PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXXIV.

PART I.

LONDON:

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MDCCCXXXIV.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions,* take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions,* that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgement of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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PHILOSOPHICAL TRANSACTIONS.

I. *On the Quantity and Quality of the Gases disengaged from the Thermal Spring which supplies the King's Bath in the City of Bath.* *By* CHARLES DAUBENY, *M.D., F.R.S., Professor of Chemistry in the University of Oxford.*

Received November 2,—Read December 19, 1833.

IN some remarks on a paper of Dr. JOHN DAVY'S, entitled "Notice on the Remains of the recent Volcano in the Mediterranean," which were inserted in the last Part of our Transactions, I expressed my regret, that no accurate statement of the quantity of gas evolved in a given time from any thermal waters had been published, and intimated my intention to make this the subject of special examination, whenever suitable opportunities should occur.

Accordingly, having in my former visits to Bath been struck with the copious evolution of gas from the centre of the principal of the hot springs of that city, I solicited from the gentlemen composing the Committee appointed to manage and regulate them, leave to institute such experiments as appeared to me desirable on the spot; and having obtained from them the requisite facilities for so doing, I determined to collect and measure the gas evolved, repeatedly, during a period sufficiently extended to enable me to fix with tolerable precision its average amount, and to ascertain, whether any greater diurnal variation in its quantity could be detected, than what might be fairly set down to errors of manipulation, or to oscillations in the quantities discharged extending over a wider range of time, than that during which it might be found convenient to protract the period of each observation.

Anticipating, also, that a variation might be discovered, I carried on during the same period a corresponding register of the leading conditions of the atmosphere as to pressure, temperature and humidity, in order to learn, whether any connexion could be traced between these and the quantities of gas evolved.

I likewise examined, on several occasions, the quality of the gas emitted, not only in order to fix with greater exactness its actual composition, but likewise to learn, whether any variation in these respects could be perceived during the course of my observations.

The Bath waters arise from three distinct sources, or springs, contiguous to each other, which supply respectively the King's Bath, the Hot Bath, and the Cross Bath. In addition to the above, which belong to the Corporation, there is likewise said to be another spring of water, which supplies the Kingston Baths, the property of an individual.

Of these by far the most copious is the first-named, which fills the King's Bath and the Queen's Bath annexed; it gives out in a regular and unremitting stream no less than 126 gallons of water per minute, and in nine hours replenishes the whole area of these two baths, when emptied, to the height of forty-six inches.

The evolution of gas takes place chiefly from this spring, the quantity discharged by the Hot and Cross Baths being comparatively insignificant; for I found that the latter of these, which, of the two, is considered to yield the most, disengaged no more than about twelve cubic inches a minute at the time I attempted to estimate it.

I therefore confined my inquiry to the King's Bath, from the centre of which gas rises in great quantities, whilst it is also given off in a slighter degree and in a more irregular manner, from the various holes and crevices, that exist in the stone pavement of the bath throughout the whole extent of the area embraced by it.

To form an exact estimate of the amount of the gas which escapes from these minor lateral spiracles, would have been an irksome and difficult task; but I ascertained, that it bore but a very small proportion to that discharged from the centre, and from a certain distance round it, being in the one case emitted in bursts at uncertain but distant intervals, in the other proceeding in a current as regular and unintermitting as the spring itself.

I therefore attempted to do no more, than collect that portion of the gas which finds its way upwards, from an area round the centre of the bath, about twenty feet in diameter*: and in order to accomplish this, I contrived a funnel-shaped apparatus, which for brevity's sake I shall in the rest of this paper call the Shield, consisting of several sheets of iron riveted together, and rendered airtight by means of white lead interposed between the seams of the joints, so that there might be no means of escape for the gas detained under the lower surface of the shield, except at its centre, where an aperture of two inches in diameter was left, towards which it would be conducted, and thus find a ready vent. The apertures in the pavement of the bath within the area above specified, which the shield was not large enough to cover, were carefully stopped up either with corks or tow, covered over with boards, which by means of weights were made to press closely upon the surface of the pavement.

The apparatus being thus arranged, it was easy, by placing inverted jars of suitable dimensions filled with water over the central orifice, to collect and measure the gas that might escape within a given period.

The iron shield being six feet square, and therefore from its size somewhat unwieldy, it was found convenient to erect a kind of temporary framework of wood, from which the apparatus, when not in use, might be suspended out of the way of bathers.

* See the ground plan of the bath.

This consisted of four upright posts, fixed by means of cords to opposite angles of the hand-rail which separates the central portion of the King's Bath from its sides; these posts being connected together by planks extending from one to the other, and resting horizontally on the tops of each.

This framework served as a point of attachment to the pulleys by means of which the iron shield was raised and depressed; and it at the same time enabled me to cover over that portion of the bath in which my operations were conducted, with an awning, to serve both as a protection from the weather, and as a screen from observation.

In this manner I was enabled to carry on my experiments for the period of a month, without being any hindrance to others, and with comparative ease to myself and my assistants, choosing a time of day after the hours of bathing were concluded, when the water was comparatively low, which, as the bath is emptied regularly once a day, and afterwards replenished from the spring at the uniform rate of nearly six inches per hour, could always be reckoned upon at a similar time in the afternoon.

On one occasion, however, I caused the water to be kept low during the space of twenty-four hours, in order to satisfy myself that there was no material deviation from the mean quantity of gas evolved each day, at the particular period that had been commonly selected; and for this purpose I collected the gas at intervals of about six hours, namely on Saturday at 1 P.M., again at 6 in the evening, again about midnight, again at 7 o'clock on Sunday morning, and lastly at noon of the same day. Finding, however, a greater variation between the quantity collected at the same hour on the two successive days, than between that obtained at different hours of the same day, I saw no advantage in departing again from the usual routine.

There might be more reason for suspecting, that the periods to which each observation extended were not sufficiently long to secure a fair average; and fearing that this might have been the case in the first trials, I gradually prolonged the duration of them from five to fifteen minutes: so that the time occupied in the experiments amounted altogether to more than four hours.

If it be objected, that this period even might have been too short to insure exact results, I may reply, that the correspondence which is frequently to be traced between the amount obtained on two or even three successive days, affords in itself a presumption, that the numbers given furnish in general a pretty fair expression of the mean quantity of gas emitted every minute from the spring.

Assuming this to be the case, it would appear from the sum total of the observations recorded, that the quantity of gas evolved averages 264 cubic inches, or, rejecting observation 3rd, (which was noted at the time as of doubtful accuracy, on account of some defect in the apparatus subsequently remedied,) 267 cubic inches each minute.

The largest quantity ever obtained in the course of one minute appears to have been 530 cubic inches; the smallest in the same time, 80: but if we take the mean of the observations, we shall find, that the quantity usually varied from 339 to 207, so that on an average the volume of gas evolved differed between one minute and others as 12 to 20, or about one third, whilst the extreme variation was as 3 to 20, or four times as great.

By comparing together periods of five minutes, the difference between one observation and another will be found greatly reduced, the greatest variation being no more than as 14 to 20, the mean only 19 to 20, as I have calculated by referring to my original notes, and taking at hazard eleven observations, each extending to ten minutes, and comparing the amount of gas obtained in the former half with that in the latter half of the time.

I conceive, therefore, we may be warranted in calculating, on the above data, that no less than 223 cubic feet of gas are usually disengaged in the space of twenty-four hours from this source alone; and, large as this amount may appear, we have positive testimony* that it was as constantly taking place almost a century and a half ago; nor is there much reason to doubt that it may have continued in an equally unintermitting flow from the earliest periods at which the springs were known: the analogy, indeed, of other thermal waters leading us to conclude, that the evolution of gas is a phenomenon as intimately connected with the constitution of the waters as the presence of a definite quantity of certain saline ingredients, or the possession of a particular temperature; both which, it is probable, continue unaltered during periods, historically speaking, of long duration.

Dr. Clarke has shown†, that the hot springs which gush out from the foot of the limestone precipices of Mount Œta at the pass of Thermopylæ, retain at this moment a temperature of 111°, which probably is as high as that belonging to them in the times of ancient Greece; and, what is more to the point, the boiling up of gaseous bubbles, which this traveller ascertained to be owing to the escape of sulphuretted hydrogen from the springs, would seem to be noticed by Sophocles, who, if we adopt Dr. Clarke's ingenious interpretation of the passage referred to‡, makes a poetical use of the phenomenon in his Trachiniæ.

Thus, too, Dr. Holland detected, at a spot on the coast of Albania, the escape of carburetted hydrogen, probably on the precise spot where the ancient writers describe a Nympheum, or a place where, owing to this cause, a perpetual flame was observed to rise§: and the baths of Bithynia, described by a poet of the time of

* Guidot, who wrote on the Bath waters in 1696, speaking of the ochreous sediment thrown up by the spring, says: "ochra hic cum scaturigine regiâ erumpens jugiter, *perenni ebullitionis motu*, ita elaboratur, ut tabulis affixa pictoriam æmuletur."

† Travels, vol. iv. p. 248.

‡ He causes Deianira to relate, that some of the wool stained with the blood of the Centaur Nessus, falling upon the Trachinian plain, in a place where the sun's rays were the most fierce, there boiled up from the earth *frothy bubbles.*

.... ἐκ δὲ γῆς, ὅθεν
προὔκειτ', ἀναζέουσι θρομβώδεις ἀφροί.—Sophoclis Trachiniæ, vers. 701. (Ed. Brunck.)

§ See Holland's Travels in Greece and Albania.

JUSTINIAN* as discharging a hot bubbling fluid†, which (as may be gathered from another passage of the same poem‡,) appeared to boil up, owing to the escape of bubbles of sulphuretted hydrogen, are still found to be impregnated with this same gaseous ingredient§.

To return to the case of the Bath waters, there is no evidence, that it has at any time been the practice to cool them down artificially, before they were employed for bathing; and hence it seems fair to conclude, that they never, since the earliest period at which they have been known, were hotter than they are now, their present temperature probably approaching the highest point which the human body can sustain without inconvenience. From the permanency of the temperature, therefore, we may be led to presume that of the other properties which at present characterize the spring.

It may also be alleged, that geology supplies us with examples of hot springs, of many distinct epochs; some, like that of Castellamare near Naples, connected with the volcano there existing; others, like that of Mount Dor in Auvergne, with volcanos long extinct, but which yet are not more remote than the tertiary period at farthest; whilst a third class, like those of Bath and Buxton, probably are of a much greater antiquity. Yet in all these cases the same evolution of nitrogen is observed; so that we are not at liberty to consider this, as a phenomenon resulting from any one particular period in the existence of a mineral spring, but as one continuing in it from first to last, or at least during a space of time of very extended duration.

Perhaps we shall best explain this regular and long-continued evolution of elastic fluid from the bowels of the earth, by supposing a large amount of these gases to be pent up in some cavern existing in a rock, which is seated at a great depth below the surface, and which had been heated at some former period by volcanic action.

If such a mass of rock were of considerable dimensions, and consisted of materials which slowly transmitted heat, we might suppose its external portions to cool far more rapidly than the internal ones; in which case, the former contracting upon the

* PAULUS SILENTIARIUS (viz. Silence-keeper in the imperial palace at Constantinople). See BRUNCK's Analecta, vol. iii. p. 94, a poem entitled *εις τα εν Πυθιοις θερμα*.

† *ουτω προηλθε πᾶσι*
το θερμοβλυστον ῥεῖθρον,
'Ιπποκρατης ἀψυχος,
τεχνης ἀνευ Γαλην0ς.

"'T was thus the hot bubbling fluid issued for the benefit of mankind, an inanimate HIPPOCRATES, a GALEN untaught by art."

‡ Thus he supports his theory as to the cause of this and other thermal waters, by alleging the mephitic offensive stench which accompanies them:

ὀδμη γαρ εστιν, οἶδας,
μυδῶσα, δυσπνοουσα,
τρανον τε μαρτυροῦσα.

§ See, in WALPOLE's Memoirs on Greece and Turkey, a notice of these springs by the traveller BROWNE.

latter, and thus creating a pressure upon the contents of its included cavity, would prove the means of propelling a stream of air, proportionate to the degree of diminution that had taken place in its own temperature, through fissures towards the surface.

In some such way, perhaps, we may imagine the evolution of gaseous matter to go on for centuries, in a manner nearly as uniform, as the shrinking in the dimensions of the cavity, caused by the yielding of its walls to the pressure from without, may be supposed to proceed*.

Be that as it may, the above estimate of the quantity discharged will afford a standard, by which a comparison may be made at present between this and other thermal springs in the above respect, and which may be appealed to hereafter, should it be wished to ascertain, from time to time, whether any change has taken place in the nature of this particular spring, or in the causes from which its heat proceeds.

Whilst engaged in thus determining the aggregate amount of all the gas evolved by the spring, saving the small quantity that finds its way from apertures near the sides of the bath, by means of the apparatus above described, which enabled me to collect whatever was disengaged within an area of twenty feet, I conceived that it might be worth while at the same time to estimate, what proportion of the whole rises up immediately through the stone cylinder, eighteen inches in diameter, which exists in the centre of the bath.

This was readily ascertained by means of a smaller shield, or funnel-shaped apparatus, which exactly fitted that opening, and the results obtained in the space of each minute are accordingly registered in separate columns by the side of the former.

The mean quantity obtained, taking the average of nineteen observations, was 34·75 cubic inches; the maximum ever obtained, 80; the minimum, 5; the average variation

* The dimensions of such a cavern, or series of caverns, need not be supposed so much more considerable than those of many which have fallen under our observation, as to give rise to any serious difficulty. That at Speedwell Mine, in Derbyshire, contains a pool of water 320 feet in depth, and rises to a height of more than 450 feet above the surface of the water. It is therefore nearly 800 feet in perpendicular height. That at Adelsburg, in Carinthia, one only of a series existing in that limestone formation, is in many places more than 100 feet in height, and extends, it is said, to a distance of nine or ten miles. One lately noticed by a traveller in the Caucasus (Colonel MONTEITH) is 600 feet high, 1200 feet in span, and 800 feet in depth. It probably communicates with others by means of fissures. See Geographical Journal, vol. iii., lately published. Now, if we suppose 250 cubic feet of gas to have been expelled daily from a cavern underneath Bath, for a period of about 5000 years, the whole quantity given off would amount to 456,250,000 cubic feet. A cavity, therefore, equal to 2000 cubic feet in its entire dimensions, if in the course of that period it had contracted to $\frac{1}{17}$th of its original size in consequence of the cooling of its walls, would have expelled a quantity of gas corresponding with that which the Bath waters have disengaged within that period. But a contraction of $\frac{1}{17}$th may without difficulty be imagined to have resulted from the cooling down of a mass of rock from 13,000° FAHR. to 400°; for glass contracts about $\frac{1}{1200}$dth part between 212° and 32°, or by cooling 180°; consequently a rock which contracted in an equal ratio with glass would diminish in bulk $\frac{1}{17}$th by an abstraction of heat equal to 12,600°, assuming even that the expansion at elevated temperatures is not more considerable than it is at low ones.

between the quantity at one minute and another appearing to be as 53·5 to 21·5 cubic inches, or as 5 to 2. So that the quantity evolved from the central orifice seems to be about one seventh of that from the area from which the gas was before collected.

Upon reviewing the aggregate of the observations above detailed, I cannot bring myself to believe, that the gas at present disengaged in a given time from the King's Bath is to be regarded as invariable in quantity; for the differences between the results obtained on one day and another are too considerable to be referred to errors of manipulation, or to the escape of the gas in a greater or less degree from other avenues. Besides, there will be seen, by referring to the annexed Table, a kind of flux and reflux in the quantities obtained; that of September 17th exceeding the mean by no less than seventeen cubic inches, those of the 18th, 19th, 20th, 21st, 23rd falling short of it by variable quantities; that of the 24th exceeding it again; on the 25th and 26th approaching it very nearly; on the 27th, and again the next time of observing, namely on October 2nd, exceeding the mean; from thence till the middle of the 5th falling short of it a little, then till the 9th exceeding the mean, on the 17th falling short of it again, but on the 18th again rising somewhat beyond the average.

If, then, a variation in the quantity of gas emitted seems to be fairly substantiated by the observations I have recorded, it becomes a subject for inquiry, to what cause this irregularity may be ascribed.

I at first imagined, that its emission might be in some degree controlled by atmospheric pressure; but the general tenor of the observations seems to dispell this notion, or at least to show, that there are other causes by which its flow is in a greater degree affected. Neither do the other conditions of the atmosphere noticed in the Table appear to exert any appreciable influence upon the current of gas, though, as the weather during the time I spent at Bath was in general fine, and during a large portion of the time remarkably steady for the season, it were to be wished that some gentleman resident on the spot would avail himself of the opportunities that might present themselves for examining the spring under a greater variety of circumstances; especially, as it has been vaguely stated with regard to some other hot springs, on the authority of casual observers, that the evolution of gas is greatest during storms and gales of wind*.

With regard to the quality of the gases given off, I have but little to add to what had been before determined. In the air I collected, oxygen certainly was present, as indeed Sir G. Gibbes had already ascertained to be the case, by the test of nitrous gas. Phosphorus heated in a bent tube with a measured portion of the air causes a diminution in its volume, which in almost all my trials amounted so nearly to 1·25 per cent., that I set down the proportion of those two gases one to the other as probably constant; and if we grant that nitrogen obtains an increase of bulk amounting to 2·5 per cent. by phosphorus vapour, I cannot be far wrong in reckoning

* The same thing has been noticed with regard to volcanos. See Scrope's Considerations on Volcanos, p. 7.

the oxygen at rather less than four parts, and the nitrogen as rather more than 96, in the 100. This, however, is to be understood as only applying to the gas remaining after having been agitated with a solution of potash; for there is always present a certain portion of carbonic acid, which on some occasions appeared to amount nearly to what Mr. R. Phillips in his analysis has stated it at, namely at 4·5 per cent. of the whole quantity.

I have at other times, however, found it to reach 8 or 9, and once even 13 per cent.; so that I am forced to conclude this element in its composition to be of variable amount.

As, however, the quantity that escapes is only the excess over and above that which the water itself holds in solution, the cause of its variation may perhaps be explained, without imagining it an indication of any change in the nature or intensity of the processes to which the heat of the springs is owing.

The quantity of water which is discharged per minute has been calculated at about 146 gallons, of which 126 gallons are received at the King's and Queen's Bath, 10 at the Cross Bath, and about 10 at what is called the Hot Bath.

Now it is stated by Mr. Phillips, that every pint of the water contains 1·2 cubic inch of carbonic acid; so that about 1400 cubic inches of carbonic acid are dissolved in the water, whilst the quantity that escapes may vary from about 12·3 to 36·0 cubic inches*.

It is evident, therefore, that an increase in the supply of water from 146 to 150 gallons would occasion the whole of the gas to be absorbed, and that a difference of only two gallons and a half in the amount of the water discharged would account for the utmost variation in the quantity of carbonic acid, which I have ever detected between one day and the next.

Now, though the supply of water is remarkably uniform, I conceive, that so slight a variation as that hinted at, might easily take place without its being observed†.

It might be worth while to ascertain, whether the quantity of gas evolved bears any relation, either to the temperature or the copiousness of the spring from which it rises; and, so far as the thermal waters of this country are concerned, it would appear,

* Mr. Walcker's analysis (Journal of Science for June 1829,) differs from the above, as he calculates only 0·95 of a cubic inch of carbonic acid in every pint of the water, or 7·6 cubic inches to the gallon. Even then, however, 957·6 cubic inches of gas would be given off every minute by the spring, even if we do not count any portion of that which exists in combination with the iron, lime, or alkalies, as derived from the interior of the earth. The latter Mr. Walcker has stated at 1·62 cubic inch in every pint.

† This, however, assumes, what perhaps will not be granted, namely, that the carbonic acid present in the water is derived from the gas emitted from below, and not from the atmosphere. A cold spring from the lower part of the city close to the Cross Bath, yielded me in the pint,

Carbonic acid	0·90
Nitrogen	0·64
Oxygen	0·13
Total	1·67 cubic inch.

that such a conjecture is somewhat contenanced by what is observed at Bath, where the hottest and most abundant emits the largest quantity of elastic products.

Thus the King's Bath, which possesses, as I have ascertained by almost daily observations for a month by a thermometer with a scale divided to half-degrees of Fahrenheit, an uniform temperature of 115°, and which evolves 126 gallons of water per minute, disengages on the average about 240 cubic inches of nitrogen, whilst the Cross Bath, which affords only about eight gallons, and is at 96°, gives out only 12 cubic inches of gas. What the quantity may be from the Hot Bath, which, besides being hotter, is also somewhat more copious than the Cross, I have not had the means of correctly ascertaining, as, at the spot where it issues from the earth, it is covered over; but I have reason to believe that the emission of gas from it must be small.

The only other warm spring, which I have as yet examined with reference to this point, is that called Taafe's Well, already noticed as occurring near Cardiff, in which the thermometer rises only to 70, and this, which discharges much less water than the others mentioned, gives out only 22·5 * cubic inches per minute †.

But we ought not to build on so scanty an induction of particulars, and must pause for the present, in the hope, that in other countries those who may be favourably circumstanced for such inquiries will repeat, with reference to the thermal springs of the Continent, the same observations which I have undertaken at Bath.

Having now, as I hope, faithfully recorded the limits, within which the quantities of elastic fluid evolved by the principal Hot Spring at Bath appeared to fluctuate, during the period of my observations, and submitted to the consideration of the members of this Society a mode of accounting for its regular and nearly equable disengagement, I shall forbear to speculate on the causes of its peculiar chemical constitution, or to dwell upon the inferences that might be deduced from its presence, with regard to thermal springs in general.

I will only remark, that the largeness of the volume of nitrogen gas which is disengaged, and the entire absence of carburetted, sulphuretted and phosphuretted hydrogen, seem to afford an additional presumption against the idea, advanced by a distinguished chemist in a paper recently published in our Transactions, that the nitrogen gas which escapes from volcanos and from thermal springs may be derived from the atmospheric air, held in chemical solution by water generally, but deprived in these instances of the greater part of its oxygen by animal and vegetable putrefaction. It seems obvious, that no amount of water, which can be supposed to obtain access to the depths at which the heat originates, could be sufficient to supply so

* This must be considered only a rude approximation, as I had no apparatus large enough to cover over the whole of the bath, and consequently to collect all the gas that rises at once.

† This gas contained no carbonic acid, but consisted of

Oxygen	3·5	(allowing an expansion of 2·5 for phosphorus vapour, as in the other cases,)
Nitrogen	96·5	

In the 100 parts.

large a quantity of nitrogen, and likewise that no such quantity of vegetable and animal matter could there exist, as would be requisite in order to absorb a corresponding proportion of oxygen.

Neither in such a case could the nitrogen that escaped arrive at the surface, without being contaminated with some of the inflammable products, that commonly arise from the decomposition of organic matter.

I look, therefore, to some process of combustion, during which the atmospheric air that finds admittance is in great measure deprived of its oxygen, as a likelier mode of accounting for the peculiar constitution of the gas emitted; and conceiving that the carbonic acid that accompanies it, is more probably derived from the calcination of earthy carbonates, than from the combustion of beds of coal or bitumen, I am led to conclude from the frequent absence of other gaseous products, that the oxygen becomes united to some base, which forms with it a compound not easily volatilized by heat.

How far these conclusions, if considered to be substantiated, tend to support that theory of volcanos and the connected phenomena, which naturally emanated from the discoveries of our former illustrious President, who also, at one period at least of his life, himself advocated it, must be left for the Society to decide, as it would ill become me to do more, than to lay before its members a statement of such facts, as appear to bear upon a question, respecting which the highest authorities in science have been divided.

In conclusion, therefore, it only remains, that I should express my obligations to the gentlemen who constitute the Committee of the Bath Waters, to whom I applied for leave to institute the above observations, for their ready acquiescence in my wishes, and for the facilities afforded me in the prosecution of these researches.

I must, likewise, acknowledge the kind assistance I received from several of the residents of Bath, particularly from Mr. G. Spry, who has long taken an active concern in the conduct and management of these springs; and from Mr. Thomas Stephens Davies, Fellow of the Royal and the Astronomical Societies, a gentleman well known for several valuable mathematical papers, to whom I am indebted, not only for much occasional information relative to the springs, but also for having, at a considerable sacrifice of time and convenience, attended at the bath whenever the observations were made, and taken upon himself the task of minuting their duration, and of noting down with the utmost regularity the quantities of gas each time obtained.

REGISTER of the quantity and quality of the gases evolved by the hot spring which supplies the King's Bath in the city of Bath, at the times and under the circumstances stated below.

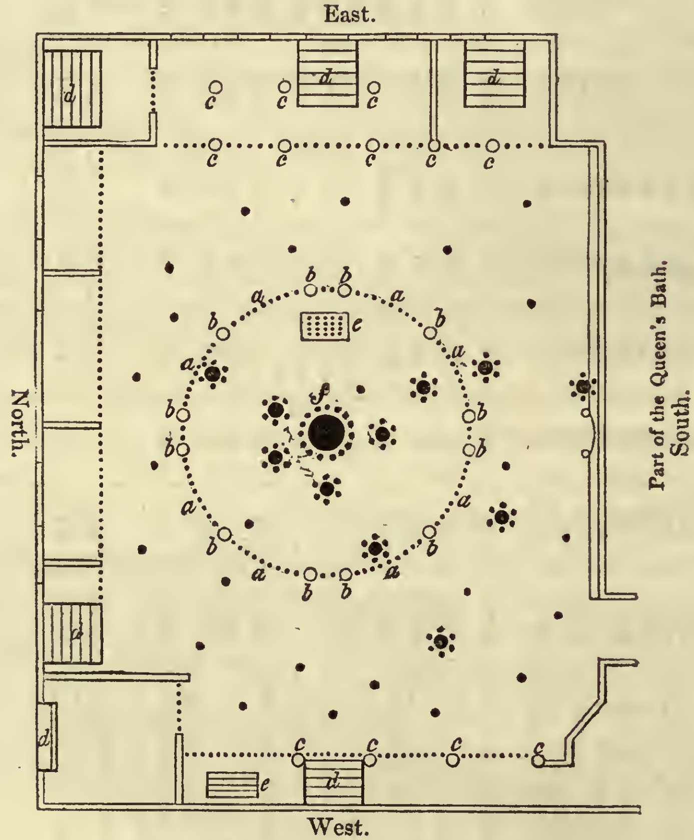
Obs.	Date.	Hour.	Weather.	Barometer.	Thermometer, FAHR.	Dew point.	Gas collected per minute from the central orifice.			Gas collected per minute from an area of twenty feet in the centre of the bath.			Amount of carbonic acid per cent. each day.	Duration of each observation.
							Max.	Min.	Mean.	Max.	Min.	Mean.		
	1833.													Minutes.
1.	Sept. 17.	4 P.M.	Sunshine and rain alternately	29·525	60	....	....	....	28	....	....	280	8·00	5
2.	18.	2 P.M.	Sunshine and rain alternately	29·7	64	50	....	....	28	....	....	253	9·25	5
3.	19.	1 P.M.	Sunshine, followed by storms of rain	29·9	66	52	80	15	39	....	....	194	....	6
4. ☽	20.	1 P.M.	Dull morning, followed by bright sunshine	30·1	60	49	65	20	39	....	....	230	....	7
5.	21.	1 P.M.	Much as the 20th	30·0	66	52	55	20	30	....	....	253	13·50	6
6.	23.	1 P.M.	Fine bright sun during most of the day	29·7	66	49	50	30	40	280	80	247	....	10
7.	24.	1 P.M.	Stormy morning; cloudy afternoon	29·2	62	58	45	35	40	310	220	276	....	10
8.	25.	1 P.M.	Storms of rain, and sunshine, alternately	29·555	61	48	40	30	35	300	220	266	....	10
9.	26.	1 P.M.	Bright sunshine, and cloudless sky	29·65	61	48	40	30	35	300	220	266	....	9
10.	27.	1 P.M.	Bright sunshine as on the 26th	29·72 *	59	49	50	10	30	345	220	276·5	....	12
○	28.													
11.	Oct. 2.	between 1 & 2 P.M.	Bright sunshine, and cloudless sky	30·05 †	63	47	60	15	35	330	240	274·5	....	10
12.	3.	between 1 & 2 P.M.	As on the 2nd	30·00	58	51	50	20	30	310	215	266	4·60	10
13.	4.	1 P.M.	As on the 3rd	29·975	59	49	60	10	36·5	330	200	266·5	4·66	10
14.	5.	1 P.M.	Occasionally overcast, with gleams of sunshine alternately	30·0	58	47	50	23	40·5	310	220	262	9·00	10
15.	....	6 P.M.	Calm, but cloudy evening	30·0	54	....	....	....	....	350	200	265	....	11
16.	....	12 midnight	Calm, but cloudy evening	....	....	....	....	....	....	340	250	280·5	....	10
17. ☾	6.	between 7 & 8 A.M.	Fine bright morning	....	....	....	....	....	....	530	220	300·5	4·60	13
18.	....	1 P.M.	Fine sultry day, somewhat less clear than the preceding ones	29·9	59	49	40	15	30	370	200	295	....	12
19.	7.	1 P.M.	Dull morning; fine afternoon	30·0	57	47	65	5	30	325	235	272	4·50	15
20.	8.	1 P.M.	Dull morning; fine afternoon	29·95	55	43	40	30	33	320	180	271·5	9·00	15
21.	9.	between 1 & 2 P.M.	Very bright morning; duller afternoon, with fleecy clouds, but sunshine.—Wind changed to the west	30·05	58	45	80	35	48	300	215	256·6	9·00	15
●	13.													
22.	17.	1 P.M.	Bright morning; afternoon overcast.—Wind north	29·625	56	....	....	....	....	355	180	244	....	10
23.	18.	Both obs. betwixt 1 and 2 P.M. at intervals of 10 min.	Dull and damp morning	29·5	54	47	40	25	33·3	400	200	266	....	10
24.	....		Rain in the afternoon	....	....	....	....	....	....	350	230	270	5·66	10
Mean of all the Observations				29·814	59·8	48·85	53·5	21·5	34·75	339	207	263·92 ‡	7·43	10·025
Maximum of all the Observations				30·1	66	52	80	35	48	530	250	300·5	13·5	15
Minimum of all the Observations				29·2	54	43	40	5	28	280	80	194 §	4·5	5
Difference between the Maximum and Minimum				0·9	12	9	40	30	20	250	170	106·5 ‖	9·0	10

* Falling since the morning. † Falling. ‡ Or, omitting obs. 3. as inexact, 266·96. § Or, omitting obs. 3., 230. ‖ Or, 70·5.

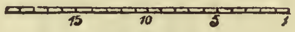
Largest quantity obtained being (300·5 × 1440 = 432,720) at the rate of 432,720 cubic inches, or, 250 cubic feet 72 cubic inches, in the 24 hours.

Smallest quantity, (194 × 1440 = 279,360) at the rate of 279,360 cubic inches, or 161 cubic feet 1152 cubic inches, in the 24 hours. Or, omitting obs. 3, (230 × 1440 = 331,200) at the rate of 331,200 cubic inches, or, 191 cubic feet 1152 cubic inches, in the 24 hours.

Mean quantity, (263,92 × 1440 = 380,044) at the rate of 380,044 cubic inches, or, 219 cubic feet 1612 cubic inches, in the 24 hours. Or, omitting obs. 3, (266·96 × 1440 = 384,422) at the rate of 384,442 cubic inches, or, 222 cubic feet 806 cubic inches, in the 24 hours.



Ground Plan of the King's Bath.



Scale of Feet.

Description.

a. Area, inclosed by the hand-rail, twenty feet in diameter.
b. Upright bars supporting the hand-rail.
c. Pillars supporting the roof of the covered portion of the bath.
d. Steps leading down into the bath.
• :●: Apertures in the bath through which the water and gas rise.

.... Portions of the sides of the bath which are under cover.

e. Drains through which the water is allowed to discharge itself.

f. Stone cylinder eighteen inches in diameter occupying the centre of the bath, through which the spring principally rises.

APPENDIX.

Received January 22,—Read January 23, 1834.

Subsequently to the reading of the above Paper, I have had an opportunity of examining two tepid springs, which, since the setting in of the wet weather, have broken out at the foot of St. Vincent's Rocks, Clifton, immediately below the cliff from which the Suspension Bridge over the Avon was designed to spring, and on the summit of which the Observatory is erected.

The temperature of the one is 72°, that of the other 66° of Fahrenheit; and both are continually emitting bubbles of gas, which I found to consist of

Carbonic acid	3
Oxygen	8
Nitrogen	92
	103

The circumstance may be worth noticing on two accounts. First, as evincing that the cause, whatever it may be, of the heat, is not limited to a single spot, but is diffused over a considerable space in the direction along which the chasm extends; thus confirming an observation made to me by that intelligent naturalist, the late Mr. Miller of Bristol, that when, in consequence of rain, a new spring appears at the foot of these cliffs, its temperature is generally higher than that of the ordinary springs of the district: and secondly, as adding one additional instance to the catalogue of those thermal waters, which are accompanied by an evolution of nitrogen gas.

This is the more important, because, owing to the circumstance of the principal tepid spring at Bristol, which is preserved for medicinal purposes, being covered over at its source, it was impossible to ascertain from this one, whether the thermal waters of this place agreed in that particular with those of other localities, or whether they constituted an exception to the generality of this rule.

January 22nd, 1834.

II. *On the Empirical Laws of the Tides in the Port of London; with some Reflexions on the Theory. By the Rev.* William Whewell, *A.M., F.R.S., Fellow and Tutor of Trinity College, Cambridge.*

Received November 13, 1833,—Read January 9, 1834.

THE present state of our knowledge of the tides is remarkably at variance with the complete and scientific character which Physical Astronomy is, in common opinion, supposed to have attained. We may, perhaps, most easily figure to ourselves the real condition of this subject, by imagining what the condition of other branches of astronomy would be, if some great natural or moral convulsion should sweep away our existing science, and replunge us in the ignorance of the dark ages, leaving extant only a few general notions concerning the theories which are at present established. In such a state of things, we may suppose that some tradition of the doctrine of universal gravitation would survive the change, and that learned men would still go on asserting that the various astronomical phenomena of the universe were owing to that cause; but the resources of mathematical art being, for the time, lost, they would be unable to prove the truth of such assertions: and, both the collected stores of observation, and the habit and apparatus of observing, being, in such a case, supposed to be annihilated, it would be long before there would arise persons able and willing to supply such deficiency; the more so as those who might make such collections would have still to seek for the mode of turning them to any use. If, in this state of things, a few persons should, by their own sagacity and labour, or by the aid of some traditionary secret, attain to the power of predicting phenomena with tolerable correctness, we may imagine that they would use their peculiar skill for purposes of gain, and that they would not readily admit the world at large to the knowledge of the secret which gave them a superiority over the rest of their countrymen.

Our knowledge of the tides, at the present time, exactly realizes this imaginary condition which we have supposed for astronomy in general. Our philosophers assert, without hesitation, that this phenomenon is the result of the law of the universal gravitation of matter; yet no one has hitherto deduced, from this law, the laws by which the phenomena are actually regulated with regard to time and place. Analysis has been largely used; but it has been employed only to deduce the consequences of certain assumed suppositions, which suppositions are acknowledged to be utterly different from the real state of the case: and where is the immediate advantage, for the purposes of sound philosophy, of analysis which does not solve the problem proposed, over no analysis at all? Some observations of the tides have no doubt been made, and more are now making; but it is not too much to say, that these are only a commencement of the collections which the subject will require, to place it on a par with the other provinces of physical astronomy. The laws which connect the course of the observed tides with the motions and distances of the sun and moon are not known for any single port; and the tables, which in every other province of physics are the result of the knowledge which our men of science have accumulated for us, are, in this department, published by persons possessing and professing no theoretical views on the subject; and the methods by which they are calculated are not only not a portion of our published knowledge, but are guarded as secrets, and handed down as private property from one generation to another*.

Of course it cannot be intended here to speak with any disrespect of the persons who have calculated tide tables under these circumstances. Their labours are useful to the community in proportion as their tables are exact, which some of them are to a very remarkable degree. And, as no one thinks of condemning other persons who make a profit of any peculiar and secret knowledge which they may possess connected with any of the useful arts, there would be no justice in blaming those who do the same with respect to secrets which concern one of the most important arts, namely, navigation. But the circumstance most worthy of remark is, that there *should be* secrets in such a matter; that on such a subject our men of science should be ignorant of, and unable to discover, that which persons of much less elevated pretensions know and apply; that the laws which are to be collected either by the observation of facts, or by the deductions of theory, should not be known to our philosophers by either method, and yet should be in the possession of other persons, to a considerable extent. This circumstance makes our knowledge of the tides assume the character rather of a mere practical art, than of a portion of that complete and perfect science of which the other consequences of the law of universal gravitation supply examples.

Some persons may conceive that, in what has been said, I am disparaging too much the labours of the great mathematicians, NEWTON, BERNOULLI, LAPLACE and others, who have employed their skill on this subject. But this opinion cannot, I conceive, be maintained with justice. It is well known that all the mathematical solutions of the problem have confessedly gone upon suppositions very remote from the real facts: NEWTON and BERNOULLI, for instance, have assumed the form of the fluid spheroid, under the influence of the sun and moon, to be the form of equilibrium: LAPLACE has supposed the whole globe to be covered with water of an uniform depth. It is in no degree clear, that investigations conducted on such assumptions will give us even an approximation to the true result; and the only way in which the assumptions could be justified, would be by our finding, from observation, that the laws of the facts are such, or nearly such, as these hypothetical calculations give. If this agreement were

* What is here asserted was strictly true till the publication of Mr. LUBBOCK's Memoir on the Tides of the Port of London, and his Tide Tables, founded on his discussion of these. At present his Tide Tables are calculated by published methods; but the laws which these methods imply have not yet been compared with theory.

established, it would then, no doubt, become highly probable that the simplifications hypothetically introduced into the natural state of things were not such as materially to alter the general course of the phenomena.

But this has not been done by any of the theoretical writers above referred to. Undoubtedly most of them have undertaken to show that *some* of the known laws of the facts are accounted for by the theory, and that the measures of *some* of the phenomena agree with those which theoretical calculations give. But this has been executed only with respect to a few of the circumstances of the case. It has not been shown, by any writer, that the *general course* of the effects produced upon the tides, by the changes of position and distance of the heavenly bodies, is such as, according to the mathematical reasoning, it ought to be. In short, the mathematicians who have treated this subject have not completed their task by giving rules for the calculation of tide tables, and showing that the tables so produced agree with the general course of the observations in all essential circumstances.

The task just mentioned would consist of two parts; the theoretical deduction of the effects produced in the tides by changes of distance and position of the sun and moon; and the examination of the laws which such changes appear to follow in the observations; with a comparison of the two sets of results. The latter part of the task had not been executed, so far as I am aware, by any one, previously to Mr. Lubbock's discussion of the Tides of the Port of London, inserted in the Philosophical Transactions for 1831; and that memoir is hitherto the only published record of such an examination. The establishment, on theoretical grounds, of rules for the calculation of tide tables, has been attempted by Bernoulli and by Laplace. The methods recommended by the former are probably the foundation of those at present used by the calculators of such tables. The method of Laplace is complicated, and would be very laborious in practice. He has unfortunately, as appears to me, not put his process in such a form as to give a principal term, with smaller corrections for declination, parallax, and other circumstances if necessary, to be combined with the principal term. When the results of such an investigation are not made to assume this shape, the comparison of the formula with observation becomes a work of very repulsive labour and trouble.

It has already been stated, that some of the published tide tables are found to be not very incorrect when compared with observation. If any tide tables were so good that they might be considered as representing the general laws of the actual phenomena, we might discuss such tables, and compare them with theory, in the same manner as if they were the records of observation; and with this additional advantage, that they would be free from the effect of the accidental causes, as wind and other circumstances, which produce irregularities in the actual tide. Nor would it be difficult, by such a discussion, to discover the rules which are followed in the construction of such tables.

It may, however, be doubted whether there are any tables which are worth this trouble. Original tide tables are very few: I know of none except those which are published for Liverpool, and those for London. The former are remarkably exact; they are calculated according to rules obtained by Mr. HOLDEN, some years ago, from the examination of five years of observations made at the Liverpool Docks by Mr. HUTCHINSON, at that time harbour-master. The calculations are at present conducted by the Rev. GEORGE HOLDEN, of Maghull, a descendant of the person who first invented the rules. Other Liverpool tide tables are also calculated by Mr. WOFFINDEN. Of London tides several, apparently independent, tables are annually published; and though the differences of these are considerable, I do not know that any one set is considered as possessing a decided superiority in the general result. I am not aware that any tide tables are published for Brest, though so large a collection of observations has been made at that port, and though so much labour has been employed in the discussion of these, for the purpose of comparing certain points of LAPLACE's theory with them: nor have, I believe, tide tables for any place been calculated according to the method recommended in the Mécanique Celeste.

The method generally practised in England for the construction of tide tables for other places has been, to take the time which is stated in the London or the Liverpool tables, and, if necessary, to add or subtract some constant quantity, according to the place. The Liverpool tide tables are in this manner used, generally without correction, for the whole of the north-western coast of England: and tables are published professing to give the hours at most of the principal ports of England, in parallel columns; the hours at different places having constant differences. Thus the hour of high water at Plymouth is stated as always $1^h\ 55^m$ later than the hour in the same half-day at London. This assumption of a constant difference in the hours of high water at different places is, however, inexact; as we should expect it to be from considering the mode in which the tide is transmitted from one place to another, and as it appears to be from observation.

It appears, therefore, that the most promising mode of advancing our knowledge of the tides, is to examine the laws which can be collected from observation, taking so great a number of observations, that the effects of all accidental causes may disappear in the average results. The collection of observations discussed by Mr. DESSIOU, under the direction of Mr. LUBBOCK, affords us an admirable opportunity for this examination; the collection including 13073 observations, and a period of nineteen years, from January 1st, 1808, to December 31st, 1826. Our object in this examination being to ascertain the manner in which the positions and distances of the heavenly bodies affect the time and height of high water, the mode of proceeding must be to examine how these two quantities depend upon the right ascension, declination and parallax of the sun and moon, and upon other astronomical elements, if such are found to be needed. The mean time of high water will be found to be affected by inequalities, depending on the elements just mentioned; and the law and amount of these inequalities may be collected from observations, without any reference to theory, (provided the observations are sufficiently numerous and their circumstances sufficiently varied,) in the same manner in which the greater inequalities of the moon, the variation, evection and annual equation, were detected by observation, long before the motions of the heavenly bodies were referred to their true causes. Indeed, I believe the instances are comparatively few in the history of philosophy, in which the general laws of the phenomena have been pointed out by the theory before they had been gathered by observation. The laws of the tides, thus empirically obtained, may be used either as tests of the extant theories, or as suggestions for the improvement of those portions of mathematical hydraulics on which the true theory must depend. And this is the way in which we are most likely to discover how the theory must be applied. The problems regarding the motion of fluids, which we are unable to solve directly, are far too numerous to allow us to be surprised that we should be obliged to desert the *à priori* road in this case. The phenomena of waves, the motions of water in tubes, in canals, in rivers, the motion of winds, the resistance of fluids to bodies in motion, are all cases in which we are yet far from having drawn our analytical mechanics into a coincidence with experiment, or even a tolerable proximity to it. The theoretical analysis of the tides is, at present, in an equally imperfect state. It is not at all improbable that, as in many other cases, this problem in the mechanism of the solar system (for such it is) may be found in the end less complex and difficult than similar problems concerning the motions of smaller masses; but the problem remains still to be solved, or at least it remains still to be shown that the solution has been approximated to. I shall therefore here proceed to examine the empirical laws of the tides of the port of London, as they appear from the records of the nineteen years of observations above mentioned.

Chap. I. *On the Empirical Laws of the Time of High Water.*

The point which I have first to determine is, the manner in which the time of high water is affected by the right ascensions, declinations, and parallaxes of the sun and moon. For this purpose I shall have to consider *the establishment, the semimenstrual inequality, the corrections for lunar parallax, lunar declination,* and *solar parallax and declination.*

1. *The Establishment.*—The vulgar establishment of any port is the interval of time by which the time of high water follows the moon's transit *on the day of the new and full moon.* But it is the *mean* value of this interval of time which we must here employ, in order to simplify our discussion. This is what Laplace calls the *fundamental hour* of the port: I have termed it, in a former paper on this subject, the *corrected establishment,* since it is the lunar hour of high water, freed from the semimenstrual inequality. Its value at the London Docks is $1^h\ 26^m$, by the mean of all the observations.

2. *The Semimenstrual Inequality.*—The interval of tide and moon's transit is affected by a considerable inequality, which goes through its period twice in the space of one month: it may be considered as depending upon the moon's distance from the sun in right ascension; or, which is the same thing, on the solar time of the moon's transit. It has been examined by Mr. Lubbock, and shown to agree, with remarkable exactness, with the formula,

$$\tan 2(\theta' - \lambda') = -\frac{h \sin 2(\varphi - \alpha)}{h' + h \cos 2(\varphi - \alpha)},$$

in which λ' is the mean interval of the tide and transit, and θ' the correct interval; φ the solar time of the moon's transit, and α a constant quantity. The ratio of the quantity h' to h is 2·9884 : 1; the quantity α is 2 hours.

According either to the method of BERNOULLI or to that of LAPLACE, there would result from the theory an expression of the above form, for the interval of tide and moon's transit. By assuming suitably the values of $\frac{h}{h'}$ and α, the results of observation at other places may also be made to agree very closely with the above formula. The curves which represent by their ordinates the successive values of the above formula, when constructed for different places, exhibit a remarkable general similarity, as may be seen in the Philosophical Transactions, 1831, where Mr. LUBBOCK has given these curves for Portsmouth, Plymouth, Sheerness, London and Brest. The curve is symmetrical with respect to the axis, intersecting it when $\varphi = \alpha$, and when $\varphi = \alpha +$ a fourth of a circumference. Its ordinate has a negative minimum and a positive maximum, which are equal in magnitude; but these values are not midway between the values 0, consequently the ordinate increases more rapidly after the minimum and before the maximum, than it diminishes before the minimum and after the maximum. This property appears very clearly in the curves constructed for all the above ports.

But in other respects the result of the observations, thus compared, does not agree with the theory. According to the theory, the quantities h and h' express the amount of the separate solar and lunar tides respectively, and as the ratio of these effects must be the same for all places, the maximum value of the semimenstrual inequality ought to be the same in all the above cases; namely, the time corresponding to half the angle whose tangent is $\frac{h}{\sqrt{h'^2 - h^2}}$. If, as LAPLACE finds from the Brest observations, $\frac{h'}{h} = 2·6157$, the angle corresponding to the above tangent is $22° 28'$; the maximum value of the inequality is 45^m, and the double of this, or $1^h 30^m$, is the difference of the greatest and least interval of the tide and moon's transit.

According to observation, the difference of the greatest and least intervals is as follows*:

London	$1^h 28^m$
Sheerness	1 29
Portsmouth	1 21
Plymouth	1 36
Brest	1 19

It appears unlikely that the difference in these values for Plymouth and Brest, or even Plymouth and Portsmouth, can depend upon accidental causes, or too limited a number of observations. It would appear, therefore, that the *coefficient of the semimenstrual inequality*, $\left(\frac{h}{h'}\right)$, is different at different places; a circumstance which no extant theory would have led us to expect. This subject, however, deserves further

* We suppose here the effects of parallax and declination to be eliminated by the averages of the observations.

examination; and it would be important for this, as well as for other purposes, to discuss some large collection of observations for other places than London, in the mode which Mr. LUBBOCK has applied to the London observations. Such collections are known to exist for Brest and for Liverpool.

The quantity α in the formula is undoubtedly different at different places. It is what Mr. LUBBOCK, following LAPLACE, calls the *retard*, and depends upon what I have termed the *age of the tide*. It cannot be determined with certainty or exactness without the use of a large body of observations. Its value at London is 2^h, at Brest $1^h\ 12^m$; at Portsmouth it is intermediate between the value at Brest and at London, as we should expect, being about $1^h\ 30^m$; but at Plymouth it is greater than it is at London, which, as Mr. LUBBOCK observes, is at present a very inexplicable circumstance; probably to be explained only by the determination of the value of this quantity for several other places.

3. *The Correction for Lunar Parallax.*—Mr. LUBBOCK has classified the tide observations which he has discussed according to the value of the moon's horizontal parallax which existed at the time when the tide occurred, and also according to the hour of the moon's transit, so as to form a table of double entry of the differences from the mean interval: this is Table XVII. in his Memoir of 1831, which I here insert.

TABLE showing the Difference in the Interval between the Time of the Moon's Transit and the Time of High Water, and the Mean Interval (Column A. Table III.) for every Minute of the Moon's Horizontal Parallax.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	m	m	m	m	m	m	m	m
0 0	+12	+ 9	+ 4	....	− 3	− 4	−13	−14
0 30	+12	+ 9	+ 2	+ 2	− 3	− 5	− 9	−11
1 0	+10	+ 8	+ 3	+ 5	− 1	− 4	− 9	−11
1 30	+ 8	+ 5	+ 3	+ 5	− 1	− 3	−10	−11
2 0	+ 8	+ 6	+ 2	+ 3	+ 1	− 1	− 8	− 9
2 30	+ 7	+ 5	+ 1	+ 1	+ 2	− 2	− 6	− 8
3 0	+ 6	+ 4	+ 2	....	+ 2	− 2	− 6	
3 30	+ 6	+ 4	+ 3	+ 1	+ 3	− 2	− 5	
4 0	+ 4	+ 3	+ 2	− 1	+ 2	− 2	− 6	
4 30	....	+ 1	+ 3	− 1	− 1	− 2	− 8	
5 0	+ 1	+ 1	+ 3	+ 1	0	− 1		
5 30	+ 1	0	− 1	+ 2	− 1	− 1		
6 0	+ 1	+ 1	− 3	+ 1	− 2	− 2		
6 30	+ 2	+ 4	− 3	− 1	− 3	− 3		
7 0	+ 4	+ 2	− 3	− 2	− 5	− 4		
7 30	+ 9	− 2	− 2	− 4	− 7	− 7	− 7	
8 0	+16	0	0	− 3	− 8	− 6	−11	
8 30	+21	+ 8	+ 3	+ 1	− 7	− 6	−12	
9 0	+19	+ 9	+ 4	+ 1	− 9	−10	−16	
9 30	+17	+11	+ 7	+ 1	− 8	−11	−18	
10 0	+16	+12	+ 8	0	− 5	−12	−17	
10 30	+15	+13	+ 8	− 1	− 2	−10	−14	−17
11 0	+13	+12	+ 7	− 2	− 2	− 8	−14	−18
11 30	+13	+10	+ 6	− 1	− 2	− 4	−16	−16

On examining this Table, it appears that in the column corresponding to H. P. 57′, the differences from the mean, or corrections for parallax, are very small for all hours of the moon's transit, (ranging from $+ 5^m$ to $- 4^m$,) and that the positive nearly balance the negative values. We may suppose, therefore, that for H. P. 57′ nearly, the correction is 0. It appears also that the correction is generally negative when the H. P. is greater than 57′, and positive when it is less, the exceptions being of small amount compared with the general mass of observations; and if we take the sums in each vertical column of Table XVII. we shall find that they are nearly as the difference of the parallax from the mean value 57′. It appears, therefore, that this correction must involve a factor $(P - p)$, when P is the mean horizontal parallax of the moon (or 57′), and p any other value of her horizontal parallax.

If we take any vertical column of this Table, and thus follow the correction through the various hours of the moon's transit, we find that for all values of the parallax the correction is very small, when the moon passes at $5^h\ 30^m$ or 6^h, and that the positive and negative values in that case nearly balance each other. In each column, when the hour of transit is either greater or less than this, the correction increases with the difference of hour, and proceeds to a maximum, which appears to occur about 9^h or 10^h transit. As a simple way of satisfying these conditions, we may suppose the correction to involve the factor $\sin^2(\phi - \beta)$ when β is a constant quantity: and combining this factor with the one already found, we shall have $B\ (P - p) \sin^2(\phi - \beta)$ for this correction in minutes of time.

It appears that in order to give the maximum value of this correction when it occurs at about 10^h, β must not be much different from 4^h. In order to determine B, take the formula $B\ (P - p) \sin^2 (\phi - \beta)$ for every half-hour: its value is

$$2\,B\,(P - p)\ \{\sin^2 7\tfrac{1}{2}° + \sin^2 15° + \sin^2 22\tfrac{1}{2}° + \sin^2 30° + \sin^2 37\tfrac{1}{2}° + \sin^2 45° + \sin^2 82\tfrac{1}{2}° + \sin^2 75° + \sin^2 67\tfrac{1}{2}° + \sin^2 60° + \sin^2 52\tfrac{1}{2}°\}$$
$$= 11\,B\,(P - p).$$

Comparing this with the sums for H. P. 54′, 55′, 56′, 57′, 58′, 59′, (the other columns being incomplete,) we have,

Horizontal parallax . .	54′	55′	56′	57′	58′	59′
Formula	33 B	22 B	11 B	0	−11 B	− 22B
Observed sums	221	135	59	8	−60	−112

Hence, taking the sums, $99\,B = 595$, whence $B = 6$, and the expression is $6\,(P - p) \sin^2 (\theta - 4^h)$.

This may be put in the form $3\,(P - p)\,\big(1 - \cos 2\,(\theta - 4^h)\big)$,

or
$$3\,(P - p)\,\big(1 + \sin 2\,(\theta - 1^h)\big).$$

The agreement with the sums observed is as follows:

Horizontal parallax . .	54′	55′	56′	57′	58′	59′
Formula	198	132	66	0	−66	−132
Observed sums	221	135	59	8	−60	−112

And on applying this correction, the residual quantities are, with one or two exceptions, within the limits + 4^{m} and — 4^{m}.

4. *The Correction for Lunar Declination.*—In Table IX. of his Memoir, Mr. Lubbock has arranged the intervals of the time of tide and moon's transit according to the declination of the moon, taken for every three degrees; and in Table XIX. he has given the difference of these intervals from the mean, arranged according to declination and time of transit. On inspecting this Table, it appears that this difference from the mean, or correction for lunar declination, is, for all values of the time of transit, 0 when the declination has its mean value of about 16°, positive when the declination is less, and negative when it is greater than this. The correction for a given declination, as shown in the vertical columns, is not constant, but it appears difficult to determine whether the variations are accidental or are the consequences of the form of the correction. Till we have better data, I will neglect these variations.

Taking, then, the sums of the vertical columns in Table XIX., we find as follows (the sums being expressed in minutes):

Table showing the Difference in the Interval between the Time of the Moon's Transit and the Time of High Water, and the Mean Interval (Column A. Table III.) for every Three Degrees of the Moon's Declination.

Moon's Transit.	0	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.
h m	m	m	m	m	m	m	m	m	m	m
0 0	+ 8	+ 5	+ 7	+ 5	+ 2	+ 2	— 3	— 6	—11	—10
0 30	+ 9	+ 6	+ 9	+ 7	— 2	....	— 2	— 4	— 7	—11
1 0	+ 8	+ 8	+10	+ 5	+ 2	+ 2	— 3	— 5	— 6	—11
1 30	+ 5	+ 8	+ 7	+ 2	+ 4	+ 3	— 4	— 7	— 6	—12
2 0	+ 6	+ 8	+ 6	+ 4	+ 3	+ 3	— 2	— 5	— 5	—10
2 30	+ 6	+ 8	+ 5	+ 7	+ 2	+ 2	— 1	— 4	— 5	— 9
3 0	+ 9	+ 9	+ 6	+ 7	+ 4	+ 2	— 2	— 7	— 7	—11
3 30	+11	+11	+ 9	+ 7	+ 5	+ 2	— 2	— 9	—10	—13
4 0	+ 9	+10	+10	+ 7	+ 7	+ 1	0	— 7	—10	—16
4 30	+ 8	+ 8	+ 8	+ 8	+ 8	— 1	0	— 6	—11	—18
5 0	+13	+12	+11	+12	+ 9	0	0	— 4	— 9	—16
5 30	+17	+14	+12	+13	+ 6	0	— 3	— 5	—10	—14
6 0	+20	+16	+13	+13	+ 6	+ 1	— 5	— 7	—13	—17
6 30	+21	+19	+12	+13	+ 8	....	— 7	— 9	—18	—21
7 0	+21	+19	+12	+17	+10	+ 4	— 6	— 8	—19	—27
7 30	+16	+16	+14	+18	+10	+ 5	— 8	— 6	—20	—34
8 0	+14	+18	+16	+16	+ 8	+ 4	— 6	— 6	—23	—30
8 30	+16	+15	+15	+12	+ 8	+ 2	— 1	— 5	—24	—22
9 0	+13	+ 9	+12	+ 7	+ 7	— 2	— 5	— 8	—18	—17
9 30	+13	+ 7	+11	+ 6	+ 7	— 2	— 6	— 7	— 7	—12
10 0	+11	+ 6	+ 6	+ 6	+ 4	— 2	— 8	— 7	— 5	—13
10 30	+11	+ 8	+ 3	+ 6	+ 3	0	— 7	— 5	— 5	—13
11 0	+ 9	+ 7	+ 3	+ 4	+ 3	+ 1	— 6	— 6	— 9	—13
11 30	+ 8	+ 3	+ 4	+ 3	+ 4	+ 3	— 3	— 8	—14	—12
Sums	+282	+250	+221	+205	+128	+30	—90	—151	—272	—382

It is tolerably manifest that these sums decrease faster for the large declinations than for the small ones; and we shall probably see the law more clearly by referring the correction to declination 0° than to the mean declination. For this purpose subtract from each sum the correction for declination 0°, and we have

Declination..	0	3°	6°	9°	12°	15°	18°	21°	24°	27°
Corrected sum	0	−32	−61	−77	−154	−252	−372	−433	−554	−664

It appears that the numbers here are not very remote from the ratio of the squares of the sines of the declinations. In fact, if we take the formula $-3168 \sin^2 \delta$, we have

For declination	0	3°	6°	9°	12°	15°	18°	21°	24°	27°
Formula	0	−9	−34	−77	−138	−229	−303	−408	−525	−654

which may pass for a first approximation to the observed result, when it is considered how much the errors are increased by addition. Dividing by 24, this gives $-132^m \sin^2 \delta$ for the correction to be applied to each result calculated for declination 0. When δ is about 16°, the mean value, this correction is 11^m. Hence $11^m - 132^m \sin^2 \delta$ is the correction to be applied to the mean value. This gives us the following Table, which is a first approximation to Table XIX. of Mr. Lubbock.

For declination ..	0°	3°	6°	9°	12°	15°	18°	21°	24°	27°
Correction	+ 11	+ 10	+ 9	+ 8	+ 5	+ 2	− 2	− 6	− 11	− 16
The sums........	+264	+240	+216	+192	+120	+48	−48	−144	−264	−384

Which agree nearly with

+282	+250	+221	+205	+128	+30	−90	−151	−272	−382

the observed results, with sufficient accuracy.

We may observe that the expression $11 - 132 \sin^2 \delta$ is 0 when $\sin^2 \delta = \frac{1}{12}$ or $\delta =$ 16° 45′. This is the mean value of Δ, because the correction is applied to the mean. Therefore the expression is $132 (\sin^2 \Delta - \sin^2 \delta)$.

In each vertical column of Table XIX., the value appears to be greatest and least when the time of the moon's transit is about 7^h and 1^h. Hence we shall take the correction given by the above formula, and try whether the *residual phenomenon*, after this correction has been applied, is governed by any fixed rule.

For this purpose apply the above correction with an opposite sign to Mr. Lubbock's Table XIX. The numbers in the columns are minutes.

TABLE XIX. freed from the Term $11 - 132 \sin^2 \delta = 132 (\sin^2 \Delta - \sin^2 \delta)$.

Decl. ….	0°	3°	6°	9°	12°	15°	18°	21°	24°	27°	Sums.	
Corr. ….	−11	−10	− 9	− 8	− 5	− 2	+ 2	+ 6	+11	+16		
☽'s Transit.											0° to 15°.	18° to 27°.
h m												
0 0	− 3	− 5	− 2	− 3	− 3	0	− 1	0	0	+ 6	−16	+ 5
0 30	− 2	− 4	0	− 1	− 7	….	0	+ 2	+ 4	+ 5	−14	+11
1 0	− 3	− 2	+ 1	− 3	− 3	0	− 1	+ 1	+ 5	+ 5	−10	+10
1 30	− 6	− 2	− 2	− 6	− 1	+ 1	− 2	− 1	+ 5	+ 4	−16	+ 6
2 0	− 5	− 2	− 3	− 4	− 2	+ 1	0	+ 1	+ 6	+ 6	−12	+13
2 30	− 5	− 2	− 4	− 1	− 3	0	+ 1	+ 2	+ 6	+ 7	−15	+16
3 0	− 2	− 1	− 3	− 1	− 1	0	0	− 1	+ 4	+ 5	− 8	+ 8
3 30	0	+ 1	0	− 1	0	0	0	− 3	+ 1	+ 3	+ 0	+ 1
4 0	− 2	0	+ 1	− 1	+ 2	− 1	+ 2	− 1	+ 1	0	− 1	+ 2
4 30	− 3	− 2	− 1	0	+ 3	− 3	+ 2	0	0	− 2	− 6	0
5 0	+ 2	+ 2	+ 2	+ 4	+ 4	− 2	+ 2	+ 2	+ 2	0	+12	+ 6
5 30	+ 6	+ 4	+ 3	+ 5	+ 1	− 2	− 1	+ 1	+ 1	+ 2	+17	+ 3
6 0	+ 9	+ 6	+ 4	+ 4	+ 1	− 1	− 3	− 1	− 2	− 1	+23	− 7
6 30	+10	+ 9	+ 3	+ 4	+ 2	….	− 5	− 3	− 7	− 5	+29	−20
7 0	+10	+ 9	+ 3	+ 6	+ 5	+ 2	− 4	− 2	− 8	−11	+35	−25
7 30	+ 5	+ 6	+ 5	+ 7	+ 5	+ 3	− 6	− 0	− 9	−18	+31	−33
8 0	+ 3	+ 8	+ 7	+ 5	+ 3	+ 2	− 4	− 0	−12	−14	+26	−30
8 30	+ 5	+ 5	+ 6	+ 1	+ 3	0	+ 1	+ 1	−13	− 6	+20	−17
9 0	+ 2	− 1	+ 3	− 1	+ 2	− 4	− 3	− 2	− 7	− 1	+ 1	−13
9 30	+ 2	− 3	+ 2	− 2	+ 2	− 4	− 4	− 1	+ 4	+ 4	− 3	− 5
10 0	0	− 4	− 3	− 2	− 1	− 4	− 6	− 1	+ 6	+ 3	−14	+ 2
10 30	0	− 2	− 6	− 2	− 2	− 2	− 5	+ 1	+ 6	+ 3	−14	+ 5
11 0	− 2	− 3	− 6	− 4	− 2	− 1	− 4	0	+ 2	+ 3	−20	− 1
11 30	− 3	− 7	− 5	− 5	− 1	+ 1	− 1	− 2	− 3	+ 4	−20	− 2

From the changes of magnitude and sign, it appears that each vertical column of differences may be represented nearly by a term $A \sin 2 (\varphi - \gamma)$, φ being the hour-angle of the first column, and γ a certain other angle. Also it appears that in each horizontal line, A passes from positive to negative, and *vice versâ*, when the declination passes through its mean value. Hence there is a factor $\delta - \Delta$, Δ being the mean value of the declination.

For declinations less than the mean, the maximum values of the correction are about the hours of transit $0^h\, 0^m$ and $7^h\, 0^m$. This would give for γ the value $3^h\, 30^m$.

For declinations greater than the mean, the maximum values of the correction would occur nearly when the hour of transit is $1^h\, 30^m$ or $7^h\, 30^m$. This would give for γ, $4^h\, 30^m$; the mean of this and the other value is 4^h.

Hence the formula for the above residual quantities will be

$$D (\Delta - \delta) \sin 2 (\varphi - \gamma);$$

where, however, instead of $\Delta - \delta$, we may have other functions, as $\sin \Delta - \sin \delta$, $\sin^2 \Delta - \sin^2 \delta$. Hence the whole correction for lunar declination appears to be

$$132 (\sin^2 \delta - \sin^2 \Delta) + D (\delta - \Delta) \sin 2 (\varphi - \gamma),$$

which will be simplified if we put $\sin^2 \delta - \sin^2 \Delta$ for $\delta - \Delta$; the expression then becomes,

$$(\sin^2 \delta - \sin^2 \Delta)\left(132 + D \sin 2 (\phi - \gamma)\right).$$

We have to find D. For that purpose take the formula

$$(\sin^2 \delta - \sin^2 \Delta) D \sin 2 (\phi - \gamma),$$

which expresses the residual phenomenon just given from Table XIX. Take the case where $\delta = 0$, and we have for the first vertical column, the expression

$$- D \sin^2 \Delta \sin 2 (\phi - \gamma).$$

The vertical column contains all the values of this for every half-hour of the value of $\phi - \gamma$, that is, for values of $\sin 2 (\phi - \gamma)$ taken at intervals of 15° round the circumference. Taking the sum of these values for one semicircle, it is, by known formulæ,

$$\frac{\sin 90^\circ \times \sin 82\tfrac{1}{2}^\circ}{\sin 7\tfrac{1}{2}^\circ} = \tan 82\tfrac{1}{2}^\circ = 7{\cdot}5957.$$

Now this sum in the Table is 45 if we take the mean of the positive and negative values; observing, however, that this value compared with the succeeding columns appears to be smaller than the general course of the numbers would give it. Hence,

$$D \sin^2 \Delta \times 7{\cdot}5957 = 45\,;\quad D = 72 \text{ nearly}.$$

Hence $D \sin^2 \Delta = 6$. But it will agree better with the general numbers to make $D = 7$, and the expression for the residual phenomenon is

$$84 (\sin^2 \delta - \sin^2 \Delta) \sin 2 (\phi - \gamma).$$

The values of $84 (\sin^2 \Delta - \sin^2 \delta)$ for the successive values of δ are hence found; and hence the corrections.

Assuming $\gamma = 4^h$, the following Table represents the table of the residual phenomenon.

Table of the Expression $84 (\sin^2 \Delta - \sin^2 \delta) \sin 2 (\phi - \gamma)$.

	$\delta =$	0°	3°	6°	9°	12°	15°	18°	21°	23°	27°
	$84 (\sin^2 \Delta - \sin^2 \delta) =$	7	7	6	5	3	1	0	− 4	− 7	−10
ϕ.	$\sin 2 (\phi - \gamma)$.										
1^h	−1·000	− 7	− 7	− 6	− 5	− 3	− 1	0	+ 4	+ 7	+10
2	−0·866	− 6	− 6	− 5	− 4	− 3	− 1	0	+ 3	+ 6	+ 9
3	−0·100	− 4	− 4	− 4	− 3	− 2	− 1	0	+ 2	+ 4	+ 5
4	0·000	0	0	0	0	0	0	0	0	0	0
5	0·500	+ 4	+ 4	+ 4	+ 3	+ 2	+ 1	0	− 2	− 4	− 5
6	0·866	+ 6	+ 6	+ 5	+ 4	+ 3	+ 1	0	− 3	− 6	− 9
7	1·000	+ 7	+ 7	+ 6	+ 5	+ 3	+ 1	0	− 4	− 7	−10
8	0·866	+ 6	+ 6	+ 5	+ 4	+ 3	+ 1	0	− 3	− 6	− 9
9	0·500	+ 4	+ 4	+ 4	+ 3	+ 2	+ 1	0	− 2	− 4	− 5
10	·000	0	0	0	0	0	0	0	0	0	0
11	−0·500	− 4	− 4	− 4	− 3	− 2	− 1	0	+ 2	+ 4	+ 5
12	−0·866	− 6	− 6	− 5	− 4	− 3	− 1	0	+ 3	+ 6	+ 9

This agrees as to its changes of magnitude and sign, and as to the mean of the numbers, with the table of the residual quantities, p. 25. The formula,

$$(\sin^2 \delta - \sin^2 \Delta) \{132 + 84 \sin 2 (\phi - 4^h)\}$$

will therefore express, with considerable accuracy, the general course and average values of the numbers in Mr. Lubbock's Table XIX.

But if the correction, instead of being applied to the *mean* value of the interval (of tide and transit), had been applied to the interval calculated for declination 0, it is clear the correction would have been

$$\{132 + 84 \sin 2 (\varphi - 4^h)\} \sin^2 \delta.$$

Mr. Lubbock has given other tables also, from which the mean correction for lunar declination may be collected. His Table XV., which contains the differences of the intervals of the time of moon's transit and high water from the mean interval, arranged according to the calendar months and to times of the moon's transit, is in fact principally a table of the correction for lunar declination. For by examination of that table, it will be seen that the correction in each month goes through its cycle of 0, +, 0, —, in one semirevolution of the moon; that is, while the declination passes from its maximum north, to its maximum south, value: and since these results are the mean of nineteen years, the moon will have been nearly as much on the north as on the south of the ecliptic, and the result will be nearly the same as if she had moved in the ecliptic. It may be observed, however, that it appears by what has been shown above, that the corrections increase faster than the declinations; and therefore the corrections due to the high declinations will not be quite balanced by those due to the declinations which correspond to an equal opposite celestial latitude.

It is to be noticed, also, that this Table XV., being arranged for calendar months, contains the effect of solar declination and parallax as well as of lunar declination. It also contains the effect of the equation of time; the times of the moon's transit being given in *mean* solar time, whereas we suppose the tide to depend on the hour-angle of the moon from the sun, that is, on the transit in true solar time. These effects may be eliminated, and the effect of the changes of lunar declination upon the tide-hour may be determined from this table in an approximate manner; but the accuracy of such a determination is necessarily less than that of the one already obtained, and I shall therefore not insert it here.

5. *The Solar Correction.*—The sums of the positive and of the negative numbers in each vertical column of Mr. Lubbock's Table XV. would be equal, if the inequality depended on the moon alone, since each column contains the corrections which occur in a half-revolution of the moon. Therefore the difference of these sums is due to a solar inequality, and the mean excess or defect must be subtracted or added in order to obtain the corrections due to the moon. These means are as follow:

	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	
	+105 — 61	+ 48 —147	+ 63 —144	+64 —96	+90 —79	+120 — 71	+128 — 62	+110 — 68	+90 —90	+ 99 —111	+ 98 —120	+103 — 74	
	+ 44	— 99	— 81	—32	+11	+ 49	+ 66	+ 42	0	— 12	— 22	+ 29	
Means	+ 2	— 4	— 3	— 1	0	+ 2	+ 3	+ 2	0	— 1	— 1	+ 1	Solar inequality.

It appears that this correction changes from positive to negative four times in the course of the year, and hence may be approximately represented by $m \sin 2 (\theta - \mu)$ when θ is the sun's right ascension. But the maximum and minimum values in different parts of the year are of unequal magnitude and at unequal intervals. This may be reconciled with an expression of the form $m \sin 2 (\theta - \mu) + n \sin (\theta - \nu)$; and we might determine m, n, μ, ν, so as to make the expression agree nearly with the result of observation. In fact, however, this would not be worth while, except we had the empirical law confirmed by the results of observations at other places; for the greatest values of this correction are $- 4^m$ and $+ 3^m$. We here exclude the effects of the equation of time.

It is not difficult to see why the solar correction assumes such a form as this. It includes the corrections due both to the sun's declination and his parallax. The former effect is twice a minimum and twice a maximum in the course of a year; the latter once only. The two effects are not immediately separated in the tables, because the sun's perigee being nearly stationary, the cycle of changes due to solar parallax and the double cycle of changes due to solar declination coincide.

When the form and amount of the solar correction are more exactly determined, it may be more exactly compared with the theory.

Chap. II. *On the Empirical Laws of the Height of High Water.*

The same kind of discussion of the observations which has enabled us to obtain approximately the laws of the *times* of high water, will also give similar information with respect to the *heights,* since these have been observed at the docks, and the results tabulated by Mr. Lubbock, in the same way as the others. The heights will be affected in the same way as the tides, by a *semimenstrual inequality,* by corrections for *lunar parallax and declination,* and by a *solar correction.*

1. *Of the Mean Level of the Water.*—The quantities which are wanted for the comparison of observed heights with the theory, are the total height of the tide, that is, the difference of high and low water. The heights of low water are not given in the London observations, and we have, therefore, only the differences of the high waters to reduce to their laws.

A comparison of these with the theory, supposes the mean level of the water to be constant, that is, the mean of the heights of high and low water to be the same, whatever be the height of the tide. I do not know whether this permanency of the mean level has been verified at the London Docks. It has been ascertained to be true in several other cases, and is probably universal, or at least liable to few and peculiar exceptions.

This mean level may be determined by the mean of many observations, and is a more fixed and distinct level than any level depending on a smaller number of observations. It is, moreover, free from the irregularities to which levels selected in any other way are exposed. Thus the level of high water, or of low water, at spring tides, or at neap tides, is different according to the different effects of lunar and solar parallax and declination.

I proceed to discuss the variations of the heights of the London tides.

2. *The Semimenstrual Inequality.*—The law of the heights is contained in the last column of Mr. LUBBOCK's Table V., which gives the heights for every half-hour, mean time, of the moon's transit, taking a mean of the months of the year.

In order to obtain a formula expressing this series of quantities, we observe that the maximum and minimum are nearly when the times of transit are 2^h and 8^h respectively; that the mean of the extreme heights is 21·1 feet, and the difference of the extremes 3·4 feet. Hence the height may be expressed approximately by the formula $21·1 + 1·7 \cos 2 (\phi - 30°)$. It appears, however, that the maximum and minimum occur a little earlier than 2^h and 8^h. I shall therefore assume for the height the formula $21·1 + 1·7 \cos (2\phi - 51°)$; we shall then have the following comparison with observations.

ϕ	$\cos (2\phi - 51)$	Height observed.	Height calculated.	Diff.	Diff. — ·23
h m		Feet.	Feet.		
2 0	·9877	22·78	22·80	·02	— ·21
3 0	·7771	22·42	22·59	·17	— ·06
4 0	·3584	21·71	22·10	·39	+ ·16
5 0	— ·1564	20·83	21·28	·45	+ ·22
6 0	— ·6293	20·03	20·37	·34	+ ·21
7 0	— ·9335	19·51	19·56	·05	— ·18
8 0	— ·9877	19·42	19·43	·01	— ·22
9 0	— ·7771	19·78	20·10	·32	+ ·09
10 0	— ·3584	20·49	20·92	·43	+ ·20
11 0	·1564	21·37	21·85	·48	+ ·25
0 0	·6293	22·17	22·46	·29	+ ·06
1 0	·9335	22·69	22·72	·03	— ·20

It is evident that this is a first approximation. Also the difference, which is always positive, follows the law of a sine. To show this, subtract from this difference ·23, as is done in the last column.

The difference in the last column will be 0 when ϕ is $0^h\ 30^m$ and $6^h\ 30^m$ nearly, and will, in the course of 6 hours of ϕ, go through all the values of $\sin \phi$. Hence it may be represented by $- c \sin (4\phi - 30°)$, and it is evident that c is nearly ·23.

Comparison of the residual Phenomenon of the Semimenstrual Series of Heights, freed of the Terms $21·1 + 1·7 \cos (2\phi - 51)$, with the Formula $·23 - ·23 \sin (4\phi - 30)$.

ϕ	$\sin (4\phi - 30)$	Form.	Obs.	Excess of Obs.
h	Feet.	Feet.	Feet.	Feet.
0	— ·5	·35	·29	—·06
1	+ ·5	·11	·03	—·08
2	+ 1·0	0	·02	+·02
3	+ ·5	·11	·17	+·06
4	— ·5	·35	·39	+·04
5	— 1·0	·46	·45	—·01
6	— ·5	·35	·34	—·01
7	+ ·5	·11	·05	—·06
8	+ 1·0	0	·01	+·01
9	+ ·5	·11	·32	+·21
10	— ·5	·35	·43	+·08
11	— 1·0	·46	·48	+·02

It appears from the nature of the still residual differences, that we might bring our formula still nearer to observation; but as the differences do not exceed $\frac{1}{10}$th of a foot, except in one instance, this exactness would be, in the present state of our knowledge, superfluous.

Hence the mean height in feet of the tide at London Dock is represented by

$$21{\cdot}1 + 1{\cdot}7 \cos (2\varphi - 51^\circ) + {\cdot}23 - {\cdot}23 \sin (4\varphi - 30^\circ),$$
$$\text{or } 21{\cdot}33 + 1{\cdot}7 \cos (2\varphi - 51^\circ) - {\cdot}23 \sin (4\varphi - 30^\circ)$$

where φ is the hour-angle of the moon's transit, mean time.

3. *Correction of the Heights for Lunar Parallax.*—Table XVIII. of Mr. Lubbock contains the effect of variations of the moon's distance.

Table showing the Difference in the Height of High Water, and the Mean Height for every Minute of the Moon's Horizontal Parallax.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 0	−0·52	−·33	−·30	+·10	+·06	+·23	+ ·33	+·53
0 30	− ·50	−·28	−·20	+·08	+·17	+·41	+ ·36	+·53
1 0	− ·58	−·44	−·09	−·07	+·26	+·24	+ ·31	+·61
1 30	− ·55	−·46	+·03	−·07	+·23	+·23	+ ·41	+·77
2 0	− ·57	−·52	−·15	−·05	+·12	+·24	+ ·50	+·75
2 30	− ·54	−·45	−·28	+·10	+·08	+·34	+ ·69	+·79
3 0	− ·68	−·37	−·25	+·06	+·05	+·42	+ ·83	
3 30	− ·63	−·23	−·13	+·08	+·13	+·56	+1·00	
4 0	− ·57	−·28	−·12	+·14	+·20	+·66	+ ·98	
4 30	− ·49	−·29	−·13	+·23	+·67	+·76	+ ·83	
5 0	− ·50	−·40	−·30	+·25	+·53	+·80		
5 30	− ·47	−·40	−·37	+·37	+·50	+·86		
6 0	− ·45	−·28	−·19	+·27	+·50	+·77		
6 30	− ·54	−·21	−·09	+·05	+·40	+·62	+ ·43	
7 0	− ·64	−·29	−·11	+·05	+·31	+·54	+ ·53	
7 30	− ·75	−·48	−·28	+·01	+·15	+·33	+ ·58	
8 0	− ·68	−·30	−·25	+·05	+·02	−·08	+ ·54	
8 30	− ·54	−·27	−·26	+·01	−·20	+·42	+ ·44	
9 0	− ·26	−·23	−·21	−·11	−·23	−·76	+ ·39	
9 30	+ ·03	−·24	−·15	−·26	−·09	+·30	+ ·45	+·87
10 0	− ·09	−·30	−·11	−·14	+·14	+·36	+ ·49	+·69
10 30	− ·20	−·37	−·07	+·03	+·32	+·36	+ ·48	+·55
11 0	− ·31	−·31	−·14	+·21	+·26	+·26	+ ·51	+·61
11 30	− ·43	−·22	−·25	+·27	+·13	+·14	+ ·46	+·68

If we take the means of the vertical columns, they are, in hundredths of feet,

Horizontal parallax	54′	55′	56′	57′	58′	59′	60′	61′
Means	−47	−33	−18	+7	+20	+37	+47	+67

These are very nearly as the differences of the parallax. We shall find that the formula $1{\cdot}7\ (p - P)$ when p is the parallax, and P is 57′, will very nearly give this result. It gives, in fact,

−51 −34 −17 0 +17 +34 +51 +68

Hence, applying these corrections with opposite signs to Table XVIII., we obtain the residual phenomenon as follows, in hundredths of feet.

Table XVIII. freed from the Sum 1·7 $(p - P)$.

Hor. Par...	54′	55′	56′	57′	58′	59′	60′	61′
Corr.	+51	+34	+17	0	−17	− 34	−51	−68
h m								
0 0	− 1	+ 1	−13	+10	−11	+ 11	−18	−15
0 30	+ 1	+ 6	− 3	+ 8	0	7	−15	−15
1 0	− 7	−10	+ 8	− 7	+ 9	− 10	−20	− 7
1 30	− 4	−12	+20	− 7	+ 6	− 11	−10	+ 9
2 0	− 6	−18	+ 2	− 5	− 5	− 10	− 1	+ 7
2 30	− 3	−11	−11	+10	− 9	− 11	+18	+11
3 0	−17	− 3	− 8	+ 6	−12	+ 8	+32	
3 30	−12	+11	+ 4	+ 8	− 4	+ 32	+49	
4 0	− 3	+ 6	+ 5	+14	+ 3	+ 32	+47	
4 30	+ 2	+ 5	+ 4	+23	+50	+ 42		
5 0	+ 1	− 6	−13	+25	+36	+ 46		
5 30	+ 4	− 6	−20	+37	+33	+ 52		
6 0	+ 6	+ 6	− 2	+27	+33	+ 43		
6 30	− 3	+13	+ 8	+ 5	+23	+ 28	− 8	
7 0	−13	+ 5	+ 6	+ 5	+14	+ 20	+ 2	
7 30	−24	−14	−11	+ 1	− 2	− 1	+ 7	
8 0	−14	+ 4	− 8	+ 5	−15	− 42	+ 3	
8 30	− 3	+ 7	− 9	+ 1	−37	+ 8	+ 7	
9 0	+26	+11	− 4	−11	−40	−110	−12	
9 30	+54	+10	+ 2	−26	−26	− 4	− 6	+19
10 0	+42	+ 4	+ 6	−14	− 3	+ 2	− 2	+ 1
10 30	+31	− 3	+10	+ 3	+15	+ 2	− 3	− 6
11 0	+20	+ 3	+ 3	+21	+ 9	− 6	0	− 7
11 30	+ 8	+12	− 8	+27	− 4	− 20	− 5	0

There is no very manifest rule in this Table. There appear to be many large positive terms between the hours $2^h 30^m$ and 7^h, and for parallaxes greater than 57′, while the terms for other hours are more generally negative.

But it appears to be better to wait for the examination of other observations than to attempt to found a formula on these circumstances.

4. *Correction of the Heights for Lunar Declination.*—Table XX. of Mr. Lubbock will supply the means of determining the law of the effect of the lunar declination upon the height, being a Table of the heights arranged according to intervals of 3° of declination, and according to the time of the moon's transit. We shall place here the Table, expressed in hundredths of feet.

TABLE showing the Difference in the Height of High Water, and the Mean Height for every Three Degrees of the Moon's Declination.

Moon's Transit.	0	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 0	− 10	+ 07	+ 06	+ 02	+ 04	− 06	− 10	− 08	− 34	− 42
0 30	+ 09	− 01	+ 06	− 05	+ 14	+ 10	− 03	− 15	− 10	− 20
1 0	+ 23	− 10	+ 11	− 01	− 02	+ 07	+ 02	+ 03	− 07	− 35
1 30	+ 34	− 13	+ 16	+ 08	− 04	+ 07	+ 12	+ 13	− 06	− 45
2 0	+ 40	− 25	+ 10	+ 15	+ 03	− 01	+ 15	− 03	− 15	− 25
2 30	+ 39	− 25	+ 08	+ 25	+ 22	+ 01	+ 24	− 18	− 18	+ 01
3 0	+ 27	− 10	+ 03	+ 27	+ 21	+ 04	+ 21	− 09	− 28	− 09
3 30	+ 22	+ 13	....	+ 31	+ 16	+ 10	+ 17	+ 01	− 34	− 20
4 0	+ 14	− 01	+ 54	+ 20	....	+ 08	+ 12	+ 03	− 32	− 18
4 30	+ 17	− 05	+ 16	+ 10	− 10	+ 06	+ 14	− 08	− 13	− 08
5 0	+ 21	− 08	+ 37	+ 07	− 01	+ 09	+ 12	− 28	− 12	− 08
5 30	+ 22	− 09	+ 53	+ 04	+ 04	+ 08	+ 04	− 05	− 08	− 21
6 0	+ 13	+ 15	+ 51	+ 18	+ 16	+ 13	+ 03	+ 11	− 19	− 37
6 30	− 09	+ 37	+ 19	+ 27	+ 22	+ 11	− 03	− 06	− 45	− 49
7 0	+ 19	− 02	+ 31	+ 56	+ 26	+ 04	0	− 46	− 24	− 53
7 30	+ 52	+ 38	+ 32	+ 64	+ 15	− 13	− 08	− 35	− 19	− 70
8 0	+ 48	+ 60	+ 44	+ 57	+ 47	+ 17	− 07	− 28	− 20	− 61
8 30	+ 22	+ 71	+ 43	+ 25	+ 74	+ 36	− 10	− 28	− 37	− 64
9 0	+ 43	+ 65	+ 32	+ 15	+ 58	+ 25	− 10	− 28	− 50	− 41
9 30	+ 66	+ 54	+ 17	+ 28	+ 37	+ 06	− 12	− 31	− 56	− 43
10 0	+ 58	+ 58	+ 37	+ 44	+ 36	+ 06	− 06	− 26	− 37	− 44
10 30	+ 32	+ 54	+ 44	+ 42	+ 21	+ 02	− 15	− 29	− 19	− 64
11 0	+ 15	+ 41	+ 31	+ 31	+ 13	+ 05	− 15	− 14	− 30	− 61
11 30	+ 12	+ 34	+ 26	+ 28	+ 12	− 08	+ 02	+ 18	− 38	− 44
Sums	+648 − 19	+547 −109	+627 0	+584 − 6	+461 − 17	+195 − 28	+138 − 99	+ 49 −365	0 −611	+ 1 −882
	+629	+438	+627	+578	+444	+167	+ 39	−316	−611	−811

It is clear that these sums decrease faster for the large declinations than for the small ones, and the series is tolerably regular with the expression of the number corresponding to declination 3°, which appears to be affected by some anomaly. If we reject this term, and subtract 629 from each of the terms, we find for

Declination . . 0°	3°	6°	9°	12°	15°	18°	21°	24°	27°
Correction . . 0	(−191)	−2	−51	−185	−462	−590	−945	−1240	−1440

It will appear that the law may be expressed nearly by $-7300 \sin^2 \delta$, which gives

−0 −20 −79 −178 −315 −490 −697 −937 −1207 −1504.

This agrees pretty well, except for the smaller numbers, which are obviously irregular. Hence, if Δ be the mean declination, we shall have the correction to be applied to the mean sums $= 7300 (\sin^2 \Delta - \sin^2 \delta)$; and the correction to the single terms will be $304 (\sin^2 \Delta - \sin^2 \delta)$.

If we suppose the mean declination to be 16° 45′, as appeared in the correction for the times, $7300 \sin^2 \Delta = 608$, and the corrections are,

Declination ..	0°	3°	6°	9°	12°	15°	18°	21°	24°	27°
Correction ..	608	588	529	430	293	118	− 89	−329	−599	−896

Hence	25	24	22	17	11	4	−4	−14	−25	−38

are the corrections for the single terms of the Table.

We shall now apply this correction with a negative sign, in order to consider the law of the residual phenomenon.

Table XX. freed from the Term 304 ($\sin^2 \Delta - \sin^2 \delta$).

Decl.	0°	3°	6°	9°	12°	15°	18°	21°	24°	27°
Corr.	25	24	22	17	11	4	−4	−14	−25	−38
h m										
0 0	−35	−17	−16	−15	− 7	−10	− 6	+ 6	− 9	− 4
0 0	−15	−25	−16	−22	+ 3	+ 6	+ 1	− 1	+15	+18
1 0	− 2	−35	−11	−18	−13	+ 3	+ 6	+17	+18	+ 3
1 30	+ 9	−37	− 6	− 9	−15	+ 3	+16	+27	+19	− 7
2 0	+15	−49	−12	− 2	− 8	− 5	+19	+11	+10	+13
2 30	+14	−49	−14	+ 8	+11	+ 3	+28	− 4	+ 7	+39
3 0	+ 2	−34	−18	+10	+10	0	+25	+ 5	− 3	+29
3 30	− 3	−11	....	+04	+ 5	+ 6	+21	+15	− 9	+18
4 0	−11	−25	+32	+ 3	....	+ 4	+16	+17	− 8	+20
4 30	− 8	−29	−06	− 7	−21	+ 2	+18	+ 6	+12	+30
5 0	− 4	−32	+15	−10	−12	+ 5	+16	−14	+13	+30
5 30	− 3	−33	+31	−13	− 7	+ 4	+ 8	+ 9	+17	+17
16 0	−12	−19	+29	− 1	+ 5	+ 9	+ 7	+25	+ 6	+ 1
6 30	−34	+13	− 3	+10	+11	+ 7	+ 1	+ 8	−20	−11
7 0	− 5	−26	+ 9	+39	+15	0	+ 4	−32	+ 1	−15
7 30	+27	+14	+10	+47	+ 4	−11	− 4	−21	+ 6	−32
8 0	+23	+36	+22	+40	+36	+13	− 3	−14	+ 5	−23
8 30	− 3	+47	+21	+ 8	+63	+32	− 7	−14	−12	−26
9 0	+18	+41	+10	− 2	+47	+21	− 7	−14	−25	− 3
9 30	+41	+30	− 5	+11	+36	+ 2	− 8	−17	−31	− 5
10 0	+33	+34	+15	+27	+25	+ 2	− 2	−12	−12	− 6
10 30	+ 7	+30	+22	+25	+10	− 2	−11	−15	+ 6	−26
11 0	−10	+17	+ 9	+ 4	+ 2	+ 1	−11	0	− 5	−23
11 30	−13	+10	+ 4	+11	+ 1	−12	+ 6	+32	−13	− 6

Though this Table exhibits great anomalies, it appears clear that all the heavy minus terms, and only small positive terms, are in the upper left-hand and lower right-hand quarter; and that all the heavy positive terms, and only small negative terms, occur in the upper right-hand and lower left-hand quarters. Also the terms are on the whole larger in the outer than in the inner columns. It appears probable, therefore, that the law from which this proceeds involves a term $d\ (\sin^2 \delta - \sin^2 \Delta) \sin 2\varphi$, which would give such a result; but the coefficient of this term cannot be determined satisfactorily; and hence the effect of declination in the moon is probably of the form

$$(\sin^2 \Delta - \sin^2 \delta)\ (7300 - d \sin 2\varphi).$$

Hence the correction to be applied to the height calculated for declination 0, is

$$- \sin^2 \delta\ (7300 - d \sin 2\varphi),$$

or

$$- \sin^2 \delta \left(7300 + d \cos 2\ (\varphi + 45^\circ)\right).$$

We may make a remark with respect to Mr. LUBBOCK's Tables for the heights, similar to one we made with respect to those for the times. Table XVI., which gives the differences of height for each hour of the moon's transit in the different calendar months, is in reality composed mainly of the effects of the moon's declination. In order to obtain these effects from the Table, we should have to eliminate the effects of the sun (including the effects of the equation of time). By this means we should obtain a result agreeing in part with that which we have obtained from Table XX.; but the accuracy of this result would necessarily be less than of that already obtained, and I shall omit it.

5. *The Solar Correction of the Heights.*—If we take the means of each month in Table XVI., we have the sun's effect on the heights in that month. These are as follow:

Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
−7	−23	−8	0	+17	+8	+8	−9	−1	+9	+8	+4

This, like the solar correction for the time, passes from positive to negative, and from negative to positive, four times in the course of the year, but has its maxima and minima of unequal magnitude, and at unequal intervals. Hence we may, as in the case of the times, express it by $m \sin 2(\theta - \mu) + n \sin(\theta - \nu)$, where θ is the sun's longitude; and we may account for this form by considering that the former term is the effect of declination, and the latter term the effect of parallax. To this is to be added the effect of the equation of time, in order to obtain the whole of the solar correction.

Recapitulation.—Hence it appears that the result of the London Dock observations, which we have now examined, may be expressed in the following manner.

If λ' be the corrected establishment, S′ the semimenstrual inequality of the time of high water, P′ the correction for lunar parallax, Q′ the correction for lunar declination, Q the solar correction, and if φ be the mean time of the moon's transit, we have for the time of high water

$$\varphi + \lambda' + S' + P' + Q' + Q.$$

In this expression it has appeared that

$$\tan 2S' = \frac{h \sin 2(\varphi - \alpha)}{h' + h \cos 2(\varphi - \alpha)}; \quad \frac{h'}{h} = 2{\cdot}9887; \quad \alpha = 2 \text{ hours}.$$

$$P' = (P - p)\{B + B \sin 2(\varphi - \beta)\}; \quad B = 3^{m}; \quad \beta = 1 \text{ hour}.$$

$$Q' = (\sin^2 \Delta - \sin^2 \delta)\{C + D \sin 2(\varphi - \gamma)\};$$

$$\Delta = 16° 45', \ C = 132^{m}, \ D = 84^{m}, \ \gamma = 4 \text{ hours}.$$

$$Q = m \sin 2(\varphi - \mu) + n \sin(\varphi - \nu),$$

m, n being small, and their determination here omitted.

In like manner, if l be the height of the mean high water, s' the semimenstrual change, p' the correction due to lunar parallax, q' the correction due to lunar declination, q the solar correction, the height of high water is

$$l + s' + p' + q' + q.$$

In this expression, the numbers being feet, l at the London Docks is 21·33 above the origin of the measures used in the Tables.

$$s' = 1{\cdot}7 \cos (2\,\varphi - 51^\circ) - {\cdot}23 \sin (4\,\varphi - 30^\circ)$$
$$p' = (p - \mathrm{P})\,\{{\cdot}17 + b \cos 2\,\varphi\}$$
$$q' = (\sin^2 \Delta - \sin^2 \delta)\,\{73 - d \cos 2\,(\varphi - 45)\}$$
$$q = m \sin 2\,(\varphi - \mu) + n \sin (\varphi - \nu),$$

b, d being not clearly shown by the London observations to be constant terms; and m, n, as before, being small, and their determination for the present omitted.

Chap. III. *Comparison of the preceding Results with the Theory.*

I shall now compare the preceding results with the theory of Daniel Bernoulli, according to which the waters of the ocean assume nearly the form in which they would be in equilibrium under the action of the sun and moon; and a supposition being made that the pole of the fluid spheroid follows the pole of the spheroid of equilibrium at a certain distance (namely, at an hour-angle λ'), and that the equilibrium corresponds to the configuration of the sun and moon, not at the moment of the tide, but at a previous moment, at which the right ascension of the moon was less by a quantity α.

I take this theory rather than that of Laplace, not only because of the difficulty and labour of the comparison in the latter case, but also because the hypothesis on which Laplace's solution proceeds appears to me likely to affect the results, so as to make them differ altogether from those of the real case; and because the assumption, by means of which his solution is obtained, appears to me to be very insecure.

According to the theory of Bernoulli, we have

$$\tan 2\,(\theta - \lambda') = \frac{h \sin 2\,(\phi - \alpha)}{h' + h \cos 2\,(\varphi - \alpha)}; \quad \ldots\ldots\ldots\ldots \quad (1.)$$

where θ' is the hour-angle corresponding to the place of high water measured from the moon, φ the hour-angle of the moon from the sun, h, h' the heights of the solar and lunar tides, λ' the hour-angle by which the tide follows the pole of equilibrium, α the *retardation*, or difference of right ascension of the moon due to the age of the tide.

Neglecting the effects of parallax and declination, this expression gives the law of the semimenstrual inequality; and this, as we have already said, agrees very clearly with observation, assuming proper values of $\frac{h}{h'}$, and of α.

But we find here some circumstances in which the theory and observation are difficult to reconcile. The value of $\frac{h'}{h}$, the ratio of the lunar and solar tide, ought to be the same at all places. We find, however, that the Brest observations give 2·6167, while the London observations require it to be 2·9887; and other places give values still more different.

Also the differences of the value of α for different places might be supposed to depend necessarily upon the time of the transmission of the tide from one place to another, and therefore to increase as we follow the tide. But it appears that though at Portsmouth this retardation is intermediate between that at Brest and at London, as it might be expected to be from the course of the tide-hours, yet that at Plymouth, where the tide is five or six hours earlier than it is at Portsmouth, the retardation implies a tide as late as London.

Leaving, however, these anomalies to be removed or confirmed by the accumulation and discussion of observations, I proceed to the effects of parallax and declination.

1. If from any cause h' receive a small increment $\delta h'$, we can easily find the corresponding change, $\delta \tan 2(\theta' - \lambda')$, in the first side of equation (1.). We have

$$\delta \tan 2(\theta' - \lambda') = \frac{h \sin 2(\varphi - \alpha) \cdot \delta h'}{\left(h' + h \cos 2(\varphi - \alpha)\right)^2}.$$

Let h' represent the mean value, and $\delta h'$ any deviation from the mean; and let S′ represent the semimenstrual inequality, that is, the value of $\theta' - \lambda'$ freed from effects of declination and parallax. Then

$$\tan 2 S' = \frac{h \sin 2(\varphi - \alpha)}{h' + h \cos 2(\varphi - \alpha)}; \text{ whence}$$

$$\delta \tan 2(\theta' - \lambda') = -\frac{\tan^2 2 S'}{\sin 2(\varphi - \alpha)}, \frac{\delta h'}{h} \quad \ldots\ldots\ldots\ldots \quad (2.)$$

Now, *cæteris paribus*, h' is as the cube of the parallax. If, therefore, P be the mean parallax, and p any other,

$$h' + \delta h' = h' \frac{p^3}{P^3} = h' \left(1 + \frac{p - P}{P}\right)^3 = h' + 3 h' \frac{p - P}{P},$$

omitting other terms, because $p - P$ is small.

Hence $\delta h' = 3 h' \frac{(p - P)}{P}$; and when we make this substitution, equation (2.) gives the change in the first side due to the effect of lunar parallax.

Since the arc $\theta' - \lambda'$ is small, we may put it for its tangent; hence, making the above substitution and calling the effect of lunar parallax P′,

$$2 P' = -\frac{h'}{h} \frac{\tan^2 2 S'}{\sin 2(\varphi - \alpha)} \cdot 3 \frac{(p - P)}{P}.$$

As a first approximation to the general form of the result, we may put $\frac{h}{h'} \sin 2(\varphi - \gamma)$ for tan 2 S′, since $\frac{h}{h'}$ is a fraction (about one third), and since the general course of the two functions, $\sin 2(\varphi - \alpha)$ and tan 2 S′, agrees.

Hence we should have

$$P' = -\frac{3h}{h'} \sin 2(\varphi - \alpha) \cdot \frac{p - P}{P}$$

$$P' = (P - p) \cdot B \sin 2(\varphi - \alpha);$$

B being a constant quantity.

The expression which we obtained from observation was

$$P' = (P - p)\big(B + B \sin 2 (\varphi - \beta)\big),$$

of which the second term agrees in form with the one given by the theory, except that the angle β is different from α; but the first term should not occur according to the theory as hitherto stated.

2. Again, for the effects of lunar declination on the time of high water.

If k' be the value of h' when the place of observation and the pole of the lunar tide spheroid are both in the equator, and if ε be the difference of declination of the place and the pole in any other situation, we shall have $h' = k' \cos^2 \varepsilon$ nearly.

In the course of a tide-day there are two tides, corresponding to two positions of the tidal spheroid; and if l be the latitude of the place, δ the declination of the moon, the two corresponding differences of declination will be $l - \delta$ and $l + \delta$, the pole of the spheroid being supposed to have the same declination as the moon has at the moment of the origin of the tide (that is, when the moon's right ascension was less by α than it is at the moment of the tide).

Then, in the first case,

$$\begin{aligned} h' = k' \cos^2 \varepsilon = k' \cos^2 (l - \delta) &= k' (\cos l \cos \delta - \sin l \sin \delta)^2 \\ &= k' \{\cos^2 l - 2 \sin l \cos l \sin \delta \cos \delta - (\cos^2 l - \sin^2 l) \sin^2 \delta\} \\ &= k' \cos^2 l - \tfrac{1}{2} k' \sin 2 l \sin 2 \delta - k' \cos 2 l \sin^2 \delta. \end{aligned}$$

In the second case, similarly,

$$h' = k' \cos^2 l + \tfrac{1}{2} k' \sin 2 l \sin 2 \delta - k' \cos 2 l \sin^2 \delta.$$

In order to find the effect of the declination upon each tide, we should put for $\delta h'$ the quantities $-\frac{1}{2} k' \sin 2 l \sin 2 \delta - k' \cos 2 l \sin^2 \delta$, and $+\frac{1}{2} k' \sin 2 l \sin 2 \delta - k' \cos 2 l \sin^2 \delta$ respectively.

Thus, according to the theory, the effect of declination on the two tides of the same day should be different. This difference is very much modified by the circumstances in which the actual state of the ocean differs from the theoretical state: the difference of the diurnal tides may, however, be detected in the observations at most places of the earth's surface, perhaps at almost all. But there are peculiar circumstances in the port of London which affect this difference, and obliterate it: the tide at London is composed of two tides, which differ by half a day from each other, and hence the difference of the two semidiurnal tides disappears altogether. Therefore, instead of the effects of declination on the two semidiurnal tides, we must take the mean of these effects, which is $- k' \cos 2 l \sin^2 \delta$.

Hence if Q' represent the effect of lunar declination on the time of high water, we have by equation (2.) (substituting $- k' \cos 2 \theta \sin^2 \delta$ for δh, and putting the arc for $\tan 2 (\theta' - \lambda')$),

$$2\,Q' = \frac{\tan^2 2\,S'}{\sin 2 (\varphi - \alpha)} \, \frac{k'}{h} \cos 2 l \sin^2 \delta.$$

In this expression we have $k' \cos^2 l$ for h' in the value of $\tan S'$; but it is clear that in considering the effect of solar declination, we should in like manner have $k \cos^2 l$ for h, whence the value of $\tan 2\,S'$ in equation (1.) would remain unaltered.

Putting, as before, $\frac{h}{p'} \sin 2\,(\varphi - \alpha)$ for $\tan 2\,S'$, the equation becomes

$$2\,Q' = \frac{k'}{h'} \cos l \sin 2\,(\varphi - \alpha) \,.\, \sin^2 \delta\,; \text{ or}$$

$$Q' = D \sin 2\,(\varphi - \alpha) \,.\, \sin^2 \delta,$$

where D is a constant quantity.

In this expression the correction for declination is supposed to be applied to the time of high water, calculated for the moon and the place, both in the equator. But in our tables this correction is applied to the mean place. Let Δ be the value of the declination at this mean place; then the correction for that case is $D \sin 2\,(\varphi - \gamma) \sin^2 \Delta$, and therefore,

$$Q' = (\sin^2 \delta - \sin^2 \Delta)\, D \sin 2\,(\varphi - \alpha)$$

is the correction to be applied to the mean.

The correction according to observation was

$$Q' = (\sin^2 \delta - \sin^2 \Delta) \big(C + D \sin 2\,(\varphi - \gamma)\big).$$

The second term agrees with the theory, except that the arc γ is different from α: the first term, $C\,(\sin^2 \delta - \sin^2 \Delta)$, has nothing corresponding to it in the theory.

3. We now proceed to the theoretical laws which regulate the height of high water.

If θ, θ' be the distance of any place in the equator from the places to which the sun and moon are vertical (these luminaries being supposed to be in the equinoctial), the height of the water at the place will be $\frac{1}{2} \big(h \cos 2\,(\theta - \lambda) + h' \cos 2\,(\theta' - \lambda')\big)$ above the mean level; and if θ' be taken the distance of the highest water from the moon, then

$$h \cos (2\,\theta - \lambda) + h' \cos 2\,(\theta' - \lambda')$$

will be the whole tide, which call y.

Now we have

$$\tan 2\,(\theta' - \lambda') = \frac{h \sin 2\,(\varphi - \alpha)}{h' + h \cos 2\,(\varphi - \alpha)},$$

where $\theta + \theta' = \varphi$.

Hence we find

$$\cos 2\,(\theta' - \lambda') = \frac{h' + h \cos 2\,(\varphi - \alpha)}{\sqrt{\{h'^2 + 2\,h\,h' \cos 2\,(\varphi - \alpha) + h^2\}}}$$

$$\cos 2\,(\theta - \lambda) = \frac{h + h' \cos 2\,(\varphi - \alpha)}{\sqrt{\{h'^2 + 2\,h\,h' \cos 2\,(\varphi - \alpha) + h^2\}}}$$

$$y = \sqrt{\{h'^2 + 2\,h\,h' \cos 2\,(\varphi - \alpha) + h^2\}} \quad \ldots\ldots\ldots \quad (3.)$$

If, as before, δy represent the variation of y in virtue of any variation of h',

$$\delta y = \frac{h' + h \cos 2\,(\varphi - \alpha)}{y} \,.\, \delta h'. \quad \ldots\ldots\ldots\ldots \quad (4.)$$

The semimenstrual inequality of the heights is given by equation (3.).

Expanding, we have

$$y = \sqrt{(h'^2 + h^2)} \left\{ 1 + \frac{h h'}{(h'^2 + h^2)} \cos 2 (\varphi - \alpha) - \frac{h^2 h'^2}{8 (h'^2 + h^2)^2} \cos^2 2 (\varphi - \alpha) +, \&c. \right\}$$

$$= \sqrt{(h'^2 + h^2)} - \frac{h^2 h'^2}{16 (h'^2 + h^2)^{\frac{3}{2}}} + \frac{h h'}{\sqrt{(h'^2 + h^2)}} \cos 2 (\varphi - 2 \alpha) - \frac{h^2 h'^2}{16 (h'^2 + h^2)^{\frac{3}{2}}} \cos (4 \varphi - 4 \alpha);$$

omitting ulterior terms, since the coefficients diminish according to powers of $\frac{\frac{h}{h'}}{1 + \left(\frac{h}{h'}\right)^2}$.

Hence the variable part of this expression is of the form

$$\text{K} \cos (2 \varphi - 2 \alpha) - \text{L} \cos (4 \varphi - 4 \alpha).$$

The expression of the semimenstrual inequality of heights, found from observation, was (in feet)

$$\text{S}' = 1{\cdot}7 \cos (2 \varphi - 51^\circ) - {\cdot}23 \cos (4 \varphi - 30^\circ),$$

which agrees with the theoretical expression, except as to the values of the arcs which take the place of 2α and 4α.

4. To find the effect of lunar parallax on the heights, substitute as before for $\delta h'$, in equation (3.), and let p' be this effect; then

$$p' = \frac{h' + h \cos 2 (\varphi - \alpha)}{y} \cdot 3 h' \frac{p - \text{P}}{\text{P}}.$$

Here y is the mean height.

Therefore p' is of the form $(p - \text{P}) \big(a + b \cos 2 (\varphi - \alpha)\big)$.

The form as given by observation is $(p - \text{P}) (a + b \cos 2 \varphi)$, where, however, the existence and constancy of b are doubtful.

5. To find the effect of lunar declination on the heights, substitute for δh, as before, $- k' \cos 2 l \sin^2 \delta$. We thus find from equation (3.), q' being the effect,

$$q' = - \frac{h' + h \cos 2 (\varphi - \alpha)}{y} k' \cos 2 l \sin^2 \delta;$$

and referring the correction to the mean declination Δ, it becomes of the form

$$q' = (\sin^2 \Delta - \sin^2 \delta) \big(c + d \cos 2 (\varphi - \alpha)\big).$$

The form given by observation was

$$q' = (\sin^2 \Delta - \sin^2 \delta) \big(c + d \cos 2 (\varphi + 45^\circ)\big),$$

where, however, d was not determined as to quantity, the observations being too anomalous.

It appears, therefore, that the results of observation and theory for the variations of height agree as to form, with the exception of the epochs α, β, γ.

Chap. IV. *Reflexions on the Theory.*

It would be unsafe to attempt to deduce any general views concerning the laws of the tides from the preceding investigations. It is very unlikely that the discussion of observations at any one place, and those the very first set which have been systematically discussed, should exhibit clearly the true principles of the theory: and besides this, it so happens, that the phenomena of the tides at London are in some measure masked by a curious combination of circumstances, namely, by the mouth of its river being on the side of an island, turned away from the side on which the tide comes, and so situated that the path of the tide round one end of the island is just twelve hours longer than round the other. It will require the accumulation and discussion of many large masses of observations, at various places, to put us in firm possession of the laws of the phenomena as given by experience; and this road, whether or not it be the *only* practicable way of arriving at the true theory, is at least that to which, founding our expectations on the past history of science, we may look with most hope. When we consider the enormous accumulation of observed phenomena and empirical laws which preceded the discovery of the true principles of the heavenly motions, we may easily suppose that we are only at the outset of what we have to do, in order to obtain the same success with regard to the tides: and we may, from the same consideration, find additional motives to desire that such observations may be made, and such existing observations may be discussed, as may most speedily lead us to a complete and scientific knowledge of the subject.

But though we cannot make our inferences from the preceding investigation with confidence, there are some reflexions concerning the mode in which the forces of the sun and moon manifest themselves in the tides, which are suggested by the comparison made in the foregoing pages, and which I will venture to state. The confirmation or refutation of these views must depend on future investigations of the same nature as that contained in this memoir: in the mean time, the views seem fitted to give some additional impulse to the curiosity with which all men of science must now look upon the progress of this subject.

Among the inequalities considered in this memoir, those in which the empirical laws are the clearest and the anomalies the smallest (after the semimenstrual inequalities,) are the inequalities of the time of high water, depending on the moon's parallax and declination. In these the comparison of the law, from theory and from observation, may be stated as follows:

Observation.	Theory.
$P' = (P - p)\big(B + B\sin 2(\varphi - \beta)\big)$;	$(P - p)\,B\sin 2(\varphi - \alpha)$.
$Q' = (\sin^2\delta - \sin^2\Delta)\big(C + D\sin 2(\varphi - \gamma)\big)$;	$(\sin^2\delta - \sin^2\Delta)\,D\sin 2(\varphi - \alpha)$.

It will be observed, that in each of these cases observation gives, in P′ and Q′, a term depending on the parallax and on the declination, (namely, the terms $(P - p)$ B and $(\sin^2\delta - \sin^2\Delta)$ C,) which term is not given by the theory; besides giving another term which coincides with the one given by the theory. The latter term depends on the hour of the moon's transit, and vanishes twice in the course of a semilunation; the former term in each case is independent of the time of the moon's transit, and depends only on the parallax and on the declination.

Now P′ and Q′ are the corrections to which $\theta' - \lambda'$ is subject, where θ' is the hour-angle of the tide from the moon. In the theory, λ' is supposed to be constant, so that the variation of $\theta' - \lambda'$ alone affects θ'.

But since $\theta' = \lambda' + (\theta' - \lambda')$, if λ' were affected by an inequality arising from parallax equal to $(P - p)$ A, we should have, taking the theoretical value of the variation of $\theta' - \lambda'$ due to this cause, and adding it to the value resulting from the common theory, the whole variation of $\theta' = (P - p)\left(A + B \sin 2(\varphi - \gamma)\right)$.

In like manner, if λ' were affected by an inequality equal to $(\sin^2\delta - \sin^2\Delta)$ C, and $\theta' - \lambda'$ by the inequality resulting from the theory, we should have for the whole inequality in θ' arising from declination, $(\sin^2\delta - \sin^2\Delta)\{C + D \sin 2(\varphi - \gamma)\}$.

Now these expressions agree with those which we have obtained from observation, excepting that we have other arcs in the place of the arc α. It appears, therefore, that the empirical laws will be verified by supposing λ' to be affected by inequalities depending upon the parallax and declination of the moon, but having an epoch different from that of the semimenstrual inequality.

The quantity λ' is the hour-angle by which the lunar tide follows the high water of the lunar spheroid of equilibrium. It appears, therefore, that the physical statement of the result just obtained is this, that the distance at which the actual elevation of the waters follows the position of equilibrium, varies as the parallax and declination of the disturbing luminary vary.

This distance was, in the theory, assumed to be constant; but there is no obvious physical reason why it may not change with changes of the force by which the fluid spheroid of equilibrium is determined. This distance, or *lagging*, of the pole of the watery spheroid behind the place which it would occupy if the earth and luminary were at rest, is owing to the resistance of the shores and of the parts of the water amongst each other; and its amount is determined by the amount of these resistances. But we are very far from being able to trace the mode in which these causes operate, so as to be entitled to affirm that changes, and even small changes, in the force or velocity of the disturbing body, may not produce corresponding changes in the extent of this lagging.

In fact, there seems to be good reason to suppose, from other circumstances, that the force and velocity of the disturbing body do affect the distance by which the actual elevation lags behind the elevation of equilibrium. For λ and λ', the lagging in the case of the solar and of the lunar tide, are quite different; the former (for the London Docks) being $3^h\ 25^m$, the latter $1^h\ 25^m$ *. It is true that this difference of 2^h is, in the

* See Mr. Lubbock's Memoir, Philosophical Transactions, 1831, p. 387.

theory, got rid of by supposing the actual tide to be referred to a configuration of the sun and moon anterior by $2\frac{1}{2}$ days to the configuration at the time of the tide. But since we refer the effects of parallax and declination to the parallax and declination contemporaneous with the tide, we must look for an analogy only when we do the same in the other case.

In the semimenstrual inequality, as determined from observation, there is no such discrepancy with theory as compels us to suppose a change in λ'. But this forms no objection to our view; for if, in the course of a semirevolution of the moon, there were a periodical change in λ', this must have the same cycle as the change in $\theta' - \lambda'$, and would therefore be confounded with that change, and would not result in a separate *form* from our discussion.

But, moreover, the difference of the *quantity* of the semimenstrual inequality at different places, which we have already shown to exist, supplies a confirmation of the opinion here put forwards. For this difference implies that the tide travels from one given place to another in different times at different periods of a semilunation; that is, it implies that the velocity of the tide-wave is different in different configurations of the sun and moon, that is, under different circumstances of the tide-producing forces. And this agrees with our doctrine, that the amount of lagging is different under different circumstances of those forces; for if the amount of lagging of the tide elevation go through a cycle of changes in a certain period, the velocity with which this elevation travels will also go through a cycle of changes in the same period. And this difference of the semimenstrual inequality at different places, does appear to betray a semimenstrual inequality affecting λ', the amount of the inequality varying with the place; and this variation, added to the theoretical semimenstrual inequality which affects $\theta' - \lambda'$, and which is the same for all places, makes up the empirical semimenstrual inequality of θ', given by our mode of investigation, which thus appears to be different at different places.

Taking all these reflexions into consideration, there appears to be good reason to believe that the amount of the lagging of the tide behind the equilibrium-tide is really affected by changes in the distances and velocities of the disturbing luminaries.

There is another circumstance in which the empirical differ from the theoretical laws: the epochs β, γ of the changes due to parallax and declination are different from the epoch α of the semimenstrual inequality.

The physical statement of this result is, that the time required to transmit to any port the general effect of the tide-producing forces, and the time required to transmit to the same port the effects of particular changes in these forces, are different. And of this result we may say, in the same manner as of the former, that we see far too obscurely the causes which determine the amount of this interval in one case, to assert that it must necessarily be the same under different circumstances. But we may illustrate this subject somewhat further. We may suppose an imaginary mean moon, moving uniformly in the equator, at a constant distance from the earth, to produce the mean tide; another auxiliary moon, by moving directly to or from the earth, in the line of the mean moon, to produce the inequality which arises from parallax; another auxiliary moon, by moving north and south in the meridian of the moon to produce the inequality which arises from declination. Now the tides produced by all these moons will require some time for the operation of the forces to take effect; that is, they will correspond to positions of the moon at a time anterior to the actual time. But there seems not to be the smallest reason to conclude that these anterior times will all be anterior by the same interval: the contrary, rather, is obvious. It is clear, for instance, that a tide oscillating in a north and south direction in the Atlantic and Pacific Oceans, will take a different portion of time to obey the forces which produce it, from the general tide which travels from east to west round the earth in virtue of the diurnal motion, and impinges against the broad sides of the great continents. We may therefore expect to find the epochs of all these partial tides different; and as every separate term in the expression of an inequality may be considered as representing a different tide, there will be nothing inconsistent with the best physical notions we can yet form on the subject in finding the epochs of the arguments of every separate term of our formulæ different from one another.

It appears, then, that though the equilibrium theory, taken in combination with the preceding considerations, may very probably give us the general form of the terms, and the variable part of the arcs on which they depend, the constant epoch which occurs in each of these arcs, and which determines when the inequality vanishes and reaches its maximum, will probably have to be determined in all cases by observation.

I will observe further, that not only the epochs, but the coefficients of each of these terms will probably have to be determined for the most part from observation. For the tides, though in the theory to which we refer considered as representing positions of equilibrium of a fluid, are in fact the results of its motion; and it is not at all clear that the elevation which results from the motion will be equal to the elevation which would be requisite for equilibrium. It is true that there must be always a tendency to this equilibrium-elevation so long as the actual elevation is greater or less than that; but this tendency may never fully appear in the circumstance of the tide; since the tide-producing forces have to supply also a residue of force which must be employed in producing the motion of the fluid.

Moreover, the motion of the fluid is of the nature of an oscillation, so that series of increasing and diminishing oscillations at intervals of a half-day, a day, and other intervals, pass through any given part of the ocean. Now it is physically, not only possible but certain, that each oscillation in each series is affected by those which precede it in the same series, and affects those which succeed it, so that their relative magnitude is different from what it would otherwise be. And the effects thus produced will depend upon the depth of the ocean, the form of its shores, and other causes, of which it is impossible to estimate the result *à priori*.

Even in the case of the semimenstrual inequality, which in its form agrees so closely with the theory, and which in its amount appears to depend only on the ratio of the forces of the sun and moon, we find that in fact its amount is different at different places, as we have already stated. We cannot expect, therefore, that the amount of the corrections for parallax and declination will agree very exactly with those from theory; and till the empirical corrections are more certainly and generally determined, I have not thought it worth while to make the comparison.

