often parallel, or nearly so, to lines of fracture.

It has been thus shewn, that the Mid-Lothian coal-district exhibits lines of fracture nearly parallel to each other;—that causes in operation had a tendency to force these broken surfaces into a nearer contact with the centre of the earth, in order to adapt themselves to the diminished capacity of the nucleus of the globe; that these causes had induced the edges of these fractures to approximate to each other; and that in the mutual force and struggle thus used, such edges of fractures as during the struggle had given way, were thrust up into mountain ridges.

It must also be evident, that in the case of any newer contracted state of the nucleus of the globe, inviting a fresh struggle of its broken crust to adapt itself to an internal surface of less capacity, old lines of fracture would of course be the lines of least resistance. And hence, broken up and tilted masses would continue to be thrust up in the same line of direction.

These efforts of a subsequent date appear to have been gradual, and even oscillatory, as will be explained hereafter. Consequently, few or none of the fractured systems of strata which I have described, shew at their lines of fault marks of sudden violence;—a circumstance which had been previously observed by Williams, in the instance of the great fault which divides the Hawthornden and Burdiehouse systems. In short, every thing indicates, that these later movements must have taken place by very slow, prolonged, and tranquil processes.

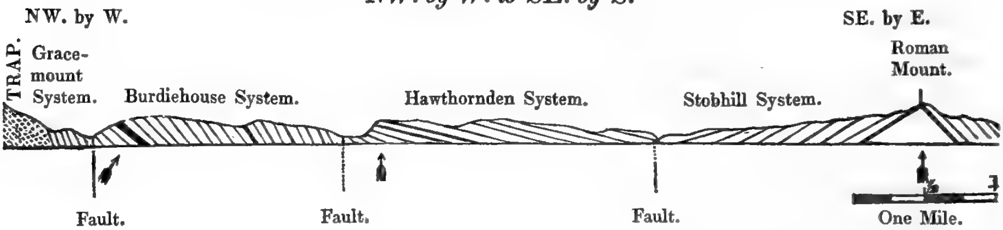
Lastly, during the tremulous efforts by which lines of fracture have been made to approximate, igneous matter has not unfrequently intruded itself through these fissures. Thus we may conceive a large fissure through which trap has protruded, to be indicated by the eruptions, continued with little interruption, which we trace from Tinto to the Pentlands, and thence to Salisbury Craigs, in a direction of nearly S. W. to N. E.

From these observations it would appear, that the carboniferous strata under our consideration shew striking marks of the upheavings and the rents which they have undergone both before and subsequent to the completion of their deposit, accompanied with eruptions through them of plutonic rocks. In consequence of these rents, and of the uplifting power which has been exerted along the line of them, the coal-fields of Mid-Lothian have been broken up into various systems of strata, severally differing from each other in the extent of the beds which have been made to emerge. This difference would naturally be in conformity with the circumstances under which the uplifting force has exerted its influence.

In examples where the anticlinal energy has acted powerfully at one and the same time upon a great extent of surface, we find that at the line of rent or fissure very deep-seated beds have been made to emerge, or crop out:—as may be shewn on the coast of North Berwick and among the Pentlands, by the emergence of beds even inferior to the carboniferous group. But examples are far more numerous of the line of fracture being distinguished by the cropping out of no deeper seated beds than those of a marine limestone, to which I have given a middle place in the carboniferous system to which the limestone of Burdiehouse belongs.

The systems of strata, separated from each other by lines of fissure which are in greater or less contiguity with the limestone of Burdiehouse, I shall now explain in reference to the following section.

Section from Morton (at the foot of the Pentland and Braid Hills) to Stobhill, NW. by W. to SE. by S.



In reference to this section, it will appear that I have distinguished four systems of strata, divided from each other by lines of fissure, viz. the Gracemount, the Burdiehouse, the Hawthornden, and the Stobhill systems.

In giving a very brief explanation of these various systems, I shall commence with the Hawthornden system, notwithstanding the greater degree of obscurity which still hangs over it, in reference to the question, long since agitated, regarding its geological position.

The Hawthornden system comprises a peculiar thick bed of reddish and softish sandstone, in which small rounded pebbles of quartz are inclosed. It is geologically known by the name of the Roslin sandstone. This sandstone, which, from its quartzose ingredients and other circumstances, appears to me to have been deposited soon after the formation of transition rocks, may have undergone an earlier elevation than perhaps any other strata belonging to the systems which are laid down. The consequence of this priority of elevation may have been that want of continuity which the coal strata superimposed upon the Roslin sandstone exhibit, in relation to other coal seams lying to the west and east of them; although, from a mineralogical correspondence, their relative date may perhaps be correctly estimated. The thick bed of Roslin sandstone, which is surmounted by eight or more conformable seams of coal, exhibits a very trifling dip of 10° or 12° to the east or north-east. It is separated from the highly inclined strata of the Loanhead coal-measure, belonging to the Burdiehouse system, by the second line of fault described, which may be observed in the channel of Bilston Burn; and from the Stobhill system, by the third line of fault which with difficulty is to be traced near the South Esk.

On the east of the Hawthornden system, occurs that of Stobhill. A line of fracture, probably accompanied with a concealed protrusion of trap, has thrown up a limestone of marine origin, which forms the ridge of the Roman Mount, and from this anticlinal centre, superincumbent strata of sandstone, shale, and coal, dip towards the surrounding low declivities. The system of Stobhill has a westerly dip, amounting in some places to 30°. It is continuous with the coal-measure of Brians, which enumerates twenty seams of coal or more.

To the west of the Hawthornden system, may be observed the most complete suite of carboniferous strata of which Mid-Lothian has to boast; namely, the conjoint systems of Gracemount and Burdiehouse.

Immediately to the east of the Pentland trap, beds of sandstone are to be found, which I have named the Gracemount system, from a quarry recently opened there, where the strata in their nearly vertical position may be observed. They belong, as I conceive, to deeper seated beds, which have been thrown up during an early eruption of the Pentlands. They are separated from the Burdiehouse system by the first line of fault enumerated.

The Burdiehouse system comprises a series of sandstones, shales, limestones, and coal, among which the Burdiehouse limestone appears in a very inferior position. The system is separated from that of Gracemount by the first line of fault described. Near this line of fissure the strata of the Burdiehouse system dip to the south-east at about 23° to 25°; but, as we approach towards the Hawthornden system, the dip may be found to gradually increase, until it attains, near the second described line of fault observable at Bilston Burn, so great a dip as 56°, or even more. It is difficult to explain this circumstance without taking into account, that this increased dip might have been caused by some renewed, yet very slow elevation of the Hawthornden system of strata, by which a sort of lateral pressure had been induced among the contiguous strata of the Burdiehouse system, sufficient to increase their inclination from 23° to 56° or more.

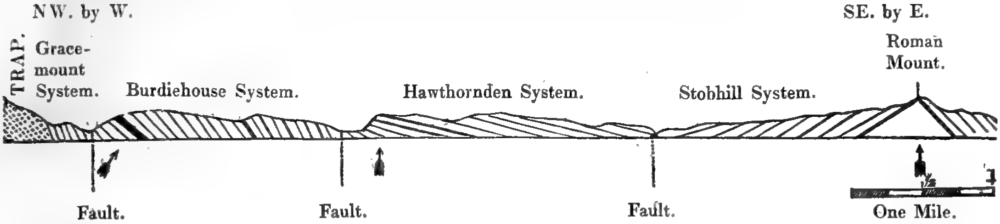
Having at length briefly described the systems of strata to be found in the great coal-field of Mid-Lothian, as indicated by fractured areas, I shall now confine my attention to the connected ones of Gracemount and Burdiehouse.

SECTION III.—THE GRACEMOUNT SYSTEM OF STRATA.

Before describing the beds to which the limestone of Burdiehouse belongs, it will be necessary to previously notice the strata lying to the west of, and separated from, the Burdiehouse system by a line of fissure. The Gracemount system occupies a place intermediate to this fault and the trap of the Pentlands.

A sandstone, which perhaps holds the lowest place in the carboniferous system of the great Lowland valley of Scotland, is composed of fine, yet very uncrystalline, grains of siliceous matter; it contains little mica; it is of a deep red colour, owing to the iron which is diffused through it; and it is of a soft consistence. This, the most deep-seated bed, rises to day in a few places only, and, if it is to be traced in the group of strata to the west of the limestone of Burdiehouse, we must perhaps look for it among the upheavings which occur in the immediate vicinity of the range of trap rocks, extending from the chain of the Pentlands, with some little interruption, to Arthur's Seat. Most probably this sandstone may be proved to be wholly, or in part, of marine origin,—a suspicion which I have formed upon certain organic remains apparently referable to this deposit.

But quitting the consideration of a sandstone, the existence of which in the Gracemount system is at best dubious, owing to the rocks being here much concealed, I shall proceed with a description of what is actually known.



To the west of the line of fracture described, strata of sandstone are thrown into a vertical position, or nearly so, as was shewn in an old quarry adjoining the village of Burdiehouse, which is now filled up. But similar phenomena may be still observed within the park of Gracemount, where the same continuous strata, consisting of a pale, yellow-coloured micaceous sandstone, with a direction from S. 10° E. to N. 10° W., are inclined to the south-east, at so great an angle as 80°. These strata appear to have been the tilted beds which had been thrown on edge during one of the convulsive movements connected with the evolution of the trap of the Pentlands, to which range of hills they are contiguous.

The limestone of Burdiehouse, however, which is situated to the east of these nearly vertical beds, dips no more than from 23° to 35° to the south-east, and shews in other circumstances that it was thrown up by an anticlinal force distinct from that by which the strata to the west of it appear to have been tilted, though originating doubtless from the same general cause.

The nearly vertical sandstone of Gracemount forms a good freestone. It is of a light yellowish colour, and it contains mica as an ingredient. In the less inclined sandstone of Craigmillar, which is apparently a continuation of the Gracemount bed, and which shews many variations of colour, as light yellowish, grey, or even pale red, some of the strata enclose very small attrited portions of quartz, as well as of a substance, which, from its mixed siliceous and argillaceous character, and from its grey, or bluish-grey colour, may be considered as the detritus of older grauwacke beds. In other localities of the sandstones which hold a similar place in the carboniferous group of the Lothians, vegetable remains have been found, though sparingly.

To this system of strata may be referred an outcrop of limestone, which is of a marine rather than of a fresh-water origin. It is to be found in the burn near Moredun Mill, about a mile to the north of Burdiehouse. A bed not many feet in thickness, and containing producti, encrinites, &c. is alternated with shale; but I suspect that more than one bed may be found to thus alternate. This limestone and the strata associated with it dip variously, and shew marks of disturbance. The inclination is to the south-east, at angles varying from 44° to 50°, and in some places it is even greater.

This limestone of marine origin is cut off, by the great fault described, from the strata connected with the Burdiehouse limestone, and hence their exact relative position cannot be well ascertained. But I am of opinion that the epoch at which the Burdiehouse and the Moredun Mill limestones were severally formed, cannot be widely different.

The limestone of Moredun Mill is interesting in this respect,—as shewing that a contiguous and shallow sea had subsisted about the time when the Burdiehouse limestone was deposited.

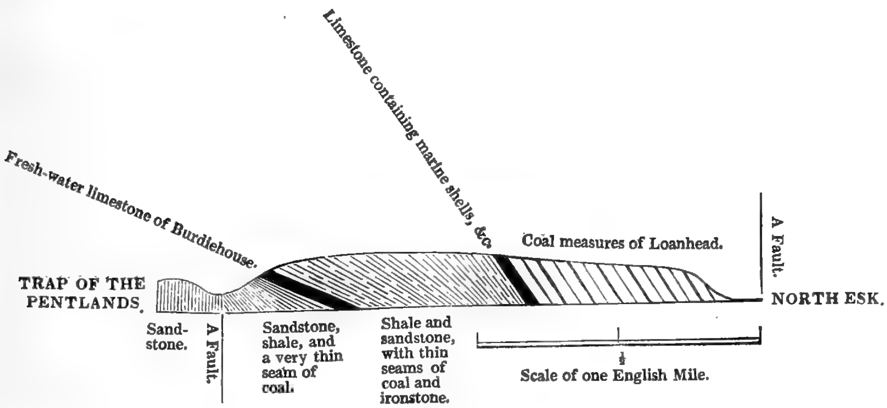
A similar knowledge I have also acquired from studying the strata near New Cumnock in Ayrshire, where there is a very deep-seated bed of marine limestone, which, when compared with another similar limestone much higher up in the series of carboniferous beds, holds the same relative place as that of Burdiehouse; and, when compared with the Moredun Mill limestone, maintains the same deep seated position, in reference to a far higher and newer marine limestone, which is that of Gilmerton.

NOTES TO SECTION III.

I have to thank Mr Charles Maclaren of Edinburgh, who is well known for the sensible descriptions which he has at various times given, in the Journal which he conducts, of the geology of Edinburgh, as well as for the perspicuous and popular style in which they are written, for calling my attention to the seams of limestone at Moredun Mill, of the existence of which I had been previously unaware. It will be seen that I have not under-rated their importance. In fact, they are in correspondence with what I have since observed in several places, that more than two, or even three alternating beds of marine limestone occupy different places in the carboniferous system of the south of Scotland.

SECTION IV.—THE BURDIEHOUSE SYSTEM OF STRATA.

The system of strata connected with the limestone of Burdiehouse, is to the east of the great fault described in the last section, which has brought to view deep-seated beds.



I may now remark, that, in reference to the section given, I shall point out, in their several orders, first, the strata in immediate association with the limestone of Burdiehouse; secondly, the description of overlying beds contained between the Burdiehouse limestone and the limestone of Fountain-well, which is the name given to the site of a cluster of houses close to the village of Loanhead; thirdly, the character of the Fountain-well limestone, which appears to differ from that of Burdiehouse, in containing marine remains; and, fourthly, the rich coal-measures of Loanhead which take the highest place in the system.

SECTION V.—THE STRATA IN IMMEDIATE ASSOCIATION WITH THE LIMESTONE OF BURDIEHOUSE.

To the sandstone of the Gracemount system, I consider the limestone of Burdiehouse, and its associated beds of argillaceous shale, as having immediately succeeded. I need scarcely recapitulate its character, but merely remark, that no confirmed marine remains have, to my knowledge, ever been found in it;—that it contains the plants usually found in coal-fields in a most remarkable abundance, as well as numerous Entomostraca, together with the relics of fish, large sauroid bones, and coprolites, and that I consider this limestone as of fresh-water origin.

No rock beneath this bed is developed, except that which forms the floor of the quarry. I was therefore anxious to obtain from Mr Torrance of Meadow Head, who possesses the lease of the lime-works, some knowledge of the beds subjacent to the limestone, which do not, however, seem to have been explored to any great depth.

The following is a list of the beds above, as well as below, the limestone of Burdiehouse, which I have given in a descending order:—

a, Argillaceous and bituminous shale of a very dark colour, alternating with which are thin seams of ironstone, and three or more very thin seams of limestone, the latter being from 2 to $2\frac{1}{4}$ inches thick, and at intervals from each other of 27 to 36 inches. From 30 to 50 feet of shale are exposed.

b, The limestone of Burdiehouse, 27 feet thick.

c, A pavement of rather soft blaes, 2 to 3 feet thick. This is an argillaceous and bituminous shale mixed with calcareous matter, and forming, near the junction of the rock, or immediately subjacent to it, an impure limestone.

d, Limestone of inferior quality, 3 or 4 feet thick.

e, Black blaes (argillaceous and bituminous shale), rather soft; 3 or 4 feet thick.

f, A seam of coal 6 to 10 inches thick.

g, "Yellow clay;" (an argillaceous and shaly sandstone?) Some of the workmen describe this bed as *metal*, and as forming a very coarse freestone. Depth unknown.

The beds of argillaceous shale, both above and below, enclose the same organic remains as are found in the limestone of

Burdiehouse, along with coprolites, shewing that they are themselves a portion of the lacustrine deposit of this locality.

In the uppermost bed of argillaceous shale, specimens of the Unio have lately been discovered by the quarrymen, which were placed in my hands by Mr Robison.

In the annexed wood-cut, the natural size of the bivalve is represented, though some specimens occur a little larger. It does not appear to me that this species has yet been described. It differs from any unio which I have yet seen represented in the tumidity of its form, which is rounded into a sort of nut-shape. The shell is much less elongated than the *Unio Urei*, which holds a similar place in the carboniferous group of Scotland. If it should prove a new species, which I suspect, I propose to call it, in reference to its shape, the Unio nuciformis.

I have at length enumerated the beds which may be accounted as inferior to the limestone of Burdiehouse, and I have also glanced at contiguous strata holding a superincumbent position. Their line of direction is from SS.W. to NN.E. The dip is 23° to 25° SE.

Owing to the covered state of the ground, Burdiehouse is the only locality where this limestone is seen to crop out. The prevailing notion is, that it reappears at the foot of the Pentlands, and that, at Carlops, it is to be identified with a seam of limestone not more than six inches thick. But this is a very doubtful opinion.

Another circumstance to be remarked is, that there is no evidence of the deposit being a continuous one. Thus, in the excellent manifestation of strata which may be traced from the vicinity of Siccar Point in Berwickshire to Cove, this fresh-water limestone is deficient. But, in the place of it, there appears to be a bed of bituminous shale, in which I detected the characteristic fern of Burdiehouse, the Sphænopteris affinis, along with coprolites, and the remains of smaller fish. And, on examining the outcrops of strata in other localities, this deposit was equally wanting.

Some other fresh-water limestones will be described in the supplement to this memoir. In the mean time, I shall remark, that they appear to me, like the limestone of Burdiehouse, mere local deposits of calcareous matter.

SECTION VI.—THE STRATA INTERMEDIATE TO THE LIMESTONE OF BURDIEHOUSE AND THAT OF FOUNTAIN-WELL.

The interval of space between the limestone of Burdiehouse and that of Fountain-well, near Loanhead, comprises alternations of sandstone, and argillaceous and bituminous shale, in which are ironstone bands, and thin and unworkable seams of coal, with the exception of one bed of coal, named the North Green seam, which lies immediately below the Fountain-well limestone. At the same time, although all the strata rest conformably upon each other, we find that their angle of inclination has gradually increased. At Burdiehouse, the limestone dips from 23° to 25° to the south-east; but, as we approach a higher series of beds, the dip is 30° or 40°, and upwards.

NOTES TO SECTION VI.

As it is of great importance that no doubts should subsist regarding the geological position which I have assigned to the limestone of Burdiehouse in the carboniferous system of strata, the details will be given at length.

It has been shewn that, immediately above the limestone of Burdiehouse, which is of the thickness of 27 feet, occur beds of argillaceous shale, through which bituminous matter appears to be diffused. These strata are visible, from the operation of quarrying, to the height perhaps of 30 to 50 feet, or even more. Alternating with these beds of shale are three, or even more, seams of a ferruginous limestone, severally from 2 to 2¼ inches thick, and occurring at intervals from each other of from 27 to 36 inches.

With respect to strata still higher, and next in succession, I was enabled to obtain some little knowledge of them, in consequence of a shaft which had been sunk with the view of carrying off the water from a freestone quarry, situated at no great distance, near the village of Straton. The distance between this freestone and the shale, was described to me as about fifty fathoms in perpendicular depth, in which nothing was found but *Dalk ;* that is, a soft argillaceous and bituminous shale. But this description I must a little qualify. I observed, from the fragments thrown out, that a stratum had been pierced, consisting of small water-worn pebbles of quartz, cemented by a soft greenish and yellowish argillaceous substance, apparently decomposed trap or felspar. The argillaceous substance of the conglomerate rock appeared to me indicative of some igneous eruption, though perhaps remotely situated, which must have taken place not long after the deposit of the Burdiehouse limestone.

The sandstone of Straton quarry is of a yellowish, or yellowish-brown colour, and of a hardness which recommends it as a durable freestone. It dips rather confusedly, varying from 14° to 30° east by south, south-east, or even south-east by south. Above it are beds of shale. It is evident that great disturbances have here taken place, which are further manifested as we approach the highroad leading to Loanhead, where the sandstone which is exposed betrays much irregularity of position. Its general angle of inclination is about 22°, or upwards, to the south-east. It is surmounted by bituminous shale.

Between Straton quarry and Fountain-well, near Loanhead, a distance of about five furlongs, we continue our section at right angles to the line of direction observed by the strata. With the exception of two or three spots of ground, where I found beds of slaty sandstone or of shale crop out, the ground is much covered. But my knowledge of this interval of distance was rendered perfectly satisfactory by the information communicated to me by Mr Ross of Loanhead, the intelligent greve of Sir George Clerk of Penicuik, Bart. Mr Ross has made himself well acquainted with the different strata of his neighbourhood; and from him I acquired the knowledge, that the covered ground between Straton and Fountain-well had been very carefully explored, in the endeavour to find out if there were any outcrops of coal. The result of the examination was, that the interval of space between Straton quarry and Fountain-well, near Loanhead, consisted of alternations of freestone and coal blaes, (sandstone and bituminous shale,) with ironstone bands, which also contained very thin and unworkable seams of coal. From this remark, however, I except a thicker seam of coal, which is said to lie immediately below the Fountain-well limestone.

As far as the few visible outcrops of this space of ground enabled me to judge, I was led to infer that the dip of the strata, in ascending from the lower to the higher beds, was gradually increasing. For instance, the deep-seated bed of the Burdiehouse limestone inclines from 23° to 25° to the south-east; but, in the strata higher up in the system, the inclination had increased to not less than 30° or 40°, or even more.

The highest bed of the series of strata, intermediate to the limestone of Burdiehouse and the limestone of Fountain-well, is the seam of coal formerly alluded to, known by the name of the "North-green seam." It is of the thickness of 4½ feet; but, having been very early wrought out, its outcrop is no longer visible. It is affirmed to lie immediately below the limestone of Fountain-well.

A bed of coal holding this position is not unknown in other vicinities. Mr Williams has remarked a seam which occurs in this position, near to the foot of the Pentlands, and I have myself observed another similar one in the vicinity of Clerkington.

SECTION VII.—THE LIMESTONE OF MARINE ORIGIN AT FOUNTAINWELL, NEAR LOANHEAD.

The limestone of Fountain-well, which occupies a sort of middle place in the system, forms a portion of the continuous bed, which may be traced, cropping out at various sites, in a line, extending from the neighbourhood of Joppa to Gilmerton, and thence to Fountain-well; a distance of three miles. It is in general very impure, being much mixed with siliceous matter, whence its great hardness, which, in the absence of whinstone or trap, has caused it to be used for repairing the highways. At Gilmerton some portion of it is so much mixed up with argillaceous matter as to pass into common shale.

This limestone, in its impure state, is named *Limestone Blaes.* But it contains *fakes*, or seams, of very pure limestone, which at Gilmerton are wrought for the kiln to the thickness of 14 feet. The bed at Fountain-well encloses three *fakes*, or seams, each about two feet thick, or $6\frac{1}{2}$ feet conjointly.

The whole of the limestone, whether pure or impure, is of a blue, or bluish grey, colour.

To farther distinctions I shall allude when I have to compare this limestone with that of Burdiehouse. In the mean time, I shall remark, that it contains marine remains, such as encrinites, corallines, producti, &c., which are severally absent in the Burdiehouse limestone.

At Fountain-well, the circumstance most worthy of remark at present, is the further increase of dip which has taken place in the tilted and upper beds of the system under examination. In the inferior bed of Burdiehouse, the dip was 23° to 25° to the south-east; but in the much higher limestone of Fountain-well, it has increased to 50° or 52° south-east.

SECTION VIII.—THE UPPER SERIES OF SANDSTONES, SHALES, AND RICH SEAMS OF COAL, FORMING THE LOANHEAD COAL-MEASURES.

I shall, lastly, describe the strata superjacent to the Fountain-well limestone.

Immediately above this limestone is another seam of coal, named "*The North Coal,*" which, like the North-green seam, is $4\frac{1}{2}$ feet thick. With this seam all marine remains disappear, and we arrive at the coal strata of Loanhead, which, as the section shews, rise still higher in the system.

The coal strata of Loanhead consist of alternating beds of sandstone, argillaceous and bituminous shale, containing ironstone bands, coal, and some little limestone. The workable beds of coal are to the number of twenty-five. They are from 2 to 10 feet in thickness.

In these beds, the angle of inclination is still upon the increase. I found, upon descending into the mine of Loanhead, that the seam named the Stair-head coal, had a dip of 56° to the south-east. But it is affirmed, that, among some of the strata, the inclination is still greater. From these circumstances, the beds have acquired the name of *Edge seams*, in contradistinction to the more horizontal strata lying to the east of them, belonging to the Hawthornden system, which bear the name of *the Flat seams.* Where the edge and the flat seams appear in contact, which I found to be in the channel of Bilston burn, south of Loanhead, some confusion of the strata ensues, indicative of a great fault, yet unaccompanied with marks of violence.

Among these coal-measures there are at least four slips or dislocations, by which the coal is variously thrown up and down from thirty to sixty fathoms.

A section of the seams of coal, "as they stand in the mines of Brughlee and Mavisbank," was made some years ago. It is not every thing which the geologist could desire, as it does not include the character of the strata intervening between the coal-seams. But this deficiency I have supplied, as well as I was able, from the information which has been given me at the mine, particularly by Mr Ross.

I have also submitted the section to some alteration in its arrangement, in order to shew at one view the overlying character of the strata.

Section of the Coal Strata of Loanhead, which repose upon the Limestone containing Marine Remains. In a descending order.

	Fathoms.	Ells.	Feet.	Inches.
No proper thickness is given of the sandstone which forms the higher part of the beds. It is stated that from the surface to Mavisbank, where the day level has been made, is 40 fathoms. From the surface to Bilstone mine is estimated at 26½ fathoms.				
Wood Coal, . .	...	...	2	
Sandstone and bituminous shale,	91	...	3	
Splint Coal, . .	...	...	3	
Argillaceous shale, alternated with seams of sandstone, severally 3 or 4 feet thick, and containing about the middle of the space 2 feet of bastard limestone, inclosing calcareous spar, . . .	48	...	...	6
Flex Coal, . . .	...	...	5	
Argillaceous shale and sandstone, containing seams of ironstone and a bastard limestone, . .	12			
Parrot Coal (also called **Rumbles**), . .	...	...	3	
Argillaceous strata and sandstone, with their seams or forke of coal, ...	4	..	4	
Great Seam (of coal),	...	...	8	
Sandstone, ...	4	...		6
Stair-Head (coal), .	...		3	4
Sandstone shale, &c., . .	12	...	3	6
Charlie's Coal, . .	...	...	2	
Various strata, ...	4			
Moffat's Coal, . .	...	...	2	
Various strata, ...	2			
Gillespie Coal, .	...	...	3	4
A very thick bed of argillaceous shale, which contains ironstone bands. Below it a bed of sandstone, which forms the roof of the Black Chapel coal, .	17	...	5	
Black Chapel (coal),	...	...	5	
Indurated argillaceous shale and ironstone bands, . .	...	3	...	
Perpetual Coal, .	...	...	2	2
Sandstone and argillaceous shale,	6	...	3	
Kittle Purse Coal.	...	...	3	
Argillaceous shale, ironstone bands, and sandstone, . . .	13	...	3	

	Fathoms.	Ells.	Feet.	Inches.
Stinkie or **Peacock**,	...	...	5	
Argillaceous shale, ironstone bands and sandstone, . . .	12	...	...	6
Coal Rough, . .	...	...	3	
Argillaceous shale, ironstone bands and sandstone, . . .	3	...	...	
Glass Coal, . .	...	...	3	6
Sandstone. . . .	5	...	...	I
Brown COal, . .	...	...	2	6
Sandstone, ...	11	...	2	
Stonie Coal, . .	...	...	4	6
Sandstone, ...	2	...	...	
Hope's Coal, . .	...	...	2	
Sandstone and shale, . .	4	...	1	
Battie's Coal, . .	...	...	3	
Sandstone and shale, . .	5	...	1	
Corbie Craig (coal) .	...	...	8	
Six feet of sandstone resting on a thick mass of argillaceous shale,	20	...	...	6
Andrew's Coal, or **Little Splinty Coal**,	...	...	4	
Sandstone and Shale, &c., .	8	...	...	6
Little Coal, . .	...	...	1	6
Stratum unknown, . . .	...	...	...	6
South Coal, .	...	...	5	
Sandstone and Shale, &c. .	...	...	6	6
Little Splinty Coal,	...	...	1	6
Sandstone and Shale, &c. .	1	1	...	
North Coal, . .	...	...	4	6
Beneath the north coal I believe that there is bituminous shale with ironstone bands, and freestone. And below these strata is, **Limestone, containing marine remains**, Consisting of limestone blaes, or an impure limestone, in which occur three seams of pure limestone. The interval between the North coal and North-Green seam is estimated at . . .	100	...	...	
North-Green Seam of Coal (at present wrought out), lying beneath the limestone,	...	...	4	6

SECTION IX.—REMARKS ON THE ALTERNATIONS OF BEDS OF FRESHWATER AND MARINE ORIGIN, AS THEY OCCUR IN THE BURDIEHOUSE SYSTEM OF STRATA.

No geological phenomena are more striking than the very miscellaneous beds which succeed each other in the Burdiehouse system of strata. But similar phenomena are frequent in all the coal districts of the south of Scotland.

Some of these alternations of different deposits are explicable in reference to causes which are still operating, although the character of ancient geological agencies far outweighs in magnitude any system of causation which is at present effecting changes on the surface of our planet.

A continued humid state of the atmosphere, for instance, which is inferred to have existed during the carboniferous epoch, would manifest itself in a power of disintegrating previously formed rocks, to an extent of which we have little conception at the present day, as well as with a proportionate degree of acceleration. The very thick mass of sandstone which has enclosed the fossil trees of Craigleith is an instance, not only of the extensive disintegration of pre-existing rocks, but likewise of the very sudden drifts to which the earth, in its ancient state, has been subjected. One of the fossil trees, 24 feet in length, was found inclosed in strata dipping at little more than 10°. The tree, however, had an inclination of 63°,—very different from that of its nearly horizontal and inclosing beds. This circumstance of inclination shews that the strata could not have been the result of a slow and quiet deposition, otherwise the tree would have decayed long before the uppermost stratum had been deposited. It was doubtless conveyed to its destination by some sudden and considerable drifting from high lands; probably, by the sweeping deluges of rain to which the humid atmosphere of so early a state of the globe would be subject.

Other phenomena, however, in their reference to causes which are still operating, meet with even a less adequate explanation, unless they may be compared with the tranquil elevations of land which have been long going on in Scandinavia, or with the comparatively trifling oscillations of movement which have been detected on the well known site of the Temple of Jupiter Serapis.

In short, no geological phenomena are more striking than the alternations of fresh water and marine beds which are manifested in strata, such as those which I have described.

The carboniferous strata of the Gracemount and Burdiehouse systems, may be summed up as follows:

The lower beds comprise sandstones of considerable thickness, alternated, as we ascend in the series, with argillaceous or bituminous shale, and with limestones, some of which give indications of a marine, and others of a fresh water origin; the latter containing remains of plants and fish. Coal begins to be developed in these beds, though sparingly.

In a second, and higher description of beds, the sandstone alternates more frequently with beds of argillaceous and bituminous shale; the latter containing nodules or bands of ironstone, as well as seams of coal, which, though numerous, are, with few exceptions, thin and unworkable. These higher strata are particularly interesting for the renewed development of a limestone containing marine remains, with which they alternate.

In a third description of beds, which take a still higher place in the carboniferous system, sandstone and argillaceous shale alternate with rich seams of coal, of which about twenty-five have been enumerated.

From this summary it appears, that these alternating beds consist of sandstone, shale, coal, a fresh-water and a marine limestone. Their separate character I shall briefly discuss, previous to investigating the circumstances of their alternation.

Deposits of sandstone, which are the disintegrated materials of previously existing rocks of a siliceous character, have no doubt been transported into the basins which they have occupied, by the sudden or gradual operation of streams or rivers. Some sandstones, as, for instance, certain beds to the east of Dunbar, contain marine shells, while the majority of other sandstones inclose numerous plants, and thus afford indications rather of lacustrine than of marine deposits.

Argillaceous shale, which, in the limestone quarries of Gilmerton, is filled with marine shells, and at Burdiehouse contains the freshwater unio, represents the mud, or silt, accumulated in the beds of ancient rivers, fresh-water lakes, or seas.

Limestones, as I have urged, may be of two kinds. While that of Gilmerton, adjacent to the Burdiehouse quarry, abounds in corallines, encrinites, and shells, all evidently marine, these are in vain sought for in the other limestone which presents the remains of fish, apparently inhabiting fresh-water, and of ferns, lycopodiaceous plants, and such aquatic vegetables as flourish most among fresh-water lakes and marshes. While the one limestone, therefore, is the memorial of a sea, the other limestone indicates some fresh-water river or lake, within which calcareous matter was elaborated.

With regard to coal, most of the seams of this substance have resulted from the decay of vegetables which have grown upon the very site in which such seams of coal are found. This fact has been incontestibly shewn in the Northumberland coal-field, where the large roots of Stigmariæ have been traced in coal or shale, indicative of the vegetable mud in which they grew. It has been therefore inferred, that many of the plants of coal-fields flourished in still and shallow water.—(*See Preface to Vol. II. of the Fossil Flora of Professor* Lindley *and Mr* Hutton.) Some species of coal, however, it is supposed, might have been brought from a distance in the form of drifted vegetable matter, to which origin certain kinds of cannel, or parrot-coal, are referred. But the absence of all attrited, or water-worn pebbles within the substance of coal, is fatal to this alleged origin.

Such being the plausible origin of carboniferous beds, the next important circumstance to notice is their alternation. And on this question the following considerations are suggested.

A sandstone, of itself, may simply indicate *sand*, which had been transported by natural causes, or familiar terraqueous convulsions ;—it may indicate a quiet transportation of its ingredients to the beds of lakes or seas, and as quiet a deposition. Argillaceous shale, also, may be regarded as a tranquil deposit of silt in some lake, or sea ; while limestone, whether it occurs beneath marine or fresh waters, may be the testimony of some calcareous elaboration through deep fissures incidental to the fractured state of our globe. And, when we find alternations of these deposits, it is possible that we may be presented with no phenomena which are not familiar to us at the present day. The river, for instance, which at one period deposited sand, may, from a change of its course, which frequently happens, be fraught with silt, or clay. Meteoric agencies may interfere, which would bury a deposit under a deep accumulation of distantly transported sand or mud. Lastly, new volcanos may open out fissures in the crust of the globe, and thus give rise to new elaborations of calcareous matter. That some few of the alternations of sandstone, shale, and limestone, which we find among coal-fields are attributable to this kind of agency, no doubt whatever can subsist.

But the inquiry is very different, when we find certain beds of fluviatile origin alternate with others of marine origin ; as, for instance, shale-beds which contain numerous plants and the fresh-water unio, alternating with beds which enclose marine remains, such as encrinites, corallines, &c. &c.

As far as information is communicated by alternations of this character, no inference can be drawn, except that the land which formed the basin of a fresh-water lake had, by a terraqueous depression, become the bed of the ocean ;—or, *vice versa*, the land which formed the bed of the ocean had, by a subsequent elevation, become the basin of an inland lake.

But, when alternations of coal are included in our consideration, still more complicated views arise.

Coal has arisen from the decay of vegetables which grew on the spot; generally in marshy sites, or in shallow waters. If, then, we find seams of coal alternating with sandstones, shales, or limestones of a fresh-water origin, no inference remains, but that oscillatory movements of the earthy crust must have taken place, by which land became submerged beneath the surface of a fresh-water lake, from which, by a subsequent elevation of the land, it has emerged, so as to give rise to a new Flora. And, in the case of coal-seams alternating with limestones of marine origin, which is also incidental to the coal-field of Mid-Lothian, a similar inference arises, that dry or marshy land had become submerged beneath marine waters, and had again arisen from the bosom of the deep, to be decked with a new flora, and with fresh verdure.

While all these inferences are suggested by the system of strata which I have described, there are still other important circumstances to be taken into consideration.

These alternations of elevation and depression have, in the coal-fields of Scotland, been conducted with a freedom from disturbance which is most remarkable. In few instances, except where local eruptions of trap-rock have prevailed, have I found derangements of strata marking the junction of marine and fluviatile deposits, or any intervening conglomerate strata, from which diluvial effects might be inferred. Near Bathgate, a limestone of marine origin may, at its junction with a fluviatile bed, be found to actually graduate into a fresh-water deposit. While the great mass of the rock encloses encrinites, corallines, &c., the unio appears in its uppermost bed, near its junction with an overlying bed of sandstone, filled with the remains of plants. East of Dunbar, also, no marks of violence whatever characterise the junction of two rocks, where, in the compass of a few inches only, we find the encrinites of a bed of limestone, abruptly, though quietly, overtopped by a sandstone containing calamites.

This varying, yet tranquil, alternation of sandstone, argillaceous shale, coal, and limestone, some of which are of fresh-water, and others of marine origin, becomes of difficult explanation. Nothing is more susceptible of proof, than that, during the carboniferous epoch, the crust of the globe was subjected to continued and prolonged oscillations of movement; and these indicate an ancient instability affecting our planet, of which, in reference to the comparative state of unchangeableness which now prevails, we can have little conception.

These circumstances of alternation have recently engaged much attention.—It has been shewn, that, during the secular refrigeration to which the globe is subject, the fractured portions of its crust would continue to adapt themselves to the diminishing capacity of its nucleus, in order to embrace it more closely; but it may be now explained, that, during this process, the same fractured portions would have their action modified by other circumstances, namely, unequal degrees of contraction, caused by thermometrical differences beneath the earth's surface. These I shall attempt to explain.

A theory to this effect, proposed by Mr De la Beche and Mr Babbage, refers to the heated state of the nucleus of the globe, and its power of radiating heat. It also assumes, that the surfaces beneath rocks possess different powers of radiating heat. It follows, then, that the numerous fractured portions into which the crust of the earth has, by convulsions, been resolved, would, in reference to subjacent thermometrical differences, manifest unequal contractions;—that occasionally these unequal contractions would cause the sides of great fissures to press against each other, so as to induce sudden dislocations and movements;—but that more frequently very slow movements would be induced, too slow, perhaps, to be measured by time, attended with new positions given to great masses of rocks or strata. Large areas would be gradually thrust upwards along their lines of fracture, while other fractured areas would be depressed; so that, in this manner, extensive tracts of surface would be raised out of water, while other extensive tracts would be submerged.

But these effects, resulting from thermometrical differences communicated to the various fractured portions of the earth's surface, would scarcely harmonize with oscillatory movements, unless we include in our theory an alternation of conditions possessed at different times by such fractured portions. We must assume, that the particular thermometrical condition by which one fractured portion of the earth's crust was raised out of the water, and another was depressed, might become, at different intervals, liable to alternation; otherwise we should fail in explaining why the bed of a fresh-water lake should have so sunk beneath its level, as to become covered with the waters of the sea;—or why the same submerged tract should have again risen from ocean's depths, to resume its prior lacustrine condition;—or why the very fresh-water lake itself should have been liable to similar oscillations;—why, for instance, a given portion of land, covered with ferns or lycopodiaceæ, should have subsided beneath the level of lacustrine waters, or why it should have been again elevated above them, and again have afforded the soil for a renewed flora.

An explanation has been thus attempted of the remarkable alternations of fluviatile and marine strata, which are found in the Lothian coal-district, and of the equally remarkable tranquillity with which these alternations have been conducted. Mr De la Beche, in reference to analogous phenomena which he has observed in newer formations, conceives, that particular situations are demanded to explain these deposits;—situations where they have not been exposed to the destructive action of waves or powerful streams of water;—and hence he argues, that fresh-water lakes, like those of America, appear the localities which offer the least difficult conditions. (*De la Beche's Theor. Geology*, p. 162, 320, *&c.*)

That the existence of large fresh-water lakes is necessary to explain the phenomena presented by the coal-fields of Scotland, I have long since advocated. (*See the Reports of the Proceedings of the Royal Society of Edinburgh*). The very great differences of level which subsist between the coal-measures of the Great Lowland Valley of Scotland, and the hills by which they are bounded, namely, the higher lands north of the Tay, the great Grampian chain, and the southerly grauwacke ridge which stretches from St Abb's Head to Galloway, are adverse to the notion that, during the carboniferous epoch, the continuity of spacious seas was only broken by occasional groups of studded islets.

If, then, it be conceded, that such elevated lands had very early risen above the level of the sea, they would rather point to the existence of large continents than of archipelagos of islets; and, in admitting the development of these lofty ridges, we must also infer that, acted upon by the rains of a humid atmosphere, they would naturally give origin to corroding water-courses, or rivers, and to large fresh-water lakes.

Accordingly, the existence of one or more of such fresh-water lakes is advocated as a very early state of the great valley of the Scottish Lowlands. But it is also admitted, that this flooded condition of the country might have been varied by islets; that even large tracts of dry land might have subsisted, and have been invaded by arms of the sea, or estuaries; and that while higher lands would encourage the growth of such coniferæ as the Craigleith fossils indicate, lesser plants, as ferns, &c. would find a soil amidst marshes or shallow lakes, or on the borders of deeper fresh-water basins.

During such a condition of the globe, the calcareous deposit of Burdiehouse was formed; new races of fish inhabiting fresh-waters were created, and among them the Megalichthys. The ocean also must have possessed its own peculiar races, and its own finny monsters, which, in pursuit of their prey, would penetrate into fresh-water lakes, and even among the shallows of marshes.

These speculations may be extended to newer deposits.

In the strata succeeding to the Burdiehouse limestone, which consist of alternating beds of argillaceous or bituminous shales, of sandstone, and of thin seams of coal containing both vegetable and animal remains, we find that the calcareous deposit of Burdiehouse was but a temporary elaboration.

At Fountain-well, a thicker seam of coal (now wrought out) was once evident, covered by a bed of limestone, which contains marine shells, encrinites, corallines, &c. In reference to this, as well as to similar limestones of the south of Scotland, I may observe, that if the geologist expects to find these deposits of the same thickness as many in England, he will not fail to be disappointed. They occur in beds, perhaps not at the utmost more than forty or fifty feet thick, except in some places, and often alternating with sandstone and argillaceous shale. It is also remarkable, that not unfrequently three distinct alternations of this limestone will be found, as I have noticed between Musselburgh and Prestonpans. These alternations certainly indicate, that fresh-water lakes, or marshy tracts, were liable to be submerged by shallow seas, and not to be overwhelmed during a very prolonged interval beneath the waters of a deep ocean.

Phenomena of this kind are calculated to excite the greatest possible interest. They may be considered as affording unequivocal proof of the gradual, but momentous geological agency, which has been in operation. Partly from changes of level, occasioned by the internal commotions of the globe, and partly from circumscribed and local circumstances, new seas have in succession overflowed much of the land which had hitherto been the site of dense marshes, or tanks. The proof of these phenomena subsists in the beds of encrinal, coralline, and conchiferous limestones, which greatly overtop the lacustrine deposits we have been contemplating.

The uppermost strata of the coal-measures of Loanhead, comprise alternating beds of sandstone, argillaceous and bituminous shale, along with ironstone bands, coal, and some little limestone ; the workable beds of coal, two to ten feet in thickness, being about twenty-five in number.

By these beds numerous oscillatory movements are made known to us. Some of the alternations of sandstone, and argillaceous shale, might have been induced by matter drifting from different localities ; and, of course, varying with such localities. But, in other instances, particularly where we find strata alternating with coal, we must refer the whole to corresponding oscillations of the earth's surface, by which a land covered with ferns and other plants has been submerged beneath a fresh-water lake, only to reappear ;—and only to be succeeded by other alternate submersions and emersions, incidental to the early history of our planet.

SECTION X.—THE EFFECTS OF OSCILLATORY MOVEMENTS OF THE EARTH'S SURFACE, CONSIDERED IN REFERENCE TO THE ANCIENT VEGETATION AND ANIMALS DESCRIBED IN THE PRESENT MEMOIR.

That these oscillatory movements of the earth's surface must have caused corresponding changes in both vegetable and animal life may be expected. M. Adolphe Brongniart has frequently made observations on the differences of such plants as occur in the lower and upper beds of the carboniferous group. And, in the present instance, the *Sphenopteris affinis* and some few other ferns are, I suspect, confined to the inferior strata of the Lothian coal-fields.

With regard to the races of animals which inhabit waters, corresponding changes have been noticed. M. Agassiz has remarked, that, in a suite of strata belonging to any given formation, he has found few fish in a lower bed to agree as a whole with those belonging to an upper stratum. Thus, for instance, the Burdiehouse limestone has a character essentially different from that of Wardie, which forms a higher stratum.

It is easy to suppose that, in the change which would cause a fresh-water lake to sink beneath the depths of the ocean, and, *vice versa,* to emerge and resume its fluviatile character, many races of animals, limited to definite conditions of water, must have been destroyed. But it would also appear, that their place has been supplied by successions of new races.

In the next place, a state of the earth's surface, subject to oscillatory movements, may afford an argument relative to another question which has been started. It has been supposed, from certain *a priori* reasonings, that during the carboniferous epoch, the air was so filled with carbonic acid gas as to be unfavourable to the respiration of animals possessing lungs. And hence it has been maintained, that no saurian animal during this period could subsist. If, however, the Megalichthys is really allied to the Lepidosteus, the researches of M. Agassiz have shewn that recent sauroid fish possess not only lungs, but likewise a trachea, and a glottis. The argument, therefore, of the air having been so filled with carbonic acid gas as to be fatal to the respiration of reptiles which possess lungs, becomes at least suspicious.

The question, therefore, remaining is to the following effect: If the Megalichthys, in common with the Lepidosteus, has possessed lungs, to what condition of the earth's surface might these lungs bear reference? The solution of this question can perhaps only meet with a satisfactory solution by an appeal to the actual habits of the recent Lepidosteus, which dwells among the large lakes of South America, but of which I can find no written account whatever.

In the absence of this information, it may perhaps be allowable to throw out a conjecture with regard to the oscillatory movements, which were going on upon the earth's surface during the period when the Megalichthys lived;—to movements by which land rose from some large lake, only to be again submerged, and by which alternations of submersion and emersion were often repeated. During such a state of our planet, lungs might have been given to sauroid fish, with the view of enabling them to support for a continuance the changes of element to which they were liable;—that when they were left dry, they might be enabled to maintain life until again floated,—either by rains, or by the renewed effect of geological agency.

There is also another circumstance to be kept in view, namely, that as the remains of the Megalichthys are found in bituminous shale, and even in coal itself, it is evident that the animal must have frequented shallows, and wet marshes, among which he would have been liable to be left dry, and consequently to perish, unless for the provision of lungs.

That fish with analogous habits exist at the present day, may be learned from Dr HAMILTON's account of the scaly Cobojius of India, which lives among the extensive marshes of the Yasor district. This fish is possessed of great tenacity of life in the open air; he is enabled to live many days without water; and he is even endowed with a considerable facility of progressive motion on land.

NOTES TO SECTION X.

A speculation has been recently thrown out, which connects itself rather with the character of the watery fluid, than of the atmosphere in which these sauroid fish lived. Upon this subject, M. AGASSIZ has remarked, that if structure be indicative of habit, this sauroid tendency of the fish of the limestone of Burdiehouse will lead to further discoveries of the aqueous fluids of the globe, which were neither salt nor fresh; as, by the character of the vegetation of remote periods, naturalists have been led to deduce a difference in the gaseous constitution of the atmosphere.—*(Report of the Edinburgh Meeting of the British Association of Science.)*

With regard to this observation I would suggest, that the argument regarding a half-saline state of waters, considered as prevailing over the whole ancient surface of the globe, is unsatisfactory, so long as it can be shewn that two distinct species of deposits, one apparently fitted for marine, and the other for fresh-water life, have existed. Proofs of this coexistence form the leading object of investigation in the present memoir.

And, with regard to a difference in the gaseous constitution of the atmosphere, when compared with its present state, I consider that such a difference might, to some little extent, have subsisted;—yet so little, as not to be capable of any geological proof whatever.

The argument for the air having been greatly charged with carbonic acid during the time when carboniferous strata were deposited, was first advanced by M. Adolphe Brongniart. Mr De la Beche, in adopting this view, has undertaken a sort of origin and history of this atmospheric diffusion.

Mr De la Beche's view is as follows: In a very early state of the globe, cracks or volcanic fissures in the earth's crust, formed the vents through which a large amount of carbon combined with oxygen, and, forming carbonic acid, made its constant escape into the atmosphere. The waters were so hot, that they could only absorb such portions of carbonic acid as were enabled to form compounds with the mineral matter rising above the level of the waters, and were sufficiently cool for the purpose. And hence it is argued, that, in consequence of the obstacles which would thus arise to the formation of carbonate of lime, at least in any very large proportion,—in consequence also of the elevated temperature of the water,—and in consequence of the difficulty of procuring disseminated oxygen, circumstances would arise unfavourable to the existence of numerous marine polypi, and shell-bearing animals, which require carbonate of lime in comparatively large proportions;—and thus, any carbonic acid thrown from the interior of the earth upon its surface, would, for the most part, remain in the atmosphere.

But, upon the earth becoming less heated, these conditions would necessarily change. The waters, in consequence of their being cooled down, would be enabled to take up more carbonic acid, which, when emitted from volcanic fissures, they would intercept. The carbonic acid, thus absorbed, would act chemically on many substances, and, among other actions, take up lime in solution, which, without this addition of carbonic acid, would have been insoluble in these waters; and hence, during this cooled down state of ancient seas, numerous marine animals would be called into existence, such as the shell-bearing Mollusca, Encrinites, and Corals, which would appropriate to themselves carbon; while an equal volume of oxygen would be liberated for their support, or would be thrown into the atmosphere.—De la Beche's Theoretical Geology, pp. 298, 308, 313.

This is Mr De la Beche's very rational theory. The question, however, still remains, whether the amount of oxygen thus liberated, would, after much of it had been arrested for the support of marine animals, form a residue capable, when thrown into the atmosphere, of contributing in any material degree to its purification. The theory shews little more than that any additional impurity would be checked, 1*st*, by the sea being so cooled down as to admit of an increased absorption of carbonic acid; and, 2*d*, by the requisition of carbon for the creation of marine animals which had been called into existence, whence the formation of calcareous deposits, produced by such animal agency.

M. Adolphe Brongniart has conceived that the atmosphere, after it had received its excessive proportion of carbonic acid, was purified by the development of the numerous plants, particularly of gigantic species, which were created during the carboniferous epoch, and for the support of which an extraordinary quantity of carbon must have been required.

All these views are unexceptionable so far as they go. There can be little doubt that the development of the abundant vegetation of coal-fields must have rendered solid much carbon which had existed in the atmosphere;—but, that this development was eventually, that is, during a still later formation of rocks, the cause of rendering the atmosphere fit for sustaining the respiration of reptiles possessing lungs, is another question; as we have no data whatever for ascertaining if such an amount of carbonic acid had subsisted, as to really render the atmosphere incompatible with reptilian vitality. On the other hand, the researches explained in the present memoir inform us, that an animal closely resembling the Lepidosteus, which M. Agassiz has proved to actually possess lungs, subsisted during a period when it was supposed that no animal possessing lungs could have possibly breathed.

SECTION XI.—SUMMARY OF THE EVIDENCE RELATIVE TO THE FRESH-WATER ORIGIN OF THE LIMESTONE OF BURDIEHOUSE.

The fresh-water limestone of Burdiehouse has at length been described, in which it has been shewn, that this bed is nothing more than an unit amidst many other fresh-water deposits, varying in their mineralogical character, and alternately deposited in the great Lowland basin of Scotland.

In concluding this memoir, I shall bring the fresh-water evidence to as severe a test as is compatible with geological reasoning, which must too frequently, in its very nature, be little more than analogical and circumstantial. In insisting upon a more rigid species of evidence, too many geological systems which we have carefully reared, would disappear " like the baseless fabric of a vision."

The fresh-water evidence of the Burdiehouse limestone has hinged upon various points.

The first has been the absence of all mollusca and conchifera of acknowledged marine origin.

In recent times, the conchifera and mollusca of marine origin, as well as of fresh waters, are distinctly recognised. We never witness seas inhabited by successive races of unios, or fresh-water rivers filled with a succession of coralline substances. If proofs have been brought forward that an interchange of element among animals dwelling in seas, or in fresh-water lakes or rivers, is possible, it is certain that such an existence could not long be maintained with immunity. Vitality would be rather tolerated than fostered, and this would be shown in the gradual diminution and eventual obliteration of the races of animals thus subjected to an interchange of element,—which fact at least indicates, that mollusca and conchifera, in their distribution, bear reference to different mediums of saline and fresh water for their prolonged support.

This principle I have analogically applied to the limestones of the carboniferous epoch. If we find that marine mollusca and conchifera are present in most limestones in the vicinity of Edinburgh, yet are absent in the limestone of Burdiehouse, to what other conclusion does this absence point, but that a condition of waters had prevailed, during the time of this deposit, unfavourable to the vitality of marine animals? And hence the supposition, that this condition was a difference of element, similar to that which still prevails, namely, fresh water, hostile to the prolonged existence of pelagic conchifera and mollusca.

A second point upon which the evidence of the fresh-water origin of the Burdiehouse limestone has hinged, is circumstantial rather than analogical.

The circumstantial evidence is to the following effect:—That the calcareous deposit of Burdiehouse must have taken place in a depression, or basin, perfectly surrounded with a dense vegetation, which has been washed into inland waters. But this circumstance would of itself prove little, as we may easily suppose that an estuary, or arm of the sea, might have stretched through a tract where a dense vegetation has prevailed. But when, in connection with a perfect absence of all acknowledged marine remains whatever, we find plants enclosed in a calcareous deposit in the greatest profusion imaginable, what conclusion remains, but that such a deposit is more indicative of a fresh-water river or lake, than of a sea or estuary?

Other points of evidence are perhaps less forcible, although they all tend to the same conclusion.

We find, for instance, that if the inland waters which deposited the limestone of Burdiehouse, were actually unfavourable to the existence in it of acknowledged marine mollusca or conchifera, they were not unfavourable to countless myriads of entomostraca, one genus of which, the Cypris, is the recent inhabitant of fresh-water marshes; and it may be fairly suspected that other genera associated with it were equally so.

I will not at present dwell upon the presence of the fish discovered in the Burdiehouse limestone, although some of the genera are found enclosed not only in such sandstones and shales as are crowded with plants, but even in coal itself. I shall hereafter shew, that if their presence does not afford an analogical proof, it is at the least circumstantially presumptive, that the Burdiehouse limestone has had a fluviatile origin.

In short, the evidence, in a general point of view, leads to the following conclusion.

There are, it is well known, numerous deposits of limestone belonging to the carboniferous group of rocks, in which, to the exclusion of any vegetables of a Tropical Flora, nothing but marine products, such as corallines, or acknowledged shells of genera hitherto found in open seas only, have been discovered. With a calcareous deposit of this kind, the comparison of another calcareous deposit, destitute of all corallines or marine shells whatever, yet containing, in an abundance perfectly remarkable, the plants of tropical marshes, along with the entomostraca of marsh waters, can surely lead to no other conclusion but the following:—That while the first formation must have taken place in a pelagic bed analogous to one of recent times, the other must have been the result of a fresh-water deposit, which, while it was no less hostile to the growth and increase of marine shells or corallines, must have flowed through marshy tracts, wherein grew all the plants observable in our coal-fields. Accordingly, in comparing the limestone of Burdiehouse with such a limestone as we find at Aberlady, which appears to be formed of one almost uninterrupted aggregate of coralline productions, or with the contiguous encrinitic limestone of Gilmerton, or with a zone of limestone filled with numerous varieties of marine remains, which crops out in a line of nearly SS.W. to NN.E., from Bathgate to Linlithgow, what doubt can remain, that while the limestone of Burdiehouse is of fresh-water origin, that of the other sites enumerated must be of marine origin?

But it is not with such unequivocal marine deposits, that I would dwell long for a comparison, as the line of demarcation is but too obvious. I would prefer drawing a line of difference with such a limestone as I have studied in Derbyshire, which is illustrative of an estuary.

The lowest visible portion of the great mass of Derbyshire limestone may be seen at Sherbrook, near Buxton. It is very crystalline, unstratified, and it contains no organic remains whatever. In the beds above it stratification commences, and the limestone encloses numerous encrinites, corallines, producti, &c. In still higher beds, such as we find at Ashford, indications are rather afforded of a marine estuary, than of an uninterrupted or open sea. Although this limestone abounds with acknowledged marine productions, among which corallines are pre-eminent, I discovered that in some sites the plants of coal-fields were mixed with them, although very sparingly. Among various plants which I collected, one of them was the Sphænopteris artemisiæfolia of Adolphe Brongniart *(See Pl. 46. of his "Histoire des Vegetaux Fossiles.")* Other species, some of them new, and perhaps of new genera, remain to be described. I also obtained Ichthyodorulites, along with palatine and incisive teeth, severally referable to M. Agassiz's family of Cestraciontes. At the same time, I did not find in the limestone any entomostraca, or any remains of smaller fish whatever, as at Burdiehouse.

In comparing, then, the limestone of Burdiehouse with that of Ashford, it will be found that most assuredly these two approach the most in character to each other. Yet still, in forming a table of comparison, such as the following, which I offer from my own personal observation, the line of demarcation will be sufficiently apparent. For instance,

The Limestone of Ashford contains	The Limestone of Burdiehouse contains
1. Encrinites, Corallines, Producti, Orthoceratites, &c. in infinite abundance.	1. No marine remains whatever.
2. Plants, which are found very sparingly, and in a few sites only.	2. Plants in the greatest possible profusion.
3. No Entomostraca whatever discovered.	3. Entomostraca, in such abundance as to occasionally impart to the limestone an oolitic character.
4. No remains of small fish.	4. Numerous small fish, as the Palæoniscus of coal-fields, &c.
5. Remains of fish referable to the family of Cestraciontes (Agass.). Genus not yet ascertained.	5. Remains of fish referable to the family of Cestraciontes (Agassiz); *e. g.* the Gyracanthus formosus.
6. Other animal remains, which have been referred to Saurian reptiles. Their true character is hitherto unknown.	6. Remains of large Sauroid fish; *e. g.* the Megalichthys.

The foregoing table of comparison, which, for the sole purpose of instituting, I revisited Ashford in Derbyshire, will, I trust, be regarded as important. From a consideration of it, the following inferences may arise:—

1*st*, While the great abundance in which Corallines, Encrinites, Producti, or Orthaceratites, are found in the limestone of Ashford, prove that it was unequivocally a marine formation, the absence of all such remains in the Burdiehouse deposit shews, that its waters were unfavourable to the manifestation of marine mollusca and conchifera.

2dly, While the comparatively sparing quantity of plants to be found in the limestone of Derbyshire rather indicates that they had been washed into an estuary, or diffused through a sea near the influx of rivers, the immense abundance of plants contained in the Burdiehouse limestone, prove the actual flowing of some river, or fluviatile expanse, through a marshy tract, in which ferns and lycopodiaceæ flourished.

The two foregoing points of difference form the great grounds of distinction between the limestone of Ashford, considered as the deposit of an estuary, and the limestone of Burdiehouse, considered as a pure fresh-water deposit. Other circumstances point to similar grounds of distinction, though perhaps less decisive.

3dly, While the limestone of Ashford contains no entomostraca whatever, they are found in such abundance in the limestone of Burdiehouse, as to occasionally impart to it an oolitic character.

Now, the presence of these entomostraca can scarcely be considered decisive so long as we know that marine waters can exhibit Cythereæ, and that the limestone of Burdiehouse encloses many different kinds of entomostraca. The circumstance, however, of the Cypris being found in the Burdiehouse limestone, is assuredly presumptive of this deposit having been formed among the stagnant fresh-waters of ancient marshes.

4thly, While the Ashford limestone encloses no remains whatever of such small fish as are abundant in the sandstones, bituminous shales, or bands of limestone, which contain plants and fresh-water unios, but in which all marine remains are absent, these fish appear most abundant in the limestone of Burdiehouse, presumed to be of fresh-water origin.

This circumstance, also, considered as an argument, is not free from ambiguity. Although I conceive that, during the carboniferous epoch, smaller fish dwelt more in fresh-water lakes or rivers, whither they were pursued by larger finny monsters, I should be sorry to affirm that they might not frequent estuaries, or arms of the sea. One genus of Palæoniscus is found in the limestone of Burdiehouse, and another in the later formation of the magnesian limestone,—which last, I presume, is of marine origin.

5thly, Remains of fish referable to the family of the Cestraciontes (Agass.), appear both in the estuarian deposit of Ashford, and in the fluviatile deposit of Burdiehouse. Now, the appearance of these large finny monsters in each formation, at least countenances the opinion which I have advanced, that estuaries, as well as fresh-water lakes or rivers, were visited by them in quest of their prey.

At the same time, the genera of Cestraciontes found at Ashford and at Burdiehouse, are not the same. During the spring of the present year, I procured large bony rays, as well as large palatine and maxillary teeth, from the limestone of Ashford; but the bony rays widely differed from the character of such as have been referred to the Gyracanthus formosus of Burdiehouse.

6thly, While the limestone of Burdiehouse encloses remains of the immense and scaly Megalichthys, it is only a probable conjecture that similar relics might have been discovered at Ashford. (See the Notes to Section XII. of Part. I.)

It follows from these premises, that, while a limestone which contains marine mollusca, &c. and, along with these remains, the plants common to coal-fields, together with the entomostraca of marshes, may be regarded as an estuarian limestone, other circumstances, although they may point to the same conclusion, are either less decisive, or ambiguous.

As for the remains of Cestraciontes, (and perhaps of the Megalichthys,) which appear in more than one description of carboniferous limestones, they point to estuaries, no less than to fresh-water lakes, as having been in primeval times frequented by large animals in quest of their prey.

The foregoing researches shew,—

1*st*, That it is by the presence or absence of acknowledged pelagic mollusca, corallines, &c. that indications are afforded of the great difference between fresh-water and marine deposits.

2*dly*, That if, along with marine mollusca or corallines, we find the plants of coal-fields in a quantity comparatively small, an estuarian limestone may be inferred. And,

3*dly*, That if marine mollusca or corallines should be entirely absent in a limestone, and if plants should be abundantly found in it, an indication would be afforded of a calcareous deposit which took place amidst the fresh-water rivers or lakes of primeval marshes; which indication would be still more favoured, if we should find, in addition, recognised genera of fresh-water shells, the entomostraca of stagnant marshes, or the fish incidental to coal-fields.

It is however admitted, that the presence of fish is an ambiguous criterion. The smaller fish of lakes, or rivers, are known in recent times to venture into estuaries, while larger fish enter fresh-water rivers, or lakes, in pursuit of their prey.

From these conclusions it is evident, that while a broad line of demarcation subsists between marine and fresh-water limestones, it is by no means impossible that limestones of an estuarian and fluviatile formation may, in some cases, be more difficult to distinguish, and particularly, when it is kept in view, that, from a remote period of the globe, communications between rivers and seas must have subsisted, as at the present time. Thus, during the actual state of our globe, the Ganges, as we approach close to its source, teems with fresh-water animals exclusively ; but, in following it to its junction with the sea, marine products, even far from its mouth, begin to appear. Yet the Ganges, notwithstanding any partial ambiguity of its products, possesses not the less a fresh-water character.

Hitherto, however, I have not found the slightest traces of marine mollusca or corallines in the limestone of Burdiehouse; and hence, I am not induced to consider it as any thing but a pure lacustrine formation; and if any marine remains should in future turn up, which I do not expect, they would be so small, in comparison with the very abundant indications afforded of an opposite state of the waters which had yielded this deposit, as to merely indicate its original connection with an ancient sea, and nothing more;—which last supposition I have, from a different description of evidence, never been once inclined to doubt.

That seas should have been actually synchronous with a fresh-water deposit, is indeed, as I have shewn, what might be naturally expected. Few rivers or lakes subsist, which do not maintain such a communication. I am, therefore, prepared to expect, that the remains of such large fish as are found in fresh-water deposits, may also be discovered in marine deposits, analogous, in fact, to what takes place at the present day in numerous parts of the globe, where we find the fish which inhabit seas penetrating many hundred miles up large rivers in quest of their prey, as, for instance, towards the source of the Ganges.

It is also very probable, when we consider the frequent oscillatory movements to which the crust of the earth was exposed during the carboniferous epoch, that the fish which were then created might possess an adaptation of animal structure calculated to enable them to sustain any occasional change of element caused by terraqueous elevations or depressions; for M. AGASSIZ states, that, among the fish of the earlier formations of strata, he has not observed those marked characteristics of structure, which, in later times only, point out these animals as distinct inhabitants of salt or of fresh water.

These considerations prove to us, that if we would distinguish between the marine and fresh-water formations of very early epochs, we must form our judgment less upon any insulated relics which may turn up, than upon an assemblage of organic remains, considered in reference to a combination of geological circumstances.

I shall, lastly, endeavour to sketch the true character of the limestone of Burdiehouse, in reference to the beautiful speculations of Mr de la Beche, upon the quietness of deposition incidental to the formation of more ancient strata.

Springs charged with the carbonate of lime, and issuing from profound crevices incidental to one of the more early fissured states of the earth's crust, had mingled their mineral contents with the waters of some river, or of some fluviatile expanse, which had sluggishly flowed through a marshy tract, principally overrun by the creeping and gigantic stems of the mysterious Lycopodiaceæ, and by a dense undergrowth of ferns, among which the luxuriant growth of the Sphenopteris affinis was particularly favoured. And hence the production of a calcareous deposit, which, in gradually and tranquilly congealing, had preserved to plants possessed of such tender form and structure as the lesser ferns, all the delicate divisions of their pinnæ, or pinnulæ, as well as all the slender and linear character of their lobes, unaffected by the violence of currents, or by any of the atmospheric commotions which a later and less heated condition of the globe has invoked.

So great, in fact, is this state of preservation, that we are irresistibly carried back to a period, when, in conformity with the distribution of a thermal ocean over various parts of the globe, equatorial and polar states, by being rendered uniform, would so adjust the temperature over the whole surface of our planet, as to induce a quietness of deposition, which no formation perhaps more happily elucidates than THE FRESH-WATER LIMESTONE OF BURDIEHOUSE.

NOTES TO SECTION XI.

Having at length carefully drawn every close and tedious distinction of which I conceived a fresh-water question of this kind was susceptible,—more, in fact, to combat the objections with which I have been assailed, than to aid in my own conviction,—a perfect weariness of fresh-water deposits, and of ancient lakes or rivers, has more than once tempted me to exclaim, in too literal a strain, with the Mantuan poet,

Claudite jam rivos, pueri : sat prata biberunt.

But this long discussion, in which I have embarked relative to the fresh-water origin of the limestone of Burdiehouse, has not been uncalled for. I have been desirous that my memoir should embrace the confutation of every charge upon which I have been attacked. I am unwilling to allude to any particular attack, but as silence has but too often been interpreted as an admission of defeat, I must impose upon myself the very annoying task of noticing a few animadversions.

The first objection strongly urged against me was that the limestone of Burdiehouse had not a fresh-water but a marine origin.

With regard to this counter opinion,—after the numerous facts which I have brought to bear against it,—I will not again combat the objection, that the deposit of Burdiehouse was less under the dominion of the Naiads than of Neptune; nor will I disturb the opinion publicly expressed against me by the President of the Edinburgh Wernerian Society, for whose old Neptunian predilections I can make every allowance.

In the second place, my account of a lacustrine limestone existing among the carboniferous group of rocks was arraigned on the score of originality.

In order that the force of this charge may be properly estimated, some previous explanation may be necessary.

Various thin seams, named bands of fresh-water limestone, are common throughout the whole of the coal-fields of Scotland, and scarcely need be enumerated. Many of them contain fresh-water Unios. Seams of this kind I have observed near Old Cumnock, and other places.

These bands of limestone cannot fail to be familiar to every geologist who has studied coal-measures. From their having come within my notice, I conceived, as well from this as from other analogies to which I have already alluded, that the existence of considerable beds of fresh-water limestones, to which the name of bands could not apply, was a rational expectation. That my expectation has not been disappointed, the present researches have, I trust, satisfactorily proved.

In reference to one of these bands of limestone, the originality of my discovery has been arraigned upon the strength of a passage, which appears in the Proceedings of the Geological Society of London, by Mr Murchison, incidental to an account of the coal-fields in the neighbourhood of Shrewsbury. It is stated, that " three thin beds of coal are for the most part observable, and the deposit is distinguished by an included *band of limestone*, similar in mineral aspect to the lacustrine limestones of central France, and containing minute shells, referable to fresh-water genera."

Mr MURCHISON has publicly pointed out to me this passage, which, from my residence abroad, I had not seen, but which I am sure, if I had read, would have escaped my recollection, from its apparently trivial importance.

The reply which I have to make is, that if my discovery of extensive beds of lacustrine limestone is to be superseded by mere bands of fresh-water limestone, Mr MURCHISON himself has been anticipated by other writers. The Rev. Mr URE, in his History of Rutherglen, written at least forty years ago, states, in reference to certain fresh-water muscles, or unios, enclosed in a band of limestone, that " the shells were commonly entire, and were *probably produced in fresh water*."—History of Rutherglen and Kilbride, p. 311.

Granting, however, that Mr MURCHISON's limestone is thicker than what is usually meant by a *band* of limestone, the various limestones which I have enumerated are not, like his, contained *within* beds of coal, but, on the contrary, form considerable deposits *beneath* beds of coal. This difference has been remarked by Mr DE LA BECHE.—*Geological Researches*, p. 319.

But waving these very subordinate considerations, I shall state in my own defence, that my memoir was drawn up at a time when many British geologists were inclined to admit, with a writer of the first weight, that no instance had yet been discovered of a pure lacustrine formation of the carboniferous era; and that although there were some instances of shells, apparently fresh water, which might have been washed in by small streams, they did not by any means imply a considerable extent of dry land. (*See LYELL's Principles of Geology*, 1st Ed. vol. i. p. 130, and vol. iii. p. 15.)

My own discovery, however, of the fresh-water character of the limestone of Burdiehouse, in conjunction with that of other similar limestones of still greater thickness and extent, viz. at Calder and elsewhere (See the Supplement to this memoir), have, I trust, tended to set geologists right upon the question of pure lacustrine deposits existing during the carboniferous epoch.

But after all,—these disputed claims of originality may neither belong to myself nor to any other British geologist. The far older speculations of M. DELUC and of M. ALEXANDRE BRONGNIART, refer all the varieties of strata comprehended in coal-measures to a lacustrine origin; while their theory upon the alternation of these strata with marine deposits has, in some few respects, coincided with the principles which I have sought to establish in the present memoir.—(*Tableau des Terrains, par BRONGNIART*, p. 280, &c.)

With a third charge I was industriously assailed during the meeting of the British Association of Science at Edinburgh. It was contended that I had given an erroneous position to the limestone. My present memoir will however show, that the view which I originally entertained on this question remains unaltered. I went over the ground with some of our most distinguished geologists, among whom was Dr

Buckland, who compared very critically the section which I had given, with his own observations made upon the spot.

Dr Buckland likewise examined with the greatest attention the Roslin, or Hawthornden sandstone, in order to set himself right with regard to a notion entertained, of its belonging to the new red sandstone, rather than to the coal-formation; an objection, which it was supposed (though I could not myself see the force of it) would materially affect the position which I had assigned to the limestone of Burdiehouse. Accordingly, in conducting Dr Buckland to the grounds of Hawthornden, with the view of examining the best natural section which could be afforded of the rock in question, we were so fortunate as to meet with the outcrop of the Jewel-coal resting conformably upon the Hawthornden sandstone. The reference, therefore, of this deposit to any other formation, except that of the carboniferous group, may be considered as set at rest. The opinion which I have myself expressed is, that the Roslin sandstone may possibly be found to rank as one of the more ancient members of the carboniferous group; and if this geological position be confirmed, it would make the Stoneyhill coal, in which the remains of large sauroid fish have been abundantly found, an older bed than is usually supposed. But this suggestion I throw out, with submission to another judgment of no little weight. No one is better acquainted with the strata of the Scottish coal-fields than Lord Greenock, to whom the merit of discovering the animal remains of the Stoneyhill coal is due.

In the incidental allusion which I have made to this nobleman, I hope for indulgence in the opportunity thus afforded me, of rendering justice to my own feelings, in acknowledging the very kind encouragement which I have uniformly received from his Lordship's hands, during the course of these researches. A zeal, such as Lord Greenock evinces, in the science which he himself so successfully prosecutes, affords an example, when united with rank, too illustrious not to render it an object of popular imitation. Nor can the example fail to render the most essential services to the cause of geology in Scotland.

CONCLUSION.

In concluding this long memoir, I should be most ungrateful not to acknowledge the very deep obligations which I have been under to the Royal Society of Edinburgh, for their flattering attention, as well as other marks of kindness.

I also feel proud in the opportunity of having brought forward these researches before a Society, whose earlier members, now no more, are associated with the brightest period of the history of Geology. The appearance of a Hutton in the field, gave token of an emergence from the darkness which pervaded the geology, not of this country alone, but of every region in Europe. This was followed by the exertions of a HALL and of a PLAYFAIR, who, in their days, were towers of strength. The researches of these philosophers, together with the important labours of some few of their Neptunian competitors, which, though conducted upon erroneous and illogical principles, embodied invaluable facts, reflect a high lustre on the active interval of Scottish geology, which has prolonged itself to the date of the important writings of a MACCULLOCH and a BOUÉ. If an interval of inertness has followed, Industry now promises to resume her sway. The exertions of the Highland Society, conjoined with the late proceedings of the British Association of Science, severally shew, that the Genius of Scottish Geology is awakening from his slumbers, like a giant refreshed.

SUPPLEMENT.

The great extent to which my Memoir has reached upon the fresh-water limestone of Burdiehouse, precludes me from doing any thing more than throwing into the form of a Supplement, notices of other fresh-water limestones in the vicinity of Edinburgh, brought before the Royal Society, although I possess materials for a description of them, almost sufficient to fill a volume.

It was a desideratum of no little moment to learn, whether fresh-water limestones, similar in their geological character to that of Burdiehouse, did not occur in other localities of the coal formation of Scotland, or even elsewhere.

Having this object in view, I did not content myself with merely visiting the neighbourhood of Edinburgh; I travelled at different intervals several hundred miles over various districts of the Basins of the Forth, the Tay, and the Clyde, and have even availed myself of the occasion which a journey to England afforded me of exploring certain parts of Derbyshire, with the self same view. My labour has not been directly, but indirectly successful. If I have not found these fresh-water limestones so abundant as I expected them, I have still been enabled to assign to them a few localities, and the acquisitions which I flatter myself it will be hereafter found I have made to our knowledge of the general relations of the strata of the south of Scotland, conjoined with the discovery of a fresh-water bed of rather a different character, but of an interest fully equalling that of Burdiehouse (I allude to the limestone of Kirkton near Bathgate), far more than recompense me for toils which have been unremitted.

1st, The Fresh-Water Limestones of Calder.

At East Calder, and to the south-west of Mid-Calder, the limestone which is there quarried appears, like that of Burdiehouse, to have a fresh-water origin. Its strata have undergone great derangement, and dip in various directions. In one of the quarries of East Calder, where a good section is exposed, the lowest rock is said to be sandstone, above which the following strata may be enumerated in an ascending order:—A yellowish coarse limestone, 16 feet thick;—limestone, 43 feet thick, in which vegetable remains are contained, such as are usually found in coal-fields, and, along with these, scales of the Megalichthys have been discovered;—nine feet of a very bituminous shale, part of which burns readily, mixed with ironstone;—shale (named Blaes) 16 feet;—and, at the top of the series, an alluvial covering of clay, sand, &c. in which large boulders occur.

2d, *The Fresh-Water Limestone of Burntisland.*

A very deep-seated bed of fresh-water limestone is to be found in the road from Newbigging to Burntisland in Fifeshire. It also crops out on the north of the Bin Hill. It is comparatively a small bed, being at Newbigging not more than eighteen feet thick. Plants as well as Coprolites are contained in it.

I have collected numerous details regarding this very deep seated deposit, but, as I have not space to enter into them, I shall merely remark, that its geological situation appears in a section of the beds exposed between Burntisland and Seafield town, which has been published by Dr Boué. This excellent geologist did not lose sight of the important fact that the limestone of Burntisland contained the remains of plants. (*See Essai Géologique sur l'Ecosse, par A. Boué*, p. 472.)

3d, The Fresh-water Limestone of Kirkton, near Bathgate, indicative of ancient Thermal Waters.

I regret most exceedingly, that I have only space to give a mere outline of this very interesting deposit.

From Bathgate to Linlithgow, a distance of about six miles, a long line of fissure extending from SS.W. to NN.E. may be traced, by which deep seated beds of limestone are brought to view. Near Linlithgow an eruption of trap occurs, but as outcrops of limestone reappear in the noted quarry of Limekilns in Fifeshire, near the seat of the Earl of Elgin, it is probable that the line of fissure has been prolonged across the Firth of Forth in the same direction of NN.E.

A mile or two to the east of Bathgate, at Kirkton, we find that a very considerable outbreak of greenstone has occurred. Close to it on the west appears the limestone of Kirkton. By this contiguity we are assured, that the limestone must have been elaborated within the immediate sphere and influence of an extensive volcanic eruption. The consequence has been, that one of the most unique formations of which Great Britain can boast, has been formed, indicative of thermal waters belonging to the carboniferous epoch.

A decidedly fresh-water formation is there exposed, which is characterized by the absence of all marine shells, corallines, &c., and the presence of the well-known vegetable remains of the coal-formation.

But the remarkable circumstance in this limestone is its mineralogical character, indicative of the very powerful chemical action under which it was elaborated.

This chemical action appears to have been so energetic, as to have caused such miscellaneous earthy matters as are found to enter into the composition of an impure limestone, like that of Kirkton, to separate into laminæ, and to assume a sort of striped disposition, (*rubané*, as it is also named), resembling what I have occasionally noticed in Auvergne, where tertiary strata have come into contact with volcanic rocks. The strata, for instance, of Kirkton quarry are composed of distinct and alternating thin laminæ, some of them being of remarkable tenuity, variously consisting either of pure calcareous matter, of translucent silex, resembling common flint, or of a mixed argillaceous substance, which approaches to the character of porcellanite, or of ferruginous, or even of bituminous layers, originating probably from vegetable matter.

Upon one of these very thin aluminous folia, which I have compared to porcellanite, I observed the impression of a Fern, apparently of a Pecopteris, which was delineated upon it like a painting upon porcelain.

It is also remarkable, that the mixed ferruginous and carbonaceous layers which enter into the structure of the limestone, often assume a sort of blistered appearance upon their surface, as if from the effect of heat.

Another effect of ancient thermal waters, may be inferred from the very singular warping which the strata have undergone. The wavings which they exhibit are extremely remarkable upon a large scale, and it is no less surprising that the same contortions should be evinced, even to a far greater degree, in small hand specimens of the limestone. That this effect has been aided by a lateral as well as superjacent pressure of eruptive rocks of volcanic origin, is highly probable.

Some part of the limestone, again, shews other species of character. I collected a specimen, in which a structure, globularly concretional, was observable, the surface having, at the same time, a mammillary appearance. This is another proof of the powerful chemical action which was excited when these limestone beds were formed.

All these appearances tend to the hypothesis, that the calcareous beds of Kirkton were elaborated under the action of great heat; or, in other words, that they had their origin in deep fissures, intimately connected with a volcanic focus.

But this view does not, in fact, demand any hypothesis whatever. An interposed mass of volcanic tufa, of a green colour, which occasionally assumes the compactness of greenstone, is developed among the higher beds of the deposit. It has an elongated, wedge-shaped appearance, and it is extended in a direction nearly conformable to the general bearing of the strata. The greatest thickness which it exhibits may be estimated at two to three yards, but in tracing it to the part of the quarry where it disappears, it may be observed to thin off to as many inches.

The inference is, that the calcareous matter was elaborated on the site of Kirkton in the form of hot springs, probably at the time in a state of ebullition. I need not add, that this is a phenomenon perfectly familiar at the present day in districts where the volcanic agency is still in activity.*

This limestone, as I have remarked, contains plants, among which is a beautiful Pecopteris, which I have transmitted for description to Professor LINDLEY and Mr HUTTON, to be inserted in their British Flora.

It could scarcely be expected that this limestone should contain any animal remains, but, to my surprise, some most singular creatures have turned up

No fewer than five specimens were at one time discovered of a crustaceous animal of extraordinary large dimensions. Unfortunately, these specimens in their broken state were soon dispersed. A very able naturalist of Glasgow, Dr SCOULER, at present Professor of Mineralogy in the University of Dublin, obtained the sight of a portion only of one of these animals, which he described under the name of Eidothea. (*Edinburgh Journal of Natural and Geographical Science*, vol. iii. p. 352.)

When my account of the limestone of Kirkton was made known through the journals, public attention was directed to its singular character, and, as a consequence, more of these dispersed remains were brought to light, particularly by Dr REID and Dr SIMPSON, severally of Bathgate. The last named gentleman allowed me to exhibit the relics in his possession at a Sectional meeting of the British Association of Science; and Mr SMITH of Jordanhill then brought forward the head of the animal described by Dr SCOULER.

In Plate XII. figs. 1. and 2. shew both sides of the animal. Fig. 3. is the head alluded to; while figs. 4. and 5. are the two sides of an extremity, apparently referable to the same specimen.

Upon this occasion Mr TORRIE of Edinburgh was so obliging as to place in my

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* In concluding my mineralogical account of this deposit, I would observe, that my intelligent young friend, Dr SIMPSON of Bathgate, was the first to inform me, after the reading of my paper, that the limestone of Kirkton had been noticed in an article by Dr FLEMING, published in the Edinburgh Journal of Science for April 1825, (p. 307.) In this paper, a brief allusion is made to the siliceous laminæ of the limestone, as well as to its botryoidal and mammillary structure. It is also stated, that "several trunks of trees with their branches" had turned up, in which concentric zones and perpendicular fibres were visible. Dr FLEMING's remark, that this limestone "encloses the remains of those marine animals which are common in the limestones of the coal formation," I consider as a mistake. The memoir is entitled, "On the Neptunian formation of Siliceous Stalactites." It is almost entirely theoretical, being a defence, in reference to siliceous developments, of the doctrine taught by WERNER.

I may also add, that since an abstract of this memoir appeared in print, Mr MACLAREN of Edinburgh has published (in the Scotsman) some very ingenious observations upon the trap-rocks in the immediate vicinity of this thermal deposit, in which their stratified character is advocated. I am sorry that the extreme length of my memoir prevents me from entering into an explanation of his views, and from expressing my own opinion regarding them.

hands Dr HARLAN's account of the Eurypterus of North America, to which the Kirkton specimens were readily referred

The Eurypterus is assigned by Dr HARLAN to the class Crustacea; and to the order of Branchiopoda. His description of the genus Eurypterus is as follows :—

Character of the Genus.—" Caput a thorace non distinctum : os ignotum : oculi duo, sessiles, distantes, lunati : abdomen elongatum, posticam versus extremitatem sensim gracilius, segmentis transversis subimbricatis divisum. Pedes octo ; duo utrinque antici branchiferi, duo utrinque postici maximi, omnes lamellosi."

Dr HARLAN's specimens were obtained from a " transition calciferous sand rock" of Westmoreland in the Oneida county of New York. He has described two fossil species, the Eurypterus lacustris and the E. Remipes. (See Plate XII. fig. 6. and 7.)

The head of the Eurypterus of Kirkton (see Plate XII. fig. 3.), which differs in many respects from the American specimens, exhibits a physiognomy so human like, that the quarrymen attributed it to some degenerate offspring of Adam. For this reason, as well as in reference to the sort of examination which it seemed to invite, the animal might have been stiled the Eurypterus phrenologicus.

A more suitable specific title is, however, suggested. In honour of the naturalist who has described in a satisfactory manner the only fragment, namely, the head and a small portion of the abdomen, which came under his observation, I propose, to name the Eurypterus found at Kirkton, the EURYPTERUS SCOULERI.

I consider the Eurypterus Scouleri to be distinguished from other species, by the prolonged eminences intervening between the eyes, which at their apex form an angle wherein appears a central tubercle ;—also by the small acutely angular protuberances, like spines, which are diffused over the surface of the head beneath the eyes. The character of the feet cannot be given, as no vestiges of them, except very slight ones, have turned up *. Perhaps, a last distinction is the greater size of the E. Scouleri, which may be learned from the following table of comparison :

	EURYPTERUS LACUSTRIS.	EURYPTERUS REMIPES.	EURYPTERUS SCOULERI.	
			Larger Specimen.	Smaller Specimen.
Length of the head,	$1\frac{1}{4}$ inch.	1 inch.	6 inches.	5 inches.
Breadth of the head,	$2\frac{1}{4}$ ...	$1\frac{1}{2}$ ...	8 ...	$6\frac{1}{2}$ ...
Distance between the eyes,	1 ...	$\frac{3}{8}$ ...	$1\frac{1}{3}$ ...	$1\frac{1}{10}$ ...
Breadth of the body,	$2\frac{5}{8}$ ...	$1\frac{5}{8}$ ...	8 ...	5 ...
Total length,	5 ...	$3\frac{3}{4}$ ...	17 ? ...	$13\frac{1}{4}$? ...

The Geological Relations of the Kirkton Limestone.—Above this deposit, we find beds of very loose friable shale with ironstone bands, some of which are alternated with its upper strata. These shale beds, composed of pure argillaceous matter, appear to have undergone few or no alterations from the action of heat.

* In concluding this account of the Eurypterus, I beg to express my best thanks to Dr SIMPSON of Bathgate, for giving me the opportunity of describing the specimens in his possession, as well as to Mr SMITH of Jordanhill, for the loan of the specimen in the Andersonian Museum of Glasgow.

The whole of the limestone strata dip generally towards the west, or north-west by west, at angles varying from 24° to 28°.

To the west of the limestone is much covered ground, but, as far as I could learn, the strata succeeding to this fresh-water deposit consist of alternations of sandstone and shale.

But, at a little farther distance, are limestone beds, containing marine shells, dipping in like manner to the west, and thus shewing that they are the uppermost, and, consequently, later strata.

The Kirkton limestone, in common with newer limestones of marine origin, appears to have not only undergone an elevation subsequent to the period when it was consolidated, but to have also had its tilted and upturned strata covered over with subsequent volcanic eruptions of felspathose trap. This trap is superimposed upon the limestone of several quarries extending from Bathgate to Linlithgow, and it occasionally assumes a prismatic form. More frequently, however, it appears under the character of a decomposed wacké. From six to fifteen feet of this substance, in perpendicular depth, repose upon the upturned edges of the Kirkton limestone.

This overlying mass must have originally covered a considerable tract of space in the form of a lava.

In concluding this account I must observe, that the limestone of Kirkton particularly recommends itself to the attention of all who are anxious to trace, in the various effects produced upon rocks, the influence of central heat. I have already transmitted specimens of this limestone to a distinguished philosopher, M. De Leonhard of Heidelberg, who, more than any other geologist, has successfully prosecuted this important branch of investigation.

Description of the Plates belonging to this Memoir.

Plate V. View of the old quarry of Burdiehouse. Described in Part I. section 17

VI. The fossil plants described in Part. I. section 3.

VII. The lesser fish. Described in Part I. sections 7. and 13.

VIII. The sauroid remains first discovered in the limestone. Described in Part I. sections 7. and 11.

IX. The large fossil teeth. Described in Part I. sections 7. and 11.

X. Part of the jaw and round scales of the Megalichthys. Described in Part I. section 11.

XI. The scales of the Megalichthys, and the dorsal rays of the Gyracanthus. Described in Part I. sections 7, 11. and 14.

XII. Specimens of the Eurypterus of Kirkton, &c. Described in the Supplement.

Analysis of Coprolites and other Organic Remains imbedded in the Limestone of Burdiehouse near Edinburgh. By Arthur Connell, Esq. F. R. S. E.

(*Read* 17*th February* 1834, *and* 19*th January* 1835.)

It is not my intention to enter into any detailed description of the external characters of the several interesting organic remains which are found imbedded in the limestone of Burdiehouse, that being a task which belongs to their distinguished discoverer Dr Hibbert.

As little do I mean to describe the geological relations of the limestone bed, that being equally the province of Dr Hibbert. It will be sufficient to state generally, that it forms one of the lowest members of the carboniferous group, being inferior in position even to the Encrinal mountain limestone of its immediate vicinity; and that besides its numerous animal remains, some of which will be mentioned in the sequel, it contains throughout its entire mass numerous impressions of land and fresh-water tropical plants.