But though there is at present this uncertainty respecting the amount of the inequalities of the tide, I do not conceive that there can be any doubt that the forms of these corrections are such as I have stated them. In the case of the times of high water especially, the general course of the variations of the quantities is as regular as can be expected, and as is requisite for the establishment of our formulæ. The heights are much more anomalous; probably they are more affected by winds, &c., than the times are: and when we reflect that the tide at London may be affected by the operation of causes in a remote part of the ocean, propagating their effect by the progression of the tide-wave, we shall not be surprised at considerable deviations from rule. The trade winds and other winds of the tropical regions may be felt in our tides, and may even affect the means of long series of observations; for it is to be recollected that the averages which we obtain are not the averages of the effects of the sun and moon alone, but the averages of their effect, together with that of meteorological causes; and it is very conceivable that the latter average may not vanish in the long run. It is moreover to be observed, that the peculiar circumstances of London, in having a tide compounded of two tides, arriving by different roads after journeys of different lengths, may easily be supposed to give rise to additional chances of irregularity.

It may not be superfluous to remark, that, independently of such a combination of circumstances, there is nothing in the situation of the port of London to diminish the value of tide observations there. The length and windings of the river by which the tide reaches the port present no objection to the comparison of the observations with theory. These circumstances may modify the tide, but they modify it alike every day, or at least alike at like periods of the tidal cycles, and therefore they introduce no irregularity. Indeed, there are some reasons for believing that the tides in rivers and deep sounds are more regular than those on the open coast; and at any rate, as they are generally larger in such situations, their variations are more observable.

Concluding Observations.

It appears from the preceding investigations and considerations, that the following are now the most important steps from which any great improvement of our knowledge on this subject may be hoped.

Large collections of observations at other places must be discussed in a manner resembling that employed for the London observations by Mr. Dessiou. The Brest and the Liverpool observations would be excellent materials for such operations. We must thus ascertain whether the empirical forms of the corrections for parallax and declination, deduced from these, agree with those obtained for London in the preceding pages. If they do, the coefficients must be compared with each other and with the theory, in order to determine the most promising mode of pursuing the latter.

The empirical formulæ obtained in the preceding pages represent the observations with tolerable exactness; probably they agree with them almost as well as any formulæ would do, and as well as the observations agree with each other. These formulæ might be used in calculating tide tables as readily as any other empirical rules; and the tables so calculated might be compared with observations made at London. Such a comparison, continued long enough, would disclose any additional corrections which may be requisite in this mode of calculating tide tables.

Tide observations are now made at the Katharine Docks, with good apparatus and a judicious system; and, so far as I can judge, with proper care. These will hereafter form materials for a better discussion of the London tides than the London Dock observations, made in a ruder manner, could allow.

III. *On the Position of the North Magnetic Pole. By Commander* James Clark Ross, *R.N. F.R.S. F.R.A.S. F.L.S. &c.*

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THE determination of the position of the Magnetic Poles of the earth has ever been considered a desideratum in the science of magnetism, of the highest importance; and the observations and experiments of the most ingenious and learned philosophers have universally been applied to the solution of this difficult and perplexing problem. Vague and unsatisfactory, however, were the results of the researches and calculations of the most indefatigable and zealous promoters of that science, arising, doubtless, in a great measure, from the discordant observations upon which they were founded,—a discordance which was considered to arise chiefly from the unequal distribution of the magnetic substances contained in the earth, and also from the great distances at which the observations were made from the centres of the powers of those magnetic substances, or, in other words, from the magnetic foci, or poles, of the earth.

The primary cause of magnetic phenomena has always been, and still is, one of the secrets of nature, although several of the laws of magnetism have of late years been gradually developed: and during our absence from England, a greater step perhaps than any former one has been made, through the indefatigable research of Dr. Faraday, by his splendid and convincing proofs of its complete identity with electricity. Still much remains to be accomplished relative to terrestrial magnetism; and accurate observations with good instruments, as near the magnetic poles as possible, and in various directions from them, were long considered amongst the desiderata for completing the magnetic theory of the globe.

These wants, as far as relates to the northern magnetic regions, have been supplied by the expeditions by land and sea that have been sent from England for the discovery of a north-west passage, to traverse the shores of the American continent, and to contribute to the advancement of science in general. In the department of magnetism, in particular, the numerous and accurate observations by their distinguished commanders, and those who accompanied them, have been eminently important. Those made to the north-west of the magnetic pole by Captain Sabine, to the south-west by Captain Franklin, and to the south-east and north-east by Captain Parry, Mr. Fisher, and Captain Forster, have furnished materials that have enabled the British philosophers to point, with a wonderful degree of precision, to the seat of magnetic concentricity.

In contemplating the equipment of the late expedition, a still nearer approach than had yet been attained to that mysterious spot was anticipated from the route that we purposed to pursue; but the smallness of the vessel in which we embarked necessarily limited the number and magnitude of our magnetic instruments. A small dipping-needle by Jones, belonging to the Admiralty, was, together with a number of other instruments, liberally offered for our use; and having been made with much care by that celebrated artist for the use of the party that travelled towards the north pole under Captain Parry, and been found on that occasion to answer every purpose for which it was intended, we did not hesitate to consider it sufficiently large and accurate for this service. A description of the instrument accompanies the Table of Observations made by Captain Parry and Lieutenant Forster in the Appendix to the Narrative of that Voyage (p. 168), and renders any further remarks here unnecessary. It is, however, to be regretted, that prior to our departure from England we had no opportunity of making any observations with that instrument; and a defect in the vertical circle, which was not detected till the spring of the year 1831, has rendered it necessary to reject all the observations on the intensity of the magnetic force made previous to that period.

The annexed Table contains most of the observations that were obtained on the dip of the magnetic needle during our late voyage in the Victory, and seems to require but little explanation. I have considered it proper to record the mean of the readings of each end of the needle in each of its eight positions, because, in looking over the Table, it will be seen that scarcely any two results show any very near accordance, and, in some instances, their differences amount to several degrees. Whether this arises from any imperfection in the instrument, from the method of magnetizing it, or from a variation in the direction of the poles of the needle, I am unable to determine. As the several readings presented themselves, so they were registered; and the resultant dips, although in some instances they show a very considerable difference, yet, upon the whole, their accordance affords a remarkable instance of the tendency of errors (if such they be) to correct each other. Be that as it may, it is proper that these discordances should be known, in order that their cause may be investigated, and that the observations should not obtain a greater degree of dependence than, on examination, they may be found to deserve. Each of the recorded observations is the mean of six to ten readings of each end of the needle in its several positions, and the method employed in the reversion of its poles is that of Du Hamel.

Only three opportunities occurred of observing the dip as we proceeded to the southward of Fury Point to our first winter quarters. But these, together with the variation, &c., were important assistants in calling our attention to the rapid approach we were making towards the magnetic pole. A series of observations during the winter led us to expect that that point would be found directly to the westward of us; but we were unconscious at that time of the existence of an ocean in that direction, and the calculated distance far exceeded anything we could hope to travel over a country whose rugged shores seemed to forbid the attempt, and to annihilate every hope of its accomplishment. The discovery of the Western Ocean, however, across a narrow neck of land to the south-west, which occurred early the following spring, gave rise to a small party being sent from the ship, to endeavour to trace the shores of the American continent as far to the south-west or west as possible. On that occasion, owing to the smallness of the party, it was found impracticable to carry more instruments than were actually indispensable for determining the outline of the coast along which we might pass. An azimuth compass, of Captain Kater's construction, was the only magnetic instrument that could be taken, and this was, soon after leaving the ship, destroyed by a fall over a precipice at Cape Isabella, soon after I had determined that its north point was directed to the north-west. Its action was uncertain to eight or ten degrees, owing to the extreme weakness of the directive force of the needle.

Imperfect as this indication was, it seemed to cherish the hope of our being able to obtain some interesting magnetic observations; when having been compelled to pass another winter near the same spot, I proposed to conduct a party, guided by some Esquimaux, across the country to the westward, and to endeavour to approach as near as possible to the source of magnetism. We accordingly commenced our journey in the middle of May 1831: but the unfavourable nature of the season prevented my obtaining any observations that could be of assistance to us until we reached the shores of the Western Ocean on the 28th of the month. Here good observations were made under the most favourable circumstances; and the magnetic dip having now increased to 89° 41′, and the horizontal needle pointing to N. 57° W., led us to expect that, at the distance of about thirty-five miles in that direction, we should attain the object of our wishes. That spot being now well within our reach, I did not hesitate to devote the larger part of the day to repeating those observations, anticipating that, after leaving that spot, little assistance could be expected from the horizontal needle in directing our approach to the magnetic pole. Having gained the calculated position on the 1st of June, without having been able, from the unfavourable state of the weather, during that interval, to obtain any more observations, I availed myself of the snow huts of a recently deserted Esquimaux village as observatories, and encamped the party at a sufficient distance to ensure their being beyond the possibility of producing any influence on the needles, &c.

My attention was first of all directed to ascertain, if possible, the direction of the magnetic meridian. For this purpose I suspended horizontally the needle that was used only for the determination of the intensity of the magnetic force, first by three or four delicate fibres of floss silk. It remained, however, exactly in the position in which it was placed. A single fibre of the floss silk was next tried, and lastly a single fibre of flax. All these failing to demonstrate the smallest amount of horizontal attraction, a second needle was treated in a similar manner, and in all these attempts I was equally unsuccessful. The top of the instrument being so constructed as to admit of a half-circle of torsion, this was next tried; but the needle was moved from its position in nearly the same amount as the arc described by the point of suspension, showing that the smallest amount of torsion was sufficient to overcome the directive energy of the needle.

The needle was now removed to the dipping apparatus, and the following observations on the intensity of the vertical force of the needle were obtained, upon the supposition, that in whatever direction a given number of vibrations in the same arc were made in the shortest time, that might be assumed as the magnetic meridian. The direction of the needle is given in true bearings.

S. 50° W. and N. 50° E.	S. 80° W. and N. 80° E.	N. 70° W. and N. 70° E.	N. 40° W. and S. 40° E.	N. 10° W. and S. 10° E.	N. 20° W. and S. 20° W.
h m s	h m s	h m s	h m s	h m s	h m s
10 34 20	10 37 28·7	10 40 50·2	10 44 3	10 46 59·5	10 49 47·5
43	52·5	41 13·5	26·5	47 23	50 10·5
35 5·2	38 14·5	36·5	49·2	45·5	33·2
27	36·2	57·5	45 10·5	48 7·5	54·7
48·5	58	42 19	32·5	29	51 16·2
36 10·2	39 18·7	40·7	54	50·7	37·5
50 Vibns in 1 50·2	1 50	1 50·5	1 51	1 51·2	1 50

From these observations it was equally impossible to assign a direction to the magnetic meridian, the slight differences being within the limits of the errors of observation, and the amount of the inclination or dip of the needle in each of these directions being precisely the same. A diminution of force, however, may seem to obtain in the directions of S. 10° E. and S. 40° E.; and a direction at right angles to that, S. 75° W., I assumed as the magnetic meridian in the first two sets of dip. The mean of these was 89° 58′ 15″. The next two sets were taken at an angle of 45° to the right of the former, and their mean result was 89° 59′ 46″; and the two last sets, exactly at right angles to the first set, gave the dip 89° 59′. In these last observations the axis of the intensity needle was put in the stead of its own axis, which accounts for the difference in the readings of the needle in its several positions, as will be seen by the table of dips. The reason for my doing this was to provide against the possibility of the observations being influenced by an injury which the axis of the needle was supposed to have sustained, by the great difference that sometimes occurred in its indications. The result of these observations, however, shows that the injury, if it had met with any, did not materially affect the results; so perfectly do the principles of its construction counteract any slight bend in the axis, or any inequality in the balance of the needle.

To complete the observations on the intensity of the magnetic force, and the various experiments which were made, and which it is unnecessary here to notice, occupied the whole of the time that I could devote to that purpose. And although there is a difference, amounting to several minutes, in the different observations made in the same direction of the needle, yet the resultant mean dip in each of the three directions in which they were obtained placed us as near to the magnetic pole as, with our limited means, we were able to determine. And although it cannot but be a rough approximation, yet it is hardly possible to be more than a few miles from the exact position. It was, at any rate, quite impossible for us to know, now that the horizontal needle had ceased to act, in what direction to proceed for the purpose of approaching it more nearly; for, in order to determine its exact position, the cooperation of several observers, placed at some distance, in various directions of its position, would be necessary. A series of observations, continued for some months, would afford the most important and interesting data. By such means, not only its actual position, but its diurnal, if not its annual motions, could be determined, and furnish the means of investigating most of the phenomena of magnetism which are now exhibited on our globe; and establishing for future ages a most important point of reference, by which any progressive movement may be ascertained, and ultimately brought within the reach of mathematical determination for any given period.

This is precisely what is still wanting; and now that its position is so nearly known, and that it is placed in a spot easy of access, and affording every facility for carrying such a series of operations into effect, it only remains to be considered whether those who have the power to promote such an undertaking may attach sufficient importance to the subject to direct its being carried into execution. It is certainly every way worthy of our country. The science of magnetism, indeed, is eminently British. There is no other country in the world whose interests are so deeply connected with it as a maritime nation, or whose glory as such is so intimately associated with it, as Great Britain. All the late discoveries and improvements are to be attributed to the perseverance of British science, and the encouragement and assistance of an enlightened and liberal Administration. Nor will the name of Felix Booth, Esq. be omitted in the list of our country's most distinguished patriots, whose munificence and princely spirit have furnished the whole pecuniary means of obtaining the results which are now presented to the Society; and, I may fearlessly venture to add, of enabling a few British seamen to plant the flag of their country upon the Northern Magnetic Pole of the earth.

Observations on the Dip of the Magnetic Needle.

Date.	Time of the day.	Poles of the Needle direct. Axis direct. Face East.	Poles of the Needle direct. Axis direct. Face West.	Poles of the Needle direct. Axis reversed. Face West.	Poles of the Needle direct. Axis reversed. Face East.	Poles of the Needle direct. Mean.	Poles of the Needle reversed. Axis direct. Face East.	Poles of the Needle reversed. Axis direct. Face West.	Poles of the Needle reversed. Axis reversed. Face West.	Poles of the Needle reversed. Axis reversed. Face East.	Poles of the Needle reversed. Mean.	Observed Dip.	Remarks.
1831.		° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	° ′	
Feb. 15.	Noon.	78 11·5	99 34·73	78 6·23	99 25·67	88 49·53	79 26·17	98 37·67	79 17·33	98 52·5	89 3·42	88 56·47	Mean observed dip at Sheriff's Bay in lat. 70° 1′ N., and long. 91° 54′ W. Variation 96° 12′·3 W. previous to my journey towards the magnetic pole = 88° 57′·04 N. (1831.)
28.	1 P.M.	81 30	98 38·78	79 35·60	98 14·50	89 29·72	72 7·86	103 38·44	72 52	105 43·75	88 35·51	89 2·61	
March 1.	2 P.M.	81 42·8	97 52	81 7	96 49·4	89 22·80	77 16	100 27	77 47·42	98 49·6	88 35·0	88 58·90	
4.	Noon.	81 34·8	96 37·4	81 18	97 25	89 13·8	76 30·4	101 10	78 15·6	99 27	88 51·25	89 2·52	
15.	1 P.M.	81 34·7	96 34·5	81 34·2	96 41·2	89 6·15	75 27·5	102 12·3	75 6·9	102 59·5	88 56·55	89 1·35	
21.	4 P.M.	75 5·67	103 6·17	74 12·83	101 8·33	88 23·14	81 14·5	97 6	81 56·1	95 35·3	88 57·97	88 40·56	
22.	4 P.M.	86 7·63	91 30·17	87 0·0	90 29	88 47·7	86 13	92 35·7	87 19·83	90 37·83	89 11·59	88 59·15	
23.	3 P.M.	86 18·2	91 11·35	87 9·14	90 47	88 51·42	86 24·17	90 17	87 28	91 22	88 47·29	88 49·36	
24.	2 P.M.	81 56·5	96 18·7	81 49·7	96 0·20	89 1·27	81 57·70	95 40·1	81 18	97 20·2	89 4·0	89 2·64	
25.	3 P.M.	78 56	98 30	78 1·1	99 27·5	88 43·65	79 51·4	99 12·17	80 8·72	97 21·4	89 8·42	88 56·04	
30.	3 P.M.	77 41	99 49·25	77 38·75	99 44·44	88 43·36	81 23·9	96 58·3	81 59·4	96 26·4	89 12·0	88 57·68	
April 1.	2 P.M.	76 47·1	100 2·90	78 29	100 14·20	88 53·27	81 52·4	95 48·14	81 27·12	97 13·4	89 5·27	88 59·27	
	4 P.M.	78 40·67	99 2·17	78 36·33	98 52	88 48·04	80 8·27	97 51·13	80 20·27	97 48·5	89 2·04	88 55·04	
May 28.	8 A.M.	86 31·7	92 47	86 26·83	93 30	89 48·87	73 9·5	106 11·5	84 20	94 35·8	89 34·2	89 41·53	89° 41′·9 N. Variation 57° 15′ W.; lat. 69° 34′ 45″ N.; long. 94° 53′ W.
	2 P.M.	86 17·22	92 51·3	87 2·14	93 32·16	89 55·71	74 42·2	104 58·22	83 24·7	94 50·18	89 28·83	89 42·27	
June 1.	Noon.	86 23·67	93 8·33	87 6·17	93 32·83	90 2·75	73 53·67	104 51·67	83 44·33	97 7·5	89 54·29	89 58·52	89° 58′·11 N. Assumed direction of meridian S. 75° W. (True.)
	3 P.M.	85 55·5	93 32·62	86 40·67	93 54	90 0·71	73 22	105 24·83	83 23·33	97 28·67	89 54·71	89 57·71	
	5 P.M.	86 32·33	93 10·33	87 16·67	93 9·83	90 1·79	74 58·83	104 16·83	83 0·83	97 5·13	89 50·15	89 55·97	89° 59′·465 N. Assumed direction of meridian N. 60° W.; lat. 70° 5′ 17″ N.; long. 96° 45′ 48″ W.
	7 P.M.	86 52·83	93 9·67	87 14·50	93 32·0	90 12·25	74 55	104 24·5	83 38·17	96 37	89 53·67	90 2·96	
2.	9 A.M.	84 24·33	96 12·67	82 46·37	96 55·1	90 4·62	82 29·5	97 14·33	85 14·5	94 58·33	89 59·14	90 1·88	89° 59′ N. Assumed direction of meridian N. 15° W.
	11 A.M.	84 3	96 25·56	82 32·60	96 35·75	89 54·08	82 36·5	97 26·75	85 20·67	94 29·0	89 58·17	89 56·12	
6.	8 A.M.	86 0·5	92 15·83	86 58·33	93 6·67	89 42·38	75 25·67	103 55·83	82 40·17	96 28·5	89 37·54	89 40·19	Same position as on the 28th of May.
8.	9 A.M.	86 27·5	92 3·33	87 13·67	92 17·83	89 30·58	75 20	101 36·17	81 22·33	98 37·83	89 14·04	89 22·33	At Cape Isabella. Lat. 69° 26′ 20″; long. 93° 51′ W.
9.	8 A.M.	84 42·83	94 33·13	84 25·67	93 42·5	89 21·03	77 48·83	100 1	81 42	97 21·5	89 13·33	89 17·18	At Padleak. Lat. 69° 30′ 1″; long. 93° 27′ 52″ W.
17.	2 P.M.	86 5	92 40·2	87 41·5	91 52·5	89 34·8	82 41	93 30	85 1·3	93 2·3	88 33·65	89 4·22	Observed dip at Sheriff Bay on my return from the magnetic pole 89° 3′·32 N. (1831.)
	5 P.M.	86 9·8	91 48·5	87 45	91 15	89 14·57	80 45	96 30·8	85 9·2	93 7	88 53	89 3·79	
July 13.	2 P.M.	85 43·33	92 4·5	87 50·33	91 42	89 20·40	82 23	95 1·5	82 37	95 14·83	88 49·04	89 4·74	
Aug. 12.	1 P.M.	86 27·5	91 41·7	87 57·5	91 14·7	89 20·35	84 9·2	93 24·2	79 45·8	97 38·3	88 44·37	89 2·36	
20.	Noon.	80 3·34	98 7·5	80 46·7	97 20·8	89 4·53	76 15·7	101 30	99 53·2	78 15	88 58·47	89 1·50	
Oct. 21.	10 A.M.	84 40·17	93 52	84 24	93 33·45	89 7·40	79 1·89	98 5·67	81 36·67	96 5	88 42·31	88 54·86	Observed dip at Victory Harbour 88° 54′·86 N. Variation 101° 32′·03 W.; lat. 70° 9′ 18″ N.; long. 91° 30′ 33″ W.
22.	9 A.M.	84 40·5	94 16·5	84 50·12	93 49·37	89 24·12	77 29·4	99 41	80 24·5	96 45·2	88 35·03	88 59·57	
23.	Noon.	84 9	93 13·9	84 40·6	93 43·2	88 56·67	78 55·4	99 5·9	81 57·6	95 32·8	88 53·93	88 55·30	
Nov. 21.	Noon.	84 18·8	94 8·1	84 18·1	94 27·9	89 17·98	77 20·5	99 22	78 41·8	98 5·6	88 22·47	88 50·22	
22.	1 P.M.	84 56·2	93 46·6	84 54·6	93 48·6	89 21·5	78 55·3	98 24	79 28·3	96 51	88 24·65	88 53·07	
23.	1 P.M.	84 43	93 37·6	84 59	93 24·4	89 11	79 39·8	98 8	80 36·2	95 55·4	88 34·88	88 52·92	
Dec. 24.	10 A.M.	84 42·9	93 50·7	84 11·7	94 32·6	89 16·97	79 47·5	98 31	81 51·5	94 46	88 44	89 0·49	
	1 P.M.	85 21·5	93 2	84 19.8	93 57	89 10·07	79 58·6	98 16·8	81 37	94 41·5	88 38·48	88 54·27	
1832.													
Jan. 21.	Noon.	85 1	94 8·6	84 24·5	94 23·2	89 29·32	76 53	100 16·2	81 7	95 59·4	88 33·9	89 1·61	
Feb. 16.	1 P.M.	84 58·5	95 11·8	83 17·3	95 14	89 40·04	76 46·1	100 52	79 51·7	96 31·1	88 30·22	89 5·31	
18.	1 P.M.	83 48·4	95 18·6	81 58·1	95 19·7	89 6·2	77 30·1	100 39·5	81 30·4	95 37·8	88 49·45	88 57·82	
March 17.	3 P.M.	83 16·2	94 41·6	82 32	95 42·1	89 2·98	74 53·9	102 45·2	78 55	96 3·1	88 9·3	88 36·14	
27.	2 P.M.	83 30·7	94 48·4	84 2·9	94 38·4	89 15·1	74 4·5	102 54·7	78 28	98 45·6	88 33·2	88 54·15	
April 13.	......	83 38·5	94 47	82 47·6	95 14·9	89 7	75 36·9	101 13	78 53	98 23·6	88 31·62	88 49·31	
15.	......	83 5·5	95 22·5	82 30·9	96 27·2	89 21·52	78 21	98 23·3	75 45·4	101 46·7	88 34·12	88 57·82	

IV. *Notice as to the supposed Identity of the large Mass of Meteoric Iron now in the British Museum, with the celebrated Otumpa Iron described by* Rubin de Celis *in the Philosophical Transactions for* 1786. *Communicated in a Letter from* Woodbine Parish, *Esq. F.R.S. to* Charles König, *Esq. Foreign Secretary of the Royal Society.*

Received and read November 21, 1833.

As the identity of the large mass of meteoric iron in the British Museum with the celebrated Otumpa iron, described by Rubin de Celis in the Philosophical Transactions for 1786, has been the subject of frequent inquiry, the following short historical notice, relating to that mass, is communicated by Woodbine Parish, Esq. F.R.S., by whom, when His Majesty's Chargé d'Affaires at Buenos Ayres, it was sent to England. —C. K.

"London,
August 10th, 1833.

"Dear Sir,

"Agreeably to my promise, I have taken some trouble to ascertain the precise history of the large mass of native iron which I sent home to Sir Humphry Davy from Buenos Ayres, and which is deposited in the British Museum. There is no doubt of its coming from the same place as that described by Rubin de Celis, though whether it be a fragment of that particular mass upon which he made his report, or a smaller one in its immediate vicinity, I am not able to say, for there certainly is an impression at Buenos Ayres that there is not only one, but that several masses of this iron are to be found in that part of the Gran Chaco referred to by Rubin de Celis. I was under the impression that it had been sent for in order to be forwarded to Madrid; but in this I was led into error; and I have only lately ascertained through Mr. Moreno, the Buenos Ayrean Minister, that the real history of its being at Buenos Ayres is as follows.

"After the people of that country had declared their independency of Spain, they were blockaded by a naval force, which cut off their communication with Europe, and especially prevented their receiving what they were in great need of, viz. arms and other warlike stores. In this dilemma it was suggested that muskets might be made if they had but the material; and it was then that the iron formerly described by De Celis was recollected as existing within their own territories, and people were sent to the Gran Chaco to bring away at least a part of it, that it might be ascertained how far it was fit for the purpose; and thus this particular mass was brought to Buenos Ayres. By the time it arrived there, early in 1813, the necessity for using it had ceased: the projected experiment, however, was tried, and a pair of pistols were made of it, which were afterwards sent as a present to the President of the United States. The remainder of the mass brought down from the Chaco, after some specimens were taken from it, was laid aside in the arsenal at Buenos Ayres until it was given to me by the Government of that country on the occasion of my having to carry into effect the recognition by Great Britain of its political independence. And this is the precise history of this iron. If you think there is anything in it worth making known to the Royal Society, you are quite at liberty to copy or extract this account of it.

"Believe me to be, my dear Sir,

"Very truly yours,

"WOODBINE PARISH."

"To CHARLES KONIG, Esq.,
British Museum."

V. *Experimental Researches in Electricity.—Sixth Series.* *By* MICHAEL FARADAY, *D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Copenhagen, Berlin, &c. &c.*

Received November 30, 1833,—Read January 11, 1834.

§ 12. *On the power of Metals and other Solids to induce the Combination of Gaseous Bodies.*

564. THE conclusion at which I have arrived in this section may seem to render the whole of it unfit to form part of a series of researches in electricity; since, remarkable as the phenomena are, the power which produces them is not considered as of an electric origin, otherwise than as all attraction of particles may have this subtil agent for their common cause. But as the effects investigated arose out of electrical researches, as they are directly connected with others which are of an electric nature, and must of necessity be understood and guarded against in a very extensive series of electro-chemical decompositions (707.), I have felt myself fully justified in detailing them in this place.

565. Believing that I had proved (by experiments hereafter to be described (705.),) the constant and definite chemical action of a certain quantity of electricity, whatever its intensity might be, or however the circumstances of its transmission through either the decomposing body or the more perfect conductors were varied, I endeavoured upon that result to construct a new measuring instrument, which from its use might be called, at least provisionally, a *Volta-electrometer* (739.).

566. During the course of the experiments made to render the instrument efficient, I was occasionally surprised at observing a deficiency of the gases resulting from the decompositions of water, and at last an actual disappearance of portions which had been evolved, collected, and measured. The circumstances of the disappearance were these. A glass tube, about twelve inches in length and $\frac{3}{4}$ths of an inch in diameter, had two platina poles fixed into its upper, hermetically sealed, extremity: the poles, where they passed through the glass, were of wire; but terminated below in plates, which were soldered to the wires with gold (Plate I. fig. 1.). The tube was filled with dilute sulphuric acid, and inverted in a cup of the same fluid; a voltaic battery was connected with the two wires, and sufficient oxygen and hydrogen evolved to occupy $\frac{4}{5}$ths of the tube, or by the graduation 116 parts. On separating the tube from the voltaic battery the volume of gas immediately began to diminish, and in about five hours only $13\frac{1}{2}$ parts remained, and these ultimately disappeared.

567. It was found, by various experiments, that this effect was not due to the escape or solution of the gas, nor to recombination of the oxygen or hydrogen in consequence of any peculiar condition *they* might be supposed to possess under the circumstances; but to be occasioned by the action of one or both of the poles within the tube upon the gas around them. On disuniting the poles from the pile after they had acted upon dilute sulphuric acid, and introducing them into separate tubes containing mixed oxygen and hydrogen, it was found that the *positive* pole effected the union of the gases, but the negative pole apparently not (588.). It was ascertained also that no action of a sensible kind took place between the positive pole with oxygen or hydrogen alone.

568. These experiments reduced the phenomena to the consequence of a power possessed by the platina, after it had been the positive pole of a voltaic pile, of causing the combination of oxygen and hydrogen at common, or even at low, temperatures. This effect is, as far as I am aware, altogether new, and was immediately followed out to ascertain whether it was really of an electric nature, and how far it would interfere with the determination of the quantities evolved in the cases of electro-chemical decomposition required in the fourteenth section of these Researches.

569. Several platina plates were prepared (fig. 2.). They were nearly half an inch wide, and two inches and a half long: some were $\frac{1}{200}$dth of an inch, others not more than $\frac{1}{600}$dth, and some were as much as $\frac{1}{70}$th of an inch in thickness. Each had a piece of platina wire, about seven inches long, soldered to it by pure gold. Then a number of glass tubes were prepared: they were about nine or ten inches in length, $\frac{5}{8}$ths of an inch in internal diameter, were sealed hermetically at one extremity, and were graduated. Into these tubes was put a mixture of two volumes of hydrogen and one of oxygen, at the water pneumatic trough, and when one of the plates described had been connected with the positive or negative pole of the voltaic battery for a given time, or had been otherwise prepared, it was introduced through the water into the gas within the tube; the whole set aside in a test-glass (fig. 3.), and left for a longer or shorter period, that the action might be observed.

570. The following result may be given as an illustration of the phenomenon to be investigated. Diluted sulphuric acid, of the specific gravity 1·336, was put into a glass jar, in which was placed also a large platina plate, connected with the negative end of a voltaic battery of forty pairs of four-inch plates, with double coppers, and moderately charged. One of the plates (569.) was then connected with the positive extremity, and immersed in the same jar of acid for five minutes, after which it was separated from the voltaic battery, washed in distilled water, and introduced through the water of the pneumatic trough into a tube containing the mixture of oxygen and hydrogen (569.). The volume of gases immediately began to lessen, the diminution proceeding more and more rapidly until about $\frac{3}{4}$ths of the mixture had disappeared. The upper end of the tube became quite warm, the plate itself so hot that the water boiled as it rose over it; and in less than a minute a cubical inch and a half of the

gases were gone, having been combined by the power of the platina, and converted into water.

571. This extraordinary influence acquired by the platina at the positive pole of the pile, is exerted far more readily and effectively on oxygen and hydrogen than on any other mixture of gases that I have tried. One volume of nitrous gas was mixed with a volume of hydrogen, and introduced into a tube with a plate which had been made positive in the dilute sulphuric acid for four minutes (570.). There was no sensible action in an hour: being left for thirty-six hours, there was a diminution of about one eighth of the whole volume. Action had taken place, but it had been very feeble.

572. A mixture of two volumes of nitrous oxide with one volume of hydrogen was put with a plate similarly prepared into a tube (569. 570.). This also showed no action immediately; but in thirty-six hours nearly a fourth of the whole had disappeared, i. e. about half of a cubic inch. By comparison with another tube containing the same mixture without a plate, it appeared that a part of the diminution was due to solution, and another part to the power of the platina; but the action had been very slow and feeble.

573. A mixture of one volume olefiant gas and three volumes oxygen was not affected by such a platina plate, even though left together for several days (640. 641.).

574. A mixture of two volumes carbonic oxide and one volume oxygen was also unaffected by the prepared platina plate in several days (645. &c.).

575. A mixture of equal volumes of chlorine and hydrogen was used in several experiments, with plates prepared in a similar manner (570.). Diminution of bulk soon took place; but when after thirty-six hours the experiments were examined, it was found that nearly all the chlorine had disappeared, having been absorbed, principally by the water, and that the original volume of hydrogen remained unchanged. No combination of the gases, therefore, had here taken place.

576. Reverting to the action of the prepared plates on mixtures of oxygen and hydrogen (570.), I found that the power, though gradually diminishing in all cases, could still be retained for a period varying in its length with circumstances. When tubes containing plates (569.) were supplied with fresh portions of mixed oxygen and hydrogen as the previous portions were condensed, the action was found to continue for above thirty hours, and in some cases slow combination could be observed even after eighty hours; but the continuance of the action greatly depended upon the purity of the gases used (638.).

577. Some plates (569.) were made positive for four minutes in dilute sulphuric acid of specific gravity 1·336: they were rinsed in distilled water, after which two were put into a small bottle and closed up, whilst others were left exposed to the air. The plates preserved in the limited portion of air were found to retain their power after eight days, but those exposed to the atmosphere had lost their force almost entirely in twelve hours, and in some situations, where currents existed, in a much shorter time.

578. Plates were made positive for five minutes in sulphuric acid, specific gra-

vity 1·336. One of these was retained in similar acid for eight minutes after separation from the battery: it then acted on mixed oxygen and hydrogen with apparently undiminished vigour. Others were left in similar acid for forty hours, and some even for eight days, after the electrization, and then acted as well in combining oxygen and hydrogen gas as those which were used immediately after electrization.

579. The effect of a caustic solution of potassa in preserving the platina plates was tried in a similar manner. After being retained in such a solution for forty hours, they acted exceedingly well on oxygen and hydrogen, and one caused such rapid condensation of the gases, that the plate became much heated, and I expected the temperature would have risen to ignition.

580. When similarly prepared plates (569.) had been put into distilled water for forty hours, and then introduced into mixed oxygen and hydrogen, they were found to act but very slowly and feebly as compared with those which had been preserved in acid or alkali. When, however, the quantity of water was but small, the power was very little impaired after three or four days. As the water had been retained in a wooden vessel, portions of it were redistilled in glass, and this was found to preserve prepared plates for a great length of time. Prepared plates were put into tubes with this water and closed up; some of them, taken out at the end of twenty-four days, were found very active on mixed oxygen and hydrogen; others, which were left in the water for fifty-three days, were still found to cause the combination of the gases. The tubes had been closed only by corks.

581. The act of combination always seemed to diminish, or apparently exhaust, the power of the platina plate. It is true that in most, if not all instances, the combination of the gases, at first insensible, gradually increased in rapidity, and sometimes reached to explosion; but when the latter did not happen, the rapidity of combination diminished; and although fresh mixtures of gas were introduced into the tubes, the combination went on more and more slowly, and at last ceased altogether. The first effect of an increase in the rapidity of combination depended in part upon the water flowing off from the platina plate, and allowing a better contact with the gas, and in part upon the heat evolved during the progress of the combination (630.). But notwithstanding the effect of these causes, diminution, and at last cessation of the power, always occurred. It must not, however, be unnoticed, that the purer the gases subjected to the action of the plate, the longer was its combining power retained. With the mixture evolved at the poles of the voltaic pile in pure dilute sulphuric acid, it continued longest; and with oxygen and hydrogen, of perfect purity, it probably would not be diminished at all.

582. Different modes of treatment applied to the platina plate, after it had ceased to be the positive pole of the pile, affected its power very curiously. A plate which had been a positive pole in diluted sulphuric acid of specific gravity 1·336 for four or five minutes, if rinsed in water and put into mixed oxygen and hydrogen, would act very well, and condense perhaps one cubic inch and a half of gas in six or seven

minutes; but if that same plate, instead of being merely rinsed, had been left in distilled water for twelve or fifteen minutes, or more, it would rarely fail, when put into the oxygen and hydrogen, of becoming, in the course of a minute or two, ignited, and would generally explode the gases. Occasionally the time occupied in bringing on the action extended to eight or nine minutes, and sometimes even to forty minutes, and yet ignition and explosion would result. This effect is due to the removal of a portion of acid which otherwise adheres firmly to the plate.

583. Occasionally the platina plates (569.), after being made the positive pole of the battery, were washed, wiped with filtering-paper or a cloth, and washed and wiped again. Being then introduced into mixed oxygen and hydrogen, they acted apparently as if they had been unaffected by the treatment. Sometimes the tubes containing the gas were opened in the air for an instant, and the plates put in dry; but no sensible difference in action was perceived, except that it commenced sooner.

584. The power of heat in altering the action of the prepared platina plates was also tried (595.). Plates which had been rendered positive in dilute sulphuric acid for four minutes were well washed in water, and heated to redness in the flame of a spirit-lamp: after this they acted very well on mixed oxygen and hydrogen. Others, which had been heated more powerfully by the blowpipe, acted afterwards on the gases, though not so powerfully as the former. Hence it appears that heat does not take away the power acquired by the platina at the positive pole of the pile: the occasional diminution of force seemed always referable to other causes than the mere heat. If, for instance, the plate had not been well washed from the acid, or if the flame used was carbonaceous, or was that of an alcohol lamp trimmed with spirit containing a little acid, or having a wick on which salt, or other extraneous matter, had been placed, then the power of the plate was quickly and greatly diminished (634. 636.).

585. This remarkable property was conferred upon platina when it was made the positive pole in sulphuric acid of specific gravity 1·336, or when it was considerably weaker, or when stronger, even up to the strength of oil of vitriol. Strong and dilute nitric acid, dilute acetic acid, solutions of tartaric, citric, and oxalic acids, were used with equal success. When muriatic acid was used, the plates acquired the power of condensing the oxygen and hydrogen, but in a much inferior degree.

586. Plates which were made positive in solution of caustic potassa did not show any sensible action upon the mixed oxygen and hydrogen. Other plates made positive in solutions of carbonates of potassa and soda exhibited the action, but only in a feeble degree.

587. When a neutral solution of sulphate of soda, or of nitre, or of chlorate of potassa, or of phosphate of potassa, or acetate of potassa, or sulphate of copper, was used, the plates, rendered positive in them for four minutes, and then washed in water, acted very readily and powerfully on the mixed oxygen and hydrogen.

588. It became a very important point, in reference to the *cause* of this action of

the platina, to determine whether the *positive* pole *only* could confer it (567.), or whether, notwithstanding the numerous contrary cases, the *negative* pole might not have the power when such circumstances as could interfere with or prevent the action were avoided. Three plates were therefore rendered negative for four minutes in diluted sulphuric acid of specific gravity 1·336, washed in distilled water, and put into mixed oxygen and hydrogen. *All* of them *acted*, though not so strongly as they would have done if they had been rendered positive. Each combined about a cubical inch and a quarter of the gases in twenty-five minutes. On every repetition of the experiment the same result was obtained; and when the plates were retained in distilled water for ten or twelve minutes, before being introduced into the gas (582.), the action was very much quickened.

589. But when there was any metallic or other substance present in the acid, which could be precipitated on the negative plate, then that plate ceased to act upon the mixed oxygen and hydrogen.

590. These experiments led to the expectation that the power of causing oxygen and hydrogen to combine, which could be conferred upon any piece of platina by making it the positive pole of a voltaic pile, was not essentially dependent upon the action of the pile, or upon any structure or arrangement of parts it might receive whilst in association with it, but belonged to the platina *at all times*, and was *always effective* when the surface was *perfectly clean*. And though, when made the *positive* pole of the pile in acids, the circumstances might well be considered as those which would cleanse the surface of the platina in the most effectual manner, it did not seem impossible that ordinary operations should produce the same result, although in a less eminent degree.

591. Accordingly, a platina plate (569.) was cleaned by being rubbed with a cork, a little water, and some coal-fire ashes upon a glass plate: being washed, it was put into mixed oxygen and hydrogen, and was found to act at first slowly, and then more rapidly. In an hour, a cubical inch and a half had disappeared.

592. Other plates were cleaned with ordinary sand-paper and water; others with whitening and water; others with emery and water; others, again, with black oxide of manganese and water; and others with a piece of charcoal and water. All of these acted in tubes of oxygen and hydrogen, causing combination of the gases. The action was by no means so powerful as that produced by plates having been in communication with the battery; but from one to two cubical inches of the gases disappeared, in periods extending from twenty-five to eighty or ninety minutes.

593. Upon cleaning the plates with a cork, ground emery, and dilute sulphuric acid, they were found to act still better. In order to simplify the conditions, the cork was dismissed, and a piece of platina foil used instead; still the effect took place. Then the acid was dismissed, and a solution of *potassa* used, but the effect occurred as before.

594. These results are abundantly sufficient to show that the mere mechanical

cleansing of the surface of the platina is sufficient to enable it to exert its combining power over oxygen and hydrogen at common temperatures.

595. I now tried the effect of heat in conferring this property upon platina (584.). Plates which had no action on the mixture of oxygen and hydrogen were heated by the flame of a freshly trimmed spirit-lamp, urged by a mouth blowpipe, and when cold were put into tubes of the mixed gases: they acted slowly at first, but after two or three hours condensed nearly all the gases.

596. A plate of platina, which was about one inch wide and two and three quarters in length, and which had not been used in any of the preceding experiments, was curved a little so as to enter a tube, and left in a mixture of oxygen and hydrogen for thirteen hours: not the slightest action or combination of the gases occurred. It was withdrawn at the pneumatic trough from the gas through the water, heated red hot by the spirit-lamp and blowpipe, and then returned when cold into the *same* portion of gas. In the course of a few minutes diminution of the gases could be observed, and in forty-five minutes about one cubical inch and a quarter had disappeared. In many other experiments platina plates when heated were found to acquire the power of combining oxygen and hydrogen.

597. But it happened not unfrequently that plates, after being heated, showed no power of combining oxygen and hydrogen gases, though left undisturbed in them for two hours. Sometimes also it would happen that a plate which, having been heated to dull redness, acted feebly, upon being heated to whiteness ceased to act; and at other times a plate which, having been slightly heated, did not act, was rendered active by a more powerful ignition.

598. Though thus uncertain in its action, and though often diminishing the power given to the plates at the positive pole of the pile (584.), still it is evident that heat can render platina active, which before was inert (595.). The cause of its occasional failure appears to be due to the surface of the metal becoming soiled, either from something previously adhering to it, which is made to adhere more closely by the action of the heat, or from matter communicated from the flame of the lamp, or from the air itself. It often happens that a polished plate of platina, when heated by the spirit-lamp and a blowpipe, becomes dulled and clouded on its surface by something either formed or deposited there; and this, and much less than this, is sufficient to prevent it from exhibiting the curious power now under consideration (634. 636.). Platina also has been said to combine with carbon; and it is not at all unlikely that in processes of heating, where carbon or its compounds are present, a film of such a compound may be thus formed, and thus prevent the exhibition of the properties belonging to *pure* platina.

599. The action of alkalies and acids in giving platina this property was now experimentally examined. Platina plates (569.) having no action on mixed oxygen and hydrogen, being boiled in a solution of caustic potassa, washed, and then put into the gases, were found occasionally to act pretty well, but at other times to fail. In

the latter case I concluded that the impurity upon the surface of the platina was of a nature not to be removed by the mere solvent action of the alkali, for when the plates were rubbed with a little emery, and the same solution of alkali (592.), they became active.

600. The action of acids was far more constant and satisfactory. A platina plate was boiled in dilute nitric acid: being washed and put into mixed oxygen and hydrogen gases, it acted well. Other plates were boiled in strong nitric acid for periods extending from half a minute to four minutes, and then being washed in distilled water, were found to act very well, condensing one cubic inch and a half of gas in the space of eight or nine minutes, and rendering the tube warm (570.).

601. Strong sulphuric acid was very effectual in rendering the platina active. A plate (569.) was heated in it for a minute, then washed and put into the mixed oxygen and hydrogen, upon which it acted as well as if it had been made the positive pole of a voltaic pile (570.).

602. Plates which, after being heated or electrized in alkali, or after other treatment, were found inert, immediately received power by being dipped for a minute or two, or even only for an instant, into hot oil of vitriol, and then into water.

603. When the plate was dipped into the oil of vitriol, taken out, and then heated so as to drive off the acid, it did not act, in consequence of the impurity left by the acid upon its surface.

604. Vegetable acids, as acetic and tartaric, sometimes rendered inert platina active, at other times not. This, I believe, depended upon the character of the matter previously soiling the plates, and which may easily be supposed to be sometimes of such a nature as to be removed by these acids, and at other times not. Weak sulphuric acid showed the same difference, but strong sulphuric acid (601.) never failed in its action.

605. The most favourable treatment, excepting that of making the plate a positive pole in strong acid, was as follows. The plate was held over a spirit-lamp flame, and when hot, rubbed with a piece of potassa fusa (caustic potash), which melting, covered the metal with a coat of very strong alkali, and this was retained fused upon the surface for a second or two*: it was then put into water for four or five minutes to wash off the alkali, shaken, and immersed for about a minute in hot strong oil of vitriol; from this it was removed into distilled water, where it was allowed to remain ten or fifteen minutes to remove the last traces of acid (582.). Being then put into a mixture of oxygen and hydrogen, combination immediately began, and proceeded rapidly; the tube became warm, the platina became red hot, and the residue of the gases was inflamed. This effect could be repeated at pleasure, and thus the maximum phenomenon could be produced without the aid of the voltaic battery.

606. When a solution of tartaric or acetic acid was substituted, in this mode of preparation, for the sulphuric acid, still the plate was found to acquire the same power,

* The heat need not be raised so much as to make the alkali tarnish the platina, although if that effect does take place it does not prevent the ultimate action.

and would often produce explosion in the mixed gases; but the strong sulphuric acid was most certain and powerful.

607. If borax, or a mixture of the carbonates of potash and soda, be fused on the surface of a platina plate, and that plate be well washed in water, it will be found to have acquired the power of combining oxygen and hydrogen, but only in a moderate degree; but if, after the fusion and washing, it be dipped in the hot sulphuric acid (601.), it will become very active.

608. Other metals than platina were then experimented with. Gold and palladium exhibited the power either when made the positive pole of the voltaic battery (570.), or when acted on by hot oil of vitriol (601.). When palladium is used, the action of the battery or acid should be moderated, as that metal is soon acted upon. Silver and copper could not be made to show any effect at common temperatures.

609. There can remain no doubt that the property of inducing combination, which can thus be conferred upon masses of platina and other metals by connecting them with the poles of the battery, or by cleansing processes either of a mechanical or chemical nature, is the same as that which was discovered by Dobereiner*, in 1823, to belong in so eminent a degree to spongy platina, and which was afterwards so well experimented upon and illustrated by MM. Dulong and Thenard†, in 1823. The latter philosophers even quote experiments in which a very fine platina wire, which had been coiled up and digested in nitric, sulphuric, or muriatic acid, became ignited when put into a jet of hydrogen gas‡. This effect I can now produce at pleasure with either wires or plates by the processes described (570. 601. 605.); and by using a smaller plate cut so that it shall rest against the glass by a few points, and yet allow the water to flow off (fig. 4.), the loss of heat is less, the metal is assimilated somewhat to the spongy state, and the probability of failure almost entirely removed.

610. M. Dobereiner refers the effect entirely to an electric action. He considers the platina and hydrogen as forming a voltaic element of the ordinary kind, in which the hydrogen, being very highly positive, represents the zinc of the usual arrangement, and like it, therefore, attracts oxygen and combines with it§.

611. In the two excellent experimental papers by MM. Dulong and Thenard||, those philosophers show that elevation of temperature favours the action, but does not alter its character, Sir Humphry Davy's incandescent platina wire being the same phenomenon with Dobereiner's spongy platina. They show that *all* metals have this power in a greater or smaller degree, and that it is even possessed by such bodies as charcoal, pumice, porcelain, glass, rock crystal, &c., when their temperatures are raised; and that another of Davy's effects, in which oxygen and hydrogen had combined slowly together at a heat below ignition, was really dependent upon the pro-

* Annales de Chimie, tom. xxiv. p. 93.

† Ibid. tom. xxiii. p. 440.; tom. xxiv. p. 380.

‡ Ibid. tom. xxiv. p. 383.

§ Ibid. tom. xxiv. pp. 94, 95. Also Bibliothéque Universelle, tom. xxiv. p. 54.

|| Annales de Chimie, tom. xxiii. p. 440.; tom. xxiv. p. 380.

perty of the heated glass, which it has in common with the bodies named above. They state that liquids do not show this effect, at least that mercury, at or below the boiling point, has not the power; that it is not due to porosity; that the same body varies very much in its action, according to its state; and that many other gaseous mixtures besides oxygen and hydrogen are affected, and made to act chemically, when the temperature is raised. They think it probable that spongy platina acquires its power from contact with the acid evolved during its reduction, or from the heat itself to which it is then submitted.

612. MM. Dulong and Thenard express themselves with great caution on the theory of this action; but, referring to the decomposing power of metals on ammonia when heated to temperatures not sufficient alone to affect the alkali. They remark that those metals which in this case are most efficacious, are the least so in causing the combination of oxygen and hydrogen; whilst platina, gold, &c., which have least power of decomposing ammonia, have most power of combining the elements of water; from which they are led to believe, that amongst gases, some tend to unite under the influence of metals, whilst others tend to separate, and that this property varies in opposite directions with the different metals. At the close of their second paper they observe, that the action is of a kind that cannot be connected with any known theory; and though it is very remarkable that the effects are transient, like those of most electrical actions, yet they state that the greater number of the results observed by them are inexplicable, by supposing them to be of a purely electric origin.

613. Dr. Fusinieri has also written on this subject, and given a theory which he considers as sufficient to account for the phenomena*. He expresses the immediate cause thus: "The platina determines upon its surface a continual renovation of *concrete laminæ* of the combustible substance of the gases or vapours, which flowing over it, are burnt, pass away, and are renewed: this combustion at the surface raises and sustains the temperature of the metal." The combustible substance, thus reduced into imperceptible laminæ, of which the concrete parts are in contact with the oxygen, is presumed to be in a state combinable with the oxygen at a much lower temperature than when it is in the gaseous state, and more in analogy with what is called the nascent condition. That combustible gases should lose their elastic state, and become concrete, assuming the form of exceedingly attenuated but solid strata, is considered as proved by facts, some of which are quoted in the Giornale di Fisica for 1824†; and though the theory requires that they should assume this state at high temperatures, and though the *similar* films of aqueous and other matter are dissipated by the action of heat, still the facts are considered as justifying the conclusion against all opposition of reasoning.

614. The power or force which makes combustible gas or vapour abandon its elastic state in contact with a solid, that it may cover the latter with a thin stratum of its own proper substance, is considered as being neither attraction nor affinity. It

* Giornale di Fisica, &c., 1825, tom. viii. p. 259.

† pp. 138, 371.

is able also to extend liquids and solids in concrete laminæ over the surface of the acting solid body, and consists in a *repulsion*, which is developed from the parts of the solid body by the simple fact of attenuation, and is highest when the attenuation is most complete. The force has a progressive development, and acts most powerfully, or at first, in the direction in which the dimensions of the attenuated mass decrease, and then in the direction of the angles or corners which from any cause may exist on the surface. This force not only causes spontaneous diffusion of gases and other substances over the surface, but is considered as very elementary in its nature, and competent to account for all the phenomena of capillarity, chemical affinity, attraction of aggregation, rarefaction, ebullition, volatilization, explosion, and other thermometric effects, as well as inflammation, detonation, &c. &c. It is considered as a form of heat, to which the term *native caloric* is given, and is still further viewed as the principle of the two electricities and the two magnetisms.

615. I have been the more anxious to give a correct abstract of Dr. Fusinieri's view, both because I cannot form a distinct idea of the power to which he refers the phenomena, and because of my imperfect knowledge of the language in which the memoir is written. I would therefore beg to refer those who pursue the subject to the memoir itself.

616. Not feeling, however, that the problem has yet been solved, I venture to give the view which seems to me sufficient, upon known principles, to account for the effect.

617. It may be observed of this action, that, with regard to platina, it cannot be due to any peculiar, temporary condition, either of an electric or of any other nature: the activity of plates rendered either positive or negative by the pole, or cleaned with such different substances as acids, alkalies, or water; charcoal, emery, ashes, or glass; or merely heated, is sufficient to negative such an opinion. Neither does it depend upon the spongy and porous, or upon the compact and burnished, or upon the massive or the attenuated state of the metal, for in any of these states it may be rendered effective, or its action may be taken away. The only essential condition appears to be a *perfectly clean* and *metallic surface*, for whenever that is provided the platina acts, whatever its form and condition in other respects may be; and though variations in the latter points will very much affect the rapidity, and therefore the visible appearances and secondary effects, of the action, i. e. the ignition of the metal and the inflammation of the gases, they, even in their most favourable state, cannot produce any effect unless the condition of a clean, pure, metallic surface be also fulfilled.

618. The effect is evidently produced by most, if not all, solid bodies, weakly perhaps by many of them, but rising to a high degree in platina. Dulong and Thenard have very philosophically extended our knowledge of the property to its possession by all the metals, and by earths, glass, stones, &c. (611.); and every idea of its being a known and recognised electric action is in this way removed.

619. All the phenomena connected with this subject press upon my mind the conviction that the effects in question are entirely incidental and of a secondary nature;

that they are dependent upon the *natural conditions* of gaseous elasticity combined with the exertion of that attractive force, possessed by many bodies in an eminent degree, and probably belonging to all, by which they are drawn into association more or less close, without at the same time undergoing chemical combination, though often assuming the condition of adhesion; and which occasionally leads, under very favourable circumstances, as in the present instance, to the combination of bodies simultaneously subjected to this attraction. I am prepared myself to admit (and probably many others are of the same opinion), both with respect to the attraction of aggregation and of chemical affinity, that the sphere of action of particles extends beyond those other particles with which they are immediately and evidently in union, and in many cases produces effects rising into considerable importance: and I think that this kind of attraction is a determining cause of Dobereiner's effect, and of the many others of a similar nature.

620. Bodies which become wetted by fluids with which they do not combine chemically, or in which they do not dissolve, are simple and well known instances of this kind of attraction.

621. All those cases of bodies which being insoluble in water and not combining with it are hygrometric, and condense its vapour around or upon their surface, are stronger instances of the same power, and approach a little nearer to the cases under investigation. If pulverised clay, protoxide or peroxide of iron, oxide of manganese, charcoal, or even metals, as spongy platina or precipitated silver, be put into an atmosphere containing vapour of water, they soon become moist by virtue of an attraction which is able to condense the vapour upon, although not to combine it with, the substances; and if, as is well known, these bodies so damped be put into a dry atmosphere, as, for instance, one confined over sulphuric acid, or if they be heated, then they yield up this water again almost entirely, it not being in direct or permanent combination*.

622. Still better instances of the power I refer to, because they are more analogous to the cases to be explained, are furnished by the attraction existing between glass and air, so well known to barometer and thermometer makers, for here the adhesion or attraction is exerted between a solid and gases, bodies having very different physical conditions, having no power of combination with each other, and each retaining, during the time of action, its physical state unchanged†. When mercury is poured into a barometer tube, a film of air will remain between the metal and glass for months, or, as far as is known, for years, for it has never been displaced except by the

* I met at Edinburgh with a remarkable case as to its extent of hygrometric action, assisted a little perhaps by very slight solvent power. Some turf had been well dried by long exposure in a covered place to the atmosphere, but being then submitted to the action of a hydrostatic press, it yielded, *by the mere influence of the pressure*, 54 per cent. of water.

† Fusinieri and Bellani consider the air as forming solid concrete films in these cases.—Giornale di Fisica, tom. viii. p. 262. 1825.

action of means especially fitted for the purpose. These consist in boiling the mercury, or, in other words, of forming an abundance of vapour, which coming in contact with every part of the glass and every portion of surface of the mercury, gradually mingles with, dilutes, and carries off the air attracted by, and adhering to, those surfaces, replacing it by other vapour, subject to an equal or perhaps greater attraction, but which when cooled condenses into the same liquid as that with which the tube is filled.

623. Extraneous bodies, which, acting as nuclei in crystallizing or depositing solutions, cause deposition of substances on them, when it does not occur elsewhere in the liquid, seem to produce their effects by a power of the same kind, i. e. a power of attraction extending to neighbouring particles, and causing them to become attached to the nuclei, although it is not strong enough to make them combine chemically with their substance.

624. It would appear from many cases of nuclei in solutions, and from the effects of bodies put into atmospheres containing the vapours of water, or camphor, or iodine, &c., as if this attraction were in part elective, partaking in its characters both of the attraction of aggregation and chemical affinity: nor is this inconsistent with, but agreeable to, the idea entertained, that it is the power of particles acting, not upon others with which they can immediately and intimately combine, but upon such as are either more distantly situated with respect to them, or which, from previous condition, physical constitution, or feeble relation, are unable to enter into decided union with them.

625. Then, of all bodies, the gases are those which might be expected to show some *mutual* action whilst *jointly* under the attractive influence of the platina or other solid acting substance. Liquids, such as water, alcohol, &c., are in so dense and comparatively incompressible a state, as to favour no expectation that their particles should approach much closer to each other by the attraction of the body to which they adhere, and yet that attraction must (according to its effects) place their particles as near to those of the solid wetted body as they are to each other, and in many cases it is evident that the former attraction is the stronger. But gases and vapours are bodies competent to suffer very great changes in the relative distances of their particles by external agencies; and where they are in immediate contact with the platina, the approximation of the particles to those of the metal may be very great. In the case of the hygrometric bodies referred to (621.), it is sufficient to reduce the vapour to the fluid state, frequently from atmospheres so rare that without this influence it would be needful to compress them by mechanical force into a bulk not more than $\frac{1}{10}$th or even $\frac{1}{20}$th of their original volume before the vapours would become liquids.

626. Another most important consideration in relation to this action of bodies, and which, as far as I am aware, has not hitherto been noticed, is the condition of elasticity under which the gases are placed against the acting surface. We have but very imperfect notions of the real and intimate conditions of the particles of a body exist-

ing in the solid, the liquid, and the gaseous state; but when we speak of the gaseous state as being due to the mutual repulsions of the particles or of their atmospheres, although we may err in imagining each particle to be a little nucleus to an atmosphere of heat, or electricity, or any other agent, we are still not likely to be in error in considering the elasticity as dependent on *mutuality* of action. Now this mutual relation fails altogether on the side of the gaseous particles next to the platina, and we might be led to expect *à priori* a deficiency of elastic force there to at least one half; for if, as Dalton has shown, the elastic force of the particles of one gas cannot act against the elastic force of the particles of another, the two being as vacua to each other, so is it far less likely that the particles of the platina can exert any influence on those of the gas against it, such as would be exerted by gaseous particles of its own kind.

627. But the diminution of power to one half on the side of the gaseous body towards the metal is only a slight result of what seems to me to flow as a necessary consequence of the known constitution of gases. An atmosphere of one gas or vapour, however dense or compressed, is in effect as a vacuum to another: thus, if a little water were put into a vessel containing a dry gas, as air, of the pressure of one hundred atmospheres, as much vapour of the water would *rise* as if it were in a perfect vacuum. Here the particles of watery vapour appear to have no difficulty in approaching within any distance of the particles of air, being influenced solely by relation to particles of their own kind; and if it be so with respect to a body having the same elastic powers as itself, how much more surely must it be so with particles, like those of the platina, or other limiting body, which, at the same time that they have not these elastic powers, are also unlike it in nature. Hence it would seem to result that the particles of hydrogen or any other gas or vapour which are next to the platina, &c., must be in such contact with it as if they were in the liquid state, and therefore almost infinitely closer to it than they are to each other, even though the metal be supposed to exert no attractive influence over them.

628. A third and very important consideration in favour of the mutual action of gases under these circumstances is their perfect miscibility. If fluid bodies capable of combining together are also capable of mixture, *they do combine* when they are mingled, not waiting for any other determining circumstance; but if two such gases as oxygen and hydrogen are put together, though they are elements having such powerful affinity as to unite naturally under a thousand different circumstances, they do not combine by mere mixture. Still it is evident that, from their perfect association, the particles are in the most favourable state possible for combination, upon the supervention of any determining cause, such either as the negative action of the platina in suppressing or annihilating, as it were, their elasticity on its side; or the positive action of the metal in condensing them against its surface by an attractive force; or the influence of both together.

629. Although there are not many distinct cases of combination under the influence of forces external to the combining particles, yet there are sufficient to remove

any difficulty which might arise on that ground. Sir JAMES HALL found carbonic acid and lime to remain combined under pressure at temperatures at which they would not have remained combined if the pressure had been removed; and I have had occasion to observe a case of direct combination in chlorine*, which being compressed at common temperatures will combine with water, and form a definite crystalline hydrate, incapable either of being formed or of existing if that pressure be removed.

630. The course of events when platina acts upon, and combines oxygen and hydrogen, may be stated, according to these principles, as follows. From the influence of the circumstances mentioned (619. &c.), i. e. the deficiency of elastic power and the attraction of the metal for the gases, the latter, when they are in association with the former, are so far condensed as to be brought within the action of their mutual affinities at the existing temperature; the deficiency of elastic power, not merely subjecting them more closely to the attractive influence of the metal, but also bringing them into a more favourable state for union, by abstracting a part of that power (upon which depends their elasticity,) which elsewhere in the mass of gases is opposing their combination. The consequence of their combination is the production of the vapour of water and an elevation of temperature. But as the attraction of the platina for the water formed is not greater than for the gases, if so great, (for the metal is scarcely hygrometric,) the vapour is quickly diffused through the remaining gases; fresh portions of the latter, therefore, come into juxtaposition with the metal, combine, and the vapour formed is also diffused, allowing new portions of gas to be acted upon. In this way the process advances, but is accelerated by the evolution of heat, which is known by experiment to facilitate the combination in proportion to its intensity, and the temperature is thus gradually exalted until ignition results.

631. The dissipation of the vapour produced at the surface of the platina, and the contact of fresh oxygen and hydrogen with the metal, form no difficulty in this explication. The platina is not considered as causing the combination of any particles with itself, but only associating them closely around it; and the compressed particles are as free to move from the platina, being replaced by other particles, as a portion of dense air upon the surface of the globe, or at the bottom of a deep mine, is free to move by the slightest impulse into the upper and rarer parts of the atmosphere.

632. It can hardly be necessary to give any reasons why platina does not show this effect under ordinary circumstances. It is then not sufficiently clean (617.), and the gases are prevented from touching it, and suffering that degree of effect which is needful to commence their combination at common temperatures, and which they can only experience at its surface. In fact, the very power which causes the combination of oxygen and hydrogen is competent, under the usual casual exposure of platina, to condense extraneous matters upon its surface, which soiling it, take away for the time its power of combining oxygen and hydrogen by preventing their contact with it.

* Philosophical Transactions, 1823, p. 161.

633. Clean platina, by which I mean such as has been made the positive pole of a pile (570.), or has been treated with acid (605.), and has then been put into distilled water for twelve or fifteen minutes, has a peculiar friction when one piece is rubbed against another. It wets freely with pure water, even after it has been shaken and dried by the heat of a spirit-lamp; and if made the pole of a voltaic pile in a dilute acid, it evolves minute bubbles from every part of its surface. But platina in its common state wants that peculiar friction: it will not wet freely with water as the clean platina does; and when made the positive pole of a pile, it for a time gives off large bubbles, which seem to cling or adhere to the metal, and are evolved at distinct and separate points of the surface. These appearances and effects, as well as its want of power on oxygen and hydrogen, are the consequences, and the indications, of a soiled surface.

634. I found also that platina plates which had been cleaned perfectly soon became soiled by mere exposure to the air; for after twenty-four hours they no longer moistened freely with water, but the fluid ran up into portions, leaving part of the surface bare, whilst other plates which had been retained in water for the same time, when they were dried (580.) did moisten, and gave the other indications of a clean surface.

635. Nor was this the case with platina or metals only, but also with earthy bodies. Rock crystal and obsidian would not wet freely upon the surface, but being moistened with strong oil of vitriol, then washed, and left in distilled water to remove all the acid, they did freely become moistened, whether they were previously dry or whether they were left wet; but being dried and left exposed to the air for twenty-four hours, their surface became so soiled that water would not then adhere freely to it, but ran up into partial portions. Wiping with a cloth (even the cleanest) was still worse than exposure to air; the surface either of the minerals or metals immediately became as if it were slightly greasy. The floating of small particles of metals under ordinary circumstances is due to the effect of this kind of soiled surface. The extreme difficulty of cleaning the surface of mercury when it has once been soiled or greased, is due to the same cause.

636. The same reasons explain why the power of the platina plates in some circumstances soon disappear, and especially upon use, and MM. Dulong and Thenard have observed the same effect with the spongy metal*, as indeed have all those who have used Dobereiner's instantaneous light machines. If left in the air, if put into ordinary distilled water, if made to act upon ordinary oxygen and hydrogen, they can still find in all these cases *that* minute portion of impurity which, when once in contact with its surface, is retained there, and is sufficient to prevent its full action upon oxygen and hydrogen at common temperatures: a slight elevation of temperature is again sufficient to compensate for their effect, and cause combination.

637. No state of things can be conceived more favourable for the production of this effect than that which is possessed by platina obtained from the ammonio-muriate by

* Annales de Chimie, tom. xxiv. p. 386.

heat: its surface is most extensive and pure, yet very accessible to the gases brought in contact with it. If placed in impurity, the interior, as Thenard and Dulong have observed, is preserved clean by the exterior; and as regards heat, it is so bad a conductor, because of its divided condition, that almost all which is evolved by the combination of the first portions of gas is retained within the mass, exalting the tendency of the succeeding portions to combine.

638. I have now to notice some very extraordinary interferences with this phenomenon, dependent, not upon the nature or condition of the metal, or other acting solid, but upon the presence of certain substances mingled with the gases acted upon; and as I shall have occasion to speak frequently of a mixture of oxygen and hydrogen, I wish it always to be understood that I mean a mixture composed of one volume oxygen to two volumes of hydrogen, being the proportions that form water. Unless otherwise expressed, the hydrogen was always that obtained by the action of dilute sulphuric acid on pure zinc, and the oxygen that obtained by the action of heat from the chlorate of potassa.

639. Mixtures of oxygen and hydrogen with *air*, containing one fourth, one half, and even two thirds of the latter, being introduced with prepared platina plates (570. 605.) into tubes, were acted upon almost as well as if no air were present: the retardation was far less than might have been expected from the mere dilution and consequent obstruction to the access of gas. In two hours and a half nearly all the oxygen and hydrogen introduced as mixture was gone.

640. But when similar experiments were made with *olefiant gas* (the platina plates having been made the positive poles of a voltaic pile (570.) in acid), very different results occurred. A mixture was made of 29·2 volumes hydrogen and 14·6 volumes oxygen, being the proportions for water; and to this was added another mixture of 3 volumes oxygen and 1 volume olefiant gas, so that the olefiant gas formed but $\frac{1}{48}$th part of the whole; yet in this mixture the platina plate would not act in forty-five hours. The failure was not for want of any power in the plate, for when after that time it was taken out of this mixture and put into one of oxygen and hydrogen, it immediately acted, and in seven minutes caused explosion of the gas. This result was obtained several times, and when larger proportions of olefiant gas were used the action seemed still more hopeless.

641. A mixture of forty-nine volumes oxygen and hydrogen (638.) with one volume of olefiant gas had a well-prepared platina plate introduced. The diminution of gas was scarcely sensible at the end of two hours, during which it was watched; but on examination twenty-four hours afterwards, the tube was found blown to pieces. The action, therefore, though it had been very much retarded, had occurred at last, and risen to a maximum.

642. With a mixture of ninety-nine volumes of oxygen and hydrogen (638.) with one of olefiant gas, a feeble action was evident at the end of fifty minutes; it went on accelerating (630.) until the eighty-fifth minute, and then became so intense that the

gas exploded. Here also the retarding effect of the olefiant gas was very beautifully illustrated.

643. Plates prepared by alkali and acid (605.) produced corresponding effects.

644. It is perfectly clear from these experiments, that *olefiant gas*, even in small quantities, has a very remarkable influence in preventing the combination of oxygen and hydrogen under these circumstances, and yet without at all injuring or affecting the power of the platina.

645. Another striking illustration of similar interference may be shown in *carbonic oxide*, especially if contrasted with *carbonic acid*. A mixture of one volume oxygen and hydrogen (638.) with four volumes of carbonic acid was affected at once by a platina plate prepared with acid, &c. (605.), and in one hour and a quarter nearly all the oxygen and hydrogen was gone. Mixtures containing less carbonic acid were still more readily affected.

646. But when carbonic oxide was substituted for the carbonic acid, not the slightest effect of combination was produced; and when the carbonic oxide was only one eighth of the whole volume, no action occurred in forty and fifty hours. Yet the plates had not lost their power; for being taken out and put into pure oxygen and hydrogen, they acted well and at once.

647. Two volumes of carbonic oxide and one of oxygen were mingled with nine volumes of oxygen and hydrogen (638.). This mixture was not affected by a plate which had been made positive in acid, though it remained in it fifteen hours. But when to the same volumes of carbonic oxide and oxygen were added thirty-three volumes of oxygen and hydrogen, the carbonic oxide being then only $\frac{1}{18}$th part of the whole, the plate acted, slowly at first, and at the end of forty-two minutes the gases exploded.

648. These experiments were extended to various gases and vapours, the general results of which may be given as follow. Oxygen, hydrogen, nitrogen, and nitrous oxide, when used to dilute the mixture of oxygen and hydrogen, did not prevent the action of the plates even when they made up four fifths of the whole volume of gas acted upon. Nor was the retardation so great in any case as might have been expected from the mere dilution of the oxygen and hydrogen, and the consequent mechanical obstruction to its contact with the platina. The order in which carbonic acid and these substances seemed to stand was as follows, the first interfering least with the action; *nitrous oxide, hydrogen, carbonic acid, nitrogen, oxygen*: but it is possible the plates were not equally well prepared in all, and that other circumstances also were unequal; consequently more numerous experiments would be required to establish the order accurately.

649. As to cases of *retardation*, the powers of olefiant gas and carbonic oxide have been already described. Mixtures of oxygen and hydrogen, containing from $\frac{1}{16}$th to $\frac{1}{20}$th of sulphuretted hydrogen or phosphuretted hydrogen, seemed to show a little action at first, but were not further affected by the prepared plates, though in contact

with them for seventy hours. When the plates were removed they had lost all power over pure oxygen and hydrogen, and the interference of these gases was therefore of a different nature from that of the two former, having permanently affected the plate.

650. A small piece of cork was dipped in sulphuret of carbon and passed up through water into a tube containing oxygen and hydrogen (638.), so as to diffuse a portion of its vapour through the gases. A plate being introduced appeared at first to act a little, but after sixty-one hours the diminution was very small. Upon putting the same plate into a pure mixture of oxygen and hydrogen it acted at once and powerfully, having apparently suffered no diminution of its force.

651. A little vapour of ether being mixed with the oxygen and hydrogen retarded the action of the plate, but did not prevent it altogether. A little of the vapour of the condensed oil-gas liquor* retarded the action still more, but not nearly so much as an equal volume of olefiant gas would have done. In both these cases it was the original oxygen and hydrogen which combined together, the ether and the oil-gas vapour remaining unaffected, and in both cases the plates retained the power of acting on fresh oxygen and hydrogen.

652. Spongy platina was then used in place of the plates, and jets of hydrogen mingled with the different gases thrown against it in air. The results were exactly of the same kind, although presented occasionally in a more imposing form. Thus, mixtures of one volume of olefiant gas or carbonic oxide with three of hydrogen could not heat the spongy platina when the experiments were commenced at common temperatures; but a mixture of equal volumes of nitrogen and hydrogen acted very well, causing ignition. With carbonic acid the results were still stronger. A mixture of three volumes of that gas with one of hydrogen caused ignition of the platina, yet that mixture would not continue to burn from the jet when attempts were made to light it by a taper. A mixture even of *seven* volumes of carbonic acid and *one* of hydrogen will thus cause the ignition of cold spongy platina, and yet, as if to supply a contrast, than which none can be greater, it cannot burn at a taper, but causes the extinction of the latter. On the other hand, the mixtures of carbonic oxide or olefiant gas, which can do nothing with the platina, are inflamed by the taper, burning well.

653. Hydrogen mingled with the vapour of ether or oil-gas liquor causes the ignition of the spongy platina. The mixture with oil-gas burns with a flame far brighter than that of the mixture of hydrogen and olefiant gas already referred to, so that it would appear that the retarding action of the hydro-carbons is not at all in proportion merely to the quantity of carbon present.

654. In connexion with these interferences, I must state that hydrogen itself, prepared from steam passed over ignited iron, was found when mingled with oxygen to resist the action of platina. It had stood over water seven days, and had lost all fetid smell; but a jet of it would not cause the ignition of spongy platina, commencing at common temperatures; nor would it combine with oxygen in a tube either under

* Philosophical Transactions, 1825, p. 440.

the influence of a prepared plate or of spongy platina. A mixture of one volume of this gas with three of pure hydrogen, and the due proportion of oxygen, was not affected by plates after fifty hours. I am inclined to refer the effect to carbonic oxide present in the gas, but have not had time to verify the suspicion. The power of the plates was not destroyed (640. 646.).

655. Such are the general facts of these remarkable interferences. Whether the effect produced by such small quantities of certain gases depends upon any direct action which they may exert upon the particles of oxygen and hydrogen, by which the latter are rendered less inclined to combine, or whether it depends upon their modifying the action of the plate temporarily (for they produce no real change on it), by investing it through the agency of a stronger attraction than that of the hydrogen, or otherwise, remains to be decided by more extended experiments.

656. The theory of action which I have given for the original phenomena appears to me quite sufficient to account for all the effects by reference to known properties, and dispenses with the assumption of any new power of matter. I have pursued this subject at some length, as one of great consequence, because I am convinced that the superficial actions of matter, whether between two bodies, or of one piece of the same body, and the actions of particles not directly or strongly in combination, are becoming daily more and more important to our theories of chemical as well as mechanical philosophy*. In all ordinary cases of combustion it is evident that an action of the kind, considered either upon the surface of the carbon in the fire, or that in the bright part of a flame, must have great influence over the combinations there taking place.

657. The condition of elasticity upon the exterior of the gaseous or vaporous mass already referred to (626. 627.), must be connected directly with the action of solid bodies as nuclei on vapours, causing condensation upon them in preference to any condensation in the vapours themselves; and in the well-known effect of nuclei on solutions a similar condition may have existence (623.), for an analogy in condition exists between the parts of a body in solution, and those of a body in the vaporous or gaseous state. This thought leads us to the consideration of what are the respective conditions at the surfaces of contact of two portions of the same substance at the same temperature, one in the solid or liquid, and the other in the vaporous state; as, for instance, steam and water. It would seem that the particles of vapour next to the particles of liquid are in a different relation to the latter to what they would be with respect to

* As a curious illustration of the influence of mechanical forces over chemical affinity, I will quote the refusal of certain substances to effloresce when their surfaces are perfect, which yield immediately upon the surface being broken. If crystals of carbonate of soda, or phosphate of soda, or sulphate of soda, having no part of their surfaces broken, be preserved from external violence, they will not effloresce. I have thus retained crystals of carbonate of soda perfectly transparent and unchanged from September 1827 to January 1833; and crystals of sulphate of soda from May 1832 to the present time, November 1833. If any part of the surface were scratched or broken, then efflorescence began at that part, and covered the whole. The crystals were merely placed in evaporating basins and covered with paper.

any other liquid or solid substance; as, for instance, mercury or platina, if they were made to replace the water, i. e. if the view of independent action which I have taken (626. 627.) as a consequence of DALTON's principles be correct. It would also seem that the mutual relation of similar particles, and the indifference of dissimilar particles which DALTON has established as a matter of fact amongst gases and vapours, extends to a certain degree amongst solids and fluids, that is, when they are in relation by contact with vapours, either of their own substance or of other bodies. But though I view these points as of great importance with respect to the relations existing between different substances and their physical constitution in the solid, liquid, or gaseous state, I have not sufficiently considered them to venture any strong opinions or statements here.

658. There are numerous well-known cases in which substances, such as oxygen and hydrogen, act readily in their *nascent* state, and produce chemical changes which they are not able to effect if once they have assumed the gaseous condition. Such instances are very common at the poles of the voltaic pile, and are, I think, easily accounted for, if it be considered that at the moment of separation of any such particle it is entirely surrounded by other particles of a *different* kind with which it is in close contact, and has not yet assumed those relations and conditions which it has in its fully developed state, and which it can only assume by association with other particles of its own kind. For, at the moment, its elasticity is absent, and it is in the same relation to particles with which it is in contact, and for which it has an affinity, as the particles of oxygen and hydrogen are to each other on the surface of clean platina (626. 627.).

659. The singular effects of retardation produced by very small quantities of some gases, and not by large quantities of others (640. 645. 652.), if dependent upon any relation of the added gas to the surface of the solid, will then probably be found immediately connected with the curious phenomena which are presented by different gases when passing through narrow tubes at low pressures, which I observed many years ago*; and this action of surfaces must, I think, influence the highly interesting phenomena of the diffusion of gases, at least in the form in which it has been experimented upon by Mr. GRAHAM in 1829 and 1831†, and also by Dr. MITCHELL of Philadelphia‡ in 1830. It seems very probable that if such a substance as spongy platina were used, another law for the diffusion of gases under the circumstances would come out than that obtained by the use of plaster of Paris.

660. I intended to have followed this section by one on the secondary piles of RITTER, and the peculiar properties of the poles of the pile, or of metals through which electricity has passed, which have been observed by RITTER, VAN MARUM, YELIN, DE LA RIVE, MARIANINI, BERZELIUS, and others. It appears to me that all these

* Quarterly Journal of Science, 1819, vol. vii. p. 106.

† Quarterly Journal of Science, vol. xxviii. p. 74., and Edinburgh Transactions, 1831.

‡ Journal of the Royal Institution for 1831, p. 101.

phenomena bear a satisfactory explanation on known principles, connected with the investigation just terminated, and do not require the assumption of any new state or new property. But as the experiments advanced, especially those of Marianini, require very careful repetition and examination, the necessity of pursuing the subject of electro-chemical decomposition obliges me for a time to defer the researches to which I have just referred.

Royal Institution,
November 30, 1833.

VI. *Experimental Researches in Electricity.—Seventh Series.* *By* MICHAEL FARADAY, *D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.*

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§. 11. *On Electro-chemical Decomposition, continued.* ¶ iv. *On some general conditions of Electro-decomposition.* ¶ v. *On a new Measurer of Volta-electricity.* ¶ vi. *On the primitive or secondary character of bodies evolved in Electro-decomposition.* ¶ vii. *On the definite nature and extent of Electro-chemical Decompositions.* §. 13. *On the absolute quantity of Electricity associated with the particles or atoms of Matter.*

Preliminary.

661. THE theory which I believe to be a true expression of the facts of electro-chemical decomposition, and which I have therefore detailed in a former series of these Researches, is so much at variance with those previously advanced, that I find the greatest difficulty in stating results, as I think, correctly, whilst limited to the use of terms which are current with a certain accepted meaning. Of this kind is the term pole, with its prefixes of positive and negative, and the attached ideas of attraction and repulsion. The general phraseology is that the positive pole *attracts* oxygen, acids, &c., or more cautiously, that it *determines* their evolution upon the surface; and that the negative pole acts in an equal manner upon hydrogen, combustibles, metals, and bases. According to my view, the determining force is *not* at the poles, but *within* the decomposing body; and the oxygen and acids are rendered at the *negative* extremity of that body, whilst hydrogen, metals, &c., are evolved at the *positive* extremity (518. 524.).

662. To avoid, therefore, confusion and circumlocution, and for the sake of greater precision of expression than I can otherwise obtain, I have deliberately considered the subject with two friends, and with their assistance and concurrence in framing them, I purpose henceforward using certain other terms, which I will now define. The poles, as they are usually called, are only the doors or ways by which the electric current passes into and out of the decomposing body (556.); and they of course, when in contact with that body, are the limits of its extent in the direction of the current. The term has been generally applied to the metal surfaces in contact with the decomposing substance; but whether philosophers generally would also apply it to the

surfaces of air (465. 471.) and water (493.), against which I have effected electro-chemical decomposition, is subject to doubt. In place of the term pole, I propose using that of *Electrode**, and I mean thereby that substance, or rather surface, whether of air, water, metal, or any other body, which bounds the extent of the decomposing matter in the direction of the electric current.

663. The surfaces at which, according to the common phraseology, the electric current enters and leaves a decomposing body, are most important places of action, and require to be distinguished apart from the poles, with which they are mostly, and the electrodes, with which they are always, in contact. Wishing for a natural standard of electric direction to which I might refer these, expressive of their difference and at the same time free from all theory, I have thought it might be found in the earth. If the magnetism of the earth be due to electric currents passing round it, the latter must be in a constant direction, which, according to present usage of speech, would be from east to west, or, which will strengthen this help to the memory, that in which the sun appears to move. If in any case of electro-decomposition we consider the decomposing body as placed so that the current passing through it shall be in the same direction, and parallel to that supposed to exist in the earth, then the surfaces at which the electricity is passing into and out of the substance would have an invariable reference, and exhibit constantly the same relations of powers. Upon this notion we purpose calling that towards the east the *anode*†, and that towards the west the *cathode*‡; and whatever changes may take place in our views of the nature of electricity and electrical action, as they must affect the natural standard referred to in the same direction, and to an equal amount with any decomposing substances to which these terms may at any time be applied, there seems no reason to expect that they will lead to confusion, or tend in any way to support false views. The *anode* is therefore that surface at which the electric current, according to our present expression, enters: it is the negative extremity of the decomposing body; is where oxygen, chlorine, acids, &c., are evolved; and is against or opposite the positive electrode. The *cathode* is that surface at which the current leaves the decomposing body, and is its positive extremity; the combustible bodies, metals, alkalies, and bases, are evolved there, and it is in contact with the negative electrode.

664. I shall have occasion in these Researches, also, to class bodies together according to certain relations derived from their electrical actions (822.); and wishing to express those relations without at the same time involving the expression of any hypothetical views, I intend using the following names and terms. Many bodies are decomposed directly by the electric current, their elements being set free; these I propose to call *electrolytes*§. Water, therefore, is an electrolyte. The bodies which,

* ἤλεκτρον, and ὁδος *a way.*

† ἀνα *upwards,* ὁδος *a way*; the way which the sun rises.

‡ κατα *downwards,* ὁδος *a way*; the way which the sun sets.

§ ἤλεκτρον, and λυω *solvo.* N. Electrolyte, V. Electrolyze.

like nitric or sulphuric acids, are decomposed in a secondary manner (752. 757.), are not included under this term. Then for *electro-chemically decomposed,* I shall often use the term *electrolyzed,* derived in the same way, and implying that the body spoken of is separated into its components under the influence of electricity: it is analogous in its sense and sound to *analyze,* which is derived in a similar manner. The term *electrolytical* will be understood at once. Muriatic acid is electrolytical, boracic acid is not.

665. Finally, I require a term to express those bodies which can pass to the *electrodes,* or, as they are usually called, the poles. Substances are frequently spoken of as being *electro-negative,* or *electro-positive,* according as they go under the supposed influence of a direct attraction to the positive or negative pole. But these terms are much too significant for the use to which I should have to put them; for though the meanings are perhaps right, they are only hypothetical, and may be wrong; and then, through a very imperceptible, but still very dangerous, because continual, influence, they do great injury to science, by contracting and limiting the habitual views of those engaged in pursuing it. I propose to distinguish these bodies by calling those *anions** which go to the *anode* of the decomposing body; and those passing to the *cathode, cations*†; and when I have occasion to speak of these together, I shall call them *ions.* Thus, the chloride of lead is an *electrolyte,* and when *electrolyzed* evolves the two *ions,* chlorine and lead, the former being an *anion,* and the latter a *cation.*

666. These terms being once well defined, will, I hope, in their use enable me to avoid much periphrasis and ambiguity of expression. I do not mean to press them into service more frequently than will be required, for I am fully aware that names are one thing and science another‡.

667. It will be well understood that I am giving no opinion respecting the nature of the electric current now, beyond what I have done on a former occasion (283. 517.); and that though I speak of the current as proceeding from the parts which are positive to those which are negative (663.), it is merely in accordance with the conventional, though in some degree tacit, agreement entered into by scientific men, that they may have a constant, certain, and definite means of referring to the direction of the forces of that current.

¶ iv. *On some general conditions of Electro-chemical Decomposition.*

669. From the period when electro-chemical decomposition was first effected to the present time, it has been a remark, that those elements which, in the ordinary phenomena of chemical affinity, were the most directly opposed to each other, and combined with the greatest attractive force, were those which were the most readily evolved at the opposite extremities of the decomposing bodies (549.).

* ἀνιον *that which goes up.* (Neuter participle.) † κατιον *that which goes down.*

‡ Since this paper was read, I have changed some of the terms which were first proposed, that I might employ only such as were at the same time simple in their nature, clear in their reference, and free from hypothesis.

670. If this result was evident when water was supposed to be essential to, and was present, in almost every case of such decomposition (472.), it is far more evident now that it has been shown and proved that water is not necessarily concerned in the phenomena (474.), and that other bodies much surpass it in some of the effects supposed to be peculiar to that substance.

671. Water, from its constitution and the nature of its elements, and from its frequent presence in cases of electrolytic action, has hitherto stood foremost in this respect. Though a compound formed by very powerful affinity, it yields up its elements under the influence of a very feeble electric current; and it is doubtful whether a case of electrolyzation can occur, where, being present, it is not resolved into its first principles.

672. The various oxides, chlorides, iodides, and salts (402.), which I have shown are decomposable by the electric current when in the liquid state, under the same general law with water, illustrate in an equally striking manner the activity, in such decompositions, of elements directly and powerfully opposed to each other by their chemical relations.

673. On the other hand, bodies dependent on weak affinities very rarely give way. Take, for instance, glasses: many of those formed of silica, lime, alkali, and oxide of lead, may be considered as little more than solutions of substances one in another*. If bottle-glass be fused, and subjected to the voltaic pile, it does not appear to be at all decomposed (408.). If flint-glass, which contains substances more directly opposed, be operated upon, it suffers some decomposition; and if borate of lead glass, which is a definite chemical compound, be experimented with, it readily yields up its elements (408).

674. But the result which is found to be so striking in the instances quoted is not at all borne out by reference to other cases where a similar consequence might have been expected. It may be said, that my own theory of electro-chemical decomposition would lead to the expectation that all compound bodies should give way under the influence of the electric current with a facility proportionate to the strength of the affinity by which their elements, either proximate or ultimate, are combined. I am not sure that that follows as a consequence of the theory; but if the objection be supposed one presented by facts, I have no doubt it will be removed when we obtain a more intimate acquaintance with, and precise idea of, the nature of chemical affinity and the mode of action of an electric current over it (518. 524.): besides which, it is just as directly opposed to any other theory of electro-chemical decomposition as the one I have propounded; for if it be admitted, as is generally the case, that the more directly bodies are opposed to each other in their attractive forces, the more powerfully do they combine, then the objection applies with equal force to any of the theories of electrolyzation which have been considered, and is an addition to those which I have taken against them.

* Philosophical Transactions, 1830, p. 49.

675. Amongst powerful compounds which are not decomposed, boracic acid stands prominent (408.). Then again, the iodide of sulphur, and the chlorides of sulphur, phosphorus, and carbon, are not decomposable under common circumstances, though their elements are of a nature which would lead to a contrary expectation. Chloride of antimony (402. 690.), the hydro-carbons, acetic acid, ammonia, and many other bodies undecomposable by the voltaic pile, would seem to be formed by an affinity sufficiently strong to indicate that the elements were so far contrasted in their nature as to sanction the expectation that the pile would separate them, especially as in some cases of mere solution (530. 544.), where the affinity must by comparison be very weak, separation takes place*.

676. It must not be forgotten, however, that much of this difficulty, and perhaps the whole, may depend upon the absence of conducting power, which, preventing the transmission of the current, prevents of course the effects due to it. All known compounds being non-conductors when solid, but conductors when liquid, are decomposed, with *perhaps* the single exception at present known of periodide of mercury (679. 691.); and even water itself, which so easily yields up its elements when the current passes, if rendered quite pure, scarcely suffers change, because it then becomes a very bad conductor.

677. If it should hereafter be proved that the want of decomposition in those cases where, from chemical considerations, it might be so strongly expected (669. 674. 672.), is due to the absence or deficiency of conducting power, it would also be proved, at the same time, that decomposition *depends* upon conduction, and not the latter upon the former (413.); and in water this seems to be very nearly decided. On the other hand, the conclusion is almost irresistible, that in electrolytes the power of transmitting the electricity across the substance is dependent upon their capability of suffering decomposition; taking place only whilst they are decomposing, and being proportionate to the quantity of elements separated (821.). I may not, however, stop to discuss this point experimentally at present.

678. When a compound contains such elements as are known to pass towards the opposite extremities of the voltaic pile, still the proportions in which they are present appear to be intimately connected with capability in the compound of suffering or resisting decomposition. Thus, the protochloride of tin readily conducts, and is decomposed (402.), but the perchloride neither conducts nor is decomposed (406.). The protiodide of tin is decomposed when fluid (402.); the periodide is not (405.). The periodide of mercury when fused is not decomposed (691.), even though it does conduct. I was unable to contrast it with the protiodide, the latter being converted into mercury and periodide by heat.

679. These important differences induced me to look more closely to certain binary compounds, with a view of ascertaining whether a *law* regulating the *decomposability*

* With regard to solution, I have met with some reasons for supposing that it will probably disappear as a cause of transference, and intend resuming the consideration at a convenient opportunity.

according to some *relation of the proportionals or equivalents* of the elements, could be discovered. The proto compounds only, amongst those just referred to, were decomposable; and on referring to the substances quoted to illustrate the force and generality of the law of conduction and decomposition which I discovered (402.), it will be found that all the oxides, chlorides, and iodides subject to it, except the chloride of antimony and the periodide of mercury, (to which may now perhaps be added corrosive sublimate,) are also decomposable, whilst many per compounds of the same elements, not subject to the law, were not so (405. 406.).

680. The substances which appeared to form the strongest exceptions to this gegeral result were such bodies as the sulphuric, phosphoric, nitric, arsenic, and other acids.

681. On experimenting with sulphuric acid, I found no reason to believe that it was by itself a conductor of, or decomposable by, electricity, although I had previously been of that opinion (552.). When very strong it is a much worse conductor than if diluted*. If then subjected to the action of a powerful battery, oxygen appears at the *anode*, or positive electrode, although much is absorbed (728.), and hydrogen and sulphur appear at the *cathode*, or negative electrode. Now the hydrogen has with me always been pure, not sulphuretted, and has been deficient in proportion to the sulphur present, so that it is evident that when decomposition occurred water must have been decomposed. I endeavoured to make the experiment with anhydrous sulphuric acid. It appeared to me that in that state, when fused, sulphuric acid was not a conductor, nor decomposed; but I had not enough of the dry acid in my possession to allow me to decide the point satisfactorily. My belief is, that when sulphur appears by the action of the pile on sulphuric acid, it is the result of a secondary action, and that the acid itself is not electrolyzable (757.).

682. Phosphoric acid is, I believe, also in the same condition; but I have found it impossible to decide the point, because of the difficulty of operating on fused anhydrous phosphoric acid. Phosphoric acid which has once obtained water cannot be deprived of it by heat alone. When heated, the hydrated acid volatilizes. Upon subjecting phosphoric acid, fused upon the ring end of a wire (401.), to the action of the voltaic apparatus, it conducted, and was decomposed; but gas, which I believe to be hydrogen, was always evolved at the negative electrode, and the wire was not affected as would have happened had phosphorus been separated. Gas was also evolved at the positive electrode. From all the facts, I conclude it was the water and not the acid which was decomposed.

683. *Arsenic acid.* This substance conducted, and was decomposed; but it contained water, and I was unable at the time to press the investigation so as to ascertain whether a fusible anhydrous arsenic acid could be obtained. It forms, therefore, at present no exception to the general result.

684. Nitrous acid, obtained by distilling nitrate of lead, and keeping it in contact

* De la Rive.

with strong sulphuric acid, was found to conduct and decompose slowly. But on examination there were strong reasons for believing that water was present, and that the decomposition and conduction depended upon it. I endeavoured to prepare a perfectly anhydrous portion, but could not spare the time required to procure an unexceptionable result.

685. Nitric acid is a substance which I believe is not decomposed directly by the electric current. As I want the facts in illustration of the distinction existing between primary and secondary decomposition, I will merely refer to them in this place (752.).

686. That these mineral acids should confer facility of conduction and decomposition on water, is no proof that they are competent to favour and suffer these actions in themselves. Boracic acid does the same thing, though not decomposable. M. De la Rive has pointed out that chlorine has this power also; but being to us an elementary substance, it cannot be due to its capability of suffering decomposition.

687. *Chloride of sulphur* does not conduct, nor is it decomposed. It consists of single proportionals of its elements, but is not on that account an exception to the rule (679.), which does not affirm that *all* compounds of single proportionals of elements are decomposable, but that such as are decomposable are so constituted.

688. *Protochloride of phosphorus* does not conduct nor become decomposed.

689. *Protochloride of carbon* does not conduct nor suffer decomposition. In association with this substance, I submitted the *hydro-chloride of carbon* from olefiant gas and chlorine to the action of the electric current; but it also refused to conduct or yield up its elements.

690. With regard to the exceptions (679.), upon closer examination, some of them disappear. Chloride of antimony (a compound of one proportional of antimony and one and a half of chlorine) of recent preparation was put into a tube (fig. 13.) (789.), and submitted when fused to the action of the current, the positive electrode being of plumbago. No electricity passed, and no appearance of decomposition was visible at first; but when the positive and negative electrodes were brought very near each other in the chloride, then a feeble action occurred and a feeble current passed. The effect altogether was so small (although quite amenable to the law before given), and so unlike the decomposition and conduction occurring in all the other cases, that I attribute it to the presence of a minute quantity of water, (for which this and many other chlorides have strong attractions, producing hydrated chlorides,) or perhaps of a true protochloride consisting of single proportionals (695. 796.).

691. *Periodide of mercury* being examined in the same manner, was found most distinctly to insulate whilst solid, but conduct when fluid, according to the law of *liquido-conduction* (402.); but there was no appearance of decomposition. No iodine appeared at the *anode*, nor mercury or other substance at the *cathode*. The case is, therefore, no exception to the rule, that only compounds of single proportionals are decomposable; but it is an exception, and I think the only one, to the statement, that

all bodies subject to the law of liquido-conduction are decomposable. I incline, however, to believe, that a portion of protiodide of mercury is retained dissolved in the periodide, and that to its slow decomposition the feeble conducting power is due. Periodide would be formed, as a secondary result, at the *anode*; and the mercury at the *cathode* would also form, as a secondary result, protiodide. Both these bodies would mingle with the fluid mass, and thus no final separation appear, notwithstanding the continued decomposition.

692. When *perchloride of mercury* was subjected to the voltaic current, it did not conduct in the solid state, but it did conduct when fluid. I think, also, that in the latter case it was decomposed; but there are many interfering circumstances which require examination before a positive conclusion can be drawn.

693. When the ordinary protoxide of antimony is subjected to the voltaic current in a fused state, it also is decomposed, although the effect from other causes soon ceases (402. 802.). This oxide consists of one proportional of antimony and one and a half of oxygen, and is therefore an exception to the general law assumed. But in working with this oxide and the chloride, I observed facts which lead me to doubt whether the compounds usually called the protoxide and the protochloride do not often contain other compounds, consisting of single proportions, which are the true proto compounds, and which, in the case of the oxide, might give rise to the decomposition above described.

694. The ordinary sulphuret of antimony is considered as being the compound with the smallest quantity of sulphur, and analogous in its proportions to the ordinary protoxide. But I find that if it be fused with metallic antimony, a new sulphuret is formed, containing much more of the metal than the former, and separating distinctly, when fused, both from the pure metal on the one hand, and the ordinary grey sulphuret on the other. In some rough experiments, the metal thus taken up by the ordinary sulphuret of antimony was equal to half the proportion of that previously in the sulphuret, in which case the new sulphuret would consist of *single* proportionals.

695. When this new sulphuret was dissolved in muriatic acid, although a little antimony separated, yet it appeared to me that a true protochloride, consisting of *single* proportionals, was formed, and from that, by alkalies, &c., a true protoxide, consisting also of *single* proportionals was obtainable. But I could not stop to ascertain this matter strictly by analysis.

696. I believe, however, that there is such an oxide; that it is often present in variable proportions in what is commonly called protoxide, throwing uncertainty upon the results of its analysis, and causing the electrolytic decomposition above described.

697. Upon the whole, it appears probable that all those binary compounds of elementary bodies which are capable of being electrolyzed when fluid, but not whilst solid, according to the law of liquido-conduction (394.), consist of single proportionals of their elementary principles; and it may be because of their departure from this

simplicity of composition, that boracic acid, ammonia, perchlorides, periodides, and many other direct compounds of elements, are indecomposable.

698. With regard to salts and combinations of compound bodies, the same simple relation does not appear to hold good. I could not decide this by bisulphates of the alkalies, for as long as the second proportion of acid remained, water was retained with it. The fused salt, therefore, conducted, and was decomposed; but hydrogen always appeared at the negative electrode.

699. A biphosphate of soda was prepared by heating, and ultimately fusing, the ammonia-phosphate of soda. In this case the fused bisalt conducted, and was decomposed; but a little gas appeared at the negative electrode, and though I believe the salt itself was electrolyzed, I am not quite satisfied that water was entirely absent.

700. Then a biborate of soda was prepared; and this, I think, is an unobjectionable case. The salt, when fused, conducted, and was decomposed, and gas appeared at both electrodes: even when the boracic acid was increased to three proportionals the same effect took place.

701. Hence this class of compound combinations does not seem to be subject to the same simple law as the former class of binary combinations. Whether we may find reason to consider them as mere solutions of the compound of single proportionals in the excess of acid, is a matter which, with some apparent exceptions occurring amongst the sulphurets, must be left for decision by future examination.

702. In any investigation of these points, great care must be taken to exclude water; for if present, secondary effects are so frequently produced as often seemingly to indicate an electro-decomposition of substances, when no true result of the kind has occurred (742. &c.).

703. It is evident that all the cases in which decomposition *does not occur may* depend upon the want of conduction (677. 413.); but that does not at all lessen the interest excited by seeing the great difference of effect due to a change, not in the nature of the elements, but merely in their proportions, especially in any attempt which may be made to elucidate and expound the beautiful theory put forth by Sir Humphry Davy*, and illustrated by Berzelius and other eminent philosophers, that ordinary chemical affinity is a mere result of the electrical attractions of the particles of matter.

¶ v. *On a new Measurer of Volta-electricity.*

704. I have already said, when engaged in reducing common and voltaic electricity to one standard of measurement (377.), and again when introducing my theory of electro-chemical decomposition (504. 505. 510.), that the chemical decomposing action of a current *is constant for a constant quantity of electricity*, notwithstanding the greatest variations in its sources, in its intensity, in the size of the *electrodes* used, in the nature of the conductors (or non-conductors (307.)) through which it is

* Philosophical Transactions, 1807, pp. 32, 39; also 1826, pp. 387, 389.

passed, or in other circumstances. The conclusive proofs of the truth of these statements shall be given almost immediately (783. &c.).

705. I endeavoured upon this law to construct an instrument which should measure out the electricity passing through it, and which, being interposed in the course of the current used in any particular experiment, should serve at pleasure, either as a *comparative standard* of effect, or as a *positive measurer* of this subtile agent.

706. There is no substance better fitted, under ordinary circumstances, to be the indicating body in such an instrument than water; for it is decomposed with facility when rendered a better conductor by the addition of acids or salts; its elements may in numerous cases be obtained and collected without any embarrassment from secondary action, and, being gaseous, they are in the best physical condition for separation and measurement. Water, therefore, acidulated by sulphuric acid, is the substance I shall generally refer to, although it may become expedient in peculiar cases or forms of experiment to use other bodies (843.).

707. The first precaution needful in the construction of the instrument was to avoid the recombination of the evolved gases, an effect which the positive electrode has been found so capable of producing (571.). For this purpose various forms of decomposing apparatus were used. The first consisted of straight tubes, each containing a plate and wire of platina soldered together by gold, and fixed hermetically in the glass at the closed extremity of the tube (Plate I. fig. 5.). The tubes were about eight inches long, 0·7 of an inch in diameter, and graduated. The platina plates were about an inch long, as wide as the tubes would permit, and adjusted as near to the mouths of the tubes as was consistent with the safe collection of the gases evolved. In certain cases, where it was required to evolve the elements upon as small a surface as possible, the metallic extremity, instead of being a plate, consisted of the wire bent into the form of a ring (fig. 6.). When these tubes were used as measurers, they were filled with the dilute sulphuric acid, and inverted in a basin of the same liquid (fig. 7.), being placed in an inclined position, with their mouths near to each other, that as little decomposing matter should intervene as possible; and also, in such a direction that the platina plates should be in vertical planes (720.).

708. Another form of apparatus was that delineated (fig. 8.). The tube is bent in the middle; one end is closed; in that end is fixed a wire and plate, *a*, proceeding so far downwards, that, when in the position figured, it shall be as near to the angle as possible, consistently with the collection, at the closed extremity of the tube, of all the gas evolved against it. The plane of this plate is also perpendicular (720.). The other metallic termination, *b*, is introduced at the time decomposition is to be effected, being brought as near the angle as possible, without causing any gas to pass from it towards the closed end of the instrument. The gas evolved against it is allowed to escape.

709. The third form of apparatus contains both electrodes in the same tube; the transmission, therefore, of the electricity, and the consequent decomposition, is far

more rapid than in the separate tubes. The resulting gas is the sum of the portions evolved at the two electrodes, and the instrument is better adapted than either of the former as a measurer of the quantity of voltaic electricity transmitted in ordinary cases. It consists of a straight tube (fig. 9.) closed at the upper extremity, and graduated, through the sides of which pass the platina wires (being fused into the glass), which are connected with two plates within. The tube is fitted by grinding into one mouth of a double-necked bottle. If the latter be one half or two thirds full of the dilute sulphuric acid, it will, upon inclination of the whole, flow into the tube and fill it. When an electric current is passed through the instrument, the gases evolved against the plates collect in the upper portion of the tube, and are not subject to the recombining power of the platina.

710. Another form of the instrument is given at fig. 10.

711. A fifth form is delineated (fig. 11.). This I have found exceedingly useful in experiments continued in succession for days together, and where large quantities of indicating gas were to be collected. It is fixed on a weighted foot, and has the form of a small retort containing the two electrodes: the neck is narrow, and sufficiently long to deliver gas issuing from it into a jar placed in a small pneumatic trough. The electrode chamber, sealed hermetically at the part held in the stand, is five inches in length, and 0·6 of an inch in diameter; the neck about nine inches in length, and 0·4 of an inch in diameter internally. The figure will fully indicate the construction.

712. It can hardly be requisite to remark, that in the arrangement of any of these forms of apparatus, they, and the wires connecting them with the substance, which is collaterally subjected to the action of the same electric current, should be so far insulated as to ensure a certainty that all the electricity which passes through the one shall also be transmitted through the other.

713. Next to the precaution of collecting the gases, if mingled, out of contact with the platinum, was the necessity of testing the law of a *definite electrolytic* action, upon water at least, under all varieties of condition; that, with a conviction of its certainty, might also be obtained a knowledge of those interfering circumstances which would require to be practically guarded against.

714. The first point investigated was the influence or indifference of extensive variations in the size of the electrodes, for which purpose instruments like those last described (709. 710. 711.) were used. One of these had plates 0·7 of an inch wide, and nearly four inches long; another had plates only 0·5 of an inch wide, and 0·8 of an inch long; a third had wires 0·02 of an inch in diameter, and three inches long; and a fourth similar wires only half an inch in length. Yet when these were filled with dilute sulphuric acid, and, being placed in succession, had one common current of electricity passed through them, very nearly the same quantity of gas was evolved in all. The difference was sometimes in favour of one, and sometimes on the side of another; but the general result was that the largest quantity of gases was evolved upon the smaller surface of the wires.

715. Experiments of a similar kind were made with the single-plate, straight tubes (707.), and also with the curved tubes (708.), with similar consequences; and when these, with the former tubes, were arranged together in various ways, the result, as to the equality of action of large and small metallic surfaces when delivering and receiving the same current of electricity, was constantly the same. As an illustration, the following numbers are given. An instrument with two wires evolved 74·3 volumes of mixed gases; another with plates 73·25 volumes; whilst the sum of the oxygen and hydrogen in two separate tubes amounted to 73·65 volumes. In another experiment the volumes were 55·3, 55·3, and 54·4.

716. But it was observed in these experiments, that in single-plate tubes (707.) more hydrogen was evolved at the negative electrode than was proportionate to the oxygen at the positive electrode; and generally, also, more than was proportionate to the oxygen and hydrogen in a double-plate tube. Upon more minutely examining these effects, I was led to refer them, and also the differences between wires and plates (714.), to the solubility of the gases evolved, especially at the positive electrode.

717. When the positive and negative electrodes are equal in surface, the bubbles which rise from them in dilute sulphuric acid are always different in character. Those from the positive plate are exceedingly small, and separate instantly from every part of the surface of the metal, in consequence of its perfect cleanliness (633.); whilst in the liquid they give it a hazy appearance, from their number and minuteness; are easily carried down by currents; and therefore not only present far greater surface of contact with the liquid than larger bubbles would do, but are retained a much longer time in mixture with it. But the bubbles at the negative surface, though they constitute twice the volume of the gas at the positive electrode, are nevertheless very inferior in number. They do not rise so universally from every part of the surface, but seem to be evolved at different points; and though so much larger, they appear to cling to the metal, separating with difficulty from it, and when separated, instantly rising to the top of the liquid. If, therefore, oxygen and hydrogen had equal solubility in, or powers of combining with, water under similar circumstances, still under the present conditions the oxygen would be far the most liable to solution; but when to these is added its well known power of forming a compound with water, it is no longer surprising that such a compound should be produced in small quantities at the positive electrode; and indeed the bleaching power which some philosophers have observed in a solution at this electrode, when chlorine and similar bodies have been carefully excluded, is probably due to the formation there, in this manner, of oxy-water.

718. That more gas was collected from the wires than from the plates, I attribute to the circumstance, that as equal quantities were evolved in equal times, the bubbles at the wires having been more rapidly produced, in relation to any part of the surface, must have been much larger; have been therefore in contact with the fluid by a much

smaller surface, and for a much shorter time than those at the plates; hence less solution and a greater collection.

719. There was also another effect produced, especially by the use of large electrodes, which was both a consequence and a proof of the solution of part of the gas evolved there. The collected gas, when examined, was found to contain small portions of nitrogen. This I attribute to the presence of air dissolved in the acid used for decomposition. It is a well-known fact, that when bubbles of a gas but slightly soluble in water or solutions pass through them, the portion of this gas which is dissolved displaces a portion of that previously in union with the liquid: and so, in the decompositions under consideration, as the oxygen dissolves, it displaces a part of the air, or at least of the nitrogen, previously united to the acid; and this proceeds *most extensively* with large plates, because the gas evolved at them is in the most favourable condition for solution.

720. With the intention of avoiding this solubility of the gases as much as possible, I arranged the decomposing plates in a vertical position (707. 708.), that the bubbles might quickly escape upwards, and that the downward currents in the fluid should not meet ascending currents of gas. This precaution I found to assist greatly in producing constant results, and especially in experiments to be hereafter referred to, in which other liquids than dilute sulphuric acid, as for instance solution of potash, were used.

721. The irregularities in the indications of the measurer proposed, arising from the solubility just referred to, are but small, and may be very nearly corrected by comparing the results of two or three experiments. They may also be almost entirely avoided by selecting that solution which is found to favour them in the least degree (728.); and still further by collecting the hydrogen only, and using that as the indicating gas; for being much less soluble than oxygen, being evolved with twice the rapidity and in larger bubbles (717.), it can be collected more perfectly and in greater purity.

722. From the foregoing and many other experiments, it results that *variation in the size of the electrodes causes no variation in the chemical action of a given quantity of electricity upon water.*

723. The next point in regard to which the principle of constant electro-chemical action was tested, was *variation of intensity.* In the first place, the preceding experiments were repeated, using batteries of an *equal* number of plates, *strongly* and *weakly* charged; but the results were alike. They were then repeated, using batteries sometimes containing forty, and at other times only five pairs of plates; but the results were still the same. *Variations therefore in the intensity,* caused by difference in the strength of charge, or in the number of alternations used, *produced no difference as to the equal action of large and small electrodes.*

724. Still these results did not prove that variation in the intensity of the current was not accompanied by a corresponding variation in the electro-chemical effects,

since the actions at *all* the surfaces might have increased or diminished together. The deficiency in the evidence is, however, completely supplied by the former experiments on different-sized electrodes; for with variation in the size of these, a variation in the intensity must have occurred. The intensity of an electric current traversing conductors alike in their nature, quality, and length, is probably as the quantity of electricity passing through a given sectional area perpendicular to the current, divided by the time (360. *note*); and therefore when large plates were contrasted with wires separated by an equal length of the same decomposing conductor (714.), whilst one current of electricity passed through both arrangements, that electricity must have been in a very different state, as to *tension*, between the plates and between the wires; yet the chemical results were the same.

725. The difference in intensity, under the circumstances described, may be easily shown practically, by arranging two decomposing apparatus as in fig. 12, where the same fluid is subjected to the decomposing power of the same current of electricity, passing in the vessel A. between large platina plates, and in the vessel B. between small wires. If a third decomposing apparatus, such as that delineated fig. 11. (711.), be connected with the wires at *a b*, fig. 12, it will serve sufficiently well, by the degree of decomposition occurring in it, to indicate the relative state of the two plates as to intensity; and if it then be applied in the same way, as a test of the state of the wires at *a' b'*, it will, by the increase of decomposition within, show how much greater the intensity is there than at the former points. The connexions of P and N with the voltaic battery are of course to be continued during the whole time.

726. A third form of experiment in which difference of intensity was obtained, for the purpose of testing the principle of equal chemical action, was to arrange three volta-electrometers, so that after the electric current had passed through one, it should divide into two parts, which, after traversing each one of the remaining instruments, should reunite. The sum of the decomposition in the two latter vessels was always equal to the decomposition in the former vessel. But the *intensity* of the divided current could not be the same as that it had in its original state; and therefore *variation of intensity has no influence on the results if the quantity of electricity remain the same.* The experiment, in fact, resolves itself simply into an increase in the size of the electrodes (725.).

727. The *third point*, in respect to which the principle of equal electro-chemical action on water was tested, was *variation of the strength of the solution used.* In order to render the water a conductor, sulphuric acid had been added to it (707.); and it did not seem unlikely that this substance, with many others, might render the water more subject to decomposition, the electricity remaining the same in quantity. But such did not prove to be the case. Diluted sulphuric acid, of different strengths, was introduced into different decomposing apparatus, and submitted simultaneously to the action of the same electric current (714.). Slight differences occurred, as before, sometimes in one direction, sometimes in another; but the final result was, that

exactly the same quantity of water was decomposed in all the solutions by the same quantity of electricity, though the sulphuric acid in some was seventyfold what it was in others. The strengths used were of specific gravity 1·495, and downwards.

728. When an acid having a specific gravity of about 1·336 was employed, the results were most uniform, and the oxygen and hydrogen (716.) most constantly in the right proportion to each other. Such an acid gave more gas than one much weaker acted upon by the same current, apparently because it had less solvent power. If the acid were very strong, then a remarkable disappearance of oxygen took place; thus, one made by mixing two measures of strong oil of vitriol with one of water, gave forty-two volumes of hydrogen, but only twelve of oxygen. The hydrogen was very nearly the same with that evolved from acid of the specific gravity 1·232. I have not yet had time to examine minutely the circumstances attending the disappearance of the oxygen in this case, but imagine it is due to the formation of oxywater, which Thénard has shown is favoured by the presence of acid.

729. Although not necessary for the practical use of the instrument I am describing, yet as connected with the important point of constant electro-chemical action upon water, I now investigated the effects produced by an electric current passing through aqueous solutions of acids, salts, and compounds, exceedingly different from each other in their nature, and found them to yield astonishingly uniform results. But many of them which are connected with a secondary action will be more usefully described hereafter (778.).

730. When solutions of caustic potassa or soda, or sulphate of magnesia, or sulphate of soda, were acted upon by the electric current, just as much oxygen and hydrogen was evolved from them as from the diluted sulphuric acid, with which they were compared. When a solution of ammonia, rendered a better conductor by sulphate of ammonia (554.), or a solution of subcarbonate of potassa was experimented with, the *hydrogen* evolved was in the same quantity as that set free from the diluted sulphuric acid with which they were compared. Hence *changes in the nature of the solution do not alter the constancy of electrolytic action upon water.*

731. I have already said, respecting large and small electrodes, that change of order caused no change in the general effect (715.). The same was the case with different solutions, or with different intensities; and however the circumstances of an experiment might be varied, the results came forth exceedingly consistent, and proved that the electro-chemical action was still the same.

732. I consider the foregoing investigation as sufficient to prove the very extraordinary and important principle with respect to water, *that when subjected to the influence of the electric current, a quantity of it is decomposed exactly proportionate to the quantity of electricity which has passed*, notwithstanding the thousand variations in the conditions and circumstances under which it may at the time be placed; and further, that when the interference of certain secondary effects (742. &c.), together with the solution or recombination of the gas and the evolution of air, are guarded against,

the products of the decomposition may be collected with such accuracy, as to afford a very excellent and valuable measurer of the electricity concerned in their evolution.

733. The forms of instrument which I have given, figg. 9, 10, 11. (709. 710. 711.), are probably those which will be found most useful, as they indicate the quantity of electricity by the largest volume of gases, and cause the least obstruction to the passage of the current. The fluid which my present experience leads me to prefer, is a solution of sulphuric acid of specific gravity about 1·336, or from that to specific gravity 1·25; but it is very essential that there should be no organic substance, nor any vegetable acid, nor other body, which, by being liable to the action of the oxygen or hydrogen evolved at the electrodes (773. &c.), shall diminish their quantity, or add other gases to them.

734. In many cases when the instrument is used as a *comparative standard*, or even as *a measurer*, it may be desirable to collect the hydrogen only, as being less liable to absorption or disappearance in other ways than the oxygen; whilst at the same time its volume is so large, as to render it a good and sensible indicator. In such cases the first and second form of apparatus have been used, figg. 7, 8. (707. 708.). The indications obtained were very constant, the variations being much smaller than in those forms of apparatus collecting both gases; and they can also be procured when solutions are used in comparative experiments, which, yielding no oxygen or only secondary results of its action, can give no indications if the educts at both electrodes be collected. Such is the case when solutions of ammonia, muriatic acid, chlorides, iodides, acetates, or other vegetable salts, &c., are employed.

735. In a few cases, as where solutions of metallic salts liable to reduction at the negative electrode are acted upon, the oxygen may be advantageously used as the measuring substance. This is the case, for instance, with sulphate of copper.

736. There are therefore two general forms of the instrument which I submit as a measurer of electricity. One, in which both the gases of the water decomposed are collected (709. 710. 711.); and the other, in which a single gas, as the hydrogen only, is used (707. 708.). When referred to as a *comparative instrument*, (a use I shall now make of it very extensively,) it will not often require particular precaution in the observation; but when used as an *absolute measurer*, it will be needful that the barometric pressure and the temperature be taken into account, and that the graduation of the instruments should be to one scale; the hundredths and smaller divisions of a cubical inch are quite fit for this purpose, and the hundredth may be very conveniently taken as indicating a DEGREE of electricity.

737. It can scarcely be needful to point out further than has been done how this instrument is to be used. It is to be introduced into the course of the electric current, the action of which is to be exerted anywhere else, and if 60° or 70° of electricity are to be measured out, either in one or several portions, the current, whether strong or weak, is to be continued until the gas in the tube occupies that number of divisions or hundredths of a cubical inch. Or if a quantity competent to produce a certain

effect is to be measured, the effect is to be obtained, and then the indication read off. In exact experiments it is necessary to correct the volume of gas for changes in temperature and pressure, and especially for moisture*. For the latter object the volta-electrometer (fig. 11.) is most accurate, as its gas can be measured over water, whilst the others retain it over acid or saline solutions.

738. I have not hesitated to apply the term *degree*, in analogy with the use made of it with respect to another most important imponderable agent, namely, heat; and as the definite expansion of air, water, mercury, &c., is there made use of to measure heat, so the equally definite evolution of gases is here turned to a similar use for electricity.

739. The instrument offers the only *actual measurer* of voltaic electricity which we at present possess. For without being at all affected by variations in time or intensity, or alterations in the current itself, of any kind, or from any cause, or even of intermissions of action, it takes note with accuracy of the quantity of electricity which has passed through it, and reveals that quantity by inspection; I have therefore named it a VOLTA-ELECTROMETER.

740. Another mode of measuring volta-electricity may be adopted with advantage in many cases, dependent on the quantities of metals or other substances evolved either as primary or as secondary results; but I refrain from enlarging on this use of the products, until the principles on which their constancy depends have been fully established (791. 843.).

741. By the aid of this instrument I have been able to establish the definite character of electro-chemical action in its most general sense; and I am persuaded it will become of the utmost use in the extensions of the science which these views afford. I do not pretend to have made its detail perfect, but to have demonstrated the truth of the principle, and the utility of the application.

¶ vi. *On the primary or secondary character of the bodies evolved at the Electrodes.*

742. Before the *volta-electrometer* could be employed in determining, as a *general law*, the constancy of electro-decomposition, it became necessary to examine a distinction, already recognised among scientific men, relative to the products of that action, namely, their primitive or secondary character; and, if possible, by some general rule or principle, to decide when they were of the one or the other kind. It will appear hereafter that great mistakes respecting electro-chemical action and its consequences, have arisen from confounding these two classes of results together.

743. When a substance under decomposition yields at the electrodes those bodies uncombined and unaltered which the electric current has separated, then they may be considered as primary results, even though themselves compounds. Thus the oxygen and hydrogen from water are primary results; and so also are the acid and alkali (themselves compound bodies) evolved from sulphate of soda. But when the sub-

* For a simple table of correction for moisture, I may take the liberty of referring to my Chemical Manipulation, edition of 1830, p. 376.

stances separated by the current are changed at the electrodes before their appearance, then they give rise to secondary results, although in many cases the bodies evolved are elementary.