The chemical constitution of certain of the animal remains is the subject which I have humbly undertaken to investigate. I shall first advert to the coprolites which are found in vast numbers imbedded in the limestone.

All the specimens of coprolites examined, appear to agree in the following general chemical characters.

When heated in a glass tube in the state of powder, they give moisture and bituminous oil; and test paper held in the tube shews the presence of a trace of ammonia. This reaction indicates the existence of a minute quantity of animal matter; and it is proper to observe, as I shall afterwards have occasion to notice more particularly, that the dark coloured limestone itself, in which the coprolites are imbedded, shews a similar reaction. The coprolites are soluble in dilute muriatic acid with moderate effervescence, leaving a residue of dark flocky matter; and when the solution is treated with ammonia, a plentiful gelatinous precipitate is thrown down, having all the appearance of phosphate of lime. Examined in the usual way for detecting phosphoric acid by means of potassium, they afford very decided indications of the presence of that acid. They also contain a little oxide of iron, but no sulphur in any state of combination. Magnesia was sought for both by fusion with carbonated alkalies and other necessary steps, in which way none was detected; and also by BERZELIUS's process for finding it in bones.* The ultimate ignited residue got by this latter method consisted almost entirely of siliceous and alkaline matter, with only a very minute quantity of magnesia. A trace of fluoride of calcium is easily detected by treating the coprolites with sulphuric acid in the usual way.† Phosphate of lime and carbonate of lime are thus the prevailing constituents.

I next proceeded to ascertain the proportion of these constituents; and, as it is stated by Dr PROUT in his analysis of the coprolites from the lias, that the proportions appeared to differ not merely in different specimens, but in different parts of the same

* Lehrbuch, iv. 445.

† In my first search for fluoric acid I merely employed the blowpipe test of fused salt of phosphorus and Brazil wood paper, with the delicacy of which, the alkaline reaction of the coprolites interfered. In the mean time, Dr GREGORY and Mr WALKER found fluoric acid, in the usual way, in a coprolite examined by them; and I have since found that, with due precaution, it may be detected even by the blowpipe test.

specimen, I wished to ascertain how far a similar variation might be observable in the present instance.

In selecting specimens for a fuller analysis, it appeared to me to be desirable, on the one hand, that such coprolites should be chosen as contained fish scales, because it is by such contents as these that the real nature of these fecal remains is best evinced; and, on the other hand, that the quantity of these contained remains should not be greater than was sufficient to afford decisive evidence of their presence, because the purity of the proper matter of which the coprolites consist would be in some measure affected by them. Two specimens were chosen, in one of which the imbedded coprolite was about 2 inches long by 1 inch broad, and in the other it was about $2\frac{1}{2}$ inches by $\frac{3}{4}$ broad. The colour of both was yellowish-white. Their texture was compact, with a conchoidal fracture. A few fish scales were scattered through their substance. 22.99 grains of the former coprolite were dissolved in diluted muriatic acid, and the loss of weight resulting from effervescence ascertained to be 1.07 grains. A good deal of brown flocky matter was left, which was separated by filtration, and dried at the temperature of 212°, when it weighed 1 grain. It then appeared as a black coally mass, which fused on the application of heat, gave a white inflammable vapour and took fire, and burned with flame, leaving a residue of silica amounting to .09 of a grain. This, in short, was bituminous matter, which, as will presently appear, had been derived from the limestone. The muriatic solution was then precipitated by ammonia, in a little excess, and the phosphate of lime collected by filtration, and ignited. It then weighed 19.56 grains. The remaining liquid was precipitated by oxalate of ammonia, and after calcining the precipitate under the usual precautions, 2.48 grains of carbonate of lime were procured, which contain 1.083 of carbonic acid, and thus correspond sufficiently with the loss of weight by effervescence.

We have thus in 22.99 grains,

Phosphate of Lime, with a little fluoride of calcium,	19.56
Carbonate of Lime,	2.48
Silica,	.09
Bituminous Matter,	.91
	23.04

In this analysis alkaline matter in the residual liquid was not sought for; but another portion of the same coprolite was decomposed for that purpose by muriatic acid, and after getting quit of the earthy constituents, a small residue of chlorides of potassium and sodium was got, amounting to .59 per cent. of alkalies, which probably were combined, in the matter under analysis, with siliceous matter, as chlorine or sulphuric acid were not detected in the coprolites.

The constituents in 100 parts were thus,

Phosphate of Lime, with a little fluoride of calcium,		85.08
Carbonate of Lime,		10.78
Phosphate of Magnesia,	Trace	
Silica,		.39
Potash and Soda,		.59
Bituminous Matter,		3.95
Animal Matter,	Trace	
		100.79

13.76 grains of the other coprolite were decomposed by the same process, the only difference being that the loss of weight by effervescence was not determined, and that the amount of the bituminous and alkaline matter was merely estimated from the deficiency on the analysis. This coprolite contained in 100 parts,

Phosphate of Lime, with a little fluoride of calcium,		83.13
Carbonate of Lime,		15.11
Phosphate of Magnesia,	Trace	
Silica,		.29
Animal Matter,	Trace	
Bituminous Matter and Alkalies,		1.47
		100.

A portion from a different part of this coprolite yielded 84.17 per cent. of phosphate of lime.

It thus appears that the proportion of phosphate of lime, the principal constituent of these remains, is pretty uniform, in so far as the examination extended. A little variation occurs in the relative quantity of the other constituents. Of these last, the bituminous matter is certainly derived from the matrix ; and it seems probable that the proportion of the carbonate of lime is also slightly influenced by the limestone, as I shall afterwards have occasion to shew. The proportion of phosphate of lime is much greater than in the lias coprolites, in which it appeared to vary, according to Dr Prout, from only $\frac{1}{4}$th to $\frac{3}{4}$ths of the whole; whereas in those of Burdiehouse it amounts to about $\frac{5}{6}$ths *.

On finding the bituminous matter in the coprolites, I subjected the limestone to the same process, to separate the bitumen from it. A portion of the limestone matrix of the coprolite first analyzed was dissolved in diluted muriatic acid, and the undissolved dark flocky matter collected on a filter, washed, and dried at the temperature of 212°. It then contained one-half of earthy matter, and hence was not so black and glossy as that from the coprolites, nor did it fuse on the application of heat ; but, when heated in the open air with the contact of flame, it took fire and burned with flame; and, when heated in a

* With reference to an analysis lately published by Dr Gregory and Mr Walker, (Edin. New Phil. Jour., Jan. 1835,) of a coprolite described as embedded in a rolled mass of clay-iron from Burdiehouse, it is necessary to observe, that this coprolite appears to have been extremely impure, containing only 10 per cent. of phosphate of lime, and much foreign matter, such as sulphuret of iron, and large quantities of carbonates of lime and magnesia. It is essential, therefore, to draw a marked line of distinction between coprolites in ironstone or shale, and those directly embedded in the limestone of Burdiehouse, to which last alone my analyses refer. I have seen coprolites from the shale of Burdiehouse, but I have never happened to see any from that locality, either in ironstone or in rolled masses.

tube, a yellow oil was sublimed, with a strong bituminous odour, and the oil concreted on cooling,—appearances which are also presented when the bituminous matter from the coprolites is treated in the same manner. The quantity of bitumen obtained from the limestone, after subtracting the earthy matter, was about 2½ per cent.; but the proportion may vary in different specimens. The bitumen might also be obtained by distillation; for when the powdered limestone is heated to redness in a glass tube, a brown oily liquid is volatilized, which concretes on cooling.

It may also be worth while to notice, that the matter composing the carbonaceous layers, covering many of the fine vegetable impressions of the limestone, possesses, in its relations to heat, all the properties of common black coal; and appears, in many instances, to be all that remains of those vegetables which have left their impressions on the matrix. Thus these miniature beds of coal afford an excellent illustration of the origin of their more gigantic likenesses in the coal-measures.

The next fossils from the limestone which I submitted to analysis, were a portion of a bony fin ray, sent to me for that purpose by Dr Hibbert; and also some scales obtained from the collection of the Royal Society.

Both these substances, as may well be supposed from their great antiquity, afford only slight traces of perishable animal matter. At a red heat, they give off a little water and ammoniacal vapour; and a slightly empyreumatic odour is also evolved, arising from a small quantity of bituminous matter contained in them. The alkaline reaction, however, particularly that of the bones, is not so decided as that of the coprolites or limestone matrix itself.

The details of the method of analysis need not be entered into, as the general nature of the process employed was the same as that used in the analysis of the coprolites.

The bony fin rays, Dr HIBBERT informs me, belong to a fossil fish, which has been designated by M. AGASSIZ, Gyracanthus formosus, and is supposed to approach in character to the Cestracion of New Holland.

20.14 grains of these rays were found to be composed as follows:

Phosphate of Lime, with a little Fluoride of Calcium,		53.87
Carbonate of Lime,		33.86
Siliceous Matter,		10 22
Potash and Soda, partly as Chlorides,		.71
Bituminous Matter,		.54
Phosphate of Magnesia,	Trace	
Animal Matter,	Trace	
		99.20

The siliceous matter contained in the fin rays was left undissolved when they were treated with the acid, and presented the appearance of small solid cylindrical masses, grouped longitudinally together. These siliceous portions may be observed in the solid bone itself, before it is acted on by the solvent, and evidently suggest the idea of longitudinal cavities in its substance, which had become filled with infiltrated siliceous matter.

The scales analyzed were those of the fossil genus of fish to which the name of Megalichthys has been given by M. AGASSIZ. The great fossil teeth found at Burdiehouse belong to the same animal. It is supposed to approach in its characters to the Lepisosteus, or Lepidosteus of AGASSIZ. The scales had the usual fine lustre, and smooth but delicately punctured surface. They were generally about three-fourths of an inch long, by somewhat less in breadth.

15.86 grains of these scales submitted to analysis gave the following result:

Phosphate of Lime, with a little Fluoride of Calcium,		50.94	
Carbonate of Lime,		11.91	
Siliceous matter,		33.10	36.58
Water,		3.48	
Potash and Soda,		.47	
Bituminous Matter,		.12	
Phosphate of Magnesia,	Trace		
Animal Matter,	Trace		
		100.02	

The most unexpected part of this analysis was the occurrence of so large a proportion of siliceous matter. Judging from the beautifully perfect appearance of these scales, presenting a lustre as fine in all likelihood as they possessed when they clothed the sides of the immense living tenants of the primeval waters, we are naturally led to expect no great change in their nature. A little reflection, however, will show us that as fish scales are known to contain a good deal of perishable animal matter, in addition to the more permanent animal earths, which enter into their composition, it is necessary that the place of this destructible matter should be taken by something more durable, in order that the original form may be preserved. This seems to be the origin of the siliceous matter; and, in proof of the justness of this view, it need only be mentioned, that when the scales are acted on by acids, the insoluble matter remains behind as a fine siliceous skeleton, formed, there can be little doubt, by the infiltration of dissolved siliceous matter which has gradually taken the place of the decaying animal matter. The fossil scales, on this view, ought to consist of the infiltrated matter, together with the original animal earths; and this is exactly what we find to be the case; and it so happens, that in the present instance we are enabled to determine, with some degree of plausibility, that even the relative proportions of the different constituents have been to a considerable extent preserved. We luckily possess an analysis of the scales of the recent Lepisosteus, as well as of several other recent fishes, by Chevreul; and, it is very interesting that, on comparing the analysis of the scales of the Megalichthys of Burdiehouse with those of the Lepisosteus, to which, on the high authority of M. Agassiz, the Megalichthys is in other respects allied, we find a considerable approximation between the proportions of the different constituents of the scales of the recent and of the fossil fish on substituting siliceous matter for destructible animal matter;—not certainly a numerical identity, but still a marked analogy, and one of a much stronger description than between the fossil scales and those of the other recent fishes. Chevreul's analyses are as follows:*

	Lepisosteus.	Perca labrax.	A Chœtodon.
Phosphate of Lime,	46.20	37.80	42
Carbonate of Lime,	10.00	3.06	3.68
Gelatinous Animal Matter,	41.10	55.	51.42
Phosphate of Magnesia,	2.2	.90	.90
Fatty Matter,	0.10	.40	1.
Carbonate of Soda,	0.10	.90	1.
	100.	98.06	100

On comparing the proportion of the first three of the above constituents with the corresponding ingredients in the fossil scales, we shall see the analogy, and it is somewhat increased by the circumstance, that the water was in combination with the siliceous matter, as I ascertained by igniting a portion of it after drying it at 212°; so that in reality both these constituents, forming together a hydrated siliceous matter, have been substituted by infiltration for the animal matter.

We may recall to remembrance, that the limestone matrix contains about $2\frac{1}{2}$ per cent. of earthy matter, so that we can be at no loss to account for the occurrence of the siliceous matter.

The following comparison, framed on this view, will show the

* Berzelius, Lehrb. iv. 628.

resemblance in composition between the recent and the fossil scales.

	Scales, Lepisosteus.	Scales, Megalichthys.
Phosphate of Lime, ...	46.20	50.94
Carbonate of Lime, ...	10.	11.91
Animal Matter replaced in the Megalichthys by Hydrated Siliceous Matter, ...	41.10	36.58
Phosphate of Magnesia and Alkalies, small quantities.		
	97.3	99.43

I am happy in being able thus to add something like chemical evidence in support of the alliance between the sauroid fish of Burdiehouse and its supposed living type.

I have found also that the scales adhering to coal discovered by Lord Greenock at Craighall, and of which his Lordship was so kind as to give me some specimens for chemical examination, leave, when acted on by acids, a siliceous skeleton, which doubtless has been supplied by substitution for animal matter in the same manner as in the other, although to a less extent. I have not yet had time to complete an analysis of these scales, but shall communicate the result to the Society when obtained. I hope to be able to communicate at the same time some other analyses, farther illustrative of the question how far the chemical nature of fossil scales may be capable of throwing light on the character of the animals from which they have been derived.

It is worthy of remark, that the manner in which the animal matter of the scales of the Megalichthys has been replaced by siliceous matter, is very conformable to that in which, according to the views of Von Buch, shells have in certain instances been silicefied. According to that eminent geologist, it is only the animal matter between the calcareous layers of the shell which is converted into siliceous matter, the calcareous layers being usually thrown off altogether, although sometimes included within the flinty substance, but never themselves silicefied.

In like manner in the scales, the phosphate and carbonate of lime are unchanged in their nature, whilst the animal matter has been converted into a hydrated siliceous substance. The mineralizing substance of the silicefied shells is often also a hydrated silica or opal.*

As we possess one or two analyses of recent fish bones, we are also enabled to institute a comparison between the result of these analyses and that of the fossil bony rays. The following are the constituents found by Dumenil in the bones of the pike, and by Chevreul in those of the cod.†

	Pike.
Phosphate of Lime,	55.26
Carbonate of Lime,	6.16
Animal matter,	37.36
Traces of Soda, &c. and loss,	1.32
	100.00

	Cod.
Phosphate of Lime,	47.96
Carbonate of Lime,	5.50
Animal Matter and Moisture,	43.94
Phosphate of Magnesia,	2.
Soda Salt,	6.
	105.

The first circumstance that here strikes us, is, that the proportion of phosphate of lime is almost identical in the recent bones of the pike, and in those of the ancient Gyracanthus of Burdiehouse; and it is very remarkable, that if we suppose the animal matter in the fresh bones of the Gyracanthus to be replaced partly by siliceous and partly by calcareous matter,—a supposition sufficiently legitimate, because both these matters are present in the matrix, and both are soluble in water under favour-

* In examining, however, the insoluble siliceous matter of the scales by the blowpipe, it appears not to be a quite pure hydrated silica, but probably to contain a small quantity of lime.

† Berzelius, Lehrb. iv. 448.

able circumstances,—we shall obtain a substance almost identical with the recent bones of the pike, and not differing much from those of the cod. In this view, if we subtract from the calcareous matter actually found in the fossil bones, a quantity equal to that in the pike bones, and add the remainder to the siliceous matter, we shall obtain the following comparison, shewing a singular resemblance.

	Bones. Pike.	Bony rays. Gyracanthus.
Phosphate of Lime,	55.26	53.87
Carbonate of Lime,	6.16	6.16
Animal Matter replaced in the Gyracanthus, by siliceous and calcareous matter,	37.36	37.92
Phosphate of Magnesia, and Alkalies, small quantities.		
	98.78	97.95

I am aware, that if we adopt these last views, we must hold either, 1*st*, That the bony rays did not belong to a cartilaginous fish, in which class I believe the Cestracion is ranked, for the bones of cartilaginous fishes contain no bone earth; or, 2*d*, That the fin spines of cartilaginous fishes have a different constitution from the other bones, a point which I do not know has been yet investigated.

Even when we view the composition of the coprolites imbedded in the limestone, in combination with that of the recent fish-bones, we get something approaching to a coincidence. In one of my analyses of these coprolites, the ratio of the phosphate of lime to the carbonate of lime was about 8 : 1, and this does not differ very much from that of those constituents in the recent fish-bones, according to the analyses of Dumenil and Chevreul. In the other coprolite which I analyzed, there is, on this view, an excess of about 5 per cent. of carbonate of lime, which, of course, was derived from the matrix. The phosphate and carbonate of lime form almost the entire mass of the coprolites from the limestone, the only other constituent of consequence being 2 or 3 per cent. of bituminous matter.

It would therefore seem that the coprolites of the limestone actually contain less foreign matter, or are less altered from their original nature in so far as regards solid contents, than the bones or scales; and this fact admits, I think, of a ready explanation. When a solid compact body, possessing a structure such as a bone or scale, has been imbedded in any situation in which water, holding solid matter in solution, has access to it, we may expect that, in the lapse of time, the perishable animal matter which it contains will disappear, whilst the dissolved solid matter will be deposited, by infiltration, and occupy its place, the original solid structure, more especially when aided by the solid organic earths, forming a kind of frame-work, on which the deposition will take place. This is just the process by which ordinary petrifying springs convert organic objects into the substance held in solution. But fecal matter, being composed of the residue of digested food, and being destitute of structure or solidity, will not present such a frame-work. The perishable animal matter will simply decay, whilst its place will not be occupied by a substitute; the solid animal earths will simply agglomerate into a compact mass; and therefore we shall expect to find them,—what they really appear to be as imbedded in the limestone of Burdiehouse,—masses of durable animal earths, having only a comparatively small admixture of foreign mineral matter.

Cases, indeed, sometimes occur of soft animal matter being mineralized. Thus a silicefied oyster in the centre of the shell is described by Von Buch; and Lord Greenock informs me that he possesses another similar fossil; but such instances are admitted by naturalists to be comparatively very rare.

Amongst all the fossil remains of Burdiehouse which have been the subject of these remarks, the phosphate of lime is preserved as the standard of comparison; and we have every reason to believe that whatever changes may have occurred in the nature or proportions of the other constituents, this one continues firm and invariable. Well, therefore, might it be called by Dr

Buckland, the imperishable phosphate of lime. It has survived the extinction of species and the wreck of formations, and now aids us, at the distance of countless ages, in our attempts to connect together the organized beings of periods separated from one another by so vast an interval.

In concluding for the present, I have only a single word to say as to the slight trace of perishable animal matter which pervades all these remains. As we know that they all contain bituminous matter, and that many kinds of common coal yield ammonia by distillation, I thought it possible that the alkaline reaction of the limestone and of the various remains when heated, might arise from the bituminous matter. But on making some comparative trials between them and the coal used at the Glasgow Gas-Works, which yields an ammoniacal liquid on distillation, I found the alkaline reaction of the Burdiehouse remains was so much more decided than that of the coal, that there can be no doubt it arises from animal matter.* The strong ammoniacal reaction of the limestone, when subjected to heat, surpassing that even of the bones and scales, is remarkable, and shews how large a quantity of decayed animal matter from the various organic remains which have been entombed in it, has been diffused through its mass. In this respect, as well as in the character and extraordinary number of its fossil relics, both vegetable and animal, the subject of the interesting discovery of Dr Hibbert is distinguished in a marked manner from the ordinary encrinal mountain limestone of the vicinity, which contains only a comparatively slight trace of animal matter.

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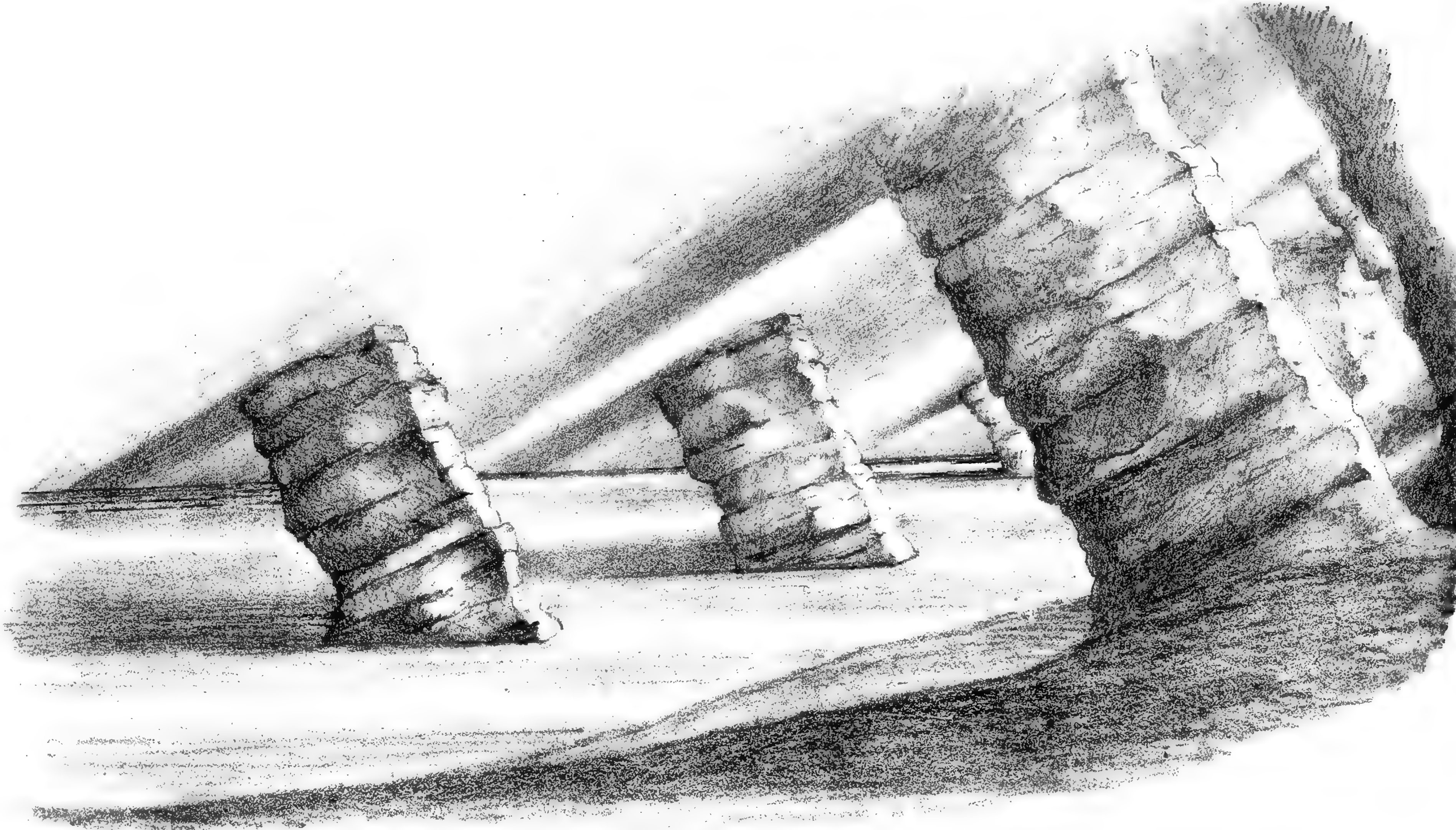
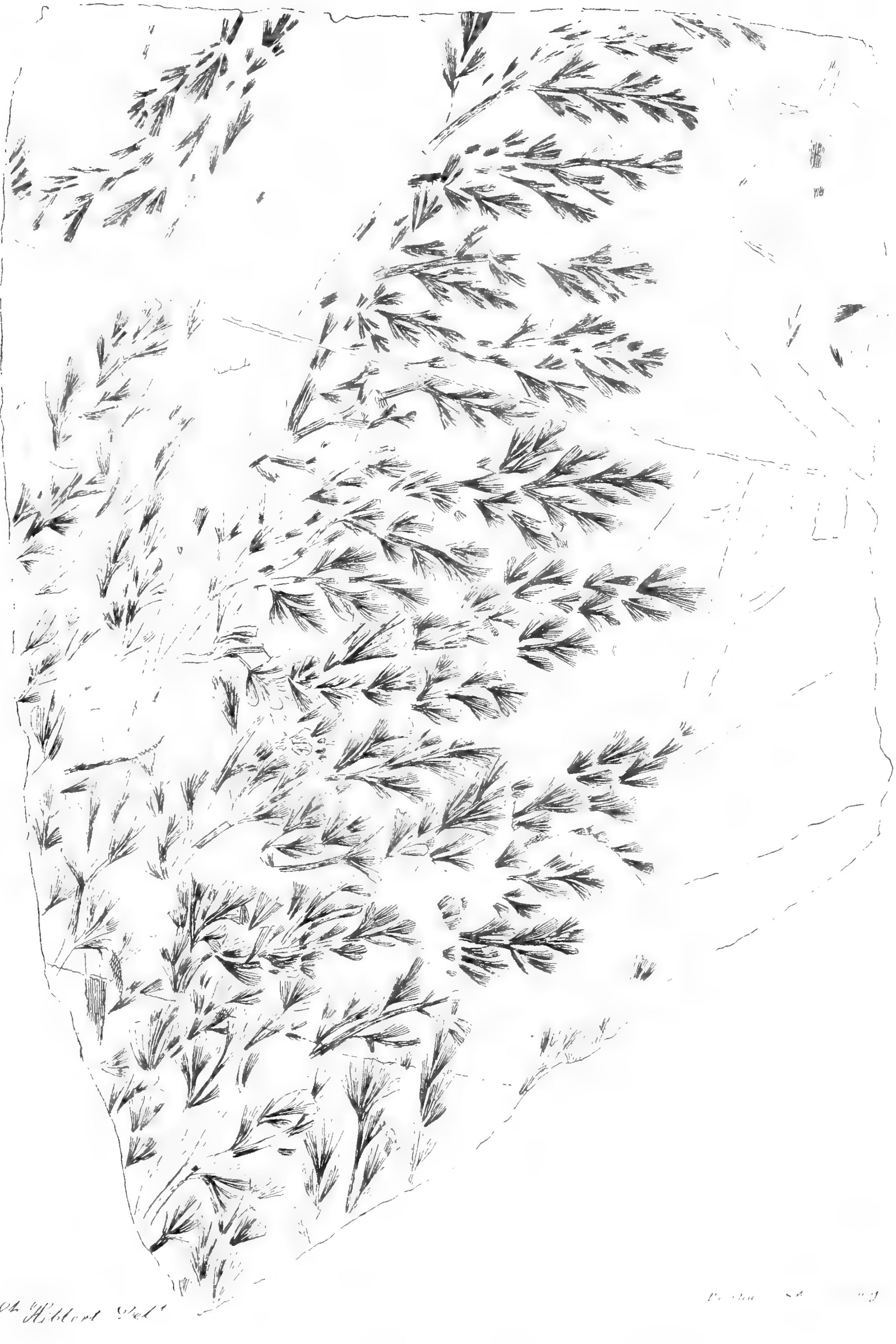
* Indeed, in the way in which these experiments were made, which was by heating portions of the substances under examination in glass tubes, and observing the effect on turmeric paper, it is difficult to notice a proper ammoniacal reaction from the coal; whilst with the substances from Burdiehouse, particularly the limestone and coprolites, the alkaline reaction is observed with great readiness. In all such experiments it is easy to distinguish the *permanent* browning effect which the bituminous vapour exerts on the paper at a high temperature, from the true ammoniacal discoloration.



S. Hibbert Delt

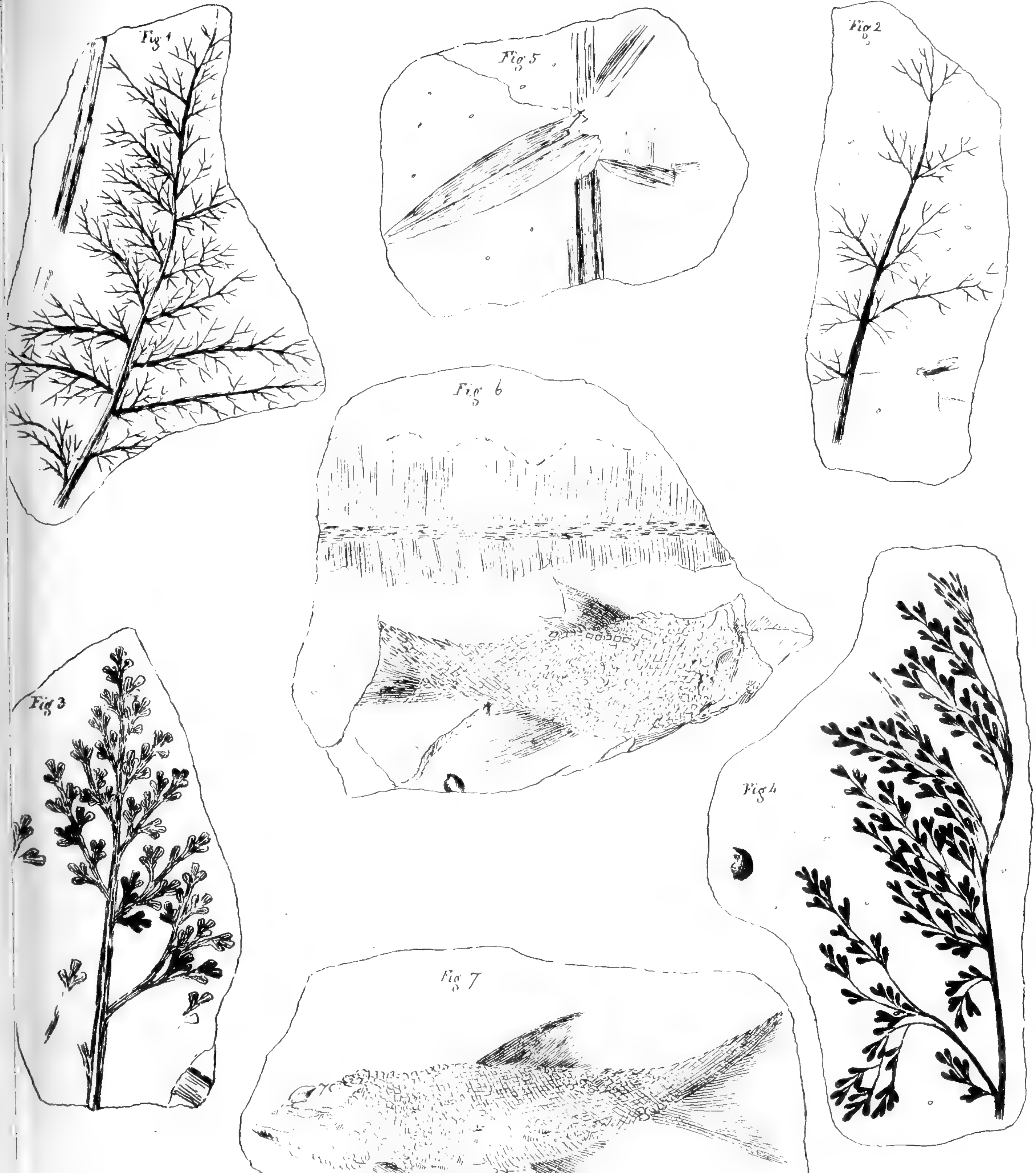
Printed by J. & W. Smith, lithog. 1, Thistle St.

THE OLD LIMESTONE QUARRY AT BURDIE HOUSE.



S. Hibbert Del.

The Sphenopteris affinis of Burdiehouse



Drawn by Mr. Hibbert

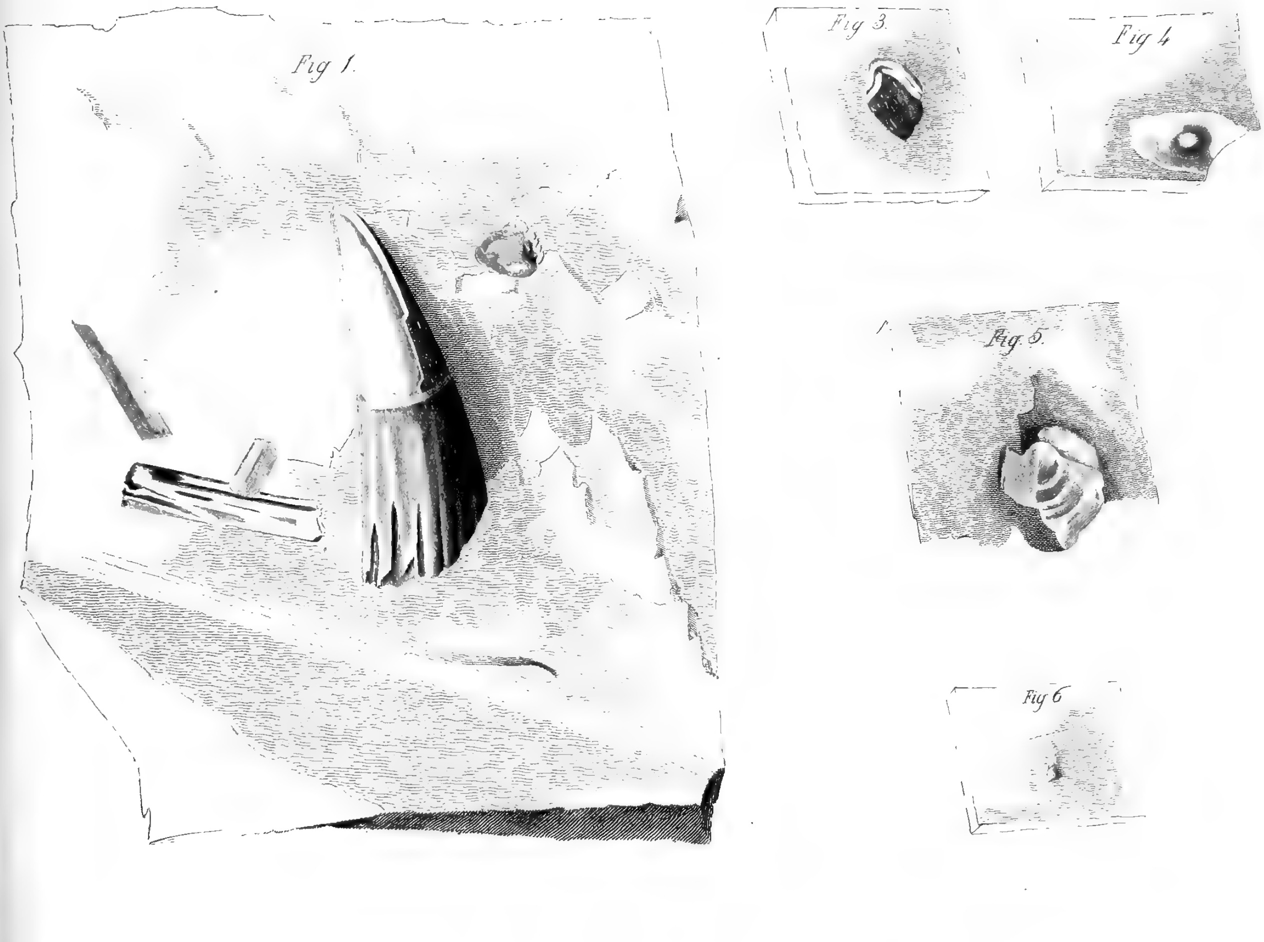
Printed by J & W Smith, lithogrs.

Specimens of the Freshwater Limestone of Burdie House.

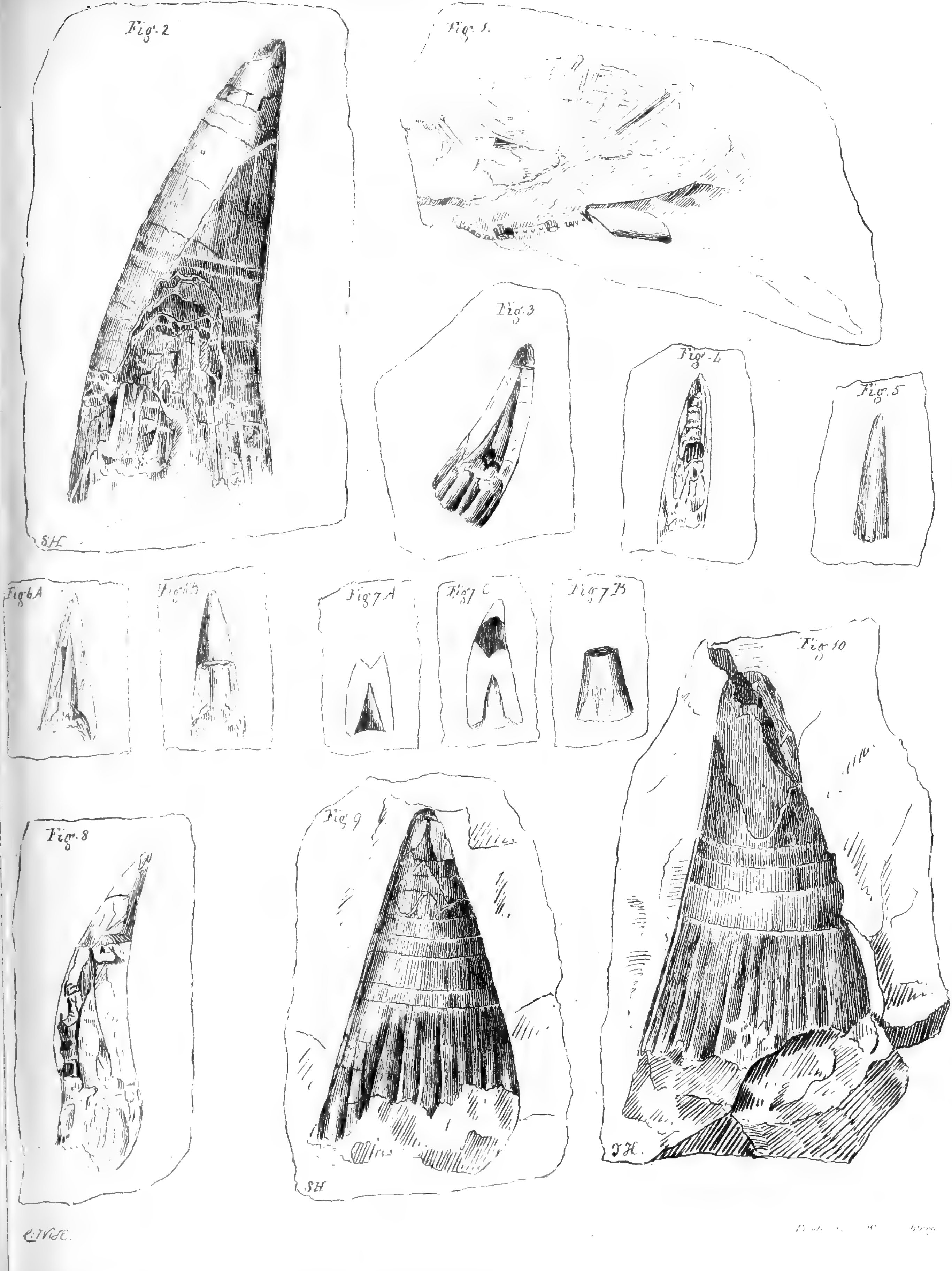
Drawn by Mrs Hibbert

Printed by J. & W. Smith Lithog: 1 Chester St

PLATE VIII.



V. Perry Sculp.



Fossil Teeth of the Burdie-House Limestone.

Natural size.

Drawn by Mrs Hibbert

Printed by J & W. Smith, Lithogrs

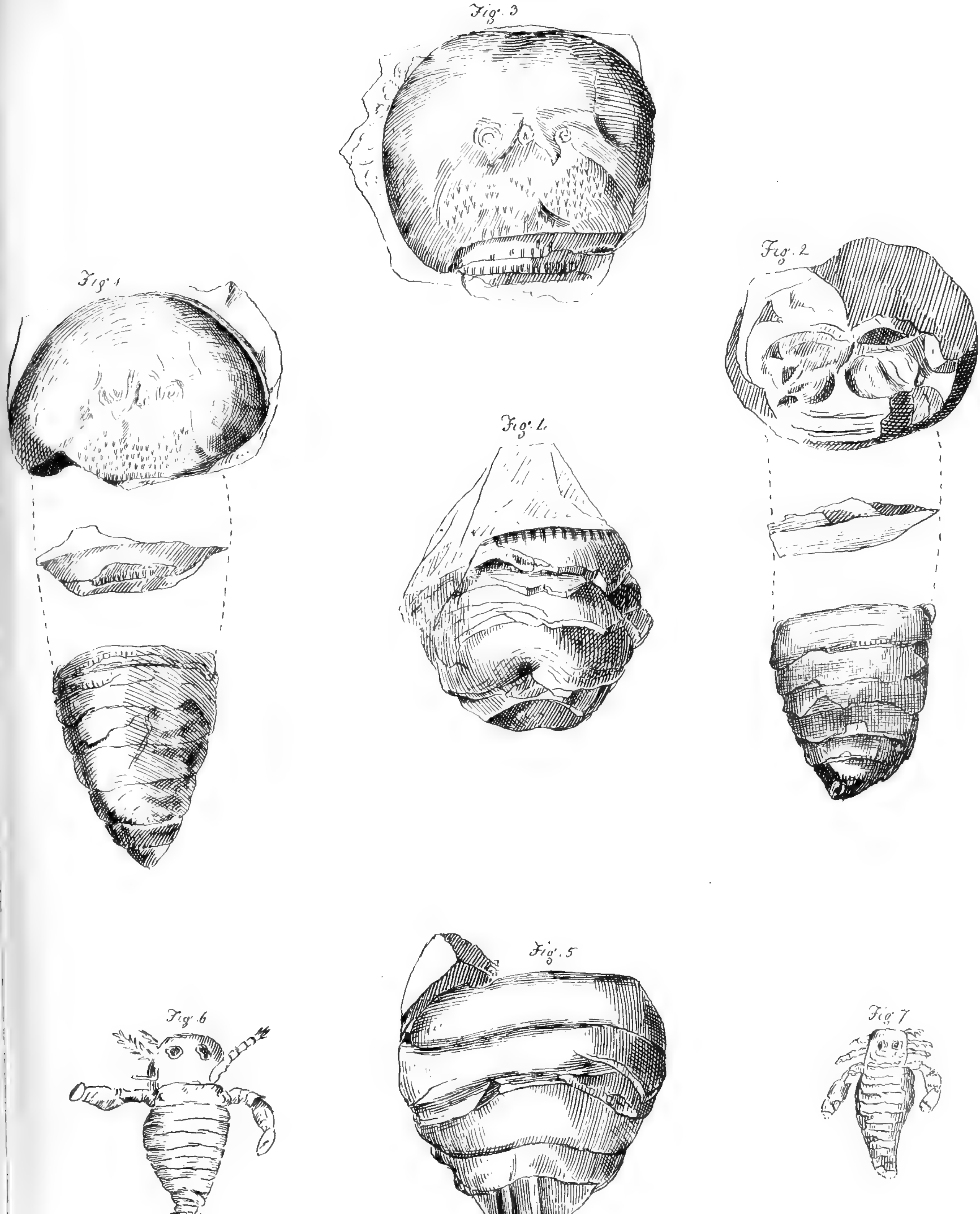
Osseous remains from the Limestone of Burdie-House.

Natural size.

Printed by J & W Smith, Lithog.

Dorsal Ray (½ size) and Scales (natural size) from the Burdie House Limestone.

PLATE 12.



Drawn by Wm Hibbert

Printed by J & W Smith, lithogrs

Remains of the Eurypterus. ¼ Size.

On Water as a Constituent of Salts. 1. *In the case of Sulphates.*

By Thomas Graham, F. R. S. Edin., V.-Pres. of the Phil. Soc. of Glasgow, &c.

(*Read 5th January* 1835.)

It may be useful to distinguish some of the functions which water is already admitted to discharge in the constitution of hydrated salts.

Every amphigene ammoniacal salt contains an atom of water, and cannot exist without it. The state of combination of the water is peculiar, and has been represented by supposing that the elements of ammonia unite with the hydrogen of the water, and form a new compound radicle, to which the name Ammonium is given, while the oxygen of the water unites with this radicle, and produces oxide of ammonium. Hence nitrate of ammonia, in which there exist the elements of one atom of nitric acid, of ammonia, and of water, is viewed as anhydrous nitrate of the oxide of ammonium, and corresponds with nitre or the nitrate of the oxide of potassium. But it is not the object of this paper to discuss particularly the state of water in the ammoniacal salts.

We have it often in the crystals of salts, united by a feeble affinity, and known under the name of water of crystallization. The number of atoms of water with which some salts unite, in crystallizing from a state of solution, is affected by temperature, and other slight causes. This water is commonly viewed as a constituent of salts which is not essential, owing to the facility with which it may in general be expelled by heat, and also to the circumstance that many salts usually hydrated, are likewise capable of existing in a crystalline state without water.

In the hydrates of the caustic alkalies and of the earths, water is retained by a strong affinity, and is generally supposed to be united, like an acid, to the alkali or earth. In such hydrates, water discharges an *acid* function.

In the case of hydrates of the acids, the portion of water which is found to be inseparable by heat, or to be very strongly retained, has generally been presumed to be in the place of a base to the acid, although little attention has been paid to the subject. The most highly concentrated sulphuric acid retains one atom of water, and is supposed to be a sulphate of water. In the case, too, of such a supersalt as bisulphate of potash or bitartrate of potash, the single atom of water which is known to be persistently attached to the salt, has been viewed of late, by our most enlightened chemical theorists, as essential to its constitution, and the possibility admitted that such salts may really be *double salts;* the bisulphate of potash, a sulphate of potash combined with sulphate of water, and the bitartrate of potash, a tartrate of potash combined with tartrate of water.

In a late publication, I have developed this view of water acting as a base in the case of phosphoric acid. That acid is capable of combining with water in three different proportions; and the number of atoms of water with which the acid combines at any time, depends upon circumstances which are understood. That the water is basic in these different hydrates, follows from the fact, that, on treating them with an alkali, the water is constantly replaced by a quantity of alkali chemically equivalent to the water. By nitrate of silver, the same precipitate is thrown down from any phosphate of soda and from the corresponding phosphate of water; the composition of the precipitate being determined in both cases by the same double decomposition. The peculiarity of phosphoric acid is, that it is capable of uniting with water as a base, in several proportions, while all other acids combine with water as a base in one proportion only, so far as is yet known. By these discoveries in regard to phosphoric acid and its salts, the ordinary conceptions entertained of the constitution of salts were completely deranged. The salts called biphosphate of soda, phosphate of soda, and subphosphate of soda, are all tribasic salts. The common idea of a supersalt is inapplicable to any of them.

I have subsequently found water to exist in a different state in certain salts, not possessed of a true basic function, being replaceable by a *salt*, and not by an alkaline base. To illustrate this new function of water as a constituent of salts, is my principal object in the present communication.

The tendency of phosphate of soda to unite with an additional dose of soda, and form a subsalt, I had traced to the existence of basic water in the former. The inquiry suggested itself, is there any analogous provision in the constitution of such salts as have a tendency to combine with other salts, and to form double salts? The salts which combine together most readily are the sulphates, and to these I therefore turned. The result was, that in that well-known class of sulphates, consisting of sulphates of magnesia, zinc, iron, manganese, copper, nickel, and cobalt, all of which crystallize with either five or seven atoms of water, one atom proved to be much more strongly united to the salt than the other four or six, which last generally may be expelled by a heat under the boiling point of water, while the remaining atom uniformly requires a heat above 400° Fahrenheit for its expulsion, and seems to be in a manner essential to the salt. The constitution of crystallized sulphate of zinc, for instance, may be expressed thus:

$$\dot{Zn}\dddot{S}\dot{H}+\dot{H}^{6}$$

We here divide the seven atoms of water into one atom, which is essential to the constitution of the salt as we know it, and six atoms which are not so; and to this last quantity we may restrict the application of the name "water of crystallization." Now, in the double sulphate of zinc and potash, the single atom of water in question pertaining to the sulphate of zinc is replaced by an atom of sulphate of potash, and the six atoms of water of crystallization remain. Sulphate of magnesia combines with sulphate of potash after the same manner, and so do all the other salts of the class. The constitution of the crystallized sulphate of zinc and potash, which may be taken as the type of this family of double salts, is therefore represented by the following formula,

$$\dot{Zn}\dddot{S}(\dot{K}\dddot{S})+\dot{H}^6;$$

which differs only from the previous formula in having the sign of sulphate of potash ($\dot{K}\dddot{S}$) substituted for the sign ($\dot{H}$) of the essential atom of water.

From a contemporaneous examination of the supersulphates, the conclusion proved to be inevitable, that they also are double salts; that the bisulphate of potash, for instance, is a sulphate of water and potash, and that its formula is as follows,

$$\dot{H}\dddot{S}(\dot{K}\dddot{S}),$$

with or without water of crystallization in addition. There is likewise a provision in the constitution of hydrated sulphuric acid for the production of such a double salt, as in the case of the sulphate of zinc. Hydrated sulphuric acid of specific gravity 1.78 contains two atoms of water, and is capable of crystallizing at a temperature so high as 40° Fahrenheit. It is the only known crystallizable hydrate of sulphuric acid. It may be represented by the formula,

$$\dot{H}\dddot{S}\dot{H},$$

$$\dot{Zn}\dddot{S}\dot{H},$$

which may be compared with that of sulphate of zinc placed below it. This second atom of water present in hydrated suphuric acid, is replaceable by sulphate of potash, a salt; and the bisulphate of potash results from the substitution. But the first atom of water in the acid hydrate can be replaced only by an alkali or true base. The function of the first atom is *basic*, but a new term is required to distinguish the function of the second atom of water, or of the essential atom of water in the sulphate of zinc. The application of the epithet *saline* to that atom of water, may, perhaps, be permitted, to indicate that it stands in the place of a salt. The hydrate of sulphuric acid in question contains, therefore, an atom of basic, and an atom of saline water. It is "a sulphate of water with saline water," as the hydrous sulphate of zinc is "a sulphate of zinc with saline water." The bisulphate of potash also is "a sulphate of water with sulphate of potash," and corresponds with the sulphate of zinc and potash; which last is "a sulphate of zinc with sulphate of potash."

A reason could now be given why there exist no supersulphates (or indeed any supersalts) of magnesia, zinc, &c. A bisulphate of magnesia would be a compound of sulphate of water with sulphate of magnesia, on our view of supersulphates. Now sulphate of magnesia, and sulphate of water, are bodies of analogous constitution, or of the same category, and should have as little disposition to combine together, as sulphate of zinc and sulphate of magnesia have.

1. *Sulphate of Water with Saline Water;* $\dot{H}\dddot{S}\dot{H}$. *Sulphuric Acid of sp. gr.* 1.78.

It appears, then, that in an exposition of the relations of the sulphates, we may set out from this body as our primary sulphate. Of the two atoms of water which it contains, that atom which is basic cannot be separated from the acid, unless by the agency of a stronger base. The second, or saline atom of water, may be separated by heat, but not by any degree of heat, under 400° Fahrenheit, and is re-absorbed with great avidity.

A diluted sulphuric acid may, I find, be concentrated at a temperature not exceeding 380°, without the loss of a particle of acid; and the quantity of water retained is reduced to two atoms most precisely. This in fact is an exact method of obtaining the definite sulphate of water with saline water; which may be kept at 380° or 390°, without sustaining any farther loss. I have observed a close approximation to the same proportion of water, even in the case of a dilute acid concentrated at a temperature not exceeding 300°. But at 400° or 410°, this hydrate begins to be decomposed, and a portion of it is apt to distil over with the water expelled. When, however, this hydrate is distilled in vacuo, at the last-mentioned temperature, it loses nothing but water for some time.

In one experiment, a small quantity of dilute sulphuric acid was found to concentrate down to three atoms of water, at a temperature not exceeding 212°, at which it was sustained in vacuo for not less than forty hours. It consisted of 100 parts dry acid united with 68.07 water, while three atomic proportions of water are 67.32 parts.

The concentrated acid of commerce, which is a definite sulphate of water, without the saline atom, does not freeze at a temperature so low as —36°, according to Dr Thomson. To sulphuric acid of sp. gr. 1.78, I added water in the proportion of two, four, and six atoms; but all these hydrates remained fluid, when kept for a short time at 0° Fahrenheit. Anhydrous sulphate of magnesia or zinc never dissolves, *as such*, in water; or exhibits any determinate chemical character. It must always combine with its saline atom of water in the first instance, or with something equivalent, and it is the compound which is soluble, &c. So it is with the sulphate of water, or concentrated sulphuric acid ($\dot{H}\dddot{S}$). In chemical character it is an *incomplete body*. There is a hiatus in its constitution, which must be filled up. When it dissolves in any menstruum, we may be sure that it has first acquired its second or saline atom of water, or something in its place. Hence a set of reactions of sulphuric acid, which are peculiar to its concentrated condition, upon alcohol and many organic bodies. But to this peculiar state of bodies I shall again have occasion to allude under sulphate of lime, a body which illustrates it more strikingly than the sulphate of water.

Sulphate of Water with Sulphate of Patash : $\dot{H}\dddot{S}(\dot{K}\dddot{S})$. *Bisulphate of Potash.*

Of all the sulphates, the acid sulphates or bisulphates of potash and soda deviate least from the primary sulphate of water. We have, in the one case, merely sulphate of potash; and, in the other, sulphate of soda, substituted for the saline atom of water of the sulphate of water. In none of the specimens of these salts which I had occasion to examine, was there any water of crystallization, and the evidence which is given of its occasional presence is of a very doubtful description. The crystals could be heated to 300°, without impairing their transparency; and they fused at a temperature not under 600°, without the loss of any thing, except a trace of water, which had been mechanically retained. Upon heating a bisulphate nearly to redness, a portion of sulphate of water is expelled. I greatly doubt whether water ever comes off in such a case unaccompanied by sulphuric acid, although Berzelius appears to be of a different opinion. It is well known that the sulphate of water is not entirely expelled from these salts by heat alone, even the most intense. Sulphate of water, however, leaves the sulphate of soda with greater facility than it leaves the sulphate of potash.

These sulphates should be crystallized from concentrated solutions at a high temperature; for their solutions are very apt to undergo decomposition at low temperatures, the neutral sulphate crystallizing, and leaving "the sulphate of water with saline water" in solution. I have often observed this decomposition to occur, even in solutions containing a great excess of sulphuric acid. At low temperatures, therefore, the affinity of sulphate of water for "saline water," prevails over its affinity for sulphate of potash. Crystals of bisulphate of soda, pounded and put under pressure in blotting paper, are apt to undergo the same decomposition, if the air is damp, and frequently impart a large quantity of their sulphate of water to the paper in the course of twenty-four hours. This circumstance must be kept in view in preparing bisulphates for analysis. The facility with which these salts are decomposed by water, accords well with their relation to sulphate of water with saline water, which we have supposed to exist. Sulphate of zinc, sulphate of magnesia, &c. are capable of separating the sulphate of water from these salts, at a temperature approaching to redness, and take its place.

I have observed that the bisulphate of soda is more prone to decomposition, when dissolved in water, than the bisulphate of potash. The double salts of sulphate of soda with sulphate of magnesia, &c., are also much less stable than the corresponding double salts containing sulphate of potash. Indeed, I believe that the former are uniformly decomposed when dissolved in water.

Sulphate of Potash, Sulphate of Soda. $\dot{K}\dddot{S}$, $\dot{N}\dddot{S}$.

These salts differ from other sulphates in having no saline water. Of the ten atoms water with which sulphate of soda crystallizes, none is essential to its constitution. The whole were lost, even at a temperature not exceeding 47° Fahrenheit, when the crystals of the salt were exposed over sulphuric acid in vacuo for five days. From the regular progress of the desiccation of the salt, which was observed by occasionally weighing it, it was evident that no portion of the water was more strongly retained than the rest. It is well known that sulphate of soda crystallizes in an anhydrous condition from a hot solution.

Sulphate of Zinc with Saline Water: $\dot{Zn}\dddot{S}H+H^6$. *Sulphate of Zinc.*

In the sulphate of zinc, we have the basic atom of water contained in sulphate of water displaced by oxide of zinc, while the saline atom remains; and to this compound six atoms of water are attached in the common crystals. These crystals, placed over sulphuric acid in vacuo, thermometer 68°, were found to lose six atoms water, retaining only one. Exposed to the air at 212°, the crystals likewise readily effloresced down to one atom; and the sulphate of zinc is known to be deposited from a boiling solution, in crystalline grains, containing one atom of water. On the other hand, the sulphate of zinc was found to retain this single atom of water at the high temperature of 410° Fahrenheit, but to lose it, and become anhydrous, at a temperature not exceeding 460°. In all such cases, the hydrated salt was heated in a tube receiver, by means of an oil or solder-bath, of which the temperature was observed by a thermometer. However strongly it has been heated, without being decomposed, the sulphate of zinc always regains this atom of water when moistened, slaking with the evolution of heat. Common sulphate of zinc is therefore "sulphate of zinc with saline water;" and the true or absolute sulphate of zinc is unknown to us in the crystalline form, or in a soluble state. But we may continue to designate the salt we possess as sulphate of zinc, as the name is attended with no dubiety.

Sulphate of Zinc with Sulphate of Potash: $\dot{Zn}\dddot{S}(\dot{K}\dddot{S})+\dot{H}^{6}$. *Sulphate of Zinc and Potash.*

In this well-known double salt, we have sulphate of potash substituted for the saline water of sulphate of zinc, and the six atoms of water of crystallization remain. It is readily formed, on mixing together solutions of sulphate of zinc and sulphate of potash, in atomic proportions. It is formed likewise, and separates by crystallization, when the sulphate of zinc is added to the bisulphate of potash; and, in that case, an interesting double decomposition occurs.

Sulphate of zinc with saline water, Sulphate of water with sulphate of potash,	yield	Sulphate of zinc with sulphate of potash. Sulphate of water with saline water.

In the sulphate of zinc and potash, the whole six atoms of water are retained with considerably greater force than in the sulphate of zinc itself; but even the double salt becomes anhydrous at 250°, and, indeed, the water retained falls below a single atomic proportion, when the salt is dried in vacuo over sulphuric acid, at a temperature not exceeding 78° Fahrenheit. The sulphate of potash in the double salt has not the effect of neutralizing the acid reaction of sulphate of zinc, according to my observations; nor has it that effect in the case of any other double salt.

I subjoin a table of observations, made on the quantity of water retained by this double salt, in different circumstances. In the first two columns, the composition of the quantities actually examined is stated in grains.

	Anhydrous Salt.	Water.	Anhydrous Salt.	Water.
Dried in vacuo over sulphuric acid for ten days, temp. from 68° to 78°,	17.2	0.68	100	3.95
Nine hours, at 238°,	19.03	1.33	100	6.99
Two hours at 250°, and one hour at 270°,	7.79	0.	100	0.
Four hours, at 250°,	6.55	0.	100	0.
Composition of sulphate of zinc and potash with one atom water (by theory),	...	...	100	5.37

Sulphate of Zinc with Sulphate of Soda: $\dot{Zn}\ddot{S}(\dot{N}\ddot{S})+\dot{H}^4$. *Sulphate of Zinc and Soda.*

This salt, I believe, has not hitherto been described. I failed in attempting to form it, by dissolving together sulphate of zinc and sulphate of soda in atomic proportions: the salts uniformly crystallized apart, either in cold or in warm weather. Each of the salts was also added in excess to the other, but with no better effect. It appears, then, that sulphate of soda does not displace the saline water of sulphate of zinc, so easily as sulphate of potash does. But the desired salt was obtained by a process of double decomposition, suggested from consideration of the relations of the sulphates. Solutions of bisulphate of soda, and of sulphate of zinc, were mixed together in atomic proportions, from which the sulphate of zinc and soda separated in a gradual manner in the course of a day or two, leaving sulphuric acid in solution.

Sulphate of zinc with saline water, Sulphate of water with sulphate of soda, . . .	yield	Sulphate of zinc with sulphate of soda. Sulphate of water with saline water.

This salt is deposited in distinct tabular crystals, of a peculiar form, which are often associated in tufts; and is best obtained by evaporating the mixed solutions over sulphuric acid without heat. It cannot be redissolved in pure water, without undergoing decomposition, which accounts for the impossibility of forming it by the direct process. The crystals contain four atoms of water, and are about as deliquescent as nitrate of soda, in a damp atmosphere. The anhydrous salt undergoes fusion, like all the other double sulphates, at an incipient red heat, without the evolution of acid fumes. The fused salt solidifies, on cooling, into a white and opaque mass.

Sulphate of Copper with Saline Water: $\dot{C}u\dddot{S}H+\dot{H}^4$. *Sulphate of Copper.*

The common blue rhomboidal crystals of sulphate of copper contain five atoms of water, four of which are readily expelled, by drying the salt in air at 212°; by which treatment the salt loses its blue colour, and becomes white, with a dirty shade of green. The sulphate of copper with one atom of water was also obtained in a crystallized state by Dr Thomson, and called by him green sulphate of copper. Dried over sulphuric acid in vacuo for seven days, when it had ceased to lose, at a temperature between 65° and 74°, the common hydrated salt retained 21.67 parts water to 100 anhydrous salt, which is somewhat under two atomic proportions of water, namely 22.57 parts. At a temperature between 430° and 470°, the sulphate of copper loses its fifth, or saline, atom of water, and is found in the state of a powder, which is white without any shade of colour. When a few drops of water are thrown upon anhydrous sulphate of copper, it slakes and becomes blue, and so much heat is evolved as to occasion the ebullition of the water. In one case the temperature was observed to rise to 276°. This arises from the resumption of saline water by the salt.

Sulphate of Copper with Sulphate of Potash : $\dot{\text{Cu}}\dddot{\text{S}}(\dot{\text{K}}\dddot{\text{S}})+\dot{\text{H}}^6$. *Sulphate of Copper and Potash.*

This salt may be formed by mixing sulphate of copper with either sulphate or bisulphate of potash, in atomic proportions. Dried in the open air, it loses six atoms water, and becomes quite anhydrous at a temperature not exceeding 270° Fr. The following Table of the composition of this hydrated salt in different circumstances, illustrates three facts,—that the salt has a disposition to retain two atoms of water when dried at 212° in open air,—that a greater portion of water of crystallization is withdrawn from the salt by drying it over sulphuric acid in vacuo, without artificial heat, than by drying it at 212° under the atmospheric pressure, and that the mechanical water retained by the crystals of this salt may exceed 3 per cent. of their weight.

	Anhydrous Salt.	Water.	Anhydrous Salt.	Water.
Dried on water-bath at 212°, for three days, or till it ceased to lose weight, . . .	19.6	2.21	100	11.27
Dried on nitre-bath at 238°, for three days,	22·06	2.37	100	10.74
Dried in vacuo over sulphuric acid for seven days, or till it ceased to lose weight; therm. from 65° to 74°, ...	22.97	1.61	100	7.09
Crystals pounded, and slightly dried at 80°, so as not to injure the lustre of an entire crystal, ...	13.94	3.4	100	32.25
Same crystals not deprived of mechanical water by the above treatment, ...	23.79	8.64	100	36.22
Composition of sulphate of copper and potash with two atoms of water (by theory), .	......	...	100	10.77
Composition of do. with six atoms of water (by theory), ...	......	...	100	32.33

I have confirmed the observation of Berzelius, that a concentrated solution of this salt, when boiled, deposits an insoluble subsalt, containing sulphate of potash, but which is decomposed by washing, and cannot be had in a proper state for analysis. But the crystals of the double salt are quite soluble after being heated to 212°, so that they do not undergo the same change as their solution does at that temperature.

This double salt retains its blue colour after being fused at a red heat and cooled, and does not become white like the sulphate of copper. Indeed it appears, that, to be coloured, the salts of the oxide of copper require the addition of some other constituent, such as saline water, sulphate of potash, or ammonia. Hence, if the absolute sulphate of copper could be obtained in a crystallized state, it would be a colourless salt.

Sulphate of Copper with Sulphate of Soda: $\dot{Cu}\dddot{S}(\dot{N}\dddot{S})+\dot{H}^2$. *Sulphate of Copper and Soda.*

Like the other double salts of sulphate of soda, this salt cannot be formed directly, being decomposed by water. Even when it is attempted to form it by double decomposition from the bisulphate of soda, in general a large quantity of sulphate of soda and of sulphate of copper are separately deposited before the double salt appears. It is then deposited in a crust, consisting of small but distinct crystals, which are slightly deliquescent, and appear to contain two proportions of water. This salt is easily made anhydrous, and thereafter fuses at an incipient red heat without loss of acid, and remains of a blue colour when cool. The fused salt does not split into thin scales in the progress of cooling, as the corresponding sulphate of copper and potash does.

Sulphate of Manganese with Saline Water : $\dot{M}n\dddot{S}\dot{H}+\dot{H}^4$. *Sulphate of Manganese.*

The water in this salt was found to be reduced from five atomic proportions to little more than one, by drying the crystals in open air at 238°, while one entire atomic proportion was retained at 410°. Flesh-coloured crystals, dried in vacuo in warm summer weather, without artificial heat, lost somewhat more than three proportions of water.

	Anhydrous Salt.	Water.	Anhydrous Salt.	Water.
Flesh-coloured crystals of salt, ...	28.42	17.07	100	60.06
Do. dried at 238°, ...	21.53	2.92	100	13.05
A portion of last, afterwards dried for one hour between 380° and 410°, . . .	9.54	1.12	100	11.74
A portion of same, dried for one hour between 415° and 468°, ...	10.90	0.56	100	5.14
Crystals dried for nine days in vacuo over sulphuric acid, thermometer 64° to 72°, but had lost nothing the last two days,	8.62	11	100	20.88
Composition of sulphate of manganese with one atom of water (by theory), . . .	......	...	100	11.88
Composition of do. with five atoms of water,	......	...	100	59.4

A crystalline crust of sulphate of manganese, deposited from a warm solution, was found to contain three atoms of water. It is likewise known to be deposited from a boiling solution with only one atom of water, namely, the saline atom. We have, therefore, sulphates of this class with no water of crystallization, and with two, four, and six atoms.

The sulphate of manganese and potash did not crystallise on mixing the solutions of its constituents. The sulphate of manganese and soda was obtained in analogous circumstances with the sulphate of copper and soda, but was not examined.

Sulphate of Iron with Saline Water: $\dot{Fe}\dddot{S}\dot{H}+\dot{H}^6$. *Sulphate of Iron.*

Of the seven atomic proportions of water which the crystals contain, 5.48 proportions were lost in vacuo over sulphuric acid; and six proportions at 238°, and probably at lower temperatures. The saline atom of water is retained by this salt at so high a temperature as 535°. But the salt can be made perfectly anhydrous, with proper caution, without appreciable loss of acid.

Sulphate of Iron with Sulphate of Potash: $\dot{Fe}\dddot{S}(\dot{K}\dddot{S})+\dot{H}^6$. *Sulphate of Iron and Potash.*

A specimen of this salt was made anhydrous by a sandbath heat, which was found not to affect the saline atom of water of the preceding compound.

Sulphate of nickel was found to correspond closely with sulphate of iron in the temperatures at which it lost its water of crystallization, and also its saline water. And in the case of both of the compounds of these salts with sulphate of potash, a considerably higher temperature was required to render them perfectly anhydrous, than in the case of the corresponding double salt of zinc.

Sulphate of Magnesia with Saline Water : $\dot{\text{Mg}}\dddot{\text{S}}\dot{\text{H}}+\dot{\text{H}}^{6}$. *Sulphate of Magnesia.*

One atom of water is retained by sulphate of magnesia at 460°, but the other six are not entirely expelled under 270° in open air. Indeed this sulphate is remarkable for a disposition to retain two atoms of water, in which respect it resembles the sulphate of lime. Dried at 212° in open air, the crystals of sulphate of magnesia were found in several experiments to retain somewhat more than two atomic proportions of water. When dried at the same temperature in vacuo over sulphuric acid, the water was reduced to two proportions. Crystals placed over sulphuric acid in vacuo, without heat, were found to retain only two and a quarter atomic proportions of water.

	Anhydrous Salt.	Water.	Anhydrous Salt.	Water.
Crystallized salt, dried in vacuo at 70° for six days, or till it ceased to lose, . . .	12.34	4.13	100	33.46
Do. in vacuo at 212°, ...	21.8	6.24	100	28.62
Do. heated between 410° and 460° for one hour, being previously dried at 238°, .	4.9	0.74	100	15.1
Relative composition of the anhydrous salt with one atom of water (by theory), .	......	...	100	14.81

The sulphate of magnesia and ammonia lost its six atoms of water of crystallization and became anhydrous, when exposed to a temperature not exceeding 270°, for one hour, having previously been dried at 212°. It retained of course the atom of water which is essential to the ammoniacal salts. A somewhat higher temperature was required to deprive the sulphate of magnesia and potash of its whole water of crystallization.

Hydrated Sulphate of Lime: $\dot{\text{Ca}}\dddot{\text{S}}\dot{\text{H}}+\dot{\text{H}}$.

The only crystalline hydrate of sulphate of lime, which is known, contains two atoms of water. It occurs native in gypsum and selenite. Pounded selenite loses little or nothing in the open air at 212°. Water begins to escape at a temperature not much higher, but is not completely expelled by any degree of heat under 270°. That hydrated sulphate of lime may contain an atom of saline water, is indicated by the existence of a double salt of sulphate of lime with sulphate of soda, constituting the mineral Glauberite. I succeeded in obtaining a definite compound of sulphate of lime with one atom of water, by drying pounded selenite, at 212°, in vacuo over sulphuric acid.* The salt which had been so dried at 212° did not form a coherent mass, like stucco, when made into a paste with water. The affinity of sulphate of lime for the saline atom of water appears to be feeble, as the salt can be made quite anhydrous under 300°; and consequently the sulphate of lime has much less disposition to form double salts than the sulphates of magnesia, zinc, &c.

	Anhydrous Salt.	Water.	Anhydrous Salt.	Water.
Selenite, dried for ten days in open air at 212°, ...	17.07	4.27	100	25.01
Do. dried in vacuo at 212°, . . .	17.61	3.04	100	14.72
Sulphate of lime with one atom of water (by theory), . . .	......	...	100	13.13
Do. with two atoms of water (by theory), ...	......	...	100	26.26

In drying gypsum, to make plaster of Paris, a third or a fourth of the water of the salt is allowed to remain, by which it sets more strongly. But the salt may be made quite anhydrous, I find, and yet retain the power of recombining with two atoms of water, if dried at a temperature not exceeding 270° F.; although the hy-

* It has subsequently been observed, that the water is reduced under one atomic proportion, by a protracted exposure to the same temperature.

drate which results on slaking in the last case is rather pulverulent. When gypsum has been dried at a higher temperature, as at 300° or 400° F., it refuses entirely to combine with water, and is technically called *burnt stucco.* The anhydrous sulphate of lime which occurs in nature exhibits the same indifference to water. In Anhydrite we have, I believe, the true or absolute sulphate of lime in a crystallized state. The body which results from exposing hydrated sulphate of lime to 270°, although composed of nothing but sulphuric acid and lime, should be viewed as the *debris* of the hydrated sulphate of lime, and not confounded with the absolute sulphate of lime, which last has no disposition to combine with water. The first, which we may call " anhydrous gypsum," is an *imperfect* body. We know sulphate of lime in four states, which may be expressed symbolically as follows :

Gypsum, ...	$\dot{\mathrm{Ca}}\dddot{\mathrm{S}}\dot{\mathrm{H}}+\dot{\mathrm{H}}$
Gypsum dried at 212°, . . .	$\dot{\mathrm{Ca}}\dddot{\mathrm{S}}\dot{\mathrm{H}}$
Anhydrous gypsum (dried at 270°),	$\dot{\mathrm{Ca}}\dddot{\mathrm{S}}$—
Anhydrite, ...	$\dot{\mathrm{Ca}}\dddot{\mathrm{S}}$

Here we distinguish the imperfect body, anhydrous gypsum, from anhydrite, by placing the minus sign after the former. In the same manner, concentrated sulphuric acid, or oil of vitriol, may be represented by $\dot{\mathrm{H}}\dddot{\mathrm{S}}$— ; anhydrous sulphate of magnesia, sulphate of zinc, &c. by $\dot{\mathrm{Mg}}\dddot{\mathrm{S}}$—, $\dot{\mathrm{Zn}}\dddot{\mathrm{S}}$—, &c. ; the absolute sulphates of water, magnesia, zinc, &c., $\dot{\mathrm{H}}\dddot{\mathrm{S}}$, $\dot{\mathrm{Mg}}\dddot{\mathrm{S}}$, $\dot{\mathrm{Zn}}\dddot{\mathrm{S}}$, &c., being unknown to us.

The view which is given in this paper of the constitution of the sulphates, must not be hastily generalized and applied to other classes of salts. From investigations not yet completed, I am satisfied that each class of salts has its peculiarities, which must be studied before the law of the class can be laid down.

On the Action of Voltaic Electricity on Alcohol, Ether, and Aqueous Solutions. By Arthur Connell, Esq., F. R. S. E.

(*Read 27th April* 1835.)

I was led into the following train of investigation from observing, that, when minute quantities of certain substances were dissolved in alcohol, and the liquid was acted on by a moderate voltaic power, evident signs of decomposition were exhibited by the evolution of elastic fluid at the negative pole. In investigating the nature of the changes produced, I was farther led to examine the action of voltaic electricity on a variety of alcoholic solutions, and also the agency of more powerful galvanic batteries on pure alcohol and on ether; and, ultimately, I was conducted into a field, into which I should have hesitated voluntarily to enter, namely, the voltaic decomposition of aqueous solutions, which has recently been investigated with so much success by a distinguished cultivator of science. In the following paper, it will be my object to give some account of the experiments which I have made on these different subjects, and of the results and views to which I have been led. I shall begin with the voltaic action on alcohol and ether, and as I hope to be able to draw some conclusions bearing on the various theories which have been formed as to the intimate nature of these fluids, it will be necessary that the experiments made on them shall be detailed with considerable minuteness, as it is only by a very careful consideration of the experimental results, that the validity of the conclusions can be judged of.

I. *Voltaic Action on Alcohol.*

I found, then, that when alcohol (sp. gr. .830), having about $\frac{1}{500}$ part of pure caustic potash dissolved in it, was acted on by the voltaic pile, the poles employed being of platinum foil, and approached to one another in an open vessel, gas was evolved from the negative pole, whilst none whatever appeared from the positive foil. A moderate voltaic power, such as a small battery of fifty pairs of two-inch plates, was amply sufficient to produce this effect. This experiment immediately recalled to my recollection a statement made a year or two ago by Dr RITCHIE,* that when alcohol, not holding any substance in solution, was acted on by a powerful galvanic battery, gas was evolved from the negative pole, whilst none appeared from the positive, and that the gas thus evolved was olefiant gas ; on which idea Dr RITCHIE concluded that the alcohol had been resolved into olefiant gas and water. As there was apparently a considerable analogy between the two observations, I was naturally led to conjecture that the gas evolved at the negative pole in my experiment, was olefiant gas ; but an examination of it soon satisfied me that this was not the case. The gas was collected simply by bringing the negative foil under a tube filled with alcohol, holding about $\frac{1}{400}$ part of potash in solution, and inverted in an evaporating basin containing the same liquid ; the power employed being sometimes fifty pairs of two-inch plates, and in other instances, seventy pairs of four-inch plates. No gas was evolved from the positive pole. A small portion of the gas collected from the negative pole was mixed with three times its bulk of chlorine, and the mixture left in a dark room over water. In the course of a few hours, an absorption had taken place to the amount of the chlorine employed, without the least appearance of the formation of oily matter : and no farther absorption occurred in twenty-four hours. The residual gas was inflammable. In short, the chlorine had left the original gas unchanged, which was therefore not olefiant gas. I then proceeded to examine it in the usual way in the voltaic eudiometer, and, after repeated and careful trials, fully satisfied

* Phil. Trans. 1832.

Fig. 1.

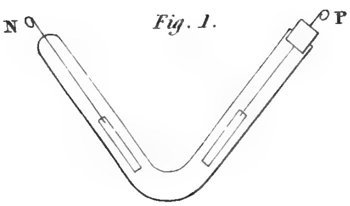


Fig. 2.

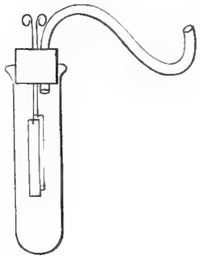


Fig. 3.

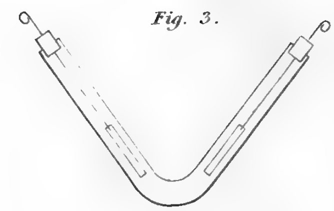


Fig. 4.

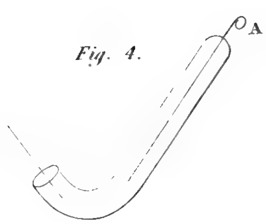


Fig. 5.

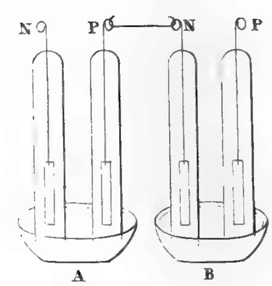


Fig. 6.

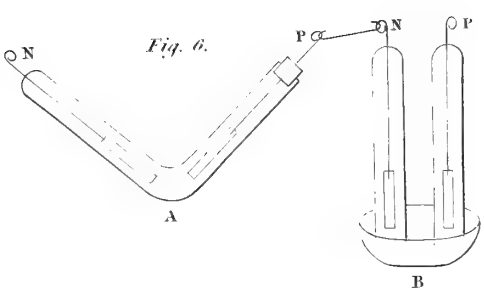
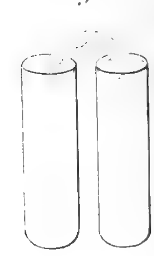


Fig. 7.



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myself that it consisted of somewhat variable proportions of hydrogen and of atmospheric air, or rather of the constituents of atmospheric air in relative quantities, also somewhat variable. The conclusion which I drew from this result was, that the gas evolved from the negative pole under the proper galvanic influence, was pure hydrogen, and that the oxygen and azote had been simply held in solution by the alcohol, and had been liberated along with the hydrogen in the manner which is known to occur when a gas is passed through a liquid holding another elastic fluid in solution.

Accordingly, I found that when the experiment was made without the contact of atmospheric air in a close tube, as Fig. 1, the proportion of atmospheric air mixed with the hydrogen was much diminished; and by acting on only a dram and a-half of fluid inclosed in a small tube, Fig. 2, and exposing the liquid, before commencing the galvanic action, to the vacuum of an air-pump, to separate the common air, I obtained a gas which, when mixed with half its bulk of oxygen, and fired by the electric spark, left no sensible residue after the explosion, and was therefore pure hydrogen. By placing the foils side by side in the manner done in this latter experiment, the quantity of gas liberated is considerably increased.

Precisely the same result was obtained by employing alcohol of sp. gr. .7928, at 66°F., as had been got with alcohol of sp. gr. 830. A dram and a-half of alcohol, sp. gr. .7928, having in solution from $\frac{1}{200}$ to $\frac{1}{250}$ of potash, yielded, when acted on in the tube, Fig. 2, by seventy pairs of four-inch plates, the poles being of platinum foil, and placed side by side at a short distance, a cubic inch of gas from the negative pole in less than a quarter of an hour, and $2\frac{1}{2}$ cubic inches in $2\frac{1}{4}$ hours from the commencement. The action was so intense, that the liquid boiled in a short time, although the lower part of the tube was kept plunged in cold water. The liquid also acquired a reddish colour soon after the commencement of the action and some white matter collected by degrees at the bottom of the tube, which proved to be carbonate of potash. The gas was found to be pure hydrogen. The reddening of the solution was due to the formation of some resinous matter, which is obtained when the liquid is afterwards mixed with a little water, and concentrated by evaporation.

Several circumstances tended to shew, at an early period of these researches, that the true nature of this action consisted in the decomposition of water contained in the alcohol, apparently as a constituent where absolute alcohol was acted on; the hydrogen being evolved at the negative pole, and the oxygen not appearing, from being employed in producing certain secondary effects. I found that there were several ways in which gas might be made to appear at the positive pole also. If alcohol considerably diluted, as with an equal bulk of water, or a little more, was acted on, a very feeble evolution of gas was observed from the positive pole, but quite insignificant compared to that from the negative. If the alcohol was stronger, as .835 or .840, and the proportion of potash was a little increased, as for instance if about $\frac{1}{100}$ potash was dissolved, then a slight effervescence ensued from the positive pole; and there was in general a greater tendency to an evolution of gas, when platinum wires were used as poles, than when foils were employed. A singular influence on the result was also observed to be produced by the nature of the vessel employed for containing the solution. Thus, if alcohol, sp. gr. .830, with $\frac{1}{500}$ of potash dissolved, was acted on in a vessel of glass or porcelain by platinum foil poles, it shewed, as usual, the effervescence at the negative pole, and none from the positive; but if the experiment was performed in a vessel of platinum, a feeble stream of bubbles was seen to rise from the positive pole also, but trifling compared to the negative gas. The same effect was produced by such other metallic vessels as I tried, which were those of silver, lead, iron, and copper. A similar appearance was observed in certain other equally singular circumstances. If, after the action had been going on for a few minutes in a glass or porcelain vessel, the poles being of platinum foil, the action was stopped by removing one of the wires from the battery, and when gas had ceased to come, the poles were reversed, the foil which before had been negative being made positive, and the former positive made negative, then a few bubbles arose for a minute or two from the positive foil, with the usual effervescence from the negative; and this might be repeated, after a sufficient interval, as often as was thought proper. The only explanation I can offer of these curious appearances is, that the negative foil, when suddenly made positive, still retains for a short time some portion of its former negative character, and therefore exerts a kind of repulsive action on the electro-negative substance the oxygen, at the very time when the electric current is determining its passage to the positive side of the pile. The action of the metallic vessels seems more difficult to explain, the property being possessed as well by electro-positive metals as by those of an electro-negative character; but probably depends on some peculiar electric influence exercised on the constituents of the alcohol, by which they are rendered less disposed to combine with the nascent oxygen.

These different modifications of the phenomena do not, however, I conceive, affect the constant nature of the action. The evolution of hydrogen from the negative pole is invariable, and the determination of oxygen to the positive pole I apprehend to be equally so, the only variation being in the circumstance of its being *visible* or not.

Before, however, attempting to develope this view farther, I wish to state the electric action on alcohol, holding small quantities of other bodies than potash in solution, and on pure alcohol.

I found that similar quantities of several other substances, soluble in alcohol, produced analogous effects to potash. Thus, when small quantities of chloride of calcium, of nitrate of lime, of iodide of potassium, of boracic acid, of arsenic acid, and of some other bodies, were dissolved in alcohol, hydrogen was evolved, as in the case of potash, from the negative pole, whilst none appeared at the positive pole; but in these cases the action was not so great as with potash. Thus, from $1\frac{1}{3}$ dram alcohol, containing about $\frac{1}{300}$ of recently ignited chloride of calcium, and acted on in the tube Fig. 2 by seventy pairs of four-inch plates, about one-third of a cubic inch of gas was obtained in half an hour. The negative foil then became coated with lime, and the action was almost entirely stopped; but it was again renewed on removing the coating from the foil, and one-sixth of a cubic inch more gas was got in from two to three hours. I shall afterwards notice more particularly the nature of the changes produced on the substances held in solution, when they appear to be decomposed.

Alcohol holding a little iodine in solution shewed no effervescence from either pole, when acted on by fifty pairs of two-inch plates, a power amply sufficient to shew the action with the other substances.

My next object was to endeavour to obtain a voltaic action on alcohol not holding any foreign body dissolved. The strongest galvanic power which I had at my command consisted of 216 pairs of four-inch plates;* and this power I found sufficient for my purpose. The alcohol acted on was placed in the tube Fig. 2, of the capacity of $1\frac{1}{3}$ dram. The platinum foil poles were about one inch long by one-fifth broad, and were soldered with gold to platinum wires, of about $\frac{1}{40}$ inch diameter, which passed through the cork. The bent tube served for collecting the gas. The

* All the galvanic batteries employed in the experiments detailed in this paper were fixed in mahogany troughs in the usual way. The charge employed was two measures of sulphuric acid, one measure of nitric acid, and about 100 measures of water.

alcohol acted on had the specific gravity of .7928 at 66° F.* To produce a proper action, it was generally necessary to bring the foils within one-twentieth or one-thirtieth of an inch of one another, although sometimes the effect took place at the distance of one-tenth. An evolution of gas was observed to begin sometimes almost immediately, and in other instances after the lapse of a minute or two, the difference in this respect, as well as in the requisite proximity of the foils, depending on the state of action of the battery, and the perfection of the various metallic connexions.

When action was obtained with the foils, at the distance of one-tenth of an inch, it was easy to observe that the gas came from the negative pole, none being liberated from the positive pole. The action was so intense, that the liquid soon began to boil, and this increase of temperature materially contributes to the decomposition which ensues. The gas was collected sometimes over water, and sometimes over mercury, the volatilized alcohol passing over at the same time, and condensing. The quantity of permanent gas liberated was small, from .2 to .3 of a cubic inch being collected in about an hour; and there was a limit to the quantity collected, arising from the boiling of the alcohol, which left the foils partly uncovered, as well as from the diminished action of the battery.

* I obtained the alcohol of this specific gravity by Mr Graham's process of exposing alcohol .830 to the vacuum of an air-pump with quicklime, the exposure being continued for some weeks, and the lime, which was common building quicklime, being renewed during the process. I could not get it of lower specific gravity. The specific gravity of .7928 at 66° F. appears, by Meissner's table, to correspond with .795 at 60° F., and with .792 at 68° F., or 20° C. In this country, absolute alcohol is usually reckoned to have a specific gravity of .796 at 60° F.; and on the Continent it is held to be of .791 at 20° C. Meissner, while he gives this latter as the specific gravity of absolute alcohol, states that the alcohol from grain, which is that used in this country, cannot be carried below .792 or .793.—*Gmelin's Handbuch*, ii. 276. The difference is in all probability due to the presence of a little of the volatile oil which grain and potato spirit contain.

The gas thus collected from the negative pole was examined both by chlorine and in the voltaic eudiometer, and was found, on repeated trials, to be hydrogen. There was usually mixed with it about one-ninth of common air or its constituents, derived, as in the former instance, from the liquid, and partly also, perhaps, from the water over which it was collected, when water was used for that purpose.

The alcohol acted on acquired a peculiar etherial odour; but I could not find that any farther change was produced on it than the admixture of a minute quantity of a yellow resinous matter, which was observed by mixing it with a little water, and evaporating nearly to dryness.

Although a battery of considerable energy is required, when it is intended to collect the gas evolved from pure alcohol, still it is possible to observe the action with smaller powers. Thus when alcohol .7928 was acted on by seventy-two pairs of four-inch plates in the tube Fig. 2, with foils parallel to one another, and from one-twentieth to one-thirtieth of an inch apart, gas was observed to arise in the course of one or two minutes; but the quantity was much less than with the greater power, and the liquid got only moderately warm and never boiled. Even with fifty pairs of two-inch plates, and the foils at the above distance, a minute stream of bubbles could be sometimes observed with a lens after action for a minute or two. With the foils one-tenth of an inch apart, alcohol of .830 shewed no action in the cold with this power, but when heated nearly to boiling and then acted on, a slight effervescence could be observed from the negative pole.

It is essential, however, that the foils be placed parallel to one another, as described in these experiments. If they were simply approached to one another horizontally, in an open vessel, I did not observe any action on pure alcohol even with the power of 216 pairs.