744. These secondary results occur in two ways, being sometimes due to the mutual action of the evolving substance and the matter of the electrode, and sometimes to its action upon the substances contained in the decomposing conductor itself. Thus, when carbon is made the positive electrode in dilute sulphuric acid, carbonic oxide and carbonic acid appear there instead of oxygen; for the latter, acting upon the matter of the electrode, produces these secondary results. Or if the positive electrode, in a solution of nitrate or acetate of lead, be platina, then peroxide of lead appears there, equally a secondary result with the former, but now depending upon an action of the oxygen on a substance in the solution. Again, when ammonia is decomposed by platina electrodes, nitrogen appears at the *anode**; but though an *elementary* body, it is a *secondary* result in this case, being derived from the chemical action of the oxygen electrically evolved there, upon the ammonia in the surrounding solution (554.). In the same manner when aqueous solutions of metallic salts are decomposed by the current, the metals evolved at the *cathode*, though elements, are *always* secondary results, and not immediate consequences of the decomposing power of the electric current.

745. Many of these secondary results are extremely valuable; for instance, all the interesting compounds which M. Becquerel has obtained by feeble electric currents are of this nature; but they are essentially chemical, and must, in the theory of electrolytic action, be carefully distinguished from those which are directly due to the action of the electric current.

746. The nature of the substances evolved will often lead to a correct judgement of their primary or secondary character, but is not sufficient alone to establish that point. Thus, nitrogen is said to be attracted sometimes by the positive and sometimes by the negative electrode, according to the bodies with which it may be combined (554. 555.), and it is on such occasions evidently viewed as a primary result†; but I think I shall show, that, when it appears at the positive electrode, or rather at the *anode*, it is a secondary result (748.). Thus, also, Sir Humphry Davy‡, and with him the great body of chemical philosophers, (including myself,) have given the appearance of copper, lead, tin, silver, gold, &c., at the negative electrode, when their aqueous solutions were acted upon by the voltaic current, as proofs that the metals, as a class, were attracted to that surface; thus assuming the metal in each case to be a primary result. These however, I expect to prove, are all secondary results; the mere consequence of chemical action, and no proofs of the attraction or the law announced §.

* Annales de Chimie, 1804, tom. li. p. 167. † Ibid. tom. li. p. 172.

‡ Elements of Chemical Philosophy, pp. 144. 161.

§ It is remarkable that up to 1804 it was the received opinion that the metals were reduced by the nascent hydrogen. At that date the general opinion was reversed by Hisinger and Berzelius (Annales de Chimie,

747. But when we take to our assistance the law of *constant electro-chemical action* already proved with regard to water (732.), and which I hope to extend satisfactorily to all bodies (821.), and consider the *quantities* as well as the *nature* of the substances set free, a generally accurate judgement of the primary or secondary character of the results may be formed: and this important point, so essential to the theory of electro-decomposition, since it decides what are the particles directly under the influence of the current, (distinguishing them from such as are not affected,) and what are the results to be expected, may be established with such degree of certainty as to remove innumerable ambiguities and doubtful considerations from this branch of the science.

748. Let us apply these principles to the case of ammonia, and the supposed determination of nitrogen to one or the other *electrode* (554. 555.). A pure strong solution of ammonia is as bad a conductor, and therefore as little liable to electro-decomposition, as pure water; but when sulphate of ammonia is dissolved in it, the whole becomes a conductor; nitrogen *almost* and occasionally *quite* pure is evolved at the *anode*, and hydrogen at the *cathode*; the ratio of the volume of the former to that of the latter varying, but being as 1 to about 3 or 4. This result would seem at first to imply that the electric current had decomposed ammonia, and that the nitrogen had been determined towards the positive electrode. But when the electricity used was measured out by the volta-electrometer (707. 736.), it was found that the hydrogen obtained was exactly in the proportion which would have been supplied by decomposed water, whilst the nitrogen had no certain or constant relation whatever. When, upon multiplying experiments, it was found that, by using a stronger or weaker solution, or a more or less powerful battery, the gas evolved at the *anode* was a mixture of oxygen and nitrogen, varying both in proportion and absolute quantity, whilst the hydrogen at the *cathode* remained constant, no doubt could be entertained that the nitrogen at the *anode* was a secondary result, depending upon the chemical action of the nascent oxygen, determined to that surface by the electric current, upon the ammonia in solution. It was the water, therefore, which was electrolyzed, not the ammonia. Further, the experiment gives no real indication of the tendency of the element nitrogen to either one electrode or the other; nor do I know of any experiment with nitric acid, or other compounds of nitrogen, which shows the tendency of this element, under the influence of the electric current, to pass in either direction along its course.

749. As another illustration of secondary results, the effects on a solution of acetate of potassa may be quoted. When a very strong solution was used, more gas was evolved at the *anode* than at the *cathode*, in the proportion of 4 to 3 nearly: that from the *anode* was a mixture of carbonic oxide and carbonic acid; that from the *cathode* pure hydrogen. When a much weaker solution was used, less gas was evolved at the *anode* than at the *cathode*; and it now contained carburetted hydrogen, as well as carbonic oxide and car-

1804, tom. li. p. 174.), who stated that the metals were evolved directly by the electricity: in which opinion it appears, from that time, DAVY coincided (Philosophical Transactions, 1826, p. 388.).

bonic acid. This result of carburetted hydrogen at the positive electrode has a very anomalous appearance, if considered as an immediate consequence of the decomposing power of the current. It, however, as well as the carbonic oxide and acid, is only a *secondary result*; for it is the water alone which suffers electro-decomposition, and it is the oxygen eliminated at the *anode* which, reacting on the acetic acid, in the midst of which it is evolved, produces those substances that finally appear there. This is fully proved by experiments with the volta-electrometer (707.); for then the hydrogen evolved from the acetate at the *cathode* is always found to be definite, being exactly proportionate to the electricity which has passed through the solution, and, in quantity, the same as the hydrogen evolved in the volta-electrometer itself. The appearance of the carbon in combination with the hydrogen at the positive electrode, and its non-appearance at the negative electrode, are in curious contrast with the results which might have been expected from the law usually accepted respecting the final places of the elements.

750. If the salt in solution be an acetate of lead, then the results at both electrodes are secondary, and cannot be used to estimate or express the amount of electro-chemical action, except by a circuitous process (843.). In place of oxygen, or even the gases already described (749.), peroxide of lead now appears at the positive, and lead itself at the negative electrode. When other metallic solutions are used, containing, for instance, peroxides, as that of copper, combined with this or any other decomposable acid, still more complicated results will be obtained; which, viewed as direct results of the electro-chemical action, will, in their proportions, present nothing but confusion, but will appear perfectly harmonious and simple if they be considered as secondary results, and will accord in their proportions with the oxygen and hydrogen evolved from water by the action of a definite quantity of electricity.

751. I have experimented upon many bodies, with a view to determine whether the results were primary or secondary. I have been surprised to find how many of them, in ordinary cases, are of the latter class, and how frequently water is the only body electrolyzed in instances where other substances have been supposed to give way. Some of these results I will give in as few words as possible.

752. *Nitric acid.*—When very strong, it conducted well, and yielded oxygen at the positive electrode. No gas appeared at the negative electrode; but nitrous acid, and apparently nitric oxide, were formed there, which, dissolving, rendered the acid yellow or red, and at last even effervescent, from the spontaneous separation of nitric oxide. Upon diluting the acid with its bulk or more of water, gas appeared at the negative electrode. Its quantity could be varied by variations, either in the strength of the acid or of the voltaic current: for that acid from which no gas separated at the *cathode*, with a weak voltaic battery, did evolve gas there with a stronger; and that battery which evolved no gas there, with a strong acid, did cause its evolution with an acid more dilute. The gas at the *anode* was always oxygen; that at the *cathode* hydrogen. When the quantity of products was examined by the volta-electro-

meter (707.), the oxygen, whether from strong or weak acid, proved to be in the same proportion as from water. When the acid was diluted to specific gravity 1·24, or less, the hydrogen also proved to be the same in quantity as from water. Hence I conclude that the nitric acid does not undergo electro-chemical decomposition, but the water only; that the oxygen at the *anode* is always a primary result, but that the products at the *cathode* are often secondary, and due to the reaction of the hydrogen upon the nitric acid.

753. *Nitre.*—A solution of this salt yields very variable results, according as one or other form of tube is used, or as the electrodes are large or small. Sometimes the whole of the hydrogen of the water decomposed may be obtained at the negative electrode; at other times, only a part of it, because of the ready formation of secondary results. The solution is a very excellent conductor of electricity.

754. *Nitrate of ammonia,* in aqueous solution, gives rise to secondary results very varied and uncertain in their proportions.

755. *Sulphurous acid.*—Pure liquid sulphurous acid does not conduct nor suffer decomposition by the voltaic current*, but, when dissolved in water, the solution acquires conducting power, and is decomposed, yielding oxygen at the *anode,* and hydrogen and sulphur at the *cathode.*

756. A solution containing sulphuric acid in addition, was a better conductor. It gave very little gas at either electrode: that at the *anode* was oxygen, that at the *cathode* pure hydrogen. From the *cathode* also rose a white turbid stream, consisting of diffused sulphur, which soon rendered the whole solution milky. The volumes of gases were in no regular proportion to the quantities evolved from water in the volta-electrometer. I conclude that the sulphurous acid was not at all affected by the electric current in any of these cases, and that the water present was the only body electro-chemically decomposed; that, at the *anode,* the oxygen from the water converted the sulphurous acid into sulphuric acid, and, at the *cathode,* the hydrogen electrically evolved decomposed the sulphurous acid, combining with its oxygen, and setting its sulphur free. I conclude that the sulphur at the negative electrode was only a secondary result; and, in fact, no part of it was found combined with the small portion of hydrogen which escaped when weak solutions of sulphurous acid were used.

757. *Sulphuric acid.*—I have already given my reasons for concluding that sulphuric acid is not electrolyzable, i. e. not decomposable directly by the electric current, but occasionally suffering by a secondary action at the *cathode* from the hydrogen evolved there (681.). In the year 1800, Davy considered the sulphur from sulphuric acid as the result of the action of the nascent hydrogen †. In 1804, Hisinger and Berzelius stated that it was the direct result of the action of the voltaic pile ‡; an opinion which from that time Davy seems to have adopted, and which has since been

* See also De la Rive, Bibliothèque Universelle, tom. xl. p. 205; or Quarterly Journal of Science, vol. xxvii. p. 407.

† Nicholson's Quarterly Journal, vol. iv. pp. 280, 281.

‡ Annales de Chimie, 1804, tom. li. p. 173.

commonly received by all. The change of my own opinion requires that I should correct what I have already said of the decomposition of sulphuric acid in a former series of these Researches (552.): I do not now think that the appearance of the sulphur at the negative electrode is an immediate consequence of electrolytic action.

758. *Muriatic acid.*—A strong solution gave hydrogen at the negative electrode, and chlorine only at the positive electrode; of the latter, a part acted on the platina and a part was dissolved. A minute bubble of gas remained; it was not oxygen, but probably air previously held in solution.

759. It was an important matter to determine whether the chlorine was a primary result, or only a secondary product, due to the action of the oxygen evolved from water at the *anode* upon the muriatic acid; i. e. whether the muriatic acid was electrolyzable, and if so, whether the decomposition was *definite.*

760. The muriatic acid was gradually diluted. One part with six of water gave only chlorine at the *anode.* One part with eight of water gave only chlorine; with nine of water, a little oxygen appeared with the chlorine: but the occurrence or non-occurrence of oxygen at these strengths depended, in part, on the strength of the voltaic battery used. With fifteen parts of water, a little oxygen, with much chlorine, was evolved at the *anode.* As the solution was now becoming a bad conductor of electricity, sulphuric acid was added to it: this caused more ready decomposition, but did not sensibly alter the proportion of chlorine and oxygen.

761. The muriatic acid was now diluted with 100 times its volume of dilute sulphuric acid. It still gave a large proportion of chlorine at the *anode,* mingled with oxygen; and the result was the same, whether a voltaic battery of 40 pairs of plates or one containing only 5 pairs were used. With acid of this strength, the oxygen evolved at the *anode* was to the hydrogen at the *cathode,* in volume, as 17 is to 64; and therefore the chlorine would have been 30 volumes, had it not been dissolved by the fluid.

762. Next, with respect to the quantity of elements evolved. On using the volta-electrometer, it was found that, whether the strongest or the weakest muriatic acid were used, whether chlorine alone or chlorine mingled with oxygen appeared at the *anode,* still the hydrogen evolved at the *cathode* was a constant quantity, i. e. exactly the *same* as the hydrogen which the *same quantity of electricity* could evolve from water.

763. This constancy does not decide whether the muriatic acid is electrolyzed or not, although it proves that if so, it must be in definite proportions to the quantity of electricity used. Other considerations may, however, be allowed to decide the point. The analogy between chlorine and oxygen, in their relations to hydrogen, is so strong, as to lead almost to the certainty, that, when combined with that element, they would perform similar parts in the process of electro-decomposition. They both unite with it in single proportional or equivalent quantities; and, the number of proportionals appearing to have an intimate and important relation to the decomposability of a

body (697.), those in muriatic acid, as well as in water, are the most favourable, or those, perhaps even necessary, to decomposition. In other binary compounds of chlorine also, where nothing equivocal depending on the simultaneous presence of it and oxygen is involved, the chlorine is directly eliminated at the *anode* by the electric current. Such is the case with the chloride of lead (395.), which may be justly compared with protoxide of lead (402.), and stands in the same relation to it as muriatic acid to water. The chlorides of potassium, sodium, barium, &c., are in the same relation to the protoxides of the same metals, and present the same results under the influence of the electric current (402.).

764. From all the experiments, combined with these considerations, I conclude that muriatic acid is decomposed by the direct influence of the electric current, and that the quantities evolved are, and therefore the chemical action is, *definite for a definite quantity of electricity*. For though I have not collected and measured the chlorine, in its separate state, at the *anode*, there can exist no doubt as to its being proportional to the hydrogen at the *cathode*; and the results are therefore sufficient to establish the general law of *constant electro-chemical action* in the case of muriatic acid.

765. In the dilute acid (761.), I conclude that a part of the water is electro-chemically decomposed, giving origin to the oxygen, which appears mingled with the chlorine at the *anode*. The oxygen *may* be viewed as a secondary result; but I incline to believe that it is not so: for, if it were, it might be expected in largest proportion from the stronger acid, whereas the reverse is the fact. This consideration, with others, also leads me to conclude that muriatic acid is more easily decomposed by the electric current than water; since, even when diluted with eight or nine times its quantity of the latter fluid, it alone gives way, the water remaining unaffected.

766. *Chlorides.*—On using solutions of chlorides in water,—for instance, the chlorides of sodium or calcium,—there was evolution of chlorine only at the positive electrode, and of hydrogen, with the oxide of the base, as soda or lime, at the negative electrode. The process of decomposition may be viewed as proceeding in two or three ways, all terminating in the same results. Perhaps the simplest is to consider the chloride as the substance electrolyzed, its chlorine being determined to and evolved at the *anode*, and its metal passing to the *cathode*, where, finding no more chlorine, it acts upon the water, producing hydrogen and an oxide as secondary results. As the discussion would detain me from more important matter, and is not of immediate consequence, I shall defer it for the present. It is, however, of *great consequence* to state, that, on using the volta-electrometer, the hydrogen in both cases was definite; and if the results do not prove the definite decomposition of chlorides, (which shall be proved elsewhere,—789. 794. 814.,) they are not in the slightest degree opposed to such a conclusion, and *do* support the *general law*.

767. *Hydriodic acid.*—A solution of hydriodic acid was affected exactly in the same manner as muriatic acid. When strong, hydrogen was evolved at the negative electrode, in definite proportion to the quantity of electricity which had passed, i. e. in

the same proportion as was evolved by the same current from water; and iodine with out any oxygen was evolved at the positive electrode. But when diluted, small quantities of oxygen appeared with the iodine at the *anode*, the proportion of hydrogen at the *cathode* remaining undisturbed.

768. I believe the decomposition of the hydriodic acid in this case to be direct, for the reasons already given respecting muriatic acid (763. 764.).

769. *Iodides.*—A solution of iodide of potassium being subjected to the voltaic current, iodine appeared at the positive electrode (without any oxygen), and hydrogen with free alkali at the negative electrode. The same observations as to the mode of decomposition are applicable here as were made in relation to the chlorides when in solution (766.).

770. *Hydro-fluoric acid and fluorides.*—Solution of hydro-fluoric acid did not appear to be decomposed under the influence of the electric current: it was the water which gave way apparently. The fused fluorides were electrolyzed (417.); but having during these actions obtained *fluorine* in the separate state, I think it better to refer to a future series of these Researches, in which I purpose giving a fuller account of the results than would be consistent with propriety here.

771. *Hydro-cyanic acid* in solution conducts very badly. The definite proportion of hydrogen (equal to that from water) was set free at the *cathode*, whilst at the *anode* a small quantity of oxygen was evolved and apparently a solution of cyanogen formed. The action altogether corresponded with that on a dilute muriatic or hydriodic acid. When the hydro-cyanic acid was made a better conductor by sulphuric acid, the same results occurred.

Cyanides.—With a solution of the cyanide of potassium, the result was precisely the same as with a chloride or iodide. No oxygen was evolved at the positive electrode, but a brown solution formed there. For the reasons given when speaking of the chlorides (766.), and because a fused cyanide of potassium evolves cyanogen at the positive electrode*, I incline to believe that the cyanide in solution is *directly* decomposed.

772. *Ferro-cyanic acid* and the *ferro-cyanides,* as also *sulpho-cyanic acid* and the *sulpho-cyanides,* presented results corresponding with those just described (771.).

773. *Acetic acid.* Glacial acetic acid, when fused (405.), is not decomposed by, nor does it conduct, electricity. On adding a little water to it, still there were no signs of action; on adding more water, it acted slowly and about as water alone would do. Dilute sulphuric acid was added to it in order to make it a better conductor; then the definite proportion of hydrogen was evolved at the *cathode*, and a mixture of oxygen in very deficient quantity, with carbonic acid, and a little carbonic oxide, at the *anode.* Hence it appears that acetic acid is not electrolyzable, but that a portion of it is decomposed by the oxygen evolved at the *anode*, producing secondary results,

* It is a very remarkable thing to see carbon and nitrogen in this case determined powerfully towards the positive surface of the voltaic battery; but it is perfectly in harmony with the theory of electro-chemical decomposition which I have advanced.

varying with the strength of the acid, the intensity of the current, and other circumstances.

774. *Acetates.*—One of these has been referred to already, as affording only secondary results relative to the acetic acid (749.). With many of the metallic acetates the results at both electrodes are secondary (746. 750.).

Acetate of soda fused and anhydrous is directly decomposed, being, as I believe, a true electrolyte, and evolving soda and acetic acid at the *cathode* and *anode*. These, however, have no sensible duration, but are immediately resolved into other substances; charcoal, sodiuretted hydrogen, &c., being set free at the former, and as far as I could judge under the circumstances, acetic acid mingled with carbonic oxide, carbonic acid, &c., at the latter.

775. *Tartaric acid.*—Pure solution of tartaric acid is almost as bad a conductor as pure water. On adding sulphuric acid to it, it conducted well, the results at the positive electrode being primary or secondary in different proportions, according to variations in the strength of the acid and the power of the electric current (752.). Alkaline tartrates gave a large proportion of secondary results at the positive electrode. The hydrogen at the negative electrode remained constant unless certain metallic salts were used.

776. Solutions of salts containing other vegetable acids, as the benzoates; of sugar, gum, &c., dissolved in dilute sulphuric acid; of resin, albumen, &c., dissolved in alkalies, were in turn submitted to the electrolytic power of the voltaic current. In all these cases, secondary results to a greater or smaller extent were produced at the positive electrode.

777. In concluding this division of these Researches, it cannot but occur to the mind that the final result of the action of the electric current upon substances placed between the electrodes, instead of being simple may be very complicated. There are two modes by which these substances may be decomposed, either by the direct force of the electric current, or by the action of bodies which that current may evolve. There are also two modes by which new compounds may be formed, i. e. by combination of the evolving substances whilst in their nascent state (658.), directly with the matter of the electrode; or else their combination with those bodies, which being contained in, or associated with, the decomposing conductor, are necessarily present at the *anode* and *cathode*. The complexity is rendered still greater by the circumstance that two or more of these actions may occur simultaneously, and also in variable proportions to each other. But it may in a great measure be resolved by attention to the principles already laid down (747.).

778. When *aqueous* solutions of bodies are used, secondary results are exceedingly frequent. Even when the water is not present in large quantity, but is merely that of combination, still secondary results often ensue: for instance, it is very possible that in Sir Humphry Davy's decomposition of the hydrates of potassa and soda, a part of the potassium produced was the result of a secondary action. Hence, also, a frequent

cause for the disappearance of the oxygen and hydrogen which would otherwise be evolved: and when hydrogen does *not* appear at the *cathode* in an *aqueous solution,* it perhaps always indicates that a secondary action has taken place there. No exception to this rule has as yet occurred to my observation.

779. Secondary actions are *not confined to aqueous solutions,* or cases where water is present. For instance, various chlorides acted upon, when fused (402.), by platina electrodes, have the chlorine determined electrically to the *anode.* In many cases, as with the chlorides of lead, potassium, barium, &c., the chlorine acts on the platina and forms a compound with it, which dissolves; but when protochloride of tin is used, the chlorine at the *anode* does not act upon the platina, but upon the chloride already there, forming a perchloride which rises in vapour (790. 804.). These are, therefore, instances of secondary actions of both kinds, produced in bodies containing no water.

780. The production of boron from fused borax (402. 417.) is also a case of secondary action; for boracic acid is not decomposable by electricity (408.), and it was the sodium evolved at the *cathode* which, reacting on the boracic acid around it, took oxygen from it and set boron free in the experiments formerly described.

781. Secondary actions have already, in the hands of M. Becquerel, produced many interesting results in the formation of compounds; some of them new, others imitations of those occurring naturally*. It is probable they may prove equally interesting in an opposite direction, i. e. as affording cases of analytic decomposition. Much information regarding the composition, and perhaps even the arrangement of the particles of such bodies as the vegetable acids and alkalies, and organic compounds generally, will probably be obtained by submitting them to the action of nascent oxygen, hydrogen, chlorine, &c., at the electrodes; and the action seems the more promising, because of the thorough command which we possess over attendant circumstances, such as the strength of the current, the size of the electrodes, the nature of the decomposing conductor, its strength, &c., all of which may be expected to have their corresponding influence upon the final result.

782. It is to me a great satisfaction that the extreme variety of secondary results have presented nothing opposed to the doctrine of a constant and definite electrochemical action, to the particular consideration of which I shall now proceed.

¶ vii. *On the definite nature and extent of Electro-chemical Decomposition.*

783. In the third series of these Researches, after proving the identity of electricities derived from different sources, and showing, by actual measurement, the extraordinary quantity of electricity evolved by a very feeble voltaic arrangement (371. 376.), I announced a law, derived from experiment, which seemed to me of the utmost importance to the science of electricity in general, and that branch of it denominated electro-chemistry in particular. The law was expressed thus: *The chemical power of*

* Annales de Chimie, tom. xxxv. p. 113.

a current of electricity is in direct proportion to the absolute quantity of electricity which passes (377.).

784. In the further progress of the successive investigations, I have had frequent occasion to refer to the same law, occasionally in circumstances offering powerful corroboration of its truth (456. 504. 505.); and the present series already supplies numerous new cases in which it holds good (704. 722. 726. 732.). It is now my object to consider this great principle more closely, and to develope some of the consequences to which it leads. That the evidence for it may be the more distinct and applicable, I shall quote cases of decomposition subject to as few interferences from secondary results as possible, effected upon bodies very simple, yet very definite in their nature.

785. In the first place, I consider the law as so fully established with respect to the decomposition of *water*, and under so many circumstances which might be supposed, if anything could, to exert an influence over it, that I may be excused entering into further detail respecting that substance, or even summing up the results here (732). I refer, therefore, to the whole of the subdivision of this series of Researches which contains the account of the *volta-electrometer*.

786. In the next place, I also consider the law as established with respect to *muriatic acid* by the experiments and reasoning already advanced, when speaking of that substance, in the subdivision respecting primary and secondary results (758, &c.).

787. I consider the law as established also with regard to *hydriodic acid* by the experiments and considerations already advanced in the preceding division of this series of Researches (767. 768.).

788. Without speaking with the same confidence, yet from the experiments described, and many others not described, relating to hydro-fluoric, hydro-cyanic, ferro-cyanic, and sulpho-cyanic acids (770. 771. 772.), and from the close analogy which holds between these bodies and the hydro-acids of chlorine, iodine, bromine, &c., I consider these also as coming under subjection to the law, and assisting to prove its truth.

789. In the preceding cases, except the first, the water is believed to be inactive; but to avoid any ambiguity arising from its presence, I sought for substances from which it should be absent altogether; and, taking advantage of the law of conduction already developed (380. &c.), soon found abundance, amongst which *protochloride of tin* was first subjected to decomposition in the following manner. A piece of platina wire had one extremity coiled up into a small knob, and having been carefully weighed, was sealed hermetically into a piece of bottle-glass tube, so that the knob should be at the bottom of the tube within (fig. 13.). The tube was suspended by a piece of platina wire, so that the heat of a spirit-lamp could be applied to it. Recently fused protochloride of tin was introduced in sufficient quantity to occupy, when melted, about one half of the tube; the wire of the tube was connected with a volta-electrometer (711.), which was itself connected with the negative end of a voltaic battery; and a platina wire connected with the positive end of the same battery was dipped into the

fused chloride in the tube; being, however, so bent, that it could not by any shake of the hand or apparatus touch the negative electrode at the bottom of the vessel. The whole arrangement is delineated fig. 14.

790. Under these circumstances the chloride of tin was decomposed: the chlorine evolved at the positive electrode formed bichloride of tin (779.), which passed away in fumes, and the tin evolved at the negative electrode combined with the platina, forming an alloy, fusible at the temperature to which the tube was subjected, and therefore never occasioning metallic communication entirely through the decomposing chloride. When the experiment had been continued so long as to yield a reasonable quantity of gas in the volta-electrometer, the battery connexion was broken, the positive electrode removed, and the tube and remaining chloride allowed to cool. When cold, the tube was broken open, the rest of the chloride and the glass being easily separable from the platina wire and its button of alloy. The latter when washed was then reweighed, and the increase gave the weight of the tin reduced.

791. I will give the particular results of one experiment, in illustration of the mode adopted in this and others, the results of which I shall have occasion to quote. The negative electrode weighed at first 20 grains; after the experiment it, with its button of alloy, weighed 23·2 grains. The tin evolved by the electric current at the *cathode* weighed, therefore, 3·2 grains. The quantity of oxygen and hydrogen collected in the volta-electrometer = 3·85 cubic inches. As 100 cubic inches of oxygen and hydrogen, in the proportions to form water, may be considered as weighing 12·92 grains, the 3·85 cubic inches would weigh 0·49742 of a grain; that being, therefore, the weight of water decomposed by the same electric current as was able to decompose such weight of protochloride of tin as could yield 3·2 grains of metal. Now 0·49742 : 3·2 : : 9 the equivalent of water is to 57·9, which should therefore be the equivalent of tin, if the experiment had been made without error, and if the electro-chemical decomposition *is in this case also definite.* In some chemical works 58 is given as the chemical equivalent of tin, in others 57·9. Both are so near to the result of the experiment, and the experiment itself is so subject to slight causes of variation (as from the absorption of gas in the volta-electrometer (716.), &c.), that the numbers leave little doubt of the applicability of the *law of definite action* in this and all similar cases of electro-decomposition.

792. It is not often I have obtained an accordance in numbers so near as that I have just quoted. Four experiments were made on the protochloride of tin, the quantities of gas evolved in the volta-electrometer being from 2·05 to 10·29 cubic inches. The average of the four experiments gave 58·53 as the electro-chemical equivalent for tin.

793. The chloride remaining after the experiment, was pure protochloride of tin; and no one can doubt for a moment that the equivalent of chlorine had been evolved at the *anode*, and having formed bichloride of tin as a secondary result, had passed away.

794. *Chloride of lead* was experimented upon in a manner exactly similar, except that a change was made in the nature of the positive electrode; for as the chlorine evolved at the *anode* forms no perchloride of lead, but acts directly upon the platina, if that metal be used, it produces a solution of chloride of platina in the chloride of lead; in consequence of which a portion of platina can pass to the *cathode,* and will produce a vitiated result. I therefore sought for, and found in plumbago, another substance, which could be used safely as the positive electrode in such bodies as chlorides, iodides, &c. The chlorine or iodine does not act upon it, but is evolved in the free state; and the plumbago has no reaction, under the circumstances, upon the fused chloride or iodide in which it is plunged. Even if a few particles of plumbago should separate by the heat or the mechanical action of the evolved gas, they can do no harm in the chloride.

795. The mean of three experiments gave the number of 100·85 as the equivalent for lead. The chemical equivalent is 103·5. The deficiency in my experiments I attribute to the solution of part of the gas (716.) in the volta-electrometer; but the results leave no doubt on my mind that both the lead and the chlorine are, in this case, evolved in *definite quantities* by the action of a given quantity of electricity (814. &c.).

796. *Chloride of antimony.*—It was in endeavouring to obtain the electro-chemical equivalent of antimony from the chloride that I found reasons for the statement I have made respecting the presence of water in it in an earlier part of these Researches (690. 693. &c.).

797. I endeavoured to experiment upon the *oxide of lead* obtained by fusion and ignition of the nitrate in a platina crucible, but found great difficulty, from the high temperature required for perfect fusion, and the powerful fluxing qualities of the substance. Green glass tubes repeatedly failed. I at last fused the oxide in a small porcelain crucible, heated fully in a charcoal fire; and as it was essential that the evolution of the lead at the *cathode* should take place beneath the surface, the negative electrode was guarded by a green glass tube, fused around it in such a manner as to expose only the knob of platina at the lower end (fig. 15.), so that it could be plunged beneath the surface, and thus exclude contact of air or oxygen with the lead reduced there. A platina wire was employed for the positive electrode, that metal not being subject to any action from the oxygen evolved against it. The arrangement is given fig. 16.

798. In an experiment of this kind the equivalent for the lead came out 93·17, which is very much too small. This, I believe, was because of the small interval between the positive and negative electrodes in the oxide of lead, so that it was not unlikely that some of the froth and bubbles formed by the oxygen at the *anode* should occasionally even touch the lead reduced at the *cathode,* and re-oxidize it. When I endeavoured to correct this by having more litharge, the greater heat required to keep it all fluid caused a quicker action on the crucible, which was soon eaten through, and the experiment stopped.

799. In one experiment of this kind I used borate of lead (408. 673.). It evolves lead, under the influence of the electric current, at the *anode*, and oxygen at the *cathode*; and as the boracic acid is not either directly (408.) or incidentally decomposed during the operation, I expected a result dependent on the oxide of lead. The borate is not so violent a flux as the oxide, but it requires a higher temperature to make it quite liquid; and if not very hot, the bubbles of oxygen cling to the positive electrode, and retard the transfer of electricity. The number for lead came out 101·29, which is so near to 103·5 as to show that the action of the current had been definite.

800. *Oxide of bismuth.*—I found this substance required too high a temperature, and acted too powerfully as a flux, to allow of any experiment being made on it, without the application of more time and care than I could give at present.

801. The ordinary *protoxide of antimony*, which consists of one proportional of metal and one and a half of oxygen, was subjected to the action of the electric current in a green glass tube (789.), surrounded by a jacket of platina foil, and heated in a charcoal fire. The decomposition began and proceeded very well at first, apparently indicating, according to the general law (679. 697.), that this substance was one containing such elements and in such proportions as made it amenable to the power of the electric current. This effect I have already given reasons for supposing may be due to the presence of a true protoxide, consisting of single proportionals (696. 693.). The action soon diminished, and finally ceased, because of the formation of a higher oxide of the metal at the positive electrode. This compound, which was probably the peroxide, being infusible and insoluble in the protoxide, formed a crystalline crust around the positive electrode; and thus insulating it, prevented the transmission of the electricity. Whether if it had been fusible and still immiscible it would have decomposed, is doubtful, because of its departure from the required composition (697.). It was a very natural secondary product at the positive electrode (779.). On opening the tube it was found that a little antimony had been separated at the negative electrode; but the quantity was too small to allow of any quantitative result being obtained.

802. *Iodide of lead.*—This substance can be experimented with in tubes heated by a spirit-lamp (789.); but I obtained no good results from it, whether I used positive electrodes of platina or plumbago. In two experiments the numbers for the lead came out only 75·46 and 73·45, instead of 103·5. This I attribute to the formation of a periodide at the positive electrode, which dissolving in the mass of liquid iodide, came in contact with the lead evolved at the negative electrode, and dissolved part of it, becoming itself again protiodide. Such a periodide does exist; and it is very rarely that the iodide of lead formed by precipitation, and well washed, can be fused without evolving much iodine, from the presence of this percompound; nor does crystallization from its hot aqueous solution free it from this substance. Even when a little of the protiodide and iodine are merely rubbed together in a mortar, a portion of the periodide is formed. And though it is decomposed by being fused and heated

to dull redness for a few minutes, and the whole reduced to protiodide, yet that is not at all opposed to the possibility, that a little of that which is formed in great excess of iodine at the *anode*, should be carried by the rapid currents in the liquid into contact with the *cathode*.

803. This view of the results was strengthened by a third experiment, where the space between the electrodes was increased to one third of an inch; for now the interfering effects were much diminished, and the number of the lead came out 89·04; and it was fully confirmed by the results obtained in the cases of transfer to be immediately described (818.).

The experiments on iodide of lead, therefore, offer no exception to the *general law* under consideration, but, on the contrary, may, from general considerations, be admitted as included in it.

804. *Protiodide of tin.*—This substance, when fused (402.), conducts and is decomposed by the electric current, tin is evolved at the *anode*, and periodide of tin as a secondary result (779. 790.) at the *cathode*. The temperature required for its fusion is too high to allow of the production of any results fit for weighing.

805. *Iodide of potassium* was subjected to electrolytic action in a tube, fig. 13. (789.). The negative electrode was a globule of lead, and I hoped in this way to retain the potassium, and obtain results that could be weighed and compared with the volta-electrometer indication; but the difficulties dependent upon the high temperature required, the action upon the glass, the fusibility of the platina induced by the presence of the lead, and other circumstances, prevented me from obtaining such results. The iodide was decomposed with the evolution of iodine at the *anode*, and of potassium at the *cathode*, as in former cases.

806. In some of these experiments several substances were placed in succession, and decomposed simultaneously by the same electric current: thus, protochloride of tin, chloride of lead, and water, were thus acted on at once. It is needless to say that the results were comparable, the tin, lead, chlorine, oxygen, and hydrogen evolved being definite in quantity and electro-chemical equivalents to each other.

807. Let us turn to another kind of proof of the *definite chemical action of electricity*. If any circumstances could be supposed to exert an influence over the quantity of the matters evolved during electrolytic action, one would expect them to be present when electrodes of different substances, and possessing very different chemical affinities for the evolving bodies, were used. Platina has no power in dilute sulphuric acid of combining with the oxygen at the *anode*, though the latter be evolved in the nascent state against it. Copper, on the other hand, immediately unites to the oxygen, as the electric current sets it free from the hydrogen; and zinc is not only able to combine with it, but can, without any help from the electricity, abstract it directly from the water, at the same time setting torrents of hydrogen free. Yet in cases where these three substances were used as the positive electrodes in three similar portions of the same dilute sulphuric acid, specific gravity 1·336, precisely the same quantity of water

was decomposed by the electric current, and precisely the same quantity of hydrogen set free at the *cathodes* of the three solutions.

808. The experiment was made thus. Portions of the dilute sulphuric acid were put into three basins. Three volta-electrometer tubes, of the form figg. 5, 7. were filled with the same acid, and one inverted in each basin (707.). A zinc plate, connected with the positive end of a voltaic battery, was dipped into the first basin, forming the positive electrode there, the hydrogen, which was abundantly evolved from it by the direct action of the acid, being allowed to escape. A copper plate, which dipped into the acid of the second basin, was connected with the negative electrode of the *first* basin; and a platina plate, which dipped into the acid of the third basin, was connected with the negative electrode of the *second* basin. The negative electrode of the third basin was connected with a volta-electrometer (711.), and that with the negative end of the voltaic battery.

809. Immediately that the circuit was complete, the *electro-chemical action* commenced in all the vessels. The hydrogen still rose in, apparently, undiminished quantities from the positive zinc electrode in the first basin. No oxygen was evolved at the positive copper electrode in the second basin, but a sulphate of copper was formed there; whilst in the third basin the positive platina electrode evolved pure oxygen gas, and was itself unaffected. But in *all* the basins the hydrogen liberated at the *negative* platina electrodes was the *same in quantity,* and the same with the volume of hydrogen evolved in the volta-electrometer, showing that in all the vessels the current had decomposed an equal quantity of water. In this trying case, therefore, the *chemical action of electricity* proved to be *perfectly definite.*

810. A similar experiment was made with muriatic acid diluted with its bulk of water. The three positive electrodes were zinc, silver, and platina; the first being able to separate and combine with the chlorine *without* the aid of the current; the second combining with the chlorine only after the current had set it free; and the third rejecting almost the whole of it. The three negative electrodes were, as before, platina plates fixed within glass tubes. In this experiment, as in the former, the quantity of hydrogen evolved at the *cathodes* was the same for all, and the same as the hydrogen evolved in the volta-electrometer. I have already given my reasons for believing that in these experiments it is the muriatic acid which is directly decomposed by the electricity (764.); and the results prove that the quantities so decomposed are *perfectly definite* and proportionate to the quantity of electricity which has passed.

811. In this experiment the chloride of silver formed in the second basin retarded the passage of the current of electricity, by virtue of the law of conduction before described (394.), so that it had to be cleaned off four or five times during the course of the experiment; but this caused no difference between the results of that vessel and the others.

812. Charcoal was used as the positive electrode in both sulphuric and muriatic acids (808. 810.); but this change produced no variation of the results. A zinc positive

electrode, in sulphate of soda or solution of common salt, gave the same constancy of operation.

813. Experiments of a similar kind were then made with bodies altogether in a different state, i. e. with *fused* chlorides, iodides, &c. I have already described an experiment with fused chloride of silver, in which the electrodes were of metallic silver, the one rendered negative becoming increased and lengthened by the addition of metal, whilst the other was dissolved and eaten away by its abstraction. This experiment was repeated, two weighed pieces of silver wire being used as the electrodes, and a volta-electrometer included in the circuit. Great care was taken to withdraw the negative electrode so regularly and steadily that the crystals of reduced silver should not form a *metallic* communication beneath the surface of the fused chloride. On concluding the experiment the positive electrode was re-weighed, and its loss ascertained. The mixture of chloride of silver, and metal, withdrawn in successive portions at the negative electrode, was digested in solution of ammonia, to remove the chloride, and the metallic silver remaining also weighed: it was the reduction at the *cathode*, and exactly equalled the solution at the *anode*; and each portion was as nearly as possible the equivalent to the water decomposed in the volta-electrometer.

814. The infusible condition of the silver at the temperature used, and the length and ramifying character of its crystals, render the above experiment difficult to perform, and uncertain in its results. I therefore wrought with a chloride of lead, using a green glass tube, formed as in fig. 17. A weighed platina wire was fused into the bottom of a small tube, as before described (789.). The tube was then bent to an angle, at about half an inch distance from the closed end; and the part between the angle and the extremity being softened, was forced upward, as in the figure, so as to form a bridge, or rather separation, producing two little depressions or basins *a*, *b*, within the tube. This arrangement was suspended by a platina wire, as before, so that the heat of a spirit-lamp could be applied to it, such inclination being given to it as would allow all air to escape during the fusion of the chloride of lead. A positive electrode was then provided, by binding up the end of a platina wire into a knob, and fusing about twenty grains of metallic lead on to it, in a small closed tube of glass, which was afterwards broken away. Being so furnished, the wire with its knob was weighed, and the weight recorded.

815. Chloride of lead was now introduced into the tube, and carefully fused. The leaded electrode was also introduced; after which the metal, at its extremity, soon melted. In this state of things the tube was filled up to *c* with melted chloride of lead; the end of the electrode to be rendered negative was in the basin *b*, and the electrode of melted lead was retained in the basin *a*, and, by connexion with the proper conducting wire of a voltaic battery, was rendered positive. A volta-electrometer was included in the circuit.

816. Immediately upon the completion of the communication with the voltaic battery, the current passed, and decomposition proceeded. No chlorine was evolved at

the positive electrode; but as the fused chloride was transparent, a button of alloy could be observed gradually forming and increasing in size at *b*, whilst the lead at *a* could also be seen gradually to diminish. After a time, the experiment was stopped; the tube allowed to cool, and broken open; the wires, with their buttons, cleaned and weighed; and their change in weight compared with the indication of the *volta-electrometer*.

817. In this experiment the positive electrode had lost just as much lead as the negative one had gained (795.), and the loss or gain was very nearly the equivalent of the water decomposed in the volta-electrometer, giving for lead the number 101·5. It is therefore evident, in this instance, that causing a *strong affinity*, or *no affinity*, for the substance evolved at the *anode*, to be active during the experiment (807.), produces no variation in the definite action of the electric current.

818. A similar experiment was then made with iodide of lead, and in this manner all confusion from the formation of a periodide avoided (803.). No iodine was evolved during the whole action, and finally the loss of lead at the *anode* was the same as the gain at the *cathode*, the equivalent number, by comparison with the result in the volta-electrometer, being 103·5.

819. Then protochloride of tin was subjected to the electric current in the same manner, using, of course, a tin positive electrode. No bichloride of tin was now formed (779. 790.). On examining the two electrodes, the positive had lost precisely as much as the negative had gained; and by comparison with the volta-electrometer, the number for tin came out 59.

820. It is quite necessary in these and similar experiments to examine the interior of the bulbs of alloy at the ends of the conducting wires; for occasionally, and especially with those which have been positive, they are cavernous, and contain portions of the chloride or iodide used, which must be removed before the final weight is ascertained. This is more usually the case with lead than tin.

821. All these facts combine into, I think, an irresistible mass of evidence, proving the truth of the important proposition which I at first laid down, namely, *that the chemical power of a current of electricity is in direct proportion to the absolute quantity of electricity which passes* (377. 783.). They prove, too, that this is not merely true with one substance, as water, but generally with all electrolytic bodies; and, further, that the results obtained with any *one substance* do not merely agree amongst themselves, but also with those obtained from *other substances*, the whole combining together into *one series of definite electro-chemical actions* (505.). I do not mean to say that no exceptions will appear: perhaps some may arise, especially amongst substances existing only by weak affinity; but I do not expect that any will seriously disturb the result announced. If, in the well considered, well examined, and, I may surely say, well ascertained doctrines of the definite nature of ordinary chemical affinity, such exceptions occur, as they do in abundance, yet, without being allowed to disturb our minds as to the general conclusion, they ought also to be allowed if they should

present themselves at this, the opening of a new view of electro-chemical action; not being held up as obstructions to those who may be engaged in rendering that view more and more perfect, but laid aside for a while, in hopes that their perfect and consistent explanation will finally appear.

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822. The doctrine of *definite electro-chemical action* just laid down, and, I believe, established, leads to some new views of the relations and classifications of bodies associated with or subject to this action. Some of these I shall proceed to consider.

823. In the first place, compound bodies may be separated into two great classes, namely, those which are decomposable by the electric current, and those which are not. Of the latter, some are conductors, others non-conductors, of voltaic electricity*. The former do not depend for their decomposability, upon the nature of their elements only; for, of the same two elements, bodies may be formed, of which one shall belong to one class and another to the other class; but probably on the proportions also (697.). It is further remarkable, that with very few, if any, exceptions (414. 691.), these decomposable bodies are exactly those governed by the remarkable law of conduction I have before described (394.); for that law does not extend to the many compound fusible substances that are excluded from this class. I propose to call bodies of this, the decomposable class, *Electrolytes* (664.).

824. Then, again, the substances into which these divide, under the influence of the electric current, form an exceedingly important general class. They are combining bodies; are directly associated with the fundamental parts of the doctrine of chemical affinity; and have each a definite proportion, in which they are always evolved during electrolytic action. I have proposed to call these bodies generally *ions*, or particularly *anions* and *cations*, according as they appear at the *anode* or *cathode* (665.); and the numbers representing the proportions in which they are evolved *electro-chemical equivalents*. Thus hydrogen, oxygen, chlorine, iodine, lead, tin, are *ions*; the three former are *anions*, the two metals are *cations*, and 1, 8, 36, 125, 104, 58, are their *electro-chemical equivalents* nearly.

825. A summary of certain points already ascertained respecting *electrolytes, ions*, and *electro-chemical equivalents*, may be given in the following general form of propositions, without, I hope, including any serious error.

826. i. A single *ion*, i. e. one not in combination with another, will have no tendency to pass to either of the electrodes, and will be perfectly indifferent to the passing current, unless it be itself a compound of more elementary *ions*, and so subject to actual decomposition. Upon this fact is founded much of the proof adduced in favour of the new theory of electro-chemical decomposition, which I put forth in a former series of these Researches (518. &c.).

827. ii. If one *ion* be combined in right proportions (697.) with another strongly

* I mean here by voltaic electricity, merely electricity from a most abundant source, but having very small intensity.

opposed to it in its ordinary chemical relations, i. e. if an *anion* be combined with a *cation*, then both will travel, the one to the *anode*, the other to the *cathode*, of the decomposing body (530. 542. 547.).

828. iii. If, therefore, an *ion* pass towards one of the electrodes, another *ion* must also be passing simultaneously to the other electrode, although, from secondary action, it may not make its appearance (743.).

829. iv. A body decomposable directly by the electric current, i. e. an *electrolyte*, must consist of two *ions*, and must also render them up during the act of decomposition.

830. v. There is but one *electrolyte* composed of the same two elementary *ions*; at least such appears to be the fact (697.), dependent upon a law, that *only single electro-chemical equivalents of elementary ions can go to the electrodes, and not multiples.*

831. vi. A body not decomposable when alone, as boracic acid, is not directly decomposable by the electric current when in combination (780.). It may act as an *ion*, going wholly to the *anode* or *cathode*, but does not yield up its elements, except occasionally by a secondary action. Perhaps it is superfluous for me to point out that this proposition has *no relation* to such cases as that of water, which, by the presence of other bodies, is rendered a better conductor of electricity, and *therefore* is more freely decomposed.

832. vii. The nature of the substance of which the electrode is formed, provided it be a conductor, causes no difference in the electro-decomposition, either in kind or degree (807. 813.); but it seriously influences, by secondary action (744.), the state in which the *ions* finally appear. Advantage may be taken of this principle in combining and collecting such *ions* as, if evolved in their free state, would be unmanageable *.

833. viii. A substance which, being used as the electrode, can combine altogether with the *ion* evolved against it, is also, I believe, an *ion*, and combines, in such cases, in the quantity represented by its *electro-chemical equivalent*. All the experiments I have made agree with this view; and it seems to me, at present, to result as a necessary consequence. Whether, in the secondary actions that take place, where the *ion* acts, not upon the matter of the electrode, but on that which is around it in the liquid (744.), the same consequence follows, will require more extended investigation to determine.

834. ix. Compound *ions* are not necessarily composed of electro-chemical equivalents of simple *ions*. For instance, sulphuric acid, boracic acid, phosphoric acid, are *ions*, but not *electrolytes*, i. e. not composed of electro-chemical equivalents of simple *ions*.

* It will often happen that the electrodes used may be of such a nature as, with the fluid in which they are immersed, to produce an electric current, either according with or opposing that of the voltaic arrangement used, and in this way, or by direct chemical action, may sadly disturb the results. Still, in the midst of all these confusing effects, the electric current, which actually passes in any direction through the decomposing body, will produce its own definite electrolytic action.

835. x. Electro-chemical equivalents are always consistent; i. e. the same number which represents the equivalent of a substance A when it is separating from a substance B, will also represent A when separating from a third substance C. Thus, 8 is the electro-chemical equivalent of oxygen, whether separating from hydrogen, or tin, or lead; and 103·5 is the electro-chemical equivalent of lead, whether separating from oxygen, or chlorine, or iodine.

836. xi. Electro-chemical equivalents coincide, and are the same, with ordinary chemical equivalents.

837. By means of experiment and the preceding propositions, a knowledge of *ions* and their electro-chemical equivalents may be obtained in various ways.

838. In the first place, they may be determined directly, as has been done with hydrogen, oxygen, lead, and tin, in the numerous experiments already quoted.

839. In the next place, from propositions ii. and iii., may be deduced the knowledge of many other *ions*, and also their equivalents. When chloride of lead was decomposed, platina being used for both electrodes (395.), there could remain no more doubt that chlorine was passing to the *anode*, although it combined with the platina there, than when the positive electrode, being of plumbago (794.), allowed its evolution in the free state; neither could there, in either case, remain any doubt, that for every 103·5 parts of lead evolved at the *cathode*, 36 parts of chlorine were evolved at the *anode*, for the remaining chloride of lead was unchanged. So also when in a metallic solution one volume of oxygen, or a secondary compound containing that proportion, appeared at the *anode*, no doubt could arise that hydrogen, equivalent to two volumes, had been determined to the *cathode*, although, by a secondary action, it had been employed in reducing oxides of lead, copper, or other metals, to the metallic state. In this manner, then, we learn from the experiments already described in these Researches, that chlorine, iodine, bromine, fluorine, calcium, potassium, strontium, magnesium, manganese, &c., are *ions*, and that their *electro-chemical equivalents* are the same as their *ordinary chemical equivalents*.

840. Propositions iv. and v. extend our means of gaining information. For if a body of known chemical composition is found to be decomposable, and the nature of the substance evolved as a primary or even a secondary result (743. 777.) at one of the electrodes, be ascertained, the electro-chemical equivalent of that body may be deduced from the known constant composition of the substance evolved. Thus, when fused protiodide of tin is decomposed by the voltaic current (804.), the conclusion may be drawn, that both the iodine and tin are *ions*, and that the proportions in which they combine in the fused compound express their electro-chemical equivalents. Again, with respect to the fused iodide of potassium (805.), it is an electrolyte; and the chemical equivalents will also be the electro-chemical equivalents.

841. If proposition viii. sustain extensive experimental investigation, then it will not only help to confirm the results obtained by the use of the other propositions, but will give abundant original information of its own.

842. In many instances, the *secondary results* obtained by the action of the evolving *ion* on the substances present in the surrounding liquid or solution, will give the electro-chemical equivalent. Thus, in the solution of acetate of lead, and, as far as I have gone, in other proto-salts subjected to the reducing action of the nascent hydrogen at the *cathode,* the metal precipitated has been in the same quantity as if it had been a primary product, (provided no free hydrogen escaped there,) and therefore gave as accurately the number representing its electro-chemical equivalent.

843. Upon this principle it is that secondary results may occasionally be used as measurers of the volta-electric current (706. 740.); but there are not many metallic solutions that answer this purpose well: for unless the metal is easily precipitated, hydrogen will be evolved at the *cathode* and vitiate the result. If a soluble peroxide is formed at the *anode,* or if the precipitated metal crystallize across the solution and touch the positive electrode, similar vitiated results are obtained. I expect to find in some vegetable salts, as the acetates of mercury and zinc, solutions favourable for this use.

844. After the first experimental investigations to establish the definite chemical action of electricity, I have not hesitated to apply the more strict results of chemical analysis to correct the numbers obtained as electrolytical results. This, it is evident, may be done in a great number of cases, without using too much liberty towards the due severity of scientific research. The series of numbers representing electro-chemical equivalents must, like those expressing the ordinary equivalents of chemically acting bodies, remain subject to the continual correction of experiment and sound reasoning.

845. I give the following brief Table of *ions* and their electro-chemical equivalents, rather as a specimen of a first attempt than as anything that can supply the want which must very quickly be felt, of a full and complete tabular account of this class of bodies. Looking forward to such a table as of extreme utility (if well constructed) in developing the intimate relation of ordinary chemical affinity to electrical actions, and identifying the two, not to the imagination merely, but to the conviction of the senses and a sound judgement, I may be allowed to express a hope, that the endeavour will always be to make it a table of *real,* and not *hypothetical,* electro-chemical equivalents; for we shall else overrun the facts, and lose all sight and consciousness of the knowledge lying directly in our path.

846. The equivalent numbers do not profess to be exact, and are taken almost entirely from the chemical results of other philosophers in whom I could repose more confidence, as to these points, than in myself.

847. TABLE OF IONS.

Anions.

Oxygen	8	Cyanogen	26	Phosphoric acid	35·7	Citric acid	58
Chlorine	35·5	Sulphuric acid	40	Carbonic acid	22	Oxalic acid	36
Iodine	126	Selenic acid	64	Boracic acid	24	Sulphur (?)	16
Bromine	78·3	Nitric acid	54	Acetic acid	51	Selenium (?)	
Fluorine	18·7	Chloric acid	75·5	Tartaric acid	66	Sulpho-cyanogen	

Cations.

Hydrogen	1	Tin	57·9	Mercury	200	Strontia	51·8
Potassium	39·2	Lead	103·5	Silver	108	Lime	28·5
Sodium	23·3	Iron	28	Platina	98·6?	Magnesia	20·7
Lithium	10	Copper	31·6	Gold	(?)	Alumina	(?)
Barium	68·7	Cadmium	55·8	———		Protoxides generally.	
Strontium	43·8	Cerium	46	Ammonia	17	Quinia	171·6
Calcium	20·5	Cobalt	29·5	Potassa	47·2	Cinchona	160
Magnesium	12·7	Nickel	29·5	Soda	31·3	Morphia	290
Manganese	27·7	Antimony	64·6?	Lithia	18	Vegeto-alkalies generally.	
Zinc	32·5	Bismuth	71	Baryta	76·7		

848. This Table might be further arranged into groups of such substances as either act with, or replace, each other. Thus, for instance, acids and bases act in relation to each other; but they do not act in association with oxygen, hydrogen, or elementary substances. There is indeed little or no doubt that, when the electrical relations of the particles of matter come to be closely examined, this division must be made. The simple substances, with cyanogen, sulpho-cyanogen, and one or two other compound bodies, will probably form the first group; and the acids and bases, with such analogous compounds as may prove to be *ions*, the second group. Whether these will include all *ions*, or whether a third class of more complicated results will be required, must be decided by future experiments.

849. It is *probable* that all our present elementary bodies are *ions*, but that is not as yet certain. There are some, such as carbon, phosphorus, nitrogen, silicon, boron, alumium, the right of which to the title of *ion* it is desirable to decide as soon as possible. There are also many compound bodies, and amongst them alumina and silica, which it is desirable to class immediately by unexceptionable experiments. It is also *possible*, that all combinable bodies, compound as well as simple, may enter into the class of *ions*; but at present it does not seem to me probable. Still the experimental evidence I have is so small in proportion to what must gradually accumulate around, and bear upon, this point, that I am afraid to give a strong opinion upon it.

850. I think I cannot deceive myself in considering the doctrine of definite electro-chemical action as of the utmost importance. It touches by its facts more directly and closely than any former fact, or set of facts, have done, upon the beautiful idea, that ordinary chemical affinity is a mere consequence of the electrical attractions of the particles of different kinds of matter; and it will probably lead us to the means by which we may enlighten that which is at present so obscure, and either fully demonstrate the truth of the idea, or develope that which ought to replace it.

851. A very valuable use of electro-chemical equivalents will be to decide, in cases of doubt, what is the true chemical equivalent, or definite proportional, or atomic number of a body; for I have such conviction that the power which governs electro-decomposition and ordinary chemical attractions is the same; and such confidence in the overruling influence of those natural laws which render the former definite, as to

feel no hesitation in believing that the latter must submit to them also. Such being the case, I can have no doubt that, assuming hydrogen as 1, and dismissing small fractions for the simplicity of expression, the equivalent number or atomic weight of oxygen is 8, of chlorine 36, of bromine 78·4, of lead 103·5, of tin 59, &c., notwithstanding that a very high authority doubles several of these numbers.

§ 13. *On the absolute quantity of Electricity associated with the particles or atoms of Matter.*

852. The theory of definite electrolytical or electro-chemical action appears to me to touch immediately upon the *absolute quantity* of electricity or electric power belonging to different bodies. It is impossible, perhaps, to speak on this point without committing oneself beyond what present facts will sustain; and yet it is equally impossible, and perhaps would be impolitic, not to reason upon the subject. Although we know nothing of what an atom is, yet we cannot resist forming some idea of a small particle, which represents it to the mind; and though we are in equal, if not greater, ignorance of electricity, so as to be unable to say whether it is a particular matter or matters, or mere motion of ordinary matter, or some third kind of power or agent, yet there is an immensity of facts which justify us in believing that the atoms of matter are in some way endowed or associated with electrical powers, to which they owe their most striking qualities, and amongst them their mutual chemical affinity. As soon as we perceive, through the teaching of Dalton, that chemical powers are, however varied the circumstances in which they are exerted, definite for each body, we learn to estimate the relative degree of force which resides in such bodies: and when upon that knowledge comes the fact, that the electricity, which we appear to be capable of loosening from its habitation for a while, and conveying from place to place, *whilst it retains its chemical force*, can be measured out, and, being so measured, is found to be *as definite in its action* as any of *those portions* which, remaining associated with the particles of matter, give them their *chemical relation*; we seem to have found the link which connects the proportion of that we have evolved to the proportion of that belonging to the particles in their natural state.

853. Now it is wonderful to observe how small a quantity of a compound body is decomposed by a certain portion of electricity. Let us, for instance, consider this and a few other points in relation to water. *One grain* of water acidulated to facilitate conduction, will require an electric current to be continued for three minutes and three quarters of time to effect its decomposition, which current must be powerful enough to retain a platina wire $\frac{1}{104}$ of an inch in thickness*, red hot, in the air during the whole time; and if interrupted anywhere by charcoal points, will produce a very

* I have not stated the length of wire used, because I find by experiment, as would be expected in theory, that it is indifferent. The same quantity of electricity which, passed in a given time, can heat an inch of platina wire of a certain diameter red hot, can also heat a hundred, a thousand, or any length of the same wire to the same degree, provided the cooling circumstances are the same for every part in both cases. This I have proved by the volta-electrometer. I found that whether half an inch or eight inches were retained at one constant

brilliant and constant star of light. If attention be paid to the instantaneous discharge of electricity of tension, as illustrated in the beautiful experiments of Mr. Wheatstone*, and to what I have said elsewhere on the relation of common and voltaic electricity (371. 375.), it will not be too much to say, that this necessary quantity of electricity is equal to a very powerful flash of lightning. Yet we have it under perfect command; can evolve, direct, and employ it at pleasure; and when it has performed its full work of electrolyzation, it has only separated the elements of a single grain of water.

854. On the other hand, the relation between the conduction of the electricity and the decomposition of the water is so close, that one cannot take place without the other. If the water is altered only in that small degree which consists in its having the solid instead of the fluid state, the conduction is stopped, and the decomposition is stopped with it. Whether the conduction be considered as depending upon the decomposition, or not (413. 703.), still the relation of the two functions is equally intimate and inseparable.

855. Considering this close and twofold relation, namely, that without decomposition transmission of electricity does not occur; and, that for a given definite quantity of electricity passed, an equally definite and constant quantity of water or other matter is decomposed; considering also that the agent, which is electricity, is simply employed in overcoming electrical powers in the body subjected to its action; it seems a probable, and almost a natural consequence, that the quantity which passes is the *equivalent* of, and therefore equal to, that of the particles separated; i.e. that if the electrical power which holds the elements of a grain of water in combination, or which makes a grain of oxygen and hydrogen in the right proportions unite into water when they are made to combine, could be thrown into the condition of *a current*, it would exactly equal the current required for the separation of that grain of water into its elements again.

856. This view of the subject gives an almost overwhelming idea of the extraordinary quantity or degree of electric power which naturally belongs to the particles of matter; but it is not inconsistent in the slightest degree with the facts which can be brought to bear on this point. To illustrate this I must say a few words on the voltaic pile†.

temperature of dull redness, equal quantities of water were decomposed in equal times in both cases. When the half-inch was used, only the centre portion of wire was ignited. A fine wire may even be used as a rough but ready regulator of a voltaic current; for if it be made part of the circuit, and the larger wires communicating with it be shifted nearer to or further apart, so as to keep the portion of wire in the circuit sensibly at the same temperature, the current passing through it will be nearly uniform.

* Literary Gazette, 1833, March 1 and 8. Philosophical Magazine, 1833, p. 204. L'Institute, 1833, p. 261.

† By the term voltaic pile, I mean such apparatus or arrangement of metals as up to this time have been called so, and which contain water, brine, acids, or other aqueous solutions or decomposable substances (476.), between their plates. Other kinds of electric apparatus may be hereafter invented, and I hope to construct some not belonging to the class of instruments discovered by Volta.

857. Intending hereafter to apply the results given in this and the preceding series of Researches to a close investigation of the source of electricity in the voltaic instrument, I have refrained from forming any decided opinion on the subject; and without at all meaning to dismiss metallic contact, or the contact of dissimilar substances, being conductors, but not metallic, as if they had nothing to do with the origin of the current, I still am fully of opinion with Davy, that it is at least continued by chemical action, and that the supply constituting the current is almost entirely from that source.

858. Those bodies which, being interposed between the metals of the voltaic pile, render it active, *are all of them electrolytes* (476.); and it cannot but press upon the attention of every one engaged in considering this subject, that in those bodies (so essential to the pile) decomposition and the transmission of a current are so intimately connected, that one cannot happen without the other. This I have shown abundantly in water, and numerous other cases (402. 476.). If, then, a voltaic trough have its extremities connected by a decomposing body, as water, we shall have a continuous current through the apparatus; and whilst it remains in this state may look at the part where the acid is acting upon the plates, and that where the current is acting upon the water, as the reciprocals of each other. In both parts we have the two conditions *inseparable in such bodies as these*, namely, the passing of a current, and decomposition; and this is as true of the cells in the battery as of the water cell; for no voltaic battery has as yet been constructed in which the chemical action is only that of combination: decomposition is always included, and is, I believe, an essential chemical part.

859. But the difference in the two parts of the connected battery, that is, the decomposing or experimental cell, and the acting cells, is simply this. In the former we urge the current through, but it, apparently of necessity, is accompanied by decomposition: in the latter we cause decompositions by ordinary chemical actions, (which are, however, themselves electrical,) and, as a consequence, have the electrical current; and as the decomposition dependent upon the current is definite in the former case, so is the current associated with the decomposition also definite in the latter (862. &c.).

860. Let us apply this in support of what I have surmised respecting the enormous electric power of each particle or atom of matter (856.). I showed in a former series of these Researches on the relation by measure of common and voltaic electricity, that two wires, one of platina and one of zinc, each one eighteenth of an inch in diameter, placed five sixteenths of an inch apart, and immersed to the depth of five eighths of an inch in acid, consisting of one drop of oil of vitriol and four ounces of distilled water at a temperature of about 60° Fahr., and connected at the other extremities by a copper wire eighteen feet long, and one eighteenth of an inch in thickness, yielded as much electricity in little more than three seconds of time as a Leyden battery charged by thirty turns of a very large and powerful plate electric machine

in full action (371.). This quantity, though sufficient if passed at once through the head of a rat or a cat to have killed it, as by a flash of lightning, was evolved by the mutual action of so small a portion of the zinc wire and water in contact with it, that the loss of weight sustained by either would be inappreciable by our most delicate instruments; and as to the water which could be decomposed by that current, it must have been insensible in quantity, for no trace of hydrogen appeared upon the surface of the platina during those three seconds.

861. What an enormous quantity of electricity, therefore, is required for the decomposition of a single grain of water! We have already seen that it must be in quantity sufficient to sustain a platina wire $\frac{1}{104}$ of an inch in thickness, red hot, in contact with the air for three minutes and three quarters (853.), a quantity which is almost infinitely greater than that which could be evolved by the little standard voltaic arrangement to which I have just referred (860. 371.). I have endeavoured to make a comparison by the loss of weight of such a wire in a given time in such an acid, according to a principle and experiment to be almost immediately described (862.); but the proportion is so high, that I am almost afraid to mention it. It would appear that 800,000 such charges of the Leyden battery as I have referred to above, would be necessary to supply electricity sufficient to decompose a single grain of water; or, if I am right, to equal the quantity of electricity which is naturally associated with the elements of that grain of water, endowing them with their mutual chemical affinity.

862. In further proof of this high electric condition of the particles of matter, and the *identity as to quantity, of that belonging to them with that necessary for their separation*, I will describe an experiment of great simplicity but extreme beauty, when viewed in relation to the evolution of an electric current and its decomposing powers.

863. A dilute sulphuric acid, made by adding about one part by measure of oil of vitriol to thirty parts of water, will act energetically upon a piece of plate zinc in its ordinary and simple state; but, as Mr. Sturgeon has shown*, not at all, or scarcely so, if the surface of the metal has in the first instance been amalgamated; yet the amalgamated zinc will act powerfully with platina as an electromotor, hydrogen being evolved on the surface of the latter metal, as the zinc is oxidized and dissolved. The amalgamation is best effected by sprinkling a few drops of mercury upon the surface of the zinc, the latter being moistened with the dilute acid, and rubbing with the fingers so as to extend the liquid metal over the whole of the surface. Any mercury in excess forming liquid drops upon the zinc, should be wiped off†.

864. Two plates of zinc thus amalgamated were dried and accurately weighed; one, which we will call A, weighed 163·1 grains; the other, to be called B, weighed 148·3

* Recent Experimental Researches, &c., 1830, p. 74, &c.

† The experiment may be made with pure zinc, which, as chemists well know, is but slightly acted upon by dilute sulphuric acid in comparison with ordinary zinc, which during the action is subject to an infinity of voltaic actions. See De la Rive on this subject, Bibliothèque Universelle, 1830, p. 391.

grains. They were about five inches long, and 0·4 of an inch wide. An earthenware pneumatic trough was filled with dilute sulphuric acid, of the strength just described (863.), and a gas jar, also filled with the acid, inverted in it*. A plate of platina of nearly the same length, but about three times as wide as the zinc plates, was put up into this jar. The zinc plate A was also introduced into the jar, and brought in contact with the platina, and at the same moment the plate B was put into the acid of the trough, but out of contact with other metallic matter.

865. Strong action immediately occurred in the jar upon the contact of the zinc and platina plates. Hydrogen gas rose from the platina, and was collected in the jar, but no hydrogen or other gas rose from *either* zinc plate. In about ten or twelve minutes, sufficient hydrogen having been collected, the experiment was stopped; during its progress a few small bubbles had appeared upon plate B, but none upon plate A. The plates were washed in distilled water, dried, and reweighed. Plate B weighed 148·3 grains, as before, having lost nothing by the direct chemical action of the acid. Plate A weighed 154·65 grains, 8·45 grains of it having been oxidized and dissolved during the experiment.

866. The hydrogen gas was next transferred to a water-trough and measured; it amounted to 12·5 cubic inches, the temperature being 52°, and the barometer 29·2 inches. This quantity, corrected for temperature, pressure, and moisture, becomes 12·15453 cubic inches of dry hydrogen at mean temperature and pressure; which, increased by one half for the oxygen that must have gone to the *anode*, i. e. to the zinc, gives 18·232 cubic inches as the quantity of oxygen and hydrogen evolved from the water decomposed by the electric current. According to the estimate of the weight of the mixed gas before adopted (791.), this volume is equal to 2·3535544 grains, which therefore is the weight of water decomposed; and this quantity is to 8·45, the quantity of zinc oxidized, as 9 is to 32·31. Now taking 9 as the equivalent number of water, the number 32·5 is given as the equivalent number of zinc; a coincidence sufficiently near to show, what indeed could not but happen, that for an equivalent of zinc oxidized an equivalent of water must be decomposed†.

867. But let us observe *how* the water is decomposed. It is electrolyzed, i. e. is decomposed voltaically, and not in the ordinary manner (as to appearance) of chemical decompositions; for the oxygen appears at the *anode* and the hydrogen at the *cathode* of the decomposing body, and these were in many parts of the experiment above an inch asunder. Again, the ordinary chemical affinity was not enough under the circumstances to effect the decomposition of the water, as was abundantly proved by the inaction on plate B; the voltaic current was essential. And to prevent any idea that the chemical affinity was almost sufficient to decompose the water, and that a smaller current of electricity might, under the circumstances, cause the hydrogen to

* The acid was left during a night with a small piece of unamalgamated zinc in it, for the purpose of evolving such air as might be inclined to separate, and bringing the whole into a constant state.

† The experiment was repeated several times with the same results.

pass to the *cathode*, I need only refer to the results which I have given (807. 813.) to show that the chemical action at the electrodes has not the slightest influence over the *quantities* of water or other substances decomposed between them, but that they are entirely dependent upon the quantity of electricity which passes.

868. What, then, follows as a necessary consequence of the whole experiment? Why, this: that the chemical action upon 32·31 parts, or one equivalent of zinc, in this simple voltaic circle, was able to evolve such quantity of electricity in the form of a current as, passing through water, should decompose 9 parts, or one equivalent of that substance: and, considering the definite relations of electricity as developed in the preceding parts of the present paper, the results prove that the quantity of electricity which, being naturally associated with the particles of matter, gives them their combining power, is able, when thrown into a current, to separate those particles from their state of combination; or, in other words, that *the electricity which decomposes, and that which is evolved by the decomposition of, a certain quantity of matter, are alike.*

869. The harmony which this theory of the definite evolution and the equivalent definite action of electricity introduces into the associated theories of definite proportions and electro-chemical affinity, is very great. According to it, the equivalent weights of bodies are simply those quantities of them which contain equal quantities of electricity, or have naturally equal electric powers; it being the ELECTRICITY which *determines* the equivalent number, *because* it determines the combining force. Or, if we adopt the atomic theory or phraseology, then the atoms of bodies which are equivalents to each other in their ordinary chemical action, have equal quantities of electricity naturally associated with them. But I must confess I am jealous of the term *atom*; for though it is very easy to talk of atoms, it is very difficult to form a clear idea of their nature, especially when compound bodies are under consideration.

870. I cannot refrain from recalling here the beautiful idea put forth, I believe, by BERZELIUS (703.) in his development of his views of the electro-chemical theory of affinity, that the heat and light evolved during cases of powerful combination are the consequence of the electric discharge which is at the moment taking place. The idea is in perfect accordance with the view I have taken of the *quantity* of electricity associated with the particles of matter.

871. In this exposition of the law of the definite action of electricity, and its corresponding definite proportion in the particles of bodies, I do not pretend to have brought, as yet, every case of chemical or electro-chemical action under its dominion. There are numerous considerations of a theoretical nature, especially respecting the compound particles of matter and the resulting electrical forces which they ought to possess, which I hope will gradually receive their development; and there are numerous experimental cases, as, for instance, those of compounds formed by weak affinities, the simultaneous decomposition of water and salts, &c., which still require

investigation. But whatever the results on these and numerous other points may be, I do not believe that the facts which I have advanced, or even the general laws deduced from them, will suffer any serious change; and they are of sufficient importance to justify their publication, even though much may remain imperfect or undone. Indeed, it is the great beauty of our science, CHEMISTRY, that advancement in it, whether in a degree great or small, instead of exhausting the subjects of research, opens the doors to further and more abundant knowledge, overflowing with beauty and utility to those who will be at the easy personal pains of undertaking its experimental investigation.

872. The definite production of electricity (868.) in association with its definite action proves, I think, that the current of electricity in the voltaic pile is sustained by chemical decomposition, or rather by chemical action, and not by contact only. But here, as elsewhere (857.), I beg to reserve my opinion as to the real action of contact, not having yet been able to make up my mind as to its being either an exciting cause of the current, or merely necessary to allow of the conduction of electricity, otherwise generated, from one metal to the other.

873. But admitting that chemical action is the source of electricity, what an infinitely small fraction of that which is active do we obtain and employ in our voltaic batteries! Zinc and platina wires, one eighteenth of an inch in diameter and about half an inch long, dipped into dilute sulphuric acid, so weak that it is not sensibly sour to the tongue, or scarcely to our most delicate test papers, will evolve more electricity in one twentieth of a minute (860.) than any man would willingly allow to pass through his body at once. The chemical action of a grain of water upon four grains of zinc can evolve electricity equal in quantity to that of a powerful thunder-storm (868. 861.). Nor is it merely true that the quantity is active; it can be directed and made to perform its full equivalent duty (867. &c.). Is there not, then, great reason to hope and believe that, by a closer *experimental* investigation of the principles which govern the development and action of this subtile agent, we shall be able to increase the power of our batteries, or invent new instruments which shall a thousandfold surpass in energy those which we at present possess?

874. Here for a while I must leave the consideration of the *definite chemical action of electricity*. But before I dismiss this series of experimental Researches, I would call to mind that, in a former series, I showed the current of electricity was also *definite in its magnetic action* (366. 367. 376. 377.); and, though this result was not pursued to any extent, I have no doubt that the success which has attended the development of the chemical effects is not more than would accompany an investigation of the magnetic phenomena.

Royal Institution,
December 31*st*, 1833.

VII. *On the Theory of the Moon.* *By* JOHN WILLIAM LUBBOCK, *Esq. V.P. and Treas. R.S.*

Received November 30,—Read December 12, 1833.

M. POISSON having lately published a very important memoir on the Theory of the Moon, I am induced again to lay before the Society some remarks on this subject.

In this memoir M. POISSON expresses the three coordinates of the moon, namely, her true longitude, her distance, and her true latitude, in terms of the time. The reasons which he adduces for so doing are the same which led me also to deviate from the course which had always been pursued by mathematicians up to the time I commenced the investigation, and which consisted in employing the equations in which the true longitude is the independent variable.

Instead, however, of integrating the equations of motion by the method of indeterminate coefficients, as I have proposed, M. POISSON recommends the adoption of the method of the variation of the elliptic constants. Having reflected much upon this question before I entered upon the investigation, I will venture now to state the reasons which determined me not to employ the latter method.

It seems, in the first place, desirable to introduce uniformity in the methods employed in the theories of the perturbations of the moon, and of the planets, as far as this can be done without the sacrifice of any facility in the solution of the problem. It is not probable, however, that the tables of the planets will be deduced from the variations of their elements. In fact, as I have shown in a former paper, although the results obtained by either method are identical (as is also obvious *à priori*), it is only by numerous reductions that those deduced from the one method are convertible into those deduced from the other. Moreover, in order to obtain, through the variations of the elliptic elements, the inequalities of any given order in the coordinates, the development of the disturbing function must be carried one step further; so that, for example, in the theory of the moon, to obtain all the inequalities depending upon the fourth power of the moon's eccentricity, it would be necessary to obtain the terms depending upon the fifth power of the same eccentricity in the development of the disturbing function.

In the theory of the moon it is necessary to develop many terms in the disturbing function depending on the square of the disturbing mass, and even some depending on the cube; or, in other words, it is, as is well known, insufficient to substitute, in the disturbing function, the elliptic values of the moon's coordinates. The variation of the disturbing function R may be obtained, according to my method, by substituting in the disturbing function the values of the moon's coordinates obtained by a second approximation, or, as in the method of M. Poisson, by substituting in the disturbing function the variations of the elliptic elements due to the disturbing force.

In the former case,

$$\delta R = r \frac{\mathrm{d} R}{\mathrm{d} r} \frac{\delta r}{r} + \frac{\mathrm{d} R}{\mathrm{d} \lambda} \delta \lambda + \frac{\mathrm{d} R}{\mathrm{d} s} \delta s.$$

In the latter,

$$\delta R = \frac{\mathrm{d} R}{\mathrm{d} r} \delta a + \frac{\mathrm{d} R}{\mathrm{d} e} \delta e + \frac{\mathrm{d} R}{\mathrm{d} \varpi} \delta \varpi + \frac{\mathrm{d} R}{\mathrm{d} \varepsilon} \delta \varepsilon + \frac{\mathrm{d} R}{\mathrm{d} \gamma} \delta \gamma + \frac{\mathrm{d} R}{\mathrm{d} \nu} \delta \nu + \frac{\mathrm{d} R}{\mathrm{d} \zeta} \delta \zeta,$$

ζ being equal to $\int n\, d t$.

In the former case it is necessary to multiply 3 series by 3 series, taken two and two; in the latter it is necessary to multiply 7 series by 7 series: and the labour required in the one is to that required in the other about in the same proportion of 3 to 7. The developments of $\delta \frac{\mathrm{d} R}{\mathrm{d} e}$, $\delta \frac{\mathrm{d} R}{\mathrm{d} \varepsilon}$, &c., which have to be separately obtained, require also the same labour.

It is important in a renewed investigation of the lunar theory, considered with a view of improving the lunar tables, to obtain by some independent method the expressions for the coordinates given finally in terms of the mean longitude by MM. Damoiseau and Plana; for when we consider the enormous number of terms which are necessary to be taken into account, and how difficult it is altogether to avoid error in numerical calculations, it is hardly to be expected that their results can be entirely free from error. If, however, the method of the variation of constants be adopted, after the variations of the elements have been obtained, it will require no small labour to effect the necessary substitutions in the elliptic expressions for the coordinates, so as finally to obtain the desired comparison. The quantity of labour necessary in order to bring to conclusion any solution of the problem is a very important consideration, as every additional work, whether in algebra or numbers, brings with it increased danger of mistakes, notwithstanding every care.

The preceding remarks, however, apply particularly to the determination of those inequalities which are not lowered by integration, that is, to almost all those which originate from the terms in R multiplied by $a_{\prime}^{-3}$; but with respect to others, particularly those selected by M. Poisson, the method which he employs is very preferable.

Laplace, in the Mécanique Céleste, vol. iii. p. 171, alludes to an equation of long period of which the argument is twice the longitude of the moon's node, plus the longitude of her perigee, minus three times the longitude of the sun's perigee.

M. Poisson has shown that the coefficient of the corresponding argument in the development of the disturbing function equals zero. I shall now show that this important result may also be arrived at very simply by means of the method of developing R, I gave formerly*.

Employing the same notation,

$$\frac{dR}{d\gamma} = r' \frac{dR}{d\gamma} \frac{dr'}{r\, d\gamma} + \frac{dR}{d\lambda'} \frac{d\lambda'}{d\gamma} + \frac{dR}{ds} \frac{ds}{d\gamma}$$

$$R = m_{\prime} \left\{ -\frac{1}{r_{\prime}} - \frac{r'^2}{4 r_{\prime}^3} \{1 + 3 \cos (2\lambda' - 2\lambda_{\prime}) - 2 s^2\} \right.$$

$$\left. - \frac{r'^3}{8 r_{\prime}^4} \{3 (1 - 4 s^2) \cos (\lambda' - \lambda_{\prime}) + 5 \cos (3 \lambda' - 3 \lambda_{\prime})\}\right.$$

It is evident, by mere inspection of the value of R, that the term in question, of which the argument is $3\tau - \xi + 3\xi_{\prime} - 2\eta$, can only arise from the development of

$$-\frac{5 r'^3}{8 r_{\prime}^4} \cos (3\lambda' - 3\lambda_{\prime}),$$

so that we may consider $R = -\frac{5 r'^3}{8 r_{\prime}^4} \cos (3\lambda' - 3\lambda_{\prime})$ in the present investigation, and $\frac{dR}{ds} = 0$.

The argument† $3\tau - \xi + 3\xi_{\prime} - 2\eta$, can only be made up, therefore, of the arguments $3\tau - \xi + 3\xi_{\prime}$ and 2η, and $3\tau + 3\xi_{\prime}$ and $\xi + 2\eta$, by subtraction, if we limit ourselves to that part of the coefficient which is multiplied by $e\, e_{\prime}^3 \gamma^2$.

It is therefore necessary to determine the coefficients of R corresponding to the arguments $3\tau + 3\xi_{\prime}$ and $3\tau - \xi + 3\xi_{\prime}$.

By the expression

$$\frac{dR}{de_{\prime}} = r_{\prime} \frac{dR}{dr_{\prime}} \frac{dr_{\prime}}{r_{\prime}\, de_{\prime}} + \frac{dR}{d\lambda_{\prime}} \frac{d\lambda_{\prime}}{de_{\prime}},$$

it is evident that the coefficient of $\cos 3\tau + 3\xi_{\prime}$ depends only upon the coefficients of $\cos 3\tau$, $\cos 3\tau + \xi_{\prime}$, and $\cos 3\tau + 2\xi_{\prime}$. On reference to the development of R‡, it will be found, that considering only these terms,

$$R = \frac{m_{\prime} a^3}{a_{\prime}^4} \left\{ -\frac{5}{8} \cos 3\tau + \frac{5}{8} e_{\prime} \cos (3\tau + \xi_{\prime}) - \frac{5}{64} e_{\prime}^2 \cos (3\tau + 2\xi_{\prime}) \right\},$$

[116] [120] [128]

and since

$$r_{\prime} \frac{dR}{dr} = a_{\prime} \frac{dR}{da_{\prime}}, \text{ and } \frac{dR}{d\lambda_{\prime}} = \frac{dR}{d\tau},$$

τ being used for $nt - n_{\prime}t$.

$$\frac{dr_{\prime}}{r_{\prime}\, de_{\prime}} = -\cos \xi_{\prime} - \frac{3}{2} e_{\prime} \cos 2\xi_{\prime} - \frac{17}{8} e_{\prime}^2 \cos 3\xi_{\prime},$$

$$\frac{d\lambda_{\prime}}{de_{\prime}} = 2 \sin \xi_{\prime} + \frac{5}{2} e_{\prime} \sin 2\xi_{\prime} + \frac{13}{4} e_{\prime}^2 \sin 3\xi_{\prime},$$

neglecting terms which are not required.

* Philosophical Transactions, 1832, p. 606.

† I use the letters τ, ξ, $\xi_{\prime}$, and η, where formerly I used t, x, z, and y.

‡ Philosophical Transactions, 1831, p. 266.

$3 \times$ numerical coefficient of $\cos (3\tau + 3\xi_{,})$

$$= -4 \times -\frac{5}{8} \times -\frac{17}{8} \times \frac{1}{2} - 4 \times \frac{5}{8} \times -\frac{3}{2} \times \frac{1}{2} - 4 \times -\frac{5}{64} \times -1 \times \frac{1}{2}$$

$$+ 3 \times -\frac{5}{8} \times \frac{13}{4} \times -\frac{1}{2} + 3 \times \frac{5}{8} \times \frac{5}{2} \times -\frac{1}{2} + 3 \times -\frac{5}{64} \times 2 \times -\frac{1}{2}$$

$$= \frac{-340 + 240 - 20 + 390 - 300 + 30}{128} = 0.$$

The first term of the coefficient, therefore, of $\cos (3\tau + 3\xi_{,})$ in the development of the disturbing function equals zero.

It is evident by the expression

$$\frac{dR}{de} = \frac{r\,dR}{dr}\frac{dr}{r\,de} + \frac{dR}{d\lambda}\frac{d\lambda}{de},$$

that the coefficient of $\cos (3\tau - \xi + 3\xi_{,})$ depends solely upon the coefficient of $\cos (3\tau + 3\xi_{,})$; and as this equals zero, the other must also equal zero; and as the coefficient of $\cos (3\tau - \xi + 3\xi_{,} - 2\eta)$ depends solely upon these two coefficients, it must also equal zero, which was the point to be ascertained.

M. Poisson has shown in the memoir before referred to, that the first term in the corresponding inequality of longitude depends only upon this coefficient in the development of R; the inequality is therefore insensible.

It follows equally that the coefficients of all arguments which result from any combination of $3\tau + 3\xi_{,}$ with any multiples of ξ and 2η are also equal to zero.

Note.—The expressions for $\frac{dr'}{r'd\gamma}$ and $\frac{d\lambda'}{d\gamma}$ I gave*, should be as follows:

$$\frac{dr'}{r'd\gamma} = -\frac{\gamma}{2} + \underset{[62]}{\frac{\gamma}{2}(1 - 4e^2)\cos 2\eta} - \underset{[65]}{\gamma e \cos (\xi - 2\eta)} + \underset{[66]}{\gamma e \cos (\xi + 2\eta)}$$

$$+ \underset{[77]}{\frac{3}{8}\gamma e^2 \cos (2\xi - 2\eta)} - \underset{[78]}{\frac{13}{8}\gamma e^2 \cos (2\xi + 2\eta)}$$

$$\frac{d\lambda'}{d\gamma} = \underset{[62]}{-\frac{\gamma}{2}(1 - 4e^2)\sin 2\eta} + \underset{[65]}{\gamma e \sin (\xi - 2\eta)} - \underset{[66]}{\gamma e \sin (\xi + 2\eta)}$$

$$+ \underset{[77]}{\frac{3}{8} e^2 \sin (2\xi - 2\eta)} - \underset{[78]}{\frac{13}{8}\gamma e^2 \sin (2\xi + 2\eta)}.$$

The coefficient of the first term (argument 146) in the expression for $\frac{dR}{ds}$†, should be $\frac{408}{137}$ instead of $\frac{204}{137}$.

* Philosophical Transactions, 1832, p. 606.

† Ibid. p. 6.

VIII. *On the Theory of the Moon.* By John William Lubbock, *Esq. V.P. and Treas. R.S.*

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When I commenced the investigations relating to the theory of the moon which I have had the honour to communicate to the Society, I proposed to show how, by a different but more direct method, the numerical results given by M. Damoiseau might be obtained. The approximations were in fact carried much further by M. Damoiseau than had been done before, and the details which accompany M. Damoiseau's work evince at once the immense labour of the undertaking, and inspire confidence in the accuracy of the results offered. But the state of the question is now changed by the appearance of M. Plana's admirable work, entitled "Théorie du Mouvement de la Lune," in which, although M. Plana employs the same differential equations as those used by M. Damoiseau, and obtains in the same manner finally the expressions for the coordinates of the moon, in terms of the mean longitude by the reversion of series, yet M. Plana's expressions have a very different analytical character and importance, from the circumstance that the author develops all the quantities introduced by integration, according to powers of the quantity called m, which expresses the ratio of the sun's mean motion to that of the moon. In this form of the expression the coefficients of the different powers of m, of the eccentricity, &c., are determinate, as are, for example, the numerical coefficients in the expression for the sine in terms of the arc, and other similar series. An inestimable advantage results from this procedure, which more than compensates for the great increase of labour it occasions, by diminishing the danger of neglecting any terms of the same order as those taken into account, and by affording the means of verifying many terms long before final and complete results shall have been obtained independently by myself or any other person. By treating the differential equations in which the time is the independent variable, as I have proposed, similar results to those of M. Plana may be obtained directly; but the calculations which are required in either method are so prodigiously irksome and laborious, that until identical expressions have actually been obtained independently, to the extent of every sensible term, the theory of the moon cannot, I think, be considered complete. It might, indeed, be supposed that already, through the labours of mathematicians, from Clairaut to the present time, the numerical values of the coefficients of the different inequalities were ascertained with sufficient accuracy for practical purposes, and that any further researches connected with the subject would be more likely to gratify curiosity than to lead to any useful result.

Astronomical observations are now made with so great precision, that the numerical values of the coefficients are wanted to at least the tenth of a second of space: very few, however, of the coefficients of MM. Damoiseau and Plana agree so nearly, and some differ much more, as may be seen in the following comparison of the numerical values of the coefficients of some of the arguments in the expression for the true longitude of the moon in terms of her mean longitude, being indeed those which differ the most.

	Argument.	Damoiseau.	Plana.
1	2τ	+ 2370·00	+ 2370·320
2	ξ	+22639·70	+22641·626
3	$2\tau-\xi$	+ 4589·61	+ 4585·648
4	$2\tau+\xi$	+ 192·22	+ 192·146
5	$\xi_{,}$	− 673·70	− 668·644
6	$2\tau-\xi_{,}$	+ 165·56	+ 165·850
7	$2\tau+\xi_{,}$	− 24·82	− 23·611
8	2ξ	+ 768·72	+ 769·477
9	$2\tau-2\xi$	+ 211·57	+ 212·363
10	$2\tau+2\xi$	+ 14·74	+ 14·119
11	$\xi+\xi_{,}$	− 109·27	− 111·099
12	$2\tau-\xi-\xi_{,}$	+ 207·09	+ 209·742
22	$2\tau+3\xi$	+ 1·27	+ 3·309
24	$2\tau-2\xi-\xi_{,}$	+ 8·99	+ 7·762
27	$2\tau-2\xi+\xi_{,}$	+ 2·55	− 1·395
64	$2\tau+2\eta$	− 5·75	− 3·376
65	$\xi-2\eta$	+ 39·51	+ 37·191
110	$\tau-\xi+\xi_{,}$	+ 2·05	+ ·466
136	$4\tau-2\xi$	+ 31·19	+ 34·518
	$4\tau-2\xi-\xi_{,}$	+ 3·05	+ 1·197

When the coefficients of the inequalities have been determined analytically, it remains to determine with corresponding precision the numerical values of the arbitrary quantities m, e, and γ. The quantity m is already accurately known, but the quantities e and γ must be obtained from the coefficients of $\sin\xi$ in the expression for the longitude, and of $\sin\eta$ in the expression for the latitude, by the reversion of series; and it seems to me that the manner in which these arbitrary quantities are to be determined must be carefully and rigorously defined.

I propose to obtain the expression for the radius vector by means of the equation,

$$\frac{d^2 . r^2}{2\,d\,t^2}-\frac{\mu}{r}+\frac{\mu}{a}+2\int d\,R+r\frac{d\,R}{d\,r}=0.$$

In order to integrate this equation, I suppose

$$\frac{a}{r}=1+r_0+r_1\cos 2\tau+e\left(1-\frac{e^2}{8}\right)\cos\xi+\&c.$$

If r be used to denote the terms in r which are found in the elliptic expression, so that

$$\frac{a}{\mathrm{r}}=1+e\left(1-\frac{e^2}{8}\right)\cos\xi+e^2\left(1-\frac{e^2}{3}\right)\cos 2\xi+\frac{9}{8}e^3\cos 3\xi+\frac{4}{3}e^4\cos 4\xi+\&c.,$$

and

$$\frac{a}{r}=\frac{a}{\mathrm{r}}+a\,\delta\frac{1}{r}$$

$$\frac{d^2 . r^2}{2 d t^2} - \frac{d^2 . r^3 \delta \frac{1}{r}}{d t^2} + \frac{3 d^2 . r^4 \left(\delta \frac{1}{r}\right)^2}{2 d t^2} - \frac{2 d^2 . r^5 \left(\delta \frac{1}{r}\right)^3}{d t^2}$$

$$- \frac{\mu}{r} + \frac{\mu}{a} + 2 \int d R + r \frac{d R}{d r} = 0.$$

Let $\mathfrak{r}_n$ be that part of the coefficient of the nth argument in the development of the quantity

$$- r^3 \delta \frac{1}{r} + \frac{3}{2} r^4 \left(\delta \frac{1}{r}\right)^2 + \&c.,$$

which corresponds to the argument of which n is the index, and let R_n be the coefficient corresponding to the argument of which n is the index in the development of R, R'_n the corresponding coefficient in the development of that part of $\delta d R$ which is multiplied by m, and only arises in the second approximation, with its sign changed, then the quantities r_n are given by equations similar to the following,

$$r_1 \left\{ \left\{ 1 + 3 e^2 \left(1 + \frac{e^2}{8}\right) \right\} \{2 - 2 m\}^2 - 1 \right\} = (2 - 2 m)^2 \mathfrak{r}_1$$

$$- 2 \left\{ \left\{ \frac{2}{2 - 2 m} + 1 \right\} m^2 R_1 + \frac{m^3}{(2 - 2 m)} R'_1 \right\}$$

Passing over terms given by M. Plana and arising from the first approximation with which I agree, I come to r_{22}.

$$r_{22} \left\{ \{2 - 2 m + 3 c\}^2 - 1 \right\} = (2 - 2 m + 3 c)^2 \mathfrak{r}_{22}$$

$$- 2 \left\{ \frac{2 + 3 c}{(2 - 2 m + 3 c)} + 1 \right\} m^2 R_{22}$$

$$\mathfrak{r}_{22} = \frac{3}{2} r_{10} - \frac{1}{16} r_1 \qquad r_{10} = \frac{7}{2} m^2 \qquad r_1 = m^2 \qquad {}^*R_{22} = - \frac{25}{32}$$

$$r_{22} = \frac{25 . 3 . 7}{24 . 2 . 2} m^2 - \frac{25}{24 . 16} m^2 + \frac{2 . 2 . 25}{24 . 32} m^2 = \frac{2125}{384} m^2$$

M. Plana has $- \frac{1175}{384} m^2$

$$r_{25} \left\{ \{2 - m + 2 c\}^2 - 1 \right\} = (2 - m + 2 c)^2 \mathfrak{r}_{25} - 2 \left\{ \frac{2 + 2 c}{2 - m + 2 c} + 1 \right\} m^2 R_{25}$$

$$\mathfrak{r}_{25} = \frac{3}{2} r_{13} \qquad r_{13} = - \frac{33}{32} m^2 \qquad R_{25} = \frac{3}{8}$$

$$r_{25} = - \frac{16 . 3 . 33}{15 . 2 . 32} m^2 - \frac{2 . 2 . 3}{15 . 8} m^2 = - \frac{7}{4} m^2.$$ M. Plana has $\frac{7}{2} m^2$

$$r_{45} \left\{ \{2 - m - 3 c\}^2 - 1 \right\} = \{2 - m - 3 c\}^2 \mathfrak{r}_{45} - 2 \left\{ \frac{2 - 3 c}{2 - m - 3 c} + 1 \right\} m^2 R_{45}$$

* Wherever I have found a disagreement with the result of M. Plana, as this might arise from an error in my development of R, I have verified the terms employed.

$$\mathfrak{r}_{45} = \frac{3}{2} r_{27} - \frac{1}{16} r_7 \qquad r_{27} = \frac{15}{8} m^2 \qquad r_7 = -\frac{m^2}{2} \qquad R_{45} = \frac{7}{64}$$

$$r_{45} = \frac{2.2.8}{3.15} m + \frac{m}{2.16.2} + \frac{2.2.7}{2.64} m = \frac{105}{64} m \qquad \text{M. Plana has } \frac{285}{64} m$$

$$r_{53}\left\{\{c + 3m\}^2 - 1\right\} = (c + 3m)^2 \mathfrak{r}_{53} - 2\left\{\frac{c}{c + 3m} + 1\right\} m^2 R_{53}$$

$$\mathfrak{r}_{53} = \frac{3}{2} r_{35} \qquad r_{35} = -\frac{53}{16} m^2 \qquad R_{53} = \frac{53}{32}$$

$$r_{53} = -\frac{3.53}{6.2.16} m - \frac{2.2.53}{6.32} m = -\frac{127}{64} m \qquad \text{M. Plana has } -\frac{53}{32} m$$

$$r_{56}\{(c - 3m)^2 - 1\} = (c - 3m)^2 \mathfrak{r}_{56} - 2\left\{\frac{c}{c - 3m} + 1\right\} m^2 R_{56}$$

$$\mathfrak{r}_{56} = \frac{3}{2} r_{35} \qquad R_{56} = \frac{53}{32}$$

$$r_{56} = \frac{3.53}{6.2.16} m + \frac{2.2.53}{6.32} = \frac{371}{192} m \qquad \text{M. Plana has } \frac{53}{64} m$$

The development of R which I gave*, results from the substitution of the elliptic values of the coordinates of the sun and moon in the disturbing function. The elliptic expression for the radius vector contains no term of which the argument is $\xi - 2\eta$, the longitude (λ) contains the term $+ \frac{3}{4} e \gamma^2 \sin (\xi - 2\eta)$. This is changed when the disturbing function is considered.

$$r_{65}\{(c - 2g)^2 (1 - 3r_0) - 1\} = (c - 2g)^2 \mathfrak{r}_{65} - 2.2.m^2 R_{65}$$

$$\mathfrak{r}_{65} = \frac{3}{2} r_{62} \qquad r_{62} = \frac{m^2}{2} \qquad c = 1 - \frac{3}{4} m^2 \qquad g = 1 + \frac{3}{4} m^2$$

$$r_0 = \frac{m^2}{6} \qquad R_{65} = \frac{9}{8} + \frac{1}{2} r_{65} \qquad (c - 2g)\, 2\, (1 - 3r_0) = 1 + 4m^2$$

$$r_{65} = \frac{3}{4.2.2} - \frac{2.2}{4}\left(\frac{9}{8} + \frac{1}{2} r_{65}\right)$$

$$r_{65} = -\frac{5}{8}.$$

This term, produced by the disturbing force, although independent of m, together with the corresponding term in λ, renders in a certain sense incomplete the coefficients of all terms in my development of R, of which the arguments are any combinations of the quantity $\xi - 2\eta$.

$$r_{73}\{(2 - 3m - 2g)^2 - 1\} = (2 - 3m - 2g)^2 \mathfrak{r}_{73} - 2m^2 R_{73}$$

$$\mathfrak{r}_{73} = 0 \qquad R_{73} = -\frac{21}{16}$$

$$r_{73} = -\frac{2.21}{16} m^2 = -\frac{21}{8} m^2 \qquad \text{M. Plana has } \frac{7}{8} m - \frac{7}{8} m^2$$

* Philosophical Transactions, 1831, p. 263.

The term above, $-\frac{5}{8}\,e\,\gamma^2\cos(\xi - 2\,\eta)$, introduces the term $+\frac{3}{4}\,e\,\gamma^2\sin(\xi - 2\,\eta)$ in the longitude, instead of $-\frac{e\gamma^2}{2}\sin(\xi - 2\,\eta)$. The terms in R produced in consequence may easily be found from the formula

$$\delta R = -\,a\left(\frac{\mathrm{d}R}{\mathrm{d}a}\right)\mathrm{r}\,\delta\,\frac{1}{r} + \frac{\mathrm{d}R}{\mathrm{d}\tau}\,\delta\,\lambda$$

taking

$$\mathrm{r}\,\delta\,\frac{1}{r} = -\frac{5}{8}\,e\,\gamma^2\cos(\xi - 2\,\eta), \quad \text{and } \delta\,\lambda = \frac{5}{4}\,e\,\gamma^2\sin(\xi - 2\,\eta)$$

and I find that R contains, instead of the terms corresponding to the same arguments given in the Philosophical Transactions, 1831, p. 263.

$$+\frac{15}{32}\,m^2\,e\,\gamma^2\cos(2\,\tau - \xi + 2\,\tau) - m^2\,e\,\gamma^2\cos(2\,\tau + \xi - 2\,\eta)$$

[68] [69]

$$+\frac{39}{32}\,m^2\,e\,e_{,}\,\gamma^2\cos(\xi + \xi_{,} - 2\,\eta) + \frac{105}{64}\,m^2\,e\,e_{,}\,\gamma^2\cos(2\,\tau - \xi - \xi_{,} + 2\,\eta)$$

[83] [86]

$$+\frac{33}{64}\,m^2\,e\,e_{,}\,\gamma^2\cos(e\,\tau + \xi + \xi_{,} - 2\,\eta) - \frac{15}{84}\,m^2\,e\,e_{,}\,\gamma^2\cos(2\,\tau - \xi + \xi_{,} + 2\,\eta)$$

[87] [92]

$$-\frac{231}{64}\,m^2\,e\,e_{,}\,\gamma^2\cos(2\,\tau + \xi - \xi_{,} - 2\,\eta)$$

[93]

The coefficient of arg. 77 is easily found as follows:

$$R = -\frac{r^2}{4\,r_{,}^3}\{1 + 3\cos(2\,\lambda - 2\,\lambda) - 2\,s^2\}.$$

This term can only arise from

$$-\frac{r^2}{4\,r_{,}^3}\{1 - 3\,s^2\}$$

$$= \frac{r^3}{2\,r_{,}^3}\,\delta\,\frac{1}{r} + \frac{3\,r^2}{4\,r_{,}^3}\,s^2.$$

In which expression it is sufficient to write for $\delta\,\frac{1}{r}$,

$$-\frac{5}{8}\,e\,r^2\cos(\xi - 2\,\eta)$$

and to make

$${}^*s^2 = -\frac{\gamma^2}{2}\cos 2\,\eta + \gamma^2\,e\cos(\xi - 2\,\eta) + \frac{1}{4}\,\gamma^2\,e^2\cos(2\,\xi - 2\,\eta)$$

[65] [77]

which gives

$$R_{77} = \frac{3\,.\,5}{2\,.\,2\,.\,8} + \frac{3}{4\,.\,4} - \frac{3}{4} + \frac{3}{4\,.\,2\,.\,2\,.\,2} = 0.$$

* This is not the expression for s^2 in the elliptic movement: the last term is altered by the disturbing force.

When the elliptic values of the coordinates are substituted in the disturbing function, the term in question arises only from the expansion of the quantity

$$+\frac{3}{4}\frac{r^2}{r_{,}^3}s^2,$$

and in the elliptic motion

$$s^2 = -\frac{\gamma^2}{2}\cos 2\eta + \gamma^2 e \cos(\xi - 2\eta) - \frac{3}{8}\gamma^2 e^2 \cos(2\xi - 2\eta) + \&c.$$

$$R_{77} = -\frac{3.3}{4.8} - \frac{3}{4} + \frac{3}{4.2.2.2} = -\frac{15}{16}.$$

Writing the index between brackets instead of the cosine of the corresponding argument, in order to save space.

$$-\int \frac{dR}{d\lambda} dt$$

$$= \frac{3}{4}\left\{1 - \frac{5}{2}e^2 - \frac{5}{2}e_{,}^2 - \frac{\gamma^2}{2}\right\} m^2 [1] - \frac{9}{2}\left\{1 - \frac{13}{24}e^2 - \frac{5}{2}e_{,}^2 - \frac{\gamma^2}{2}\right\} m^2 e [3]$$

$$+ \frac{1}{2}\left\{1 - \frac{19}{8}e^2 - \frac{5}{2}e_{,}^2 - \frac{\gamma^2}{2}\right\} m^2 e [3] \quad + \frac{21}{8}\left\{1 - \frac{5}{2}e^2 - \frac{123}{56}e_{,}^2 - \frac{\gamma^2}{2}\right\} m^2 e_{,} [6]$$

$$+ \frac{3}{8}\left\{1 - \frac{5}{2}e^2 - \frac{e_{,}^2}{8} - \frac{\gamma^2}{2}\right\} m^2 e_{,} [7] \quad - \frac{15}{8}\left\{1 - \frac{5}{2}e_{,}^2 - \frac{\gamma^2}{2}\right\} m e^2 [9]$$

$$+ \frac{3}{8}\left\{1 - \frac{5}{2}e^2 - \frac{5}{2}e_{,}^2 - \frac{\gamma^2}{2}\right\} m^2 e^2 [10] \quad - \frac{63}{4}\left\{1 - \frac{91}{128}e^2 - \frac{123}{56}e_{,}^2 - \frac{\gamma^2}{2}\right\} m^2 e e_{,} [12]$$

$$- \frac{1}{4}\left\{1 - \frac{19}{8}e^2 - \frac{e_{,}^2}{8} - \frac{\gamma^2}{2}\right\} m^2 e e_{,} [13] \quad + \frac{9}{4}\left\{1 - \frac{13}{24}e^2 - \frac{e_{,}^2}{8} - \frac{\gamma^2}{2}\right\} m^2 e e_{,} [15]$$

$$+ \frac{7}{4}\left\{1 - \frac{19}{8}e^2 - \frac{123}{56}e_{,}^2 - \frac{\gamma^2}{2}\right\} m^2 e_{,}^2 [16] \quad + \frac{51}{8}\left\{1 - \frac{5}{2}e^2 - \frac{115}{51}e_{,}^2 - \frac{\gamma^2}{2}\right\} m^2 e_{,}^2 [18]$$

$$+ \frac{5}{16} m e^3 - \frac{35}{8} m e_{,}^2 e_{,} [24] \quad - \frac{3}{16} m^2 e^2 e_{,} [25] \quad + \frac{15}{8} m e^2 e_{,} [27] \quad + \frac{21}{16} m^2 e^2 e_{,} [28]$$

$$- \frac{153}{4} m^2 e e_{,}^2 [30] \quad + \frac{17}{4} m^2 e e_{,}^2 [34] \quad + \frac{845}{64} m^2 e_{,}^3 [36] \quad + \frac{m^2 e_{,}^3}{64} [37] \quad + \frac{3}{64} m^2 e^4 [39]$$

$$+ \frac{9}{32} m^2 e^4 [40] \quad + \frac{49}{32} m^2 e^3 e_{,} [42] \quad + \frac{25}{32} m^2 e^3 e_{,} [43] \quad - \frac{7}{32} m^2 e^3 e_{,} [45]$$

$$+ \frac{35}{32} m^2 e^3 e_{,} [46] \quad - \frac{255}{32} m^2 e^2 e_{,}^2 [48] \quad + \frac{51}{16} m^2 e^2 e_{,}^2 [52] \quad - \frac{2535}{64} m^2 e e_{,}^3 [54]$$

$$- \frac{1}{96} m^2 e e_{,}^3 [55] \quad - \frac{3}{32} m^2 e_{,}^3 [57] \quad - \frac{15}{32} m^2 e_{,}^3 [58] \quad + \frac{2453}{128} m^2 e_{,}^4 [60] \quad - \frac{741}{128} m^2 e_{,}^4 [70]$$

I have verified some of the terms in the expression for the reciprocal of the radius vector given by M. PLANA, which depend on $\frac{a^3}{a_{,}^4}$, and arise from the second portion of R; very few, however, of these can be obtained without a further development of the disturbing function, in consequence particularly of the term

$$\frac{5a}{4a_{,}} e_{,} \cos(\tau + \xi_{,}),$$

which is independent of m. In consequence of this term, all the terms in my development of R, of which the arguments are any combination of the quantity $\tau + \xi_{,}$ are incomplete.

When the terms depending on γ^2, and those depending on the square of the disturbing force, are neglected, the inequalities of longitude are given by the equation

$$\frac{d\lambda}{dt} = \frac{h}{r^2} - \frac{1}{r^2}\int \frac{dR}{d\lambda} dt.$$

I find from this equation in λ_4 the term

$$\frac{175}{32} m e^2 = \left\{ \frac{405}{32} m e^2 + \frac{15}{4} m e^2 + \frac{15}{8} m e^2 \right\} \frac{1}{3},$$

instead of $\frac{195}{32} m^2$, according to M. Plana*.

λ_6 contains the term $-\frac{1353}{128} m^2 e_{,}^2$ instead of $-\frac{1253}{128} m^2 e_{,}^2$, for

$$R \ldots\ldots\ldots\ldots + \frac{369}{64} e_{,}^3 \cos(2\tau - \xi_{,})$$

$$\frac{a}{r} \ldots\ldots\ldots\ldots - \frac{123}{16} m^2 e_{,}^3 \cos(2\tau - \xi_{,})$$

$$\lambda_6 \ldots\ldots\ldots \left\{ -\frac{123}{8} m^2 - \frac{369}{64} m^2 \right\} e_{,}^2 = -\frac{1353}{64} m^2 e_{,}^2.$$

If the numerical coefficient of the corresponding term in the quantity $-\int \frac{dR}{d\lambda} dt$, be called $\mathfrak{R}$, then I find

$$\lambda_{22} = \left\{ 2 r_{22} + r_{10} + r_{11} + \frac{9}{8} r_1 + \mathfrak{R}_{22} + \mathfrak{R}_{10} + \frac{5}{4} \mathfrak{R}_4 + \frac{13}{8} \mathfrak{R}_1 \right\} \frac{1}{(2 - 2m + 3c)}$$

$$= \left\{ \frac{2125}{192} m^2 + \frac{7}{2} m^2 + \frac{33}{16} m^2 + \frac{9}{8} m^2 + \frac{5}{16} m^2 + \frac{3}{8} m^2 + \frac{5}{8} m^2 + \frac{13.3}{8.4} m^2 \right\} \frac{1}{5}$$

$$= \frac{779}{192} m^2.$$

M. Plana has $\frac{1093}{64} m^2$, and for the numerical value of the coefficient converted into sexagesimal seconds 3″·309. M. Damoiseau has 1″·27; I obtain ·77″.

I find

$$\lambda_{25} = \left\{ 2 r_{25} + r_{13} + r_7 + \mathfrak{R}_{25} + \mathfrak{R}_{13} + \frac{5}{4} \mathfrak{R}_7 \right\} \frac{1}{(2 - m + 2c)}$$

$$= \left\{ -\frac{7}{2} m^2 - \frac{33}{32} m^2 - \frac{m^2}{2} - \frac{3}{16} m^2 - \frac{1}{4} m^2 + \frac{5.3}{4.8} \right\} \frac{1}{4} = -\frac{5}{4} m^2.$$

M. Plana has $-\frac{95}{64} m^2$, and for the numerical value of the coefficient converted into sexagesimal seconds — ·087. M. Damoiseau has — ·19; I obtain — ·073.

* The figures are indistinct in the copy before me.

I find

$$\lambda_{28} = \left\{ \frac{49}{2} m^2 + \frac{231}{32} m^2 + \frac{7}{2} m^2 + \frac{21}{16} m^2 + \frac{9}{4} m^2 + \frac{5 \cdot 21}{4 \cdot 8} m^2 \right\} \frac{1}{4} = \frac{673}{64} m^2.$$

M. Plana has $\frac{665}{64} m^2$, and for the numerical value of the coefficient converted into sexagesimal seconds $''{\cdot}607$. M. Damoiseau has $''{\cdot}90$; I obtain $''{\cdot}615$.

I find

$$\lambda_{49} = \left\{ 2 r_{49} + r_{31} + r_{19} + \mathbb{R}_{49} + \mathbb{R}_{31} + \frac{5}{4} \mathbb{R}_{19} \right\} \frac{1}{(2 + 2c)} = 0;$$

M. Plana has $-\frac{885}{64} m$.

I find

$$\lambda_{59} = 2 r_{59} \times \frac{1}{4m} = \frac{591}{16} m, \qquad \text{M. Plana has } -\frac{77}{32} m;$$

I find

$$\lambda_{60} = \{ 2 r_{60} + \mathbb{R}_{60} \} \frac{1}{(2 - 6m)} = -\frac{55}{8} m.$$

These discordances will appear very trifling, considering the nature of the calculations; and it is by no means impossible, after all, that M. Plana may be right, and that the mistake may be with me, notwithstanding all the pains I have used.

Before the terms in the longitude can be arrived at which depend on γ^2, it is necessary to obtain the expression for the tangent of the latitude s: this may be done by means of the equations

$$\frac{d^2 z}{d t^2} + \frac{\mu z}{r^3} + \frac{m_{\prime} z}{r_{\prime}^3} + \frac{3 m_{\prime} z r' r \cos(\lambda' - \lambda_{\prime})}{r_{\prime}^5} = 0$$

$$z = \frac{r s}{\sqrt{1 + s^2}} \qquad \frac{z}{r} = s - \frac{s^3}{2} + \frac{3}{8} s^5 - \&c.$$

$$s = \frac{z}{r} + \frac{z^3}{2 r^3} - \&c.$$

$$\tan^{-1} s = s - \frac{s^3}{3} + \frac{s^5}{5} - \&c.$$

It is, however, more convenient in the determination of s, to adhere to the method of Clairaut, that is, to the method adopted by M. Plana, notwithstanding the difficulties which occur in that method, and to which I have before alluded.

The following is the differential equation employed.

$$\left\{ \frac{d^2 s}{d \lambda'^2} + s \right\} \left\{ 1 - \frac{2}{h^2} \int r'^2 \frac{d R}{d \lambda'} d \lambda' \right\}$$
$$+ \frac{r'^2}{h^2} \left\{ (1 + s^2) \left(\frac{d R}{d s} \right) - r' s \left(\frac{d R}{d r'} \right) - \left(\frac{d R}{d \lambda'} \right) \frac{d s}{d \lambda'} \right\} = 0$$

Substituting in this equation in the terms multiplied by $m_{\prime}$, for s, $r \sin (g \lambda' - \nu)$, and for $\frac{d s}{d \lambda'}$, $- r \cos (g \lambda' - \nu)$, neglecting the square of the disturbing force and the cube of s, I obtain

$$\frac{d^2 s}{d \lambda^2} + s - \frac{3 r^4}{2 h^2 r_{\prime}^3} \{\sin (2 \lambda - 2 \lambda_{\prime} - g \lambda + \nu) - \sin (g \lambda - \nu)\}$$

$$- \frac{r^5}{8 h^2 r_{\prime}^4} \left\{ \frac{21}{2} \cos (\lambda - \lambda_{\prime} - g \lambda + \nu) + \frac{15}{2} \cos (\lambda - \lambda_{\prime} + g \lambda - \nu) \right.$$

$$\left. + 9 \cos (3 \lambda - \lambda_{\prime} - g \lambda + \nu) + 6 \cos (3 \lambda - 3 \lambda_{\prime} + g \lambda - \nu) \right\}$$

The simplest method of substituting for $\lambda_{\prime}$ in terms of λ seems to me to be by first obtaining expressions for $\cos \lambda_{\prime}$, $\sin \lambda_{\prime}$, $\cos 2 \lambda_{\prime}$, $\sin 2 \lambda_{\prime}$, in terms of $n_{\prime} t$. Having obtained these expressions, they may be reduced to terms of λ by LAGRANGE's theorem*; but when the higher powers of m are neglected, it is sufficient to write $m \lambda$ instead of $n_{\prime} t$.

$$\sin 2 \lambda_{\prime} = (1 - 4 e_{\prime}^2) \sin 2 n_{\prime} t - 2 e \sin (2 n_{\prime} t - \xi_{\prime}) + 2 e_{\prime} \sin (2 n_{\prime} t + \xi_{\prime})$$

$$+ \frac{3}{4} e_{\prime}^2 \sin (2 n_{\prime} t - 2 \xi_{\prime}) + \frac{13}{4} e_{\prime}^2 \sin (2 n_{\prime} t + 2 \xi_{\prime}) + \&c.$$

$$\cos 2 \lambda_{\prime} = (1 - 4 e_{\prime}^2) \cos 2 n_{\prime} t - 2 e_{\prime} \cos (2 n_{\prime} t - \xi_{\prime}) + 2 e_{\prime} \cos (2 n_{\prime} t + \xi_{\prime})$$

$$+ \frac{3}{4} e_{\prime}^2 \cos (2 n_{\prime} t - 2 \xi_{\prime}) + \frac{13}{4} e_{\prime}^2 \cos (2 n_{\prime} t + 2 \xi_{\prime}) + \&c.$$

Great facility results in the following substitutions in consequence of the coefficients being alike in the corresponding arguments of the expressions $\sin \lambda_{\prime}$, $\cos \lambda_{\prime}$; $\sin 2 \lambda_{\prime}$, $\cos 2 \lambda_{\prime}$, &c.; so

$$\sin (2 \lambda - 2 \lambda_{\prime} - g \lambda) = (1 - 4 e^2) \{\sin (2 \lambda - g \lambda) \cos 2 m \lambda - \cos (2 \lambda - g \lambda) \sin 2 m \lambda\}$$

$$- 2 e_{\prime} \{\sin (2 \lambda - g \lambda) \cos (2 m \lambda - c_{\prime} m \lambda) - \cos (2 \lambda - g \lambda) \sin (2 m \lambda - c_{\prime} m \lambda)\}$$

$$+ 2 e_{\prime} \{\sin (2 \lambda - g \lambda) \cos (2 m \lambda + c_{\prime} m \lambda) - \cos (2 \lambda - g \lambda) \sin (2 m \lambda + c_{\prime} m \lambda)\}$$

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* By LAGRANGE's theorem, if

$$u = \theta - f \theta$$

$$\theta = u + f u + \frac{d . (f u)^2}{2 d u} + \frac{d^2 (f u)^3}{2 . 3 d u^2} + \&c.$$

So

$$a_{\prime}^3 r_{\prime}^{-3} = 1 + \frac{3}{2} e_{\prime}^2 \left(1 + \frac{5}{4} e_{\prime}^2\right) + 3 e_{\prime} \left(1 + \frac{9}{8} e_{\prime}^2\right) \cos \xi_{\prime}$$

$$+ \frac{9}{2} e_{\prime}^2 \left(1 + \frac{7}{9} e_{\prime}^2\right) \cos 2 \xi_{\prime} + \frac{53}{8} e_{\prime}^3 \cos 3 \xi_{\prime} + \frac{77}{8} e_{\prime}^4 \cos 4 \xi_{\prime}$$

$$m \lambda = n_{\prime} t + 2 m e \left(1 - \frac{e^2}{8}\right) \sin \xi + \frac{5}{4} m e^2 \left(1 - \frac{11}{30} e^2\right) \sin 2 \xi$$

$$+ \frac{13}{12} m e^3 \sin \xi + \frac{103}{96} m e^4 \sin 4 \xi + \&c.$$

Hence evidently

$$a_{\prime}^3 r_{\prime}^3 = 1 + \frac{3}{2} e_{\prime}^2 \left(1 + \frac{5}{4} e_{\prime}^2\right) + 3 e_{\prime} \left(1 + \frac{9}{8} e_{\prime}^2\right) \cos (c_{\prime} m \lambda)$$

$$+ \frac{9}{2} e_{\prime}^2 \left(1 + \frac{7}{9} e_{\prime}^2\right) \cos (2 c_{\prime} m \lambda) + \frac{53}{8} e_{\prime}^3 \cos (3 c_{\prime} m \lambda) + \frac{77}{9} e_{\prime}^4 \cos (4 c_{\prime} m \lambda)$$

+ terms multiplied by m, $c_{\prime}$ may be considered as equal to unity.

$$+\frac{3}{4}e_{\prime}^{2}\{\sin(2\lambda-g\lambda)\cos(2m\lambda-2c_{\prime}m\lambda)-\cos(2\lambda-g\lambda)\sin(2m\lambda-2c_{\prime}m\lambda)\}$$

$$+\frac{13}{4}e_{\prime}^{2}\{\sin(2\lambda-g\lambda)\cos(2m\lambda+2c_{\prime}m\lambda)-\cos(2\lambda-g\lambda)\cos(2m\lambda+2c_{\prime}m\lambda)\}$$

$$=(1-4e_{\prime}^{2})\sin(2\lambda-2m\lambda-g\lambda)$$

$$-2e_{\prime}\sin(2\lambda-2m\lambda+c_{\prime}m\lambda-g\lambda)+2e_{\prime}\sin(2\lambda-2m\lambda-c_{\prime}m\lambda-g\lambda)$$

$$+\frac{3}{4}e_{\prime}^{2}\sin(2\lambda-2m\lambda+2c_{\prime}m\lambda-g\lambda)+\frac{13}{4}e_{\prime}^{2}\sin(2\lambda-2m\lambda-2c_{\prime}m\lambda-g\lambda)$$

$$\frac{m_{\prime}r^{4}}{h^{2}r_{\prime}^{3}}=m^{2}\left\{1+\frac{2}{3}e_{\prime}^{2}+3e_{\prime}\cos c_{\prime}m\lambda+\frac{9}{2}e_{\prime}^{2}\cos 2c_{\prime}m\lambda\right\}$$

$$\{1+2e^{2}-4e\cos c\lambda+5e^{2}\cos 2c\lambda\}$$

$$\frac{3}{2}\frac{m_{\prime}r^{4}}{h^{2}r_{\prime}^{3}}=m^{2}\left\{\frac{3}{2}+\frac{9}{4}e_{\prime}^{2}+3e^{2}-6e_{\prime}\cos c\lambda+\frac{9}{2}e_{\prime}\cos c_{\prime}m\lambda+\frac{15}{2}e^{2}\cos 2c\lambda\right.$$

$$\left.-9e_{\prime}\cos(e\lambda-c_{\prime}m\lambda)-9ee_{\prime}\cos(e\lambda+c_{\prime}m\lambda)+\frac{27}{4}e_{\prime}^{2}\cos 2c_{\prime}m\lambda\right\}$$

All the terms which I have verified in the expression for the latitude in terms of the true longitude agree with those given by M. PLANA.