It is now time to recur to the nature of the changes which I conceive to take place during this agency; and I am the more anxious to endeavour to establish fully the view which I have taken of the nature of the action, from the obvious bearing which it has on the intimate constitution of alcohol. The essential change appears, as already stated, to consist in the decomposition, by the direct agency of the voltaic current, of water contained in the alcohol, and apparently entering into its constitution where absolute alcohol is acted on, the hydrogen being liberated at the negative pole, and the oxygen being employed in producing certain secondary changes in the liquid, and, therefore, not appearing at its proper place. In order to pave the way for understanding this view, it is necessary to recall to recollection the strong affinity which alcohol or its constituents have for oxygen, and the variety of circumstances under which we know that an oxidation of alcohol ensues, as well as the variation on the nature of the products, according to the energy of the oxidating circumstances. The most familiar example is in the acetous fermentation, whether it takes place under the ordinary circumstances, or be accelerated by platinum powder, in DÖBEREINER's process, the effect being truly an absorption of oxygen, and consequent formation of water and acetic acid, by oxidation of the constituents of alcohol. Another example is afforded by the action of nascent oxygen proceeding from oxide of manganese and sulphuric acid, the oxidation being in that case of a more powerful description and a more highly oxygenated acid, the formic being produced at the same time with the acetic. A third instance I had occasion to point out a year or two ago,* in which the disposing affinity of potash was sufficient to lead to the absorption of oxygen, from the atmosphere, by alcoholic solutions of potash, and to an oxidation sufficiently energetic to occasion the formation of formic as well as of acetic acid, and some resinous matter. By the agency of glowing platinum also, with access of air, ether and

* New Edinb. Philos. Jour. April 1833, vol. xxviii. p. 231.

alcohol are oxidated, and a liquid results which has been called the lampic acid, and which, in addition to acetic acid, contains, as I also had occasion to show, formic acid, to which its reducing properties are owing.*

Now, in the action of voltaic electricity on absolute alcohol, holding $\frac{1}{250}$ part of potash in solution, I have stated that hydrogen was evolved at the negative pole, and that a yellow resinous matter resulted, and a precipitation of carbonate of potash ensued.† These changes appear to be of the same character as those which result from the spontaneous absorption of oxygen by a strong solution of potash in alcohol, the differences being, that in the latter case the oxygen is derived from an external source, and the products of the oxidation are, besides the resinous matter, acetic and formic acids; while, in the former instance, the oxygen comes from the liquid itself, by the decomposition of water under the electric agency, and the products of the oxidation are the resinous matter, carbonic acid, and probably water. The latter, in short, is a still more energetic oxidation than the former, for carbonic acid is a more highly oxidated body than either acetic or formic acids, and if alcohol or ether were oxidated to the utmost possible pitch, the products would be carbonic acid and water. The oxidating power here is not only the disposing affinity of the potash, but the presence of nascent oxygen arising from decomposing water, and the operation of these causes is greatly promoted by the great elevation of temperature attending the action.

Accordingly, I have not been able to ascertain that carbonic acid is a product in the other cases of electric agency which I have mentioned, where so powerful a disposing affinity is not in action as when potash is present. I examined the gas liberated

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* New Edinb. Philos. Jour. April 1833, vol. xxviii. p. 231.

† Such a weak solution suffers no *spontaneous* change in the same time, even with contact of air.

from pure alcohol, under powerful galvanic agency, and collected over mercury, without discovering any carbonic acid in it. The only secondary product I observed in that instance was a minute quantity of resinous matter.

Nor is it always easy to observe the formation of carbonic acid, even where potash is present, when the galvanic action is less energetic, from not placing the foils side by side, or from using a smaller power. Thus, where alcohol .802, with $\frac{1}{250}$ of potash, was acted on by thirty-six pair of four-inch plates in the tube, Fig. 3, it was only after many hours' action that I got traces of carbonate of potash, the liquid acquiring also a pale yellow tint from the formation of resinous matter. It is in such circumstances of energetic agency as those formerly mentioned* that the true nature of the action is best seen.

I formerly mentioned the occasional appearance of gas at the positive pole, either when very dilute alcohol was acted on, or under peculiar circumstances, when alcohol of greater strength was employed. This gas I have assumed to be oxygen, from the circumstance of its appearing most readily when the alcohol was weakest, and from the whole bearing of the phenomena leading to the conclusion, that water was the immediate subject of the voltaic decomposition. My attempts to collect this gas proved all unsuccessful. In a platinum vessel, although a little gas, as already stated, was evolved from the positive pole, yet, in attempting to make it pass up into a tube, the very fine stream was gradually absorbed by the liquid, and nothing collected. I expected to be more successful by acting on alcohol of moderate strength, containing a rather larger proportion of potash dissolved. Alcohol sp. gr. .838 at 60°, having $\frac{1}{100}$ of potash dissolved, was acted on in the tube Fig. 4, the poles being platinum wire, and the wire A made positive by a power, in one experiment of

* Page 317–18

fifty pairs, and in another of 100 pairs of two-inch plates. The gas arising from the positive wire was observed to consist partly of a very fine stream of gas coming from the upper part of the wire, and partly of rather larger bubbles originating at the bottom of the wire, and increasing a little in size as they ascended, by running together. The fine stream never reached the top of the tube, being gradually absorbed by the liquid. The larger bubbles collected by degrees; but although the action was continued for above an hour, the quantity of permanent gas which was obtained was only .01 of a cubic inch, and, on examination, proved to be azote. Its origin is very manifest. The minute quantity of oxygen evolved at the positive pole, liberated along with it, as usual, some of the common air held dissolved in the liquid; and in ascending, and partly also probably after gaining the top, the oxygen was absorbed by the liquid, the azote only remaining. I tried also to separate the positive gas from solutions of chloride of calcium and of boracic acid in alcohol of .838, thinking that such solutions would exercise a less powerful absorptive action on oxygen; but the whole agency in such cases was so much diminished, that, in the case of chloride of calcium, amounting in different experiments to $\frac{1}{50}$ and $\frac{1}{25}$, a very feeble stream only appeared from the positive wire, and in the case of boracic acid amounting to $\frac{1}{50}$, no positive gas was evolved at all.

The evidence, however, from the whole phenomena of the various experiments which have been detailed, appears to me to be quite sufficient to shew, that the action consists essentially in the decomposition of water by the immediate electric agency, and I had fully made up my mind that such was the nature of the action before the appearance of Mr FARADAY's paper describing the voltaic arrangement, to which he has given the name of the Volta-electrometer. It then, of course, immediately occurred to me, that that arrangement afforded the means of bringing additional evidence in support of the view which I have taken.

Before applying it to this purpose, I thought it right to investigate the principles on which it was founded, and the degree of confidence which might be placed in its indications, and I shall afterwards have occasion to allude to this subject more particularly. In the mean time I shall only observe, that I have fully satisfied myself of the accuracy of Mr Faraday's view, that when the same electric current passes through two different aqueous solutions, and exerts a decomposing agency on the water of these solutions, the quantity of that liquid decomposed in each solution will be exactly the same; and hence, conversely, when the same quantity of hydrogen and oxygen, or of one or other of these gases, is evolved from two different fluids by the passage through them of the same current of electricity, we have strong grounds for concluding that in both cases water is the subject of decomposition.

In applying these principles to the present case, I employed tubes having platinum wires cemented into them by the blowpipe, and terminated by pieces of platinum foil of one inch long by one-fifth broad, soldered with gold to their extremities. Two of these tubes were filled with the alcohol under examination, and inverted in a small evaporating basin; whilst two others were filled with the aqueous liquid with which the alcoholic fluid was compared, and inverted as the others. These tubes were then connected as in Fig. 5. Occasionally, also, the alcoholic liquid was placed in the tube A of Fig. 6, having a wire cemented at N, and another passing through a cork at P; and this tube was connected with those containing the aqueous liquid, as in Fig. 6.

The experiments were made on alcohol of specific gravity .802, and also of specific gravity .796. The vessel A, Fig. 5, with its tubes was filled with alcohol of the former specific gravity, holding in solution $\frac{1}{200}$ of potash; and water acidulated with $\frac{1}{12}$ of sulphuric acid was placed in B. The tube P of the vessel B was connected with the positive pole of a battery of thirty-six pairs of four-inch plates; and the tube N of the vessel A was connected with the negative pole of the same battery; the other two tubes being connected with one another by a short copper-wire. The same electric current thus traversed both solutions. Gas slowly collected in all the tubes except the positive tube of the vessel A, containing the alcoholic fluid; and after an action of an hour and three-quarters, the negative tube of A contained .3 of a cubic inch, and the negative tube of B .35.

The experiment was repeated, placing alcohol .802 with $\frac{1}{230}$ potash dissolved in A, and water with $\frac{1}{230}$ potash in solution in B, the same galvanic power being employed. The gas in the two negative tubes increased very uniformly, and in one hour and fifty minutes, there was found in the negative tube of A .32, and in that of B .34. Both gases were analyzed by the electric spark, and were found to be hydrogen; that from the alcohol containing mixed with it, as I was fully prepared to expect from the result of former experiments, from one-fifth to one-sixth of common air, or rather of azote, derived from the alcoholic fluid, a corresponding portion of hydrogen having doubtless been absorbed by the liquid.

Thus, then, we see, that as nearly as possible the same quantity of hydrogen was evolved by the agency of the same current, whether the liquid acted on was water or alcohol; and I therefore conclude, on the principles of the Volta-electrometer, that in reality water was in both cases the subject of decomposition, an equal portion of it being resolved into its elements in both vessels.

Similar results were obtained with alcohol, specific gravity .796 at 60° F., having in solution, in different trials, small quantities of potash, of iodide of potassium, and of fused chloride of calcium. These different solutions were always compared with water holding the same quantity of the same substance in solution, the alcoholic solution being sometimes placed in the tubes of one of the vessels of Fig. 5, and sometimes in the bent tube A of Fig. 6, the power employed being usually thirty-six pairs of four-inch plates. On the first influence of the current, the negative gas from the aqueous solution sometimes increased a little faster than that from the alcoholic liquid, but the quantities were not very long in becoming equalized. When the same current was made to pass through alcohol of .796, holding $\frac{1}{100}$ of iodide of potassium in solution, and water holding the same quantity of that substance dissolved, the negative tube of the former liquid was found to contain in three-quarters of an hour .11 of gas, and the negative tube of the latter .13. In a similar trial, in which one-fiftieth of chloride of calcium was the dissolved body, each negative tube contained .037 of gas in the same time. The results with $\frac{1}{250}$ of potash closely corresponded with those already stated as to alcohol of .802, the negative gas of the alcoholic solution collected in the tube A of Fig. 6, being in an hour and fifty minutes .33, and that of the aqueous .37 in the same time; and in another trial the proportions were very similar. The negative gases, when examined, were always found to be hydrogen, that from the alcohol being as usual mixed with a small variable proportion of common air or azote. In one instance I found this mixture to amount to about one-fourth part; an equivalent absorption of hydrogen having without doubt occurred.

The various experiments which have been detailed, leave, I conceive, no doubt that water is truly the subject of the direct agency of the voltaic current transmitted through alcohol. Where the alcohol is concentrated, and holds no foreign matter in solution, the quantity of water decomposed even by a powerful stream is small, owing to the indifferent conducting power of the liquid. The effect on the needle of the galvanic multiplier when a stream of moderate power, as from fifty pairs of two-inch plates, was transmitted through absolute alcohol, was very trifling, but still perceptible. The galvanometer employed was of the original simple construction, consisting of a single magnetic needle, seven inches long, and suspended by silk fibres, in the centre of a coil of about thirty circuits of insulated copper wire; * and its indications were quite sufficient for my purpose, as my principal object was to detect such electric currents as might be the cause of chemical decomposition, and not more trifling streams, if such exist. When the current was powerful, as from 216 pairs of four-inch plates, the effect was considerably more marked, although still limited. When, however, minute quantities of certain soluble foreign bodies, particularly of potash, were added, the conducting power was much augmented, as was shewn by the increased action on the galvanometer, and by the great increase in the decomposing action. I have already shewn that $1\frac{1}{3}$ dram of absolute alcohol yielded with difficulty .2 or .3 of a cubic inch of hydrogen in an hour's action of the power of 216 pairs; whilst the very same alcohol, with only $\frac{1}{250}$ of potash dissolved, and with a power of seventy-two pairs, yielded one cubic inch of gas in one-fourth of that time.

It is remarkable how minute a quantity of potash is capable of causing symptoms of decomposition to be seen in alcohol, under circumstances where otherwise it cannot be observed. Thus, if absolute alcohol be acted on in a watch-glass by a power of fifty pairs of two-inch plates, by simply approaching the platinum foil poles to one another horizontally, no action whatever is observed when the alcohol holds nothing in solution; but the presence of even $\frac{1}{8000}$ part of potash renders a minute stream of bubbles visible from the negative pole.

I formerly mentioned, that, when a solution of iodine in alcohol is acted on by a moderate power, no symptoms of decomposition are observed. The effect on the galvanometer is, however, increased by the presence of the iodine.

* I compared the indications of this galvanometer with those of two astatic sewing needles, each two inches long, one of which was placed in the centre of a coil of twenty circuits, and found the indications of the long needle fully the more delicate.

II. *Voltaic Action on Ether.*

I have given this title to the experiments made with ether, although nearly the whole of them gave negative results. The ether acted on was rectified, first by agitation with water, and then by careful distillation from chloride of calcium, and possessed all the properties of pure ether. From the powers of ether as a solvent being greatly more limited than those of alcohol, the former presents many fewer opportunities of studying the influence of the solution of foreign bodies on the voltaic agency. It is usually said that ether is capable of dissolving a minute quantity of potash; but I found the quantity taken up to be very insignificant. The ether containing this scarcely perceptible quantity was acted on by fifty pairs of two-inch plates, but no symptoms of change were observed. Results equally negative were obtained with a moderately strong etherial solution of corrosive sublimate, and also with ether holding in solution as much dry chloride of platinum and as much dry chromic acid as it was capable of dissolving. Neither was any effect on the galvanometer observed with these liquids when they were acted on by the power of fifty pairs of two-inch plates. A dram of ether holding a few drops of bromine in solution, caused a slight deviation of the needle with this power; but no evolution of gas from either pole or other symptom of decomposition was noticed.

The action of the power of 216 pairs of four-inch plates was next tried on pure ether, in the tube, Fig. 2, both when cold and when heated near its boiling point; but no evolution of gas or other symptom of decomposition whatever was observed, although the action was continued for six or seven minutes; nor was the galvanometer affected.*

* This experiment was made with the long needle formerly mentioned. What might be the effect on a more delicate instrument I do not pretend to say.

In none of these experiments, therefore, was there the least appearance of those indications which led to the conclusion of the existence of water in alcohol; and it would appear, that, if the results were sufficient in the one case to warrant that conclusion, the experiments with ether equally justify the contrary inference, that water does not enter into the constitution of the latter fluid. If water existed as *such* in ether, I have little doubt, from the example of alcohol, that it would have yielded to the decomposing influence of the pile, and that we should have observed the evolution of at least one of its constituents; but in none of the analogous circumstances was such an indication observed.

III. *Some general considerations on the intimate Constitution of Alcohol and Ether.*

Every one knows that the elements of alcohol are in such proportions, that, when taken in conjunction with the specific gravity of its vapour, it may be represented by one volume of olefiant gas and one volume of the vapour of water condensed into one volume; and in like manner, that ether may be represented by two volumes of olefiant gas and one volume of aqueous vapour condensed into one volume. But it is one thing to say that these fluids may be thus represented, and another and a very different thing to hold that olefiant gas and water actually enter into their constitutions *as such* in these proportions. We at present know so little of the manner in which the ultimate elements of those substances which occur in or result from organic nature are united, that it is only with great caution that special views of their mode of combination should be received. Of late a great disposition has been manifested, particularly on the Continent, to adopt such particular views, and to express the constitution of those substances more closely allied to organic bodies, by what are called rational formulæ. The principal recommendation of this method consists in the greater clearness which it introduces into our ideas of the relative constitution of such substances; but undoubtedly there is the greatest possible risk that, when not founded on grounds strictly experimental, these rational formulæ may not give an accurate representation of what really exists. We have the authority of the highest names for the application of such views to alcohol and ether; but it is remarkable that these views, although founded on the same principle, have differed considerably in their details. GAY LUSSAC appears to have adopted the above-mentioned view, that alcohol consists of water and olefiant gas, and ether of water and a greater proportion of olefiant gas.* BERZELIUS modified this opinion,† by assuming that a peculiar hydrocarbon C^4H^8, which he called Etherine (Ae), having the same proportions of constituents as olefiant gas, but a greater total number of atoms, entered into the constitution of both alcohol and ether, which were both regarded as hydrates of this substance, alcohol being $Ae+2\dot{\underline{H}}$, and ether $Ae+\dot{\underline{H}}$. He afterwards saw cause to alter his view, and to assume alcohol and ether to be oxides of different radicles, the former C^2H^6+O, and the latter $C^4H^{10}+O$. ‡ LIEBIG has adopted this latter opinion in regard to ether, but has rejected it with respect to alcohol. || He considers ether as the oxide of a hydrocarbon (E), different from olefiant gas, and represented by C^4H^{10}, and alcohol as the hydrate of that oxide; *i. e.* a hydrate of ether. Ether is therefore $\dot{E}$, and alcohol $\dot{E}+\dot{\underline{H}}$; and he has given a view of the composition of all the compound ethers and other relative substances, which is remarkable for consistency, and its strong analogy with the laws of inorganic com-

* Annales de Chim., tom. xcv.

† Annales de Chim. et de Phys., tom. li. p. 313.

‡ Poggend. Annal. xxviii. 627.

|| Annales de Chim. et de Phys., tom. lv. p. 132.

binations. These different views, however, are rather to be considered as more or less probable *inferences* from experimental results, than as such experimental results themselves. Even the fact which they nearly all assume, that alcohol is a hydrate, is merely an inference like the rest; for, when in the process of etherification, the ultimate result is, that sulphuric acid takes water from alcohol, we cannot tell whether this water existed as such in the ether, or arose from a new arrangement of its elements by the affinities brought into play.

We are led, then, to inquire how far the researches detailed in the first parts of this paper are capable of increasing the probability of this inference, or of affording more direct experimental proof of the existence of water in alcohol? I confess I do not see how it is possible to avoid admitting that water was the immediate subject of voltaic decomposition in the experiments detailed. The hydrogen was evolved from the negative pole, in the same proportion as from water; the disappearance of the oxygen was accounted for; and, by particular arrangements, that element could even be made visible. The only remaining point, therefore, is, Is that water a constituent of the alcohol employed, or is its presence accidental? This point was investigated as carefully as was in my power. The alcohol acted on had, I have reason to believe, as low a specific gravity as alcohol from grain has ever been obtained, without decomposition,* and yet it still yielded hydrogen at the negative pole, under powerful voltaic agency, and more freely when its conducting power was improved by an insignificant morsel of potash. The legitimate conclusion, therefore, seems to be, that the water decomposed entered as such into the constitution of the alcohol acted on. I have not, however, the least wish to press this latter view farther than the circumstances may be supposed to warrant; and, if it can be afterwards shewn that it is possible to prepare alcohol which will

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* See Note, p. 321.

not shew the same appearances with powerful batteries capable of producing these effects on such alcohol as I employed, I shall be ready to admit that the question as to the nature of alcohol, remains as it was.

Assuming, however, in the mean time, that we have direct experimental proof of the existence of water as a constituent in alcohol, the next question is, With what is it combined? Is alcohol a hydrate of olefiant gas, as GAY LUSSAC supposes, or of ether, as LIEBIG maintains? Here, then, we are once more thrown into the field of inference; with this difference, however, that our speculations are no longer gratuitous; for, if alcohol is proved to be a hydrate, it must necessarily be a hydrate of something or other; and it is a legitimate subject of probable reasoning to inquire, with what the water is combined. On the whole, I incline to give the preference to LIEBIG's view, for the following reasons.

The experiments on ether lead to the conclusion that this latter body does not contain water. Not only did pure ether resist the strongest voltaic power which was brought into operation on it, but I could not find any substance capable of giving it such conducting power as led to any action which countenanced the supposition that it contained water. Now, the view that alcohol is a hydrate of olefiant gas has always been taken in connection with the idea that ether also is a hydrate of the same body, having a less proportion of water; and I think that there is little doubt that the supporters of these two views will be less inclined to adopt one of them if the other is held to be disproved. The specific gravity of the vapour of ether, the arithmetic mean between that of the vapours of alcohol and water, evidently favours, as LIEBIG has remarked, the idea of a less intimate union, such as that of a hydrate.

The theory of etherification is also much simplified by holding alcohol to be a hydrate of ether; for we have only to withdraw an atom of water, and the ether remains ready formed, without any change of elements; and the antecedent formation of sulphovinic acid, which is now held to consist of sulphuric acid and alcohol, scarcely interferes with the simplicity of this view.

Strong analogical grounds would undoubtedly be afforded for the same opinion, from the composition of the compound ethers, if we had good reason for holding, with Liebig, that ether is the oxide of an unknown radicle. But the galvanic experiments with ether, seem to me to be rather against that view; for it would appear that on electro-chemical grounds, such an oxide ought to give way under voltaic agency, by a separation of its electro-positive and electro-negative elements. Of such a separation, however, I obtained no indication whatever.

It may perhaps, therefore, be safer simply to regard ether as a ternary compound of its elements, and to express its constitution by an empirical formula $C^4H^{10}O$. Alcohol, on the other hand, may be regarded as a hydrate of ether, and its formula will be $C^4H^{10}O + \underline{\dot{H}}$.

IV. *Voltaic Action on Aqueous Solutions.*

In the course of the preceding investigations, I was naturally led into an examination of the nature of the voltaic action on solutions of various acids, alkalies, and salts in water; my principal object being to endeavour to distinguish between those cases in which the solvent and those in which the dissolved body was the immediate subject of the voltaic action, and thus to be better able to draw a similar distinction in the case of alcoholic solutions. In this inquiry, I was of course necessarily led to examine many of those experimental results and conclusions which have been unfolded by Mr Faraday, in his interesting series of electrical researches; and therefore I may be pardoned for stating such views as have occurred to me on this part of the subject, seeing that I did not gratuitously enter upon it, but was necessarily conducted into it, by a train of investigation which, in its origin, had no reference to this particular part of the inquiry.

From the important aid which Mr FARADAY's principle of the direct connection between the quantity and chemical action of an electric current, is capable, if well founded, of affording, in determining such questions as I had in view, this principle was, of course, one of my chief subjects of examination; and I found it necessary to endeavour to satisfy myself, in so far as regarded solutions, how far the evidence was sufficient to establish that the chemical action of an electric stream is proportional to the absolute quantity of transmitted electricity, and that a given quantity of electricity will decompose a chemical equivalent of the substances on which it acts.

For the purpose of bringing these principles to the test of experiment, Mr FARADAY has employed an arrangement to which he gives the name of the *Volta Electrometer*, of which there are several modifications, but all of them depending on the principle that the hydrogen and oxygen evolved from water under decomposition by a given electric current, are collected, and measured, and compared with the same or different elements, as the case may be, separated from the same or a different substance, which is under decomposition by the same electric stream.* Now, I have had ample opportunities of confirming experimentally the truth of the above law, in so far as regards water, and have no doubt whatever that the same current of electricity will always decompose the same absolute quantity of water, when it exerts its decomposing

* It is only doing justice to MM. GAY LUSSAC and THENARD to remember, that they long ago employed the quantity of the gases evolved by water under the agency of the voltaic current as a measure of its chemical effects (Recherch. Phys. Chim., Part i.) They did not, however, compare this quantity with other effects produced by the same current at the same time.

influence on that body. I have compared dilute sulphuric acid of a given strength, with the same acid of different strengths, with alkaline solutions, and with solutions of alkaline sulphates, by transmitting the same electric stream through them, and have found that the quantity of hydrogen evolved from the negative pole was always, with trifling deviations, a fixed quantity for the same current. The quantity of oxygen separating at the positive pole was subject to those variations from the absorption of the fluid, which Mr Faraday has pointed out; but the quantity in one experiment with another was evidently such as corresponded with the hydrogen evolved, after allowing for the absorption.

It is evident how important an aid this principle affords us, in explaining what passes during the electric decomposition of aqueous solutions; for if, in comparing with one another, solutions of substances of different atomic constitutions, we obtain hydrogen or oxygen, or both, in the proportions contained in water, we have strong grounds for concluding that the solvent, and not the matter dissolved, has been the subject of decomposition.

Accordingly, it is in this way that we are enabled to determine, with every appearance of probability, that in a great many, perhaps in all cases of solutions of the oxyacids, water only is the subject of direct voltaic decomposition. Thus the same voltaic current was passed through dilute sulphuric acid, and a solution of boracic acid, and it was found that very nearly the same relative quantities of hydrogen and oxygen were evolved from both solutions in the same time, notwithstanding the difference in the atomic constitution of the two acids. There was, therefore, little doubt that, in both solutions, the water, and not the acid, had suffered decomposition. Iodic acid appeared to me to be extremely well calculated for such an experiment, as the feeble affinity by which its elements are held together, afforded the most favourable circumstances for a direct decomposition of an oxyacid in solution, if such a decomposition is likely to occur. Two of the tubes (Fig. 5), formerly described,* were filled with a solution of one part of iodic acid, in about ten parts of water, and inverted in an evaporating basin. Two others were filled with sulphuric acid, diluted with twelve parts of water, and also inverted. After connecting them together, as formerly described, the current from fifty pairs of two inch plates was passed through them. Iodine separated from the negative pole of the iodic solution, without any evolution of elastic fluid from that pole. Gas was liberated from all the other poles. In a quarter of an hour, the following quantities of gas were obtained:—

	Cub. In.
In the positive tube of iodic solution, ...	.14
.... sulphuric solution, ...	.12
In the negative tube of do. do.	.33

Now, here there can hardly be a doubt that, in the iodic solution, the water only was directly decomposed, and the iodine was a secondary product, arising from the reducing action of hydrogen. Had the iodic acid been directly decomposed, the quantity of oxygen liberated ought to have been five times greater than that got from the dilute sulphuric acid, in which we already know that water is the subject of direct decomposition.

The next point of inquiry is, whether, in the case of the hydracids, water or the acid is the subject of direct electric agency? and this question becomes one of considerable importance; because, if the acid is directly decomposed, the experimental results obtained afford an example of the application of the principle of definite voltaic action to another substance than water, whilst, if the water is decomposed, it is only another instance applicable to that liquid. It is with considerable hesitation that I have brought myself to differ on this point from so high an authority as the first propounder of the doctrine of definite voltaic action; and I have

* Page 327.

only done so after obtaining what appears to me to be experimental proof that the decomposition of the hydracids is a secondary result. That hydrogen is evolved in such cases in definite quantity, there can be no doubt whatever. I have repeatedly found, on comparing dilute solutions of muriatic acid and of hydriodic acid with dilute sulphuric acid in the volta-electrometer, that the quantity of hydrogen evolved was exactly the same from all these solutions. It is the question, whether, in the case of these hydracids, the hydrogen proceeds from the acid, or from the water, that affords room for difference of opinion. Mr Faraday contends, that it is derived from the acid, and that consequently the hydracids afford another instance of definite voltaic action. His reasons, which, at the utmost, are merely analogical, are, first, that chlorine and iodine combine with hydrogen atom to atom, as oxygen does; and, secondly, that chlorine can be directly separated from lead, potassium, &c. in the dry state, by electric agency. The first being an argument which can only be addressed, in its full extent, to British chemists, can hardly be held to be of great weight; and, with respect to the second, I think I shall soon make it at least doubtful whether one substance, namely, iodic acid, although it resists decomposition in solution, may not give way in the dry and fused state. But it is unnecessary to dwell longer on mere argument from supposed analogy; for I think I can bring forward a few experiments on the hydracids themselves, which appear to lead directly to the conclusion that the evolution of chlorine and iodine during voltaic action on solutions of those acids, is a secondary result, and that the hydrogen is derived from water directly decomposed by the electric current.

Into a tube of the capacity of one and a half dram, moderately strong muriatic acid was poured, and another tube of the same size was filled with distilled water. These two tubes were connected, as in Fig. 7, by asbestus moistened with distilled water; the asbestus having been previously washed with diluted muriatic acid, and then thoroughly freed from all traces of acid by water. The muriatic acid was then connected with the negative side of fifty pairs of two-inch plates, and the distilled water with the positive, when a slight effervescence took place from both poles. After a quarter of an hour's action, not the slightest trace of chlorine could be detected in either tube, either by the smell or by bleaching action on test-paper, but a feeble trace of acid was shewn by test-paper in the positive tube. After an action of one hour and a quarter, there was still not a trace of chlorine in either tube, and the reddening action on test-paper of the positive liquid was somewhat increased. In nine hours, the reddening action of the water was still more decided, and a very feeble and doubtful odour of chlorine was observed in it. Now, what inference is to be drawn from this experiment. I apprehend the following is the explanation. The slight effervescence which took place from both poles, arose from decomposed water, its elements going to their proper poles. The muriatic acid was not decomposed; had it been so, the chlorine would have immediately passed towards the positive pole, and would have betrayed itself in one or both tubes by its usual characters. If, only after nine hours' action, a doubtful trace of it could be observed in the positive water, its origin was due to the secondary action of the oxygen on the muriatic acid, which had passed over partly by the natural tendency of the acid to the positive side, and partly by capillary action. That this is the true explanation, will still farther appear from the following experiments. Muriatic acid, diluted with two or three times its bulk of water, was placed in A as before, and water acidulated with a few drops of sulphuric in B, with which latter liquid the connecting asbestus was moistened. The diluted muriatic was made negative, and the acidulated water positive. A brisker effervescence than before took place from both poles, from the better conducting power of the positive liquid; but still in ten minutes not a trace of chlorine could be detected in either tube, either by the smell or by test-paper. The battery was then reversed, the muriatic acid being now connected by the same platinum foil as before, with the positive side of the battery, and the acidulated water with the negative. A brisk effervescence took place as before from the negative pole, and a slight effervescence from the positive, and in less than two minutes, a decided smell of chlorine was evolved from the positive tube, and shortly after test-paper held close to the positive foil was bleached, and the muriatic liquid got a yellow tint from dissolving chlorine. In this experiment it is evident that, before the reversal of the poles, no decomposition of muriatic acid took place, and that the whole, or greater part, of the oxygen which was evolved before the reversal from the positive wire, was employed *after* that change in reducing muriatic acid by combining with its hydrogen and liberating chlorine. When the diluted muriatic was at once connected with the positive pole of the battery in fresh action, and the acidulated water with the negative side, the smell of chlorine was even sooner, and more distinctly observed.

In another experiment, I examined more particularly the gas evolved from the positive pole, when the muriatic acid was positive, and found it to be chlorine. A small evaporating basin containing muriatic acid diluted with twice its bulk of water, was connected with another evaporating basin containing sulphuric acid diluted with five parts of water, by asbestus moistened with the latter fluid. The negative foil was placed in the dilute sulphuric acid, and the positive foil brought under a tube filled with the dilute muriatic acid, and inverted in the basin containing the latter liquid. The gas evolved from the positive pole was shewn to be chlorine, by being gradually entirely absorbed by the liquid, and by the strong smell of chlorine which the liquid acquired.

The experiments with hydriodic acid led all to the same views, and, from the phenomena being more apparent to the senses, were even more satisfactory.

A moderately strong solution of recently prepared and colourless hydriodic acid was placed in A, and connected with the negative side of fifty pairs of two-inch plates. Distilled water was placed in B, and connected with the positive side; and the connexion was made by asbestus, moistened with distilled water. A slight effervescence ensued from both poles. During the first ten minutes, there was no appearance of discoloration of the liquid in either tube. A slight yellow tint then appeared in the positive liquid, and no change in the negative, and shortly after, a just perceptible acid reaction on test-paper was observed in the positive liquid. In a quarter of an hour, the battery was reversed in the manner already described as to muriatic acid. Instantly, notwithstanding the diminished action of the battery, the hydriodic solution which had been previously colourless, acquired a brownish colour round the positive pole, and not a bubble of gas appeared from that pole, whilst gas rose from the positive foil. The brown colour gradually diffused itself through the liquid.

This experiment was now repeated with the difference that the level of the distilled water was made about a line higher than that of the hydriodic solution, which was perhaps a little weaker than in the preceding experiment. A slight evolution of gas soon arose from both poles, and in a quarter of an hour there was not the slightest discoloration of the liquid in either tube, nor did the water shew any acid reaction. The battery was then reversed. The hydriodic solution was immediately discoloured round the positive foil as before, without any liberation of elastic fluid, whilst there was a slight effervescence from the negative foil.

This last experiment was repeated with a power of thirty-six pairs of four-inch plates, with precisely the same result.

The explanation of these experiments seems very apparent. When the hydriodic acid solution is in connexion with the negative side, and the water with the positive, water is resolved into its elements, but the acid is not decomposed. If it were, iodine would have immediately made itself manifest. If after ten minutes in the first mentioned experiment a trace appeared in the positive water, this was due to a small quantity of the acid itself which had passed over, and was reduced by nascent oxygen, as is evident from the acid action on test-paper which soon followed, and from the circumstance that no iodine appeared in a longer time, when precautions were taken to diminish the quantity of acid passing over. On the other hand, the instant the acid was made positive, iodine appeared, and oxygen disappeared, the latter giving rise to the former by combining with the hydrogen of the acid.

From these experiments, therefore, I consider myself entitled to conclude, that, when solutions of muriatic and hydriodic acids, and doubtless of other hydracids, in water are submitted to voltaic agency, the chlorine, iodine, or other electro-negative constituent of the acid, is a secondary product, and the hydrogen arises from water directly decomposed by the voltaic agency. This action, consequently, does not furnish an instance of definite voltaic action on the hydracids, but is merely another example of the application of that principle to water.

Questions of a similar nature arise relative to the decomposition of the haloid salts, and are also of importance; because, if chlorides, iodides, &c. are dissolved as such, and are directly decomposed by the electric current, we have, as before, an instance of definite action in regard to another class of substances than water. Mr Faraday inclines to the affirmative of this opinion, although not with the same confidence as in regard to the hydracids. The fact is equally certain in this instance, that the hydrogen is evolved in a fixed proportion, as I have verified for alkaline chlorides and iodides; but, by a set of experiments of the same description as those detailed in regard to the hydracids, I have been led to the conclusion, that the chlorine and iodine evolved in the case of solutions of the haloid salts also, are secondary products, and that the hydrogen arises from the direct decomposition of water.

A rather weak solution of chloride of potassium was placed in a tube of the same size as before, and distilled water in another similar tube, its level being a little higher than that of the saline liquid, and the connexion made by moistened asbestus. The saline solution was then made negative, and the water positive, by a power of fifty pairs of two-inch plates. Gas appeared in one or two minutes from both poles. In half an hour, not the least trace of chlorine could be detected in either tube by the smell or by any bleaching action on test-paper. The battery was then reversed. An effervescence took place from the negative pole, but none from the positive, and in half an hour more, notwithstanding the diminished action of the battery, a faint odour of chlorine was discerned.

In a repetition of the experiment connecting the solution of chloride of potassium with the negative side, and the water with the positive, gas soon appeared as before from both poles, and alkali was very soon afterwards detected on touching the negative foil with test paper, and acid on touching the asbestus on the positive side of the negative tube, shewing that acid was travelling towards the water in the positive tube; but no smell of chlorine was detected. In twenty minutes a doubtful trace of acid was found in the positive liquid, but still no trace of chlorine. In two and a half hours the trace of acid in the positive tube was still slight, and a very doubtful trace of the odour of chlorine observed. In seven hours an acid reaction and a feeble odour of chlorine were quite manifest in the positive liquid; but not the slightest trace of chlorine in the negative tube.

In these experiments it is manifest, that when the saline solution was negative and the water positive, chlorine was not liberated until acid had passed into the positive tube, so as to enable the oxygen there evolved to occasion the appearance of the chlorine by a secondary action. When the saline solution was positive and the water negative no oxygen was evolved, but it was employed in causing the secondary appearance of chlorine which was observed at a far earlier period than when the reverse arrangement was adopted.

When water acidulated with sulphuric acid was substituted for pure water, and used for moistening the asbestus, the effects were much more marked. The saline solution was made positive, and the acidulated water negative; there was effervescence from both poles, but no smell of chlorine, or bleaching action in thirteen or fourteen minutes. The battery was then reversed, when a distinct smell of chlorine was observed in less than one minute, with slight effervescence from the positive pole and stronger from the negative, and in a few minutes more, test-paper was bleached in the neighbourhood of the positive foil.

A rather weak solution of iodide of potassium was then rendered negative, and distilled water positive, the connexion being made by asbestus moistened with water. A slight effervescence ensued from both poles. In half an hour there was not the slightest discoloration of the liquid in either tube. The battery was then reversed. Instantly the liquid near the positive foil in the saline solution was discoloured, without any evolution of gas from that pole, whilst gas appeared from the negative foil. The iodine gradually increased in the positive liquid.

These results were quite analogous to those with chloride of potassium. When the solution of the iodide was negative, and the water positive, no iodine appeared, but gas was evolved from each pole by the decomposition of water. On the reversal of the battery the oxygen ceased to come, and iodine appeared in its place by its secondary action.

The question with what substance the oxygen combines to cause the separation of the chlorine and iodine, depends on the point whether chlorides and iodides are dissolved as such, or as muriates and hydriodates. If in the former state, then the oxygen must combine with potassium, and liberate the electro-negative constituent of the haloid salt. If in the latter condition, then it will unite with the hydrogen of the acid of the dissolved salt and liberate the other element. Either view will explain the appearance of the chlorine or iodine.

It is evident that we can easily explain, conformably to these views of the secondary origin of chlorine and other analogous substances in solutions of the hydracids and haloid salts, the greater facility with which it has been observed that some of such solutions are decomposed, than water acidulated with an oxyacid. The attraction of the element with which the nascent oxygen, or in the case of ordinary metallic solutions, the nascent hydrogen, combines, will aid the electric action; and the more feeble the existing combination of that element, the less will its union with the nascent oxygen or hydrogen be opposed, and the more will the voltaic agency be facilitated.

We can also explain, in a more satisfactory manner, on these views, that on the idea of the primary origin of the chlorine, the known fact, that in strong solutions of muriatic acid or of chlorides, chlorine separates at the positive pole with scarcely any accompaniment of oxygen, whilst in weak solutions of these bodies a mixture of these gases is liberated. When the nascent oxygen finds around it abundance of the dissolved body, it enters wholly into the new combination; but in weak solutions the overplus is liberated along with the chlorine produced. Analogous views apply to the combinations of iodine.

If the above views as to the haloid salts are well founded, it follows that the appearance of hydrogen in definite quantity in the voltaic decomposition of solutions of those salts is only, as in the case of the hydracids, another example of the definite decomposition of water.

It is therefore to the experiments on dry substances that we must look for the proof of the application of the principle to other bodies. On such substances I have made no experiments, with this particular view, and therefore am not entitled to make any other observations on Mr Faraday's researches on them, than to observe that many of the experiments, particularly those with the chlorides and iodides of lead (814, 818, 794), the chloride of tin (819, 789), and the borate of lead (799), undoubtedly appear to lead to the establishment of this highly important principle with respect to other bodies than water.

We must, however, take care that we do not extend our ideas of this definite action farther than any of the experiments which have been adduced in support of it by its author will warrant, a misconception into which I have reason to believe that some who have not fully considered the evidence have fallen. One limit to it has been set, on the ground that the electric action itself is supposed to be capable of decomposing such bodies only as are composed of the same, or at least a like number of atoms of their elements, a point to which I shall presently advert. Accordingly there are no grounds whatever for holding that any definite action applies to such bodies as the great class of oxyacids and many other substances, admitted on all hands to consist of an unequal number of elementary atoms, and if such bodies are really undecomposable, no such definite action of course is possible. But, farther, there is as yet no evidence of the application of definite voltaic action to the extensive class of ordinary salts consisting of an acid and an alkali, and yet many of them are composed of a like number of chemical equivalents, and are undeniably subject to electric decomposition. According to the experiments hitherto made, the protoxides, water included, and the principal haloid salts, are the chief examples of the application of the law, and for this reason, that these are the principal substances on which the voltaic current operates.

The point to which I have just alluded, whether the decomposing agency of the electric fluid reaches only to substances composed of a like number of elementary atoms, is second only in importance to the law of definite action, and would require very conclusive evidence before it could be adopted as a universal law.

Independently of the consideration, that, according to almost all the continental chemists, the principal haloid salts are not composed of a like number of atoms, there is at least one substance, the oxide of antimony, which Mr FARADAY found to be decomposed, in its fused state, by voltaic action, and which has been hitherto held, by the universal opinion both of British and continental chemists, to be a sesqui-oxide.* The oxide of bismuth, it is also extremely likely, is in a similar situation, according to the views on the Continent, as to its atomic constitution, which the specific heat of the metal renders very probable.

Neither does it necessarily follow, because a substance is not decomposed in solution, that it will not give way in the dry and fused state; because, if water is in its own nature more susceptible of voltaic agency than the dissolved body, the action may be limited to the solvent when the solution is acted on. Assuming potash to suffer direct decomposition in its fused state, we have no reason to suppose that it gives way in solution. Our views as to the oxyacids will therefore be taken, from experiments made on them in the anhydrous and fused state, which, however, it is not always very easy to accomplish.

Arsenic acid I could not succeed in freeing from water without decomposing it. Of anhydrous sulphuric acid I possessed too little to attempt any experiments with it.

I made some experiments on dry and fused iodic acid, which, from the slight affinity of its constituents, I thought likely to throw light on the subject.† A difficulty, however, presented it-

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* The attempts to show that it contains traces of a protoxide are evidently too imperfect to be entitled to regard, in their present state.

† The iodic acid was prepared in the usual way by the agency of nitric acid, and was several times alternately dissolved in water and evaporated to dryness, to free it from nitric acid.

self, arising from the circumstance, that, when completely freed from water, its points of fusion and decomposition are extremely near one another. I freed it from water by keeping it in a fused state, in a tube, for a considerable time after a portion of the acid was decomposed, and until water was no longer evolved. The residue was dry and hard, and was immediately transferred to a long bent and narrow tube; where platinum wires connected with the two ends of a battery of fifty pairs of two-inch plates were brought into contact with it, the before-mentioned galvanometer being also introduced into the circuit. The iodic acid was then heated to fusion by a spirit-lamp, when immediately a considerable and even permanent deflection of the needle took place. Although it was thus quite manifest that a current passed, it was impossible for me to say with certainty that the acid was decomposed by the voltaic agency, because the heat applied was itself sufficient to cause decomposition and volatilization of iodine on both sides.

I repeated Mr Faraday's experiment on fused boracic acid, and at first thought that I had succeeded in decomposing it. The acid was fused on platinum wire in the reducing flame of a large lamp of melted tallow, acted on by a hydrostatic table blowpipe, the voltaic power employed consisting of 216 pairs of four-inch plates. There was an evident action on the galvanometer, and sparks were visible from the fused acid. Similar results were afterwards obtained with only thirty-six pairs of four-inch plates. But, on repeating the experiment with the 216 pairs, and fusing the acid by the oxyhydrogen blowpipe, I was surprised to find that there was no action on the galvanometer. The idea which I then formed was, that the reducing action of the carbonaceous vapour of the lamp flame had aided the voltaic agency, but that in the oxyhydrogen flame, which is of course destitute of carbonaceous matter, the voltaic current alone could not produce the effect. This view, if well founded, would undoubtedly have been an example of voltaic agency, other affinities aiding the action, as is known to happen in other cases. But on heating the acid strongly in the oxyhydrogen flame, and then transferring it in its pure and transparent state to the table blowpipe flame, I could get no action on the galvanometer or sparks with thirty-six pairs of four-inch plates, whilst it remained clear and transparent, although I observed both, when it became dark coloured from carbonaceous matter derived from the flame. The most probable explanation, therefore, seems to be, although the case is not free from ambiguity, that the carbonaceous matter merely gives a certain degree of conducting power to the mass, and that this is the source of the action on the galvanometer and of the sparks. I have thought it worth while to detail the results, because they may at least serve as a caution to others in making similar experiments; if they are insufficient to alter our ideas of the non-action of the electric current on this substance.

In the whole circumstances, however, I cannot help having some doubts as to the universality of the law of the necessary correspondence between voltaic decomposition and equality of atomic constitution, although I am by no means prepared to reject it altogether, and, on the contrary, think that, to a great extent, it may be well founded. It certainly appears very probable, that, when the constituent elements of substances are nearly balanced in number, and particularly when that number is small, they will be peculiarly in that state of electric opposition of character which will make them susceptible of voltaic agency; and, on the other hand, where the number of atoms is considerable and unequally balanced, the mode of combination may partake more of that character of union of which organic nature appears to present numerous instances in its ternary and quaternary combinations, and in which the electric nature of the combined elements is probably less directly opposed, and will therefore be less adapted for voltaic action, and may very probably even afford many cases where it will be altogether excluded. Feebleness of affinity in the constituent elements may, however, sometimes supply the place of simple atomic constitution, by opposing less resistance to decomposing agency, and, on this ground, may be explained the decomposition of fused iodic acid, if that really is a case of decomposition; and it seems not unlikely that the circumstance that the oxide of antimony is easily reducible, is the reason why it is subject to galvanic action, although by no means a simple atomic compound. But I throw out these views merely as speculations on a subject which cannot be satisfactorily rested except on the sure basis of experiment.

There still remains another part of this memoir, relating to the nature of the changes produced by voltaic agency on certain of the substances held in solution by alcohol, particularly in more concentrated solutions. On this part of the subject I hope to be able to make a communication to the Society in the course of next session.

POSTSCRIPT.

Since the preceding paper was read, I have succeeded in obtaining alcohol of still lower specific gravity, by continuing the exposure in the vacuum of an air-pump with quicklime for eight weeks, and renewing the quicklime after the first week. The alcohol thus obtained had the specific gravity of .7928 at 62°$\frac{1}{3}$F., which corresponds with .7938 at 60°F. and with .790 at 68°F., or 20°C. A portion of this alcohol submitted to the action of 216 pairs of four-inch plates in the tube, Fig. 2, the platinum foil poles being at the distance of from $\frac{1}{50}$ to $\frac{1}{40}$ of an inch from one another, still yielded gas from the negative pole, although in smaller quantity than the alcohol of sp. gr. .792 at 20°C. That the quantity of gas was smaller, was merely owing to the circumstance that the conducting power of the former alcohol was inferior to that of the latter; for the addition of the most insignificant morsel of potash caused the liberation of a comparatively speaking abundant supply of elastic fluid, and that with a smaller voltaic power. When only $\frac{1}{1000}$ part of pure caustic potash was dissolved in the alcohol of .790, and it was acted on in the tube, Fig. 2, by seventy-two pairs of four-inch plates, the foils being $\frac{1}{20}$ of an inch apart, half a cubic inch of gas was collected from the negative pole in ten minutes, and the evolution still went on. This gas was found to be hydrogen as usual. But this was by no means the limit to the operation of the alkali in giving conducting power. With $\frac{1}{2000}$ of potash in solution, this alcohol still gave a considerable stream of gas; and when this solution was farther diluted with the same alcohol, so as to leave only $\frac{1}{10.000}$ of potash dissolved, the stream from the negative pole, with the power of seventy-two pairs, was scarcely diminished, and much more abundant than that from the alcohol holding nothing in solution, with the power of 216 pairs. The dilution might evidently have been carried much farther without destroying the effect of the alkali. Since alcohol of so low specific gravity thus still yielded hydrogen from the negative pole, additional confirmation is evidently afforded of the views set forth in the preceding paper, as to the intimate nature of alcohol and ether; and it appears to me that no reasonable doubt can any longer exist, that the researches which have been detailed afford experimental proof of the presence of water as a constituent in absolute alcohol, and of its absence in ether.

The Figures referred to in the preceding paper are contained in Plate XIII.

On the Expansion of different kinds of Stone from an increase of Temperature, with a Description of the Pyrometer used in making the Experiments. By Alexander J. Adie, Civil Engineer.

(*Read 20th April* 1835.)

For a long time I was anxious to know the rate of expansion of the common building-stone of this neighbourhood, as it is not given in any of the tables of the expansions of substances, because I have sometimes thought that the vertical cracks frequently seen intersecting rubble walls might arise from the contraction caused by a diminution of temperature. During a long-continued and severe frost which occurred in 1826, I thought the rents in a considerable stretch of wall, which I passed at all seasons, appeared more open than usual. This, however, was merely conjectural, and I paid no more attention to the subject until 1830, when, as I mentioned in a notice on the commencement of my experiments read at the meeting of the British Association in September last, an interdict of the Dean of Guild Court of Edinburgh rendered the rate of expansion of stone a matter of more importance than merely a curious philosophical speculation. The interdict was issued against a gentleman who wished to alter his property, by supporting the front on cast-iron pillars, in the manner now commonly followed in this city in converting the ground-floor of dwelling-houses into shops. The front of the premises alluded to was narrow, and of a great height; and the reasons given for interdicting the operations were, that the pillars had not sufficient strength to support the weight of the front, and that the difference of the expansion of cast-iron and stone was so great, that very prejudicial effects might arise from the use of such pillars in this situation. Mr Jardine, civil engineer, was desired to report to the Dean of Guild on the subject, and the proprietor requested my father to calculate the comparative expansibility of the cast-iron pillars and the stone, for a considerable alteration of temperature. Sufficient data were easily got for the rate of expansion of cast-iron ; but the only experiments which could then be found on that of stone, are contained in a short notice by M. Destigny, in the 7th volume of the *Quarterly Journal of Science, Literature, and Art.* These are not extensive, and have, moreover, been determined by taking the difference between the expansion of the stones and rods of iron and copper to which they were attached, thereby diminishing quantities naturally very small; and the difference was again augmented and rendered more visible by a double system of levers; a method liable to many objections in point of accuracy, even though the experiments were performed with great care.

Since the date of the interdict formerly referred to, the almost universal adoption of cast-iron supports in constructing elegant fronts for shops, and the perfect manner in which they have been found to answer the purpose, has given that practical refutation to the alleged objections which, in such cases, is always most to be relied on. And although I do not recollect that, in any instance, a fire has taken place in a building, the front of which was supported on cast-iron, yet, from the experiments I have made, it will be seen that, even under such circumstances, the iron is not likely to be hurtful from the excess of its expansion over that of stone. Nevertheless it appears to have been a common opinion till within a very recent period, and one which has been supported by names of the very highest authority, that the length of a rod of Carrara marble could not be sensibly altered in length by a change of temperature. This opinion was founded on a very beautiful speculation on the arrangement of the crystals of marble; and although none of my experiments tend to show that it is correct with regard to either of the specimens of Carrara marble which I have examined, yet the supposition may not be altogether groundless, as I have found no stone which expands less than the black marble from Galway in Ireland.

Since the commencement of my experiments, I have seen a notice in the *London and Edinburgh Journal of Science*, of a letter read on the 12th of March 1834 to the Geological Society of London by Mr Charles Babbage, in which the author states, that, from the experiments of Colonel Totten, recorded in Silliman's *Journal*, he has calculated a table of the expansion, in feet and decimal parts, of granite, marble, and sandstone, from which he finds the alteration in bulk so great, that, supposing the strata under the temple of Serapis to expand at the same rate as sandstone, and an increase of temperature equal to 100° to act on them to the depth of five miles, the temple would be raised 25 feet. He therefore concludes, that the different changes of level which this edifice has undergone in reference to the surface of the sea, may be accounted for, simply by supposing the temperature of the subjacent rocks to have been altered. The sandstone which Colonel Totten had experimented on has been even more sensible to change than any of my specimens; because I find that a similar increase of temperature acting on a mass of what the marble-cutters call Sicilian white marble, five miles thick, would produce an elevation of only 19.8 feet, or about $\frac{4}{5}$ths of the other; and it expands more than any other stone I have tried.

Before making any farther remarks on the experiments themselves, I shall give an account of the construction of my pyrometer, and of the manner in which the experiments were conducted, so that every one may be enabled to judge how far he may rely on the accuracy of the results at which I have arrived.

Description of a Pyrometer heated by a Current of Steam.

The pyrometer I employed was constructed of a large piece of an oak tree of very straight growth, which was squared to eight inches and a half. In order to get the wood as well seasoned as possible, I selected it from the best of the oak beams got in the old houses lately pulled down in forwarding the improvements of Edinburgh; and from the number of coats of size-paint which covered three sides of it, there could be no doubt that it had been long in an open situation. After it was worked to the proper shape, it was allowed to stand for some weeks, as it is sometimes said that, to remove the surface from timber, will cause it to warp, however well it may have been seasoned beforehand. The stand for this instrument, however, did not alter; and even smaller pieces of the same tree, and of another tree from the same place, which were accurately worked into other parts connected with the experiments, have not altered in the least degree. I therefore concluded that the wood had been very long kept in a dry situation. To the oak stand A, which was $4\frac{1}{2}$ feet long, all the other parts of the instrument were attached. B is a double metal case containing the specimen C under experiment. The bottom of the case B rests in a strong brass plate, which is fixed on the top of a block of oak 8 inches broad and 11 long, and the block is strongly hooped and bolted with iron to the stand. The upper part of the case B is fixed to the stand by a brass clamp. Fig. 2. is a cross section of the double case of the full size, and the part marked *ss* is the space through which the current of steam passes: so that the specimen C, placed within the inner case, can be heated by the steam, and yet kept quite dry. P, Fig. 1, is the pipe by which the steam is brought from the boiler and thrown into the bottom of the double case; and DD are eduction-pipes, from the top and bottom of the case B, by which the steam is carried off and thrown into the chimney, to prevent the atmosphere of the room from being rendered damp from its escaping into it. On each of the eduction-pipes DD there is a stop-cock E, to regulate the quantity of steam which ought to pass off from the top and bottom of the double case, in order to keep the thermometers TT, which are inserted into the top and bottom of the inner case, at the same height; and by regulating the

Fig. 1.

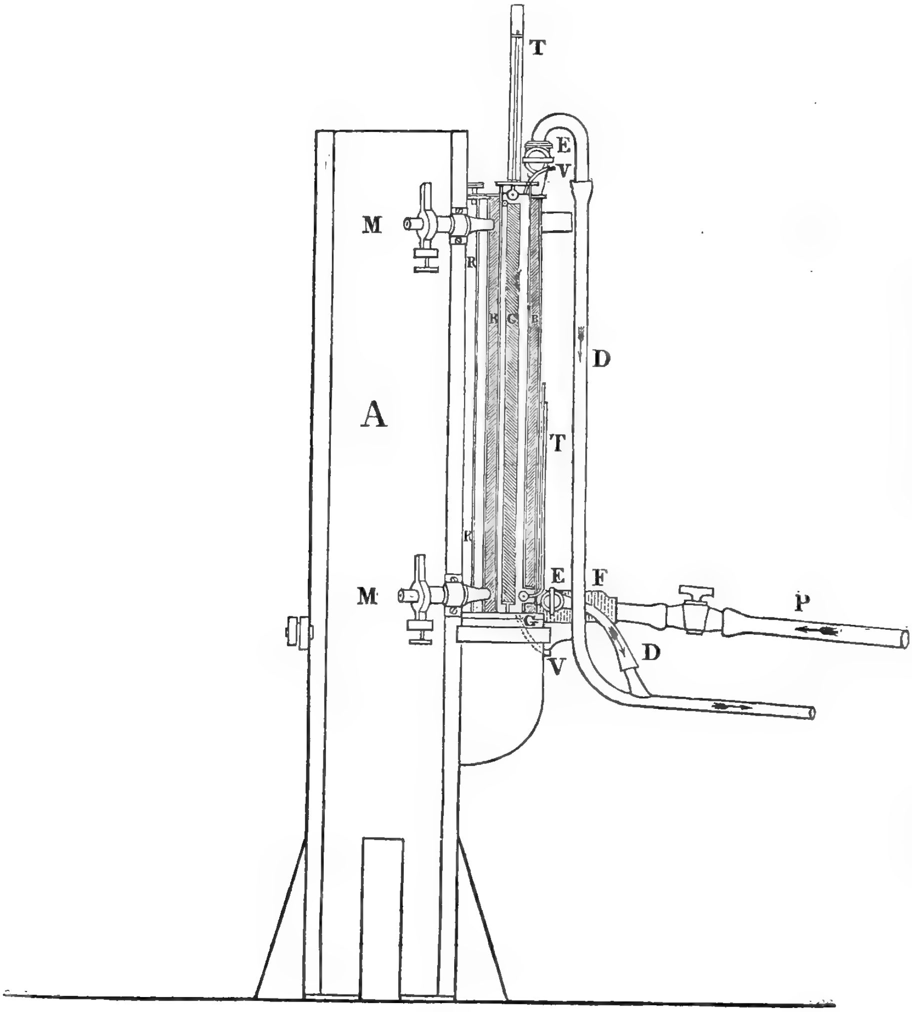
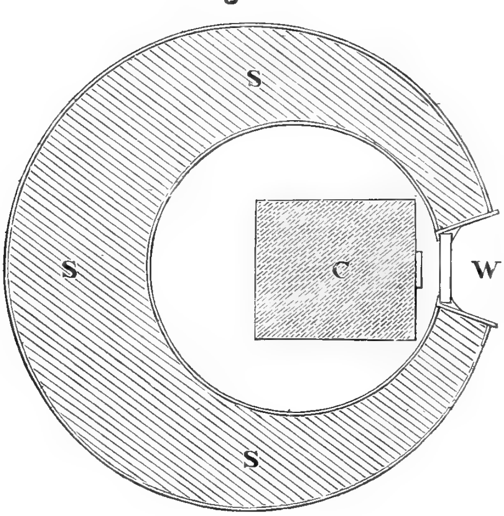


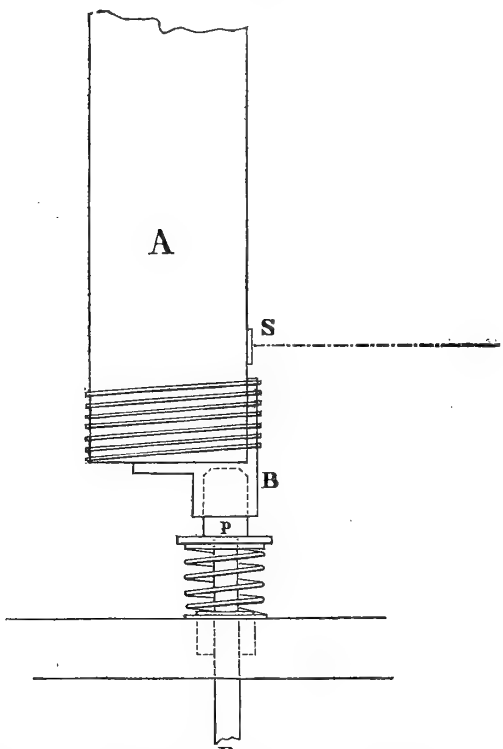
Fig. 2.



openings of these stop-cocks, the thermometers may easily be kept at the same temperature for any length of time. MM are the micrometer microscope, strongly fixed to the stand A, and opposite to plate-glass windows W, Fig. 2, which are placed in the double case B, so as to suit the lengths of the rods of which it was wished to measure the expansion. The upper micrometer reads the thirty-thousandth part of an inch, and the under one was intended to have been used to read a much smaller quantity; but it required great care to draw the lines on the silver studs on which the lengths were laid off, so as to make the upper micrometer read to the same division; it was therefore found more convenient to employ the under micrometer as a fixed one, by moving the zero line of the under stud on the rod, till it intersected the angle of the cross spider-threads in the under micrometer. This was done by the screw G working on the long end of a lever, which raised and lowered the rod under experiment; the lever, besides reducing the quickness of the screw G, gave it a more convenient position, and made the rod move without any tendency to turn. The whole body of the double case B was covered with a jacket, about the third of an inch thick, made of several plies of flannel; and all the metal parts of the top and bottom, as well as the upper stop-cock E, and the greater part of the upper thermometer, were also thickly covered with flannel. This was found necessary to get the upper and under thermometer to stand at the same temperature. There was a screen R of polished tin-plate interposed between the stand supporting the micrometer microscopes and the double case B, in order to prevent any heat from being radiated from the one to the other. Also, in order to keep the stand of the pyrometer at the same temperature during the experiment, it was placed near the window of the room, and the furnace for the boiler was built in a common fire-place, the front of which was closed with a screen of thick boards; and the polished tin-pipes which conducted the steam from the boiler to the instrument and back again to the chimney passed through holes in the screen-boards. A detached thermometer was hung over the instrument, and it was always kept at the same height by admitting more or less air by the window. In order to have the same direction of light during each experiment, I employed two small lamps, placed about a foot from the instrument, and their light was thrown in at the windows by lenses and reflectors. The reflectors were fixed on soft wires, so that they could be bent to reflect a bright light from the bottom of the lines on the studs. All the parts of the instrument were made as heavy as consistent with convenience, to prevent their being affected by sudden changes of temperature.

The rods of which the expansions were determined varied from half an inch to an inch square, and the length of twenty-three inches was carefully laid off on silver-studs, which were fixed into holes drilled in the rods, the centres of the holes being twenty-three inches apart. It was necessary to divide the head of the upper stud into fiftieths of an inch, in order to determine the value of the micrometer for each experiment, as it was liable to a little variation, from the impossibility of adjusting it to exactly the same focus, which it had when it was adjusted to a fixed value. To guard against this error arising from alteration of the focal distance, I read the micrometer for $\frac{3}{50}$ of an inch on the stud in the rod which was to be heated; and this was repeated at each experiment made on the rod, after every thing had been adjusted. In placing the rod in the instrument a socket was tied to the lower end below the lower stud, and the socket was fitted so as to slip tightly upon the top of a square head, on the end of a pin P, Fig. 3, moved by the lever and screw G, Fig. 1. This served to raise and depress the rod, and was very convenient in fixing it in the proper place, for the under microscope. In Fig. 3, A is the lower end of the rod, S the under stud in it, B the socket, which is tied to A, by the piece on the front, so that it was of no consequence what the thickness of the rod might be, as the stud was

Fig. 3.



always in the same position in the instrument. *p* is the square pin fitting into the socket, and moved by the screw G, Fig. 1. The upper part of the rod was kept in the proper position, by means of slender springs of wire, which pressed a small roller against the front of the case, so that the upper stud always stood at the same distance from the object-glass of the upper micrometer. This is shewn by Fig. 4, which is a section of the upper part of the double case of the full size. A is the upper end of the rod under experiment. R the roller pressing on the front of the double case. SS the space through which the steam passes. B the upper stud opposite to the window and upper micrometer M. CC the back spring, E one of the side springs. F the cover of the top, which is double; and the space between the upper and under plate is stuffed with cotton. T the bulb of the upper thermometer. V is the upper pipe, by which any vapour coming from the enclosed rod may escape, when it is wished to allow the

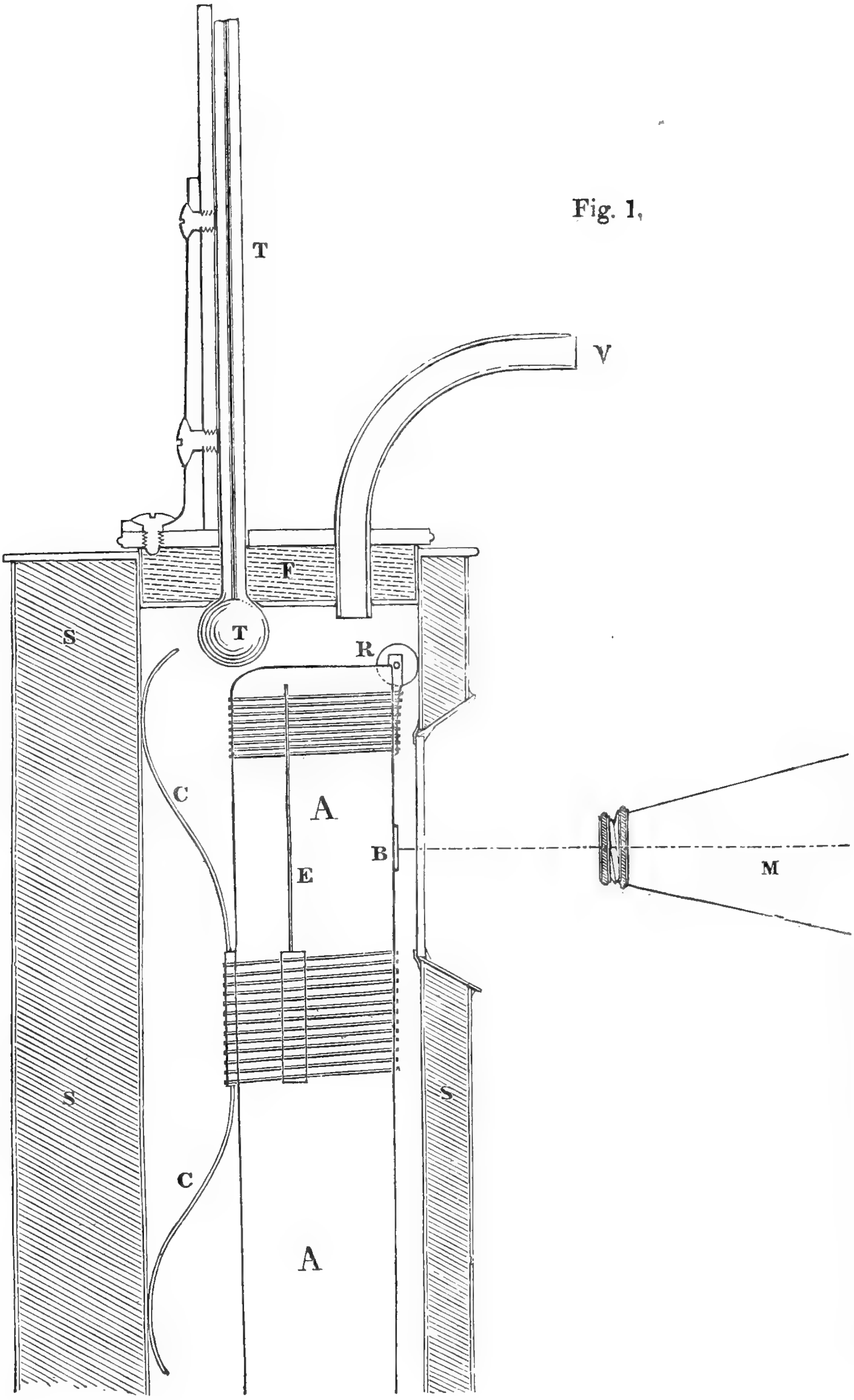


Fig. 1.

rod to become drier; but the whole of the inner case is so tight as to prevent any evaporation, unless the upper pipe V, and the under one of the same size, V, Fig. 1, which passes through the bottom plate, are left open. The springs and roller represented in this figure were firmly tied on the rods, and could easily be shifted from the one to the other It was my intention to have ascertained the expansion of the different rods between the freezing and the boiling points, by first filling the space SS, Fig. 4, with melting ice, and then by passing a current of steam to get the higher point, but the difficulty of getting ice until a few weeks since made me abandon this; besides, I found that, to keep the thermometers above 208° F., sent a great heat through the room, on account of the strong fire it was necessary to keep up. As the thermometers were made with every precaution to insure their accuracy, I contented myself with the range I could get between the temperature of the room, which was generally under 50°, and 207° or 208°.

The rod was put into the pyrometer, the micrometers set, and allowed to stand over the night, and the lowest point taken at the height at which the thermometers stood next day, by making the line on the under stud intersect the angle of the spider threads of the lower micrometer; and the index error of the line on the upper stud was read from the zero of the upper micrometer: This was repeated four times, by deranging and again adjusting the height of the under line by means of the screw G.

After completing the readings, the steam was turned into the case, and the expansion of the rod measured by a similar set of readings, as soon as the line on the upper stud was found to be stationary, which sometimes required the temperature of the pyrometer to be kept at the same degree for an hour and a half.

I thought that my application of steam for heating a pyrometer was the first time it had been done, as I could find no account of its having been formerly used in any of the descriptions of the instruments for similar operations; but Lieutenant Murphy informed me, that he thinks it was employed in some pyrometric operations connected with a trigonometrical survey in India; and I am satisfied, from the uniformity of temperature it maintains in the instrument, no better method of heating can be resorted to for such operations, because the substance, if of wood or stone, is kept dry, and the fire or lamps are placed at such a distance as not to incommode the operator in the least degree. Some of the rods of stone experimented on are now before the Society. I had considerable difficulty in making rods of Roman cement, as they repeatedly cracked in drying, and I was anxious to have this substance tried, as I found that rods of lime were too weak, not more to settle the general question, as to the expansion of buildings, than to have the pleasure, if possible, of assisting Mr Brunel in accounting for the results of some experiments he mentioned at the last meeting of the British Association, when describing one of the many beautiful contrivances for which his fertile ingenuity has rendered him so celebrated. I succeeded in making a rod of Roman cement, by at first mixing as little water with the powder as would make it work, and when it had become too plastic, from continued working, more of the powder was added, and the cement again made plastic by working and beating. This process was continued so long as any of the cement in powder could be added to the mass, and the whole again worked into the consistency of soft putty, without any addition of water, as the strength of the mortar appears to depend on the smallness of the quantity of water with which we can make the dry powder plastic, this is accomplished by long continued working. After preparing the mortar, it was put into a mould of Bristol board, and when it had set, it was immersed in water, and left for a fortnight to harden.

Having ascertained that all the parts of the pyrometer worked well from the uniformity of the results I obtained in determining the expansion of cast zinc, which was selected as the metal most sensible to a change of temperature, I commenced the experiments for which the instrument was made, but, before giving the results of those on stone, I may mention what took place in trying the expansion of a rod of oak, cut from the same tree of which the pyrometer stand was made. On account of a very small quantity of steam escaping through a hole in the inner case, the wood expanded very much the first time it was heated, and when taken out, and allowed to stand some days to dry, I found it lengthened about the thirtieth part of an inch. The inner case was made perfectly air-tight, even under considerable pressure, as it appeared absolutely necessary to prevent steam from escaping into it, for all experiments to ascertain the effect of heat on different kinds of wood; and I also wished to keep the rods of stone quite dry, to be certain what effect moisture had on their expansibility. When the rod of oak was again thoroughly dried, and the length of 23 inches laid off anew on the studs, it was found that for an increase of temperature of 180° Fahr. it only expanded .001426177 of an inch, or .000062007 in decimals of the length of the rod; which is just one-fifteenth part of the amount of the expansion of platinum, the least expansible of the metals. This insensibility to change of temperature in this wood, provided it be kept free from moisture, has induced me to investigate the subject a little farther I have procured rods of many of the most straight-grained kinds of wood, and after I have determined their rates of expansion, I intend to varnish or cover them with different substances, to find their expansion again, and then to see to what extent, and for how long, they will resist moisture, as it has been said that a wooden rod answers best for a pendulum when unvarnished, and left in its natural state.

The following Table gives the expansions of the different kinds of stone I have examined, and also of two rods of cast-iron. The one which expanded least, was cast half an inch square, and was put into the pyrometer with the outer surface rough as it came from the mould. The other rod was also half an inch square, but was cut out of the centre of a bar of the same iron cast two inches square, in order to get more nearly the rate of expansion of a large mass.

In the Table, the substances are arranged in the order of their rates of expansion, that which expands most being placed first.

TABLE OF THE EXPANSION OF STONE, &c.

	Decimals of an Inch on 23 Inches, for 180° F.	Decimal of Length for 180° F.	Decimal of Length for 1° F.	
1. Roman cement, .	·0330043	·0014349	·0000075	When the rod contained more moisture it expanded more.
2. Sicilian white marble,	·0325392	·0014147	·0000078	From first experiment, when moist.
	·0253946	·00110411	·00000613	Mean of three experiments, when dry.
3. Carrara marble, .	·0274344	·0011928	·00000662	From first experiment, when moist.
	·0150405	·0006539	·00000363	Mean of two experiments, when dry.
4. Sandstone from the liver rock of Craigleith Quarry, . .	·0270093	·0011743	·00000652	Mean of four do.
5. Cast-iron from a rod cut from a bar cast two inches square,	·0263755	·00114676	·00000637	Mean of two do.
6. Cast-iron from a rod cast half an inch sq.	·0253498	·001102166	·00000612	Mean of two do.
7. Slate from Penrhyn Quarry, Wales,	·0238659	·0010376	·00000576	Mean of three do.
8. Peterhead red granite,	·0220416	·0009583	·00000532	From first experiment, when moist.
	·0206266	·0008968	·00000498	Mean of two do., when dry.
9. Arbroath pavement,	·0206652	·0008985	·00000499	Mean of four do.
10. Caithness pavement,	·0205788	·0008947	·00000497	Mean of three do.
11. Greenstone from Ratho,	·0186043	·0008089	·00000449	Mean of three do.
12. Aberdeen grey granite,	·01815695	·00078943	·00000438	Mean of two do.
13. Best stock brick, .	·0126542	·0005502	·00000306	Mean of two do.
14. Fire-brick, . . .	·0113334	·0004928	·00000274	Mean of two do.
15. Stalk of a Dutch tobacco-pipe, . . .	·0105177	·0004573	·00000254	Mean of three do.
16. Round rod of Wedgewood-ware (11 inches long), ...		·00045294	·00000251	Mean of two do.
17. Black Marble from Galway, Ireland, .	·0102394	·00044519	·00000247	Mean of three do.

From the results given in the Table, it is evident that no danger is to be apprehended from a change of temperature affecting cast-iron and sandstone, in a very different degree, as their expansion, in so far as regards all building operations at least, may be considered as the same. The difference between the expansion of bricks and malleable iron amounts only to .00042 for 90° F., or half an inch on 100 feet; and Roman cement, when in as damp a state as all buildings must be, will expand more than malleable iron, since it did so in my experiment, even after it had been dried for eleven hours at a temperature of 207°. This, therefore, will in a great measure serve for an explanation of the fact mentioned by Mr Brunel, in describing his method of constructing arches, by suspending the courses of brick with straps of hoop-iron, viz. that he had had some anxiety as to the manner in which variations of temperature would affect his mode of operating, by expanding the metal more than the brick and mortar; but, on examining his experimental arch, both in summer and in winter, not a crack was to be seen. The substances at the bottom of the Table are black marble, and the rod of Wedgewood's ware; their expansions are nearly alike, and they expand very nearly half as much as platinum for the same number of degrees.*

M. Destigny mentions in his paper before alluded to, that he found the expansion of stone not at all affected by a difference in its state of humidity. From what I have observed in conducting my experiments, I am quite of a different opinion, for although no increased expansibility was observed by wetting sandstone after it had been dried, and I did not try any direct experiment of this nature with any of the marbles, on account of the very small quantity of water which they were found to ab-

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* I have since procured another rod of Galway black marble, which contained more fossils, and was softer than the first. Its expansion for 180° F. was .0004793, and for 1° F. .00000266.

sorb after having been dried, nevertheless the experiments on greenstone show the effect of moisture, and some of the marbles certainly became less sensible to the change of temperature, after they had been heated, and this was probably caused by their natural moisture being driven off. This diminution is distinctly shewn by the following series of experiments on Sicilian marble :—

1st, ... .0325392	......Diff.	.0058229	Decimals of an inch for 23 inches, the change of temperature being 180° F.
2d, ... .0267163	......	.0013143	
3d, ... .0254020	......	.0013364	
4th, ... .0240656			

The rod gave off a considerable quantity of moisture. This gradual diminution of the expansion was scarcely observable in the sandstones and Aberdeen grey granite, which contained a great deal of quartz ; and in the bricks, in the stalk of a Dutch tobacco-pipe, and in a rod of Wedgewood's ware, no such change took place, although all these substances had been wet in cutting the holes, &c. before they could be put into the pyrometer. The Peterhead granite, again, which contains much more felspar than that from Aberdeen, and also the greenstone, were affected by losing their moisture, but more especially the latter, as shewn by the following series of experiments made in November. Unluckily the first observation, which always differs most from the others, was lost.

2d, ... .0198379	......Diff.	.0017485	Decimals of an inch, the expansion of 23 inches for 180° F.
3d, ... .0180894	......	.0002037	
4th, ... .0178857			

As greenstone absorbs moisture much more readily than the marbles, I replaced the rod again in the pyrometer, after it had hung in the room for more than three months, from the time of performing the first experiments. The instrument was heated to 207° F., and this temperature was kept up without change during the four hours in which I watched the alterations of the length of the rod. The inner case being closed to prevent the evaporation of the moisture, the ultimate expansion of the greenstone for 155° F., was 5.02 revolutions of the screw of the upper micrometer. The pipes V V, for drying the inner case were then opened, a good deal of vapour came off, and when the rod had been allowed to dry for three quarters of an hour, the micrometer read only 3.24; hence, by the effect of drying alone, the rod contracted rather more than $\frac{1}{170}$th part of an inch. I then poured boiling water on the rod, and closed the drying pipes, and although this could not moisten it thoroughly, yet when its expansion was again at the greatest, the upper micrometer read 4.15. The drying pipes were then opened a second time, and although the temperature always remained unaltered, the reading of the upper micrometer was again reduced to 3.32. This experiment clearly shews, that the expansibility of some kinds of stone very much depends on their state of humidity. An analogous experiment was tried with a rod of satin wood, and the length of the wood altered with the similar changes in the same manner, but to a greater extent. With the latter substance this might have been expected, but such an action of moisture on stone is certainly very remarkable. I have therefore given two rates of expansion in the table for several of the rods, the greater is when the rod contained most moisture, the lesser is a mean of several experiments made when it was in a dryer state, and when the results of the experiments agreed very nearly among themselves.

A very curious effect of the heat on the white marbles, was that of causing a permanent increase of their length, which, in the Sicilian marble, was gradually augmented by the heating for each experiment. The rod of this marble was heated five times, but, unfortunately, as the change is always decreasing, I at first adjusted the micrometers without noting the quantity, by which the line on the stud had not returned to the adjustment for the previous experiment. This was the first time I had met with this gradual increase in the length of the rod; and by the effect of the two heatings noted after I observed it, the rod was lengthened .0096, or nearly the hundredth of an inch. On the rod of Carrara marble, the elongation took place mostly at the first heating, and it amounted to .0135 of an inch. This may probably serve to account for the warping which sometimes takes place on those parts of chimney-pieces which are subjected to the greatest action of the fire. It is impossible to say how far this lengthening would be increased by a greater change of temperature; but, since Mr Babbage has applied the expansion of stone to the explanation of geological phenomena, this property of limestone may also be taken into account, as it only requires the heat to have been once in action after the strata have been formed. In the experiment on the rod of Carrara marble, the whole effect was produced by an increase of temperature of 157° F.; and if we suppose this to have acted on strata five miles deep, the depth taken by Mr Babbage, in accounting for the changes of the level of the Temple of Serapis, the surface would have been raised by not less than 15½ feet. Little weight, however, can be attached to such explanations of the great operations of nature, as there may be a vast difference between the effect of heat quickly communicated to the small pieces of stone on which we perform our experiments, and the necessarily slow diffusion of the heat through such a depth of strata as we have supposed.

In order to ascertain if there was any connexion between the density of stones of the same composition and their expansibility, as takes place with the metals, I determined the specific gravity of several of the rods, but the one property does not appear to have the least dependence on the other. For instance, the specific gravity of Sicilian white marble and Galway black marble is 2.7127 and 2.7093, almost exactly alike; the expansion for 180° F. of the former is .0325392, while that of the latter is only .0102394, not one-third of the first. In determining the specific gravities, the stones were first dried on a sand-bath, and, after being weighed in a dry state, they were put into distilled water; and although the pieces of the black and white marble weighed about 200 grains, and were left three weeks in water, they only absorbed one-tenth of a grain, yet the white marble, in particular, gave off a considerable quantity of moisture when first heated. The Arbroath pavement absorbed $\frac{1}{23}$ of its bulk of water, and the Caithness pavement $\frac{1}{247}$ part; which shews the great superiority of the latter pavement for all purposes, where it is wished to exclude dampness.

In conclusion, it may be observed that, from the results of the experiments given in the Table, it is perfectly evident that not the slightest danger can arise from the use of cast-iron in buildings, on account of the difference of their expansion for all ordinary temperatures. Nor do I think that there is any cause to dread the effects of fire where such pillars are used, although that was one of the arguments employed to retard their introduction; on the contrary, there is every reason to believe, that even in cases where the fire would rage with all the fury that a strong wind could impart to it, cast-iron pillars would support the lintels of the windows as safely as those of stone. And I do not state this as a mere matter of speculation. The late great burning of part of the North Bridge New Buildings, is a proof of the correctness of the opinion. Of the extraordinary fierceness of this conflagration, those who did not witness it might easily have satisfied themselves, by inspecting the injury done to the stones of the ashler front. I was present at this terrific scene of destruction, before the flames burst from the windows, and remained till the fire was quite under the power of the engines. Very soon after the flames came sweeping out at the front windows, the stones began to crack and skirt off, and a constant shower of such large masses fell, during probably half-an-hour, that the firemen were obliged to draw back their leathern hose to a considerable distance from the buildings; and, so long as the stones continued to fall, it was impossible to save any of the very valuable property from the shops below, which, but for this, might have been easily and leisurely done. Now, no part of the stone was at any time red-hot, and the firemen were standing on the window-soles very soon after the flames retired from them, so that no danger was to be apprehended from the risk of cast-iron being melted in such a situation. Indeed, the cast-iron columns in the interior of the building did not appear to be much injured by the fire.

Edinburgh, *April* 4. 1835.

On the Application of the Hot Blast, in the Manufacture of Cast-Iron. By THOMAS CLARK, M. D., Professor of Chemistry in Marischal College, Aberdeen.

(Read 16th March 1835.)

AMONG persons interesting themselves in the progress of British manufactures, it can scarce fail to be known, that Mr NEILSON of Glasgow, manager of the Gas Works in that city, has taken out a patent for an important improvement in the working of such furnaces as, in the language of the patent, "are supplied with air by means of bellows, or other blowing apparatus." In Scotland, Mr NEILSON'S invention has been extensively applied to the making of cast-iron, insomuch that there is only one Scotch iron-work where the invention is not in use, and in that work, apparatus is under construction to put the invention into operation. Apart from the obvious importance of any considerable improvement in the manufacture of so valuable a product as cast-iron, the invention of Mr NEILSON would merit attention, were it only for the singular extent of the improvement effected, compared with the apparent simplicity—I had almost said inadequacy—of the means employed. Having therefore, by the liberality of Mr DUNLOP, proprietor of the Clyde Iron-Works, where Mr NEILSON'S invention was first put into operation, obtained full and free access to all information regarding the results of trials of the invention in those works, on the large scale of manufacture, I cannot help thinking that an authentic notice of these results, together with an attempt to explain the cause of them, will prove acceptable to the Royal Society of Edinburgh. And that these results, as well as the cause of them, may be set forth with clearness, I shall advert,

First, To the process of making iron, as formerly practised,
Second, To Mr Neilson's alteration on that process,
Third, To the effect of that alteration,
Fourth, To the cause of that effect.

I. In proceeding to advert to the process of making cast-iron, as formerly practised, it cannot here be necessary to enter into much detail in explanation of a process, long practised and extensively known, as this has been ; nor, indeed, shall I enter into detail, farther than, to the general scientific reader, may be proper to elucidate Mr Neilson's invention.

In making cast iron, then, the materials made use of were three,—

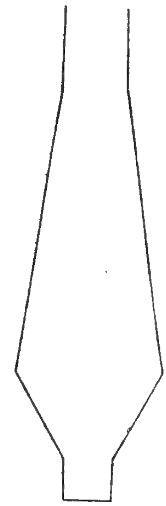
The Ore,
The Fuel,
The Flux.

The Ore was clay iron-stone, that is to say, carbonate of iron, mixed, in variable proportions, with carbonates of lime, and of magnesia, as well as with aluminous and siliceous matter.

The Fuel made use of at Clyde Iron-Works, and in Scotland generally, was coke, derived from splint-coal. During its conversion into coke, this coal underwent a loss of 55 parts in the 100, leaving 45 of coke. The advantage of this previous conversion consisted in the higher temperature produced by the combustion of the coke, in consequence of none of the resulting heat disappearing in the latent form, in the vapours arising from the coal, during its conversion into coke.

The Flux was common limestone, which was employed to act upon the aluminous and siliceous impurities of the ore, so as to produce a mixture more easy to melt than any of the materials of which it was made up, just as an alloy of tin and lead serves as a solder, the resulting alloy being more easy to melt than either the lead or the tin apart.

These three materials—the ore, the fuel, and the flux—were put into the furnace, near the top, in a state of mixture. The only other material supplied was air, which was driven into the furnace by pipes from blowing apparatus, and it entered the furnace by nozzles, sometimes on two opposite sides of the furnace, sometimes on three, and sometimes, but rarely, on four. The air supplied in this manner entered near the bottom of the furnace, at about 40 feet from the top, where the solid materials were put in. The furnace, in shape, consisted, at the middle part, of the frustums of two cones, having a horizontal base common to both, and the other and smaller ends of each prolonged into cylinders, which constituted the top and bottom of the furnace, as may be well enough conceived from the sectional sketch on the margin.



The whole of the materials put into the furnace, resolved themselves into gaseous products, and into liquid products. The gaseous products, escaping invisible at the top, included all the carbonaceous matter of the coke, probably in the form of carbonic acid, except only the small portion of carbon retained by the cast-iron. The liquid products were collected in the cylindrical reservoir, constituting the bottom of the furnace, and there divided themselves into two portions, the lower and heavier being the melted cast-iron, and the upper and lighter being the melted slag, resulting from the action of the fixed portion of the flux upon the fixed impurities of the fuel and of the ore.

II. Thus much being understood in regard to the process of making cast-iron, as formerly practised, we are now prepared for the statement of Mr Neilson's improvement.

This improvement consists essentially in heating the air in its passage from the blowing apparatus to the furnace. The heating has hitherto been effected by making the air pass through cast-iron vessels, kept at a red heat. In the specification of the patent, Mr Neilson states, that no particular form of heating-apparatus is essential to obtaining the beneficial effect of his invention; and, out of many forms that have been tried, experience does not seem to have yet decided which is best. At Clyde Iron-Works, the most beneficial of the results that I shall have occasion to state, were obtained by the obvious expedient of keeping red-hot the cast-iron cylindrical-pipes conveying the air from the blowing apparatus to the furnace.

III. Such being the simple nature of Mr Neilson's invention, I now proceed to state the effect of its application.

During the first six months of the year 1829, when all the cast-iron in Clyde Iron-Works was made by means of the cold blast, a single ton of cast-iron required for fuel to reduce it, 8 tons $1\frac{1}{4}$ cwt. of coal, converted into coke. During the first six months of the following year, while the air was heated to near 300° Fahr., one ton of cast-iron required 5 tons $3\frac{1}{4}$ cwt. of coal, converted into coke.

The saving amounts to 2 tons 18 cwt. on the making of one ton of cast-iron; but from that saving comes to be deducted the coals used in heating the air, which were nearly 8 cwt. The nett saving thus was $2\frac{1}{2}$ tons of coal on a single ton of cast-iron. But during that year, 1830, the air was heated no higher than 300° Fahr. The great success, however, of those trials, encouraged Mr Dunlop, and other iron masters, to try the effect of a still higher temperature. Nor were their expectations disappointed. The saving of coal was greatly increased, insomuch that, about the beginning of 1831, Mr Dixon, proprietor of Calder Iron-Works, felt himself encouraged to attempt the substitution of raw coal for the coke before in use. Proceeding on the ascertained advantages of the hot blast, the attempt was entirely successful; and, since that period, the use of raw coal has extended so far as to be adopted in the majority of the Scotch iron-works.

The temperature of the air under blast had now been raised so as to melt lead, and sometimes zinc, and therefore was above 600° Fahr., instead of being only 300°, as in the year 1830.

The furnace had now become so much elevated in temperature, as to require, around the nozzle of the blowpipes, a precaution borrowed from the finery-furnaces, wherein cast-iron is converted into malleable, but seldom or never employed where cast-iron is made by means of the cold blast. What is called the *Tweer*, is the opening in the furnace to admit the nozzle of the blowpipe. This opening is of a round funnel-shape, tapering inwards, and it used always to have a cast-iron lining, to protect the other building materials, and to afford them support. This cast-iron lining was just a tapering tube nearly of the shape of the blowpipe, but large enough to admit it freely. Now, under the changes I have been describing, the temperature of the furnace became so hot near the nozzles, as to risk the melting of the cast-iron lining, which, being essential to the *tweer*, is itself commonly called by that name. To prevent such an accident, an old invention called the *water-tweer* was made available. The peculiarity of this tweer consists in the cast-iron lining already described being cast hollow instead of solid, so as to contain water within, and water is kept there continually changing as it heats, by means of one pipe to admit the water cold, and another to let the water escape when heated. *

During the first six months of the year 1833, when all these changes had been fully brought into operation, one ton of cast-iron was made by means of 2 tons 5¼ cwt. of coal, which had not previously to be converted into coke. Adding to this 8 cwt. of coal for heating, we have 2 tons 13¼ cwt. of coal required to make a ton of iron; whereas, in 1829, when the cold blast was in operation, 8 tons 1¼ cwt. of coal had to be used. This being

* An incidental advantage attended the adoption of the water-tweers, inasmuch as these made it practicable to lute up the space between the blowpipe nozzle and the tweers, and thus prevent the loss of some air that formerly escaped by that space, and kept up a bellowing hiss, which, happily, is now no longer heard.

almost exactly three times as much, we have, from the change of the cold blast to the hot, combined with the use of coal instead of coke, *three times as much iron made from any given weight of splint coal.*

During the three successive periods that have been specified, the same blowing apparatus was in use; and not the least remarkable effect of Mr Neilson's invention, has been the increased efficacy of a given quantity of air in the production of iron. The furnaces at Clyde Iron-Works, which were at first three, have been increased to four, and, the blast machinery being still the same, the following were the successive weekly products of iron during the periods already named, and the successive weekly consumpt of fuel put into the furnace, apart from what was used in heating the blast:

	Tons.		Tons.		Tons.	
In 1829, from 3 furnaces,	111	Iron from	403	Coke, from	888	Coal.
In 1830, from 3 furnaces,	162	Iron from	376	Coke, from	836	Coal.
In 1833, from 4 furnaces,	245	Iron		from	554	Coal.

Comparing the product of 1829 with the product of 1833, it will be observed that the blast, in consequence of being heated, has reduced more than double the quantity of iron. The fuel consumed in these two periods we cannot compare, since, in the former, coke was burned, and, in the latter, coal. But on comparing the consumpt of coke in the years 1829 and 1830, we find that although the product of iron in the latter period was increased, yet the consumpt of coke was rather diminished. Hence the increased efficacy of the blast appears to be not greater than was to be expected, from the diminished fuel that had become necessary to smelt a given quantity of iron.

On the whole, then, the application of the hot blast has caused the same fuel to reduce three times as much iron as before, and the same blast twice as much as before.

The proportion of the flux required to reduce a given weight of the ore, has also been diminished. The amount of this diminution, and other particulars, interesting to practical persons, will appear on reference to a tabular statement supplied by Mr Dunlop, and printed as an Appendix to this paper. Not further to dwell on such details, I proceed to the last division of this paper, which is,

IV. To attempt an explanation of the foregoing extraordinary results.

Subsidiary to this attempt, it is necessary to discriminate between the quantity of fuel consumed and the temperature produced. For instance, we may conceive a stove to be kept at the temperature of 500° Fahrenheit, and lead to be put into such a stove for the purpose of being melted. Then, since the melting point of lead is more than 100° higher, it is evident that whatever fuel might be consumed in keeping that stove at the temperature of 500°, the fuel is all consumed to no purpose, so far as regards the melting of lead, in consequence of deficiency in the temperature. In the manufacture of cast-iron likewise, experience has taught us, that a certain temperature is required in order to work the furnace favourably, and all the fuel consumed so as to produce any lower degree of temperature, is fuel consumed in vain. And how the hot blast serves to increase the temperature of a blast furnace, will appear on adverting to the relative weights of the solid and of the gaseous materials made use of in the reduction of iron.

As nearly as may be, a furnace, as wrought at Clyde Iron-works in 1833, had two tons of solid materials an hour put in at the top, and this supply of two tons an hour was continued for 23 hours a-day, one half-hour every morning, and another every evening, being consumed in letting off the iron made. But the gaseous material—the hot air—what might be the weight of it? It can easily be ascertained thus: I find, by comparing the quantities of air consumed at Clyde Iron-works, and at Calder Iron-works, that one furnace requires of hot air from 2500 to 3000 cubical feet in a minute. I shall here assume 2867 cubical feet to be the quantity; a number that I adopt for the sake of simplicity, inasmuch as, calculated at an avoirdupois ounce and a quarter, which is the weight of a cubical foot of air at 50° Fahrenheit, these feet correspond precisely with 2 cwt. of air a minute, or *six tons an hour*. Two tons of solid material an hour, put in at the top of the furnace, can scarce hurtfully affect the temperature of the furnace, at least in the hottest part of it, which must be far down, and where the iron, besides being reduced to the state of metal, is melted, and the slag too produced. When the fuel put in at the top is coal, I have no doubt that, before it comes to this far-down part of the furnace—the place of its useful activity—the coal has been entirely coked; so that, in regard to the fuel, the new process differs from the old much more in appearance than in essence and reality. But if two tons of solid material an hour, put in at the top, are not likely to affect the temperature of the hottest part of the furnace, can we say the same of six tons of air an hour, forced in at the bottom near that hottest part? The air supplied is intended, no doubt, and answers to support the combustion; but this beneficial effect is, in the case of the cold blast, incidentally counteracted by the cooling power of six tons of air an hour, or two cwt. a minute, which, when forced in at the ordinary temperature of the air, cannot be conceived otherwise than as a prodigious refrigeratory passing through the hottest part of the furnace, and repressing its temperature. The expedient of previously heating the blast obviously removes this refrigeratory, leaving the air to act in promoting combustion, without robbing the combustion of any portion of the heat it produces.

Such, I conceive, is the palpable, the adequate, and very simple explanation of the extraordinary advantages derived in the manufacture of cast-iron, from heating the air in its passage from the blowing apparatus to the furnace.

Marischal College,
Aberdeen, *Jan.* 10. 1835.

APPENDIX.

***TABLE** shewing the Weight of Cast-Iron produced, and the Average Weight of Coals made use of, in producing a Ton of Cast-Iron, at Clyde Iron-Works, during the Years* 1829, 1830, *and* 1833, *the Blowing Engine being the same.*

COKE AND COLD AIR.			COKE AND HEATED AIR.			COAL AND HEATED AIR.		
1829.	Weekly Product of Cast-Iron by Three Furnaces.	Average of Coals used to 1 Ton of Cast-Iron.	1830.	Weekly Product of Cast-Iron by Three Furnaces.	Average of Coals used to 1 Ton. of Cast-Iron.	1833.	Weekly Product of Cast-Iron by Four Furnaces.	Average of Coals used to 1 Ton of Cast-Iron.
	Tons. Cwt. Qrs.	Tons. Cwt. Qrs.		Tons. Cwt. Qrs.	Tons. Cwt. Qrs.		Tons. Cwt. Qrs.	Tons. Cwt. Qrs.
Jan. 7.	137 18 2	8 12 1	Jan. 6.	176 10 2	5 2 2	Jan. 9.	375 8 0	2 12 3
14.	148 2 0	6 9 2	13.	181 12 2	5 0 2	16.	267 18 0	2 4 0
21.	148 8 2	6 11 3	20.	172 5 2	5 0 2	23.	270 7 2	2 3 1
28.	138 9 2	7 0 2	27.	178 7 0	4 19 0	30.	250 9 0	2 4 0
Feb. 4.	125 13 0	7 12 1	Feb. 3.	164 8 0	5 4 0	Feb. 6.	265 3 2	2 1 0
11.	136 19 0	7 13 1	10.	172 12 0	5 4 0	13.	202 10 0	2 4 3
18.	130 16 2	7 11 3	17.	163 9 0	5 9 0	20.	257 1 0	2 4 3
25.	105 12 2	7 10 0	24.	170 1 0	5 3 0	27.	264 0 0	2 5 1
Mar. 4.	101 8 1	7 17 2	Mar. 3.	154 19 0	5 10 3	Mar. 6.	234 13 0	2 5 2
11.	111 2 0	8 2 2	10.	154 16 0	5 9 2	13.	238 7 2	2 7 1
18.	114 10 0	7 6 2	17.	151 8 2	5 9 3	20.	205 13 0	2 10 2
25.	110 14 0	8 8 1	24.	163 17 0	5 5 1	27.	217 14 0	2 2 3
Ap. 1.	111 4 0	8 7 2	31.	163 8 2	5 11 0	Ap. 3.	220 7 0	2 14 2
8.	107 7 0	8 3 0	Ap. 7.	147 10 0	5 7 0	10.	280 9 2	2 0 3
15.	91 12 2	8 15 0	14.	154 9 2	5 2 0	17.	304 7 0	1 17 3
22.	85 13 0	9 13 0	21.	163 4 0	4 19 0	24.	248 12 2	2 3 0
29.	91 14 2	9 6 2	28.	148 12 2	5 4 0	May 1.	245 7 2	2 6 0
May 6.	92 7 2	8 8 2	May 5.	162 10 2	5 2 2	8.	200 17 0	2 8 0
13.	94 6 0	9 2 1	12.	149 13 0	5 3 2	15.	246 4 2	2 5 3
July 8.	88 4 2	8 16 3	19.	162 4 0	5 5 0	22.	219 1 2	2 6 0
15.	91 13 0	8 5 0	26.	165 7 2	4 18 3	29.	231 2 0	2 8 0
22.	97 2 0	8 2 1	June 2.	160 4 0	5 2 2	June 5.	235 16 0	2 6 2
29.	104 15 2	7 10 2	9.	157 17 0	5 1 0	12.	232 10 0	2 7 1
Aug. 5.	106 17 2	7 7 2	16.	164 0 0	4 17 3	19.	271 1 2	2 1 0
12.	93 1 0	8 6 0	23.	149 3 0	4 18 0	26.	262 3 2	2 3 1
19.	113 7 0	8 18 2	30.	162 16 2	4 16 3	½ w. 30.	122 16 0	2 5 1
	2878 18 0	209 19 0		4215 6 0	134 6 2		6370 3 0	58 18 3
Average	110 14 2	8 1 1		162 2 2	5 3 1		245 0 0	2 5 1

The Blowing-engine has a steam-cylinder of 40 inches diameter, and a blowing-cylinder of 8 feet deep and 80 inches diameter, and goes 18 strokes a minute. The whole power of the engine was exerted in blowing the three furnaces, as well as in blowing the four, and in both cases there were two tweers of 3 inches diameter to each furnace. The pressure of the blast was 2½ lb. to the square inch. The fourth furnace was put into operation after the water tweers were introduced, and the open spaces round the blowpipes were closed up by luting. The engine then went less than 18 strokes a minute, in consequence of the too great resistance of the materials contained in the three furnaces to the blast in its passage upwards.

Materials constituting a Charge.

		Cwt.	Qrs.	Lb.
1829,	Coke, ...	5	0	0
	Roasted Ironstone, . .	3	1	14
	Limestone, ...	0	3	16
1830,	Coke, ...	5	0	0
	Roasted Ironstone, . .	5	0	0
	Limestone, ...	1	1	16
1833,	Coal, ...	5	0	0
	Roasted Ironstone, . .	5	0	0
	Limestone, ...	1	0	0

On the Poisonous Properties of Hemlock, and its Alkaloid Conia. By ROBERT CHRISTISON, M. D., F. R. S. E., Professor of Materia Medica in the University of Edinburgh.

(*Read 7th December* 1835.)

FEW poisons are of greater interest, in a historical or scientific point of view, than Hemlock. It has been known ever since the most classic periods of antiquity, being generally believed to have been the κωνειον of NICANDER and THEOPHRASTUS, and commonly thought to have been the poison with which state-criminals were despatched in ancient Athens. Since that period it has occupied a prominent place in all works on Toxicology ; and it has been immemorially familiar as a deadly poison to the vulgar in every part of Europe, where there is scarcely a country or even a province which does not produce it in abundance. For nearly a century, too, since the writings of *Baron STORCK* of Vienna in 1762, it has been constantly in the hands of the physician as a remedy, and has been currently employed at different times in the treatment of some of the most common, as well as in some of the most malignant, of all the maladies to which the human body is liable.

To its importance, as thus indicated, the attention which it has received from scientific men, and more especially from the chemist and the physiologist, has been by no means commensurate. There is scarcely a chemical analysis of hemlock worth mentioning till *GISEKE*, in 1827, succeeded in concentrating its active properties in a compound with sulphuric acid, of such energy, that two grains killed a small animal in fifty-five minutes ;*

* Journal de Pharmacie, xiii. 266 ; or, Archiv des Apothekervereins in Nördlichen Deutschland, xx. 97.

and it was not till 1831 that its active principle was detected and detached by *Professor Geiger* of Heidelberg, and proved by him to be one of a new order of organic alkaloids,—not fixed and crystalline like those previously known, such as morphia, strychnia, cinchonia, and the like, but volatile and oleaginous in their physical form.* Prior to this discovery, the knowledge possessed of the physiological effects of hemlock was vague and meagre; and little has since been done to supply the defect. The ideas entertained by the Greek and Roman naturalists and physicians of the poisonous properties of the ancient κωνειον or *cicuta*, were for the most part contradictory or obscure. Their statements, however, were long adopted by modern physiologists without examination, and applied to the *Conium maculatum*, or spotted hemlock of botanists; and the small amount of original inquiry which has been attempted by late experimentalists, has added little to the previous stock of knowledge. It is surprising, however, that some late researches were not carried farther than they have been; for it would appear scarcely possible for any accurate observer to attend carefully to the phenomena produced by hemlock and its alkaloid in their action on the animal body, without remarking that they are in many respects among the most interesting and extraordinary of all poisons.

These views, and the physiological facts to be subsequently related, were brought under my notice during an attempt made last autumn to repeat the analytic researches of Professor Geiger. As these researches are too little known in this country, or indeed out of Germany, and I have had occasion to confirm almost every fact advanced by the Heidelberg Professor, I have thought it not inopportune to reproduce here the general heads of his analysis, as introductory to the principal object of this paper,—which is, "The Poisonous Properties of Hemlock, and its Alkaloid Conia."

A short time before the analysis of Professor Geiger, it was

* Magazin für Pharmacie, xxxv. 72 and 259.

observed by *Giseke*, that, on distilling hemlock with water and caustic lime, an alkaline liquid of a strong and peculiar odour was obtained; from which, when neutralized by sulphuric acid and concentrated by evaporation, he separated with alcohol a substance of the nature of an extract, possessing the poisonous qualities of hemlock in a very eminent degree. Two grains of it killed a rabbit in less than an hour. But Giseke was unable to detach either an alkaloidal or a crystalline principle.

Proceeding in the same line of investigation, but with more precision, Geiger first found that the distilled water of hemlock leaves, or of the green seeds, although it gives out very strongly the peculiar mousy odour of the plant, is scarcely if at all poisonous;—a remarkable fact, when we consider that this odour or aroma is usually thought to be a correct measure of the relative activity of different specimens, and to possess a narcotic or stupifying tendency on those exposed to it. The imagination has probably had much to do in the formation of these notions. At all events, we now know that the singular aroma of hemlock is owing, like other vegetable odours, to a volatile oil; and that this oil is very feebly if at all deleterious.

But if the green seeds or leaves, either after or before the separation of the volatile oil, be distilled with water and caustic potass or lime,—the heat being applied through means of a muriate of lime bath, to prevent charring,—it will be found that the liquid which passes over is strongly alkaline and highly poisonous; and I have also commonly observed, where ten or twelve pounds of seeds were worked in one operation, that an oily-like matter comes over with the first few ounces of liquid, which is soluble in acids, insoluble in alkalis, strongly alkaline in its action on turmeric, and of a powerful, peculiar, suffocating odour, allied to, yet by no means identical with, that of the fresh herb. This, in fact, is a small quantity of tolerably pure conia.

But the greater part of the alkaloid remains in solution in the water which is distilled over. If this distilled water be distilled anew, it is simply reproduced without any material change except some loss of strength. But if it be previously neutralized with an acid, such as the sulphuric, the volatile poisonous principle becomes fixed, and water alone is distilled over. The residuum consists of sulphate of conia, sulphate of ammonia, and resinoid matter, the resin and ammonia being produced by decomposition of a part of the conia under the operation of heat and the access of air. In order to obtain the conia, the mass is subjected to a mixture of two parts of rectified spirit and one of sulphuric ether, which leaves the sulphate of ammonia undissolved. And then, the ether and alcohol being distilled carefully off, the remaining sulphate of conia is heated gently with a little water and caustic potassa; upon which there is obtained in the receiver a watery solution of conia in the lower part, and floating on this a layer of nearly pure conia, colourless, transparent, and presenting the physical appearance of an oil.

In this state the conia contains a little ammonia and a fourth of its weight of water, the latter of which may be removed by chloride of calcium, and the former by exposing it to the air-pump vacuum so long as bubbles of gas escape. By neither process of purification, however, is the physical appearance of the conia materially changed.

Conia thus obtained has the appearance of a colourless volatile oil. It is lighter than water, of a very powerful diffusible repulsive odour, somewhat like that of hemlock itself, and intensely acrid to the taste. It has a strong alkaline action on reddened litmus or turmeric. It is readily soluble in diluted acids, which it neutralizes; but its salts have not yet been obtained in the crystalline form. It is sparingly soluble in water, to which it imparts its odour and taste. It also combines with about a fourth of its weight of water to form a hydrate of conia. Both this hydrate and the watery solution possess the property of becoming opaque when slightly heated, and recovering their transparency on being again cooled. When exposed to the air it quickly contracts a dark brown colour, and is slowly resolved into a resinous matter, with the disengagement of ammonia. This change takes place more promptly under the co-operation of heat; but even at common temperatures it is so apt to ensue, that unless the alkaloid be kept very carefully excluded from the air, discoloration will be accomplished in a few hours. When heated with water it readily distils over at the temperature of 212°, in the same manner as the volatile oils; but its boiling point is 370° Fahr. It cannot be distilled either alone or with water, without a considerable part being decomposed and converted into a resin. Like other vegetable alkaloids, it is an azotized principle; and according to an analysis by *Liebig*, it is composed of Carbon 66.91, Hydrogen 12.0, Oxygen 8.28, and Azote 12.8.*

By the process mentioned above, conia may be obtained from the leaves of hemlock collected immediately before or during inflorescence of the plant. It exists, however, in much larger proportion in the seeds when fully developed, but still green. Even in them the quantity is small: from forty pounds I obtained about $2\frac{1}{2}$ ounces of hydrated conia. Geiger states that he obtained a still larger proportion from the ripe seeds; a result, however, which has not been confirmed in the trials I have made. It is very probable that a much larger proportion exists in both the leaves and seeds than has yet been obtained. For at every stage of the process where heat is applied, however carefully the heat may be managed, it is evident, from the abundant formation of ammonia, that much of the alkaloid is decomposed.

An important fact observed by Geiger is, that the dried leaves of hemlock and some extracts of their juice do not contain any conia. This observation I have also had occasion to make in regard to various extracts. It is interesting, in relation to the well known circumstance that the greatest discrepancy prevails among medical men as to the activity of hemlock, not merely as a remedy but even also as a poison. Two drachms of extract

* Magazin für Pharmacie, xxxvi. 161.

have been given to a dog without marked effect of any kind, and even an ounce has acted only as a feeble poison;* while it will presently be found that the same preparation is sometimes a poison of exceeding energy.

These discrepancies are easily understood now, on considering the extreme proneness of conia to decomposition, provided the conia of GEIGER be the true active principle of hemlock. From what has come under my observation, the extracts of hemlock may become feeble, if not even inert, in one of two ways,—either by the heat being continued after the concentration has been carried to a certain extent, or by long keeping. On the one hand, I have always observed, that from the point at which the extract attains the consistence of thin syrup, ammonia begins to be given off in abundance, together with a modified odour of conia. And on the other hand, I have found extracts which were unquestionably well prepared at first, entirely destitute of conia in the course of a few years,—a remark which applies even to the superior extract prepared by *Mr BARRY* of London by evaporation *in vacuo*. The mode of ascertaining the presence of conia is simply to triturate the extract or other preparation with solution of potassa, upon which an odour of conia is given off. I have no doubt that potassa is in this way a test of very great delicacy.

Of the various extracts I have examined, that which has yielded the largest proportion of conia is one prepared by alcohol from the ripe seeds. Two hundred and twenty grains gave towards five grains of colourless hydrate of conia. From this preparation, indeed, it is probably to be obtained with more ease than from any other. I have prepared it quite colourless and free of all impurity but water, by exhausting ground hemlock-seeds with cold rectified spirit in a percolator; distilling off the spirit and concentrating in an open vessel over the vapour-

* ORFILA, Toxicologie générale, ii. 305.

bath till the residue had the consistence of syrup, and subjecting this extract, in a proper distilling apparatus, with its own weight of water and a little caustic potassa, to the heat of a concentrated boiling solution of muriate of lime. The conia passes over readily with the water, floating on its surface and quite colourless.

In the proximate analysis of organic substances, it is of primary consequence that the agents employed be such as will accomplish simple separation of the proximate principles from each other, without producing new compounds by a new arrangement of elements. Some chemists have entertained doubts whether even any of the methods of analysis at present in use fulfil correctly this condition. But all are agreed in thinking that the agency of strong acids or strong alkaline solutions, more especially when concurring with an elevated temperature, should in general be avoided, as tending rather to form new compounds, than simply to detach compounds already formed by nature. Hence a question may justly arise, whether the substance I have been describing is the real active principle of hemlock, or a new product formed by the action of caustic potassa aided by heat?

Here it may be observed, in the first instance, that heat is not necessary for the development of conia in hemlock and its preparations; for its peculiar odour is at once disengaged from the powder of the seeds when treated with solution of potassa at ordinary atmospheric temperatures.

By far the most direct and satisfactory test, however, of the force of the above objection in such circumstances, is the effect of the detached principle on the animal body, and the relation the phenomena bear to those produced by the crude substance from which the principle is obtained. In the present case experiment amply proves, that the conia of Geiger concentrates in itself the properties of hemlock,—and, if not itself the true active principle, must contain it in large quantity.

The researches of Geiger on this head are few in number, and were chiefly confined to small birds as the subject of experiment.

Nor does he seem to me to have correctly interpreted the phenomena, since he describes the animals as having been affected with paralysis and tetanic convulsions, and as presenting, immediately after death, congestion and loss of irritability of the heart, with unimpaired irritability of the voluntary muscles, the diaphragm, and the intestinal canal. The natural inference would be that conia proves fatal by paralyzing the heart.

On making trial of its effects on one of the higher orders of animals, I obtained results so different and so remarkable, that I was led to investigate its physiological action in detail. The facts thus brought under my notice will shew that this substance is one of the most extraordinary of all known poisons, looking either to its uncommon energy and subtilty, or to the peculiar phenomena and nature of its operation. In what follows I shall confine myself to a general summary, reserving the details of special experiments for an appendix.

I should premise that the anatomical details were conducted by Dr Sharpey, and the whole experiments made in presence of various practised observers. This it is material to state, because in physiological inquiries like the present, where the incidents succeed one another with extreme swiftness, it is indispensable that the anatomical part be executed with facility, certainty, and despatch, and that the account taken of what passes be checked by several competent observers.

Conia is probably a deadly poison to every order of animals. It acts at least with great, and apparently equal energy on the dog, cat, rabbit, mouse, kite,* pigeon,* sparrow,* frog, slow-worm,* earth-worm,* fly, and flea.

It acts through every texture of the body where absorption is carried on readily, namely, when put into the stomach, or dropped into the eye, or inhaled into the lungs, or introduced into the cellular tissue under the skin, or brought in contact with the peri-

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* Animals experimented on by Geiger.

tonæum, or injected directly into the veins. Its activity through these several channels seems on the whole proportional to the speed with which absorption is carried on by each texture; so that it is one of the poisons which act through absorption. Nevertheless, a fact will be mentioned by-and-bye, which would indicate that something more than absorption into the blood is required before it can affect those vital functions, whose arrestment constitutes the cause of death and the essence of its operation.

The activity of conia is not impaired, but rather the reverse, by neutralization with an acid. GEIGER arrived at a different conclusion, for he says "its poisonous effect is greatly lessened by union with acids." Such a fact would be at variance with a law in physiology hitherto found to be universal; that poisons acting through absorption are not at all, or very little, altered in their effects by any change in chemical form, provided they continue equally soluble. As the salts of conia are more soluble than the alkaloid itself, we should expect them to act with at least equal energy. And, accordingly, I have always found that the activity of a poisonous dose was materially increased by using it neutralized with muriatic acid. It will follow as a corollary, that the discovery of a chemical antidote for conia or hemlock is extremely improbable.

The chief features in the action of conia are the following. It is, in the first place, a local irritant. It has an acrid taste; when dropped into the eye or on the peritonæum, it causes redness or vascularity; and to whatever texture or part it is applied, expressions of pain are immediately excited. But these local effects are soon overwhelmed by the indirect or remote action which speedily follows. This consists essentially of swiftly-spreading palsy of the muscles,—affecting first those of voluntary motion, then the respiratory muscles of the chest and abdomen, lastly the diaphragm, and thus ending in death by asphyxia. The paralytic state is usually interrupted from time to time by slight convulsive twitches of the limbs and trunk in the early stage of the poisoning; but this is not an essential phenomenon. The muscular contractility of parts directly acted on,—as when a voluntary muscle, a loop of intestine, or the heart, is brushed over with conia or its muriate,—is sometimes impaired, sometimes almost immediately annihilated. But this effect, as will be evident from the details of the experiments, is not invariable. Under the remote or indirect action of the poison, the muscular contractility remains altogether unaffected: when an animal is killed with the poison applied to the eye, a wound, or the like, both the voluntary and involuntary muscles contract for a long time after death, when stimulated, either directly or through the medium of their nerves, by mechanical irritation or by galvanism. The blood undergoes no apparent alteration, except those incidental to death by asphyxia; it coagulates firmly after death, if immediately withdrawn from the bloodvessels. The heart, contrary to GEIGER'S statement, remains wholly unaffected,—contracting vigorously for a long time after all motion and respiration and other signs of life are extinct,—and containing after death, not florid, but dark blood in its left cavities. The external senses continue little, if at all, impaired, till the breathing is nearly arrested; and volition is also retained. The action of conia, in short, is exerted chiefly on the spinal chord. In its nature that action is the counterpart of the action of nux-vomica and its alkaloid strychnia. Strychnia irritates the spinal chord, producing violent, permanent spasm of the muscles, and death by asphyxia from spasmodic fixing of the chest. Conia, on the contrary, exhausts the nervous energy of the spinal chord, producing general muscular paralysis, and asphyxia from relaxation.

Few poisons equal conia in subtilty or swiftness. A single drop put into the eye of a rabbit killed it in nine minutes; three drops used in the same way killed a strong cat in a minute and a-half; five drops poured into the throat of a small dog began to act in thirty seconds, and in as many seconds more motion and respiration had entirely ceased. But the most extraordinary evidence of its power is obtained by injecting it into a vein. MAGENDIE, speaking of the concentrated or pure prussic acid when similarly applied, compares its action to that of a cannon-ball or thunderbolt: " La foudre n'est pas plus prompte." Figurative as this language may be, it is the only mode of conveying an adequate idea of the effect of conia when injected into the blood. Proceeding to inject into the femoral vein of a young dog two grains of the alkaloid exactly neutralized with thirty drops of diluted muriatic acid, I was prepared for great rapidity of action, and was going on the instant to observe the time by seconds; but on glancing for a moment over the watch at the animal, I observed it was dead. In two seconds, or three at farthest, and without the slightest warning struggle, respiration had ceased, and with it all external signs of life.

Of the effects previously related some are most clearly seen where the progress of the poisoning is slow, others where the action is rapid. It is only, for example, where death takes place rather slowly, that we can satisfy ourselves of the maintenance of the functions of the external senses; because in other circumstances the instant invasion of paralysis of the voluntary muscles takes away all power of expression, by which alone we can judge of the state of the senses. But when the poison is given so as to operate slowly, then distinct evidence may be obtained that both sight, touch, and hearing are retained so long as the most feeble power of movement is preserved, so that sensation may be followed by expression. The integrity of the circulation and muscular contractility is, on the other hand, best shewn where death is prompt. When the poison acts slowly, the heart after death is found gorged, and contracting feebly or imperfectly;—a physiological phenomenon not connected with any peculiar action of the poison on the heart, but which is common to all modes of death by slowly formed asphyxia. But where the action is swift, and asphyxia prompt and complete, the heart acts spontaneously with great force even in the higher orders of animals for a great length of time. I have seen spontaneous contraction of the ventricles of the heart go on ten minutes, twenty minutes, nay, even thirty minutes, after death in the rabbit; and have witnessed unequivocal contraction of the auricles when scratched even so late as after an interval of sixty minutes. The most striking illustration, however, of the integrity of the function of the heart is obtained by keeping up respiration artificially when it has ceased: After the breathing had almost ceased in seventeen minutes in a dog poisoned with six drops through a wound, and when two minutes more would undoubtedly have put an end to life, artificial inflation of the lungs was commenced, and continued with occasional intervals for thirty-five minutes. During all that time the heart beat with its natural force, except when the inflation of the lungs was suspended;—the animal remaining all the while in a state of paralytic flaccidity, interrupted only by slight muscular twitches. It appears probable that there is scarcely any limit to the maintenance of the circulation under artificial breathing, except what may arise from the difficulty of imitating exactly the natural respiration, as well as from the several causes which occasion cooling of the body. There is little doubt, therefore, that where the dose of the poison is not very great, animation may be restored by maintaining the function of respiration artificially till the deleterious agent or its effects be thrown off,—just as in some other forms of narcotic poisoning. And when we consider the whole physiological phenomena, it will appear that this is the only treatment which promises material success.

There are several other physiological facts relative to the action of this poison, which are not devoid of interest, but which it would be tedious to dwell on here. They will be best examined by consulting the Appendix of experiments. This department of my subject, therefore, may now be concluded with a few remarks on the question, through what channel the action of conia on the spinal chord is accomplished: Does it act by being carried substantively with the blood to that organ, or by the transmission along the nerves of a peculiar impression made on the texture where it is directly applied? I must leave the question, however, in an unsettled state. Every physiologist who has attended to the late researches in this field, must agree that we are not at present in possession of any accurate criterion for settling the question in the case of any poison which acts remotely, that is, on organs at a distance from the part where it is immediately applied. That absorption is somehow connected with the action of conia, will appear from its activity seeming proportional to the activity of absorption in the texture with which it comes in contact. This inference would be strengthened if we could actually detect it in the blood after death; but my observations on this head are contradictory; for, in one instance, where the muriate of conia was put into the stomach, and secured there by a ligature on the gullet, its odour was distinctly remarked after death in the general cavity of the abdomen; while in another case, where death followed in ninety seconds the application of conia to the eye, not the slightest odour could be detected in the blood of the heart. It would seem to me, however, to be very nearly made out by a fact already mentioned, that, although absorption into the blood may be a part of the chain of sequences which attend the action of conia, yet this is not all; and that the poison does not act by being carried substantively with the blood to the spinal chord. I allude to its tremendous rapidity when injected into a vein. That it acts more swiftly in this way than in any other, is evidence enough perhaps that it enters the blood before it operates. But farther, its effect, when thus introduced, is too swift for its action to depend entirely on the blood becoming poisoned, and itself acting on the spine; for it is impossible that, in three seconds, which was certainly the limit of interval when all voluntary movement and respiration had ceased, the poison could have passed with the blood from the femoral vein to the heart, from the heart to the extreme ramifications of the pulmonary artery, back again to the heart by the pulmonary veins, and, lastly, by the general arterial system to the spine. If a correct view of the facts be here taken, there scarcely seems any refuge for the physiologist, except in the novel, and, at first, startling doctrine,* that conia acts by entering the blood, and producing on the inner membrane of the bloodvessels a peculiar nervous impression, which is instantly conveyed by sympathy along the nerves to the organ remotely and ultimately affected.

After these remarks on the properties of conia, it remains to be seen whether they coincide with what is known of the properties of hemlock itself. On this question hangs the ulterior one, whether conia is the true active principle of the plant.

I have in vain attempted to settle the point by reference to the existing descriptions of the effects of hemlock by toxicological authors. Passing by, for the present, the statements of ancient authors, which will be reverted to afterwards, and proved not to be confidently referrible to the modern hemlock; we come first to the older modern writers, such as *MATTHIOLI* and *KIRCHER*. The former says, a vine-dresser and his wife, who ate hemlock roots for parsnips, became so delirious, that they ran about the house, frantically knocking themselves against every thing in their way ;† and the latter says, two monks from the same cause became raving mad, plunged into a pond, taking themselves for geese, and suffered long after from incomplete palsy and muscular pain.‡ It is difficult, certainly, to connect these narratives with the preceding details of the action of conia. As little connection can be traced with the matter of fact narratives of cases in more recent times, which all point at delirium, coma, and convulsions, as the leading symptoms. These cases, however, are li-

* Lately advanced by Messrs *Morgan* and *Addison* in regard to the action of poisons generally.

† Petri Andreæ Matthioli Commentarii in sex libros Dioscoridis, p. 736. Edit. Venetiis 1582.

‡ *Wibmer*, die Wirkung der Arzneimittel und Gifte. i. 172.

mited in number; few are minutely described; several occurred before physicians had learned to discriminate accurately the external characters of the true hemlock, and other poisonous umbelliferous plants resembling it; in scarcely any of them is allusion made to the risk of error from this cause, or any pains taken to establish the identity of the poison; and in some, including one related by myself, which occurred twelve years ago, the symptoms were taken at second hand, the individuals having died before being seen by a competent observer.* The resemblance subsisting between hemlock and various umbelliferous plants, such as *Cicuta virosa, Æthusa cynapium, Œnanthe crocata, Chærophyllum temulentum,* is alone sufficient to vitiate most of the modern descriptions of the poisonous effects of the first species. We know that they often have been confounded together; and hence we can have no certainty what the plant was in special cases of poisoning, where the name merely is given, and the narrative does not contain internal evidence of its exact nature. Singular then as it may appear, we are very imperfectly acquainted with the real effects of one of our most familiar poisons on the human body.

For the like reasons, our knowledge of the effects of hemlock on the lower animals is far from being precise or positive. The only unequivocal experiments, indeed, are those of *Professor* ORFILA,† and a few performed by *Professor* SCHUBARTH;‡ and from these we should infer, that, besides possessing irritant properties, hemlock induces giddiness, convulsions, loss of sensibility, palsy, and coma. This account does not agree with the account given above of the action of conia, which does not seem to affect the senses so long as the respiration goes on. But it is possible that the difference is more apparent than real, and that hemlock has

* *Gmelin's* Pflanzengifte 605. *Corvisart's* Journal de Médecine, xxix. 107. Philosophical Transactions, xliii. 18. *Wibmer, ut supra,* i. 171. My Treatise on Poisons.

† Toxicologie Générale, ii. 303.

‡ *Horn's* Archiv für Medizinische Erfahrung, 1824.

been supposed to extinguish sensation, merely because, by inducing general paralysis, it takes away the power of expression. At least, in some experiments I have made, sensation did not appear to be affected; and the whole phenomena were identical with those produced by conia. In these experiments I used very strong extracts prepared by absolute alcohol from the fresh leaves, or the full-grown seeds; and each of them occasioned, in doses of thirty grains or thereabouts, paralysis of the voluntary muscles, with occasional slight convulsions, then paralysis of the respiratory muscles of the chest and abdomen; and finally cessation of the action of the diaphragm; sensation appeared to continue so long as it was practicable to make an observation on the subject; and the heart contracted vigorously for a long time after death. From these extracts a very powerful odour of conia was disengaged by caustic potassa.

It seems to me clear, therefore, that the action of conia and of hemlock are identical, or nearly so; and that conia is either the active principle of the plant, or contains it in a modified form.

I wish I could have added to these observations on the poisonous effects of conia and hemlock, some account of their physiological properties in small doses. This branch of the inquiry into their action I have not yet been able to investigate. It cannot be pursued with any accuracy by experiments on the lower animals. The phenomena must be ascertained in the human subject chiefly, which I have not hitherto been able to accomplish. On this head it may merely be observed, that, if physicians or physiologists would acquire definite information as to the physiological effects of hemlock in small or medicinal doses, they must begin the inquiry anew. Little importance can be attached to any thing already done in this field, as I have no doubt whatever, that by far the greater proportion of the preparations of hemlock hitherto employed have been of very little energy, and in the doses commonly used are absolutely inert.

Having now accomplished the chief object of this paper, I ought perhaps to conclude; but there is a topic remotely connected with it, to which I may be allowed briefly to advert.

I have several times been asked by my literary friends what the poison could have been which was used by the ancient Greeks, and particularly the Athenians, for putting state-criminals to death. This question has engaged the attention of many commentators, and of some modern physiologists attached to the literature and medicine of the classic ages; and the general result has been a belief that our hemlock, the *Conium maculatum* of botanists, is the *Cicuta* of the Romans, the Κωνειον of the Greeks, and the Athenian state-poison. Others, however, have doubted the correspondence here supposed. And, although the adoption by modern botanists of the term *Conium*, for designating the genus to which our spotted hemlock belongs, may seem to set the matter at rest, the question is really far from settled; and the proofs given above of the erroneous or imperfect ideas entertained, even in the present day, of the effects of hemlock, would reopen it even if it had been closed.

This inquiry is obviously one of much interest to every scholar and physician; for, on the one hand, it involves the identification of an ancient medicine of no mean repute; and on the other, it tends to enliven our conceptions of one of the most interesting periods of classic history, by enabling us to point to a known substance as the poison by which SOCRATES and PHOCION died.

In considering the subject, it is right to inquire, in the first place, whether our knowledge of the botanical and poisonous properties of hemlock corresponds with what ancient medical authors have said of the plant Κωνειον. Here we ought to be at no loss; for both *PLINY* and *DIOSCORIDES* have left tolerably minute descriptions of the plant; and *NICANDER*, in his poetical treatise on poisons, has described its effects on the body, and been followed in his description by succeeding writers. *DIOSCORIDES*, in his fourth book, thus lays down its botanical characters:—" The plant Κωνειον produces a tall stem, jointed like that of fennel [*Anethum fæniculum*];* leaves like the ferula [*Ferula communis*], but narrower, and of a heavy smell; branch-shoots and umbels at the summit; a whitish flower; a seed like that of anise, but whiter; a hollow root, not deep."† The description given by PLINY of the Cicuta, which is well made out to have been the Greek Κωνειον, follows closely that of DIOSCORIDES. "The stem * * * is smooth, jointed like a reed, blackish, taller frequently than two cubits, very branchy at top. The leaves are more tender than those of coriander, of a heavy odour; the seed thicker than anise; the root hollow." ‡

Now it appears to me impossible to refer the plant by these descriptions to the modern *Conium maculatum*. The description of the stem, leaves, summit, flower and seed, is so vague, that it will apply to twenty umbelliferous species as well as to our hemlock. PLINY's term *nigricans*, applied to the stem, is but a feeble approach to the very remarkable character of the modern plant, the purple-spotted stem,—a character so obvious, that one can scarcely imagine an ancient herbalist omitting it in taking a description from an actual specimen. Leaves narrower than those of ferula, more tender than those of coriander, if modern scholars and physicians are right in so translating the Greek ναρθηξ,§ and

* "Μαραθρον, Anethum fæniculum; hodie αγριομαλαθρον. Icon. English Botany, t. 1208." Sibth. Flor. Græc. Prod. i. 204.

† Κωνειον καυλον ἀνιησι γονατωδη ὡς μαραθρον, μεγαν [some read μελανα], φυλλα δε ναρθηκι ἐμφερη, στενωτερα δε και βαρυοσμα. ἐπ' ἀκρων δε ἀποφυσεις και σκιαδια. ἀνθος ὑπολευκον. σπερμα ἐμφερες ἀνισω, λευκωτερον. ριζα κοιλη και ȣ̓ βαθεια. Dioscorides, iv. 79. Edit. J. A. Saraceni, 1598, p. 276.

‡ Cicuta quoque venenum est * * * Caulis autem et viridis estur a plerisque et in patinis. Lævis hic et geniculatus, ut calami, nigricans, altior sæpe binis cubitis, in cacuminibus racemosus: folia coriandri teneriora, gravi odoratu: semen aneso crassius: radix concava, nullius usus. *Plinii Historia Naturalis*, xxv. 95.; *p.* 421 *in Edit. Brotier, Paris* 1779.

§ Sibthorpe found the *Ferula communis* of Linnæus growing on Cyprus, where it is now called αναρθηκα; and he conceives it to be the Ναρθηξ of Dioscorides. [Floræ Græcæ Prodromus, i. 190.] He refers for a representation of it to Dodonæus, Historia Stirpium, Antverpiæ 1676, p. 321. The rude drawing there given has certainly considerable resemblance to hemlock in the leaves; but equally resembles many other umbelliferous plants.

Latin *coriandrum*, cannot be held to designate with any accuracy the leaes of our conium ; and still less will the *radix concava* of Pliny, the ῥιζα κοιλη και ȣ βαθεια of Dioscorides, designate its root, which is perfectly solid, and even at the present season [December], when the plant is young, very commonly penetrates twelve or fifteen inches into the soil. Of the poisonous Umbelliferæ, our *Cicuta virosa*, or water-hemlock, comes nearer the ancient description than any other ; and, in reference to the use alleged to have been made of the ancient Κωνειον, it has the farther advantage of being generally considered a much more active poison than the common hemlock.

It may be right to add, however, that the *Conium maculatum* of modern botanists has been ascertained by *Dr Sibthorpe* to grow abundantly in different parts of Greece. This eminent authority states he found it " on rubbish-heaps near Constantinople: not unfrequently in the Pelopponesus ; and most abundantly between Athens and Megara."* It would have been very extraordinary if this species had not been a common plant in Greece, considering that it is abundant in every country in Europe. But I do not see how such a circumstance should be considered any proof that the ancient Κωνειον was our spotted hemlock, although some authors seem to look upon it as ample evidence of their identity.† It is not unworthy of remark, that the ancient term has been apparently lost in the modern Greek language, and that it bears no relation whatever to the modern name βρομοχορτον, now applied, according to Sibthorpe, to the spotted hemlock.

The ancient accounts of the properties of Κωνειον as a poison

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* In ruderatis prope Byzantium. In Pelopponeso haud infrequens. Copiosissime inter Athenas et Megaram.—Sibthorpe Flora Græca, i. 187.

† Ce qui prouve que c'était la ciguë dont les Athéniens se servaient pour faire périr certains personnages, et dont Socrate mourut. Il ne peut pas y avoir le moindre doute à ce sujet, car la ciguë vireuse ne se trouve pas dans ce pays, non plus que, &c. *Mérat et Delens.* Dict. de Matière Méd. ii. 385.

harmonize better than their botanical descriptions with what is known of the modern *Conium.* The fullest account is that of *NICANDER* in his Αλεξιφαρμακα; and subsequent writers have either followed him, or, where they have deviated, seem to have had in their eye the supposed properties of the Athenian state-poison. "Behold also," says he, "the baneful draught of Κωνειου. For this potion carries destruction to the powers of the mind, [*literally*, to the head], bringing shady darkness; and makes the eyes roll. But staggering on their footsteps and tripping on the streets, they creep on their hands. And mortal stifling seizes the upper part of the neck, and obstructs the narrow passage of the throat. The extremities grow cold; the strong vessels in the limbs contract; he ceases to draw in the thin air, like one fainting; and the soul visits Pluto." * The Greek Κωνειον, according to this poetical version, rendered into brief prose, brings on obliteration of the mental faculties, dimness of sight, giddi-

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* Και τε συ κωνειȣ βλαβοεν τεκμαιρεο πωμα.
Κεινο ποτον δη γαρ τε καρηατι φοινον ἰαπτει,
Νυκτα φερον σκοτοεσσαν. ἐδινησεν δε και ὀσσε.
Ἰχνεσι δε σφαλεροι τε και ἐμπαιοντες ἀγυιας,
Χερσιν ἐφερπυζȣσι· κακος δ᾽ ὑπο νειατα πνιγμος
Ἰσθμια και φαρυγγος στεινην ἐμφρασσεται οἰμον.
Ἀκρα δε τοι ψυχει. περι δε φλεβες ἐνδοθι γυιων
Ῥωμαλεαι στελλονται. ὁ δ᾽ ἠερα παυρον ἀτιζει,
Ὁια καταβολεων· ψυχη δ ἀιδωνεα λευσσει.
Νικανδρȣ Αλεξιφαρμακα. Editio Parisiis 1587, p. 140.

Tu quoque signa malæ jam contemplere cicutæ.
Hæc primum tentat caput, et caligine densa
Involvit mentes; oculi vertuntur in orbem;
Genua labant. Quod si cupit ocyus ire, caducum
Sustentant palmæ corpus; faucesque premuntur
Obsessæ, et colli tenuis præcluditur isthmus.
Extremi frigent artus, latet abditus imis
In venis pulsus, nihil inspiratur ab ore.
Fata instant, Ditemque miser jamjam aspicit atrum.
[Interprete J. Gorræo.]

ness, staggering, stifling, coldness of the limbs, and death by asphyxia;—a view of its effects which differs little from the modern notions of the poisonous action of the spotted hemlock. But the poetical effusion of NICANDER will apply equally well to many narcotics, and among others to various umbelliferous plants: It is a generic, though probably intended for a specific, description.

NICANDER, who appears to have lived about 160 years before Christ, or, according to some, a century later, has evidently been followed by *DIOSCORIDES*, *PLINY*, and other subsequent authors, in his description; and where any deviation is observable, it has been in favour of *PLATO's* account of the effects of the Athenian state-poison in the case of SOCRATES,—this being either tacitly or expressly assumed to have been a preparation of Κωνειον. It seems needless, therefore, to prosecute the present branch of the inquiry by reference to other ancient narratives.

The result at which we must arrive is, that the Greek Κωνειον, so far as regards its effects, *may* be the modern *Conium maculatum*, but may be equally referred to various other plants; and that, if its botanical description by classical authors is to be allowed any weight at all, or, which amounts to the same thing, if we admit that the ancient naturalists did describe, and could describe, from nature, it must have been a totally different vegetable.

Turning, in the last place, to the Athenian state-poison, we find both historians and political authors who lived during the time it was in frequent use, assuming very much, as a matter of course, that this was the Greek Κωνειον; and subsequent writers identify it either with this plant, or with the Roman *cicuta*, which we have already seen to be the same with the Κωνειον.

Thus *XENOPHON*, who died forty years after SOCRATES, and during whose lifetime the state-poison was the constant instrument of judicial murder, speaking of the death of THERAMENES, condemned for his political acts by the thirty tyrants, says, "and when he was condemned to die, and was drinking the Κωνειον, it is said he tossed away what remained, exclaiming, &c.* In like manner the orator *Lysias* observes, in his oration against Eratosthenes, who had put his brother Polemarchus to death: "The thirty despatched to Polemarchus their customary order to drink Κωνειον."† About the middle of the second century of the Christian era, we find Diogenes Laertius following these authorities, when he mentions the death of Socrates. "Socrates imprisoned," observes he, "after a few days drank the Κωνειον, discoursing many beautiful and good things, which Plato has given in his Phædo."‡ The same assumption is made by Pliny between six and seven centuries later. "The *cicuta*," says he, "is a poison, abhorred because the instrument of public punishment among the Athenians, yet applicable to many purposes which are not to be omitted."§

It is, nevertheless, not a little singular, that no mention is made of the Κωνειον as the state-poison of the Athenians by an author in natural history, who flourished at a time when the memory of the death of Socrates, Phocion, and many other eminent individuals, must have been fresh in the minds of all philosophers, and who nevertheless mentions both the plant and its poisonous qualities,—I mean *Theophrastus*. Theophrastus was born but twenty-eight years after the death of Socrates, namely, 371 years before Christ, and was Aristotle's successor

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* *Και ἐπει γε ἀποθνησκειν ἀναγκαζομενος το κωνειον ἐπιε, το λειπομενον ἐφασαν ἀποκοτταβισαντα ἐιπεῖν αὐτον. Κριτια τȣτ᾽ ἐστω τω καλω.* Xenophontis Hist. Græc. ii. 3. 24. Tom. iii. 103. Edit. Dodwell, 1770 Oxon.

† *Πολεμαρχω δε παρηγγειλαν οἱ τριακοντα το ἐπ᾽ ἐκεινων ἐιθισμενον παραγγελμα, πινειν κωνειον.* Lysias. Orat. in Eratosthenen. In Orat. Græc. vol. v. p. 394. Editio Reiske. Lipsiæ 1772.

‡ Cicuta quoque venenum est publicâ Atheniensium pœnâ invisa, ad multa tamen usus haud omittendi. Plinii Hist. Nat. xxv. 95.

§ *Σωκρατης δε δεθεις, μετ᾽ ȣ̓ πολλας ἡμερας ἐπιε το κωνειον, πολλα καλα κ᾽ ἀγαθα διαλεχθεις, ἁ Πλατων ἐν τω Φαιδωνι φησιν.* Diogenes Laertius. Editio Meibomii. Amstelod. I. ii. 42. Vol. i. 105.

in the Lyceum; yet he is altogether silent on the point. Every commentator has adduced the following remarkable passage from this author. "THRASYAS, the Mantinean, said, that by making use of the juices of the Κωνειον, the poppy, and such other things, he had discovered a substance which occasioned death easily and without pain, and so portable and minute, that the weight of a drachm was sufficient, and absolutely without a remedy, and capable of being preserved any length of time without alteration."* It has been supposed by some that THEOPHRASTUS has here described the state-poison of the Athenians; but there is no evidence to this effect; and his silence on the point would rather tend to shew that the state-poison was a different substance.

Leaving these vague inquiries, however, let us see what has been said of the effects of the Athenian poison in those who were put to death with it; and we may then be able to settle, independently of either the assumptions or omissions of classic authors, whether it was the κωνειον of the Greek physicians or the *conium* of the moderns, or what else it may have been.

So far as I am aware, there is but one account extant of the effects of the Athenian state-poison, but it is clear and precise. I allude to the familiar and pathetic narrative by PLATO of the last hours of SOCRATES. Having first stated that the executioner told the philosopher that nothing could be spared from the dose for a libation to the Gods, and that he was to walk about till he should feel his legs becoming heavy,—PLATO goes on to say that SOCRATES drained the cup with tranquillity, upbraided his friends for their weakness when they burst into tears, and proceeded to walk as he had been directed. "At length," continues the narrative, "when he felt his limbs grow heavy, he lay down on his back; for so the man had told him to do. And at the same

* Θρασυας δε ὁ Μαντινευς ευρηκεναι τι τοιουτον ὡσπερ ελεγεν. ὡστε ῥαδιαν ποιειν και απονον την απολυσιν, τοις ὀποις χρωμενος κωνειου και μηκωνος, και ἑτερων τοιουτων. ὡς τε ευογκον ειναι πανυ και μικρον ὁσον εις δραχμης ὁλκην. αβοηθητον δε παντη και δυναμενον διαμενειν ὁποσουν χρονον, και ουδεν αλλοιουμενον. Theophrastus, Lib. ix. I. xviii. Editio Amstelodami 1644.

time the person who administered the poison went up to him, and examined for a little while his feet and legs, and then squeezing his foot strongly, asked whether he felt him do so? SOCRATES replied that he did not. After this the man did the same to his legs, and proceeding upwards in this way shewed us that he was cold and stiff. And he approached him and said to us, that when the effects of the poison should reach the heart, SOCRATES would depart. And now the parts about the lower belly were cold, when he uncovered himself (for he was covered up), and said, which were his last words: 'CRITO, we owe ÆSCULAPIUS a cock; pay the debt, and do not forget it.' 'It shall be done,' replied CRITO; 'but consider whether you have any thing else to say.' SOCRATES answered not, but in a short time was convulsed. The man then uncovered him. His eyes were fixed; and when CRITO observed this, he closed his eyelids and his mouth."*

If this narrative be altered to a modern toxicological description, it is plain that the Athenian state-poison must be regarded as producing spasm and coldness of the limbs, gradually advancing to the internal parts, causing death eventually by acting either on the heart or respiration, and without affecting the functions of the mind even to the very last.

Such a view of its action is altogether at variance equally with the effects usually ascribed in recent times to the spotted

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* Ὁ δε περιελθων, ἐπειδη οἱ βαρυνεσθαι ἐφη τα σκελη, κατεκλιθη ὑπτιος· ὅτω γαρ ἐκελευσεν ὁ ἀνθρωπος. και ἁμα ἐφαπτομενος αὐτου οὗτος ὁ δους το φαρμακον, διαλιπων χρονον ἐπεσκοπει τους ποδας και τα σκελη. κἄπειτα σφοδρα πιεσας αὐτου τον ποδα ἠρετο εἰ αἰσθανοιτο· ὁ δ᾽ οὐκ ἐφη. και μετα τουτο αὖθις τας κνημας· και ἐπανιων οὑτως ἡμιν ἐπεδεικνυτο ὁτι ψυχοιτο τε και πηγνυτο. και αὐτος ἡπτετο, και εἰπεν ὁτι ἐπειδαν προς τη καρδια γενηται αὐτω, τοτε οἰχήσεται. ἠδη οὐν σχεδον τι αυτου ἠν τα περι το ἠτρον ψυχομενα, και ἐκκαλυψαμενος, ἐνεκεκαλυπτο γαρ, εἰπεν, ὁ δη τελευταιον εφθεγξατο, Ὦ Κριτων, ἐφη, τω Ἀσκληπιω ὀφειλομεν ἀλεκτρυονα. ἀλλ᾽ ἀποδοτε και μη ἀμελησητε. Αλλα ταυτα, ἐφη, ἐσται, ὁ Κριτων· ἀλλ᾽ ὁρα εἰ τι ἀλλο λεγεις. Ταυτα ἐρομενου αὐτου οὐδεν ἐτι ἀπεκρινατο, ἀλλ᾽ ὀλιγον χρονον διαλιπων ἐκινηθη τε και ὁ ἀνθρωπος ἐξεκαλυψεν αυτον, και ὁς τα ὀμματα ἐστησεν· ἰδων δε ὁ Κριτων ξυνελαβε το στομα τε και τους ὀφθαλμους. Platonis Dialogi. Ex Recensione I. Bekkeri. Berol. 1817, vol. iii. par. 2, p. 127. Phædo.

hemlock, and with the phenomena presented in the experiments described in the present paper. It seems also not less at variance with the ideas entertained by the Greeks of the poisonous operation of their κωνειον, as will be apparent on comparing PLATO's narrative with the general description of NICANDER. And lastly, it seems to me incompatible with the ascertained effects of every poison whatsoever, which is known in modern times; for I think it will puzzle the most learned toxicologist to point out any poison which has the property of occasioning coldness and stiffness of the limbs, proceeding gradually upwards, and proving fatal without causing either pain or sopor.

There seems, then, no alternative but to conclude, either that the description of PLATO—who, it must be remarked, was not present at the death of SOCRATES,* as many imagine—is not a detail of facts, but an embellished narrative, written for effect; or that, although we are now acquainted probably with fifty times as many poisons as the ancient Athenians, and with many which are fifty times as active as any in their list, we have lost acquaintance with one with which the ancients were quite familiar, and which differs totally from every known poison in its action.

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* PLATO makes PHÆDO inform ECHECRATES in the Dialogue, that he was absent owing to sickness. "Πλατων δε, οιμαι, ησθενει." (Platonis Dialogi. &c. p. 6.)

APPENDIX OF SELECTED EXPERIMENTS.

I. *Experiments with Conia.*

EXP. I. Six drops of conia were allowed to fall into the back of the throat of a young active puppy ten weeks old. In thirty seconds there was sudden convulsive respiration, and some stiffness of the hind-legs, immediately followed by great feebleness of these legs. In a few seconds the fore-legs also became very feeble. In sixty seconds from the time the poison was introduced, the breathing ceased. Slight convulsive tremors followed for a single minute more.

In three minutes the chest was laid open. The intestines were moving and the heart pulsating briskly. In fifteen minutes the auricles of the heart ceased to contract spontaneously, but they began again to contract when the pericardium was slit open. In eighteen minutes, when all spontaneous motion had ceased, galvanism directly applied to the right ventricle excited feeble, tremulous contraction. The blood then discharged from the heart flowed out quite fluid, equally dark from both sides, and coagulated firmly afterwards. In eight minutes the diaphragm, as well as the *serratus magnus* muscle, contracted briskly when a galvanic current was transmitted from their respective nerves to their substance; and the latter also contracted when its nerve was either pinched or galvanized by touching it with both poles of the pile; not so, however, the diaphragm. The vermicular movement of the intestines was instantly arrested by allowing a drop of conia to fall on a loop of the gut, but only in the part touched, which also contracted much, and became greyish-white.

EXP. II. A small aperture was made into the peritoneal sac of an active puppy of the same litter with the last, and three drops of conia were introduced completely within the peritoneum by a pipette. Instant acute expression of pain followed. For some little time there was no visible effect, and it pursued a rabbit briskly up and down the room. But in two minutes it suddenly stopped, and the hind-legs became stiff. Then the breathing became laboured, and the hind-legs feeble. In three minutes and a half there was great feebleness of the whole limbs, so that it rose with difficulty. In four minutes and a half it fell forward in a paralytic state on trying to walk; and the limbs were completely palsied. In five minutes the respiration was diaphragmatic only, the chest being perfectly flaccid. The sensibility was still quite entire, the eyes turning in the direction of one calling to it, and the countenance expressing uneasiness when the paws were squeezed. In seven minutes the breathing was confined to feeble respiratory twitches, and the whole body became flaccid, with occasional tremulous movement of the muscles of the trunk. In seven minutes and a half the breathing ceased. Slight muscular twitches continued for two minutes and a half more.

The body was opened in twelve minutes. In thirteen minutes the spinal accessary nerve was pinched and galvanized with the effect of exciting brisk contraction of the *trapezius* muscle. In fifteen minutes the fore-leg contracted strongly by similar stimulation of the axillary plexus. In twenty minutes the diaphragm contracted strongly when galvanism was transmitted from the phrenic nerve to its muscular structure, but not when both poles of the pile were applied to the nerve. In twenty-five minutes the *trapezius* could not be made to contract by pricking or galvanizing its nerve. In twenty minutes the heart ceased to contract. The blood in it was fluid, dark in the left side, and coagulable in the usual way. A small portion of intestine near the aperture through the parietes was very vascular, and with a yellowish-red blush. In thirty-two minutes the vermicular motion was going on feebly in the intestines.

This experiment illustrates well the leading points of the description in the text,—the gradual progress of paralysis, the integrity of the senses, the integrity after death of muscular contractility, the freedom of the heart from depressed action, and the natural state of the blood. The gradual progress of the paralysis and integrity of the senses are still better shown in the following experiment, where the successive phenomena succeeded one another more slowly.

Exp. III. Three drops of conia were introduced through a small wound between the skin and aponeurosis in the nape of a puppy of the same litter as the two former animals. In three minutes there was slight stiffness of all the legs. In four minutes and a half the animal seemed very weak, weary, and uneasy, but perfectly sensible. In ten minutes it suddenly staggered forward and fell on trying to run, and had extreme feebleness of all the limbs. In twelve minutes the breathing was hurried and short, with perfect flaccidity of the chest, extreme general debility and apparent stupor; but the animal was quite sensible in reality, changing its position when touched, and shuffling forward on its belly when called to. In fifteen minutes the breathing was entirely diaphragmatic, and the four limbs stretched out at right angles to the trunk completely paralyzed; but when called to it wagged its tail feebly, turned the eyes in the direction of the voice, and made a feeble effort to raise the head, but could not; when a rabbit was placed near, it made evident attempts to reach it; and when the tail was pinched it tried to bark, but could produce only a few indistinct croaking sounds. In twenty-six minutes it could still make a few indistinct efforts to change posture by moving the hips and upper joints of the hind-legs, and exhibited the same proofs of sensibility. The breathing continued gradually less and less distinct. In thirty-five minutes the whole muscular system was completely paralyzed, except that there were feeble respiratory movements of the diaphragm, and occasional tremulous contractions of the muscles of the whole trunk. In forty minutes these movements were scarcely perceptible except at occasional intervals accompanying feeble efforts to breathe. The eyelids still contracted when the cornea was touched. In forty-two minutes all motion had ceased. There was copious salivation from an early period till the very last.

In forty-six minutes the chest was laid open. The heart was acting vigorously; and in sixty minutes the right auricle continued to contract. The veins were much gorged; the blood fluid, but coagulable as usual. In forty-eight minutes the diaphragm and *trapezius* muscles contracted when their respective nerves were galvanized.

Exp. IV. Two drops of conia were dropped into the eye of a strong rabbit. In one minute the hind-legs became stiff and spread, and in one minute and a half the fore-legs also, with hurried breathing. In two minutes and a half convulsive twitches of the neck occurred. In three minutes and a half the eye which had received the poison was so insensible, that the eye-lids did not wink when the cornea was irritated, while the other eye continued quite sensible. In four minutes and a half the convulsive twitches were more marked. In five minutes the eye not touched with the poison continued sensible. In eight minutes and a half strong convulsions were excited by dipping the animal in cold water. In nine minutes and a half the breathing was convulsive. In ten minutes the respiration ceased; and then, for the first time, the untouched eye became insensible.

In sixteen minutes the diaphragm and *serratus magnus* muscle contracted freely when their respective nerves were touched or galvanized.

Exp. V. Two drops of conia were introduced into a wound near the shoulder of a very strong rabbit. In one minute and a half the hind-legs became stiff, and in two minutes paralyzed and spread out. In two minutes and a half the fore-legs became stiff. In three minutes and a quarter the whole legs were extremely weak, and the position of the animal prone, with the legs extended from the sides, the respiration laboured and short, as if from debility, and the muscles of the chest flaccid. General tremor occurred whenever it tried to move. In four minutes and a half the head was feebly twitched backwards. In five minutes the paralysis was complete, and the breathing diaphragmatic; but in six minutes there was more convulsive tremor than was remarked in any other experiment. In seven minutes and a half the sensibility seemed entire, so far as could be judged of in the paralyzed condition of the animal; the eyelids winked when approached, tremors were occasioned by touching it, or even by striking the table near it, and one iris was sensible to light. In eight minutes the respiration ceased.

The chest being laid open soon after, the heart was seen in twelve minutes pulsating vigorously. A drop of conia allowed to fall on the right auricle, at once stopped its action, and rendered it no longer irritable, even under galvanism; and the same effect was produced, though less quickly, on the right ventricle. In nineteen minutes, while the vermicular movement of the intestines was vigorous, a loop was pulled aside, and touched with conia; upon which it instantly contracted with force, and the vermicular action was no longer continued through that portion, but was arrested on approaching it; nor was this part irritable under galvanism. This experiment was repeated on various portions of intestine, and invariably with the same effects. In twenty-three minutes the muscles on the inside of both thighs were laid bare, and several drops of conia were spread over those of one leg. In a few seconds galvanism, which previously excited vigorous contraction in the muscles of both sides, caused much less in the poisoned limb than in the other; and this difference quickly increased, till the contractility was all but extinct in the former, while it remained tolerably entire in the latter.

Exp. VI. The preceding experiments, on the direct action of conia on muscular contractility, were repeated on the muscles of a frog. The hind legs were severed from the body, and then from one another, and skinned. In the intervals between the experiments, the limbs were covered with the detached skin to preserve the external muscles from drying. One limb was not otherwise treated; but the other had conia carefully spread over the whole exposed muscles,—in the state, however, of neutralization with muriatic acid, in order to get rid of the effects possibly arising from the direct irritant action of conia itself. In forty-five minutes, when the crural nerve of the limb not touched with the solution, was galvanized, powerful contraction was excited in all the muscles; but in the poisoned limb the thigh alone moved, and that much less than the other. In sixty minutes the contractions in the thigh of the poisoned limb were very faint, while in the other the whole limb was powerfully contracted. In seventy-five minutes contractions were excited very feebly in the poisoned limb, and only when the galvanic circle was interrupted; while, in the other, they were nearly as strong as ever. Next morning, twenty-one hours from the beginning of the experiment, the femoral muscles of the unpoisoned limb contracted obviously when a galvanic current was transmitted to them from the femoral nerve, but not when both poles were applied to the nerve. In the poisoned limb, no trace of movement could be elicited in any manner.

The facts in this and the two preceding experiments show, that conia and its muriate produce exhaustion of muscular contractility wherever they are directly applied; although this property of muscular fibre seems wholly unaffected by the indirect action of the poison. It is not unworthy of remark, that, in the tenth experiment, muriate of conia seemed to have no direct exhausting effect on the contractility of the heart.

EXP. VII. Two drops of conia were introduced under the skin of a strong frog into the cellular tissue of the back. In three minutes there was obviously great general weakness; and in four minutes such complete paralysis of the fore-legs, that even galvanism applied to them excited no voluntary motion, but merely the usual twitches produced by that agent after death. In five minutes and a half the respiration ceased; in six minutes there was no movement anywhere, except slight twitches of the hind-legs; in eight minutes the eyelids were insensible; and after this no movement of any kind could be induced, when the skin was pinched and pricked in various parts. In twenty minutes the muscles of the hind legs did not contract when their nerves were galvanized; but they contracted briskly when a galvanic current was passed from the nerve to the muscles. The heart continued to contract long after death.

I believe there are few poisons which act on the frog with so much energy as conia is here proved to do.

EXP. VIII. The following experiment was tried on the effect of artificial inflation of the lungs, the subject of experiment being a middle-sized dog, thin, and full-grown. A tube was fixed in the wind-pipe, having a large elliptic lateral hole; and a brass syringe was fitted upon the extremity of the tube. By applying the finger over the lateral hole, while the piston was gently thrust down,—then withdrawing the finger, and allowing the injected air to rush out under the impulse of the contracting chest,—next drawing up the piston,—and then re-applying the finger, it was found easy to maintain the respiration between fourteen and twenty-four times per minute, which was the natural rate in this animal. Four drops of conia were then introduced through a small aperture into the cellular tissue behind the fore-shoulder. For fourteen minutes little effect was observed, possibly owing to the animal being firmly tied, and also appearing to be of singularly dull sensibility from the first. At this time three drops more were introduced into another wound; but at the very same instant, the animal made a few hurried inspirations and expirations, followed by gradual diminution of the breathing, till it became diaphragmatic only, and then, in seventeen minutes, nearly extinct. At this time the eyelids winked, when the hand was brought rather suddenly near them; and when the muzzle was pricked, the muscles of the nose acted feebly, and the head was feebly withdrawn. Artificial inflation of the lungs was immediately begun. The heart, which had previously begun to beat slowly, very soon pulsated more firmly and quickly; and the usual convulsive twitches consequent on cessation of the breathing ceased. After continuing the inflation for six minutes, and observing that the eyelids winked when the hand was waved before the eyes, and that the head was very feebly withdrawn when the muzzle was pricked, the process of inflation was discontinued for one minute; convulsive twitches of the limbs, chest, and abdomen immediately ensued, in consequence of which faint respiration was occasionally accomplished, but nothing like regular breathing. These movements had nearly subsided, and the heart had begun to beat more slowly, when the artificial inflation was resumed, and continued for four minutes. During this period the eyelids and muzzle still showed some sensibility, and the heart again pulsated vigorously. The artificial inflation of the chest was again interrupted for one minute with precisely the same phenomena as before, and again resumed, also with the same results, for nine minutes. At this time, thirty-eight minutes after the poison was applied, the sensibility of the eyelids and muzzle was distinctly improved, the head in particular being plainly withdrawn a little; but the pupil was dilated, and the iris quite insensible. The inflation was now discontinued for five minutes, with the same results as formerly; the animal was very nearly at the point of death, and the eyelids greatly less sensible, when, on again resuming artificial respiration, the heart recovered its usual rate and force, and the sensibility was somewhat improved. In fifty-two minutes, however, under continued inflation, the sensibility was greatly impaired, although the heart pulsated vigorously. The experiment of inflation was then abandoned. The usual convulsive twitches of asphyxia followed; and for six or seven minutes there were feeble, irregular, respiratory movements, as proved by placing the felt of a hat over the hole in the tube.

In sixty-one minutes every muscular movement had ceased, except that the heart was felt pulsating through the parietes of the chest. It continued to beat for some minutes after death. The blood was fluid and coagulated as usual.

Exp. IX. Three drops of conia were dropped into the eye of a strong and furious cat. Instantly it made violent efforts with its fore-paws to rub out the poison. While continuing to do so, the hind legs became, in forty seconds, affected with slight convulsions, and so weak that it could not support itself on them. Soon afterwards the fore-legs also became weaker and weaker; but they were still constantly employed in efforts to rub the poison out of the eye, and these efforts continued, though gradually becoming more and more feeble and imperfect, so long as there was any power of motion left. On account of the frequency of these voluntary efforts, it was impossible to say how soon the respiration was affected, till in ninety seconds, when all voluntary motion ceased; and it was then found that the respiratory movements had ceased also, even the diaphragm being motionless. A few convulsive twitches succeeded, as in ordinary cases of death by asphyxia. The heart pulsated vigorously for five minutes after the breathing ceased. It was then tied firmly at its base, removed from the body, and washed. The blood was immediately afterwards discharged from it into a clean saucer, and ascertained, by four persons present, not to present the slightest odour of conia.

The continuance of voluntary efforts to the very last in this instance is a decided proof that volition, and consequently sensation, were unaffected. The absence of any odour of conia in the blood of the heart, seems sufficient proof that, if absorbed, it is nevertheless not necessary that the poison should reach the heart in order to exercise its peculiar action.

II. *Experiments with Muriate of Conia.*

EXP. X. Four drops of conia, neutralized with diluted muriatic acid, or rather with the faintest excess of acid, were introduced through a small wound into the cellular tissue behind the fore-shoulder of a strong rabbit. In fifteen seconds, all the legs became for a few seconds somewhat stiff, then paralyzed, and the animal fell down prostrate, flaccid, entirely palsied, but affected occasionally with rather brisk convulsive twitches, renewable by touching the body. In one minute the respiration was entirely diaphragmatic; and in ninety seconds it ceased altogether. For two minutes more the usual convulsive twitches of death by asphyxia presented themselves. The eyes were quite sensible till the breathing ceased; after which the lids did not contract even when the cornea was touched.

In four minutes, the chest being laid open, the heart was seen contracting briskly in all its parts; and the vivacity of its contractions was not affected by spreading solution of muriate of conia over the heart. In seven minutes the diaphragm, and the adductor muscle of the thigh, did not contract when their respective nerves were galvanized; but both contracted freely when galvanism was transmitted from their nerves to their substance. Even twenty minutes after the poison was applied, the heart continued its contractions briskly, the left side acting as well as the right. The vermicular movement of the intestines was also at that time active.

EXP. XI. The preceding experiment was repeated with two drops of conia, exactly neutralized with muriatic acid; but the rabbit was smaller and younger. In fifteen seconds the animal, which had been sitting, rose with the hind-legs stiff; but paralysis of these legs instantly succeeded, the fore-legs became similarly affected, complete prostration and general palsy ensued, and the breathing was short, slow, laboured, and diaphragmatic. So far as could be judged the animal was sensible. In sixty seconds the breathing ceased, without any previous convulsive movements. Convulsive twitches followed as usual.

In two minutes the chest was laid open. The heart contracted vigorously in all its parts. Even in thirty minutes the force of its contractions was little diminished. In forty-two minutes the auricles alone continued to contract a little. The blood in the left side of the heart was as dark as in the right side, fluid, and coagulable.

The tenth and eleventh experiments, when compared with the fifth, show that conia is considerably more rapid in its action when neutralized than when free. I do not know any variety of death, in which the heart remains so long and so remarkably contractile as these experiments show to be the case in poisoning with muriate of conia

Exp. XII. Seven drops of a faintly acidulated solution of muriate of conia, containing two drops of conia, were diluted to one drachm, and blown from a pipette into the stomach of a small rabbit, by a hole in the gullet, which was afterwards tied. In one minute the hind-legs became stiff, causing the animal to rise as usual straight up from the sitting position. Instantly, however, paralysis ensued, and it fell down prostrate and flaccid. The respiration at the same time was short, laboured, and diaphragmatic, and the eyes sensible. In two minutes the breathing ceased, and the eyes became insensible even when touched. Convulsive twitches succeeded for two minutes.

The heart contracted vigorously long after death, the ventricles acting a little forty minutes afterwards, and the right auricle feebly even for sixty minutes. There was a strong odour of conia in the peritoneal sac, when the belly was laid open, about five minutes after death.

The odour of conia in the peritoneum was here, I presume, owing to its transmission by imbibition, or exosmose. Similar phenomena have been observed of various other poisons, and among the rest of oxalic acid, as was proved by *Dr* Coindet and myself in 1823. [Edinburgh Med. and Surg. Journal, xix. 163.]

Exp. XIII. Thirty drops of water, containing two drops of conia, exactly neutralized with muriatic acid, and preserved in this state for a month, were blown from a pipette into the left femoral vein of a middle-sized puppy, care being taken to prevent any air entering the vein, and to tighten a noose round the vein instantly afterwards. I had taken precautions for noting the time, at the instant after injecting the poison, leaving the care of the ligature to another; but, in the very act of observing the time, I happened to glance over my watch at the animal, and saw that respiration and motion had ceased. Certainly three seconds had not elapsed before this observation was made. The usual slight convulsive twitches followed, as in all cases of death by asphyxia.

The body was opened immediately. Two minutes after the cessation of breathing, the external arteries of the chest discharged blood *per saltum*. The heart contracted spontaneously for two or perhaps three minutes more, and remained contractile under excitement much longer. The blood was equally dark in the left as in the right side, fluid and coagulable. There was no air in the heart.

III. *Experiments with Extract of Hemlock.*

Exp. XIV. Six ounces of hemlock seeds fully developed, but still quite green, were carefully bruised and exhausted with absolute alcohol of density 797 in a percolator. The alcohol being in a great measure distilled off, a thick dark-green fluid remained, which was promptly evaporated in the vapour-bath to the consistence of a firm extract. This extract had a beautiful green colour, and a faint, sweetish, herbaceous odour, which gave place to a powerful odour of conia, when it was rubbed with solution of caustic potassa. It weighed seventy grains.

Thirty-one grains were dissolved in the same quantity of water, and the solution was injected between the skin and muscles of the back of a rabbit, care having been taken to detach them previously with the finger; and the poison was secured by a ligature. The animal immediately cried. In a minute and a half there was trembling and stiffness of the hind legs, hurried breathing, general languor, but no insensibility. In two minutes it fell down, with the chest flaccid, and the respiration diaphragmatic, and becoming quickly more and more limited in extent. In four minutes there were some convulsions of the hind-legs, and tossing back of the head, renewable by pulling the legs. The eyelids winked when the hand approached the eye, and the snout was twitched when the whiskers were pulled. In five minutes respiration ceased. The usual twitches of asphyxia followed for two minutes more.

The body being then laid open, the heart was found in nine minutes contracting, though not vigorously, and it was somewhat gorged. The blood flowed out as dark from the left as from the right side, and coagulated as usual.

Exp. XV. Thirty-one grains of the same extract, similarly dissolved, were injected between the skin and muscles of a stout small dog, five months old. In three minutes and a half the hind-legs were weak, and rather stiff, and in six minutes the fore-legs also. In seven minutes the bowels, and in nine minutes the stomach, were evacuated. In eleven minutes the breathing was diaphragmatic, and the chest flaccid; the animal had great difficulty in rising, from extreme muscular debility; and it made efforts to vomit, confined, however, to the diaphragm,—the muscles of the chest and parietes of the abdomen remaining flaccid. In fifteen minutes the breathing was slower and more limited. The legs were slightly twitched at times, but in the intervals paralytic. The efforts to vomit had ceased. The eyelids were quite active when the eye was approached with the hand. Occasional efforts were made to change posture as if from uneasiness, but ineffectually; and the same result followed in whatever position it was placed. In twenty minutes the respiration ceased; and then the eyelids for the first time became insensible. Convulsive twitches followed for two minutes more.

The chest was then laid open. In twenty-three minutes the heart was contracting feebly, and was gorged. Stronger contractions took place when the pericardium was slit open. Dark venous-like blood issued from the left ventricle when it was opened, and soon coagulated firmly. In thirty minutes the muscles of the chest and the diaphragm contracted strongly, when either the muscles themselves, or their nerves, were pinched.

Exp. XVI. Thirty-three grains of a different alcoholic extract, also of great strength, as proved by trituration with solution of caustic potassa, were dissolved in thrice their weight of water, and injected between the skin and muscles of the back of a strong adult rabbit. In three minutes the lower joints of the hind-legs were stiff, and this symptom was speedily followed by weakness of the hind-legs. In five minutes all the legs were extremely feeble, and in one minute more they were spread out laterally from the body, and affected with occasional convulsive twitches, which were renewed by touching the body. The general paralytic state was so great, that the animal was unable to alter its attitude, in whatever position it might be placed. In seven minutes the respiration was almost entirely diaphragmatic, and the paralysis of the trunk and limbs complete. The eyelids continued, however, to wink when the finger was brought near them, and the head was very feebly withdrawn when the hind-legs were pricked. In nine minutes the respiration ceased. In sixteen minutes the heart continued to pulsate vigorously.

To these experiments it may be added, that a flea touched with conia instantly dropped motionless and dead, and that a fly touched in like manner rose in flight, but suddenly dropped down, and shewed no farther sign of life. In a strong young puppy, where two drops were introduced into the eye, extreme uneasiness was produced from irritation, the tears were discharged profusely, and the sclerotic and conjunctiva quickly became vascular and considerably swelled; but, except staggering for an hour, apparently from weakness, no other marked effect resulted.

Account of the Invention of the Pantograph, and a Description of the Eidograph, a Copying Instrument invented by William Wallace, A. M., F. R. S. Edin., F. R. A. S., Memb. Cam. Phil. Soc., &c., Professor of Mathematics in the University of Edinburgh.

(*Read 13th January* 1831.)

The power of making such a representation of any object, as shall give a distinct idea of its form, is a faculty which artists possess in different degrees of perfection. The principal difficulty is, to get a first delineation of any subject; from this a copy may be made in various ways, with less exertion of talent than was required for the composition of the original.

Various geometrical and optical inventions have been proposed to assist the artist in making an outline of an object which he wishes to represent. The *Reticulated Square* and other contrivances, for placing every point of the thing to be represented in its proper place in the picture, belong to the first class; the *Camera Obscura* and the *Camera Lucida* to the second. When a design is to be *copied*, a different kind of contrivances will in general be more convenient. It is only of these that I propose to treat here.

In making a copy, the assistance the artist seeks from mechanical invention is, the power of forming a correct outline. In many cases, as in the formation of maps and some architectural designs, this is all that is wanted: the gradations of light and shade must, in general, be given by the imitative skill which the artist exerts by the eye alone.

There are various well known contrivances by which a copy of any design may be made, when it is to be exactly the size of the original. Of these, a sheet of semi-transparent tracing paper laid on the subject to be copied, is one of the most simple and elegant. On this, the engraver may make an outline of a subject which he means to transfer to his copperplate. If he set no value on the original design, he may dispense with the transparent paper; and having made a trace with black lead along the lines to be copied, he may render them visible on the copper by the pressure of the rolling-press.

Another elegant way of making a copy is by means of a tracing glass. This, fixed in a frame, is placed in a sloping direction like a writing desk: the sheet to be copied is laid on the glass, and the paper on which the copy is to be made above it: a strong light, either natural or artificial, is directed upwards through the glass, while the surface on which the copy is to be made is skreened from the light. The lines to be copied are thus made visible through the two sheets, and a trace is made on the uppermost by carrying a point over them.

If a subject is to be diminished or enlarged, none of these methods will apply. Now this is by far the most common case at the present time, when numberless Encyclopædias, Atlases, and other works illustrated by figures, are in progress of publication, the materials of which are, for the most part, taken from writers of established eminence.

It may be supposed that attempts must long ago have been made to furnish the various classes of artists with instrumental aid. In investigating this subject, however, I did not find that the mechanical contrivances for copying on an enlarged or reduced scale, had been numerous or considerably different, or that their application can be traced to a very remote period. There is a well known instrument called the PANTOGRAPH: this appearing to have been the earliest, and that from which all the others have been imitated, I was induced to look into its history; I found it figured and described by various writers, who yet have given no indication of its inventor. In a work called "Geometrical and Graphical Essays," by GEORGE ADAMS, printed in 1791, I found it described under the name of a *Pantographer*. ADAMS says, "I have not been able to ascertain who was the inventor of this useful instrument. The earliest account I find is that of the Jesuit SCHEINER, in a small tract entitled, *Pantographice, sive ars nova delineandi.*"

ADAMS must either never have looked into SCHEINER's book, or assuredly must have forgotten what is in it: MONTUCLA, in his *Histoire des Mathematiques,* says expressly that SCHEINER was the inventor. In enumerating this learned Jesuit's writings, he mentions his work called *Pantographia,* in which, he says, the construction and use of the Pantograph is described; and he adds, "that, independently of his other writings (which MONTUCLA did not highly esteem), the invention of this instrument alone ought to have given immortality to his name."

Besides this notice of MONTUCLA, and SCHEINER's own book, I have met with no other recorded testimony that he had the merit of being the inventor of the Pantograph, although such may exist.

There is an instrument, exactly the same as SCHEINER's, described and figured in a folio work on Geometry and Perspective (of which there were various editions), by SAMUEL MAROLOIS, who must have been his contemporary. There is also a figure and description of a like instrument in a small book called *Thaumaturgus Mathematicus,* printed in 1651; but in neither are we told who was the inventor, nor is even a name given to the instrument, although I have no doubt it was that of SCHEINER's invention.

I see it described in BION's treatise on Mathematical Instruments (edit. 1723), where it is called the *Pentagraph*: and again, in the History of the Academy of Sciences for 1793, where it is called the *Pantograph;* still the inventor is not named. In this last mentioned work, some defects in its construction are pointed out, and improvements suggested by LANGLOIS, Engineer to the

King and the Academy. In this state it was taken up by ADAMS (the father of GEORGE) the English Instrument-maker, who gave it nearly the form it has at the present day. MONTUCLA having said that the preface to SCHEINER'S book is very curious, I was desirous to see the work. It must be rare, for I could not find a copy in any of our Edinburgh public libraries. At length I found one in the Catalogue of the University Library of St Andrew's, from which, by courtesy, it was lent to me. I found the preface, or rather the first chapter, very curious indeed. I have shewn to several friends, members of this Society, a translation of it, who entertain the same opinion; and it has been suggested, that in this communication, in which I propose to describe an instrument for a like purpose, I may not improperly give the most interesting part of SCHEINER'S own account of his invention.

Translation of a part of the first Chapter of Scheiner's work, entitled "Pantographice, seu Ars delineandi res quaslibet per Parallelogramum lineare seu Cavum, Mechanicum, mobile," &c.

"Being, in the year 1603, a professor in the celebrated German Academy at Dillingen, where I taught in general polite literature, but sometimes also mathematical science, I contracted a friendship with an excellent painter named GEORGIUS, a man lame and deformed, from whom I learned some secrets of the arts and of nature, and communicated some discoveries of my own in return.

"This person boasted to me of possessing an admirable invention, namely, a compendious method of delineating any object, most easy, sure, and speedy to practise; so that whoever would take a drawing from any original, did it by regarding the original alone, without needing to look at the copy he made, and yet without erring in his delineation by one hair's-breadth. He declared farther, that in the drawing of any figure whatsoever, he was so assured of accuracy, that he could pass at once from forming the feet to represent the nose; then, after producing the hands, he could pass from them to delineate the eyes, or any other part. All these copies he asserted he could make either equal to, greater, or less than the original; which alone required to be seen, but always most exactly true: he never looking at the copy he made, although he could point out and designate any part of it he pleased.

"Upon hearing these particulars, and many more like them which he told me, being inflamed with a desire to learn his invention, I asked him to communicate it, promising to recompense the benefit by disclosing to him some similar and equivalent discoveries in the art of painting, which I thought were not commonly known. He replied, that he valued his invention so highly, that so far from thinking that any thing comparable with it existed, he did not believe that such could be even imagined; in fact, that it was not a human so much as a divine invention, which he thought had been brought and disclosed to him by no human efforts, but rather by some celestial genius. Therefore he declared that he could not bring his mind to barter a secret of that nature for any others whatsoever.

"I begged that at least he would give me some specimen of his art, by copying a picture before me. He replied, that to exercise his art before a bystander, was the same thing as teaching it, for he that saw it practised could not fail to learn to imitate it.

"Confounded beyond belief at this assertion, I asked him seriously if he was advancing fables or facts, hyperboles or the naked truth? He declared that he said even less than truth warranted.

"Upon this I, more full of admiration than ever, again inquired, how, if the artist was guided by seeing the original only, he could direct the copying pen or pencil without error? He replied, that the nature of the thing was such, that accuracy ensued infallibly, and as it were spontaneously: so that it was impossible to err without design. I inquired whether the effect was wrought by the drawing of lines, or by the help of a material instrument? Here he began to hesitate, and to speak evasively, avoiding a clear answer, and hiding a thing, dark and unintelligible in itself, in obscure language. This alone he admitted, that the thing was done by the help of compasses depending upon a firm centre. I asked him to shew me these compasses: he refused, upon the plea that whoever saw them would at once comprehend the whole mystery. At length I earnestly entreated that he would make a disclosure to me under the seal of secrecy, and a pledge of keeping silence, promising to reveal it to none without his knowledge or against his will; to all which he gave a round denial. Seeing that I talked to one deaf to importunity, I changed my style; telling him that I trusted to discover the thing by the blessing of God, who would, according to my desire, communicate the invention to me in his own good time, while he might chafe and fret in vain. He laughed at my threats, saying, that the invention surpassed the power even of the Devil himself! These things happened in the beginning of the year 1603, at which period, turning my attention to the investigation, I laboured with all my might. I tried the thing at first with a cord, which I imagined fixed at one end (for I formed the whole in my mind only, and in thought, until I had attained a true knowledge, along with a clear demonstration); then taking the other end in my fingers, I moved it round on imaginary paper, there being upon it two small spheres acting as cursors, to mark out points and proportional lines. These, indeed, I saw, might be formed about a fixed centre; but then, neither was there any motion to or from the centre by means of the flexible thread, nor did the cursors change their positions upon it, which yet was necessary to increase or diminish the motion proportionally. Dismissing, therefore, lines formed by threads, I had recourse to iron rods. These, however, it was difficult to get of the requisite straightness, while to connect them by joints convenient for motion seemed impossible. I consequently put them away, and betook me to rods of wood.

"Placing one of these before my thoughts, I supposed it perforated at certain distances, and hollowed out; with this, so fitted for use, I found out something regarding the motion *round* the centre, but with regard to that *to* and *from* the centre, it proved to little purpose. Next I joined two rods, using their common junction as a moving centre, and assuming a point, which might be in either, as a fixed centre; but neither in this way did I attain my object. At length, making an attempt with four rods, forming a gnomon round a small parallelogram, I began to conceive better hopes of success. However, owing to placing the tracer and drawing pen almost always on the same rod, or in a position deviating beyond the straight line, I did not reach the desired end on the first day that I entered on the inquiry. The object of this invention related to the producing a copy broader than the original, but in what manner to perform correspondingly what referred to the height greatly perplexed me. I had, indeed, learned how to go round the fixed centre, but I was uncertain as to the method either of receding from, or approaching to it. However, I did not despair, but after some consideration, I resumed the design of accomplishing the work at the hours and days of my leisure; telling nothing to any one of my attempt, but sedulously commending it to God and my guardian genius.

"These at last were not wanting to my arduous efforts, but most graciously imparted a knowledge of the whole secret, along with its scientific principles, on that same night which precedes the day sacred to Saints Fabian and Sebastian. In this revelation, the form itself of the instrument was so represented to my mind, its practice, and a demonstration of the whole shewn, as it were, at one glance, as if I had seen all things with my bodily eyes, and was receiving the lesson of a master guiding and teaching me. This impression upon my mind was so strong, that after twenty-seven years it appears as of yesterday. Immediately on arising, giving thanks to God and my tutelary angel, and being full of joy, I exclaimed often within myself, ʿευρηκα, ʿευρηκα ! Then having fitted together four wooden rods by means of needles, I took a picture of St Ignatius, and drew it on paper, the copy being of like form, but on a larger scale; also, around this, and from the same picture, I carefully drew others of a distorted proportion. All these, along with the instrument I had invented, I sent to the before mentioned painter, by the hands of a certain novice, MELCHIOR SCHENCK, at present Father of our Society, who was commissioned, along with fifty other young men of my jurisdiction in the Monastery of St Jerome, to ask and interrogate him, 1. Whether he knew or had ever seen this kind of instrument? 2. Whether he knew the use of it, and could assign, at pleasure, points for the fixed centre, for the tracer, and for the drawing pen? 3. Whether from a given picture he could draw an unlike distorted one? 4. Whether from a distorted picture he could restore the symmetrical one? 5. Whether the picture shewn to him corresponded to its original? 6. Whether he believed that this invention was made by Master SCHEINER?