If $\lambda = nt + \alpha$, then by TAYLOR's theorem,

$$s=(s)+\left(\frac{ds}{d\lambda}\right)\alpha+\left(\frac{d^{2}s}{d\lambda^{2}}\right)\frac{\alpha^{2}}{2}+\left(\frac{d^{3}s}{d\lambda^{3}}\right)\alpha^{3}+\&c.$$

(s) being the quantity arising from the substitution of nt for λ, in the expression for s in terms of λ. In this manner I found the same terms as those given by M. PLANA, except $-\frac{15}{32}me^{2}\gamma\sin(2\tau-2\xi+\eta)$ instead of $-\frac{15}{64}me^{2}\gamma\sin(2\tau-2\xi+\eta)$, and $-\frac{3}{8}mee_{\prime}\gamma\sin(2\tau+\xi+\xi_{\prime}-\eta)$ instead of $\frac{1}{2}mee_{\prime}\gamma\sin(2\tau+\xi+\xi_{\prime}-\eta)$.

I next obtained s^2, in order to procure the terms in the longitude depending on γ^2. The quantity e in my notation does not accord with that quantity in the work of M. PLANA, but with $e\left(1-\frac{\gamma^{2}}{4}\right)$; so that, in order to arrive at the same figures in some of the terms multiplied by γ^2, this circumstance must be attended to.

I find

$$\lambda_{70}=\left\{-\frac{39}{16}m^{2}-\frac{11}{16}m^{2}-\frac{33}{32}m^{2}-m^{2}-\frac{1}{4}m^{2}-\frac{3}{8}m^{2}\right\}\frac{1}{5}=-\frac{37}{32}m^{2}$$

instead of $-\frac{39}{32}m^2$, according to M. PLANA; and I find

$$\lambda_{85}=-\left\{\frac{35}{8}m+\frac{7}{8}m-\frac{35}{16}m-\frac{7}{8}m-\frac{7}{16}m\right\}=-\frac{7}{4}m$$

instead of $\frac{7}{8}m$, according to M. PLANA.

These are the only discrepancies which I have noted in the terms multiplied by γ^2 which I have examined. In making use of the development of R before alluded to,

$$1 - \frac{\gamma^2}{2} + \frac{7}{16}\gamma^4 \text{ is to be substituted for } \cos^4 \frac{\gamma}{2}.$$

The relation between the constants h and a is to be obtained from the equations

$$r^4 \frac{d\lambda^2}{dt^2} = h^2 - 2\int r^2 \frac{dR}{d\lambda} d\lambda$$

$$\frac{r^2 d\lambda^2 + dr^2}{dt^2} - \frac{2\mu}{r} + \frac{\mu}{a} + 2\int dR = 0;$$

where, for simplicity, I have neglected at present quantities depending on γ^2.

$$d.\frac{1}{r} = -\frac{dr}{r^2}$$

$$dr^2 = r^4\left(d.\frac{1}{r}\right)^2 = \left(\frac{1}{r}\right)^{-4}\left(d.\frac{1}{r}\right)^2 = a^2(1-4r_0)\left\{\frac{e^2c^2n^2}{2} + \frac{e^2r_3^2n^2}{2}\right\}$$

$2\int dR$ can give no constant term *; therefore, considering the constant part only of the equation above, since the coefficients corresponding to all the arguments must be separately identical,

$$h^2\left\{1 + 2r_0 + \frac{e^2}{2} + \frac{e^2r_3^2}{2}\right\} = a\mu\left\{1 + 2r_0 - (1-4r_0)\right\}\frac{e^2c^2}{2} + \frac{e^2r_3^2}{2}\right\}$$

r_4^2, being already multiplied by m^4, is not to be taken into account in this approximation.

$$c^2\{1 - 3r_0\} - 1 + 2m^2 = 0 \qquad\qquad c^2 = 1 - \frac{7}{2}m^2$$

$$h^2\left\{1 - m^2 + \frac{e^2}{2} + \frac{33}{128}m^2e^2 - \frac{3}{2}m^2e_\prime^2\right\} = a\mu\left\{1 - \frac{e^2}{2} - m^2 - \frac{321}{128}m^2e^2 - \frac{3}{2}m^2e_\prime^2\right\}$$

$$h^2 = a\mu\left\{1 - \frac{e^2}{2} - m^2 - \frac{321}{128}m^2e^2 - \frac{3}{2}m^2e_\prime^2\right\}$$

$$\left\{1 + m^2 - \frac{e^2}{2} - \frac{33}{128}m^2e^2 + \frac{3}{2}m^2e_\prime^2 - m^2e^2\right\}$$

$$= a\mu\left\{1 - e^2 - \frac{241}{64}m^2e^2\right\}$$

$$h = a\mu\left\{1 - \frac{e^2}{2} - \frac{241}{128}m^2e^2\right\}$$

$$\frac{d\lambda}{dt} = \frac{h}{r^2} - \frac{1}{r^2}\int\frac{dR}{d\lambda}dt.$$

* This seems at variance with the equation of M. PLANA, vol. i. p. 122,

$$-\frac{2\alpha}{\sigma}\int d'\Omega = -\frac{m^2}{2}\left\{1 + \frac{1}{2}\gamma^2 + \frac{3}{2}e^2 + \frac{3}{2}e^2\gamma^2\cos(2\varpi - 2\theta) + \&c.\right\}$$

which equation I am unable to understand.

The second term on the right-hand side of this equation gives no constant quantity: hence

$$\frac{h}{r^2} = \sqrt{\frac{\mu}{a^3}}\left\{1 - \frac{e^2}{2} - \frac{241}{128}m^2 e^2\right\}\left\{1 - m^2 + \frac{e^2}{2} + \frac{33}{128}m^2 e^2 - \frac{3}{2}m^2 e_{\prime}^2\right\}$$

$$\lambda = \sqrt{\frac{\mu}{a^3}}\left\{1 - m^2 - \frac{9}{8}m^2 e^2 - \frac{3}{2}m^2 e_{\prime}^2\right\} t + \&c.$$

If the constant quantity which multiplies t be called

$$\mathrm{n} = \sqrt{\frac{\mu}{\mathrm{a}^3}}$$

$$\sqrt{\frac{\mu}{a^3}}\left\{1 - m^2 - \frac{9}{8}m^2 e^2 - \frac{3}{2}m^2 e_{\prime}^2\right\} = \sqrt{\frac{\mu}{\mathrm{a}^3}}$$

$$\mathrm{a} = a\left\{1 + \frac{2}{3}m^2 + \frac{3}{4}m^2 e^2 + m^2 e_{\prime}^2\right\}$$

$$\frac{a}{r} = \frac{\mathrm{a}\left\{1 + \frac{2}{3}m^2 + \frac{3}{4}m^2 e^2 + m^2 e_{\prime}^2\right\}^{-1}}{r} = 1 - \frac{m^2}{2} - \frac{3}{4}m^2 e^2 - \frac{3}{4}m^2 e_{\prime}^2.$$

Reverting to the equation

$$\frac{\mathrm{d}\lambda}{\mathrm{d}t} = \frac{h}{r^2} - \frac{1}{r^2}\int \frac{\mathrm{d}R}{\mathrm{d}\lambda}\mathrm{d}t.$$

The second term of this equation gives no term multiplied by $\cos\xi$; therefore,

$$\mathrm{d}\lambda = \mathrm{n}\,\mathrm{d}t + \frac{2h}{a^2}(1 + r_0)\, e\cos\xi\,\mathrm{d}t$$

$$= \mathrm{n}\,\mathrm{d}t + 2\mathrm{n}\left(1 - \frac{m^2}{2}\right)(1 + m^2)\, e\cos\xi\,\mathrm{d}t$$

$$c = 1 - \frac{7}{4}m^2 \qquad \mathrm{c}\,\mathrm{n} = c\,n \qquad \mathrm{c} = 1 - \frac{3}{4}m^2$$

$$\lambda = \mathrm{n}t + 2\left(1 + \frac{3}{4}m^2 + \frac{m^2}{2}\right) e\sin\xi$$

$$\lambda = \mathrm{n}t + 2\left(1 + \frac{5}{4}m^2\right) e\sin\xi + \&c.$$

$$\frac{\mathrm{a}}{r} = 1 + \frac{m^2}{6} + \frac{m^2 e_{\prime}^2}{4} + e\left(1 + \frac{2}{3}m^2\right)\cos\xi + \&c.$$

These are the expressions for λ and for $\frac{1}{r}$, when the quantity e is retained; but if the coefficient of $\sin\xi$, in the expression for λ, be called $2\,\mathrm{e}$, after the manner adopted for the planets in the first volume of the Mécanique Céleste, so that

$$\lambda = \mathrm{n}t + 2\,\mathrm{e}\sin\xi + \&c., \quad \text{then}$$

$$\frac{\mathrm{a}}{r} = 1 + \frac{m^2}{6} + \frac{m^2 e_{\prime}^2}{4} + \mathrm{e}\left(1 - \frac{7}{12}m^2\right)\cos\xi + \&c.$$

As the preceding results do not quite agree with those of M. PLANA, I shall endeavour to show how they may be obtained from the same equations which he employs.

$$\frac{d^2 . \frac{1}{r}}{d\lambda^2} + \frac{1}{r} - \frac{1}{h^2}\left\{1 + 2\int r^2 \frac{dR}{d\lambda} d\lambda\right\} - \frac{r}{h^2}\left\{r\frac{dR}{dr} - \frac{1}{r}\frac{dR}{d\lambda} \cdot \frac{dr}{d\lambda}\right\} = 0$$

$$R = -\frac{r^2}{4 r_{\prime}^3}\left\{1 + 3\cos(2\lambda - 2\lambda_{\prime})\right\}$$

In order to integrate the preceding differential equation, let

$$\frac{h^2}{r} = \mu\{1 + r_0 + r_1 \cos(2\lambda - 2m\lambda) + e\cos(c\lambda - \varpi) + e r_3 \cos(2\lambda - 2m\lambda - c\lambda + \varpi)\}$$
[0] [1] [2] [3]

The letters r_0, r_1, &c., being now used in a somewhat different sense to heretofore, having now reference to the expression for $\frac{1}{r}$ in terms of the true longitude.

$$\frac{1}{r}^{-3} = \frac{h^6}{\mu^3}(1 + 3e^2 + \&c.)$$

Neglecting $e_{\prime}^2$,

$$r^0 + \frac{h^6}{2 a_{\prime}^3}\frac{m_{\prime}}{\mu}(1 + 3e^2) = 0.$$

Substituting in this equation for h its elliptic value which is allowable, $r_0 = -\frac{m^2}{2}$, also $r_3 = \frac{15}{8} m$,

$$\left(\frac{1}{r}\right)^{-2} = \frac{h^4}{\mu^2}\left\{1 + m^2 + \frac{225}{128} \times 3m^2e^2 + \frac{3e^2}{2} + 3m^2e^2\right\}$$

$$= \frac{h^4}{\mu^2}\left\{1 + \frac{3e^2}{2} + m^2 + \frac{1059}{128}m^2e^2\right\}$$

$$\frac{dt}{d\lambda} = \frac{r^2}{h} = \frac{h^3}{\mu^2}\left\{1 + \frac{3e^2}{2} + m^2 + \frac{1059}{128}m^2e^2\right\} = \sqrt{\frac{a^3}{\mu}}$$

$$h^2 = a\left\{1 + \frac{3e^2}{2} + m^2 + \frac{1059}{128}m^2e^2\right\}^{-\frac{2}{3}}$$

$$= a\left\{1 - e^2 - \frac{2}{3}m^2 - \frac{739}{192}m^2e^2\right\}$$

$$= a\left\{1 - e^2 - \frac{2}{3}m^2 - \frac{739}{192}m^2e^2\right\}\left\{1 + \frac{2m^2}{3} + \frac{3}{4}m^2e^2\right\}$$

$$= a\left\{1 - e^2 - \frac{241}{64}m^2e^2\right\} \text{ as before.}$$

$$\frac{h^2}{r} = \frac{\mathrm{a}\left\{1 - e^2 - \frac{2}{3}m^2 - \frac{739}{192}m^2 e^2\right\}}{r}$$

$$= 1 - \frac{m^2}{2} + e \cos(\mathrm{c}\,\lambda - \varpi) + \&c.$$

$$\frac{\mathrm{a}}{r} = 1 + e^2 + \frac{m^2}{6} + \frac{899}{192} m^2 e^2 + e\left(1 + \frac{2}{3}m^2\right)\cos(\mathrm{c}\,\lambda - \varpi) + \&c.$$

My letter a appears to correspond with a $(1 + p)$, or a in M. Plana's notation.

n	n
c	c
μ	σ
R	Ω
$\frac{1}{r}$	u
λ	v.

Putting $e\left((1 - \frac{m^2}{2}\right)$ instead of e in the various expressions found above,

$$\lambda = \mathrm{n}\,t + 2\left(1 + \frac{3}{4}m^2\right) e \sin \xi,$$

which then so far agrees with the expression of M. Plana*, and I then find

$$\frac{\mathrm{a}}{r} = 1 + \frac{m^2}{6} + \frac{m^2 e_{\prime}^2}{4} + e\left(1 + \frac{m^2}{6}\right)\cos\xi + \&c.$$

$$\frac{\mathrm{a}}{r} = 1 + e^2 + \frac{m^2}{6} + \frac{707}{192} m^2 e^2 + e\left(1 + \frac{m^2}{6}\right)\cos(\mathrm{c}\,\lambda - \varpi).$$

M. Plana has

$$\frac{\mathrm{a}}{r} = 1 + \frac{m^2}{6} - \frac{45}{16} m^2 e^2 + \frac{m^2 e_{\prime}^2}{4} + e\left(1 + \frac{m^2}{6}\right)\cos\xi + \&c.\dagger$$

$$\frac{\mathrm{a}}{r} = 1 + e^2 + \frac{m^2}{6} + \frac{167}{192} m^2 e^2 + e\left(1 + \frac{m^2}{6}\right)\cos(\mathrm{c}\,\lambda - \varpi) + \&c.\ddagger$$

which equations do not agree with those I have found. I am, however, well aware how difficult it is to escape error in these inquiries, and wish to be understood as not offering any of the results contained in this paper too confidently.

Wherever I presumed to have arrived at figures differing from those of M. Plana, I verified afresh all the steps of the process contained in previous papers, particularly the corresponding term in the development of R. Thus I have found by means of the expressions given §, that three times the numerical coefficient of $e_{\prime}^3 \cos(2\,\tau - \xi_{\prime})$

* Vol. i. p. 574. † Vol. i. p. 664. ‡ Vol. i. p. 636.

§ Philosophical Transactions, 1832, p. 601.

in the development of R,

$$= \frac{63}{16} + \frac{81}{64} + \frac{45}{16} + \frac{27}{32} - \frac{153}{16} + \frac{18}{32} + \frac{30}{8} + \frac{30}{32} + \frac{204}{16} = \frac{1107}{64}.$$

The coefficient in question $= \frac{369}{64} = \frac{21}{8} \times \frac{153}{56}$.

But I have found that the following corrections are required. For

$$+ \frac{3}{8} \left\{ 1 - \frac{5}{2} e^2 - 4 e_{\prime}^2 \right\} \cos^4 \frac{\iota}{2} \frac{a^2}{a_{\prime}^3} e_{\prime} \cos (2\tau + \xi_{\prime})$$

[7]

read

$$+ \frac{3}{8} \left\{ 1 - \frac{5}{2} e^2 - \frac{e_{\prime}^2}{8} \right\} \cos^4 \frac{\iota}{2} \frac{a^2}{a_{\prime}^3} e_{\prime} \cos (2\tau + \xi_{\prime})$$

and for

$$- \frac{51}{64} \frac{a^2}{a_{\prime}^3} e_{\prime}^2 \gamma^2 \cos (2\xi_{\prime} - 2\eta) - \frac{45}{64} \frac{a^2}{a_{\prime}^3} e_{\prime}^2 \gamma^2 \cos (2\xi_{\prime} + 2\eta) - \frac{195}{64} \frac{a^2}{a_{\prime}^3} e_{\prime}^2 \gamma^2 \cos (2\tau - 2\xi_{\prime} - 2\eta)$$

[95] [96] [97]

read

$$- \frac{27}{16} \frac{a^2}{a_{\prime}^3} e_{\prime}^2 \gamma^2 \cos (2\xi_{\prime} - 2\eta) - \frac{27}{16} e_{\prime}^2 \gamma^2 \cos (2\xi_{\prime} + 2\eta) - \frac{51}{16} \frac{a^2}{a_{\prime}^3} e_{\prime}^2 \gamma^2 \cos (2\tau - 2\xi_{\prime} - 2\eta).$$

IX. *On the Tides.* *By* J. W. Lubbock, *Esq. V. P. and Treas. R.S.*

Received December 16, 1833,—Read February 20, 1834.

I HAVE already presented to the Society some tables exhibiting results obtained by the discussion of many observations of the tides made at the London Docks. I have now to communicate other tables, also calculated, according to my instructions, by Mr. Dessiou. In those already published in the Philosophical Transactions, which have reference to the corrections due to the influence of the parallax and declination of the moon, Mr. Dessiou employed only observations of the tides made between conjunction and opposition; but considering it of importance to establish these corrections upon a greater number of observations, and also to ascertain whether any appreciable difference existed, Mr. Dessiou undertook to obtain similar corrections from observations made between opposition and conjunction. The difference, if there be any, is very small.

It formerly escaped my notice, that the correction due to the influence of the moon's declination is mixed up with that for the calendar months: but the inclination of the moon's orbit to the ecliptic is small; and when observations are considered throughout an entire revolution of her node, she may be taken to move in the ecliptic; so that her time of transit being given on any day of the year, her declination is also given. This suggestion was made to me by Mr. Whewell, and I have endeavoured to remedy this difficulty, and have now calculated the correction for the calendar months roughly, which may serve until there has been time to obtain it with greater precision.

My corrections for the influence of the moon's parallax and declination may, I think, be safely adopted; and I doubt if they be susceptible of much further improvement. Their discrepancy from the theory of Bernoulli is worthy of remark, as may be seen in the following Table, which gives Bernoulli's correction for the influence of the moon's parallax, and that which Mr. Dessiou has calculated.

Time of Moon's Transit.	Bernoulli.	Observation.	Bernoulli.	Observation.	Time of Moon's Transit.
	Moon's Horizontal Parallax.				
	54′		60′		
0	+ 4	+12	− 4	−11	0
1	+ 2	+11	− 2	− 9	1
2	0	+ 7	0	− 7	2
3	− 2	+ 7	+ 2	− 6	3
4	− 4	+ 4	+ 4	− 6	4
5	− 7	+ 1	+ 5	− 3	5
6	− 9	+ 1	+ 7	− 3	6
7	− 9	+ 6	+ 6	− 7	7
8	0	+14	0	−11	8
9	+ 9	+18	− 6	−15	9
10	+ 9	+15	− 7	−16	10
11	+ 7	+13	− 5	−14	11

The correction for parallax 57′ being considered as given by the semimenstrual inequality, Bernoulli's correction for the influence of the moon's declination is equally erroneous; but his accurate determination of the law of the semimenstrual inequality is one of the most important results ever obtained *à priori* by means of the theory of universal gravitation.

The theory of the tides is now, as Mr. Whewell remarks, in the state which that of the motions of the moon and planets presented about a century ago; and unless considerable exertions be made, it may so continue for many years to come. The tables of the planets have only acquired their present accuracy through the liberal encouragement of learned bodies, and of some of the governments of Europe; nor can tables of the tides, adapted to the present state of science, be now constructed, unless a very considerable expense be incurred, from the immense labour required.

In discussing tide observations, when the greatest possible accuracy is desired, and when the correctness of the observations appears to warrant such a nicety, in order to obtain any particular correction, all known approximate corrections of a different nature should first be separately applied to each observation with a contrary sign.

So, in the determination of the correction for the calendar months, it would be well to correct all the observations first for parallax and declination; and this consideration should be particularly attended to, where it is practicable, in attempting to determine, from a few observations, the establishment of any port and the semimenstrual inequality. The best method of verifying my tables would be to determine by their means the times and heights of high water at the London Docks for nineteen years, and then to classify the transits of the moon with the errors of those determinations, according to calendar months, and according to parallaxes and declinations: the average error corresponding to each calendar month would be the error of the Table* for the calendar months, and so for the rest.

* I allude now more particularly to that which accompanies this paper, not to any given previously.

Mr. W. Peirce, of the London Docks, under whose care the observations have been made, has kindly communicated to me the following information.

The observations were originally instituted at the instigation of Mr. W. Vaughan, of Fenchurch-street, then one of the directors, in consequence of having seen an account of the tides kept at Liverpool. The time was always taken by Wapping church clock, which is considered in Wapping as good a timekeeper as any in London. The observations, previously to the opening of the Docks, viz. from 1801 to January 1805, were taken by Mr. Peirce in the day, and by a foreman in the night. After the Docks opened, from 1805 to 1828, inclusive, they were taken by the watchman in the day, and by Mr. Peirce and two foremen, who attended alternately, fourteen nights each. From 1823 up to the present time they are taken in the day by the foreman at the entrance lock, in the night by two foremen alternately. The time has, at my instigation, been more particularly attended to since I had the accounts (1829), when Mr. Peirce gave the foreman charge to be particularly careful. The heights, previously to the opening of the Docks (viz. 1801 to 1805), were taken by the averaged eighteen-feet tide at the Trinity marks, or, as it is called, Trinity datum. The marks were fixed in a wall where the entrance now is. The lock being made five feet deeper than Trinity datum, there are twenty-three feet at the lock when the water is at the eighteen-feet mark Trinity datum; therefore the difference between the lines from which the heights are reckoned in the books containing the observations is five feet; so that eighteen feet previously to 1805 is the same height or depth as twenty-three feet after that time. In similar accounts of tide observations, the initials or the name at full length of the observer should be affixed to each observation, in order to afford a check upon the care with which they are made.

Sir John Hall has kindly favoured me with the following information relative to the influence of the wind upon the tides in the port of London. Sir John Hall procured the joint opinion of some nautical men, including the dock-master of the St. Katharine Docks, and the senior harbour-master of the port. The following is the result of their sentiments respecting the influence of the wind upon the tides in the river Thames.

During strong north-westerly gales, the tide marks high water earlier than otherwise, and does not give so much water, whilst the ebb-tide runs out later, and marks lower; but upon the gales abating, and the weather moderating, the tides put in, and rise much higher, whilst they also run longer before high water is marked, and with more velocity of current, nor do they run out so long or so low. The reason assigned for all this is, that the strong north-west winds drive the sea along the Dutch coast, through the straits of Dover, and consequently away from the mouth of the Thames; so that the tides, during north-west winds, are always much higher (producing frequently ruinous flooding,) on the Dutch than upon the English coast. A south-westerly gale has a contrary effect generally, and an easterly one gives some water; but the tides, in all these cases, always improve the moment the weather moderates.

This is the opinion of those most competent to form one, from their daily experience, and is no doubt correct. The subject is one of considerable importance, as regards the accuracy of which tide predictions are susceptible, and merits further inquiry, in order to ascertain, if possible, the error which may be expected for a wind of given force and direction.

In order to obtain the correction for the calendar months, I begin by forming the following Table, which gives the moon's declination roughly, but sufficiently near for my purpose, in different months of the year, supposing her to move in the ecliptic.

TABLE A.

Moon's Transit.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Moon's Transit.
h	°	°	°	°	°	°	°	°	°	°	°	°	h
0	21	13	2	10	19	23	22	14	3	8	18	23	0
1	18	6	4	15	22	23	19	8	4	14	21	23	1
2	11	0	10	19	23	22	14	2	10	18	23	22	2
3	8	6	16	22	23	19	9	4	15	21	23	19	3
4	3	12	20	23	22	14	2	10	19	23	22	15	4
5	5	17	22	23	19	9	4	15	22	23	19	9	5
6	11	20	23	22	15	3	10	19	23	22	15	3	6
7	16	23	23	19	10	4	15	22	23	19	10	3	7
8	20	23	21	14	3	10	19	23	23	16	4	9	8
9	22	23	18	9	3	15	22	23	19	10	2	15	9
10	23	20	14	3	9	19	23	21	15	4	9	19	10
11	21	17	9	4	15	22	23	18	9	2	14	22	11

The difference between the interval from the mean time of the moon's transit and the time of high water, and the mean interval, is according to the Table given in the Philosophical Transactions for 1831, p. 412.

TABLE B.

Moon's Transit.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Moon's Transit.
h	m	m	m	m	m	m	m	m	m	m	m	m	h
0	− 5	+ 2	+ 4	+ 7	− 2	− 7	− 4	0	+ 9	+ 7	− 3	− 7	0
1	− 3	+ 3	+ 6	+ 4	− 3	− 7	− 1	+ 8	+ 8	+ 3	− 8	− 7	1
2	− 1	+ 3	+ 8	+ 3	− 6	− 6	− 1	+11	+ 8	+ 2	−10	− 7	2
3	+ 2	+ 6	+ 5	+ 1	− 9	− 5	+ 2	+10	+ 7	− 4	−11	− 8	3
4	+10	+ 7	+ 2	− 6	− 8	− 1	+10	+12	+ 4	− 9	−14	− 3	4
5	+13	+ 3	− 4	−12	− 9	+ 5	+15	+ 9	− 3	−15	−11	+ 1	5
6	+16	− 2	−12	−14	− 4	+14	+18	+ 5	−10	−18	− 4	+13	6
7	+13	−11	−22	−12	+ 7	+21	+14	− 5	−19	−14	+ 8	+16	7
8	0	−27	−26	− 4	+14	+16	+ 5	−11	−13	+ 6	+18	+14	8
9	− 7	−23	− 9	+ 5	+13	+ 3	− 8	−11	− 1	+13	+15	+ 6	9
10	− 8	− 8	+ 2	+ 6	+ 7	− 3	−11	− 5	+ 4	+14	+ 5	− 4	10
11	− 6	− 1	+ 3	+ 6	+ 4	− 7	− 7	− 2	+ 7	+ 8	+ 3	− 3	11

The number − 3, January, moon's transit 1^h, was inferred by subtracting $1^h\ 39^m$ from $1^h\ 42^m$, column A.* Column A. is the semimenstrual inequality + a constant. If we suppose the proper argument of this inequality to be the apparent time of the

* Philosophical Transactions, 1831, p. 401.

moon's transit, as theory suggests, the equation of time for the middle of January being + 10^m, then $1^h\ 39^m$ should be subtracted from $1^h\ 44^m{\cdot}8$, the semimenstrual inequality corresponding to the moon's transit at 50^m. This gives − 2·8 to be added to − 3. I therefore formed the following Table, which gives the quantity to be added for a given equation of time, in order to reduce the Table B. to what it should be, having the argument of the moon's transit in apparent time.

Table C.

Moon's Transit.	Equation of Time.		
	+ 5	+ 10	+ 15
h	m	m	m
0	−1·1	−2·3	−3·3
1	−1·2	−2·4	−3·6
2	−1·3	−2·7	−4·0
3	−1·3	−2·6	−3·9
4	− ·9	−1·8	−2·7
5	− ·5	−1·0	−1·5
6	0	0	0
7	+1·9	+3·9	+5.8
8	+2·8	+5·7	+8·4
9	+2·1	+4·2	+6·3
10	0	0	0
11	− ·7	−1·4	−2·1

By interpolation I then formed the following Table, taking the equation of time for the middle of the month.

Table D.

Time of Moon's Transit.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Time of Moon's Transit.
	+10	+14	+ 9	0	− 4	0	+ 5	+ 4	− 5	−14	−15	− 4	
h													h
0	−2·3	−3·2	−1·9		+ ·9		−1·1	− ·9	+1·1	+3·2	+3·3	+ ·9	0
1	−2·4	−3·3	−2·1		+ ·9		−1·2	− ·9	+1·2	+3·3	+3·6	+ ·9	1
2	−2·7	−3·7	−2·4		+1·0		−1·3	−1·0	+1·3	+3·7	+4·0	+1·0	2
3	−2·6	−3·6	−2·3		+ ·9		−1·3	−1·0	+1·3	+3·6	+3·9	+ ·9	3
4	−1·8	−2·5	−1·6		+ ·7		− ·9	− ·7	+ ·9	+2·5	+2·7	+ ·7	4
5	−1·0	−1·4	− ·9		+ ·4		− ·5	− ·4	+ ·5	+1·4	+1·5	+ ·4	5
6	0	0	0		0		0	0	0	0	0	0	6
7	+3·0	+5·4	+3·5		−1·3		+1·4	+1·5	−1·4	−5·4	−5·8	−1·3	7
8	+5·7	+7·9	+5·1		−2·2		+2·8	+2·2	−2·8	−7·9	−8·4	−2·2	8
9	+4·2	+5·8	+3·9		−1·6		+2·1	+1·6	−2·1	−5·8	−6·3	−1·6	9
10	0	0	0		0		0	0	0	0	0	0	10
11	−1·4	−1·9	−1·2		+ ·5		− ·7	− ·5	+ ·7	+1·9	+2·1	+ ·5	11

Adding the figures in the preceding Table to those in Table B., neglecting fractions of a minute, I obtained the following.

Table E.

Moon's Transit.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Moon's Transit.
h	m	m	m	m	m	m	m	m	m	m	m	m	h
0	− 7	− 1	+ 2	+ 7	− 1	− 7	− 5	− 1	+10	+10	0	− 6	0
1	− 5	0	+ 4	+ 4	− 2	− 7	− 2	+ 7	+ 9	+ 6	− 4	− 6	1
2	− 4	− 1	+ 6	+ 3	− 5	− 6	− 2	+10	+ 9	+ 6	− 6	− 7	2
3	− 1	+ 2	+ 2	+ 1	− 8	− 5	+ 1	+ 9	+ 8	0	− 7	− 7	3
4	+ 8	+ 5	0	− 6	− 7	− 1	+ 9	+11	+ 5	− 7	−11	− 2	4
5	+12	+ 2	− 5	−12	− 9	+ 5	+15	+ 9	− 3	−14	−10	+ 1	5
6	+16	− 2	−12	−14	− 4	+14	+18	+ 5	−10	−18	− 4	+13	6
7	+17	− 6	−19	−12	+ 6	+21	+15	− 4	−20	−19	+ 2	+15	7
8	+ 6	−19	−21	− 4	+12	+16	+ 8	− 9	−16	− 2	+10	+12	8
9	− 3	−17	− 5	+ 5	+11	+ 3	− 6	− 9	− 3	+ 7	+ 9	+ 4	9
10	− 8	− 8	+ 2	+ 6	+ 7	− 3	−11	− 5	+ 4	+14	+ 5	− 4	10
11	− 7	− 3	+ 2	+ 6	+ 4	− 7	− 8	− 2	+ 8	+10	+ 5	− 3	11

I next formed the following Table, which gives that portion of the preceding which is due to the moon's declination, the correction for the moon's declination being obtained, by means of Table A., from Table XIX.

Table F.

Moon's Transit.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Moon's Transit.
h	m	m	m	m	m	m	m	m	m	m	m	m	h
0	− 6	+ 1	+ 7	+ 3	− 4	− 7	− 7	+ 1	+ 7	+ 5	− 3	− 7	0
1	0	+ 5	+ 5	+ 1	− 7	− 7	− 2	+ 4	+ 5	+ 1	− 6	− 7	1
2	+ 3	+ 5	+ 3	− 5	− 7	− 7	+ 1	+ 4	+ 1	− 7	− 7	− 7	2
3	+ 7	+ 7	− 1	− 6	− 6	− 4	+ 6	+ 6	0	− 6	− 6	− 4	3
4	+ 8	+ 6	− 4	− 7	− 7	+ 3	+ 8	+ 7	− 4	− 7	− 7	0	4
5	+12	− 1	− 7	− 7	− 4	+12	+12	+ 1	− 7	− 8	− 4	+12	5
6	+11	− 5	− 9	− 9	+ 2	+15	+11	− 5	− 9	− 9	+ 2	+15	6
7	− 1	−13	−13	− 6	+10	+17	+ 2	−11	−13	− 6	+10	+19	7
8	− 7	−14	− 8	+ 3	+17	+ 9	− 7	−15	−12	− 3	+15	+10	8
9	− 7	− 9	− 5	+ 8	+11	0	− 7	− 8	− 5	+ 7	+11	0	9
10	− 8	− 4	+ 3	+ 8	+ 7	− 4	− 6	− 4	0	+ 8	+ 7	− 4	10
11	− 7	− 3	+ 6	+ 7	0	− 8	− 8	− 5	+ 5	+ 6	+ 1	− 8	11

Subtracting the figures in the preceding Table from those in Table E., I get the following, which gives the correction for the calendar months.

Table G.

Moon's Transit.	January.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	October.	Nov.	Dec.	Moon's Transit.
h	m	m	m	m	m	m	m	m	m	m	m	m	h
0	− 1	− 2	− 5	+ 4	+ 3	0	+ 2	− 2	+ 3	+ 5	+ 3	+ 1	0
1	− 5	− 5	− 1	+ 3	+ 5	0	....	+ 3	+ 4	+ 5	+ 2	+ 1	1
2	− 8	− 6	+ 3	+ 8	+ 2	+ 1	− 3	+ 6	+ 8	+13	+ 1	0	2
3	− 8	− 5	+ 3	+ 7	− 2	− 1	− 5	+ 3	+ 8	+ 6	− 1	− 3	3
4	0	− 1	+ 4	+ 1	0	− 4	+	+ 4	+ 9	0	− 4	− 2	4
5	+ 2	+ 3	+ 2	− 5	− 5	− 7	+ 3	+ 8	+ 4	− 6	− 6	−11	5
6	+17	+ 3	− 3	− 5	− 6	− 1	+ 7	+10	− 1	− 9	− 6	− 2	6
7	+25	+ 7	− 6	− 6	− 4	+ 4	+13	+ 7	− 7	−13	− 8	− 4	7
8	+19	− 5	−13	− 7	− 5	+ 7	+15	+ 6	− 4	+ 1	− 5	+ 2	8
9	+ 6	− 8	0	− 3	0	+ 3	+ 1	− 1	+ 2	0	− 2	+ 4	9
10	− 2	− 4	− 5	− 2	0	+ 1	− 5	− 1	+ 4	+ 6	− 2	0	10
11	0	0	− 4	− 1	+ 4	− 1	0	+ 3	+ 2	+ 4	+ 4	+ 5	11

The numbers in the last Table are extremely irregular, and leave the correction due to the calendar months subject to much uncertainty.

The following Tables are subjoined, forming a sequel to those already printed in the Philosophical Transactions for 1831.

Table XXII. shows the height of high water at the London Docks, corresponding to the mean time of the moon's transit in each month of the year, from 6565 observations, made between the 1st of January 1808, and the 31st of December 1826 and between opposition and conjunction.

Table XXIII. is interpolated from Table XXII.

Table XXIV. gives the mean of Table XXIII. and Table V.

Table XXV. shows the time and height of high water at the London Docks, corresponding to the time of the moon's transit for every minute of horizontal parallax, from 5413 observations, made between the 1st of January 1808 and the 31st of December 1826, between opposition and conjunction.

Tables XXVI. and XXVII. are interpolated from Table XXV.

Table XXVIII. shows the time and height of high water at the London Docks, corresponding to the time of the moon's transit, and for every three degrees of her declination, north and south, from 5424 observations, made between the 1st of January 1808 and the 31st of December 1826, between opposition and conjunction.

Tables XXIX. and XXX. are interpolated from Table XXVIII.

Table XXXI. shows the difference in the interval between the time of the moon's transit and the time of high water, and the mean interval (Column A. Table III.) for every minute of the moon's horizontal parallax between opposition and conjunction.

Table XXXII. shows the difference in the height of high water, and the mean height (Column B. Table XXIII.) for every minute of the moon's horizontal parallax, between opposition and conjunction.

Table XXXIII. shows the difference in the interval between the time of the moon's transit and the time of high water, and the mean interval (Column A. Table III.) for every three degrees of the moon's declination, between opposition and conjunction.

Table XXXIV. shows the difference in the height of high water, and the mean height (Column B. Table XXIII.) for every three degrees of the moon's declination, between opposition and conjunction.

Table XXXV. (mean of Tables VII. and XXVI.) shows the interval between the moon's transit and the time of high water at the London Docks for every minute of her horizontal parallax, from 10,812 observations, made between the 1st of January 1808 and the 31st of December 1826.

Table XXXVI. shows the time and height of high water at the London Docks, corresponding to the mean time of the moon's transit for every minute of her horizontal parallax, from 10,812 observations, made between the 1st of January 1808 and 31st of December 1826.

Table XXXVII. is interpolated from Table XXXVI.

Table XXXVIII. (mean of Tables XI. and XXIX.) shows the interval between the moon's transit and the time of high water at the London Docks for every three degrees of her declination north or south, from 10,796 observations.

Table XXXIX. (mean of Table IX. and XXX.) shows the height of high water at the London Docks for every three degrees of the moon's declination north or south, from 10,796 observations.

Table XL. shows the difference in the interval between the time of the moon's transit and the time of high water, and the mean interval (Column A. Table III.) for every three degrees of her declination.

Table XLI. shows the difference in the height of high water, and the mean height (Column B. Table XXIV.) for every three degrees of the moon's declination.

Table XLII. shows the difference in the interval between the time of the moon's transit and the time of high water, and the mean interval (Column A. Table III.) for every minute of her horizontal parallax.

Table XLIII. shows the difference in the height of high water, and the mean height (Column B. Table XXIV.) for every minute of the moon's horizontal parallax.

Tables VI., IX., &c., given in the Philosophical Transactions for 1831, were formed from observations corresponding to transits of the moon between conjunction and opposition.

Table XXII.

Showing the Height of High Water at the London Docks corresponding to the mean time of the Moon's Transit in each month of the year; from 6565 observations made between the 1st of January 1808 and the 31st of December 1826, and between opposition and conjunction.

January.			February.			March.			April.			May.			June.		
Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.
h m	Feet.		h m	Feet.		h m	Feet.		h m	Feet.		h m	Feet.		h m	Feet.	
26	22·12	23	28·2	22·42	21	24	22·61	28	16·5	22·89	22	10	22·82	21	15	22·33	20
56	22·16	21	58·8	22·66	23	53·2	22·91	22	46·5	23·12	22	40·2	23·91	23	45	22·44	20
1 26	22·62	22	1 28·3	22·9	23	1 24	23·04	25	1 14·8	23·25	22	1 11·2	22·94	22	1 14	22·63	20
1 54	22·91	21	1 59	23·18	22	1 55	23·36	27	1 44·5	23·36	21	1 41·5	23·02	20	1 45·5	22·66	23
2 26	22·58	27	2 29·5	22·94	27	2 25	23·19	20	2 14·5	23·14	21	2 10	22·91	21	2 17	22·56	22
2 57	22·73	23	3 0·2	23·11	20	2 54	22·94	25	2 44·5	22·69	23	2 41	22·83	23	2 47	22·64	20
3 26	22·54	21	3 29·5	22·66	24	3 23	22·76	22	3 16·5	22·68	21	3 10·5	22·48	20	3 15	22·3	22
3 54	22·2	24	4 1	22·47	25	3 52	22·4	21	3 47	22·39	21	3 38·2	21·15	20	3 46	22·13	25
4 26	21·41	26	4 31·2	22·05	20	4 23	22·0	26	4 15·5	20·06	19	4 9·2	21·92	25	4 16	21·79	22
4 55	21·47	25	4 59	21·96	20	4 55·5	21·33	22	4 45	21·72	22	4 41·8	21·5	26	4 45	21·4	24
5 25·5	21·06	26	5 32	21·48	23	5 24	20·94	20	5 16·2	21·15	22	5 12·2	21·09	22	5 15·5	21·2	26
5 56	20·69	25	5 59	21·16	19	5 50·5	20·56	20	5 46	20·37	20	5 40·5	20·47	23	5 46	20·64	25
6 24	20·51	23	6 26·5	20·27	18	6 23	19·87	24	6 13·2	20·04	19	6 10·5	20·11	24	6 15	20·19	22
6 56	19·96	25	6 58·8	19·82	25	6 54	19·13	20	6 44	19·46	25	6 40	19·89	26	6 45	20·08	30
7 27	19·28	23	7 29·5	19·37	17	7 23	18·8	23	7 15·5	18·96	24	7 10·5	19·45	24	7 17·5	19·79	24
7 56	19·63	23	7 59	19·2	20	7 53·5	18·81	22	7 45	19·32	21	7 40	19·62	26	7 46	19·8	21
8 26	19·61	22	8 28	19·17	19	8 23·3	18·94	23	8 14·5	19·55	24	8 11·5	19·83	27	8 14	19·98	24
8 55	19·66	20	8 59·2	19·11	20	8 54	19·18	25	8 45·5	19·72	26	8 41·8	20·34	24	8 45	20·29	25
9 24	19·94	22	9 33	20·2	19	9 24	20·08	22	9 16	20·35	23	9 11·2	20·84	26	9 17	20·37	24
9 54	20·33	21	9 57·5	20·2	19	9 53	20·33	23	9 45·5	20·86	25	9 42·5	21·15	25	9 46	21·0	20
10 26	21·12	23	10 28·8	20·78	21	10 23·5	20·44	26	10 14·5	21·39	22	10 11·2	21·54	24	10 15	21·29	22
10 57	21·44	21	11 0	20·47	23	10 55·5	21·16	27	10 45·8	21·86	30	10 40·2	21·97	21	10 47	21·55	22
11 26	21·55	21	11 31	20·68	20	11 26	21·78	23	11 17	22·55	20	11 10·8	22·09	26	11 17	21·81	21
11 55	22·03	20	12 0	21·77	21	11 54	22·34	22	11 45·8	22·56	22	11 41·2	22·38	22	11 46	21·13	20

Table XXII. (Continued).

July.			August.			September.			October.			November.			December.		
Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.
h m	Feet.		h m	Feet.		h m	Feet.		h m	Feet.		h m	Feet.		h m	Feet.	
23	22·03	21	20	22·37	26	10	22·45	23	1	23·04	23	0	22·42	22	9	21·96	23
53	22·36	22	51	22·56	23	39	22·68	23	29·5	22·82	25	29	22·63	20	40	21·36	22
1 20	22·55	23	1 18·5	22·85	21	1 10	22·95	27	1 0	23·11	22	1 0	22·78	21	1 11	22·62	21
1 51	22·65	21	1 48	22·86	26	1 41	23·07	25	1 30	23·13	24	1 30	22·86	23	1 41·5	22·16	25
2 20	22·62	25	2 19·2	22·81	26	2 12	22·88	23	2 0	22·6	22	2 1	22·53	21	2 14	22·45	21
2 51	22·49	25	2 50	22·62	23	2 41	22·48	22	2 29	22·63	23	2 30	22·3	20	2 43	22·7	21
3 21	22·32	25	3 19	22·6	22	3 10	22·67	23	3 0	22·59	22	3 0	22·16	21	3 11	22·14	23
3 52	22·31	25	3 49·8	22·25	27	3 39·5	22·54	21	3 30	22·3	22	3 30	22·36	22	3 41	22·09	25
4 21	22·09	27	4 21·8	22·0	22	4 9	22·8	23	4 1	22·63	23	3 59	21·76	20	4 12	21·65	23
4 52	21·6	26	4 51·5	21·6	22	4 40	21·25	22	4 31	21·33	20	4 30	21·61	24	4 40·5	21·39	24
5 22	21·39	23	5 19	21·6	22	5 11	21·2	23	5 0·5	21·03	23	5 1	21·11	24	5 11	20·87	29
5 50	20·97	26	5 48	20·86	22	5 41	20·3	19	5 31	20·2	23	5 30	20·19	20	5 42	20·38	25
6 22	20·43	28	6 19·5	20·22	25	6 10·5	19·42	20	6 2	19·86	21	6 0	19·7	26	6 12	19·95	26
6 51	20·17	24	6 50	19·76	22	6 41	19·62	19	6 30·5	19·26	24	6 31·5	19·54	25	6 41	19·78	24
7 20	19·92	23	7 20	19·54	23	7 10·5	19·11	21	7 0	18·78	20	7 2	19·78	22	7 9·5	19·58	24
7 49	19·75	22	7 50	19·26	21	7 41·5	18·66	21	7 29·2	18·71	26	7 30	19·36	23	7 40	19·81	28
8 20	19·79	24	8 19·5	19·47	24	8 11	18·87	22	8 1	19·09	25	7 59·5	19·39	26	8 12	20·41	25
8 50	19·49	22	8 49	19·39	21	8 41	19·36	20	8 31	19·55	23	8 29	20·07	23	8 40	19·8	23
9 21	20·16	22	9 19·2	19·86	23	9 10	19·79	22	9 1	20·12	25	9 0·5	20·66	26	9 13·5	20·33	25
9 51	20·5	21	9 51	20·32	23	9 40	20·46	25	9 30	21·0	23	9 31	21·17	24	9 42·5	21·26	19
10 20	20·87	20	10 21	20·91	21	10 10	21·25	23	10 0·5	21·53	29	10 1·5	21·76	23	10 11	21·39	22
10 50	21·29	21	11 51·5	21·25	22	10 40	21·2	21	10 32	22·04	25	10 31	22·49	22	10 41·5	21·75	22
11 20	21·68	22	11 19	21·45	21	11 95	22·05	26	11 2	22·35	24	10 59	22·22	19	11 10·5	21·48	20
11 51	21·85	24	11 49	22·03	22	11 39	22·51	22	11 31	22·8	22	11 28	22·58	23	11 41	22·4	21

Table XXIII. (Interpolated from Table XXII.)

Showing the Height of High Water at the London Docks.

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
Moon's Transit.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	B.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 0	22·05	21·77	22·39	22·71	22·68	21·71	21·9	22·15	22·49	23·03	22·42	22·1	22·18
0 30	22·13	22·44	22·67	22·99	23·54	22·38	22·11	22·43	22·61	22·83	22·63	21·56	22·53
1 0	22·22	22·67	22·94	23·2	23·29	22·54	22·41	22·65	22·86	23·11	22·78	22·17	22·74
1 30	22·66	22·91	23·08	23·31	22·99	22·64	22·58	22·85	23·03	23·13	22·86	22·33	22·86
2 0	22·85	23·17	23·33	23·25	22·95	22·61	22·64	22·84	22·95	22·6	22·54	22·33	22·84
2 30	22·6	22·94	23·15	22·97	22·86	22·6	22·58	22·74	22·63	22·63	22·3	22·59	22·72
3 0	22·71	23·11	22·9	22·69	22·59	22·48	22·44	22·61	22·6	22·59	22·16	22·36	22·60
3 30	22·5	22·66	22·7	22·61	21·53	22·22	22·32	22·48	22·58	22·3	22·36	22·11	22·36
4 0	22·09	22·48	22·3	22·26	21·69	21·97	22·25	22·17	22·72	21·65	21·76	21·82	22·10
4 30	21·42	22·07	21·88	21·89	21·66	21·6	21·95	21·89	21·43	21·34	21·61	21·49	21·69
5 0	21·43	21·95	21·25	21·5	21·25	21·3	21·54	21·6	21·22	21·03	21·13	21·05	21·35
5 30	21·03	21·45	20·87	20·8	20·7	20·93	21·27	21·32	20·63	20·23	20·19	20·57	20·83
6 0	20·66	21·14	20·4	20·2	20·23	20·42	20·80	20·62	19·73	19·88	19·7	20·12	20·33
6 30	20·44	20·19	19·71	19·72	19·96	20·13	20·35	20·06	19·55	19·27	19·55	19·84	19·90
7 0	19·87	19·8	19·01	19·17	19·6	19·95	20·09	19·69	19·29	18·78	19·76	19·65	19·55
7 30	19·32	19·37	18·8	19·14	19·57	19·8	19·86	19·45	18·83	18·72	19·36	19·73	19·33
8 0	19·63	19·2	18·84	19·43	19·76	19·89	19·76	19·33	18·8	19·08	19·4	20·19	19·44
8 30	19·62	19·17	18·98	19·64	20·15	20·14	19·69	19·43	19·18	19·53	20·09	20·02	19·64
9 0	19·71	19·13	19·26	20·03	20·65	20·33	19·71	19·56	19·64	20·1	20·65	20·12	19·91
9 30	19·98	20·15	20·19	20·6	21·03	20·65	20·26	20·02	20·24	21·0	21·15	20·86	20·51
10 0	20·45	20·23	20·37	21·12	21·39	21·14	20·61	20·5	20·99	21·52	21·73	21·34	20·95
10 30	21·19	20·76	20·62	21·63	21·82	21·42	21·03	21·01	21·22	22·01	22·47	21·61	21·40
11 0	21·46	20·47	21·26	22·17	22·05	21·62	21·42	21·31	21·77	22·33	22·23	21·58	21·64
11 30	21·61	20·67	21·84	22·55	22·28	21·5	21·73	21·66	22·37	22·78	22·56	22·08	21·97

Table XXIV. (Mean of Table XXIII. and Table V.)

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Mean.
Moon's Transit.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	Height of Tide.	B.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 0	22·17	22·00	22·42	22·69	22·71	22·01	21·92	22·14	22·52	23·0	22·55	22·30	22·32
0 30	22·10	22·58	22·76	22·94	23·15	22·40	22·22	22·39	22·68	22·92	22·85	21·97	22·58
1 0	22·29	22·69	22·90	23·10	23·08	22·53	22·44	22·60	22·87	23·12	22·91	22·19	22·71
1 30	22·56	22·83	23·01	23·11	22·95	22·61	22·60	22·76	22·94	23·12	22·93	22·36	22·81
2 0	22·80	22·94	23·06	23·09	22·95	22·64	22·67	22·74	22·99	22·78	22·73	22·53	22·82
2 30	22·61	22·96	22·94	22·95	22·78	22·57	22·62	22·75	22·80	22·61	22·42	22·60	22·72
3 0	22·69	22·92	22·67	22·66	22·53	22·41	22·51	22·65	22·72	22·58	22·48	22·35	22·60
3 30	22·25	22·53	22·59	22·44	21·88	22·18	22·40	22·49	22·66	22·46	22·36	22·11	22·38
4 0	22·12	22·30	22·29	22·12	21·79	22·01	22·31	22·12	22·51	21·80	21·85	21·89	22·10
4 30	21·66	21·93	21·83	21·77	21·58	21·64	22·01	21·78	21·63	21·48	21·58	21·56	21·71
5 0	21·51	21·64	21·32	21·36	21·09	21·26	21·58	21·60	21·27	20·95	21·01	21·13	21·32
5 30	21·04	21·31	20·99	20·69	20·53	20·89	21·28	21·26	20·74	20·44	20·39	20·76	20·86
6 0	20·78	20·80	20·43	20·14	20·13	20·50	20·85	20·65	20·02	19·80	19·80	20·30	20·35
6 30	20·28	19·89	19·76	19·62	19·92	20·24	20·45	20·10	19·67	19·56	19·54	19·99	19·92
7 0	19·85	19·58	19·16	19·09	19·70	19·96	20·14	19·76	19·25	19·04	19·47	19·74	19·56
7 30	19·48	19·30	19·03	19·04	19·66	19·80	19·92	19·51	18·97	18·88	19·46	19·66	19·37
8 0	19·48	19·15	18·86	19·25	19·87	19·92	19·90	19·35	18·90	19·11	19·51	19·93	19·44
8 30	19·74	19·06	19·12	19·51	20·19	20·21	19·88	19·42	19·24	19·68	19·87	20·11	19·67
9 0	19·82	19·22	19·35	20·01	20·66	20·48	19·98	19·64	19·72	20·15	20·65	20·40	20·20
9 30	20·11	20·00	20·17	20·65	21·08	20·86	20·31	20·03	20·23	21·08	20·94	22·99	20·52
10 0	20·48	20·21	20·48	21·18	21·46	21·16	20·64	20·49	20·90	21·45	21·45	21·32	20·94
10 30	21·16	20·87	20·88	21·69	21·95	21·46	21·02	20·95	21·31	22·05	22·64	21·55	21·42
11 0	21·45	20·96	21·45	22·26	22·20	21·78	21·40	21·34	21·85	22·26	22·34	21·65	21·74
11 30	21·64	20·94	22·00	22·59	22·46	21·81	21·72	21·76	22·31	22·62	22·57	22·02	21·98

Table XXV.

Showing the Time and the Height of High Water at the London Docks, corresponding to the Mean Time of the Moon's Transit for every minute of Horizontal Parallax; from 5413 Observations made between the 1st of January 1808 and 31st of December 1826, between opposition and conjunction.

Hor. Par. 54′.				Hor. Par. 55′.				Hor. Par. 56′.				Hor. Par. 57′.			
Moon's Transit. A.M.	High Water.	Height of Tide.	No. of Obs.	Moon's Transit. A.M.	High Water.	Height of Tide.	No. of Obs.	Moon's Transit. A.M.	High Water.	Height of Tide.	No. of Obs.	Moon's Transit. A.M.	High Water.	Height of Tide.	No. of Obs.
h m	h m	ft. in.		h m	h m	ft. in.		h m	h m	ft. in.		h m	h m	ft. in.	
0 28·1	2 31·9	22 2·9	84	0 31·5	2 32	22 2·5	57	0 29·5	2 24·5	22 1·7	47	0 28·6	2 20·8	22 8·5	37
1 30·9	3 13·8	22 3·8	84	1 33·3	3 15·3	22 3·2	61	1 31·1	3 9·7	22 9·4	43	1 33·6	3 5·6	22 9·9	41
2 31·8	3 5·6	21 11·5	82	2 34·4	3 55·5	22 1·9	60	2 35·9	3 55·6	22 4·1	51	2 34·6	3 51·3	22 7·3	38
3 32·7	4 40·7	21 8	79	3 34·2	4 38·1	21 9·9	65	3 32·8	4 38	22 1·3	54	3 30·9	4 36	22 5·6	45
4 37	5 26·3	20 11·1	73	4 32·3	5 49	21 0·6	75	4 32·9	5 25·3	21 5·2	50	4 32·2	5 22·6	21 6·3	55
5 34·5	6 16·7	20 0	66	5 31	6 16·5	20 4·1	80	5 31	6 14·4	21 0·5	60	5 31·5	6 14·4	21 0·8	62
6 32·4	7 21·2	19 0·3	66	6 31·3	7 16·9	19 6·5	88	6 30·9	7 17·3	19 7·7	55	6 31·1	7 15·8	19 11·6	58
7 31	8 44·1	18 8·9	73	7 30·4	8 42·6	18 11·5	78	7 29·7	8 41·6	19 0·7	56	7 29·3	8 32·3	19 5	48
8 30·3	10 26·6	19 4·9	79	8 29·8	10 21·1	19 4·5	69	8 30·9	10 16·3	19 6·7	56	8 32·2	10 15·7	19 5·9	47
9 32·1	11 54·1	20 2·9	84	9 32·3	11 50·9	20 2	65	9 31·9	11 45·2	20 4·7	49	9 30·2	11 36·7	20 8·2	40
10 32·1	12 56·4	21 0·6	86	10 29·2	12 51·3	21 0·7	58	10 31·9	12 46·8	21 2·6	48	10 32·8	12 45·2	21 4·3	39
11 31·1	13 46·9	21 7·4	86	11 29·5	13 40	21 10	56	11 31	13 39·7	21 11	43	11 31·5	13 36	22 0	39

Table XXV. (Continued).

Hor. Par. 58′.				Hor. Par. 59′.				Hor. Par. 60′.				Hor. Par. 61′.			
Moon's Transit. A.M.	High Water.	Height of Tide.	No. of Obs.	Moon's Transit. A.M.	High Water.	Height of Tide.	No. of Obs.	Moon's Transit. A.M.	High Water.	Height of Tide.	No. of Obs.	Moon's Transit. A.M.	High Water.	Height of Tide.	No. of Obs.
h m	h m	ft. in.		h m	h m	ft. in.		h m	h m	ft. in.		h m	h m	ft. in.	
32·4	2 20	22 6·9	35	0 32	2 15	22 4·8	42	0 32·2	2 13·2	22 11·3	49	0 30·4	2 5·4	23 2·7	102
1 28·2	2 59·5	23 1·1	37	1 33·8	3 1·6	23 4·8	43	1 33·6	2 58·6	23 4·2	51	1 31·3	2 53·1	23 4·7	92
2 28·3	3 49·7	23 0·9	41	2 35·1	3 44·4	23 3·7	48	2 34·6	3 47·1	23 3·2	79	2 25·7	3 34·5	23 6·4	51
3 34·5	4 33·8	22 6·6	47	3 30·9	4 28·3	23 0	63	3 29·8	4 25·8	23 2·3	97				
4 29·8	5 20·3	21 11·7	57	4 33	5 20·3	22 5	103	4 26	5 10·9	22 8·1	38				
5 30·8	6 12	20 11·7	62	5 28·6	6 7·9	21 7·6	121	5 15·3	5 22·3	21 10·3	3				
6 31·8	7 12·4	20 2·4	64	6 30·8	7 10·8	20 5·5	121								
7 29·5	8 30·5	19 8	64	7 27·8	8 22·1	19 9·1	103	7 41·2	8 42·8	19 11·7	25				
8 25·8	10 2·7	19 10·5	54	8 25·6	9 54·7	19 10·6	67	8 35·8	10 5·4	20 0·5	77				
9 31·3	11 32·2	20 10	48	9 30·5	11 23·7	21 0·5	53	9 27·2	11 15·6	21 1·4	84	9 43·9	11 36·1	21 0·8	31
10 32·4	12 40·2	21 5·6	44	10 30·7	12 31·9	21 8·3	40	10 31·3	12 26·8	21 9·2	57	10 32·9	12 27·3	21 10	79
11 31·2	13 30·9	22 4·7	43	11 29·1	13 26·4	22 6·5	39	11 28·1	13 18·7	22 4·9	46	11 31·2	13 18·5	22 5·7	98

Table XXVI. (Interpolated from Table XXV.)

Showing the Interval between the Moon's Transit, A.M., and the Time of High Water at the London Docks, for every minute of her Horizontal Parallax between opposition and conjunction.

Moon's Transit. A.M.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	h m	h m	h m	h m	h m	h m	h m	h m
0 0	2 10	2 6	2 3	1 58·8	1 55	1 51	1 46·7	1 41·5
0 30	2 3·2	2 0·7	1 54·9	1 51·8	1 48·2	1 43·5	1 41·5	1 35
1 0	1 53	1 51·8	1 46·9	1 41·8	1 39·5	1 36·2	1 33·6	1 28·5
1 30	1 42·8	1 43	1 38·9	1 31·8	1 30·9	1 28·9	1 26	1 22
2 0	1 33·9	1 32·8	1 30·6	1 24·5	1 25·9	1 19·7	1 19·8	1 16
2 30	1 25	1 22·6	1 21·4	1 17·7	1 20·9	1 10·6	1 13·7	1 10
3 0	1 16·8	1 13·9	1 13·5	1 11·5	1 10·7	1 4·1	1 4·8	
3 30	1 8·7	1 5·1	1 5·9	1 5·3	1 0·5	0 57·6	0 56	
4 0	1 0·5	0 59·7	0 59·4	0 58	0 55·4	0 52·8	0 50	
4 30	0 52·3	0 54·4	0 53	0 50·8	0 50·3	0 48	0 44	
5 0	0 46·3	0 49·6	0 48	0 46·5	0 45·8	0 43·6		
5 30	0 42·8	0 45·7	0 43·6	0 43	0 41·3	0 39·2		
6 0	0 44	0 43	0 42	0 41	0 39	0 37·8		
6 30	0 51	0 45·6	0 46·3	0 44·5	0 40·3	0 39·9		
7 0	1 0	0 55·6	0 56·8	0 52·9	0 48	0 46		
7 30	1 13·5	1 12·3	1 12	1 3·3	1 1·2	0 55·2		
8 0	1 34	1 31	1 28	1 22	1 20	1 11·5		
8 30	1 56·3	1 51·3	1 44·9	1 42·4	1 38·8	1 31·2	1 27	
9 0	2 12·6	2 7·5	2 1·5	1 57	1 52	1 44·5	1 40·5	
9 30	2 21·2	2 17·6	2 12·4	2 6·5	2 0·7	1 53	1 49	
10 0	2 24·6	2 22·5	2 16	2 12	2 6	1 59	1 53·5	1 54
10 30	2 24·2	2 22·2	2 14·8	2 12·5	2 7·8	2 1·2	1 55·5	1 54·8
11 0	2 21	2 17·5	2 13	2 10	2 5·5	2 1	1 54	1 52
11 30	2 15·9	2 10·4	2 8·6	2 4·8	2 0	1 57·2	1 50·5	1 47

Table XXVII. (Interpolated from Table XXV.)

Showing the Height of High Water at the London Docks for every minute of the Moon's Horizontal Parallax between opposition and conjunction.

Moon's Transit. A.M.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 0	21·86	22·05	22·06	22·39	22·48	22·48	22·67	22·85
0 30	22·30	22·28	22·14	22·70	22·56	22·39	22·93	23·22
1 0	22·40	22·26	22·48	22·76	22·83	22·85	23·14	23·25
1 30	22·30	22·24	22·78	22·81	23·08	23·33	23·33	23·47
2 0	22·20	22·22	22·58	22·73	23·07	23·35	23·31	23·47
2 30	21·96	22·19	22·37	22·62	23·05	23·30	23·25	23·54
3 0	21·84	22·04	22·24	22·54	22·81	23·15	23·22	
3 30	21·67	21·86	22·11	22·46	22·58	23·01	23·18	
4 0	21·36	21·44	21·81	22·02	22·24	22·72	22·92	
4 30	21·03	21·06	21·47	21·56	21·96	22·44	21·64	
5 0	20·56	20·73	21·24	21·30	21·48	21·96		
5 30	20·08	20·35	21·04	21·06	20·98	21·61		
6 0	19·58	19·96	20·36	20·53	20·59	21·04		
6 30	19·10	19·55	19·66	19·99	20·22	20·49		
7 0	18·73	19·18	19·36	19·69	19·95	20·11		
7 30	17·72	18·96	19·08	19·42	19·66	19·76		
8 0	19·00	19·12	19·31	19·45	19·76	19·72		
8 30	19·41	19·38	19·55	19·48	19·92	19·95	20·03	
9 0	19·82	19·73	19·96	20·05	20·37	20·44	20·55	
9 30	20·22	20·14	20·36	20·68	20·82	21·04	21·14	
10 0	20·62	20·60	20·78	21·02	21·13	21·36	21·44	21·31
10 30	21·03	21·06	21·24	21·33	21·45	21·68	21·76	21·79
11 0	21·33	21·44	21·55	21·66	21·91	22·21	22·09	22·13
11 30	21·60	21·83	21·91	21·99	22·37	22·53	22·42	22·55

Table XXVIII.

Showing the Time and Height of High Water at the London Docks, corresponding to the mean time of the Moon's Transit for every three degrees of her declination north and south; from 5424 observations, made between the 1st of January 1808 and the 31st of December 1826, between opposition and conjunction.

1° 30′ S. to 1° 30′ N. Decl.			
Moon's Transit.	High Water.	Height of Tide.	No. of Obs.
h m	h m	ft. in.	
0 35·5	2 29	22 7·6	19
1 38·1	3 16·2	22 11·8	20
2 41·3	4 2·6	22 6·3	21
3 36·3	4 45·8	22 6·1	19
4 36·9	5 40	21 4·2	18
5 33·8	6 31·3	20 10·3	16
6 31·8	7 31·7	19 7·4	14
7 23·3	8 45·9	19 9·4	16
8 22·1	10 16·6	20 0·6	18
9 28·8	11 35	20 8·3	17
10 33·2	12 52·7	21 10·4	20
11 30·8	13 41	22 1	20

Table XXVIII. (Continued).

1° 30′ to 4° 30′ North Decl.				4° 30′ to 7° 30′ North Decl.				7° 30′ to 10° 30′ North Decl.			
Moon's Transit.	High Water.	Height of Tide.	No. of Obs.	Moon's Transit.	High Water.	Height of Tide.	No. of Obs.	Moon's Transit.	High Water.	Height of Tide.	No. of Obs.
h m	h m	ft. in.		h m	h m	ft. in.		h m	h m	ft. in.	
0 32·4	2 27·6	22 8·7	19	0 32	2 24·7	23 0	22	0 33·9	2 22·3	22 10·6	15
1 34·5	3 11·6	23 0·9	18	1 38	3 12·7	23 3·2	24	1 34·7	3 13·7	22 10·6	13
2 41·6	4 6	22 11	14	2 37·5	4 0·2	22 5·2	21	2 38	4 1·1	22 8·8	22
3 42	4 47·3	22 5·2	17	3 35·7	4 48·7	22 3·7	19	3 38	4 48·1	22 3·7	19
4 38·9	5 37·9	21 10	17	4 29·8	5 27·6	21 11·8	19	4 37·2	5 39·4	21 7·5	19
5 32·1	6 28·5	20 11	17	5 32·3	6 23	20 10·9	16	5 31·4	6 30·2	20 5·6	21
6 27·8	7 32	19 9·9	20	6 31·8	7 28·5	20 6·6	20	6 26·1	7 20·2	19 6·2	20
7 25·8	8 46·3	19 10·9	19	7 29·2	8 45·5	19 5·2	19	7 25	8 48·3	19 9·2	18
8 26·8	10 21	20 2·7	19	8 26·4	10 16·6	20 0	18	8 23·7	10 13·7	19 11·4	20
9 23·4	11 36	20 8·9	20	9 27·1	11 44·2	21 1·1	20	9 23·4	11 42·6	20 8·8	21
10 28·1	12 42·4	21 10·8	21	10 25·6	12 43·7	21 11	21	10 32·4	12 50·3	22 0·7	17
11 28·9	13 43·6	22 3·5	18	11 27	13 37·6	22 4·8	17	11 30	13 39	22 3·7	20

1° 30′ to 4° 30′ South Decl.				4° 30′ to 7° 30′ South Decl.				7° 30′ to 10° 30′ South Decl.			
0 30·5	2 27·8	22 8·1	18	0 33·2	2 27·8	22 10·4	21	0 34·8	2 32·5	22 8	22
1 35	3 12·4	22 11·7	19	1 37·6	3 17·5	23 2·8	16	1 36·9	3 14·2	22 10·8	19
2 39·2	4 1·8	23 0·6	19	2 45·3	4 6·5	22 10·9	19	2 36·4	4 1·4	22 10·4	24
3 35·5	4 45·5	22 8·3	20	3 44·2	4 52·5	22 8·5	18	3 34·3	4 43·3	22 5·5	18
4 41·7	5 41·3	21 5·8	15	4 33·8	5 37·5	21 4·8	16	4 42·5	5 40·8	21 9·2	18
5 36·3	6 31·7	20 11·5	17	5 34·3	6 29·4	20 9·8	18	4 35	6 28·2	20 10·1	17
6 23·2	7 20·8	20 3·8	17	6 31·9	7 31·9	20 3	21	6 28·3	7 24·5	20 4·6	19
7 27·8	8 45·5	19 10·3	18	7 21·6	8 35	19 3·1	18	7 28	8 49·1	19 6·7	18
8 29·9	10 20·3	20 5·4	17	8 25·3	10 14·5	19 11·6	20	8 33·1	10 24·5	19 7·4	20
9 28·2	11 43·5	20 10·9	17	9 25·1	11 39·7	20 9·7	19	9 31·2	11 41·5	20 10·1	20
10 27·5	12 46·5	21 8·4	17	10 33·3	12 49	21 6·8	18	10 23·5	12 46·7	21 8	21
11 28·1	13 40·8	22 5·3	19	11 32·7	13 44·3	22 4	16	11 30	13 40·4	22 3·4	22

10° 30′ to 13° 30′ North Decl.				13° 30′ to 16° 30′ North Decl.				16° 30′ to 19° 30′ North Decl.			
h m	h m	ft. in.		h m	h m	ft. in.		h m	h m	ft. in.	
0 28·6	2 23·8	22 3·3	22	0 27·4	2 20	22 7·1	28	0 26·4	2 17·6	22 5·6	36
1 33·8	3 8	23 2·7	25	1 26·9	3 0·1	22 9·8	28	1 28·2	3 5·8	22 8·6	37
2 35·4	3 54·4	22 10	18	2 28·4	3 49	22 1·9	27	2 24	3 41·5	22 7·3	38
3 36	4 41·5	22 7·1	20	3 30·1	4 29·4	22 6·4	26	3 28	4 29·5	22 5·4	42
4 31·2	5 32·5	21 6	24	4 30·5	5 22·3	21 8·9	26	4 25·3	5 18·7	21 7·2	34
5 28·6	6 18·2	21 1·8	22	5 28·3	6 14·2	20 8·7	26	5 25·1	6 5	21 0	38
6 29·5	7 16·1	19 11·6	17	6 31·7	7 21·9	19 7·3	26	6 28·9	7 12·6	19 9·2	40
7 24	8 35·2	19 4·5	21	7 24·2	8 38·2	19 9·9	25	7 24·8	8 29·7	19 6	38
8 27·7	10 18·1	19 10	23	8 26·8	10 15·9	19 7·7	28	8 31·2	10 9·8	19 8·5	37
9 24·1	11 35·2	20 10·6	22	9 31·9	11 38·7	20 7·2	24	9 31	11 37·1	20 5·8	37
10 29·2	12 47·3	21 7·1	26	10 31·7	12 47	21 4·4	27	10 31·4	12 42·5	21 5·1	34
11 24·7	13 31·6	22 4·6	25	11 31·6	13 35	22 1·6	28	11 32·3	13 27·8	22 0·1	38

10° 30′ to 13° 30′ South Decl.				13° 30′ to 16° 30′ South Decl.				16° 30′ to 19° 30′ South Decl.			
0 33	2 27·4	22 9·2	25	0 32	2 15·8	22 9·6	21	0 32·7	2 16·3	22 7·5	34
1 30·5	3 10·4	22 11·1	23	1 33·2	3 7·2	23 2·2	29	1 33·2	3 2·8	23 1·9	29
2 35·4	3 53·7	22 6·4	24	2 37·5	3 55	22 10	24	2 31·3	3 46·7	22 11·6	37
3 37·3	4 41·8	22 4·8	25	3 41·6	4 43	22 6·7	20	3 31·4	4 31	22 6·8	43
4 38	5 28·8	21 7·2	27	4 37·7	5 24·8	21 9·8	26	4 34·4	5 19·6	21 9·8	39
5 35·5	6 25·7	21 1·2	20	5 35·4	6 15·3	21 2·3	33	5 35·6	6 14·1	20 10·5	36
6 34·4	7 31·6	20 0·1	25	6 35·6	7 22·9	19 8	24	6 38·1	7 19·3	20 0·5	39
7 31·2	8 41·8	19 1·9	21	7 30·3	8 35·5	19 5·2	26	7 37·7	8 40·2	19 2·1	40
8 29·7	10 14·1	20 0·6	22	8 29·9	10 10·9	19 7·1	22	8 38·8	10 10·6	19 7·8	40
9 32·9	11 39·3	20 4·5	22	9 31·4	11 32·5	20 9·5	32	9 36·5	11 36·1	20 4·7	26
10 31	12 41·4	21 10·9	22	10 32·2	12 41·3	21 10·1	24	10 33·6	12 37·1	21 5·3	38
11 29	13 30·6	22 6	24	11 29·8	13 27·7	21 10·9	27	11 31·1	13 27·8	22 0·1	37

Table XXVIII. (Continued).

19° 30′ to 22° 30′ North Decl.				22° 30′ to 25° 30′ North Decl.				Above 25° 30′ North Decl.			
Moon's Transit.	High Water.	Height of Tide.	No. of Obs.	Moon's Transit.	High Water.	Height of Tide.	No. of Obs.	Moon's Transit.	High Water.	Height of Tide.	No. of Obs.
h m	h m	ft. in.		h m	h m	ft. in.		h m	h m	ft. in.	
0 26·2	2 10·4	22 6·5	28	0 28·9	2 7·9	22 1	31	0 25·2	2 6·8	22 3·4	14
1 26	2 49	23 1·3	31	1 24·3	2 52·3	22 7·1	17	1 23·1	2 50·2	22 3	21
2 24·5	3 35	22 7·6	27	2 22·7	3 35·5	22 5·6	26	2 23·9	3 29·2	22 6	14
3 26·1	4 23	22 0·9	25	3 27·1	4 16	22 4·1	20	3 16·4	4 7·9	22 3·4	24
4 23·7	5 9·8	21 10·7	34	4 23·6	5 1·2	21 6	24	4 13·2	4 49·5	21 5·5	21
5 23·2	5 56·2	20 10·1	36	5 28·7	5 58·6	20 5·3	19	5 22	5 44·5	20 7	23
6 30·7	7 3·9	20 0	27	6 21·8	6 42·3	19 3·3	51	6 24·5	6 52·4	19 8	25
7 27·8	8 19·8	18 10·2	31	7 38·2	8 26·7	18 11·1	23	7 28·5	8 14·8	19 0	23
8 33·1	10 11·5	19 6·7	29	8 30·5	10 31	19 4·5	24	8 29·7	9 55·9	19 4·3	22
9 29·2	11 34·2	20 10·7	31	9 36·1	11 33·2	20 1·8	23	9 34·1	11 24·8	19 11·5	21
10 35·1	12 40·2	21 2·2	30	10 31·2	12 31·6	21 1·1	22	10 33·4	12 32·4	20 10·5	21
11 31	13 31·3	21 10·9	29	11 35·8	13 27	22 0·7	25	11 30·2	13 23·1	21 10·3	18
19° 30′ to 22° 30′ South Decl.				22° 30′ to 25° 30′ South Decl.				Above 25° 30′ South Decl.			
0 30·3	2 19	22 7·5	32	0 34·9	2 19·4	22 3·9	25	0 32·9	2 11	22 4·9	20
1 33·6	3 2	22 11·8	30	1 35·2	3 3·9	22 8·1	19	1 30	2 54·3	22 6·1	22
2 32·7	3 46·4	22 10·9	28	2 33·4	3 44·8	22 11·6	28	2 28·6	3 34·5	22 6·4	20
3 30·9	4 30·3	22 3·1	32	3 32·5	4 27	22 4	22	3 31·7	4 20·9	22 4·5	21
4 39·3	5 25·8	21 11·3	23	4 32·9	5 18·8	21 6·2	25	4 32·6	5 6·7	21 9	23
5 34·1	6 9·8	20 9·7	28	5 34·8	6 9·4	20 10·6	25	5 29·8	5 51·8	20 7·9	24
6 35·3	7 14	19 4·9	26	6 37	7 3·4	19 7·6	23	6 35·2	6 53·2	19 5·1	25
7 40·1	8 43·2	18 11·2	29	7 40·7	8 32	19 1·7	24	7 38·2	8 9·5	19 0·1	21
8 33·6	10 12·5	19 4·8	31	8 37·7	10 6·4	19 2·5	24	8 43·4	16 6·9	18 9·3	18
9 39·4	11 46·4	20 6·9	28	9 37·7	11 31·8	20 1·6	24	9 36	11 24·5	19 11·4	21
10 34·8	12 40·8	21 4·6	33	10 33·7	12 30·8	20 11·1	18	10 37·5	12 30·7	21 1·5	20
11 30·2	13 26·6	21 11·3	27	11 35·2	13 30·4	22 1·6	23	11 30·5	13 19·4	21 11·3	18

Table XXIX. (Interpolated from Table XXVIII.)

Showing the Interval between the Moon's Transit and the Time of High Water at the London Docks, for every three degrees of her declination north or south, between opposition and conjunction.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Mean.
h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m
0 30	1 54·9	1 57	1 54·4	1 58·8	1 55	1 48·2	1 47·3	1 46	1 42·1	1 40·5	1 50·4
1 30	1 40	1 38·7	1 39·2	1 39·6	1 37·6	1 33·6	1 33·6	1 25·7	1 28·3	1 24·2	1 34
2 30	1 24·3	1 25·6	1 24·6	1 25·6	1 20·2	1 19·8	1 15·8	1 11·8	1 11·5	1 4·2	1 18·3
3 30	1 10·8	1 10	1 12·4	1 11	1 6·5	1 2·2	1 0·5	0 57·4	0 51·7	0 48·8	1 3
4 30	1 4	1 0·7	1 1	1 1·2	0 56·6	0 50·2	0 49·3	0 47	0 41·4	0 33·7	52·3
5 30	0 57·9	0 56·2	0 53·2	0 56·2	0 50·1	0 43·2	0 39·2	0 34·5	0 32·4	0 22·3	44·5
6 30	0 59·8	1 1·6	0 58·4	0 55·6	0 51·8	0 48·5	0 41·8	0 35·8	0 23·6	0 23	46
7 30	1 26	1 20·4	1 16·9	1 19	1 12·1	1 10·8	1 3·5	0 55·5	0 45·2	0 38	1 6·7
8 30	1 57·3	1 54	1 51·8	1 51·5	1 48	1 45·7	1 33	1 36·7	1 28·3	1 21·3	1 42·8
9 30	2 7	2 14·5	2 16·3	2 15·3	2 9·1	2 3·3	2 1·8	2 4·1	1 53·5	1 48·3	2 5·3
10 30	2 18·9	2 16·5	2 16·9	2 20·4	2 14·2	2 12	2 7	2 4·7	1 58·7	1 55·5	2 10·5
11 30	2 10	2 12·7	2 11·1	2 9·7	2 3·7	2 0·7	1 56·4	1 58·5	1 54·5	1 50·9	2 3

Table XXX. (Interpolated from Table XXVIII.)

Showing the Height of High Water at the London Docks for every three degrees of the Moon's Declination north and south, between opposition and conjunction.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Mean.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 30	22·58	22·69	22·90	22·78	22·52	22·68	22·54	22·57	22·20	22·31	22·58
1 30	22·94	22·99	23·21	22·87	23·04	23·07	22·94	23·04	22·62	22·38	22·91
2 30	22·61	22·98	22·76	22·80	22·69	22·52	22·77	22·75	22·70	22·51	22·71
3 30	22·51	22·63	22·52	22·42	22·51	22·54	22·50	22·15	22·33	22·30	22·44
4 30	21·50	21·80	21·70	21·80	21·62	21·82	21·70	21·93	21·48	21·50	21·69
5 30	20·90	20·98	20·89	20·70	21·13	21·02	20·93	20·80	20·68	20·56	20·86
6 30	19·65	20·05	20·41	19·94	19·63	19·71	19·96	19·75	19·44	19·54	19·81
7 30	19·81	19·90	19·40	19·67	19·55	19·62	19·34	18·94	19·06	19·03	19·43
8 30	20·14	20·34	20·03	19·80	19·96	19·64	19·64	19·45	19·27	19·08	19·74
9 30	20·71	20·88	20·99	20·84	20·65	20·67	20·38	20·64	20·04	19·88	20·57
10 0	21·81	21·81	21·73	21·88	21·75	21·58	21·40	21·25	20·97	20·91	21·51
11 30	22·07	22·37	22·36	22·29	22·44	22·00	22.00	21·92	22·09	21·90	22·14

Table XXXI.

Showing the Difference in the Interval between the Time of the Moon's Transit and the Time of High Water, and the Mean Interval (A.M. part of Table II.) for every minute of the Moon's Horizontal Parallax, between opposition and conjunction.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	m	m	m	m	m	m	m	m
0 30	+ 3	+11	+ 5	+ 2	− 2	− 6	− 8	−15
1 30	+ 8	+ 8	+ 4	+ 3	− 4	− 6	− 9	−13
2 30	+ 7	+ 5	+ 3	0	+ 3	− 7	− 4	− 8
3 30	+ 7	+ 3	+ 4	+ 3	− 1	− 4	− 6	
4 30	0	+ 2	+ 1	− 1	− 2	− 4	− 8	
5 30	+ 1	+ 4	+ 2	+ 1	− 1	− 3		
6 30	+ 7	+ 2	+ 2	+ 1	− 3	− 4		
7 30	+ 9	+ 7	+ 7	− 2	− 4	−10		
8 30	+16	+11	+ 5	+ 2	− 1	− 9	−13	
9 30	+16	+13	+ 7	+ 2	− 4	−12	−16	
10 30	+14	+12	+ 5	+ 3	− 2	− 9	−14	−15
11 30	+12	+ 6	+ 5	+ 1	− 4	− 7	−13	−17

Table XXXII.

Showing the Difference in the Height of High Water, and the Mean Height (Column B. Table XXIII.) for every minute of the Moon's Horizontal Parallax, and between opposition and conjunction.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 30	−·23	−·25	−·39	+·17	+·03		+1·07	+·69
1 30	−·56	−·62	−·08	−·05	+·22	+·47	+ ·61	+·61
2 30	−·74	−·53	−·35	−·10	+·33	+·58	+ ·53	+·82
3 30	−·69	−·50	−·25	+·10	+·22	+·65	+ ·82	
4 30	−·66	−·63	−·22	−·13	+·27	+·75		
5 30	−·75	−·48	+·21	+·23	+·15	+·78		
6 30	−·80	−·35	−·24	+·09	+·32	+·59		
7 30	−·61	−·37	−·25	+·09	+·33	+·43		
8 30	−·23	−·26	−·09	−·16	+·28	+·31	+ ·39	
9 30	−·29	−·37	−·15	+·17	+·31	+·53	+ ·63	
10 30	−·37	−·34	−·16	−·07	+·05	+·28	+ ·36	
11 30	−·37	−·14	−·06	+·02	+·40	+·56	+ ·45	

Table XXXIII.

Showing the Difference in the Interval between the Time of Moon's Transit and the Time of High Water, and the Mean Interval (a.m. part of Table II.) for every three degrees of the Moon's Declination, between opposition and conjunction.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.
h m	m	m	m	m	m	m	m	m	m	m
0 30	+ 5	+ 7	+ 4	+ 9	+ 5	− 2	− 3	− 4	− 8	− 9
1 30	+ 5	+ 4	+ 4	+ 5	+ 3	− 1	− 1	− 9	− 7	−11
2 30	+ 6	+ 8	+ 7	+ 8	+ 2	+ 2	− 2	− 6	− 6	−14
3 30	+ 9	+ 8	+10	+ 9	+ 5	0	− 1	− 5	−10	−13
4 30	+12	+ 9	+ 9	+ 9	+ 5	− 2	− 3	− 5	−11	−18
5 30	+16	+14	+11	+14	+ 8	+ 1	− 3	− 7	−10	−20
6 30	+16	+18	+14	+12	+ 8	+ 5	− 2	− 8	−20	−21
7 30	+21	+15	+12	+14	+ 7	+ 6	− 1	− 9	−20	−27
8 30	+17	+14	+12	+12	+ 8	+ 8	− 7	− 3	−12	−19
9 30	+ 2	+10	+11	+10	+ 4	− 2	− 3	− 1	−11	−17
10 30	+ 9	+ 7	+ 7	+10	+ 4	+ 2	− 3	− 5	−11	−14
11 30	+ 6	+ 9	+ 7	+ 6	0	− 3	− 8	− 5	− 9	−13

Table XXXIV.

Showing the Difference in the Height of High Water, and the Mean Height (Column B. Table XXIII.) for every three degrees of the Moon's Declination, between opposition and conjunction.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 30	+·03	+·16	+·37	+·25	−·01	+·15	+·01	+·04	−·33	−·22
1 30	+·08	+·13	+·35	+·01	+·18	+·21	+·08	+·18	−·24	−·48
2 30	−·11	+·26	+·04	+·08	−·03	−·20	+·05	+·03	−·02	−·21
3 30	+·15	+·27	+·16	+·06	+·15	+·18	+·14	−·21	−·03	−·06
4 30	−·19	+·12	+·01	+·11	−·07	+·13	+·01	+·24	−·21	−·19
5 30	+·07	+·15	+·06	−·13	+·30	+·19	+·10	−·03	−·15	−·27
6 30	−·25	+·15	+·51	+·04	−·27	−·19	+·06	−·15	−·46	−·36
7 30	+·48	+·57	+·07	+·34	+·22	+·20	+·01	−·39	−·27	−·30
8 30	+·50	+·70	+·39	+·16	+·32	0	0	−·19	−·37	−·58
9 30	+·20	+·37	+·48	+·33	+·14	+·16	−·13	+·13	−·47	−·63
10 30	+·41	+·41	+·33	+·48	+·35	+·18	0	−·15	−·43	−·49
11 30	+·10	+·40	+·39	+·32	+·47	+·03	+·03	−·05	+·12	−·07

Table XXXV. (Mean of Tables VII. and XXVI.)

Showing the Interval between the Moon's Transit and the Time of High Water at the London Docks for every minute of her Horizontal Parallax; from 10,812 observations, made between the 1st of January 1808 and the 31st of December 1826.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	h m	h m	h m	h m	h m	h m	h m	h m
0 0	2 9·7	2 5·8	2 1·5	1 57·6	1 54·6	1 52	1 45	1 42
0 30	2 2·5	1 59·9	1 53·2	1 51·9	1 47·6	1 44·2	1 41·3	1 36·8
1 0	1 52·8	1 50·7	1 46	1 44·5	1 40·1	1 37·6	1 33·5	1 30
1 30	1 43·2	1 41·3	1 38·2	1 35·9	1 32·5	1 30·7	1 25·7	1 23·2
2 0	1 34	1 32·5	1 29·6	1 27	1 26·5	1 22·3	1 19·2	1 16·5
2 30	1 25	1 22·8	1 20·5	1 18·3	1 20·3	1 13·3	1 13	1 10·3
3 0	1 16·9	1 14·3	1 12·9	1 11	1 11·6	1 6·2	1 5	
3 30	1 8·8	1 6	1 6	1 4·6	1 3·4	0 59	0 56·7	
4 0	1 1·1	0 59·5	0 59	0 56·8	0 56·7	0 53·4	0 50	
4 30	0 51·5	0 53·4	0 53·6	0 50·2	0 50·2	0 48·3	0 43·3	
5 0	0 46·3	0 48·1	0 47·8	0 46·2	0 45·4	0 43·8		
5 30	0 43·4	0 44·2	0 42·8	0 43·7	0 41·3	0 40·5		
6 0	0 43·5	0 43·2	0 40·7	0 42·2	0 39·4	0 39·1		
6 30	0 48·6	0 46·7	0 43·6	0 44	0 40·6	0 40·5		
7 0	0 58·1	0 54·6	0 52·9	0 51·2	0 47·5	0 46·5		
7 30	1 14	1 7·7	1 7·6	1 2·4	0 59·6	0 57		
8 0	1 36·1	1 26·5	1 25	1 20·4	1 17·4	1 14	1 11·6	
8 30	1 58·4	1 49·1	1 43·8	1 41·4	1 35·4	1 31·6	1 27·3	
9 0	2 13·6	2 6·1	2 0	1 56·8	1 49·2	1 44·8	1 39·8	
9 30	2 21·7	2 16·6	2 12·4	2 6·4	1 58·9	1 53·2	1 48·2	
10 0	2 25·3	2 22	2 16·7	2 11	2 3·4	1 58·5	1 53·3	
10 30	2 24·8	22·7	2 16·5	2 11	2 7·8	2 0·6	1 55·7	1 54
11 0	2 21·4	2 18·7	1 14·2	2 8·1	2 5·7	2 0·7	1 54·1	1 51
11 30	2 16	2 11·6	2 9	2 3·3	2 0·4	1 58	1 48·7	1 46·9

Table XXXVI.

Showing the Time and Height of High Water at the London Docks, corresponding to the mean time of Moon's Transit for every minute of her Horizontal Parallax; from 10,812 observations, made between the 1st of January 1808 and 31st of December 1826.

Hor. Par. 54′.			Hor. Par. 55′.			Hor. Par. 56′.			Hor. Par. 57′.		
Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.
h m	Feet.		h m	Feet.		h m	Feet.		h m	Feet.	
0 31·8	22·15	179	0 31·5	22·24	108	0 31·5	22·24	85	0 31·9	22·67	80
1 25·2	22·27	173	1 27·7	22·32	115	1 26·4	22·80	93	1 30	22·76	79
2 33·5	22·04	162	2 36	22·23	131	2 34·2	22·38	89	2 34	22·73	83
3 27·5	21·69	152	3 30	21·97	130	3 27·8	22·15	110	3 25·8	22·45	91
4 33·2	21·00	147	4 32	21·11	149	4 33·4	21·42	100	4 32·6	21·66	114
5 34·5	20·09	121	5 32	20·30	170	5 31	20·73	118	5 32·7	21·05	119
6 33·6	19·15	134	6 34·3	19·54	159	6 34	19·67	121	6 32·4	19·91	115
7 28·4	18·73	145	7 28	19·00	151	7 28	19·12	114	7 26·9	19·45	104
8 32·4	19·36	165	8 33·3	19·50	123	8 33·1	19·63	121	8 33	19·67	91
9 35·3	20·21	166	9 35·9	20·31	131	9 32·5	20·43	91	9 33	20·75	84
10 35	21·14	170	10 34·2	21·06	114	10 33·8	21·28	98	10 34	21·38	79
11 33·8	21·64	169	11 32·6	21·85	116	11 32·6	21·87	78	11 35	22·19	82

Hor. Par. 58′.			Hor. Par. 59′.			Hor. Par. 60′.			Hor. Par. 61′.		
Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.	Moon's Transit.	Height of Tide.	No. of Obs.
h m	Feet.		h m	Feet.		h m	Feet.		h m	Feet.	
0 34·9	22·77	68	0 34·5	22·66	75	0 32	22·93	101	0 31	23·18	194
1 27·7	23·08	74	1 33·1	23·13	85	1 32	23·25	117	1 29·3	23·41	170
2 30·6	22·93	91	2 32·6	23·13	96	2 34·3	23·35	151	2 25·6	23·55	94
3 29·3	22·50	93	3 27·4	22·93	136	3 29	23·24	187			
4 31	22·09	112	4 32·7	22·36	217	4 25·1	22·58	65			
5 31·3	20·94	136	5 30	21·60	240						
6 31·2	20·23	121	6 32·4	20·45	242						
7 29	19·65	128	7 29·1	19·85	191	7 38·3	20·03	65			
8 30·7	19·75	103	8 28·6	20·04	123	8 35·2	20·18	176			
9 35·5	20·73	94	9 33	20·78	104	9 31·2	21·09	182	9 40·8	21·21	53
10 32·8	21·57	87	10 34	21·74	82	10 32·6	21·85	120	10 32·6	21·85	152
11 35	22·31	82	11 30·4	22·52	82	11 32·2	22·47	96	11 33	22·61	195

Table XXXVII. (Interpolated from Table XXXVI.)

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 30	22·13	22·23	22·23	22·65	22·73	22·65	22·91	23·17
1 30	22·26	22·32	22·78	22·76	23·07	23·12	23·26	23·41
2 30	22·06	22·24	22·40	22·73	22·92	23·13	23·34	23·54
3 30	21·67	21·97	22·13	22·42	22·50	22·91	23·23	
4 30	20·97	21·14	21·49	21·69	22·10	22·39	22·51	
5 30	20·14	20·33	20·74	21·08	20·96	21·60		
6 30	19·19	19·59	19·72	19·94	20·24	20·48		
7 30	18·74	19·02	19·12	19·46	19·65	19·86	20·04	
8 30	19·33	19·47	19·60	19·64	19·75	20·05	20·17	
9 30	20·14	20·23	20·40	20·71	20·69	20·74	21·07	21·11
10 30	21·12	21·02	21·22	21·33	21·53	21·69	21·82	21·82
11 30	21·61	21·81	21·84	22·12	22·26	22·52	22·45	22·57

TABLE XXXVIII. (Mean of Tables XI. and XXIX.)

Showing the Interval between the Moon's Transit and the Time of High Water at the London Docks for every three degrees of her Declination north or south; from 10,796 observations.

Moon's Transit. A.M.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Mean.
h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m
0 0	2 3·5	2 4	2 3	2 2	1 59	1 56	1 54	1 51	1 48	1 46·5	1 56·7
0 30	1 56	1 56·7	1 56·5	1 55·6	1 52	1 49·3	1 48	1 45·5	1 42·4	1 39·6	1 50·2
1 0	1 48	1 47	1 48·5	1 47	1 45	1 43·5	1 39	1 36	1 35	1 32	1 42·1
1 30	1 40·4	1 36	1 40	1 38·5	1 38·8	1 36·2	1 29	1 27	1 28·3	1 24	1 33·8
2 0	1 32	1 28·5	1 32	1 32	1 30	1 29·5	1 23	1 20	1 21	1 16·5	1 26·5
2 30	1 24	1 22·5	1 24·4	1 26·3	1 20·6	1 19·8	1 16·8	1 12·7	1 12·2	1 7·6	1 18·7
3 0	1 18	1 17	1 17·5	1 18	1 13·5	1 12	1 9	1 5	1 4·5	0 59	1 11·3
3 30	1 12·4	1 12	1 11	1 10·1	1 6·8	1 2·8	1 0·8	0 58·4	0 56	0 50	1 4
4 0	1 7	1 6	1 5	1 4·5	1 2	0 56	0 54	0 51	0 49	0 41	0 57
4 30	1 1·7	1 0·4	1 0	1 0·3	0 58·1	0 50·6	0 47·5	0 45·5	0 40·5	0 34	0 52
5 0	1 0	0 57	0 57	0 57·0	0 54	0 46·5	0 43·5	0 40·5	0 35	0 29	0 48
5 30	0 59·4	0 56	0 54·6	0 55·7	0 49·6	0 43·3	0 39·8	0 36·1	0 32	0 25·1	0 45·4
6 0	1 0	0 57·5	0 55	0 56	0 50·5	0 44	0 39	0 34·5	0 28	0 22	0 44·7
6 30	1 3·5	1 2·7	0 58	0 57·1	0 52·7	0 46·9	0 40·2	0 35·3	0 25·7	0 23	0 46·5
7 0	1 11	1 11	1 7	1 5	1 1	0 54	0 48	0 43·5	0 34	0 26	0 54
7 30	1 23	1 21·8	1 19·5	1 16	1 14	1 4·4	1 0·4	0 56·9	0 47	0 35·3	1 5·8
8 0	1 40	1 39	1 36	1 32	1 30	1 22	1 16	1 14	1 3	0 54	1 22·6
8 30	1 56·4	1 54	1 52·7	1 51·2	1 46·5	1 44·2	1 34·4	1 36	1 22·9	1 18	1 41·3
9 0	2 4·2	2 6	2 7	2 6	2 0	1 55	1 50	1 50	1 42	1 36	1 55·6
9 30	2 8	2 13·6	2 16·2	2 13·4	2 10·5	2 2·7	2 1	2 3	1 55	1 48	2 5·1
10 0	2 14·5	2 18	2 16	2 18	2 15	2 8	2 6	2 6	2 1·5	1 54	2 9·7
10 30	2 19·8	2 18·3	2 15	2 19·2	2 14·6	2 11·3	2 5·9	2 5·6	2 2·4	1 56·4	2 10·8
11 0	2 16	2 16	2 13	2 15	2 11	2 8	2 4	2 2·5	1 58·5	1 55	2 7·9
11 30	2 11	2 10·4	2 9·3	2 8·5	2 5	2 2·6	1 59·7	1 56·8	1 52·6	1 51·2	2 2·7

TABLE XXXIX. (Mean of Tables IX. and XXX.)

Showing the Height of High Water at the London Docks for every three degrees of the Moon's Declination north or south; from 10,796 observations.

Moon's Transit. A.M.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Mean.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 0	22·42	22·44	22·58	22·49	22·61	22·36	22·32	22·28	22·13	22·09	22·37
0 30	22·68	22·47	22·80	22·64	22·64	22·71	22·57	22·55	22·29	22·39	22·57
1 0	22·85	22·62	22·94	22·76	22·77	22·86	22·75	22·77	22·49	22·39	22·66
1 30	22·99	22·80	23·09	22·85	22·92	22·92	22·93	22·99	22·68	22·38	22·85
2 0	22·90	22·84	23·00	22·87	22·95	22·78	23·20	22·83	22·72	22·52	22·86
2 30	22·81	22·83	22·85	22·91	22·93	22·63	23·44	22·67	22·64	22·64	22·83
3 0	22·70	22·72	22·68	22·67	22·78	22·58	22·91	22·50	22·37	22·45	22·64
3 30	22·58	22·56	22·49	22·37	22·56	22·54	22·38	22·32	22·10	22·27	22·42
4 0	22·12	22·26	22·18	22·10	22·10	22·17	22·08	22·08	21·80	22·04	22·09
4 30	21·67	21·95	21·85	21·82	21·63	21·81	21·77	21·84	21·50	21·81	21·76
5 0	21·32	21·48	21·50	21·36	21·31	21·39	21·35	21·27	21·14	21·20	21·33
5 30	21·00	20·95	21·14	20·81	21·02	20·96	20·92	20·69	20·78	20·60	20·89
6 0	20·37	20·56	20·76	20·37	20·59	20·42	20·42	20·30	20·14	20·05	20·40
6 30	19·77	20·16	20·34	20·11	20·12	19·86	19·93	19·89	19·52	19·50	19·92
7 0	19·82	19·99	19·88	19·97	19·69	19·61	19·62	19·39	19·32	19·17	19·65
7 30	19·87	19·91	19·58	19·84	19·42	19·45	19·32	18·89	19·13	18·85	19·43
8 0	19·91	20·12	19·83	19·68	19·75	19·64	19·47	19·18	19·14	18·87	19·56
8 30	20·00	20·33	20·08	19·53	20·08	19·83	19·64	19·52	19·29	19·07	19·74
9 0	20·45	20·66	20·47	20·17	20·43	20·19	20·04	20·0	19·69	19·50	20·16
9 30	20·93	20·99	20·87	20·83	20·78	20·60	20·43	20·49	20·11	19·93	20·60
10 0	21·36	21·45	21·36	21·36	21·25	21·05	20·88	20·84	20·61	20·42	21·06
10 30	21·79	21·89	21·84	21·88	21·70	21·50	21·33	21·19	21·12	20·91	21·51
11 0	21·99	22·16	22·10	22·12	22·13	21·78	21·69	21·60	21·52	21·33	21·94
11 30	22·16	22·99	22·37	22·32	22·56	22·03	22·06	22·00	21·90	21·76	22·21

Table XL.

Showing the Difference in the Interval between the Time of the Moon's Transit and the Time of High Water, and the Mean Interval (Column A. Table III.) for every three degrees of her Declination.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.
h m	m	m	m	m	m	m	m	m	m	m
0 0	+ 7	+ 7	+ 6	+ 5	+ 2	− 1	− 3	− 6	− 9	−11
0 30	+ 6	+ 7	+ 6	+ 5	+ 2	− 1	− 2	− 4	− 8	−10
1 0	+ 6	+ 5	+ 6	+ 5	+ 3	+ 1	− 3	− 6	− 7	−10
1 30	+ 5	+ 1	+ 5	+ 4	+ 4	+ 1	− 6	− 8	− 7	−11
2 0	+ 6	+ 3	+ 6	+ 6	+ 4	+ 3	− 3	− 6	− 5	−10
2 30	+ 6	+ 5	+ 6	+ 8	+ 3	+ 2	− 1	− 5	− 6	−10
3 0	+ 7	+ 6	+ 7	+ 7	+ 3	+ 1	− 2	− 6	− 6	−12
3 30	+ 9	+ 9	+ 8	+ 7	+ 4	0	− 2	− 5	− 7	−13
4 0	+11	+10	+ 9	+ 8	+ 6	0	− 2	− 5	− 7	−15
4 30	+11	+ 9	+ 9	+ 9	+ 7	0	+ 2	− 5	−10	−17
5 0	+15	+12	+12	+12	+ 9	+ 1	− 1	− 4	−10	−16
5 30	+16	+13	+12	+13	+ 7	0	− 3	− 7	−11	−18
6 0	+18	+16	+13	+14	+ 8	+ 2	− 3	− 7	−14	−20
6 30	+19	+19	+18	+13	+ 9	+ 3	− 4	− 9	−18	−21
7 0	+19	+19	+15	+13	+ 9	+ 2	− 4	− 8	−18	−26
7 30	+18	+17	+15	+11	+ 9	− 1	− 4	− 8	−18	−30
8 0	+17	+16	+13	+ 9	+ 7	− 1	− 7	− 9	−20	−29
8 30	+17	+15	+14	+12	+ 7	+ 5	− 5	− 3	−16	−21
9 0	+ 8	+10	+11	+10	+ 4	− 1	− 6	− 6	−14	−20
9 30	+ 3	+ 9	+11	+ 8	+ 5	− 2	− 4	− 2	−10	−17
10 0	+ 5	+ 8	+ 6	+ 8	+ 5	− 2	− 4	− 4	− 8	−16
10 30	+10	+ 8	+ 5	+ 9	+ 5	+ 1	− 4	− 4	− 8	−14
11 0	+ 8	+ 8	+ 5	+ 7	+ 3	0	− 4	− 5	− 9	−13
11 30	+ 8	+ 7	+ 6	+ 5	+ 2	0	− 3	− 6	−10	−12

Table XLI.

Showing the Difference in the Height of High Water and the Mean Height (Column B. Table XXIV.) for every three degrees of the Moon's Declination.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 0	+·10	+ ·12	+·26	+·17	+·29	+·04	....	−·04	−·19	−·23
0 30	+·10	− ·11	+·22	+·08	+·06	+·13	−·01	−·03	−·29	−·19
1 0	+·14	− ·09	+·23	+·05	+·06	+·15	+·04	+·06	−·22	−·32
1 30	+·18	− ·01	+·28	+·04	+·11	+·11	+·12	+·18	−·13	−·43
2 0	+·08	+ ·02	+·18	+·05	+·13	−·04	+·38	+·01	−·10	−·30
2 30	+·09	+ ·11	+·13	+·19	+·21	−·09	+·72	−·05	−·08	−·08
3 0	+·10	+ ·12	+·08	+·07	+·18	−·02	+·31	−·10	−·23	−·15
3 30	+·20	+ ·18	+·11	−·01	+·18	+·16	....	−·06	−·28	−·11
4 0	+·02	+ ·16	+·08	......	....	+·07	−·02	−·02	−·30	−·06
4 30	−·04	+ ·24	+·14	+·11	−·08	+·10	+·06	+·13	−·21	+·10
5 0	......	+ ·16	+·18	+·04	−·01	+·07	+·03	−·05	−·18	−·12
5 30	+·14	+ ·09	+·28	−·05	+·16	+·10	+·06	−·17	−·08	−·26
6 0	+·02	+ ·21	+·41	+·02	+·24	+·07	+·07	−·05	−·21	−·30
6 30	−·15	+ ·24	+·42	+·19	+·20	−·06	+·01	−·03	−·40	−·42
7 0	+·26	+ ·43	+·32	+·41	+·13	+·05	+·06	−·17	−·24	−·39
7 30	+·50	+ ·54	+·21	+·47	+·05	+·08	−·05	−·48	−·24	−·52
8 0	+·47	+ ·68	+·39	+·24	+·31	+·20	+·03	−·26	−·30	−·57
8 30	+·33	+ ·66	+·41	−·14	+·41	+·16	−·03	−·15	−·38	−·60
9 0	+·25	+ ·46	+·27	−·03	+·23	−·01	−·16	−·20	−·51	−·70
9 30	+·41	+ ·47	+·35	+·31	+·26	+·08	−·09	−·03	−·41	−·59
10 0	+.42	+ ·51	+·42	+·42	+·31	+·11	−·06	−·10	−·33	−·52
10 30	+·37	+ ·47	+·42	+·46	+·28	+·08	−·09	−·23	−·30	−·51
11 0	+·25	+ ·42	+·36	+·38	+·39	+·04	−·05	−·14	−·22	−·41
11 30	+·18	+1·01	+·39	+·34	+·58	+·05	+·08	+·02	−·08	−·22

Table XLII.

Showing the Difference in the Interval between the Time of the Moon's Transit and the Time of High Water, and the Mean Interval (Column A. Table III.) for every minute of her Horizontal Parallax.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	m	m	m	m	m	m	m	m
0 30	+12	+10	+ 3	+ 2	− 2	− 6	− 9	−13
1 30	+ 8	+ 6	+ 3	+ 1	− 2	− 4	− 7	−12
2 30	+ 7	+ 5	+ 3	0	+ 2	− 5	− 5	−18
3 30	+ 6	+ 3	+ 3	+ 2	0	− 4	− 6	
4 30	+ 1	+ 2	+ 3	+ 1	− 1	− 3	− 8	
5 30	0	+ 1	0	+ 1	− 2	− 2		
6 30	+ 5	+ 3	0	0	− 3	− 3		
7 30	+ 9	+ 3	+ 3	− 3	− 5	− 8		
8 30	+19	+10	+ 5	− 2	− 4	− 7	−12	
9 30	+17	+12	+ 7	− 1	− 5	−12	−17	
10 30	+15	+13	+ 7	− 1	− 2	− 9	−14	−16
11 30	+13	+ 9	+ 6	0	− 3	− 5	−14	−16

Table XLIII.

Showing the Difference in the Height of High Water and the Mean Height (Column B. Table XXIV.) for every minute of the Moon's Horizontal Parallax.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	H. P. 61′.
h m	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
0 30	−·45	−·35	−·35	+·07	+·15	+·07	+·33	+·59
1 30	−·55	−·49	−·03	−·05	+·26	+·31	+·45	+·60
2 30	−·66	−·48	−·32	+·01	+·20	+·41	+·62	+·82
3 30	−·71	−·41	−·25	+·04	+·12	+·53	+·85	
4 30	−·74	−·57	−·22	−·02	+·39	+·68	+·80	
5 30	−·72	−·53	−·12	+·22	+·10	+·74		
6 30	−·73	−·33	−·20	+·02	+·32	+·56		
7 30	−·63	−·35	−·25	+·09	+·28	+·49	+·67	
8 30	−·34	−·20	−·07	−·03	+·08	+·38	+·50	
9 30	−·38	−·29	−·12	+·19	+·17	+·22	+·55	+·59
10 30	−·30	−·40	−·20	−·09	+·11	+·27	+·40	+·40
11 30	−·37	−·17	−·14	+·14	+·28	+·54	+·47	+·59

The following Tables may be used in predicting the phenomena of the tides, with the argument of the moon's transit in apparent time.

For the Time of High Water at the London Docks.

Table III., column A., containing the semimenstrual inequality + a constant, which is again inserted here for the convenience of reference.

Table XLIV. containing the correction for the moon's parallax.

Table XLV. containing the correction for the moon's declination.

The last two Tables have been obtained from Tables XL. and XLII., by removing arbitrarily the irregularities which those Tables present.

Table XLVI., formed by arbitrary alterations from Table G., p. 148, gives the correction for the calendar months.

To the result obtained must be added the equation of time, in order to have the time of high water sought in mean time.

Tables XLIII. and XLIV. may probably be safely employed for all the ports in the United Kingdom. It should be borne in mind that these Tables cannot in any case be depended upon to within two or three minutes, from the great irregularities of the phenomena to which they refer, and from the difficulty of ascertaining by observation the precise time of high water. The observations upon which they are founded are only recorded to the nearest five minutes, and they were not always made with so much care as might have been desired.

For the Height of High Water at the London Docks.

Table XXIV., column B., containing the semimenstrual inequality + a constant.

Table XLVII. containing the correction for the moon's parallax.

Table XLVIII. containing the correction for the moon's declination.

The last two Tables have been formed by arbitrary changes from Tables XLIII. and XLII., which Tables present great irregularities; for the height of high water is subject to much greater irregularity than the time. The effect of changes in the moon's parallax upon the height appears to be considerably greater than that of changes in her declination.

Column A. Table III.

Containing the semimenstrual inequality + a constant, and showing the Interval between the Moon's Transit and the Time of High Water, from the Philosophical Transactions, 1831, p. 401.

Moon's Transit.	h 0	h 1	h 2	h 3	h 4	h 5	h 6	h 7	h 8	h 9	h 10	h 11
Interval	h m 1 57	h m 1 42	h m 1 26	h m 1 11	h m 0 56	h m 0 45	h m 0 42	h m 0 52	h m 1 23	h m 1 56	h m 2 10	h m 2 8

Table XLIV.

Showing the Correction for the Moon's Parallax, formed by arbitrary alterations from Table XLII.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	Moon's Transit.
h m	m	m	m	m	m	m	m	h m
0 0	+11	+ 7	+ 3	0	− 3	− 7	−11	0 0
0 30	+11	+ 7	+ 3	0	− 3	− 7	−11	0 30
1 0	+10	+ 6	+ 3	0	− 3	− 6	−10	1 0
1 30	+ 9	+ 6	+ 3	0	− 3	− 6	− 9	1 30
2 0	+ 8	+ 5	+ 2	0	− 2	− 5	− 8	2 0
2 30	+ 7	+ 4	+ 2	0	− 2	− 4	− 7	2 30
3 0	+ 6	+ 4	+ 2	0	− 2	− 4	− 6	3 0
3 30	+ 5	+ 4	+ 2	0	− 2	− 4	− 5	3 30
4 0	+ 4	+ 3	+ 1	0	− 1	− 3	− 4	4 0
4 30	+ 3	+ 2	+ 1	0	− 1	− 2	− 3	4 30
5 0	+ 2	+ 1	0	0	0	− 1	− 2	5 0
5 30	+ 1	0	0	0	0	0	− 1	5 30
6 0	+ 1	0	0	0	0	0	− 1	6 0
6 30	+ 4	+ 3	+ 1	0	− 1	− 3	− 4	6 30
7 0	+ 6	+ 4	+ 2	0	− 2	− 4	− 6	7 0
7 30	+ 9	+ 5	+ 2	0	− 2	− 5	− 9	7 30
8 0	+12	+ 8	+ 4	0	− 4	− 8	−12	8 0
8 30	+15	+10	+ 5	0	− 5	−10	−15	8 30
9 0	+17	+11	+ 6	0	− 6	−11	−17	9 0
9 30	+17	+11	+ 6	0	− 6	−11	−17	9 30
10 0	+15	+10	+ 5	0	− 5	−10	−15	10 0
10 30	+14	+10	+ 5	0	− 5	−10	−14	10 30
11 0	+13	+ 9	+ 4	0	− 4	− 9	−13	11 0
11 30	+12	+ 8	+ 4	0	− 4	− 8	−12	11 30

Table XLV.

Showing the Correction for the Moon's Declination, formed from Table XL.

Moon's Transit.	0	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Moon's Transit.
h m	m	m	m	m	m	m	m	m	m	m	h m
0 0	+ 8	+ 6	+ 4	+ 2	+ 1	0	− 3	− 6	− 9	−12	0 0
0 30	+ 7	+ 6	+ 4	+ 2	+ 1	0	− 2	− 6	− 9	−12	0 30
1 0	+ 6	+ 5	+ 4	+ 3	+ 1	0	− 2	− 6	− 9	−11	1 0
1 30	+ 6	+ 5	+ 4	+ 3	+ 1	0	− 3	− 6	− 9	−11	1 30
2 0	+ 5	+ 4	+ 3	+ 2	+ 1	0	− 3	− 6	− 9	−10	2 0
2 30	+ 6	+ 5	+ 4	+ 3	+ 2	0	− 2	− 5	− 6	−11	2 30
3 0	+ 7	+ 5	+ 4	+ 3	+ 2	0	− 2	− 5	− 6	−12	3 0
3 30	+ 8	+ 6	+ 4	+ 2	+ 1	0	− 3	− 6	− 8	−13	3 30
4 0	+ 9	+ 7	+ 5	+ 5	+ 2	0	− 3	− 7	−11	−15	4 0
4 30	+12	+10	+ 8	+ 6	+ 3	0	− 3	− 7	−11	−16	4 30
5 0	+15	+12	+ 9	+ 6	+ 3	0	− 3	− 7	−11	−17	5 0
5 30	+17	+15	+11	+ 7	+ 3	0	− 3	− 7	−13	−18	5 30
6 0	+18	+16	+12	+ 8	+ 3	0	− 4	− 9	−15	−20	6 0
6 30	+19	+17	+12	+ 8	+ 3	0	− 5	−10	−16	−23	6 30
7 0	+19	+17	+12	+ 8	+ 3	0	− 5	−12	−21	−26	7 0
7 30	+18	+16	+11	+ 8	+ 3	0	− 6	−12	−18	−28	7 30
8 0	+17	+15	+10	+ 7	+ 2	0	− 5	−12	−18	−28	8 0
8 30	+16	+15	+10	+ 7	+ 2	0	− 4	−10	−15	−23	8 30
9 0	+15	+12	+ 9	+ 6	+ 3	0	− 3	− 8	−14	−21	9 0
9 30	+13	+11	+ 8	+ 4	+ 1	0	− 4	− 8	−13	−18	9 30
10 0	+12	+10	+ 7	+ 5	+ 1	0	− 4	− 8	−12	−17	10 0
10 30	+11	+ 9	+ 6	+ 3	+ 1	0	− 3	− 6	−10	−15	10 30
11 0	+10	+ 8	+ 5	+ 2	+ 1	0	− 3	− 6	− 9	−14	11 0
11 30	+ 9	+ 7	+ 4	+ 2	+ 1	0	− 3	− 8	− 8	−13	11 30

Table XLVI.

Showing the correction for the calendar months, formed from Table G, p. 148.

Moon's Transit.	Jan. July.	Feb. Aug.	March. Sept.	April. Oct.	May. Nov.	June. Dec.	Moon's Transit.
h	m	m	m	m	m	m	h
0	− 2	− 2	0	+ 2	+ 2	0	0
1	− 3	− 2	0	+ 3	+ 2	0	1
2	− 7	− 2	+ 2	+ 7	+ 2	− 2	2
3	− 4	0	+ 3	+ 4	0	− 3	3
4	0	+ 1	+ 4	0	− 1	− 4	4
5	+ 3	+ 5	+ 6	− 3	− 5	− 6	5
6	+ 8	+ 6	0	− 8	− 6	0	6
7	+11	+ 5	− 3	−11	− 5	+ 3	7
8	+ 8	+ 2	− 5	− 8	− 2	+ 5	8
9	+ 3	− 1	− 1	− 3	+ 1	+ 1	9
10	0	− 1	0	0	+ 1	0	10
11	− 1	− 1	− 1	+ 1	+ 1	+ 1	11

Table XLVII.

Showing the correction for the Moon's Parallax in the Height of High Water, formed by arbitrary alterations from Table XLIII.

Moon's Transit.	H. P. 54′.	H. P. 55′.	H. P. 56′.	H. P. 57′.	H. P. 58′.	H. P. 59′.	H. P. 60′.	Moon's Transit.
h m	Feet.	Feet.	Feet.		Feet.	Feet.	Feet.	h m
0 0	−·42	−·28	−·14	0	+·14	+·28	+·42	0 0
1 0	−·45	−·30	−·15	0	+·15	+·30	+·45	1 0
2 0	−·55	−·36	−·18	0	+·18	+·36	+·55	2 0
3 0	−·65	−·44	−·22	0	+·22	+·44	+·64	3 0
4 0	−·72	−·48	−·24	0	+·24	+·48	+·72	4 0
5 0	−·75	−·50	−·25	0	+·25	+·50	+·75	5 0
6 0	·72	−·48	−·24	0	+·24	+·48	+·72	6 0
7 0	−·67	−·44	−·22	0	+·22	+·44	+·67	7 0
8 0	−·62	−·40	−·20	0	+·20	+·40	+·62	8 0
9 0	−·57	−·38	−·19	0	+·19	+·38	+·57	9 0
10 0	−·52	−·36	−·18	0	+·18	+·36	+·52	10 0
11 0	−·47	−·32	−·16	0	+·16	+·32	+·47	11 0

Table XLVIII.

Showing the Correction for the Moon's Declination in the Height of High Water, formed by arbitrary alterations from Table XLI.