"The painter on seeing the drawings and instrument, stood, as they told me, mute and astonished for a quarter of an hour. At length, recovering himself, he replied, That he had never seen a similar instrument, nor knew its use: That the drawing pen, the point of the tracer, and fixed centre in his compasses, were connected with certain holes, besides which, he could not assign others. He allowed that he had never seen distorted pictures, being neither able to produce a distorted copy from a symmetrical original, nor from such a copy to reproduce the original. He admitted that the figure of the saint in the middle of the copy shewn to him, corresponded most exactly with the picture. He declared, lastly, his doubt if Mr SCHEINER, without his previous instruction, would ever have made so illustrious a discovery, which in his opinion should have been kept secret, and not promulgated to be trodden under the feet of the vulgar.

"He that had the commission from Mr SCHEINER then returned thanks in his name to the painter, for having, by so industriously concealing his small invention, given occasion for the discovery of a much greater; and by denying a little gem, having paved the way to lay open a concealed treasure: and also, that whereas at one time he might, by the doing of a small good office, have bound his friend to him for ever, so by his refusal he had raised him to the dignity of being a benefactor to the whole world. He added farther, that the inventor would take care that this divine blessing should not fall into contempt, by too promiscuous an imparting of it.

"After this I gave instructions to some friends and pupils upon the subject, and having, after teaching of Humanity for four years at Dillingen, returned to Ingoldstadt to pursue Theology, I there also communicated the art to some others. At this time, WILLIAM Duke of Bavaria, a person well skilled in the art of painting, hearing of the fame of my instrument, invited me to Munich to exhibit it before him; and so wonderfully was he pleased with its niceness, its sureness, and facility of execution, that he desired me to write a short description of the instrument itself, and the manner of drawing a figure.

"In requital of this service he paid me the compliment of making known to me that curious artifice by which water is raised and projected upward. In this method the air is extracted by means of two wheels closely connected, and by the suction thus produced, the water is first raised and afterwards impelled; the *rationale* of the experiment being nature's dread of a vacuum, and the impenetrability of bodies.

"Having now returned from Munich to Ingolstadt, I went through the prescribed course of Theology and published my thesis. Being then requested by my superiors to teach Mathematics, I was sufficiently liberal in communicating a knowledge of my parallelogram ; for both publicly in school did I deliver to my pupils precepts in the art of delineating objects on a plane, along with sound demonstrations, and at home also, privately, I revealed to many the abstruser doctrines of the science. By this means it happened that a general knowledge of the art got abroad. I made, however, a more free communication to those who were going into the Spains, and thence about to traverse the eastern and western worlds for the dissemination of the Catholic faith, that they might have by this means a more ready access to conciliate the benevolence of mankind, and thence instil more easily truths of deeper moment into minds thus prepared to receive them."*

The Pantograph has no defect in its geometrical principles, but, considered as an instrument for practice, it is by no means perfect. It is composed of metallic bars, which turn on five centres ; it is supported on six rollers turning also on centres, and continually changing their direction while they move along the paper ; and when the instrument is used, the force which changes its figure is in some positions applied obliquely, and thus acts at a disadvantage. On all these accounts, its excellence as a working machine is by no means equal to the perfection of its geometrical theory. Land-surveyors sometimes use it from necessity, in reducing or enlarging plans, but the engraver hardly ever employs it ; indeed, in his more delicate work, he can derive from it very little assistance.

In the summer of the year 1821, my attention was directed to copying instruments. I found that the Pantograph was then the only one used, and I formed the resolution of attempting to invent a new instrument, which should be free from its imperfections.

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* The remainder of SCHEINER's first chapter contains nothing now particularly interesting ; it is therefore needless to continue the extract farther.

About the middle of July I shewed my friend Mr Jardine my first essay, a rough model made of wood. His favourable opinion encouraged me to persevere, and I succeeded in contriving an instrument, which gave reason to expect that, when properly made, by its use any plan could be copied, of the same or a smaller size, or even larger than the original, with as much accuracy as a tracing point can be carried along a line. To distinguish the instrument from others designed for a like purpose, I have called it an Eidograph, from the two Greek words, ειδος, *a form*, and γραφω, *I write.*

I next attempted to contrive another instrument that should make a reversed copy, that is, such as would be shewn by the reflection of the original from a mirror. In this also I succeeded. but the construction was more complicated than that of the other. The object of the reversing instrument was to make a trace, at once, on copper, with a view to the etching of any subject on a varnished ground. An engraver whom I consulted did not think this form so necessary and likely to be so useful as the other, and therefore the invention has not been followed out beyond the rough model; which, however, serves to shew that the thing is quite possible. By this second construction, which may be applied also to make a *direct* copy, an instrument might be made, by which a person sitting in one room may write at a considerable distance in another, notwithstanding intervening walls or floors. I mention this, because cases may be imagined in which it might be useful for persons in different parts of an establishment to have the power of holding intercourse by writing, without moving from their places.

I shall not trouble the Society with a detail of the pains I have taken to improve the Eidograph, and of my experience of that law of the human mind, by which it tenaciously adheres to old habits and methods to which it has been accustomed, and refuses to adopt new ones, although more perfect. My object in this paper is to prevent an useful invention from being lost, by having it recorded. Indeed, its utility is now pretty generally known ; for besides occasional applications to the construction of enlarged maps and scientific diagrams, exhibited in the meetings of this Society, and to other purposes of public utility, it has been extensively employed by some engravers in constructing drawings for the plates of literary undertakings, and in particular, the seventh edition of the Encyclopædia Britannica, now in course of publication ; to the anatomical engravings of which, as well as to those of other works of a like kind published in Edinburgh, it has given a degree of perfection much beyond what could have been attained without its aid.

I am sorry to have reason to say, that at least one imitation of my instrument has been pressed on public attention, in opposition to mine, which, for an obvious reason, has been disparaged by comparison with the imitation. I possess, however, in writing the testimony of competent judges that the defects alleged to be in my instrument really did not exist. In fact, the imitation is a modification of the first form that occurred to me, but which was abandoned for a better. My case, however, is no worse than that of many inventors : their labours have frequently been imitated after they have been at great pains and expense in bringing them to some degree of perfection.

The Eidograph has been shewn to every ingenious person who has visited me since 1821, amongst whom have been many foreigners. This has been done in order to spread a knowledge of the invention, and to make it, if possible, useful to society ; and I have had the satisfaction of knowing that an imitation of it has been publicly exhibited amongst works of ingenuity in Russia by a British officer, who, however, had the candour to acknowledge to my friend who saw it, that it really was an imitation of my instrument.

I shall now state some of the advantages which the graphic art may derive from the Eidograph.

1. The instrument is applicable to the copying and reducing of very nice works of design ; for example, the lineaments of a portrait. Indeed, it has actually been applied to the tracing re-

ductions of a subject, of various sizes, on an etching ground on copper, and the process afterwards completed by an acid in the usual way, and impressions printed from the plate.* Of course the figure is reversed in respect of the original. This limits the application, except the copy be made by a reversing instrument; but in many cases this is a matter of no moment. I know of no attempt to do any thing of the same kind by the Pantograph.

2. In estimating the value of the instrument, the economy of time by its use is important. This will vary according to the intricacy of the design to be copied. I have been assured by a skilful engraver who had used the instrument, that in ordinary cases the saving may be three-fourths of the time required by the methods used before its invention; and that, in more intricate subjects, the saving may be nine parts out of ten.

3. Another advantage is, that a youth whose labour costs little, may be taught in a few days to accomplish what would require years of practice, by the eye alone, in the ordinary way.

4. In the last place, the work may be better done. In the old methods, its accuracy could only stand the test of the unassisted eye; but if done by the Eidograph, it will bear an examination by measurement on a scale.

The principle which has directed the construction of the Pantograph and Eidograph is the same: it is taken from geometry, and may be stated thus: " If two points move on a plane, so that the straight line which joins them passes always through a fixed point as a pole; while in every position the distances of the moving points from the fixed point have to each other the same invariable proportion: then if, by any means, one of the points is carried along any line, the other shall trace a line exactly similar; the parts of the two lines passed over by any change of position of the points, having always to each other the invariable proportion of their distances from the fixed point."

* Specimens of these etchings were deposited in the library of the Society some years ago.

Fig. 1.

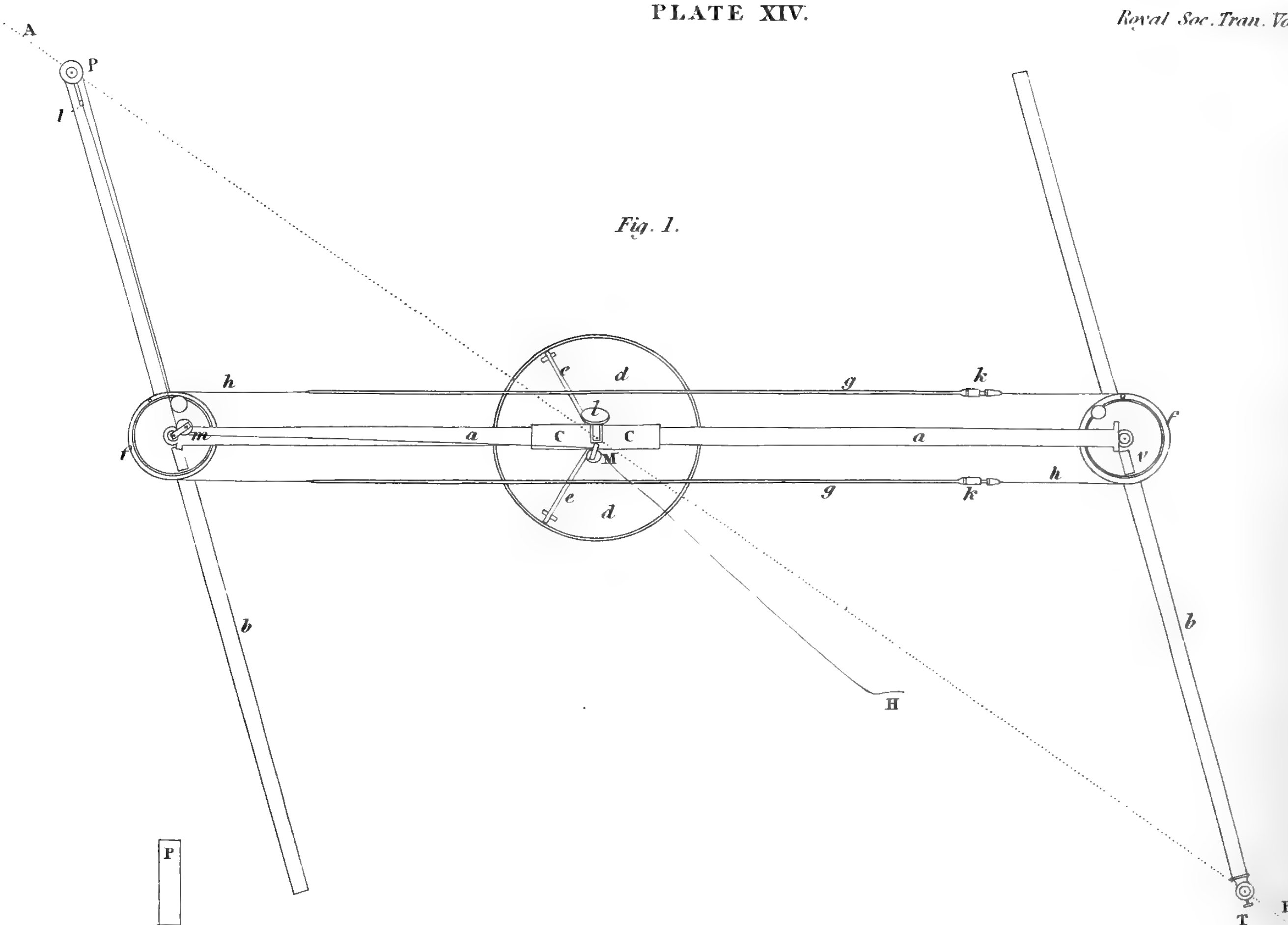


Fig. 3.

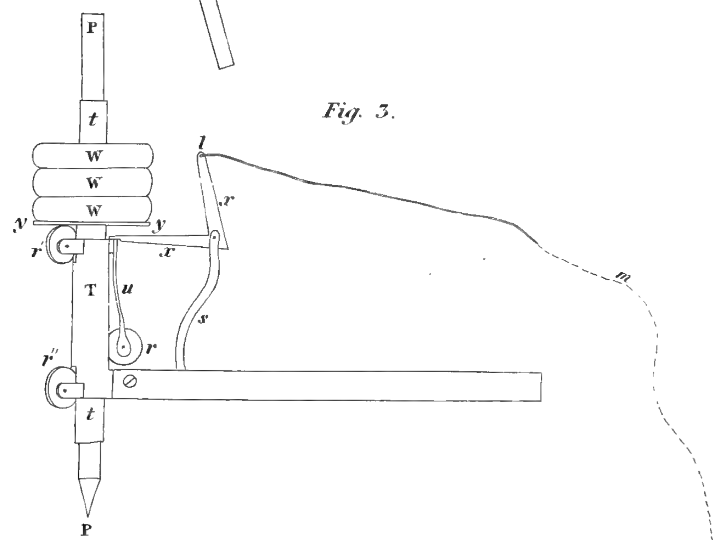


Fig. 2.

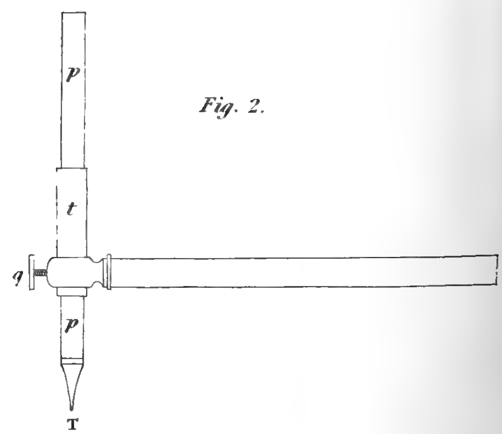


Fig. 4.

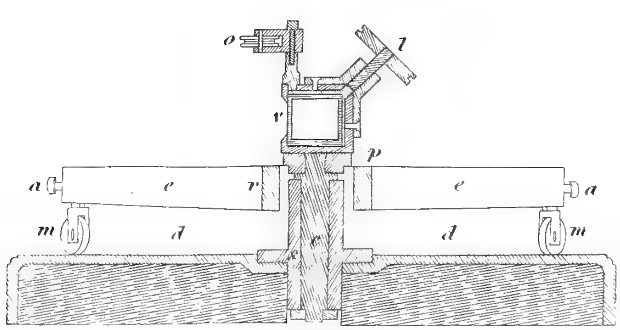
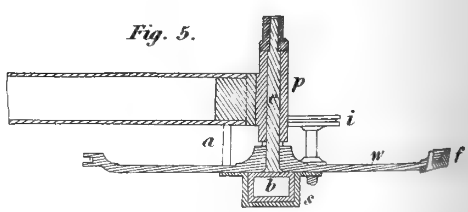


Fig. 5.



Lizars sculp^t

Or, generally, and more briefly, thus:

"If, from a given point, two straight lines be drawn, containing a given angle, and having to each other a given ratio; and if one of them terminate in a line of any kind given in position, the other terminates in a similar line, which is also given in position."

The proposition in this second form gives great latitude in the construction of the instrument. I have, however, only employed the principle as first enunciated.

In applying the principle, the object to be accomplished was, to connect by mechanism two material points, which should move with perfect freedom, subject to the specified conditions of the abstract proposition; or so that one of them, a tracing point, being carried along the lines of any proposed design, the other, a copying point, should trace on paper a true copy of the design in any given proportion. This has been effected by the instrument now to be described.

Its general appearance is shewn in Plate XIV. Fig. 1, which is a projection of it on the plane of the surface on which the design and its copy are placed. The beam *a a* is a square prism of brass; it passes through a socket *c c*, in which it may slide either way. The socket is supported by, and turns on an axis rising out of a base *d d*, which is a brass cylinder. There are two wheels *f*, *f*, having equal diameters, below the beam; these have vertical axes fixed in them, which turn in bored tubes attached to its extremities. The wheels are connected by a metallic *band fg*, *fg*, which passes round them, and is fixed at a point in the circumference of each, so that it cannot slide along their edges.

Two arms P *b*, T *b*, pass through sockets (in which they may slide), under the centres of the wheels, and turn along with them in a plane parallel to that on which the instrument stands. By the equality of the wheels, and their connection by the band, the arms when once placed parallel continue so, although the wheels are turned about their centres. A tracer with a steel point is fixed at T, the extremity of one arm, and a black lead pencil, which serves as a copying point, at P, the extremity of the other arm on the opposite side of the beam. The distances of these points from the centres of the wheels are in every case equal to the distances of the latter from the centre of the beam, whatever proportion these have to each other. By this disposition of the members of the instrument, a straight line joining the tracing and copying points passes through the axis of motion of the beam, and is there divided into two parts, which have to each other the proportion of the distances between the centres of the wheels and the centre of the beam. Thus the conditions required by the mathematical theory are satisfied.

Having described the instrument generally, I shall now notice its parts, particularly, in detail.

THE BEAM *a a*, for the sake of lightness and stiffness, is hollow; it is about thirty inches long, but may be of any required length; its cross section is a square, about nine-sixteenths of an inch in the side. There is a SCALE of 200 equal parts engraved on one of its vertical faces. The length of the scale is the exact distance between the centres of the axes of the wheels, and each division is $\frac{1}{200}$th part of this length. The division at the middle of the beam is 0 (zero), and the scale is numbered both ways; the extreme divisions would be 100, if they were numbered so far, but they go only to about 70, no more being ever required.

THE SOCKET *c c*, in which the beam slides, is four inches long; it has an opening on one side, through which the divisions on the scale appear, and there is an index engraved on it, against which the zero division is set, when the middle of the beam is exactly over the centre of the vertical axis on which the socket turns. Fig. 4. is a vertical cross section, through the centre of the beam socket, of half the actual size. The opening for viewing the scale is at *v*; *c* is a steel conical axis, fixed into a strong plate *p*, screwed into the bottom of the socket, and turning in the bored tube *s*, which is screwed to *d d*, the base of the instrument.

The base is a cylinder, externally of brass, but filled with lead to give it stability; two finger-screws pass through it, so that, if thought necessary, it may be fixed to the table on which the instrument stands when the instrument is used; or the ends of the screws may have each a sharp steel point, which may just enter the wood, and keep the instrument from sliding on the table. The shaded part of the section is a ring, to which the short *arms e e* are fixed; there are three of these, making equal angles round the centre: only two, however, are seen, as in Fig. 1, the third being under the beam, and screwed to a strong plate which connects the socket and beam. From the extremities of these arms vertical *rollers* descend, Fig. 4. (*m*, *m*, are two of them), which turn on their centres by the motion of the instrument, and press on the upper surface of the base *d d*. Their use is to prevent flexion of the main axis *c*, when the middle point of the beam is on one side of its support, as happens in making a reduced or an enlarged copy. There are *adjustments a a*, by which the weight of the moveable part of the instrument is made to bear equally on the three rollers, which thus transfer the weight from the axis to the base. There is a screw in the lower end of the axis which serves to give it greater or less tightness in the conical tube in which it turns. Returning to the socket of the beam, *l* is a finger screw which passes diagonally through one of its angles. It acts on a spring interposed between two sides of the beam (the upper, and that opposite to the scale), and clamps it by pressing it into the opposite angle formed by the other two.

By drawing the beam along in the socket, its parts on each side of the centre may have any assigned proportion to each other; and this is indicated by the *scale* on its side. Thus, when the division 1 on the scale is placed opposite to the index on the socket, one part of the beam has to the other the proportion of 99 to 101; and when 5 is opposite, the proportion is that of 95 to 105, or of 19 to 21, and so on. The rule to find the number on the scale, which shall give a proposed proportion, is this:

"Annex two ciphers to the difference of the numbers which express the proportion, and divide the result by their sum; the quotient is the number on the scale which must be set opposite to the index on the socket." Thus, to give the proportion of 3 to 2, the difference of which, with two ciphers annexed, is 100, divide 100 by 5; the quotient 20 on the scale must be set opposite to the index. If the proportion be that of 5 to 3, we have for the dividend 200, and for the divisor 8; this gives 25 for the division to be set at the index; and so on. In general, the quotient is a fraction, but the nearest whole number is accurate enough in practice. Thus, if a design is to be reduced in the proportion of 8 to 5, the sum being 13, and the difference, with two ciphers annexed, 300; the number on the scale to be set to the index is $\frac{300}{13} = 23$, nearly. In some instruments a vernier scale has been engraved on the socket, by which each half of the beam was actually divided into 1000 equal parts; and then, between the proportion of equality and that of 6 to 1, there might be interposed $\frac{5000}{7} = 714$ different ratios, namely, those of 1001 to 999, of 1002 to 998, &c.

The wheels *ff*. These turn on steel axes which pass through vertical bored tubes fixed to the ends of the beam. The diameter of each is about four inches, and they ought to be exactly equal, because on this the accuracy of the instrument mainly depends.

There is a portion of each wheel, about a third of an inch in breadth from its circumference, which is thicker than the part within it. This is shewn in Fig. 5, which is a section of a wheel through its centre *c* and a part of the beam, of half the actual size. The upper surface of this bounding ring rises higher than the surface it incloses; and the lower surface also rises higher than the lower central surface. Thus, additional space is given for the groove in the circumference in which the band lies, and the band thus raised is kept quite clear of the arms which pass freely below it.

THE BAND is composed partly of very thin and narrow watch-springs *f h*, *f h*, and partly of steel wires *g g*; of course, the former only can be applied to the circumferences, but neither wheel requires to be turned more than about half a revolution; and there are *stop-pins* fixed vertically in them, which come against the beam and prevent them from turning farther. Hence the spring parts of the bands need not be much longer than the arcs on which they are applied, and each is attached by soldering to a piece of brass screwed to the wheel, so that it cannot slide along the convex surface. The stops prevent the soldered points from ever being detached from the wheel. The bottoms of the grooves to which the band is applied ought to be truly cylindric surfaces, of exactly the same diameter, and concentric with their axes. The springs are connected by steel wires, the junctions are made by swivel screws *k*, *k*, by which the band may be tightened, or one wheel turned round a little, while the other is at rest; this last motion is required in the adjustment of the instrument.

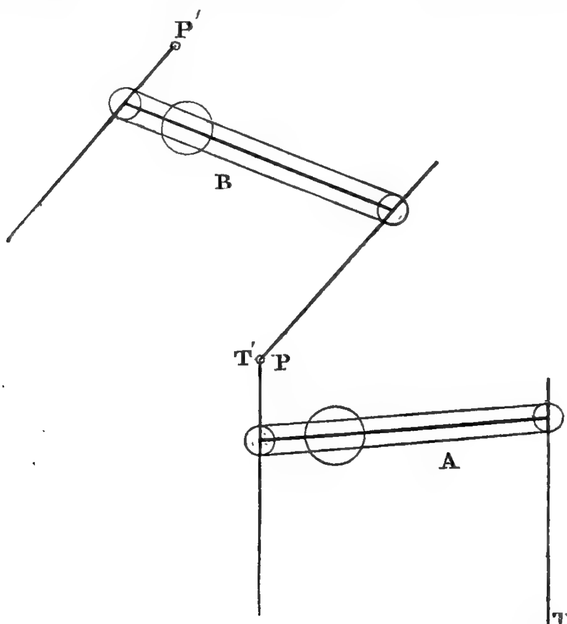
THE ARMS. These are represented by P*b*, T*b*, Fig. 1; they are hollow four-sided prisms of brass; their upper and lower faces are half an inch broad, and their other sides a quarter of an inch; their length is about twenty-eight inches; they fit into SOCKETS which are directly under the centre of the wheels, and go quite across them, so that the direction of the sliding motion of an arm in its socket is in a vertical plane, which should pass always along the same diameter of the wheel. When the instrument is used, each arm is firmly fixed to its wheel by a *clamping screw*. In Fig. 5, *b* is the socket of an arm, and *i* the head of its clamping screw. The sockets are *slit* and *sprung*, to allow the arms to enter and pass freely along them. The *tracer* T, the point of which is of steel, is fixed at the extremity of one arm, and the *copying pencil* P (the point is of black lead) at the extremity of the other arm. Figs. 2. and 3. represent the tracer and copying pencil of half their actual size. The tracer *p p*, Fig. 2, passes through a tube *t*, in which it slides, and there is a finger screw in the side of the tube to fix it at the proper height in working. Fig. 3. shews the copying point, and its apparatus; P P is a black lead pencil, *t t* a tubular case into which the pencil is fitted tightly: these, when the instrument is not used, are detached from the arm and kept apart. The pencil-case has a *flanch y y*, which surrounds it like a collar; the part *t* of the case above the flanch, may be called its *neck*: W, W, W, are round discs of metal (three are shewn in the figure); each has a hole in it, through which the neck of the pencil-case may be passed; one or more of these are placed on the neck when the instrument is used, they serve by their weight to bring down the copying point when raised in working, and also to give the proper degree of shade to the pencil line traced on the paper. There is a bent LEVER *x x*, the arms of which are nearly perpendicular to each other. It is supported at its angle by the fulcrum *s*, which stands on the arm of the instrument. The use of the lever is to raise the copying point from the paper by lifting the pencil-case in the tube T. For this purpose, there is a silk line *l m n* H, Fig. 1. (of which *l m*, Fig. 3, is a part), fixed to it at *l*; this passes over the wheels of two small pulleys, one, *m*, supported on the beam over the centre of the wheel, Fig. 1, and another, *n*, fixed to the socket *c*, over its centre. The end H of the silk line is held in the *operator's* left hand while he works; by drawing it he turns the lever about its centre. By this angular motion, the arm under the flanch raises the pencil in the tube; by slackening the line he allows it to descend, until its point again reaches the paper.

The *pencil-case*, when it is put into the tube T, does not come into absolute contact with its inside, but is held firmly in an upright position, by pressure on the convex surfaces of five *friction rollers* which enter the tube, and project a little beyond its inner surface. Two of these are at the top of the tube and two at its bottom, directly under the former (only one of each pair, viz. r' r'', are seen in the figure); the fifth, *r*, is on the opposite side of the tube, towards the wheel, so that its pressure may be opposed to that resulting from the combined pressure of the other four. The rollers at the top and bottom of the tube T, turn on axes fixed in short projections, which form a part of it, as shewn in the figure. There is a *spring, u,* passing along the side of the tube, which is fixed to it at the upper end, and loose at the lower. On this, the axis of the fifth roller turns when it is pressed upon by the pencil-case, which is thus acted on by three pressures in directions making equal angles round the axis. In this way the pencil is held steady while tracing on the paper, and little force is required to raise or depress it, because the rollers revolve on their centres by the pencil passing along their round surfaces, and the only material friction generated is that on their axes, which is but little in comparison of what it would be, if the pencil-case moved with equal freedom from shake in the tube T.

There is a SCALE of equal parts on the upper surface of each arm, exactly like that on the beam, the divisions on all the three being of the same length, and numbered both ways from 0. Part of each scale is seen through an oblong opening in the wheels. There is an index engraved on a side of each opening, and the divisions are so numbered, that when the distances between the axes of the tracer and copying pencil and the centres of motion of the wheels are each equal to one hundred divisions; then the zero divisions on the scales are opposite to the indices. There are vernier scales on the wheels exactly the same as that on the beam. The artist in constructing the instrument, takes care that the planes which pass along the axes of the tracer and copying points and the axes of motion of their wheels, be truly parallel; when they are nearly so, he completes this adjustment by the swivel screws in the band. If the parallelism be disturbed by any accident, it may be restored in the same way. There is a mass of lead belonging to the instrument, which may be put on the shorter portion of the beam, as a *saddle*, when its centre is on one side of its point of support, as happens in making a considerably reduced copy. This acts as a counterpoise, and restores in some measure the equilibrium of the instrument on its base.

The instrument has these two properties, by which its accuracy may be verified: When the zero divisions on the scales are at their indices, in which case, a copy made would be exactly the size of the original; if two corresponding dots be marked anywhere on a surface, one by the tracer and the other by the copying pencil, and the point of the tracer be carried rondu and put on the mark made by the copying pencil, the latter should fall exactly on the dot marked at first with the tracer. Of course some allowance must be made for the impossibility of perfect workmanship in this as in other instruments. 2. If a straight line be drawn on paper, and there be laid off on it any number of equal parts; then if an enlarged trace of the line, in some known proportion, be made by running the tracer along a ruler, and the divisions of the original be marked on the copy; this last ought to be sensibly straight, the divisions equal, and the original and copy ought to have the prescribed proportion.

For obvious reasons, there is a limit not very remote to the power of making an enlarged copy; that of making a reduced



copy has, however, more scope. For two instruments, A and B, adjusted so as to make each a reduced copy, may be united by passing an axis through the tubes of P the copying point of A, and T′ the tracing point of B. If this axis contained a copying pencil, it would make a reduced copy of the original, and P′, the copying point of B, would at the same time make a copy of this first copy; if the intermediate copy be not wanted, it need not be made. If each instrument were adjusted so as to make a copy, reduced in the proportion of five to one, the copy made at P′ would be one twenty-fifth of the original. It is evident that two or even more reduced copies might be made at the same time from one original, by using several instruments. I have not, however, made more than one at a time by two instruments. It was in this way that some of the etchings on copper referred to in this paper were made; their sizes were $\frac{1}{3}$, $\frac{1}{6}$, $\frac{1}{9}$, and $\frac{1}{12}$ of the original. The place of the copying pencil was supplied by a steel point, which just cut through the varnish of the etching ground; this was all that was required to allow the acid to act on the copper.

Some Observations on Atmospheric Electricity. By John Davy, M. D., F. R. S. Communicated by Professor Forbes.

(*Read 4th January* 1836.)

The few facts we possess relative to the chemical agency of atmospheric electricity, and a certain degree of obscurity connected with these facts, as pointed out by Mr Faraday,* in commenting on the late Mr Barry's results, published in the Philosophical Transactions for 1831,† have induced me to institute some experiments on the subject, with the hope of acquiring additional information.

Reflecting on the manner in which, in certain instances, the great experiment of Franklin, in apparent proof of the identity of common and of atmospheric electricity, had been repeated, especially in our own country ;‡ reflecting also on the experiments of M. Colladon on atmospheric electricity,§ made by means of a lightning-conductor,—it appeared to me not improbable that results might be witnessed of some interest by substituting for an electrical kite, the means employed by Mr Barry, an insulated wire raised a few feet above the summit of any building of moderate height.

As most convenient for the trial, I chose a turret just fifty feet above the street, the highest part of my own house—situated rather high—though not in the loftiest part of Valletta, and overtopped considerably by the summits of several of the public buildings of the city. There I elevated, by means of an iron rod

* Philosophical Transactions, 1833, p. 42.

† On the Chemical Action of Atmospheric Electricity, by Alexander Barry, Esq. F. L. S.

‡ Phil. Trans. abridged, vol. x. p. 303.

§ Annales de Chimie et de Physique, tome xxx. p. 72.

well secured, a glass tube, to which was attached, and by means of which was insulated, a copper wire of moderate thickness, furnished with short projecting lengths of fine wire of pure gold. This upper conductor thus terminating, stood about six feet above the summit of the turret. For communication with the earth, another copper wire was used, the inferior end of which was immersed in water contained in a small cistern, from whence a leaden pipe descended through the building to the ground.

On the 13th of last October I began the experiments, and continued them till the middle of March, when I was obliged to stop, in consequence of having to prepare to leave Malta and return to England.

The first experiment instituted was on the gelatinous transparent compound, formed by mixing together moist starch and a strong solution of the iodide of potassium, which is, I believe, the most delicate of all the tests yet known of electro-chemical action. The two conductors were connected with naked platina wires, which passed through a cork into a glass tube containing the compound in which they were plunged, about a quarter of an inch asunder. The results of this experiment were clear and decisive. I shall limit myself to mentioning them briefly. In fine weather, even when the sky was cloudless, a slight precipitation of iodine was commonly observed, in the course of twenty-four hours, on the platina wire of the inferior conductor. When the wind was strong, the effect was occasionally greater; and it was almost invariably so when clouds were passing, especially low clouds. The direction of the wind did not appear to influence the effect materially; on the whole, *cæteris paribus*, perhaps the effect was more marked when the south-east wind, the damp sirocco, was blowing, than when the cooler and commonly drier north-west wind prevailed. These remarks apply to dry weather, at least to absence of rain. During thunder storms, and during showers of hail even without thunder, and heavy showers of rain, the effect was more strongly marked. In the two first instances, the precipitation of iodine was always copious and rapid, and almost invariably on both the platina wires—sometimes in greatest abundance on that of the inferior conductor, but occasionally on the superior;—even when the platina wires were placed rather more than half an inch asunder, the results were decisive. In these instances it may be inferred that the precipitation on the two wires was not simultaneous but in succession, in consequence of the passing clouds, or portions of air in motion, being in different electrical states,—and, as might be expected, in accordance with this, at no time when the wires were under observation was there any indication contrary to this inference. It may be right to mention, that in these trials with the solution of iodide of potassium and starch, the platina wires were cleaned as often as was required, on account of iodine precipitated and adhering to them; and that the gelatinous solution itself was often changed, which was necessary both owing to its tendency to spontaneous decomposition, and to the iodine precipitated electro-chemically, in part becoming diffused through it.

Other experiments which I have made on the electro-chemical agency of atmospheric electricity, have been far less satisfactory. Using Volta's eudiometer, filled with a strong solution of common salt, no indications could be obtained of the decomposition of water, although the platina wires of the eudiometer-tube connected with the conductors were within one-tenth of an inch of each other, and though the experiment was more than once made when it was thundering.

The only result obtained clearly shewing the decomposition of water, was in an experiment in which fine sewing needles, coated with sealing-wax, excepting at their points, were substituted for the eudiometer. On one occasion, using this apparatus with a strong solution of salt, the point of the needle communicating with the inferior conductor was oxidated, whilst the other point communicating with the superior remained bright, and round the latter very minute bubbles of gas were collected.

This result was obtained during stormy and showery weather, with a sirocco wind.

Experiments were tried with a view to the decomposition of some metallic solutions, such as the nitrate of silver, muriate of platinum, and sulphate of copper, using wires of different metals not capable of acting on them chemically, but no satisfactory result was witnessed, excepting once with a solution of sulphate of copper and platina wires, and during a thunder storm, when a slight, just perceptible, precipitate of copper took place on each of the wires immersed.

Occasionally, for comparison, the electro-chemical experiments were interrupted, and the conductors were connected with a galvanometer, and also with a spiral wire, containing a sewing needle, placed at about right angles to the magnetic meridian. The magnetic effects witnessed were very inconsiderable. On one occasion only, when the atmosphere was in a highly electrical state—lightning frequent, near and vivid—was the galvanometer very distinctly affected. The deviation of the needle was from 8° to 10°. I may mention a particular instance, in which the absence of magnetic effect to me appeared remarkable. It was on the 20th December, when I happened to be present during a shower of hail without thunder. For two minutes that I attentively watched the galvanometer, I could not observe the slightest movement of the needle. I then rapidly connected the conductors with the platina wires immersed in the compound of the iodide of potassium and starch,—now, in less than half a minute, there was a considerable precipitation of iodine on the wire communicating with the inferior conductor.

On the needle in the spiral the effect was even less than on the galvanometer. Sometimes it appeared on trial to have acquired a very feeble magnetic power, but which it soon lost on continuing the experiment. It may be worthy of mention, that in the instance in which the galvanometer was affected, as before described, the needle, in a spiral which was then included in the circle, was not sensibly magnetized.

As the copper-conducting wires, during the period of experimenting, were undergoing oxidation, it was not impossible that the feeble effects which occurred in fine weather, indicated by a very slight precipitation of iodine, might be connected with the chemical change the wires were experiencing. To put this to the proof, gold wires were substituted for the copper throughout. The results were still similar. Whence it may be inferred that the results in question were not connected with the oxidation of the copper wires, and that they depended solely on the foreign influence they were the means of communicating.

It was my wish to have instituted other experiments, in more favourable situations, for witnessing the effects of atmospheric electricity acting with greater energy,—but hitherto I have been prevented. Other inquirers who have the means and leisure, I trust will enter upon the subject and investigate it farther. In the mean time, it may be asked, how do the results of electro-chemical action described above, accord with those referred to obtained by Mr BARRY? Making allowance for difference of intensity, they appear sufficiently well to harmonize. Whilst water was rapidly undergoing decomposition in his experiment, he experienced shocks on touching the conducting thread connected with the electrical kite. In none of my experiments did I ever obtain any shock or spark from the insulated conductor, therefore it is not surprising that the chemical effects I witnessed were so much feebler, and that only slight indications were afforded of the decomposition of water.

Mr FARADAY, comparing Mr BARRY's results with the effects of common electricity and of voltaic electricity, justly points out that they are not identical with either. The same remark will apply not less forcibly to the results of the foregoing experiments.*

* In the abstract of Mr BRANDE's Bakerian Lecture for 1819, though, not that can find in the lecture itself, it is stated, that " the brilliant light occasioned by

He is of opinion that, if confirmed, they will prove the existence of " a form of electrical current, which, both in quantity and intensity, is exactly intermediate between those of the common electrical machine and the voltaic pile."* Another view may be taken. It may be said, that they seem to be in favour of atmospheric electricity not being a simple and single power, but compounded somewhat after the analogy of the solar ray, and of its possessing even a principle peculiar to itself.

This notion I throw out merely conjecturally at present: it might be equally difficult to prove it correct or to refute it. At present it seems most desirable to accumulate facts, waiting for the time when, by means of extensive induction, we may hope to be enabled to arrive at a satisfactory conclusion. But whatever that conclusion ultimately may be, in the mean time it hardly admits of a doubt, that what is called atmospheric electricity exercises a powerful influence both in the aërial regions and on the surface of the earth; and on the latter, perhaps, greater and more constantly than has hitherto been supposed. The results of the experiments I have described may warrant this inference in regard to constancy of feeble effect; and very many facts, well established and universally known, especially the changes not unfrequently witnessed in the cellar and the dairy during thunderstorms, may be adduced in proof of its more energetic action, in accordance with Mr Barry's results, as an agent of chemical change.

Fort Pitt, Chatham, *June* 7. 1835.

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the discharge of the voltaic apparatus presently blackens the chloride of silver."—(*Abstracts of the Philosophical Transactions*, vol. ii. p. 121.) I have exposed this compound moist, just made, to the intensely bright light of a succession of flashes of very vivid lightning, during a thunder-storm by night of unusual violence, even in Malta, and which lasted several hours, without the least change of its colour. If the above statement be correct (and it probably is, as Mr Brande, in 1819, was senior Secretary of the Royal Society, by whom the abstracts of papers are I understand generally made), we have here *apparently* another point of difference between voltaic and atmospheric electricity.

* Phil. Trans. 1833, p. 43.

Researches on Heat. Second Series. By James D. Forbes, Esq. F. R. SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh.

(*Read 2d May* 1836.)

§ 1. *On the Use of the Thermo-Multiplier.* § 2. *On the Polarization of Heat by Tourmaline.* § 3. *On the Laws of the Polarization of Heat by Refraction.* § 4. *On the Laws of the Polarization of Heat by Reflection.* § 5. *On the Circular Polarization of Heat.*

1. The first series of these researches, in the exact form in which they are printed, were laid before the Royal Society on the 19th January 1835. The whole of the experiments there described were made, and the paper written and printed, within a space of time little exceeding two months. This haste, unfavourable to composition, I considered as a less evil than postponing for a period of time, which must have been considerable, the publication of a class of facts which might be said almost to embrace a new science. The professional duties which pressed upon me during the whole continuance of those experiments, then called imperatively on my attention, and during the remainder of the Session my time was devoted to them. The summer I devoted to a foreign excursion, some of the results of which I afterwards digested; and it was not till the commencement of the winter session which has just closed, that I prepared, with a fresh stock of health and spirits, to reinvestigate the whole subject of the Polarization of Heat, and to assign numerical values to the effects, whose *existence* I had before been contented to prove.

Fig. 1.

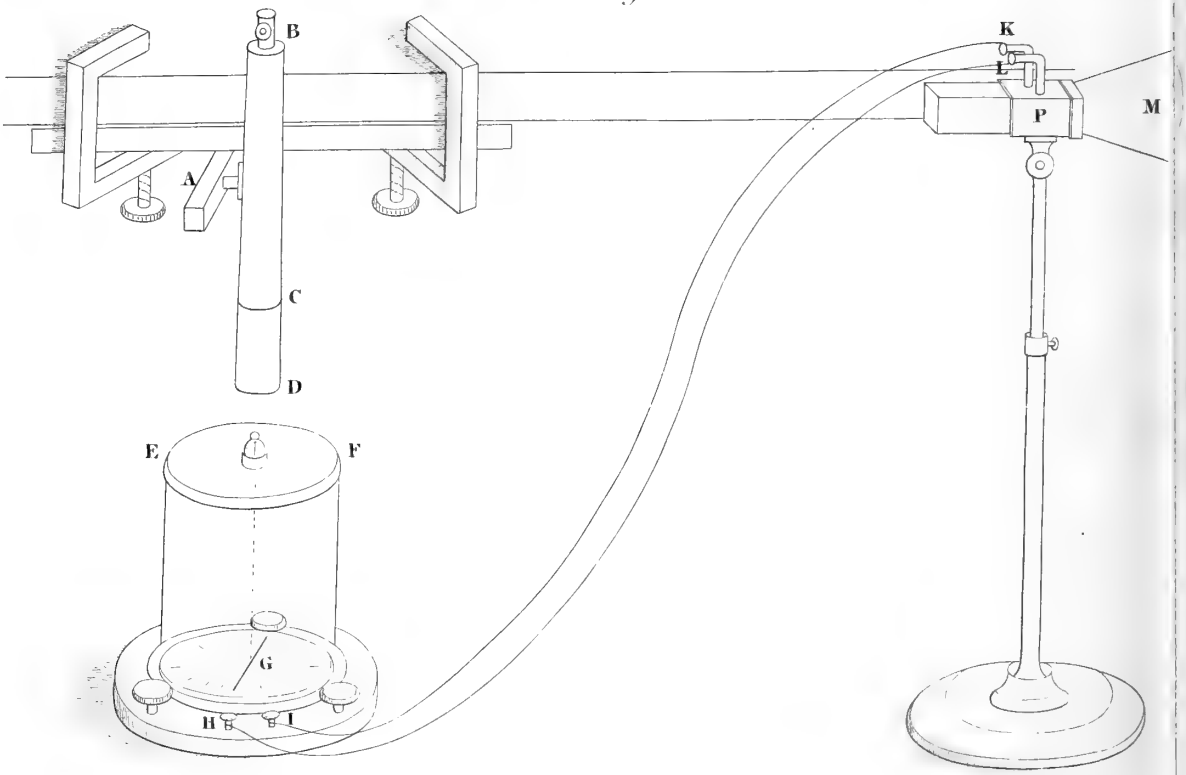


Fig. 7.

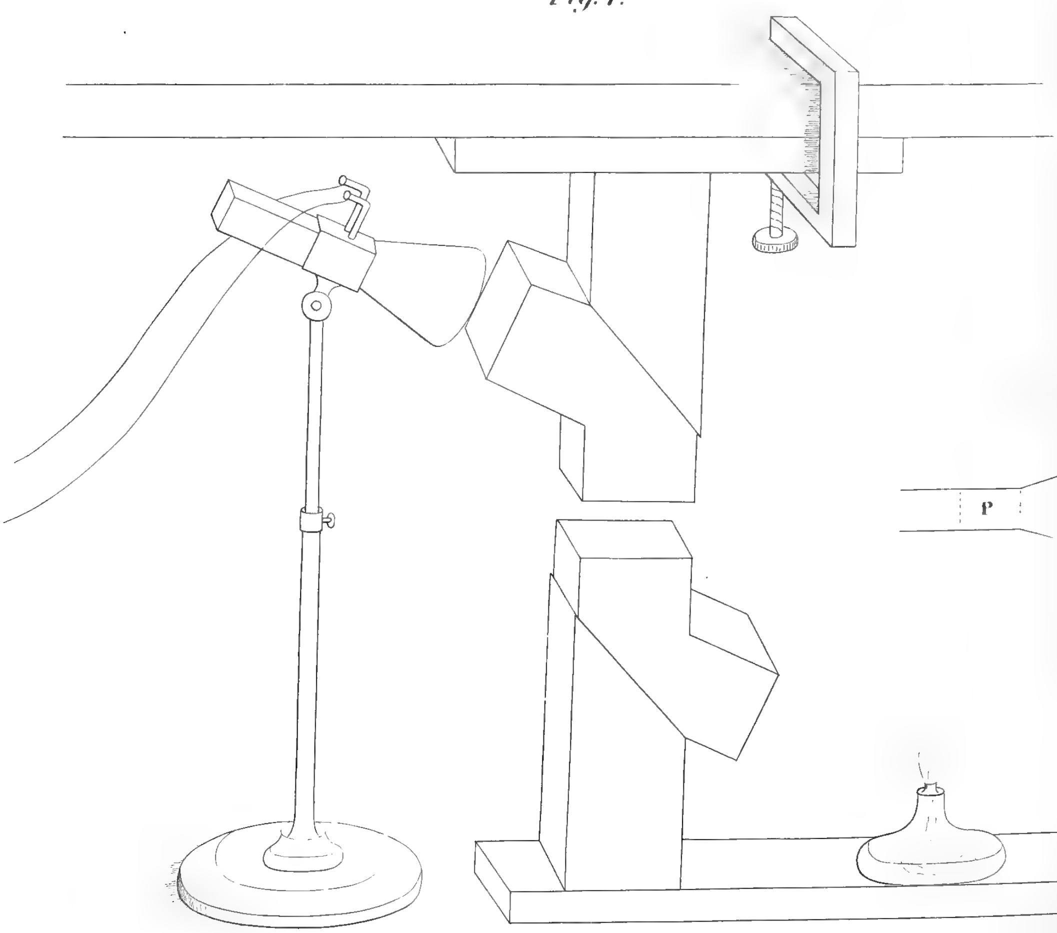
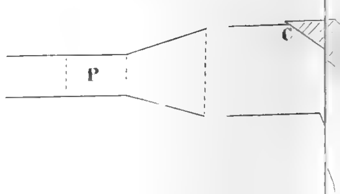


Fig. 8.

Ground Pl



Royal Soc. Trans. Vol. XIII. p. 447.

Fig. 2.

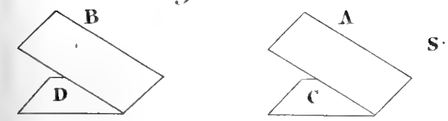


Fig. 3.

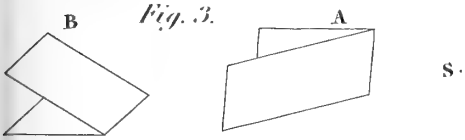


Fig. 4.

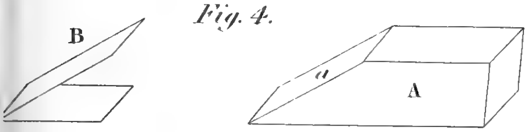


Fig. 5.

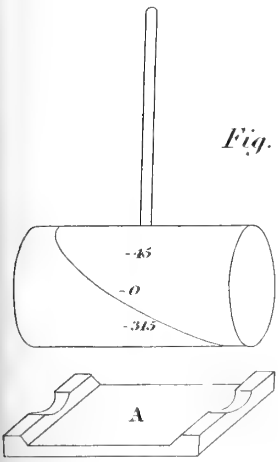


Fig. 6.

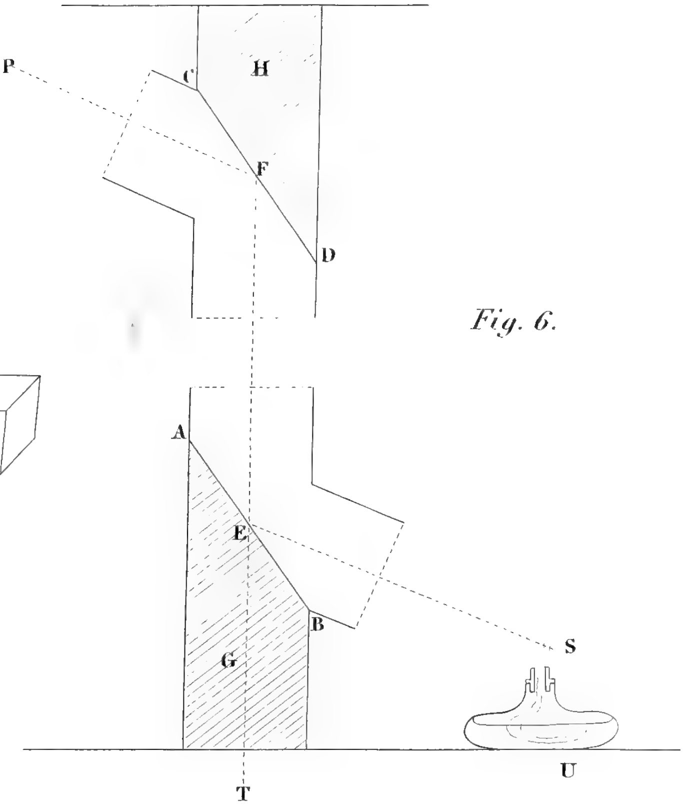
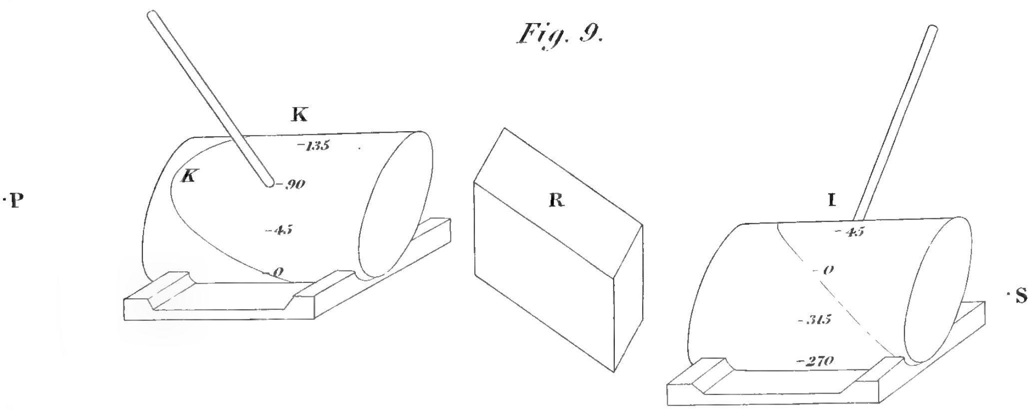


Fig. 9.



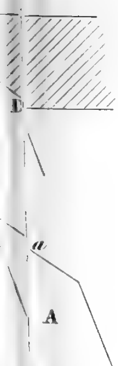
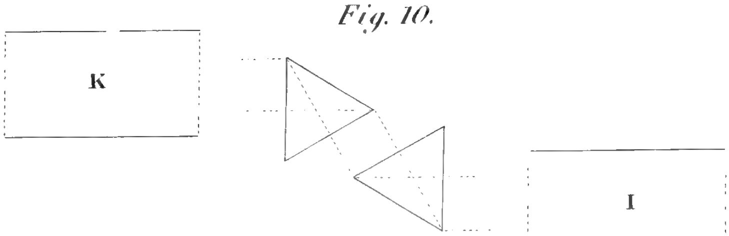


Fig. 10.



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2. The mode of publication which circumstances have led me to adopt, requires that the author should from time to time state how far fresh experiments, have justified his first conclusions, and which, if any, he is prepared to sacrifice. A want of perspicuity in this respect has led to considerable ambiguity in several cases of progressive publication.

3. It affords me, then, no small satisfaction to be able to state that a persevering examination of the laws of polarized heat, under the improved circumstances which experience put at my disposal, has confirmed every general statement which my first paper contained; the *numerical* results there given, never professed to be exact, nor will the more accurate ones now to be substituted, disprove any general conclusions derived from these preliminary investigations.

4. I proceed, then, to detail the results which have been obtained, defining more clearly the experimental laws already developed, and the new discoveries to which I have this winter been led, reserving for another communication the detail of some farther investigations not yet completed.

§ 1. *On the Use of the Thermo-Multiplier.*

5. I have succeeded in rendering the application of the thermo-multiplier considerably easier, and more delicate than formerly. In my last paper, art. 5, I described the application of the telescope to determine accurately the amount of the deviation of the needle of the instrument indicating degrees of temperature. I have made the arrangement more permanent, placing the instrument on one shelf of a solid press, and clamping a little arm A, Plate, Fig. 1, carrying the telescope, and centered at a point in the prolongation of a vertical line, passing through the centre of the card of the galvanometer, to a shelf above, as seen in the figure. The little arm bearing the telescope, therefore, traverses the divided part of the galvanometer card, just as

a micrometer does the limb of an astronomical circle.* The result of this application of optical power is, that equally accurate conclusions may be drawn within small limits of deviation, as if the deviations were greater, and observed in the ordinary way. This is important on several accounts, 1. The instrument is not liable to those derangements which follow from exposure to considerable heat,—derangements difficult to allow for, and which I have not succeeded in obviating. 2. The value of the degrees is more nearly uniform, and less liable to abrupt change; so that (as will presently be seen) within the narrow limits under which I am accustomed to operate, the deviations are almost as the forces. 3. The motions of the needle being much more speedy and certain within small ranges (particularly in its return to zero) much time is saved, and the consecutive observations are more accurately comparative. 4. By the use of the telescope all parallax in reading is avoided, and if a diagonal eye-piece be employed, the posture is much less fatiguing than in any other method of observation.

6. Besides using this optical contrivance, I have increased the delicacy of the instrument, by adding a conical reflector (seen at M, Fig. 1), so as to concentrate nearly parallel rays upon the surface of the pile. This contrivance is not my own. I saw one in an instrument made on the model of M. NOBILI'S, in the possession of M. QUETELET, at Brussels, in 1832, which was the first multiplier I had seen. This reflector increases the sensibility of the pile to heat from a given source seven or eight times, or in a proportion not very different from the area of its aperture

* *References to Fig.* 1.—P, the thermal pile. KI, LH, the wires conveying the electricity from the pile to the galvanometer. G, the galvanometer card, over which the needle traverses. EF, the cover of the galvanometer, which has a plane glass top. BC, a telescope, with a diagonal eye-piece, having an additional object-glass at D, in order to give distinct vision of the galvanometer-needle and the scale (see First Series, art. 5). A, the telescope-bearer, moving round a point concentric with G M, the trumpet-mouthed reflector, applied to the pile (art. 6, below).

to that of the pile.* The length of the conic frustum which I employ is $1\frac{3}{4}$ inches, and its aperture $1\frac{3}{4}$ inches.

7. It is well known that the deviations of galvanometers are not, generally speaking, proportional to the forces producing them, and that for the most part angular spaces at greater distances from zero correspond to increments of force greater than for equal spaces near zero. Thus to cause the needle to advance from 25° to 30° requires a force greater than to make it deviate from 0° to 5°. Also the force indicated by a deviation of 30°, is more than six times the force indicated by a deviation of 5°. M. MELLONI has pointed out an ingenious method of comparing the values of the different parts of the scale. This consists in employing two constant sources of heat to affect the opposite extremities of the pile, and after observing their separate effects, noting their joint effect, which will not generally be equal to the arithmetical difference of the others. Thus let one source of heat force the needle in a *positive* direction to 30° on the scale, and a second source of heat acting separately produce a *negative* deviation of 25°, the effect of both acting at once will not be a *positive* deviation of 5° merely, but probably will indicate some greater number, as 6° or 7°. Thus, a *true scale* of degrees equal in value to those near zero may be constructed. The execution of this investigation is not so simple as it appears, chiefly owing to a tendency of the zero point to shift during experiment, apparently owing to a permanent electro-magnetic condition of the conducting wire. It seems that this electro-magnetic condition

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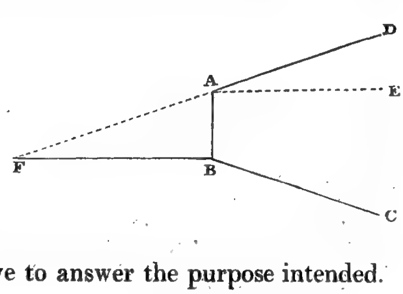
* Suppose that we wish to have a conical reflector ABCD, such that the whole of the parallel rays which fall upon it shall reach *some part* of the surface AB of the pile, which is all that we want, we have this simple construction. Let the length of the trumpet-mouth AE be given. Make FB equal and parallel to it. Join FA, and prolong the line to D, then is DAE the greatest inclination that the sides of the cone can have to answer the purpose intended.



may continue to exist in one direction, even after the wire has been transmitting a powerful current in the opposite one. We have not now time to dwell upon the peculiarities of the instrument. I content myself, therefore, with stating, that I obtained satisfactory results after several patient trials, and obtained the following table of reduction to *true degrees*, or uniform measures of heating effect, by projecting my results, and drawing through them an interpolating curve.

Table for reducing Galvanometer Readings to Degrees of uniform value.

Reading.	Corresponding Intensity.
0.0	0.0
2.0	2.1
4.0	4.2
6.0	6.3
8.0	8.6
10.0	10.8
12.0	13.0
14.0	15.4
16.0	17.8
18.0	20.0
20.0	22.4

The measures in the first column refer to the stationary deviation of the needle of the galvanometer by the influence of any heating cause. The result is remarkably uniform ; the curve from which these numbers are derived, not differing very materially from a straight line.

8. Another mode of estimating the indications of the instrument has been used by M. Melloni, and it is one particularly adapted to our researches. It likewise gives much more uniform results than might have been anticipated. Instead of noting the *final* or stationary deviation due to any heating cause, it is sufficient if we note the arc through which the needle is *first* impelled, and

employ a table of reduction, indicating the relation subsisting between the dynamical effect or first arc of impulsion, and the statical effect or that of final deviation. My experiments have given the following results, and the last column indicates the actual intensities taken from the preceding table, corresponding to the statical deviation in second column.

Dynamical effect, or first arc passed over.	Statical effect, or Permanent Deviation.	Intensity.
1.0	1.2	1.2
2.0	2.3	2.35
4.0	4.5	4.65
6.0	6.7	7.1
8.0	8.9	9.6
10.0	11.1	12.05
12.0	13.2	14.4
14.0	15.3	16.9
16.0	17.4	19.35
18.0	19.45	21.75
20.0	21.5	24.3

9. The mode of observation by the first impulsive arc I have invariably adopted for obtaining numerical results, and chiefly for these reasons: 1. It saves time, and thus renders consecutive observations comparable. 2. It prevents a long exposure of the pile to heat, which alters the zero point and injures its action. 3. It almost annihilates the effect of conduction where substances, capable of retaining heat, are placed between the source of heat and the pile.

10. It is a remarkable circumstance, that when *both* the corrections obtained from these tables are applied, we obtain (as far as 20° at least) a measure of intensity increasing, almost uniformly, with the arc first run through. This is found to depend on the circumstance that the curve, expressive of forces, in terms of the *stationary* deviation, is convex towards the axis, or the forces increase *more* rapidly than the arcs; whilst the curve, expressing the *stationary* in terms of the *momentary* deviations,

is concave to the axis, or the Statical effect increases in a *less* ratio than the Dynamical effect. The convexity of the one curve, almost compensating the concavity of the other, the relation obtained between the first impulsive arc and the calorific force, is nearly linear.

11. This will be best illustrated by comparing the true ratios of the forces obtained from the above table, with the simple ratio of the first deviations; and to put it in greater evidence we shall suppose a deviation of 20°, which is greater than we have ever employed in these experiments.

First Deviations compared.	Ratio.	Ratio of Intensities.
20° and 1°	.050	.049
20 — 2	.100	.097
20 — 4	.200	.191
20 — 8	.400	.395
20 — 12	.600	.593
20 — 16	.800	.796

Since, therefore, even in this case, we should never have an error amounting to a unit in the second decimal place, I have contented myself in this paper with the employment of the simple arithmetical ratio of the first arcs passed over.

12. The construction of the table in Art. 8. was attended with much more trouble than I anticipated. The relation between the Dynamical and Statical effects appears to depend upon circumstances very easily overlooked. Comparisons repeated apparently under the same circumstances often differ very materially, relatively to the magnitude of the quantity sought, namely, the difference of the two effects, which is always small. This seems chiefly to depend upon the condition of the conducting wire which has transmitted the electric current. It is well known, that such wires retain for some time the molecular condition, into which they have been thrown, by transmitting electricity, even after the current has ceased. This may be expected perhaps to be even more strongly the case in electricity of small

tension, such as that of the thermal pile. I have even found (as already stated) that this peculiar electro-magnetic arrangement is not destroyed by producing a brisk action in the opposite direction. Now, in relation to the statical or dynamical effects, we cannot be surprised if the galvanometer coil be brought into action after a short interval of repose, and in the same direction as before, that this molecular arrangement, of whatever kind it be, not being completely destroyed or undone (which it requires many minutes completely to effect), the electricity finds a more ready passage through the wire than if it had not previously been transmitting electricity; consequently, the whole electro-magnetic effect is more nearly instantaneously developed, and the extent of the first impulsive arc bears a greater proportion to the total effect, which, after an unlimited time, the electric current is capable of producing (indicated by the statical deviation), than if the wire had been in a perfectly neutral state when the action commenced. This view is suggested by my experiments, which shewed the relation between the dynamical and statical effects to depend materially upon the position of zero, upon the time allowed to elapse between the experiments, and upon the degree of previous excitement. It also shews why the difference of the two effects diminishes, relatively to the arc, as the arc increases. The time of performing the first sweep is nearly (I apprehend not exactly) the same for large and small arcs. The time for the production of the effect is, therefore, not greater in the one case than in the other. But the intensity of the force being greater in one case, and acting for as long a time, the *coercive force* or inertia of the conducting material is already nearly overcome by the time that the needle has reached the extremity of its oscillation. The effect, therefore, will (as observed) approach more nearly to the permanent effect.

13. The tabular numbers (art. 8) refer to the experiments made with the conical reflector (art. 6) applied to the pile. They are derived from several distinct series of experiments se-

parately tabulated and projected. It is remarkable enough, however, that there is a striking numerical difference between these results and those obtained when the simple aperture of the pile was employed without any reflector, being merely incased in its square brass tube. The excess of *permanent* above *momentary* deviation was greater in the latter case than in the former. In other words, the effect of heat (to the same amount) appeared to be more quickly developed in the pile when it was concentrated by the reflector, than when it fell directly on the pile. There is nothing paradoxical in this result, since the distribution of the heat on the sentient extremity of the pile differs in the two cases.

14. It appears, then, that these effects are developed on the whole in a simple and uniform manner; and though such an investigation as we have undertaken of the instrument, was necessary to give us confidence in the numerical accuracy of the results, all *facts* of importance might be determined without it: and even quantitative laws ascertained by a judicious conduct of experiments. Many persons, even though not unaccustomed to physical reasoning, have strangely inaccurate conceptions of the *limits of possible errors.* Nor is there a more important part of the science of experiment than to perceive where *physical proof* becomes satisfactory, though yet far removed from mathematical certainty. To supply the latter is a humbler and more mechanical task, which may be undertaken at leisure, as in this paper we shall partly attempt to do.

§ 2. *Polarization of Heat by Tourmaline.*

15. On this subject I have little to add. It does not possess the same theoretical importance as in the case of polarization by other methods. Double refraction is better shewn by the phenomena of depolarization by mica, described in my last paper (art. 46 *et seq.*), and the phenomena attendant on the absorption of one of the doubly refracted pencils, are so capricious and ill understood, even in the case of light, that it might hold or not for heat with-

out altering our views as to the probable identity of the cause of both those physical agents. With heat from incandescent platinum the effects are extremely well marked. Thus with tourmalines E and F (see First Series, 22) I obtained for the ratio of the quantities of heat transmitted, with axes of crystals parallel, and axes crossed, 100 : 76 ; or 24 per cent. of the heat was polarized (Jan. 19. 1836). When one of these tourmalines was combined with a mica plate, marked G, polarizing by transmission (see below art. 20), the proportion was 100 : 62, or 32 per cent. polarized of heat from incandescent platinum.

16. With dark heat incomparably greater difficulty was experienced. Excessively little heat could be obtained through the combined mass of tourmaline and the glass to which it is cemented, and of that little it appeared that but a minute portion was polarized, or at least absorbed by the action of the former. At one time I seriously doubted whether any perfectly dark heat came out of tourmaline polarized in one plane only. I have reason to believe that, in my first experiments, there was a source of error, arising from the form of the plates, which was not adverted to formerly. I have, however, satisfied myself that even dark heat is capable of being acted upon by tourmalines in the same manner as light. In my experiments the quantity apparently polarized did not exceed one-seventh or one-eighth of the small quantity transmitted. This was in combination with a polarizing mica plate (marked I).

§ 3. *On the Laws of the Polarization of Heat by Refraction or Transmission.*

17. In my last paper on this subject, I stated the *fact* of the polarization of all the kinds of heat which I tried by transmission through thin bundles of mica placed obliquely. I stated the difficulties which I experienced, and the quantitative errors to which the results were liable. I shewed at the same time that these errors were of a kind calculated to *mask* the effects of po-

larization, but not to produce them. The numerical results which I gave (I. Series, 44), were intended chiefly to shew that, even under all these disadvantages, the effects which I observed were of an extremely important and obvious character, such as no slight or capricious anomaly could possibly have produced. No one who candidly reads the paper, or is aware of the labour of so extensive an investigation in so new a field, can suppose that I intended to give these results as to the quantities of heat, of different kinds, polarized by passing through mica bundles, as *definitive* numerical results. I certainly did suppose that the different kinds of heat were polarizable in different degrees under the same circumstances, a conclusion which I am now prepared to establish.

18. The extent to which the former paper had swelled, likewise prevented me from inserting the description of a multitude of precautionary measures, taken to shew that errors, of whose existence I was perfectly aware had no influence in *producing* the effects which I stated; nor am I now going to enter into details of manipulation, which the experimentalist must learn for himself, and which would be highly tedious to any other reader. I will content myself with recalling two proofs (which I have elsewhere * stated), in justification of my experiments. Could an effect similar to polarization have been produced by the conduction of heat, it must have been in the following way: My earliest experiments were made with bundles of thin mica, A and B, Fig. 2, cemented to soles or bases of wood, C and D, forming, with the mica, an angle of about 34°. Two of these being placed between the source of heat S, and the thermo-electric pile P, the *fact* observed was, that when *either* of these bundles was set on edge, as in Fig. 3, so that the planes of refraction in the two bundles were perpendicular, *less* heat reached the pile than in the first position. If conduction produced this effect, it must have been owing solely to the different quantities

* Philosophical Magazine and Annals, for November 1835 and March 1836.

of heat communicated from A to B, in the positions of Fig. 2. and Fig. 3. To any one who had tried any experiment of the kind, the *enormous* difference in the effect observed (rising to 40 per cent. even in my first experiments), and the instantaneous nature of the change, would have been a sufficient answer. But I farther shewed that the effect was wholly independent of the interval between A and B, and that, therefore, the objection must fall to the ground.

19. To place the matter beyond all cavil, however, I afterwards devised this experiment: I had a tin canister A, made, Fig. 4, having a surface *a*, similar in size, figure, and position to the mica plate A of Fig. 2, whose absorbed heat was supposed to affect the pile differently, by being placed in a rectangular position. The second mica plate B, was interposed as before,—that is, between the pile P and the heated body A, the canister being now filled with boiling water. The effect on the pile, of the surface *a*, was now very great, exceeding, perhaps some hundred times, that of the mica surface with its absorbed heat, which it replaced. Yet, upon turning the canister A into successive rectangular positions, no very decided difference of effect upon the pile could be observed (Dec. 21. 1835.) If any, the effect indicated a *greater* supply of heat in the *crossed* than in the *parallel* positions, or was opposed to that which would have indicated polarization.

20. The polarizing effect takes place only at the *surfaces* of plates,—the absorptive effect depends upon their thickness. Hence to polarize heat effectively, a minute subdivision of mica into thin laminæ is essential. This I formerly effected by a penknife. I have since, however, discovered a method much more effectual. A piece of mica, thrown into a brisk red fire, is split up, by the expansion of the air between its films, into a multitude of pellicles, which reflect light with almost metallic brilliancy, and polarize it intensely by transmission. Such mica plates I have used, one pair being marked G and H; the other I and K.

21. By experiments with both these pairs of plates I have

been led to the conclusion, that some kinds of heat are more polarizable at a given incidence than others. Between heat from an Argand lamp, and that from platinum rendered incandescent by the flame of alcohol, there is little difference; but heat, unaccompanied by light, is much less polarizable than luminous heat, and that apparently in proportion to the lowness of its temperature. I may take the opportunity of giving one or two examples of such experiments.

Mica Plates I and K. *Argand Lamp, with Chimney and Reflector:* 16 Inches from Centre of Pile.

	Dynamical Effect.	Mean.	Ratio.
Parallel	15°.6		
Crossed	4.2	15°.67	27 : 100
Parallel	15.75		
Crossed	4.4	15.85	28 : 100
Parallel	15.95		

Argand without Reflector: 16 Inches.

	Dynamical Effect.	Mean.	Ratio.
Parallel	10.4		
Crossed	3.0	10.42	29 : 100
Parallel	10.45		
Crossed	2.95	10.55	28 : 100
Parallel	10.65		

Incandescent Platinum: 16 Inches.

	Dynamical Effect.	Mean.	Ratio.
Parallel	11.9		
Crossed	3.45	12.42	28 : 100
Parallel	12.95		
Crossed	3.7	12.87	29 : 100
Parallel	12.8		

Dark Heat from Brass, warmed by Alcohol: 16 Inches.

	Dynamical Effect.	Mean.	Ratio.
Parallel	10.55		
Crossed	4.05	10.57	38 : 100
Parallel	10.6		
Crossed	3.9	10.55	37 : 100
Parallel	10.5		

22. These are given as examples of the usual mode of proceeding with such experiments, the zero point being ascertained between each observation, and the dynamical effect reckoned from it. On the whole, I obtained for the proportion of heat polarized, or stopped in the transverse position of the plates, the following numbers:

Plates I and K.

Source of Heat.	Rays out of 100, polarized.
Argand Lamp,	72 to 74
Incandescent Platinum,	72
Brass heated to about 700°,	63
Heat from the same source transmitted through glass,	72
Mercury in a crucible at 410°,	48
Boiling Water,	44

Plates G and H.

Argand Lamp,	82
Incandescent Platinum,	79
Brass heated to about 700°,	68
Heat from the same source transmitted through glass,	73
Boiling Water,	49

It thus appears that the Plates G and H are capable of polarizing no less than 82 per cent. of some kinds of incident heat: these plates I began to use in the commencement of December last.

23. The unequally polarizable nature of different kinds of heat having been controverted, I took several methods of assuring myself that the observed effects were not due to inequalities in the dimensions of the sources of heat employed, or to their variable distances from the pile. A multitude of proofs might be given, but I will content myself with stating one or two of the most decisive. 1. Incandescent platinum and dark hot brass were successively placed at the same distance of twelve inches from the pile; a thin plate of glass being placed between the latter and the pile, and two thick plates of glass between the

former and the pile. The quantities of heat reaching the pile were thus almost equalized, and the result was, that the *heat from a dark source, after transmission through glass, became as polarizable as that from incandescent platinum, although before* NINE *per cent. less of it was stopped.* 2. If heat from boiling water or hot mercury be not really *less* polarizable than that from luminous bodies, it must *appear* to be so in consequence of the surface being larger, or closer to the pile, and therefore seen at the pile under a greater angle. To shew that this effect, if it do exist, is at least insignificant in relation to the effect due to the variable nature of the heat, I placed the brass heated to 700° at 12 inches from the pile, and caused its rays to be *sifted* by a plate of glass. I found that 73 per cent. of this heat was polarized by the mica plates I and K. I then removed the glass plate, and withdrew the heated brass from the pile, until the impression on the pile was nearly the same as before. This was at a distance of 26 inches, instead of 12. The source of heat was therefore seen under a much smaller angle than before. But instead of the polarization being augmented by this circumstance, the change in the *quality* of the heat by the removal of the interposed glass reduced it to 64 per cent.—an effect which must have been owing to that cause, and to that alone. Another experiment similarly conducted with the mica plates G and H gave 73 per cent. of heat sifted by glass polarized at a distance of 12 inches, and only 68 per cent. of heat from brass at 700° in its simple state, at a distance of 27 inches. In all these experiments it is clear that the result is true, independently of any reduction for the degrees of the galvanometer, since in each set the deviation is made the same.

24. The general fact that heat from sources of higher temperature is more polarizable by refraction, agrees with the corresponding case of light. Heat of low temperature is least refrangible, and Sir DAVID BREWSTER has found that light of less refrangibi-

lity is less completely polarized by a bundle of plates placed at a given angle, than the more refrangible rays.

25. Mica is pre-eminently adapted for the purpose of polarizing by its considerable *diathermancy*, and by the extreme thinness of its laminæ. I have, however, succeeded in polarizing heat by transmission through a bundle of rock-salt plates with parallel surfaces.* Two bundles consisting of three plates, or six surfaces each, polarized about one-seventh of the heat which passed in the parallel position, the angle of inclination to the incident heat being about 55°; but when all the six plates were combined into one bundle, and the mica plate I used along with it, not far from a half of the transmitted heat was polarized.†

26. A very convenient mode of mounting the mica plates for polarizing is shewn in Fig. 5. A cylindrical wooden tube is cut across at an obliquity of 34° to the axis. The plate of split mica is interposed and the parts reunited. The plane of polarization or analyzation may thus be made to shift through any angle by turning the tube containing the mica round its axis, and a small support A is provided to preserve it in any position; whilst a graduation may easily be applied to the exterior of the tube, so as to mark the angular revolution. The convenience of this construction will afterwards appear.

§ 4. *On the Laws of Polarization of Heat by Reflection.*

27. The general fact of the polarization of heat by reflection was ascertained by me in December 1834, and I stated the re-

* For a supply of this valuable substance I have been greatly indebted both to Sir PHILIP GREY EGERTON, Baronet, of Oulton Park, Cheshire, and to Dr TRAILL.

† Such plates being equally permeable to every kind of heat, as M. MELLONI's admirable experiment shews, would probably enable us to *polarize cold*, or to shew the negative effects due to a reduction of temperature. This experiment I have not tried.

sult in my former paper, art. 45. Under any circumstances the experiment is a troublesome one, but I have succeeded in arranging it in perhaps as satisfactory a way as it admits of being done. The great difficulty arises from the minuteness of the quantity of heat reflected, and consequently the large quantity absorbed by the plates, which complicates and obscures the effect. This is more particularly true with dark heat, which, at the same time, furnishes the most important case to be examined. The effect of the absorbed heat is to produce a powerful secondary radiation.

28. My first inquiry on resuming the subject was to ascertain the relative order of several different substances as to their power of reflecting heat. This was not proposed to be done with a view to a general inquiry into that important subject, which I reserved to another occasion, but simply to ascertain what reflecting surfaces might be best employed in polarizing by reflection. Several series of experiments gave the following arrangement of substances according to their power of reflecting heat, at an incidence of 45°, beginning with the most perfect reflector.

Polished speculum metal.
Mica, split by the hand into thin plates.
Mica, split by heat (see art. 20).
Thick plate of mica.
Rock-salt, with a thin coating of varnish.
{ Polished rock-salt.
{ Glass.
{ Alum.

The three last substances (so different in their diathermancy) were nearly equal in their reflective power for dark heat (from brass about 7000°). The above order did not, however, appear to be changed for heat from incandescent platinum, except that glass seemed to stand *above* alum and even salt. In a general way, we may consider the measure of metallic reflection to be from two to three times as great as that from mica split by heat, which is also very superior to a single surface of mica. Glass, salt, and alum seemed to reflect but a third or a fourth part, or even less, of that furnished by the laminated mica.

29. I did not, as I have said, stop to prosecute these experiments; they clearly shewed that of the substances which I tried, mica split into thin films afforded the most copious reflection (next to the metals), and this was the very substance which from the first I had employed. They likewise satisfactorily shewed the cause of the failure in former attempts to polarize by reflection, seeing that, for dark heat, glass is almost the worst reflector that could have been used, and as it likewise absorbs almost all the heat, transmitting very little, the effect of secondary radiation is increased. Hence the difficulty experienced by Professor Powell and others in getting any results at all* before the thermo-multiplier was devised, and the failure of the attempts of Signor Nobili of Florence even with its aid.† The last-named eminent philosopher failed also in obtaining traces of polarization by metallic reflection, which was not to be wondered at, as on another occasion we shall be able to make to appear.

30. The form of apparatus which I have more recently employed for experiments on polarization by reflection was suggested to me by the present astronomer royal, Mr Airy, after the publication of my first paper. It is represented in section in Fig. 6, and a perspective view is given in Fig. 7. AB and CD are two reflecting surfaces of mica fixed to blocks G and H; the former of which is attached to a board TU, carrying the lamp or source of heat S, and revolving in a horizontal plane round T as a centre;—the latter (H) is permanently fixed relatively to the pile P, provided with its conical reflector. The surfaces AB, CD are parallel, and make angles of about 56° with the horizon;

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* Edinburgh Journal of Science, N. S., vols. iii. and v.

† Bibliotheque Universelle, Sept 1834.

consequently the heat falling on AB at an angle of 34° with the surface, is reflected in the direction EF, which, by the construction, is a vertical line. From the surface CD, on which, at incidence, it also falls at an angle of 34°, it is reflected to the pile, whose opening inclines downwards at an angle of 22°, so as to receive the rays directly. From this it is clear that the whole apparatus connected with the first plate AB may revolve round the vertical line EF as an axis, until the plane of section be perpendicular to the plane of the paper, and that yet the heat shall be correctly reflected to the pile. In this case it is clear that the planes of reflection becoming perpendicular, a minimum of heat will be reflected if polarization take place.*

31. Such appears to be the case with all the kinds of heat that I have tried. The disturbing influence of conduction is here more difficultly avoided, and serves to *diminish* the apparent effect. The quantities of heat reaching the pile from any non-luminous source are always small. The results, however, are well marked, and seem decidedly to indicate that under the particular circumstances of the observation, dark heat is *more* completely polarized than the more reflexible heat from an Argand lamp, whilst that from incandescent platinum was more polarizable than either. The following results were obtained on the 12th March 1836. The source of heat was in all cases at a distance of six and a half inches from the centre of the first reflecting plate, and the whole length of the dotted line PFES Fig. 6, was about sixteen inches. The reflecting plates were composed of ten or twelve laminæ of mica, split with a pen-knife, and the plane of reflection was perpendicular to the principal section of the mica.

* Square tubes of wood, seen distinctly in the perspective view, serve to enclose the apparatus and facilitate its adjustment. Other means not represented in the figure were also used for preventing direct heat from reaching the pile in any position.

	Rays out of 100 polarized.
Argand-lamp without reflector, . . .	55
Dark heat, from brass about 700°, . . .	61
Incandescent platinum, ...	65

32. This result would be explicable on the supposition that the angle of incidence was that which corresponds to the polarizing angle for heat from incandescent platinum, whilst it was too *small* for the more refrangible heat from an Argand burner, and too *great* for the less refrangible heat wholly unaccompanied by light; nor is this view devoid of plausibility, as will immediately be seen, though the troublesome nature of the experiments, and the smallness of the numerical results, prevented me from prosecuting them farther in this way, than merely to verify generally the preceding results.

33. These experiments were made, as usual, by disclosing the source of heat only for the time that the needle was making its first swing. Thus the effect of absorbed heat was to a great extent avoided. It was worth inquiring, however, whether the acquired heat of the first plate AB, Fig. 6, could by possibility produce an effect similar to that of polarization in rectangular positions. For this purpose I used the tin water-canister already described (art. 19), which was subsituted for the plate AB. Whilst, however, it remained vertically below CD, as in Fig. 6, such a stream of hot air was carried upwards to the pile as to spoil the experiment. But when the plane of reflection at CD was made horizontal, as shewn in Fig. 8, the effect could be accurately observed, and the result was the same as in the case of transmission, namely, that even on this enormously exaggerated scale of error, the quantities of heat reflected to the pile in parallel and rectangular positions of the surface were almost precisely the same. (Dec. 21. 1835.)

34. A mica-plate placed between the two reflecting surfaces in Fig. 6, perpendicularly to the reflected ray, is capable of depolarizing the heat, as in the case of heat polarized by transmis-

sion (17th December 1835). The fact is simply mentioned here, as we do not at present resume the subject of depolarization.

35. I made some experiments, with a view to the determination of the maximum polarizing angle for heat, with a more convenient apparatus than the one above described. Heat was simply reflected from the first surface of a thick mica-plate, and its state of polarization examined by means of a refracting bundle of mica, fixed in a tube (art. 26). Even then it is difficult to arrive at a direct conclusion ; for whilst, in the case of light, the variation in the quantity of polarized light reflected or transmitted at the polarizing angle varies with extreme rapidity, the same does not seem to hold true of heat,* the angle of minimum reflection at a second plate depending sensibly on the increased quantity of heat reflected at great angles of incidence, and therefore making the apparent polarizing angle too small ; a source of error which is not perceptible in the case of light, because of the abruptness of the polarizing action near its maximum. Thus, we must not simply incline the incident rays of heat differently to the reflecting surface until the intensity of the analyzed pencil reaching the pile is a minimum, because that minimum is only *apparent*, being due to two sets of effects varying according to different laws ; one, the effect of polarization increasing up to a certain unknown angle of incidence and then diminishing ; the other, the intensity of a reflected pencil, constantly increasing up to an incidence of 90°. To make the experiment correctly, we must measure accurately, for a great number of angles, the *proportion* of the reflected pencil polarized in the plane of reflection, and take that which is a maximum. Such an investigation I have only very partially performed.

36. I have, however, been enabled to determine approximately the polarizing angle in a way which might appear at first sight much more complicated than the other, but which, at the same

* Probably owing to its still more heterogeneous character.

time, proves the extension of one of the most important laws of light to the case of heat.

37. It is well known that the following law holds for polarized light. *When light, polarized in any plane, is reflected from a refracting surface* AT *the polarizing angle for that surface, it is wholly polarized in the plane of incidence. If it be incident at a* SMALLER *angle than the polarizing angle, the reflected light is polarized in a plane lying on the farther side of the plane of incidence from the plane of primitive polarization. If it be incident at a* GREATER *angle than the polarizing angle, the plane of polarization will be on the same side of the plane of incidence as at first.* Now, this I have fully verified in the case of heat. Having polarized heat by transmission through a mica bundle, mounted as in Fig. 5, in a plane inclined + 45° to the plane of reflection, which it subsequently underwent at the first surface of a thick mica plate, I examined its state of polarization by another similar mica bundle interposed between the reflecting mica and the thermal pile. I found that at great incidences the plane of polarization was on the same side of the plane of reflection as at first, whilst at smaller incidences it was thrown to the opposite side. I varied the incidence until the plane of polarization coincided with the plane of reflection, when I concluded that I had reached the polarizing angle. This was found by the quantity of effect when the plane of analyzation was inclined + 45° and — 45° to the plane of reflection. With *dark heat,* from brass at 700°, I estimated the polarizing angle to be 57° nearly (16th March 1836). By experiment I found that the polarizing angle for the same mica surface and for homogeneous red light was 59°.

§ 5. *On the Circular Polarization of Heat.*

38. In my last paper I shewed (art. 75) that heat may be circularly polarized, like light, by the doubly refracting action of a plate of proper thickness. This circumstance is indicated by

an equal quantity of heat reaching the pile in all positions of the analyzing plate.

39. Last summer it occurred to me that it was probable that rock-salt, refracting heat almost as it does light, would cause it to undergo total reflection at a proper incidence. Supposing this to be the case (and I had afterwards reason to believe that such had been shewn to be the fact by M. MELLONI), I then foresaw the possibility of trying an experiment of the most conclusive character, as to the nature of heat,—its susceptibility of becoming circularly polarized by means of two total internal reflections, as in the admirable experiment of FRESNEL in the case of light.

40. Various circumstances prevented me from trying this experiment until the end of January last, when I procured a rock-salt rhomb, similar to that of glass used by FRESNEL, but having its angles calculated by FRESNEL'S formula, for the refractive index for light of rock-salt. I took the smaller of the two angles which the double solution of the quadratic equation gives, on account of the smaller dimensions required for the rhomb. This angle is nearly 45°. On the 1st of February I performed the experiment with complete success, though with an apparatus less perfect than I afterwards procured. The arrangement represented in Fig. 9. proved exceedingly convenient. R is the rhomb of salt laid on its side, so that the plane of reflection within it is horizontal; S the source of heat, P the pile, I the polarizing mica bundle, K the analyzing mica bundle. The following facts were observed:

41. When the plane of reflection *coincided with*, or was *perpendicular to*, the plane of primitive polarization, the heat (whether wholly dark, or derived from incandescent platinum) came out *unchanged*, that is, on placing the analyzing plate in azimuth 0° and 90° relatively to the polarizing plate, the ratio of the effects was the same as if no reflection had taken place.

42. When the plane of first polarization was inclined + 45°

or — 45° to the plane of reflection, and the analyzing plate was placed in the parallel and rectangular positions to the polarizing plate, the ratio of the effects was totally changed, and was, in general, reduced nearly to unity. This took place whether the rhomb or the polarizing plate was moveable. The following experiments will illustrate these statements:

Polarizing and Analyzing Plates I *and* M; *Rhomb* B; *Heat from brass about* 700°. Plane of total reflection perpendicular to plane of primitive polarization.

February 2. 1836.

	Dynamical Effect.	Mean.	Ratio.*
Plates parallel . .	12°.4		
crossed . .	6.0	11°.92	50 : 100
parallel . .	11.45		
parallel . .	12.25		
crossed . .	6.05	11.85	51 : 100
parallel . .	11.45		

Other things as before. Plane of reflection inclined 45° to plane of primitive polarization.

February 2. 1836.

Plates parallel . .	9°.25	85 : 100
crossed . .	7.9	
parallel . .	9.85	85 : 100
crossed . .	8.4	

43. When the plates I and K were used, the ratio was raised.

* Only 50 per cent. of the heat was polarized instead of 63, as in the Table (art 22). The reason is, that the mica bundle M, used on this occasion, polarized less completely than that marked K. I have invariably found the per-centage of heat polarized after total reflection within rock-salt, *in*, or *perpendicular to*, the plane of primitive polarization, to agree most closely with the results obtained when no reflection had taken place.

by inclining the plane of reflection 45°, from 37 : 100 to 60 : 100; and when heat from incandescent platinum was employed, from 28 : 100 to 64 : 100.

44. It occurred to me, that, somewhat above the *superior* angle of total reflection indicated for light, the effect of apparent depolarization would be more perfect, and a ready way of doing this presented itself by the use of two prisms of rock-salt, having angles of 60°, with which I provided myself. The *superior* angle of total reflection for rock-salt (whose index of refraction is 1.56) is 57°28′ nearly, for light, and since it increases rapidly as the refrangibility diminishes, it was reasonable to expect it to be still higher, or not far from 60° for *dark heat* (of low refrangibility). The two prisms, arranged as in Fig. 10. (which is a ground-plan), fulfilled the required conditions, the dotted lines indicating the path of the rays of heat through the prisms, and the result corresponded to my expectation. When the plates I and K were used to polarize and analyze, and the planes of total reflection and polarization were parallel, the ratio in the rectangular positions of the analyzing plate was 40 : 100; whilst, when the plane of first polarization was inclined 45°, the ratio was raised as high (in one series of experiments) as 94.5 : 100. With the same apparatus, and with heat from incandescent platinum, the ratio was raised from 29 : 100 to 84 : 100. Thus the astonishing properties of rock-salt enable us most completely to extend the analogies of light even in their most complicated cases to the phenomena of heat.