Moon's Transit.	0° Decl.	3° Decl.	6° Decl.	9° Decl.	12° Decl.	15° Decl.	18° Decl.	21° Decl.	24° Decl.	27° Decl.	Moon's Transit.
h m	Feet.	Feet.	Feet.	Feet.	Feet.		Feet.	Feet.	Feet.	Feet.	h m
0 0	+·25	+·20	+·15	+·10	+·05	0	−·05	−·10	−·15	−·20	0 0
1 0	+·17	+·13	+·10	+·07	+·03	0	−·03	−·06	−·09	−·30	1 0
2 0	+·12	+·09	+·06	+·04	+·02	0	−·02	−·04	−·06	−·09	2 0
3 0	+·07	+·05	+·03	+·02	+·01	0	−·01	−·02	−·03	−·05	3 0
4 0	+·02	+·02	+·01	0	0	0	0	0	−·01	−·02	4 0
5 0	+·05	+·04	+·03	+·02	+·01	0	−·01	−·02	−·03	−·04	5 0
6 0	+·15	+·12	+·09	+·06	+·03	0	−·03	−·06	−·09	−·12	6 0
7 0	+·25	+·29	+·15	+·10	+·05	0	−·05	−·10	−·15	−·20	7 0
8 0	+·50	+·40	+·30	+·20	+·10	0	−·10	−·20	−·30	−·40	8 0
9 0	+·50	+·40	+·30	+·20	+·10	0	−·10	−·20	−·30	−·40	9 0
10 0	+·42	+·33	+·24	+·16	+·08	0	−·08	−·16	−·24	−·33	10 0
11 0	+·35	+·28	+·21	+·14	+·07	0	−·07	−·14	−·21	−·28	11 0

The last Tables, intended to serve for the prediction of the tides in the port of London, seem to me fairly to embody the results furnished by more than 10,000 observations. They might perhaps be amended in a subsequent revision, by applying these corrections, considered as approximate, to each of the observations employed, with a contrary sign, so as to obtain a discussion of their errors. I am confident, however, that they are not susceptible of any material improvement; and that we may proceed safely to investigate the laws or expressions which they represent. It will also be desirable to ascertain, by a discussion of their errors, when used in predicting the phenomena, whether the fluctuations of the atmosphere, as indicated by the barometer, have any sensible effect, as is the case, according to M. Daussy, on the coast of France, and also to discover the extent to which the phenomena are modified by winds and storms.

The Tables might be rendered more convenient for practice by the addition of a constant, so as to render all the corrections positive, but I have retained them in their present state in order that the law of the inequalities may be more apparent. This change can easily be made if required, but by such a process the intrinsic character of the Tables is not altered.

In the Philosophical Transactions for 1833 I gave the semimenstrual inequality, including of course the establishment for Brest, Plymouth, Portsmouth, and Sheerness. M. Dessiou has since deduced the following Tables for Pembroke Dockyard, Liverpool, Howth, and Ramsgate.

Pembroke Dockyard.			Liverpool Docks.			Howth Harbour.			Ramsgate Harbour.		
Moon's Transit.	Corresponding Interval.	No. of Obs.	Moon's Transit.	Corresponding Interval.	No. of Obs.	Moon's Transit.	Corresponding Interval.	No. of Obs.	Moon's Transit.	Corresponding Interval.	No. of Obs.
h m	h m		h m	h m		h m	h m		h m	h m	
0 14·8	6 0	29	0 17·2	11 17·4	56	0 18	10 57	8	0 13	11 41·5	32
0 45·1	5 53·7	30	0 45·5	11 11·1	60	0 44	10 55	7	0 43	11 40·4	29
1 16·1	5 46·9	30	1 15·5	11 4·3	55	1 12	10 47	8	1 15·2	11 32·3	33
1 46·5	5 39·5	29	1 44·7	10 57	59	1 45	10 30	9	1 42·2	11 31·8	26
2 15·7	5 30·3	27	2 15·3	10 58·5	59	2 19·5	10 22·5	8	2 15·6	11 12·4	30
2 44	5 23·5	27	2 45·1	10 43·8	59	2 48	10 10	7	2 45·1	11 9·4	27
3 14·9	5 15·6	30	3 14·4	10 38·2	57	3 13	10 14	5	3 15·7	11 2·7	32
3 44·5	5 14·3	26	3 44·6	10 30·4	63	3 45	10 10	9	3 45·4	10 54·2	26
4 12·7	5 8·6	28	4 15·5	10 26·7	61	4 19	10 6	7	4 15·5	10 43·1	32
4 44·5	5 1·4	34	4 45·9	10 24·8	62	4 45	10 4	8	4 45·3	10 41·6	28
5 17	4 59·7	28	5 16	10 25·5	58	5 16	10 22	8	5 17·3	10 36·7	34
5 45·5	4 55·3	28	5 45·2	10 26·5	55	5 45	10 16	9	5 46·7	10 34·6	26
6 14	5 1·7	28	6 13·8	10 34·2	54	6 13·3	10 26	14	6 17	10 44·9	32
6 42·7	5 9·1	27	6 44·5	10 49·5	65	6 47	10 33	18	6 43·9	10 56·6	31
7 13·7	5 26·7	33	7 16·6	11 6·5	57	7 19	10 58	13	7 14	11 1·9	48
7 46·3	5 47·7	33	7 45·5	11 21	54	7 47	11 3	13	7 47	11 11·1	32
8 16·4	6 2·6	27	8 14·7	11 36·5	58	8 15	11 17	14	8 18	11 31·6	29
8 44·5	6 10	29	8 44	11 45·6	58	8 44	11 27	13	8 45·5	11 53·8	27
9 16·2	6 20·5	31	9 16·4	11 46·4	59	9 13·3	11 32	15	9 14·3	11 53·9	31
9 47·5	6 23·1	31	9 43·7	11 48·5	58	9 43	11 33	13	9 47	12 2·8	33
10 16·7	6 23·3	26	10 14·8	11 44·6	59	10 13·5	11 33·5	15	10 18	11 54·3	27
10 45	6 20·6	29	10 43·5	11 39·4	55	10 45·7	11 29	13	10 44·5	11 59·2	26
11 13	6 15·6	27	11 11·5	11 34·6	58	11 17·0	11 23	10	11 15·1	11 52·6	31
11 42·6	6 8·7	30	11 44·3	11 26·4	63	11 45	11 17	10	11 44·3	11 52·6	28

The Pembroke tides are from 697 observations made between the 1st of November 1832 and the 31st of October 1833. Those for the Liverpool Docks are from 1402 observations made in the years 1772 and 1791. Those for Howth Harbour are from only 254 observations, made in 1817 between the 16th of June and the 15th of December. Those for Ramsgate Harbour are from 730 observations, made between the 1st of September 1831 and the 31st of August 1833.

TABLE showing the Interval between the Moon's Transit and the Time of High Water.

Moon's Transit.	Pembroke.	Liverpool.	Howth.	Ramsgate.	Moon's Transit.
h m	h m	h m	h m	h m	h m
0 0	6 4	11 22	11 8	11 46	0 0
0 30	5 57	11 14	11 0	11 41	0 30
1 0	5 50	11 8	10 50	11 36	1 0
1 30	5 43	11 1	10 40	11 30	1 30
2 0	5 35	10 54	10 28	11 19	2 0
2 30	5 27	10 47	10 18	11 11	2 30
3 0	5 20	10 41	10 13	11 5	3 0
3 30	5 15	10 34	10 10	10 59	3 30
4 0	5 11	10 29	10 8	10 49	4 0
4 30	5 5	10 26	10 7	10 42	4 30
5 0	5 1	10 25	10 8	10 39	5 0
5 30	4 57	10 26	10 12	10 36	5 30
6 0	4 58	10 30	10 20	10 40	6 0
6 30	5 5	10 42	10 29	10 50	6 30
7 0	5 18	10 58	10 42	10 59	7 0
7 30	5 37	11 13	10 56	11 7	7 30
8 0	5 55	11 29	11 10	11 20	8 0
8 30	6 7	11 41	11 22	11 41	8 30
9 0	6 16	11 46	11 30	11 54	9 0
9 30	6 22	11 48	11 33	11 58	9 30
10 0	6 23	11 46	11 33	11 59	10 0
10 30	6 22	11 42	11 31	11 57	10 30
11 0	6 18	11 37	11 26	11 56	11 0
11 30	6 12	11 30	11 19	11 53	11 30

X. *On the Nature of Death.* *By* A. P. W. Philip, *M.D. F.R.S. L. & E. &c.*

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I NEED hardly say, that in such a communication as the present, I have no intention of entering into the part of the subject of this paper which may justly be termed metaphysical. The veil which separates it from experimental science must ever remain impenetrable, there being no source of information respecting it, but a direct revelation from the great Author of our being, or the instincts he has implanted in our nature, for all knowledge is not acquired. We come into the world with knowledge essential to our existence. The infant knows as well how to breathe and how to suck as the adult, and these acts depend as much on mental operations as those which are the results of experience. He perceives his wants, and he knows how to relieve them; and the extent to which this species of knowledge exists in some animals, whose reasoning powers are extremely limited, justly excites our wonder and admiration. They know what is essential to their condition with an accuracy which sets at defiance all the efforts of human reasoning, for their knowledge is the knowledge of their Creator.

To the physiological part of the subject alone I wish to direct the attention of the Society. It forms part of the same subject with the three last papers I had the honour to present to it, published in the Philosophical Transactions for 1831 and 1833; namely, the relation which the different powers of the living animal body bear to each other. In these papers I endeavoured to trace the nature of their influence on each other while their state of vigour remains; in the following paper I shall attempt to point out the manner in which they influence each other in their state of decay.

In the course of my Inquiry into the Laws of the Vital Functions, it became necessary to determine, with more precision than had been done, the line of distinction between the sensorial and nervous functions.

The function of the muscular system, from its nature and the peculiar structure of its organs, is readily defined; but in the nervous system we perceive more than one set of functions, and yet, both from the variety of ways in which they are interwoven, and from the peculiar mechanism of the active parts of their organs being so minute as to escape our senses and consequently the investigations of the anatomist, the difficulty of correctly distinguishing them is considerable. It is only by experiments instituted for the purpose, and founded on the very different nature of these sets of functions, that the line of distinction can be drawn.

In order to render the results more certain, I endeavoured to ascertain this line by two sets of experiments, conducted on different principles; the object of the one being to ascertain what functions remain after the sensorial power is withdrawn, and of the other, what functions fail on withdrawing the nervous power; and in prosecuting this subject, I found it requisite to study the process of dying, to determine the steps by which the body of the more perfect animal becomes subject to the laws of inanimate matter.

The experiments by which this was more immediately attempted were not laid before the Society as were the other parts of the investigation. They are detailed at length in the second part of my Inquiry into the Laws of the Vital Functions. I have there, however, entered no further into the nature of death than was necessary for the purpose I then had in view. I am now about to compare the results of these experiments with those of others, made since the publication of that treatise, with a view, as far as experiment can apply to it, of explaining the nature of death.

It appears to me that the various facts ascertained in the course of the inquiries in which I have been so long engaged, throw light on this subject. I shall, as I proceed, refer to the passages, either in my papers in the Philosophical Transactions or my Inquiry into the Laws of the Vital Functions, where the proofs of the different positions I shall have occasion to state, will be found.

In the last of the papers above referred to, I had occasion to observe, that there is no question relating to the animal economy which involves a more general view of its phenomena than the nature of sleep. The nature of death also includes a general view of the functions of health, for such we shall find are the laws of our frame, that these functions alone necessarily lead to death; but the nature of death is a more complicated question. It includes the various ways in which the functions are influenced by disease, the effects of which are so numerous that they seem at first view a train of countless phenomena which defy all attempts to refer them to general principles.

I need not say that many advantages would arise from a correct knowledge of the immediate cause of death, and of the different sources from which the state that constitutes that cause arises. The most important would be, that it would give to the physician a clearer view of the tendencies of disease, and consequently of the indications of cure; but it would not be the least of its advantages, that it would tend to strip a change which all must undergo of the groundless terrors with which, we have reason to believe, the timid and fanciful have clothed it.

IT appears from the experiments in question, that in the more perfect animals there are three distinct classes of functions, the sensorial, the nervous, and the muscular, which, having no direct dependence, are yet, through their organs, dependent on each other; for the destruction of any one of these classes of functions more or less immediately destroys the organs of all.

We know that the immediate organs of the nervous and sensorial functions, although both residing in the brain and spinal marrow, are distinct sets of organs, because they have not the same locality; the former, as appears from direct experiments, being distributed throughout the whole brain and spinal marrow, and, as far as experiment can determine, equally so, except that the lower part of the spinal marrow either partakes of them less, or they are there of less power*; while the latter, in all the more perfect animals, are chiefly, and in man almost wholly, confined to the brain; and because in disease we often see the functions of the one class greatly impaired without those of the other being at all affected, and in the process of dying, we shall find all the sensorial functions finally lost, while all the nervous functions remain, and are only indirectly impaired by the loss of the former.

The sensorial functions constitute the sensitive system,—that by which we perceive and act,—and consequently are connected with the world which surrounds us. The nervous and muscular, the vital system, that by which we are maintained.

From the same experiments it appears, that what is called death consists in the loss of the first of these classes of functions, the sensorial, the nervous and muscular functions still continuing, which are lost only in consequence of the failure of respiration, the only vital function to which the cooperation of the sensorial power is necessary.

Many hypotheses have been framed for the purpose of explaining why the motions of the heart and blood-vessels are not, like those of the limbs, subjected to the will. Among these is the hypothesis of Dr. Johnstone of Worcester†, adopted from Winslow, Prochaska, and other writers, which professes to rest on the evidence of experiment, and ascribes to the ganglions the power of intercepting the influence of the brain, and consequently of the will. We have seen, however, that the influence of both the brain and spinal marrow reaches the heart and blood-vessels as readily as the muscles of voluntary motion ‡.

All that has been written on this question seems only to perplex it. When we dismiss the various hypotheses on the subject, the answer appears easy. There are evidently two conditions necessary to render a muscle subject to the will: the stimulus which excites it must be so, and it must be capable of effecting an end desired. If we had no wish to handle, the muscles of the hand would never have become subject to the will. The heart and blood-vessels in all their usual motions are excited by the blood, the stimulating properties of which the will can neither increase nor impair; and what act of volition could be performed by these organs? The only internal organs which can effect an end desired, are the rectum and bladder, when their contents have accumulated to a certain extent; and they are both, under such circumstances, subjected to the will. Their action here may be said to be vital functions, to which the cooperation of the sensorial power is necessary; but, to say nothing of the sensorial

* Philosophical Transactions for 1815, 1829, and 1833; and my Experimental Inquiry into the Laws of the Vital Functions, Part II. chap. ii. Third Edition: wherever this Inquiry is referred to, the references are to the Third Edition.

† Essay on the Use of the Ganglions; published in 1771.

‡ Philosophical Transactions for 1815; and Experimental Inquiry, Part II. chap. i. and ii.

power under all circumstances not being essential to them, they are not so immediately essential to life as to be comprehended in the term vital functions, according to its usual acceptation.

When the animal no longer feels and wills, his breathing ceases and he is, according to the common acceptation of the term, dead, although his body still retains its other powers, which, while they last, prevent its obeying the laws of inanimate nature*; but the changes which after this take place, of course no more affect the individual than if they took place in any other mass of matter.

In inquiring into the physiological nature of what is called death, therefore, it is to the ceasing of the sensorial functions alone that the attention must be directed. Thus the subject divides itself into two parts; the final loss of the sensorial functions, which in common language has obtained the name of Death; and absolute death, that is, the loss of all the functions, which we shall find in the more perfect animals is the necessary consequence of the loss of the sensorial functions.

The latter functions, as I have already had occasion to point out in my paper on the Nature of Sleep, published in the first part of the Philosophical Transactions for 1833, belong to those parts of the brain and spinal marrow which are associated with the nerves of the sensitive system, and which, it appears, from another paper which the Society did me the honour to publish in the same part of the Transactions, are the only active parts of the sensorial system; those on which the power of all its other parts depends. To them, therefore, we must look for the immediate cause of failure when the functions of the sensitive system, whether temporarily or finally, fail. It is here we found that the immediate cause of sleep exists; and it appears, from what has just been said, that to the same parts we must look for the immediate cause of what is called death.

The state which immediately precedes the last act of dying, then, according to the common acceptation of the term, and sleep, depend on a failure of function in the same organs. In what, then, consists the difference of these states? The most evident is, that the one is a temporary, the other a final failure; and it will appear, that in the only death which can strictly be called natural, the state of the sensitive system which immediately precedes death differs from its state in sleep in no respect but in degree.

The cause of sleep, as appears from the paper above referred to, is uniformly the same,—a diminished excitability of the sensitive parts of the brain and spinal marrow, in consequence of the action of the ordinary stimulants of life; but a loss of excitability in those parts we shall find is never the sole cause of death, and often makes no part of its cause. In sleep we have seen that the sensitive parts of the brain and spinal marrow regain their functions in consequence of the continued vigour of the vital system, by which their excitability is restored. To render the exhaustion which constitutes sleep permanent, therefore, the powers of this system also must fail;

* Experimental Inquiry, Part II. chap. xi.

and if any cause of failure in these powers occur, it is evident, that whatever be the state of the sensitive system, its powers must fail with them.

THE natural death of the animal is the death of old age; and as this is the simplest form of death, it is that which I shall first consider. We shall find that the state which immediately precedes this death, and must consequently be considered as its cause, must, in the nature of things, differ from sleep in no other respect than the less vigorous state of the functions of both systems, and consequently that these states are identical; the greater or less general vigour making no difference in their nature.

We are not necessarily born to suffering. All natural states, with the exception of child-bearing, (and in its most natural state even this is hardly an exception,) are more or less pleasurable. It will appear from the nature of our constitutions, that the last feelings in natural death are necessarily of the same nature as those which precede sleep. It is only where the course of our decay is disturbed, that suffering of any kind attends it.

From a knowledge of the animal economy, we might, independently of experience, have foretold that a state of sleep would be that which immediately precedes the last act of dying from old age. It appears from what was said of the nature of sleep in the paper above referred to, that although the vital organs do not, in it, partake of the peculiar state which constitutes sleep, their functions are all, for the time, impaired by the exhaustion of the sensitive system. The respiration, we have seen, is rendered less frequent, in consequence of which the activity both of the circulation and the other assimilating functions which depend on it, is, for the time, lessened.

Now, as the death of old age arises from the gradual failure of those functions, it must necessarily take place at the time at which their vigour is most impaired. If the vital powers are still capable of restoring the sensitive system under the disadvantage of a diminished frequency of respiration, it is evident that, if their decay be gradual, nothing occurring suddenly to accelerate it, they cannot fail to maintain the functions of that system during the short time which intervenes before the recurrence of sleep again exposes them to the same difficulty. Their failure necessarily takes place at the time when their functions are most difficult. The death of old age, therefore, is literally the last sleep, uncharacterized by any peculiarity. The general languor of the functions in the last waking interval is attended with no peculiar suffering, and the last sleep commences with the usual grateful feelings of repose, the last feelings experienced; for with what takes place after them, the feelings, being suspended, have no concern.

The only difference between the last, and the sleep of former times, is, that the exhaustion of the sensitive system, which is at first, as in the latter case, only partial, (for in the beginning of this sleep the sleeper may be roused by more powerful stimulants than those which preceded it,) becomes in its continuance, in consequence of the failure of those powers which formerly restored the sensitive system, complete.

As it is by the continued action of the vital parts in sleep that the sensitive parts are restored, the less active the former become, they necessarily effect their restoration the less readily; and when they can no longer effect it, the individual awakes no more; but the circumstance of the vital being no longer capable of restoring the sensitive system, makes no alteration in the nature of its exhaustion. It is still, while it lasts, the same exhaustion which constitutes sleep. The sleep proves final; but the sleeper is wholly unconscious of the cause which renders it so; and there is nothing in its commencement to inform us whether it will be final or not. Thus the sensibility is extinguished, and consequently respiration ceases. The extinction of the sensibility is the last act of dying, in the common acceptation of the term. As the ordinary stimulants of the day produce the sleep of daily occurrence, those of life produce the sleep of death.

Although the sleep of each day restores the sensitive system from the exhaustion which causes it, the daily recurrence of the exhaustion has the effect of permanently lessening the excitability of that system; a change not to be perceived from day to day, but which, from many phenomena, becomes sensible in the course of years. As the sensitive system becomes less excitable as the day advances than on first awaking, so it becomes less excitable as life advances than in childhood; and in like manner, as the repeated excitement of the sensitive system tends to the final decay of its sensibility, the continued excitement of the vital system, as we might *à priori* have supposed, has a similar tendency with respect to the excitability of this system. We find the pulse becoming slower as we advance in life, in consequence of the lessened excitability of the heart and blood-vessels, and the vital organs less readily influenced by the parts of the nervous system associated with them, proving that their functions also are under the process of decay. On the functions of these parts and the powers of circulation, all the assimilating processes depend; and the shrinking frames of the aged indicate their weakened state and the approach of their final extinction; for those were deceived who taught that there is nothing in the laws of our frame which should lead us to believe that it is not formed to last for ever.

The greatest degree of excitability, either in the sensitive or vital system, is not that which produces the most vigorous state of health. We may be too excitable as well as too little so. Many of the more serious diseases of children arise from this cause. The derangement of the digestive organs, which in the adult produces the nervous irritations of indigestion, produces in the infant inflammation of, and effusion on, the brain. The irritation of the gums, which produces pain and restlessness in the former, in the latter produces convulsions and death. Thus it is that the habit of the child is less firm and vigorous than that of the adult, which has acquired steadiness by the diminution of its excitability, in consequence of the continued action of the stimulants of life; but, after a certain period, the fault is a deficiency, not a redundance, of excitability, a defect apparently the necessary consequence of the laws of our frame, and to which every day unavoidably adds.

The redundance of excitability in children, the cause of many evils, we may be assured answers some important end. There is reason to believe that it is on it that the growth of the body depends, and that the due proportion between the excitability and the stimulants of life, by the gradual diminution of the former, determines the period at which the growth is completed in each individual. While the excitability continues redundant, the ordinary stimulants of life necessarily support a greater activity of the functions than is required for the mere maintenance of the body, and thus its volume enlarges, on the same principle that we have just seen it shrinks in the aged, in consequence of their excitability having become defective. It seems to be on this principle, namely, by a premature exhaustion of the excitability, that the hardships of life, that is, the greater than usual application of its stimulants, check the growth. On the same principle we should expect to find that the growth would cease soonest in the most excitable habits, because in them the excitability will soonest be reduced to a due balance with the stimulants of life. Thus it seems to be that the growth of women, who are more excitable than men, generally stops sooner, and consequently that they are of shorter stature, large women, for the most part, having less of the habit peculiar to their sex; and that by far the greater number of the most excitable men, who, in consequence of this constitution, make the greatest figure in their day, are men of short stature, while giants are generally of an opposite habit of body. There must, of course, to such rules be many exceptions. Where so many causes are operating, no result can be uniform.

THE form of death above described is the only one which, strictly speaking, can be regarded as natural. In all its other forms the regular course is disturbed by adventitious causes. But the causes which interfere with the regular course of nature, and which make their impression either directly on our bodies, or through the medium of our mental powers, are, in civilized society, so numerous and complicated, that it is rare to see an instance of such a death. At whatever period death arrives, it is almost always the effect of disease; and at advanced periods of life we only become more liable to death in consequence of our weakened powers rendering us more subject to disease.

Of the various instances of death I have witnessed, there was none that could be regarded as wholly the effect of age. It was always possible to point out some one or more of the vital organs more deranged than the rest, to which death was chiefly to be ascribed. We have, however, accounts of death from old age alone, which were such as has just been described, so that the inferences afforded by the laws of the animal economy are here confirmed by experience.

If we wish to prolong life, we must keep the attention so far directed to the health as to watch the first tendency to failure in any of the vital functions. In a great majority of instances, to a very late period of life, the failure in the commencement is capable of being corrected. By continuance it becomes obstinate, and by the laws of sympathy spreads to other parts. We may be assured there is, in all, the capability of long life if they can escape the effects of disease. Thus it is that those who lead a quiet and retired life, little exposed to powerful impressions either of mind or body, often attain a great age. It is an additional motive for watching the state of health at advanced periods of life, that the longer we live the less in general is our suffering at the last; the nature of our death partaking the more of that of old age. For the further consideration of this subject I beg to refer to my Treatise *On the Preservation of Health, and particularly the Prevention of Organic Diseases.*

ALL modes of death, with the exception of that from old age, may be regarded as more or less violent; but in considering their nature, we must not confound the last act of dying with the suffering which precedes it, and which is often no less when it terminates in recovery than in death, which equally relieves it; and as death, in the usual acceptation of the word, from whatever cause it arises, consists in the loss of the sensorial functions alone, the act of dying is, in this respect, in all cases essentially the same. In all my experiments I found the nervous and muscular surviving the sensorial functions*.

When the animal no longer feels and wills, he is what we call dead; but for a certain time the motion of the blood in every part of the system still continues, and all the assimilating functions still go on, as may be demonstrated by dividing the vital nerves immediately after death, which produces the same change of structure in the organs supplied by them, though in a less degree, as during the life of the animal†; and that all this would be the case, a knowledge of the animal economy would have told us, independently of the aid of experiment, if we could, without this aid, have acquired it.

The removal of the sensorial powers neither destroys the muscular power nor deprives the muscles of involuntary motion of the stimulus which excites them. The heart, indeed, is incapable of its function, because, from the interruption of respiration, its left side is no longer supplied with the kind of blood which is its natural stimulant; and the accumulation of the blood in the lungs from the same cause affecting a great proportion of its vessels, prevents the right side from emptying itself. These are the necessary and almost immediate effects of the interruption of respiration; but the change in the blood of all the capillaries, with the exception of those which belong to this class of vessels, necessarily takes place more slowly. A certain time must always elapse before the stoppage of respiration greatly affects it. It has been sent to these vessels more or less in its proper state, and it still finds its vessels capable of being influenced by their usual stimulant‡. Thus, as I have ascertained

* Experimental Inquiry, Part II. chap. xi.

† Ibid. pp. 175, 176, compared with a paper which the Society did me the honour to publish in the Philosophical Transactions for 1827, entitled, *Some Observations on the Effects of dividing the Nerves of the Lungs, &c.*

‡ Philosophical Transactions for 1833.

by many experiments, the motion of the blood continues in these vessels for several hours after respiration has ceased, that is, as long as the blood can be drawn from the larger arteries,—the cause of these arteries being found empty some time after death*.

But this is not all; the nerves of the ganglionic, as well as cerebral system, retain their power for a certain time after the supply of that power from the brain and spinal marrow has ceased†. The blood therefore still finds the secreting surfaces in a state more or less capable of their functions, and the secreting processes, as I ascertained by frequently repeated experiments, still go on‡: nor is even this all, for the brain and spinal marrow depend for the continuance of their functions on the same powers as other organs; and I found, by an experiment made on so large a scale that it was impossible to be deceived in the result, that there is an actual supply of nervous influence after the sensorial functions have ceased, that is, after what is called death§.

SUCH is the natural decay of our frames; but, as I have already had occasion to observe, it is very rare for it to run its course uninterruptedly, particularly in civilized life. It is almost always disturbed by adventitious causes accelerating it, or the decay of particular parts, which, in consequence of the mutual dependence of the various functions, disorders the whole. Although these causes are of infinite variety, the laws of our frame are limited, and therefore many must operate on the same principle. This leads us to believe that, however varied the causes of disease, it may be possible to reduce their more ultimate effects to a few general heads. The exhaustion of the sensitive system, for example, is of the same nature, whatever be the cause of excitement; and other forms of debility, affecting either the sensitive or vital system, cannot be very various, however various the causes which produce them. We have reason to believe that the endless variety of disease depends more on the peculiar nature and functions of the different organs affected, and the peculiar manner in which different causes affect them, than on any great variety in the states which constitute the more immediate causes of death. However various the effects of disease, there must be but a few points to which they all tend, because the last in the chain of causes which produces what is called death, we shall find, is always the same, and seated in the same parts. On these principles we may hope to reduce the effects of the adventitious causes of death to a few heads, and thus to obtain such a view of the subject as shall enable us to trace the nature, and consequently the operation, of the causes of our decay in individual instances, and therefore to perceive more clearly the operation of the means which tend to counteract them. In the prosecution of the

* Experimental Inquiry, Part II. Experiments 66 and 67.

† See the Observations on the Experiments, which prove the evolution of caloric from the blood after what is called death, in the second part of the Inquiry just referred to.

‡ Ibid. Experiments 65, 69, 70.

§ Experimental Inquiry, Part II. Experiment 65.

subject I shall commence with those causes of disease whose operation most resembles that of the wholesome stimulants of life; and in pursuing, by means of the various experiments which tend to unfold the laws of the animal economy, the consequences of these causes, we shall be led to the effects of such as have nothing in common with them.

IT appears, from what was said of the nature of sleep, that all degrees of excitement in the parts of the brain and spinal marrow associated with the nerves of the sensitive system, are followed by proportional exhaustion. The only limit to this law is the capability of bearing in those parts. Exhausted by mental excitement, the criminal is often awakened for his execution; and the soldier, both by mental and bodily excitement, sleeps by the roaring cannon.

Now although the usual stimulants of the day never, except in old age, where we have seen all our powers have long been in a state of decay, produce such exhaustion as to endanger life, the exhaustion from stimulants of greater power cannot with safety be frequently repeated, because by their continued operation the sensitive parts of the brain and spinal marrow being both more exhausted than is consistent with the due state of the functions before sleep takes place, and roused before they have been refreshed to the usual degree by repose, a state of disease is induced; and all diseased states affecting the system generally, if their causes continue to operate, necessarily prove fatal.

Although in ordinary sleep the vital functions are for the time impaired in consequence of the lessened sensibility rendering the act of respiration less frequent, the state both of the vital and sensitive system is as much a state of health as in our waking hours. The insensibility of the latter only extends to the effects of the daily stimulants of life; and there are ample means in the functions of health for the restoration of this system, the powers of the vital system, as I have already had occasion to observe, being in no degree diminished, but only, in consequence of a slower respiration, less readily excited.

As soon as a diseased state of the sensitive system is established from the causes just mentioned, it begins to affect the vital system otherwise than through the intervention of respiration, the only medium, we have seen, through which the healthy exhaustion of the former affects the latter; for such is the sympathy between the sensitive and vital parts of the brain and spinal marrow, that any deviation from the healthy state of either is immediately felt by the other.

The characteristic of the mode of death I am considering, is the tendency of its causes to produce sleep in the first instance. So far their operation is the same, but greater in degree, with the common stimulants of life. At this period, if the cause of suffering be removed, the sleep is only more profound than on former occasions; and, as on them, it continues till the sensitive system again becomes obedient to those stimulants; if not, this system soon partakes of a species of debility so different from the healthy exhaustion, that instead of being relieved by the continued action of the vital parts of the brain and spinal marrow, it spreads to them. Hence the nutritive and other vital processes begin to fail*, and the various irritations which attend their failure, still further contribute to the debility of the sensitive system, and consequently, indirectly, to increase the cause of their failure. The derangement of each system thus aggravating that of the other, the evil proceeds not by simple addition, but in an increasing ratio, till all their powers are extinguished.

WHATEVER be the suffering which precedes what is called death, the moment of that death is but its termination, but the conclusion, as far as our feelings are concerned, of the process of dying. As soon as disease is established the act of dying is begun, and we have no reason to believe that, as far as the body is concerned, its nature is in any respect changed in what is called its termination. It is, from the first to the final ceasing of all the functions, a more rapid than natural decay of the powers of life, with, while sensibility lasts, more or less suffering, according to the cause which produces it. In recovery, our suffering terminates by the removal of that cause; in what is called death, by our becoming insensible to its effects; the bodily process being in no other way influenced by our total insensibility, to which the name of death is applied, but that the consequent ceasing of respiration accelerates it.

The body at this moment can no more be regarded as in the act of dying than at any other period of the disease; and the removal of the offending cause will not only in many cases at this period, if proper means be employed, but in some, even a short time after it, be followed by recovery. Thus, even after the period at which, according to the common meaning of the word, the process of dying is completed, it is, under certain circumstances, not too late to arrest that process, and restore the sufferer to the perfect enjoyment of his faculties. Recovery may take place after respiration has, from submersion, for a few minutes ceased, and the sufferer is, in the common acceptation of the term, dead, his sensibility, and consequently his respiration, independently of artificial means, being finally extinguished.

That this may happen, it is necessary not only that the vital system should have been just before in a state of healthful vigour, but also that the respiration should not have failed from the failing sensibility, but the operation of the offending cause. Here the sensibility fails from the failure of respiration, not, as in other cases, the respiration from the failure of the sensibility; but this difference in the succession of events makes no difference in the general nature of the actual state induced.

The recovery depends on our being able, more or less perfectly, to restore the function, the failure of which has caused the failure of all the others, as far as it has taken

* That the assimilating processes depend on the action of the nervous influence on the blood, appears from various experiments, an account of which has been laid before the Society. Many of these experiments are detailed at greater length, and others, illustrating the same position, added in my Inquiry into the Laws of the Vital Functions, Part II. chap. v., vii. and viii.

place, before the process of dying has proceeded too far for the restoration of the sensitive system. If no artificial means are employed, the date of death here is the time at which the sensibility ceased, and justly, because at that time death, according to the common meaning of the word, has taken place. The individual no longer feels and wills.

If there were even the last remains of sensibility, breathing would take place without external aid, as happens when the submersion has not been long enough wholly to extinguish it. The individual has, without such aid, finally ceased to feel and will, and is therefore what we call dead. His blood still continues to move, and all the assimilating processes, as appears from the experiments above referred to, are still going on; but this is no more than happens, more or less, in all cases after what is called death; the only difference being that from the nature of the offending cause, and the short duration of the disease, these functions are in a state of greater vigour than when the loss of respiration has been the effect of the loss of sensibility, which makes no difference in the nature either of their remaining powers or the circumstances in which they are placed, and would not prevent their ceasing, as usual, if no means were employed to arrest the dying process. I have dwelt the longer on this case, because it affords a good illustration of some of the preceding as well as following parts of the subject*.

THE approach of death, if we are aware of it, must always be more or less impressive, not only because we are about to undergo an unknown change, but are leaving all

* From the experiments which have been laid before the Society (Philosophical Transactions for 1822, 1827, and 1829, and Experimental Inquiry, Part II. chap. xii.), we have reason to believe that the effects of artificial respiration in restoring those whose breathing has been interrupted till the sensibility is destroyed, would be greatly aided by the use of voltaic electricity sent through the lungs in the direction of their nerves, and that many might thus be restored in whom inflation of the lungs alone fails. The inflation of the lungs in such cases acts in two ways. It gives to the blood of the smaller vessels of the lungs some of the arterial properties by which they are often excited, and acting through the blood of these vessels, it communicates to that of the larger vessels, and of the heart itself, more or less of the same properties, independently of the blood already changed being moved on towards this organ; for M. LE GALLOIS has shown that after the circulation has permanently ceased, the blood may, to a certain degree, be changed by inflating the lungs, not only in the trunks of the pulmonary veins and the heart itself, but even in the great arteries.

There is reason to believe, from the whole of my experiments, that the lungs should not be inflated more than eight or ten times in the minute, and that the injection of large quantities of air and great force in its injection should be avoided, and consequently the patient placed in the position in which the chest expands with greatest ease†. One of the chief defects of artificial breathing is, that in it the chest is expanded by

† Experiments relating to the effects of artificial respiration in the newly dead animal: Experimental Inquiry, Part II. chap. xii. If the air be thrown in more frequently or in greater quantity than the remaining powers of the lungs are capable of employing, it acts as a cooling process and is highly injurious. It is one of the defects of artificial respiration that we cannot tell either the precise quantity of air or the frequency of its injection required by the particular state of the circulating system in the lungs. We know that in the case before us, the demand cannot be equal to what it is in health.

that has hitherto interested and been grateful to us. Even here, however, for the most part, the laws of our nature are merciful. Most diseases of continuance, (for we shall find there are some exceptions,) not only gradually impair our sensibility, but alter our tastes. They not only render us less sensible to all impressions, but less capable of enjoying as far as we are still sensible to them. The sight of a feast to a man who has lost his appetite is disgustful, and a similar change takes place in a greater or less degree with respect to all other means of enjoyment.

These circumstances constitute a great part of the difference of our feelings with respect to what, in common language, is called a violent and a natural death. In the latter, as far as the sensibility is impaired, we are more or less in the state of old age, and, in addition to this change, our tastes are perverted. By these means the relish for life is in a great degree destroyed before we lose it. Thus in disease, the most timid often meet death with composure, and sometimes, as I have repeatedly witnessed, with pleasure. I have even known the information that the danger was passed, received only with expressions of regret.

To the form of death I am considering, belong a large proportion of the diseases of long standing, and whatever else tends gradually to exhaust the powers of the sensitive system, great mental excitement, too laborious a life, &c. The diseased state of the sensitive parts of the brain and spinal marrow, thus induced, spreading to the vital parts of those organs, terminates in a state of nervous apoplexy, the nature of which I had occasion to explain in the paper on Sleep above referred to, and to contrast with that of apoplexy from compression, in the most unmixed cases of which, the offending cause

the pressure of the injected air, whereas in natural breathing the air enters in consequence of its expansion. But the most essential difference between natural and artificial breathing in such circumstances is, that there cannot, till recovery is far advanced, be the proper supply of nervous influence, the due action of the vital parts of the brain and spinal marrow only being restored in proportion as the due force of circulation returns. Now it appears from what is said in the Philosophical Transactions for 1822 and 1827, and more fully in my Inquiry into the Laws of the Vital Functions, that voltaic electricity sent through the lungs in the direction of their nerves, is capable of performing as perfectly as that influence itself, the part which belongs to it in respiration, which is so essential, that the more perfect animal always dies from impeded respiration if the nervous influence be withdrawn from the lungs, unless voltaic electricity be supplied, which enables it to breathe as well as when the nervous influence is entire.

A proper apparatus, therefore, for sending voltaic electricity through the lungs in the direction of their nerves and in due power, should be added to the other means of resuscitation, which would render them, and probably to a great degree, more successful. The force of this observation will be perceived when it is considered that it is at the time of the first application of the remedies that the chance of recovery is greatest, and consequently that the immediate application of the whole means of healthy respiration, as far as we possess them, is of most consequence. It appears from what has been said, that the due functions of respiration cannot be restored till the due degree of nervous influence is supplied, and this cannot happen from inflation of the lungs till the due force of circulation returns. Now the fact, explain it as we may, is, that voltaic electricity so perfectly supplies the place of the nervous influence in the lungs, that their functions are equally perfect under the influence of either. The one can only be supplied at an advanced period of recovery, that is, in fact, only in those cases where the success of our endeavours can be secured by other means; the other is, in all cases, within our reach on the instant.

only producing a state analogous to the healthy exhaustion of the sensitive system but greater in degree, its influence is throughout confined to that system*. In the former case we see all the vital functions deranged; in the latter, the breathing alone affected, except as far as its state affects the others, death arising merely from respiration ceasing in consequence of the loss of sensibility; and so exclusively is this sometimes the case, that I had occasion to refer to an instance in which the patient breathed only two or three times in the last ten minutes, but each time drew the air freely into the lungs; a proof that he died without any accumulation of phlegm there, and consequently without any disorder of the vital functions, but such as arose from the increasing insensibility†. Here the failing powers of the sensitive affected the vital system in no other way than in sleep, the only difference being the degree in which the sensibility was impaired. Such cases are extremely rare. In by far the majority, from some inequality in the effects, or other peculiarity of the cause of pressure, at the same time that the sensibility is morbidly impaired, either a diseased state of a different kind is induced on the sensitive parts of the brain, which, as soon as established, begins to spread to the vital parts of that organ, or the cause of the disease itself more immediately affects the latter.

In the more rapid cases, the diseased state of the sensitive, which spreads to the vital parts of the brain and spinal marrow, supervenes without being preceded by a state of exhaustion, only differing from sleep in being greater in degree, in proportion as the stimulants which produce it are more powerful and protracted.

The effects of diseased states of the sensitive on the vital parts of the brain and spinal marrow, differ according to the nature and degree of the offending cause. When they are such as in the first instance to produce a state analogous to sleep, their injurious effects are necessarily more or less gradual, the first operation of the agent differing only in degree from that of the usual stimulants of life; but where the offending cause is more powerful in degree, or of a more injurious nature, the stage of exhaustion is lost, and the immediate effect on the sensitive system is that species of debility which the vital parts of the brain and spinal marrow having no power to relieve, partake

* It appears from experiments related in my Inquiry into the Laws of the Vital Functions, that simple and uniform pressure of the brain does not produce such a state of the vital parts of that organ as to derange the circulation, the effect of such pressure on the sensitive organs of the brain being of the same nature, as far as relates to the vital system, as the exhaustion occasioned by the exercise of their functions; which further appears from the whole functions of health being immediately restored on the removal of the pressure, which only proves fatal by its continuance more and more impairing, and at length destroying, the sensibility. (Experimental Inquiry, Part II. Experiment 18.) Many years ago, a man in whom the ossification of the skull had never been completed, exhibited himself in this country. By pressure made on the unossified part he was immediately brought into a state of apoplexy, which always disappeared, leaving him wholly uninjured, on the removal of the pressure.

† It has been shown by many experiments, detailed in the Philosophical Transactions, and in the second part of my Inquiry into the Laws of the Vital Functions, that derangement of the assimilating functions is always attended with accumulation of phlegm in the lungs, this being the first indication of derangement of these functions in them.

of; and when the cause is both violent and sudden, its effects on these parts are often such as immediately to destroy the circulation.

The Experiments, an account of which the Society did me the honour to publish in two papers in the Philosophical Transactions for 1815, prove that, although the heart and vessels do not derive their power from the brain and spinal marrow, it may be destroyed by impressions made on them. Thus it is that violent passions, either of a pleasurable or painful nature, in consequence of the sympathy which subsists between the sensitive and vital parts of these organs, have sometimes proved instantly fatal.

Here we have an effect from the causes of disease wholly different from that of the usual stimulants of life. The direct operation of the agent produces a state of debility in the sensitive system altogether of a different nature from that which constitutes the healthy exhaustion of sleep; and it will assist the memory and facilitate the means of reference to regard as the second form of what, for the sake of distinction, I call violent death, that which arises from all those causes which produce in the sensitive system this species of debility in the first instance, that is, debility without previous excitement, in whatever degree they have this effect; regarding, as the first species of such a death, the form of death we have been considering, that in which the cause, in the first instance, produces the stimulant effect, and consequently the exhaustion of sleep.

WHEN the cause of the second form of violent death, according to this division of the subject, is extreme, no time is afforded for its less powerful effects to show themselves. When it is less violent, so that the circulation, though impaired, still goes on, we find all the vital functions impaired along with it. The assimilating processes are doubly assailed by the failing supply of nervous influence and the lessened powers of circulation*. These effects, we have seen, may arise from the excess of the stimulant operation of agents†, but they are not necessarily the consequence of any operation of this kind, but may be as much the direct effect of the agent as the stimulant effect itself. It is, the offending cause and state of body being the same, when the operation of that cause is most powerful, that its debilitating effect is most unmixed. In proportion as it is less powerful, the case partakes more of the nature of the form of death, in which the first effect of the offending cause is that of a stimulant.

This is readily explained. I have been at much pains, in my Inquiry into the Laws of the Vital Functions, to point out that all agents capable of affecting the living animal, whether making their first impression on the mind or body, applied in a certain degree, act as stimulants, in a greater degree, as sedatives; that is, as means of directly impairing the power of the part they act upon ‡. We know of no exception

* See note in p. 177. † See pp. 176 and 177.

‡ Experimental Inquiry, Part II., the last ten pages of chap. xi.; and the observations on the term sedative in my Treatise on the Influence of minute Doses of Mercury, which, from the want of some more appropriate term, I shall here employ for all agents which impair the power of the whole or any part of the animal frame without producing previous excitement.

to this law, and the stimulant and sedative effect of different agents bear no particular proportion to each other; but the greater the stimulant power of the agent, it must be applied to the greater extent to produce the sedative effect, and the greater its sedative power, in the smaller extent, to obtain from it the stimulant effect. The proportion which the stimulant and sedative effects of the same agent bear to each other is always the same, that is, its mode of application and the state of the body being the same; for the more gradual the application, the more the stimulant; the more sudden, the more the sedative effect prevails; and the less vigorous the functions, the less they are capable of the stimulant, and the more they are subject to the sedative effect. Thus torture, which, in the hardy savage, produces sleep, that is, the exhaustion which is the effect of the stimulant operation, acts as a sedative in the less robust European. While the former sleeps, the latter dies; and the more sudden its application the less the constitution is capable of resisting it.

The sedative effect, in whatever degree, is of a nature so different from the exhaustion which constitutes sleep, that its tendency always is to prevent the latter; and when the stimulant operation of the causes of disease exceeds that of the usual stimulants of life, and thus tends to the sedative effect, in the same proportion the tendency of these causes, although in the first instance to produce sleep proportioned to their stimulant effect, is eventually to prevent it. The repetition of fatigue at length produces fever, not sleep.

Such being the principles on which all agents capable of affecting the living animal operate, we readily perceive why the more sudden and powerful the cause of disease, the more it inclines directly to produce a state of debility, and when it is most so, why this tendency is unmixed with any degree of the stimulant effect.

But it is not necessary, as appears from what is said of nervous apoplexy in the preceding paper, that the operation of the agent should be either violent or sudden, to produce, even in the first instance, more or less of the sedative effect, if it be of a nature suited to produce it. In proportion as its application is less powerful, however, its peculiar effects are necessarily so also. Instead of preventing the tendency to sleep, it only impairs it; and the morbid state of the brain and spinal marrow shows itself by symptoms which less immediately threaten life. The sedative effect of agents may exist in all possible degrees, from the effect of the rage and joy which has produced instant death, to that of the settled grief, which only in the course of years destroys its victim; from the pain of a scald so extensive as to produce death in a few minutes, to the irritations of confirmed indigestion, under which the patient often lingers for a great portion of life. Whether the effects be sudden or gradual, the tendency, in all such cases, is the same, to terminate in a state of general debility, that is, nervous apoplexy, in which all the powers of the system are equally impaired.

The first impression of the cause is on the sensitive parts of our frame, which, without previous excitement proportioned to the debility which ensues, impairs their functions; and this debility, in consequence of the sympathy which exists between the sensitive and vital parts of the brain and spinal marrow, spreads to the latter, and thus the vital functions are, more or less quickly, so impaired that they can no longer maintain those of the sensitive system.

The nature of this death is well illustrated by the effects of severe accidents, many of which operate on the same principle as the scald. The effects of severe blows on the head and spine are very complicated. They at once impress equally the sensitive and vital systems; but when the cause of injury is confined to less vital parts, as in the case of the scald, its first impression is on the sensitive system alone, or so nearly so, that the difference may be overlooked. Such was the cause of death in the case of the late Mr. Huskisson, with the circumstances of which the members of the Society are well acquainted; and hence it is that life is often saved by amputating a limb in which a cause of extreme irritation exists, that caused by the operation being more easily borne than the protracted irritation of a shattered limb, if the accident has not so subdued the strength that the additional irritation of the operation would prove immediately fatal.

To the same head belongs the death from the bite of rabid animals. The hydrophobia is a disease of the sensitive, spreading to the vital, parts of the brain and spinal marrow; and such is the effect of many other poisons.

It is evident that the form of death I am now considering is of the same nature as the preceding, with the exception of the early stage of the latter. The sedative state produced in the sensitive organs is of the same nature, whether it has arisen from the excess of the stimulant operation, or from the more direct effect of the agent, when applied in such extent as at once to produce this state. The symptoms produced in the sensitive, and the manner in which they influence the vital, system are the same in both. The same observations, therefore, which apply to the latter stage of the first of these forms, apply, more or less, to the whole progress of that we are considering. In both, what is called death is the final extinction of the sensibility; the termination, as far as relates to our consciousness, of the process which has been going on from the first establishment of the disease. As sleep is the completion of the temporary and limited exhaustion of the excitability which has been going on during the day, death is here the completion of its absolute and final exhaustion, which has been going on during the disease; and it is evident, that as the sensibility decreases, the suffering must become less, and consequently that it is least of all at the moment of what is called death. These observations, however, we shall find do not apply, in the same extent, to the forms of death which still remain to be considered.

THE three forms of death to which the attention has been directed in the preceding part of this paper, namely, that from old age, that from excessive stimulants acting on the sensitive parts of the brain and spinal marrow, and that from agents applied to such extent as to act as sedatives on those parts, agree in an essential respect.

The offending cause makes its impression on the organs of the sensitive system, and therefore in all, the sensibility is more or less directly impaired by it; and although it is only in the first that sleep can be regarded as the immediate cause of what is called death, the cause of injury in the second stage of the second form, and throughout the whole of the third form, producing the sedative effect, and consequently more or less tending to prevent sleep, yet tends, although in a different way, to impair the sensibility; and the termination in all such cases, as I have already had occasion to observe, if no other cause of injury arise in the course of the disease, is a state of nervous apoplexy, in so many cases the prelude of death, which, if not sufficiently violent or sudden, so to impair the powers of circulation as thus immediately to destroy those of the sensitive system, proves fatal by equally impairing the sensibility and impeding the assimilating processes; and as sleep relieves us from the ordinary stimulants of the day, the insensibility thus induced, relieves us from the sufferings of the disease, which, although it is not, like sleep, preceded by the grateful feelings of repose, is preceded by a gradual diminution of those sufferings.

THE forms of death which remain to be considered differ essentially from the foregoing. It will place in a clearer point of view both what I am about to say of these forms of it, and what has been said of its preceding forms, to consider more minutely than has hitherto been done in this paper, or, as far as I know, in any other discussion on the subject, the nature and relation of the functions of the living animal.

IN the community of functions which constitutes the life of man and all the more perfect animals, the sensitive are the working functions, those by which we perceive and act; the vital, those by which they are maintained. To the former, therefore, belong the immediate wear and tear of intercourse with the external world, and, consequently, the necessity of accommodating themselves to an infinite variety of circumstances. The vital functions, having but one object, pursue a steady course, from which, in health, they never deviate, except as far as is necessary to accommodate themselves to the necessities of the more eccentric functions of the sensitive system, the well-being of the organs of which depends on them; for they are capable of immediately influencing as well as being influenced by the inanimate agents which exist within our bodies; on the action of which the due structure as well as functions of every part depend. On this principle our food is digested; on the same principle the heart beats, and the secreting and other assimilating organs effect all their chemical changes. Thus the sensitive parts of the brain and spinal marrow are maintained, and thus also are maintained two sets of organs; through one of which, namely, the organs of the external senses with the nerves which convey the impressions made on them, these parts are capable of being influenced by the inanimate agents external to our bodies; and through the other of which, namely, the nerves and muscles of voluntary motion, they are capable of influencing those agents. These two sets of organs, allied by their vital properties to the sensitive parts of the brain and spinal marrow, and by their capability of being excited by inanimate agents to the world which surrounds us, form the links which connect and enable to conduce to one end the operations of the sensitive organs, namely, the immediate organs of the sensorial powers, and the operations of inanimate nature; two classes of operations which have nothing in common. Let us here pause to consider more particularly the positions stated in this paragraph.

However repugnant it may be to our preconceived opinions, we shall, I think, when the whole of the facts on the subject are carefully weighed, find it impossible to avoid the conclusion, that all the vital functions, and all those functions of the sensitive system by which the sensorial powers influence and are influenced by the external world, are the results of inanimate agents acting on living parts, or living parts on them. Such, as far as I am capable of judging, must be the conclusion, if we compare the results of experiments, an account of which has been laid before the Society, and published in their Transactions *, with observations too simple to require any illustration from experiment.

With regard to the first of these classes, the vital functions, it is evident that the functions of the alimentary canal are excited by the food, of the lungs by the air, and of the heart and blood-vessels by the stimulating contents of the blood.

The blood, as it circulates in the vessels, is justly said to be alive. It possesses properties essentially different from those of inanimate matter; but we know that it is not by its vital properties, which are bestowed on it for other purposes, that it stimulates the heart and vessels, because its stimulating contents, when separated from it, produce the same effects on them. The experiments relating to the evolution of the caloric which supports animal temperature, point out one of the purposes answered by the vital properties of the blood†, and all the experiments relating to secretion and the other assimilating processes, point out the other purposes of its vitality. It possesses vital properties, not for the purpose of acting on other parts, but for that of duly responding to the inanimate agent, which acts on it in all these processes; for that the secreting and other assimilating processes depend on the action of an inanimate agent, appears from the experiments which prove that they depend on the nervous influence, which has been shown by direct experiment to be capable of its functions after it has been made to pass through other conductors than the nerves‡, and cannot therefore have the properties of a vital power; to say nothing of those experiments by which it has been shown that all its functions may be performed by an agent which operates in inanimate nature§.

With regard to those functions by which the intercourse of the sensitive parts of

* Philosophical Transactions for 1817, 1822, 1827 and 1829; and Experimental Inquiry, Part. II. chap. xii.

† Experimental Inquiry, Part. II. Experiments 80, 81, 82, 83, 84, 85 and 86.

‡ Philosophical Transactions for 1822, 1829 and 1833; and Experimental Inquiry, Part. II. chap. xii.

§ Ibid.

the brain and spinal marrow with the external world is maintained, it is evident that the organs of the external senses are excited by inanimate agents external to our bodies, and that the muscles of voluntary motion are capable of influencing those agents; and we know that the impressions made on the external senses are propagated, and the muscles of voluntary motion excited, by the nerves, whose powers, as appears from the experiments just referred to, depend on an inanimate agent.

While the results of these experiments remain undisputed, if we assert that the nervous influence is a vital power, we must allow that such a power may exist in a mechanism wholly different from that to which it belongs in the living animal, and that all the functions of a living power may be performed by an agent which operates in inanimate nature; positions, which I believe no man, acquainted with the laws of the living animal, will be hardy enough to maintain.

Such, then, it would appear, is the nature of our frame. The sensitive parts of the brain and spinal marrow which are at once the immediate organs of enjoyment, the end of our being, and the source of those powers on which our intercourse with the external world depends, are maintained by a set of organs, the functions of which are excited by certain agents which belong to inanimate nature, and operate by other sets of organs which are capable of influencing, and being influenced by, every object around us, the functions of which are also excited by an agent of the same description. And these inferences are in no slight degree strengthened by another and distinct set of experiments, to which I referred in an early part of this paper, namely, those relating to the order in which the functions cease in the act of dying; for the whole of the phenomena traced by these experiments, as will more clearly appear from what I shall have occasion to say a little lower, tend to the same conclusions. Why do the nervous and muscular survive the sensorial functions? Why are the failing powers of life maintained in the organs of the two former classes of functions, after all trace of them is lost in the last class?

To the same conclusions, also, I cannot help thinking the following very simple train of reasoning might, without the aid of experiment, have led us. Although a single fact is often sufficient to establish the truth, when it is once arrived at, we almost always find others ready to give it their aid.

The phenomena of the three classes of functions above enumerated, namely, those by which our bodies are maintained, those by which the sensorial organs are influenced by the external world, and those by which they influence it, appear themselves sufficient to evince that the agents employed in their production partake of the nature of that world. Were not this the case, is it possible that the analogy between them and its phenomena could be such as we find it? Can we conceive a stronger analogy than the phenomena of inanimate nature bear to the propagation of an impulse along a nerve? Do not a thousand inanimate agents excite the muscular fibre in precisely the same way as the nervous influence does*? and it would be difficult to believe that

* See the first of my papers in the Philosophical Transactions for 1833.

the agent which operates in the formation of the secreted fluids from the blood and the other assimilating processes, is of a nature essentially different from that which effects similar changes in the laboratory of the chemist, even if the facts to which I have had occasion to refer had not been experimentally ascertained ; but these facts, bearing more directly on the question, necessarily make a stronger impression.

Let us for a moment glance at those phenomena in which we are assured that no inanimate agent interferes. It is evident that the organs to which impressions made on the nerves are conveyed, must be those organs from which the nerves in question originate and derive their power. The sensitive nerves must communicate the impressions made on them to the sensitive parts of the brain and spinal marrow. It therefore follows that the sensorial functions, consequent on impression made on the nerves of the sensitive system, are the effects of the influence of the nerves on those parts of these organs. What are the results of this action of one vital part on another? Can we see any analogy between the phenomena of inanimate nature and pleasure or pain, the excitement of the feelings, or of the powers of reflection ?

We thus readily perceive why the sensorial functions are the first which cease in dying. The stimulating parts of the blood are still present to excite the vessels, and the nervous influence, as appears from direct experiments above referred to, is still present to support the functions of the assimilating organs; but the sensorial functions being the results of vital parts acting on each other, as the vital powers fail, the powers of the parts acted on, and those which act upon them failing together, these functions necessarily cease. Here there is no inanimate agent present, as in the case of the nervous and muscular functions, to excite the languid powers of life*.

It is evident that in such a system as that I have been describing, there are two principles, either of which may determine the decay of all the sensitive functions. These, the functions by which the intercourse with the external world is maintained, may become incapable of their work, or those functions which maintain them, of their office. In the only natural death, that of old age, we have seen both these principles of decay in operation. The sensitive functions are gradually dimmed, and the vital functions gradually become less active.

Life, without much violence done to language, has been called a forced state. It consists of excitable parts called into action by suitable stimulants. These stimulants, it appears from what has been said, are all of an inanimate nature, for although the sensorial can only be excited through the nervous system, the action of the former, it

* It is observed in my Inquiry into the Laws of the Vital Functions, that in the most sudden death arising from causes which instantly destroy the powers of the nervous system, all the vital powers are at once destroyed; but this is only to be understood comparatively. The time in such cases required for their destruction is short; but in all the instances I have witnessed, the same succession, however rapid, could be observed. It was still evident that the muscular and nervous survived the sensorial functions. After the sensorial functions had ceased, slight flutterings of the heart and fleeting contractions of the muscles of voluntary motion could still be observed.

is evident, equally, though not so immediately, depends on the agents which excite the latter. Hence the harmony which exists between the living powers of the animal body and the powers of inanimate nature. There is nothing in common in the nature of these powers; but the organs of the former, being composed of the same materials with the world which surrounds us, can be excited by no means but the agents which operate in that world; and on what principle could we expect any other result?

These organs themselves are a part of inanimate nature. Deprived of their vital powers, they may still, as far as we see, be perfect in all their parts. On what their vital powers depend, we know not. In the study of these powers, and the relation they bear to the other powers of nature, we must be satisfied to take the facts as we find them. And what other knowledge have we of the inanimate powers themselves? Do we know more of the nature of gravitation or electricity than of life? It is the properties, not the essences, of things which are the objects of our senses. Our nature must be changed before the latter can be made a subject of inquiry. Life is a certain train of phenomena, depending on the peculiar state of its organs, produced by the action of the same agents, which operate in other parts of nature, on the material organs of our frame. We may arrange these phenomena in the way that best assists the memory, and best shows their relation to each other and the other phenomena of nature; but no task can be more hopeless than the attempt to proceed one step further, either with respect to the living powers or any other principle of action. Such an attempt is beyond not merely the limits, but the nature, of our minds. It is the blind attempting a knowledge of colours.

When we say we understand any of the phenomena of nature, we only mean that we are able to class them with other similar phenomena. We say that we know why a stone falls to the ground, because we class its fall with the other phenomena of gravitation. With regard to the phenomena of animal life, we at once see the limit of our inquiries, because it is self-evident that these phenomena exist nowhere but in the living animal, and consequently that there is no more general principle to which they can be referred; a position so evident that it is difficult to understand how it could ever have been overlooked.

It is customary to speak of life as a subject of peculiar mystery. But if what has just been said be correct, we have precisely the same means of acquaintance with it as with the other powers of nature. Its phenomena are as open to observation and experiment as the phenomena of any of these powers; and we possess no information respecting any of them but such as is derived from those sources. The greater appearance of mystery arises, not from the greater obscurity of the nature of life, but from its phenomena bearing less analogy to those of the other powers of nature than these bear to each other; in consequence of which the former are less familiar objects of contemplation. Simple as such observations are, they cannot be regarded as superfluous, when we see them overlooked by such writers as Hartley, Hunter, and others of almost equal name.

We cannot be surprised that the inanimate agents which are incapable of any change that unfits them for their office, should at length effect a permanent change in the vital parts on which they operate, of all parts of nature the most changeable. Hence the death of old age.

The sensorial functions we have seen fail first, because their organs are removed from the immediate action of the inanimate agents which still excite the organs to which they are directly applied; but for the same reason, it is in the latter, the organs of the nervous and muscular systems, that the decay begins. Their powers are gradually impaired by the operation of the inanimate agents which excite them, and the sensorial powers, as appears from all the phenomena of our decay, only fail in consequence of their failure; but as a certain vigour is necessary to render the latter capable of maintaining the sensorial functions, these necessarily cease before the total extinction of those which maintain them.

IN the forms of violent death which have been considered, the offending cause makes its impression on the organs of the sensitive, in those which remain to be considered, on the organs of the vital system.

IT is evident from what has been said of the nature and relations of the functions of the living animal, that there is one class of the causes of death which is necessarily confined to the vital organs. On them, we have seen, the inanimate agents on the operation of which life depends, make their impression. Those which impress the organs of the sensitive system excite only the functions by which our intercourse with the external world is maintained, and consequently may cease to operate without at all endangering life. But the withdrawal of the agents which excite the vital organs as certainly proves fatal as the loss of power in these organs themselves.

The operation of such causes is too simple to require any comment. It is evident that the want of food must destroy the digestive and other assimilating functions; that of air, the functions of the lungs; and the loss of blood, to a certain extent, those of the heart and blood-vessels.

THE other causes which belong to the forms of death I am now to consider, operate in a manner analogous to the offending causes which make their impression on the organs of the sensitive system; for although the vital organs are not subject to the same species of exhaustion with those of the sensitive system*, like them they may be debilitated either by the excess of the stimulant, or the more direct, effect of the agent, according to the degree in which it is applied. The excitement of fever terminates in debility of the heart and blood-vessels, or where the cause is more powerful, as we see in the worst forms of typhus, it may directly impair their powers;

* See my paper on Sleep in the Philosophical Transactions for 1833.

and similar observations apply to the effects of the offending cause on all the other vital organs. Although such are uniformly its effects on the parts on which it operates, its effects on the system in general, in consequence of the sympathies of our frame, admit of greater variety. These also may be divided into two classes.

In considering the second of the forms of death in which the impression of the offending cause is confined to the organs of the sensitive system, it appeared that when it is both violent and sudden, it immediately, in consequence of the sympathy of the sensitive and vital parts of the brain and spinal marrow, and the influence of the latter on the heart and blood-vessels, destroys the circulation*; whereas, when less powerful, it proves fatal, not only more slowly, but also in a different way. A similar observation applies to the causes of death which make their impression on the vital organs; for the circumstance of their being more or less violent and sudden, or making their impression on an organ more or less immediately essential to life, not only renders their effects more or less sudden, but essentially influences their nature.

When the cause affects an organ immediately essential to life, and is of such power as at once to destroy its function, death, depending wholly on the loss of that function, may be instantaneous; but when the cause operates less rapidly, or affects organs less immediately essential to life, death is not only more protracted, but the various causes of continued irritation which attend derangement of the vital, influencing the state of the sensitive system, it often arises as much from the impression made indirectly on the organs of this system, as on those to which the cause is applied, and sometimes more so. Thus, any cause which suddenly destroys the function of the heart or lungs, at once proves fatal, and the cause of death is simply the loss of a function immediately essential to life; but a loss of function in the intestines produces, not immediate death, but a series of causes of irritation, which exhaust the powers of the sensitive system, and death arises as much from this cause as from loss of function in the seat of the injury. Thus a blow on the stomach may instantly prove fatal by the impression it makes on the vital parts of the brain and spinal marrow without producing any other cause of derangement†; but inflammation of that organ, by the torture it occasions, often exhausts the powers of the sensitive system, before the inflammation has time to run the course that would prove fatal by its effects on the stomach itself.

We observe the same thing in a more remarkable degree where the organ is still less immediately essential to life, and the disease consequently is more protracted. It is in this way that stone in the bladder proves fatal. If such local mischief do not occur as disturbs the usual course of the disease, life terminates in the same way as from torture, only more slowly as the suffering is less severe and continued, that is, in a morbid debility of the powers of the sensitive system, more or less, according to circumstances, affecting the vital parts of the brain and spinal marrow, and the last

* Philosophical Transactions for 1815; and Experimental Inquiry, Part II. chap. ii.
† Ibid.

symptoms, as in cases where the cause of the disease makes its first impression on the sensitive organs themselves, are those of nervous apoplexy.

In this way death from causes of injury, making their impression on the vital organs, often approaches very nearly to the nature of the other forms of death which have been considered; and in almost all instances, with the exception of the most sudden, this is more or less the case; and consequently many of the observations made respecting the other forms of death, apply to the form I am now considering, particularly those relating to the gradual diminution of sensibility and perversion of taste which so generally precede, and more or less reconcile us to death.

I have already had occasion to observe, that even in some protracted cases there is little of this tendency. This, of course, is most apt to happen where the sensitive system is least affected, and therefore where the cause of injury makes its impression on vital organs of little sensibility,—on the lungs, for example, organs of peculiarly dull feeling,—a wise provision, for the air is so variously impregnated, and in so many ways which it is impossible to guard against, that were their sensibility acute, we should be exposed to constant causes of irritation. It is probably from its being so little so that, of all our organs, their sensibility is least apt to be increased by disease, the common effect of continued irritation. Those who have been troubled with carious teeth know how sensible the gums, parts of comparatively dull feeling, often become in disease. Even the most severe inflammation of the lungs may exist without pain, although the difficulty of breathing, cough and fever, which attend it, sometimes exhaust the feelings as much as pain. In its more chronic forms, however, it is often but little distressing even in these ways; and I have seen a few cases of pulmonary consumption, in which the sensibility and relish of life continued so entire, long after the patient was sensible of his approaching end, as to produce a state of mind peculiarly distressing, differing but little from that of those who look forward to what is called a violent death. This, however, is rare. In all serious and particularly tedious illness there is generally sufficient bodily suffering and perversion of taste, more or less, to blunt the sensibility, and in some measure to wean the patient from the love of life; and we generally find the grief and agitation on the part of the relatives, and on that of the patient, a degree of indifference and composure, which those who have only experienced the feelings of healthful vigour are at a loss to comprehend. Even the dread of death at length prepares us for it. The feelings of the criminal who is hanged on the instant are those of horror; of him who has languished in prison, of resignation.

But of whatever kind and degree the previous suffering may be, and by whatever cause produced, the last act of dying, in the common sense of the word, is still but the extinction of the sensibility, and consequently the termination of all suffering; and, as might from its nature have been foretold, so calm in general is this last act, that the most anxious observer often finds it impossible to ascertain the moment at which it takes place.

The circumstance which has given rise to our notions respecting the sufferings of our last moments is, that in certain diseases there is a convulsive action of the muscles at the time at which the sensibility is extinguished. But these are not acts of volition. The laws of our nature tell us that they are not the effects of suffering; and we never see in the patient any indication that he suffers. They are of the same nature with the convulsive motions of the epileptic, of which he is wholly unconscious. Were they indications of a struggle of feeling, necessarily connected with the last act of dying, as has been supposed, they would be a constant symptom; whereas they only occur under certain circumstances of the constitution or the disease. One of the least painful of violent deaths is that from loss of blood; yet here this struggle very uniformly attends the last act of dying, according to the common acceptation of the term; and it is evident that here the sensibility, in consequence of the failure of circulation, is almost extinguished before this involuntary action of the muscles takes place*.

It is generally supposed that the struggle of the criminal after the drop falls is the measure of his sufferings. The most vigorous necessarily suffer most, because in them the sensibility is with most difficulty extinguished; but it is not uniformly in them that this struggle is greatest. We have reason to believe that it is little, if at all, connected with the feelings of the sufferer. All such convulsive motions are of the same nature with what is called *subsultus tendinum*, so apt to occur in fever, even while the sensibility is little, if at all, impaired, but which gives no uneasiness but what arises from the motions of the limbs it occasions.

The causes of disease under various circumstances must act more or less interruptedly. In some cases their operation wholly ceases, and is renewed at intervals, causing the disease to intermit. There is a principle in the animal body on which the cure of all diseases depends, termed by writers the *vis medicatrix*, in consequence of which the more immediate effects of the offending cause are followed by others which tend to counteract them. If the surface of the bowels, for example, be irritated, a more copious secretion of their fluids and an increase of the peristaltic motion are excited, by which the irritation is relieved and the cause of injury expelled; and although there are few cases in which the operation of this power is so simple as in this instance, in all diseases its effects may more or less be observed, and a great part of the object of medical treatment, as far as the nature of the disease is understood, is to assist and

* It may appear at first view that our condition would have been improved had we not been endowed with the sensibility which often renders disease so great an evil; but in the same proportion as our ease would have thus been consulted, our danger would have been increased. It is by the quick sensibility of our frame that we are warned of a thousand dangers, and enabled to guard against them. Such is the imperfection of our present state, that we enjoy few advantages which have not occasionally their accompanying evils. But there is no instance but that of sleep, which is rather an imperfection than a positive evil, in which the evil necessarily exists; and thus we have reason to believe that the sum of enjoyment is the greatest of which that state admits. The species is protected at the expense of the individual.

regulate its operations*. We find even in those diseases which are of the most continued form, partly from its operation and partly from the cause of the disease acting more or less interruptedly, more or less evident remissions. Hence, and from a thousand accidental circumstances which influence the course of disease, and many of which it is impossible to trace, we find in diseases of continuance, that at one time the stimulant, at another the sedative effect prevails. Thus the sufferer appears at one time to be sinking, and at another to revive, without our always being able to trace the cause of such variations. All this the complicated nature of the animal body, and the various ways in which it may be influenced, would lead us to expect. We might also be led to expect that it would sometimes happen that when the excitability is nearly exhausted, such a cause of excitement might under certain circumstances occur as would suddenly exhaust that which still remains, and thus, by causing a sudden but temporary revival, prove the prelude to death. Hence what is termed a lightening before death, on which so many superstitions have been founded. This is seldom strongly marked. That it occasionally is so, we have sufficient evidence, and that it should be so, is perfectly consistent with the laws of the animal economy; but it will appear from what has been said, that, like the convulsive motions I have been considering, it has no essential connexion with the act of dying, and is not the consequence, but the cause, of its immediate approach.

Before I proceed to the last part of the subject, namely, the order in which the nervous and muscular functions cease, on which a very few remarks will be sufficient, I shall shortly recapitulate the leading features of the different forms of death, without recurring to the other parts of the subject, which are too numerous to admit of recapitulation; and make such additional observations as the recapitulation suggests.

WE have seen that the forms of death,—for, as I have already had occasion to observe, the whole operation of the causes of decay in strict language constitutes the act of dying,—may be arranged under five heads.

1. The only natural death, that from old age, where all the powers of life, in consequence of the operation of the agents which excite their organs, gradually decline, and death is only the last sleep, characterized by no peculiarity, in which these powers,

* Here, as in other instances, that imperfection of our present state, which we have reason to believe inseparable from it, appears. Nature, for example, relieves inflammation sometimes by exciting discharges from the inflamed part, sometimes by the process of suppuration; but she still employs the same means, although the effusion or suppuration by which the inflammation is relieved, from the nature or situation of the part affected, generally proves fatal. Such is the case in croup, the disease termed internal water of the head, inflammation of many vital organs, &c. In these cases it is the object of the physician to cure the inflammation by artificial means before it has time to run to such terminations. In other instances, as in some external inflammations, his object is to promote these operations of the *vis medicatrix*, as the least injurious way of removing the disease.

partly from their own decay, and partly from the lessened sensibility increasing the difficulty of restoring the sensitive system, become incapable of this office, in consequence of which the individual awakes no more; for it is to be recollected that it is not in the commencement, but in the progress of the last sleep that what we call death takes place. In its commencement, we have seen, the sleeper may always be roused by stronger stimulants than those which preceded it.

All the other forms of death, it appears from what has been said, may be regarded as more or less violent, some adventitious cause disturbing the natural process. They were divided into two classes; in the one the offending cause makes its impression on the sensitive, in the other, on the vital organs. The former were divided into those cases in which the debility which precedes the total loss of sensibility, arises from the excess of the stimulant operation of the offending cause, and those in which it is the direct effect of that cause; the latter into those causes in which the vital powers fail in consequence of their organs being deprived of the stimulants which excite them, and those in which the offending cause makes its impression on these organs themselves, the power of which, analogous to the operation of the offending cause on the sensitive organs, is destroyed, either by the excess of its stimulant, or its more directly debilitating operation, according to the nature or degree of that cause. Thus are induced,

2. The death which in its nature most nearly resembles the death of old age, that from excessive exhaustion of the sensitive system from the operation of stimulants of greater power than this system can bear, notwithstanding the intervals of such imperfect repose as their continued operation admits of, without the supervention of disease; which, not being capable of relief from the continued action of the vital parts of the brain and spinal marrow, by sympathy spreads to them, the affection of each system increasing that of the other, till all the powers of the sensitive system are destroyed.

3. The death in which disease of the sensitive system arises, not from causes over-exciting, but directly debilitating it; the debility they produce, being of the same nature with that from excessive excitement, and running the same course as in the second stage of the preceding form.

4. The death which arises from the privation of the natural stimulants of the organs of life; and lastly,

5. That which arises from diseased states of those organs, analogous to the states produced in the organs of the sensitive system by the causes which make their impression on them.

IF the foregoing include all the modes of decay, the physiological nature of death in its various forms is referable to very simple principles. In the natural decay the excitability of the organs of both the sensitive and vital systems is gradually impaired by stimulants, which, whether existing within our bodies or making their impression from without, belong to inanimate nature; for it is by the impression of such stimulants alone that the functions of life are maintained. In the different kinds of violent death, with the exception of the death which arises from a failure of the natural stimulants of the vital organs, which is comparatively rare and extremely simple in its nature, we find the excitability of one or both of these systems, or some parts of one or both of them, capable of influencing all the others, more quickly destroyed by the continued operation of causes which either stimulate beyond the limits of health, or, applied beyond the limits of their stimulant operation, destroy the powers of life, either by directly destroying the powers of the sensitive system or depriving it of those powers by which it is maintained. All these causes, it is evident, tend to the same effect, the extinction of the sensibility, which constitutes death according to the common acceptation of the term, the immediate cause of which, therefore, exists in the sensitive parts of the brain and spinal marrow.

Thus it appears that, in every instance,—for it will be found, I believe, that there is no case of death which may not be referred to one of the foregoing heads,—what is called death and the loss of sensibility are one and the same, and therefore that the last act of dying can in no instance be an act of suffering; and this we have seen confirmed by direct observation, as far as the observation of the bystander can confirm it; to which may be added the experience of the sufferer himself, because those who, from submersion or other similar causes, have passed that portion of the act of dying where suffering can alone take place, and who have, as above explained, been in the common sense of the word dead, and in consequence of the degree of vigour still remaining in the vital organs restored by inflating the lungs, declare that they had been sensible of no suffering but such as arises from a less degree of the same cause which in them had wholly extinguished sensibility; an observation well illustrated by the circumstance, that those who are restored by artificial respiration, and could not have returned to life without this aid, and those whose breathing, not having been long enough suspended wholly to destroy the sensibility, and who consequently, although to all appearance equally insensible, in a short time after the cause is removed, breathe spontaneously, give precisely the same account of their sufferings.

In those in whom the sensibility has been extinguished by submersion, it is in the first part of the process by which they recover, not in the last part of that by which they lose it, that they suffer, which it is not difficult to explain.

In the latter the sensibility is almost lost before it is wholly so. The apoplectic who has still feeling enough to breathe, who may still be roused to remove the extreme cause of suffering which the want of a supply of air in the lungs occasions, may be insensible to all other causes of excitement; for in proportion to the immediate importance of that supply, is the feeling which impels us to obtain it. We have instances of the hand being voluntarily held in the fire; but none, of the breathing voluntarily stopped till the lungs were injured. The circumstance of the breathing, independently of artificial means, being finally lost, is a proof that the sensibility is wholly extinguished; and as its extinction in such a case must be more or less gradual, the capability of acute suffering, it is evident, must be lost some time before the period at which the want of air in the lungs cannot even be felt.

In the act of recovery, on the other hand, the sensibility necessarily begins to revive before the vital organs perfectly recover their functions after so severe a shock. The sensitive, on its revival, thus finds the vital system still more or less in a state of disease, to which the former, as its powers increase, is every moment becoming more sensible; for while the powers of both remain, all derangement of the vital is felt by the sensitive system; a wise provision, by which we are warned to guard against causes of danger confined to the former.

IT will readily occur from what has been said, to those whom I have the honour to address, that under certain circumstances more than one of the preceding forms of death may concur. The first indeed, the death of old age, may be regarded as so far a combination of more than one of the other forms, that the cause makes its impression on both the sensitive and vital systems; but its effects on both, as appears from all that has been said, are essentially different from those of disease.

In certain cases the cause of disease makes its impression on both systems, and then more than one of the last four forms concur. This, I have already had occasion to point out, necessarily happens from mechanical injury of considerable portions either of the brain or spinal marrow. When both systems are directly impressed by the cause of the disease, which is comparatively rare, it produces, as follows from what has been said, a combination of the third and fifth, or second and fifth forms, according as its effects are more or less sudden and severe.

SUCH in different cases, is the varied course of our decay previous to the moment at which the sensibility is extinguished, emphatically called that of death, because it completes the decay of the sensorial powers, and leaves us only those which we possess in common with the vegetable world; for the vegetable, like the animal, can convey its juices, form its secreted fluids, and in some instances move its limbs, if proper stimulants be applied; an additional argument, it might be shown, if any were required, for all such functions being the effects of inanimate agents acting on living parts.

After the removal of the sensorial functions, none remain to us but such as are maintained by the immediate action of those agents. Our bodies are hastening to be mingled with the matter of inanimate nature. They retain only those powers which immediately depend on its agents, and these are rapidly failing, because, for reasons which have been pointed out at length*, the due application of those agents in the more perfect animals cannot long survive the loss of the sensorial powers.

The power of organizing the elements of inanimate nature belongs, and some have

* Philosophical Transactions for 1829; and Experimental Inquiry, Part II. chap. xi.

supposed exclusively, to the vegetable world; but as we see plants, the mushroom tribe, possessed of no organizing power, and therefore, like animals, nourished only by matter already organized, some of the lower species of animals, on the other hand, seem to possess this power. Thus, it would appear that there is a class of animals and of plants in which the animal and vegetable, in this essential respect, exchange their natures. As the animal becomes imperfect, and approaches the nature of the vegetable, the sensorial powers dwindle, and the lowest animals appear to extract their nutriment from air and water, which, being generally diffused, are at hand, and consequently obtained without any sensible effort on the part of the animal. His life, therefore, although not independent of the external world, is, like that of the vegetable, independent of any act of volition. As we rise in the scale of animals, the sensorial powers increase, and, in the same proportion, become more essential to existence. From those animals which obtain food without any act of volition, we come to those who can only obtain it by such an act, but who still without any act of this kind obtain the influence of the air, yet more immediately necessary to their existence. We arrive at length at the most perfect class, which can neither obtain food nor air, except by an act of the sensorium. In them the sensorial power is as necessary for the inhalation of the air, as the ingestion of the food. When sensation ceases, they as certainly cease to breathe as they cease to eat. Thus it is that in this class of animals the due application of the inanimate agents on which life depends, cannot long survive the loss of the sensorial functions.

AS we have been enabled, by the aid of the experiments referred to in the foregoing paper, to trace the steps by which the sensibility in the various forms of death is extinguished, that is, of our decay up to that moment which has for very evident reasons obtained the name of death, by the same means we may with more ease trace the steps by which the remaining powers of life are extinguished.

AS the powers of life fail, we have seen, the first functions which cease are those which wholly depend on these powers. The others, being the results of inanimate agents acting on vital parts, continue as long as those agents are supplied, for the purpose of exciting their organs. The first of these powers which fails is evidently the power of the capillary vessels, because their function continues as long as any blood can be supplied to them from the larger arteries. The circumstance of the action of the capillaries only ceasing when the larger arteries are empty, affords a proof that the assimilating processes, without which their power would fail, are still more or less in a state of activity. These processes, we have seen, are immediately dependent on the vital parts of the brain and spinal marrow. The due mechanism of every part, it appears from direct experiment, depends on the action on the blood of the agent they supply. When the capillaries can no longer supply the blood on which it acts, it is evident that the functions of this agent must cease, and consequently that those parts of the brain and spinal marrow, by which it is supplied, being thus deranged, their powers must cease also*. These are the last of the powers of life which fail, and thus the body of the more perfect animal is left subject to the laws of inanimate matter. The first functions which cease are those of the sensitive parts of the brain and spinal marrow; the last, those of the vital parts of these organs.

* In the first of any of the more perfect animals, the nervous influence must have been supplied from without, or the rudiments of the organs which supply it and those of the sanguiferous system must have been simultaneous creations, because neither is capable of producing the other, the functions of each being inseparable from those of the other. We have seen that it is a necessary inference from direct experiment, that while the vital principle is unimpaired, the powers of circulation, provided the blood be duly exposed to the influence of the air, are capable, with the aid of voltaic electricity, of all the assimilating functions. No other powers are required for the maintenance and growth of the animal body.

We have reason to believe that the vital parts of the brain and spinal marrow may, like the lungs, be inactive in the fœtal state, some other means in this state being employed to supply the agent which, after birth, can only be supplied by them. Well grown fœtuses, perfect in all their other parts, have been born without either brain or spinal marrow. The growth of such fœtuses must depend on the same causes as the growth of other monstrous productions in the uterus, namely, as far as relates to the brain and spinal marrow, on the powers of the mother alone, how applied it is impossible to say.

XI. *An Account of a Concave Achromatic Glass Lens, as adapted to the Wired Micrometer when applied to a Telescope, which has the property of increasing the magnifying power of the Telescope without increasing the diameter of the Micrometer Wires. By* George Dollond, *F.R.S. &c.*

Received February 19,—Read February 27, 1834.

WHEN the application of any optical or other arrangement is found to be useful, a correct statement of the manner in which it became so is essentially requisite, in order that each person who may have had a share in bringing it forward may have his due proportion of the merit.

The achromatic lens which I have applied to the wired micrometer, and which has been found to produce such very considerable advantages to that instrument, arose out of a trial that was made at the suggestion of Professor Barlow, for the purpose of improving the chromatic aberrations which affected the field of the eye-glasses applied to the telescope invented by that gentleman with a fluid correcting lens, and made by myself for the Royal Society.

The lens in question not being found so effective for his purpose as he expected, was laid aside. It has now been introduced for my purpose, and is made, with some trifling variations, in accordance with his calculations.

The interposition of a concave lens between the object-glass and the eye-glass of a telescope has been generally known by opticians to produce an increase of the magnifying power, in proportion to its focal length and distance from the object-glass: also that a convex lens, if so applied, would diminish the power.

Except in the Huygenian eye-tube, I am not aware that either of these lenses have been so applied generally, it having been considered that their introduction would materially diminish the light proceeding from the object-glass of the telescope, and also, by deranging the aberrations, disturb the image.

In the lens I am now describing, these errors are very materially obviated, owing to its being constructed upon achromatic principles*, by which the magnifying power of the telescope is increased in a twofold ratio, without so much diminution of light as is produced by the introduction of a simple lens.

For example, if the eye-glasses in the original arrangement of the telescope gave 100 of magnifying power, the same eye-glasses with the new lens, if I may so term it, will give 200, and the light will be fully equal to that power if obtained by the usual means. The field of view will also be considerably flattened.

* The discovery of John Dollond, F.R.S., in the year 1758.

Thus it will be seen that we have the advantage of using longer eye-glasses with an extension of power, whereby the wires or spiderwebs of the micrometer are not increased in diameter, a very essential advantage when observing minute double stars; nor is the eye of the observer so much distressed as when the magnifying power is obtained by shortening the focal lengths of the eye-glasses.

The advantages of this improvement having been shown by the foregoing introduction, I will now proceed to give an account of the causes which led to its being applied to the micrometer, and the result of its application.

The Rev. W. R. Dawes of Ormskirk, a gentleman pursuing practical astronomy with great zeal and perseverance, and to whom the public are already much indebted for several valuable communications, being desirous of carrying his measurements, &c., of the double and revolving stars, to a greater extent than the powers of his micrometer then allowed, applied to me to construct for him an arrangement of eye-glasses that would increase the magnifying power of his telescope without increasing the apparent diameter of the spiderwebs in his micrometer, or interfering with the mode of illumination. Several combinations were tried without success, when it occurred to me that the achromatic concave lens, which had been decided by Mr. Barlow to be of no use for his purpose, might accomplish what was required.

The result I will now state from a letter I soon after received from Mr. Dawes, to whose micrometer this improvement had been applied.

"Ormskirk, March 14, 1833.

"My dear Sir,—You will doubtless be surprised at not receiving from me any account of the performance of your scheme for the improvement of the achromatic telescope.

"My general opinion of your improvement is, that it is, for the purpose it is designed to answer, as useful as it is elegant.

"By a careful determination of the value of the micrometer divisions, I find the magnifying power of any eye-tube is increased in the proportion of 2·1068 to 1: each part originally = 0″·555922 is now = 0″·263867. To obtain the magnifying powers of the eye-tubes, I content myself with multiplying the original powers by 2·1. But I will detail a few particulars noted in my journal on the subject. I have thus set down the advantages of the additional lens.

"1st. The micrometer threads are only half the thickness, with the same magnifying power on the object; small stars are therefore neither obliterated nor distorted.

"2nd. The parallel threads are both very nearly in focus with any power up to 600; before, only up to 285 (the same eye-piece).

"3rd. The value of the micrometer divisions is less than one half its former amount, permitting a proportionally fine motion in measuring the distances of delicate objects.

"4th. A much greater extent of the field being flat, and the threads distinct further from the centre; of great importance in accurately determining the zero of position by the passage of a star along the thread.

"5th. The definition of the stars seems quite as good; and the false light does not appear to be increased, or the regularity of its distribution affected. The discs of the stars seem in fact to be, if anything, rather smaller and cleaner with the concave. Perhaps their brightness might be perceived to be a trifle less; but even this is doubtful. See below.

"6th. The shallower eye-glasses are much more easily cleaned; of great importance in high powers.

"7th. The prism can be conveniently applied to all powers as high as 600; before, only to 285. This prism is of essential utility in other respects besides facilitating zenith observations; and it is no small improvement that its use is thus extended.

"From the performance of this additional lens, it is evidently a perfect production. Against all the advantages detailed above, the trifling addition to the length of the telescope is not to be mentioned; indeed it is to me surprising that so great an effect should be produced with so minute an increase of focus.

"As a severe trial of the difference in illuminating power, I have examined Saturn's satellites, and *κ* Geminorum. I could discover no decided difference in the apparent brightness of the satellites, allowance being made for the difference of power employed. It happens awkwardly, that among moderate powers, fit for planets, none coincide sufficiently with and without the concave lens. The nearest I can get are a negative 195 with the new lens, and a double convex 208 without it: with these, little difference in brightness; but the planet might be a trifle sharper with the latter. Have you ever seen the minute companion of *κ* Geminorum? It is the finest test of a five-feet achromatic I have yet seen: distance about 6″. I saw it steadily with negative 140 without the concave, and quite as well with negative 116 with it; but these powers are not near enough to each other.

"For tolerably bright stars, I have on the micrometer 475 with the concave lens, and without it 480; also 600 with, and 625 without. These afford an excellent comparison. Vision appears to me equally good with both; and the fineness of the micrometer threads leads me always to prefer the new arrangement, as I can then use the same eye-piece generally for the distances, as I use for the positions.

"In clear weather, I always use 600 for stars of the fifth magnitude and upwards, and sometimes even of the sixth; and last night I got a very good set of positions of Castor with a power of 1010, with which the discs were occasionally perfectly well formed, though of course not so sharply defined. I also obtained last night very satisfactory measures of *ζ* Cancri, certainly one of the most difficult stars for a telescope of five feet. That you may judge for yourself of the way in which it was seen, I will detail here my measures, exactly transcribed from my observation paper.

Power.	Position.		
600	336° 56′	Mean =	335° 28′
Stars placed between the	335 50	*z* =	− 271 26
parallel wires thus:	336 22		
	335 8		64 2 nf.
	335 7		
	334 1	=	25 58 from N.
	334 49		

Power.		Distance. P. decl.		P. decl.	
295	+	4·0	—	4·2	Mean of all = $4^{p}\cdot 33$, which = $1''\cdot 143$.
		4·2		5·0	
		4·3		4·4	
		4·3		4·2	
		4·4		4·3	

N.B. The plus and minus measures are taken alternately, and not one rejected or altered.

"Though 600 did well for the angles, the stars were not sharp enough with that high power for accurate bisection. The parallel threads are sweetly fine and sharp with 295 (formerly 140). Indeed, this is a very efficient and generally useful power.

"Thus you will see, my dear Sir, that a long-lamented desideratum has been efficiently supplied by your elegant invention. I have thus nearly all the advantages, and none of the disadvantages, of a ten-feet telescope of the same aperture.

"I remain, my dear Sir,

"Yours faithfully,

(Signed) "W. R. Dawes."

I shall now introduce some extracts from a letter I have since received from Professor Barlow, in which his formulæ for constructing the lens are given.

"Woolwich, February 1st, 1834.

"Dear Sir,—In answer to your letter of January 30th, 1834, I will endeavour to state the views which led to my requesting you to make the achromatic concave lens you allude to, and explain the formulæ and principles on which I computed the curves.

"First, with regard to my views. Every one is aware of the ease and comfort of observing objects in a long telescope in comparison with viewing the same in a short one, supposing the powers equal in both instruments; and my object was to produce this effect by taking up the rays before they arrived at their focus, extending them to a greater distance, and thereby increasing the size of the image, which is of course the same as increasing the length of the telescope in a like proportion.

"In order to render this lens achromatic, it is only necessary to make the foci of the lenses proportional to their dispersive powers, as in the object-glass itself; except that here the crown lens must be made concave and the flint lens convex.

"Suppose, for example, the compound lens is to be placed at a distance, d, from the focus, and that the image is to be doubled, then the focal length of the compound lens must be $2\,d$; for $\frac{1}{d} - \frac{1}{2\,d} = \frac{1}{2\,d}$: again, δ being the dispersive ratio, we have

$$f = 2\,d\,(1 - \delta) = \text{focal length of the crown lens,}$$

$$f' = \frac{2\,d\,(1 - \delta)}{\delta} = \text{focal length of the flint lens.}$$

"To correct the spherical aberration requires more labour. Let us suppose the crown lens placed towards the object-glass. Assume its radii r, r', or rather their ratio $\frac{r}{r'} = q$, at pleasure, and compute its aberration for rays converging to the distance d, which may be done by the following formulæ, a being the index.

"Find

$$d' = \frac{(a+1)}{a\,d - r}\,d\,r,\quad b = \frac{a}{a+1},\quad \frac{d}{r'} = c,\quad \frac{d'}{r'} = c',\ \text{and}\ \frac{r}{r'} = q;$$

then the aberration will be

$$\text{aberration} = \left\{ \frac{(c+q)^2}{(a\,c-q)^2} \times \frac{c + (a+2)\,q}{c\,(a\,c' + a + 1)^2} + \frac{(c'+1)^2}{(b\,c'+1)^2} \times \frac{(c'+2-b)\,q}{c'} \right\} \times \frac{a}{2\,r}\ *$$

"Let the quantity when found be called m, then for the flint lens proceed as below, the radii being r'', r''', the latter towards the eye, and the index a'.

"Find

$$d' = \frac{(a'+1)\,2\,d\,r'''}{2\,a'\,d - r'''},\quad b = \frac{a'}{a'+1},\quad \frac{2\,d}{r'''} = c,\quad \frac{d'}{r'''} = c',\ \text{and}\ \frac{r''}{r'''} = q.$$

"Then find r''', r'' and q, such, that

$$\left\{ \frac{(c+q)^2}{(a'\,c-q)^2} \times \frac{c + (a+2)\,q}{c\,(a'\,c' + a' + 1)^2} + \frac{(c'+1)^2}{(b\,c'+1)^2} \times \frac{(c'+2-b)\,q}{c'} \right\} \times \frac{a'}{2\,r'''} = m,$$

and the resulting curves will be those required.

"To produce this latter equality is the only difficulty in the operation, and to treat it as a common equation would lead to immense labour. I have therefore always contented myself with pursuing the more simple method of trial and error, its facility fully compensating, in my mind, for its want of scientific elegance.

"It may be proper to observe, that I proposed the lens to double the magnifying power, and the curves were computed accordingly, but the formulæ will of course apply to magnifying in any ratio.

"I hope this explanation will be found intelligible, and I am pleased to find my proposition has been found useful.

"I remain, dear Sir,

"Yours very truly,

(Signed) "PETER BARLOW."

I have only to add to the foregoing relation of facts, that I do hope they will prove satisfactory to those friends who have felt so much interested upon the subject as to induce me to write this Paper, it not being my wish to take credit to myself for anything like an invention, but merely for the application of the lens to the micrometer, as I am fully convinced that a concave lens, either simple or achromatic, was never so applied before.

* See Philosophical Transactions, 1827, p. 244.

February 17th, 1834.

XII. *On the Principle of Construction and general Application of the Negative Achromatic Lens to Telescopes and Eyepieces of every description.* By Peter Barlow, *Esq. F.R.S. &c.*

Received May 20,—Read May 29, 1834.

THE great advantage which has attended Mr. Dollond's ingenious application of the negative achromatic lens to the micrometer eyepiece, seems to make it desirable that the principles on which that lens is constructed, and its general application, should be more fully illustrated than is done in the short extract made from my letter to Mr. Dollond, and given by him in his recent paper in the Philosophical Transactions.

In my original fluid telescope, the negative lens was employed for the double purpose of lengthening out the focus and correcting the colour of the front lens; and the great advantage of the lengthening principle was manifested by the high penetrating power of the instrument in the centre of the field. Unfortunately, however, the perfect part of this was very limited, so that when Mr. Dollond constructed the second telescope for the Royal Society, I gave up this advantage for the sake of enlarging the field; but I found that by this means much of the penetrating power of the former telescope was lost; for although I had the same aperture, many small stars which were before very perspicuous were in this instrument seen only with difficulty and under advantageous circumstances of weather, absence of moonlight, &c.

This led me to consider whether it would not be possible to retain the advantages I had obtained in the new instrument, and to restore the power of the other principle (that of penetration) by an artificial lengthening of the focus; but as the rays were now as nearly achromatic as I could make them, it was necessary in this case to have the lengthening lens also achromatic. I had no authority from the Royal Society to make any collateral experiment, but having mentioned my idea to Mr. Dollond, he very readily undertook to construct the small lens, and it was accordingly made and tried; but owing, as I now imagine, to the imperfect means I had of fixing it, its advantages were not perceived. It was laid aside, was not referred to in my paper, and would most likely have been altogether lost sight of, had it not occurred again to Mr. Dollond to try its effect on the micrometer eyepiece for the Rev. Mr. Dawes. It is therefore to Mr. Dollond we are indebted for snatching this lens from the oblivion into which I had allowed it to fall.

It must not, however, be understood that it is only applicable to this eyepiece, for it may be applied to any eyepiece, positive or negative, or to the erecting eyepiece, or indeed to any telescope of fluid or glass, or to refractors; for it is, in fact, not a part of the eyepiece, but of the telescope itself: and it is for this reason its advantages are so conspicuous in the application Mr. Dollond has so ingeniously made of it; for by lengthening the focus before the rays arrive at the eyepiece, the image is magnified, while the wires retain only their original size.

Having thus shown the origin of the negative achromatic lens, I may be allowed to state the motives and reasonings which guided me in the computation of the curves, and what appears to me to constitute the advantages it is found to possess. Notwithstanding the extreme difficulty there is in constructing an achromatic object-glass, yet with perfect materials the difficulty is only in the manipulation; and this being overcome, there is not so great a natural impediment to perfection in this part as in the eyepiece,—for we know that it is impossible to make a perfect positive power *; and if the same absolute impediment does not occur in the negative eyepiece, yet the thicknesses of the lenses render the task very difficult, not only to execute, but to compute the proper curvatures to ensure perfection. If this view of the case be correct, we see at once the advantage of magnifying the object as much as possible before we apply the eyepiece; and this, in fact, is the whole theory of the negative achromatic lens: that is, supposing the rays to be rendered achromatic by the object-glass, they are intercepted by the negative lens before they cross, which, being itself also achromatic, extends them to any length, and thereby produces the effect of lengthening the whole focus in the same proportion, and consequently the power of the telescope, the eyepiece remaining unaltered.

In the conclusion of my letter to Mr. Dollond, I have offered a suggestion, whether it would not be possible to retain the same eyepiece for all powers by changing only the negative lens. This must of course, as he has observed, change the scale of the micrometer; but this being changed, by so adapting the lens as to render the powers simple multiples of each other, would not, I conceive, be attended with any disadvantage. In other cases, where a micrometer is not employed, and where the utmost perfection is not looked for, every variety of power may be produced by simply moving the negative lens nearer to or further from the eyepiece; for both the object-glass and lengthening lens being achromatic, the image, wherever the focus is formed, will be achromatic also; and the spherical aberration of the lens is so inconsiderable, as only to be discovered by the most perfect eye, when removed from that point in which it is computed to be perfectly corrected. The negative lens is therefore admirably suited for day telescopes with correcting eyepieces, as also for astronomical telescopes where the micrometer is not applied; for by giving an adjustment to the lengthening lens, the power may be changed in any proportion, even without removing the eye or losing sight of the object. I have no doubt that these and other applications of the lengthening lens will be made, and amongst others, I am willing to hope that

* See Professor Airy on the Eyepieces of Telescopes, Transactions of the Cambridge Philosophical Society, vol. iii.

it is not impossible the negative secondary spectrum of this lens may, by careful experiment, be so proportioned as in part to counteract the positive secondary spectrum of the object-glass so as to render the image more nearly aplanatic; some experiments, at all events, directed to this inquiry are very desirable.

I have already, in my letter to Mr. Dollond, given the formulæ for computing the proper curves according to any distance between the focus and the lengthening lens, and for magnifying the image in any required proportion; but unfortunately the calculation is very laborious, and difficult to be rendered general, or tabulated for general practice. I would therefore recommend opticians to use the same curves as are commonly adopted for short telescopes of six, eight, or ten inches, making those of the plate or crown concave instead of convex, and those of the flint convex instead of concave, turning the plate towards the object-glass and the flint towards the eyepiece, which will in general bring out a close approximation for spherical aberration, and the colour will be sure to be corrected. Starting from this point, practical skill will readily supply the means of making corrections, if any such should be found necessary after all has been done that can be done by changing the position of the lens as regards its distance from the eyepiece. I hope these additional directions for constructing and applying the lengthening lens will not be thought superfluous, nor undeserving the attention of practical opticians.

XIII. *Some Suggestions relative to the best Method of employing the New Zenith Telescope lately erected at the Royal Observatory. By* John Pond, *Esq. A.R. F.R.S.*

Received March 11,—Read March 13, 1834.

The erection of a zenith telescope of twenty-five feet focal length at the Greenwich Observatory was determined upon by the Visitors in the year 1815, for the purpose of measuring the zenith distance of γ Draconis with greater accuracy than could be effected by any instrument then existing at the Observatory.

This instrument was fixed in its place by Messrs. Troughton and Simms, in July 1833; and although when first erected it was not complete in some of its minute parts, yet, by various improvements which have been made, it is now approaching to a perfect state.

During the course of the last summer I made a great many observations with it, with the view of determining the most advantageous method of using it. To describe this method, and not the instrument itself, is the object of the present brief communication.

Among various experiments that occurred to me, I was led to a mode of observing which has not, that I am aware of, been suggested or put in practice by any other observer; and which appears to me to possess advantages sufficient to justify my making it the subject of a separate communication.

These relate, not only to the determination of the zenith distance of γ Draconis, (for which purpose the instrument was especially constructed,) but to the measurement of the minute variations from which the equations of aberration, parallax, nutation, and others employed in the reduction of the star, are to be elicited.

I shall first treat the subject with reference to the zenith distance of γ Draconis, presuming that the usual mode of using a zenith sector, altitude and azimuth instrument, or other instrument constructed on the same principle, is well known.

If the star be observed on one night with the instrument facing the east, and on the next or any subsequent night with the instrument turned half round, and facing the west, the double zenith distance will be obtained, on the supposition that the instrument has continued identical during the interval.

If, however, either by accident or design, the instrument should have suffered any change between the two observations, it is evident that the result will not give the measure of the required distance. I am now to show how my mode of observation is adapted to overcome this difficulty.

It so happens, that a small star of about the fifth magnitude, having nearly the same zenith distance towards the south that γ Draconis has towards the north, passes the meridian between 20 and 30 minutes in time after it. It is the different modes of employing this star in combination with γ Draconis, as a means of determining the various smaller equations, which I now wish to explain.

The angular distance between the two stars will be determined with this instrument in the usual manner, by observing them on the same night, and in the same position of the instrument; which distance in this case is the sum of the zenith distances of the two stars: but if, on the next or some following night, γ Draconis be observed, and after its passage the instrument be turned half round, and the other star observed, then the difference of the measure, as read on the micrometer, will be the difference of the zenith distances of the two stars. Thus, the sum being ascertained on one night, and the difference on another, these sums and differences will be independent of any change that may happen to the instrument from one night's observation to another; and the zenith distance of each star respectively may be deduced from these data. Whatever may be the superiority of this method of observation in ascertaining the zenith distance of the principal star, it is inconsiderable compared with the powerful assistance it affords in determining, with almost unlimited precision, the value of the small equations which necessarily become the subject of investigation.

Let it be supposed that the two stars have precisely the same zenith distance, the one to the north and the other to the south, then it is evident that if after the observation of one the instrument be turned half round, the micrometer wire will be placed in the exact position for bisecting the second star in its passage; but if the two stars have not exactly the same zenith distance, the micrometer wire will require a corresponding alteration. The distance between the two positions of the wire I call the subsidiary angle. It is to the properties of this angle that I wish to direct attention.

Whoever considers the nature of this angle, will perceive that it is measured by a very small motion of a micrometer screw, and therefore may be obtained with great precision: moreover, that any equation which may become the subject of consideration will be doubled in its effect on this subsidiary angle, and quadrupled when each star is affected equally by the same equation*.

This property of the subsidiary angle may be illustrated by observing, that the new instrument stands about seventy feet north of the principal meridian instruments of the Observatory. This produces in the zenith distance of each star a corresponding variation of about three quarters of a second; but the subsidiary angle will be altered by double that quantity, or a second and a half. It is probable that this

* Suppose the subsidiary angle equal $1' 00''$ when the aberration is nothing, and that the maximum of this equation is equal to $20''\cdot5$, then the extremes of the subsidiary angle will be $0' 19''$ and $1' 41''$, the difference of which is $1' 22''$, or quadruple of $20''\cdot5$, the equation to be investigated.

property may at some future period be applied with advantage in investigations made with moveable zenith instruments.

For the present purpose it is only essential to remark, that for the investigation of these small equations it is by no means required to have determined either the exact zenith distance of either star, or the exact difference of their zenith distances, or the absolute magnitude of this subsidiary angle, its variation from time to time being the only important object of research.

It is the more necessary to keep this in mind, as occasions may arise when it may be found advisable to omit altogether the investigation of the real zenith distance, and to confine the attention to the variations only of this small angle.

The improving performance of the zenith telescope leads me to hope that ere long I shall be able to illustrate the principle of the method of observation I have described, by a series of observations made with it; and should I not be disappointed in my expectations, the instrument will, I am of opinion, be found to rank among the most important in the Observatory.

The principles above explained apply with equal correctness to altitude and azimuth instruments, and may be advantageously adopted in their use.

Example.

Date.	Observed Subsidiary Angle.	Equations.	Reduced Subsidiary Angle.
1833. July 26.	1′ 26″·61	26″·19	1′ 0″·42
August 1.	1 30·99	29·13	1 0·86
3.	1 30·85	30·05	1 0·80
4.	1 31·26	30·50	1 0·75
11.	1 34·46	33·46	1 1·00
13.	1 34·94	34·24	1 0·70
14.	1 35·60	34·61	1 0·99
Mean			= 1 0·79
The angular distance between the two stars, or sum of their zenith distances			= 3 3·53
Sum............			= 4 4·32
Half the sum, or zenith distance of γ Draconis			= 2 2·16
Difference			= 2 2·74
Half the difference, or zenith distance of the small star			= 1 1·37

The angular distance between the two stars, or sum of their zenith distances, was obtained as follows :

				′	″
1833.	August	23.	....	3	3·11
		25.	....	3	3·39
		26.	....	3	3·87
	September	4.	....	3	3·41
		5.	....	3	3·60
		6.	....	3	3·68
		7.	....	3	3·65
	Mean		....	3	3·53

This last angular distance remains nearly constant throughout the year, except in the case of a parallax supposed greater in the one star than in the other, the rest of the equations being nearly the same for each star.

METEOROLOGICAL JOURNAL,

KEPT BY THE ASSISTANT SECRETARY

AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF

THE PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL FOR JULY, 1833.

1833. July.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	External Thermometer. Fahrenheit. 3 P.M.	External Thermometer. Self-registering. Lowest.	External Thermometer. Self-registering. Highest.	Rain, in Inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
☽ 1	29.869	75.5	29.835	69.9	47	62.2	61.6	53.7	66.6	0.050	SW	A.M. Fine and clear. P.M. Heavy thunder showers, with hail and brisk wind.
○ ♂ 2	29.953	72.4	29.968	68.7	47	61.2	65.0	47.3	67.0	0.272	W	Fine and clear—light clouds.—Evening, very calm.
☿ 3	30.041	69.8	30.071	68.2	47	63.2	64.3	50.2	66.7		W	Fair—cloudy and overcast.—Evening, fine.
♃ 4	30.204	73.6	30.174	70.2	48	65.8	72.7	51.7	73.7	0.094	SSW	Fine.—A.M. Cloudless. P.M. Cloudy.
♀ 5	30.171	67.3	30.095	70.4	56	63.9	71.0	56.6	73.3	0.011	SSW	Fine—lightly cloudy.
♄ 6	29.867	68.7	29.775	70.4	55	68.9	71.5	55.3	73.8		E	Fine.—A.M. Light high clouds. P.M. Cloudless.
☉ 7	29.657	68.7	29.697	70.9	56	67.5	67.8	61.3	69.7		E	Lightly overcast.—Light rain, P.M.
☽ 8	29.827	66.2	29.893	68.7	57	61.6	66.2	58.3	68.6	0.105	W	Overcast—showery.—Light breeze at night.
♂ 9	30.116	65.2	30.120	68.4	52	60.2	63.7	54.8	65.8	0.400	N	Cloudy—light unsteady airs.—Evening, fine and clear.
☿ 10	30.053	70.7	29.991	70.3	55	65.0	73.2	52.7	73.4		NNW	Fine—lightly cloudy.
♃ 11	29.950	68.6	29.922	71.3	60	64.5	68.0	57.6	71.9		NNW	Lightly cloudy and overcast.—Light shower, P.M.
♀ 12	29.867	65.3	29.838	67.0	53	58.5	63.6	55.6	64.5		E	Overcast—light wind.
♄ 13	29.933	62.4	29.970	66.0	51	56.7	61.1	52.9	61.7		N	Overcast—light wind.
☉ 14	30.012	65.6	29.968	66.7	53	59.8	65.2	55.2	70.4		N	Overcast.
☽ 15	30.031	73.0	30.073	68.9	56	63.0	69.7	58.3	71.4		SE	A.M. Cloudy. P.M. Fine—light clouds and wind.
♂ 16	30.232	72.3	30.223	70.7	54	65.2	73.6	56.0	74.2		N	Fine and cloudless.
● ☿ 17	30.241	71.6	30.220	73.2	62	69.0	78.0	58.6	80.4		SW	A.M. Fine. P.M. Cloudy.
♃ 18	30.184	71.0	30.109	74.5	61	70.5	78.4	63.7	80.4		S	Lightly cloudy.—Fine, P.M.
♀ 19	29.905	71.1	29.802	73.2	58	66.5	70.6	62.8	74.8		SW	A.M. Fine. P.M. Heavy rain at 2¼ h.
♄ 20	29.665	73.3	29.660	73.6	58	67.2	68.0	60.0	71.4		SW	Cloudy.—Light wind and rain, P.M.
☉ 21	29.778	70.6	29.821	70.7	53	62.1	64.4	53.3	66.3	0.092	W	A.M. Cloudy. P.M. Fine and clear.
☽ 22	29.827	65.7	29.693	68.6	58	59.0	67.8	52.2	69.7	0.161	SSW	A.M. Rain. P.M. Cloudy.—Evening, fine.
♂ 23	29.769	68.6	29.682	69.8	57	63.9	62.1	57.3	70.7	0.200	WSW	Overcast.—Heavy shower, P.M.—Evening clear.
☿ 24	30.065	70.7	30.108	69.6	50	63.3	67.2	51.4	67.8	0.297	NNW	Fine and clear—light clouds.
♃ 25	30.307	67.8	30.307	69.8	57	63.1	70.3	53.1	71.8		SW	Overcast.—Evening, fine and clear.
♀ 26	30.339	71.3	30.279	70.7	50	65.2	73.9	53.3	76.3		WSW	Fine and nearly cloudless.
♄ 27	30.301	72.4	30.283	73.9	63	71.4	79.6	63·2	80.5		W	Fine.—A.M. Light high clouds. P.M. Nearly cloudless.
☉ 28	30.326	73.9	30.301	75.6	64	71.7	75.3	64.7	78.4		NNE	A.M. Overcast. P.M. Fine and clear.
☽ 29	30.347	73.6	30.324	75.1	59	67.3	74.3	58.7	75.7		E	A.M. Light soft clouds. P.M. Clear and cloudless—light breeze.
♂ 30	30.461	70.4	30.455	73.0	58	61.0	67.2	56.3	70.7		N	Overcast—light wind.—Evening, clear.
○ ☿ 31	30.434	66.2	30.366	70.6	54	59.6	68.6	53.3	68.8		NNE	Fine—lightly cloudy.
	Mean 30.056	Mean 69.8	Mean 30.033	Mean 70.6	Mean 55.0	Mean 64.1	Mean 69.2	Mean 56.1	Mean 71.5	Sum 1.682		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. .... 9 A.M. 29.942 3 P.M. 29.916

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge........................ = 83 feet 2¼ in.

———————— above the mean level of the Sea (presumed about) .... = 95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House........................... = 79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR AUGUST, 1833.

1833. August.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	External Thermometer. Fahrenheit. 3 P.M.	External Thermometer. Self-registering. Lowest.	External Thermometer. Self-registering. Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
♃ 1	30.392	65.7	30.404	70.2	50	61.4	65.3	57.3	66.4		NNE	Cloudy—light wind.
♀ 2	30.378	63.3	30.352	67.2	54	57.6	63.3	53.5	64.5		NNE	Overcast—light wind.—Light rain, early A.M.
♄ 3	30.301	63.3	30.314	67.4	54	60.0	64.2	54.8	64.7		N	Cloudy—light wind.
☉ 4	30.366	66.3	30.297	66.9	43	59.4	65.1	48.7	68.3		N	Fine and clear—light clouds.
☽ 5	30.111	64.4	30.087	68.0	51	63.0	67.4	51.7	68.2		NNW	A.M. Lightly cloudy. P.M. Fine and clear.—Shower at night.
♂ 6	30.172	66.3	30.150	67.4	47	58.8	65.3	49.4	66.7		NNE	Fine and clear—light clouds.
☿ 7	30.141	64.5	30.084	66.8	45	58.4	66.6	48.3	68.7		N	Fine.—A.M. Nearly cloudless. P.M. Cloudy—light wind.
♃ 8	30.091	66.3	30.071	67.6	50	62.2	67.2	52.8	69.7		W	A.M. Fine and cloudless. P.M. Cloudy. Evening, lightly cloudy—calm.
♀ 9	30.063	67.3	30.012	68.8	53	65.8	69.5	58.6	73.7		SSW	A.M. Fine—nearly cloudless. P.M. Lightly overcast.
♄ 10	30.031	67.3	29.978	69.7	55	63.8	70.5	55.2	73.6		W	Fine—light broken clouds.—Heavy rain at 6 P.M.
☉ 11	30.002	67.3	30.030	69.8	58	63.7	67.7	51.2	70.0	0.078	WSW	Fine—lightly cloudy.
☽ 12	30.210	66.2	30.154	68.7	49	59.7	67.0	49.8	68.3		W	Fine—light clouds—calm.
♂ 13	29.827	62.8	29.728	66.8	51	57.1	64.2	52.7	65.5		WNW	Overcast.
☿ 14	29.649	63·4	29.675	66.6	51	59.0	63.8	50.7	65.3	0.008	NNE	Fair—lightly cloudy.
● ♃ 15	29.768	62.2	29.760	65.8	52	59.3	65.2	50.2	66.3		N	A.M. Fine. P.M. Lightly overcast.
♀ 16	29.794	64.4	29.838	66.5	46	61.2	65.6	52.2	65.3		SSW	A.M. Cloudy. P.M. Fine.
♄ 17	29.918	64.3	29.848	66.4	49	60.2	67.3	50.6	69.9		SSW	A.M. Fine. P.M. Cloudy.
☉ 18	29.827	62.5	29.836	66.4	51	59.3	65.2	52.6	69.6		SW	A.M. Fine. P.M. Rain.
☽ 19	29.833	65.4	29.875	67.7	56	64.0	68.3	57.7	69.4	0.204	WSW	Fine—light clouds.
♂ 20	29.986	66.4	29.934	68.9	50	62.7	70.2	54.5	71.7		SW	Fine—lightly cloudy.
☿ 21	29.899	66.3	29.794	69.9	55	64.5	72.6	61.8	75.5		SW	A.M. Lightly overcast. P.M. Fine—light clouds.
♃ 22	29.782	67.8	29.722	69.1	47	60.3	67.8	52.0	68.6		SW	Fine—nearly cloudless.
♀ 23	29.658	66.6	29.707	68.8	48	61.1	67.8	51.0	68.3		SW var.	Fine—light clouds and wind.
♄ 24	29.964	64.2	29.990	68.2	53	61.3	68.7	54.2	69.4		W	Fine—lightly cloudy.
☉ 25	30.126	60.4	30.196	67.3	52	59.9	61.8	53.3	64.5		NE var.	A.M. Fine. P.M. Cloudy.
☽ 26	30.303	61.8	30.259	66.2	51	57.5	67.3	54.0	68.2		NW	A.M. Lightly overcast. P.M. Fine.
♂ 27	30.255	64.2	30.194	66.7	51	58.3	69.5	50.4	71.5		SW	Fine and cloudless—light wind.
☿ 28	30.257	65.7	30.225	68.9	54	63.2	72.8	53.0	73.7		SSW	Fine—light clouds.
♃ 29	30.159	67.4	30.010	69.6	59	61.5	72.8	55.3	75.3		S	Fine.—A.M. Light clouds. P.M. Cloudless.
○ ♀ 30	29.893	66.7	29.713	69.0	52	61.3	66.2	56.4	68.2		SW	A.M. Cloudless. P.M. From 6½ h. rain and high wind.
♄ 31	28.970	61.2	29.011	60.3	51	52.5	50.9	48.2	52.5	0.277	SW var.	Light rain, with high wind.
	Mean 30.004	Mean 64.9	Mean 29.976	Mean 67.7	Mean 51.2	Mean 60.6	Mean 66.7	Mean 53.0	Mean 68.4	Sum 0.567		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. .... 9 A.M. 29.904 3 P.M. 29.867

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge .... =83 feet 2½ in.

———————— above the mean level of the Sea (presumed about) .... =95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House .... =79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR SEPTEMBER, 1833.