45. We are naturally led from the consideration of circular polarization produced by two known methods in the case of light, viz. by transmission through a thin doubly refracting plate, and by total reflection in a refracting medium, to consider the third mode in which it has been effected, that is, by metallic reflection. In this case, also, the analogy holds as to the general fact which I have succeeded in completely establishing under several circumstances. Whilst the copious reflection of heat which takes

place at metallic surfaces, renders it easier to obtain distinct results than in some other cases, the intricacy of the subject, and some deviations from the laws of light, as established in Sir DAVID BREWSTER's remarkable paper on this subject,* demand a more prolonged investigation than I have yet been able to give to it. In the hope of being able to resume it in another paper, I content myself at present with a reference to the facts respecting Metallic Reflection, communicated to the Society on the 21st of March last, a memorandum of which will appear in their printed Proceedings.

EDINBURGH, *2d May* 1836.

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* Philosophical Transactions, 1830.

On Single and Correct Vision, by means of Double and Inverted Images on the Retinæ. By W. P. Alison, M.D. F.R.S.E. Professor of the Institutes of Medicine in the University of Edinburgh.

(*Read* 11*th April* 1836.)

In entering on a question which may be said to occupy a portion of the debateable land between Physiology and Metaphysics, it seems, in the first place, necessary to state with precision the nature of the difficulty, which has long been felt on this subject, and endeavour to determine the degree to which it is reasonable to expect, that this difficulty may be removed; and on these points there is such a discrepancy of opinion, even among the latest and most esteemed authors, as obviously to make farther inquiry desirable.

No one can be more thoroughly convinced than I am, of the utter futility and absurdity of all attempts "to shoot the gulf which separates the sensible world from the sentient soul." In all our inquiries in the Physiology of the Nervous System, as connected with mental acts, we must keep in mind, that the end of these inquiries can only be, to determine the physical conditions under which the different mental phenomena take place; and those under which, when they have taken place, they affect the different organs of the body. The question, how it comes about, that when those conditions are fulfilled, these results follow, must be held, in every case, to be beyond our powers.

But as it is clearly in our power to ascertain the *general* conditions under which any mental phenomena are connected with a living body, so it may also be in our power to ascertain the *special* conditions under which any particular idea, or other mental act, takes place; and particularly, to determine the exact sensations with which any particular notion formed in the mind is naturally connected, or by which it is suggested; and, in a case where the very same notions seem to be suggested by different sensations, we may expect to arrive at the knowledge of the manner, in which the intimations acquired by the different senses are made to correspond.

When the attention is fixed for the first time on the fact, that, although the notions which we acquire of the number and position of external objects, during the ordinary exercise of the sense of sight, are correct, yet the images on the retinæ, which are the essential conditions of our seeing any one of these objects, are double, inverted, and reverted,--the natural inference certainly is, that some explanation should be sought, and may be had, of so singular an anomaly. But a little reflection will shew, that our notions of the number and position of objects are not connected *merely* with the exercise of the sense of Sight, but very much with that of Touch. And when we find it stated by many philosophers, that we have *no notions* on those points naturally and originally connected with Sight, and that it is only by *experience*, and by association with the notions acquired by Touch, that we learn to form judgments of the number and position of objects by the eye—we must admit, that it is only by appeal to facts, not by any reasoning *à priori*, that the truth or falsehood of this doctrine can be determined. We must therefore satisfy ourselves, that number and position are original, not acquired, perceptions of the eye, before we are entitled to ask for an explanation of single and correct vision, by double and inverted images.

The late Dr BROWN was so confident of the perception of the number and position of visible objects being acquired only by association or custom, that he thought himself justified in dismissing the subject with the following observations: "In the single vision of the erect object, from a double image of the object inverted, there is nothing at all mysterious to any one who has learnt to consider, how much of the visual perception is referable to *association*. If the light reflected from a single object *touched by us,* had produced, not two merely, but two thousand separate images in our eyes, erect or inverted, or in any intermediate degree of inclination, the visual feeling thus excited would still have *accompanied the touch of a single object;* and if only it had accompanied it uniformly, the *single* object would have been suggested by it, precisely in the same manner as it is now suggested by the particular visual feeling that attends the double inverted image."*

But, with all deference to this illustrious metaphysician, I will take the liberty of stating, that this view of the subject had been previously fully considered, and, as far as I can judge, completely set aside by Dr Reid, at least in reference to single vision by two images on the retinæ; and that, not by any abstract reasoning, but by appeal to facts.

If it were only by experience, and association with the perceptions of touch, that we learned that any object placed before the eyes, and seen by two images, is nevertheless single, it seems *primâ facie* reasonable to conclude, that we should never see an object double, which we know by touch to be single; whereas we all know, that if, by pressure on the ball of one eye, or by any other means, we direct the axes of the two eyes to different points in an object, we immediately see it double, and cannot, by any means, avoid seeing it double, so long as that condition of the eyes continues, notwithstanding the full conviction, derived from touch, of its being single.

The only answer that I can conceive to this is, that the association, by which we are informed of an object of sight being single, is formed with the natural and healthy state of the vision of that object, when the axes of the two eyes are directed to the same point in the object, and its images are formed on *corresponding points* of the retinæ of the two eyes; and that when its images are formed on dissimilar points of the retinæ, the diffe-

* Lect. 29.

rence of the sensation excited, from that which is usually felt, is at once perceived, and the association, by which its unity had been made known to us, is broken.

But this answer does not apply to the cases stated on this point by Dr Reid, of persons who had *squinted from infancy*, and in whom, therefore, the association of single objects must have been formed with images on *dissimilar* points of the retinæ, and must have been broken when the images of an object were formed on corresponding points. These persons, according to the theory in question, should have seen objects single when they squinted in their usual way, and not when the axes were brought to bear on the same point, in a way quite unusual to them. But the reverse was the fact. They saw double when they squinted (excepting in particular positions of the eyes, when, as Dr Reid supposed, one of the images was formed on the well known insensible spot on the retina); and "when they learned to direct both eyes to an object, they saw it single."

I can myself confirm the observations of Dr Reid from pretty numerous trials on persons who habitually squinted; in which it always appeared, if the vision of both eyes was tolerably good, that, when the attention was fairly fixed on the sensations of both eyes, single objects held directly before the face were seen double, and again, that different and distant objects held carefully in the direction of the axes of the two eyes, seemed to coincide.

Again, says Dr Reid, "from the time we are capable of observing the phenomena of single and double vision, *custom* makes no change in them. I have amused myself," he adds, "with such observations for more than thirty years; and in every case, wherein I saw the object double at first, I see it so to this day, notwithstanding the constant experience of its being single. In other cases, where I know there are two objects, there appears only one, after thousands of experiments.

"Effects produced by habit must vary, according as the acts by which the habit is acquired are more or less frequent; but the phenomena of single and double vision are so invariable and uniform in all men, and so exactly regulated by mathematical rules, that I think we have good reason to conclude, that they are not the effect of custom, but of fixed and immutable laws of Nature."*

In fact, it is a very imperfect and inaccurate expression of the phenomenon in question, to speak of it merely as single vision resulting from two images on the retinæ. The precise expression of the fact, as fully illustrated by Dr Reid, is, that when images are formed *on corresponding points of the retinæ*, they appear as one; and *in all other circumstances they appear as two*, as they really are; and this general fact holds good, equally in the case of those, in whom the experience of the sense of Touch habitually *opposes* the inference drawn from Sight, as in that of those in whom it habitually *confirms*, and has been thought to suggest that inference.

The difficulty which is presented by the inversion of the images on the retina is, I think, most correctly expressed thus: The sensations, both of Sight and of Touch, obviously differ from one another in position; and by doing so, both convey to us intimations of the situation of external objects. But the judgments which we form of the relative position of objects, or of the parts of an object, from the relative position of the impressions which they make on the sensitive surface of the *retina*, are just the reverse of those which we form of the relative position of objects or their parts, from impressions made on the sensitive surface of the *skin*. Thus, if two impressions are made on the upper and lower portions of the eye-ball, and felt through the fifth nerve, the inference immediately drawn is, that the upper impression is from a higher object, and the lower from a lower; but if two impressions are made on the upper and lower part of the retina, and felt through the optic nerve, the inference

* Inquiry into the Human Mind, &c. Sect. 17, *ad fin.*

is, that the impression on the upper part is from the lower object, and that on the lower part from the higher. Why this difference should exist, is the point in question.

It is perhaps difficult to find decisive evidence in the human body, that the intimation of the position of the erect object, given by the inverted image, is *originally* correct, and in harmony with the intimation given through the sense of touch, before experience and association can have time to operate; but it is unnecessary to argue this point, because it is allowed even by Dr Brown, that, in the case of many of the lower animals, there is an original perception of the true position of objects, acquired by the sense of sight; so that those who have so humble an idea of their own powers of visual perception as to believe, that it is only by experience and association that *they* learn to judge, by the eye, whether an object is erect or inverted, may acquiesce in what is here to be said, as applicable to the lower animals.

But it is farther necessary to premise here, that, while some philosophers have thought it unnecessary to seek for any explanation of correct vision by double and inverted images, because they believe that the eye gives *no original intimations* whatever as to the number or position of objects,—others are equally convinced of the futility of the inquiry, because they maintain, that *the eye does give intimations* which necessarily imply, that the objects, of which inverted images are formed on corresponding points of the two retinæ, are erect and single.

This is done by reference to the *Law of Visible Direction*, fully illustrated by Dr Reid, and many others, according to which, every object appears to be in the direction of a straight line drawn perpendicularly to the retina at the point where its image is formed. This law has not been similarly expressed by all who have referred to it; but the terms here used are those employed by Sir D. Brewster and Mr Mayo. In conformity with this law, when an image is formed on a concave surface, the lower part of that image must be referred to the upper part of the external object corresponding to it, and *vice versâ;* and again, images formed on corresponding parts of the two retinæ, and referred, according to this law, to external space, must appear to come from the same points, *i. e.* to represent the same object.

But here the question immediately presents itself, How is this law of Visible Direction originally formed in, or impressed upon, the mind? If it be thought to be acquired by experience and association, the observations already made apply to, and I think set aside that supposition. If it be thought to be independent of experience, it implies, in the first place, that the mind has an original perception of distance by the eye, *i. e.* that it is originally aware of impressions on the retina being produced by causes at a distance from the body, although it draws no such inference from impressions on the skin; whereas not only Berkeley and his followers, but Reid, and most other authors on this subject, have believed that it is only by experience that we learn that visible objects do not touch the eye; and this has been generally thought to be supported by observations on persons to whom the sense of sight has been given suddenly, and at a mature age, by couching.

But farther, this doctrine implies that the mind naturally draws an inference, not only as to the position of any impression that may be made on the retina, but as to the *direction in which the ray of light came*, that made that impression; and as that direction is not a direct object of sense, and as I apprehend that no reason can be given, why a ray should be supposed to have come in the direction of a perpendicular to the surface of the retina, rather than of any other line falling on that surface,—this theory really ascribes our perception of the true number and position of objects by the eye merely to the principle of *Intuition*, *i. e.* it merely *states the fact*, that the images formed on the retina are referred to places in the external world according to this law; and if we regard the theory as a sufficient explanation, we must regard this as an *ultimate fact* in our mental constitution.

Now, no disciple of Reid or Stewart can have any hesitation about admitting, that this principle of Intuition is part of the cause of all the information we derive from this or any other sense. I hold it to be equally certain, that we learn some things intuitively, we know not how, as that we do some things instinctively, we know not why. And, admitting the principle of Intuition, it is impossible for us to say *à priori*, without special investigation of any alleged case, how far it may extend, or how much of the information which we habitually acquire by the senses, is explicable in no other way. But it is obvious, that this must be our last resource in attempting to account for these phenomena; and it is unphilosophical to assume, that the limit to our curiosity is to be found on the very threshold of our inquiry.

Dr Reid has stated this with his usual candour and precision. After observing that *he could trace* the phenomenon of correct vision by inverted images no farther than to the law of visible direction above stated, he adds the following words, which may be taken as the groundwork of any farther speculations on this subject. "We acknowledge that the retina is not the last and most immediate instrument of the mind in vision. If ever we come to know the structure and use of the choroid membrane, *the optic nerves and brain*, and what impressions *are made on them by means of the pictures on the retinæ*, some more links of the chain may be brought within our view, and a more general law of vision be discovered."—*Inquiry, &c.* ch. vi. § 12.

I apprehend, then, that two facts are established,—are not to be explained by experience or association,—and, not being *necessarily* ultimate facts, afford a fair subject of physiological inquiry. 1. That *images formed on corresponding points of the retinæ of the human eyes, and on those only, naturally affect our minds in the same manner as a single image formed on the retina of one eye*; and, 2. That *impressions made on different points of the retina of the eye are naturally followed by inferences, as to the relative position of the objects producing these impressions, exactly opposite to* *those which follow impressions made on different points of the surface of the body.*

I. Of the first of these facts, *i. e.* the single vision by means of double images, it is well known that an explanation was proposed by Newton, fully considered by Reid, and since supported by Wollaston (often called the Theory of Wollaston, but quite incorrectly), proceeding on the supposition of a *semi-decussation* of the human optic nerves at their commissure; whereby the fibres, from the right half of the retina of each eye, go to the right *optic lobe** in the brain, and *vice versa;* the consequence of which may probably be, that the fibres which connect themselves with, or terminate at *corresponding points* of the retinæ, may originate at the *same points* in the optic lobes. If this be so, impressions made on *corresponding points* of the retinæ are in fact impressions made on the *same points* in the optic lobes; and, as they are effectual in exciting sensations in the mind, only inasmuch as they are made on the optic lobes, *they must necessarily co-operate in exciting the same sensations.*

Dr Reid fairly admits that, if the anatomical part of this theory were ascertained to be correct, "it would lead us a step forward in discovering the cause of the correspondence and sympathy of the two retinæ;"—he ought rather to have said—the cause of single vision by impressions made on corresponding points of the two retinæ. I think we may add, that it is the *only step* which we can conceive to be taken, or can desire to take, in that inquiry. And I will farther venture to maintain, that a precisely similar step may be taken, even with more confidence, as to the correct vision by means of inverted images.

We must admit, that the anatomical evidence of the theory

* The term optic lobe is here used as a short expression for that portion (perhaps not yet absolutely determined) of the contents of the cranium, from which the optic nerves originate, and on which their sensibility depends.

of the mammalia in the adult state; but in the fœtal state, in man, in the horse, ox, sheep, rabbit, and guinea-pig, he distinctly saw the internal fibres of the tractus opticus separate themselves from the external, and traced them across from the right tractus to the left nerve, and *vice versa.* In crossing, they form a plexus, and, as the animal grows, this plexus is soon loaded with fresh deposits of medullary matter, so that the course of the fibres, distinct in the fœtal state, is hardly to be traced in the adult.

The idea that the partial decussation of the optic nerves is designed to give single vision by two eyes directed to the same object, is farther strongly confirmed by the fact, that in those animals in which this structure is generally, if not universally, found, *i. e.* in mammalia and birds, the power of directing the axis of both eyes to the same object, generally, if not universally, exists, although in many quadrupeds and most birds the object which can be thus contemplated must be at a considerable distance; whereas in those animals in which there is (generally, if not universally,) no intermixture of the filaments of the optic nerves, *i. e.* in reptiles and fishes, the eyes are so situated, as remarked by Cuvier, that any object *must* in general, andprobably in most instances *can,* only be contemplated by one eye at a time.*

But notwithstanding this strong evidence of the existence of the partial decussation in the higher animals, and of its connexion with the single vision by two eyes, the foundations of the theory must be allowed to be deficient. The partial decussation of the nervous fibres will explain the single vision by two eyes only on the supposition, that the fibres which terminate in *corresponding*

* Anat. Comp. Leç. xii. Art. 2. It is true that the axes of the two eyes, at least in some fishes and reptiles, may be brought to bear on the same objects, if very distant, but as the vision of very distant objects is seldom requisite for these animals, it is probable that they are not habitually guided by simultaneous impressions on the two eyes.

points on the retinæ (at least in those corresponding points on the retinæ which can be brought to bear on the same objects), originate in the *same* points on the optic lobes. Now, in order that this may be effected, there must be a very peculiar arrangement of the nervous matter, both on the retinæ in front, and on the optic lobes behind. The entrance of the optic nerve in the human eye being considerably on the inner side of the optic axis, the separation of the right and left portions of the retina cannot take place there; the fibres on the inner side of the nerve, coming from the opposite lobe, must extend outwards as far as the central foramen, in order that all the inner half of the retina may be connected with the opposite optic lobe; and, at that central point of the retina, these fibres, or the membrane continuous with them, must be overlapped by those which come from the lobe on their own side, and form the outer part of the optic nerve. Again, the fibres passing backwards from the outer portion of the right, and inner portion of the left retina, to form the right tractus, must be there so combined, as that those which come from corresponding points in the retinæ may be implanted at the same points in the lobes. And I am not aware that these peculiarities in the course of the fibres, which the theory seems obviously to require, have hitherto been detected by any anatomist.

It is to be observed here, however, and obviates one objection that has been stated to this theory, that in the case of most quadrupeds and birds, whose eyes are generally directed widely asunder, it can only be a small segment of the outer part of the retina of either eye, which will ever be brought to bear on objects situated within the sphere of vision of the other eye; and it is only this small portion which, if the principle of single vision has been correctly stated, requires to be associated at its root with the corresponding portion of the retina of the other eye. Accordingly, in such animals, it is obviously more than a semi-decussation which takes place at the commissure of the optic nerves;

and indeed in all animals the term *partial* decussation is the more proper. In such animals, we should expect, from anatomical observation, what we find, from the effects of injury and disease, to be the case, that the vision of each eye is more dependent on the opposite optic lobe than on that on its own side. Partial decussation, according to the theory, should exist, and, as far as observations have been made, I believe does exist, in all animals which habitually bring their optic axes to bear on the same point, however distant; but semi-decussation should exist only in those, in which the natural direction of the axes is parallel.

On the whole, it may be said, that there is very strong presumption, though not absolute certainty, in favour of the doctrine of the dependence of single vision by two eyes on the partial decussation of the optic nerves.

II. The explanation, which seems to me satisfactory, of the erect vision by inverted images, was first suggested to me by Mr Dick, veterinary surgeon, and turns on the alleged fact, that the course of the optic nerves and tractus optici is such, that impressions on the upper part of the retina, are in fact impressions on the lower part of the optic lobes, *i. e.* of the sensorium, and impressions on the outer part of the former, are on the inner part of the latter; and *vice versa*.

If this be so, it appears to me, after repeated consideration, that it will furnish an explanation, and the only one of which the subject admits, of the harmony or correspondence, which I believe to exist from the first, between the intimations acquired by sight and by touch, as to the relative position of objects or their parts, notwithstanding that the impressions made by them on the external organs of sight and of touch are arranged inversely in regard to one another.

In regard to the nerves which are truly the organs of touch, *i. e.* the posterior portions of the spinal nerves, and the larger portion of the fifth cerebral, which is truly a spinal nerve, the general law certainly is, that all impressions made on their branches are felt by us to be higher or lower, as the points of the cerebro-spinal axis, from which they originate, and on which their sensibility depends, are truly higher or lower ; and if there be such a peculiarity in the insertion of the optic nerves into the cerebro-spinal axis, that its highest portion is inserted lowest, and its outermost portion inserted innermost, then the fact of impressions on the upper surface of the retina and optic nerve being felt by us as lower, and of impressions on their outer surface being felt as inner, will be reduced to the same law as regulates our perception of the relative position of objects of touch.

In all the vertebrated animals, it is well known that the optic nerves, behind the commissure or decussation, or Tractus optici, cross and embrace the Crura cerebri ; and that in the Mammalia they are in connexion, behind or above the crura, with the bodies called Thalami nervorum opticorum, and Corpora quadrigemina.

It has been disputed, even lately, whether the true origin of the optic nerves in the human body is in the Thalami, as was formerly thought, or in the Corpora quadrigemina, as maintained by Gall and Spurzheim ; and I have repeatedly noticed the accuracy of the observation of the late Dr Gordon, that the fibres of the tractus opticus are not merely expanded over the outer surface of the thalamus in the human body, but at various points plunge into its interior. Even those which thus dip inwardly, however, follow the same direction as the more superficial fibres, tending inwards and downwards towards the Corpora quadrigemina.

In others of the mammalia, the connexion of the optic nerves with the thalami is much more superficial ; and in birds, reptiles, and fishes, it seems perfectly ascertained, that the optic lobes, in which the optic nerves exclusively originate, correspond to the Corpora quadrigemina only.

The question of the true origin of the optic nerves, however, cannot be decided merely by anatomical inquiry. It is now well known, not only that the endowments of different nervous filaments, of precisely the same structure, and contained in the same mass, or bound in the same sheath, are often perfectly different; but even that different portions of the same nervous filaments, especially where they connect themselves with the central masses of nervous matter, may have perfectly different endowments; as, *e. g.* in the case of the fibres ascending from the Corpora pyramidalia, through the crura cerebri and corpora striata, to the bottom of the convolutions, the whole of which are continuous, but only part of which possess the power of exciting muscular contraction when irritated.

Now, if we inquire in what part of the contents of the cranium the sensations of the eye are found *by experiment* to reside, the experiments of Mayo and of Flourens, particularly those of the latter author, which were repeated before, and reported on by Cuvier, are generally regarded as affording satisfactory evidence that they reside in the Corpora quadrigemina. These bodies, and the optic nerves, appeared distinctly to be the only parts of the nervous system, the irritation of which uniformly excited the contraction of the iris, no doubt by producing a sensation of light; and the destruction of which uniformly stopped the play of the iris, and extinguished vision.*

Our business is, therefore, to learn in what manner those fibres of the Tractus optici, which can be distinctly traced into the Corpora quadrigemina, are there implanted; and when we trace the course of these fibres in the brains of the mammalia (hardened by alcohol), whether they descend on the Corpora quadrigemina from the Thalami, or pass more directly backwards below the Corpora geniculata, it seems to me quite obvious that they first turn inwards, and then enter the Corpora quadrigemina from above downwards, and are so expanded over the superior of

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* See Recherches Experimentales, &c. p. 150, *et seq.*

these bodies (the nates), that the outer portions of the Tractus pass over to the inner part of the nates, and the upper portions of the Tractus pass down to the lower part of the nates.*

In the lower classes of vertebrated animals, where the course of the Tractus opticus on each side to the optic lobe, is shorter and less winding, it is not so easy to ascertain whether the whole of the fibres passing backwards from the nerve, and turning round the crura cerebri, are inserted in the same way as in the mammalia; but that they are expanded over the optic lobes from before backwards, is easily shewn, and if we can trust the representations of Serres, the mode of their implantation into the optic lobes is quite in conformity with what we observe in the Mammalia.†

Now, there is no such contortion or involution of the nervous filaments of the fifth, or of any other nerve of the symmetrical system, where it is implanted in the cerebro-spinal axis, and so constituted a nerve of Touch; and from this I think it clearly follows, that although the impressions made *on the retina*, by the different parts of an object, are situated in regard to one another in the *inverse* order of those made on the surface of the body, yet the impressions made, *through the retina* and optic nerves, on the *cerebro-spinal axis*, are *in the same order*, as those made through the nerves of touch, on that central portion of the nervous system, on which the sensibility of all nerves depends; and therefore, that the notions which we form of the relative position of the parts of objects, by the senses of sight and of touch, will naturally correspond.

But another difficulty here presents itself. Although we understand, from what has been stated, in what manner the impres-

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* See Plate XVI. Fig. 2. of this volume.

† See Anat. Comp. du Cerveau, Plates vi. vii. Figs. 149, 151, 159, 165, 170, 181, 188–89, and 193; and Pl. XVI. Fig. 3. of this volume.

sions, made *on each of the optic lobes,* by the images on the retinæ, correspond with the real position of the parts of external objects, which these images represent; yet, in the case of man, and of all other animals, in whom the partial decussation of the optic nerves exists, and in whom, if that form of structure has been rightly explained, *both optic lobes* are concerned in vision even by one eye, (the right lobe in the vision of the right division of the retina, and the left lobe in that of the left), only one portion of the field of vision, even of one eye, produces any impression on one optic lobe; and the *left portion of the field of vision,* being represented (by the laws of light) on the right division of the retina, makes its impression on the *right optic lobe,* while the right portion of the field of vision, represented on the left division of the retina, impresses the left optic lobe; therefore, although the individual parts of each of these impressions are in the right order, yet the two impressions are transposed; and both are necessarily, at one and the same moment, objects of attention to the mind.

Now, if it be true, as is here supposed, that the impressions on the eye, by which we are informed of the relative position of objects, harmonize with those made on the sense of touch, only because when transmitted to the optic lobes they are arranged in the real relative position of the objects exciting them, this transposition of the impressions made by two distinct portions of the field of vision, even of one eye, appears fitted to deceive us, and I believe would do so, were it not compensated by another piece of structure, the use of which has long puzzled physiologists, and which I do not remember to have seen connected by any one with the sensations of the eye, viz. the Decussation at the Pyramidal bodies;* whereby, as is generally believed, the whole common sensation, and the whole voluntary motion, of the left half of the body, are put in connection with the right half of the brain, and those of the right half of the body with the left half of the brain. Therefore, while man, and all other animals, that have the power of looking directly forwards, see what is to their

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* Plate XVI. Fig 4.

left when doing so, only by their right optic lobes, so they feel and move, on their left, also by the right hemispheres of their brains. And the sensation and motion of the left sides of their bodies are put in connection with the right sides of their brains, only because the laws of light necessarily imply, that the images of whatever lies to their left in the external world, should be formed on the right side of the retinæ of their eyes, and impress their right optic lobes.

If this be the true use of the decussation at the pyramids, the following consequences appear naturally to follow :—

1. That this piece of structure will be found only in those animals which are, more or less, in the habit of directing both eyes to the same object.

2. That, where it is found, there will be found also the partial decussation of the optic nerves, connecting each eye with both optic lobes, and each optic lobe with both eyes.

3. That where the decussation at the pyramids exists,—as the sensation and motion of each side of the body will be dependent on the opposite side of the brain,—injuries of either side of the brain, or in general of the parts superior to the decussation, if they produce palsy, will produce it on the opposite side of the body.

4. That where the decussation at the pyramids does not exist, this crossing of the effect of an injury of the brain to the opposite side of the body will not be observed.

Now, all these things are so; at least, according to the most general statements of accurate observers, certainly unconnected with the theory which is here advanced; and it seems to me extremely improbable that these coincidences should have existed, if the piece of structure in question had not been designed for the purpose, and had the effects now stated.

The decussation at the pyramids exists in the mammalia and in birds; but the number of crossing fibres becomes less in the lower parts of the scale, as does the size of the cerebral lobes, in proportion to the spinal cord. In reptiles and fishes it does not exist.* In the two former classes the power of directing the axis of both eyes to the same object, exists, although in the case of many quadrupeds and most birds, that object must be very distant. In reptiles and fishes, the eyes appear to be dissociated from one another, and directed either laterally, or vertically upwards, in such a manner that they evidently regard objects, in general, only with one eye at a time.†

In the mammalia and birds, the commissure and partial decussation of the optic nerves are found; but in reptiles and fishes there is the complete decussation and no commissure.‡

In the human body, it is well known that the usual effect of injury of the brain or cerebellum, when it produces palsy, is seen on the opposite side of the body; but the effect of injury of one side of the spinal cord, even in the neck, is seen on the same side of the body; and in the experiments of Flourens, the *crossing* influence of injuries was uniformly seen in the mammalia and birds, when they were inflicted any where above the decussation at the pyramids, but not when they were below this point. In reptiles and fishes, no crossing influence, and indeed hardly any influence, on sensation or voluntary motion, from injury of contents of the cranium, could be observed.§

I do not, however, offer this speculation as altogether satisfactory or free from difficulties; and two difficulties, in particular, present themselves so obviously as to demand notice.

1. It may be said, that, in the case of reptiles and fishes, although the impressions made on each optic lobe may be in the

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* Cuvier, Rapport sur l'Anat. Comp. du Cerveau, &c. par Serres, in latter work, pref. p. 26.

† Cuvier, Leçon 12, Art. 2.

‡ Serres, p. 326, *et seq.*

§ Recherches Experimentales, &c. p. 121.

true position of the objects whence they proceed; yet, as the optic nerves decussate completely, the vision of the left eye, and therefore of objects on the left side of the body, will be dependent on the right optic lobe, and *vice versa*. Here, it may be said, there is an apparent cause of discord between tactual and visual impressions; yet there is no decussation at the Corpora pyramidalia to compensate for it.

I believe the true answer to this to be, that as the eyes of these animals, in general, cannot be directed to the same point, and as any object, accurately observed, is contemplated only by one eye, so their attention is never fixed simultaneously on the sensations of both eyes; and when we attend to our own sensations, when we produce artificial squinting, we shall see no difficulty in this supposition. What makes it necessary, as I conceive, that the decussation at the pyramids should exist, to preserve harmony between the intimations of sight and of touch, is not the circumstance of the visual impressions from objects on the left side of the body being made on the right optic lobe, but the circumstance of the impressions made on both optic lobes concurring in producing one sensation, on which the attention is necessarily fixed; and it is where both optic lobes are found to be concerned in vision, even by one eye, that this structure is therefore to be expected.

2. When it is said that the decussation at the pyramids transfers the sensation and motion of the right side of the body to the left side of the brain, the well known objection immediately presents itself, that the sensitive and motor nerves of the *face* arise higher than that decussation; and therefore that if this piece of structure explains the harmony of impressions on the retinæ, with those on the body and limbs, it leaves unexplained the still more remarkable fact, that impressions on the skin of the *left* side of the face are felt to belong to the same side of the body, as impressions on the *right* side of the retina, and therefore on the right optic lobe.

In answer to this, I would observe, *first*, that although there may be an anatomical difficulty in understanding *how* the sensation and motion of the left side of the face are put in connection with the right side of the brain, yet observation of the effects of injury or disease demonstrate that they *are* in connection with it; the palsy depending on injury or disease of the right side of the brain extending very generally to the left side of the face, as well as of the body. And *secondly*, I would say that there is no great difficulty in understanding that this should be the case, if we suppose, as Mr MAYO appears inclined to do,* that the true origin of the nerves is in those columns of the cerebro-spinal axis which do not decussate (and this is pretty certainly the case as to the fifth nerve, which is easily traced down into the spinal cord behind the Corpora pyramidalia), and that the decussating fibres of the Corpora pyramidalia are not to be considered as the continuation of the columns of the spinal cord, but as the cords of communication between these columns, and the masses of the brain and cerebellum.† If this be so, it is easy to conceive, that an injury of the right side of the brain, transmitted through these cords of communication, should strike downwards to the left side of the body, and upwards to the left side of the face in palsy; and that, in the natural state, the sensation and motion of the left side of the face, as well as trunk and limbs, should be in connexion, although by a circuitous route, with the right side of the brain; and therefore in harmony with impressions on the right optic lobe.

If the foregoing speculation be just, it would seem that the structure to which the attention of physiologists was first directed by NEWTON, is only a part of the arrangements, by which the in-

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* Outlines of Human Pathology, p. 202.

† "The two cords of the spinal marrow do not cross, but merely the middle or pyramidal fasciculi of each, which give origin to the Crura cerebri by expanding and becoming broader."—*Tiedemann's Anatomy of the Fœtal Brain, Translated by Bennett*, p. 144. I am aware of the difficulty of tracing the course of the fibres at the medulla oblongata, and what Sir CHARLES BELL describes, I believe correctly, decussating fibres behind the pyramids; but all are agreed that there are fibres in this part which do not decussate.

timations given by the sense of Sight are made to harmonize with those which result from that of Touch.

When we reflect on the importance and pre-eminence of that sense, by which we are placed in relation almost with the Infinity of Space, we should bear in mind at the same time, that the conditions of that sense are necessarily put in dependence on the laws of Light; while the sense of Touch, to which we are indebted for our most accurate knowledge of things on the Earth's surface, is altogether independent of those laws.

In order that the intimations given by these two senses may correspond with one another, it would appear, *first*, that certainly in some, probably in all animals, the structure of the optic nerve brings the impressions, which form inverted and reverted images on the retina, into the same order on the sensorium, as those which might result from the touch of the same objects; *secondly*, that in those animals which can direct both eyes to one point, the partial decussation of the optic nerves, generally, if not universally present, enables the images produced by an object on the corresponding parts of the retinæ of the two eyes, to co-operate in producing one impression on the sensorium, and one sensation in the mind; and *lastly*, that the decussation at the pyramidal bodies enables those animals to acquire correct information as to objects of sight, from impressions made by them simultaneously on both optic lobes, *i. e.* on both sides of the sensorium, notwithstanding that the impression on each side of the sensorium comes from the opposite side of the object in view.

Nothing is farther from my intention than to represent this subject as exhausted, or these conclusions as ascertained; but in the present state of our knowledge, I think it may be said, that the inquiry has led to a *probable* solution of two difficulties, long felt in Physiology,—the cause of single and correct vision by double and inverted images, and the use of the decussation at the pyramids. And if the theory shall be found to be incorrect, it may still be of use, by acting as a stimulus, and to a certain degree as a guide, to farther inquiry.

On one Source of the Non-Hellenic Portion of the Latin Language. By the Rev. Archdeacon Williams, F.R.S.E., Rector of the Edinburgh Academy, &c. &c.

(*Read 7th March* 1836.)

Payne Knight, no mean name among philologers, after a masterly and convincing proof, that neither Zenodotus nor Aristarchus, the great critics of the Alexandrian school, could be acquitted of the charge of "scarcely credible ignorance" of the primitive form of the Homeric language, thus proceeds :—

"The grammarians and critics of Alexandria were all guilty of the same fault. They never investigated the original sources of the language, but classed among anomalous dialects and poetic licenses every thing that was not in unison with their own usual style of speaking. In their age there existed many clews to the inquiry, which have now disappeared, but which, at that time, might easily have been found in written records, and in the rude and semi-barbarous languages of Italy and other countries adjacent to Greece. Had any one, however, suggested to Aristarchus that the true form and character of the Homeric dialect was to be extracted from the Latin, Tuscan, or Oscan languages, he would in my opinion have been as much astonished as if he had heard of the claims of the Irish antiquary, who affirmed that the Homeric poems had been translated furtively from the "Gaelic into Greek."[1]

Whatever might have been the astonishment of Aristarchus on being referred to the rude and semi-barbarous dialects of isolated and mountainous districts for a resolution of his philological difficulties, every man acquainted with the subject knows that the

[1] Prolegomena, p. 35.

great superiority of the modern over the ancient philologers, is not attributable solely to the successful prosecution of the study of comparative etymology, a science unknown to the ancients, but also to the careful examination of a mass of words and expressions preserved in ancient glossaries. Many of these, although not now to be found in classic authors, serve as clews to the discovery of meanings, which, without their aid, must have remained unknown. In the works of HESYCHIUS, SUIDAS, and in other semi-barbarous lexicons or glossaries, we have not only the corrupted, or rather, in many instances, the incorrupted form of Greek words, but innumerable fragments of languages which have ceased to be spoken among men.

The scholars of our days are wiser than the one-eyed critics of the Alexandrian school; and we do not disdain the aid of every cognate language in explaining works composed nearly three thousand years ago. Even our own language and literature have experienced the beneficial influence of this example. Our English dictionaries are assuming a more scientific form; and the language daily receives new light and vigour from the publication of provincial glossaries, which cease not to issue from the press. But the English glossaries alone will not suffice. For (with due respect to Dr JAMIESON be it said) it will eventually be found true, as has been lately suggested by a writer admirably qualified to give an opinion upon the subject, that no satisfactory thesaurus of our magnificent and copious language, can ever be completed without the aid of Celtic scholars.

I speak from knowledge, when I say that the Anglo-Saxon is deeply tinged with the language of the Britons of Wales, Cornwall, and Armorica, and that the meaning of countless words, commonly regarded as pure Saxon, will in vain be sought in the forests of Germany or the wilds of Scandinavia. Even household terms, the language of every-day life, without the aid of scholars acquainted with the primitive language or languages of these islands, must be handed down to posterity as mystic signs devoid of meaning. But of this at another time.

My present object is to shew that a portion of even the Latin vocabulary is to be referred to the same primitive language still spoken within this island, and which was once the medium of intellectual intercourse between no small section of the inhabitants of south-western Europe.

I need not mention that the question, whether the original population of Italy was to be regarded as Celtic, Hellenic, or Teutonic, has been argued ponderously and learnedly. But the result has not been either conclusive or satisfactory. I omit the claims advanced in favour of the Aramæan or of the Slavonian origin of this population, as being barely entitled to a serious consideration. Even in examining the mode in which the pretensions of the first named races have been either affirmed or denied, we must be compelled to express our wonder that prudent men could have ever hoped to bring their labours to a successful termination. Not one of those who piled volume on volume on the subject, seems to have possessed the necessary knowledge. The very elements of the science, by which the problem was to be solved, were unknown to them. Some were Greek, Latin, and French or Italian scholars. Others, to the knowledge of these, added an intimate acquaintance with the Teutonic dialects. But this was not enough. A thorough knowledge of the Latin itself, of the Greek, and of one type at least of the Celtic and Teutonic dialects, must precede a successful examination of the claims of the three cognate families. But hitherto no one thus qualified has entered the field.

Early in life, I became satisfied that nothing could be done in clearing away the cloud that sinks deep and dense on the early history of Europe, without an extensive and accurate knowledge, not only of the language, but also of the literature, laws, habits, and religion of the three races. I had seen enough of the errors committed by one-eyed scholars, to satisfy me that the greatest caution is necessary in assigning their several sources to disputed words, and that the comparative etymology which is grounded on index-hunting alone, and consists in the juxta-position of words similar in form, and casually, perhaps, in sense, is more likely to lead to error than to the truth. What would be thought of a person, who, without a knowledge of the grammar and construction of a Greek verb, should, from a Greek index, attempt to find the various forms in which the word τεινω is found in Grecian literature? What guide could he have in placing τεταμαι as its perfect passive, or εταθην as its historical tense, in the same voice? But it may be said that such knowledge as is required must be beyond the reach of any individual, and can scarcely be attained in the course of a long life. This is, I fear, too true, although it is only of late that I have been compelled to adopt the conclusion.

The Greek and Latin language and literature, I must, from my situation, be supposed to understand. The Cumrian[1] or Welsh was known to me from my childhood, both colloquially and as the language of popular poetry, and of the public service of the Church. But there exists a long series of literary works, extending from certainly the seventh to the fifteenth century, to which an acquaintance with the colloquial popular and ecclesiastical dialects furnished no sufficient key. To master these the study of more than one form of the language was necessary, and, to a certain extent, the task was achieved. The prose style of our most ancient chronicles, triads, and laws, is familiar to me, and I can read with satisfaction most of the works of the bards, as far back as the twelfth century. But there remains a body of poetry, ascribed to Aneurin, Taliessin, and other well known names, which, if Cumrian, is Cumrian in a very suspicious form. This language I do not altogether understand, nor, according to my belief, is it completely understood by any man living. The

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[1] Much error, both in History and Geography, would have been avoided, had the names by which nations denominated themselves been alone used to designate them. Welsh was the general name by which the German tribes designated all the Roman provincials whose territories they invaded, but especially the Italians.

words are Celtic, and individually recognizable, but their meaning, when strung into sentences, requires an Œdipus. In confirmation of this, I appeal to the various attempts which profess to be translations of the Gododin of Aneurin. With the Anglo-Saxon literature, both for its antiquity and extent, the best representative of the ancient Teutonic, I do not profess an accurate acquaintance. I have read much of it, and have studied its vocabulary and grammar with care, but cannot call myself an Anglo-Saxon scholar. To a general knowledge of the modern languages of the south-west of Europe, I can add a slight acquaintance with the Gaelic and Basque tongues.

In a course of study thus followed up, with occasional excursions into other paths of knowledge, for more than twenty years, it has been my fortune to make, as I conceive, some important discoveries. Had I the command of the requisite time, they might be embodied in one great work, but as that is not the case, I willingly avail myself of the medium of this Society, in order to communicate some detached fragments to those who take an interest in such studies. I confine myself, on the present occasion, to the subject of the original population of central Italy, and to the question which I intend to affirm, that it was of the Cumrian or Cimbrian race, cognate with the Cumri of our island, and that their language formed some portion of the non-Hellenic elements of the Latin tongue. To pave the way for this, I propose to show that the Umbri, an ancient people of Italy, were of the same race and lineage with those who within our islands call themselves Cumri, and that they, through their colonies, formed no small part of the original population of Rome.[1] We are told by Florus, "that the Umbri were the most ancient people of

[1] It is my intention to write the words as they sound to the ear, not as they appear disguised in the modern spelling of the Welsh, a spelling which has done more to throw the language into obscurity, and render its very appearance disgusting to the eye of a civilized man, than all other causes put together. It not only has done this, but has absolutely served to render it more abstruse to the Cumri themselves. The vowel w, equal in power to the double o in wood, I shall retain, and ü with two points

Italy;"[1] by PLINY,[2] that "they were deemed to be the most ancient nation of Italy;" by DIONYSIUS of Halicarnassus,[3] that the "Ombrici were a nation of peculiar greatness and antiquity."

HERODOTUS[4] states, that the Lydian Tyrsenians (according to him, the germ of the Tuscan race), settled among the people called by the Greeks Ομβρικοι, and supposed by some of their writers to have derived their name from their having survived the general deluge [ομβρος]. We also learn from him that their country was of great and indefinite extent. In the words of NIEBUHR, "It stretched to the foot of the Alps, for the rivers Karpis and Alpis, one of which is certainly represented by the Inn,[5] flowed from the country above the Umbrici." According to SCYLAX,[6] Umbria included Picenum, as he places Ancona within its limits.

From PLINY[7] we further learn that the Tuscans took no less than three hundred towns from the Umbri, in other words, "that the whole Tuscan territory had once been Umbrian." From these authorities it is evident that the Umbri, at a remote period, occupied the greatest portion of Northern Italy. The Li-

over it, to represent the sound ee. The soft sound of the d I shall leave unmarked, as it was in the older writers.

[1] "Antiquissimus Italiæ populus." Lib. i. cap. 17.

[2] "Gens antiquissima Italiæ." Lib. iii. cap. xiv.

[3] ὀι ομβρικοι. εθνος εν τοις πανυ μεγα και αρχαιον. Lib. i.

ομβρα γενος γαλατων και σαλπιων. Jzetzes, Lyc. cap. 199.

To these may be added the testimony of the Historical Fragments ascribed to Varro: "Ex his venisse Janum ceu Ogri et Gallis progenitoribus Umbrorum." Ed. Lugd. 1560.

[4] Book i. cap. 94.

[5] Herod. Lib. iv. cap. 49.

Εκ δε της καθυπερθε χωρας ομβρικων Καρπις ποταμος και αλλος Αλπις ποταμος, προς βορην ῥεοντες ανεμον, και ὀυτοι εκδιδουσι ες Ιστρον.

[6] Page 6.

[7] Lib. 3. cap. 14. "Tercentum eorum oppida Tusci debellasse referuntur."

gurians,[1] a nation confessedly Celtic, seem to have shared the country with them. In historical times these are described as possessing the upper vale of the Po, the Maritime Alps, and the Northern Apennines, while the Umbri were confined to the central group, the most important natural fortresses of Italy. The whole of the original population of eastern Italy, with the exception of those who took refuge in the central Apennines, was reduced under the power and influence of the Hellenic colonists, who encircled the southern Peninsula with a line of Grecian cities of surpassing wealth and magnificence. But time has grudged to us the knowledge of the history of Sybaris, Crotona, Elea, and Pæstum. We read that Sybaris was once the chief of five and twenty cities, and of four nations, and that it could bring three hundred thousand warriors into the field. Crotona was not inferior in arms, arts, or philosophy—but internal dissensions, the destruction of the enlightened classes by a riotous democracy, and the fatal aid of their half conquered subjects, the native tribes, hurried them to a premature decay. Sybaris fell A. C. 508, barely within the verge of accredited history, and Crotona soon followed; destined, however, to a more lingering death by the hands of the barbarian Bruttii. The Lucanians, a mountain tribe, which had taken the lead in the attacks upon the Grecian States, had undoubtedly imbibed some portion of Grecian civilisation. In Niebuhr's[2] words, "hereditary enemies as they were, they nevertheless acquired the language to such a degree, that their ambassador filled the popular Assembly at Syracuse with surprise and enthusiasm by his pure Doric. Nor would the authors of Pytha-

[1] The Ligurians were themselves Ambrones, or Ombro-nes, as is evident from the story told by Plutarch in the life of Marius :—" The Ambrones came on crying out Ambrones, Ambrones; this they did either to encourage each other, or to terrify the enemy with their name. The Ligurians were the first of the Italians that moved against them, and when they heard the enemy cry Ambrones, they echoed back the word, which was their own ancient name." The English reader may not know that the Cumrian name for England is to this day Loiger or Liguria.

[2] Vol. i. p. 84.

gorean Treatises have used the titles of imaginary Lucanians, had it not been notorious that this philosophy had found reception there, or had it been unusual for Lucanians to write Greek."

If the fate of these colonies, whose foundation is supposed to fall within the bounds of history, be so obscure and unknown, what can be said of Arpi and Canusium, connected with Greece by mythology alone? STRABO[1] infers from their remains, "that they were the greatest of the Italiot cities." Yet Arpi is first mentioned in history as an Apulian city of no importance, of which the Romans possessed themselves without difficulty. In confirmation of STRABO's authority, we have the strongest modern testimony. [2]"Those writers, however, who have traced the circuits of the Walls of Canusium from the remaining vestiges, state, that they must have embraced a circumference of sixteen miles;" and antiquaries dwell with rapture on the elegance and beauty of the Greek vases of Canosa, which (according to MILLINGEN) in point of size, numbers, and decoration, far surpass those discovered in the tombs of any other ancient city, not even excepting Nola." It is in vain for us to attempt to raise the veil, and to form conjectures concerning the state of Italy, both central and maritime, when a city of this magnitude and magnificence could have been founded, and have flourished, in this apparently unfavourable position. Assuredly the neighbouring mountaineers must at one period have been in close connection with it, either as subjects or allies. In either case, the civilisation of Canusium, as well as of Arpi, must have produced ameliorating effects upon the manners, laws, and language of the neighbouring aborigines. It is probably to this period that a sound historian would be inclined to ascribe the deep impression of the Hellenic character, which is visible in the language, laws, arts, and religion, of the tribes of ancient Italy, and not to the intervention of some wandering tribes or fraternities, who, under the name of Pelasgi, came, we know not whence, and departed we know not whither.

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[1] STRABO, vi. 248.

[2] CRAMER's Italy, vol. ii. p. 92.

It is not equally vain to ask, who were the destroyers of a power which could build such cities, of a civilisation so advanced as this. History, indeed, is silent, but the occupation of the country by the Lucanians and Apulians leaves us no room to doubt as to the agents in the work of destruction. The original Apulia, according to Strabo, was a small tract of country immediately to the south of the Frentani, whence they must have spread over the larger province which, in consequence of the conquest, was called after their name.

The reaction of the native population against the foreign colonists, which proved thus fatal in the south, was carried on with equal energy in central Italy by the Samnites, who assailed with equal fury and success both the Greek colonies and Tuscan invaders of Campania. The Samnites, like the Lucanians, were Sabellians by blood, of Sabine race, and cognate with the Marsi, Marrucini, Peligni, Hernici, Vestini, Hirpini, and Frentani. If, therefore, we can trace the origin of the Sabines themselves, we at once discover the origin of all the tribes that sprung from them. In the words of Niehbuhr,[1] "the Italian national migrations came down like others from the north; and Cato's opinion that the origin of all the Sabellians was derived from the neighbourhood of Amiternum, admits of no other rational meaning than that the most ancient traditions, whether they may have been Sabine or Umbrian, assigned that district as the habitation of the people who conquered Reate." The same author[2] says, "Strabo calls the Sabines autochthons. This name, applied to a people whose spreading so clearly falls within the historical period, can only mean that they cannot be taken for any colony from any nation out of Italy." The question is thus very considerably narrowed, and we have only to ascertain whether they are the offshoot of any other people in Italy or not. To this a satisfactory answer

[1] In the chapter on the Sabelli and Sabini, vol. i.

[2] Same chapter.

can be given, because ZENODOTUS of Troezene, a writer quoted by DIONYSIUS of Halicarnassus, as the author of a History of the Umbri, and who must be supposed to have examined into the subject, expressly asserts that the Sabini were originally Umbrians. These are his words,—"The Sabines, who are indigenous, inhabited first the Reatine District, but being driven thence by the Pelasgi, entered that country which they still inhabit, and having changed their name together with their situation, were called Sabini instead of Umbri." [1]

To connect the Sabini with the original population of Rome is an easy task. Whatever was the origin of Alba, the supposed metropolis of Rome, its blood could not have entered largely into the veins of the Roman people. According to the national tradition, the first occupiers of the strongholds which became the site of Rome were a half-pastoral, half-bandit race, composed probably of the broken men of many a tribe; but according to the same tradition, the mothers of the second generation of Rome were all Sabines. This would make all the children half Sabine in blood, and far more so in language and feeling. When to this we add that the same tradition assigns to the young state a new settlement, which brings King TATIUS and his countrymen into the city in equal numbers, and with equal privileges, the portion of Roman blood, not Sabine, is reduced to one-fourth at least. Moreover, had not the habits, laws, and institutions been essentially Sabine in the early ages of the Republic, the creation of a Sabine legislator in the person of NUMA, the citizen of the Sabellian Cures, would never have been invented, much less tolerated. And had not the mass of the population been decidedly Sabine in its inclinations, the State would not, after the losses

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[1] Lib. ii. p. 49.—*Ζηνοδοτος δε Τροιζηνιος, συγγραφευς Ομβρικου εθνοῦς, αυθιγενεεις ἱστορει το μεν πρωτον οικησαι περι την καλουμενην Ρεατινην, εκειθεν ὑπο Πελασγων εξελαθεντας, εις ταυτην αφικεσθαι την γην ενθα νυν οικοουσιν, και μεταβαλοντας αμα τῳ τοπῳ τουνομα Σαβινους εξ Ομβρικων προσαγορευθηναι.*

and disgrace of the war with Porsena, have recruited its exhausted strength by the admission of Atta Clausus, with five thousand Sabines, into the full right and privileges of Roman citizens. It may therefore be safely inferred, that the great body of the original population of Rome was of Sabine, that is, of Umbrian race and blood.

The next step is to prove that this Umbrian race was cognate in blood and language with our own Cumrians. In appealing to ancient testimony, it will be at once felt that much cannot be expected which will throw any light upon the subject; yet, contrary to such an expectation, there do exist certain testimonies of the clearest nature. The first is that of Cornelius Bocchus, a learned friend and freedman of Sylla, who, according to Solinus, "completely proved that the Umbri are an offset of the ancient Gauls."[1] To give its full weight to this testimony, we ought to remember, that Sylla, the patron of Bocchus, had every reason, both during the social war and in his wars against the Marians and their allies, to institute the most serious inquiries into the origin of the Sabellian tribes, and the circumstances that had changed them from their long continued character of faithful allies, into the most powerful and inveterate enemies of Rome. The second testimony is that of Antonius Gnipho, a high name among Roman grammarians, himself a native of Cisalpine Gaul, and one of the most accomplished scholars of his day, to whose instruction even Julius Cæsar owed much.[2] Servius, Virgil's learned commentator, writes, "M. Antonius states that undoubtedly the Umbrians were the offspring of the ancient Gauls." Isidorus,[3] in his Origines, combines this

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1 Solin. Pol. Hist. cap. 8.
Absolvit Umbros Gallorum veterum propaginem esse.

2 Virg. Serv. p. 724, near the end of the twelfth book.
Sane Umbros veterum Gallorum propaginem esse refert.

3 Origines, lib. 9, cap. 2.

double testimony by his statement, " The Umbrians are a nation of Italy, but the offspring of the ancient Gauls."

The opinion of these learned men that the Umbri were the descendants of the Galli Veteres cannot, of course, be wrested to prove that some very ancient preoccupation of Italy by Gauls took place at a period antecedent to history. The only legitimate inference to be drawn from such a statement is, that they were convinced by some evidence which has not reached us, that the Umbri and " Galli Veteres" were one and the same race.

It thus becomes an important question to inquire who these " Galli Veteres" were. They certainly could not be the Cisalpine Gauls who, within the historical period, had crossed the Alps and taken possession of the greatest part of the Vale of the Po. Originally, as we have already seen, the plains of modern Lombardy were in the possession of the Umbri. The Tuscans dispossessed them, and, in their turn, were exterminated by the transalpine invaders. But Ravenna and its vicinity, and the whole of the ancient Venetia, escaped both the Tuscan and Gallic yoke. The very same position which made them the last resource of the Romans of the western empire, defended these fortresses amongst lagoons and marshes from the assaults of more early invaders. According to STRABO,[1] Ravenna was built by Thessalians, and surrendered to the Umbri. Hence it is called by PLINY[2] a Sabine, that is, an Umbrian town. That Venetia was not conquered by the Tuscans is evident from the fact, that the Veneti were a powerful tribe as late as the year 390 A. C.; for in that year they made so vigorous an incursion into the territories of the Gauls, the invaders and captors of Rome, that, according to POLYBIUS,[3] BRENNUS was induced by this powerful diversion to grant terms to the Romans besieged in the capitol,

[1] STRABO, book v. p. 214. [2] Ibid. p. 217.

[2] STRABO.—Το δε Αριμινον Ομβρικων εσ̆ι κᾰοικια καθα περ και ἡ Ραουεννα.

[3] Book xi. cap. 18.

and to return home. The Veneti, like the Romans, claimed a Trojan descent, and referred to the Ενετοι of Asia Minor as the founders of their name and race. They,[1] like the Romans, called the spot where their eastern ancestors had landed by the sacred name of Troja. But the Greeks, who seldom regarded the testimony of a people themselves as sufficient proof of their origin, indulged in various speculations on this head. Strabo,[2] in one place, informs us, that some writers held that these Italian Veneti were colonies of the Gallic Veneti. In another place[3] he states it as his own opinion, that these latter were the founders of the former Veneti, and that it was the similarity of the name alone that caused them to be referred to the Paphlagonian Ενετοι.

Without attaching any peculiar value to this opinion of Strabo, it may be adduced to prove that there were some stronger reasons than a mere similarity of name which led him to this conclusion. And there would probably be a similarity in language and habits between the Veneti on the Adriatic and their namesakes on the shores of the western ocean; and this clew, if carefully unravelled, may perhaps lead us to a discovery of who the "Veteres Galli" of Bocchus and Antonius were. Now, Polybius, a writer well acquainted from personal intercourse with all the tribes of the Vale of the Po, thus speaks of the Veneti[4]:—"The territory thence stretching to the Adriatic is occupied by another nation extremely ancient; they are called Veneti; in habits and institutions not much differing from the Κελτοι, but using a various dialect." This has been by some of the most judicious inquirers into ancient history regarded as a conclusive testimony against the Celtic origin of the Veneti; but

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[1] Festus, under Troja.

[2] Book v. p. 212.—Περι των Ενετων διττος εστι λογος, οι μεν γαρ, και αυτους φασι Κελτων ειναι αποικους, των ομωνυμων, Παρωκεανιτων.

[3] Book v. p. 195.—Τουτους οιμαι τους ουενετους οικιστας ειναι των κατα τον Αδριαν.

[4] Τα δε προς τον Αδριαν. ηδη προσηκοντα, γενος αλλο πανυ το παλαιον διακατεσχε. Προσαγορευονται δε ουενετοι. τοις εθεσι και τω κοσμω βραχυ διαφεροντες Κελτων, γλωττη δ' αλλοια χρωμενοι.

if we admit such proof, we must also confess that all the Celtic tribes of Gaul were not cognate, for STRABO expressly affirms, "that these did not all speak the same language, but that some spoke dialects varying a little in form."[1]

The inference, on the contrary, ought to be, that they were originally of the same race, but changed in some respects by time and circumstances. This will appear more probable, if we take into consideration that it is impossible for any one acquainted with the ancient history, laws, and institutions of the Cumrian race, to recognize it in the accounts given by historians of the invaders of Italy under BELLOVESUS. Their blue eyes, red hair, and great stature, together with their pastoral rather than agricultural habits, are strong proofs that the majority at least must have been rather of German than of Celtic origin. But a rational account of the various races between the Alps and the ocean, the Pyrenees and the Rhine, as recorded by ancient historians, is as yet a desideratum. The materials are ample, and the key for the solution of all difficulties will be found in an adequate knowledge of the Basque, the Cumrian, the Teutonic, and perhaps the Gaelic, which languages alone, previous to the Greek settlements and Roman invasion, could have been spoken within those limits. It will be sufficient for me, on the present occasion, to hint, that, in addition to the undoubted Teutonic origin of all the Belgian tribes, with the exception of the Menapii and Morini, a considerable portion of the territory assigned by Cæsar to the Celtæ or Galli, was possessed by a population semi-Teutonic in their names, habits, and religion. It appears evident that various eruptions of the northern nations into the south, prior to that of the Cimbri and Teutones A. C. 110, and of ARIOVISTUS at a later period, had entirely broken up the whole frame-work of society in Gall, and precipitated the several races upon each other.

[1] STRABO.—*ὁμο-χγλωττους ου παντας, αλλ᾽ενιους μικρον παραλλαττοντας ταῖς γλωσσαις.*

The *γλωττη αλλοια* of POLYBIUS is not, as is well known to every scholar, "another language," as it has been inaccurately translated, but a "variety in dialect."

Should we therefore wish to discover who were the "Veteres Galli," we must look for them in the natural fortresses of France, where they could defend themselves both from the foreign invaders, and the mixed bands of half-bandits, half-warriors, who ceased not to infest the country after the main body of invaders had passed away.

Such a race were the Veneti of Western Gaul, so similar in their history, at an earlier period, to their kin-tribe in Italy. When the Roman empire was sinking under the repeated assaults of the northern barbarians, the Italian Veneti sought refuge in their marshes and among their lagoons, where necessity soon compelled them to become a naval and commercial people. This had been the fate of the Gallic *Veneti*, at a period antecedent to history. For JULIUS CÆSAR[1] found them a highly civilized people, intimately connected with Britain, hardy sailors, and masters of practical arts, unknown, or at least unrecorded, among Greek and Roman practices. These Veneti, with their cognate tribes, the Osismii, Lexovii, Nannetes, Ambialites, Morini, Diablintes, and Menapii, together with auxiliaries from the opposite coast of Britain, prepared to resist CÆSAR. Their fleet, consisting of 220 vessels of immense size and strength, built entirely of oak, and trusting to their sails alone, was destroyed by the row-galleys of the Romans, more owing to a chance calm than any other cause. The position of their cities, on tongues of land and promontories, to which there was no access except by sea, proves that a superior force had driven them for protection into such fortresses, which their naval skill and power could make good against land-

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[1] Hujus civitatis est longè amplissima auctoritas omnis oræ maritimæ, quod et naves habent Veneti plurimas, quibuscum in Britanniam navigare consueverunt; et scientia et usu nauticarum rerum cæteros antecedunt; et in magno impetu maris, atque aperto, paucis portubus interjectis, quos tenent ipsi, omnes fere, qui eodem mari uti consueverunt, habent vectigales.—Com. Bell. Gall. lib. iii. cap. 8.

[2] Especially their chain-anchors and sails of finely tanned leather.

troops alone.[1] That these Veneti were of the Cumrian race in Britain, there can be no doubt, even the name of the present Cumri of North Wales is, if spelt after the Roman manner, the same, Gwined,[2] Venetia, Gwinedig Veneticus, Gwinedigion Venetici. The modern latinized form Venedocia and Venedocians, are pure barbarisms, for which there is not the slightest analogical support.

The trade which alone could have enabled the Gallic Veneti to have maintained so powerful a navy, was, according to Cæsar, carried on with Britain, but assuredly not with that part of Britain which he afterwards invaded, as the Britons, with whom he came in contact, do not appear to have been acquainted with naval affairs, nor to have possessed a vessel larger than a coracle. The traffic of the Veneti must have been carried on with their kindred tribes in the west of the island, where alone within the historical period the Cumrian race is found. Upon this important question—important I mean as connected with the early history of our island—I entirely agree with the learned author of Celtic Researches. The Coritani, an invading tribe,[3] " enlarged their territories, and comprehended not only the inland regions round the wide spreading arms of the Humber, but also much of the eastern coast of England."

" When Cæsar arrived in Britain, the aborigines were those

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[1] Erant ejusmodi fere situs oppidorum, ut posita in extremis linguis promontoriisque, neque pedibus aditum haberent, quum ex alto se æstus incitavisset, quod bis semper accidit horarum xii. spatio; neque navibus, quod, rursus minuenti æstu naves in vadis afflictarentur. In utraque re oppidorum oppugnatio impediebatur. Ac si quando, magnitudine operis forte superati, extruso mari aggere ac molibus, atque his ferme moenibus adæquatis, suis fortunis desperare cœperant; magno numero navium appulso, cujus rei summam habebant facultatem, sua omnia deportabant, atque se in proxima oppida recipiebant, ubi se rursus iisdem opportunitatibus loci defendebant—Com. Bell. Gall. lib. iii. cap. 12.

[2] See Pugh. Dict. under the words.

[3] P. 201.

of the interior parts and of the western coast. Their character and their habits were different from those of the other Britons, with whom Cæsar fought. We are not apprised, and have no reason to conjecture, that he saw the interior inhabitants. The armies that opposed him were similar, in their general habits, in their military art and resources, to each other, as they were also to the Belgæ of Kent.

The monuments we call Druidical must be appropriated exclusively to Aborigines of the midland and western divisions. They are found in such corners and fortresses as have, in all ages and countries, been the last retreat of the conquered. In Wales and in Mona they were used and venerated until the aborigines were completely subjugated by Roman arms. In the central counties, and in the west, they perpetually occur, from Cornwall to Cumberland, whereas few traces of them are discovered in the eastern part of the island, which therefore appears to have been occupied by those people who did not construct buildings of this nature, and who obtained possession before the aborigines deeply impressed their character upon the soil."

Of the race of the "Veteres Galli," were also the Ædui and Arverni. Both these nations claimed the sovereignty of all Gaul, and seem to have been regarded as the natural chiefs[1] of the central tribes. The Ædui seem to have derived their name from Aeð the Great,[2] the father of Prudain, one of the three pillars of the race of the Cumri. Nor is it improbable, that Cæsar's notion "that all the Gauls held that they sprang from Father Dis, and that this was taught by the Druids," originated in a mistake, which led him to confound the Greek Ἅδης with the Cumbrian Aeð the Great.[3] It is, moreover, difficult, without

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[1] Quod summa auctoritas antiquitus erat in Æduis.—Cæsar. Lib. i. 43, Ut omni tempore totius Galliæ principatum Ædui tenuissent.

[2] Triad 2 and 34, &c.

[3] Galli se omnes ab Dite patre prognatos prædicant, idque ab Druidibus proditum dicunt.—Cæsar, Lib. vi. cap. 17.

recurring to the affirmed consanguinity of the Umbri and the "Veteres Galli," to account for a circumstance so striking as the confession on the part of the Romans, "that the Ædui were their brothers and kinsmen."[1] As this name was repeatedly conferred upon them by solemn decrees of the Senate, a name which, to the best of my recollection, was never conferred except upon nations supposed to have been connected with them by blood, it is a fair inference that a case in proof of a common origin was satisfactorily made out, before an honour of so high a nature could have been bestowed upon people of barbarian, and what was more hateful, of Gallic race.[2] The Arverni, the neighbours of the Ædui, and who, from the natural strength of their country, might be expected to represent the "Veteres Galli," were, at a later period, allowed to assume the same honours, as we read in LUCAN:[3]

> "Arvernique ausi Latio se fingere fratres
> Sanguine ab Iliaco populi."

It was undoubtedly in this common claim to a Trojan origin, a claim eventually to be traced to a common religion and religious rites, that we are to recognize the principle, which induced the Veneti, the Romans, the Arverni, and the Cumrians of our island, who, from the remotest antiquity, traced themselves

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[1] Imprimis quod Æduos fratres consanguineosque sæpenumero ab Senatu appellatos, videbat.—Lib. i. cap. 43.

[2] Docebat etiam quam veteres, quamque justæ causæ necessitudinis ipsis cum Æduis intercederent: quæ Senatûs consulta, quoties, quamque honorifice, in eos facta essent.—Lib. i. cap. 43.

Ὧν ἐστιν ἐν προς Ρωμαιους εχον συγγενειαν και φιλιαν την μεχρι των καθ' ἡμας χρονων διαμενουσαν.—DIODORUS SICULUS, Lib. 4. p. 210.

Οἱ δε Εδουοι και συγγενεις των Ρωμαιων ονομαζοντο.—STRABO, l. 192.

[3] Book i. ver. 427; see also the testimony of SIDONIUS APPOLLINARIS to the same effect.

to Troy and Asia Minor, to regard themselves as "fratres" and "consanguinei."

This community of origin produced very marked political sympathies, which were of no small aid to the Romans in extending their empire. In the very romantic account given by Livy of the passage of the Cimminian Ridge by a Fabius, and the defeat of the Etrurians in the vicinity of Perusia, one thing alone appears historical, that at a very remote period a league was concluded between the Romans and Camertian Umbrians, for the sake of mutual protection against the Tuscans. Of this treaty Cicero says,[2] that even in his time it was regarded "sanctissimum et æquissimum." From the same speech we learn that there was a similar treaty with the Umbrians of Iguvium. Wars with the Umbrians there were none, and the one battle which preceded their final submission to Rome, was, if we can believe Livy,[3] almost a bloodless one. In the words of Cramer,[4] "the Umbri appeared to have offered but little resistance to the Romans, nor is it improbable that this politic people took advantage of their differences with the Etruscans, to induce them at least to remain neuter, whilst they were contending with the latter power." Here, also, Cramer's opinion on the question,[5] partly examined by me, may be advanced, and as he had no hypothesis to maintain, great weight ought to be given to his conclusion. "Zenodotus was of opinion that the Sabines were descended from the Umbri, and although it is customary to regard them as belonging to the Oscan race, I see no reason why the latter people, who are very indistinctly classed and defined, should not be considered as descended from the same indigenous stock; nay, rather when we consider the analogy which is allowed to exist between the several ancient dialects of Italy, and the uniformity of topographical nomenclature, which may be traced

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[1] Lib. 9. 36.
[2] Oratio pro Balbo.
[3] Lib. 9. cap. 4.
[4] Vol. i. 254.
[5] Do. 252.

throughout a great part of the Peninsula, there seems to be a strong argument in favour of such a hypothesis. Considering, therefore, the Umbri as confessedly the most ancient people of Italy, I think we may safely ascribe to them the population of the central and mountainous parts of that country, as also the primitive form of its language, until the several communities of the Etruscans, Sabines, and Latins successively detached themselves from the parent nation, and, from a combination of different elements, adopted also different modifications of the same primeval tongue."

The history of the Veneti,[1] " in the words of the same author, contains little that is worthy of notice, if we except the remarkable feature, of their being the sole people of Italy who not only offered no resistance to the ambitious projects of Rome, but even at an early period rendered that power an essential service, if it be true, as POLYBIUS reports, that the Gauls, who had taken Rome, were suddenly called away by an incursion of the Veneti into their territory (ii. 18.). The same author also expressly states, that an alliance was afterwards formed between the Romans and Veneti (ii. 23,) a fact which is confirmed by STRABO (v. 216)."

This state of security and peace would seem to have been very favourable to the prosperity of the Venetian nation. According to an old geographer, they counted within their territory fifty cities, and a population of a million and a half; " when the Gauls had been subjugated, the Veneti do not appear to have manifested any unwillingness to constitute a part of the new province, an event which we may suppose to have happened not long after the second Punic War. Their territory from that period was included under the general denomination of Cisalpine Gaul, and they were admitted to all the privileges which that province successively obtained."

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[1] Vol. i. p. 113.

It is difficult to account for such a general and complete amalgamation, without suspecting that the two races, the Roman and Venetian, were originally the same, especially when we call to mind, that the invading Gauls of the Vale of the Po, had absolutely to be exterminated by their conquerors.

The Ædui* were the first of their countrymen to embrace the friendship and alliance of Rome, and continued to a late period to partake of the advantages therefrom resulting. Pliny calls them in his day "Foederati," and their neighbours the Arverni, "Liberi." Even as late as the age of Sidonius Apollinaris,† the latter nation still prided themselves on their Roman blood, and supposed descent from Italy.

> Est mihi quæ Latio se sanguine tollit alumnam,
> Tellus clara viris.

It is well known that the Cumri of our island claim a Trojan origin, and that this tradition was held by them long before the impudent fabrication of Geoffroy of Monmouth, and that even Nennius gave a historical aspect to the belief. We read in the Life of Agricola, that the Britons embraced with avidity the prominent arts of Roman civilization, a circumstance which an examination of history should induce us to regard as almost impossible, except in cases where the cognation of the victors and the vanquished is very close.

On the whole, it may therefore be regarded as proved, that there were at the earliest period, when the history of western Europe commences, fragments of a great nation to be found in the fastnesses and natural fortresses of Italy, Gaul and Great Britain,‡ who all agreed in claiming a common origin, and in

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* Strabo. *Πρωτοι των ταυτῃ προηλθον προς την φιλιαν και συμμαχιαν.*

† Sidon. Apollinar. Poem. 62.

‡ Tacitus, 21. Jam vero principum filios liberalibus artibus erudire et in-

tracing it, directly or indirectly, to a name called by them Troy.

I believe that I can solve this riddle, and explain the cause of this common agreement, and the symbols which enabled the dispersed members of a great family to recognise each other. But the investigation of this question alone would require a volume, and must for the present be dropped.

Having thus shown, from important testimonies, the close union between the original population of Rome and the Umbrians, and between the Umbrians, Romans, "Veteres Galli" and Veneti, and the Cumri of our island, it now remains that we should examine into the language of the Umbri, and its close connection with the language of the Cumri. If the Iguvinian or Eugubian tables were really Umbrian, this might be supposed to furnish us with a certian test as to the identity of the two languages. But this they assuredly do not. The double set of characters, and the very imperfect representation in the tables, written in Latin characters, of the sense contained, or supposed to be contained, in those inscribed with ancient Greek letters, are in themselves a proof, that the language of the latter was a sacred one, known to a few alone, and not therefore to be accounted the language of the country. Although terrified by the fate of learned men, who have made a fearful shipwreck of their sagacity and judgment, upon these disinterred records of an older world, I might refuse to meddle with "such dangerous matter," I hesitate not to propose my firm belief, that these inscriptions are the remnants of a language, which, for want of a better name, may be called Pelasgic, and that we are on the eve of a dis-

genia Britannorum studiis Gallorum anteferre, ut, qui modò linguam Romanam abnuebant, eloquentiam concupiscerent, inde etiam habitus nostri honor et frequens toga, paullatimque discessum ad de linimenta vitiorum porticus et balnea et conviviorum elegantiam; idque apud imperitos humanitas vocabatur, cum pars servitutis esset.

covery, which will enable us to read them with perfect facility. They are evidently hymns, composed by priests, similar in character to the Curetes, Idæi Dactyli, Telchines, Orphic initiators, and other such colleges, whose mission it was to communicate the blessings of corn and wine, under religious and mysterious sanctions, to those nations, which had not previously been made acquainted, with those main supports of civilized life.

Among these names the Pelasgi occupy a conspicuous station, not, however, as teachers of the art of agriculture and vine-dressing, but as hewers of stone and builders of fortresses and cities. It would be most difficult to prove that they ever were a distinct nation, but it is clear that they belonged to that race, call them Trojans, Pelasgians, Thracians, Phrygians, or any other equivalent name, who sunk under the superior energies of the Achæan and Hellenic races. PELASGUS, the patron saint or mythological representation of the Pelasgi, is described by PAUSANIAS,[1] as the person " who first invented huts to defend mankind from the cold and rain." Even FYNES CLINTON, with all his judgment and sagacity, cannot press this PELASGUS into the historical curriculum, without multiplying him into five distinct personages, a clear proof that there is not the slightest rational ground for clothing him at all with a real existence. That the Pelasgi were everywhere to be found, is most true, in Asia, in Crete, in the other islands, in Thrace, Thessaly, Epirus, Peloponnesus, and Italy, but it is impossible to prove that they were a race distinct in blood from the other older inhabitants of those countries. The word, twisted as it has been by various attempts at etymology, seems a very simple compound of the two words πελα, the old Macedonian[2]

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[1] Lib. 8. cap. 1.

Πελασγος δε βασιλευσας. τουτο μεν ποιησασθαι καλυβας επενοηκεν, ὡς μη ῥιγουντες και υεσθαι τους ανθρωπους.

[2] See the Macedonian Glossary at the end of Steph. Thes.

name for a stone, and ασκειν, to work, to dress with care. These two words would form Πελασκος, which, by the slightest change in pronunciation, would become πελασγος. The common name for a Pelasgian fortress was Larissa. "Seventeen places (writes FYNES CLINTON) bearing this name may be traced, most of which, probably all, were founded by the Pelasgi." Perhaps it would have been better to have said "built, rather than founded, by the Pelasgi." For the general name Larissa, there ought to be as general a reason, and this the meaning of the term seems to supply λα-ας a stone, and ῥαιω ῥαισω to hammer or hew. Larissa would thus signify the fortress of "dressed stones," in opposition to the Cyclopian masses which were superseded by the Pelasgian style of building. This view of the subject is rendered more probable by the only account of Pelasgi which can be regarded as historical. According to HERODOTUS, in a well-known passage, certain Pelasgi had agreed, for a stipulated reward, to surround the Athenian Acropolis with a wall. They performed their bargain, but were afterwards expelled by the Athenians, first to Lemnos, and at a later period from Lemnos to the Hellespont. Now, it is from the language of these Hellespontian Pelasgi that HERODOTUS drew his conclusions as to the original dialect of the older Pelasgi. But PAUSANIAS[1] informs us, that he had ascertained that they were originally Siceli, and had passed over into Acarnania. The leaders of these erratic stone-masons are called by the same author Αγρολας and Υπερβιος:[2] literally, "stone-griper," and "very strong," names, which betray their fabulous origin and

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[1] Paus. lib. i. cap. 28.

Τῃ δε ακροπολει πλην όσον κιμων ῳκοδομησεν αυτησο Μιλτιαδος, περιβαλεῖν το λοιπον του τειχους Πελασγους. οικησαντας ποτε υπο την ακροπολιν φασι γαρ Αγρολαν και ὑπερβιον. πυνθανομενος δε οίτινες ησαν ουδεν αλλο εδυναμην μαθεῖν η Σικιλους το εξαρχης οντας Ακαρνανιαν μετοικησαι.

[2] Αγρα-captura, e. g. πυρ-αργα, a smith's forceps, and λα.ας, a stone. ὑπερ, exceedingly, and Βια, force.

which can no more be regarded as historical, than the Eucheir and Eugrammus, the two Corinthian artists, who are supposed to have attended Demaratus into Tuscany. As a proof that the more uncommon form of the Eugubian inscription is a remnant of the older Greek, I give the following specimen from the beginning of one of the tables, with the omission of the first line.

2. Famerias Pumplerias xii Atiiriate etre Atierate.
3. Clavernie etre Clavernie. Kuriate etre Kuriate.
4. Satane etre Satane. Pieriate etre Pierate. Talenas
5. Etre Talenate. Museiate etre Museiate. Iviscane
6. Etre Iviscane. Caselate etre Caselate. Tertie Caselate.

In this list of communities, or rather of colleges, we see distinctly the first and second of the Curiate or Curetes, the first and second of the Museiate or Muses, the first and second of the Pieriate or Pierides, of which were the constant companions of the Orphic priests. The Clavernie, the Satane, and the Talenate are also known to me, but would, for an explanation, require more time than can be given to them at present. The rest of the inscription is nothing more than a long repetition of the names and titles of *Juvis Pater*, upon the same plan as the Orphic indigitamenta. For a full explanation of each title, we must have recourse to the Sanscrit language.

Throwing aside, therefore, for the present the consideration of the Eugubian Tables, as not furnishing us with a trustworthy specimen of the anterior language, I proceed to call other witnesses, to whom we can safely appeal when a question is raised about the identity of language spoken in various countries, the names of those eternal monuments (eternal at least as far as human testimony can reach), the mountains, vales, and rivers, and even cities, which, in all cases where extermination has not preceded occupation, preserve the mark impressed upon them by the primitive occupiers of the soil.

Rivers.—And first of the rivers in Umbria, Venetia, or countries once occupied by the Umbri and their descendant tribes.

And in making these selections, it will be my object to give specimens, not to accumulate all that can be gathered together.

The most general Cumrian name for a stream is gwy or wy (Wye), wys (Ouse Isis), and wysk (Esk). The word gwy[1] itself has perished in this island, and is only found in names of rivers, such as Ed-wy, Myn-wy, Onwy, &c. In Italy we find the

Æsis.[2]
Bed-Esis.[3]
Ath-Esis.[4]
Æs-ar-us.[5]
Nat-Iso.[6]
Apr-Usa.[7]
Pad-Usa.[8]
Osa.[9]
Is.[10]
Aus-ar.[11]
Gal-Æsus.[12]
Ver-Esis, now L'osa in Latium.

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1 "This word and aw, are in the composition of a great many words which relate to fluidity."—Ow. Dict.

For the sake of the general reader if such should read so far, it should be mentioned that the Cumrian w is pronounced like the English oo in *good*, and that the y following it merely iotacizes the *oo*.

2 A river in Picenum, now called Fiumesino (Flumen Æsinum).

3 Not far from Bononia, now called Ronco.

4 The Adige. Observe the tendency of the v or w to become g, aev-um, Age, &c.

5 Now Isaro, near Crotona. Æs, water,—ar, mountain.

6 Now Natisone, not far from Aquileia.

7 Now Ausa, not far from Ariminum. Wys apparently compounded with aper.

8 A branch of the Po. Pad-wys, water of the *Pad*, called by Polybius Padoa. Now called the Po de Primano.

9 Osa, retains its name, a small river not far from Cosa.

10 Is, now Issa, between Petilia and Velia.

11 Ausar, now Serchio, near Pisæ.

12 The Tarentine stream, so celebrated by poets, now called Galeso. This,

The Cumrian name for a river, next in importance, is Avon, the same word as the Latin Amn-is. It must be remembered by etymologists, that the Latin M was not our strong labial consonant, but a vocal sound corresponding to our English *w* at the end of words. Without this explanation, it is impossible to account for its perishable nature in verse at the end of words followed by vowels. In those terms which the provincial Cumrians originally borrowed from their conquerors the Romans, and have since retained, the M, especially in the middle of words, becomes a *v*, thus—

Animal	Anivail.
Numerus	Niver.
Romani	Rhuveiniaid.

Thus also Amn-is, Avn, then Aven and Avon. In its simple form we find in Italy only two,—

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among many others, may be quoted as a proof of the indestructibility of names, except by the absolute extermination of the inhabitants. The Dorians of Tarentum gave it the name of Eurotas, after their own Laconian stream; and, in the days of POLYBIUS, it was, as we are informed by him, more generally termed the Eurotas. But as Greek influence declined, the original name prevailed, and the Galæsus has been immortalized in many a poet's verses. Assuredly, it is not owing to chance that the two words meant the same thing, the one in the Cumrian, the other in the Greek language. Ευ-ϱωτ-ας, the fair stream; Gâl-wys, fair water. In OWEN'S Dictionary, we find the following explanation: Gâl, "clear," "fair."

Avon reawg ai hynt hîr mewn gwaündîr gâl.
A river running with its course long in meadow-land fair.

Here, perhaps, I ought to add, that the Bradanus, the largest stream which falls into the Tarentine Gulf, still called the Bradano, has its representative in the Guildford river in Surrey, which bore both the generic name "Wey," and the specific one Brad-an. The specific name has, however, I understand, perished among the people, although retained in history.

[1] Aven-tia,

[2] Savo; to which should be added the [3] Ufens, and the Avens, a tributary of the Himella.

But in the names of places we find

Antemnæ and Antennæ,

Interamna repeatedly, and also a river Scultenna; whence it is credible, that the word, in its contracted form of An or Ann, or En, with the termination Us, is often to be found, as,

[4] Vom-an-us,

[5] Amas-en-us,

[6] Fibr-en-us, etc. etc.

The full meaning of this latter termination may be proved from the river Gari-en-us in Norfolk, now called the Yare, according to the common English custom of changing the older g into y.

To these should undoubtedly be added the celebrated

[7] Clit-umn-us,

formed on the same principle as the Gallic Garumna (Garw Avon, Rough River). To these generic names of rivers may be

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[1] Now L'Avenza, near Luna.

[2] Now Savone, in Campania, after the manner of the Gallic Savona now Saone; originally it was Avon Ara, the Slow River, (Vide *Cæsar's description of it, Lib. i. cap. 12.) in time the specific name perished, the generic remained.

[3] Now Aufente, in Latium.

[4] Now the Vomano in Picenum.

[5] In the Pomptine Marshes.

[6] A tributary of the Liris; evidently the Beaver (Fiber) Stream.

[7] Still called Clitunno. That the Clit is a separable prefix is apparent from its being joined with ernum also, a very common topographical affix, as Clit-ernum. The word is apparently synonymous with the Scottish and Welsh Clyde, which means in Cumrian "*warm*," clüd, a term equally applicable to the Italian and British rivers.

* Flumen est Arar, quod per fines Æduorum et Sequanorum in Rhodanum influit incredibili lenitate, ita ut oculis in utram partem fluat judicari non possit.—Ara means "slow."

added, a long list of Italian streams bearing the same names as rivers in our island. Such are the

[1] Duria Major,
Duria Minor,
Turia,
Sturia,
[2] Tinia,
[3] Tinna,
[4] The Umbro,
The Ambra,
[5] The Truentum,

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[1] Dwr, pronounced Door, is the common name among the Cumrian tribes for water, river, sea. The two Duriæ, tributaries of the upper Po, are still called Doria Baltea, and Doria Riparia. The Turia, a small tributary of the Tiber, about six miles from Rome, is not recognised by modern geographers. The Stura still retaining its ancient name, is also a tributary of the upper Po. In the Sussex Adur, and the Kentish Stour, we still retain the original appellations. There was also a river in Latium, called both Astura and Stura, now Store.

[2] Now Timia, in Umbria.

[3] Still called Tinna, in Picenum. Both synonymous with our Tyne, and with another Italian Tinea, a tributary of the Var.

[4] Cramer, Vol. i. p. 191, has the following observation:—" A short distance from the lake Prilis brings us to the mouth of the Ombrone, anciently Umbro, one of the most considerable rivers of Etruria. It is represented as navigable by Pliny, and its name, as the same writer observes, is indicative of the Umbri having once been in possession of Etruria." The strength of the argument is doubled, by the occurrence of a second Umbro in Etruria, not far from Arretium, called by Cramer, Ambra, but written Umbro in the Peutingerian tables. The British Humber, or Humyr, is evidently the same name, and equally conclusive of the presence of the Umbri in Britain.

[5] Truentum, now called Tronto, is in Picenum. The Traens, not far from Sybaris, bears the modern name of Trionto. They are both evidently the same word; the first being the Latinized, and the other the Hellenized form. The Greeks seem, as I shall have further occasion to remark, to have formed imaginary nominatives, in order to reduce the Italian names to the analogy of their own declensions. The

The Traens.
[1] The Liris.
The Farfarus.

To these leading streams many others might be added; such as

The Suinus, the Cornish Heyne,
Himella, pronounced Hivella the Bedfordshire Ivel,
Misus, and
Misio, the Cumberland Mite, &c.

But these are enough to prove that the same people gave the names to the Italian and British rivers. Even the name of the Roman river itself was, in old times, pure Cumrian, Alb, a hill, (*e.g.* Albyn, Scotland,) and ul, water, (see Owen's Dict.) whence the Latin, uligo, &c., so that the Alb-ul-a meant the mountain stream, or the highland river. The more common name Thybris, or Tybris (never Tiber), was, as we are informed by Varro, in ancient times, written Thebris or Tebris; but the same learned author tells us in another place, that the Sabines called mountains Tebæ, or (with the hard breathing) Thebæ. This, with the Greek ῥεω, to flow, will again give us a literal translation of the indigenous appellation.

Saxon name of the great river Trent was Tre-onta, as well as Trenta; a form which identifies the British with the two Italian streams, Tronto and Trionto. If these be compared with the Alpine Druentia, and the British Derventio, the modern Derwent, it will probably be inferred, that they were all originally the same word.

[1] Probably the same as the Gallic Liger (now Loire), especially if it be compared with the Saxon *Leire* (now Soar), the river of Leicester, originally Legerceaster, or Ligora-ceaster.

[2] Near Gabii, now Farfa, evidently the British Wharfe.

Varro, de Lingua Latina, liber V. cap. 6. "Sed de Tiberis nomine anceps historia, nam suum Etruria et Latium suum esse credit, quòd fuerunt qui ab Thebri vicino regulo Veientum dixerunt appellatum Thebrim. Sunt qui Tiberim priscum nomen Latinum Albulam vocitatum literis tradiderunt." Compare the same author de Re Rustica, Lib. iii. cap. 1. "Lingua prisca et in Græcia Æoleis Bœotii sine afflatu vocant collis *Tebas*; et in Sabinis, quo e Græcia venerunt Pelasgi, etiam nunc ita dicunt."

The Mountains.—Should a list of Italian mountains be placed before a classical scholar, he will find no difficulty in recognising many as referable to the Greek, and many more to the Latin language. To the Greek he would instantly refer

[1] Epopeus,	Prospect-hill,	επι-ωψ.
[2] Gaurus,	lofty,	Γαυρος, elatus.
[3] Pausilypus,	sorrow-ceasing,	παυω-λυπη.
[4] Physcus, &c.	swelling bladder-like,	φυσκη.

To the Latin he would with no less confidence refer

[5] Algidus,	Cold Hill.
[6] Argentarius,	Silver Do.
[7] Carbonarius,	Charcoal Do.
[8] Gravis,	Heavy Do.
[9] Suismontium,	Boar Do.
[10] Tetricus,	Craggy Do.
[11] Severus, &c.	Hard Do.

and innumerable others.

But many names will still remain, to the meaning of which

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[1] A high hill in Ischia so called, for the same reason as the Acro-Corinthus was called επωπη. See Stephan. de Urbibus, under the word.

[2] The height above the Arvnian lake.

[3] "It seems allowed that the Greek term Pausilypus was applied to the ridge of hills which separates the Bay of Naples from that of Pozzuoli, probably on account of its delightful situation and aspect."—Cramer's Italy, page 173.

[4] A hill near the Neaethus.

[5] In Latium.

[6] In Etruria.

[7] Near Marrubium.

[8] On the Via Sublacensis.

[9] In Liguria.

[10] Among the Sabini not far from the source of the Nar, Tetricus and Severus are supposed at present to be represented by the high peaks of the Sibilla, among the

the mere classical scholar possesses no key. We, like the learned VARRO, must say, " of such words the etymology is most difficult, for, in general, they have nothing in common with the Greek, and are vernacular terms, to the origin of which our records do not reach." Had he always remembered this truth, he would not have been guilty of such absurd mistakes as too often are visible in his great work. In explaining the meaning of some of the names of mountains in ancient Italy, it is not my intention to meddle with Alb, Tor, or any other such well known roots. The names selected are the following:

[1] { Peninus.
Ap-Peninus.
Le-Pinus. }

[2] Canterius.

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loftiest in the Apennine ridge. The Poets are very fond of alluding to the meaning of the name when they knew it. Hence

" Nam quæ *nivali* pascitur algido."—Hor. Od. lib. i. 21.

And,

" Qui Tetrici *horrentes* rupes montemque Severum."—Virg. Æn. vii. b. 714.

[1] " Et locus difficillimus est ἔτυμα, quod neque his fere societas cum Græca lingua, neque vernacula ea, quorum in partum memoria adfuerit nostra."—Lib. vi. de Lin. Sat. cap. 5.