1833. Septr.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	External Thermometer. Fahrenheit. 3 P.M.	External Thermometer. Self-registering. Lowest.	External Thermometer. Self-registering. Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
☉ 1	29.467	57.5	29.598	59.8	41	51.3	55·0	48.0	55.3	0.113	ESE	Fine—lightly cloudy.
☽ 2	29.958	55.8	29.950	59.7	39	50.2	60.2	41.0	61.9		SW	Fine.—A.M. Cloudless. P.M. Light clouds.
♂ 3	29.707	58.5	29.610	59.8	53	56.6	57.9	49.8	61.2		SSW	Fine—light clouds.—Heavy rain at 1½ P.M.
☿ 4	29.944	57.2	30.056	59.3	45	54.0	57.4	47.2	58.6	0.113	NE var.	Fine—light clouds and wind.
♃ 5	30.305	54.7	30.299	58.4	45	52.3	59.7	43.0	59.3		NNE	Fine—light wind.—A.M. Nearly cloudless. P.M. Light clouds.
♀ 6	30.281	56.2	30.181	59.9	49	54.9	62.8	45.4	63.3		NE var.	Fine—light wind.—A.M. Light clouds. P.M. Cloudless.
♄ 7	30.059	57.0	29.996	62.5	53	57.4	66.0	49.6	67.2		NE var.	Fine—lightly cloudy.
☉ 8	29.816	58.0	29.804	61.9	52	55.2	61.8	52.9	62.8		SE var.	Overcast—light wind.—Light rain, A.M.
☽ 9	29.899	59.8	30.027	63.2	55	57.9	64.3	54.8	64.8	0.025	NNW	A.M. Lightly overcast. P.M. Fine—light clouds.
♂ 10	30.083	60.7	30.027	61.5	53	57.6	59.8	56.4	60.2		ENE	Lightly cloudy and overcast.
☿ 11	29.792	60.7	29.703	64.5	56	58.7	65.2	55.8	66.8		NNW	A.M. Overcast—very light rain. P.M. Fine—light clouds.
♃ 12	29.973	59.8	30.033	62.0	45	55.5	60.3	48.2	61.2		SSW	A.M. Cloudy. P.M. Fine.
● ♀ 13	30.146	57.2	30.097	61.6	43	52.9	61.0	43.0	62.4		SW	Fine.—A.M. Nearly cloudless. P.M. Light clouds.
♄ 14	29.922	59.6	29.901	63.5	51	59.4	65.7	52.2	66.3		NE var.	A.M. Fine. P.M. Cloudy.
☉ 15	29.942	58.8	29.903	62.5	50	53.0	63.8	46.0	64.7		SSW	Fine—nearly cloudless.
☽ 16	29.689	61.0	29.641	63.4	52	58.2	62.0	52.4	64.4		SW	A.M. Fine. P.M. Cloudy.—At night, rain.
♂ 17	29.530	59.7	29.623	63.3	53	56.4	62.6	52.8	63.5	0.185	ENE	Cloudy.—Light rain, early A.M.
☿ 18	29.778	58.7	29.812	62.3	52	54.3	59.6	47.5	62.2		SW	Fine—light clouds.
♃ 19	30.043	57.7	30.081	61.4	48	53.8	59.2	45.5	61.7		SW	A.M. Fine. P.M. Light rain.
♀ 20	30.253	56.0	30.235	60.0	50	50.6	60.6	45.0	61.2	0.041	ENE	A.M. Fine. P.M. Lightly cloudy.
♄ 21	30.188	57.3	30.107	60.7	50	52.8	61.6	49.9	63.2		E	A.M. Light fog. P.M. Fine.
☽ 22	30.053	57.6	29.990	61.5	49	53.2	62.6	50.0	63.7		E	A.M. Fog. P.M. Fine—light clouds.
☽ 23	29.877	58.8	29.808	63.6	53	56.2	63.2	51.4	64.6		ESE	Fine—light haze and clouds.
♂ 24	29.443	60.2	29.415	62.0	55	59.6	61.2	53.2	61.5		NW var.	Overcast—light rain and brisk wind.—At night, high wind.
☿ 25	29.707	61.7	29.754	63·9	52	59.9	62.4	55.0	64.5		S	Fine—nearly cloudless—brisk wind.
♃ 26	29.798	61.2	29.839	64.2	54	59.0	61.5	54.2	63.7		S	Fine—nearly cloudless.
♀ 27	29.895	57.7	29.819	61.6	46	49.9	61.0	46.0	61.5		SSW	A.M. Fog. P.M. Fine.
○ ♄ 28	29.609	59.7	29.581	61.7	57	57.4	58.4	49.4	59.3		ESE	A.M. Cloudy. P.M. Fair.
☉ 29	29.934	57.7	30.010	61.3	53	52.6	60.6	46.3	61.5		WSW	A.M. Fine and cloudless. P.M. Lightly cloudy.
☽ 30	30.254	57.3	30.260	60.7	50	50.7	61.7	48.8	62.3		ESE	A.M. Fog. P.M. Fine—light clouds.
	Mean 29.912	Mean 58.5	Mean 29.905	Mean 61.7	Mean 50.1	Mean 55.1	Mean 61.3	Mean 49.4	Mean 62.5	Sum 0.477		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr................. { 9 A.M. 29.830 — 3 P.M. 29.813 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge .... =83 feet 2½ in.

———————————— above the mean level of the Sea (presumed about).............. =95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House .... =79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR OCTOBER, 1833.

1833. October.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	Fahrenheit. 3 P.M.	Self-registering. Lowest.	Self-registering. Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
♂ 1	30.303	55.7	30.264	57.7	45	47.7	55.8	45.3	56.7		E	A.M. Fog. P.M. Fine—light clouds and haze.
☿ 2	30.239	55.3	30.154	58.4	47	48.2	60.3	46.8	60.3		E	A.M. Light fog. P.M. Fine—light clouds and wind.
♃ 3	30.063	56.2	30.039	57.4	51	51.8	55.2	47.7	55.1		NNE	Haze.
♀ 4	30.142	55.2	30.106	58.8	51	51.3	59.6	45.6	60.4		NNE	Fine.—A.M. Cloudless. P.M. Light clouds.
♄ 5	30.119	54.7	30.122	58.6	52	52.2	58.4	45.5	58.8		NNE	Lightly cloudy and overcast.
☉ 6	30.149	55.7	30.126	58.7	53	53.4	60.7	49.3	61.3		NNE	A.M. Overcast. P.M. Fine—light clouds.
☽ 7	30.158	55.2	30.095	58.4	51	50.9	59.7	48.3	59.7		NNE	A.M. Overcast. P.M. Fine and clear—light clouds and wind.
♂ 8	30.032	55.4	29.990	58.7	53	52.7	60.7	47.6	60.8		NNE	A.M. Light fog. P.M. Fine—light clouds.
☿ 9	30.166	55.3	30.202	58.3	51	51.0	57.4	48.3	57.4		NNE	Lightly cloudy and overcast—light wind.
♃ 10	30.243	54.9	30.170	57.4	49	53.5	55.7	47.5	56.5		E	Fine and cloudless.
♀ 11	29.994	52.7	29.948	56.4	43	47.2	57.2	43.7	58.3		E	A.M. Fog. P.M. Fair—lightly overcast.
♄ 12	29.950	54.3	29.871	57.8	50	50.6	58.2	46.7	58.8		WSW	Lightly overcast.—Evening, light rain.
● ☉ 13	30.109	51.0	30.039	55.3	41	43.8	51.9	38.6	57.6		WSW	A.M. Fine and cloudless. P.M. Fair—lightly cloudy.
☽ 14	29.540	55.8	29.478	58.6	57	57.7	57.7	43.3	59.7	0.014	SSW	Rain.
♂ 15	29.225	55.3	29.128	57.6	44	49.7	53.7	47.8	54.3	0.014	SW	Fine and clear—light clouds.—Evening, light rain.
☿ 16	29.215	51.8	29.294	53.8	45	45.4	50.3	41.3	50.3	0.017	W	Lightly cloudy.—At noon, fine and clear: evening, light rain.
♃ 17	29.556	51.4	29.633	53.7	49	49.1	51.7	43.2	52.6	0.028	NW	A.M. Light rain. P.M. Overcast.
♀ 18	29.668	50.3	29.547	53.3	43	45.7	51.2	41.2	51.2	0.008	W	Lightly cloudy and hazy.—Light wind, A.M.
♄ 19	29.433	50.9	29.893	54.2	47	47.8	51.3	42.8	53.3	0.031	W	Fine—lightly cloudy.
☉ 20	29.572	48.6	29.715	52.3	41	41.7	50.8	38.3	55.5	0.008	W	Fine—haze.—A.M. Cloudless. P.M. Light clouds and wind.
☽ 21	29.671	52.3	29.627	55.4	55	55.8	57.7	41.2	57.7		S var.	Overcast.—At night, rain with strong wind.
♂ 22	29.677	57.3	29.692	60.3	56	56.0	59.3	54.8	60.7	0.028	SSW	Fair—lightly overcast.—At night, rain with strong brisk wind.
☿ 23	29.430	57.6	29.507	59.9	55	56.9	56.6	53.2	59.3	0.014	S	Light wind and broken clouds.
♃ 24	29.686	55.7	29.576	59.6	52	53.3	61.8	47.3	61.7	0.008	ESE	A.M. Lightly cloudy. P.M. Light clouds and wind.
♀ 25	29.470	58.3	29.428	60.6	53	55.9	60.5	53.3	61.7		E	Fine—light high clouds.—Evening, cloudless.
♄ 26	29.567	58.9	29.522	61.2	54	56.0	60.9	54.7	62.3		ESE	A.M. Fine and clear. P.M. Overcast. Evening, light rain.
☉ 27	29.875	57.7	29.847	60.7	51	52.1	59.3	48.8	59.4	0.006	ESE	A.M. Haze. P.M. Fine—light flocculous clouds—light unsteady wind.
○ ☽ 28	29.776	59.2	29.744	61.6	56	56.5	60.5	51.8	60.5		E	A.M. Light fog and deposition. P.M. Fine.
♂ 29	29.729	58.8	29.878	61.9	55	55.8	62.3	52.7	62.3		E	Fine—nearly cloudless.—Haze, A.M.
☿ 30	30.011	58.3	30.008	60.4	52	52.6	60.7	49.7	61.3		E	A.M. Haze. P.M. Fine and cloudless.
♄ 31	30.071	56.4	29.999	58.4	48	48.2	54.5	47.2	56.7		W	Fog.
	Mean 29.834	Mean 55.0	Mean 29.827	Mean 57.9	Mean 50.0	Mean 51.3	Mean 57.1	Mean 46.9	Mean 58.1	Sum 0.176		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. .... 9 A.M. 29.762 — 3 P.M. 29.746

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge........................ =83 feet 2¼ in.

———————— above the mean level of the Sea (presumed about) .... =95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House .... =79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR NOVEMBER, 1833.

1833. Nov.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	External Thermometer. Fahrenheit. 3 P.M.	External Thermometer. Self-registering. Lowest.	External Thermometer. Self-registering. Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
♀ 1	29.840	58.3	29.841	60.3	57	57.2	58.2	47.7	59.7	0.007	WSW	A.M. Rain, early. P.M. Fine and cloudless.
♄ 2	30.056	55.4	29.804	58.3	47	49.0	55.2	45.3	56.3	0.008	WSW	A.M. Lightly cloudy. P.M. Fine.
☉ 3	29.721	54.7	29.858	55.7	41	48.8	51.6	45.8	51.6		WNW	A.M. Fine and cloudless—light brisk wind. P.M. Cloudy.
☽ 4	30.135	49.8	30.144	52.4	38	40.8	48.2	37.3	49.2		WSW	Fine.—A.M. Nearly cloudless. P.M. Light clouds.
♂ 5	30.284	48.7	30.134	51.0	41	44.5	50.3	37.3	53.7		WSW	Overcast.—Light rain, P.M.
☿ 6	30.004	53.7	29.990	57.6	52	54.6	55.8	43.7	57.4		WSW	Overcast and cloudy—light wind.
♃ 7	29.853	51.9	29.501	51.7	43	46.8	43.6	43.6	46.8		WSW	Rain.—Dark and lowering, P.M.
♀ 8	29.695	46.6	29.819	48.4	36	38.7	44.7	35.5	44.7	0.556	NNW	Fine—light clouds.
♄ 9	29.998	45.9	30.053	47.4	38	39.7	42.9	37.2	45.8	0.008	WSW	A.M. Haze. P.M. Fine—light clouds.
☉ 10	30.122	47.3	30.110	49.7	43	46.9	51.1	37.8	51.1		SSW	Lightly cloudy and overcast—light airs.
● ☽ 11	30.076	50.3	30.053	51.5	45	48.6	50.2	46.4	50.7		S	Overcast.—Light rain, P.M.
♂ 12	30.223	49.7	30.224	51.2	42	40.7	45.9	41.3	45.9		SW	Strong fog.
☿ 13	30.303	49.4	30.263	50.4	45	45.1	47.0	40.3	47.0		SW	Lightly overcast and hazy.
♃ 14	30.158	45.3	30.122	46.6	38	38.2	42.1	35.2	42.1		NE	Fine—light haze.—Cloudless, A.M.
♀ 15	30.071	44.4	29.976	44.9	35	37.8	40.3	36.3	40.3		E	Overcast—light fog.
♄ 16	29.874	44.8	29.867	47.2	38	41.3	45.0	37.4	45.7		S	Lightly cloudy—haze.—Light rain at night.
☉ 17	29.986	46.7	30.036	50.0	44	47.2	51.9	40.7	52.4		SSW	A.M. Cloudless—fog—deposition. P.M. Lightly cloudy.
☽ 18	30.291	50.6	30.293	52.0	48	50.3	52.7	46.4	52.7		ESE	Overcast.—Light fog and deposition, A.M.
♂ 19	30.251	52.7	30.198	53.8	49	49.7	50.2	49.2	50.3		W	Overcast—light fog.
☿ 20	30.109	49.1	30.057	51.3	40	40.8	46.3	40.3	48.7		SW	Light fog.—A.M. Overcast. P.M. Cloudless.
♃ 21	30.057	51.2	29.964	52.5	45	45.6	49.7	40.7	53.7		W	Lightly cloudy.—Light fog, A.M.
♀ 22	29.647	53.7	29.469	55.2	52	53.7	54.8	45.0	54.8		SSW	Cloudy—light wind.
♄ 23	29.723	51.8	29.724	53.3	44	45.3	48.4	43.7	48.4		WSW	A.M. Lightly overcast. P.M. Fine.
☉ 24	29.604	49.8	29.519	51.2	45	45.8	48.1	40.5	48.4		SSW	A.M. Fine and clear—light cloudiness. P.M. Dark—showery.
☽ 25	29.691	46.7	29.788	47.5	36	38.1	42.6	36.5	42.6	0.122	WSW	A.M. Cloudless—light fog and deposition. P.M. Lightly cloudy.
♂ 26	30.155	43.3	30.134	44.9	31	34.1	40.3	31.6	41.4	0.067	SSW	A.M. Fog and hoar frost. P.M. Fine—nearly cloudless.
○ ☿ 27	29.889	45.4	29.738	46.0	39	41.4	40.3	33.2	42.7		ESE	Fine—light wind.—A.M. Lightly cloudy. P.M. Cloudless.
♃ 28	29.405	44.4	29.058	46.7	42	42.7	47.7	34.6	50.6		SSE	A.M. Overcast—light fog and deposition. P.M. Rain.
♀ 29	29.227	47.3	29.441	48.3	37	44.4	47.2	42.3	47.2	0.031	WSW	Light wind.—A.M. Fine—lightly cloudy. P.M. Lt. rain. Evening clear.
♄ 30	30.029	45.3	30.013	47.5	39	41.1	46.3	38.7	51.3	0.009	W	Fine—light haze.—Cloudless, A.M.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	29.949	49.1	29.906	50.8	42.3	44.6	48.0	40.4	49.1	0.808		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. .... { 9 A.M. 29.896 — 3 P.M. 29.847 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge .... = 83 feet 2½ in.

——————————— above the mean level of the Sea (presumed about) .... = 95 feet.

The external Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House .... = 79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR DECEMBER, 1833.

1833. Dec.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	External Thermometer. Fahrenheit. 3 P.M.	External Thermometer. Self-registering. Lowest.	External Thermometer. Self-registering. Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
☉ 1	30.140	49.4	30.062	50.8	47	51.6	53.6	40.3	54.3		SW	A.M. Light fog and deposition. P.M. Overcast.—Evening, light rain.
☽ 2	30.122	51.3	30.085	51.8	47	48.6	48.0	46.7	54.3	0.006	WSW	A.M. Overcast. P.M. Fine.
♂ 3	29.971	49.7	29.942	51.7	44	46.3	50.8	41.7	52.3		WSW	A.M. Lightly cloudy—light wind. P.M. Overcast.
☿ 4	29.656	51.8	29.540	54.3	51	52.8	55.2	45.8	55.2		SSW	Cloudy—light wind.
♃ 5	29.511	53.4	29.475	55.3	48	48.5	48.2	47.4	48.5	0.019	SW	A.M. Lightly cloudy. P.M. Fine.—At night, rain and unsteady wind.
♀ 6	29.579	49.4	29.711	50.6	39	39.9	46.7	38.7	50.7	0.014	WSW	Fine.—A.M. Cloudless. P.M. Light clouds.
♄ 7	29.392	50.7	29.314	52.4	50	51.5	51.5	39.3	54.3	0.006	SSW	Rain, with light unsteady wind.
☉ 8	30.069	46.7	30.088	48.3	37	38.8	43.7	37.2	53.3	0.011	W	Fine—light haze.—A.M. Cloudless. P.M. Lightly cloudy.—Rain at night.
☽ 9	29.564	50.7	29.564	51.9	50	53.9	55.6	37.9	55.6	0.008	WSW	Light rain.
♂ 10	29.881	49.6	29.808	50.2	41	42.3	46.2	41.4	46.2	0.006	SW	Fine.—A.M. Cloudless. P.M. Light clouds.
● ☿ 11	29.685	47.3	29.673	47.9	36	40.6	43.8	38.9	43.8		NNW	Light haze.—A.M. Cloudless. P.M. Fair—lightly overcast.
♃ 12	29.773	43.8	29.848	45.3	37	37.8	40.4	34.7	40.4	0.006	W	Fine—nearly cloudless—light wind.
♀ 13	30.072	42.3	30.111	43.5	35	36.3	39.8	32.9	41.9		WNW	Light haze.—A.M. Lightly cloudy—deposition—hoar frost. P.M. Fine—nearly cloudless.
♄ 14	30.132	44.4	30.128	46.6	41	42.9	46.5	35.5	47.3		WSW	A.M. Overcast—fog. P.M. Fine—light clouds.
☉ 15	30.038	47.4	29.983	52.3	43	43.4	48.8	42.4	52.3		WSW	Overcast—light fog—deposition.
☽ 16	29.732	50.7	29.522	51.6	48	52.1	53.2	48.3	52.3		WSW	Overcast—light unsteady airs.—A.M. Deposition. P.M. Light rain.
♂ 17	29.369	50.3	29.562	50.8	43	46.4	47.7	42.9	52.8	0.008	WSW	Strong unsteady wind, early A.M.—Fine—lightly cloudy.
☿ 18	29.417	51.4	29.701	52.4	50	52.8	50.0	42.5	52.7	0.008	W	Lightly cloudy.—Rain at night.
♃ 19	29.789	53.3	29.643	54.5	51	53.3	53.6	46.8	54.5		SSW	Overcast.—Evening, gale of wind with rain.
♀ 20	29.386	52.8	29.435	52.4	41	47.8	47.6	46.7	48.3	0.008	WSW	Fine—light clouds.—Morning and night, unsteady wind.
♄ 21	29.342	47.8	29.576	49.0	40	40.2	43.6	36.7	43.6	0.383	WNW	A.M. Lightly cloudy. P.M. Fine—nearly cloudless.
☉ 22	29.594	46.7	29.269	48.5	41	42.4	48.3	37.3	52.4	0.008	SSE	Light rain.
☽ 23	29.342	49.5	29.211	49.6	45	46.4	46.8	41.7	48.5		E	Fog—dark and overcast—heavy continued rain.
♂ 24	29.404	50.0	29.311	51.7	49	49.3	51.0	43.8	51.7	0.944	S	Lightly cloudy and overcast.—Light wind and deposition, A.M.
☿ 25	29.457	50.0	29.748	49.4	44	46.7	43.4	43.8	46.7	0.083	NNW	A.M. Light fog.—Noon, rain. P.M. Clear—light clouds.—Evening, very clear.
○ ♃ 26	30.176	44.3	30.041	45.3	33	35.6	42.5	34.3	46.0		WSW	Lightly overcast—light fog.—Evening, rain.
♀ 27	29.796	46.8	29.744	48.1	43	43.8	46.8	34.6	46.8	0.133	WSW	A.M. Fog deposition. P.M. Fine—nearly cloudless.
♄ 28	29.952	44.8	29.988	46.4	35	37.3	42.9	35.5	51.4	0.061	WSW	Lightly cloudy and overcast.—Light fog and deposition, A.M.
☉ 29	29.746	49.6	29.798	51.0	46	51.8	51.3	36.5	52.3	0.011	W	Fine—cloudy.—Light wind, A.M.
☽ 30	29.665	50.9	29.659	53.2	50	51.8	54.5	47.8	54.7		WSW	Rain.
♂ 31	29.641	50.7	29.474	52.2	49	50.3	49.6	43.6	53.2		SSW	A.M. Drizzling rain. P.M. Fine—nearly cloudless.
	Mean 29.722	Mean 49.0	Mean 29.710	Mean 50.3	Mean 43.7	Mean 45.9	Mean 48.1	Mean 40.8	Mean 50.3	Sum 1.723		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. .... 9 A.M. 29.661 — 3 P.M. 29.650

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge .... =83 feet 2¼ in.

——————————— above the mean level of the Sea (presumed about)............... = 95 feet.

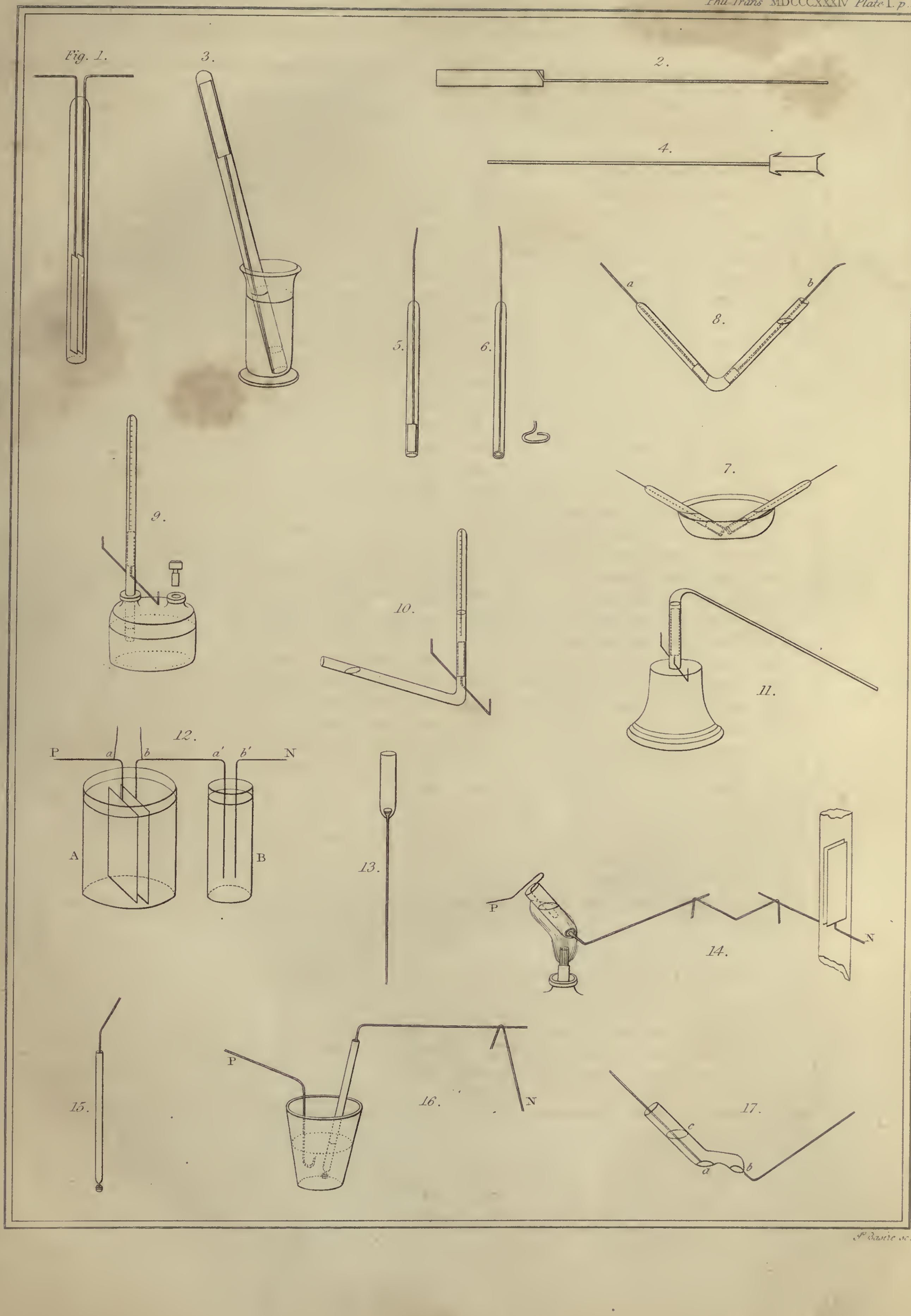
The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House .... =79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.



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PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXXIV.

PART II.

LONDON:

PRINTED BY RICHARD TAYLOR, RED LION COURT, FLEET STREET.

MDCCCXXXIV.

ROYAL MEDALS.

HIS MAJESTY KING WILLIAM THE FOURTH, in restoring the Foundation of the Royal Medals, graciously Commanded a Letter, of which the following is an extract, to be addressed to the Royal Society, through His Royal Highness the Duke of Sussex, K.G., President:

"Windsor Castle, March 25, 1833.

"It is His Majesty's wish,—

"First, That the Two Gold Medals, value of Fifty Guineas each, shall "henceforth be awarded on the day of the Anniversary Meeting of the "Royal Society, on each ensuing year, for the most important discoveries "in any one principal subject or branch of knowledge.

"Secondly, That the subject matter of inquiry shall be previously settled "and propounded by the Council of the Royal Society, three years pre-"ceding the day of such award.

"Thirdly, That Literary Men of all nations shall be invited to afford the "aid of their talents and research: and,

"Fourthly, That for the ensuing three successive years, the said Two "Medals shall be awarded to such important discoveries, or series of in-"vestigations, as shall be sufficiently established, or completed to the "satisfaction of the Council, within the last five years of the days of award, "for the years 1834 and 1835, including the present year, and for which "the Author shall not have previously received an honorary reward."

(Signed) "H. Taylor."

The Council propose to give one of the Royal Medals in the year 1836, to the most important unpublished paper in Astronomy, communicated to the Royal Society for insertion in their Transactions, after the present date and prior to the month of June in the year 1836.

The Council also propose to give one of the Royal Medals in the year 1836 to the most important unpublished paper in Animal Physiology, communicated to the Royal Society for insertion in their Transactions, after the present date and prior to the month of June in the year 1836.

The Royal Medals for the year 1833 were awarded to

SIR JOHN FREDERICK WILLIAM HERSCHEL, K.H. F.R.S.,

for his Paper on the Investigation of the Orbits of Revolving Double Stars; and to

PROFESSOR AUGUSTE PYRAME DE CANDOLLE, of Geneva, Foreign Member of the Royal Society,

for his Discoveries and Investigations in Vegetable Physiology.

Those for 1834 were awarded to

JOHN WILLIAM LUBBOCK, ESQ., V.P. & TREAS. R.S.,

for his Papers on the Tides published in the Philosophical Transactions; and to

CHARLES LYELL, ESQ.,

for his Work entitled "Principles of Geology."

The Council propose to give one of the Royal Medals in the year 1837 to the most important unpublished paper in Physics, communicated to the Royal Society for insertion in their Transactions, after the present date and prior to the month of June in that year.

The Council also propose to give one of the Royal Medals in the year 1837 to the author of the best paper, to be entitled "Contributions towards a System of Geological Chronology founded on an examination of fossil remains, and their attendant phenomena," such paper to be communicated to the Royal Society after the present date and prior to the month of June 1837.

CONTENTS.

ERRATA.

Phil. Trans. 1833.—page 756, line 6, *for* excreting *read* secreting

page 759, line 25, *for* (Plate XXI.) *read* (Plate XXIII.)

PHILOSOPHICAL TRANSACTIONS.

XIV. *On some Elementary Laws of Electricity.* *By* W. Snow Harris, *Esq. F.R.S. &c.*

Received January 7,—Read April 10, 1834.

1. A MORE perfect apprehension of those subtile agencies, the effects of which are continually present in various operations of nature, seems of paramount importance to the future advancement of science. Thus the physical causes of heat, light, electricity and magnetism, have become subjects of deep interest to the natural philosopher; little apology, therefore, may perhaps be deemed requisite for my venturing to submit to the consideration of the Royal Society an account of some inquiries, the object of which is to improve our knowledge of one of these great natural powers. As it is only by a patient and repeated induction from well investigated facts that we can hope to attain a higher degree of perfection in natural knowledge, I have thought it not altogether undesirable to inquire further into the elementary laws of common electricity: indeed, upon considering the late fine discoveries of Dr. Faraday, this seems to a certain extent requisite. The researches of this distinguished philosopher have invested electrical phenomena generally with a new interest, and exposed novel and important features in the theory of electrical action.

The investigations in this department of science, which I have now the honour of presenting to the Royal Society, will, I hope, be found to contain matter of sufficient interest to render them not unworthy of its acceptance.

2. The existence of an invisible agency in the natural world, designated by the term electricity, may be inferred from the tendency of bodies toward each other, when subjected to a peculiar kind of excitation, by means of various operations, such as by the contact of dissimilar bodies, friction, changes of temperature, of form, and the like. Many striking facts seem to warrant the supposition that this agency is dependent on an extremely subtile species of matter, either of a compound or elementary character, everywhere present, and operating according to certain laws, which it is the province of experiment and analysis to determine.

3. This hypothesis appears, upon the whole, to be not ill adapted to an easy explanation of appearances, and to the purposes for which hypothesis may be legitimately

resorted to in the prosecution of physical inquiries. Accordingly, I am led to avail myself of it, but without extending it beyond the simple principle above mentioned. The properties of this subtile matter, whether of an elementary or compound character, if such should hereafter be more fully proved to exist, I leave only to be determined by adequate induction from observed phenomena.

4. Assuming, then, as an elementary principle, not upon the whole unwarranted by facts, the existence of a subtile material agent essentially involved in the constitution of ordinary matter, and known to us only through the medium of its effects, we may distinguish its presence under two different forms of what may be termed electrical excitation; that is to say, a state of excitation produced by a different relative state of the electricity possessed by a body to that which is more or less common to all bodies, in which case the quantity remains unchanged; and a state of excitation derived from an actual addition, or subtraction, of the electricity of a given substance, or of any component part of its electricity, in which case the quantity may be said to vary.

5. The latter of these states has been termed excitation by communication; and the former, when produced by the influence of this last, operating at a distance, excitation by induction.

6. A body, when excited according to either of these forms of excitation, displays apparently an attractive force, so that other bodies, when all impediments to motion are removed, tend toward it, and the accumulated electricity seeks to regain its previously existing state: a peculiar action is in this case found to obtain, either in the excited substance itself, or otherwise between it and the surrounding masses. Such may be considered, on the above hypothesis, the great characteristics of ordinary electrical action, those which were the first observed, and which, with their attendant phenomena, demand the most rigorous scrutiny.

7. In order to facilitate the progress of inquiries concerning the elementary laws of electrical action, I have been led to construct one or two new instruments, as also to resort to other electrical arrangements, which it is essential to notice. Fig. 1. A. (Plate II.) represents an electroscope which acts on the principle of divergence: a small elliptical ring of metal, *a*, is attached obliquely to a small brass rod, *a b*, by the intervention of a short tube of brass at *a*; the rod *a b* terminates in a brass ball, *b*, and is insulated through the substance of the wood ball *n*: two arms of brass, *r r'*, are fixed vertically in opposite directions, on the extremities of the long diameter of the ring, and terminate in small balls; and in the direction of the shorter diameter, within the ring, there is a delicate axis set on extremely fine points: this axis carries, by means of short vertical pins, two light reeds of straw, terminating in balls of pith, and constituting a long index A, corresponding in length to the fixed arms above mentioned. The index thus circumstanced is susceptible of an extremely minute force; its tendency to a vertical position is regulated by small sliders of straw, moveable with sufficient friction on either side of the axis. To mark the angular position of the index

in any given case, there is a narrow graduated ring of cardboard or ivory, $r\,r'$, placed behind it, the divisions being distinctly legible through sight-slits cut in the reeds: the graduated circle is supported on a transverse rod of glass, by the intervention of wood caps, and is sustained by means of the brass tube *a*, in which the glass rod is fixed. The whole is insulated on a long rod of glass, A, by means of wood caps terminating in spherical ends. In this arrangement, as is evident, the index diverges from the fixed arms whenever an electrical charge is communicated to the ball *b*, as in fig. 10. This instrument is occasionally placed out of the vertical position at any required angle, by means of a joint at *n*, and all the insulating portions are carefully varnished with a solution of shell lac in alcohol.

8. Fig. 2. B. represents an electrometer which measures directly the attractive force of an electrified body in terms of a known standard of weight estimated in degrees on the graduated arc $x\ y$. An insulated conductor, *f*, is fixed on a varnished rod of glass, $f\,g$, sustained by the intervention of a wood ball on the extremity of a micrometer screw, *s*: by aid of the screw the whole may be raised or depressed, through given intervals, to within the one hundredth of an inch of any required point. A moveable and similar conductor, *m*, made of light wood, hollowed and gilded, is suspended immediately over the former from the periphery of a small brass wheel W by means of a fine silver thread attached near its vertical arm, and passing from thence over its grooved circumference, as shown in fig. 3. The conductor *m* is counterpoised by a short cylinder of wood, $p\ n$, figs. 2, 3, suspended in a similar manner from the opposite side of the wheel, by means of a silk thread: this counterpoise is partly immersed in water contained in the glass vessel *n*, fig. 1.

The extremities of the axis of the wheel W, figs. 2, 3, are turned to extremely fine pivots, and rest on two large friction wheels, after the manner represented in fig. 4*, by which great freedom of motion is obtained. There is a fine index of light straw, W *c*, attached to the extremity of a small steel needle inserted diametrically through the circumference, which indicates on the graduated arc $x\,y$ the force exerted between the conductors $m\ f$: the weight of this index is accurately poised by a small globule of brass, *t*, fig. 3, moveable on a screw, cut in the opposite arm of the steel needle carrying the index.

The centre of the wheel W is accurately placed in the centre of the arc $x\,y$, which, with its radii of support, is made of varnished wood, the graduated scale being of cardboard or ivory. The arc is the sixth part of a circle; it is divided into 120 equal parts, sixty in the direction $c\ x$, and sixty in direction $c\ y$, the centre C being marked zero.

* I resorted to this method of employing friction rollers, as being more efficient than that in which the axis is allowed to rest in the angles formed between the peripheries of four smaller wheels. In this case it rolls fairly on a large circumference, and is prevented from passing off it on either side by the check wheels, either of which, when acted on, opposes little or no resistance to motion. When this machine is equipoised with 500 grains, less than the $\frac{1}{50}$th of a grain will set the whole in motion.

Fig. 3. represents the wheel W with the suspended conductor and counterpoise, the index and its balance weight, together with the lines of suspension, passing freely over the circumference, and fixed at the points *i i*.

The various wheels above mentioned, with the graduated arc, are sustained on a projecting metallic plate, which is united by a spherical nut to a metallic rod passing through a glass column B. The column is secured by means of the rod to a sort of double stand, *h h*, fig. 2, supported on three levelling screws. The interval between the plates of this stand contains the glass vessel *n* and the micrometer screw *s*; the upper plate has a circular hole, *p*, through which the cylindrical counterpoise passes into the water, *n*; the levelling screws serve to regulate the position of this counterpoise through the hole *p*, so that when it hangs in it centrally, the instrument is accurately adjusted.

The gravity of the suspended conductor *m* being in the above arrangement opposed by that of the counterpoise, it may be so far considered as existing in free space devoid of weight, and will therefore become very readily moved by any new force applied to it. It may consequently be caused to approach, or recede from, the fixed conductor *f*, by the operation of forces acting in either of these directions; the motion will however be speedily arrested by the counterpoise *n*, which (becoming either further immersed, or otherwise raised in the water,) furnishes in the greater or less quantity of water displaced, a measure of the force. In this way the force may be estimated either in degrees or in grains of actual weight, since the number of grains requisite to add to either side, in order to advance the index in either direction, a given number of divisions may be immediately found by experiment, and which, as the sections of the cylinder are all similar, will increase or decrease with the degrees of the arc. Thus, if one grain advance the index in either direction five degrees, then two grains will advance it ten degrees, and so on*.

9. In the application of this instrument to electrical inquiries, the force to be measured is first communicated to the fixed conductor *f*, a free communication being established between the suspended conductor *m* and the ground, or otherwise with the negative side of the jar or battery, should the attractive force be derived from this species of accumulation; this is readily effected through the brass work of the apparatus in connexion with the rod passing through the interior of the glass column B.

For the repulsive force we connect the conductor *f* as before, and suspend *m* by a silk thread, so as to allow it to rest on *f*; it will then, after being electrified similarly to *f*, recede from it; but this method of experiment I have seldom resorted to; it is evidently more complicated than the former, and occasionally liable to objection.

10. The distance between the conductors *m f* corresponding to a given force, is easily ascertained by means of the degrees indicated on the arc *x y*. In the instrument

* The counterpoise should be free from grease or varnish of any sort, and should, previously to being used, be kept immersed in water; the insulation of the conductor *f*, also, should be made extremely dry, and occasionally warmed by a stick of burning charcoal.

above described, each degree corresponds to a variation of distance between the conductors equal to the ·01 of an inch. If, therefore, at the commencement of any given experiment, we first bring the nearest points of the conductors *m f* in contact, the index being in zero, and then depress the inferior conductor *f* a given distance, known by means of the micrometer screw *s*, then all subsequent distances may be readily determined between these points.

11. It is now only requisite to observe, that the interior of the cylindrical counterpoise *p n* is hollow, in order to weight it accurately, and cause it to hang vertically in the water; and there is a small hemispherical cup, *p*, fixed on its stem, for the reception of small adjusting weights *, by which the position of the index at 0 of the scale is regulated with great nicety. With respect to the form of the conductors *m f*, they are generally plane circular areas, backed by small cones, and are of about two inches diameter. Conductors of other forms, however, such as spheres and cylinders, may be occasionally used when the object is to experiment more particularly on bodies of peculiar forms.

Experiments with this instrument are remarkably clear, notwithstanding the subtile character of the principle we have to investigate: thus, when the insulations are perfect, and the atmosphere dry, the index immediately exhibits the amount of the attractive force, and remains stationary for a much longer time than is required to note the result.

12. Considering that electrical inquiries would be much facilitated by an accurate method of estimating comparative quantity, I endeavoured, as being essential to my purpose, to obtain a unit of measure, and at length arrived at the following methods of estimating quantity, which are simple and accurate.

According to the known laws of electrical accumulation on coated jars, the quantity accumulated on one coating is proportionate to the quantity given off by the other: hence, if instead of transmitting the electricity evolved by the machine immediately from its conductor, we communicate the charge from the outer coating of a small jar furnished with a discharging electrometer, we may estimate pretty accurately by the number of explosions, that is to say, by the number of charges which have passed the smaller jar, the quantity accumulated.

13. On this principle, I inverted a small jar, K, fig. 5, exposing about six inches square of coating, on a brass rod fixed to the conductor of the machine, or otherwise sustained on a separate insulation, and connected the jar or battery to be charged with its outer surface, through the intervention of a brass ball, *b*. In this arrangement, electricity is continually supplied to the jar, and the amount of the accumulation accurately measured by the number of charges which the unit jar has received, the charges being determinable by means of the discharging balls *n n'*. By diminishing or increasing the distance between the discharging balls, the value of the unit may be rendered as small or as great as we please: hence, if the balls be securely

* Small lead shot may be employed for this purpose.

fixed, and the distances between their points of discharge accurately measured by means of a micrometer-screw and index at *s*, comparative quantities may be always estimated and restored from time to time with a great degree of accuracy.

14. Comparative quantities of electricity may be transferred to simple conductors, by abstracting sparks from an insulated jar, D, fig. 6. charged with a given accumulation by the preceding process. The sparks may be taken immediately on the conductor, or otherwise, on an insulated transfer plate, *p*, fig. 7, of given capacity, and then deposited on the conductor, as in *a*, fig. 2. This method of estimating quantity is extremely efficient in researches with simple conductors. The following experiments in illustration of it are not unimportant:

(*a*.) An insulated metallic disc, *a*, fig. 2, electrified many times in succession by a series of sparks transferred to it from the charged jar D by means of the insulated plate *p*, was found at each transfer to be electrified to so nearly the same amount, that the differences were not apparent on the electrometer, fig. 2, or on the electroscope, fig. 1; the disc being supposed in contact with either of these instruments. It is only requisite in this case to restore the opposite coating of the jar to its previous state, after each contact with the transfer plate *.

When a portion of the charge is abstracted so as to sensibly decrease the quantity in the jar, a new point may be arrived at, from whence another series of sparks can be obtained of less magnitude, but differing extremely little in quantity as compared with each other; and this process may be continued to a low point of accumulation in the jar.

(*b*.) The quantity given off by the positive coating will depend on the dimensions of the conductor to be charged, and on the state of the negative coating: thus a conductor of a double capacity becomes charged by a single contact with a double quantity; a conductor of a treble capacity, with a treble quantity (56.); and generally, conductors varying in superficial dimensions are electrified by one contact, in such way as to exhibit precisely the same force when connected with the electrometer. The extent of this action is considerable, provided the opposite coating be placed in a sufficiently free state.

15. It would seem by these experiments, that in the discharging of a charged jar, by the successive abstraction of small sparks, series may be obtained of such slow convergence, that certain terms near each other may be taken as equal †.

* It is of no consequence to the experiment what part of the electrical conductor touches the contact ball of the electrometer; the same force is invariably indicated whether we make the contact at either of its extremities or centre.

† In the various experiments with simple conductors, described in this paper, it is essential to remark, that the most perfect system of insulation was requisite: all the glass rods were therefore as slender as possible, and were varnished with a solution of shell-lac in alcohol. The experiments also have been carried on always in a dry atmosphere, and the various insulations occasionally warmed with a stick of burning charcoal. The success of this process is not a little remarkable: the index of the electrometer remains, as it were, fixed on a given point for a comparatively long period of time; hence the results are decisive. It is, on the contrary,

16. The superficial dimensions of a given conductor, or the quantity of electricity disposed on it, being varied, considerable differences are observed to arise in the attractive force; and of these, the instruments above described (7.) (8.) are extremely susceptible: by carefully pursuing the inquiry under these conditions, I arrived at very interesting results: the most important of these are the following:

A given quantity, divided upon two perfectly similar conductors, was found to exert upon external bodies, only a fourth part of the attractive force apparent when disposed upon one of them.

When divided upon three perfectly similar conductors, the force upon either is only one ninth of the force apparent when disposed upon one of them, and so on; that is, the quantity being constant, the force is as the square of the surface inversely; or the surface being constant, as the square of the quantity directly.

17. The following experiments may be adduced as illustrative of the above laws:

(*c*.) Three or four perfectly similar and equal conductors, *a*, *b*, *c*, fig. 8, of a cylindrical form, being well insulated, a given quantity of electricity was communicated to one of them by means of the charged jar D (14.), and the attractive force measured by the electrometer, fig. 2, with the contact ball of which it was subsequently made to communicate, as in fig. 8. The electrified bodies being now reduced to a neutral state, a second equal quantity was again communicated to the same conductor as before; after which it was caused to touch one of the others, so as to divide the charge on both. In this case, each conductor was observed to be, on transferring it to the electrometer, equally charged; the force, however, after making the requisite correction for distance between the attracting bodies *m f*, fig. 2, (8.) amounted only to the one fourth of the previous force. This process, repeated with three and with four similar conductors, reduced the force to the one ninth and one sixteenth part of the first respectively. The actual results of a series of experiments, conducted under extremely favourable conditions of the air, are given in the following Table:

TABLE I.

Comparative quantity.	Force in degrees.	Distance of attracting surfaces.	Force at distance of one inch.
1	30°	1	30°
$\frac{1}{2}$	5 —	1·25	7·8 —
$\frac{1}{3}$	2 +	1·28	3·27 +
$\frac{1}{4}$	1 +	1·29	1·8 +

quite impossible to insure accuracy in a moist atmosphere, or with imperfect insulation. Flame of all kinds should be studiously removed from near the subject of experiment; the dissipation of a charge being rapid under the influence of a lamp or a candle. When, however, the system of insulation is perfect, the electricity remains stationary on the conductors for a much longer time than is requisite for patient observation; and the electricity abstracted from the charged jar, upon an insulated plate of metal, will pass off again from the plate, without dissipation, in a sharp spark.

The approximations observable in this Table to the law in question are as perfect as can be expected, which is further evident when given quantities are transferred to a plane conductor in contact with the electrometer *a*, fig. 2, as will be more fully explained hereafter (56.).

18. Similar results may also be arrived at in disposing given quantities of electricity on coated jars. By aid of the unit jar (11.), and an improved adaptation of the common balance as a measure of electrical attraction, they can be exhibited without the least difficulty. It becomes necessary that I should briefly mention here the more recent mechanical arrangements connected with these and similar experiments, as I shall have occasion to refer to them frequently.

19. Fig. 9. N. represents a simple balance, suspended from the curved brass rod *n h*. It can be raised or depressed through small distances by a micrometer-screw at *h*, and can be also elevated or depressed by the graduated sliding tube *n o*: the tube *o* is screwed on a brass cap, fixed on the glass column N, through the centre of which passes a stout brass wire. A conducting substance *m*, of any required form, is suspended by a double silver thread from one of the arms of the beam: it is made of light wood, is hollow, and gilded. This body is accurately counterpoised by weights placed in the scale-pan *t*, suspended from the opposite arm. A similar conductor *m'* is fixed immediately under the former, and is supported on a graduated sliding tube *s*, insulated on the glass pillar *h*: the pan *t*, when loaded with given weights, rests on a small plate of wood, whose altitude can be easily adjusted by means of the sliding brass rod *r*: the whole is fixed on an elliptical base, furnished with three levelling screws.

When the lower conductor *m'* is connected with one side of an electrical jar E, through the substance of the ball *b*, and the suspended conductor *m* with the opposite side, by means of the suspension thread and the wire passing through the glass column N, then the attractive force arising from a given accumulation is caused to act immediately between these conductors *m m'*, and may be measured, under given conditions, by weights placed in the pan *t*.

The distance between the nearest points of the conductors *m m'* is accurately estimated in the following way: The insulated conductor *m'* being raised to zero of the graduated tube, so as to touch, or very nearly so, the suspended body *m*, the points of contact are minutely found by the micrometer screw *h*. The body *m'* is now depressed a given quantity, as measured by the divisions on the slide, and hence the distance between *m m'* is accurately known. When this distance requires to be greatly increased, it is effected by raising the beam, which is easily done by means of the graduated slide *n o*; but in effecting this it is essential to raise at the same time the pan *t*, so as to preserve the index rod of the beam exactly vertical.

20. These conditions understood, the following experiments will be easily apprehended:

(*d*.) A jar, E, fig. 9, exposing about five square feet of coating, being connected with the unit of measure *u*, the number of charges was noted corresponding to an accumulation, the attractive force of which operating between the two plane surfaces, *m m′*, was equivalent to a force of 4·5 grains.

When the quantity of electricity accumulated was doubled, the force amounted to exactly 18 grains; three times the accumulation balanced a force of 40·5 grains, and so on.

(*e*.) When a second and precisely similar jar was connected with the former, so as to double the extent of coating, similar quantities, measured as before, only exhibited one fourth of the previous forces respectively.

With three similar jars, that is, with three times the surface, the force was only one ninth part of the respective forces first observed.

By substituting the electrometer, fig. 2, in place of the balance, the march of the attractive force may be gradually observed, so as to exhibit the above results by minute degrees, thereby furnishing very interesting experiments.

21. The physical causes of these effects are not very apparent; they seem, however, to have some connexion with the following fact. The force exerted between two given substances, is more or less diminished by the presence of a neutral or other body sharing in the attraction.

Thus, the excited balls of an electrometer tend to close when an uninsulated neutral body is brought near them.

The attractive force evinced by any description of electrometer in connexion with a charged conductor, will apparently diminish when a neutral body is presented toward the conductor.

A similar phenomenon is observable when neutral bodies are interposed between two conductors, *m n*, fig. 10; one, *n*, being permanently electrified, and influencing the other, *m*, by induction. An intervening plate, *p*, appears to operate as a screen, and to arrest, as it were, to a greater or less extent, the inductive influence; and such will also be the case when the plate *p* is applied near any other part of the electrified conductor *n*, without coming between *m* and *n*. This effect is strikingly analogous to the operation of screens in diminishing the force of a revolving magnet on metallic discs*.

It may be likewise remarked, that when a neutral conductor, *m*, fig. 10, is exposed to the inductive action of an excited body, *n*, and is at the same time touched with an uninsulated conductor, it will have its original quantity of electricity either increased or diminished according as the electricity of the excited body *n* is positive or negative.

Now an electroscope, A, will not indicate the change which has been effected in the conductor *m* so long as it remains exposed to the influence of the excited body; but if we remove the excited body, or otherwise make it neutral, then the electroscope A will immediately diverge.

It is not essential here to enter upon the theoretical explanations of these phenomena, the mere facts being alone requisite; for whatever theory may be considered

* Philosophical Transactions for 1831, p. 497.

as sufficient to account for them, it should be equally applicable under whatever peculiar form they may present themselves.

22. If these phenomena, then, be considered in reference to the accumulation of electricity on conducting bodies, there may appear some reason to conclude, that a portion of the whole force becomes, as it were, masked in respect of the electrometer. Thus, taking two terms only, the force evinced by a single quantity, by the method of experiment above explained (20.), fig. 9, amounted to three grains, whilst the addition of a second equal quantity produced a force of nine grains, making a total of twelve grains : the mean of this would be six grains ; so that if, for the sake of illustration, it is admissible to reason in this way, at least one half the attractive force of which the first quantity is susceptible has been masked by the operation of some peculiar influence. Now this influence may consist in an electrical change induced by the redundant electricity in the superficial particles of the given substance, by which they exert on the accumulation, an attractive force of a greater or less extent, and hence, as in the examples above cited (21.), neutralize some of the force in respect of the electrometer. This is not altogether an hypothetical view, since the attractive force itself is evident (27. *f.*), and we know of no instance of electrical attraction unaccompanied by previous induction.

23. These considerations lead us to distinguish three elements peculiar to the conditions of electrical accumulation.

1°. The comparative quantity actually accumulated.

2°. The quantity not sensible to the electrometer.

3°. The quantity appreciable by the electrometer.

We may distinguish the first of these by the general term quantity, and the latter by the terms controlled and free quantity, or otherwise, controlled and free action.

24. We are here led to consider the more immediate acceptation of the terms tension and intensity as applied to electricity,—terms not unfrequently employed in this department of science in an indefinite sense. Tension denotes the elastic force of a given quantity accumulated in a given space, and is therefore directly as the density of the stratum ; and this I apprehend should be really the true sense of the term tension in electricity on the hypothesis that electricity is an elastic fluid. It is accordingly so accepted by many profound writers in physics*.

But the term intensity, as universally understood, must be taken in a somewhat different sense to this, since it has been invariably applied to the indications of the electrometer, and is immediately referable to what we have called the free action (23.), that is, to the operation of either a part, or the whole of the total force in a given direction up to the point of discharge: thus, for example, when a double quantity of electricity is accumulated on a given extent of surface, the action in the direction of the electrometer is four times as great. We must not, therefore, confound the terms intensity and tension (except by an especial convention in language), since by the

* HAÜY's Natural Philosophy.

hitherto universally received acceptation of the former, it relates especially to the indications of the electrometer, which are found by experiment, all other things being the same, to vary in certain cases with the square of the density; whereas the latter is expressive of the relation of the whole quantity accumulated to the space occupied, and is always in the direct simple ratio of the density*.

25. But in these reasonings on the probable source of electrical phenomena, we must not overlook the evidences in favour of electricity being a fluid, operating for the most part by attraction alone, without regard to its elasticity, according to the laws observable in cases of simple pressure, its peculiar property being a tendency to a state of equal action; hence it endeavours, when accumulated in given points, to flow upon surrounding masses, thereby producing currents, and the various phenomena of electrical induction.

26. It is not essential that I should here enter upon the merits of the above hypothesis; but supposing it to rest on an adequate induction of facts, then it is clear that the term tension would be ill applied, as expressing other than elastic power: we should rather employ some such term as pressure, which would be immediately associated with altitude or thickness of the electrical stratum. We might, however, still retain the term intensity as expressive of the operation of either the whole or part of the pressure in a given direction, and employ it to measure the quantity on a given surface by the aid of its known relations.

27. It has been supposed by the late Mr. Singer, in his excellent work on Electricity, that the diminished intensity observable in disposing a given quantity on an extended surface is altogether referable to the attractive force of the atmosphere, to the influence of which the electric particles become more extensively exposed: this view, however, seems inconsistent with experience.

1°. In disposing half the quantity on a given surface, we find the intensity reduced to one fourth; now the extent of the atmospheric contact is in this case unchanged.

2°. The attractive force exerted between electrified bodies and neutral non-conducting matter is inconsiderable, so as in some cases to be indefinitely small in respect of the more sensible forces under investigation: hence in experiments similar to those already described (20.), with an opposed semi-conducting or non-conducting plane *m*, fig. 9, the attractive force was found eventually to be exceedingly small.

3°. It is apparently at variance with more direct experiments, as in the following instances.

(*f.*) A brass ball, *b*, fig. 11, about two inches diameter, being placed in the centre of a large receiver, and extremely well insulated, was connected with the electroscope, A, by means of a brass rod passing airtight through a collar fixed in a glass plate and socket, *o*; a quantity of electricity was then communicated to the ball sufficient

* Should we employ the term intensity to designate any phenomenon of tension, it can only be to express its force in a given direction; we should therefore understand clearly what is expressed by the compound term intensity of the tension, as measured by the electrometer.

to diverge the electroscope forty degrees. Now this divergence remained when at least $\frac{59}{60}$ths of the air was withdrawn from the receiver. In this state of the exhaustion a similar ball, *b'*, in a neutral state, was made to approach the former by means of a similar sliding-rod and collar fixed in the side of the receiver: as the ball *b'* approached, the electroscope began to collapse, and again opened as it was withdrawn, so that at the point of contact, the divergence was permanently diminished.

Since the atmospheric particles in this experiment were to a great extent withdrawn, without any change being indicated by the electroscope,—whilst, on the contrary, its divergence became instantly decreased, and again restored on withdrawing the neutral ball *b'*, or otherwise permanently diminished on contact,—we may conclude, that the atmospheric influence was indefinitely small in respect of the indications of the instrument; and that the subsequent collapsing of the electroscope was occasioned by causes altogether connected with the metallic bodies themselves.

(*g*.) An excited gold-leaf electroscope, *c*, fig. 12, inclosed in an airtight bulb of glass so as to prevent any escape of the contained air, was placed on an insulated rod, and covered by a large receiver: the divergence remained unchanged when $\frac{69}{70}$ths of the air was withdrawn. On approaching an insulated ball, *n*, to the cap of the instrument, which also terminated in a large sphere, *c*, the leaves gradually closed*.

28. The decreased intensity observable by the electrometer (16.) may be referred therefore, partly, to the change of density of the electrical stratum arising from the diminished quantity in any given point, and partly, to the influence of the electrified substance itself, by which a portion of the force on external bodies becomes more or less masked, or controlled.

29. The conditions of the controlled action, in cases of electrical accumulation on coated glass, are precisely the same as those above mentioned (28.). A coated jar may be considered as a species of compound conductor, in which the controlling effect of the insulated coating in respect of the electrometer is greatly increased by its proximity to the other in a free state; hence a much greater quantity may be accumulated on a given extent of surface with the same intensity. The difference, therefore, between electrical accumulation on coated glass and that on simple conductors is only in degree of effect; the laws incidental to the electrified substance remain the same.

30. We may infer on the principles above exposed (21.), that the controlling force of bodies when electrified, in respect of the action exerted upon their electricity by those which are neutral, would continually decrease as the quantity accumulated on a given point increases, so that at last, by the superior force of the neutral body, it would become nothing, or very nearly so; hence a discharge ensues, for the force in the

* The facility with which electrified bodies retain their charge in rarefied air, under perfect insulation, and when removed sufficiently from the influence of neutral conducting substances, is somewhat at variance with the elastic hypothesis of electricity as generally understood. Having been at first led to adopt this hypothesis in all its generality, I was not prepared for such a result.

direction of the opposed substance is continually increasing; and at length, in virtue of its connexion with the mass of the earth, if it be in a free state, indefinitely great. This reasoning applies also to the discharge of an electrical accumulation between the coatings of a jar, the force in the direction of the discharging circuit, being at the instant of the discharge indefinitely great, in respect of the controlling force exerted on the accumulated electricity by the metallic coatings, taken either singly or as acting one on the other through the intervening glass.

31. The phenomena of tension and intensity as above explained, are quite independent of the effect of the whole quantity accumulated, when discharged through various substances. Thus, the heating effect of a given quantity, discharged through a metallic wire, under the same conditions of circuit, &c., is always the same, whatever may have been its previous tension or intensity, as relating to the conductors on which the accumulation has taken place; *e. g.* a given quantity, accumulated on coated jars, always produces the same heat in a metallic wire, *c d*, fig. 13, inclosed in the bulb of the electro-thermometer N*, and discharged by means of the drop-ball *f*, whether accumulated on thick glass, or on thin, or on a greater or less extent of surface, the number of jars and the length of the circuit being the same. Dr. Faraday, in his capital researches in magneto-electricity, has further shown, that the same is true in respect of the magnetic effects produced, as also in respect of the electro-chemical effects; we have therefore arrived at a distinguishing property of quantity, of great consequence to inquiries in this branch of science.

32. The circumstances attending the transmission of a momentary electrical current between two conductors, under the form of a dense explosion, merits, in relation to the above deductions, an attentive consideration.

When the attractive force operating between two conductors can overcome the atmospheric pressure, a discharge ensues between the nearest points of the opposed surfaces. In these points the force appears to become at length indefinitely great, in respect of points more remote, so that the whole quantity accumulated, is finally determined through them. Thus, the precise points of contact between two spheres being found, and the spheres subsequently separated by given distances measured between these points, it may be shown, that the respective quantities requisite to produce a discharge will vary with the distances directly.

(*h.*) A discharging electrometer, fig. 14, was so constructed that given distances might be obtained between the nearest points of the spheres *c c'* by means of a micrometer screw, *s*. This instrument being affixed to a jar, D, exposing about five square feet of coating, it was easy to estimate very exactly by means of the unit jar *u* (11.) the quantity of electricity requisite to cause a discharge at any given distance between the balls *c c'*. Under these circumstances it is found that the number of measures indicated by the unit jar, vary exactly with the distances between the nearest points of

* For a description of this instrument I may refer to the Philosophical Transactions for 1827, p. 18; also to the Transactions of the Royal Society of Edinburgh for 1832.

the balls *c c'* of the discharging electrometer. Similar results ensue in accumulating different quantities on simple conductors, the distances through which a discharge occurs in air of the same density being directly as the quantity accumulated.

33. In order to conduct these and other experiments on electrical attraction, by means of simple conductors, with greater accuracy, I employed the mechanical arrangement represented in fig. 15. It consists of an oblong base, *a b*, a portion of which, *m n*, may be drawn out to a certain length by means of an easy groove in which it slides. There is a micrometer-screw and frame, *f*, fixed on this sliding portion, which moves the insulating glass rod *q* between the guides *m o*, either backward or forward, and by very small quantities. On the distant extremity *a* of the base *b a*, is fixed a second insulating glass rod, *r*, which passing with friction through some compressed cork in the ball *r*, may be either elevated or depressed for an inch or more. By this machine two conductors, *h h'*, placed on the glass rods *r q*, may be exactly opposed to each other in the same right line, and may be also set to any given distance within the ·01 of an inch, measured between their nearest points, a graduated circle and index being affixed to the micrometer-screw at S for this purpose. We may also charge either of the conductors *h h'* with a given quantity of electricity, without the influence of the other, by withdrawing the sliding portion of the base *m n*.

(*h*.) Two conductors, *h h'*, fig. 15, being separated by a given distance, measured between the nearest points, one of them *h'* was withdrawn, so as not to influence the quantity which the opposed conductor *h* could receive. When this last *h* had been charged, then the conductor *h'* was again restored, in an uninsulated state, to its previous position, and the precise distance at which the discharge took place observed by a final approximation with the micrometer-screw *s*. This distance being found, the same was repeated when the conductor *h* was charged with only one half the previous quantity, and so on. In these experiments the distances of discharge varied directly with the respective quantities accumulated*.

34. Comparing these results with those before arrived at (17. 20.), it may be seen, that whilst the distances of discharge between two points increase in the simple ratio of the quantity, the attractive forces increase as its square.

35. This is not only applicable to discharges produced by different quantities disposed on the same conductor, but it is also true in disposing the same quantity on many conductors precisely similar, so as to double, treble, &c., the extent of similar surface: we have in all cases the distance of discharge, in a simple ratio of the quantity contained on a unit of similar surface.

36. The distance, therefore, through which an electrical accumulation can discharge in air of a given density, is an accurate measure of the comparative quantity contained

* I do not advert to these experiments as containing any very new or unexpected results in electricity, but in explanation of the application of particular methods of research, in demonstrating more completely than has been hitherto done, a class of facts essentially involved in the subject of these inquiries.

in an unit of space, or (supposing the electrical particles to repel each other) of the tension; now the attractive force evinced by the electrometer, and which we have termed intensity, is directly as the *square* of the quantity contained in a unit of space, and cannot be taken as a measure of the tension, except under this condition.

37. On reviewing these phenomena, as connected with the discharge of electricity between conductors, we may trace an interesting and consistent relation between them. If we call the force exerted between two points *c c*', fig. 14, at the instant of a discharge, unity, and we now suppose the balls to be placed with the same accumulation at twice the previous distance, then, according to the general law of electrical attraction, the force will be reduced to one fourth, since it varies in an inverse ratio of the squares of the respective distances (67.), at three times the distance it would be one ninth, and so on: hence the discharge could not occur at these distances with the same quantity. But since double, treble, &c., accumulations develop free quantities or intensities, which are as the squares of the whole quantity accumulated (16.), we have with double, treble, &c., quantities accumulated, attractive forces which exactly compensate the decreased force due to the respective increases of distance; and hence at the instant of the discharge at double, treble, &c., distances, with double, treble, &c., accumulations, the force is precisely the same; that is to say, it is in every case sufficient to overcome the atmospheric pressure at each given distance.

38. A similar result ensues when the same quantity is disposed on an increased surface of similar dimensions, where the distance of discharge (35.) becomes reduced in an inverse ratio of the surface: now in this case the intensity being as the square of the quantity contained in a given space, it decreases in the inverse ratio of the square of the surface (20. *e*.), whilst the attractive force increases in an inverse ratio of the squares of the respective distances (67.); hence in decreasing the distance between the discharging points, whilst at the same time the extent of surface is proportionably increased, we preserve the attractive force constant, and are thus enabled to overcome the atmospheric pressure at any required distance, as before.

39. It would seem to follow from this, that the resistance of the atmosphere to the passage of electricity is not really greater through any one discharging distance than through another, and is in no case greater than the existing pressure of the air; an induction which is found to correspond very completely with experiment.

40. I have examined carefully the influence of an atmosphere of variable density and temperature, in restraining electrical discharges, and have arrived at some interesting results; these are comprised in the following experiments.

(*i.*) The electro-thermometer N, fig. 13, being placed in connexion with the discharging electrometer *f*, the effects of given quantities of electricity discharged through the wire *d c* were carefully observed, the circuit *m h f r i d c n* being varied, both as to its extent and the nature of the substance, in the portion *i d*. A very few trials served to shew, that the effect on the wire decreased in some inverse ratio of the resistance to the transmission of the accumulated electricity; thus, the effect was less with a long circuit than with a short one; and when it consisted of imperfect conductors, such as wood, or water contained in a glass tube, the instrument was scarcely at all affected.

41. When long circuits of metallic wire were employed, the effect varied in an inverse ratio of the length. Thus, with an insulated circuit of 300 feet of thick copper wire, the transmission of a given quantity through the electrometer, N, elevated the fluid ten degrees; with a circuit of 600 feet, the resistance was such that it only rose between five and six degrees; with 900 feet, rather more than three degrees.

42. This law was not so fully apparent on circuits of 50 or 100 feet, circumstances, necessarily involved in the experiment, being such as greatly to interfere with an exact result on short lengths of metal; thus the final equalization of the electricity through the metallic coatings of the battery, as also through the connecting rods and the like, seemed of little consequence when great lengths of circuit were compared, but interfered considerably in small ones, the resistance of each comparative circuit being increased by this constant.

43. These experiments on the resistance of conducting substances to the transmission of electricity through them, will enable us better to appreciate the kind of resistance arising from a non-conducting medium, such as air, as in the following cases.

The electro-thermometer N, fig. 16, (Plate IV.) was placed in connexion with the opposed spheres *c c'* in the receiver R; the spheres were separable to a greater or less extent by means of a brass rod sliding through an airtight collar on the glass plate *p*, the distance being regulated by a micrometer-screw and index at *p*: the receiver was connected with a good air-pump at *r*, furnished with a long mercurial gage, *g*, and had within it a thermometer, R, to indicate the temperature of the contained air. The temperature could be raised considerably, when required, by means of a metallic envelop, fig. 18, and a powerful lamp at D: this envelop was so contrived as to be easily removed at the time of experiment, without disturbing the fixed pieces *q q'*, fig. 16, and cross-bars of glass *q y*, by which the expansive effect of the heated air on the plates *p p'* was effectually resisted.

(*j*.) A given quantity being accumulated in the jar E, it was discharged between the balls *c c'*, placed at different distances apart, within the extreme limit of the distance at which the accumulated electricity could of itself escape. In order to effect this, the jar was discharged by the drop-ball *f*, which was allowed to fall with force on a small plate of varnished glass *a*, placed on the opposed ball *a* in connexion with the positive coating: by this the transmission of the electricity became impeded up to the point of fracture of the glass, as appeared by the retention of the charge, when the ball *f* rested on *a*. The results of thirty successive experiments gave an invariable effect on the wire *c d*, at whatever intermediate distance the balls *c c'* were placed within the limits of the whole discharging distance.

By diminishing the density of the air, the discharging distance could be extremely increased: the effect, however, on the instrument remained the same.

44. The ball *c* being connected with the positive coating, and *c'* with the negative

coating, fig. 17, given quantities of electricity were accumulated, and the distances at which the discharge occurred in air varying in density observed, this series of experiments led to the following results:

(*k.*) 1st, The respective quantities requisite to pass a given interval, *c c'*, varied in a simple ratio of the density of the air. When the density was one half as great, the discharge occurred with one half the quantity accumulated; that is to say, with one fourth of the intensity or free action (16. 20.).

(*l.*) 2ndly, The distance *c c'* through which a given accumulation could discharge, was found to be in an inverse simple ratio of the density of the air, the intensity or free action being supposed constant. In air of one half the density, the discharge occurred at twice the distance.

45. These results are in complete accordance with the conclusion already derived (37.), since the attractive force between the points of discharge *c c'* was, 1°, varied by varying the whole quantity accumulated; 2°, by varying the distance: the force, therefore, was in each case the same (20. 67.), that is, as the square of the density of the air directly.

46. By diminishing the density, and increasing the distance between the points of discharge, we may very completely represent the beautiful phenomena of summer lightning: the electrical explosion approaches nearer and nearer to the state of a diffuse luminous flash without noise: and this also happens when the distance between the discharging points is the same, the quantity accumulated becoming continually reduced. We may hence infer, that in atmospheric discharges between clouds opposed to each other in air greatly rarefied, either by heat or by diminished pressure (44. *l.*, 50.), the electrical accumulation never proceeds beyond a certain limit; so that discharges in diffuse flashes, without noise, repeatedly occur, whilst the exciting cause of the electrical accumulation continues to operate.

47. The resistance of a column of non-conducting matter, such as air, to the passage of an electrical discharge, appears by the foregoing results (43.) to produce a somewhat different result to that of conducting bodies; the resistance in the former arising solely from the pressure of non-conducting particles, by which the whole accumulation is restrained within given limits. Now when the attractive forces are sufficiently great to remove the atmospheric column interposed between the points of discharge, the accumulated electricity escapes in a dense form between those points, without any regard to the distance traversed; and without any intermediate operation on the force of the electrical current, as in the case of electricity passing through an interposed circuit of metal of greater or less extent (40.).

48. I endeavoured to find, by varying the temperature of a given volume of air forcibly retained within the receiver R, figs. 16, 18, so as to prevent expansion, whether the influence of heat was such as to impair its insulating property. It may be here remarked, that the numerous experiments hitherto instituted, in order to show the conducting power of heated air, are by no means conclusive. The great source

of fallacy appears to consist in a neglect of a very important element,—the density of the air immediately operated on,—and which has great influence in the restraining of electrical discharges (44.). By means of the arrangement above described (43.), this source of fallacy is altogether avoided, and we are enabled to experiment on a constant volume of air of variable temperature.

(*m.*) The experiment being disposed as above stated (44.), an accumulation was effected sufficient to discharge through a certain interval of air of a given temperature, and whose volume was fixed by closing the cock *r*, fig. 16. This being ascertained, the temperature was varied from between 50 to 300 degrees of Fahrenheit, but without in the least affecting the result; the discharge invariably occurred when the same quantity was accumulated. The influence of heat was therefore evidently not in any way opposed to the restraining power of the air.

(*n.*) The heated air was now permitted to expand, by opening the cock at *r*, and allowing an escape through the long gage *g*, from under the surface of the mercury in the cistern *w*: when the full expansion had taken place, the cock was again closed. The thermometer within now stood at about 280 degrees. This preparation being accomplished, the quantity requisite to cause a discharge between the balls *c c'* was again determined; but although greatly reduced, it was found to remain the same through each succeeding decrease of temperature, as the whole gradually acquired the temperature of the room. When this was attained, the cock at *r* was again opened, in order to admit of the ascent of the mercury in the gage, and by which the density of the air in the receiver could be sufficiently well estimated. The comparative accumulation, as in the preceding cases (44.), was then found to be as the diminished density directly, or nearly so.

49. These experiments on the power of heated air to restrain electrical discharges, were varied in the following way: A portion of the air within the receiver R, fig. 17, was first withdrawn, so as to raise the mercury in the long gage *g* about five or six inches; a given accumulation was then effected, sufficient to produce a discharge between the opposed spheres *c c'*. The receiver was now heated as before, and the descent of the mercury in the gage observed. By this method the actual tension of the air within could be estimated, whilst the expansive force on the plates *h h'* terminating the receiver, was efficiently resisted by the atmospheric pressure from without, so that the plates did not require further support. The results were the same as those before arrived at. The insulating power of the air was found to be quite independent of its temperature, and to depend only on the density.

50. We may conclude from these experiments,—1°. That heated air is not, as frequently stated, a conductor of electricity, and that heat does not facilitate electrical transmission through air in any other way than by diminishing its density;—2°. Supposing heat to be material, it is a non-conductor of electricity; because the incorporation of a conducting with a non-conducting substance is found to impair the insulating power of the latter, as in the case of air charged with free vapour;

whereas, in the intimate union of two non-conductors, the insulating power remains perfect. Since, then, heat does not impair the insulating power of a given volume of air, heat, if a substance, should necessarily have non-conducting properties.

51. The converse of this reasoning furnishes additional evidence in favour of the above conclusion; it is a well-known fact, that the excitation of heat in good conductors, such as the metals, is inimical to their conducting power. This result always ensues in mixing a conducting with a non-conducting substance, and is also evident in amalgamating a good conducting metal with an inferior one *.

This curious effect of heat in impairing the conducting power of metals, has been clearly and beautifully illustrated by Sir Humphry Davy †. I have also arrived at similar results ‡, and find, as stated by him, that heat in any way excited in metallic conductors, whilst transmitting an electrical current, tends to impair their conducting power. Mr. Christie, likewise, has observed the same fact, as appears in his last interesting paper on the Laws of Magneto-electric Induction §.

52. Although the experiments in evidence of this influence of heat or metallic conductors are numerous and very conclusive, yet opposite views have been advanced by Dr. Ritchie in his paper on Electric Conduction ||. Dr. Ritchie's principal experiment consists in transmitting common electricity over a forked iron rod, one of the legs of which he heated to redness: he finds, under these circumstances, that the electricity will rather pass from the heated side, than from the cool side; but this result cannot be taken in evidence of the superior conducting power of the heated iron, so long as the experiment is made in air, since, as has been just shown (48.), air rarefied by heat, loses to a greater or less extent, its restraining power. Now the air immediately in contact with an iron rod heated to redness, is necessarily in an extremely rare state: hence the impaired conducting power of the metal becomes more than compensated by the diminished resistance on its surface; so that the conducting power of the metal, together with the greatly diminished density of the air on the one side, may still afford an easier passage to the electricity than the conducting power of the metal alone on the other (44.). It is hence essential, in such an experiment as that proposed by Dr. Ritchie, to place the bent iron rod in a well-exhausted receiver before any fair conclusion can be drawn as to the influence of heat on its conducting power. Of this the talented author of the paper alluded to seems to be in a great measure aware, as appears in his account of his seventh experiment. Dr. Ritchie has, however, taken an objection to one of the many phenomena so decisive of this important question: he appears to think that the effect of a heated wire would be a species of electrical evaporation from its surface; but it will be immediately perceived that this notion is purely hypothetical. Electricity is never found to escape from a

* Philosophical Transactions, 1827, p. 18.

† Ibid., 1821.

‡ Transactions of the Royal Society of Edinburgh, 1832.

§ Philosophical Transactions, 1833.

|| Philosophical Transactions, 1828, p. 373.

body, even *in vacuo,* except to flow upon some other body towards which it tends: thus, in the experiment of charging a jar under an exhausted receiver, the electrical current invariably flows from one coating to the other. If the rod of a charged jar be caused to project into the middle of a large receiver, the charge will not leave the jar; for the ball in which the rod terminates is still without the influence of the points of attraction toward which the electricity would otherwise tend. In short, electrical currents generally, may be shown to be almost exclusive actions between given points (74. 78.). Independently, however, of these considerations, it is evident, that the excitation of heat is the sole cause of the less effective transmission of the electricity. Thus, a fine wire passed through an exhausted receiver, has its conducting power impaired when heated: now in this instance the atmospheric pressure is extremely diminished, as well for the wire in its cool state, as when subsequently heated. Moreover, the converse of this experiment, the increased conducting power by the application of cold to the wire, is equally demonstrable: thus, a wire under the ordinary atmospheric conditions has its conducting power greatly increased by evaporating ether from its surface *.

53. Although the disposition of electricity on insulated conductors is subject to the laws above deduced (16.), and which are invariable when the surface remains the same, or is perfectly similar in respect of dimensions and form, yet these laws do not appear, under every condition, incidental to the conducting surface. It has been already observed by Volta, that extension in length greatly contributes to increase the capacity of a conductor; so that of two plane surfaces of equal area, that which has the greatest extension has also the greatest capacity for electricity. I have pursued this interesting fact, and have arrived at some further results which seem of importance.

(*o.*) Having procured some rectangular plates of equal area, such as represented in fig. 19, whose figures varied from a circle, through a square, up to a long parallelogram, I submitted them to experiment, according to the methods already described (14.). Each plate was placed in connexion with the electrometer *a*, fig. 2, and a given quantity of electricity transferred on it, from a jar charged to a known extent, by means of a small insulated transfer plate. After a few trials it became evident that the intensity varied in an inverse ratio of the perimeter of the respective plates, the differences being inconsiderable between the circle and square, but more decided as the area became extended in length. Thus, in the parallelograms *a, d, e,* fig. 19, the intensities, as corresponding with the dimensions, were as in the following Table: these intensities have been calculated for a distance = 0·5 of an inch between the attracting surfaces (10.); and it may be observed, that in these instances, the numerical agreements are sufficiently near.

* Transactions of the Royal Society of Edinburgh, 1832; also Philosophical Transactions, 1821, p. 425.

TABLE II.

Area = 75 square inches.

Dimensions.		Perimeter.	Intensity.	Parallelograms.
Length.	Breadth.			
		Inches.	°	
12·5	6	37	9	*a*
25	3	56	6	*d*
54·5	1·4	112	3	*e*

54. At first, these results led me to believe, that the diminished intensity was caused by the increased extent of edge acquired by the plate, when its area was extended in length; but after a careful inquiry I found this was not the case: the same plates formed into cylinders, either in the direction of their lengths or breadths, evinced with the same quantity, precisely the same intensity, which may be considered as a somewhat novel result. The intensity of a sphere also, was found to be the same as that of a plane circular area of the same superficial extent; neither did any differences arise in turning the plates into other figures approaching cylinders, such as triangular and hexagonal prisms. The mere circumstance of the extent of edge, therefore, has evidently no influence on the intensity: hence the increased capacity would seem to arise from some peculiar disposition of the electricity depending on the form of the conductor; it has accordingly been considered by VOLTA, to consist in the removal of the electrical particles further without the sphere of each other's influence. On reviewing these phenomena, we must therefore consider the perimeter as being merely a function of the peculiar kind of extension to which the given area has been subjected, and by which the electrical particles have become so placed in respect of each other, that their operation on external bodies is diminished. For the sake of clearness, therefore, and to avoid a direct association of the cause of the diminished intensity with the extent of edge acquired by the plate, it may be perhaps advisable to consider the intensity as more immediately dependent on the form of the respective plates, the area being constant; which equally well coincides with the results before deduced.

55. The greatest intensity of a given quantity of electricity, disposed on a given area, will appear, therefore, when the area is contained under a circle *c*, fig. 18; and the least, when expanded into an indefinite right line, as is shown also by experiment.

56. The intensities of conductors being inversely as their perimeters, when the area is constant, I thought it not unlikely that the intensity might also vary in an inverse ratio of the area, when the perimeters remained the same.

(*p*.) With a view of ascertaining this, I procured some additional plates, such as *m n*, *m n'*, fig. 20, which were so constructed, that their perimeters did not materially differ, whilst their areas greatly varied: these being submitted to experiment, as before (14.), the respective capacities were found to be in a simple inverse ratio of the areas. Thus, when the area *m n'*, fig. 20, was doubled, so as to become equal area *m n*, whilst at the same time $m\ h + h\ n$ equalled $m\ h' + h'\ n'$, the intensity was only one half as great.

57. Since, then, the intensity of a rectangular plate, is inversely as its perimeter when the area is constant, and inversely as its area, when the perimeter is constant, it follows that the intensity must vary inversely with those quantities jointly; or calling I the intensity, A the area, and P the perimeter, we have

$$I \propto \frac{1}{A\ P}$$

This, however, is on the supposition that the quantity of electricity is constant; but if the quantity varies, whilst the form and size of the conductor remain the same, then from the results obtained (20.) the intensity, is as the square of the quantity; therefore, if x represent the number of measures of electricity (12.), we have

$$I \propto \frac{x^2}{A\ P}$$

Now the *capacity* of a conductor, is measured, by *the quantity* of *electricity* it *can receive under a given intensity*; and from the above formula we have

$$x^2 \propto I\ A\ P$$

If, therefore, we take the intensity constant, x will represent the capacity, and we shall have

$$\text{Capacity} \propto \sqrt{A\ P}$$

To obtain, therefore, rectangular plane conductors, having capacities, double, treble, &c., of a given conductor, we must construct them so, that the areas and perimeters shall be also, double, treble, &c., of the first respectively; a deduction which is in a great measure confirmed by the following experiments.

(*q*.) A circular plate *a*, fig. 2, being placed in connexion with the electrometer, a given quantity of electricity was transferred on it by means of a well insulated plate of given dimensions. The intensity being observed, a second equal quantity, transferred as before, was added to the former, when, according to the general law (16.), the resulting intensity amounted to just four times the first.

The electricity of the different bodies was now neutralized, and a transfer-plate applied to the jar, the *area* and perimeter of which, was just double that of the former. This plate being deposited on the circular area in contact with the electrometer, the intensity was found to be exactly the same as that produced by two contacts of the first plate (14. *b*.). In a similar manner, a transfer-plate of a treble area, and treble linear boundary, abstracted from the jar as much electricity at one contact, as the first did by three successive contacts; but this result could not be obtained under any other disposition of the areas of the respective plates.

It may perhaps be requisite to observe, that slight differences may occasionally arise when the same transfer-plate is employed for successive transfers of the electricity, in consequence of again withdrawing a small portion of the charge deposited on the circular plate; commonly, however, this is of little consequence, the capacity of the large plate being very considerable in respect of the capacity of the smaller one. We may, however, avoid the discrepancies by means of two or three small plates, precisely equal, so as to place each in succession on the larger one.

(*r.*) Two plane conductors being alternately connected with the electrometer, whose areas and perimeters were, in one, double of the other respectively, the intensity of a given quantity, when disposed on them, was, according to the general law (16.), in an inverse ratio of the square of the surface. This law, however, did not obtain when the area only was double, without regard to the perimeter. Thus, in two circular plates or parallelograms, in which the area of one plate, was double of the other, the lengths of the latter being in the ratio of 2 : 1, the respective intensities were found to be very nearly in an inverse ratio of the areas; a somewhat remarkable fact.

58. We may conclude from these phenomena, that the intensity does not vary in an inverse ratio of the square of the surface, according to the general law (16.), except when the areas are so disposed; that the whole perimeter of the various plates, is as the respective surfaces; a result which applies also to cylindrical conductors, the electrical capacities of these being the same as the plane areas, into which we may conceive them to be expanded (54.).

59. The curious fact, that the capacity of a sphere or cylinder is the same as that of the plane area into which it may be supposed to be rectified, seems to afford some new views in electricity. We find in the case of electricity accumulated on a hollow sphere, that a conducting substance, insulated and placed entirely within the sphere, remains in a neutral state; from which it has been inferred that the charge resides only on the exterior surface. Now the intensity of a sphere being the same as that of a plane circle of equal area, it should follow that the distribution is in each alike, since it is difficult, from any known fact, to suppose a given quantity of electricity expanded over twice the surface, as may be inferred in the latter case, and yet maintain the same intensity: the redundant electricity, therefore, if the above deduction be true, should be also disposed on one side of the plate only, notwithstanding that it may be determined to either when operated upon by a neutral body.

60. The great difference in the condition of an electrified sphere and that of a plane of equal area, seems to consist in the difference of the relation of one of the surfaces of the sphere in respect to neutral bodies. It may be observed, in the case of a neutral body becoming electrified from either side of the plane area, that some portion of the body is always elevated without the surrounding plane; and if a similar condition be fulfilled in respect of the interior surface of a sphere, there will remain no difficulty in obtaining electricity from that surface, and as readily as from the other: thus, a substance insulated within the sphere, at the extremity of a conducting rod, projecting in

the least beyond the interior, as in fig. 21, becomes immediately electrified when the sphere is charged with electricity.

61. We have yet to notice another seeming exception to the general law (16.) observable in accumulating variable quantities of electricity on insulated conductors, and which is found to occur in that peculiar kind of accumulation induced in a body by electrical influence.

Many striking facts lead us to conclude, that excitation by induction, as above stated (4. 5.), is the immediate effect of a tendency of accumulated electricity to a given state, or mode of existence: hence an electrified substance is observed to exert a peculiar kind of influence upon surrounding bodies. The immediate result of this influence, is a sort of temporary change, or displacement, of the electricity which these bodies already possess; so that if they be insulated, a species of accumulation is apparent in certain parts of them, depending upon a new disposition or state of their own electricity. Now the attractive force thus induced in a neutral substance, by the immediate influence of a charged conductor, appears to be as the quantity of the free electricity in operation directly, that is, as the intensity or exciting cause; and as the simple distance between the points of the opposed bodies inversely, which may be gathered from the following experiments.

(*s.*) Two conductors, *h h'*, fig. 15, terminating in plane surfaces, as in fig. 22, being insulated on the stand fig. 15. above described (33.), one of them, *h*, was connected with the electrometer, fig. 2, and the other charged with a given quantity (14.), whilst withdrawn from the influence of the former. These two conductors were now placed within a known distance of each other, and the induced force in *h* observed: after numerous repetitions of this experiment, the distances between the conductors being varied, I found, that the force induced in the distant extremity of *h* was in the simple inverse ratio of the distances between the opposed surfaces.

(*t.*) When the distance was constant, and the accumulated quantity in *h'* variable, then the induced force in *h* varied with the square of the accumulation in *h'* directly.

62. I repeated these experiments with the balance, and with an electrical jar, according to the method already explained (19.), fig. 9, and found the result invariable. In this latter case the conductor *h* was immediately connected with the jar by a straight wire, and *h'* with the insulated ball *b*, so as to interpose the conductors between the jar and the balance, the distance between the opposed surfaces being adjusted by the micrometer-screw *f*, fig. 15.

63. Assuming in these experiments, what is quite consistent with strict philosophical reasoning, that every effect is directly proportionate to its cause, we have additional evidence of the law already deduced (16.); since to excite an attractive force in a distant body, varying as the square of the quantity of electricity accumulated in the exciting conductor, the free quantity, or intensity, in the latter must at least vary in the same ratio; hence it follows that with a double quantity accumulated there is four times the intensity, or free action.

64. Upon considering attentively the march of the attractive force in the body h excited by induction, it would appear that the intensity of the induced accumulation is not subject to the same law as observed in permanently electrified bodies (16.), the force in the one case being as the induced accumulation simply, in the other as the square of the quantity communicated.

The cause of this difference may possibly be traced to an essential difference in the nature of the respective accumulations. In the induced accumulation, the attractive force arises from the change effected in the electricity originally possessed by the body itself: hence the accumulation by induction may be considered rather as a species of electrical development in the neutral body. Now the quantity of developed electricity being altogether a free quantity, it must consequently be always as the exciting cause directly, all other things being the same; the induced action on the neutral body will therefore be always as the free action of the electricity accumulated on the charged conductor, as is shown by experiment (s.).

65. This influence of free electricity on a neutral conductor which we have been just considering, is quite independent of atmospheric pressure, it being precisely the same in a partially exhausted receiver as in air. I examined the effects of electrical influence in a rarefied medium, by means of two conductors, h h', attached to rods, passing through the sides of a spherical receiver, as represented in fig. 22. These conductors were separable to a greater or less extent by micrometer-screws, s s', acting on the rods, by which the conductors were sustained, the rods being moveable through airtight collars.

(u.) By connecting one of the rods with the electroscope A, fig. 1, or with the insulated ball b acting on the balance, fig. 9, or with the electrometer, fig. 2, and the opposite rod with a charged conductor or jar, as in the previous cases (17. 18.), the induction between the opposed bodies h h' under different atmospheric pressures was easily observed.

The results of numerous experiments led to the conclusion, that the operation of electricity on distant bodies by induction, is quite independent of atmospheric pressure, and is precisely the same in vacuo as in air; a result which was demonstrable when three fourths of the air was withdrawn from the receiver, the charge employed being such as could be retained on the conductor h under the influence of h' placed at three inches' distance.

66. These experiments were varied by giving the neutral body h' a temporary connexion with the ground, whilst exposed to the inducing action of h: in this case, as in that above described (21.), the induced effect upon the neutral body is not sensible so long as the accumulation remains on the charged conductor; but on reducing this last to a neutral state, the divergence of the index of the electroscope, or otherwise the attractive effect as indicated by the electrometer (A), or the balance (N), fig. 9, is immediately apparent.

The general result by this method was the same precisely as in the preceding experiments, the subsequent effect on the electrometer being quite independent of the presence of the air.

67. The law according to which the force of electrical attraction varies, when exerted between bodies at different distances, has been justly considered by many profound philosophers an important object of physical research: it may be satisfactorily arrived at by the methods of experiment so frequently referred to in the course of this paper (20.). The results are for the most part of an extremely simple kind, without any complication; and being strikingly illustrative of an influential law, applicable to many forces in nature, they may not be altogether undeserving of attention.

(*v.*) A weight of eighteen grains being placed in the pan *t*, fig. 9, the parallel and even surfaces of the opposed bodies *m m'* were placed at 0·5 of an inch distant. A given quantity of electricity was now accumulated in the jar E, the attractive force of which just balanced the given weight, the quantity being determined by means of the unit jar *u*. The distance between the bodies *m m'* being now increased to an inch, that is, to twice the former distance, and the same quantity again accumulated in the jar, the attractive force was found equivalent to 4·5 grains precisely. In like manner a weight of two grains exactly balanced the former force, when the distance between the bodies was increased to 1·5 inch, or three times the first.

In these experiments, the attractive forces varied as the squares of the respective distances inversely with great precision; a law of much importance in its consequences, but which has received but comparatively little elucidation from methods of research not involving complicated conditions.

68. By substituting the electrometer, fig. 2, for the balance, the same law is immediately arrived at, either with simple electrified conductors, in the way already described (14.), or otherwise by means of a coated jar, as in the preceding case. When the electrometer is employed, we may compare readily the force in degrees with the distance between the attracting surfaces, the quantity being constant, and hence, as before, arrive at results which present nothing but the mere effects of the law under investigation, as given in the following Table.

Table III.

Distance.	Force.
Inches.	°
0·5	20
0·8	8 —
1·0	5
1·2	3·5 —
1·5	2 +

The approximations in the above Table are so close, that the numerical results may be taken as exact.

69. The law observable in the preceding investigations is immediately apparent

when the opposed surfaces are parallel planes or rings; but in the case of spheres or bodies of other forms, the experiment assumes a somewhat complicated character. I have succeeded, however, in reducing it to extremely simple conditions, by the aid of some further inquiries into the peculiar mode of action of the attractive force, the results of which merit an attentive examination.

1°. The attractive force exerted between an electrified and a neutral uninsulated conductor, is not at all influenced by the form or disposition of the unopposed portions. The force is precisely the same, whether the opposed bodies are merely circular planes, as represented in fig. 23, or are otherwise backed by hemispheres or cones, &c., as in figs. 24, 25; hence two hemispheres were found to attract each other with precisely the same force as the spheres.

2°. The force is as the number of attracting points in operation directly, and as the squares of the respective distances inversely (67.); hence the attractive force between two parallel plane circles being found, the force between any other two similar planes will be given.

3°. The attractive force between two unequal circular areas, is no greater than that between two similar areas, each equal to the lesser.

4°. The attractive force also of a mere ring and a circular area on each other, is no greater, than that between two similar rings.

5°. The force between a sphere and an opposed spherical segment of the same curvature, is no greater than that of two similar segments, each equal to the given segment: thus, the attraction between the sphere m and the uninsulated segment n, fig. 26, is the same as that of the similar and equal segments n n'.

These results have been arrived at by the same methods of research as those above given (19.), figs. 9, 2. The intensity in the different experiments is supposed to be the same, the electrified body being connected with a charged jar of such capacity that trifling differences in the dimensions of the conductors connected with it may be considered as indefinitely small.

70. A careful induction from the above facts, led me to consider the attractive force exerted between a charged and neutral sphere of equal diameters, as being made up of a system of parallel forces, operating in right lines between the homologous points of the opposed hemispheres, a conclusion quite in accordance with what has been already shown (21.); for these being in exactly equal and opposite electrical states, and similarly placed, each two corresponding points should exactly neutralize each other's action in respect of points more distant. The whole force also may be further considered to be as the number of attracting points directly (69.), and as the squares of the distances inversely (67.), and to be no greater than that arising from the opposed hemispheres (69.).

71. These simple conditions, enable us to determine a point q q' within each hemisphere, in which the whole attractive power may be supposed to be condensed, and to exert the same force as if emanating from every point of the hemisphere. The exact

position of this point within the surface, will depend on the distance between the nearest points of the spheres, and may be readily found by the expression $z = \frac{(a^2 + 2ar)^{\frac{1}{2}} - a}{2}$, a being the distance between the nearest points of attraction, and r the radius.

The points $q\ q'$ being thus determined for given distances between the spheres, the whole force should vary between them, according to the general law (67.), and also as $\frac{1}{a(a+2r)}$, that is, inversely as the distance between the nearest points, multiplied into the distance between the centres, as shown in sec. (72.)*.

72. These deductions accord very completely with experiment, so nearly, indeed,

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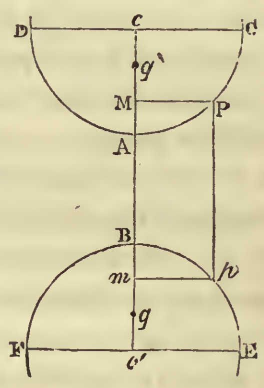

* Let C A D E B F be two hemispheres, attracting each other at distance A B.
Let $AB = a$, $AM = x$, $PM = y$, $AP = s$, $r =$ rad., and $\pi = 3{\cdot}14159$.
$\mu =$ absolute force exerted by a unit of force at a unit of distance.
$Pp = Mm = (a + 2x)$.

Then unit of force at distance $Pp = \frac{\mu}{(a+2x)^2}$.

Now circumference whose radius $= PM$ is $= 2\pi y$,
And annulus, whose breadth $= ds$, $= 2\pi y\, ds$;
$\therefore$ Force exerted by an indefinitely small annulus at P on a corresponding annulus at $p = 2\pi y\, ds \times \frac{\mu}{(a+2x)^2}$.

But in circle $y\, ds = r\, dx$;

$\therefore$ Force of annulus P on annulus $p = \frac{2\pi\mu r\, dx}{(a+2x)^2}$, the corrected sum of which $=$ total force from A to P. Now

$$\int \frac{2\pi\mu r\, dx}{(a+2x)^2} = -\pi\mu r \frac{1}{a+2x} + C.$$

If $x = 0$, we have

$$-\pi\mu r \frac{1}{a+2x} + C = 0, \text{ and } C = \pi\mu r \frac{1}{a}.$$

$$\therefore -\pi\mu r \frac{1}{a+2x} + C = \pi\mu r \left(\frac{1}{a} - \frac{1}{a+2x}\right), \text{ when } x = 0.$$

When $x = r$, this expression becomes $2\pi\mu \frac{r^2}{a(a+2r)} =$ the force upon the whole hemisphere.

Now area of hemisphere $= 2\pi r^2$, and if $q\ q'$ be the points in which we may suppose the whole force of each hemisphere to be concentrated, we have, putting $Aq' = Bq = z$,

$$\frac{2\pi\mu r^2}{(a+2z)^2} = \frac{2\pi\mu r^2}{a(a+2r)}, \text{ or } \frac{1}{(a+2z)^2} = \frac{1}{a(a+2r)},$$

that is, $(a + 2z)^2 = a(a + 2r)$,

$$\therefore a + 2z = \sqrt{a(a+2r)} = (a^2 + 2ar)^{\frac{1}{2}},$$

$$\text{and } z = \frac{(a^2 + 2ar)^{\frac{1}{2}} - a}{2}.$$

When both hemispheres are equal, as we have supposed, and the distances variable, the attractive forces will vary as $\frac{1}{a(a+2r)}$.

that the weight requisite to balance the force exerted between two equal spheres at given distances may be invariably predicted with extraordinary precision.

The following Table exhibits the results of a few experiments with two equal spheres, obtained by the method already given (19.), the radius of the spheres being each = one inch.

Table IV.

Dist. of Centres $= c c' = a + 2 r$.	Dist. of nearest points = A B = a.	Dist. of Points $q q'$.	Force in Grains.
2·3	0·3	0·83	15
2·5	0·5	1·11	8·25 +
2·8	0·8	1·49	4·6 +
3·0	1·0	1·73	3·5 −

73. With a view of verifying the above results, I obtained two circular areas, each equal to the area of the given hemisphere expanded into a plane: these were opposed as before, and were placed at the distances $q q'$ given in the above Table, so as to pass through the points $q q'$. The experiments being repeated with the planes, the attractive forces were extremely near those deduced by the hemispheres; indeed, upon a mean of five observations for each distance, there did not arise any sensible difference.

The various planes, spheres, and other conductors employed, were constructed of light wood, neatly covered with gold-leaf: those intended to be suspended from the balance were made hollow. I repeated the experiments with the electrometer B, fig. 2, and arrived at similar results.

74. Upon a due consideration of these, and the preceding experiments in air of diminished density, we are led to conclusions of no inconsiderable consequence to our views of electrical action. By the latter, (45.), it is demonstrated, that the resistance of the air to the passage of electricity, is as the square of the density directly, so that a given quantity, having a given intensity, and about to discharge or flow upon a given point, will remain in the same relative state in air of half the density, if the distance between the points of discharge be doubled (44. *l*.); or generally, if as the density of the air be decreased, the distance between the points of action be increased, the electrical accumulation will still remain complete. If, therefore, the density of the air be indefinitely diminished, and the distance between the points of action indefinitely increased, we shall have eventually the same relative electrical state continued, without dissipation; so that if we imagine the opposed body c', fig. 17, to become nothing, then the accumulated electricity will not tend to leave the electrified body c at all, supposing it to be without the influence of all other substances. Discharges of electricity under a diminished atmospheric pressure, therefore, do not seem to occur so much in consequence of a tendency of the electric principle to evaporate, as it were, in all directions into space, but rather in consequence of the removal of the non-conducting

particles interposed between the points, *from* and *toward* which, the accumulated electricity tends to flow. It is hence extremely doubtful, whether a general distribution of electricity in mere space would ever occur, supposing the electrified substance to be the only existing body in the universe: directly, however, that we assume the existence of another body, then in a space devoid of resistance, the resulting induction would generate an attractive force, which, however small, would cause an electrical current to flow through a distance, however great.

75. In accordance with this deduction, it may be shown, that an electrified sphere having an extremely perfect insulation, and projecting within the centre of a very large receiver, retains its electricity more completely under a diminished pressure, than in the atmosphere; especially under ordinary conditions of aerial currents, imperfect insulation arising from extraneous bodies, and the like. This fact seems to have hitherto escaped detection, and therefore, the notice it merits; and I am inclined to believe, that we may eventually find it requisite to modify, to some extent, our views of the cause of electrical dissipation. The following experiment is of singular interest as bearing upon this point.

(*w.*) A small brass sphere of about two inches in diameter *b*, fig. 11, was placed immediately in the centre of a very large globular receiver, by means of a brass rod projecting into the receiver, and cemented airtight by an appropriate flanche of brass and sealing-wax. The exterior extremity of the rod was connected with a delicate electroscope, and the sphere charged with a given quantity of electricity. Under these circumstances the air was gradually withdrawn from the receiver, but no sensible collapse of the electroscope had occurred when $\frac{59}{60}$ths of the air was withdrawn.

76. Common electricity traverses with greater or less facility, under an *adequate attractive force*, the surface of any substance relieved from the pressure of a non-conducting medium. If a glass rod, or a rod of wood, be passed through a tall receiver, and be opposed to a point projecting from the conducting plate covering its upper extremity, then on exhausting the air and continuing to electrify the insulated plate, we shall eventually perceive electrical streams flowing over the rod; and if we substitute a small wire for the rod, the same thing happens, except that the streams do not usually appear in the surrounding glass, presenting in each very beautiful phenomena.

77. Discharges of common electricity are transmitted in this way more readily on the surface of bodies, in an exhausted medium, than voltaic currents, the latter requiring but little comparative insulation: it is difficult to fuse a fine wire in an extremely exhausted receiver by ordinary electricity, whilst voltaic electricity will soon heat it to redness. I have discharged upwards of twenty-five square feet of coated glass upon a fine wire of iron, inclosed in a well exhausted receiver, without in the least affecting it; the redundant electricity appeared to find an easier passage through the rarefied air on its surface, producing an extremely brilliant effect; whereas on ad-

mitting the air, the wire was immediately fused by only a single jar, exposing not more than five square feet of coating.

78. It would therefore seem impossible to prevent the flow of electricity between bodies in a space altogether void of resistance, so long as the least attractive force is exerted between them, or otherwise to restrain a similar current, in a less perfect void, with an attractive force between the bodies proportionate to the square of the density of the resisting medium. I have succeeded, by means of a very powerful electrical machine, in the transmission of continuous electrical streams through long exhausted tubes of above four inches in diameter and upwards of six feet in length. The phenomena, beside being very instructive, were of peculiar beauty. The extremities of the tubes were ground airtight to brass plates, A *a*, fig. 28, each plate being furnished with a projecting point. When this long receiver was moderately exhausted, and the plates connected with the positive and negative conductors of the machine, luminous streamers ensued, branching upon the sides of the receiver toward the negative plate. When the upper plate A was connected with the positive conductor, and *a* with the negative, the currents appeared as in fig. 29; and when these connexions were reversed by connecting A with the negative conductor, and *a* with the positive, the currents appeared as in fig. 30. If either of the plates had its connexion with the negative conductor removed, so as to leave it insulated, then the flowing from the opposite plate ceased. As the exhaustion was more complete, the distinctions of the branches gradually became less, so that finally the whole interior surface of the glass was covered with a continuous mass of white light.

In no case did the electricity appear to be transmitted through the intermediate space, except in the act of flowing from the points upon the interior surface of the glass: when it is possible, however, to cause the electric matter to pervade the partially exhausted space, it is frequently attended by a sort of beautifully luminous glow.

79. Much discussion has occasionally arisen in this department of science respecting the conducting power of a vacuum; but surely this must be regarded as a somewhat anomalous form of expression. If by a vacuum we are to understand the absence of all matter, and to consist in mere vacant space, it seems unphilosophical to suppose it endowed with any positive quality whatever. It cannot, therefore, have either conducting or insulating properties, but must be a mere passive condition, under which an electrified substance may be imagined to be placed. Hence, as already stated (74.), an attractive force, however small, exerted between two bodies so circumstanced, must cause electrical currents to flow through a distance however great; the only difference would probably be the absence of the electric light usually observed in transmitting electricity through an imperfect void, as may be gathered from the fine researches of Sir Humphry Davy on this subject*.

80. With a view of accommodating the phenomenon of electrical divergence to the

* Philosophical Transactions, 1822, p. 64.

hypothesis of FRANKLIN and ÆPINUS, many acute inquirers have contended, on the authority of the Earl of STANHOPE*, that the recession of electrified bodies is dependent on atmospheric attraction, and that such recession would not occur in a void; whilst others, in accommodating electrical divergence to the hypothesis of the French philosophers, endeavour to show, that although greatly dependent on the presence of an atmospheric medium, it still arises out of a repulsive force existing in the elements of the electrical principle itself†. It was upon the above statements that I adopted, by way of precaution, the method of using the gold-leaf electrometer in experiment (*f.*), fig. 12. My subsequent researches, however, with electrified bodies in receivers, more or less exhausted of the contained air, led me to investigate this point very rigorously; and I am inclined to think that the following fact, taken in connexion with the preceding (44. 74. 75.), will go far to show, that any explanation of the phenomenon of electrical divergence involving the necessity of an atmospheric action, upon any principle of mechanism whatever, is likely to be quite fallacious.

(*x.*) Two gold-leaves were suspended in free space from a stout brass wire supported horizontally on a long insulating stem of glass: these being electrified so as to diverge freely, were covered by a capacious receiver, made extremely dry, and somewhat warm within; the insulating glass stem also being varnished, was warmed with a stick of burning charcoal. The leaves did not, under these circumstances, cease to diverge when the receiver was exhausted of its air to the greatest extent which could be effected by a moderately good air-pump of the common kind. Dr. TURNER has been so good as to repeat the experiment with a more perfect apparatus, and he finds the divergence equally perfect when only $\frac{1}{300}$th part of the air remains in the receiver.

81. Experiments of this kind, *demand the most perfect manipulation* and the most rigorous mode of investigation, without which we are extremely liable to be deceived by appearances: thus, the slightest deposition of moisture on the insulations becomes fatal to a delicate experiment in vacuo, as also the proximity of conducting bodies (44.). Two bodies also will frequently seem to open by a sort of flotation, on admitting air into the receiver, however carefully the operation be managed; whilst, in the electrization of bodies suspended from rods passing into receivers through brass plates, the electricity is liable to dissipation from the causes above assigned (78.).

82. Upon a careful review of these inquiries, it would seem, that the more immediate cause of electrical phenomena may be traced to certain peculiar states or conditions under which common matter may become placed in respect of an extremely subtile and universally pervading agency; from which results an attractive force, and, in the absence of an equivalent resistance, electrical currents. When these states, which, for distinction sake, we may term electrical, are incomplete, they are made perfect by the process termed induction; in which case the attractive effect immediately ensues: when they already exist, they become still further increased by the same process, and a similar result happens. When the tendency to produce these

* SINGER's Electricity, p. 24.

† HAÜY's Philosophy.

peculiar states is exerted between bodies whose electrical conditions are such as to be subversive of the inductive influence, then the bodies recede from each other. Such is, in plain terms, the amount of our experience of the nature of electrical attraction and repulsion; and every hypothesis of a more refined and extended character must include these elementary actions.

83. Electrical divergence is, unquestionably, an extremely intricate phenomenon. If it be assumed to depend on a repulsive force immediately impressed upon the molecules of certain kinds of matter, then it must be admitted to be a species of repulsive action essentially different from any repulsive agency in nature of which we have the least experience. Its operation is at great distances, and is exerted between distinct and concentrated accumulations of the repulsive matter disposed on the surfaces of bodies; and whilst thus exerted at sensible distances, the assumed force of repulsion is between the molecules themselves at insensible distances, either altogether controlled by some other force, or otherwise so feeble as to be incapable of producing an electrical diffusion by expansion, under an extremely diminished atmospheric pressure (75, *w*. 80, *x*.).

84. Many of the phenomena treated of in the course of this paper do not seem to have been contemplated in the more perfect theories of electricity: they may not, however, on that account be the less deserving of consideration; indeed, it is extremely uncertain whether any views of electricity hitherto adopted have been so completely verified as to render all doubts of their accuracy unpardonable. The conditions of electrical action generally assumed as the basis of calculation, do not unfrequently give rise to equations extremely complicated—in some cases very impracticable; and although the highest efforts of genius have been exerted in vanquishing the difficulties, it remains yet to be seen, whether, by an extended induction of facts, we may not succeed in arriving at easier views of electricity, and hence bring this department of science more completely under the dominion of analysis.

Plymouth,
December 1, 1833.

CORRIGENDA.

P. 214, line 7, *for* essentially involved, &c. *read* intimately associated with the molecules of....
P. 214, line 4 from the bottom, *for* A *read* *a*
P. 215, line 23 from the top, *for* fig. 1. *read* fig. 2.
P. 218, Experiment *b*, *for* (56.) *read* (57.)
P. 220, line 4 from the top, *for* (56.) *read* (57.)

XV. *On a General Method in Dynamics; by which the Study of the Motions of all free Systems of attracting or repelling Points is reduced to the Search and Differentiation of one central Relation, or characteristic Function.* *By* William Rowan Hamilton, *Member of several scientific Societies in the British Dominions, and of the American Academy of Arts and Sciences, Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland. Communicated by Captain* Beaufort, *R.N. F.R.S.*

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Introductory Remarks.

THE theoretical development of the laws of motion of bodies is a problem of such interest and importance, that it has engaged the attention of all the most eminent mathematicians, since the invention of dynamics as a mathematical science by Galileo, and especially since the wonderful extension which was given to that science by Newton. Among the successors of those illustrious men, Lagrange has perhaps done more than any other analyst, to give extent and harmony to such deductive researches, by showing that the most varied consequences respecting the motions of systems of bodies may be derived from one radical formula; the beauty of the method so suiting the dignity of the results, as to make of his great work a kind of scientific poem. But the science of force, or of power acting by law in space and time, has undergone already another revolution, and has become already more dynamic, by having almost dismissed the conceptions of solidity and cohesion, and those other material ties, or geometrically imaginable conditions, which Lagrange so happily reasoned on, and by tending more and more to resolve all connexions and actions of bodies into attractions and repulsions of points: and while the science is advancing thus in one direction by the improvement of physical views, it may advance in another direction also by the invention of mathematical methods. And the method proposed in the present essay, for the deductive study of the motions of attracting or repelling systems, will perhaps be received with indulgence, as an attempt to assist in carrying forward so high an inquiry.

In the methods commonly employed, the determination of the motion of a free point in space, under the influence of accelerating forces, depends on the integration of three equations in ordinary differentials of the second order; and the determination of the motions of a system of free points, attracting or repelling one another, depends on the integration of a system of such equations, in number threefold the number of the attracting or repelling points, unless we previously diminish by unity this latter number, by considering only relative motions. Thus, in the solar system, when we consider only the mutual attractions of the sun and of the ten known planets, the determination of the motions of the latter about the former is reduced, by the usual methods, to the integration of a system of thirty ordinary differential equations of the second order, between the coordinates and the time; or, by a transformation of LAGRANGE, to the integration of a system of sixty ordinary differential equations of the first order, between the time and the elliptic elements: by which integrations, the thirty varying coordinates, or the sixty varying elements, are to be found as functions of the time. In the method of the present essay, this problem is reduced to the search and differentiation of a single function, which satisfies two partial differential equations of the first order and of the second degree: and every other dynamical problem, respecting the motions of any system, however numerous, of attracting or repelling points, (even if we suppose those points restricted by any conditions of connexion consistent with the law of living force,) is reduced, in like manner, to the study of one central function, of which the form marks out and characterizes the properties of the moving system, and is to be determined by a pair of partial differential equations of the first order, combined with some simple considerations. The difficulty is therefore at least transferred from the integration of many equations of one class to the integration of two of another: and even if it should be thought that no practical facility is gained, yet an intellectual pleasure may result from the reduction of the most complex and, probably, of all researches respecting the forces and motions of body, to the study of one characteristic function*, the unfolding of one central relation.

The present essay does not pretend to treat fully of this extensive subject,—a task which may require the labours of many years and many minds; but only to suggest the thought and propose the path to others. Although, therefore, the method may be used in the most varied dynamical researches, it is at present only applied to the orbits and perturbations of a system with any laws of attraction or repulsion, and with one predominant mass or centre of predominant energy; and only so far, even in this one research, as appears sufficient to make the principle itself understood. It may be mentioned here, that this dynamical principle is only another form of that idea which has already been applied to optics in the *Theory of systems of rays*, and that an intention of applying it to the motions of systems of bodies was announced †

* LAGRANGE and, after him, LAPLACE and others, have employed a single function to express the different forces of a system, and so to form in an elegant manner the differential equations of its motion. By this conception, great simplicity has been given to the statement of the problem of dynamics; but the solution of that problem, or the expression of the motions themselves, and of their integrals, depends on a very different and hitherto unimagined function, as it is the purpose of this essay to show.

† Transactions of the Royal Irish Academy, vol. xv. page 80. A notice of this dynamical principle was also lately given in an article "On a general Method of expressing the Paths of Light and of the Planets," published in the Dublin University Review for October 1833.

at the publication of that theory. And besides the idea itself, the manner of calculation also, which has been thus exemplified in the sciences of optics and dynamics, seems not confined to those two sciences, but capable of other applications; and the peculiar combination which it involves, of the principles of variations with those of partial differentials, for the determination and use of an important class of integrals, may constitute, when it shall be matured by the future labours of mathematicians, a separate branch of analysis.

WILLIAM R. HAMILTON.

Observatory, Dublin,
March 1834.

Integration of the Equations of Motion of a System, characteristic Function of such Motion, and Law of varying Action.

1. The known differential equations of motion of a system of free points, repelling or attracting one another according to any functions of their distances, and not disturbed by any foreign force, may be comprised in the following formula:

$$\Sigma \, . \, m \, (x'' \, \delta x + y'' \, \delta y + z'' \, \delta z) = \delta \mathrm{U} . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

In this formula the sign of summation Σ extends to all the points of the system; m is, for any one such point, the constant called its mass; x'', y'', z'', are its component accelerations, or the second differential coefficients of its rectangular coordinates x, y, z, taken with respect to the time; δx, δy, δz, are any arbitrary infinitesimal displacements which the point can be imagined to receive in the same three rectangular directions; and $\delta \mathrm{U}$ is the infinitesimal variation corresponding, of a function U of the masses and mutual distances of the several points of the system, of which the form depends on the laws of their mutual actions, by the equation

$$\mathrm{U} = \Sigma \, . \, m \, m_{\prime} \, f(r), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

r being the distance between any two points m, $m_{\prime}$, and the function $f(r)$ being such that its derivative or differential coefficient $f'(r)$ expresses the law of their repulsion, being negative in the case of attraction. The function which has been here called U, may be named the *force-function* of a system: it is of great utility in theoretical mechanics, into which it was introduced by LAGRANGE, and it furnishes the following elegant forms for the differential equations of motion, included in the formula (1.):

$$\left.\begin{array}{l} m_1 x''_1 = \dfrac{\delta \mathrm{U}}{\delta x_1}; \; m_2 x''_2 = \dfrac{\delta \mathrm{U}}{\delta x_2}; \; \ldots m_n x''_n = \dfrac{\delta \mathrm{U}}{\delta x_n}; \\ m_1 y''_1 = \dfrac{\delta \mathrm{U}}{\delta y_1}; \; m_2 y''_2 = \dfrac{\delta \mathrm{U}}{\delta y_2}; \; \ldots m_n y''_n = \dfrac{\delta \mathrm{U}}{\delta y_n}; \\ m_1 z''_1 = \dfrac{\delta \mathrm{U}}{\delta z_1}; \; m_2 z''_2 = \dfrac{\delta \mathrm{U}}{\delta z_2}; \; \ldots m_n z''_n = \dfrac{\delta \mathrm{U}}{\delta z_n}; \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

the second members of these equations being the partial differential coefficients of the first order of the function U. But notwithstanding the elegance and simplicity of this known manner of stating the principal problem of dynamics, the difficulty of solving that problem, or even of expressing its solution, has hitherto appeared insuperable; so that only seven intermediate integrals, or integrals of the first order, with as many arbitrary constants, have hitherto been found for these general equations of motion of a system of n points, instead of $3n$ intermediate and $3n$ final integrals, involving ultimately $6n$ constants; nor has any integral been found which does not need to be integrated again. No general solution has been obtained assigning (as a complete solution ought to do) $3n$ relations between the n masses $m_1, m_2, \ldots m_n$, the $3n$ varying coordinates $x_1, y_1, z_1, \ldots x_n, y_n, z_n$, the varying time t, and the $6n$ initial data of the problem, namely, the initial coordinates $a_1, b_1, c_1, \ldots a_n, b_n, c_n$, and their initial rates of increase, $a'_1, b'_1, c'_1, \ldots a'_n, b'_n, c'_n$; the quantities called here initial being those which correspond to the arbitrary origin of time. It is, however, possible (as we shall see) to express these long-sought relations by the partial differential coefficients of a new central or radical function, to the search and employment of which the difficulty of mathematical dynamics becomes henceforth reduced.

2. If we put for abridgement

$$T = \tfrac{1}{2} \Sigma . m (x'^2 + y'^2 + z'^2), \quad \ldots \ldots \ldots \ldots \quad (4.)$$

so that 2 T denotes, as in the Mécanique Analytique, the whole living force of the system; (x', y', z', being here, according to the analogy of our foregoing notation, the rectangular components of velocity of the point m, or the first differential coefficients of its coordinates taken with respect to the time;) an easy and well known combination of the differential equations of motion, obtained by changing in the formula (1.) the variations to the differentials of the coordinates, may be expressed in the following manner,

$$d\mathrm{T} = d\mathrm{U}, \quad \ldots \ldots \ldots \ldots \quad (5.)$$

and gives, by integration, the celebrated law of living force, under the form

$$T = U + H. \quad \ldots \ldots \ldots \ldots \quad (6.)$$

In this expression, which is one of the seven known integrals already mentioned, the quantity H is independent of the time, and does not alter in the passage of the points of the system from one set of positions to another. We have, for example, an initial equation of the same form, corresponding to the origin of time, which may be written thus,

$$T_0 = U_0 + H \quad \ldots \ldots \ldots \ldots \quad (7.)$$

The quantity H may, however, receive any arbitrary increment whatever, when we pass in thought from a system moving in one way, to the same system moving in another, with the same dynamical relations between the accelerations and positions of its points, but with different initial data; but the increment of H, thus obtained, is evidently connected with the analogous increments of the functions T and U, by the relation

$$\Delta T = \Delta U + \Delta H, \quad \ldots \ldots \ldots \ldots \quad (8.)$$

which, for the case of infinitesimal variations, may conveniently be written thus,

$$\delta T = \delta U + \delta H; \quad \ldots \ldots \ldots \ldots \quad (9.)$$

and this last relation, when multiplied by $d\,t$, and integrated, conducts to an important result. For it thus becomes, by (4.) and (1.),

$$\int \Sigma \, . \, m\,(d\,x \, . \, \delta\,x' + d\,y \, . \, \delta\,y' + d\,z \, . \, \delta\,z') =$$
$$\int \Sigma \, . \, m\,(d\,x' \, . \, \delta\,x + d\,y' \, . \, \delta\,y + d\,z' \, . \, \delta\,z) + \int \delta\,H \, . \, d\,t, \quad \ldots \ldots \quad (10.)$$

that is, by the principles of the calculus of variations,

$$\delta V = \Sigma \, . \, m\,(x'\,\delta\,x + y'\,\delta\,y + z'\,\delta\,z) - \Sigma \, . \, m\,(a'\,\delta\,a + b'\,\delta\,b + c'\,\delta\,c) + t\,\delta\,H, \, . \, . \quad (A.)$$

if we denote by V the integral

$$V = \int \Sigma \, . \, m\,(x'\,d\,x + y'\,d\,y + z'\,d\,z) = \int_0^t 2\,T\,d\,t, \quad \ldots \ldots \quad (B.)$$

namely, the accumulated living force, called often the action of the system, from its initial to its final position.

If, then, we consider (as it is easy to see that we may) the action V as a function of the initial and final coordinates, and of the quantity H, we shall have, by (A.), the following groups of equations; first, the group,

$$\left.\begin{aligned}
&\frac{\delta V}{\delta x_1} = m_1 x'_1\,; \quad \frac{\delta V}{\delta x_2} = m_2 x'_2\,; \ldots \frac{\delta V}{\delta x_n} = m_n x'_n\,; \\
&\frac{\delta V}{\delta y_1} = m_1 y'_1\,; \quad \frac{\delta V}{\delta y_2} = m_2 y'_2\,; \ldots \frac{\delta V}{\delta y_n} = m_n y'_n\,; \\
&\frac{\delta V}{\delta z_1} = m_1 z'_1\,; \quad \frac{\delta V}{\delta z_2} = m_2 z'_2\,; \ldots \frac{\delta V}{\delta z_n} = m_n z'_n\,;
\end{aligned}\right\} \quad \ldots \ldots \quad (C.)$$

Secondly, the group,

$$\left.\begin{aligned}
&\frac{\delta V}{\delta a_1} = -\,m_1 a'_1\,; \quad \frac{\delta V}{\delta a_2} = -\,m_2 a'_2\,; \ldots \frac{\delta V}{\delta a_n} = -\,m_n a'_n\,; \\
&\frac{\delta V}{\delta b_1} = -\,m_1 b'_1\,; \quad \frac{\delta V}{\delta b_2} = -\,m_2 b'_2\,; \ldots \frac{\delta V}{\delta b_n} = -\,m_n b'_n\,; \\
&\frac{\delta V}{\delta c_1} = -\,m_1 c'_1\,; \quad \frac{\delta V}{\delta c_2} = -\,m_2 c'_2\,; \ldots \frac{\delta V}{\delta c_n} = -\,m_n c'_n\,;
\end{aligned}\right\} \quad \ldots \quad (D.)$$

and finally, the equation,

$$\frac{\delta V}{\delta H} = t: \quad \ldots \ldots \ldots \ldots \quad (E.)$$

So that if this function V were known, it would only remain to eliminate H between the $3\,n + 1$ equations (C.) and (E.), in order to obtain all the $3\,n$ intermediate integrals, or between (D.) and (E.) to obtain all the $3\,n$ final integrals of the differential equations of motion; that is, ultimately, to obtain the $3\,n$ sought relations between the $3n$ varying coordinates and the time, involving also the masses and the $6n$ initial data above mentioned; the discovery of which relations would be (as we have said) the general solution of the general problem of dynamics. We have, therefore, at least reduced that general problem to the search and differentiation of a single function V, which we shall call on this account the CHARACTERISTIC FUNCTION of motion of a system; and the equation (A.), expressing the fundamental law of its variation, we shall call the *equation of the characteristic function*, or the LAW OF VARYING ACTION.

3. To show more clearly that the action or accumulated living force of a system, or in other words, the integral of the product of the living force by the element of the time, may be regarded as a function of the $6n+1$ quantities already mentioned, namely, of the initial and final coordinates, and of the quantity H, we may observe, that whatever depends on the manner and time of motion of the system may be considered as such a function; because the initial form of the law of living force, when combined with the $3n$ known or unknown relations between the time, the initial data, and the varying coordinates, will always furnish $3n+1$ relations, known or unknown, to connect the time and the initial components of velocities with the initial and final coordinates, and with H. Yet from not having formed the conception of the action as a *function* of this kind, the consequences that have been here deduced from the formula (A.) for the variation of that definite integral, appear to have escaped the notice of LAGRANGE, and of the other illustrious analysts who have written on theoretical mechanics; although they were in possession of a formula for the variation of this integral not greatly differing from ours. For although LAGRANGE and others, in treating of the motion of a system, have shown that the variation of this definite integral vanishes when the extreme coordinates and the constant H are given, they appear to have deduced from this result only the well known law of *least action*; namely, that if the points or bodies of a system be imagined to move from a given set of initial to a given set of final positions, not as they do nor even as they could move consistently with the general dynamical laws or differential equations of motion, but so as not to violate any supposed geometrical connexions, nor that one dynamical relation between velocities and configurations which constitutes the law of living force; and if, besides, this geometrically imaginable, but dynamically impossible motion, be made to differ infinitely *little* from the actual manner of motion of the system, between the given extreme positions; then the varied value of the definite integral called action, or the accumulated living force of the system in the motion thus imagined, will differ infinitely *less* from the actual value of that integral. But when this well known law of least, or as it might be better called, of *stationary action*, is applied to the determination of the actual motion of a system, it serves only to form, by the rules of the calculus of variations, the differential equations of motion of the second order, which can always be otherwise found. It seems, therefore, to be with reason that LAGRANGE, LAPLACE, and POISSON have spoken lightly of the utility of this principle in the present state of dynamics. A different estimate, perhaps, will be formed of that

other principle which has been introduced in the present paper, under the name of the *law of varying action*, in which we pass from an actual motion to another motion dynamically possible, by varying the extreme positions of the system, and (in general) the quantity H, and which serves to express, by means of a single function, not the mere differential equations of motion, but their intermediate and their final integrals.

Verifications of the foregoing Integrals.

4. A verification, which ought not to be neglected, and at the same time an illustration of this new principle, may be obtained by deducing the known differential equations of motion from our system of intermediate integrals, and by showing the consistence of these again with our final integral system. As preliminary to such verification, it is useful to observe that the final equation (6.) of living force, when combined with the system (C.), takes this new form,

$$\frac{1}{2}\Sigma . \frac{1}{m}\left\{\left(\frac{\delta V}{\delta x}\right)^2 + \left(\frac{\delta V}{\delta y}\right)^2 + \left(\frac{\delta V}{\delta z}\right)^2\right\} = U + H; \quad \ldots \ldots \ldots \quad (F.)$$

and that the initial equation (7.) of living force becomes by (D.)

$$\frac{1}{2}\Sigma . \frac{1}{m}\left\{\left(\frac{\delta V}{\delta a}\right)^2 + \left(\frac{\delta V}{\delta b}\right)^2 + \left(\frac{\delta V}{\delta c}\right)^2\right\} = U_0 + H. \quad \ldots \ldots \ldots \quad (G.)$$

These two partial differential equations, initial and final, of the first order and the second degree, must both be identically satisfied by the characteristic function V: they furnish (as we shall find) the principal means of discovering the form of that function, and are of essential importance in its theory. If the form of this function were known, we might eliminate $3n - 1$ of the $3n$ initial coordinates between the $3n$ equations (C.); and although we cannot yet perform the actual process of this elimination, we are entitled to assert that it would remove along with the others the remaining initial coordinate, and would conduct to the equation (6.) of final living force, which might then be transformed into the equation (F.). In like manner we may conclude that all the $3n$ final coordinates could be eliminated together from the $3n$ equations (D.), and that the result would be the initial equation (7.) of living force, or the transformed equation (G.). We may therefore consider the law of living force, which assisted us in discovering the properties of our characteristic function V, as included reciprocally in those properties, and as resulting by elimination, in every particular case, from the systems (C.) and (D.); and in treating of either of these systems, or in conducting any other dynamical investigation by the method of this characteristic function, we are at liberty to employ the partial differential equations (F.) and (G.), which that function must necessarily satisfy.

It will now be easy to deduce, as we proposed, the known equations of motion (3.) of the second order, by differentiation and elimination of constants, from our intermediate integral system (C.), (E.), or even from a part of that system, namely, from the group (C.), when combined with the equation (F.). For we thus obtain

$$\left.\begin{aligned}
m_1 x''_1 = \frac{d}{dt}\frac{\delta V}{\delta x_1} &= x'_1 \frac{\delta^2 V}{\delta x^2_1} + x'_2 \frac{\delta^2 V}{\delta x_1 \delta x_2} + \dots + x'_n \frac{\delta^2 V}{\delta x_1 \delta x_n} \\
&+ y'_1 \frac{\delta^2 V}{\delta x_1 \delta y_1} + y'_2 \frac{\delta^2 V}{\delta x_1 \delta y_2} + \dots + y'_n \frac{\delta^2 V}{\delta x_1 \delta y_n} \\
&+ z'_1 \frac{\delta^2 V}{\delta x_1 \delta z_1} + z'_2 \frac{\delta^2 V}{\delta x_1 \delta z_2} + \dots + z'_n \frac{\delta^2 V}{\delta x_1 \delta z_n} \\
= \frac{1}{m_1}\frac{\delta V}{\delta x_1}\frac{\delta^2 V}{\delta x^2_1} &+ \frac{1}{m_2}\frac{\delta V}{\delta x_2}\frac{\delta^2 V}{\delta x_1 \delta x_2} + \dots + \frac{1}{m_n}\frac{\delta V}{\delta x_n}\frac{\delta^2 V}{\delta x_1 \delta x_n} \\
+ \frac{1}{m_1}\frac{\delta V}{\delta y_1}\frac{\delta^2 V}{\delta x_1 \delta y_1} &+ \frac{1}{m_2}\frac{\delta V}{\delta y_2}\frac{\delta^2 V}{\delta x_1 \delta y_2} + \dots + \frac{1}{m_n}\frac{\delta V}{\delta y_n}\frac{\delta^2 V}{\delta x_1 \delta y_n} \\
+ \frac{1}{m_1}\frac{\delta V}{\delta z_1}\frac{\delta^2 V}{\delta x_1 \delta z_1} &+ \frac{1}{m_2}\frac{\delta V}{\delta z_2}\frac{\delta^2 V}{\delta x_1 \delta z_2} + \dots + \frac{1}{m_n}\frac{\delta V}{\delta z_n}\frac{\delta^2 V}{\delta x_1 \delta z_n} \\
= \frac{\delta}{\delta x_1} \Sigma \,.\, \frac{1}{2m} &\left\{ \left(\frac{\delta V}{\delta x}\right)^2 + \left(\frac{\delta V}{\delta y}\right)^2 + \left(\frac{\delta V}{\delta z}\right)^2 \right\} = \frac{\delta}{\delta x}(U + H);
\end{aligned}\right\} \quad \dots \quad (11.)$$

that is, we obtain

$$m_1 x''_1 = \frac{\delta U}{\delta x_1} : \quad \dots \dots \dots \quad (12.)$$

And in like manner we might deduce, by differentiation, from the integrals (C.) and from (F.) all the other known differential equations of motion, of the second order, contained in the set marked (3.); or, more concisely, we may deduce at once the formula (1.), which contains all those known equations, by observing that the intermediate integrals (C.), when combined with the relation (F.), give

$$\left.\begin{aligned}
&\Sigma \,.\, m (x'' \delta x + y'' \delta y + z'' \delta z) = \Sigma \left(\frac{d}{dt}\frac{\delta V}{\delta x} . \delta x + \frac{d}{dt}\frac{\delta V}{\delta y} . \delta y + \frac{d}{dt}\frac{\delta V}{\delta z} . \delta z \right) \\
&= \Sigma \,.\, \frac{1}{m} \left(\frac{\delta V}{\delta x}\frac{\delta}{\delta x} + \frac{\delta V}{\delta y}\frac{\delta}{\delta y} + \frac{\delta V}{\delta z}\frac{\delta}{\delta z} \right) \Sigma \left(\frac{\delta V}{\delta x}\delta x + \frac{\delta V}{\delta y}\delta y + \frac{\delta V}{\delta z}\delta z \right) \\
&= \Sigma \left(\delta x \frac{\delta}{\delta x} + \delta y \frac{\delta}{\delta y} + \delta z \frac{\delta}{\delta z} \right) \Sigma \,.\, \frac{1}{2m} \left\{ \left(\frac{\delta V}{\delta x}\right)^2 + \left(\frac{\delta V}{\delta y}\right)^2 + \left(\frac{\delta V}{\delta z}\right)^2 \right\} \\
&= \Sigma \left(\delta x \frac{\delta}{\delta x} + \delta y \frac{\delta}{\delta y} + \delta z \frac{\delta}{\delta z} \right) (U + H) \\
&= \delta U.
\end{aligned}\right\} . \quad (13.)$$

5. Again, we were to show that our intermediate integral system, composed of the equations (C.) and (E.), with the $3n$ arbitrary constants $a_1, b_1, c_1, \dots a_n, b_n, c_n$, (and involving also the auxiliary constant H,) is consistent with our final integral system of equations (D.) and (E.), which contain $3n$ other arbitrary constants, namely, $a'_1, b'_1, c'_1, \dots a'_n, b'_n, c'_n$. The immediate differentials of the equations (C.), (D.), (E.), taken with respect to the time, are, for the first group,

$$\left.\begin{array}{l}\frac{d}{dt}\frac{\delta V}{\delta x_1} = m_1 x''_1 ; \ \frac{d}{dt}\frac{\delta V}{\delta x_2} = m_2 x''_2 ; \ \ldots \frac{d}{dt}\frac{\delta V}{\delta x_n} = m_n x''_n ; \\ \frac{d}{dt}\frac{\delta V}{\delta y_1} = m_1 y''_1 ; \ \frac{d}{dt}\frac{\delta V}{\delta y_2} = m_2 y''_2 ; \ \ldots \frac{d}{dt}\frac{\delta V}{\delta y_n} = m_n y''_n ; \\ \frac{d}{dt}\frac{\delta V}{\delta z_1} = m_1 z''_1 ; \ \frac{d}{dt}\frac{\delta V}{\delta z_2} = m_2 z''_2 ; \ \ldots \frac{d}{dt}\frac{\delta V}{\delta z_n} = m_n z''_n ;\end{array}\right\} \quad \text{(H.)}$$

for the second group,

$$\left.\begin{array}{l}\frac{d}{dt}\frac{\delta V}{\delta a_1} = 0 ; \ \frac{d}{dt}\frac{\delta V}{\delta a_2} = 0 ; \ \ldots \frac{d}{dt}\frac{\delta V}{\delta a_n} = 0 ; \\ \frac{d}{dt}\frac{\delta V}{\delta b_1} = 0 ; \ \frac{d}{dt}\frac{\delta V}{\delta b_2} = 0 ; \ \ldots \frac{d}{dt}\frac{\delta V}{\delta b_n} = 0 ; \\ \frac{d}{dt}\frac{\delta V}{\delta c_1} = 0 ; \ \frac{d}{dt}\frac{\delta V}{\delta c_2} = 0 ; \ \ldots \frac{d}{dt}\frac{\delta V}{\delta c_n} = 0 ;\end{array}\right\} \quad \text{(I.)}$$

and finally, for the last equation,

$$\frac{d}{dt}\frac{\delta V}{\delta H} = 1. \quad \text{(K.)}$$

By combining the equations (C.) with their differentials (H.), and with the relation (F.), we deduced, in the foregoing number, the known equations of motion (3.); and we are now to show the consistence of the same intermediate integrals (C.) with the group of differentials (I.), which have been deduced from the final integrals.

The first equation of the group (I.) may be developed thus:

$$\left.\begin{array}{l}0 = x'_1 \frac{\delta^2 V}{\delta a_1 \delta x_1} + x'_2 \frac{\delta^2 V}{\delta a_1 \delta x_2} + \ldots + x'_n \frac{\delta^2 V}{\delta a_1 \delta x_n} \\ \quad + y'_1 \frac{\delta^2 V}{\delta a_1 \delta y_1} + y'_2 \frac{\delta^2 V}{\delta a_1 \delta y_2} + \ldots + y'_n \frac{\delta^2 V}{\delta a_1 \delta y_n} \\ \quad + z'_1 \frac{\delta^2 V}{\delta a_1 \delta z_1} + z'_2 \frac{\delta^2 V}{\delta a_1 \delta z_2} + \ldots + z'_n \frac{\delta^2 V}{\delta a_1 \delta z_n} ;\end{array}\right\} \quad \text{(14.)}$$

and the others may be similarly developed. In order, therefore, to show that they are satisfied by the group (C.), it is sufficient to prove that the following equations are true,

$$\left.\begin{array}{l}0 = \frac{\delta}{\delta a_i} \Sigma \cdot \frac{1}{2m} \left\{ \left(\frac{\delta V}{\delta x}\right)^2 + \left(\frac{\delta V}{\delta y}\right)^2 + \left(\frac{\delta V}{\delta z}\right)^2 \right\}, \\ 0 = \frac{\delta}{\delta b_i} \Sigma \cdot \frac{1}{2m} \left\{ \left(\frac{\delta V}{\delta x}\right)^2 + \left(\frac{\delta V}{\delta y}\right)^2 + \left(\frac{\delta V}{\delta z}\right)^2 \right\}, \\ 0 = \frac{\delta}{\delta c_i} \Sigma \cdot \frac{1}{2m} \left\{ \left(\frac{\delta V}{\delta x}\right)^2 + \left(\frac{\delta V}{\delta y}\right)^2 + \left(\frac{\delta V}{\delta z}\right)^2 \right\},\end{array}\right\} \quad \text{(L.)}$$

the integer i receiving any value from 1 to n inclusive; which may be shown at once, and the required verification thereby be obtained, if we merely take the variation of the relation (F.) with respect to the initial coordinates, as in the former verification we took its variation with respect to the final coordinates, and so obtained results which agreed with the known equations of motion, and which may be thus collected,

$$\left.\begin{aligned}
\frac{\delta}{\delta x_i} \Sigma \cdot \frac{1}{2m} \left\{ \left(\frac{\delta V}{\delta x}\right)^2 + \left(\frac{\delta V}{\delta y}\right)^2 + \left(\frac{\delta V}{\delta z}\right)^2 \right\} &= \frac{\delta U}{\delta x_i}; \\
\frac{\delta}{\delta y_i} \Sigma \cdot \frac{1}{2m} \left\{ \left(\frac{\delta V}{\delta x}\right)^2 + \left(\frac{\delta V}{\delta y}\right)^2 + \left(\frac{\delta V}{\delta z}\right)^2 \right\} &= \frac{\delta U}{\delta y_i}; \\
\frac{\delta}{\delta z_i} \Sigma \cdot \frac{1}{2m} \left\{ \left(\frac{\delta V}{\delta x}\right)^2 + \left(\frac{\delta V}{\delta y}\right)^2 + \left(\frac{\delta V}{\delta z}\right)^2 \right\} &= \frac{\delta U}{\delta z_i}.
\end{aligned}\right\} \quad \ldots\ldots \quad \text{(M.)}$$

The same relation (F.), by being varied with respect to the quantity H, conducts to the expression

$$\frac{\delta}{\delta H} \Sigma \cdot \frac{1}{2m} \left\{ \left(\frac{\delta V}{\delta x}\right)^2 + \left(\frac{\delta V}{\delta y}\right)^2 + \left(\frac{\delta V}{\delta z}\right)^2 \right\} = 1; \quad \ldots\ldots \quad \text{(N.)}$$

and this, when developed, agrees with the equation (K.), which is a new verification of the consistence of our foregoing results. Nor would it have been much more difficult, by the help of the foregoing principles, to have integrated directly our integrals of the first order, and so to have deduced in a different way our final integral system.

6. It may be considered as still another verification of our own general integral equations, to show that they include not only the known law of living force, or the integral expressing that law, but also the six other known integrals of the first order, which contain the law of motion of the centre of gravity, and the law of description of areas. For this purpose, it is only necessary to observe that it evidently follows from the conception of our characteristic function V, that this function depends on the initial and final positions of the attracting or repelling points of a system, not as referred to any foreign standard, but only as compared with one another; and therefore that this function will not vary, if without making any real change in either initial or final configuration, or in the relation of these to each other, we alter at once all the initial and all the final positions of the points of the system, by any common motion, whether of translation or of rotation. Now by considering three coordinate translations, we obtain the three following partial differential equations of the first order, which the function V must satisfy,

$$\left.\begin{aligned}
\Sigma \frac{\delta V}{\delta x} + \Sigma \frac{\delta V}{\delta a} &= 0; \\
\Sigma \frac{\delta V}{\delta y} + \Sigma \frac{\delta V}{\delta b} &= 0; \\
\Sigma \frac{\delta V}{\delta z} + \Sigma \frac{\delta V}{\delta c} &= 0;
\end{aligned}\right\} \quad \ldots\ldots \quad \text{(O.)}$$

and by considering three coordinate rotations, we obtain these three other relations between the partial differential coefficients of the same order of the same characteristic function,

$$\left.\begin{aligned}\Sigma\left(x\frac{\delta V}{\delta y}-y\frac{\delta V}{\delta x}\right)+\Sigma\left(a\frac{\delta V}{\delta b}-b\frac{\delta V}{\delta a}\right)=0;\\ \Sigma\left(y\frac{\delta V}{\delta z}-z\frac{\delta V}{\delta y}\right)+\Sigma\left(b\frac{\delta V}{\delta c}-c\frac{\delta V}{\delta b}\right)=0;\\ \Sigma\left(z\frac{\delta V}{\delta x}-x\frac{\delta V}{\delta z}\right)+\Sigma\left(c\frac{\delta V}{\delta a}-a\frac{\delta V}{\delta c}\right)=0;\end{aligned}\right\}\quad\ldots\ldots\quad\text{(P.)}$$

and if we change the final coefficients of V to the final components of momentum, and the initial coefficients to the initial components taken negatively, according to the dynamical properties of this function expressed by the integrals (C.) and (D.), we shall change these partial differential equations (O.) (P.), to the following,

$$\Sigma\,.\,m\,x'=\Sigma\,.\,m\,a'\,;\ \Sigma\,.\,m\,y'=\Sigma\,.\,m\,b'\,;\ \Sigma\,.\,m\,z'=\Sigma\,.\,m\,c'\,;\quad\ldots\quad\text{(15.)}$$

and

$$\left.\begin{aligned}\Sigma\,.\,m\,(x\,y'-y\,x')=\Sigma\,.\,m\,(a\,b'-b\,a')\,;\\ \Sigma\,.\,m\,(y\,z'-z\,y')=\Sigma\,.\,m\,(b\,c'-c\,b')\,;\\ \Sigma\,.\,m\,(z\,x'-x\,z')=\Sigma\,.\,m\,(c\,a'-a\,c').\end{aligned}\right\}\quad\ldots\ldots\quad\text{(16.)}$$

In this manner, therefore, we can deduce from the properties of our characteristic function the six other known integrals above mentioned, in addition to that seventh which contains the law of living force, and which assisted in the discovery of our method.

Introduction of relative or polar Coordinates, or other marks of position of a System.

7. The property of our characteristic function, by which it depends only on the internal or mutual relations between the positions initial and final of the points of an attracting or repelling system, suggests an advantage in employing internal or relative coordinates; and from the analogy of other applications of algebraical methods to researches of a geometrical kind, it may be expected that polar and other marks of position will also often be found useful. Supposing, therefore, that the $3\,n$ final coordinates $x_1\,y_1\,z_1\ldots x_n\,y_n\,z_n$ have been expressed as functions of $3\,n$ other variables, $\eta_1\,\eta_2\ldots\eta_{3n}$, and that the $3\,n$ initial coordinates have in like manner been expressed as functions of $3\,n$ similar quantities, which we shall call $e_1\,e_2\ldots e_{3n}$, we shall proceed to assign a general method for introducing these new marks of position into the expressions of our fundamental relations.

For this purpose we have only to transform the law of varying action, or the fundamental formula (A.), by transforming the two sums,

$$\Sigma\,.\,m\,(x'\,\delta\,x+y'\,\delta\,y+z'\,\delta\,z),\text{ and }\Sigma\,.\,m\,(a'\,\delta\,a+b'\,\delta\,b+c'\,\delta\,c),$$

which it involves, and which are respectively equivalent to the following more developed expressions,

$$\left.\begin{aligned}\Sigma\,.\,m\,(x'\,\delta x + y'\,\delta y + z'\,\delta z) = m_1\,(x'_1\,\delta x_1 + y'_1\,\delta y_1 + z'_1\,\delta z_1)\\ + m_2\,(x'_2\,\delta x_2 + y'_2\,\delta y_2 + z'_2\,\delta z_2)\\ + \&c. + m_n\,(x'_n\,\delta x_n + y'_n\,\delta y_n + z'_n\,\delta z_n);\end{aligned}\right\}\quad (17.)$$

$$\left.\begin{aligned}\Sigma\,.\,m\,(a'\,\delta a + b'\,\delta b + c'\,\delta c) = m_1\,(a'_1\,\delta a_1 + b'_1\,\delta b_1 + c'_1\,\delta c_1)\\ + m_2\,(a'_2\,\delta a_2 + b'_2\,\delta b_2 + c'_2\,\delta c_2)\\ + \&c. + m_n\,(a'_n\,\delta a_n + b'_n\,\delta b_n + c'_n\,\delta c_n).\end{aligned}\right\}\quad (18.)$$

Now x_i being by supposition a function of the $3\,n$ new marks of position $\eta_1 \ldots \eta_{3n}$, its variation δx_i, and its differential coefficient x'_i, may be thus expressed:

$$\delta x_i = \frac{\delta x_i}{\delta \eta_1}\,\delta \eta_1 + \frac{\delta x_i}{\delta \eta_2}\,\delta \eta_2 + \ldots + \frac{\delta x_i}{\delta \eta_{3n}}\,\delta \eta_{3n}; \quad (19.)$$

$$x'_i = \frac{\delta x_i}{\delta \eta_1}\,\eta'_1 + \frac{\delta x_i}{\delta \eta_2}\,\eta'_2 + \ldots + \frac{\delta x_i}{\delta \eta_{3n}}\,\eta'_{3n}; \quad (20.)$$

and similarly for y_i and z_i. If, then, we consider x'_i as a function, by (20.), of $\eta'_1 \ldots \eta'_{3n}$, involving also in general $\eta_1 \ldots \eta_{3n}$, and if we take its partial differential coefficients of the first order with respect to $\eta'_1 \ldots \eta'_{3n}$, we find the relations,

$$\frac{\delta x'_i}{\delta \eta'_1} = \frac{\delta x_i}{\delta \eta_1};\ \frac{\delta x'_i}{\delta \eta'_2} = \frac{\delta x_i}{\delta \eta_2};\ \ldots \frac{\delta x'_i}{\delta \eta'_{3n}} = \frac{\delta x_i}{\delta \eta_{3n}}; \quad (21.)$$

and therefore we obtain these new expressions for the variations δx_i, δy_i, δz_i,

$$\left.\begin{aligned}\delta x_i &= \frac{\delta x'_i}{\delta \eta'_1}\,\delta \eta_1 + \frac{\delta x'_i}{\delta \eta'_2}\,\delta \eta_2 + \ldots + \frac{\delta x'_i}{\delta \eta'_{3n}}\,\delta \eta_{3n},\\ \delta y_i &= \frac{\delta y'_i}{\delta \eta'_1}\,\delta \eta_1 + \frac{\delta y'_i}{\delta \eta'_2}\,\delta \eta_2 + \ldots + \frac{\delta y'_i}{\delta \eta'_{3n}}\,\delta \eta_{3n},\\ \delta z_i &= \frac{\delta z'_i}{\delta \eta'_1}\,\delta \eta_1 + \frac{\delta z'_i}{\delta \eta'_2}\,\delta \eta_2 + \ldots + \frac{\delta z'_i}{\delta \eta'_{3n}}\,\delta \eta_{3n}.\end{aligned}\right\}\quad (22.)$$

Substituting these expressions (22.) for the variations in the sum (17.), we easily transform it into the following,

$$\left.\begin{aligned}\Sigma\,.\,m\,(x'\,\delta x + y'\,\delta y + z'\,\delta z) = \Sigma\,.\,m\left(x'\,\frac{\delta x'}{\delta \eta'_1} + y'\,\frac{\delta y'}{\delta \eta'_1} + z'\,\frac{\delta z'}{\delta \eta'_1}\right).\,\delta \eta_1\\ + \Sigma\,.\,m\left(x'\,\frac{\delta x'}{\delta \eta'_2} + y'\,\frac{\delta y'}{\delta \eta'_2} + z'\,\frac{\delta z'}{\delta \eta'_2}\right).\,\delta \eta_2\\ + \&c. + \Sigma\,.\,m\left(x'\,\frac{\delta x'}{\delta \eta'_{3n}} + y'\,\frac{\delta y'}{\delta \eta'_{3n}} + z'\,\frac{\delta z'}{\delta \eta'_{3n}}\right).\,\delta \eta_{3n}\\ = \frac{\delta \mathrm{T}}{\delta \eta'_1}\,\delta \eta_1 + \frac{\delta \mathrm{T}}{\delta \eta'_2}\,\delta \eta_2 + \ldots + \frac{\delta \mathrm{T}}{\delta \eta'_{3n}}\,\delta \eta_{3n};\end{aligned}\right\}\quad (23.)$$

T being the same quantity as before, namely, the half of the final living force of the system, but being now considered as a function of $\eta'_1 \ldots \eta'_{3n}$, involving also the masses, and in general $\eta_1 \ldots \eta_{3n}$, and obtained by substituting for the quantities $x'\, y'\, z'$ their values of the form (20.) in the equation of definition

$$T = \tfrac{1}{2} \Sigma \, . \, m \, (x'^2 + y'^2 + z'^2). \quad \ldots \ldots \ldots \quad (4.)$$

In like manner we find this transformation for the sum (18.),

$$\Sigma \, . \, m \, (a' \, \delta \, a + b' \, \delta \, b + c' \, \delta \, c) = \frac{\delta \, T_0}{\delta \, e'_1} \delta \, e_1 + \frac{\delta \, T_0}{\delta \, e'_2} \delta \, e_2 + \ldots + \frac{\delta \, T_0}{\delta e'_{3n}} \delta \, e_{3n}. \quad . \quad (24.)$$

The law of varying action, or the formula (A.), becomes therefore, when expressed by the present more general coordinates or marks of position,

$$\delta \, V = \Sigma \, . \, \frac{\delta \, T}{\delta \, \eta'} \delta \, \eta - \Sigma \, . \, \frac{\delta \, T_0}{\delta \, e'} \delta \, e + t \, \delta \, H \, ; \quad \ldots \ldots \quad (Q.)$$

and instead of the groups (C.) and (D.), into which, along with the equation (E.), this law resolved itself before, it gives now these other groups,

$$\frac{\delta \, V}{\delta \, \eta_1} = \frac{\delta \, T}{\delta \, \eta'_1} ; \quad \frac{\delta \, V}{\delta \, \eta_2} = \frac{\delta \, T}{\delta \, \eta'_2} ; \ \ldots \ \frac{\delta \, V}{\delta \, \eta_{3n}} = \frac{\delta \, T}{\delta \, \eta'_{3n}} ; \quad \ldots \ldots \quad (R.)$$

and

$$\frac{\delta \, V}{\delta \, e_1} = - \frac{\delta \, T_0}{\delta \, e'_1} ; \quad \frac{\delta \, V}{\delta \, e_2} = - \frac{\delta \, T_0}{\delta \, e'_2} ; \ \ldots \ \frac{\delta \, V}{\delta \, e_{3n}} = - \frac{\delta \, T_0}{\delta \, e'_{3n}} . \quad \ldots \quad (S.)$$

The quantities $e_1 \, e_2 \ldots e_{3n}$ and $e'_1 \, e'_2 \ldots e'_{3n}$ are now the initial data respecting the manner of motion of the system; and the $3\,n$ final integrals, connecting these $6\,n$ initial data, and the n masses, with the time t, and with the $3\,n$ final or varying quantities $\eta_1 \, \eta_2 \ldots \eta_{3n}$, which mark the varying positions of the n moving points of the system, are now to be obtained by eliminating the auxiliary constant H between the $3\,n + 1$ equations (S.) and (E.); while the $3\,n$ intermediate integrals, or integrals of the first order, which connect the same varying marks of position and their first differential coefficients with the time, the masses, and the initial marks of position, are the result of elimination of the same auxiliary constant H between the equations (R.) and (E.). Our fundamental formula, and intermediate and final integrals, can therefore be very simply expressed with any new sets of coordinates; and the partial differential equations (F.) (G.), which our characteristic function V must satisfy, and which are, as we have said, essential in the theory of that function, can also easily be expressed with any such transformed coordinates, by merely combining the final and initial expressions of the law of living force,

$$T = U + H, \quad \ldots \ldots \ldots \ldots \quad (6.)$$

$$T_0 = U_0 + H, \quad \ldots \ldots \ldots \ldots \quad (7.)$$

with the new groups (R.) and (S.). For this purpose we must now consider the function U, of the masses and mutual distances of the several points of the system, as depending on the new marks of position $\eta_1 \, \eta_2 \ldots \eta_{3n}$; and the analogous function U_0, as depending similarly on the initial quantities $e_1 \, e_2 \ldots e_{3n}$; we must also suppose that T is expressed (as it may) as a function of its own coefficients $\frac{\delta T}{\delta \eta'_1}, \frac{\delta T}{\delta \eta'_2} \dots \frac{\delta T}{\delta \eta'_{3n}}$, which will always be, with respect to these, homogeneous of the second dimension, and may also involve explicitly the quantities $\eta_1\ \eta_2 \dots \eta_{3n}$; and that T_0 is expressed as a similar function of its coefficients $\frac{\delta T_0}{\delta e'_1}, \frac{\delta T_0}{\delta e'_2}, \dots \frac{\delta T_0}{\delta e'_{3n}}$; so that

$$\left.\begin{aligned} T &= F\left(\frac{\delta T}{\delta \eta'_1}, \frac{\delta T}{\delta \eta'_2}, \dots \frac{\delta T}{\delta \eta'_{3n}}\right), \\ T_0 &= F\left(\frac{\delta T_0}{\delta e'_1}, \frac{\delta T_0}{\delta e'_2}, \dots \frac{\delta T_0}{\delta e'_{3n}}\right); \end{aligned}\right\} \quad \text{(25.)}$$

and that then these coefficients of T and T_0 are changed to their values (R.) and (S.), so as to give, instead of (F.) and (G.), two other transformed equations, namely,

$$F\left(\frac{\delta V}{\delta \eta_1}, \frac{\delta V}{\delta \eta_2}, \dots \frac{\delta V}{\delta \eta_{3n}}\right) = U + H, \quad \text{(T.)}$$

and, on account of the homogeneity and dimension of T_0,

$$F\left(\frac{\delta V}{\delta e_1}, \frac{\delta V}{\delta e_2}, \dots \frac{\delta V}{\delta e_{3n}}\right) = U_0 + H. \quad \text{(U.)}$$

8. Nor is there any difficulty in deducing analogous transformations for the known differential equations of motion of the second order, of any system of free points, by taking the variation of the new form (T.) of the law of living force, and by attending to the dynamical meanings of the coefficients of our characteristic function. For if we observe that the final living force 2 T, when considered as a function of $\eta_1\ \eta_2 \dots \eta_{3n}$ and of $\eta'_1\ \eta'_2 \dots \eta'_{3n}$, is necessarily homogeneous of the second dimension with respect to the latter set of variables, and must therefore satisfy the condition

$$2\,T = \eta'_1 \frac{\delta T}{\delta \eta'_1} + \eta'_2 \frac{\delta T}{\delta \eta'_2} + \dots + \eta'_{3n} \frac{\delta T}{\delta \eta'_{3n}}, \quad \text{(26.)}$$

we shall perceive that its total variation,

$$\left.\begin{aligned} \delta T = {} & \frac{\delta T}{\delta \eta_1} \delta \eta_1 + \frac{\delta T}{\delta \eta_2} \delta \eta_2 + \dots + \frac{\delta T}{\delta \eta_{3n}} \delta \eta_{3n} \\ & + \frac{\delta T}{\delta \eta'_1} \delta \eta'_1 + \frac{\delta T}{\delta \eta'_2} \delta \eta'_2 + \dots + \frac{\delta T}{\delta \eta'_{3n}} \delta \eta'_{3n}, \end{aligned}\right\} \quad \text{(27.)}$$

may be put under the form

$$\left.\begin{aligned} \delta T &= \eta'_1 \delta \frac{\delta T}{\delta \eta'_1} + \eta'_2 \delta \frac{\delta T}{\delta \eta'_2} + \dots + \eta'_{3n} \delta \frac{\delta T}{\delta \eta'_{3n}} \\ &\quad - \frac{\delta T}{\delta \eta_1} \delta \eta_1 - \frac{\delta T}{\delta \eta_2} \delta \eta_2 - \dots - \frac{\delta T}{\delta \eta_{3n}} \delta \eta_{3n} \\ &= \Sigma\,.\,\eta' \delta \frac{\delta T}{\delta \eta'} - \Sigma\,.\,\frac{\delta T}{\delta \eta} \delta \eta \\ &= \Sigma \left(\eta' \delta \frac{\delta V}{\delta \eta} - \frac{\delta T}{\delta \eta} \delta \eta\right), \end{aligned}\right\} \quad \text{(28.)}$$

and therefore that the total variation of the new partial differential equation (T.) may be thus written,

$$\Sigma \left(\eta' \delta \frac{\delta V}{\delta \eta} - \frac{\delta T}{\delta \eta} \delta \eta \right) = \Sigma . \frac{\delta U}{\delta \eta} \delta \eta + \delta H : \quad \ldots \ldots \quad \text{(V.)}$$

in which, if we observe that $\eta' = \frac{d\eta}{dt}$, and that the quantities of the form η are the only ones which vary with the time, we shall see that

$$\Sigma . \eta' \delta \frac{\delta V}{\delta \eta} = \Sigma \left(\frac{d}{dt} \frac{\delta V}{\delta \eta} . \delta \eta + \frac{d}{dt} \frac{\delta V}{\delta e} . \delta e \right) + \frac{d}{dt} \frac{\delta V}{\delta H} . \delta H, \quad \ldots \quad \text{(29.)}$$

because the identical equation $\delta d V = d \delta V$ gives, when developed,

$$\left. \begin{aligned} &\Sigma \left(\delta \frac{\delta V}{\delta \eta} . d\eta + \delta \frac{\delta V}{\delta e} . de \right) + \delta \frac{\delta V}{\delta H} . dH \\ = &\Sigma \left(d \frac{\delta V}{\delta \eta} . \delta \eta + d \frac{\delta V}{\delta e} . \delta e \right) + d \frac{\delta V}{\delta H} . \delta H. \end{aligned} \right\} \quad \ldots \ldots \quad \text{(30.)}$$

Decomposing, therefore, the expression (V.), for the variation of half the living force, into as many separate equations as it contains independent variations, we obtain, not only the equation

$$\frac{d}{dt} \frac{\delta V}{\delta H} = 1, \quad \ldots \ldots \quad \text{(K.)}$$

which had already presented itself, and the group

$$\frac{d}{dt} \frac{\delta V}{\delta e_1} = 0, \quad \frac{d}{dt} \frac{\delta V}{\delta e_2} = 0, \ldots \frac{d}{dt} \frac{\delta V}{\delta e_{3n}} = 0, \quad \ldots \ldots \quad \text{(W.)}$$

which might have been at once obtained by differentiation from the final integrals (S.), but also a group of $3n$ other equations of the form

$$\frac{d}{dt} \frac{\delta V}{\delta \eta} - \frac{\delta T}{\delta \eta} = \frac{\delta U}{\delta \eta}, \quad \ldots \ldots \quad \text{(X.)}$$

which give, by the intermediate integrals (R.),

$$\frac{d}{dt} \frac{\delta T}{\delta \eta'} - \frac{\delta T}{\delta \eta} = \frac{\delta U}{\delta \eta} : \quad \ldots \ldots \quad \text{(Y.)}$$

that is, more fully,

$$\left. \begin{aligned} &\frac{d}{dt} \frac{\delta T}{\delta \eta'_1} - \frac{\delta T}{\delta \eta_1} = \frac{\delta U}{\delta \eta_1}; \\ &\frac{d}{dt} \frac{\delta T}{\delta \eta'_2} - \frac{\delta T}{\delta \eta_2} = \frac{\delta U}{\delta \eta_2}; \\ &\ldots\ldots \\ &\frac{d}{dt} \frac{\delta T}{\delta \eta'_{3n}} - \frac{\delta T}{\delta \eta_{3n}} = \frac{\delta U}{\delta \eta_{3n}}. \end{aligned} \right\} \quad \ldots \ldots \quad \text{(Z.)}$$

These last transformations of the differential equations of motion of the second order, of an attracting or repelling system, coincide in all respects (a slight difference of notation excepted,) with the elegant canonical forms in the *Mécanique Analytique* of LAGRANGE ; but it seemed worth while to deduce them here anew, from the properties of our characteristic function. And if we were to suppose (as it has often been thought convenient and even necessary to do,) that the n points of a system are not entirely free, nor subject only to their own mutual attractions or repulsions, but connected by any geometrical conditions, and influenced by any foreign agencies, consistent with the law of conservation of living force; so that the number of independent marks of position should be now less numerous, and the force-function U less simple than before; it might still be proved, by a reasoning very similar to the foregoing, that on these suppositions also (which, however, the dynamical spirit is tending more and more to exclude,) the accumulated living force or action V of the system is a *characteristic motion-function* of the kind already explained; having the same law and formula of variation, which are susceptible of the same transformations; obliged to satisfy in the same way a final and an initial relation between its partial differential coefficients of the first order; conducting, by the variation of one of these two relations, to the same canonical forms assigned by LAGRANGE for the differential equations of motion; and furnishing, on the same principles as before, their intermediate and their final integrals. To those imaginable cases, indeed, in which the law of living force no longer holds, our method also would not apply; but it appears to be the growing conviction of the persons who have meditated the most profoundly on the mathematical dynamics of the universe, that these are cases suggested by insufficient views of the mutual actions of body.

9. It results from the foregoing remarks, that in order to apply our method of the characteristic function to any problem of dynamics respecting any moving system, the known law of living force is to be combined with our law of varying action; and that the general expression of this latter law is to be obtained in the following manner. We are first to express the quantity T, namely, the half of the living force of the system, as a function (which will always be homogeneous of the second dimension,) of the differential coefficients or rates of increase η'_1, η'_2, &c., of any rectangular coordinates, or other marks of position of the system: we are next to take the variation of this homogeneous function with respect to those rates of increase, and to change the variations of those rates $\delta \eta'_1$, $\delta \eta'_2$, &c., to the variations $\delta \eta_1$, $\delta \eta_2$, &c., of the marks of position themselves; and then to subtract the initial from the final value of the result, and to equate the remainder to $\delta \mathrm{V} - t \, \delta \mathrm{H}$. A slight consideration will show that this general rule or process for obtaining the variation of the characteristic function V, is applicable even when the marks of position η_1, η_2, &c., are not all independent of each other; which will happen when they have been made, from any motive of convenience, more numerous than the rectangular coordinates of the several points of the system. For if we suppose that the $3\,n$ rectangular coordinates $x_1\, y_1\, z_1 \ldots x_n\, y_n\, z_n$, have been expressed by any transformation as functions of $3\,n + k$ other marks of position, $\eta_1\, \eta_2 \ldots \eta_{3\,n+k}$, which must therefore be connected by k equations of condition,

$$\left.\begin{array}{l} 0 = \varphi_1(\eta_1, \eta_2, \dots \eta_{3n+k}), \\ 0 = \varphi_2(\eta_1, \eta_2, \dots \eta_{3n+k}), \\ \dots\dots \\ 0 = \varphi_k(\eta_1, \eta_2, \dots \eta_{3n+k}), \end{array}\right\} \quad \dots \dots \quad (31.)$$

giving k of the new marks of position as functions of the remaining $3n$,

$$\left.\begin{array}{l} \eta_{3n+1} = \psi_1(\eta_1, \eta_2, \dots \eta_{3n}), \\ \eta_{3n+2} = \psi_2(\eta_1, \eta_2, \dots \eta_{3n}), \\ \dots\dots \\ \eta_{3n+k} = \psi_k(\eta_1, \eta_2, \dots \eta_{3n}), \end{array}\right\} \quad \dots \dots \quad (32.)$$

the expression

$$\mathrm{T} = \tfrac{1}{2} \Sigma \,.\, m\,(x'^2 + y'^2 + z'^2), \quad \dots \dots \quad (4.)$$

will become, by the introduction of these new variables, a homogeneous function of the second dimension of the $3n + k$ rates of increase $\eta'_1, \eta'_2, \dots \eta'_{3n+k}$, involving also in general $\eta_1, \eta_2, \dots \eta_{3n+k}$, and having a variation which may be thus expressed:

$$\left.\begin{array}{l} \delta \mathrm{T} = \left(\frac{\delta \mathrm{T}}{\delta \eta'_1}\right) \delta \eta'_1 + \left(\frac{\delta \mathrm{T}}{\delta \eta'_2}\right) \delta \eta'_2 + \dots + \left(\frac{\delta \mathrm{T}}{\delta \eta'_{3n+k}}\right) \delta \eta'_{3n+k} \\ \quad + \left(\frac{\delta \mathrm{T}}{\delta \eta_1}\right) \delta \eta_1 + \left(\frac{\delta \mathrm{T}}{\delta \eta_2}\right) \delta \eta_2 + \dots + \left(\frac{\delta \mathrm{T}}{\delta \eta_{3n+k}}\right) \delta \eta_{3n+k}\,; \end{array}\right\} \dots \quad (33.)$$

or in this other way,

$$\left.\begin{array}{l} \delta \mathrm{T} = \frac{\delta \mathrm{T}}{\delta \eta'_1} \delta \eta'_1 + \frac{\delta \mathrm{T}}{\delta \eta'_2} \delta \eta'_2 + \dots + \frac{\delta \mathrm{T}}{\delta \eta'_{3n}} \delta \eta'_{3n} \\ \quad + \frac{\delta \mathrm{T}}{\delta \eta_1} \delta \eta_1 + \frac{\delta \mathrm{T}}{\delta \eta_2} \delta \eta_2 + \dots + \frac{\delta \mathrm{T}}{\delta \eta_{3n}} \delta \eta_{3n}, \end{array}\right\} \quad \dots \dots \quad (34.)$$

on account of the relations (32.), which give, when differentiated with respect to the time,

$$\left.\begin{array}{l} \eta'_{3n+1} = \eta'_1 \frac{\delta \psi_1}{\delta \eta_1} + \eta'_2 \frac{\delta \psi_1}{\delta \eta_2} + \dots + \eta'_{3n} \frac{\delta \psi_1}{\delta \eta_{3n}}, \\ \eta'_{3n+2} = \eta'_1 \frac{\delta \psi_2}{\delta \eta_1} + \eta'_2 \frac{\delta \psi_2}{\delta \eta_2} + \dots + \eta'_{3n} \frac{\delta \psi_2}{\delta \eta_{3n}}, \\ \dots\dots \\ \eta'_{3n+k} = \eta'_1 \frac{\delta \psi_k}{\delta \eta_1} + \eta'_2 \frac{\delta \psi_k}{\delta \eta_2} + \dots + \eta'_{3n} \frac{\delta \psi_k}{\delta \eta_{3n}}, \end{array}\right\} \quad \dots \dots \quad (35.)$$

and therefore, attending only to the variations of quantities of the form η',

$$\left.\begin{array}{l} \delta \eta'_{3n+1} = \frac{\delta \psi_1}{\delta \eta_1} \delta \eta'_1 + \frac{\delta \psi_1}{\delta \eta_2} \delta \eta'_2 + \dots + \frac{\delta \psi_1}{\delta \eta_{3n}} \delta \eta'_{3n}, \\ \delta \eta'_{3n+2} = \frac{\delta \psi_2}{\delta \eta_1} \delta \eta'_1 + \frac{\delta \psi_2}{\delta \eta_2} \delta \eta'_2 + \dots + \frac{\delta \psi_2}{\delta \eta_{3n}} \delta \eta'_{3n}, \\ \dots\dots \\ \delta \eta'_{3n+k} = \frac{\delta \psi_k}{\delta \eta_1} \delta \eta'_1 + \frac{\delta \psi_k}{\delta \eta_2} \delta \eta'_2 + \dots + \frac{\delta \psi_k}{\delta \eta_{3n}} \delta \eta'_{3n}. \end{array}\right\} \quad \dots \dots \quad (36.)$$

Comparing the two expressions (33.) and (34.), we find by (36.) the relations

$$\left.\begin{aligned}
\frac{\delta T}{\delta \eta'_1} &= \left(\frac{\delta T}{\delta \eta'_1}\right) + \left(\frac{\delta T}{\delta \eta'_{3n+1}}\right)\frac{\delta \psi_1}{\delta \eta_1} + \left(\frac{\delta T}{\delta \eta'_{3n+2}}\right)\frac{\delta \psi_2}{\delta \eta_1} + \dots + \left(\frac{\delta T}{\delta \eta'_{3n+k}}\right)\frac{\delta \psi_k}{\delta \eta_1},\\
\frac{\delta T}{\delta \eta'_2} &= \left(\frac{\delta T}{\delta \eta'_2}\right) + \left(\frac{\delta T}{\delta \eta'_{3n+1}}\right)\frac{\delta \psi_1}{\delta \eta_2} + \left(\frac{\delta T}{\delta \eta'_{3n+2}}\right)\frac{\delta \psi_2}{\delta \eta_2} + \dots + \left(\frac{\delta T}{\delta \eta'_{3n+k}}\right)\frac{\delta \psi_k}{\delta \eta_2},\\
&\dots\dots\\
\frac{\delta T}{\delta \eta'_{3n}} &= \left(\frac{\delta T}{\delta \eta'_{3n}}\right) + \left(\frac{\delta T}{\delta \eta'_{3n+1}}\right)\frac{\delta \psi_1}{\delta \eta_{3n}} + \left(\frac{\delta T}{\delta \eta'_{3n+2}}\right)\frac{\delta \psi_2}{\delta \eta_{3n}} + \dots + \left(\frac{\delta T}{\delta \eta'_{3n+k}}\right)\frac{\delta \psi_k}{\delta \eta_{3n}};
\end{aligned}\right\} \quad (37.)$$

which give, by (32.),

$$\left.\begin{aligned}
&\frac{\delta T}{\delta \eta'_1}\delta \eta_1 + \frac{\delta T}{\delta \eta'_2}\delta \eta_2 + \dots + \frac{\delta T}{\delta \eta'_{3n}}\delta \eta_{3n} =\\
&\left(\frac{\delta T}{\delta \eta'_1}\right)\delta \eta_1 + \left(\frac{\delta T}{\delta \eta'_2}\right)\delta \eta_2 \dots + \left(\frac{\delta T}{\delta \eta'_{3n+k}}\right)\delta \eta_{3n+k};
\end{aligned}\right\} \quad \dots\dots\dots \quad (38.)$$

we may therefore put the expression (Q.) under the following more general form,

$$\delta V = \Sigma \,.\left(\frac{\delta T}{\delta \eta'}\right)\delta \eta - \Sigma \,.\left(\frac{\delta T_0}{\delta e'}\right)\delta e + t\,\delta H, \quad \dots\dots\dots\dots \quad (A^1.)$$

the coefficients $\left(\frac{\delta T}{\delta \eta'}\right)$ being formed by treating all the $3\,n + k$ quantities $\eta'_1, \eta'_2, \dots \eta'_{3n+k}$, as independent; which was the extension above announced, of the rule for forming the variation of the characteristic function V.

We cannot, however, immediately decompose this new expression (A^1.) for δ V, as we did the expression (Q.), by treating all the variations $\delta\,\eta$, $\delta\,e$, as independent; but we may decompose it so, if we previously combine it with the final equations of condition (31.), and with the analogous initial equations of condition, namely,

$$\left.\begin{aligned}
0 &= \Phi_1\left(e_1, e_2, \dots e_{3n+k}\right),\\
0 &= \Phi_2\left(e_1, e_2, \dots e_{3n+k}\right),\\
&\dots\dots\\
0 &= \Phi_k\left(e_1, e_2, \dots e_{3n+k}\right),
\end{aligned}\right\} \quad \dots\dots\dots\dots\dots \quad (39.)$$

which we may do by adding the variations of the connecting functions $\varphi_1, \dots \varphi_k$, $\Phi_1, \dots \Phi_k$, multiplied respectively by factors to be determined, $\lambda_1, \dots \lambda_k$, $\Lambda_1, \dots \Lambda_k$. In this manner the law of varying action takes this new form,

$$\delta V = \Sigma \,.\left(\frac{\delta T}{\delta \eta'}\right)\delta \eta - \Sigma \,.\left(\frac{\delta T_0}{\delta e'}\right)\delta e + t\,\delta H + \Sigma \,.\,\lambda\,\delta\,\varphi + \Sigma \,.\,\Lambda\,\delta\,\Phi; \quad . \quad . \quad (B^1.)$$

and decomposes itself into $6\,n + 2\,k + 1$ separate expressions, for the partial differential coefficients of the first order of the characteristic function V, namely, into the following,

$$\left.\begin{array}{l}\frac{\delta V}{\delta \eta_1} = \left(\frac{\delta T}{\delta \eta'_1}\right) + \lambda_1 \frac{\delta \varphi_1}{\delta \eta_1} + \lambda_2 \frac{\delta \varphi_2}{\delta \eta_1} + \ldots + \lambda_k \frac{\delta \varphi_k}{\delta \eta_1}, \\ \frac{\delta V}{\delta \eta_2} = \left(\frac{\delta T}{\delta \eta'_2}\right) + \lambda_1 \frac{\delta \varphi_1}{\delta \eta_2} + \lambda_2 \frac{\delta \varphi_2}{\delta \eta_2} + \ldots + \lambda_k \frac{\delta \varphi_k}{\delta \eta_2}, \\ \ldots\ldots \\ \frac{\delta V}{\delta \eta_{3n+k}} = \left(\frac{\delta T}{\delta \eta'_{3n+k}}\right) + \lambda_1 \frac{\delta \varphi_1}{\delta \eta_{3n+k}} + \ldots + \lambda_k \frac{\delta \varphi_k}{\delta \eta_{3n+k}}, \end{array}\right\} \quad \ldots\ldots (C^1.)$$

and

$$\left.\begin{array}{l}\frac{\delta V}{\delta e_1} = -\left(\frac{\delta T_0}{\delta e'_1}\right) + \Lambda_1 \frac{\delta \Phi_1}{\delta e_1} + \Lambda_2 \frac{\delta \Phi_2}{\delta e_1} + \ldots + \Lambda_k \frac{\delta \Phi_k}{\delta e_1}, \\ \frac{\delta V}{\delta e_2} = -\left(\frac{\delta T_0}{\delta e'_2}\right) + \Lambda_1 \frac{\delta \Phi_1}{\delta e_2} + \Lambda_2 \frac{\delta \Phi_2}{\delta e_2} + \ldots + \Lambda_k \frac{\delta \Phi_k}{\delta e_2}, \\ \ldots\ldots \\ \frac{\delta V}{\delta e_{3n+k}} = -\left(\frac{\delta T_0}{\delta e'_{3n+k}}\right) + \Lambda_1 \frac{\delta \Phi_1}{\delta e_{3n+k}} + \ldots + \Lambda_k \frac{\delta \Phi_k}{\delta e_{3n+k}}, \end{array}\right\} \quad \ldots\ldots (D^1.)$$

besides the old equation (E.). The analogous introduction of multipliers in the canonical forms of LAGRANGE, for the differential equations of motion of the second order, by which a sum such as $\Sigma . \lambda \frac{\delta \varphi}{\delta \eta}$ is added to $\frac{\delta U}{\delta \eta}$ in the second member of the formula (Y.), is also easily justified on the principles of the present essay.

Separation of the relative motion of a system from the motion of its centre of gravity; characteristic function for such relative motion, and law of its variation.

10. As an example of the foregoing transformations, and at the same time as an important application, we shall now introduce relative coordinates, $x_{\prime}\, y_{\prime}\, z_{\prime}$, referred to an internal origin $x_{\prime\prime}\, y_{\prime\prime}\, z_{\prime\prime}$; that is, we shall put

$$x_i = x_{\prime i} + x_{\prime\prime}, \quad y_i = y_{\prime i} + y_{\prime\prime}, \quad z_i = z_{\prime i} + z_{\prime\prime}, \quad \ldots\ldots (40.)$$

and in like manner

$$a_i = a_{\prime i} + a_{\prime\prime}, \quad b_i = b_{\prime i} + b_{\prime\prime}, \quad c_i = c_{\prime i} + c_{\prime\prime}; \quad \ldots\ldots (41.)$$

together with the differentiated expressions

$$x'_i = x'_{\prime i} + x'_{\prime\prime}, \quad y'_i = y'_{\prime i} + y'_{\prime\prime}, \quad z'_i = z'_{\prime i} + z'_{\prime\prime}, \quad \ldots\ldots (42.)$$

and

$$a'_i = a'_{\prime i} + a'_{\prime\prime}, \quad b'_i = b'_{\prime i} + b'_{\prime\prime}, \quad c'_i = c'_{\prime i} + c'_{\prime\prime}. \quad \ldots\ldots (43.)$$

Introducing the expressions (42.) for the rectangular components of velocity, we find that the value given by (4.) for the living force 2 T, decomposes itself into the three following parts,

$$2\,T = \Sigma . m\,(x'^2 + y'^2 + z'^2) = \Sigma . m\,(x'^2_{\prime} + y'^2_{\prime} + z'^2_{\prime})$$
$$+ 2\,(x'_{\prime\prime}\,\Sigma . m\,x'_{\prime} + y'_{\prime\prime}\,\Sigma . m\,y'_{\prime} + z'_{\prime\prime}\,\Sigma . m\,z'_{\prime}) + (x'^2_{\prime\prime} + y'^2_{\prime\prime} + z'^2_{\prime\prime})\,\Sigma m; \quad (44.)$$

if then we establish, as we may, the three equations of condition,

$$\Sigma . m x_{,} = 0, \qquad \Sigma . m y_{,} = 0, \qquad \Sigma . m z_{,} = 0, \quad \ldots \ldots \quad (45.)$$

which give by (40.),

$$x_{,,} = \frac{\Sigma . m x}{\Sigma m}, \qquad y_{,,} = \frac{\Sigma . m y}{\Sigma m}, \qquad z_{,,} = \frac{\Sigma . m z}{\Sigma m}, \quad \ldots \ldots \quad (46.)$$

so that $x_{,,}\, y_{,,}\, z_{,,}$ are now the coordinates of the point which is called the centre of gravity of the system, we may reduce the function T to the form

$$T = T_{,} + T_{,,}, \quad \ldots \ldots \quad (47.)$$

in which

$$T_{,} = \tfrac{1}{2} \Sigma . m (x'^2_{,} + y'^2_{,} + z'^2_{,}), \quad \ldots \ldots \quad (48.)$$

and

$$T_{,,} = \tfrac{1}{2} (x'^2_{,,} + y'^2_{,,} + z'^2_{,,}) \Sigma m. \quad \ldots \ldots \quad (49.)$$

By this known decomposition, the whole living force 2 T of the system is resolved into the two parts 2 $T_{,}$ and 2 $T_{,,}$, of which the former, 2 $T_{,}$, may be called the *relative living force*, being that which results solely from the relative velocities of the points of the system, in their motions about their common centre of gravity $x_{,,}\, y_{,,}\, z_{,,}$; while the latter part, 2 $T_{,,}$, results only from the absolute motion of that centre of gravity in space, and is the same as if all the masses of the system were united in that common centre. At the same time, the law of living force, T = U + H, (6.), resolves itself by the law of motion of the centre of gravity into the two following separate equations,

$$T_{,} = U + H_{,}, \quad \ldots \ldots \quad (50.)$$

and

$$T_{,,} = H_{,,}; \quad \ldots \ldots \quad (51.)$$

$H_{,}$ and $H_{,,}$ being two new constants independent of the time *t*, and such that their sum

$$H_{,} + H_{,,} = H. \quad \ldots \ldots \quad (52.)$$

And we may in like manner decompose the action, or accumulated living force V, which is equal to the definite integral $\int_0^t 2\, T\, dt$, into the two following analogous parts,

$$V = V_{,} + V_{,,}, \quad \ldots \ldots \quad (E^1.)$$

determined by the two equations,

$$V_{,} = \int_0^t 2\, T_{,}\, dt, \quad \ldots \ldots \quad (F^1.)$$

and

$$V_{,,} = \int_0^t 2\, T_{,,}\, dt. \quad \ldots \ldots \quad (G^1.)$$

The last equation gives by (51.),

$$V_{,,} = 2\, H_{,,}\, t; \quad \ldots \ldots \quad (53.)$$

a result which, by the law of motion of the centre of gravity, may be thus expressed,

$$V_{,,} = \sqrt{(x_{,,} - a_{,,})^2 + (y_{,,} - b_{,,})^2 + (z_{,,} - c_{,,})^2} . \sqrt{2\, H_{,,}\, \Sigma m} : \quad \ldots \quad (H^1.)$$

$a_{\prime\prime}\, b_{\prime\prime}\, c_{\prime\prime}$ being the initial coordinates of the centre of gravity, so that

$$a_{\prime\prime} = \frac{\Sigma \,.\, m\, a}{\Sigma\, m}, \quad b_{\prime\prime} = \frac{\Sigma \,.\, m\, b}{\Sigma\, m}, \quad c_{\prime\prime} = \frac{\Sigma \,.\, m\, c}{\Sigma\, m}. \quad \ldots \ldots \quad (54.)$$

And for the variation $\delta\, \mathrm{V}$ of the whole function V, the rule of the last number gives

$$\left.\begin{array}{l} \delta\, \mathrm{V} = \Sigma \,.\, m\, (x'_{\prime}\, \delta\, x_{\prime} - a'_{\prime}\, \delta\, a_{\prime} + y'_{\prime}\, \delta\, y_{\prime} - b'_{\prime}\, \delta\, b_{\prime} + z'_{\prime}\, \delta\, z_{\prime} - c'_{\prime}\, \delta\, c_{\prime}) \\ \quad + (x'_{\prime\prime}\, \delta\, x_{\prime\prime} - a'_{\prime\prime}\, \delta\, a_{\prime\prime} + y'_{\prime\prime}\, \delta\, y_{\prime\prime} - b'_{\prime\prime}\, \delta\, b_{\prime\prime} + z'_{\prime\prime}\, \delta\, z_{\prime\prime} - c'_{\prime\prime}\, \delta\, c_{\prime\prime})\, \Sigma\, m \\ \quad + t\, \delta\, \mathrm{H} + \lambda_1\, \Sigma \,.\, m\, \delta\, x_{\prime} + \lambda_2\, \Sigma \,.\, m\, \delta\, y_{\prime} + \lambda_3\, \Sigma \,.\, m\, \delta\, z_{\prime} \\ \quad + \Lambda_1\, \Sigma \,.\, m\, \delta\, a_{\prime} + \Lambda_2\, \Sigma \,.\, m\, \delta\, b_{\prime} + \Lambda_3\, \Sigma \,.\, m\, \delta\, c_{\prime}\,; \end{array}\right\} \quad . \quad (\mathrm{I}^1.)$$

while the variation of the part $\mathrm{V}_{\prime\prime}$, determined by the equation (H^1.), is easily shown to be equivalent to the part

$$\delta\, \mathrm{V}_{\prime\prime} = (x'_{\prime\prime}\, \delta\, x_{\prime\prime} - a'_{\prime\prime}\, \delta\, a_{\prime\prime} + y'_{\prime\prime}\, \delta\, y_{\prime\prime} - b'_{\prime\prime}\, \delta\, b_{\prime\prime} + z'_{\prime\prime}\, \delta\, z_{\prime\prime} - c'_{\prime\prime}\, \delta\, c_{\prime\prime})\, \Sigma\, m + t\, \delta\, \mathrm{H}_{\prime\prime}\,; \quad . \quad (\mathrm{K}^1.)$$

the variation of the other part $\mathrm{V}_{\prime}$ may therefore be thus expressed,

$$\left.\begin{array}{l} \delta\, \mathrm{V}_{\prime} = \Sigma \,.\, m\, (x'_{\prime}\, \delta\, x_{\prime} - a'_{\prime}\, \delta\, a_{\prime} + y'_{\prime}\, \delta\, y_{\prime} - b'_{\prime}\, \delta\, b_{\prime} + z'_{\prime}\, \delta\, z_{\prime} - c'_{\prime}\, \delta\, c_{\prime}) \\ \qquad + t\, \delta\, \mathrm{H}_{\prime} + \lambda_1\, \Sigma \,.\, m\, \delta\, x_{\prime} + \lambda_2\, \Sigma \,.\, m\, \delta\, y_{\prime} + \lambda_3\, \Sigma \,.\, m\, \delta\, z_{\prime} \\ \qquad + \Lambda_1\, \Sigma \,.\, m\, \delta\, a_{\prime} + \Lambda_2\, \Sigma \,.\, m\, \delta\, b_{\prime} + \Lambda_3\, \Sigma \,.\, m\, \delta\, c_{\prime}: \end{array}\right\} \quad . \quad . \quad (\mathrm{L}^1.)$$

and it resolves itself into the following separate expressions, in which the part $\mathrm{V}_{\prime}$ is considered as a function of the $6\,n + 1$ quantities $x_{\prime i}\, y_{\prime i}\, z_{\prime i}\, a_{\prime i}\, b_{\prime i}\, c_{\prime i}\, \mathrm{H}_{\prime}$, of which, however, only $6\,n - 5$ are really independent:

first group,

$$\left.\begin{array}{l} \dfrac{\delta\, \mathrm{V}_{\prime}}{\delta\, x_{\prime 1}} = m_1\, x'_{\prime 1} + \lambda_1\, m_1\,; \ \ldots\ \dfrac{\delta\, \mathrm{V}_{\prime}}{\delta\, x_{\prime n}} = m_n\, x'_{\prime n} + \lambda_1\, m_n\,; \\ \dfrac{\delta\, \mathrm{V}_{\prime}}{\delta\, y_{\prime 1}} = m_1\, y'_{\prime 1} + \lambda_2\, m_1\,; \ \ldots\ \dfrac{\delta\, \mathrm{V}_{\prime}}{\delta\, y_{\prime n}} = m_n\, y'_{\prime n} + \lambda_2\, m_n\,; \\ \dfrac{\delta\, \mathrm{V}_{\prime}}{\delta\, z_{\prime 1}} = m_1\, z'_{\prime 1} + \lambda_3\, m_1\,; \ \ldots\ \dfrac{\delta\, \mathrm{V}_{\prime}}{\delta\, z_{\prime n}} = m_n\, z'_{\prime n} + \lambda_3\, m_n\,; \end{array}\right\} \quad . \quad . \quad . \quad (\mathrm{M}^1.)$$

second group,

$$\left.\begin{array}{l} \dfrac{\delta\, \mathrm{V}_{\prime}}{\delta\, a_{\prime 1}} = -\, m_1\, a'_{\prime 1} + \Lambda_1\, m_1\,; \ \ldots\ \dfrac{\delta\, \mathrm{V}_{\prime}}{\delta\, a_{\prime n}} = -\, m_n\, a'_{\prime n} + \Lambda_1\, m_n\,; \\ \dfrac{\delta\, \mathrm{V}_{\prime}}{\delta\, b_{\prime 1}} = -\, m_1\, b'_{\prime 1} + \Lambda_2\, m_1\,; \ \ldots\ \dfrac{\delta\, \mathrm{V}_{\prime}}{\delta\, b_{\prime n}} = -\, m_n\, b'_{\prime n} + \Lambda_2\, m_n\,; \\ \dfrac{\delta\, \mathrm{V}_{\prime}}{\delta\, c_{\prime 1}} = -\, m_1\, c'_{\prime 1} + \Lambda_3\, m_1\,; \ \ldots\ \dfrac{\delta\, \mathrm{V}_{\prime}}{\delta\, c_{\prime n}} = -\, m_n\, c'_{\prime n} + \Lambda_3\, m_n\,; \end{array}\right\} . \quad . \quad (\mathrm{N}^1.)$$

and finally,

$$\frac{\delta\, \mathrm{V}_{\prime}}{\delta\, \mathrm{H}_{\prime}} = t. \quad \ldots \ldots \quad (\mathrm{O}^1.)$$

With respect to the six multipliers $\lambda_1\, \lambda_2\, \lambda_3\, \Lambda_1\, \Lambda_2\, \Lambda_3$ which were introduced by the 3 final equations of condition (45.), and by the 3 analogous initial equations of condition,

$$\Sigma \,.\, m\, a_{\prime} = 0, \ \ \Sigma \,.\, m\, b_{\prime} = 0, \ \ \Sigma \,.\, m\, c_{\prime} = 0\,; \quad \ldots \ldots \quad (55.)$$

we have, by differentiating these conditions,

$$\Sigma \,.\, m x'_{,} = 0,\ \ \Sigma \,.\, m y'_{,} = 0,\ \ \Sigma \,.\, m z'_{,} = 0, \quad \ldots \ldots \quad (56.)$$

and

$$\Sigma \,.\, m a'_{,} = 0,\ \ \Sigma \,.\, m b'_{,} = 0,\ \ \Sigma \,.\, m c'_{,} = 0\,; \quad \ldots \ldots \quad (57.)$$

and therefore

$$\lambda_1 = \frac{\Sigma \frac{\delta \mathrm{V}_{,}}{\delta x_{,}}}{\Sigma m},\ \ \lambda_2 = \frac{\Sigma \frac{\delta \mathrm{V}_{,}}{\delta y_{,}}}{\Sigma m},\ \ \lambda_3 = \frac{\Sigma \frac{\delta \mathrm{V}_{,}}{\delta z_{,}}}{\Sigma m}, \quad \ldots \ldots \quad (58.)$$

and

$$\Lambda_1 = \frac{\Sigma \frac{\delta \mathrm{V}_{,}}{\delta a_{,}}}{\Sigma m},\ \ \Lambda_2 = \frac{\Sigma \frac{\delta \mathrm{V}_{,}}{\delta b_{,}}}{\Sigma m},\ \ \Lambda_3 = \frac{\Sigma \frac{\delta \mathrm{V}_{,}}{\delta c_{,}}}{\Sigma m}. \quad \ldots \ldots \quad (59.)$$

11. As an example of the determination of these multipliers, we may suppose that the part $\mathrm{V}_{,}$, of the whole action V, has been expressed, before differentiation, as a function of $\mathrm{H}_{,}$, and of these other $6n-6$ independent quantities

$$\left.\begin{array}{l} x_{,1} - x_{,n} = \xi_1,\ \ x_{,2} - x_{,n} = \xi_2,\ \ldots\ x_{,n-1} - x_{,n} = \xi_{n-1}, \\ y_{,1} - y_{,n} = \eta_1,\ \ y_{,2} - y_{,n} = \eta_2,\ \ldots\ y_{,n-1} - y_{,n} = \eta_{n-1}, \\ z_{,1} - z_{,n} = \zeta_1,\ \ z_{,2} - z_{,n} = \zeta_2,\ \ldots\ z_{,n-1} - z_{,n} = \zeta_{n-1}, \end{array}\right\} \quad \ldots \quad (60.)$$

and

$$\left.\begin{array}{l} a_{,1} - a_{,n} = \alpha_1,\ \ a_{,2} - a_{,n} = \alpha_2,\ \ldots\ a_{,n-1} - a_{,n} = \alpha_{n-1}, \\ b_{,1} - b_{,n} = \beta_1,\ \ b_{,2} - b_{,n} = \beta_2,\ \ldots\ b_{,n-1} - b_{,n} = \beta_{n-1}, \\ c_{,1} - c_{,n} = \gamma_1,\ \ c_{,2} - c_{,n} = \gamma_2,\ \ldots\ c_{,n-1} - c_{,n} = \gamma_{n-1}; \end{array}\right\} \quad \ldots \quad (61.)$$

that is, of the *differences* only of the *centrobaric* coordinates; or, in other words, as a function of the coordinates (initial and final) of $n-1$ points of the system, referred to the n^{th} point, as an internal or moveable origin: because the centrobaric coordinates $x_{,i}, y_{,i}, z_{,i}, a_{,i}, b_{,i}, c_{,i}$, may themselves, by the equations of condition, be expressed as functions of these, namely,

$$x_{,i} = \xi_i - \frac{\Sigma \,.\, m\,\xi}{\Sigma m},\ \ y_{,i} = \eta_i - \frac{\Sigma \,.\, m\,\eta}{\Sigma m},\ \ z_{,i} = \zeta_i - \frac{\Sigma \,.\, m\,\zeta}{\Sigma m}, \quad \ldots \quad (62.)$$

and in like manner,

$$a_{,i} = \alpha_i - \frac{\Sigma \,.\, m\,\alpha}{\Sigma m},\ \ b_{,i} = \beta_i - \frac{\Sigma \,.\, m\,\beta}{\Sigma m},\ \ c_{,i} = \gamma_i - \frac{\Sigma \,.\, m\,\gamma}{\Sigma m}; \quad \ldots \quad (63.)$$

in which we are to observe, that the six quantities $\xi_n\ \eta_n\ \zeta_n\ \alpha_n\ \beta_n\ \gamma_n$ must be considered as separately vanishing. When $\mathrm{V}_{,}$ has been thus expressed as a function of the centrobaric coordinates, involving their differences only, it will evidently satisfy the six partial differential equations,

$$\left.\begin{array}{l} \Sigma \frac{\delta \mathrm{V}_{,}}{\delta x_{,}} = 0,\ \ \Sigma \frac{\delta \mathrm{V}_{,}}{\delta y_{,}} = 0,\ \ \Sigma \frac{\delta \mathrm{V}_{,}}{\delta z_{,}} = 0, \\ \Sigma \frac{\delta \mathrm{V}_{,}}{\delta a_{,}} = 0,\ \ \Sigma \frac{\delta \mathrm{V}_{,}}{\delta b_{,}} = 0,\ \ \Sigma \frac{\delta \mathrm{V}_{,}}{\delta c_{,}} = 0; \end{array}\right\} \quad \ldots \ldots \quad (\mathrm{P}^1.)$$

after this preparation, therefore, of the function $V_{,}$, the six multipliers determined by (58.) and (59.) will vanish, so that we shall have

$$\lambda_1 = 0, \lambda_2 = 0, \lambda_3 = 0, \Lambda_1 = 0, \Lambda_2 = 0, \Lambda_3 = 0, \quad \ldots \ldots \quad (64.)$$

and the groups (M'.) and (N'.) will reduce themselves to the two following:

$$\left.\begin{array}{l} \frac{\delta V_{,}}{\delta x_{,1}} = m_1 x'_{,1}; \ \frac{\delta V_{,}}{\delta x_{,2}} = m_2 x'_{,2}; \ \ldots \frac{\delta V_{,}}{\delta x_{,n}} = m_n x'_{,n}; \\ \frac{\delta V_{,}}{\delta y_{,1}} = m_1 y'_{,1}; \ \frac{\delta V_{,}}{\delta y_{,2}} = m_2 y'_{,2}; \ \ldots \frac{\delta V_{,}}{\delta y_{,n}} = m_n y'_{,n}; \\ \frac{\delta V_{,}}{\delta z_{,1}} = m_1 z'_{,1}; \ \frac{\delta V_{,}}{\delta z_{,2}} = m_2 z'_{,2}; \ \ldots \frac{\delta V_{,}}{\delta z_{,n}} = m_n z'_{,n}; \end{array}\right\} \quad \ldots \ldots \quad (Q^1.)$$

and

$$\left.\begin{array}{l} \frac{\delta V_{,}}{\delta a_{,1}} = - m_1 a'_{,1}; \ \frac{\delta V_{,}}{\delta a_{,2}} = - m_2 a'_{,2}; \ \ldots \frac{\delta V_{,}}{\delta a_{,n}} = - m_n a'_{,n}; \\ \frac{\delta V_{,}}{\delta b_{,1}} = - m_1 b'_{,1}; \ \frac{\delta V_{,}}{\delta b_{,2}} = - m_2 b'_{,2}; \ \ldots \frac{\delta V_{,}}{\delta b_{,n}} = - m_n b'_{,n}; \\ \frac{\delta V_{,}}{\delta c_{,1}} = - m_1 c'_{,1}; \ \frac{\delta V_{,}}{\delta c_{,2}} = - m_2 c'_{,2}; \ \ldots \frac{\delta V_{,}}{\delta c_{,n}} = - m_n c'_{,n}; \end{array}\right\} \quad \ldots \quad (R^1.)$$

analogous in all respects to the groups (C.) and (D.). We find, therefore, for the relative motion of a system about its own centre of gravity, equations of the same form as those which we had obtained before for the absolute motion of the same system of points in space. And we see that in investigating such relative motion only, it is useful to confine ourselves to the part $V_{,}$ of our whole characteristic function, that is, to the *relative action* of the system, or accumulated living force of the motion about the centre of gravity; and to consider this part as the *characteristic function* of such relative motion, in a sense analogous to that which has been already explained.

This relative action, or part $V_{,}$, may, however, be otherwise expressed, and even in an infinite variety of ways, on account of the six equations of condition which connect the $6n$ centrobaric coordinates; and every different preparation of its form will give a different set of values for the six multipliers $\lambda_1 \lambda_2 \lambda_3 \Lambda_1 \Lambda_2 \Lambda_3$. For example, we might eliminate, by a previous preparation, the six centrobaric coordinates of the point m_n from the expression of $V_{,}$, so as to make this expression involve only the centrobaric coordinates of the other $n - 1$ points of the system, and then we should have

$$\frac{\delta V_{,}}{\delta x_{,n}} = 0, \frac{\delta V_{,}}{\delta y_{,n}} = 0, \frac{\delta V_{,}}{\delta z_{,n}} = 0, \frac{\delta V_{,}}{\delta a_{,n}} = 0, \frac{\delta V_{,}}{\delta b_{,n}} = 0, \frac{\delta V_{,}}{\delta c_{,n}} = 0, \quad \ldots \quad (S^1.)$$

and therefore, by the six last equations of the groups (M'.) and (N'.), the multipliers would take the values

$$\lambda_1 = - x'_{,n}, \lambda_2 = - y'_{,n}, \lambda_3 = - z'_{,n}, \Lambda_1 = a'_{,n}, \Lambda_2 = b'_{,n}, \Lambda_3 = c'_{,n}, \quad . \quad (65.)$$

and would reduce, by (60.) and (61.), the preceding $6n - 6$ equations of the same groups (M'.) and (N'.), to the forms

$$\left.\begin{array}{l}\frac{\delta V_{,}}{\delta x_{,1}} = m_1 \xi'_1, \frac{\delta V_{,}}{\delta x_{,2}} = m_2 \xi'_2, \ldots \frac{\delta V_{,}}{\delta x_{,n-1}} = m_{n-1} \xi'_{n-1}, \\ \frac{\delta V_{,}}{\delta y_{,1}} = m_1 \eta'_1, \frac{\delta V_{,}}{\delta y_{,2}} = m_2 \eta'_2, \ldots \frac{\delta V_{,}}{\delta y_{,n-1}} = m_{n-1} \eta'_{n-1}, \\ \frac{\delta V_{,}}{\delta z_{,1}} = m_1 \zeta'_1, \frac{\delta V_{,}}{\delta z_{,2}} = m_2 \zeta'_2, \ldots \frac{\delta V_{,}}{\delta z_{,n-1}} = m_{n-1} \zeta'_{n-1},\end{array}\right\} \quad (T^1.)$$

and

$$\left.\begin{array}{l}\frac{\delta V_{,}}{\delta a_{,1}} = - m_1 \alpha'_1, \frac{\delta V_{,}}{\delta a_{,2}} = - m_2 \alpha'_2, \ldots \frac{\delta V_{,}}{\delta a_{,n-1}} = - m_{n-1} \alpha'_{n-1}, \\ \frac{\delta V_{,}}{\delta b_{,1}} = - m_1 \beta'_1, \frac{\delta V_{,}}{\delta b_{,2}} = - m_2 \beta'_2, \ldots \frac{\delta V_{,}}{\delta b_{,n-1}} = - m_{n-1} \beta'_{n-1}, \\ \frac{\delta V_{,}}{\delta c_{,1}} = - m_1 \gamma'_1, \frac{\delta V_{,}}{\delta c_{,2}} = - m_2 \gamma'_2, \ldots \frac{\delta V_{,}}{\delta c_{,n-1}} = - m_{n-1} \gamma'_{n-1}.\end{array}\right\} \quad (U^1.)$$

12. We might also express the relative action $V_{,}$, not as a function of the centrobaric, but of some other internal coordinates, or marks of relative position. We might, for instance, express it and its variation as functions of the $6n - 6$ independent internal coordinates $\xi\ \eta\ \zeta\ \alpha\ \beta\ \gamma$ already mentioned, and of their variations, defining these without any reference to the centre of gravity, by the equations

$$\left.\begin{array}{l}\xi_i = x_i - x_n, \eta_i = y_i - y_n, \zeta_i = z_i - z_n, \\ \alpha_i = a_i - a_n, \beta_i = b_i - b_n, \gamma_i = c_i - c_n.\end{array}\right\} \quad (66.)$$

For all such transformations of $\delta V_{,}$ it is easy to establish a rule or law, which may be called the *law of varying relative action* (exactly analogous to the rule (B^1.)), namely, the following:

$$\delta V_{,} = \Sigma . \left(\frac{\delta T_{,}}{\delta \eta'_{,}}\right) \delta \eta_{,} - \Sigma . \left(\frac{\delta T_{,0}}{\delta e'_{,}}\right) \delta e_{,} + t \delta H_{,} + \Sigma . \lambda_{,} \delta \varphi_{,} + \Sigma . \Lambda_{,} \delta \Phi_{,}; \quad (V^1.)$$

which implies that we are to express the half $T_{,}$ of the relative living force of the system as a function of the rates of increase $\eta'_{,}$ of any marks of relative position; and after taking its variation with respect to these rates, to change their variations to the variations of the marks of position themselves; then to subtract the initial from the final value of the result, and to add the variations of the final and initial functions $\varphi_{,}\ \Phi_{,}$, which enter into the equations of condition, if any, of the form $\varphi_{,} = 0$, $\Phi_{,} = 0$, (connecting the final and initial marks of relative position,) multiplied respectively by undetermined factors $\lambda_{,}\ \Lambda_{,}$; and lastly, to equate the whole result to $\delta V_{,} - t \delta H_{,}$, $H_{,}$ being the quantity independent of the time in the equation (50.) of relative living force, and $V_{,}$ being the relative action, of which we desired to express the variation. It is not necessary to dwell here on the demonstration of this new rule (V^1.), which may easily be deduced from the principles already laid down; or by the calculus of variations from the law of relative living force, combined with the differential equations of the second order of relative motion.

But to give an example of its application, let us resume the problem already mentioned, namely to express $\delta V_{\prime}$ by means of the $6n-5$ independent variations $\delta\xi_i\ \delta\eta_i\ \delta\zeta_i\ \delta\alpha_i\ \delta\beta_i\ \delta\gamma_i\ \delta H_{\prime}$. For this purpose we shall employ a known transformation of the relative living force $2T_{\prime\prime}$, multiplied by the sum of the masses of the system, namely the following:

$$2T_{\prime}\Sigma m = \Sigma . m_i m_k \left\{(x'_i - x'_k)^2 + (y'_i - y'_k)^2 + (z'_i - z'_k)^2\right\} : \quad . \ . \ . \ (67.)$$

the sign of summation Σ extending, in the second member, to all the combinations of points two by two, which can be formed without repetition. This transformation gives, by (66.),

$$\left.\begin{aligned} 2T_{\prime}\Sigma m &= m_n \Sigma_{\prime} . m(\xi'^2 + \eta'^2 + \zeta'^2) \\ &+ \Sigma_{\prime} . m_i m_k \left\{(\xi'_i - \xi'_k)^2 + (\eta'_i - \eta'_k)^2 + (\zeta'_i - \zeta'_k)^2\right\}; \end{aligned}\right\} \quad . \ . \ (68.)$$

the sign of summation $\Sigma_{\prime}$ extending only to the first $n-1$ points of the system. Applying, therefore, our general rule or law of varying relative action, and observing that the $6n-6$ internal coordinates $\xi\ \eta\ \zeta\ \alpha\ \beta\ \gamma$ are independent, we find the following new expression:

$$\left.\begin{aligned} \delta V_{\prime} &= t\delta H_{\prime} + \frac{m_n}{\Sigma m} . \Sigma_{\prime} . m(\xi'\delta\xi - \alpha'\delta\alpha + \eta'\delta\eta - \beta'\delta\beta + \zeta'\delta\zeta - \gamma'\delta\gamma) \\ &+ \frac{1}{\Sigma m} . \Sigma_{\prime} . m_i m_k \left\{(\xi'_i - \xi'_k)(\delta\xi_i - \delta\xi_k) + (\eta'_i - \eta'_k)(\delta\eta_i - \delta\eta_k) + (\zeta'_i - \zeta'_k)(\delta\zeta_i - \delta\zeta_k)\right\} \\ &- \frac{1}{\Sigma m} . \Sigma_{\prime} . m_i m_k \left\{(\alpha'_i - \alpha'_k)(\delta\alpha_i - \delta\alpha_k) + (\beta'_i - \beta'_k)(\delta\beta_i - \delta\beta_k) + (\gamma'_i - \gamma'_k)(\delta\gamma_i - \delta\gamma_k)\right\} : \end{aligned}\right\} (W^1.)$$

which gives, besides the equation $(O^1.)$, the following groups:

$$\left.\begin{aligned} \frac{\delta V_{\prime}}{\delta\xi_i} &= \frac{m_i}{\Sigma m} . \Sigma . m(\xi'_i - \xi') = m_i\left(\xi'_i - \frac{\Sigma_{\prime} m\xi'}{\Sigma m}\right), \\ \frac{\delta V_{\prime}}{\delta\eta_i} &= \frac{m_i}{\Sigma m} . \Sigma . m(\eta'_i - \eta') = m_i\left(\eta'_i - \frac{\Sigma_{\prime} m\eta'}{\Sigma m}\right), \\ \frac{\delta V_{\prime}}{\delta\zeta_i} &= \frac{m_i}{\Sigma m} . \Sigma . m(\zeta'_i - \zeta') = m_i\left(\zeta'_i - \frac{\Sigma_{\prime} m\zeta'}{\Sigma m}\right), \end{aligned}\right\} \quad . \ . \ . \ . \ . \ . \ . \ . \ (X^1.)$$

and

$$\left.\begin{aligned} \frac{\delta V_{\prime}}{\delta\alpha_i} &= \frac{-m_i}{\Sigma m} . \Sigma . m(\alpha'_i - \alpha') = -m_i\left(\alpha'_i - \frac{\Sigma_{\prime} m\alpha'}{\Sigma m}\right), \\ \frac{\delta V_{\prime}}{\delta\beta_i} &= \frac{-m_i}{\Sigma m} . \Sigma . m(\beta'_i - \beta') = -m_i\left(\beta'_i - \frac{\Sigma_{\prime} m\beta'}{\Sigma m}\right), \\ \frac{\delta V_{\prime}}{\delta\gamma_i} &= \frac{-m_i}{\Sigma m} . \Sigma . m(\gamma'_i - \gamma') = -m_i\left(\gamma'_i - \frac{\Sigma_{\prime} m\gamma'}{\Sigma m}\right); \end{aligned}\right\} \quad . \ . \ . \ . \ . \ . \ . \ (Y^1.)$$

results which may be thus summed up:

$$\left.\begin{array}{l}\delta V_{,} = t\,\delta H_{,} + \Sigma_{,}.\,m\,(\xi'\,\delta\xi - \alpha'\,\delta\alpha + \eta'\,\delta\eta - \beta'\,\delta\beta + \zeta'\,\delta\zeta - \gamma'\,\delta\gamma) \\ - \frac{1}{\Sigma m}(\Sigma_{,}m\,\xi'.\,\Sigma_{,}m\,\delta\xi + \Sigma_{,}m\,\eta'.\,\Sigma_{,}m\,\delta\eta + \Sigma_{,}m\,\zeta'.\,\Sigma_{,}m\,\delta\zeta) \\ + \frac{1}{\Sigma m}(\Sigma_{,}m\,\alpha'.\,\Sigma_{,}m\,\delta\alpha + \Sigma_{,}m\,\beta'.\,\Sigma_{,}m\,\delta\beta + \Sigma_{,}m\,\gamma'.\,\Sigma_{,}m\,\delta\gamma),\end{array}\right\} \quad . \; (Z^1.)$$

and might have been otherwise deduced by our rule, from this other known transformation of $T_{,}$,

$$T_{,} = \frac{1}{2}\,\Sigma_{,}.\,m(\xi'^2 + \eta'^2 + \zeta'^2) - \frac{(\Sigma_{,}m\,\xi')^2 + (\Sigma_{,}m\,\eta')^2 + (\Sigma_{,}m\,\zeta')^2}{2\,\Sigma m}. \quad . \; . \; . \; . \; (69.)$$

And to obtain, with any set of internal or relative marks of position, the two partial differential equations which the characteristic function $V_{,}$ of relative motion must satisfy, and which offer (as we shall find) the chief means of discovering its form, namely, the equations analogous to those marked (F.) and (G.), we have only to eliminate the rates of increase of the marks of position of the system, which determine the final and initial components of the relative velocities of its points, by the law of varying relative action, from the final and initial expressions of the law of relative living force; namely, from the following equations:

$$T_{,} = U + H_{,}, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; (50.)$$

and

$$T_{,0} = U_0 + H_{,}. \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; (70.)$$

The law of areas, or the property respecting rotation which was expressed by the partial differential equations (P.), will also always admit of being expressed in relative coordinates, and will assist in discovering the form of the characteristic function $V_{,}$; by showing that this function involves only such internal coordinates (in number $6n - 9$) as do not alter by any common rotation of all points final and initial, round the centre of gravity, or round any other internal origin; that origin being treated as fixed, and the quantity $H_{,}$ as constant, in determining the effects of this rotation. The general problem of dynamics, respecting the motions of a free system of n points attracting or repelling one another, is therefore reduced, in the last analysis, by the method of the present essay, to the research and differentiation of a function $V_{,}$, depending on $6n - 9$ internal or relative coordinates, and on the quantity $H_{,}$, and satisfying a pair of partial differential equations of the first order and second degree; in integrating which equations, we are to observe, that at the assumed origin of the motion, namely at the moment when $t = 0$, the final or variable coordinates are equal to their initial values, and the partial differential coefficient $\frac{\delta V_{,}}{\delta H_{,}}$ vanishes; and, that at a moment infinitely little distant, the differential alterations of the coordinates have ratios connected with the other partial differential coefficients of the characteristic function $V_{,}$, by the law of varying relative action. It may be here observed, that, although the consideration of the point, called usually the centre of gravity, is very simply suggested by the process of the tenth number, yet this internal centre is even more simply indicated by our early corollaries from the law of varying action; which show that the components of relative final velocities, in any system of attracting or repelling points, may be expressed by the differences of quantities of the form $\frac{1}{m}\frac{\delta V}{\delta x}$, $\frac{1}{m}\frac{\delta V}{\delta y}$, $\frac{1}{m}\frac{\delta V}{\delta z}$: and therefore that in calculating these relative velocities, it is advantageous to introduce the final sums $\Sigma m x$, $\Sigma m y$, $\Sigma m z$, and, for an analogous reason, the initial sums $\Sigma m a$, $\Sigma m b$, $\Sigma m c$, among the marks of the extreme positions of the system, in the expression of the characteristic function V; because, in differentiating that expression for the calculation of relative velocities, those sums may be treated as constant.

On Systems of two Points, in general; Characteristic Function of the motion of any Binary System.

13. To illustrate the foregoing principles, which extend to any free system of points, however numerous, attracting or repelling one another, let us now consider, in particular, a system of two such points. For such a system, the known *force-function* U becomes, by (2.),

$$U = m_1 m_2 f(r), \quad \dots \dots \dots \quad (71.)$$

r being the mutual distance

$$r = \sqrt{(x_1 - x_2)^2 + (y_1 - y_2)^2 + (z_1 - z_2)^2}, \quad \dots \dots \quad (72.)$$

between the two points m_1, m_2, and $f(r)$ being a function of this distance such that its derivative or differential coefficient $f'(r)$ expresses the law of their repulsion or attraction, according as it is positive or negative. The known differential equations of motion, of the second order, are now, by (1.), comprised in the following formula:

$$m_1 (x''_1 \delta x_1 + y''_1 \delta y_1 + z''_1 \delta z_1) + m_2 (x''_2 \delta x_2 + y''_2 \delta y_2 + z''_2 \delta z_2) = m_1 m_2 \delta f(r); \quad . \quad (73.)$$

they are therefore, separately,

$$\left. \begin{aligned} x''_1 &= m_2 \frac{\delta f(r)}{\delta x_1}, \quad y''_1 = m_2 \frac{\delta f(r)}{\delta y_1}, \quad z''_1 = m_2 \frac{\delta f(r)}{\delta z_1}, \\ x''_2 &= m_1 \frac{\delta f(r)}{\delta x_2}, \quad y''_2 = m_1 \frac{\delta f(r)}{\delta y_2}, \quad z''_2 = m_1 \frac{\delta f(r)}{\delta z_2}. \end{aligned} \right\} \quad \dots \quad (74.)$$

The problem of integrating these equations consists in proposing to assign, by their means, six relations between the time t, the masses $m_1\, m_2$, the six varying coordinates $x_1\, y_1\, z_1\, x_2\, y_2\, z_2$, and their initial values and initial rates of increase $a_1\, b_1\, c_1\, a_2\, b_2\, c_2\, a'_1\, b'_1\, c'_1\, a'_2\, b'_2\, c'_2$. If we knew these six final integrals, and combined them with the initial form of the law of living force, or of the known intermediate integral

$$\tfrac{1}{2} m_1 (x'^2_1 + y'^2_1 + z'^2_1) + \tfrac{1}{2} m_2 (x'^2_2 + y'^2_2 + z'^2_2) = m_1 m_2 f(r) + H; \quad . \quad . \quad (75.)$$

that is, with the following formula,

$$\tfrac{1}{2} m_1 (a'^2_1 + b'^2_1 + c'^2_1) + \tfrac{1}{2} m_2 (a'^2_2 + b'^2_2 + c'^2_2) = m_1 m_2 f(r_0) + \mathrm{H}, \quad . \quad . \quad (76.)$$

in which r_0 is the initial distance

$$r_0 = \sqrt{(a_1 - a_2)^2 + (b_1 - b_2)^2 + (c_1 - c_2)^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (77.)$$

and H is a constant quantity, introduced by integration; we could, by the combination of these seven relations, determine the time t, and the six initial components of velocity $a'_1\, b'_1\, c'_1\, a'_2\, b'_2\, c'_2$, as functions of the twelve final and initial coordinates $x_1\, y_1\, z_1\, x_2\, y_2\, z_2\, a_1\, b_1\, c_1\, a_2\, b_2\, c_2$, and of the quantity H, (involving also the masses:) we could therefore determine whatever else depends on the manner and time of motion of this system of two points, as a function of the same extreme coordinates and of the same quantity H. In particular, we could determine the action, or accumulated living force of the system, namely,

$$\mathrm{V} = m_1 \int_0^t (x'^2_1 + y'^2_1 + z'^2_1)\, d\,t + m_2 \int_0^t (x'^2_2 + y'^2_2 + z'^2_2)\, d\,t, \quad . \quad . \quad (\mathrm{A}^2.)$$

as a function of those thirteen quantities $x_1\, y_1\, z_1\, x_2\, y_2\, z_2\, a_1\, b_1\, c_1\, a_2\, b_2\, c_2$ H: and might then calculate the variation of this function,

$$\left.\begin{aligned} \delta \mathrm{V} = \frac{\delta \mathrm{V}}{\delta x_1} \delta x_1 + \frac{\delta \mathrm{V}}{\delta y_1} \delta y_1 + \frac{\delta \mathrm{V}}{\delta z_1} \delta z_1 + \frac{\delta \mathrm{V}}{\delta x_2} \delta x_2 + \frac{\delta \mathrm{V}}{\delta y_2} \delta y_2 + \frac{\delta \mathrm{V}}{\delta z_2} \delta z_2 \\ + \frac{\delta \mathrm{V}}{\delta a_1} \delta a_1 + \frac{\delta \mathrm{V}}{\delta b_1} \delta b_1 + \frac{\delta \mathrm{V}}{\delta c_1} \delta c_1 + \frac{\delta \mathrm{V}}{\delta a_2} \delta a_2 + \frac{\delta \mathrm{V}}{\delta b_2} \delta b_2 + \frac{\delta \mathrm{V}}{\delta c_2} \delta c_2 \\ + \frac{\delta \mathrm{V}}{\delta \mathrm{H}} \delta \mathrm{H}. \end{aligned}\right\} \quad . \quad (\mathrm{B}^2.)$$

But the essence of our method consists in *forming previously the expression of this variation, by our law of varying action*, namely,

$$\left.\begin{aligned} \delta \mathrm{V} = m_1 (x'_1 \delta x_1 - a'_1 \delta a_1 + y'_1 \delta y_1 - b'_1 \delta b_1 + z'_1 \delta z_1 - c'_1 \delta c_1) \\ + m_2 (x'_2 \delta x_2 - a'_2 \delta a_2 + y'_2 \delta y_2 - b'_2 \delta b_2 + z'_2 \delta z_2 - c'_2 \delta c_2) \\ + t\, \delta \mathrm{H}; \end{aligned}\right\} \quad . \quad (\mathrm{C}^2.)$$

and in *considering* V *as a characteristic function of the motion*, from the form of which may be deduced all the intermediate and all the final integrals of the known differential equations, by resolving the expression (C².) into the following separate groups, (included in (C.) and (D.),)

$$\left.\begin{aligned} \frac{\delta \mathrm{V}}{\delta x_1} = m_1 x'_1, \ \frac{\delta \mathrm{V}}{\delta y_1} = m_1 y'_1, \ \frac{\delta \mathrm{V}}{\delta z_1} = m_1 z'_1, \\ \frac{\delta \mathrm{V}}{\delta x_2} = m_2 x'_2, \ \frac{\delta \mathrm{V}}{\delta y_2} = m_2 y'_2, \ \frac{\delta \mathrm{V}}{\delta z_2} = m_2 z'_2; \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{D}^2.)$$

and

$$\left.\begin{aligned} \frac{\delta \mathrm{V}}{\delta a_1} = - m_1 a'_1, \ \frac{\delta \mathrm{V}}{\delta b_1} = - m_1 b'_1, \ \frac{\delta \mathrm{V}}{\delta c_1} = - m_1 c'_1, \\ \frac{\delta \mathrm{V}}{\delta a_2} = - m_2 a'_2, \ \frac{\delta \mathrm{V}}{\delta b_2} = - m_2 b'_2, \ \frac{\delta \mathrm{V}}{\delta c_2} = - m_2 c'_2; \end{aligned}\right\} \quad . \quad . \quad . \quad (\mathrm{E}^2.)$$

besides this other equation, which had occurred before,

$$\frac{\delta V}{\delta H} = t. \quad \text{(E.)}$$

By this new method, the difficulty of integrating the six known equations of motion of the second order (74.), is reduced to the search and differentiation of a single function V; and to find the form of this function, we are to employ the following pair of partial differential equations of the first order:

$$\frac{1}{2m_1}\left\{\left(\frac{\delta V}{\delta x_1}\right)^2 + \left(\frac{\delta V}{\delta y_1}\right)^2 + \left(\frac{\delta V}{\delta z_1}\right)^2\right\} + \frac{1}{2m_2}\left\{\left(\frac{\delta V}{\delta x_2}\right)^2 + \left(\frac{\delta V}{\delta y_2}\right)^2 + \left(\frac{\delta V}{\delta z_2}\right)^2\right\}$$
$$= m_1 m_2 f(r) + H, \quad \text{(F}^2\text{.)}$$

$$\frac{1}{2m_1}\left\{\left(\frac{\delta V}{\delta a_1}\right)^2 + \left(\frac{\delta V}{\delta b_1}\right)^2 + \left(\frac{\delta V}{\delta c_1}\right)^2\right\} + \frac{1}{2m_2}\left\{\left(\frac{\delta V}{\delta a_2}\right)^2 + \left(\frac{\delta V}{\delta b_2}\right)^2 + \left(\frac{\delta V}{\delta c_2}\right)^2\right\}$$
$$= m_1 m_2 f(r_0) + H, \quad \text{(G}^2\text{.)}$$

combined with some simple considerations. And it easily results from the principles already laid down, that the integral of this pair of equations, adapted to the present question, is

$$V = \sqrt{(x_{\prime\prime} - a_{\prime\prime})^2 + (y_{\prime\prime} - b_{\prime\prime})^2 + (z_{\prime\prime} - c_{\prime\prime})^2} \cdot \sqrt{2 H_{\prime\prime}(m_1 + m_2)}$$
$$+ \frac{m_1 m_2}{m_1 + m_2}\left(h \vartheta + \int_{r_0}^{r} \varrho \, dr\right); \quad \text{(H}^2\text{.)}$$

in which $x_{\prime\prime}\ y_{\prime\prime}\ z_{\prime\prime},\ a_{\prime\prime}\ b_{\prime\prime}\ c_{\prime\prime}$, denote the coordinates, final and initial, of the centre of gravity of the system,

$$\left.\begin{aligned} x_{\prime\prime} &= \frac{m_1 x_1 + m_2 x_2}{m_1 + m_2}, & y_{\prime\prime} &= \frac{m_1 y_1 + m_2 y_2}{m_1 + m_2}, & z_{\prime\prime} &= \frac{m_1 z_1 + m_2 z_2}{m_1 + m_2}, \\ a_{\prime\prime} &= \frac{m_1 a_1 + m_2 a_2}{m_1 + m_2}, & b_{\prime\prime} &= \frac{m_1 b_1 + m_2 b_2}{m_1 + m_2}, & c_{\prime\prime} &= \frac{m_1 c_1 + m_2 c_2}{m_1 + m_2}, \end{aligned}\right\} \quad \text{(78.)}$$

and ϑ is the angle between the final and initial distances r, r_0: we have also put for abridgement

$$\varrho = \pm\sqrt{2(m_1 + m_2)\left(f(r) + \frac{H_{\prime}}{m_1 m_2}\right) - \frac{h^2}{r^2}}, \quad \text{(79.)}$$

the upper or the lower sign to be used, according as the distance r is increasing or decreasing; and have introduced three auxiliary quantities h, $H_{\prime}$, $H_{\prime\prime}$, to be determined by this condition,

$$0 = \vartheta + \int_{r_0}^{r} \frac{\delta \varrho}{\delta h} dr, \quad \text{(I}^2\text{.)}$$

combined with the two following,

$$\left.\begin{aligned} \frac{m_1 m_2}{m_1 + m_2}\int_{r_0}^{r} \frac{\delta \varrho}{\delta H_{\prime}} dr &= \sqrt{(x_{\prime\prime} - a_{\prime\prime})^2 + (y_{\prime\prime} - b_{\prime\prime})^2 + (z_{\prime\prime} - c_{\prime\prime})^2} \cdot \sqrt{\frac{m_1 + m_2}{2 H_{\prime\prime}}}, \\ H_{\prime} + H_{\prime\prime} &= H; \end{aligned}\right\} \quad \text{(K}^2\text{.)}$$

which auxiliary quantities, although in one view they are functions of the twelve extreme coordinates, are yet to be treated as constant in calculating the three definite integrals, or limits of sums of numerous small elements,

$$\int_{r_0}^{r} \varrho \, d r, \int_{r_0}^{r} \frac{\delta \varrho}{\delta h} d r, \int_{r_0}^{r} \frac{\delta \varrho}{\delta \mathrm{H}_{,}} d r.$$

The form (H^2.), for the *characteristic function of a binary system,* may be regarded as a central or radical relation, which includes the whole theory of the motion of such a system; so that all the details of this motion may be deduced from it by the application of our general method. But because the theory of binary systems has been brought to great perfection already, by the labours of former writers, it may suffice to give briefly here a few instances of such deduction.

14. The form (H^2.), for the characteristic function of a binary system involves explicitly, when ϱ is changed to its value (79.), the twelve quantities $x_{,,} \, y_{,,} \, z_{,,} \, a_{,,} \, b_{,,} \, c_{,,}$ $r \, r_0 \, \vartheta \, h \, \mathrm{H}_{,} \, \mathrm{H}_{,,}$, (besides the masses $m_1 \, m_2$ which are always considered as given ;) its variation may therefore be thus expressed:

$$\left.\begin{aligned} \delta \mathrm{V} = \frac{\delta \mathrm{V}}{\delta x_{,,}} \delta x_{,,} + \frac{\delta \mathrm{V}}{\delta y_{,,}} \delta y_{,,} + \frac{\delta \mathrm{V}}{\delta z_{,,}} \delta z_{,,} + \frac{\delta \mathrm{V}}{\delta a_{,,}} \delta a_{,,} + \frac{\delta \mathrm{V}}{\delta b_{,,}} \delta b_{,,} + \frac{\delta \mathrm{V}}{\delta c_{,,}} \delta c_{,,} \\ + \frac{\delta \mathrm{V}}{\delta r} \delta r + \frac{\delta \mathrm{V}}{\delta r_0} \delta r_0 + \frac{\delta \mathrm{V}}{\delta \vartheta} \delta \vartheta + \frac{\delta \mathrm{V}}{\delta h} \delta h + \frac{\delta \mathrm{V}}{\delta \mathrm{H}_{,}} \delta \mathrm{H}_{,} + \frac{\delta \mathrm{V}}{\delta \mathrm{H}_{,,}} \delta \mathrm{H}_{,,}. \end{aligned}\right\} \quad . \quad (\mathrm{L}^2.)$$

In this expression, if we put for abridgement

$$\lambda = \sqrt{\frac{2 \mathrm{H}_{,,} (m_1 + m_2)}{(x_{,,} - a_{,,})^2 + (y_{,,} - b_{,,})^2 + (z_{,,} - c_{,,})^2}}, \quad . \quad . \quad . \quad . \quad (80.)$$

we shall have

$$\left.\begin{aligned} \frac{\delta \mathrm{V}}{\delta x_{,,}} = \lambda (x_{,,} - a_{,,}), \ \frac{\delta \mathrm{V}}{\delta y_{,,}} = \lambda (y_{,,} - b_{,,}), \ \frac{\delta \mathrm{V}}{\delta z_{,,}} = \lambda (z_{,,} - c_{,,}), \\ \frac{\delta \mathrm{V}}{\delta a_{,,}} = \lambda (a_{,,} - x_{,,}), \ \frac{\delta \mathrm{V}}{\delta b_{,,}} = \lambda (b_{,,} - y_{,,}), \ \frac{\delta \mathrm{V}}{\delta c_{,,}} = \lambda (c_{,,} - z_{,,}); \end{aligned}\right\} \quad . \quad . \quad . \quad (\mathrm{M}^2).$$

and if we put

$$\varrho_0 = \pm \sqrt{2 (m_1 + m_2) \left(f(r_0) + \frac{\mathrm{H}_{,}}{m_1 m_2} \right) - \frac{h^2}{r_0^2}}, \quad . \quad . \quad . \quad . \quad (81.)$$

the sign of the radical being determined by the same rule as that of ϱ, we shall have

$$\frac{\delta \mathrm{V}}{\delta r} = \frac{m_1 m_2 \varrho}{m_1 + m_2}, \ \frac{\delta \mathrm{V}}{\delta r_0} = \frac{- m_1 m_2 \varrho_0}{m_1 + m_2}, \ \frac{\delta \mathrm{V}}{\delta \vartheta} = \frac{m_1 m_2 h}{m_1 + m_2}; \quad . \quad . \quad . \quad . \quad (\mathrm{N}^2.)$$

besides, by the equations of condition (I^2.), (K^2.), we have

$$\frac{\delta \mathrm{V}}{\delta h} = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{O}^2.)$$

and

$$\frac{\delta \mathrm{V}}{\delta \mathrm{H}_{,,}} = \frac{\delta \mathrm{V}}{\delta \mathrm{H}_{,}} = \int_{r_0}^{r} \frac{d r}{\varrho}, \qquad \delta \mathrm{H}_{,} + \delta \mathrm{H}_{,,} = \delta \mathrm{H}. \quad . \quad . \quad . \quad . \quad (\mathrm{P}^2.)$$

The expression (L^2.) may therefore be thus transformed:

$$\delta V = \lambda \{(x_{\prime\prime} - a_{\prime\prime})(\delta x_{\prime\prime} - \delta a_{\prime\prime}) + (y_{\prime\prime} - b_{\prime\prime})(\delta y_{\prime\prime} - \delta b_{\prime\prime}) + (z_{\prime\prime} - c_{\prime\prime})(\delta z_{\prime\prime} - \delta c_{\prime\prime})\}$$

$$+ \frac{m_1 m_2}{m_1 + m_2}(\varrho \delta r - \varrho_0 \delta r_0 + h \delta \vartheta) + \int_{r_0}^{r} \frac{d r}{\varrho} . \delta H; \quad . \; . \; . \; . \; . \; . \; . \; . \quad (Q^2.)$$

and may be resolved by our general method into twelve separate expressions for the final and initial components of velocities, namely,

$$\left.\begin{aligned}
x'_1 &= \frac{1}{m_1}\frac{\delta V}{\delta x_1} = \frac{\lambda}{m_1 + m_2}(x_{\prime\prime} - a_{\prime\prime}) + \frac{m_2}{m_1 + m_2}\left(\varrho \frac{\delta r}{\delta x_1} + h \frac{\delta \vartheta}{\delta x_1}\right),\\
y'_1 &= \frac{1}{m_1}\frac{\delta V}{\delta y_1} = \frac{\lambda}{m_1 + m_2}(y_{\prime\prime} - b_{\prime\prime}) + \frac{m_2}{m_1 + m_2}\left(\varrho \frac{\delta r}{\delta y_1} + h \frac{\delta \vartheta}{\delta y_1}\right),\\
z'_1 &= \frac{1}{m_1}\frac{\delta V}{\delta z_1} = \frac{\lambda}{m_1 + m_2}(z_{\prime\prime} - c_{\prime\prime}) + \frac{m_2}{m_1 + m_2}\left(\varrho \frac{\delta r}{\delta z_1} + h \frac{\delta \vartheta}{\delta z_1}\right),\\
x'_2 &= \frac{1}{m_2}\frac{\delta V}{\delta x_2} = \frac{\lambda}{m_1 + m_2}(x_{\prime\prime} - a_{\prime\prime}) + \frac{m_1}{m_1 + m_2}\left(\varrho \frac{\delta r}{\delta x_2} + h \frac{\delta \vartheta}{\delta x_2}\right),\\
y'_2 &= \frac{1}{m_2}\frac{\delta V}{\delta y_2} = \frac{\lambda}{m_1 + m_2}(y_{\prime\prime} - b_{\prime\prime}) + \frac{m_1}{m_1 + m_2}\left(\varrho \frac{\delta r}{\delta y_2} + h \frac{\delta \vartheta}{\delta y_2}\right),\\
z'_2 &= \frac{1}{m_2}\frac{\delta V}{\delta z_2} = \frac{\lambda}{m_1 + m_2}(z_{\prime\prime} - c_{\prime\prime}) + \frac{m_1}{m_1 + m_2}\left(\varrho \frac{\delta r}{\delta z_2} + h \frac{\delta \vartheta}{\delta z_2}\right),
\end{aligned}\right\} \quad . \; . \quad (R^2.)$$

and

$$\left.\begin{aligned}
a'_1 &= \frac{-1}{m_1}\frac{\delta V}{\delta a_1} = \frac{\lambda}{m_1 + m_2}(x_{\prime\prime} - a_{\prime\prime}) + \frac{m_2}{m_1 + m_2}\left(\varrho_0 \frac{\delta r_0}{\delta a_1} - h \frac{\delta \vartheta}{\delta a_1}\right),\\
b'_1 &= \frac{-1}{m_1}\frac{\delta V}{\delta b_1} = \frac{\lambda}{m_1 + m_2}(y_{\prime\prime} - b_{\prime\prime}) + \frac{m_2}{m_1 + m_2}\left(\varrho_0 \frac{\delta r_0}{\delta b_1} - h \frac{\delta \vartheta}{\delta b_1}\right),\\
c'_1 &= \frac{-1}{m_1}\frac{\delta V}{\delta c_1} = \frac{\lambda}{m_1 + m_2}(z_{\prime\prime} - c_{\prime\prime}) + \frac{m_2}{m_1 + m_2}\left(\varrho_0 \frac{\delta r_0}{\delta c_1} - h \frac{\delta \vartheta}{\delta c_1}\right),\\
a'_2 &= \frac{-1}{m_2}\frac{\delta V}{\delta a_2} = \frac{\lambda}{m_1 + m_2}(x_{\prime\prime} - a_{\prime\prime}) + \frac{m_1}{m_1 + m_2}\left(\varrho_0 \frac{\delta r_0}{\delta a_2} - h \frac{\delta \vartheta}{\delta a_2}\right),\\
b'_2 &= \frac{-1}{m_2}\frac{\delta V}{\delta b_2} = \frac{\lambda}{m_1 + m_2}(y_{\prime\prime} - b_{\prime\prime}) + \frac{m_1}{m_1 + m_2}\left(\varrho_0 \frac{\delta r_0}{\delta b_2} - h \frac{\delta \vartheta}{\delta b_2}\right),\\
c'_2 &= \frac{-1}{m_2}\frac{\delta V}{\delta c_2} = \frac{\lambda}{m_1 + m_2}(z_{\prime\prime} - c_{\prime\prime}) + \frac{m_1}{m_1 + m_2}\left(\varrho_0 \frac{\delta r_0}{\delta c_2} - h \frac{\delta \vartheta}{\delta c_2}\right);
\end{aligned}\right\} \quad . \quad (S^2.)$$

besides the following expression for the time of motion of the system:

$$t = \frac{\delta V}{\delta H} = \int_{r_0}^{r} \frac{d r}{\varrho}, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (T^2.)$$

which gives by (K^2.), and by (79.), (80.),

$$t = \frac{m_1 + m_2}{\lambda}. \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (U^2.)$$

The six equations (R^2.) give the six intermediate integrals, and the six equations (S^2.) give the six final integrals of the six known differential equations of motion (74.) for any binary system, if we eliminate or determine the three auxiliary quantities

h, $H_{,}$, $H_{,,}$, by the three conditions (I^2.) (T^2.) (U^2.). Thus, if we observe that the distances r, r_0, and the included angle ϑ, depend only on relative coordinates, which may be thus denoted,

$$\left.\begin{array}{l} x_1 - x_2 = \xi, y_1 - y_2 = \eta, z_1 - z_2 = \zeta, \\ a_1 - a_2 = \alpha, b_1 - b_2 = \beta, c_1 - c_2 = \gamma, \end{array}\right\} \quad \text{(82.)}$$

we obtain by easy combinations the three following intermediate integrals for the centre of gravity of the system:

$$x'_{,,} t = x_{,,} - a_{,,}, y'_{,,} t = y_{,,} - b_{,,}, z'_{,,} t = z_{,,} - c_{,,}, \quad \text{(83.)}$$

and the three following final integrals,

$$a'_{,,} t = x_{,,} - a_{,,}, b'_{,,} t = y_{,,} - b_{,,}, c'_{,,} t = z_{,,} - c_{,,}, \quad \text{(84.)}$$

expressing the well-known law of the rectilinear and uniform motion of that centre. We obtain also the three following intermediate integrals for the relative motion of one point of the system about the other:

$$\left.\begin{array}{l} \xi' = \varrho \frac{\delta r}{\delta \xi} + h \frac{\delta \vartheta}{\delta \xi}, \\ \eta' = \varrho \frac{\delta r}{\delta \eta} + h \frac{\delta \vartheta}{\delta \eta}, \\ \zeta' = \varrho \frac{\delta r}{\delta \zeta} + h \frac{\delta \vartheta}{\delta \zeta}, \end{array}\right\} \quad \text{(85.)}$$

and the three following final integrals,

$$\left.\begin{array}{l} \alpha' = \varrho_0 \frac{\delta r_0}{\delta \alpha} - h \frac{\delta \vartheta}{\delta \alpha}, \\ \beta' = \varrho_0 \frac{\delta r_0}{\delta \beta} - h \frac{\delta \vartheta}{\delta \beta}, \\ \gamma' = \varrho_0 \frac{\delta r_0}{\delta \gamma} - h \frac{\delta \vartheta}{\delta \gamma}; \end{array}\right\} \quad \text{(86.)}$$

in which the auxiliary quantities h, $H_{,}$, are to be determined by (I^2.) (T^2.), and in which the dependence of r, r_0, ϑ, on ξ, η, ζ, α, β, γ, is expressed by the following equations:

$$\left.\begin{array}{l} r = \sqrt{\xi^2 + \eta^2 + \zeta^2}, \quad r_0 = \sqrt{\alpha^2 + \beta^2 + \gamma^2}, \\ r r_0 \cos \vartheta = \xi \alpha + \eta \beta + \zeta \gamma. \end{array}\right\} \quad \text{(87.)}$$

If then we put, for abridgement,

$$A = \frac{\varrho}{r} + \frac{h}{r^2 \tan \vartheta}, \ B = \frac{h}{r r_0 \sin \vartheta}, \ C = \frac{-\varrho_0}{r_0} + \frac{h}{r_0^2 \tan \vartheta}, \quad \text{(88.)}$$

we shall have these three intermediate integrals,

$$\xi' = A \xi - B \alpha, \quad \eta' = A \eta - B \beta, \quad \zeta' = A \zeta - B \gamma, \quad \text{(89.)}$$

and these three final integrals,

$$\alpha' = B \xi - C \alpha, \quad \beta' = B \eta - C \beta, \quad \gamma' = B \zeta - C \gamma, \quad \text{(90.)}$$

of the equations of relative motion. These integrals give,

$$\left.\begin{aligned} \xi\eta' - \eta\xi' &= \alpha\beta' - \beta\alpha' = \mathrm{B}\,(\alpha\eta - \beta\xi), \\ \eta\zeta' - \zeta\eta' &= \beta\gamma' - \gamma\beta' = \mathrm{B}\,(\beta\zeta - \gamma\eta), \\ \zeta\xi' - \xi\zeta' &= \gamma\alpha' - \alpha\gamma' = \mathrm{B}\,(\gamma\xi - \alpha\zeta), \end{aligned}\right\} \quad \ldots \ldots \quad (91.)$$

and

$$\zeta(\alpha\beta' - \beta\alpha') + \xi(\beta\gamma' - \gamma\beta') + \eta(\gamma\alpha' - \alpha\gamma') = 0; \quad \ldots \ldots \quad (92.)$$

they contain therefore the known law of equable description of areas, and the law of a plane relative orbit. If we take for simplicity this plane for the plane $\xi\eta$, the quantities $\zeta\,\zeta'\,\gamma\,\gamma'$ will vanish; and we may put,

$$\left.\begin{aligned} &\xi = r\cos\theta,\ \eta = r\sin\theta,\ \zeta = 0, \\ &\alpha = r_0\cos\theta_0,\ \beta = r_0\sin\theta_0,\ \gamma = 0, \end{aligned}\right\} \quad \ldots \ldots \quad (93.)$$

and

$$\left.\begin{aligned} &\xi' = r'\cos\theta - \theta' r\sin\theta,\ \eta' = r'\sin\theta + \theta' r\cos\theta,\ \zeta' = 0, \\ &\alpha' = r'_0\cos\theta_0 - \theta'_0 r_0\sin\theta_0,\ \beta' = r'_0\sin\theta_0 + \theta'_0 r_0\cos\theta_0,\ \gamma' = 0, \end{aligned}\right\} \quad \ldots \quad (94.)$$

the angles $\theta\,\theta_0$ being counted from some fixed line in the plane, and being such that their difference

$$\theta - \theta_0 = \vartheta. \quad \ldots \ldots \quad (95.)$$

These values give

$$\xi\eta' - \eta\xi' = r^2\theta',\ \alpha\beta' - \beta\alpha' = r_0^2\theta'_0,\ \alpha\eta - \beta\xi = r r_0 \sin\vartheta, \quad \ldots \quad (96.)$$

and therefore, by (88.) and (91),

$$r^2\theta' = r_0^2\theta'_0 = h; \quad \ldots \ldots \quad (97.)$$

the quantity $\frac{1}{2}h$ is therefore the constant areal velocity in the relative motion of the system; a result which is easily seen to be independent of the directions of the three rectangular coordinates. The same values, (93.), (94.), give

$$\left.\begin{aligned} &\xi\cos\theta + \eta\sin\theta = r,\ \xi'\cos\theta + \eta'\sin\theta = r',\ \alpha\cos\theta + \beta\sin\theta = r_0\cos\vartheta, \\ &\alpha\cos\theta_0 + \beta\sin\theta_0 = r_0,\ \alpha'\cos\theta_0 + \beta'\sin\theta_0 = r'_0,\ \xi\cos\theta_0 + \eta\sin\theta_0 = r\cos\vartheta, \end{aligned}\right\} (98.)$$

and therefore, by the intermediate and final integrals, (89.), (90.),

$$r' = \varrho,\ r'_0 = \varrho_0; \quad \ldots \ldots \quad (99.)$$

results which evidently agree with the condition (T^2.), and which give by (79.) and (81.), for all directions of coordinates,

$$\left.\begin{aligned} &r'^2 + \frac{h^2}{r^2} - 2\,(m_1 + m_2) f(r) = \\ &r'^2_0 + \frac{h^2}{r^2_0} - 2\,(m_1 + m_2) f(r_0) = 2\,\mathrm{H}_{\prime}\left(\frac{1}{m_1} + \frac{1}{m_2}\right); \end{aligned}\right\} \quad \ldots \ldots \quad (100.)$$

the other auxiliary quantity $\mathrm{H}_{\prime}$ is therefore also a constant, independent of the time, and enters as such into the constant part in the expression for $\left(r'^2 + \frac{h^2}{r^2}\right)$ the square of the relative velocity. The equation of condition (I^2), connecting these two constants h, $H_{\prime}$, with the extreme lengths of the radius vector r, and with the angle ϑ described by this radius in revolving from its initial to its final direction, is the equation of the plane relative orbit; and the other equation of condition (T^2.), connecting the same two constants with the same extreme distances and with the time, gives the law of the velocity of mutual approach or recess.