The name Apenninus may safely be merged in the Cumrian form Penninus, an appellation by which both the god who was worshipped at the pass of the Great St Bernard (Alpes Penninæ), and the god whom the Umbrian shepherds adored in the vicinity of Iguvium, was called. The spot is marked in the Peutingerian Tables as the temple of Jupiter Penninus. The Cumrian Pen is the English head chief summit, and is still used as a common word. " Penin" is the capital of a pillar, and was doubtless applied to designate the highest peaks among the hills. I am inclined to reckon Le-Pinus as another form, especially as the town Pinna, in the high Apennines, is now called Penne.

[2] VARRO, in talking of *Pecudes*, writes (De Re Rust. lib. 2. cap. 1.), " Annon in mari terraque ab his regionum notæ? in mari, quod nominarunt a capris Ae-

[1] Ciminus.
Si-Cimina.
[2] Cumerium Promontorium.

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gæum pelagus, ad Syriam montem Taurum, in Sabinis Canterium montem." "Have not the characteristics of regions, both by sea and land, been derived from them? was not the Ægean Sea derived from Αιγες? the mountain near Syria from Ταυρος? and that in the Sabini, from Cantherium, 'a beast of burden?'" Now, if it can be shown that the learned antiquary is wrong in the first, it may be inferred that he may be also mistaken with respect to the last. Now from Αισσω, "I rush violently, I spring," came Αιξ, "a goat, a springing animal," and αιξ, "impetuosity." From this second Αιξ came Αιγις, "an impetuous squall of wind," as may be seen in the compound Καταιγις, thus defined by Aristotle, "Των γεμην βιαιων πνευματων καταιγις μεν εστι πνευμα ανωθεν τυπτον εξαιφνης." Of the violent winds, the καταιγις is "a blast that suddenly strikes from above." The Ægean is therefore not the Goat Sea, but the Squally Sea, a name which, all who know it say, it well deserves. Hence also in Homer, Αιγιοχος, the epithet of Homer's Jupiter, the Storm-restrainer, not the Goat-skin-holder, as later commentators interpreted the word. Mount Taurus was not named after "the Bull," but from *Tor*, one of the most universally diffused names for a bold and aspiring peak. Canterius Mons, also, has nothing to do with a "gelding," but was named, on the same principle as many other hills, from "Can," or Canus, white, and Terra (originally Tera), land; or, in the Cumrian form, from the same words, "Cantir," from "Can," white, and Tîr, land.

[1] Mons Ciminus was a long and lofty ridge in Etruria, the passage of which (at least if we credit the annals of the Fabian family) formed an era in the history of early Rome. I have already observed that the M of the Romans was more of a vocal than consonantal letter. Hence, this same name of a hill, when applied to the range of hills in the south-east of France, and written Κεμενος by the Greeks, and Cebenna by the Latins, still keeps its original sound in the French *Cevennes*. But in the language of the modern Cumri (see Owen's Dict.), Cevyn (pronounced Ceven, pl. Cevenau), is "a ridge, as Cevyn o dîr, a ridge of land, a long extended mountain." From the same root comes Ceba, now Ceva, a town and district of Piedmont; the Cevin or Chevin Hills, in Yorkshire; and the Cheviot Hills, called formerly Chevy, the well-known ridge between Scotland and England. To these may be added the ancient Si-Cimino, a mountain of Liguria.

[2] Cumerium Promontorium, now Monte Comero, a bold headland in Picenum, still commemorating the possession of that district by the Cumri, under their true name, and a record as lasting as the Mont-Gomeri, in France, and the Comri Isles, in the Frith of Clyde, probably in all cases the last retreat of the bravest spirits of a vanquished district.

[1] Cunarus.

In entering upon the question of the names of places, it should be remembered that many of the ancient names of towns, cities, and fortresses, are merely the names of the people or tribes who originally possessed them; and that, first, the district, secondly, the city, is nothing more than the appellation of the nation. In the following remarks, I shall, however, as much as possible, avoid words of this class, and confine the examination to such as seem to have a local meaning, borrowed from some accident, whether the work of man or of nature. Again, I have to request a fair hearing, and to beseech the classical scholar especially, for a short time to commit himself on subjects unknown to him to my guidance, and to assure him, that I will not knowingly abuse his confidence in the slightest degree. But as he, in reading a list of the names of Italian cities, would, without scruple, infer, without historical evidence, that by the Greeks were given the following names:

Neapolis.	Heraclea.
Dicaeopolis.	Poseidonium.
Metapontum.	Rhegium, &c. &c.

And by the Latins after the formation of their language, the following:

Concordia.	Vicentia.
Consentia.	Foro-Juliensis.
Valentia.	Faventia, &c. &c.

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[1] Cunarus. This name is supposed to have been attached to the highest peak of the Apennines, the modern Monte Corno, or il Gran Sasso d'Italia. And the etymology strongly confirms the conclusions of comparative geographers, for Cun-Ar means the chief hill. (See OWEN's Dictionary, under the words.) Cûn, "a leader or chief." Ar-an, "a high place, alp. It is the name of several of the highest mountains in Britain." Ar itself is not used as a noun, but as the preposition "above, upon," is in constant use.

It would be easy to extend this examination with the same success to Garganus, Gurgures, Gurgunium, Massicus, and many others. But, as I only wish to give a specimen, the above may suffice.

So ought he to allow me to select those names which are as plain to me as the Latin and Greek are to him, and refer them to the people by whom they were originally given. But it will be better, in the first place, to give a list of those accidents, the work either of man or of nature, from which Cumrian local names are more commonly borrowed. These I copy alphabetically, and attach to each word the corresponding definition from "Owen's Dictionary."

Bala, "an eruption, a discharge of a lake. Bala Lin, the outlet of a lake. Hence it is the name of many places in Wales, Ireland, and Scotland."

Ban, "prominence. It is the appellative of several mountains, as Banuçdeni, and Bannau (pronounced Bannae), the beacons of Breconshire, and the highlands of Tal-y-van in Glamorganshire."

Ca, "a keep or hold."

Cae, "an inclosure, a hedge, and metaphorically a field."

Caer, "a wall or mound of defence; the walls of a city, a castle or fortress. Caer-y-Vunwent, the churchyard wall."

Cor, "a round or circle, a close." Bangor, "the principal row or circle." It was a name given to some of our most noted monasteries, one in Flintshire, one in Carnarvonshire, one in Ireland, and another in Belleisle off the coast of Brittany.

Clâs, "a space of ground inclosed, a region, a country, an island, old name of Britain, Clas Merdin," Bü Cuvranc.

"Cuvrwng gliw Powis, a clâs Gwined."

"There was contention between the chief of Powys and the region of Gwined."

Din, "a fortified hill or mount, a camp or fort. It forms the names of a great number of places in those countries which were inhabited by the Cumri. Hence the *Dunum, Dinum, Dinium* of the Romans."

Din-as, "a fortress or fortified town, a city." It forms the names of several places in Wales, as Brin-dinas, &c.

Gwâl, " a cultivated country. The Cymry appropriated this name to regions that were cultivated, and had a fixed residency, opposed to the wilds, or unsettled residences."

Gwely, " a bed or couch, also a family or tribe, a family district."

Gwent, " a fair or open region, a champaign. It is a name now confined to nearly all Monmouthshire, but which anciently comprehended parts of the counties of Gloucester aud Hereford, being the district of which Caer-went, or the ' Venta Silurum' was the capital."

Gwern, " a swamp, a bog, a meadow, also alder trees." He might have added that it was a generic name for trees, *e. g.* Gwernen, a mast of a tree.

Gweridre, " Cultivated land, an inhabited region, a country ;" apparently from Gwerin " people," tre " habitation."

Gwys, Gwês, " a people, a peopled region, a country."

Ty, " a house."

Trev, " a dwelling-place, a hamlet, a township, a town. It forms the name of many places, as Trev-Ithel " Ithelham," Treva " Hamburgh." Treva g tulwith Têg, a fairy circle ; Treva o îd, a thrave of corn."

First, Under Ca or Cae, we find, among others, the following :—

[1] { Ca-latia campana.
Ca-latia samnis. }

Ca-letra, in Etruria.

[2] Ca-Mars or Clusium, in Etruria.

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[1] A comparison of these two names with Col-Latium, Pa-Latium, &c. will shew that *Ca* is a separable prefix. Compare also the rare coin, published by SESTINI, and bearing the inscription PALACIVM, ascribed to the Sabine Palantium, from which, according to VARRO (Ling. Lat. IV.) the Palatine hill derived its name.

[2] For the same reason compare Mars Martis, the God, and the river Marta in Etruria ; also Ma-mertium.

[1] Ca-Mere, near the Crathis, a field.
[2] Ca-Meria, in Latium.
[3] Ca-Merinum, in Umbria.
Ca-merte, in Umbria.
[4] Ca-Silum.
[4] Ca-Silinum.

Under Caer, or Car, pl. Ceiræ, written Caerau, we find—

[5] Cære.
Car-eia, on the Via Clodia, in Etruria.
Car-rea, near Turin on the Po; still called Chieri.
Car-sula, in Umbria, } Both towns are now called
Car-seoli, in the Sabini, } Carsoli.
Car-istum, in Liguria; now Carosio.

Car-meianus Ager, in Apulia; naturally supposed to imply a *Carmeia*.

Car-Minianum, in Apulia; still called Car-miano.
Car-bina, in Apulia; now Carovigno
Car-aceni, a division of the Samnites.
Cere-atæ, in Latium.
Cer-fennia, in the Sabini.
[6] A-cerræ, in Campania; still so called.
Lu-ceria, in Apulia; still called Lucera.
[7] Nu-ceria Alfaterna, in Campania; still Nocera.

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[1] *Purus ager*, Ca-meren incola turba vocat.—*Ov. Fast.* 581.

[2] Compare A-meria in Umbria.

[3] Compare Merinum, near Mons Gargânus, in Apulia.

[4] Compare with the English Sil-Chester, Caer-silin, Sil-innæ isles, Carsula, an island of the western coast of Britain, mentioned by the geographer of Ravenna.

[5] Called by the Greeks Agylla. Caerè is to this day a common name in Wales.

[6] Another Acerræ in Cisalpine Gaul is now called Gherra.

[7] Nu-ceria means New town; an old town in Welsh would be Hengaer, from hên, sen-ex. Nola, in an inscription given by Lanzi, was Nu-flan, where the F supplies the place of the aspirated l, New-Lan, or in modern Welsh, Llan-Newyd. Thus also Latius ager is in an inscription ager Tlatie on the same principle.

Nu-ceria Camellana, also Nocera, in Umbria.

Nu-ceria Cisalpina, on the right bank of the lower Po.

Nu-Cria, also Nocera, a supposed town of the Brutii.

Many more might be added without adding, however, to the strength of the induction, although I may have to add others under some other heading. It is to be remarked that the Greek legends on certain coins give us Νουκριναν; and an Oscan inscription, quoted by LANZI,[1] has the word Nue-krinûm. Apparently Nu is the same word as the Latin Nov-us, Cumrian New-îd, or English *new*.

Under Cor we have the following names :—

Cora, in Latium ; still Cora.

Corbio, also in Latium.

Corfinium, capital of the Peligni.

Cor-ioli, in Latium.

[2] Cormones, still Cormons, in Venetia.

[3] Cor-sula, in the Sabini.

Cures, now Correse, also in the Sabini.

Curia, in Cisalpine Gaul, now Coire.

If we compare these with the Coria Ottadinorum in North Britain, with Corinium in South Britain, with the Bangor of the Cumri, it will be difficult to escape the conviction that they all represent the same original word.

Under Clas we have one :—

Clas-tidium on the Po,

which, compared with At-tidium, proves that both Clas and At were separable prefixes.

Under Din we have—

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[1] Vol. i. p. 209.

[2] Gle-Mona in the same vicinity proves that Cor-Mones is made up of Cor and Mona.

[3] Compare this word with Car-sula, and Carseoli, and Ca-silum, and with Sul-Môn-e

Vin-Dinum in Umbria.

Ve-Dinum in Venetia; now Udine.

And with the D hardened, as in the British Tin-tagel—

An-tinum among the Marsi.

A-tina of the Lucanians; now Atena.

A-tina of the Volscians; now Atino.

Al-tinum in Venetia; still Altino.

Re-tina in Campania.

Ma-tinum, which gave its name to the mountain.

Ses-tinum in Umbria; now Ses-tino.

Gwâl and Gwely.

But here I must remark, that the Cumri prefix the letter G to all the radical words which in Latin commence with V, on the same principle which induces the French to write Walter Gualtier. That this was the original pronunciation, in Italy also, of Latin words commencing with V, is clear from the resuscitation of the pronunciation in such words as the following:—

Guado,	A ford,	Vadum.
Guagina, or Guaina	A sheath,	Vagina, Cum, Gwain.
Guastare,	To waste,	Vastare.

This was the true digamma, which has so much puzzled scholars, merely classical. The more refined age of Greece abolished almost all the hard guttural and labial breathings, and wrote:

οινος,	Vinum,	Gwin,	Wine.
εκυρος,	Socer,	Chwegir.	

In the simple expression, "Woe's me," we have in the different languages a complete scale of the original form and subsequent disappearance of this digamma.

Cumrian,	Gwai vi.
Latin,	Vae-mihi.
Greek,	οι-μοι.
Hellenistic form,	ουαι-μοι.

Nor is it less instructive to see it reappear in its original shape in the the modern Italian, the language of the common people :—

Italian, Guai a me.

With this observation, and the remark that gwâl means *low* also, I refer to it and gwely the following words :—

Val-lia, in Etruria, a river.
Vel-eia, near Placentia.
Vel-itræ in Latium ; now Velletri.
Fel-sina, afterwards Bononia ; now Bologna.
Vola-Terra, now Volterra, in Etruria.
Vul-sinii, or Volsinii, in Etruria ; now Bolsena.
Fala-crinæ in the Sabini.
Fal-erii in Picenum ; now Faleroni.
Fal-isci, the people of Falerii.
Fal-ernus ager, in Campania.
Fel-tria in Venetia ; now Feltri.
Fal-erii in Etruria ; now Falari.

It may excite some surprise to see Vola-terra and Vol-sinii in this list ; but on comparing Vol-sinii and Fel-sina with Vel-athri, the coin-name of Vola-terra, it will be immediately seen that Vel, or in a harsher form Fel, is the root of all the prefixes. Should it be asked what sina is, it may be suggested that it is probably a portion of Ra-sena, the name of the Tuscans[1] retained pure in Sena in Etruria, and in Sena on the Adriatic ; not called after the Senones. The same word is visible in Por-sena, which, in the Cumrian language, means, Por, the Lord, of Sena or *the* Sena.

"Gwent" appears to be a favourite name among the Cumrian tribes, and its appearance either in its simple or compound

[1] One paper has been already read by me on the Tuscan language, but will not be published till the second is finished. It certainly is not Greek, and the Cumrian words in it are not numerous, not more, indeed, than a dominant tribe might be supposed to have borrowed from their vanquished subjects.

form, may be regarded as a certain proof of the presence of the race. In ancient Britain, we had Venta Belgarum, Winchester, Venta Silurum Caerwent, Venta Icenorum Caister, near Norwich. In Italy we have—

Arx [1] Car-ventana, in Latium;
[2] Tre-vent-um, now Trivento, in Samnium;
[3] Bene-vent-um;
Cas-ventum, in Umbria;
Vi-ventum, in Umbria;
Ver-entum, in Etruria, now Varentano,
Vis-entium, do. do. Bisentino.

This list might be increased indefinitely, but it will not be amiss to add—

No-mentum an ancient city of Latium,

and to compare it with Nu-mantia,[4] the Novant æ, and Tri-nobantes, of Britain: As a prefix, it appears without the T, in the names

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[1] Supposed to have been situated on one of the summits of Algidus. The adjective Carventana, necessarily implies the existence at some period of a Caer-went, or Car-venta, in the vicinity.

[2] On coins this name, in Oscan characters, is Trebint-im; similar names, both in Wales and Cornwall, will occur to persons acquainted with the locality.

[3] I place Bene-vent-um under this head without scruple, without paying deference to the story mentioned by Livy, Pliny, and Festus, that before it became a Roman colony it was called Male-vent-um; because we have coins of this city bearing the Oscan inscription "BENEVENTOD," a proof that such was its name before it received a colony from Rome, and because there was another Bene-ventum between Brixa and Verona, in Cisalpine Gaul, and Bennaventa* in Britain. I may also add, that I look upon such words as Tarentum, &c. as pure Italian, and that it was the Greeks who formed imaginary nominatives, like Taras, &c. to suit their own fables. Pausanias informs us that Tarentum was "a very considerable and opulent town before the arrival of Phalanthus and his Spartans."

[4] The Celtiberi were undoubtedly Cumri; Diodorus Siculus even calls them by the name, *των δε κιμβρων οι Λουσιτανοι*, &c.; but I have nothing to do with Spain at present.

* Now Daventry.

of places commencing with the separable word Ven—Venusia and Venusini, for example. It is to be observed, that the men of Gwent are called Cumraicè Gwenhwyson, which a Roman would write Venusones, or perhaps Venusini.

[1] Caradawg o Went ai Wenhwyson;
"Caradog of Gwent and his Venusini."

Gwern, a swamp or wood, is one of the commonest sources of Cumrian names—Penwern Tynwern, Werndü, &c. It seems to have been no less in use in ancient Italy; witness

Amit-ern-um, an ancient Sabine town;
At-ern-um, in Picenum, on the Aternus;
Av-ern-us, the lake. Aw, water; Gwern, wood;
Clat-ern-a, near Bononia;
[2] Cli-ternum, in the Sabine territory;
Clit-ernia, in Apulia;
Lit-ernum, in Campania;
Leut-erni, in Apulia;

and many others. To the same source should be referred

[3] Pri-vernum, in Latium, now Piperno;
Cla-vernia, in Umbria, now Chiaserna;
Pri-fernum, in the Sabine territory;
Ti-fernum, in Umbria, on the Metaurus;
Ti-fernum, in Umbria, on the Tiber;
Ti-fernum, in Samnium;
Ti-fernus, the adjacent river, now Biferno;
Ti-fernus, the adjacent mountain.

Gwery-dre. The root appears to be Gwyr Viri, warriors,

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[1] See OWEN's Dict. under Gwenhwyson.

[2] It is curious that the hundreds so well known under the name of Chiltern, were in the Saxon period written "Clitern." The word is "Clûd-wern," warm-wood.

[3] From these examples we see that the Cumrian Gw, of the radical word Gwern, became V, as in Privernum, F, as in Prifernum, or totally disappeared, as in Aternum. Priv-wern means primitive or chief wood, Ti-fernum Tŷ-wern, wood house.

written Gwer also, as in Gwersyll, a camp, Gwer, warriors, Syll, a seat or *sella*. Under this head we have the places often called after the people; such are

[1] Ver-ona, on the Athesis;
[2] Ver-entum, before mentioned under Gwent;
[3] Ver-cellæ, now Vercelli, in Gallia Transpadana;
[4] Ver-tinæ, among the Brutii, now Verzine;
[5] Ver-ulæ, in Latium, now Veroli.

And with F instead of V,—

Fer-entinum, in Etruria;
Fer-entinum, in Latium.

Gwys or Gwês. Apparently a fertile source of names, hence,

Ves-bula, in the Sabine country;
Ves-cia, in Latium;
Ves-cellium, in Samnium;
Ves-eris, in Campania, a town or river;
Ves-idia, a small river in Etruria;
Ves-ionica, in Umbria;
[6] Ves-tini, the well-known people;
[7] Ves-ul-us, the Alpine hill Monte Viso.

And with F instead of V,—

[8] Fes-cennium;
Fæs-ulæ, in Etruria.

Ty, or Tî, or Te. Under this head we find,—

Te-anum, in Apulia;
[9] Te-anum, in Campania, now Teano;

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[1] Avon or Awn, a river. [2] Gwent, Gwyr Gwent.

[3] Gelli, Groves, Welsh name of the "Hay," the town.

[4] Ul, water. Compare this Ver with Vero-lamium, Vero-metæ, &c. in Britain.

[5] Din the Town.

[6] Compare the Welsh Vale, Festini-og in north Wales.

[7] The district of water.

[8] Cennium, Cevenæ, ridges, range of hills.

[9] There still remain many coins of this town bearing the epigraph TIANO.

[1] Te-ate, in the Sabine district, now Chieti;
[2] Ti-cinum;
Ti-fernum, three as before described;
[3] Ti-ora Matiena, in the Sabine Hills, near the source of the Velinus.

It would appear that this prefix was once Tegi or Tig (cognate with Τεγος and Tectum) from combining the following with the above:—

Tegi-anum in Lucania, on the Tanager, which now, along with its valley, is called Diano, an evident corruption of Tegianum, and closely allied to the Tiano of the coins.

Tig-ulia in Liguria, now Tre-gosa, a word in which Tre- has usurped the place of its synonym Tig or Ty, and gwys that of ul.

Trev and Treva, pronounced Tre, plur. Trevi, under this common prefix of the Cumri, we have

Treva in the Sabini, now Trevi;
Trevia in Umbria, now Trevi;
Treia in Picenum;
[4] Tre-bula, Balinea, in Campania;
Tre-bula, Mutusca in the Sabini;

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[1] This word, compared with Re-ate, shows that the Te was a separable prefix; and the several coins bearing the inscription TIATI, proves that its primitive form was Ti.

[2] Now Pavia, probably gave its name to the Ticinus river, the Tessino.

[3] The meaning of Ti-ora, "Ty. oera," is "coldest house," a fit name for its situation.

[4] These names, compared with Ves-bula, will shew that Tre is a separable prefix, and if LANZI (page 508, vol. ii.), is right in affirming, on the faith of inscriptions, that the citizens of this town were called TREBALAces, as the Brutii are called by ENNIUS Brutaces, it will necessarily follow that the name of the city was originally Tre-bala (see Bala in the list of roots). The epithet Balinea, is confirmative of this explanation.

Tre-bula, Suffenas in the Sabini;
Tre-ventum, before mentioned.

To these may be added,

Tar-visum in Venetia, now Tre-viso;
[1] Ter-geste in Venetia, now Trieste;
Ter-ioli in Rhætia, now the Tyrol.

This induction might have been increased, by examining the names in which the following words are component parts:—

Man, a place;
Ban, a hill;
Gwy, water;
Glan, the brink, or side of a river, lake, &c.;
Bre, or Bryn, or Bren, a bræ', or brow of a hill, and others.

But sufficient, I suppose, has been adduced to shew that the Cumrian language is the only key to a right understanding of local names in Italy, and to induce the learned reader to allow, that the same tongue which entered so deeply into the composition of the names of places, rivers, and mountains, might also have left strong proofs of its existence in the more artificial language which sprung up when the civilization of Magna Græcia and Etruria sunk before the energy and hardihood of the mountain tribes; and the victors of Septimontium, as they advanced in power and a knowledge of the arts and sciences, had to extend their vernacular vocabulary.

But before I enter upon an examination of this proof, it will be right to declare, that the Gael as well as the Cumro can claim in common some of the words which have been placed before the reader, and more to which his claim appears exclusive. But this, if granted, does not affect the value of the principle. Let the Gael and the Cumro decide on their respective rights. It is something gained to have expelled the encroaching Teutons. The

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[1] Este is a common root in the names of places. See At-Este, Præn-Este, Greek, ἀστυ.

field of conflict is thus narrowed, and the dispute will be the sooner decided in proportion to the closeness of the lists. If there be any stout-hearted Teuton, who will persist in still claiming these nations as of his immediate race, let him go over the ground over which I have travelled, and select the Bergs, the Hams, the Forts, the Bachs, the Dens, which must be still found there, if those hills were peopled by his race. Tor, Wick, and even Briga, are common to both races, and would, if brought forward, prove nothing. One thiug, also, he must not do; he must not bring names from Bohemia, nor from the right bank of the Danube, as far as Illyricum, nor from modern Hungary, nor from the vicinity of the Cimbric Chersonese, as all these countries were once held by people of the Cumrian race.

If then the Cumri and the Umbri were the same people, it may be asked, what became of the letter C in the name of the latter? I cannot tell; but I know that the initial κ or γ was very loose not only in the Greek, where we have Γαια and αια for the earth κιων, and ιων for going, κιχανω and ικανω for venio and invenio, ἐλευθω and κελευθος for go and gait; but also among the Latins, where we have cocles and ocles (see Varo under the word) for a one-eyed man, and Caulon and Aulon, significant of the same thing in Magna Græcia. Among the Cumri the guttural is still looser, as they drop it to express a difference even of gender, as goleu, light, ei oleu, his light. But the Latins probably dropped it, because the Greeks of the historical era wrote ομβρικοι and ομβροι, whence fashion prevailed, and the word became disguised. In older times, the Greeks themselves wrote it κιμμεριοι, as we find in the Odyssey. The mythos, by which the author of that poem places them in utter darkness, only tells us that they were placed in what was to him the extreme west. For as the east, ευρος, was the house of light εως, so Ζεφυρος, the west, was the house of misty darkness (Ζοφος ηεροεις). Ephorus,[1] therefore, following

[1] Quoted by Strabo. Lib. v. page 244.

the older commentators, and the tradition of the inhabitants, places the Cimmerians near the Lake Avernus; others placed them a day's sail to the west of Circeii, in the very country which the Umbri of history are said to have occupied.

But in addition to the retention of the C in this ancient Cumerium promontorium, the modern Monte Comaro, there still remains a convincing proof that the C was attached to the name even within the year 585 A. C. There is in the Capitoline marbles the following notice:[2] "Quintus Aufidius, Banker, at the sign of the Cimbric shield, absconded as he was deep in debt." Now as this authentic record, more authentic as many believe than manuscripts, mentions a Cimbric shield sixty-seven years before the well-known invasion of the Cimbri and Teutones, the shield must have been named after some Cimbri other than those of Jutland, whose name the Romans never heard, before their passage of the Rhine, and their devastations in Gaul and Spain. "The arms of the Cimbri were first heard of in the 640th year of our city," writes TACITUS.[3] This Cimbricum scutum could not therefore have derived its name from them. Probably it drew its origin from some Cumrian tribe, among the Gallic invaders of Italy, if not from the Umbrians themselves. I have omitted many coincidences of note between the ancient names of places in Italy and in Britain, but I find it impossible to omit the following—

[4] Mevania, in Umbria, Mevania, Anglesea.

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[1] The whole of the inscription applicable to the case is the following:—

"Quintus. Aufidius. Mensarius. Tabernæ. Argentariæ. Ad. scutum. Cimbricum. Cum. Magna. Vi. Æris. Cessit. Foro. retractus. ex. itinere. causam. dixit." I owe this quotation to THIERRY's history of the Gauls, vol. i. page 46.

[2] Taciti Germ.

Sexcentessimum et quadragessimum annum urbs nostra agebat quum primum Cimbrorum arma audita sunt.

[4] Mevania, a city in a plain (" Projecta in campis," or, as LUCAN describes it, " ubi se Mevania campis explicat,") watered by the sacred river Clitumnus. Now,

[1] Spina, on the Adriatic,	Spinæ, in Berkshire.
[2] Ceneta in Venetia,	Cunetio, in Wiltshire.
[3] The Morgetes,	Morgant, Morgan-wg Gla-Morgan.

Having thus, as I hope, proved the close connexion between the Italian tribes of Umbrian race and origin with our own Cumri, it remains that I should conclude with shewing from the still existing language of the latter that the ancient Umbrian entered into the composition of the former.

And first, let me premise that (as it has been lately shewn, and is now acknowledged by all linguists) two languages may have a common vocabulary but different grammars. The Latin language, whether from Pelasgic or Achæan influence, adopted at an early period the Hellenic grammar, and under the skilful hands of the bilingual ENNIUS, became that polished interpreter of thought, which yields in regularity and majesty to the Greek alone. The Cumri either retained, which is more probable, a

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Mai, Cumricè, is a plain, and Man (in composition Van), a place, hence Meivan or Maivan, means " a city of the plain." It is from the Saxons that we learn that Anglesea also bore this name; they called it Mon-ege, *i. e.* " Mona isle," or Man. Cyn, *i. e*, " Chief spot," from its holiness, and, as it appears, Meivan or Mevania, from its champaign character.

[1] Spina, supposed to have been a Pelasgic city, was placed at the south-eastern mouth of the Po. Spina on the river Kennett, is still called Speen. As the Pelasgi gave the name to the one city, it might be inferred that they gave it also to the other; but it is far more probable that the same primitive race which named Spina before the visit of the Pelasgi, gave the same name to the British city.

[2] These two words, together with the name of the river Cunetio, may serve to fix the original position of the Cunetes of HERODOTUS (iv. cap. 49.), " the Danube flows through all Europe, beginning from the Celtæ, who, after the Cunetes, are the most western inhabitants of Europe."

[3] These Morgetes, called also Morgentes, as may be inferred from their city, Μοργεντιον, at the mouth of the Symaithus, in Sicily, were one of the earliest Italian tribes, so denominated apparently from their position on the sea coast. Môr, sea, Gant, brink or side, compare Morgan-wg, in South Wales, Vor-ganium or Morganium in Aremorica. The Samnite Murgantia was, according to the coins, Murtantia.

still more ancient, or invented a grammar, now peculiar to themselves. This, although it be simple and scientific in the highest degree, is so completely at variance with all the other grammars of the civilized world that scholars who have to acquire it late in life feel the strongest repugnance to its forms and principles, and are tempted to regard a language more fixed and unchangeable in its principles than any other existing, as more slippery and grasp-escaping than the Proteus of the Grecian mythologists.

To persons who have formed their conception of verbal roots from the unvarying character of those important elements in the Greek, Latin, and modern languages, it appears strange that these radicals themselves should be continually shifting in form. Let such, however, be assurred that the system is as fixed and regular as that which from

τυπτω derives *τυψω τετυφα τετυμμαι, ετυφθην τυφθησομαι.*

Now, although the grammars of the Latin and Cumrian lan-

[1] Take, for example, the following examples :—

Pen, a head.—Plur. Pennae.	Trôd, a foot.—Plur. Traed.
Ei-Ben, his head.	Ei-Drôd, his foot.
Ei-Phen, her head.	Ei-Thrôd, her foot.
Fy-Mhen, my head.	Fy-Nhrod, my foot.
Gavel, a hold.—Plur. Gaveilae.	Cam, a step.—Cammae, steps.
Ei-Avel, his hold.	Ei-Gam, his step.
Ei-Gavel, her hold.	Ei-Cham, her step.
Fg-Nghavel, my hold.	Fy-Ngham, my step.
Bwyd, food.	Carü, to love.
Ei-Fwyd, his food.	Ei-Garü, to love him.
Ei-Bwyd, her food.	Ei-Charü, to love her.
Fy-Mwyd, my food.	Im-Carü, to love me.

These and such changes never for a moment cause a scholar to confound two radicals, which change only on certain conditions and fixed principles. But when a language formed on such a principle breaks up, and a new one is reconstructed from its fragments, and perhaps that of others, we may expect to see such grammatical forms figuring in the new language as independent radicals; thus, under one of the above described forms, we have three English words :—

Bwyd, bait, either for a fish or horse.
Ei-Fwyd, his food.
Fy-Mwyd, my meat.

guages thus entirely disagree, there is a wonderful similarity in their vocabulary, a similarity by no means to be accounted for by a supposed common descent from a Caucasian race, but approaching far nearer than the old Teutonic, or as it is called Moeso-Gothic tongue does to the Homeric language. "GIRALDUS CAMBREUSIS,[1] both a Cumrian and classical scholar, remarked this similarity nearly 600 years ago." It is to be remarked that almost *all the words* of the British tongue agree either with the Greek or Latin. It is this strong similarity of features between their own language and those of Greece and Italy that has induced so many of my countrymen to claim for it the honour of being the mother-tongue of all, and to scorn all examination which did not commence with this confession. Even the late learned Dr OWEN PUGH has in his dictionary, by arbitrarily, selecting certain syllables as the roots of all Cumrian words done much to foster this overweening conceit. The system was carried to its extreme point of absurdity by the Rev. EDWARD DAVIES, who, by the help of such syllables, expected to unravel the mysteries of all languages. This failure has, I hope, paved the way for the more sober consideration of the question, which if worked out fairly will, in my opinion, establish the claim of the Cumrian tongue, if not to be the mother of all tongues, at least to be a valuable branch of the Caucasian tree of languages. Now, had the two races, the Roman and Cumrian, remained always separate, a comparative etymology would have been an easy task, for no more would be necessary than to put the similar roots, having the same meaning, side by side. But unfortunately for the scholar who undertakes to prove the question, the Romans were in this island 400 years, colonized it partly, and partly gave it their own form of civilization. As before mentioned, the inhabitants adopted with avidity the Roman dress, language, and literature. That language, must, therefore, be sup-

[1] Notandum etiam, quod verba linguæ Brittanicæ omnia fere vel græco conveniunt vel Latino." Cambriæ Descriptio.

posed to have entered deeply into the composition of the present Cumrian tongue. The sceptical examiner may, therefore, reasonably object that any similarity between the two languages might have originated in the adoption of that of Rome by the British provincials.[1] In answer to this I refer, in the first place, to Lloyd's reasoning, quoted in the note. In the second place, to the fact, that Wales and Cornwall do not appear to have been occupied like the rest of England by the Romans.[2] The main roads of the Antonine itinerary do not pass to the westward of a line drawn from Eggerton (Muridunum) to Abergavenny (Gobannium) thence from a spot near Draiton (perhaps Mediolanium) to Chester (Deva). The main road connecting Isca Silurum (Caerleon on Usk), the station of the legion 2nda Augusta, with

[1] Humphrey Lhwyd (Humphrey Lloyd), to whom the original inhabitants of Great Britain, Ireland, and France, owe so much, states the question as plainly as the prejudices of the day would allow him. "Additions to Merionethshire in Camden." "It seems to me the word Torques was Celtic before it was Roman. For although I acknowledge it to be derived from Torqueo, yet *we* also have the verb Torchi in the same sense; and seeing that both the British words Torch and Torchi are in all appearance derived from the common word Troi, *i. e.* to turn; and also that grammarians know not well whence to derive Torqueo, I know not but we may find the origin of it in the British Torch. Nor ought any one to think it absurd that I thus endeavour to derive Latin words from the Welsh, for there are hundreds of words in that language that agree in sound and signification with the Latin, which yet could not be borrowed from the Romans, because the Irish retain the same, who must have been a colony of the Britons long before the Roman conquest; and also that the Welsh or British is one dialect of the old Celtic, whence, as the best critics allow, the Roman tongue borrowed several words, and I presume, by the help of the Irish, which was never altered by a Roman conquest, it might be traced much farther. For instance, we must acknowledge these British words, Tîr, Awyr, Môr, Avon, &c. to have one common origin with those of the same signification in the Latin, Terra, Aer, Mare, Amnis; but seeing the Irish also have them, it is evident they were not left here by the Romans, and I think it no absurdity to suppose them used in these islands before Rome was built."

[2] With the exception of the road along the sea-shore from Chester to Carnarvon, which appears to have been merely the road to Ireland.

Deva (Chester), the station of the twentieth legion, would to this day almost serve as a boundary between the English and Welsh counties. Add to this, that the inscriptions of Roman workmanship to the west of this line are too trifling to allow us to suppose that any long occupation of the country could have taken place. Were it otherwise it would be difficult to account for the stationary position of two out of the three legions by which Britain was garrisoned, the one at Caerleon, the other at Chester. The Roman legions in the provinces, liable to hostile attacks, had their head-quarters almost invariably on the frontiers. It is, therefore, almost impossible to account for the selection of two such points as these, without the supposition that the legions were placed there to guard against the attacks of a race which, if vanquished, was nevertheless unbroken in spirit. Even the very grammar of the language and the traditions of better times long anterior to the Roman invasion, are proofs that as far as the western portions of Great Britain are concerned, the amalgamation with Rome was never completed. Still, however, the long residence of the Romans in the island, with the known influence always produced by such a state of things, renders every statement grounded on the similarity alone of the languages of the two races, the conquered and the conquerors, liable to suspicion. I have, therefore, been compelled to enter upon an exceedingly difficult investigation, which, if successful, must prove the radical identity of the Latin and Cumrian tongues. The proof is this—

If there are derivative words in the Latin, of which we must seek the primitives in the Cumrian, and if these primitives be shewn to furnish an explanation of many words before inexplicable on etymological principles. For example, if the verb "to tread," under various forms, be found with the meaning, "to trample with the feet," in most of the western languages of Europe, and have no noun to base itself upon in these languages, and yet the noun, "traed the feet" be found in one of them, the inference is irresistible that the verb in all its forms was derived from this root. To deny this would be equivalent to a denial that the Latin verb, calcare, came from calx the heel. In the following list such words alone, with a few exceptions, for the sake of etymological illustration, have been introduced. It might have been indefinitely extended, but the difficulty was to confine the examples within moderate limits.

Am, around; Am-Terminum,[1] quoted by Macrobius from Cato's Origines. It is needless to quote the various opinions of the learned upon the subject. In Owen's Dictionary under the word we find: "Am, prep. round, about. In composition it answers to circum, as 'amgylchü,' to circumscribe." "Am y tan, round the fire." Hence

Amo, I go round—I embrace—I love. Amplector, I clasp in my embrace, has the same meaning. Cicero, Fam. Ep. Lib. vi. Ep. 6. "Me amicissime quotidie magis Cæsar amplectitur;" *i. e.* adds Forcellini, Amat. So, Sall. Bell. Jug. cap. 7: "Aliquem in amicis habere magis magisque indies amplecti." Thus, "Amplexor, (Cicer. ad Quint. Frat. Lib. ii. Ep. 12.) Appius me totum amplexatur," *h. e.* Valde Amat. The Greek Αγαπη, has the same meaning.

[2] Am-truo, to turn round; compounded of Am, and Troi, to turn. "Amdroi (vid. Ow. Dict.), to revolve, to circulate."

Axilla, contracted; Ala, a wing; πτερον, membrum illud quo aves volant; metaphorically, an armpit. Now Asgel (by a common change Ag-sel), plur. Esgyl, is a wing (vid. Ow. Dict.), of which the Cornish form was Esel, plur. Esili. But Asg, the root, signifies separation, a splinter; the same idea which guided man in naming limbs, μερη, Brachia, &c.

Astula—As-del (vulgo Astel, plur. Estyl), a plank; compounded of As, a plain or flat, and Del, separation; verb *Delti*, to split. The English word Deal is cognate in origin and meaning. Without any collusion or suspicion of any such occurrence, Barker's Forcellini, has under Astula, "a board, lath, shingle;" and Ow. Dict. under Asdel, "a board, plank, shingle."

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1 "Quidam putant, antiquitus fuisse separabilem afferuntque illud fragmentum."—Caton. in originibus apud Macrobium, Lib. i. satur. cap. 14. Am-Terminum, Circa-Terminum, super quo tamen miras eruditi lites excitarunt.—Forcell. in loco. Hence we have a preposition in common use among the Cumri, which nevertheless had ceased to be so used in Rome long before the Romans invaded Britain.

2 An ancient word, which, like most other expressions which they did not understand, has been especially maltreated by commentators. I add Forcellini's account of it: "Am truo vel Amptruo, to turn round in the dance. Antiquum verbum ab Am, circum, et trua, quæ est instrumentum ad movendum vel agitandum. Significat motus et saltus quos edebant Salii sacerdotes in suis sacris. Horum enim qui primus erat, amtruare dicebatur, et qui post eum movebantur et saltitabant, invicem motus reddentes, redamtruare." Cels. apud Festum, Redamptruare. Something analogical to the Strophe and Antistrophe of the Greek Chorus.

Avidus and Aveo, root Aw. Thus described in Owen's Dict.: "Aw, a fluid, also a flowing. From this expressive root are derived all words that imply fluidity, or the motion or action of fluids, and also immaterial qualities, as the impulse or emotion of the will, mind, or soul." Hence Awid, "an ardent desire, eagerness, greediness," the undoubted source of the Latin "avidus."

Aurum, Cum. Aur (also Air and Oir), root *Air*, brightness; which also produced the Latin *Aura*, a gleam, confounded with the Greek word Αυϱα, a breeze. Virgil, who undoubtedly was acquainted with one form of the language called Celtic, thus plays upon the source of the meaning of both words:

[1] "Discolor unde auri per Ramos aura refulsit."

Hence also—

Aurora, Cum. Gwawr, a compound of Aura, brightness, and hora, the hour or time of splendour—"the golden dawn."

Barba, a beard; Cum. Barv, Arm. Baro, the same. Root, "Bar" (see Ow. Dict.) an excrescence, a bunch or tuft."

Benna, a carriage, a *van.* Festus under the word writes: "Linguâ Gallicâ, genus Vehiculi unde vocantur combennones in eâdem bennâ sedentes." Cum. Ben, "a wain or cart;" and Menwr, Men, "a cart;" Gwr, "a man, a carter." Hence the prolific term, Menare in Italian, and Mener in French, to lead or manage matters. From Benna came also the British or Belgic Covinus or Covinnus. "Genus Carri, quo in bello utebantur Britanni et Belgæ." Now Cym, Cyv, or Cy is the Cumrian form of the Latin Cum, and in composition Com. What would be written in Welsh Cywain, or Co-van, a διφϱος, would in Cornish be Coven, and in Latin Covin-us. The English word Country would be written in Welsh,

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[1] Servius, on the words "Auri, Aura," has this observation: "Splendor auri," Horatius. "Tua ne retardet Aura maritos," *i. e.* Splendor. Hinc et aurum dicitur a splendore qui est in eo metallo." Thus Varro also seems to have had access to some source of knowledge afterwards shut, when, under Aurora (Lib. vi. de Lin. Latina, cap. v.) he writes: "Aurora dicitur ante solis ortum, ab eo, quod ab igne solis tum aureo, aer aurescit." Aureus is used to express brightness, without any reference to gold, as in "aurea Phœbe," "aurea Venus." And Manilius has even, "Aureus olor" (Lib. v. v. 383), "*i. e.*" adds Forcellini, "Nitidissimi et candentis coloris," brilliant white. Perhaps also in the famous passage (Hor. lib. i. od. v.)—

———"Heu, quoties fidem,
Mutatosque Deos flebit; et aspera
Nigris æquora ventis
Emirabitur insolens,
Qui nunc te fruitur credulus *aurea*;
Qui semper vacuam, semper amabilem,
Sperat; nescius auræ
Fallacis,"

aurea ought to be translated, "in all your brightness," the same as Aurea Venus, "all smiles:" and aura, "a gleam of light," the deceitful sunshine, ought to be contrasted with "aspera *nigris* æquora ventis."

Cyv-dre, a συνοικια, from Cyv, and Tre, a residence; but in Cornish it is Contre. Hence Contrevak, Anglice, a countryman. It is to be remarked that the first meaning of Country is the Latin Patria, and that its use to represent Rus is secondary.

Bestia, a wild beast. Ulpian (Dig. lib. 3, tit. 1, et lib. 9, tit. 1, Leg. 1) tells us: "Bestia sunt omnia animalia quæ naturâ fera sunt et hominibus noxia, ut Ursi, Leones, Apri, Tigres, item canes feri, serpentes, venenati aranei, et hujuscemodi alia quæ aliquo in modo in hominem sæviunt." The root is the Cum. bwyst—"wildness, ferocity" (see Ow. Dict.) as an adjective,[1] wild, ferocious, or savage. In common language, it is joined to Mil, an animal, as Bwystvil, a ferocious, noxious *beast.*

Brassica, a cabbage—Cum. Bresych; From Bar-aisg, "head-spreading." The adjective is Braisg, "gross, thick."

Ceres, first, corn, secondly, the goddess—Cum. Ceirch. In Corn. cerh signifies oats. Among the Sabines, as we are told by Servius (1 Georg. v. 7), ceres meant bread, as in Scotland to this day, meal means oatmeal. From Ceres comes

Cervisia, "a drink (writes Forcellini) made from various kinds of corn macerated and bruised, but principally from barley. It was formerly much used by the Gauls, therefore its origin ought to be looked for in their language. For what some say is not probable, that it is formed by syncope from Cereres vis, because the strength of corn is concentrated in it." The roots are—Cerh, oats, and probably all other grain; and Wys, water. Those who claim distillation as a modern discovery, forbid us to consider this Celtic drink to be whisky, the strong water of the Celts of Scotland and Ireland. But in Celia, or Ceria, its Spanish name, we recognise Cooroo, the still existing name of ale in Wales. I have written it as it is pronounced, as its true form Cwrw, has often been quoted as an instance of the impossibilty of pronouncing Welsh names. Such persons forget that the Greek ω is a vowel, and that it was used long in England to do the same service which it still performs in Wales.

Catena, a chain. This the etymologers would fain extort from the Greek καθ' ἓν, one by one, linked together. But both the quantity and meaning reject this derivation, as catena signifies any[2] restrainer. Vitruvius[3] says, "Hique asseres, catenis dispositis ad contignationes crebriter clavis ferreis fixi religentur." Here they must be translated, "bands, pins, wooden brackets, or cramps." Again, Palladius[4] has a passage of a similar kind, "Asseres, catenis ligneis ex junipero aut cupresso factis ad contignationes suspendemus." Here, also, catenæ ligneæ must be translated "wooden cramps or knees." But no abuse of metaphor could have

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[1] Once again let the reader be told, that the favourite vocal sound of the Cumri is that represented by wy, or oo-ee pronounced as diphthong The same word Bwyst, is in Corn. Buest, Ang. Beast.

[2] Forcellini, under the word, "Sumptum etiam pro quovis nexu, quo aliquid conjungitur aut ligatur."

[3] Lib. vii. cap. 3.

[4] Lib. i. tit. 13.

induced plain writers like Vitruvius or Palladius to have called wooden cramps a chain of linked rings. Some other etymon is therefore required, and this we have in the Cumrian, cadwyn or cadwen, plur. cadwynae, "a chain or bond (see Ow. Dict.), root, Cadw, "to keep, preserve, or save." This verb must, therefore, have once formed a part of the Latin language, and perhaps of other languages also, as the Greek καδος, the Latin cadus, and the English caddie, signify a vessel which will hold or keep things. From the same root came catinum, a deep dish or pot, and its diminutive catellus, our kettle. The change of the Cumrian d into the Latin t. is almost invariable, as Lloyd has remarked, who adds in another place, "The Spaniards, to mention once for all, agree with us in changing the Latin t into d, and the p into b, especially in the middle syllables. In the termination their d often answers our dd (pronounced th soft as in *the*), which we also formerly writ only one d."

Cippus, and diminutive cippulus. "Valli genus ex trunco arboris, unde et Vet. Gloss. κορμον, truncum exponit." Forcell. under the word. It also signified a terminus, and sometimes stocks, of which meaning the Italian "ceppo" remains yet a living witness. Now, under the Cumrian "cŷf," pronounced "keef," we have in Owen's Dict. the following explanation:—"A stock, a stem, trunk or body, a stump; cyf pren, a stump of a tree, plural, cyfion (pronounced cuffion)-stocks (hence Anglice hand-cuffs)." The diminutive "cippil," means also "a stump of a tree with the branches dried on," which suits the description given by Cæsar of his "cippi."

Crumena and Crumina. A leathern bag or purse, from croven, an outside crust, whence croen and croenen, a skin or hide; on the same principle as purse comes from Βυρσα, a hide.

Cicuta, hemlock, a pipe; also a "fistula ad canendum apta." In Humph. Lloyd's British Etymologicon "cecut" is put down as an old Cum. form of this word. The common name is cegid or coegyd, "the hollow one," from coeg, hollow. Servius observes, "Cicuta autem est spatium quod est inter cannarum nodos," "the cicuta (or hollow) is that space which is between the knots of the reeds."

Cuneus, a wedge, from Cum. cun or cŷn, "a head, first part or wedge, also a chisel." Quotation in Ow. Dict., gyrrü r cûn a gerdo, "to drive the wedge that will go."

Carpentum, a carriage, a longer form of Cum. car-ven (see Benna), a cart. It might have been originally spelt carmen, and hence the "porta Carmentalis" of Rome, if this was not derived from leading to the Arx Carventana before mentioned. To trace its name to an imaginary mother of as imaginary an Evander, was only an old woman's tale.

Codex, or Caudex, "the body, trunk, stump, or stock of a tree," from Cum. "coed, trees, Coeden, a standing tree." In Latin it signifies secondarily a book, but never bears that meaning in Welsh.

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[1] De Bello Gall. Lib. vii. cap. 73.

[2] Virg. Eclogues, ii. b. 36.

Collis, a hill, from Cum. Col, (vid. Ow. Dict.) " any projecting body, a peak," whence

Columna, Cum. Col-ov and colov-yn, (vid. do.) " a stem or stalk, a prop." These may not necessarily be Cumrian, as the Greek κολωνη seems derivable from a common root. But assuredly the Cumrian is the only one which has kept the root to this day; Colla also is " the awns of barley." Collum, " a neck," should be referred to the same root.

Froenum, " a bridle," Cum. Frwyn, from Froen; plur. Froenæ, " nostrils." The first bridle was a ring through the cartilage of the nostrils, to which the reins were attached.

Frustro and Frustror, to disappoint, to baffle, to prevent, from Cum. Rhwystro, (vid. Ow. Dict.) to go before, to obstruct, to hinder; root, Rhwystyr, " opposition, hinderance, impediment, an obstacle."

Maceria, " any wall inclosing grounds." " Paries sub dio positus ad sepiendos hortos, villas, vineta, et hujuscemodi, sive ex calce fiat, sive sicco lapide construatur." The last would have been the mode of construction originally in use, whence the name. H. Lloyd, under Maceria, has " Magwyr," a dry wall of stones without mortar. But Magwyr is generally called " Y. Vagwyr," i. e. gwag-vür (vacu-us mur-us) a wall not closely connected, having intervals. This is exemplified by the Latin cognate word, Vacerræ, a paling of hurdle-work used as fences in fields, and by the Welsh, Gwagar, a sieve formed by slips of wood, with intervals between, crossing each other at right angles.

Miror, to wonder at, to gaze on, from Cum. Miraz, " to see," (vid. H. Lloyd's Brit. Etymologicon). The Spanish language also keeps the primitive meaning, " mirar," to see. Nay, even the modern Italian has, as in many other instances, retained the older meaning, " mirare," to view or behold, while there are but faint traces of the secondary meaning, which alone is found in classical writers. The root is the Cum. Mîr, " what is fair or bright." Mirror also comes from the primitive meaning.

Mola, a mill. For the origin of this word we are referred to the Greek μυλη, which gives us no new information. The Cum. " Mâl" means what is reduced into small particles, bruised or ground. Hence, " Malu", " to reduce small, to grind." And a longer form, Maluri, " to bruise, break, or pound." Hence, both the stones and the teeth, Molares.

Mox, quickly, soon after. A word left in the Latin language without a single relation to express its source. The root is the Cum. Môch, " ready, quick." (Vid. Ow. Dict.) " Moch dysg nawv Mab hwyad," in Latin, " Cito" or " Mox discet nare filius anatis."

Mactus, honoured, increased, said to be derived from " magis augeo," which assuredly would never give, especially in older times, the word Mactus. The Latin philologists seem at an early period to have confounded this old participle from an obsolete verb, with the frequentative macto mactare, to slaughter. The cognates of this latter word are μαχη μαχαιρα, and the Latin macellum, the shambles. The root of Mactus is the Cum. Magü, to breed, to make great, as grow is of great, alo of altus, and in and gigno of ingens. From Magü came Maethü, to educate, to nurse, to cause to increase. Mactus is the same word, although it requires some explanation before the truth appears to an English eye Now, the ct and pt at the end of Latin words is almost invariably represented in Welsh by th ; *e. g.* Vir doctus becomes in Cumrian Gwr-dôth, Aur-um coct-um Aur côth, Mil-es capt-us Mil-wr câth. Plenus fructus Llawn frwyth, Fluv-ius Lact-is Lliv o laeth, &c. &c. On this principle Mactus would be Maethüs, "full of increase." Sis macte, "be full of increase," grow, tua virtute. To the same root should be assigned the Gallic word, ambacti, mentioned by Cæsar, for as Tâd-Maeth is a foster father, and Mam-Vaeth a foster mother, so Am-Vaethi would be the circle of foster brothers, which in Celtic countries formed the strength and pride of a chief.

Occo occare, to harrow, from Cum. "ogi, to use the harrow, to harrow, (See Ow. Dict); root, og and oged, " the instrument." Occa, once admitted into Latin dictionaries, is now rejected. The derivatives of oc or og are so numerous in the Cumrian, with the meaning of quick motion and sharp points, as to make it clear that it is an original part of the language. Persons who wish to draw subtle inferences say, that all the terms of the Romans connected with agriculture may be referred to a Greek source, while the terms expressive of war or hunting are non-Hellenic. The induction fails completely in both parts, as might easily be shewn. When Cæsar landed in Britain, the natives were agriculturists, densely planted, and Halley proved that the harvest which Cæsar's soldiers reaped, had ripened at the average period of a Kentish harvest in his days Assuredly, then, the Britons had not the agricultural names to learn from the Romans of an after age.

Navo, to "perform vigorously, to work diligently." Navare operam et opus, "to perform a work ;" root, Nâv, " a former, a creator ;" Nav-Neivion, in the Cum. "opifex opificum," "God." Hence Navawl, relating to formation, and Naviad an operation.

Natrix,[2] Gael Nathair, Cum. Neidür, Saxon, Nadder and Neddre. The English adder was formed by a mistake, an adder, instead of a nadder. By an opposite one an evet or eft became a newt.

Nerv-us, a muscle, Nervosus, strong. Undoubtedly the same as the Greek Νευρος. The Cumrian root is Ner-th, strength, and Nêr, the powerful one, God. Now according to Aulus Gellius, "Neris et Nerienes, et Neriene et Neria, Sabinum verbum quo significatur virtus et fortitudo—unde ex Claudiis, quos a Sabinis oriundos accepimus, qui erat egregia ac præstanti fortitudine Nero appellatus est."

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[1] De Bello Gallico, Lib. vi. cap. 14. Equitum ut genere opibusque amplissimus, ita plurimos circa se ambactos clientesque habet.

[2] Etymologists would derive this from No, to swim, and refer to the Greek ὕδρος as an illustration, but the masculine, swimmer, is Natator, and Natrix is itself masc. ; " Et natrix violator aquæ." See Forc. in verbo.

LACRYMÆ, "tears." Undoubtedly the same as the Greek Δακρυα, and quoted merely to shew that it was not through the Latin that many words common to the Greek, Latin, and Cumrian languages crept into the latter, as the Cum. word is still Dagræ, "tears."

RHEDA,[1] "a carriage with four wheels a cart," a Gallic word, as we are told by QUINTILIAN, although both CÆSAR and CICERO scrupled not to use it. The root is the Cum. Rhedeg, "to run," on the same principle as currus comes from curro. Thus also, on the same principle as τροχος, "a wheel" comes from τρεχω, the Cum. Rhod, "a wheel," comes from Rhedeg. But Rhod is the Cum. form of Rota, a word confessedly Latin. Rhod is, both in itself and its derivatives, used as extensively in the Cumrian, as Rota and its derivatives in Latin. Petorritum, a four-wheeled carriage is Cum. Pedair-Rhod, "four wheels."

RITUS, rites, ceremonies, customs. Cum. Rhaith, Plur. Rheithæ, "legal decisions, law, rights, privileges." Pen Rhaith Yw Duw, "Caput Ritûs est Deus."

PALMA, Greek παλαμη, "the hand or palm." Cum, Palv. (contracted Pawen a paw) Armor. Palf, whence we can illustrate the Latin Palpo, to grope, to *feel* one's way, and Palpator, Cum. Palvadur, a groper, &c. Hence also Palpebræ, "eye feelers," and probably papillæ also.

PELLO, to drive from, to push away, from the Cum. Pêll, far distant; Pelli and Pellaü to put far from one, to render remote or distant

PENATES, household gods, originally the chief gods of either the state or a family. According to MACROBIUS,[2] the Penates of Rome were Jupiter, Juno, Minerva, and Vesta. CICERO[3] derives their name from Penus or Penitus, "quod penitus insident," others from different sources. The root appears to be in any case the Cum. Pen, a head. Hence Penaeth, (which in a Roman's mouth would become Penāt) "a principal, he who is pre-eminent, a chief. The old nominative, singular, was Penas, on the same principle as Primas, Optimas, &c.

PENITUS, "within, interior, inmost and innermost," from which the preceding, according to some, was derived, is also connected with the Cum. Pen, which signifies not only a head, but also an end, as "Pen-Tîr Lloegur, "Caput terræ Liguriæ," "the Land's End of England." Pen-ucha r tî, "Caput interius Tecti," the innermost room in a cottage, Scotice, Ben, Latin Penus, but latterly confined to express the innermost cell of the sanctuary of Vesta. Heliogabalus, "in Penum Vestæ, quod solæ Virgines Solique Pontifices adeunt, irrupit." From the same root, prolific of words in this and other languages, spring.

PENES, Penes me, "in my house," consequently "in my power," answering in the first place to apud me, in Latin, and chez moi, in French, and in its secondary meaning to the επι μοι of the Greeks, in my power. Of its first meaning we have ample in-

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[1] The Cumrian, like the Greek, aspirates the letter R at the commencement of a word.

[2] Lib. iii. Saturn, c. 4.

[3] De Natura Deorum, Lib. iii. cap. 29.

[4] Lampridius in Vita, c. 5.

stances. Ter. Adelph. 4 24: Istæc jam penes vos psaltria est! Is that dancing girl in your house? S. Ellam intus. She is within, &c. &c. The secondary meaning needs no illustration. From Pen, with its signification of end, came Penis and Peniculus, a tail, and many other words.

POPINA, "a cook-shop, an eating-house," derived by scholars from POPA, the minister who struck the sacrifical victim with a mallet, and who was supposed, on authority which I cannot find, to vend the flesh of victims. By an edict of NERO, "nothing cooked except pulse and herbs was allowed to be sold in Popinis," although previously every kind of obsonium was exposed to sale in such places; and TIBERIUS, during a dearth, forbad even pastry-work to be sold in cook-shops. On the whole, the Popinæ of Rome appear to have united our eating-house and pastry-shop. The root appears to have been the Cum. Pobi, "to bake to roast;" Pobi bara, "to bake bread;" i bobi golwyn, "to roast a joint." (Vid. Ow. Dict.) Poban, "an oven," Pobur, Pobwr and Pobid, "a baker."

POPULUS, the people, the community, Plebs (says AULUS GELLIUS,[2] on the authority of a competent witness, ATEIUS CAPITO) differt a *populus*, quia hoc nomine *omnis pars civitatis omnesque ejus ordines* continentur, Plebs vero ea dicitur in quâ gentes civium patriciæ non insunt." The old form of writing the word was Popolus, Cum. Pobol, and contracted Pobl, not unlike the Etruscan form, Puplu, as seen in old inscriptions. The root is Pawb and Pob, (vid. Ow. Dict.) "every body, all persons." Pob ac un, omnis et unus, "one and all." "Pob-un," every one.

PRETIUM or Precium, the money value of an article; apparently the general name at an early period for the precious metals. Even in later times, it seems to have retained this primary meaning. Thus HORACE,[3] alluding to Jupiter's transmutation into gold,

"Converso in pretium deo."

And OVID:

"In pretio pretium est, dat census honores."

And still stronger:

[4] "Argentum felix, omnique beatius auro,
Quod pretium fuerit quum rude, numen erit."

Now Pres is in Cum. copper, the only metal which the early Romans coined into money. It is also to be remarked that, to this day, especially in North Wales, the word Pres is limited to copper coin. Prîs, Anglice price, is closely connected both in spelling and pronunciation with it.

PRO-CERUS, long, compounded of the preposition, and Cērus, unknown. It is the

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[1] SUETONIUS in Ner. cap. 16. [2] Lib. x. cap. 20.

[3] Lib. iii. od. 16. ver. 8.

[4] Fast. v. 217. Lib. ii. Ex. Pont. Ep. 8. v. 5.

Cum.[1] "hîr" (vid. Ow. Dict.), "long." The Triads have recorded three lines, which they attribute to King Arthur, and which some antiquaries may wish to see as the only literary composition of that famous monarch:—

Sev-ynt fy nhri Cad-varchog,
Mael hîr a Llîr Llüydog,
A cholovn Cymrü Caradog.
Hi sunt mei tres Pugnæ equites,
Mael pro-cērus ac Lear, copiis instructus,
Atque columna Cambriæ Caradog.

Pulvinar and Pulvinus, "a pillow and bolster." Some of the older scholars acknowledged pulvinar to be a change of letters from pluvinum, root plumæ, feathers; but in Cum. plumæ are plüv, whence pluvinar, without any change. In Cornish with a different termination, but from the same root, it was Pluvog. Again, by a change similar to that in the Latin, the common name in Wales is Pilwg, Engl, Pillow.

Planta, a Latin word, single in form, but double in meaning. In one sense it comes from planum, flat, and means the sole of the foot. In another sense, it means a young sucker, which, if detached from the parent tree, and again committed to the earth, will itself become a tree. In the first sense it has only one derivative, "plantaris," used by Statius and Valerius Flaccus, to describe the feet wings of Mercury. The other has a numerous offspring, and has entered deeply into the composition of the English language. Now the Cum. "Plant" means "offspring, children" (see Ow. Dict.), root, plan, "a scion or shoot," whence the verb planü, "to plant, to set shoots." One might imagine that the Cumrian meaning was before Virgil's eyes when he wrote the following line:

"Hic plantas tenero abscindens de corpore matrum;"

literally, "one separating the children from the tender bosom of their mother."

Quæro, and Quæso, I seek. Thus Festus, "Quæso ut significat idem quod rogo, ita quæsere ponitur ab antiquis pro quærere, ut apud Ennium libro secundo:

—— "nautisque mari quæsentibu' vitam," &c.

From this observation, and from the practice of the classical writers, we find that quæro, in the sense of seeking, had ceased to be used by the Romans long before their settlement in Britain. But the Cum. ceisio, and ceiso (vid. Dict.), still means "to seek, to go after, and fetch," and has its own noun cais, "both a petition and an attempt." Quæso and ceiso are in utterance the same word.

Quis, Who? Cum. Pwy? This interchange of the p and qu, refers to a period still more remote than that in which the r and s were mutually interchanged. We know

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[1] It is a constant practice to represent the Latin S, by the Cum. H, and *vice versa*, e. g.—

Sērus, hwyr.
Sag-um, hyg and hygan.
Sal, Halen.
Sol, Haul, &c.

[2] See H. Llwyd's Brit. Etym. p. 283.

not when the qu or q was thrown out of the Greek alphabet; probably it was rejected with all the harsher sounds represented by the letter w, whether single or preceded by gutturals. We know that in some cases it was replaced in Attic Greek by the π, in Ionic by the κ, e. g. the Latin qua became in the former dialect πῃ, in the latter κῃ. This change was imported to a certain extent into Italy, as we find from Festus that the Oscans called quidquid, pitpit or pirpit, and quispiam and nuspiam became Latin words. But the Cumrian, which broke off its connection with the Latin language before it formed or admitted the pronoun relative qui, quæ, quod, (a form of speech unknown both to the Cumrian and Homeric languages) writes the important interrogatives Pwy, "who?" and Pa, "what?" invariably with a P, while the latter word is again in Irish ka. With this previous explanation I venture to submit, that the Cum. Pa val, of what kind? compounded of pa and mal, "like," and Pa vaint, of what size? made up of pa and maint, "size," furnish us, if we change again the Cum. pa into the Latin qua, with the true origin and meaning of the Latin

Qualis, of what kind;
Quantus, of what size.

For further illustration, I furnish a copy of some observations from Ow. Dict. under maint, mal, pa. Maint,[1] "magnitude, size, bigness, greatness, quantity." Pa vaint syd yna, "quantum sit in eo" (loco). Mal, "like, similar to," mal hyn, pronounced val hyn, "in this manner." Pa, "what," pa vod, "in what manner," Quo modo.

Qualus, a basket of twigs, wicker work. Cum. cawell, a hamper or basket; root, caw, "a holder which retains or keeps things together."

Talpa, a mole, from Cum. talp and talpen, "a mass, a lump, a knoll, a round heap not large, a knob;" in the same manner as Mole came from mould-warp, "the caster up of earth."

Trans, "on the further side of, beyond, over, across;" root, Tra, as may be seen in ultra contra, intra, &c. But Cum. Tra is a noun (vid. Ow. Dict.), "an extreme, an excess," also a preposition, as tra munid, "over the mountain," trans montem, tra môr, "trans mare." The Latin noun trabs, a cross-beam, is the Cum. Traws, "a cross beam, or a cross man."

Vagina, a sheath. Italian, guaina. Cum. gwain, signifying both "a sheath or the carriage of a sword," as our ancestors called it, and "a waggon," the vulgar pronunciation of vagina, as wain is that of gwain.

Vates, a name for Roman poets before the time of Ennius, as he thus attacks Nævius,

—— scripsere alii rem
Versibus quos olim Fauni Vatesque canebant."

[1] Maint in French (magnitude applied to numbers, in Cum. to size, two relations which continually interchange, as παυροι, a few, parvus, small), is a derivative from Magnus, or some cognate form. Magnitas in French, would become Maint, as Magis becomes Mais, Pagus Pais, &c.

That it was a Gallic word we know well, as Strabo, when speaking of the Gauls in general, writes, *παρ' απασι δε ὡς επιπαν τρια φυλα των τιμωμενων διαφεροντως εστι· Βαρδοι, τε και Ουατεις, και Δρυιδαι. Βαρδοι μεν ὑμνηται και ποιηται, Ουατεις δε ιεροποιοι και φυσιολογοι, Δρυιδαι τε προς τῃ φυσιολογιᾳ και την ηθικην φιλοσοφιαν ασκουσιν.* "But among all the Gauls, there are three classes of men who are held in especial honour, Bards, Vates, and Druids. The Bards are singers and poets; the Vates sacred ministers and natural philosophers; the Druids to physiology add the study of moral philosophy." It was this sacred order that left its name to the poets and prophets of ancient Italy. Now, the Cum. form of Vates is Ovid, written Ovydd, of which we have the following explanation in Ow. Dict. :—"One who is initiated into first principles or elements, a scientific personage, a natural philosopher, a teacher of science, the name for a member of the scientific class, in the bardic system. An ovate." Probably the name Ovidius is a derivative of the Cum. Ovid.

Verus, "true, genuine, not false, not disguised." Cum. Gwîr, "the pure fluid, the ether, truth, right." In Latin also Verus had this meaning of right, as Servius says on the passage, Æn. 12. v. 694.—

me Verius unum
Pro vobis fœdus luere et decernere ferro.

"Verius justius.—Verum enim quod rectum et bonum appellabant. Gwirod, a derivative from Gwir, "the pure fluid," means strong unadulterated liquors of all kinds. (See Ow. Dict.) Hence the connection of merus, noun, merum, with Verus. The Greek αληθεια, no concealment, transparency, agrees in meaning with the Cum. Gwîr. If the English, truth, come from the verb, to trow, as Horne Took inferred, it is a memorable proof of barbarism in our Saxon forefathers, and enough of itself to destroy all their ideas of truth and right.

Vergo, "to incline, to bend to." Cum. Gwyro, "to make crooked, to bend, to *swerve*, to go *awry*."

Viridis, "green, flourishing," Cum. Gwyrd. In the Cornish dialect, which, less harsh and guttural, throws great light upon the cognate languages, it is *Gwer*, French Ver-d. That Viridis had this form in Latin, appears not only from Ver, "the green season," but from Verbena, "any green sod or tree," and Vervactum, "ground ploughed down in the green sod," in opposition to stubble land. See the word in Forcellini, and the mistake of Pliny there alluded to.

Severus, "strict, severe, sharp;" root, Chwevyr, "violence," Chwevri, "to act or affect severely," Chwevryd, "a severe one." I have only time to allude to a large class of words beginning with Chw, and which are found in Latin with an S or V. Suffice it to give two examples :—

Socer a father-in-law, in Cum. Chwegyr. The German Schwager means "a brother-in-law." When Homer wrote, either Chw, or Schw, was used, as can be proved from φιλε εκυρε, so well known

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[1] P. 194.

Soror, "a sister," Cum. Chwaer, Corn. Hôr, the Latin form without the termination, Germ. Schwester. Italian Suora, where the aspirate still seems to have a place. But this head is inexhaustible.

Sors, Sortis, Sortes, "lot, chances." That which falls out, id quod accidit, Cum. Syrth and Swrth, "a fall, a lot, a chance," from the verb Syrthio, to fall. In Corn. Swrth would be written "sort."

I am now compelled by want of room to conclude, and to pass over in silence this long list, which, as I believe, are all Cumrian in origin and meaning:—

Blœsus	Ecce	Rigeo	Sulcus
Bellum	Gula	Rixa	Quatio
Bellua	Lamina	Ruga	Quiesco
Brutus	Latus	Rana	Sapor
Fenestra	Latium	Salix	Suavis
Ferrum	Lucrum	Scateo	Torrens
Calamus	Laus	Scelus	Talio
Caseus	Mas	Scopa	Talus
Carus	Mando	Scrotum	Telum
Canus	Nævus	Sebum	Titio
Colus	Paries	Serus	Vilis
Copula	Pastino	Solus	Vibro
Crena	Pruina	Solum	Venus
Dolor	Purus	Splendeo	Caius
Donec	Puteus	Stannum	Marcus
Dies	Raucus	Sudor	

A few of these may seem to be derivatives from Greek roots, but if examined comparatively, they will be found to be more immediately Cumrian. But I cannot conclude the paper without entering more at large into an explanation of two words, which, in my opinion, would of themselves be sufficient to prove the radical connection of the Latin and Cumrian languages, these are,

Mœnia, "walls," and Præda, "a prey."

And first of the first. Mœnia, "walls," sing. Mœne, from the Cum. Mæn, "a stone," plural, Meini "stones." If this be the root, Mœnia must signify stone-walls, in opposition to wooden defences or earthen-ramparts. These were expressed by Vallum, from "Vallus a stake," and Agger, from Ad-gero, "to throw up." The probability therefore is, that Moenia, by which the strongest defences are in general meant, were made of stone. An examination of Moenio, the verb of Mœne and Mœnia, and more commonly written Munio, will furnish us with a proof that such probability is almost certain. For we find that Munire viam, is "eam sternere lapidibus;" *e. g.* "Perinde[1] quasi Appius ille cœcus viam munierit, non qua populus uteretur, sed ubi posteri ejus impune latrocinarentur." Nay more, Tacitus, who is fond of recurring to the original meaning of words, puts in the mouth of Galgacus the strong expression, "Sylvas et Paludes emunire," "to make stone causeways through woods and marshes." Closely connected with Moenia is Munus, written by the ancients Moenus, which, if Varro is right, was "the metal or earth required from each soldier when a position was to be fortified," for after explaining, according to his theory, the source of Munus, "a gift," he adds, "Alterum Munus, quod muniendi causa imperatum," "Another Munus, is that which is ordered for the purpose of fortifying;" hence he derives "Municipes, qui una Munus fungi debent," "who are bound to the common defence of their town;" hence Immunes, those who are free from such or any other public burden; hence Munera, offices, originally as in England, *parish burdens*, but latterly, when wealth increased, sought after, and highly valued. Cognate with Munus, in the sense of metal, we have the Cum. Mwn, or Mwyn, "ore, any thing dug out of the earth," *e. g.* Mwyn aur, "gold mine," Mwyn arian, "silver mine," &c. whence, Anglice, Mine. It is difficult to account how Minium, red lead, took the generic name. We only know, that the Romans supposed it to have been derived from the Minius amnis (the Minho), which probably derived its name from it. Because Vitruvius, "Minium inquit et Indicum nominibus ipsis indicant, quibus in locis procreantur."

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[1] Cicer. Pro Milon. cap 7.
[2] De Ling. Latin. Lib. v. 42.
In fine Cap. ix. 1–7.

It is a curious coincidence, that, to this day, Gwaith Mwyn, in Wales, always means "a lead mine." From Moenus, or Munus, a metal, came undoubtedly Moneta, "money." Cum. Mwnai, "money coin," as in Ovid, Fast. v. 221,—

> "Æra dabant olim: melius nunc omen in auro est,
> Victaque concedit prisca moneta novæ,"

(The fable about Juno Moneta, root, moneo, with a Greek termination, is unworthy of serious notice), and Munus and Munera, gifts.

Secondly, Præda, a prey, from the Cum. Praid, plur. Preidæ.

In Ow. Dict. we have the following explanation of Praid, "a flock or herd; also a booty or spoil of cattle taken in war."

"Praid gyv-reithiol, pedair bu ar ügaint a Tharw," Welsh Laws. "A legal herd, twenty-four cows, and a bull." In similar Latin order, Præda Corritualis, Quatuor boves super viginti et Taurus.

In the Latin transmitted to us, we find Præda with the secondary meaning alone, acquired by the Romans at an early age, when the robber wolf was their favourite emblem, and their neighbours' flocks and herds were regarded as legitimate objects of plunder. But there are, if I am not much mistaken, some Latin words still extant, which were formed in a more Saturnian age, when Præda had not lost its proper signification of flocks and herds. Among these are,

Prædium,
Præs,
Præditus.

Concerning the meaning of the word Prædium, there is no doubt. All interpret it to be a farm or landed property. "Possessio," says Forcellini, "omnia bona complectitur, mobilia et immobilia; Prædium immobilia tantum." The Roman philologists, if we can honour them with the name, wished to derive Prædium from Præs, a personal security, as if the first quality of landed property, which would occur to a simple and early race, was the power of mortgaging it. In later times, such a confusion of ideas is perhaps possible, from the close connection between landed property and mortgages. But assuredly its liability to be mortgaged, could not in an early age have been the accident most likely to strike the sense, and to induce men to name land after it. What among the Sabine hills, and in a half pastoral state, could have given a better idea of a farm than to call it from Præda, flocks and herds, Prædium their grazing ground, just as we to this day call a mountain farm a *sheep-walk ?*

From Prædium (on the same principle as from Pretium, interpres-pretis, a broker, a price-settler between two foreign merchants) came Præs, Prædis, "the possessor of a prædium, or of Prædia, secondarily, "one who could give good security, heritable security as it is called in Scotland." ASCONIUS, in his commentary on CICERO's speech against VERRES,[1] has the two following passages :—" Bona Prædia, dicuntur bona, satisdationibus obnoxia, sive sint in mancipiis sive in pecunia numerata ; Prædia vero domus, agri." "Prædia sunt res ipsæ, Prædes homines, id est fidejussores, quorum res bona Prædia dicuntur," of which this appears to be the translation :—" Bona prædia (two terms as usual in Roman law formularies, put together without a conjunction, as patres conscripti, &c.) are called the goods liable to be seized by creditors, whether they consist of saleable property or of ready money, but Prædia are house, lands." "Prædia are the things themselves, Prædes the men who have given security, whose property is called by one name, Bona-prædia," i. e. what we call property personal and real. Vendere Prædem was what is still called by lawyers to "sell up a man," that is to sell all his property. In ancient times, under the cruel law of Rome, the man himself might have been liable to sale. The Roman etymologers have written much nonsense on this word, but not so much as emendators have compelled them to write. VARRO says it comes from Præsto est, which has been changed into an adverbial Præs, as if there had been any

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[1] Orat. adver. Lib. iii. 54.

such word in the language. In taking security, no lawyer regards the mere person, or cares whether he be absent or present, if he be not rich and able to answer when pressed for the debt. Præs is therefore equivalent to "locuples," full of land, "rich in land." Hence "locuples reus," is one "qui cum se obligarit, habet unde solvat, et idoneus est implendo promisso." Besides all this, had the imaginary Praes, or the real Præsto, been the root of Præs, Prædis, it would have applied with equal force to one who gave security in criminal cases either to present himself, "se sistere," or to bring into court him for whose appearance he had become bail. But, as FORCELLINI properly observes, "differt præs a vade, vas enim est qui pro alio spondet in re capitali, præs qui in re nummaria."

Præditus, endowed or vested. This we know cannot come from Præ, and "do, to give." First, because there is no such compound; secondly, because if it had once existed in the language, it would signify that its substantive "had been given beforehand," since datus and donatus cannot be interchanged. It appears to have been first applied to a person invested or endowed with landed property, and that it was thence metaphorically transferred to all endowments physical and intellectual. We have now no hesitation in saying that "a man is possessed of many good qualities," because possideo, at the period when Roman literature commenced, signified "to possess." But in older times possessio, formed out of pro-sedeo, signified only pre-occupation, "a sitting," or as the Americans have it, "a squatting," on ground not legally conveyed. We know what radical mistakes in Roman history occurred from a mistake in the meaning of the two words, "Possessores agrorum."

Connected with the idea of property are three other words, which are also derived from the Cumrian, these are—

Idoneus,
Divitiæ,
Bonus and bona.

Idoneus, "proper, suitable," &c. originally applied to men of property, hence "rich, wealthy." Hear Caius,[1] (for lawyers, as I have before observed, are more retentive of the original meanings of words,) "Si ab idoneo debitore ad inopem transtulerit obligationem," i. e. "ab eo debitore, qui est solvendo."[2] Here idoneus, rich, and inops, poor, are put in opposition to each other. So, also, in Ulpian, "Tutores et Fidejussores idonei," are twice opposed to those "qui lapsi sunt facultatibus" (ruined men). Thus, also, it changes places with Locuples, as Testis idoneus, Testis locuples, Auctor locuples, Auctor idoneus. The root of this is the Cum. eid-ion, vulg. eidon, an ox, the generic name of kine, for beef is always called "cîg eidon." In the Cum. Italy is always called "Y'r eidal," close enough to identify it with Italia, derived from italus, an ox.

Divitiæ seem as clearly referable to the Cum. Dav-ad, plur. Deveid, or as the word is written in old documents, Deveit, "a sheep, and sheep." Indeed I doubt not that Davad and Devaid are compound words, made up of Dav or Dôv, "tame," and Eid, the root of Eidon, still to be found in the literary language of Ireland as the generic name for "cattle," (see Ed. and Eid. in H. Lloyd's Irish Dict.); and that the animals, reclaimed from their wild state, and domesticated by man, bore this name, which was subsequently transferred to express the wealth of man. He who therefore is inclinedto deny that pecunia and pecuniosus come from pecus, may also refuse his assent to the inference that Divitiæ comes from Deveit. I could as easily shew that Duonus, the original form of bonus; is also of Cum. origin, but I abstain, and reluctantly close the present paper.

To follow it up, three more at least are absolutely necessary—

1. An examination of the facts which made so many tribes in Italy, Gaul, and Britain, claim a Trojan origin, and a sa-

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[1] Lib. et tit. 4. Leg. 27.

[2] Lib. xxvii. tit. 8. Leg. 1.

tisfactory account of one Troja or Ilium at least, if it was not also the Homeric one.

2. An illustration of the mythology of primitive Rome, by comparing it with the oldest fragments of the Cumrian system, with a satisfactory explanation of the fable of the Wolf and Eagle, the Sus alba, and other matters which have hitherto baffled the learned.

3. An illustration of the system of the Agricultural Republics of ancient Italy, of which Rome was one, by a reference to the Welsh Laws, where much valuable information on the constitution of society in south-western Europe, during the Saturnian Age, is to be found.

Desiderata for a further prosecution of the subject:

Vocabularies of the languages spoken among the hills of Umbria, Rhætia, Liguria, the Maritime Alps, and Auvergne.

EXPLANATION OF PLATE XVI.

Fig. 1. Commissure of the optic nerves in the human body, examined after the nervous fibres had been hardened by alcohol. From Mayo's Engravings. A A, Optic nerves. B B, Tractus optici, or continuation of the nerves behind the commissure. C, Semi-decussation.

Fig. 2. Course of the Tractus optici, examined after similar preparation, in the sheep. A, Part of the right hemisphere of the brain. B, Olfactory lobes. C, Right Tractus opticus just behind the commissure. D, Crus cerebri. E, Tuber annulare. F, Right Tractus opticus cut across and torn backwards, as it expands upon and winds round the Crus cerebri and back of the Thalamus nervi optici, H, to be implanted on G, The upper portion of the right division of the Corpora quadrigemina, or optic lobes. Part of the fibres at this, their posterior termination, are turned back, to shew that those which lie lowest in the Tractus opticus are implanted in the highest part of the optic Lobe, and those which lie on the inner side of the former, are implanted in the outer side of the latter. I, Medulla oblongata. K, Cerebellum. L, Implantation of the fifth nerve (*i. e.* the nerve of touch of the face and eye) on the side of the tuber annulare, in which no such contortion is observed.

Fig. 3. Arrangement of the fibres of the optic nerves as they are implanted in the optic lobes of the fish, from Serres' Anat. Comp. du Cerveau, &c. A, Cerebral lobes. B, Optic lobes laid open from behind. C, Cerebellum.

Fig. 4. Decussation of the fibres which descend from the brain, through the Crura cerebri and Corpora pyramidalia, to the spinal cord in the human body; found also, in a greater or less degree, in all the mammalia and birds, therefore coexistent in the Animal Kingdom, with the partial decussation of the optic nerves (Fig. 1). A, Corpus olivare. B, Corpus pyramidale. C, Decussating fibres. D, Spinal cord.

PROCEEDINGS

OF THE

EXTRAORDINARY GENERAL MEETINGS,

AND

LIST OF MEMBERS ELECTED AT THE ORDINARY MEETINGS,

SINCE MAY 4. 1833.

PROCEEDINGS, &c.

November 25. 1833.

At a General Meeting held this day, Mr Russell, V. P. in the Chair, the following Office-bearers were elected for the ensuing year :—

Lieut.-Gen. Sir Thomas Makdougall Brisbane, President.

The Hon. Lord Glenlee, Dr T. C. Hope, Sir David Brewster, James Russell, Esq. — Vice-Presidents.

John Robison, Esq. General Secretary.

Dr Christison, Professor Forbes, — Secretaries to the Ordinary Meetings.

John G. Kinnear, Esq. Treasurer.

James Skene. Esq. Curator of the Museum.

John Stark, Esq. Assistant Curator.

COUNSELLORS.

Major-Gen. Sir Joseph Straton.
Alexander Adie, Esq.
James Wilson, Esq.
Hon. Mountstuart Elphinstone.
Dr Abercrombie.
Sir Henry Jardine.
Arthur Connell, Esq.
Right Hon. Lord Greenock.
Dr Traill.
J. D. Forbes, Esq.
Professor Jameson.
William A. Cadell, Esq.

The following gentlemen were appointed a Committee to audit the late Treasurer's accounts :—

Sir Henry Jardine.
Thomas Guthrie Wright, Esq.
James Nairne, Esq.

December 2. 1833.

The Keith Prize was presented by the President to Thomas Graham, Esq. pursuant to the award of the Council, announced on the 6th May, for his paper "On the Law of the Diffusion of the Gases."

January 6. 1834.

MEMBER ELECTED.

ORDINARY.

Mungo Ponton, Esq.

The President announced that the Council had resolved to award the Keith Biennial Prize, for the second period, to Sir David Brewster, for his paper "On a new Analysis of Solar Light."

January 20. 1834.

MEMBERS ELECTED.

ORDINARY.

Isaac Wilson, M. D., F. R. S.
Lieut.-Col. Murray of Ochtertyre.
John Gladstone, Esq. of Fasque.

February 3. 1834.

MEMBER ELECTED.

ORDINARY.

David Low, Esq. Professor of Agriculture in the University of Edinburgh.

February 17. 1834.

MEMBERS ELECTED.

ORDINARY.

Thomas Henderson, Esq. late Astronomer at the Cape.
Reverend Dr Chalmers, Professor of Divinity in the University of Edinburgh.
William Macgillivray, Esq. Keeper of the Museum of the Royal College of Surgeons.

March 3. 1834.

MEMBER ELECTED.

ORDINARY.

Alexander Kinnear, Esq.

Lord Greenock gave notice, that at the next meeting of the Society he should move the following resolution :—

" That Edinburgh having been fixed on for the Fourth Annual Meeting of the British Association for the Advancement of Science, which is to take place in September next, it is necessary that a subscription be immediately set on foot for defraying the expense of such entertainments as it may be found expedient to be given on this occasion, in a manner suitable to it, and worthy of the hospitality for which Scotland has always been celebrated.

" That the Noblemen and Gentlemen of Scotland generally, but more especially those resident in Edinburgh or its Vicinity, be as soon as possible requested to enrol their names as subscribers to such a fund; and that a hope be expressed that public bodies and chartered societies will be induced to contribute their aid, as far as they may possess the means, in increasing the amount of this fund.

" That the Royal Society having been the channel through which the invitation to the British Association to hold its meeting this year in Edinburgh was conveyed, and this Society being charged conjunctly with the local Office-bearers of the Association with the preparatory arrangements, shall take upon itself the collection and management of this subscription, and shall appoint a Special Committee for that purpose; and shall open Subscription Lists at the principal Banking Establishments, and at the Office of the Treasurer of the Society.

" That the sum of £50 be paid out of the Funds of the Royal Society on account of this subscription, and that all the members of the Society be specially invited to contribute individually to this fund; it being understood that no donation should be of less amount than One Guinea."

March 16. 1834.

MEMBERS ELECTED.

ORDINARY.

PATRICK BOYLE MURE, Esq. Advocate. WILLIAM SHARPEY, M. D.
THOMAS BALFOUR, Esq. Advocate.

Lord GREENOCK's motion, announced at the last meeting, was put from the chair, and being seconded by Sir GEORGE CLERK, was carried unanimously.

April 21. 1834.

MEMBER ELECTED.

ORDINARY.

JOHN DAVIE MORRIES, M. D.

November 24. 1834.

At a General Meeting held this day, JAMES RUSSELL, Esq. V. P. in the Chair, the following Office-bearers were appointed for the ensuing year:—

Lieut.-Gen Sir T. M. BRISBANE, President.

The Hon. Lord GLENLEE,
JAMES RUSSELL, Esq.
Dr T. C. HOPE,
Sir DAVID BREWSTER,
The Right Hon. Lord GREENOCK, } Vice-Presidents.

JOHN ROBISON, Esq. General Secretary.

Dr CHRISTISON,
Professor FORBES, } Secretaries to the Ordinary Meetings.

GEORGE FORBES, Esq. Treasurer.

Dr TRAILL, Curator of the Museum.

JOHN STARK, Esq. Assistant Curator.

COUNSELLORS.

Dr ABERCROMBIE.
Sir HENRY JARDINE.
ARTHUR CONNELL, Esq.
Professor JAMESON.
WILLIAM A. CADELL, Esq.
JAMES SKENE, Esq.
Rev. Dr CHALMERS.
Sir GEORGE CLERK, Bart.
LEONARD HORNER, Esq.
Dr J. H. DAVIDSON.
Sir GEORGE BALLINGALL.
JAMES T. GIBSON-CRAIG, Esq.

The following Committee was appointed to audit the Treasurer's accounts: —

Sir HENRY JARDINE.
Dr ABERCROMBIE.
Lord GREENOCK.

December 15. 1834.

MEMBERS ELECTED.

ORDINARY.

THOMAS JAMESON TORRIE, Esq.
WILLIAM COPLAND, Esq. of Colliston.
JOHN STEUART NEWBIGGING, Esq. W.S.
Rev. Dr WELSH, Professor of Church History in the University of Edin.

Sir HENRY JARDINE, with the approbation of the Council, gave notice of a motion for altering the Laws regarding Fees and Compositions.

January 5. 1835.

MEMBERS ELECTED.

ORDINARY.

CHARLES FORBES, Esq. JOHN HUTTON BALFOUR, M. D.

Sir HENRY JARDINE, as announced at last meeting, moved that the Laws II., III. and IV. be altered and stand thus:

" Law II. Every Ordinary Member, within three months after his election, shall pay Five Guineas as the fees of his admission, and Three Guineas as his contribution for the session in which he has been elected, and Three Guineas annually at the commencement of every session.

" Law III. Members on paying twenty-five years' Annual Contribution, shall be exempt from all farther payment.

" Law IV. Ordinary Members not residing in Scotland shall compound for the Annual Contribution at the rate of fifteen years' purchase."

The consideration of this motion was delayed till a subsequent meeting.

The Council having brought forward a list of eminent foreigners, and others, whom it was proposed to elect in room of various Honorary and Foreign Members lately deceased, Sir W. HAMILTON moved that this list be remitted to the Council for further consideration; which was seconded and agreed to.

Sir WILLIAM HAMILTON thereafter gave notice of a motion which he proposed to make, on the first meeting in February, relative to the proposed list of Honorary and Foreign Members, and to certain improvements he had to suggest in the constitution of the Society, with a view to a more perfect equilibrium and greater efficiency of the two classes of which the Society is composed.

January 19. 1835.

MEMBERS ELECTED.

ORDINARY.

Sir JOHN CAMPBELL, Attorney-General. DAVID GEORGE SANDEMAN, Esq.

Sir HENRY JARDINE's motion for an alteration in the Fees and Compositions was taken into consideration, and the farther discussion of it adjourned to a special meeting to be held for the purpose on the 26th January.

The Council reported that they could not recommend any alteration in the list of Honorary and Foreign members.

The President intimated that it had been proposed to the Council, " That personages of Royal blood in the Honorary List should be deemed to be Extraordinary Members of that class, and should not preclude the placing on it the names of twenty-one persons eminently distinguished in science or literature," according to Law XI. of the Society. It was resolved to postpone the consideration of this suggestion till a future meeting.

January 26. 1835.

At a Special General Meeting, Lord Greenock, V. P. in the Chair, Sir Henry Jardine's motion respecting Fees and Compositions was taken into further consideration, and agreed to by a large majority—an amendment to postpone coming to a decision on the clause regarding the Composition Fees for Members not resident in Scotland having been put and negatived. It was further determined, that the new Laws regarding Fees should not have a retrospective effect on the candidates admitted since the commencement of the present session till this date.

February 2. 1835.

MEMBER ELECTED.

ORDINARY.

John Mackean, Esq. Accountant.

Sir William Hamilton postponed his motion regarding the constitution of the Society; but gave notice that at the next meeting he would lay before the Society a proposal tending to improve its constitution and general well-being; in particular, to restore the equilibrium and efficiency of both its Classes, and would move for a Special Committee to take this proposal into consideration, and to report. He further intimated, that he should move,—

"That as the Royal Society was founded and chartered for the promotion of Literature and Physical Knowledge equally, and as the Literary and Physical Classes of which the Society consists possess in all respects equal privileges and rights, it is therefore expedient and just, that each Class should be adequately represented in both the Extraordinary Classes of Honorary and Foreign Members."

And, "That as the list of individuals proposed to the Society for election as Honorary and Foreign Members, wholly disregards the equality of the two constituent Classes, leaving to the Literary, out of a full complement of twenty-one Honorary Members, only one, and out of a full complement of thirty-six Foreign Members, only five places;—therefore the election of the proposed gentlemen shall be suspended until the general question in regard to the constitution of the Society and the rights of the several Classes be determined."

On the motion of Dr HOPE, it was unanimously resolved to postpone the ballot for the Honorary and Foreign Members suggested by the Council.

February 16. 1835.

MEMBER ELECTED.

ORDINARY.

WILLIAM BROWN, Esq. President of the Royal College of Surgeons.

Sir WILLIAM HAMILTON made his statement and motion announced at last meeting; and Dr HOPE made a reply in regard to that part of the mover's statement which related to the constitution of the Society. After a protracted discussion, it was moved as an amendment by Dr MACLAGAN, "That a Special Committee be appointed to consider and report if any measures can be adopted to improve the constitution and well-being of the Royal Society, and, in particular, to ascertain the rights, and promote the equilibrium and efficiency of both its Classes, to which Committee the proposals of Sir WILLIAM HAMILTON shall be referred." This amendment having been seconded by Mr HORNER, and put from the Chair, was carried by a considerable majority; and the following Committee was appointed:—

Dr ALISON.	JOHN ROBISON, Esq.	Professor PILLANS.
Dr TRAILL.	WILLAM WOOD, Esq.	THOMAS THOMSON, Esq.
LEONARD HORNER, Esq.	Professor BELL.	Rev. Archdeacon WILLIAMS.
Professor WALLACE.	Lord MEADOWBANK.	GEORGE FORBES, Esq.
Dr CHRISTISON.	J. SHANK MORE, Esq.	Sir W. HAMILTON.

Sir W. HAMILTON, *Convener.*

March 16. 1835.

MEMBER ELECTED.

ORDINARY.

THOMAS EDINGTON, Esq. F. G. S.

April 6. 1835

MEMBER ELECTED.

ORDINARY.

Reverend EDWARD CRAIG.

It was moved by Dr HOPE, seconded by Sir JOSEPH STRATON, and carried by a large majority, that the Society should proceed at its next meeting to ballot for the Honorary and Foreign Members proposed by the Council in December last.

A report was laid upon the table, prepared by a Committee of the Council, with the aid of a professional accountant, on the state of the Society's finances, and suggesting several alterations in the laws and practice of the Society, the chief articles of which had been previously, at the suggestion of the Committee, brought before the Society, and passed into laws on the 26th January.

April 20. 1835.

MEMBERS ELECTED.

ORDINARY.

R. Mayne, Esq.

HONORARY.

Dr Dalton. James Ivory, Esq. Baron de Prony.
Dr Faraday. Baron Poisson. Professor Airy.

FOREIGN.

Mons. Agassiz, Professor of Natural History, Neuchatel.
—— Cousin, Pair de France, &c.
—— Plana, Director of the Turin Observatory.
—— Quetelet, Director of the Brussels Observatory.
—— Struve, Director of the Dorpat Observatory.

November 23. 1836.

At a General Meeting held this day, for the purpose of electing Office-bearers, Dr Hope, V. P. in the Chair, announced that it was the wish of Mr Russell that he should not again be elected to the situation of Vice-President; upon which, on the motion of Dr Hope, it was resolved, by acclamation, that the cordial thanks of the Society be offered to Mr Russell for his long and valuable services to the Society, from the time of its institution in 1783. The following Office-bearers were then appointed:—

Lieut.-General Sir T. M. Brisbane, President.

Dr T. C. Hope,
Right Hon. Lord Greenock,
Reverend Dr Chalmers,
Sir David Brewster,
Hon. Lord Glenlee,
Dr Abercrombie,
} Vice-Presidents.

John Robison, Esq. General Secretary.

Dr Christison,
Professor Forbes,
} Secretaries to the Ordinary Meetings.

George Forbes, Esq. Treasurer.
Dr Traill, Curator of the Museum.
John Stark, Esq. Assistant Curator.

COUNSELLORS.

WILLIAM A. CADELL, Esq.	JAMES T. GIBSON-CRAIG, Esq.
JAMES SKENE, Esq.	Hon. Lord MEADOWBANK.
Sir GEORGE CLERK, Bart.	THOMAS THOMSON, Esq.
LEONARD HORNER, Esq.	Rev. Archdeacon WILLIAMS.
Dr J. H. DAVIDSON.	Dr WILLIAM GREGORY.
Sir GEORGE BALLINGALL.	Professor HENDERSON.

The following Committee was appointed to audit the Treasurer's accounts:—

Dr NEILL. J. T. GIBSON-CRAIG, Esq. Professor HENDERSON.

Dr TRAILL laid on the Table certain resolutions which he announced that he should move at the first meeting in January next, for modifying the Laws regarding Honorary and Foreign Members.

December 7. 1835.

MEMBERS ELECTED.

ORDINARY.

JAMES MONCREIFF, Esq. Advocate. JAMES STEWART WOOD, Esq.

January 18. 1836.

MEMBERS ELECTED.

ORDINARY.

WILLIAM PAUL, Esq. Accountant. ROBERT PAUL, Esq. Secretary to Commercial Bank.

Dr TRAILL brought forward, in an amended form, his motion announced on the 23d November, namely, that the Laws I., X., XI. and XII. should stand thus:

" LAW I. The Royal Society of Edinburgh shall consist of Ordinary and Honorary Fellows.

" LAW X. The Honorary Fellows shall not be subject to any contribution. This Class shall consist of persons eminent in Science or Literature. Its number shall not exceed Fifty-six, of whom Twenty may be British subjects, and Thirty-six subjects of foreign states.

" LAW XI. Personages of Royal blood may be elected Honorary Fellows, without regard to the limitation of numbers specified in Law X.

" LAW XII. Honorary Fellows may be proposed by the Council, or by a recommendation in the form given below, subscribed by three Ordinary Members; and in case the Council shall decline to bring this recommendation before the Society, it shall be competent for the proposers to bring the same before a General Meeting.

" The election shall be by ballot, after the proposal has been communicated *viva voce* from the Chair at one meeting, and printed in the circular for the meeting at which the ballot is to take place.

" *Form of Recommendation.*

" We hereby recommend ———— for the distinction of being made an Honorary Fellow; declaring that each of us, from his own knowledge of his services to [*Literature or Science, as the case may be*] believes him to be worthy of that honour.

" To the President and Council of the Royal Society of Edinburgh." [*Signatures of the three Ordinary Fellows.*]

The motion having been put from the Chair in this form, it was carried unanimously.

It was announced that the Council had adjudged the KEITH Prize for the biennial period, which had terminated with the last Session, to Professor FORBES, for his paper on the Refraction and Polarization of Heat.

February 15. 1836.

The KEITH Prize for the most important discovery recorded in the Society's Transactions during the years 1834 and 1835, was presented, according to the decision of the Council, to Professor FORBES, by Dr HOPE, V. P.

March 21. 1836.

MEMBERS ELECTED.

ORDINARY.

DAVID RHIND, Esq. Architect.
JAMES ANDERSON, Esq. Civil-Engineer.

April 18. 1836.

MEMBERS ELECTED.

ORDINARY.

MARTIN BARRY, M. D.
ROBERT STEUART, Esq. M. P.

May 2. 1836.

MEMBERS ELECTED.

ORDINARY.

ARCHIBALD ROBERTSON, M. D., F. R. S.
MACPHERSON GRANT, Esq. younger of Ballindalloch.
ALEX. GIBSON CARMICHAEL, Esq.
EDWARD SANG, Esq.
Rev. Profess. NICHOL, Univ. of Glasgow.

LIST OF THE PRESENT ORDINARY MEMBERS IN THE ORDER OF THEIR ELECTION.

His Majesty THE KING, Patron.

Date of Election.

Sir William Miller, Baronet, Lord Glenlee.

The above Gentleman is the only surviving member of the Edinburgh Philosophical Society.

1783. Honourable Baron Hume.

The Honourable Baron was associated with the Members of the Edinburgh Philosophical Society at the institution of the Royal Society in 1783.

THE FOLLOWING MEMBERS WERE REGULARLY ELECTED.

1784. Reverend Archibald Alison, LL. B. *Edinburgh.*
1787. James Home, M. D. *Professor of the Practice of Physic.*
1788. Thomas Charles Hope, M. D. F. R. S. Lond. *Professor of Chemistry.*
Right Honourable Charles Hope, *Lord President of the Court of Session.*
1795. The Very Reverend Dr George Husband Baird, *Principal of the University.*
1796. The Honourable Baron Sir Patrick Murray, Bart.
1798. Alexander Monro, M. D. *Professor of Anatomy, &c.*
1799. Sir George Stuart Mackenzie, Baronet, F. R. S. Lond.
Robert Jameson, Esq. *Professor of Natural History.*
1802. Colonel D. Robertson Macdonald.
1803. John Jamieson, D. D.
1804. William Wallace, Esq. *Professor of Mathematics.*
1805. Thomas Thomson, M. D. F. R. S. Lond. *Professor of Chemistry, Glasgow.*
1806. Robert Ferguson, Esq. *of Raith*, F. R. S. Lond.
George Dunbar, Esq. *Professor of Greek.*
1807. Sir James Montgomery, Baronet, *of Stanhope.*
John Campbell, Esq. *of Carbrook.*
Thomas Thomson, Esq. *Advocate.*
1808. James Wardrop, Esq. *Surgeon Extraordinary to his Majesty.*

Date of Election.

1808. Sir David Brewster, Knight, LL. D. F.R.S. Lond.

1811. Sir Charles Bell, Knight, F. R. S. Lond. *Professor of Surgery.*
Reverend Andrew Stewart, M. D. *Erskine.*
David Ritchie, D. D. *Emeritus Professor of Logic.*
Major-General Sir Thomas Makdougal Brisbane, Bart. K.C.B., F. R. S. Lond.
John Thomson, M. D. *Professor of General Pathology, Edinburgh.*
James Jardine, Esq. *Civil Engineer.*
Captain Basil Hall, R. N. F. R. S. Lond.
J. G. Children, Esq. F. R. S. Lond.
Alexander Gillespie, Esq. *Surgeon, Edinburgh.*
W. A. Cadell, Esq. F. R. S. Lond.
Macvey Napier, Esq. F. R. S. Lond. *Professor of Conveyancing.*
James Pillans, Esq. *Professor of Humanity.*
Sir George Clerk, Baronet, F. R. S. Lond.
Daniel Ellis, Esq. *Edinburgh.*

1813. William Somerville, M. D. F. R. S. *London.*
J. Henry Davidson, M. D. *Edinburgh.*

1814. Sir Henry Jardine, *King's Remembrancer in Exchequer.*
Patrick Neill, LL. D. *Secretary to the Wernerian and Horticultural Societies.*
Right Honourable Lord Viscount Arbuthnot.
Reverend John Thomson, *Duddingston.*
John Fleming, D. D. *Professor of Natural Philosophy, King's Coll. Aberdeen.*
John Cheyne, M. D. *Dublin.*
Alexander Brunton, D. D. *Professor of Oriental Languages.*
Professor George Glennie, *Marischal College, Aberdeen.*

1815. Robert Stevenson, Esq. *Civil Engineer.*
Sir Thomas Dick Lauder, Baronet, *of Fountainhall.*
Henry Home Drummond, Esq. *of Blair-Drummond.*
Charles Granville Stuart Menteath, Esq. *of Closeburn.*
William Thomas Brande, Esq. F. R. S. Lond. and *Professor of Chemistry in the Royal Institution.*

1816. Colonel Thomas Colby, F. R. S. Lond. *Royal Engineers.*
Leonard Horner, Esq. F. R. S. Lond.
Henry Colebrooke, Esq. *Director of the Asiatic Society of Great Britain.*
George Cooke, D.D. *Professor of Moral Philosophy, St Andrew's.*
Right Hon. William Adam, *Lord Chief Commissioner.*
Honourable Lord Fullerton.
Thomas Jackson, LL. D. *Professor of Natural Philosophy, St Andrew's.*
John Robison, Esq. *Edinburgh.*
Hugh Murray, Esq. *Edinburgh.*

Date of Election.

1817. Right Honourable Earl of Wemyss and March.
John Wilson, Esq. *Professor of Moral Philosophy.*
Hon. Lord Meadowbank.
Sir James Hamilton Dickson, M.D. *Clifton.*
William P. Alison, M. D. *Professor of the Theory of Physic.*
James Skene, Esq. *of Rubislaw.*
Reverend Robert Morehead, *Northumberland.*
Robert Bald, Esq. *Civil Engineer.*

1818. Robert Richardson, M. D. *Harrowgate.*
Harry William Carter, M. D. *Oxford.*
Patrick Miller, M. D. *Exeter.*
John Craig, Esq. *Edinburgh.*
John Watson, M. D.
John Hope, Esq. *Dean of Faculty.*
William Ferguson, M. D. *Windsor.*

1819. Right Honourable Lord John Campbell, F. R. S. Lond. and M. R. I. A.
Patrick Murray, Esq. *of Simprim.*
James Muttlebury, M. D. *Bath.*
Thomas Stewart Traill, M. D. *Professor of Medical Jurisprudence.*
Alexander Adie, Esq. *Optician, Edinburgh.*
William Couper, M. D. *Glasgow.*
Marshall Hall, M.D. *Nottingham.*
John Borthwick, Esq. *Advocate.*
Richard Phillips, Esq. F. R. S. Lond.
Reverend William Scoresby, *Exeter.*
George Forbes, Esq. *Edinburgh.*

1820. James Hunter, Esq. *of Thurston.*
Right Honourable David Boyle, *Lord Justice-Clerk.*
James Keith, Esq. *Surgeon, Edinburgh.*
Right Hon. Sir Samuel Shepherd.
James Nairne, W. S. *Edinburgh.*
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Lieutenant-Colonel M. Stewart.
Charles Babbage, Esq. F. R. S. Lond.
Thomas Guthrie Wright, Esq. *Auditor of the Court of Session.*
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Adam Anderson, A. M. LL. D. *Rector of the Perth Academy.*
John Shank More, Esq. *Advocate.*
George Augustus Borthwick, M. D. *Edinburgh.*
Robert Dundas, Esq. *of Arniston.*

Date of Election.

1820. Samuel Hibbert, M. D.
Robert Haldane, D. D. *Principal of St Mary's College, St Andrew's.*
Sir John Meade, M. D. *Weymouth.*
Dr William Macdonald *of Ballyshear.*
Sir John Hall, Baronet, *of Dunglass.*
Sir John Hay, Baronet, *of Smithfield and Hayston*, M. P.
Sir George Ballingall, M. D. *Professor of Military Surgery.*

1821. Major-General Sir Joseph Straton, K. C. B.
Robert Graham, M. D. *Professor of Botany.*
Sir James M. Riddell, Baronet, *of Ardnamurchan.*
Archibald Bell, Esq. *Advocate.*
John Clerk Maxwell, Esq. *Advocate.*
John Lizars, Esq. *Professor of Surgery to the Royal College of Surgeons, Edin.*
John Cay, Esq. *Advocate.*
Robert Kaye Greville, LL. D. *Edinburgh.*
Robert Hamilton, M. D. *Edinburgh.*
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Sir David Milne, K. C. B.
A. R. Carson, Esq. LL. D. *Rector of the High School.*

1822. Sir Francis Chantrey, F. R. S. Lond.
James Smith, Esq. *of Jordanhill*, F. R. S. Lond.
William Bonar, Esq. *Edinburgh.*
Rev. H. Parr Hamilton, *late Fellow of Trinity College, Cambridge.*
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George A. Walker Arnott, Esq. *Advocate.*
Rev. John Lee, D. D. *one of the Ministers of Edinburgh.*
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Lieutenant-Colonel Martin White, *Edinburgh.*
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George Joseph Bell, Esq. *Professor of Scots Law.*
Dr William Dyce, *Aberdeen.*
W. C. Trevelyan, Esq. *Wallington.*
Sir Robert Abercromby, Baronet, *of Birkenbog.*
Thomas Shortt, M. D. *Edinburgh.*
Dr Wallich, *Calcutta.*

1823. The Right Honourable Sir George Warrender, Baronet, *of Lochend.*
John Russell, Esq. W. S. *Edinburgh.*
John Shaw Stewart, Esq. *Advocate.*
Alexander Hamilton, M. D. *Edinburgh.*
Right Honourable Sir William Rae, Baronet, *of St Catherine's.*

Date of Election.

1823. William Cadell, Esq. *of Cockenzie.*
Sir Edward Ffrench Bromhead, Baronet, A.M. F.R.S. Lond. *Thurlsby Hall.*
Sir Andrew Halliday, M. D.
Captain Thomas David Stuart, *of the Hon. East India Company's Service.*
Andrew Fyfe, M. D. *Lecturer on Chemistry, Edinburgh.*
Robert Bell, Esq. *Advocate, Procurator for the Church of Scotland.*
Captain Norwich Duff, R. N.
Warren Hastings Anderson, Esq.
Alexander Thomson, Esq. *of Banchory, Advocate.*
Liscombe John Curtis, Esq. *Ingsdon House, Devonshire.*
Robert Knox, M. D. *Lecturer on Anatomy, Edinburgh.*
Robert Christison, M. D. *Professor of Materia Medica.*
John Gordon, Esq. *of Cairnbulg.*

1824. Dr Lawson Whalley, *Lancaster.*
William Bell, Esq. W. S. *Edinburgh.*
Alexander Wilson Philip, M. D. *London.*
James Hamilton, M. D. *Professor of Midwifery in the University of Edinburgh.*
Sir Charles Adam, R. N.
Robert E. Grant, M. D. *Professor of Comparative Anatomy in the London University.*
Claud Russell, Esq. *Accountant, Edinburgh.*
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John Argyle Robertson, Esq. *Surgeon, Edinburgh.*
James Pillans, Esq. *Edinburgh.*
James Walker, Esq. *Civil-Engineer.*
William Newbigging, Esq. *Surgeon, Edinburgh.*
William Wood, Esq. *Surgeon, Edinburgh.*

1825. The Venerable Archdeacon John Williams, *Rector of the Edinburgh Academy.*
W. Preston Lauder, M. D. *London.*
Right Honourable Lord Ruthven.
Major Sir E. Leith Hay *of Rannes.*
Edward Turner, M. D. *Professor of Chemistry in the London University.*
Dr Reid Clanny, *Sunderland.*
Sir John Archibald Stewart, Baronet, *of Grandtully.*
Sir William Jardine, Baronet, *of Applegarth.*
Alexander Wood, Esq. *Advocate.*
Rev. Dionysius Lardner, LL. D. *London.*

1826. George Macpherson Grant, Esq. *of Ballindalloch.*

Date of Election.

1826. William Renny, Esq. W. S. *Solicitor of Stamps.*
Elias Cathcart, Esq. *Advocate.*
Andrew Clephane, Esq. *Advocate.*
Rev. George Coventry.
Sir David Hunter Blair, Baronet.
George Moir, Esq. *Advocate, Professor of Rhetoric and Belles Lettres.*
John Stark, Esq. *Edinburgh.*
Dr Macwhirter, *Edinburgh.*
1827. John Gardiner Kinnear, Esq. *Edinburgh.*
William Burn, Esq. *Edinburgh.*
James Russell, M. D. *Edinburgh.*
Prideaux John Selby, Esq. *Twizel House, Northumberland.*
Henry Thornton Maire Witham, Esq. *of Lartington.*
Rev. Dr Robert Gordon, *one of the Ministers of Edinburgh.*
James Wilson, Esq. *Edinburgh.*
Rev. Edward Bannerman Ramsay, A. B. *of St John's College, Cambridge.*
Right Rev. Bishop James Walker, D. D. *Edinburgh.*
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George Swinton, Esq. *Edinburgh.*
1828. Sir Francis Walker Drummond, Baronet, *of Hawthornden.*
Erskine Douglas Sandford, Esq. *Advocate.*
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Major Maxwell, *K. D. Guards.*
John Forster, Esq. *Architect, Liverpool.*
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Thomas Hamilton, Esq. *Edinburgh.*
David Milne, Esq. *Advocate.*
Dr Manson, *Nottingham.*
William Burn Callender, Esq.
1829. A. Colyar, Esq.
William Gibson-Craig, Esq. *Advocate.*
James Ewing, LL. D. *Glasgow.*
Charles Fergusson, Esq. *Advocate.*
Duncan Macneill, Esq. *Sheriff-depute of Perthshire.*
Rev. John Sinclair, A. M. *Pembroke College, Oxford.*
Arthur Connell, Esq. *Advocate.*
James Hope Vere, Esq. *of Craigiehall.*
Bindon Blood, M. R. I. A.
James Walker, Esq. W. S.
William Bald, Esq. M. R. I. A.

Date of Election.

1829. Sir Whitelaw Ainslie, M. D. M. R. A. S.
1830. Colonel Pitman, *Hon. East India Company's Service.*
J. T. Gibson-Craig, Esq. W. S.
Archibald Alison, Esq. *Advocate, Sheriff-depute of Lanarkshire.*
Hon. Mountstuart Elphinstone.
James Syme, Esq. *Professor of Clinical Surgery.*
Thomas Brown, Esq. *of Langfine.*
James L'Amy, Esq. *Advocate, Sheriff-depute of Forfarshire.*
Thomas Barnes, M. D. *Carlisle.*
1831. James D. Forbes, Esq. F. R. S. Lond. *Professor of Natural Philosophy.*
Right Honourable James Abercromby, *Speaker of the House of Commons.*
John Abercrombie, M. D. *Edinburgh, First Physician to his Majesty in Scotland.*
Donald Smith, Esq.
Captain Samuel Brown, R. N.
O. Tyndal Bruce, Esq. *of Falkland.*
David Boswell Reid, M. D. *Lecturer on Chemistry, Edinburgh.*
T. S. Davies, Esq. A. M. *Woolwich.*
1832. John Sligo, Esq. *of Carmyle.*
Major Alexander Anderson.
James Dunlop, Esq. *Astronomer, New South Wales.*
James F. W. Johnston, A. M. *Reader of Chemistry in the University of Durham.*
William Gregory, M. D. *Edinburgh.*
Robert Allan, Esq. *Advocate.*
Robert Morrieson, Esq. *Hon. E. I. C. Civil Service.*
Montgomery Robertson, M. D.
William Dyce, Esq. A. M.
1833. Captain Milne, R. N.
Alexander Earle Monteith, Esq. *Advocate.*
George Meikle, Esq. *Surgeon Hon. E. I. C. Service.*
His Grace the Duke of Buccleuch.
A. T. J. Gwynne, Esq.
David Craigie, M. D. *Edinburgh.*
George Buchanan, Esq. *Civil-Engineer.*
Sir John Stuart Forbes, Baronet, *of Pitsligo.*
John Dunlop, Esq. *Advocate.*
Alexander Hamilton, Esq. W. S.
Right Honourable Lord Greenock.
1834. Mungo Ponton, Esq. W. S.

Date of Election.

1834. Isaac Wilson, M. D. F. R. S. Lond.
Lieutenant-Colonel Murray *of Ochtertyre.*
David Low, Esq. *Professor of Agriculture.*
Thomas Henderson, Esq. *Professor of Astronomy.*
Rev. Dr Chalmers, *Professor of Divinity.*
William Macgillivray, Esq. *Keeper of the Museum of Royal Coll. of Surgeons.*
Alexander Kinnear, Esq.
Patrick Boyle Mure, Esq. *Advocate.*
Thomas Balfour, Esq. *Advocate.*
John Davie Morries, M. D.
Thomas Jameson Torrie, Esq.
William Copland, Esq. *of Colliston.*
John Steuart Newbigging, Esq. W. S.
Rev. Dr Welsh, *Professor of Church History.*
John Haldane, Esq. *Haddington.*
1835. Charles Forbes, Esq.
John Hutton Balfour, M. D.
Sir John Campbell, M. P. *Attorney-General.*
John Mackean, Esq. *Accountant.*
William Brown, Esq. *Surgeon, Edinburgh.*
Thomas Edington, F. G. S.
Reverend Edward Craig.
R. Mayne, Esq.
John Stewart Wood, Esq.
1836. William Paul, Esq. *Accountant.*
Robert Paul, Esq. *Secretary to Commercial Bank.*
David Rhind, Esq. *Architect.*
James Anderson, Esq. *Civil-Engineer.*
Martin Barry, M. D.
Archibald Robertson, M. D. F. R. S. Lond.
J. Macpherson Grant, Esq. *younger of Ballindalloch.*
Alexander Gibson Carmichael, Esq.
Edward Sang, Esq. *Lecturer on Natural Philosophy, Edinburgh.*
Rev. J. P. Nichol, *Professor of Practical Astronomy, University of Glasgow.*
William Sharpey, M. D.

LIST OF NON-RESIDENT AND FOREIGN MEMBERS,

ELECTED UNDER THE OLD LAWS.

NON-RESIDENT.

Right Honourable Lord Wallace.
Rev. Bishop Gleig, *Stirling*.
Charles Hatchett, Esq. F. R. S. Lond.
Sir William Blizzard, M. D. F. R. S. Lond.
Thomas Blizzard, Esq.
Sir William Ouseley, Baronet.
Sir James Macgrigor, Baronet, M. D.
Richard Griffiths, Esq. *Civil-Engineer*.

FOREIGN.

M. Le Chevalier, *Paris*.
Dr S. L. Mitchell, *New York*.
M. P. Prevost, *Geneva*.

LIST OF HONORARY FELLOWS.

His Majesty the King of the Belgians.
His Royal Highness the Duke of Sussex.
His Imperial Highness the Archduke John of Austria.
His Royal Highness the Archduke Maximilian.

BRITISH SUBJECTS (LIMITED TO TWENTY BY LAW X.)

Robert Brown, Esq. F. R. S.
Davies Gilbert, Esq. V. P. R. S.
Sir John F. W. Herschel, F. R. S.
Dr Dalton, F. R. S.
Dr Faraday, F. R. S.
James Ivory, Esq. K. H. F. R. S.
G. B. Airy, Esq. F. R. S. *Astronomer Royal.*

THE FOLLOWING EIGHT NAMES WERE INCLUDED WITH THE ABOVE PRIOR TO THE CHANGE IN THE LAW, 18TH JANUARY 1836.

Le Baron Humboldt,	*Berlin.*
M. Gay Lussac,	*Paris.*
M. Biot,	*Do.*
M. Arago,	*Do.*
Chevalier Hammer.	
J. Berzelius,	*Stockholm.*
Le Baron Poisson,	*Paris.*
Le Baron de Prony,	*Do.*

FOREIGNERS (LIMITED TO THIRTY-SIX.)

	M. Brochant,	*Paris.*
	Le Baron Von Buch,	*Berlin.*
	M. Gauss,	*Göttingen.*
	M. Blumenbach,	*Do.*
5.	M. Simond de Sismondi.	*Geneva.*
	Le Baron Degerando,	*Paris.*

	Le Baron Krusenstern,	*St Petersburgh.*
	M. Oersted,	*Copenhagen.*
	M. Schumacher,	*Altona.*
10.	M. Mohs,	*Freyberg.*
	N. Bowdich, Esq.	*United States.*
	Le Baron Larrey,	*Paris.*
	Sir Henry Bernstein,	*Berlin.*
	M. De Candolle,	*Geneva.*
15.	Dr Olbers,	*Bremen.*
	Bishop Munter,	*Zealand.*
	Le Baron Dupin,	*Paris.*
	M. Brongniart,	*Do.*
	Chevalier Bürg,	*Vienna.*
20.	M. Bessel,	*Konigsberg.*
	M. Thenard,	*Paris.*
	M. Haidinger,	*Vienna.*
	M. Mitscherlich,	*Berlin.*
	M. G. Rose,	*Berlin.*
25.	M. G. Moll,	*Utrecht.*
	M. Hausmann,	*Göttingen.*
	J. J. Audubon, Esq.	*United States.*
	Chevalier Bouvard,	*Paris.*
	M. L. A. Necker,	*Geneva.*
30.	M. Agassiz,	*Neuchatel.*
	Le Baron Cousin,	*Paris.*
	M. Plana,	*Turin.*
	M. Quetelet,	*Brussels.*
	M. Struve,	*Dorpat*
35.	M. Dulong,	*Paris*

LIST OF FELLOWS DECEASED, RESIGNED, AND CANCELLED,

FROM 1833 TO 1836.

(This List is necessarily incomplete.)

Sir Gilbert Blane, M. D. F. R. S. Lond.
Right Honourable Sir Robert Liston, Bart.
Sir Henry Steuart, Baronet, *of Allanton.*
John Gillies, LL. D. *Historiographer to his Majesty.*
Rev. Dr Brinkley, F. R. S. Lond. *Pres. Royal Irish Academy.*
M. Stromeyer, *Professor of Chemistry, Göttingen.*
James Hamilton, M. D. *senior, Edinburgh.*
James Russell, Esq. *late Professor of Clinical Surgery, Edin. and Vice-President.*
Right Honourable Sir John Sinclair, Bart.
Thomas Macknight, D. D. *one of the Clergymen of Edinburgh.*
Honourable Lord Robertson, *late Senator of the College of Justice.*
Rev. Dr Andrew Brown, *Professor of Rhetoric.*
General Dyce.
Thomas Sivright, Esq. *of Meggetland.*
Right Honourable Lord Napier.
John H. Wishart, Esq. *Fellow of the Royal College of Surgeons, Edinburgh.*
James Buchan, M. D. *Fellow of the Royal College of Physicians, Edinburgh.*
Sir Robert Dundas, Baronet, *of Beechwood.*
John Bonar, Esq. *of Kimmerghame.*
Andrew Skene, Esq. *Advocate, late Solicitor-General for Scotland.*
John W. Turner, *Professor of Surgery, University of Edinburgh.*
Edward Troughton, Esq. F. R. S. Lond.
Sir William Knighton, Baronet.
James Weddell, Esq. R. N.
D. G. Sandeman, Esq.
M. Ampere, *Member of the French Institute.*
Dr Hosack, *United States.*

RESIGNATIONS.

William Fraser Tytler, Esq. *Advocate.*
Lieutenant-Colonel Tytler.
Sir William Hamilton, Baronet.
James Tytler, Esq. *of Woodhouselee.*
John Gladstone, Esq. *of Fasque.*

ELECTIONS CANCELLED.

John Reddie, Esq. LL. D. *Edinburgh.*
Anthony Dickson, Esq. *late President of Bengal Medical Board.*
George Anderson, Esq. *Rector of Inverness Academy.*
A. N. Macleod, Esq. *of Harris.*

LIST OF DONATIONS.

(Continued from Vol. XII. p. 574.)

December 2. 1833.

DONATIONS.	DONORS.
Flora Batava. Nos. 93 and 94.	King of Holland.
The Fortunate Union, a Romance. Translated from the Chinese original, with Notes and Illustrations, by John Francis Davis, F.R.S. 2 vols.	Oriental Translation Fund.
Hoeï-Lan-Ki, ou l'Histoire du Cercle de Craie, Drame en Prose et en Vers, traduit du Chinois, et accompagné de Notes. Par Stanislas Julien.	Ditto.
San Kokf Tsou Ran To Sets, ou Aperçu Général des Trois Royaumes. Traduit de l'original Japonais-Chinois, by Mr J. Klaproth.	Ditto.
The Shah Nameh of the Persian Poet Firdoosi. Translated and Abridged, in Prose and Verse, with Notes and Illustrations, by James Atkinson, Esq.	Ditto.
History of the Pirates who infested the China Sea from 1807 to 1810. Translated from the Chinese original, with Notes and Illustrations, by Charles Fried. Neumann.	Ditto.
The Life of Hafiz Ool-Moolk, Hafiz Rehmut Khan, written by his Son. Abridged and Translated from the Persian, by Charles Elliot, Esq.	Ditto.
The Geographical Works of Sádik Isfáhani. Translated by J. C. from Original Persian MSS. in the collection of Sir William Ouseley, the Editor.	Ditto.
The Algebra of Mahommed Ben Musa. Edited and Translated by Frederic Rosen.	Ditto.
The Life of Sheikh Mahommed Ali Hazin, written by himself. Translated from two Persian Manuscripts, and illustrated with Notes, by F. C. Belfour, M. A., Oxon, F. R. A. S., LL. D.	Ditto.
History of the War in Bosnia, during the years 1737–8–9. Translated from the Turkish by C. Fraser, Professor of German in the Naval and Military Academy, Edinburgh.	Ditto.
Customs and Manners of the Women of Persia, and their Domestic Superstitions. Translated from the original Persian Manuscript, by James Atkinson, Esq.	Ditto.

DONATIONS.	DONORS.
Miscellaneous Translations from Oriental Languages. Vol. I.	Oriental Translation Fund.
Yakkun Nattannawā; and Kōlan Nattannawā. Cingalese Poems. Translated by John Callaway.	Ditto.
Memoirs of a Malayan Family, written by themselves, and translated from the original by W. Marsden, F. R. S., &c. &c.	Ditto.
The Siyar-ul-Mutakherin; A History of the Mahommedan Power in India during the last century. By Mir Gholam Hussein-Khan. Revised from the Translation of Haji Mustefa, and collated with the Persian original by John Briggs, M. R. A. S., Lieutenant-Col. in the Madras Army.	Ditto.
History of the early Kings of Persia. Translated from the original Persian of Mirkhond, by David Shea.	Ditto.
The History of the Maritime Wars of the Turks. Translated from the Turkish of Haji Khalifeh, by James Mitchell. Chapters 1 to 4.	Ditto.
The History of Vartan, and of the Battle of the Armenians; containing an Account of the Religious Wars between the Persians and the Armenians. By Elisæus, Bishop of the Amadunians. Translated from the Armenian by C. F. Neumann.	Ditto.
The Mulfuzāt Timūry, or Auto-Biographical Memoirs of the Moghul Emperor Timūr. Translated from the Persian by Major Charles Stewart.	Ditto.
The Adventures of Hatim Tai; a Romance. Translated from the Persian by Duncan Forbes, A. M.	Ditto.
History of the Afghans. Translated from the Persian of Neamet Ullah by Bernard Dorn. Part 1.	Ditto.
Hān Koong Tsew, or the Sorrows of Hān; a Chinese Tragedy. Translated from the original, with Notes, by John Francis Davis, F.R.S.	Ditto.
The Travels of Macarius, Patriarch of Antioch; written by his attendant Archdeacon Paul of Aleppo, in Arabic. Translated by F. C. Belfour, A. M., Oxon. Parts 1, 2, and 3.	Ditto.
Memoirs of the Emperor Jahangueir, written by himself; and translated from a Persian manuscript by Major David Price.	Ditto.
Raghuvansa Kalidasæ, Carmen Sanskritè et Latinè edidit Adolphus Fredericus Stenzler.	Ditto
Private Memoirs of the Moghul Emperor Humāyūn. Written in the Persian language by Jouher, a confidential domestic of his Majesty. Translated by Major Charles Stewart, M. R. A. S., &c.	Ditto.
Annals of the Turkish Empire, from 1591 to 1659 of the Christian Era, by Naiman. Translated from the Turkish by Charles Fraser. Vol. 1.	Ditto.
Sexagesimal Logarithms.	Fletcher Raincock, Esq.
Astronomische Nachrichten. Nos. 39 and 40.	Prof. Schumacher.

DONATIONS.	DONORS.
Alphabetical Index of the Transactions of the Royal Society of London, from vol. cxi. to cxx. inclusive.	The Society.
Abhandlungen der Koniglichen Akademie der Wissenschaften zu Berlin, aus den Jahre 1830 und 1831. 2 vols.	The Academy.
Reports of the Commissioners appointed to fix the boundaries of the English Burghs under the Reform Bill. 10 vols.	—— Drummond, Esq.
Reports of the Commissioners appointed to fix the boundaries of the Irish Burghs under the Reform Bill. 1 vol.	Ditto.
Transactions of the Cambridge Philosophical Society. Vol. v. part 1.	The Society.
Recueil de Voyages et de Mémoires publié par la Société de Géographie de Paris. 3 tomes.	Ditto.
The Internal Structure of Fossil Vegetables, found in the Carboniferous and Oolitic Deposits of Great Britain, described and illustrated. By Henry T. M. Witham of Lartington, F. G. S., F. R. S. E.	The Author.
The Quarterly Journal of Agriculture; and the Prize Essays and Transactions of the Highland Society of Scotland. No. 21.	Highland Society.
Mémoire Explicatif des Phénomènes de l'Aiguille Aimantée. Par Demonville.	The Author.
Sur la possibilité de mesurer l'Influence des Causes qui modifient les Elémens Sociaux. Par A. Quetelet.	The Author.
Bulletin de l'Académie Royale des Sciences et Belles-Lettres de Bruxelles, 1832–33. Nos. 1–12.	The Society.
Recherches sur les Poids de l'homme aux differens Ages. Par A. Quetelet.	The Author.
Philosophical Transactions of the Royal Society of London, 1833. Part 1.	The Society.
Address to the third General Meeting of the British Association for the Advancement of Science. By the Rev. W. Whewell.	Professor Forbes.
A Letter to the Members of the Temperance Society. By James Henry, M. B.	The Author.
Miliaria accuratius Descripta, a Jacobo Henry, M. D.	Ditto.
Catalogue of Preparations, &c. in Morbid, Natural, and Comparative Anatomy, contained in the Museum of the Army Medical Department, Fort Pitt, Chatham.	Sir James M'Grigor, Bart.
Bulletin de la Société Géologique de France. Tome iii. Feuilles 6–9 and 14–16.	The Society.
arometrical Tables for the use of Engineers, Geologists, and Scientific Travellers. By William Galbraith, M. A.	The Author.
Transactions of the Literary and Historical Society of Quebec. Vol. iii. Parts 1 and 2.	The Society.
Transactions of the Royal Irish Academy. Vol. xv.	The Academy.
Asiatic Researches, or Transactions of the Society instituted in Bengal, for inquiring into the History, the Antiquities, the Arts and Sciences, and Literature of Asia. Vol. xvii.	The Society.

DONATIONS.	DONORS.
The American Journal of Science and Arts. Conducted by Benjamin Silliman, M. D., LL. D., Professor of Chemistry and Mineralogy, &c. in Yale College. Vols. xx.–xxiv.	The Editor.
Transactions of the Zoological Society of London. Vol. i. part 1.	
Proceedings of the Committee of Science and Correspondence of the Zoological Society of London. Parts 1 and 2.	The Society.
Tabelle über die Geologie, von Hermann von Meyer.	The Author.
Lettre à la Nation Anglaise, sur l'Union des Peuples et la Civilization comparée; sur l'Instrument économique du Tems, appelé Biometre, ou Montre Morale. Par Marc-Antoine Jullien de Paris.	Ditto.
Proceedings of the Geological Society of London, 1833. Nos. 31–32.	
List of the Members of the Geological Society of London, 1833.	The Society.
Memorie della Reale Academia delle Scienze di Torino. Tome xxxvi.	The Academy.
Transactions of the Royal Asiatic Society of Great Britain and Ireland. Vol. i. parts 1 and 2, vol. iii. parts 1 and 2.	The Society.
Conjectures relative to the Origin of Numerical Hieroglyphics. By T. S. Davies.	The Author.
Bulletin de la Société d'Encouragement par l'Industrie Nationale, pour les Années 1824–33. Jan. Fevr. Mars, Avril, Mai, Juin.	The Society.
Astronomical Observations made at the Royal Observatory at Greenwich. By the Rev. Dr N. Maskelyne. Vols. 2, 3, and 4.	Royal Astronomical Society.
Astronomical Observations made at the Royal Observatory at Greenwich, from 1811 to 1832, 44 parts. By John Pond, Esq.	Ditto.
Mémoires de l'Académie Impériale des Sciences de St Petersburg, VIme Série (Sciences, Mathématiques, Physiques, et Naturelles). Tomes i. and ii. Livrs 1, 2, 3, 4.	Imperial Academy.
Mémoires de l'Académie Impériale des Sciences de St Petersburg, VIme série (Sciences, Politiques, Histoire, et Philologie.) Tomes i. and ii.	Ditto.
Mémoires de l'Académie Impériale des Sciences de St Petersburg. Presentés par divers Savans, et lus dans ses Assemblées. Tome i. Livr. 1–6.	Ditto.
Recueil des Actes de la Séance publique de l' Académie Impériale des Sciences de St Petersburg, 1828–32. 5 parts.	Ditto.
Verzeichniss der Pflanzen. Vom Dr C. Anton Meyer.	The Author.
Catalogue Raisonné des Objets de Zoologie recueillis dans un Voyage au Caucase. Par E. Ménétries.	Ditto.
Hyperanthraxis, or the Cholera of Sunderland. By W. Reid Clanny, M. D., F. R. S. E., M. R. I. A.	Ditto.
December 16.	
Transactions of the Agricultural and Horticultural Society of India. Vol. ii. parts 1. and 2.	The Society.

DONATIONS.	DONORS.
January 20. 1834.	
Proceedings of the Royal Society, No. 13, and Statement of the Receipts and Payments of the Royal Society between Nov. 29. 1832 and Nov. 29. 1833.	The Society.
Researches on Spherical Geometry, Polar Triangles, &c. By T. S. Davies, Esq.	The Author.
Bulletin de la Société Géologique de France. Tome iii. Feuilles, 17–24.	The Society.
Memoirs of the Royal Astronomical Society. Vol. vi.	Ditto.
Astronomical Observations, made at the Royal Observatory at Greenwich, in October, November, and December 1832, and January, February, and March 1833. By John Pond, Esq.	The Author.
Observations of Nebulæ and Clusters of Stars, made at Slough with a twenty feet reflector, between the years 1825 and 1833. By Sir J. F. W. Herschel, K. H.	Ditto.
On the Absorption of Light by Coloured Media, viewed in connexion with the Undulatory Theory. By Sir J. F. W. Herschel, K. H.	Ditto.
Supplement to Dr Bradley's Miscellaneous Works, with an Account of Harriat's Astronomical Works. By Professor Rigaud.	Ditto.
Mémoire sur le Choléra-Morbus compliqué d'une Epidémie de Fièvre Jaune, qui a regné simultanément à la Nouvelle-Orléans en 1832. Par M. Michel Halphen, Docteur en Médecine.	Ditto.
Astronomische Nachrichten. Nos. 241 to 250.	Prof. Schumacher.
On the Influence of Colour and Heat on Odours. By James Stark, M. D.	The Author.
Beiträge zur Petrefactenkunde. Von Hermann von Mayer.	Ditto.
February 3.	
Transactions of the Society of Arts, Manufactures, and Commerce, vol. xlix.	The Society.
Entomologia Edinensis; or a History and Description of the Insects found in the Neighbourhood of Edinburgh. By James Wilson, Esq., and the Rev. James Duncan.	The Authors.
February 17.	
History of the Berwickshire Naturalist's Club, instituted September 1831.	The Club.
Mémoires de la Société Géologique de France. Tome i. part 1.	The Society.
Mémoires de la Société de Physique et d'Histoire Naturelle de Génève. Tome ii. Part 2. Tomes iii. and iv.	Ditto.
Mémoires de l'Académie Royale des Sciences de l'Institut de France. Tome xii.	The Institute.

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March 3.	
Letter to his Grace the Duke of Hamilton and Brandon, respecting the Parochial Registers of Scotland. By James Cleland, LL. D., &c.	The Author.
Quarterly Journal of Agriculture; and the Prize Essays and Transactions of the Highland Society of Scotland. No. 24.	Highland Society.
Print of the Statue of Sir Joseph Banks, Bart., G. C. B.	Committee for conducting its execution.
April 7.	
List of the Fellows of the Royal Society. November 30. 1833.	The Society.
Address delivered at the Anniversary Meeting of the Royal Society, on Saturday, Nov. 30. 1833, by His Royal Highness the Duke of Sussex, K. G., &c. &c. &c., the President.	Ditto.
Philosophical Transactions of the Royal Society of London, for the year 1833, part 2.	Ditto.
Proceedings of the Royal Society, 1832–33, No. 14.	Ditto.
Astronomical Observations made at the Royal Observatory at Greenwich, in April, May, June, July, August, and September 1833. By John Pond, Esq. Astronomer-Royal.	
Supplements to the Greenwich Observations for the years 1830–32. By John Pond, Esq. Astronomer-Royal.	
Catalogue of 1112 Stars, reduced from Observations made at the Royal Observatory at Greenwich, from the years 1816 to 1833.	The Society.
Nouveaux Mémoires de l'Académie Royale de Bruxelles. Tomes ii, iii, iv, v, and vii.	Royal Academy of Brussels.
Mémoires de Prix de l'Académie Royale de Bruxelles. Tomes ii, iii, v, vi, vii, ix.	Ditto.
Notices et Extraits des Manuscrits de la Bibliothêque dite de Bourgogne, rélatifs aux Pays-Bas; publiés par l'Académie Royale des Sciences et Belles-Lettres, pour faire suite à ses Mémoires. Par le Baron de Reiffenberg. Tome i, part 1.	Ditto.
Rapport à Monsieur le Ministre de l'Intérieur sur les Travaux de l'Académie Royale des Sciences et Belles-Lettres de Bruxelles depuis le mois de Juillet 1830.	Ditto.
Bulletin de l'Académie Royale des Sciences et Belles-Lettres de Bruxelles, 1833–34. Nos. 15–19.	Ditto.
Statistique des Tribunaux de la Bélgique, pendant les années 1826–30. Par MM. A. Quetelet, Directeur de l'Observatoire de Bruxelles, et Ed. Smits, Directeur du Bureau de Statisque.	The Authors.
Recherches sur les Degrés successifs de Force Magnétique qu'une Aiguille d'Acier reçoit pendant les Frictions multiples qui servent à l'aimanter. Par M. Quetelet.	The Author.

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Astronomische Nachrichten. Nos. 251–257.	Prof. Schumacher.
Essai sur quelques Zodaiques apportés des Indes. Par M. de Paravey.	The Author.
Etudes sur l'Archéologie. Par M. de Paravey.	Ditto.
Planum et Statuta Societatis Eruditæ Hungaricæ.	Hungarian Literary Society.
Annalium Societatis Eruditæ Hungaricæ volumen primum.	Ditto.
The Second Fasciculus of Anatomical Drawings, selected from the collection of Morbid Anatomy in the Army Medical Museum at Chatham.	Sir James M'Grigor, Bart.
December 1.	
An Essay on the Deaf and Dumb: shewing the Necessity of Medical Treatment in Early Infancy; with Observations on Congenital Deafness. By John Harrison Curtis, Esq.	The Author.
The Quarterly Journal of Agriculture; and the Prize Essays and Transactions of the Highland and Agricultural Society of Scotland, for September 1834.	Highland Society.
Reports of the Scarborough Philosophical Society, for 1831, 1832, and 1833.	The Society.
Bulletin de la Société Géologique de France. Tome iv. Feuilles 10–24; and Tome v.	Ditto.
Archiv fur Chemie und Meteorologie, herausgegeben vom Dr K. W. G. Kastner. Bands 1, 2, 3, 4, 5, 6, and Band. 7, Heft. 1.	Brit. Association.
Quelques Observations de Physique Terrestre. Par MM. Aug. De La Rive and F. Marcet.	The Authors.
Esquisse Historique des Principales Découvertes faites dans l'Electricité depuis quelques Années. Par M. Auguste De La Rive.	The Author.
Notice Biographique sur M. le Professeur G. De La Rive.	Ditto.
Bulletins de l'Académie Royale des Sciences et Belles-Lettres de Bruxelles. Nos. 19–24.	The Society.
Annales de l'Observatoire de Bruxelles, publiées par le Directeur, A. Quetelet. Part 1.	The Author.
Astronomische Beobachtungen auf der Königlichen Universitäts Sternwarte in Königsberg. Von F. W. Bessel, for 1830.	Ditto.
Astronomische Nachrichten. Nos. 258–264.	Prof. Schumacher.
Transactions of the Royal Irish Academy. Vol. xvii. part 1.	The Academy.
Asiatic Researches—Transactions of the Physical Class of the Asiatic Society of Bengal. Vol. xviii. part 2.	The Society.
A Treatise on Insects, being the article Entomology, from the 7th Edition of the Encyclopædia Britannica. By James Wilson, Esq.	The Author.
Nova Acta Regiæ Societatis Upsaliensis. Vol. x.	The Society.
The Journal of the Royal Asiatic Society of Great Britain and Ireland. No. 1.	Ditto.
Transactions of the Royal Asiatic Society. Vol. iii. part 3.	Ditto.

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A Manual of Mineralogy, comprehending the more recent Discoveries in the Mineral Kingdom. By Robert Allan, Esq.	The Author.
Transactions of the Zoological Society of London. Vol. i. part 2.	The Society.
Proceedings of the Zoological Society of London. 1833; part 1.	Ditto.
The American Journal of Science and Arts, conducted by Benjamin Silliman, M. D., LL. D. For April and October 1834.	The Editor.
The Climate of London deduced from Meteorological Observations made in the Metropolis, and at various places around it. By Luke Howard, Esq. 3 vols.	The Author.
Proceedings of the Fifteenth Anniversary Meeting of the Hunterian Society, held on the 4th February 1834, with the Report and List of Officers and Members.	The Society.
A Practical and Pathological Inquiry into the Sources and Effects of Derangements of the Digestive Organs. By William Cooke, Esq.	The Author.
Fauna Americana, being a Description of the Mammiferous Animals inhabiting North America. By Richard Harlan, M. D.	Ditto.
Proceedings of the Geological Society of London. Nos. 33, 34, and 35.	The Society.
Report of the Managers of the Franklin Institute of the State of Pennsylvania, for the promotion of the Mechanic Arts, in relation to Weights and Measures.	The Institute.
Bulletins de la Société d'Encouragement pour l'Industrie Nationale, 1833 and 1834 January to April.	The Society.
Mémoires de l'Institut Royal de France.—Académie des Inscriptions et Belles-Lettres. Tome x.	The Institute.
Conjectures concerning the Origin of Alphabetic Writing. By Thomas Stephens Davies, Esq.	The Author.
Transactions of the Linnean Society of London. Vol. xvii. part 1.	The Society.
Memoirs of the Royal Astronomical Society. Vol. vii.	Ditto.
Transactions of the Cambridge Philosophical Society. Vol. v. part 2.	Ditto.
Nautical and Hydraulic Experiments, with numerous Scientific Miscellanies. By Colonel Mark Beaufoy, F. R. S. Vol. i.	The Editor.
Transactions of the American Philosophical Society, held at Philadelphia, for promoting Useful Knowledge. Vol. iv. New Series, part 3.	The Society.
Abhandlungen der Königlichen Akademie der Wissenschaften zu Berlin, 1832.	The Academy.
An Engraving of the Royal William Yard, Plymouth. By J. Rennie, Esq.	J. Rennie, Esq.
Mémoires de la Société d'Agriculture, a 1814 au 1833. 25 Tomes.	The Society.
Rapport au Conseil Supérieur de Santé sur le Choléra Morbus Pestilentiel. Par Alexandre Moreau de Jonnès.	The Author.
Statistique de l'Espagne. Par Alexandre Moreau de Jonnès.	Ditto.

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Histoire Physique des Antilles Françaises. Par Alexandre Moreau de Jonnès.	The Author.
Origines Biblicæ; or Researches on Primeval History. By Charles Tilstone Beke, Esq. Vol. i.	Ditto.
Population Abstracts of Great Britain. 3 vols.	J. Rickman, Esq.
Proceedings of the Royal Society. No. 15.	The Society.
Transactions of the Royal Society of London, 1834. Part 1.	Ditto.
Elements of Chemistry, including the recent Discoveries and Doctrines of the Science. By Edward Turner, M. D., F. R. S. L. and E. Fifth Edition.	The Author.
Astronomical Observations made at the Royal Observatory at Greenwich, under the direction of John Pond, Esq. Astronomer-Royal, for the years 1831 and 1832; January to September.	Royal Astronomical Society.
Observations Sommaires, sur les Canaux Navigables, et les Chemins de Fer, et sur les avantages que la France peut obtenir de sa Canalisation, notamment pour la prosperité de son Agriculture. Par M. Huerne de Pommeuse.	The Author.
A Cameleon, a Fly Fish, and a Lantern Fly, preserved in Spirits.	John Gordon, Esq.

December 15.

Mémoires de l'Académie Impériale de St Petersbourg (Sciences Mathématiques, &c). Tome ii. Livraisons 5 et 6.	The Society.
Mémoires de l'Académie Impériale de St Petersbourg (Sciences, Politiques, &c). Tome ii. Livraisons 2, 3, 4, 5.	Ditto.
Mémoires de l'Académie Impériale de St Petersbourg (par divers Savans). Tome ii. Livraisons 1, 2, 3.	Ditto.
Recueil des Séances publiques de l'Académie Impériale de St Petersbourg, tenues en Decembre 1826, Decembre 1827, and Decembre 1833.	Ditto.
Transactions of the Royal Society of Literature of the United Kingdom. Vol. ii.	Ditto.
Mémoires de la Société de Physique et d'Histoire Naturelle de Geneve. Tome vi.	Ditto.

January 5. 1835.

Astronomische Nachrichten, Nos. 265, 266, and 267.	Prof. Schumacher.
Distances of the Sun, and the four Planets, Venus, Mars, Jupiter, and Saturn, from the Moon, calculated according to Mr Bessel's method, together with their places for every day in the year 1835. Calculated under the direction of H. C. Schumacher, Professor of Astronomy at Copenhagen, &c.	The Author.
Proceedings of the Berwickshire Naturalists' Club. No. II.	The Club.
Kongl. Vetenskaps-Academiens Handlingar för Ar 1833.	The Academy.

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Arsberättelser om Vetenskapernas Framsteg, afgifne af Kongl. Vetenskaps-Academiens Embetsmän, D. 31 Mars 1833.	The Academy.
Series of Geological Specimens, illustrative of the Greywacke series of Shropshire, Herefordshire, Gloucestershire, and Wales.	Mr Murchison.
January 19.	
Report to the Committee of the Commissioners of Northern Lights, appointed to take into consideration the subject of Illuminating the Lighthouses by means of Lenses. By Alan Stevenson, M. A. Civil Engineer.	The Author.
Bulletin de la Société Geologique de France. Tome iv. Feuilles 28, 29.	The Society.
February 2.	
Transactions of the Royal Irish Academy. Vol. xvii. Part 1.	The Academy.
February 16.	
Records of General Science. By Robert Thomson, M. D., with the assistance of Thomas Thomson, M. D., F. R. S. L. and E. No. I. for January 1835.	The Editors.
Bulletin de la Société Géologique de France. Tome iv. Feuilles 28–29.	The Society.
Recherches sur l'Année Vague des Egyptiens. Par M. Biot.	The Author.
Descriptive and Illustrative Catalogue of the Physiological Series of Comparative Anatomy contained in the Museum of the Royal College of Surgeons in London. Vol. ii.	Royal College.
March 2.	
The Quarterly Journal of Agriculture; and the Prize Essays and Transactions of the Highland and Agricultural Society of Scotland, No. 28, for March 1835.	Highland Society.
The American Almanac and Repository of Useful Knowledge for 1835; and	American Philosophical Society.
Transactions of the American Philosophical Society, held at Philadelphia, for promoting Useful Knowledge. Vol. v. Part i. (New Series.)	Ditto.
Mécanique Céleste, by the Marquis de La Place, Peer of France, &c. Translated by Nathaniel Bowdich, LL. D. Vol. iii.	The Translator.
Carte Physique de l'Isle de Teneriffe, levée sur les lieux. Par Leopold de Buch, en 1814.	The Author.
Carte des Côtes de France, sur laquelle on a indiqué la position et la nature des diverses especes de Feux établis ou à etablir sur ces côtes.	Mons. Fresnel.

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March 16.	
Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce, for the Session 1833–34; being Part 1. of Vol. 50.	The Society.
Bulletin de la Société Géologique de France. Tome vi. Feuilles 1–4.	The Society.
Correspondance Mathématique et Physique de l'Observatoire de Bruxelles, publiée par le Directeur A. Quetelet. Tome viii. Livraison 4.	The Author.
Nouveaux Mémoires de l'Académie Royale des Sciences et Belles-Lettres de Bruxelles. Tome viii.	The Academy.
Bulletin de l'Académie Royale des Sciences et Belles-Lettres de Bruxelles, 1834. No. 25.	Ditto.
Memorie della Reale Academia della Scienze di Torino. Tome xxxvii.	Ditto.
Description des Nouvelles Montres à Seconde, à l'usage des Ingénieurs, des Physiciens, des Médecins, &c. Composés par Henri Robert.	The Author.
Philosophical Transactions of the Royal Society of London, for the year 1834. Part 2.	The Royal Society.
Proceedings of the Royal Society. Nos. 17, 18.	Ditto.
List of the Fellows of the Royal Society, 1834–35.	Ditto.
Report on the Adjudication of the Copley, Rumford, and Royal Medals; and Appointment of the Bakerian, Croonian, and Fairchild Lectures. By James Hudson, Assistant Secretary and Librarian.	Ditto.
Astronomical Observations made at the Royal Observatory at Greenwich, in the months of April to September 1834. By John Pond, Esq. Astronomer-Royal.	Royal Astronomical Society.
April 6.	
On the Vegetation and Temperature of the Faroe Islands. By W. C. Trevelyan, Esq.	The Author.
Natuur en Scheikundig Archief, intgegeven door G. J. Mulder. 2 vols.	The Author.
Leerboek voor Scheikundige Werktingkunde door G. J. Mulder. Vols. i. and ii. Part 1.	Dutch Government.
A List of Test Objects, principally Double Stars, arranged in Classes, for the trial of Telescopes in various respects as to Light, Distinctness, &c. By Sir J. F. W. Herschel.	The Author.
A second Series of Micrometrical Measures of Double Stars, chiefly performed with the seven feet Equatorial at Slough, in 1831, 1832, and 1833. By Sir J. F. W. Herschel.	Ditto.
On the Satellites of Uranus. By Sir J. F. W. Herschel.	Ditto.

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April 20.	
Voyage autour du Monde, entrepris par ordre du Roi, exécuté sur les Corvettes de S. M. l'Uranie et la Thysicienne. Par M. Louis de Freycinet. 2 tomes 4to; and Atlas, folio.	Le Ministère de la Marine de France.
Voyage autour du Monde, exécuté par ordre du Roi, sur la Corvette de Sa Majesté La Coquille. Par L. J. Duperrey. 1 Atlas, in folio.	Ditto.
Voyage de la Corvette L'Astrolabe, exécuté sous le Commandement de M. Jules Dumont D'Urville. 1 Atlas, in folio.	Ditto.
Voyage autour du Monde, exécuté sur la Corvette la Favorite, commandée par M. Laplace. 1 Atlas, in folio.	Ditto.
Le Pilote du Bresil, par le Baron Roussin. 1 vol. 8vo; and Atlas, in folio.	Ditto.
Description Nautique des Côtes de la Martinique. Par M. P. Monnier. 1 vol. 8vo; and Atlas, in folio.	Ditto.
Pilote de I'Ile de Corse. Par M. Hell. 1 Atlas, in folio.	Ditto.
Pilote Français. 3 Atlas, in folio.	Ditto.
Exposé des Travaux Rélatifs à la Reconnaisance Hydrographique des Côtes Occidentales de France. Par M. Beautemps-Beaupré. 4to.	Ditto.
Mémoires sur les Attérages des Côtes Occidentales de France. Par M. le Saulnier de Vanhello. 4to.	Ditto.
Collection de 66 Cartes and Plans.	Ditto.
Table des Positions Géographiques. Par M. Daussy.	The Author.
Catalogue des Préparations Anatomiques laissées dans la Cabinet d'Anatomie Comparée du Muséum d'Histoire Naturelle. Par G. Cuvier.	Madame Cuvier.
Nouvelles Annales du Muséum d'Histoire Naturelle, 1834. Tome iii. Livraison 3.	Les Editeurs.
The Journal of the Royal Asiatic Society of Great Britain and Ireland. No. 3.	The Society.
Hints on the Trisection of an Angle, and Duplication of the Cube in Elementary Geometry. By Nasmyth Morrieson, W. S.	The Author.
Chart of the Chinese Sea. By Captain Horsburgh, Hydrographer to the Hon. East India Company.	Ditto.
December 7.	
An Account of the Rev. John Flamsteed, the first Astronomer-Royal; compiled from his own Manuscripts, and other authentic Documents never before published. To which is added, his British Catalogue of Stars. By Francis Baily, Esq., Vice-President of the Royal Astronomical Society.	The Lords Commissioners of the Admiralty.
Memoirs of the Royal Astronomical Society. Vol. viii.	Royal Astronomical Society.
Astronomical Observations made at the Royal Observatory at Greenwich, under the direction of John Pond, Esq.—for 1829, part 5; 1833, part 5; 1834, parts 1, 2, 3, 4; and 1835, part 1.	Ditto.

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Observations sur le Choléra Morbus qui a régné à la Nouvelle Orleans en 1833 et en 1834. Par M. Michel Halphen, M. D.	The Author.
Considerations sur la Nature et le Traitement du Choléra Morbus, suivies d'une instruction sur les preceptes Hygieniques contre cette Maladie. Par le Chevalier J. R. L. Kirckhoff, M. D.	Ditto.
Bulletin de la Société de Geographie. Deuxieme serie. Tomes i. ii.	The Society.
Bulletin de la Société d'Encouragement pour l'Industrie Nationale. Mai à Decembre 1834, et Janvier à Mars 1835.	Ditto.
Leerboek voor Scheikundige Werktingkunde. Door G. J. Mulder. Vol. ii. part 2.	The Author.
Tijdschrift voor Natuurlijke Geschiedenis. Uitgegeven door J. Van der Hoeven, M. D., en W. H. de Vriese, M. D. Vol. i. parts 1, 2, 3.	The Editors.
Geological Report of an Examination made in 1834 of the elevated country between the Missouri and Red Rivers. By G. W. Featherstonhaugh, Esq.	The Author.
Index to the first Eighteen Volumes of the Asiatic Researches, or Transactions of the Society instituted in Bengal for inquiring into the History and Antiquities, the Arts, Sciences, and Literature of Asia.	The Society.
Mémoires d'Agriculture, d'Economie Rurale et Domestique, publiées par la Société Royale et Centrale d'Agriculture. Pour l'année 1834.	Ditto.
Memoirs of the American Academy of Arts and Sciences. (New Series.) Vol. i.	The Academy.
Voyage autour du Monde par les Mers de l'Inde et de Chine, éxécuté sur la Corvette de l'Etat La Favorite pendant les années 1830, 1831, et 1832, sous le Commandement de M. Laplace, Capitaine de Frégate. 4 tomes.	Le Ministère de la Marine de France.
Voyage de Decouvertes de l'Astrolabe éxécuté par ordre du Roi pendant les années 1826–27–28–29, sous le Commandement de M. J Dumont d'Urville, Capitaine de Vaisseau. 2 tomes.	Ditto.
The Journal of the Royal Asiatic Society of Great Britain and Ireland. No. 4.	The Society.
Transactions of the Royal Asiatic Society of Great Britain and Ireland. (Appendix to Vol. iii.)	Ditto.
Memorias da Academia Real das Sciencias de Lisboa. Vols. i. to xi. part 1.	Royal Academy of Lisbon.
Noticias para a Historia a Geografia das Naçoes Ultramarinas, publicada pela Academia Real das Sciencias. Vols. i. to iv. part 1.	Ditto.
Sur l'Homme et le Dèveloppement de ses Facultés, ou Essai de Physique Sociale. Par A. Quetelet, Secretaire Perpétuel de l'Academie Royale de Bruxelles, &c. 2 tomes.	The Author.

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The Journal of the Asiatic Society of Bengal. Edited by James Prinsep, Esq. F. R. S. Vol. iii.	The Editor.
A Grammar of the Tibetan Language in English. By Alexander Csoma de Körös.	The Author.
A Dictionary, Tibetan and English. By Alexander Csoma de Körös.	Ditto.
Arsberättelse om Framstegen i Fysik och Keml afgifven den 31 Mars 1833 af Jac. Berzelius, K. V. Acad. Secret.	Ditto.
Kongl. Vetenskaps-Academiens Handlingar för Ars 1827, 1828, and 1831.	The Academy.
Arsberattelser om Vetenskapernas Framsteg, afgifne af Kongl. Vetenskaps-Academiens Embetsman 1828 and 1831.	Ditto.
Report of the Fourth Meeting of the British Association for the Advancement of Science, held at Edinburgh in 1834.	The Association.
American Journal of Science and Arts. Conducted by Benjamin Silliman, M D., LL.D. April 1835.	The Editor.
Catalogue of the Works in Medicine and Natural History contained in the Radcliff Library.	Dr Kidd, Librarian.
Proceedings of the Geological Society of London. Nos. 37. and 38.	The Society.
Transactions of the Geological Society of London. (Second Series.) Vol. iii. part 3.	Ditto.
A Catalogue of 606 principal Fixed Stars in the Southern Hemisphere. By Manuel J. Johnston, Lieutenant St Helena Artillery.	The Author.
The Cyclopædia of Anatomy and Physiology. Edited by Robert B. Todd, M. B., &c. Part 1.	The Editor.
Le Régne Mineral ramèné aux Methodes de l'Histoire Naturelle. Par L. A. Necker, de l'Académie et de la Société de Physique et d'Histoire Naturelle de Génève. 2 tomes.	The Author.
Mémoires de la Société de Physique et d'Histoire Naturelle de Génève. Tome vii. ptie. 1.	The Society.
Natuur-en Scheikundig Archief. Uitgegeven door G. J. Mulder. Jaargang, 1835. Stuk 1.	The Author.
Lettres Cosmologiques. Par M. le Comte de Montlivault.	Ditto.
Nova Acta Physico-Medica Academiæ Cæsareæ Leopoldino-Carolinæ Naturæ Curiosorum. Vol. xvii. part 1.	The Academy.
Quarterly Journal of Agriculture; and Prize Essays and Transactions of the Highland and Agricultural Society of Scotland, for June, September, and December 1835.	Highland Society.
A Treatise on Poisons, in relation to Medical Jurisprudence, Physiology, and the Practice of Physic. By Robert Christison, M. D., F. R. S. E., Professor of Materia Medica in the University of Edinburgh, &c. &c.	The Author.
Annual Report of the Council of the Yorkshire Philosophical Society, for 1834.	The Society.

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Report of the Directors of the Manchester Mechanics' Institution, and Proceedings at the Annual Meeting of the Members, held on 26th February 1835.	The Institution.
Catalogue of the Library of the Manchester Mechanics' Institution, with the Rules, and a Sketch of the Objects and Advantages of the Institution.	Ditto.
Journal of the Bahama Society for the Diffusion of Knowledge. May 1835. No. 1.	The Society.
Annual Reports of the Leeds Philosophical and Literary Society for 1833–4 and 1834–5.	Ditto.
Maps of the Ordnance Survey of Great Britain, published by the Board of Ordnance. Nos. 1, 2, 13, 34, 35, 36, 37, 41, 42, 43, 44, 45, 46, 47, 48, 53, 54, 55, 56, 57, 58, 61, 62, 64, 65, 69, 70, 73, 83, 84, 85, 86.	Board of Ordnance.
December 21.	
Address delivered in the Hall of Marischal College, Aberdeen, 5th November 1835, on occasion of his Installation as Lord Rector of the University, by John Abercrombie, M. D. Oxon and Edinburgh, V. P. R. S. E., &c. &c.	The Author.
Astronomische Nachrichten. Nos. 268 to 288.	Prof. Schumacher.
Natuur-en Scheikundig Archief. Uitgegeven door G. J. Mulder. 1835. Stuk iii.	The Editor.
Proceedings of the Royal Society of London. Nos. 19 and 20.	The Society.
Transactions of the Royal Society of London, 1835. Part 1.	Ditto.
Geometrical Investigations concerning the Phenomena of Terrestrial Magnetism. By Thomas Stephens Davies, Esq. F. R. S. L. & E.	The Author.
Proceedings of the Geological Society of London. Nos. 40 and 41.	The Society.
Bulletin de l'Académie Royale des Sciences et Belles-Lettres de Bruxelles. 1834, Nos. 25, 26, 27 ; and 1835, Nos. 1, 2, 3.	The Academy.
Annuaire de l'Académie Royale des Sciences et Belles-Lettres de Bruxelles.	Ditto.
Annuaire de l'Observatoire de Bruxelles pour l'an 1835. Par le Directeur A. Quetelet.	The Author.
A Treatise on Insects. By James Wilson, Esq. F. R. S. E.	The Author.
A Treatise on Fishes. By James Wilson, Esq. F. R. S. E.	Ditto.
De l'Influence de la Lune sur l'Atmosphére Terrestre, déterminée par les Observations Météorologiques. Par. M. Eug. Bouvard.	Ditto.
Memorias para a Historia das Navagaçoes e Descobrimentes das Portuguezes. Par Joaquim José Da Costa De Macedo.	Ditto.
A Collection of Specimens from the Volcanic District of Auvergne.	Prof. Forbes.

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Mémoires de la Société Géologique de France. Tome i. Part 1.	The Society.
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Nouvelles Annales du Museum d'Histoire Naturelle. Tome iv. Liv. 2. and 3.	Les Editeurs.
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The American Journal of Sciences and Arts, conducted by Benjamin Silliman, M. D., LL. D. Vol. xxix. No. 1. (October 1835.)	The Editor.
Flora Batava. Nos. 100, 101, 102, and 103.	King of Holland.
Neue Wirbelthiere zu der Fauna von Abyssinien gehörig entdeckt und beschrieben von Dr Eduard Rüppell. 4 parts.	The Author.
February 1.	
No. 52. of the Map of the Ordnance Survey of Great Britain; published by the Board of Ordnance.	The Board.
Proceedings of the Berwickshire Naturalists' Club. No. 3.	The Club.
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The American Almanac and Repository of Useful Knowledge for the year 1836.	American Philosophical Society.
Historical Account of Bills of Mortality, and the Probability of Human Life, in Glasgow and other large Towns. By James Cleland, LL. D., &c. &c.	The Author.
Proceedings of the Geological Society of London. No. 42.	The Society.
Description of a Bronze or Cast-iron Columnal Lighthouse, with reference to a model of one-fourth the full size, designed for the Wolf Rock, situated between the Island of Scilly and the Land's-End. By Captain Samuel Brown, R. N.	The Author.
Report respecting the Construction of Low-water Piers on the North and South Shores of the Frith of Forth, for the Fife and Mid-Lothian Ferries.	James Anderson, Civil-Engineer.
Report on the Present State of Leith Harbour, and the practicability of rendering available its Wet-Docks, by means of a Deep-water Entrance, and a communicating Dock or Ship Canal. By James Anderson, Civil-Engineer.	The Author.
March 7.	
Descriptions of the Inferior Maxillary Bones of Mastodons in the Cabinet of the American Philosophical Society, with Remarks on the Genus Tetracaulodon, &c. By Isaac Hays, M. D.	The Author.
Catalogue of Fossil Fish in the Collections of Lord Cole and Sir Philip Grey Egerton, arranged alphabetically, with references to the	Ditto.

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Localities, Strata, and published Descriptions of the Species. By Sir Philip Grey Egerton.	
Treatise on the more obscure Affections of the Brain, on which the nature and successful treatment of many Chronic Diseases depend. By A. P. W. Philip, M. D., F. R. S. L. & E., &c.	The Author.
Mémoires de l'Académie Impériale des Sciences de St Petersbourg. (Sciences Mathematiques.) Tome iii. Livrs 2, 3, 4, 5, 6.	Imperial Academy.
Mémoires de l'Académie Impériale des Sciences de St Petersbourg. (Sciences Mathématiques et Physiques.) Tome i. Livr. 1, 2.	Ditto.
Do. do. (Sciences Politiques, &c.) Tome ii, liv. 6 ; et tome iii, liv. 1.	Ditto.
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Recueil des Actes de la Séance publique de l'Académie Impériale des Sciences de St Petersbourg, tenue le 29. Decembre 1834.	Ditto.

March 21.

The Quarterly Journal of Agriculture ; and the Prize Essays and Transactions of the Highland and Agricultural Society of Scotland. No. 31. for March 1836.	Highland Society.
Catalogue of Recent Shells in the Cabinet of John C. Jay, M. D., Member of the Lyceum of Natural History, New York.	The Author.
Thirteenth Report of the Whitby Literary and Philosophical Society, presented at the Annual Meeting, October 31. 1835.	The Society.
The British and Foreign Medical Review, or Quarterly Journal of Practical Medicine and Surgery. Edited by John Forbes, M. D., F. R. S., and John Conolly, M. D. January 1836. No. 1.	The Editors.
Descriptive and Illustrated Catalogue of the Physiological Series of Comparative Anatomy of the Royal College of Surgeons in London. Vol. iii. part 1.	Royal College of Surgeons, London.

April 4.

Transactions of the Geological Society of London. (Second Series.) Vol. iv. part 1.	The Society.
Proceedings of the Geological Society of London, Nos. 42, 43, and 44.	Ditto.
Bulletin de la Société Géologique de France. Tome vii. Feuilles 1–2.	Ditto.
Tables des Positions Géographiques des Principaux Lieux du Globe. Par M. Daussy.	The Author.
Second Mémoire sur les Marées des Côtes de France. Par M. Daussy.	Ditto.

April 11.

The Statutes of the Realm. Vols. ii. to ix., and 2 vols of Indices.	Commissioners on Public Records.
Foedera, Conventiones, Litteræ, et cujuscunque Generis Acta Publica, inter Reges Angliæ et alios quosvis Imperatores, Reges, &c. Cura et studio Thomæ Rymer. 3 vols. in 6.	Ditto.

DONATIONS.	DONORS.
The Parliamentary Writs and Writs of Military Summons, together with the Records and Muniments relating to the Suit and Service due and performed to the King's High Court of Parliament and the Councils of the Realm. Collected and Edited by Francis Palgrave, Esq. 2 vols. in 4.	Commissioners on Public Records.
Rotuli Scotiæ in Turri Londinensi et in Domo Capitulari Westmonasteriensi asservati. 2 vols.	Ditto.
Rotuli Hundredorum Temp. Hen. III. et Edw. I. in Turr. Lond. et in Curia Receptæ Scaccarii Westm. asservati. 2 vols.	Ditto.
Rotulorum Originalium in curia Scaccarii Abbreviatio. 2 vols.	Ditto.
Rotuli Litterarum Clausarum in Turri Londinensi asservati. Accurante Thoma Duffus Hardy. Vol. i.	Ditto.
Rotuli Litterarum Patentium in Turri Londinensi asservati. Accurante Thoma Duffus Hardy. Vol. i. part 1.	Ditto.
Calendarium Inquisitionum post Mortem sive Escaetarum. Vols. i. ii. iii. iv.	Ditto.
Nonarum Inquisitiones in Curia Scaccarii. 1 vol.	Ditto.
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Placitorum in Domo Capitulari Westmonasteriensi asservatorum Abbreviatio.	Ditto.
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Valor Ecclesiasticus Temp. Henr. VIII. auctoritate Regia institutus. 6 vols.	Ditto.
Calendars of the Proceedings in Chancery in the Reign of Queen Elizabeth; from the originals in the Tower. 3 vols.	Ditto.
Inquisitionum in Officio Rotulorum Cancellariæ Hiberniæ asservatarum Repertorium. 2 vols.	Ditto.
Rotulorum Patentium et Clausorum Cancellaria Hiberniæ Calendarium. Vol. i. part 1.	Ditto.
Catalogue of the Harleian Manuscripts in the British Museum. 4 vols.	Ditto.
Catalogue of the Lansdowne Manuscripts in the British Museum. 1 vol.	Ditto.
Rotuli Curiæ Regis. Rolls and Records of the Court held before the King's Justiciars or Justices. Edited by Sir Francis Palgrave, K. H. 2 vols. 8vo.	Ditto.
Rotuli de Oblatis et Finibus in Turri Londinensi asservati tempore Regis Johannis. Accurante Thoma Duffus Hardy, S. A. S.	Ditto.
Fines, sive Pedes Finium: sive Finales Concordiæ in Curia Domini Regis: A. D. 1195—A. D. 1214. Edente Josepho Hunter. Vol. I.	Ditto.
Excerpta a Rotulis Finium in Turri Londinensi asservatis, Henrico Tertio Rege, A. D. 1216–1272. Cura Caroli Roberts. Vol. i. A. D 1214–1246.	Ditto.

DONATIONS.	DONORS.
Rotuli Normanniæ in Turri Londinensi asservati, Johanne et Henrico Quinto Angliæ Regibus. Accurante Thoma Hardy, S. A. S.	Commissioners on Public Records.
Proceedings and Ordinances of the Privy Council of England. Edited by Sir Harris Nicolas. 10 Richard II. 1306 to 21 Henry VI. 1443. 5 vols.	Ditto.
General Introduction to Domesday Book, by Sir Henry Ellis, K. H., F. R. S.	Ditto.
Transactions of the Society instituted at London for the Encouragement of Arts, Manufacture, and Commerce. Vol. i. part 2.	The Society.
The Third Annual Report of the Royal Cornwall Polytechnic Society.	Ditto.

April 18.

Nouveaux Mémoires de l'Académie Royale des Sciences et Belles Lettres de Bruxelles. Tome ix.	The Academy.
Mémoires couronnés par l'Académie Royale des Sciences et Belles-Lettres de Bruxelles. Tome x.	Ditto.
Bulletin de l'Académie Royale des Sciences et Belles-Lettres de Bruxelles. 1835. Nos. 8, 9, 10, 11, 12.	Ditto.
Annuaire de l'Académie Royale des Sciences et Belles-Lettres de Bruxelles. 1836.	Ditto.
Annuaire de l'Observatoire de Bruxelles pour l'An 1836. Par le Directeur A. Quetelet.	The Author.
Compte de l'Administration de la Justice Criminelle en Belgique pendant les Années 1831, 1832, 1833, et 1834, presenté au Roi par le Ministre de la Justice. 1835.	M. Quetelet.
Astronomische Nachrichten. Nos. 289 to 294.	M. Schumacher.
Du Spiritualisme au xix^{me} Siècle, ou Examen de la Doctrine de Maine de Biran, par L. A. Gruyer.	The Author.
Institut Royal de France pour l'An 1836.	The Institute.
The American Journal of Science and Arts; conducted by Benjamin Silliman, M. D., LL. D., &c. for January 1836.	The Editor.
On the Occurrence of the Megalichthys in a Bed of Cannel Coal in the West of Fifeshire, with Observations on the supposed lacustrine Limestone at Burdiehouse.	Leonard Horner, Esq.
On an Artificial Substance resembling Shell; by Leonard Horner, Esq. With an Account of an Examination of the same; by Sir David Brewster, LL. D. With Specimen of the substance.	Ditto.

May 2.

Tijdschrift voor Natuurlijke Geschiedenis en Physiologie uitgegeven door J. van der Hoeven, M. D., W. H. de Vriese, M. D. Deel. ii. Stuk. 4.	The Editors.

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A Memoir of the late Lewis David Von Schweinitz, P. D., with a Sketch of his Scientific Labours. Read before the Academy of Natural Sciences of Philadelphia, May 12. 1835.	Walter R. Johnson.
A Biographical Sketch of the late Thomas Say, Esq. Read before the Academy of Natural Sciences of Philadelphia, December 16. 1834. By Benjamin H. Coates, M. D.	The Academy.
Tables of Continental, Lineal, and Square Measures. By W. S. B. Woolhouse, Head Assistant of the Nautical Almanac Establishment.	The Author.

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TO THE

FIRST THIRTEEN VOLUMES

OF THE

TRANSACTIONS OF THE ROYAL SOCIETY OF EDINBURGH.

N.B.—The first five volumes are subdivided into three parts. Part I., comprehending the ***History of the Society***, is contained under one set of pages; and Part II., comprehending the ***Papers***, is contained under two sets, viz. Papers of the Physical Class, under the one; and Papers of the Literary Class under the other. The references in this Index are accordingly designated in the class to which they belong by the letters P. C., L. C., and H. The numeral letters refer to the volume, the figures to the pages.

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END OF VOLUME XIII.

LAWS

OF THE

ROYAL SOCIETY OF EDINBURGH.

JANUARY 18. 1836.

LAWS.

I.

The Royal Society of Edinburgh shall consist of Ordinary and Honorary Fellows.

II.

Every Ordinary Fellow, within three months after his election, shall pay Five Guineas as fees of admission, and Three Guineas as his contribution for the Session in which he has been elected; and annually at the commencement of every Session, Three Guineas into the hands of the Treasurer*.

III.

All Fellows who shall have paid Twenty-five years' annual contributions shall be exempt from further payment.

IV.

Ordinary Fellows, not residing in Scotland, shall compound for the annual contribution at the rate of fifteen years' purchase.

V.

Members failing to pay their contribution for three successive years (due application having been made to them by the Treasurer), shall be reported to the Council, and, if they see fit, shall be declared from that period to be no longer Fellows, and the legal means for recovering such arrears shall be employed.

* A modification of this rule, in certain cases, was agreed to 3d January 1831.

VI.

None but Ordinary Fellows shall bear any office in the Society, or vote in the choice of Fellows or Office-bearers, or interfere in the patrimonial interests of the Society.

VII.

The number of Ordinary Fellows shall be unlimited.

VIII.

The Ordinary Fellows, upon producing an order from the TREASURER, shall be entitled to receive from the Publisher, gratis, the Parts of the Society's Transactions which shall be published subsequent to their admission.

IX.

No person shall be proposed as an Ordinary Fellow, without a recommendation subscribed by *One* Ordinary Fellow, to the purport below*. This recommendation shall be delivered to the Secretary, and by him laid before the Council, and shall afterwards be printed in the circulars for three ordinary meetings of the Society, previous to the day of the election, and shall lie upon the table during that time.

X.

Honorary Fellows shall not be subject to any Contribution. This class shall consist of persons eminently distinguished for science or literature. Its number shall not exceed Fifty-six, of whom twenty may be British subjects, and thirty-six may be subjects of foreign states.

* "A. B., a gentleman well skilled in several branches of Science *(or Polite Literature as the case may be)*, being to my knowledge desirous of becoming a Fellow of the Royal Society of Edinburgh, I hereby recommend him as deserving of that honour, and as likely to prove an useful and valuable Member."

This recommendation to be accompanied by a request of admission signed by the Candidate.

XI.

Personages of Royal Blood may be elected Honorary Fellows, without regard to the limitation of numbers specified in Law X.

XII.

Honorary Fellows may be proposed by the Council, or by a recommendation (in the form given below) * subscribed by three Ordinary Fellows ; and in case the Council shall decline to bring this recommendation before the Society, it shall be competent for the proposers to bring the same before a General Meeting. The election shall be by ballot, after the proposal has been communicated *viva voce* from the Chair at one meeting, and printed in the circular for the meeting at which the ballot is to take place.

XIII.

The election of Ordinary Fellows shall take place at the ordinary meetings of the Society. The election shall be by ballot, and shall be determined by a majority of at least two-thirds of the votes, provided Twenty-four Fellows be present, and vote.

XIV.

The Ordinary Meetings shall be held on the first and third Mondays of every month, from November to June inclusive. Regular minutes shall be kept of the proceedings, and the Secretaries shall do the duty alternately, or according to such agreement as they may find it convenient to make.

* We hereby recommend ______________________________
for the distinction of being made an Honorary Fellow of this Society, declaring that each of us from our own knowledge of his services to *(Literature or Science as the case may be)* believe him to be worthy of that honour.

(To be signed by three Ordinary Fellows.)

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To the President and Council of the Royal Society of Edinburgh.

XV.

The Society shall from time to time publish its Transactions and Proceedings. For this purpose the Council shall select and arrange the papers which they shall deem it expedient to publish in the *Transactions* of the Society, and shall superintend the printing of the same.

XVI.

The Transactions shall be published in Parts or *Fasciculi* at the close of each session, and the expense shall be defrayed by the Society.

There shall be elected annually for conducting the publications and regulating the private business of the Society, a Council, consisting of a President; Six Vice-Presidents, two at least of whom shall be resident; Twelve Counsellors, a General Secretary, Two Secretaries to the Ordinary Meetings, a Treasurer, and a Curator, and an Assistant Curator of the Museum and Library.

XVII.

Four Counsellors shall go out annually, to be taken according to the order in which they stand on the list of the Council.

XVIII.

An Extraordinary Meeting for the Election of Office-Bearers shall be held on the 4th Monday of November annually.

XIX.

Special Meetings of the Society may be called by the Secretary, by direction of the Council; or on a requisition signed by six or more Ordinary Fellows. Notice of not less than two days must be given of such meetings.

XX.

The Treasurer shall receive and disburse the money belonging to the

Society, granting the necessary receipts, and collecting the money when due.

He shall keep regular accounts of all the cash received and expended, which shall be made up and balanced annually ; and at the last Ordinary Meeting in January, he shall present the accounts for the preceding year, duly audited. At this Meeting, the Treasurer shall also lay before the Council a list of all arrears due above two years, and the Council shall thereupon give such directions as they may deem necessary for recovery thereof.

XXI.

At the Extraordinary Meeting in November, a Committee of Three Fellows shall be chosen to audit the Treasurer's accounts, and give the necessary discharge of his intromissions.

The report of the examination and discharge shall be laid before the Society at the last Ordinary Meeting in January, and inserted in the records.

XXII.

The General Secretary shall keep Minutes of the Extraordinary Meetings of the Society, and of the Meetings of the Council, in two distinct books. He shall, under the direction of the Council, conduct the correspondence of the Society, and superintend its publications. For these purposes, he shall, when necessary, employ a clerk, to be paid by the Society.

The Secretaries to the Ordinary Meetings shall keep a regular Minute-book, in which a full account of the proceedings of these Meetings shall be entered : they shall specify all the Donations received, and furnish a list of them, and of the donors' names, to the Curator of the Library and Museum : they shall likewise furnish the Treasurer with notes of all admissions of Ordinary Fellows. They shall assist the General Secretary in superintending the publications, and in his absence shall take his duty.

XXIII.

The Curator of the Museum and Library shall have the custody and charge of all the Books, Manuscripts, objects of Natural History, Scientific Productions, and other articles of a similar description belonging to the Society ; he shall take an account of these when received, and keep a regular catalogue of the whole, which shall lie in the Hall, for the inspection of the Fellows.

XXIV.

All articles of the above description shall be open to the inspection of the Fellows, at the Hall of the Society, at such times, and under such regulations, as the Council from time to time shall appoint.

XXV.

A Register shall be kept, in which the names of the Fellows shall be enrolled at their admission, with the date.

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