We may remark that the part $V_{\prime}$ of the whole characteristic function V, which represents the relative action and determines the relative motion in the system, namely,

$$V_{\prime} = \frac{m_1 m_2}{m_1 + m_2}\left(h\vartheta + \int_{r_0}^{r} \varrho\, d\, r\right), \quad \ldots \quad (V^2.)$$

may be put, by (I^2.), under the form

$$V_{\prime} = \frac{m_1 m_2}{m_1 + m_2}\int_{r_0}^{r}\left(\varrho - h\frac{\delta\varrho}{\delta h}\right) d\, r, \quad \ldots \quad (W^2.)$$

or finally, by (79.),

$$V_{\prime} = 2\int_{r_0}^{r}\frac{m_1 m_2 f(r) + H_{\prime}}{\varrho}\, d\, r; \quad \ldots \quad (X^2.)$$

the condition (I^2.) may also itself be transformed, by (79.), as follows:

$$\vartheta = h\int_{r_0}^{r}\frac{d\, r}{r^2\varrho}: \quad \ldots \quad (Y^2.)$$

results which all admit of easy verifications. The partial differential equations connected with the law of relative living force, which the characteristic function $V_{\prime}$ of relative motion must satisfy, may be put under the following forms:

$$\left.\begin{aligned} \left(\frac{\delta V_{\prime}}{\delta r}\right)^2 + \frac{1}{r^2}\left(\frac{\delta V_{\prime}}{\delta \vartheta}\right)^2 &= \frac{2\, m_1 m_2}{m_1 + m_2}(U + H_{\prime}), \\ \left(\frac{\delta V_{\prime}}{\delta r_0}\right)^2 + \frac{1}{r_0^2}\left(\frac{\delta V_{\prime}}{\delta \vartheta}\right)^2 &= \frac{2\, m_1 m_2}{m_1 + m_2}(U_0 + H_{\prime}); \end{aligned}\right\} \quad \ldots \quad (Z^2.)$$

and if the first of the equations of this pair have its variation taken with respect to r and ϑ, attention being paid to the dynamical meanings of the coefficients of the characteristic function, it will conduct (as in former instances) to the known differential equations of motion of the second order.

On the undisturbed Motion of a Planet or Comet about the Sun: Dependence of the Characteristic Function of such Motion, on the chord and the sum of the Radii.

15. To particularize still further, let

$$f(r) = \frac{1}{r}, \quad \ldots \quad (101.)$$

that is, let us consider a binary system, such as a planet or comet and the sun, with the Newtonian law of attraction; and let us put, for abridgement,

$$m_1 + m_2 = \mu, \quad \frac{h^2}{\mu} = p, \quad \frac{-m_1 m_2}{2H_{\prime}} = \mathrm{a}. \quad \ldots \quad (102.)$$

The characteristic function $V_{\prime}$ of relative motion may now be expressed as follows:

$$V_{\prime} = \frac{m_1 m_2}{\sqrt{\mu}} \left(\vartheta \sqrt{p} + \int_{r_0}^{r} \pm \sqrt{\frac{2}{r} - \frac{1}{a} - \frac{p}{r^2}} \,.\, d r \right); \quad \ldots \ldots \quad (A^3.)$$

in which p is to be considered as a function of the extreme radii vectores r, r_0, and of their included angle ϑ, involving also the quantity a, or the connected quantity $H_{\prime}$, and determined by the condition

$$\vartheta = \int_{r_0}^{r} \frac{\pm d r}{r^2 \sqrt{\frac{2}{r p} - \frac{1}{a p} - \frac{1}{r^2}}}, \quad \ldots \ldots \quad (B^3.)$$

that is, by the derivative of the formula (A³.), taken with respect to p: the upper sign being taken in each expression when the distance r is increasing, and the lower sign when that distance is diminishing, and the quantity p being treated as constant in calculating the two definite integrals. It results from the foregoing remarks, that this quantity p is constant also in the sense of being independent of the time, so as not to vary in the course of the motion; and that the condition (B³.), connecting this constant with $r\, r_0\, \vartheta$ a, is the equation of the plane relative orbit; which is therefore (as it has long been known to be) an ellipse, hyperbola, or parabola, according as the constant a is positive, negative, or zero, the origin of r being always a focus of the curve, and p being the semiparameter. It results also, that the time of motion may be thus expressed:

$$t = \frac{\delta V_{\prime}}{\delta H_{\prime}} = \frac{2 a^2}{m_1 m_2} \frac{\delta V_{\prime}}{\delta a}, \quad \ldots \ldots \quad (C^3.)$$

and therefore thus:

$$t = \int_{r_0}^{r} \frac{\pm d r}{\sqrt{\frac{2 \mu}{r} - \frac{\mu}{a} - \frac{\mu p}{r^2}}}; \quad \ldots \ldots \quad (D^3.)$$

which latter is a known expression. Confining ourselves at present to the case a > 0, and introducing the known auxiliary quantities called excentricity and excentric anomaly, namely,

$$e = \sqrt{1 - \frac{p}{a}}, \quad \ldots \ldots \quad (103.)$$

and

$$v = \cos^{-1}\left(\frac{a - r}{a e}\right), \quad \ldots \ldots \quad (104.)$$

which give

$$\pm \sqrt{2 a r - r^2 - p a} = a e \sin v, \quad \ldots \ldots \quad (105.)$$

v being considered as continually increasing with the time; and therefore, as is well known,

$$\left. \begin{aligned} & r = a(1 - e \cos v),\ r_0 = a(1 - e \cos v_0), \\ & \vartheta = 2 \tan^{-1}\left\{\sqrt{\frac{1+e}{1-e}} \tan \frac{v}{2}\right\} - 2 \tan^{-1}\left\{\sqrt{\frac{1+e}{1-e}} \tan \frac{v_0}{2}\right\}, \end{aligned} \right\} \quad . \quad (106.)$$

and

$$t = \sqrt{\frac{a^3}{\mu}} \cdot (v - v_0 - e \sin v + e \sin v_0) \, ; \quad \ldots \ldots \ldots \quad (107.)$$

we find that this expression for the characteristic function of relative motion,

$$V_, = \frac{m_1 m_2}{\sqrt{\mu}} \int_{r_0}^{r} \frac{\pm \left(\frac{2}{r} - \frac{1}{a}\right) d r}{\sqrt{\frac{2}{r} - \frac{1}{a} - \frac{p}{r^2}}}, \quad \ldots \ldots \ldots \quad (E^3.)$$

deduced from (A^3.) and (B^3.), may be transformed as follows:

$$V_, = m_1 m_2 \sqrt{\frac{a}{\mu}} \, (v - v_0 + e \sin v - e \sin v_0) : \quad \ldots \quad (F^3.)$$

in which the excentricity e, and the final and initial excentric anomalies v, v_0, are to be considered as functions of the final and initial radii r, r_0, and of the included angle ϑ, determined by the equations (106.). The expression (F^3.) may be thus written:

$$V_, = 2 \, m_1 m_2 \sqrt{\frac{a}{\mu}} \, (v_, + e_, \sin v_,), \quad \ldots \ldots \ldots \quad (G^3.)$$

if we put, for abridgement,

$$v_, = \frac{v - v_0}{2}, \quad e_, = e \cos \frac{v + v_0}{2} \, ; \quad \ldots \ldots \ldots \quad (108.)$$

for the complete determination of the characteristic function of the present relative motion, it remains therefore to determine the two variables $v_,$ and $e_,$, as functions of $r \, r_0 \, \vartheta$, or of some other set of quantities which mark the shape and size of the plane triangle bounded by the final and initial elliptic radii vectores and by the elliptic chord.

For this purpose it is convenient to introduce this elliptic chord itself, which we shall call $\pm \tau$, so that

$$\tau^2 = r^2 + r^2_0 - 2 \, r \, r_0 \cos \vartheta \, ; \quad \ldots \ldots \ldots \quad (109.)$$

because this chord may be expressed as a function of the two variables $v_,$, $e_,$, (involving also the mean distance a,) as follows. The value (106.) for the angle ϑ, that is, by (95.), for $\theta - \theta_0$, gives

$$\theta - 2 \tan^{-1} \left\{ \sqrt{\frac{1+e}{1-e}} \tan \frac{v}{2} \right\} = \theta_0 - 2 \tan^{-1} \left\{ \sqrt{\frac{1+e}{1-e}} \tan \frac{v_0}{2} \right\} = \varpi, \quad . \quad (110.)$$

ϖ being a new constant independent of the time, namely, one of the values of the polar angle θ, which correspond to the minimum of radius vector; and therefore, by (106.),

$$\left. \begin{array}{l} r \cos (\theta - \varpi) = a (\cos v - e), \;\; r \sin (\theta - \varpi) = a \sqrt{1 - e^2} \sin v, \\ r_0 \cos (\theta_0 - \varpi) = a (\cos v_0 - e), \;\; r_0 \sin (\theta_0 - \varpi) = a \sqrt{1 - e^2} \sin v_0 \, ; \end{array} \right\} \quad . \quad . \quad (111.)$$

expressions which give the following value for the square of the elliptic chord:

$$\left.\begin{aligned}\tau^2 &= \{r\cos(\theta-\varpi)-r_0\cos(\theta_0-\varpi)\}^2+\{r\sin(\theta-\varpi)-r_0\sin(\theta_0-\varpi)\}^2\\ &= \mathrm{a}^2\{(\cos v-\cos v_0)^2+(1-e^2)(\sin v-\sin v_0)^2\}\\ &= 4\,\mathrm{a}^2\sin v_{\prime}^2\left\{\left(\sin\frac{v+v_0}{2}\right)^2+(1-e^2)\left(\cos\frac{v+v_0}{2}\right)^2\right\}\\ &= 4\,\mathrm{a}^2(1-e_{\prime}^2)\sin v_{\prime}^2:\end{aligned}\right\}\quad(112.)$$

we may also consider τ as having the same sign with $\sin v_{\prime}$, if we consider it as alternately positive and negative, in the successive elliptic periods or revolutions, beginning with the initial position.

Besides, if we denote by σ the sum of the two elliptic radii vectores, final and initial, so that

$$\sigma = r + r_0, \quad \ldots \quad (113.)$$

we shall have, with our present abridgements,

$$\sigma = 2\,\mathrm{a}\,(1 - e_{\prime}\cos v_{\prime})\,; \quad \ldots \quad (114.)$$

the variables $v_{\prime}\ e_{\prime}$ are therefore functions of σ, τ, a, and consequently the characteristic function $V_{\prime}$ is itself a function of those three quantities. We may therefore put

$$V_{\prime} = \frac{m_1\,m_2\,w}{m_1+m_2}, \quad \ldots \quad (H^3.)$$

w being a function of σ, τ, a, of which the form is to be determined by eliminating $v_{\prime}\ e_{\prime}$ between the three equations,

$$\left.\begin{aligned} w &= 2\sqrt{\mu\,\mathrm{a}}\,(v_{\prime}+e_{\prime}\sin v_{\prime}),\\ \sigma &= 2\,\mathrm{a}\,(1-e_{\prime}\cos v_{\prime}),\\ \tau &= 2\,\mathrm{a}\,(1-e_{\prime}^2)^{\frac{1}{2}}\sin v_{\prime}\,;\end{aligned}\right\}\quad\ldots\quad(I^3.)$$

and we may consider this new function w as itself a characteristic function of elliptic motion; the law of its variation being expressed as follows, in the notation of the present essay:

$$\delta w = \xi'\delta\xi - \alpha'\delta\alpha + \eta'\delta\eta - \beta'\delta\beta + \zeta'\delta\zeta - \gamma'\delta\gamma + \frac{t\mu\,\delta\,\mathrm{a}}{2\,\mathrm{a}^2}. \quad \ldots \quad (K^3.)$$

In this expression, $\xi\ \eta\ \zeta$ are the relative coordinates of the point m_1, at the time t, referred to the other attracting point m_2 as an origin, and to any three rectangular axes; $\xi'\ \eta'\ \zeta'$ are their rates of increase, or the three rectangular components of final relative velocity; $\alpha\ \beta\ \gamma\ \alpha'\ \beta'\ \gamma'$ are the initial values, or values at the time zero, of these relative coordinates and components of relative velocity; a is a quantity independent of the time, namely, the mean distance of the two points m_1, m_2; and μ is the sum of their masses. And all the properties of the undisturbed elliptic motion of a planet or comet about the sun may be deduced in a new way, from the simplified characteristic function w, by comparing its variation (K3.) with the following other form,

$$\delta w = \frac{\delta w}{\delta\sigma}\delta\sigma + \frac{\delta w}{\delta\tau}\delta\tau + \frac{\delta w}{\delta\,\mathrm{a}}\delta\,\mathrm{a}\,; \quad \ldots \quad (L^3.)$$

in which we are to observe that

$$\left.\begin{array}{l}\sigma = \sqrt{\xi^2 + \eta^2 + \zeta^2} + \sqrt{\alpha^2 + \beta^2 + \gamma^2}, \\ \tau = \pm\sqrt{(\xi - \alpha)^2 + (\eta - \beta)^2 + (\zeta - \gamma)^2}.\end{array}\right\} \quad \ldots \ldots \quad (\text{M}^3.)$$

By this comparison we are brought back to the general integral equations of the relative motion of a binary system, (89.) and (90.); but we have now the following particular values for the coefficients A, B, C:

$$\text{A} = \frac{1}{r}\frac{\delta \varpi}{\delta \sigma} + \frac{1}{\tau}\frac{\delta \varpi}{\delta \tau}, \quad \text{B} = \frac{1}{\tau}\frac{\delta \varpi}{\delta \tau}, \quad \text{C} = \frac{1}{r_0}\frac{\delta \varpi}{\delta \sigma} + \frac{1}{\tau}\frac{\delta \varpi}{\delta \tau}; \quad \ldots \ldots \quad (\text{N}^3.)$$

and with respect to the three partial differential coefficients, $\frac{\delta \varpi}{\delta \sigma}, \frac{\delta \varpi}{\delta \tau}, \frac{\delta \varpi}{\delta \text{a}}$, we have the following relation between them:

$$\text{a}\frac{\delta \varpi}{\delta \text{a}} + \sigma\frac{\delta \varpi}{\delta \sigma} + \tau\frac{\delta \varpi}{\delta \tau} = \frac{\varpi}{2}, \quad \ldots \ldots \quad (\text{O}^3.)$$

the function ϖ being homogeneous of the dimension $\frac{1}{2}$ with respect to the three quantities a, σ, τ; we have also, by (I³.),

$$\frac{\delta \varpi}{\delta \sigma} = \sqrt{\frac{\mu}{\text{a}}} \cdot \frac{\sin v_{\prime}}{e_{\prime} - \cos v_{\prime}}, \quad \frac{\delta \varpi}{\delta \tau} = \sqrt{\frac{\mu}{\text{a}}} \cdot \frac{\sqrt{1 - e_{\prime}^2}}{\cos v_{\prime} - e_{\prime}}, \quad \ldots \ldots \quad (\text{P}^3.)$$

and therefore

$$\frac{\delta \varpi}{\delta \sigma}\frac{\delta \varpi}{\delta \tau} = \frac{-2\mu\tau}{\sigma^2 - \tau^2}, \quad \left(\frac{\delta \varpi}{\delta \sigma}\right)^2 + \left(\frac{\delta \varpi}{\delta \tau}\right)^2 + \frac{\mu}{\text{a}} = \frac{4\mu\sigma}{\sigma^2 - \tau^2}, \quad \ldots \ldots \quad (\text{Q}^3.)$$

from which may be deduced the following remarkable expressions:

$$\left.\begin{array}{l}\left(\frac{\delta \varpi}{\delta \sigma} + \frac{\delta \varpi}{\delta \tau}\right)^2 = \frac{4\mu}{\sigma + \tau} - \frac{\mu}{\text{a}}, \\ \left(\frac{\delta \varpi}{\delta \tau} - \frac{\delta \varpi}{\delta \sigma}\right)^2 = \frac{4\mu}{\sigma - \tau} - \frac{\mu}{\text{a}}.\end{array}\right\} \quad \ldots \ldots \quad (\text{R}^3.)$$

These expressions will be found to be important in the application of the present method to the theory of elliptic motion.

16. We shall not enter, on this occasion, into any details of such application; but we may remark, that the circumstance of the characteristic function involving only the elliptic chord and the sum of the extreme radii, (besides the mean distance and the sum of the masses,) affords, by our general method, a new proof of the well-known theorem that the elliptic time also depends on the same chord and sum of radii; and gives a new expression for the law of this dependence, namely,

$$t = \frac{2\,\text{a}^2}{\mu}\frac{\delta \varpi}{\delta \text{a}}. \quad \ldots \ldots \quad (\text{S}^3.)$$

We may remark also, that the same form of the characteristic function of elliptic motion, conducts, by our general method, to the following curious, but not novel property, of the ellipse, that if any two tangents be drawn to such a curve, from any common point outside, these tangents subtend equal angles at one focus;

they subtend also equal angles at the other. Reciprocally, if any plane curve possess this property, when referred to a fixed point in its own plane, which may be taken as the origin of polar coordinates r, θ, the curve must satisfy the following equation in mixed differences:

$$\cot\left(\frac{\Delta\theta}{2}\right) . \Delta \frac{1}{r} = (\Delta + 2) \frac{d}{d\theta} \frac{1}{r}, \quad \ldots \ldots \quad (115.)$$

which may be brought to the following form,

$$\left(\frac{d}{d\theta} + \frac{d^3}{d\theta^3}\right) \frac{1}{r} = 0, \quad \ldots \ldots \quad (116.)$$

and therefore gives, by integration,

$$r = \frac{p}{1 + e \cos(\theta - \varpi)}; \quad \ldots \ldots \quad (117.)$$

the curve is, consequently, a conic section, and the fixed point is one of its foci.

The properties of parabolic are included as limiting cases in those of elliptic motion, and may be deduced from them by making

$$H_{\prime} = 0, \text{ or } a = \infty; \quad \ldots \ldots \quad (118.)$$

and therefore the characteristic function w and the time t, in parabolic as well as in elliptic motion, are functions of the chord and of the sum of the radii. By thus making a infinite in the foregoing expressions, we find, for parabolic motion, the partial differential equations

$$\left(\frac{\delta w}{\delta \sigma} + \frac{\delta w}{\delta \tau}\right)^2 = \frac{4\mu}{\sigma + \tau}, \quad \left(\frac{\delta w}{\delta \sigma} - \frac{\delta w}{\delta \tau}\right)^2 = \frac{4\mu}{\sigma - \tau}; \quad \ldots \ldots \quad (T^3.)$$

and in fact the parabolic form of the simplified characteristic function w may easily be shown to be

$$w = 2\sqrt{\mu}\,(\sqrt{\sigma + \tau} \mp \sqrt{\sigma - \tau}), \quad \ldots \ldots \quad (U^3.)$$

τ being, as before, the chord, and σ the sum of the radii; while the analogous limit of the expression (S^3.), for the time, is

$$t = \frac{1}{6\sqrt{\mu}} \left\{ (\sigma + \tau)^{\frac{3}{2}} \mp (\sigma - \tau)^{\frac{3}{2}} \right\}: \quad \ldots \ldots \quad (V^3.)$$

which latter is a known expression.

The formulæ (K^3.) and (L^3.), to the comparison of which we have reduced the study of elliptic motion, extend to hyperbolic motion also; and in any binary system, with Newton's law of attraction, the simplified characteristic function w may be expressed by the definite integral

$$w = \int_{-\tau}^{\tau} \sqrt{\frac{\mu}{\sigma + \tau} - \frac{\mu}{4a}} \, . \, d\tau, \quad \ldots \ldots \quad (W^3.)$$

this function w being still connected with the relative action $V_{\prime}$ by the equation (H^3.); while the time t, which may always be deduced from this function, by the law of varying action, is represented by this other connected integral,

$$t = \frac{1}{4}\int_{-\tau}^{\tau} \left(\frac{\mu}{\sigma + \tau} - \frac{\mu}{4\,\mathrm{a}}\right)^{-\frac{1}{2}} d\tau : \quad \ldots \quad (\mathrm{X}^3.)$$

provided that, within the extent of these integrations, the radical does not vanish nor become infinite. When this condition is not satisfied, we may still express the simplified characteristic function w, and the time t, by the following analogous integrals:

$$w = \int_{\tau_{\prime}}^{\sigma_{\prime}} \pm \sqrt{\frac{2\mu}{\sigma_{\prime}} - \frac{\mu}{\mathrm{a}}}\, d\sigma_{\prime}, \quad \ldots \quad (\mathrm{Y}^3.)$$

and

$$t = \int_{\tau_{\prime}}^{\sigma_{\prime}} \pm \left(\frac{2\mu}{\sigma_{\prime}} - \frac{\mu}{\mathrm{a}}\right)^{-\frac{1}{2}} d\sigma_{\prime}, \quad \ldots \quad (\mathrm{Z}^3.)$$

in which we have put for abridgement

$$\sigma_{\prime} = \frac{\sigma + \tau}{2}, \quad \tau_{\prime} = \frac{\sigma - \tau}{2}, \quad \ldots \quad (119.)$$

and in which it is easy to determine the signs of the radicals. But to treat fully of these various transformations would carry us too far at present, for it is time to consider the properties of systems with more points than two.

On Systems of three Points, in general; and on their Characteristic Functions.

17. For any system of three points, the known differential equations of motion of the 2nd order are included in the following formula:

$$\left.\begin{array}{l} m_1 (x''_1 \delta x_1 + y''_1 \delta y_1 + z''_1 \delta z_1) + m_2 (x''_2 \delta x_2 + y''_2 \delta y_2 + z''_2 \delta z_2) \\ + m_3 (x''_3 \delta x_3 + y''_3 \delta y_3 + z''_3 \delta z_3) = \delta \mathrm{U}, \end{array}\right\} \quad \ldots \quad (120.)$$

the known force-function U having the form

$$\mathrm{U} = m_1 m_2 f^{(1,\,2)} + m_1 m_3 f^{(1,\,3)} + m_2 m_3 f^{(2,\,3)}, \quad \ldots \quad (121.)$$

in which $f^{(1,\,2)}$, $f^{(1,\,3)}$, $f^{(2,\,3)}$, are functions respectively of the three following mutual distances of the points of the system:

$$\left.\begin{array}{l} r^{(1,\,2)} = \sqrt{(x_1 - x_2)^2 + (y_1 - y_2)^2 + (z_1 - z_2)^2}, \\ r^{(1,\,3)} = \sqrt{(x_1 - x_3)^2 + (y_1 - y_3)^2 + (z_1 - z_3)^2}, \\ r^{(2,\,3)} = \sqrt{(x_2 - x_3)^2 + (y_2 - y_3)^2 + (z_2 - z_3)^2}: \end{array}\right\} \quad \ldots \quad (122.)$$

the known differential equations of motion are therefore, separately, for the point m_1,

$$\left.\begin{array}{l} x''_1 = m_2 \dfrac{\delta f^{(1,\,2)}}{\delta x_1} + m_3 \dfrac{\delta f^{(1,\,3)}}{\delta x_1}, \\[2ex] y''_1 = m_2 \dfrac{\delta f^{(1,\,2)}}{\delta y_1} + m_3 \dfrac{\delta f^{(1,\,3)}}{\delta y_1}, \\[2ex] z''_1 = m_2 \dfrac{\delta f^{(1,\,2)}}{\delta z_1} + m_3 \dfrac{\delta f^{(1,\,3)}}{\delta z_1}, \end{array}\right\} \quad \ldots \quad (123.)$$

with six other analogous equations for the points m_2 and m_3; x''_1, &c., denoting the component accelerations of the three points $m_1\, m_2\, m_3$, or the second differential coefficients of their coordinates, taken with respect to the time. To integrate these equations is to assign, by their means, nine relations between the time t, the three masses $m_1\, m_2\, m_3$, the nine varying coordinates $x_1\, y_1\, z_1\, x_2\, y_2\, z_2\, x_3\, y_3\, z_3$, and their nine initial values and nine initial rates of increase, which may be thus denoted, $a_1\, b_1\, c_1\, a_2\, b_2\, c_2\, a_3\, b_3\, c_3\, a'_1\, b'_1\, c'_1\, a'_2\, b'_2\, c'_2\, a'_3\, b'_3\, c'_3$. The known intermediate integral containing the law of living force, namely,

$$\left.\begin{array}{l}\frac{1}{2} m_1 (x'^2_1 + y'^2_1 + z'^2_1) + \frac{1}{2} m_2 (x'^2_2 + y'^2_2 + z'^2_2) + \frac{1}{2} m_3 (x'^2_3 + y'^2_3 + z'^2_3) \\ = m_1 m_2 f^{(1,2)} + m_1 m_3 f^{(1,3)} + m_2 m_3 f^{(2,3)} + \mathrm{H},\end{array}\right\} \quad (124.)$$

gives the following initial relation:

$$\left.\begin{array}{l}\frac{1}{2} m_1 (a'^2_1 + b'^2_1 + c'^2_1) + \frac{1}{2} m_2 (a'^2_2 + b'^2_2 + c'^2_2) + \frac{1}{2} m_3 (a'^2_3 + b'^2_3 + c'^2_3) \\ = m_1 m_2 f_0^{(1,2)} + m_1 m_3 f_0^{(1,3)} + m_2 m_3 f_0^{(2,3)} + \mathrm{H},\end{array}\right\} \quad (125.)$$

in which $f_0^{(1,2)}$, $f_0^{(1,3)}$, $f_0^{(2,3)}$, are composed of the initial coordinates, in the same manner as $f^{(1,2)}\, f^{(1,3)}\, f^{(2,3)}$ are composed of the final coördinates. If then we knew the nine final integrals of the equations of motion of this ternary system, and combined them with the initial form (125.) of the law of living force, we should have ten relations to determine the ten quantities $t\, a'_1\, b'_1\, c'_1\, a'_2\, b'_2\, c'_2\, a'_3\, b'_3\, c'_3$, namely, the time and the nine initial components of the velocities of the three points, as functions of the nine final and nine initial coordinates, and of the quantity H, involving also the masses; we could therefore determine whatever else depends on the manner and time of motion of the system, from its initial to its final position, as a function of the same extreme coordinates, and of H. In particular, we could determine the action V, or the accumulated living force of the system, namely,

$$\left.\begin{array}{l}\mathrm{V} = m_1 \int_0^t (x'^2_1 + y'^2_1 + z'^2_1)\, dt + m_2 \int_0^t (x'^2_2 + y'^2_2 + z'^2_2)\, dt \\ + m_3 \int_0^t (x'^2_3 + y'^2_3 + z'^2_3)\, dt,\end{array}\right\} \quad . \quad (\mathrm{A}^4.)$$

as a function of these nineteen quantities, $x_1\, y_1\, z_1\, x_2\, y_2\, z_2\, x_3\, y_3\, z_3\, a_1\, b_1\, c_1\, a_2\, b_2\, c_2\, a_3\, b_3\, c_3\, \mathrm{H}$; and might then calculate the variation of this function,

$$\left.\begin{array}{l}\delta \mathrm{V} = \frac{\delta \mathrm{V}}{\delta x_1} \delta x_1 + \frac{\delta \mathrm{V}}{\delta y_1} \delta y_1 + \frac{\delta \mathrm{V}}{\delta z_1} \delta z_1 + \frac{\delta \mathrm{V}}{\delta a_1} \delta a_1 + \frac{\delta \mathrm{V}}{\delta b_1} \delta b_1 + \frac{\delta \mathrm{V}}{\delta c_1} \delta c_1 \\ + \frac{\delta \mathrm{V}}{\delta x_2} \delta x_2 + \frac{\delta \mathrm{V}}{\delta y_2} \delta y_2 + \frac{\delta \mathrm{V}}{\delta z_2} \delta z_2 + \frac{\delta \mathrm{V}}{\delta a_2} \delta a_2 + \frac{\delta \mathrm{V}}{\delta b_2} \delta b_2 + \frac{\delta \mathrm{V}}{\delta c_2} \delta c_2 \\ + \frac{\delta \mathrm{V}}{\delta x_3} \delta x_3 + \frac{\delta \mathrm{V}}{\delta y_3} \delta y_3 + \frac{\delta \mathrm{V}}{\delta z_3} \delta z_3 + \frac{\delta \mathrm{V}}{\delta a_3} \delta a_3 + \frac{\delta \mathrm{V}}{\delta b_3} \delta b_3 + \frac{\delta \mathrm{V}}{\delta c_3} \delta c_3 \\ + \frac{\delta \mathrm{V}}{\delta \mathrm{H}} \delta \mathrm{H}.\end{array}\right\} \quad . \quad (\mathrm{B}^4.)$$

But the law of varying action gives, *previously*, the following expression for this variation:

$$\left.\begin{aligned}\delta V = m_1 (x'_1 \delta x_1 - a'_1 \delta a_1 + y'_1 \delta y_1 - b'_1 \delta b_1 + z'_1 \delta z_1 - c'_1 \delta c_1)\\ + m_2 (x'_2 \delta x_2 - a'_2 \delta a_2 + y'_2 \delta y_2 - b'_2 \delta b_2 + z'_2 \delta z_2 - c'_2 \delta c_2)\\ + m_3 (x'_3 \delta x_3 - a'_3 \delta a_3 + y'_3 \delta y_3 - b'_3 \delta b_3 + z'_3 \delta z_3 - c'_3 \delta c_3)\\ + t \delta H;\end{aligned}\right\} \quad . \quad (C^4.)$$

and shows, therefore, that the research of all the intermediate and all the final integral equations, of motion of the system, may be reduced, reciprocally, to the search and differentiation of this one characteristic function V; because if we knew this one function, we should have the nine intermediate integrals of the known differential equations, under the forms

$$\left.\begin{aligned}\frac{\delta V}{\delta x_1} = m_1 x'_1, \frac{\delta V}{\delta y_1} = m_1 y'_1, \frac{\delta V}{\delta z_1} = m_1 z'_1,\\ \frac{\delta V}{\delta x_2} = m_2 x'_2, \frac{\delta V}{\delta y_2} = m_2 y'_2, \frac{\delta V}{\delta z_2} = m_2 z'_2,\\ \frac{\delta V}{\delta x_3} = m_3 x'_3, \frac{\delta V}{\delta y_3} = m_3 y'_3, \frac{\delta V}{\delta z_3} = m_3 z'_3,\end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (D^4.)$$

and the nine final integrals under the forms

$$\left.\begin{aligned}\frac{\delta V}{\delta a_1} = - m_1 a'_1, \frac{\delta V}{\delta b_1} = - m_1 b'_1, \frac{\delta V}{\delta c_1} = - m_1 c'_1,\\ \frac{\delta V}{\delta a_2} = - m_2 a'_2, \frac{\delta V}{\delta b_2} = - m_2 b'_2, \frac{\delta V}{\delta c_2} = - m_2 c'_2,\\ \frac{\delta V}{\delta a_3} = - m_3 a'_3, \frac{\delta V}{\delta b_3} = - m_3 b'_3, \frac{\delta V}{\delta c_3} = - m_3 c'_3,\end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \; . \; . \quad (E^4.)$$

the auxiliary constant H being to be eliminated, and the time t introduced, by this other equation, which has often occurred in this essay,

$$t = \frac{\delta V}{\delta H}. \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (E.)$$

The same law of varying action suggests also a method of investigating the form of this characteristic function V, not requiring the previous integration of the known equations of motion; namely, the integration of a pair of partial differential equations connected with the law of living force; which are,

$$\left.\begin{aligned}\frac{1}{2m_1}\left\{\left(\frac{\delta V}{\delta x_1}\right)^2 + \left(\frac{\delta V}{\delta y_1}\right)^2 + \left(\frac{\delta V}{\delta z_1}\right)^2\right\} + \frac{1}{2m_2}\left\{\left(\frac{\delta V}{\delta x_2}\right)^2 + \left(\frac{\delta V}{\delta y_2}\right)^2 + \left(\frac{\delta V}{\delta z_2}\right)^2\right\}\\ + \frac{1}{2m_3}\left\{\left(\frac{\delta V}{\delta x_3}\right)^2 + \left(\frac{\delta V}{\delta y_3}\right)^2 + \left(\frac{\delta V}{\delta z_3}\right)^2\right\} = m_1 m_2 f^{(1,2)} + m_1 m_3 f^{(1,3)} + m_2 m_3 f^{(2,3)} + H,\end{aligned}\right\} (F^4).$$

and

$$\left.\begin{aligned}\frac{1}{2m_1}\left\{\left(\frac{\delta V}{\delta a_1}\right)^2 + \left(\frac{\delta V}{\delta b_1}\right)^2 + \left(\frac{\delta V}{\delta c_1}\right)^2\right\} + \frac{1}{2m_2}\left\{\left(\frac{\delta V}{\delta a_2}\right)^2 + \left(\frac{\delta V}{\delta b_2}\right)^2 + \left(\frac{\delta V}{\delta c_2}\right)^2\right\}\\ + \frac{1}{2m_3}\left\{\left(\frac{\delta V}{\delta a_3}\right)^2 + \left(\frac{\delta V}{\delta b_3}\right)^2 + \left(\frac{\delta V}{\delta c_3}\right)^2\right\} = m_1 m_2 f_0^{(1,2)} + m_1 m_3 f_0^{(1,3)} + m_2 m_3 f_0^{(2,3)} + H.\end{aligned}\right\} (G^4.)$$

And to diminish the difficulty of thus determining the function V, which depends on 18 coordinates, we may separate it, by principles already explained, into a part $V_{\prime\prime}$ depending only on the motion of the centre of gravity of the system, and determined by the formula (H^1.), and another part $V_{\prime}$, depending only on the relative motions of the points of the system about this internal centre, and equal to the accumulated living force, connected with this relative motion only. In this manner the difficulty is reduced to determining the relative action $V_{\prime}$; and if we introduce the relative coordinates

$$\left.\begin{array}{lll} \xi_1 = x_1 - x_3, & \eta_1 = y_1 - y_3, & \zeta_1 = z_1 - z_3, \\ \xi_2 = x_2 - x_3, & \eta_2 = y_2 - y_3, & \zeta_2 = z_2 - z_3, \end{array}\right\} \quad \ldots \quad (126.)$$

and

$$\left.\begin{array}{lll} \alpha_1 = a_1 - a_3, & \beta_1 = b_1 - b_3, & \gamma_1 = c_1 - c_3, \\ \alpha_2 = a_2 - a_3, & \beta_2 = b_2 - b_3, & \gamma_2 = c_2 - c_3, \end{array}\right\} \quad \ldots \quad (127.)$$

we easily find, by the principles of the tenth and following numbers, that the function $V_{\prime}$ may be considered as depending only on these relative coordinates, and on a quantity $H_{\prime}$ analogous to H (besides the masses of the system); and that it must satisfy two partial differential equations, analogous to (F^4.) and (G^4.), namely,

$$\left.\begin{array}{l} \frac{1}{2m_1}\left\{\left(\frac{\delta V_{\prime}}{\delta \xi_1}\right)^2 + \left(\frac{\delta V_{\prime}}{\delta \eta_1}\right)^2 + \left(\frac{\delta V_{\prime}}{\delta \zeta_1}\right)^2\right\} + \frac{1}{2m_2}\left\{\left(\frac{\delta V_{\prime}}{\delta \xi_2}\right)^2 + \left(\frac{\delta V_{\prime}}{\delta \eta_2}\right)^2 + \left(\frac{\delta V_{\prime}}{\delta \zeta_2}\right)^2\right\} \\ + \frac{1}{2m_3}\left\{\left(\frac{\delta V_{\prime}}{\delta \xi_1} + \frac{\delta V_{\prime}}{\delta \xi_2}\right)^2 + \left(\frac{\delta V_{\prime}}{\delta \eta_1} + \frac{\delta V_{\prime}}{\delta \eta_2}\right)^2 + \left(\frac{\delta V_{\prime}}{\delta \zeta_1} + \frac{\delta V_{\prime}}{\delta \zeta_2}\right)^2\right\} \\ = m_1 m_2 f^{(1,2)} + m_1 m_3 f^{(1,3)} + m_2 m_3 f^{(2,3)} + H_{\prime}; \end{array}\right\} (H^4.)$$

and

$$\left.\begin{array}{l} \frac{1}{2m_1}\left\{\left(\frac{\delta V_{\prime}}{\delta \alpha_1}\right)^2 + \left(\frac{\delta V_{\prime}}{\delta \beta_1}\right)^2 + \left(\frac{\delta V_{\prime}}{\delta \gamma_1}\right)^2\right\} + \frac{1}{2m_2}\left\{\left(\frac{\delta V_{\prime}}{\delta \alpha_2}\right)^2 + \left(\frac{\delta V_{\prime}}{\delta \beta_2}\right)^2 + \left(\frac{\delta V_{\prime}}{\delta \gamma_2}\right)^2\right\} \\ + \frac{1}{2m_3}\left\{\left(\frac{\delta V_{\prime}}{\delta \alpha_1} + \frac{\delta V_{\prime}}{\delta \alpha_2}\right)^2 + \left(\frac{\delta V_{\prime}}{\delta \beta_1} + \frac{\delta V_{\prime}}{\delta \beta_2}\right)^2 + \left(\frac{\delta V_{\prime}}{\delta \gamma_1} + \frac{\delta V_{\prime}}{\delta \gamma_2}\right)^2\right\} \\ = m_1 m_2 f_0^{(1,2)} + m_1 m_3 f_0^{(1,3)} + m_2 m_3 f_0^{(2,3)} + H_{\prime}: \end{array}\right\} (I^4.)$$

the law of the variation of this function being, by (Z^1.),

$$\left.\begin{array}{l} \delta V_{\prime} = t\,\delta H_{\prime} + m_1 (\xi'_1 \delta \xi_1 - \alpha'_1 \delta \alpha_1 + \eta'_1 \delta \eta_1 - \beta'_1 \delta \beta_1 + \zeta'_1 \delta \zeta_1 - \gamma'_1 \delta \gamma_1) \\ \qquad + m_2 (\xi'_2 \delta \xi_2 - \alpha'_2 \delta \alpha_2 + \eta'_2 \delta \eta_2 - \beta'_2 \delta \beta_2 + \zeta'_2 \delta \zeta_2 - \gamma'_2 \delta \gamma_2) \\ - \frac{1}{m_1 + m_2 + m_3}\left\{\begin{array}{l} (m_1 \xi'_1 + m_2 \xi'_2)(m_1 \delta \xi_1 + m_2 \delta \xi_2) - (m_1 \alpha'_1 + m_2 \alpha'_2)(m_1 \delta \alpha_1 + m_2 \delta \alpha_2) \\ + (m_1 \eta'_1 + m_2 \eta'_2)(m_1 \delta \eta_1 + m_2 \delta \eta_2) - (m_1 \beta'_1 + m_2 \beta'_2)(m_1 \delta \beta_1 + m_2 \delta \beta_2) \\ + (m_1 \zeta'_1 + m_2 \zeta'_2)(m_1 \delta \zeta_1 + m_2 \delta \zeta_2) - (m_1 \gamma'_1 + m_2 \gamma'_2)(m_1 \delta \gamma_1 + m_2 \delta \gamma_2) \end{array}\right\} \end{array}\right\} (K^4.)$$

which resolves itself in the same manner as before into the six intermediate and six final integrals of relative motion, namely, into the following equations:

$$\left.\begin{aligned}
\frac{1}{m_1}\frac{\delta V_{\prime}}{\delta \xi_1} &= \xi'_1 - \frac{m_1 \xi'_1 + m_2 \xi'_2}{m_1 + m_2 + m_3}; \quad \frac{1}{m_2}\frac{\delta V_{\prime}}{\delta \xi_2} = \xi'_2 - \frac{m_1 \xi'_1 + m_2 \xi'_2}{m_1 + m_2 + m_3};\\
\frac{1}{m_1}\frac{\delta V_{\prime}}{\delta \eta_1} &= \eta'_1 - \frac{m_1 \eta'_1 + m_2 \eta'_2}{m_1 + m_2 + m_3}; \quad \frac{1}{m_2}\frac{\delta V_{\prime}}{\delta \eta_2} = \eta'_2 - \frac{m_1 \eta'_1 + m_2 \eta'_2}{m_1 + m_2 + m_3};\\
\frac{1}{m_1}\frac{\delta V_{\prime}}{\delta \zeta_1} &= \zeta'_1 - \frac{m_1 \zeta'_1 + m_2 \zeta'_2}{m_1 + m_2 + m_3}; \quad \frac{1}{m_2}\frac{\delta V_{\prime}}{\delta \zeta_2} = \zeta'_2 - \frac{m_1 \zeta'_1 + m_2 \zeta'_2}{m_1 + m_2 + m_3};
\end{aligned}\right\} \quad . \quad . \quad (\mathrm{L}^4)$$

and

$$\left.\begin{aligned}
\frac{-1}{m_1}\frac{\delta V_{\prime}}{\delta \alpha_1} &= \alpha'_1 - \frac{m_1 \alpha'_1 + m_2 \alpha'_2}{m_1 + m_2 + m_3}; \quad \frac{-1}{m_2}\frac{\delta V_{\prime}}{\delta \alpha_2} = \alpha'_2 - \frac{m_1 \alpha'_1 + m_2 \alpha'_2}{m_1 + m_2 + m_3};\\
\frac{-1}{m_1}\frac{\delta V_{\prime}}{\delta \beta_1} &= \beta'_1 - \frac{m_1 \beta'_1 + m_2 \beta'_2}{m_1 + m_2 + m_3}; \quad \frac{-1}{m_2}\frac{\delta V_{\prime}}{\delta \beta_2} = \beta'_2 - \frac{m_1 \beta'_1 + m_2 \beta'_2}{m_1 + m_2 + m_3};\\
\frac{-1}{m_1}\frac{\delta V_{\prime}}{\delta \gamma_1} &= \gamma'_1 - \frac{m_1 \gamma'_1 + m_2 \gamma'_2}{m_1 + m_2 + m_3}; \quad \frac{-1}{m_2}\frac{\delta V_{\prime}}{\delta \gamma_2} = \gamma'_2 - \frac{m_1 \gamma'_1 + m_2 \gamma'_2}{m_1 + m_2 + m_3};
\end{aligned}\right\} \quad . \quad (\mathrm{M}^4.)$$

which must be combined with our old formula,

$$\frac{\delta V_{\prime}}{\delta H_{\prime}} = t. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{O}^1.)$$

18. The quantity $H_{\prime}$ in $V_{\prime}$, and the analogous quantity $H_{\prime\prime}$ in $V_{\prime\prime}$, are indeed independent of the time, and do not vary in the course of the motion; but it is required by the spirit of our method, that in deducing the absolute action or original characteristic function V from the two parts $V_{\prime}$ and $V_{\prime\prime}$, we should consider these two parts H and $H_{\prime\prime}$ of the original quantity H, as functions involving each the nine initial and nine final coordinates of the points of the ternary system; the forms of these two functions, of the eighteen coordinates and of H, being determined by the two conditions,

$$\frac{\delta V_{\prime}}{\delta H_{\prime}} = \frac{\delta V_{\prime\prime}}{\delta H_{\prime\prime}}, \; H_{\prime} + H_{\prime\prime} = H. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{N}^4.)$$

However, it results from these conditions, that in taking the variation of the whole original function V, of the first order, with respect to the eighteen coordinates, we may treat the two auxiliary quantities $H_{\prime}$ and $H_{\prime\prime}$ as constant; and therefore that we have the following expressions for the partial differential coefficients of the first order of V, taken with respect to the coordinates parallel to x,

$$\left.\begin{aligned}
\frac{\delta V}{\delta x_1} &= \frac{\delta V_{\prime}}{\delta \xi_1} + \frac{m_1}{m_1 + m_2 + m_3}\frac{\delta V_{\prime\prime}}{\delta x_{\prime\prime}}, \quad \frac{\delta V}{\delta a_1} = \frac{\delta V_{\prime}}{\delta \alpha_1} + \frac{m_1}{m_1 + m_2 + m_3}\frac{\delta V_{\prime\prime}}{\delta a_{\prime\prime}},\\
\frac{\delta V}{\delta x_2} &= \frac{\delta V_{\prime}}{\delta \xi_2} + \frac{m_2}{m_1 + m_2 + m_3}\frac{\delta V_{\prime\prime}}{\delta x_{\prime\prime}}, \quad \frac{\delta V}{\delta a_2} = \frac{\delta V_{\prime}}{\delta \alpha_2} + \frac{m_2}{m_1 + m_2 + m_3}\frac{\delta V_{\prime\prime}}{\delta a_{\prime\prime}},\\
\frac{\delta V}{\delta x_3} &= -\frac{\delta V_{\prime}}{\delta \xi_1} - \frac{\delta V_{\prime}}{\delta \xi_2} + \frac{m_3}{m_1 + m_2 + m_3}\frac{\delta V_{\prime\prime}}{\delta x_{\prime\prime}}, \quad \frac{\delta V}{\delta a_3} = -\frac{\delta V_{\prime}}{\delta \alpha_1} - \frac{\delta V_{\prime}}{\delta \alpha_2} + \frac{m_3}{m_1 + m_2 + m_3}\frac{\delta V_{\prime\prime}}{\delta a_{\prime\prime}},
\end{aligned}\right\} (\mathrm{O}^4.)$$

together with analogous expressions for the partial differential coefficients of the same order, taken with respect to the other coordinates. Substituting these expressions in the equations of the form (O.), namely, in the following,

$$\left.\begin{array}{l}\frac{\delta V}{\delta x_1} + \frac{\delta V}{\delta x_2} + \frac{\delta V}{\delta x_3} + \frac{\delta V}{\delta a_1} + \frac{\delta V}{\delta a_2} + \frac{\delta V}{\delta a_3} = 0, \\ \frac{\delta V}{\delta y_1} + \frac{\delta V}{\delta y_2} + \frac{\delta V}{\delta y_3} + \frac{\delta V}{\delta b_1} + \frac{\delta V}{\delta b_2} + \frac{\delta V}{\delta b_3} = 0, \\ \frac{\delta V}{\delta z_1} + \frac{\delta V}{\delta z_2} + \frac{\delta V}{\delta z_3} + \frac{\delta V}{\delta c_1} + \frac{\delta V}{\delta c_2} + \frac{\delta V}{\delta c_3} = 0,\end{array}\right\} \quad \ldots \quad (P^4.)$$

we find that these equations become identical, because

$$\frac{\delta V_{\prime\prime}}{\delta x_{\prime\prime}} + \frac{\delta V_{\prime\prime}}{\delta a_{\prime\prime}} = 0, \; \frac{\delta V_{\prime\prime}}{\delta y_{\prime\prime}} + \frac{\delta V_{\prime\prime}}{\delta b_{\prime\prime}} = 0, \; \frac{\delta V_{\prime\prime}}{\delta z_{\prime\prime}} + \frac{\delta V_{\prime\prime}}{\delta c_{\prime\prime}} = 0. \quad \ldots \quad (Q^4.)$$

But substituting, in like manner, the expressions (O4.) in the equations of the form (P.), of which the first is, for a ternary system,

$$\left.\begin{array}{l} x_1 \frac{\delta V}{\delta y_1} - y_1 \frac{\delta V}{\delta x_1} + x_2 \frac{\delta V}{\delta y_2} - y_2 \frac{\delta V}{\delta x_2} + x_3 \frac{\delta V}{\delta y_3} \quad y_3 \frac{\delta V}{\delta x_3} \\ + a_1 \frac{\delta V}{\delta b_1} - b_1 \frac{\delta V}{\delta a_1} + a_2 \frac{\delta V}{\delta b_2} - b_2 \frac{\delta V}{\delta a_2} + a_3 \frac{\delta V}{\delta b_3} - b_3 \frac{\delta V}{\delta a_3}; \end{array}\right\} \quad \ldots \quad (R^4.)$$

and observing that we have

$$x_{\prime\prime} \frac{\delta V_{\prime\prime}}{\delta y_{\prime\prime}} - y_{\prime\prime} \frac{\delta V_{\prime\prime}}{\delta x_{\prime\prime}} + a_{\prime\prime} \frac{\delta V_{\prime\prime}}{\delta b_{\prime\prime}} - b_{\prime\prime} \frac{\delta V_{\prime\prime}}{\delta a_{\prime\prime}} = 0, \quad \ldots \quad (S^4.)$$

along with two other analogous conditions, we find that the part $V_{\prime}$, or the characteristic function of relative motion of the ternary system, must satisfy the three following conditions, involving its partial differential coefficients of the first order and in the first degree,

$$\left.\begin{array}{l} 0 = \xi_1 \frac{\delta V_{\prime}}{\delta \eta_1} - \eta_1 \frac{\delta V_{\prime}}{\delta \xi_1} + \xi_2 \frac{\delta V_{\prime}}{\delta \eta_2} - \eta_2 \frac{\delta V_{\prime}}{\delta \xi_2} + \alpha_1 \frac{\delta V_{\prime}}{\delta \beta_1} - \beta_1 \frac{\delta V_{\prime}}{\delta \alpha_1} + \alpha_2 \frac{\delta V_{\prime}}{\delta \beta_2} - \beta_2 \frac{\delta V_{\prime}}{\delta \alpha_2}, \\ 0 = \eta_1 \frac{\delta V_{\prime}}{\delta \zeta_1} - \zeta_1 \frac{\delta V_{\prime}}{\delta \eta_1} + \eta_2 \frac{\delta V_{\prime}}{\delta \zeta_2} - \zeta_2 \frac{\delta V_{\prime}}{\delta \eta_2} + \beta_1 \frac{\delta V_{\prime}}{\delta \gamma_1} - \gamma_1 \frac{\delta V_{\prime}}{\delta \beta_1} + \beta_2 \frac{\delta V_{\prime}}{\delta \gamma_2} - \gamma_2 \frac{\delta V_{\prime}}{\delta \beta_2}, \\ 0 = \zeta_1 \frac{\delta V_{\prime}}{\delta \xi_1} - \xi_1 \frac{\delta V_{\prime}}{\delta \zeta_1} + \zeta_2 \frac{\delta V_{\prime}}{\delta \xi_2} - \xi_2 \frac{\delta V_{\prime}}{\delta \zeta_2} + \gamma_1 \frac{\delta V_{\prime}}{\delta \alpha_1} - \alpha_1 \frac{\delta V_{\prime}}{\delta \gamma_1} + \gamma_2 \frac{\delta V_{\prime}}{\delta \alpha_2} - \alpha_2 \frac{\delta V_{\prime}}{\delta \gamma_2}, \end{array}\right\} . \quad (T^4.)$$

which show that this function can depend only on the shape and size of a pentagon, not generally plane, formed by the point m_3 considered as fixed, and by the initial and final positions of the other two points m_1 and m_2; for example, the pentagon, of which the corners are, in order, m_3 (m_1) (m_2) m_2 m_1; (m_1) and (m_2) denoting the initial positions of the points m_1 and m_2, referred to m_3 as a fixed origin. The shape and size of this pentagon may be determined by the ten mutual distances of its five points, that is, by the five sides and five diagonals, which may be thus denoted:

$$\left.\begin{array}{l} m_3 (m_1) = \sqrt{s_1}, (m_1)(m_2) = \sqrt{s_2}, (m_2) m_2 = \sqrt{s_3}, m_2 m_1 = \sqrt{s_4}, m_1 m_3 = \sqrt{s_5}, \\ m_3 (m_2) = \sqrt{d_1}, (m_1) m_2 = \sqrt{d_2}, (m_2) m_1 = \sqrt{d_3}, m_2 m_3 = \sqrt{d_4}, m_1 (m_1) = \sqrt{d_5}; \end{array}\right\} \quad (128.)$$

the values of $s_1 \ldots d_5$ as functions of the twelve relative coordinates being

$$\left.\begin{aligned}
s_1 = \alpha_1^2 + \beta_1^2 + \gamma_1^2,\ s_2 &= (\alpha_2 - \alpha_1)^2 + (\beta_2 - \beta_1)^2 + (\gamma_2 - \gamma_1)^2,\\
s_3 &= (\xi_2 - \alpha_2)^2 + (\eta_2 - \beta_2)^2 + (\zeta_2 - \gamma_2)^2,\\
s_5 = \xi_1^2 + \eta_1^2 + \zeta_1^2,\ s_4 &= (\xi_1 - \xi_2)^2 + (\eta_1 - \eta_2)^2 + (\zeta_2 - \zeta_2)^2,\\
d_1 = \alpha_2^2 + \beta_2^2 + \gamma_2^2,\ d_2 &= (\xi_2 - \alpha_1)^2 + (\eta_2 - \beta_1)^2 + (\zeta_2 - \gamma_1)^2,\\
d_3 &= (\xi_1 - \alpha_2)^2 + (\eta_1 - \beta_2)^2 + (\zeta_1 - \gamma_2)^2,\\
d_4 = \xi_2^2 + \eta_2^2 + \zeta_2^2,\ d_5 &= (\xi_1 - \alpha_1)^2 + (\eta_1 - \beta_1)^2 + (\zeta_1 - \gamma_1)^2.
\end{aligned}\right\} \quad (129.)$$

These ten distances $\sqrt{s_1}$, &c., are not, however, all independent, but are connected by one equation of condition, namely,

$$\left.\begin{aligned}
0 = {} & s_1^2 s_3^2 + s_2^2 s_4^2 + s_3^2 s_5^2 + s_4^2 s_1^2 + s_5^2 s_2^2\\
& + s_1^2 d_3^2 + s_2^2 d_4^2 + s_3^2 d_5^2 + s_4^2 d_1^2 + s_5^2 d_2^2\\
& + d_1^2 d_2^2 + d_2^2 d_3^2 + d_3^2 d_4^2 + d_4^2 d_5^2 + d_5^2 d_1^2\\
& - 2 s_1^2 s_3 s_4 - 2 s_2^2 s_4 s_5 - 2 s_3^2 s_5 s_1 - 2 s_4^2 s_1 s_2 - 2 s_5^2 s_2 s_3\\
& - 2 s_1^2 s_3 d_3 - 2 s_2^2 s_4 d_4 - 2 s_3^2 s_5 d_5 - 2 s_4^2 s_1 d_1 - 2 s_5^2 s_2 d_2\\
& - 2 s_1^2 s_4 d_3 - 2 s_2^2 s_5 d_4 - 2 s_3^2 s_1 d_5 - 2 s_4^2 s_2 d_1 - 2 s_5^2 s_3 d_2\\
& - 2 s_1 d_2 d_3^2 - 2 s_2 d_3 d_4^2 - 2 s_3 d_4 d_5^2 - 2 s_4 d_5 d_1^2 - 2 s_5 d_1 d_2^2\\
& - 2 s_1 d_3^2 d_4 - 2 s_2 d_4^2 d_5 - 2 s_3 d_5^2 d_1 - 2 s_4 d_1^2 d_2 - 2 s_5 d_2^2 d_3\\
& - 2 d_1 d_2^2 d_3 - 2 d_2 d_3^2 d_4 - 2 d_3 d_4^2 d_5 - 2 d_4 d_5^2 d_1 - 2 d_5 d_1^2 d_2\\
& - 4 s_1 s_3 s_4 d_3 - 4 s_2 s_4 s_5 d_4 - 4 s_3 s_5 s_1 d_5 - 4 s_4 s_1 s_2 d_1 - 4 s_5 s_2 s_3 d_2\\
& - 4 s_1 d_2 d_3 d_4 - 4 s_2 d_3 d_4 d_5 - 4 s_3 d_4 d_5 d_1 - 4 s_4 d_5 d_1 d_2 - 4 s_5 d_1 d_2 d_3\\
& - 2 s_1 s_2 s_3 d_4 - 2 s_2 s_3 s_4 d_5 - 2 s_3 s_4 s_5 d_1 - 2 s_4 s_5 s_1 d_2 - 2 s_5 s_1 s_2 d_3\\
& - 2 s_1 s_3 d_1 d_2 - 2 s_2 s_4 d_2 d_3 - 2 s_3 s_5 d_3 d_4 - 2 s_4 s_1 d_4 d_5 - 2 s_5 s_2 d_5 d_1\\
& - 2 s_1 d_1 d_3 d_5 - 2 s_2 d_2 d_4 d_1 - 2 s_3 d_3 d_5 d_2 - 2 s_4 d_4 d_1 d_3 - 2 s_5 d_5 d_2 d_4\\
& + 2 s_1 s_2 s_3 s_4 + 2 s_2 s_3 s_4 s_5 + 2 s_3 s_4 s_5 s_1 + 2 s_4 s_5 s_1 s_2 + 2 s_5 s_1 s_2 s_3\\
& + 2 s_1 s_2 s_4 d_3 + 2 s_2 s_3 s_5 d_4 + 2 s_3 s_4 s_1 d_5 + 2 s_4 s_5 s_2 d_1 + 2 s_5 s_1 s_3 d_2\\
& + 2 s_1 s_3 s_4 d_1 + 2 s_2 s_4 s_5 d_2 + 2 s_3 s_5 s_1 d_3 + 2 s_4 s_1 s_2 d_4 + 2 s_5 s_2 s_3 d_5\\
& + 2 s_1 s_2 d_3 d_4 + 2 s_2 s_3 d_4 d_5 + 2 s_3 s_4 d_5 d_1 + 2 s_4 s_5 d_1 d_2 + 2 s_5 s_1 d_2 d_3\\
& + 2 s_1 s_3 d_2 d_3 + 2 s_2 s_4 d_3 d_4 + 2 s_3 s_5 d_4 d_5 + 2 s_4 s_1 d_5 d_1 + 2 s_5 s_2 d_1 d_2\\
& + 2 s_1 s_4 d_1 d_2 + 2 s_2 s_5 d_2 d_3 + 2 s_3 s_1 d_3 d_4 + 2 s_4 s_2 d_4 d_5 + 2 s_5 s_3 d_5 d_1\\
& + 2 s_1 s_4 d_1 d_3 + 2 s_2 s_5 d_2 d_4 + 2 s_3 s_1 d_3 d_5 + 2 s_4 s_2 d_4 d_1 + 2 s_5 s_3 d_5 d_2\\
& + 2 s_1 s_4 d_2 d_3 + 2 s_2 s_5 d_3 d_4 + 2 s_3 s_1 d_4 d_5 + 2 s_4 s_2 d_5 d_1 + 2 s_5 s_3 d_1 d_2\\
& + 2 s_1 s_4 d_3 d_4 + 2 s_2 s_5 d_4 d_5 + 2 s_3 s_1 d_5 d_1 + 2 s_4 s_2 d_1 d_2 + 2 s_5 s_3 d_2 d_3\\
& + 2 s_1 d_1 d_2 d_3 + 2 s_2 d_2 d_3 d_4 + 2 s_3 d_3 d_4 d_5 + 2 s_4 d_4 d_5 d_1 + 2 s_5 d_5 d_1 d_2\\
& + 2 s_1 d_3 d_4 d_5 + 2 s_2 d_4 d_5 d_1 + 2 s_3 d_5 d_1 d_2 + 2 s_4 d_1 d_2 d_3 + 2 s_5 d_2 d_3 d_4\\
& + 2 d_1 d_2 d_3 d_4 + 2 d_2 d_3 d_4 d_5 + 2 d_3 d_4 d_5 d_1 + 2 d_4 d_5 d_1 d_2 + 2 d_5 d_1 d_2 d_3;
\end{aligned}\right\} \quad (130.)$$

they may therefore be expressed as functions of nine independent quantities; for example, of four lines and five angles, $r^{(1)}\ r_0^{(1)}\ r^{(2)}\ r_0^{(2)}$, $\theta^{(1)}\ \theta_0^{(1)}\ \theta^{(2)}\ \theta_0^{(2)}\ \iota$, on which they depend as follows:

$$\left.\begin{aligned}
s_1 &= r_0^{(1)\,2},\\
s_2 &= r_0^{(1)\,2} + r_0^{(2)\,2} - 2\, r_0^{(1)}\, r_0^{(2)} (\cos\theta_0^{(1)} \cos\theta_0^{(2)} + \sin\theta_0^{(1)} \sin\theta_0^{(2)} \cos\iota),\\
s_3 &= r^{(2)\,2} + r_0^{(2)\,2} - 2\, r^{(2)}\, r_0^{(2)} \cos(\theta^{(2)} - \theta_0^{(2)}),\\
s_4 &= r^{(2)\,2} + r^{(1)\,2} - 2\, r^{(2)}\, r^{(1)} (\cos\theta^{(1)} \cos\theta^{(2)} + \sin\theta^{(1)} \sin\theta^{(2)} \cos\iota),\\
s_5 &= r^{(1)\,2},\\
d_1 &= r_0^{(2)\,2},\\
d_2 &= r^{(2)\,2} + r_0^{(1)\,2} - 2\, r^{(2)}\, r_0^{(1)} (\cos\theta^{(2)} \cos\theta_0^{(1)} + \sin\theta^{(2)} \sin\theta_0^{(1)} \cos\iota),\\
d_3 &= r_0^{(2)\,2} + r^{(1)\,2} - 2\, r_0^{(2)}\, r^{(1)} (\cos\theta_0^{(2)} \cos\theta^{(1)} + \sin\theta_0^{(2)} \sin\theta^{(1)} \cos\iota),\\
d_4 &= r^{(2)\,2},\\
d_5 &= r^{(1)\,2} + r_0^{(1)\,2} - 2\, r^{(1)}\, r_0^{(1)} \cos(\theta^{(1)} - \theta_0^{(1)}),
\end{aligned}\right\} \quad (131.)$$

the two line-symbols $r^{(1)}\ r^{(2)}$ denoting, for abridgement, the same two final radii vectores which were before denoted by $r^{(1,\,3)}\ r^{(2,\,3)}$, and $r_0^{(1)}\ r_0^{(2)}$ representing the initial values of these radii; while $\theta^{(1)}\ \theta^{(2)}\ \theta_0^{(1)}\ \theta_0^{(2)}$ are angles made by these four radii, with the line of intersection of the two planes $r_0^{(1)}\ r^{(1)}$, $r_0^{(2)}\ r^{(2)}$; and ι is the inclination of those two planes to each other. We may therefore consider the characteristic function $V_{\prime}$ of relative motion, for any ternary system, as depending only on these latter lines and angles, along with the quantity $H_{\prime}$.

The reasoning which it has been thought useful to develope here, for any system of three points, attracting or repelling one another according to any functions of their distances, was alluded to, under a more general form, in the twelfth number of this essay; and shows, for example, that the characteristic function of relative motion in a system of four such points, depends on the shape and size of a heptagon, and therefore only on the mutual distances of its seven corners, which are in number $\left(\frac{7 \times 6}{2} =\right)$ 21, but are connected by six equations of condition, leaving only fifteen independent. It is easy to extend these remarks to any multiple system.

General method of improving an approximate expression for the Characteristic Function of motion of a System in any problem of Dynamics.

19. The partial differential equation (F.), which the characteristic function V must satisfy, in every dynamical question, may receive some useful general transformations, by the separation of this function V into any two parts

$$V_1 + V_2 = V. \qquad \text{(U}^4.)$$

For if we establish, for abridgement, the two following equations of definition,

$$\left.\begin{aligned} T_1 &= \Sigma \cdot \frac{1}{2m}\left(\left(\frac{\delta V_1}{\delta x}\right)^2 + \left(\frac{\delta V_1}{\delta y}\right)^2 + \left(\frac{\delta V_1}{\delta z}\right)^2\right), \\ T_2 &= \Sigma \cdot \frac{1}{2m}\left(\left(\frac{\delta V_2}{\delta x}\right)^2 + \left(\frac{\delta V_2}{\delta y}\right)^2 + \left(\frac{\delta V_2}{\delta z}\right)^2\right), \end{aligned}\right\} \qquad \text{(V}^4.)$$

analogous to the relation

$$T = \Sigma \cdot \frac{1}{2m}\left(\left(\frac{\delta V}{\delta x}\right)^2 + \left(\frac{\delta V}{\delta y}\right)^2 + \left(\frac{\delta V}{\delta z}\right)^2\right), \qquad \text{(W}^4.)$$

which served to transform the law of living force into the partial differential equation (F.); we shall have, by (U^4.),

$$T = T_1 + T_2 + \Sigma \cdot \frac{1}{m}\left(\frac{\delta V_1}{\delta x}\frac{\delta V_2}{\delta x} + \frac{\delta V_1}{\delta y}\frac{\delta V_2}{\delta y} + \frac{\delta V_1}{\delta z}\frac{\delta V_2}{\delta z}\right); \qquad \text{(X}^4.)$$

and this expression may be further transformed by the help of the formula (C.), or by the law of varying action. For that law gives the following symbolic equation,

$$\Sigma \cdot \frac{1}{m}\left(\frac{\delta V}{\delta x}\frac{\delta}{\delta x} + \frac{\delta V}{\delta y}\frac{\delta}{\delta y} + \frac{\delta V}{\delta z}\frac{\delta}{\delta z}\right) = \frac{d}{dt}, \qquad \text{(Y}^4.)$$

the symbols in both members being prefixed to any one function of the varying coordinates of a system, not expressly involving the time; it gives therefore by (U^4.), (V^4.),

$$\Sigma \cdot \frac{1}{m}\left(\frac{\delta V_1}{\delta x}\frac{\delta V_2}{\delta x} + \frac{\delta V_1}{\delta y}\frac{\delta V_2}{\delta y} + \frac{\delta V_1}{\delta z}\frac{\delta V_2}{\delta z}\right) = \frac{dV_2}{dt} - 2\,T_2. \qquad \text{(Z}^4.)$$

In this manner we find the following general and rigorous transformation of the equation (F.),

$$\frac{dV_2}{dt} = T - T_1 + T_2; \qquad \text{(A}^5.)$$

T being here retained for the sake of symmetry and conciseness, instead of the equal expression U + H. And if we suppose, as we may, that the part V_1, like the whole function V, is chosen so as to vanish with the time, then the other part V_2 will also have that property, and may be expressed by the definite integral,

$$V_2 = \int_0^t (T - T_1 + T_2)\, dt. \qquad \text{(B}^5.)$$

More generally, if we employ the principles of the seventh number, and introduce any $3n$ marks $\eta_1, \eta_2, \ldots \eta_{3n}$, of the varying positions of the n points of any system, (whether they be the rectangular coordinates themselves, or any functions of them,) we shall have

$$T = F\left(\frac{\delta V}{\delta \eta_1}, \frac{\delta V}{\delta \eta_2}, \cdots \frac{\delta V}{\delta \eta_{3n}}\right), \qquad \text{(C}^5.)$$

and may establish by analogy the two following equations of definition,

$$\left.\begin{aligned} T_1 &= F\left(\frac{\delta V_1}{\delta \eta_1}, \frac{\delta V_1}{\delta \eta_2}, \ldots \frac{\delta V_1}{\delta \eta_{3n}}\right), \\ T_2 &= F\left(\frac{\delta V_2}{\delta \eta_1}, \frac{\delta V_2}{\delta \eta_2}, \ldots \frac{\delta V_2}{\delta \eta_{3n}}\right), \end{aligned}\right\} \quad \text{(D}^5.)$$

the function F being always rational and integer, and homogeneous of the second dimension; and being therefore such that (besides other properties)

$$T = T_1 + T_2 + \frac{\delta T_1}{\delta \frac{\delta V_1}{\delta \eta_1}} \frac{\delta V_2}{\delta \eta_1} + \frac{\delta T_1}{\delta \frac{\delta V_1}{\delta \eta_2}} \frac{\delta V_2}{\delta \eta_2} + \ldots + \frac{\delta T_1}{\delta \frac{\delta V_1}{\delta \eta_{3n}}} \frac{\delta V_2}{\delta \eta_{3n}}, \quad \text{(E}^5.)$$

$$\frac{\delta T}{\delta \frac{\delta V}{\delta \eta_1}} = \frac{\delta T_1}{\delta \frac{\delta V_1}{\delta \eta_1}} + \frac{\delta T_2}{\delta \frac{\delta V_2}{\delta \eta_1}}, \ldots \frac{\delta T}{\delta \frac{\delta V}{\delta \eta_{3n}}} = \frac{\delta T_1}{\delta \frac{\delta V_1}{\delta \eta_{3n}}} + \frac{\delta T_2}{\delta \frac{\delta V_2}{\delta \eta_{3n}}}, \quad \text{(F}^5.)$$

and

$$\frac{\delta T_2}{\delta \frac{\delta V_2}{\delta \eta_1}} \frac{\delta V_2}{\delta \eta_1} + \frac{\delta T_2}{\delta \frac{\delta V_2}{\delta \eta_2}} \frac{\delta V_2}{\delta \eta_2} + \ldots + \frac{\delta T_2}{\delta \frac{\delta V_2}{\delta \eta_{3n}}} \frac{\delta V_2}{\delta \eta_{3n}} = 2\, T_2. \quad \text{(G}^5.)$$

By the principles of the eighth number, we have also,

$$\frac{\delta T}{\delta \frac{\delta V}{\delta \eta_1}} = \eta'_1, \frac{\delta T}{\delta \frac{\delta V}{\delta \eta_2}} = \eta'_2, \ldots \frac{\delta T}{\delta \frac{\delta V}{\delta \eta_{3n}}} = \eta'_{3n}; \quad \text{(H}^5.)$$

and since the meanings of $\eta'_1, \ldots \eta'_{3n}$, give evidently the symbolical equation,

$$\eta'_1 \frac{\delta}{\delta \eta_1} + \eta'_2 \frac{\delta}{\delta \eta_2} + \ldots + \eta'_{3n} \frac{\delta}{\delta \eta_{3n}} = \frac{d}{dt}, \quad \text{(I}^5.)$$

we see that the equation (A^5.) still holds with the present more general marks of position of a moving system, and gives still the expression (B^5.), supposing only, as before, that the two parts of the whole characteristic function are chosen so as to vanish with the time.

It may not at first sight appear, that this rigorous transformation (B^5.), of the partial differential equation (F.), or of the analogous equation (T.) with coordinates not rectangular, is likely to assist much in discovering the form of the part V_2 of the characteristic function V, (the other part V_1 being supposed to have been previously assumed;) because it involves under the sign of integration, in the term T_2, the partial differential coefficients of the sought part V_2. But if we observe that these unknown coefficients enter only by their squares and products, we shall perceive that it offers a general method of improving an approximation in any problem of dynamics. For if the first part V_1 be an approximate value of the whole sought function V, the second part V_2 will be small, and the term T_2 will not only be also small, but will be in general of a higher order of smallness; we shall therefore in general improve an approximate value V_1 of the characteristic function V, by adding to it the definite integral,

$$V_2 = \int_0^t (T - T_1)\, dt; \quad \ldots \ldots \ldots \quad (K^5.)$$

though this is not, like (B^5.), a perfectly rigorous expression for the remaining part of the function. And in calculating this integral (K^5.), for the improvement of an approximation V_1, we may employ the following analogous approximations to the rigorous formulæ (D.) and (E.),

$$\left.\begin{array}{l} \frac{\delta V_1}{\delta a_1} = - m_1 a'_1; \ \frac{\delta V_1}{\delta a_2} = - m_2 a'_2; \ldots \frac{\delta V_1}{\delta a_n} = - m_n a'_n; \\ \frac{\delta V_1}{\delta b_1} = - m_1 b'_1; \ \frac{\delta V_1}{\delta b_2} = - m_2 b'_2; \ldots \frac{\delta V_1}{\delta b_n} = - m_n b'_n; \\ \frac{\delta V_1}{\delta c_1} = - m_1 c'_1; \ \frac{\delta V_1}{\delta c_2} = - m_2 c'_2; \ldots \frac{\delta V_1}{\delta c_n} = - m_n c'_n; \end{array}\right\} \quad \ldots \ldots \quad (L^5.)$$

and

$$\frac{\delta V_1}{\delta H} = t; \quad \ldots \ldots \ldots \quad (M^5.)$$

or with any other marks of final and initial position, (instead of rectangular coordinates,) the following approximate forms of the rigorous equations (S.),

$$\frac{\delta V_1}{\delta e_1} = - \frac{\delta T_0}{\delta e'_1}, \ \frac{\delta V_1}{\delta e_2} = - \frac{\delta T_0}{\delta e'_2}, \ldots \frac{\delta V_1}{\delta e_{3n}} = - \frac{\delta T_0}{\delta e'_{3n}}, \quad \ldots \ldots \quad (N^5.)$$

together with the formula (M^5.); by which new formulæ the manner of motion of the system is approximately though not rigorously expressed.

It is easy to extend these remarks to problems of relative motion, and to show that in such problems we have the rigorous transformation

$$V_{,2} = \int_0^t (T_{,} - T_{,1} + T_{,2})\, dt, \quad \ldots \ldots \ldots \quad (O^5.)$$

and the approximate expression

$$V_{,2} = \int_0^t (T_{,} - T_{,1})\, dt, \quad \ldots \ldots \ldots \quad (P^5.)$$

$V_{,1}$ being any approximate value of the function $V_{,}$ of relative motion, and $V_{,2}$ being the correction of this value; and $T_{,1}, T_{,2}$, being homogeneous functions of the second dimension, composed of the partial differential coefficients of these two parts $V_{,1}, V_{,2}$, in the same way as $T_{,}$ is composed of the coefficients of the whole function $V_{,}$. These general remarks may usefully be illustrated by a particular but extensive application.

Application of the foregoing method to the case of a Ternary or Multiple System, with any laws of attraction or repulsion, and with one predominant mass.

20. The value (68.), for the relative living force $2\,T_{,}$ of a system, reduces itself successively to the following parts, $2\,T_{,}^{(1)}, 2\,T_{,}^{(2)}, \ldots 2\,T_{,}^{(n-1)}$, when we suppose that

all the $n-1$ first masses vanish, with the exception of each successively; namely, to the part

$$2\,T_{\prime}^{(1)} = \frac{m_1 m_n}{m_1+m_n}\,(\xi'^2_1 + \eta'^2_1 + \zeta'^2_1), \quad \ldots \ldots \quad (132.)$$

when only m_1, m_n, do not vanish; the part

$$2\,T_{\prime}^{(2)} = \frac{m_2 m_n}{m_2+m_n}\,(\xi'^2_2 + \eta'^2_2 + \zeta'^2_2), \quad \ldots \ldots \quad (133.)$$

when all but m_2, m_n, vanish; and so on, as far as the part

$$2\,T_{\prime}^{(n-1)} = \frac{m_{n-1} m_n}{m_{n-1}+m_n}\,(\xi'^2_{n-1} + \eta'^2_{n-1} + \zeta'^2_{n-1}), \quad \ldots \quad (134.)$$

which remains, when only the two last masses are retained. The sum of these $n-1$ parts is not, in general, equal to the whole relative living force $2\,T_{\prime}$ of the system, with all the n masses retained; but it differs little from that whole when the first $n-1$ masses are small in comparison with the last mass m_n; for the rigorous value of this difference is, by (68.), and by (132.) (133.) (134.),

$$\left.\begin{array}{l} 2\,T_{\prime} - 2\,T_{\prime}^{(1)} - 2\,T_{\prime}^{(2)} - \ldots - 2\,T_{\prime}^{(n-1)} = \\ \frac{2\,m_1}{m_n}(T_{\prime}^{(1)} - T_{\prime}) + \frac{2\,m_2}{m_n}(T_{\prime}^{(2)} - T_{\prime}) + \ldots + \frac{2\,m_{n-1}}{m_n}(T_{\prime}^{(n-1)} - T_{\prime}) \\ + \frac{1}{m_n}\Sigma_{\prime} \,.\, m_i m_k \left\{(\xi'_i - \xi'_k)^2 + (\eta'_i - \eta'_k)^2 + (\zeta'_i - \zeta'_k)^2\right\}: \end{array}\right\} \quad (135.)$$

an expression which is small of the second order when the $n-1$ first masses are small of the first order. If, then, we denote by $V_{\prime}^{(1)}$, $V_{\prime}^{(2)}$, ... $V_{\prime}^{(n-1)}$, the relative actions, or accumulated relative living forces, such as they would be in the $n-1$ binary systems, $(m_1\, m_n)$, $(m_2\, m_n)$, ... $(m_{n-1}\, m_n)$, without the perturbations of the other small masses of the entire multiple system of n points; so that

$$V_{\prime}^{(1)} = \int_0^t 2\,T_{\prime}^{(1)}\,dt,\ V_{\prime}^{(2)} = \int_0^t 2\,T_{\prime}^{(2)}\,dt, \ldots V_{\prime}^{(n-1)} = \int_0^t 2\,T_{\prime}^{(n-1)}\,dt, \quad (Q^5.)$$

the perturbations being neglected in calculating these $n-1$ definite integrals; we shall have, as an approximate value for the whole relative action $V_{\prime}$ of the system, the sum $V_{\prime 1}$ of its values for these separate binary systems,

$$V_{\prime 1} = V_{\prime}^{(1)} + V_{\prime}^{(2)} + \ldots + V_{\prime}^{(n-1)}. \quad \ldots \ldots \quad (R^5)$$

This sum, by our theory of binary systems, may be otherwise expressed as follows:

$$V_{\prime 1} = \frac{m_1 m_n\, w^{(1)}}{m_1 + m_n} + \frac{m_2 m_n\, w^{(2)}}{m_2 + m_n} + \ldots + \frac{m_{n-1} m_n\, w^{(n)}}{m_{n-1} + m_n}, \quad \ldots \quad (S^5.)$$

if we put for abridgement

$$\left.\begin{aligned}
w^{(1)} &= h^{(1)}\,\vartheta^{(1)} + \int_{r_0^{(1)}}^{r^{(1)}} r'^{(1)}\, d\, r^{(1)},\\
w^{(2)} &= h^{(2)}\,\vartheta^{(2)} + \int_{r_0^{(2)}}^{r^{(2)}} r'^{(2)}\, d\, r^{(2)},\\
&\dots\\
w^{(n-1)} &= h^{(n-1)}\,\vartheta^{(n-1)} + \int_{r_0^{(n-1)}}^{r^{(n-1)}} r'^{(n-1)}\, d\, r^{(n-1)}.
\end{aligned}\right\} \dots \quad (\text{T}^5.)$$

In this expression,

$$\left.\begin{aligned}
r'^{(1)} &= \pm\sqrt{2\,(m_1 + m_n) f^{(1)} + 2\, g^{(1)} - \frac{h^{(1)^2}}{r^{(1)^2}}},\\
&\dots\\
r'^{(n-1)} &= \pm\sqrt{2\,(m_{n-1} + m_n) f^{(n-1)} + 2\, g^{(n-1)} - \frac{h^{(n-1)^2}}{r^{(n-1)^2}}},
\end{aligned}\right\} \dots \quad (\text{U}^5.)$$

$r^{(1)}, \dots r^{(n-1)}$, being abridged expressions for the distances $r^{(1,n)}, \dots r^{(n-1,n)}$, and $f^{(1)}, \dots f^{(n-1)}$, being abridgements for the functions $f^{(1,n)}, \dots f^{(n-1,n)}$, of these distances, of which the derivatives, according as they are negative or positive, express the laws of attraction or repulsion: we have also introduced $2\,n - 2$ auxiliary quantities $h^{(1)}\, g^{(1)} \dots h^{(n-1)}\, g^{(n-1)}$, to be eliminated or determined by the following equations of condition:

$$\left.\begin{aligned}
0 &= \vartheta^{(1)} + \int_{r_0^{(1)}}^{r^{(1)}} \frac{\delta\, r'^{(1)}}{\delta\, h^{(1)}}\, d\, r^{(1)},\\
0 &= \vartheta^{(2)} + \int_{r_0^{(2)}}^{r^{(2)}} \frac{\delta\, r'^{(2)}}{\delta\, h^{(2)}}\, d\, r^{(2)},\\
&\dots\dots\\
0 &= \vartheta^{(n-1)} + \int_{r_0^{(n-1)}}^{r^{(n-1)}} \frac{\delta\, r'^{(n-1)}}{\delta\, h^{(n-1)}}\, d\, r^{(n-1)},
\end{aligned}\right\} \dots \quad (\text{V}^5.)$$

and

$$\int_{r_0^{(1)}}^{r^{(1)}} \frac{d\, r^{(1)}}{r'^{(1)}} = \int_{r_0^{(2)}}^{r^{(2)}} \frac{d\, r^{(2)}}{r'^{(2)}} = \dots = \int_{r_0^{(n-1)}}^{r^{(n-1)}} \frac{d\, r^{(n-1)}}{r'^{(n-1)}}, \quad \dots \quad (\text{W}^5)$$

or

$$\frac{\delta\, w^{(1)}}{\delta\, g^{(1)}} = \frac{\delta\, w^{(2)}}{\delta\, g^{(2)}} = \dots = \frac{\delta\, w^{(n-1)}}{\delta\, g^{(n-1)}}, \quad \dots \quad (\text{X}^5.)$$

along with this last condition,

$$\frac{m_1\, g^{(1)}}{m_1 + m_n} + \frac{m_2\, g^{(2)}}{m_2 + m_n} + \frac{m_3\, g^{(3)}}{m_3 + m_n} + \dots + \frac{m_{n-1}\, g^{(n-1)}}{m_{n-1} + m_n} = \frac{\text{H}_{\prime}}{m_n}; \quad \dots \quad (\text{Y}^5.)$$

and we have denoted by $\vartheta^{(1)}, \dots \vartheta^{(n-1)}$, the angles which the final distances $r^{(1)}, \dots r^{(n-1)}$, of the first $n-1$ points from the last or nth point of the system, make respectively with the initial distances corresponding, namely, $r_0^{(1)}, \dots r_0^{(n-1)}$. The variation of the sum $V_{,1}$ is, by (S^5.),

$$\delta V_{,1} = \frac{m_1 m_n \delta \varpi^{(1)}}{m_1 + m_n} + \frac{m_2 m_n \delta \varpi^{(2)}}{m_2 + m_n} + \dots + \frac{m_{n-1} m_n \delta \varpi^{(n-1)}}{m_{n-1} + m_n}; \quad \text{(Z}^5.)$$

in which, by the equations of condition, we may treat all the auxiliary quantities $h^{(1)} g^{(1)} \dots h^{(n-1)} g^{(n-1)}$ as constant, if $H_{,}$ be considered as given: so that the part of this variation $\delta V_{,1}$, which depends on the variations of the final relative coordinates, may be put under the form,

$$\left.\begin{aligned} \delta_{\xi,\eta,\zeta} V_{,1} = {} & \frac{m_1 m_n}{m_1+m_n}\left(\frac{\delta \varpi^{(1)}}{\delta \xi_1}\delta\xi_1 + \frac{\delta \varpi^{(1)}}{\delta \eta_1}\delta\eta_1 + \frac{\delta \varpi^{(1)}}{\delta \zeta_1}\delta\zeta_1\right) \\ & + \frac{m_2 m_n}{m_2+m_n}\left(\frac{\delta \varpi^{(2)}}{\delta \xi_2}\delta\xi_2 + \frac{\delta \varpi^{(2)}}{\delta \eta_2}\delta\eta_2 + \frac{\delta \varpi^{(2)}}{\delta \zeta_2}\delta\zeta_2\right) \\ + \dots & \\ + \frac{m_{n-1} m_n}{m_{n-1}+m_n} & \left(\frac{\delta \varpi^{(n-1)}}{\delta \xi_{n-1}}\delta\xi_{n-1} + \frac{\delta \varpi^{(n-1)}}{\delta \eta_{n-1}}\delta\eta_{n-1} + \frac{\delta \varpi^{(n-1)}}{\delta \zeta_{n-1}}\delta\zeta_{n-1}\right). \end{aligned}\right\} \quad \text{(A}^6.)$$

By the equations (T^5.) (U^5.), or by the theory of binary systems, we have, rigorously,

$$\left.\begin{aligned} &\left(\frac{\delta \varpi^{(1)}}{\delta \xi_1}\right)^2 + \left(\frac{\delta \varpi^{(1)}}{\delta \eta_1}\right)^2 + \left(\frac{\delta \varpi^{(1)}}{\delta \zeta_1}\right)^2 = 2\,(m_1 + m_n) f^{(1)} + 2 g^{(1)}; \\ &\left(\frac{\delta \varpi^{(2)}}{\delta \xi_2}\right)^2 + \left(\frac{\delta \varpi^{(2)}}{\delta \eta_2}\right)^2 + \left(\frac{\delta \varpi^{(2)}}{\delta \zeta_2}\right)^2 = 2\,(m_2 + m_n) f^{(2)} + 2 g^{(2)}; \\ &\dots\dots \\ &\left(\frac{\delta \varpi^{(n-1)}}{\delta \xi_{n-1}}\right)^2 + \left(\frac{\delta \varpi^{(n-1)}}{\delta \eta_{n-1}}\right)^2 + \left(\frac{\delta \varpi^{(n-1)}}{\delta \zeta_{n-1}}\right)^2 = 2\,(m_{n-1} + m_n) f^{(n-1)} + 2 g^{(n-1)}; \end{aligned}\right\} \text{(B}^6.)$$

and the rigorous law of relative living force for the whole multiple system, is

$$T_{,} = U + H_{,}, \quad \dots \quad (50.)$$

in which

$$U = m_n (m_1 f^{(1)} + m_2 f^{(2)} + \dots + m_{n-1} f^{(n-1)}) + \Sigma_{,} \,.\, m_i m_k f^{(i,k)}, \quad \text{(C}^6.)$$

and

$$\left.\begin{aligned} T_{,} = {} & \tfrac{1}{2}\left(\frac{1}{m_1} + \frac{1}{m_n}\right)\left\{\left(\frac{\delta V_{,}}{\delta \xi_1}\right)^2 + \left(\frac{\delta V_{,}}{\delta \eta_1}\right)^2 + \left(\frac{\delta V_{,}}{\delta \zeta_1}\right)^2\right\} \\ & + \tfrac{1}{2}\left(\frac{1}{m_2} + \frac{1}{m_n}\right)\left\{\left(\frac{\delta V_{,}}{\delta \xi_2}\right)^2 + \left(\frac{\delta V_{,}}{\delta \eta_2}\right)^2 + \left(\frac{\delta V_{,}}{\delta \zeta_2}\right)^2\right\} \\ + \dots & + \tfrac{1}{2}\left(\frac{1}{m_{n-1}} + \frac{1}{m_n}\right)\left\{\left(\frac{\delta V_{,}}{\delta \xi_{n-1}}\right)^2 + \left(\frac{\delta V_{,}}{\delta \eta_{n-1}}\right)^2 + \left(\frac{\delta V_{,}}{\delta \zeta_{n-1}}\right)^2\right\} \\ & + \frac{1}{m_n}\Sigma_{,}\left(\frac{\delta V_{,}}{\delta \xi_i}\frac{\delta V_{,}}{\delta \xi_k} + \frac{\delta V_{,}}{\delta \eta_i}\frac{\delta V_{,}}{\delta \eta_k} + \frac{\delta V_{,}}{\delta \zeta_i}\frac{\delta V_{,}}{\delta \zeta_k}\right). \end{aligned}\right\} \quad \text{(D}^6.)$$

We have therefore, by changing in this last expression the coefficients of the characteristic function $V_{,}$ to those of its first part $V_{,1}$, and by attending to the foregoing equations,

$$T_{,1} = m_n \Sigma_{,} . m_i f^{(i)} + H_{,} + m_n \Sigma_{,} . \frac{m_i}{m_n + m_i} \frac{m_k}{m_n + m_k} \left(\frac{\delta \varpi^{(i)}}{\delta \xi_i} \frac{\delta \varpi^{(k)}}{\delta \xi_k} + \frac{\delta \varpi^{(i)}}{\delta \eta_i} \frac{\delta \varpi^{(k)}}{\delta \eta_k} + \frac{\delta \varpi^{(i)}}{\delta \zeta_i} \frac{\delta \varpi^{(k)}}{\delta \zeta_k} \right); \quad (E^6.)$$

and consequently

$$T_{,} - T_{,1} = \Sigma_{,} . m_i m_k \left\{ f^{(i,k)} - \frac{m_n}{(m_n + m_i)(m_n + m_k)} \left(\frac{\delta \varpi^{(i)}}{\delta \xi_i} \frac{\delta \varpi^{(k)}}{\delta \xi_k} + \frac{\delta \varpi^{(i)}}{\delta \eta_i} \frac{\delta \varpi^{(k)}}{\delta \eta_k} + \frac{\delta \varpi^{(i)}}{\delta \zeta_i} \frac{\delta \varpi^{(k)}}{\delta \zeta_k} \right) \right\}. \quad (F^6.)$$

The general transformation of the foregoing number gives therefore, rigorously, for the remaining part $V_{,2}$ of the characteristic function $V_{,}$ of relative motion of the multiple system, the equation

$$V_{,2} = \int_0^t T_{,2}\, dt + \Sigma_{,} . m_i m_k \int_0^t \left\{ f^{(i,k)} - \frac{\frac{\delta \varpi^{(i)}}{\delta \xi_i} \frac{\delta \varpi^{(k)}}{\delta \xi_k} + \frac{\delta \varpi^{(i)}}{\delta \eta_i} \frac{\delta \varpi^{(k)}}{\delta \eta_k} + \frac{\delta \varpi^{(i)}}{\delta \zeta_i} \frac{\delta \varpi^{(k)}}{\delta \zeta_k}}{\frac{1}{m_n}(m_n + m_i)(m_n + m_k)} \right\} dt; \quad (G^6.)$$

and, approximately, the expression

$$V_{,2} = \Sigma_{,} . m_i m_k \int_0^t \left\{ f^{(i,k)} - \frac{1}{m_n} (\xi'_i \xi'_k + \eta'_i \eta'_k + \zeta'_i \zeta'_k) \right\} dt : \quad . \quad . \quad . \quad (H^6.)$$

with which last expression we may combine the following approximate formulæ belonging in rigour to binary systems only,

$$\xi'_i = \frac{\delta \varpi^{(i)}}{\delta \xi_i}, \quad \eta'_i = \frac{\delta \varpi^{(i)}}{\delta \eta_i}, \quad \zeta'_i = \frac{\delta \varpi^{(i)}}{\delta \zeta_i}, \quad . \quad . \quad . \quad . \quad (I^6.)$$

$$\alpha'_i = \frac{\delta \varpi^{(i)}}{\delta \alpha_i}, \quad \beta'_i = - \frac{\delta \varpi^{(i)}}{\delta \beta_i}, \quad \gamma'_i = - \frac{\delta \varpi^{(i)}}{\delta \gamma_i}, \quad . \quad . \quad . \quad . \quad (K^6.)$$

and

$$t = \frac{\delta \varpi^{(i)}}{\delta g^{(i)}}. \quad . \quad . \quad . \quad . \quad (L^6.)$$

We have also, rigorously, for binary systems, the following differential equations of motion of the second order,

$$\xi''_i = (m_n + m_i) \frac{\delta f^{(i)}}{\delta \xi_i}; \quad \eta''_i = (m_n + m_i) \frac{\delta f^{(i)}}{\delta \eta_i}; \quad \zeta''_i = (m_n + m_i) \frac{\delta f^{(i)}}{\delta \zeta_i}; \quad . \quad . \quad (M^6.)$$

which enable us to transform in various ways the approximate expression (H^6.). Thus, in the case of a ternary system, with any laws of attraction or repulsion, but with one predominant mass m_3, the *disturbing part* $V_{,2}$ of the characteristic function $V_{,}$ of relative motion, may be put under the form

$$V_{,2} = m_1 m_2 W, \quad . \quad . \quad . \quad . \quad (N^6.)$$

in which the coefficient W may approximately be expressed as follows:

$$W = \int_0^t \left\{ f^{(1,2)} - \frac{1}{m_3} (\xi'_1 \xi'_2 + \eta'_1 \eta'_2 + \zeta'_1 \zeta'_2) \right\} dt, \quad . \quad . \quad . \quad . \quad (O^6.)$$

or thus:

$$\left.\begin{array}{l} W = \int_0^t \left(f^{(1,2)} + \xi_2 \frac{\delta f^{(1)}}{\delta \xi_1} + \eta_2 \frac{\delta f^{(1)}}{\delta \eta_1} + \zeta_2 \frac{\delta f^{(1)}}{\delta \zeta_1} \right) dt \\ - \frac{1}{m_3} \left(\xi_2 \frac{\delta \varpi^{(1)}}{\delta \xi_1} + \eta_2 \frac{\delta \varpi^{(1)}}{\delta \eta_1} + \zeta_2 \frac{\delta \varpi^{(1)}}{\delta \zeta_1} + \alpha_2 \frac{\delta \varpi^{(1)}}{\delta \alpha_1} + \beta_2 \frac{\delta \varpi^{(1)}}{\delta \beta_1} + \gamma_2 \frac{\delta \varpi^{(1)}}{\delta \gamma_1} \right), \end{array}\right\} . \quad (P^6.)$$

or finally,

$$\left.\begin{array}{l} W = \int_0^t \left(f^{(1,2)} + \xi_1 \frac{\delta f^{(2)}}{\delta \xi_2} + \eta_1 \frac{\delta f^{(2)}}{\delta \eta_2} + \zeta_1 \frac{\delta f^{(2)}}{\delta \zeta_2} \right) dt \\ - \frac{1}{m_3} \left(\xi_1 \frac{\delta \varpi^{(2)}}{\delta \xi_2} + \eta_1 \frac{\delta \varpi^{(2)}}{\delta \eta_2} + \zeta_1 \frac{\delta \varpi^{(2)}}{\delta \zeta_2} + \alpha_1 \frac{\delta \varpi^{(2)}}{\delta \alpha_2} + \beta_1 \frac{\delta \varpi^{(2)}}{\delta \beta_2} + \gamma_1 \frac{\delta \varpi^{(2)}}{\delta \gamma_2} \right). \end{array}\right\} \quad (Q^6.)$$

In general, for a multiple system, we may put

$$V_{,2} = \Sigma_{,} . m_i m_k W^{(i,k)} ; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (R^6.)$$

and approximately,

$$\left.\begin{array}{l} W^{(i,k)} = \int_0^t \left(f^{(i,k)} + \xi_k \frac{\delta f^{(i)}}{\delta \xi_i} + \eta_k \frac{\delta f^{(i)}}{\delta \eta_i} + \zeta_k \frac{\delta f^{(i)}}{\delta \zeta_i} \right) dt \\ - \frac{1}{m_n} \left(\xi_k \frac{\delta \varpi^{(i)}}{\delta \xi_i} + \eta_k \frac{\delta \varpi^{(i)}}{\delta \eta_i} + \zeta_k \frac{\delta \varpi^{(i)}}{\delta \zeta_i} + \alpha_k \frac{\delta \varpi^{(i)}}{\delta \alpha_i} + \beta_k \frac{\delta \varpi^{(i)}}{\delta \beta_i} + \gamma_k \frac{\delta \varpi^{(i)}}{\delta \gamma_i} \right), \end{array}\right\} . \quad (S^6.)$$

or

$$\left.\begin{array}{l} W^{(i,k)} = \int_0^t \left(f^{(i,k)} + \xi_i \frac{\delta f^{(k)}}{\delta \xi_k} + \eta_i \frac{\delta f^{(k)}}{\delta \eta_k} + \zeta_i \frac{\delta f^{(k)}}{\delta \zeta_k} \right) dt \\ - \frac{1}{m_n} \left(\xi_i \frac{\delta \varpi^{(k)}}{\delta \xi_k} + \eta_i \frac{\delta \varpi^{(k)}}{\delta \eta_k} + \zeta_i \frac{\delta \varpi^{(k)}}{\delta \zeta_k} + \alpha_i \frac{\delta \varpi^{(k)}}{\delta \alpha_k} + \beta_i \frac{\delta \varpi^{(k)}}{\delta \beta_k} + \gamma_i \frac{\delta \varpi^{(k)}}{\delta \gamma_k} \right). \end{array}\right\} . \quad (T^6.)$$

Rigorous transition from the theory of Binary to that of Multiple Systems, by means of the disturbing part of the whole Characteristic Function; and approximate expressions for the perturbations.

21. The three equations (K^6.) when the auxiliary constant $g^{(i)}$ is eliminated by the formula (L^6.), are rigorously (by our theory) the three final integrals of the three known equations of the second order (M^6.), for the relative motion of the binary system ($m_i\ m_n$); and give, for such a system, the three varying relative coordinates $\xi_i\ \eta_i\ \zeta_i$, as functions of their initial values and initial rates of increase $\alpha_i\ \beta_i\ \gamma_i\ \alpha'_i\ \beta'_i\ \gamma'_i$, and of the time t. In like manner the three equations (I^6.), when $g^{(i)}$ is eliminated by (L^6.), are rigorously the three intermediate integrals of the same known differential equations of motion of the same binary system. These integrals, however, cease to be rigorous when we introduce the perturbations of the relative motion of this partial or binary system ($m_i\ m_n$), arising from the attractions or repulsions of the other points m_k, of the whole proposed multiple system; but they may be corrected and rendered rigorous by employing the remaining part $V_{,2}$ of the whole characteristic

function of relative motion $V_{,}$, along with the principal part or approximate value $V_{,1}$.

The equations (X^1.) (Y^1.) of the twelfth number, give rigorously

$$\xi'_i = \frac{1}{m_i}\frac{\delta V_{,}}{\delta \xi_i} + \frac{1}{m_n}\Sigma_{,}\frac{\delta V_{,}}{\delta \xi_i}, \quad \eta'_i = \frac{1}{m_i}\frac{\delta V_{,}}{\delta \eta_i} + \frac{1}{m_n}\Sigma_{,}\frac{\delta V_{,}}{\delta \eta_i}, \quad \zeta'_i = \frac{1}{m_i}\frac{\delta V_{,}}{\delta \zeta_i} + \frac{1}{m_n}\Sigma_{,}\frac{\delta V_{,}}{\delta \zeta_i}, \quad . \quad (U^6.)$$

and

$$-\alpha'_i = \frac{1}{m_i}\frac{\delta V_{,}}{\delta \alpha_i} + \frac{1}{m_n}\Sigma_{,}\frac{\delta V_{,}}{\delta \alpha_i}, \quad -\beta'_i = \frac{1}{m_i}\frac{\delta V_{,}}{\delta \beta_i} + \frac{1}{m_n}\Sigma_{,}\frac{\delta V_{,}}{\delta \beta_i}, \quad -\gamma'_i = \frac{1}{m_i}\frac{\delta V_{,}}{\delta \gamma_i} + \frac{1}{m_n}\Sigma_{,}\frac{\delta V_{,}}{\delta \gamma_i}, \quad . \quad (V^6.)$$

and therefore, by (A^6.),

$$\left.\begin{aligned}
\frac{\delta \varpi^{(i)}}{\delta \xi_i} &= \xi'_i - \Sigma_{,,} \cdot \frac{m_k}{m_k + m_n}\frac{\delta \varpi^{(k)}}{\delta \xi_k} - \frac{1}{m_i}\frac{\delta V_{,2}}{\delta \xi_i} - \frac{1}{m_n}\Sigma_{,}\frac{\delta V_{,2}}{\delta \xi_i},\\
\frac{\delta \varpi^{(i)}}{\delta \eta_i} &= \eta'_i - \Sigma_{,,} \cdot \frac{m_k}{m_k + m_n}\frac{\delta \varpi^{(k)}}{\delta \eta_k} - \frac{1}{m_i}\frac{\delta V_{,2}}{\delta \eta_i} - \frac{1}{m_n}\Sigma_{,}\frac{\delta V_{,2}}{\delta \eta_i},\\
\frac{\delta \varpi^{(i)}}{\delta \zeta_i} &= \zeta'_i - \Sigma_{,,} \cdot \frac{m_k}{m_k + m_n}\frac{\delta \varpi^{(k)}}{\delta \zeta_k} - \frac{1}{m_i}\frac{\delta V_{,2}}{\delta \zeta_i} - \frac{1}{m_n}\Sigma_{,}\frac{\delta V_{,2}}{\delta \zeta_i},
\end{aligned}\right\} \quad . \; . \; . \; . \quad (W^6.)$$

and similarly

$$\left.\begin{aligned}
-\frac{\delta \varpi^{(i)}}{\delta \alpha_i} &= \alpha'_i + \Sigma_{,,} \cdot \frac{m_k}{m_k + m_n}\frac{\delta \varpi^{(k)}}{\delta \alpha_k} + \frac{1}{m_i}\frac{\delta V_{,2}}{\delta \alpha_i} + \frac{1}{m_n}\Sigma_{,}\frac{\delta V_{,2}}{\delta \alpha_i},\\
-\frac{\delta \varpi^{(i)}}{\delta \beta_i} &= \beta'_i + \Sigma_{,,} \cdot \frac{m_k}{m_k + m_n}\frac{\delta \varpi^{(k)}}{\delta \beta_k} + \frac{1}{m_i}\frac{\delta V_{,2}}{\delta \beta_i} + \frac{1}{m_n}\Sigma_{,}\frac{\delta V_{,2}}{\delta \beta_i},\\
-\frac{\delta \varpi^{(i)}}{\delta \gamma_i} &= \gamma'_i + \Sigma_{,,} \cdot \frac{m_k}{m_k + m_n}\frac{\delta \varpi^{(k)}}{\delta \gamma_k} + \frac{1}{m_i}\frac{\delta V_{,2}}{\delta \gamma_i} + \frac{1}{m_n}\Sigma_{,}\frac{\delta V_{,2}}{\delta \gamma_i},
\end{aligned}\right\} \quad . \; . \; . \quad (X^6.)$$

the sign of summation $\Sigma_{,,}$ referring only to the disturbing masses m_k, to the exclusion of m_i and m_n; and these equations (W^6.) (X^6.) are the rigorous formulæ, corresponding to the approximate relations (I^6.) (K^6.). In like manner, the formula (L^6.) for the time of motion in a binary system, which is only an approximation when the system is considered as multiple, may be rigorously corrected for perturbation by adding to it an analogous term deduced from the disturbing part $V_{,2}$ of the whole characteristic function; that is, by changing it to the following:

$$t = \frac{\delta \varpi^{(i)}}{\delta g^{(i)}} + \frac{\delta V_{,2}}{\delta H_{,}}, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (Y^6.)$$

which gives, for this other coefficient of $\varpi^{(i)}$, the corrected and rigorous expression

$$\frac{\delta \varpi^{(i)}}{\delta g^{(i)}} = t - \frac{\delta V_{,2}}{\delta H_{,}}: \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (Z^6.)$$

$V_{,2}$ being here supposed so chosen as to be rigorously the correction of $V_{,1}$. If therefore by the theory of binary systems, or by eliminating $g^{(i)}$ between the four equations (K^6.) (L^6.), we have deduced expressions for the three varying relative coordinates $\xi_i\ \eta_i\ \zeta_i$ as functions of the time t, and of the six initial quantities $\alpha_i\ \beta_i\ \gamma_i\ \alpha'_i\ \beta'_i\ \gamma'_i$, which may be thus denoted,

$$\left.\begin{aligned} \xi_i &= \varphi_1 (\alpha_i, \beta_i, \gamma_i, \alpha'_i, \beta'_i, \gamma'_i, t), \\ \eta_i &= \varphi_2 (\alpha_i, \beta_i, \gamma_i, \alpha'_i, \beta'_i, \gamma'_i, t), \\ \zeta_i &= \varphi_3 (\alpha_i, \beta_i, \gamma_i, \alpha'_i, \beta'_i, \gamma'_i, t); \end{aligned}\right\} \quad \text{(A}^7.)$$

we shall know that the following relations are rigorously and *identically* true,

$$\left.\begin{aligned} \xi_i &= \varphi_1 \left(\alpha_i, \beta_i, \gamma_i, -\frac{\delta w^{(i)}}{\delta \alpha_i}, -\frac{\delta w^{(i)}}{\delta \beta_i}, -\frac{\delta w^{(i)}}{\delta \gamma_i}, \frac{\delta w^{(i)}}{\delta g^{(i)}}\right), \\ \eta_i &= \varphi_2 \left(\alpha_i, \beta_i, \gamma_i, -\frac{\delta w^{(i)}}{\delta \alpha_i}, -\frac{\delta w^{(i)}}{\delta \beta_i}, -\frac{\delta w^{(i)}}{\delta \gamma_i}, \frac{\delta w^{(i)}}{\delta g^{(i)}}\right), \\ \zeta_i &= \varphi_3 \left(\alpha_i, \beta_i, \gamma_i, -\frac{\delta w^{(i)}}{\delta \alpha_i}, -\frac{\delta w^{(i)}}{\delta \beta_i}, -\frac{\delta w^{(i)}}{\delta \gamma_i}, \frac{\delta w^{(i)}}{\delta g^{(i)}}\right), \end{aligned}\right\} \quad \text{(B}^7.)$$

and consequently that these relations will still be rigorously true when we substitute for the four coefficients of $w^{(i)}$ their rigorous values (X6.) and (Z6.) for the case of a multiple system. We may thus retain in rigour for any multiple system the final integrals (A7.) of the motion of a binary system, if only we add to the initial components $\alpha'_i\ \beta'_i\ \gamma'_i$ of relative velocity, and to the time t, the following perturbational terms:

$$\left.\begin{aligned} \Delta \alpha'_i &= \Sigma_{\prime\prime} \cdot \frac{m_k}{m_k + m_n} \frac{\delta w^{(k)}}{\delta \alpha_k} + \frac{1}{m_i} \frac{\delta \mathrm{V}_{\prime 2}}{\delta \alpha_i} + \frac{1}{m_n} \Sigma_{\prime} \frac{\delta \mathrm{V}_{\prime 2}}{\delta \alpha_i}, \\ \Delta \beta'_i &= \Sigma_{\prime\prime} \cdot \frac{m_k}{m_k + m_n} \frac{\delta w^{(k)}}{\delta \beta_k} + \frac{1}{m_i} \frac{\delta \mathrm{V}_{\prime 2}}{\delta \beta_i} + \frac{1}{m_n} \Sigma_{\prime} \frac{\delta \mathrm{V}_{\prime 2}}{\delta \beta_i}, \\ \Delta \gamma'_i &= \Sigma_{\prime\prime} \cdot \frac{m_k}{m_k + m_n} \frac{\delta w^{(k)}}{\delta \gamma_k} + \frac{1}{m_i} \frac{\delta \mathrm{V}_{\prime 2}}{\delta \gamma_i} + \frac{1}{m_n} \Sigma_{\prime} \frac{\delta \mathrm{V}_{\prime 2}}{\delta \gamma_i}, \end{aligned}\right\} \quad \text{(C}^7.)$$

and

$$\Delta t = -\frac{\delta \mathrm{V}_{\prime 2}}{\delta \mathrm{H}_{\prime}}. \quad \text{(D}^7.)$$

In the same way, if the theory of binary systems, or the elimination of $g^{(i)}$ between the four equations (I6.) (L6.), has given three intermediate integrals, of the form

$$\left.\begin{aligned} \xi'_i &= \psi_1 (\xi_i, \eta_i, \zeta_i, \alpha_i, \beta_i, \gamma_i, t), \\ \eta'_i &= \psi_2 (\xi_i, \eta_i, \zeta_i, \alpha_i, \beta_i, \gamma_i, t), \\ \zeta'_i &= \psi_3 (\xi_i, \eta_i, \zeta_i, \alpha_i, \beta_i, \gamma_i, t), \end{aligned}\right\} \quad \text{(E}^7.)$$

we can conclude that the following equations are rigorous and identical,

$$\left.\begin{aligned} \frac{\delta w^{(i)}}{\delta \xi_i} &= \psi_1 \left(\xi_i, \eta_i, \zeta_i, \alpha_i, \beta_i, \gamma_i, \frac{\delta w^{(i)}}{\delta g^{(i)}}\right), \\ \frac{\delta w^{(i)}}{\delta \eta_i} &= \psi_2 \left(\xi_i, \eta_i, \zeta_i, \alpha_i, \beta_i, \gamma_i, \frac{\delta w^{(i)}}{\delta g^{(i)}}\right), \\ \frac{\delta w^{(i)}}{\delta \zeta_i} &= \psi_3 \left(\xi_i, \eta_i, \zeta_i, \alpha_i, \beta_i, \gamma_i, \frac{\delta w^{(i)}}{\delta g^{(i)}}\right), \end{aligned}\right\} \quad \text{(F}^7.)$$

and must therefore be still true, when, in passing to a multiple system, we change the coefficients of $w^{(i)}$ to their rigorous values (W⁶.) (Z⁶.). The three intermediate integrals (E⁷.) of the motion of a binary system may therefore be adapted rigorously to the case of a multiple system, by first adding to the time t the perturbational term (D⁷.), and afterwards adding to the resulting values of the final components of relative velocity the terms

$$\left.\begin{aligned} \Delta \xi'_i &= \Sigma_{\prime\prime} \cdot \frac{m_k}{m_k + m_n} \frac{\delta w^{(k)}}{\delta \xi_k} + \frac{1}{m_i} \frac{\delta V_{\prime 2}}{\delta \xi_i} + \frac{1}{m_n} \Sigma_{\prime} \frac{\delta V_{\prime 2}}{\delta \xi_i}, \\ \Delta \eta'_i &= \Sigma_{\prime\prime} \cdot \frac{m_k}{m_k + m_n} \frac{\delta w^{(k)}}{\delta \eta_k} + \frac{1}{m_i} \frac{\delta V_{\prime 2}}{\delta \eta_i} + \frac{1}{m_n} \Sigma_{\prime} \frac{\delta V_{\prime 2}}{\delta \eta_i}, \\ \Delta \zeta'_i &= \Sigma_{\prime\prime} \cdot \frac{m_k}{m_k + m_n} \frac{\delta w^{(k)}}{\delta \zeta_k} + \frac{1}{m_i} \frac{\delta V_{\prime 2}}{\delta \zeta_i} + \frac{1}{m_n} \Sigma_{\prime} \frac{\delta V_{\prime 2}}{\delta \zeta_i}. \end{aligned}\right\} \quad \ldots \quad (G^7.)$$

22. To derive now, from these rigorous results, some useful approximate expressions, we shall neglect, in the perturbations, the terms which are of the second order, with respect to the small masses of the system, and with respect to the constant $2\,H_{\prime}$ of relative living force, which is easily seen to be small of the same order as the masses: and then the perturbations of the coordinates, deduced by the method that has been explained, become

$$\left.\begin{aligned} \Delta \xi_i &= \frac{\delta \xi_i}{\delta \alpha'_i} \Delta \alpha'_i + \frac{\delta \xi_i}{\delta \beta'_i} \Delta \beta'_i + \frac{\delta \xi_i}{\delta \gamma'_i} \Delta \gamma'_i + \frac{\delta \xi_i}{\delta t} \Delta t, \\ \Delta \eta_i &= \frac{\delta \eta_i}{\delta \alpha'_i} \Delta \alpha'_i + \frac{\delta \eta_i}{\delta \beta'_i} \Delta \beta'_i + \frac{\delta \eta_i}{\delta \gamma'_i} \Delta \gamma'_i + \frac{\delta \eta_i}{\delta t} \Delta t, \\ \Delta \zeta_i &= \frac{\delta \zeta_i}{\delta \alpha'_i} \Delta \alpha'_i + \frac{\delta \zeta_i}{\delta \beta'_i} \Delta \beta'_i + \frac{\delta \zeta_i}{\delta \gamma'_i} \Delta \gamma'_i + \frac{\delta \zeta_i}{\delta t} \Delta t, \end{aligned}\right\} \quad \ldots \quad (H^7.)$$

in which we may employ, instead of the rigorous values (C⁷.) for $\Delta \alpha'_i$, $\Delta \beta'_i$, $\Delta \gamma'_i$, the following approximate values:

$$\left.\begin{aligned} \Delta \alpha'_i &= \Sigma_{\prime\prime} \frac{m_k}{m_n} \frac{\delta w^{(k)}}{\delta \alpha_k} + \frac{1}{m_i} \frac{\delta V_{\prime 2}}{\delta \alpha_i}, \\ \Delta \beta'_i &= \Sigma_{\prime\prime} \frac{m_k}{m_n} \frac{\delta w^{(k)}}{\delta \beta_k} + \frac{1}{m_i} \frac{\delta V_{\prime 2}}{\delta \beta_i}, \\ \Delta \gamma'_i &= \Sigma_{\prime\prime} \frac{m_k}{m_n} \frac{\delta w^{(k)}}{\delta \gamma_k} + \frac{1}{m_i} \frac{\delta V_{\prime 2}}{\delta \gamma_i}. \end{aligned}\right\} \quad \ldots \ldots \quad (I^7.)$$

To calculate the four coefficients

$$\frac{\delta V_{\prime 2}}{\delta \alpha_i}, \quad \frac{\delta V_{\prime 2}}{\delta \beta_i}, \quad \frac{\delta V_{\prime 2}}{\delta \gamma_i}, \quad \frac{\delta V_{\prime 2}}{\delta H_{\prime}},$$

which enter into the values (I⁷.) (D⁷.), we may consider $V_{\prime 2}$, by (R⁶.) (T⁶.), and by the theory of binary systems, as a function of the initial and final relative coordinates, and initial components of relative velocities, involving also expressly the time t, and the

$n-2$ auxiliary quantities of the form $g^{(k)}$; and then we are to consider those initial components and auxiliary quantities and the time, as depending themselves on the initial and final coordinates, and on $H_{,}$. But it is not difficult to prove, by the foregoing principles, that when t and $g^{(k)}$ are thus considered, their variations are, in the present order of approximation,

$$\delta t = \frac{\Sigma_{,} \cdot m \left(\frac{\delta^2 \varpi}{\delta g^2}\right)^{-1} \delta_{,} \frac{\delta \varpi}{\delta g} + \delta H_{,}}{\Sigma_{,} \cdot m \left(\frac{\delta^2 \varpi}{\delta g^2}\right)^{-1}} \quad \ldots \ldots \ldots \quad (K^7.)$$

and

$$\delta g^{(k)} = \left(\frac{\delta^2 \varpi^{(k)}}{\delta g^{(k)2}}\right)^{-1} \left(\delta t - \delta_{,} \frac{\delta \varpi^{(k)}}{\delta g^{(k)}}\right), \quad \ldots \ldots \ldots \quad (L^7.)$$

the sign of variation $\delta_{,}$ referring only to the initial and final coordinates; and also that

$$\frac{\delta^2 \varpi^{(i)}}{\delta g^{(i)2}} \frac{\delta \xi_i}{\delta t} = \frac{\delta^2 \varpi^{(i)}}{\delta \alpha_i \, \delta g^{(i)}} \frac{\delta \xi_i}{\delta \alpha'_i} + \frac{\delta^2 \varpi^{(i)}}{\delta \beta_i \, \delta g^{(i)}} \frac{\delta \xi_i}{\delta \beta'_i} + \frac{\delta^2 \varpi^{(i)}}{\delta \gamma_i \, \delta g^{(i)}} \frac{\delta \xi_i}{\delta \gamma'_i}, \quad \ldots \quad (M^7.)$$

along with two other analogous relations between the coefficients of the two other coordinates $\eta^{(i)}$, $\zeta^{(i)}$; from which it follows that t and $g^{(k)}$, and therefore $\alpha'_k \, \beta'_k \, \gamma'_k$, may be treated as constant, in taking the variation of the disturbing part $V_{,2}$, for the purpose of calculating the perturbations (H⁷.): and that the terms involving Δt are destroyed by other terms. We may therefore put simply

$$\left.\begin{aligned} \Delta \xi_i &= \frac{\delta \xi_i}{\delta \alpha'_i} \Delta \alpha'_i + \frac{\delta \xi_i}{\delta \beta'_i} \Delta \beta'_i + \frac{\delta \xi_i}{\delta \gamma'_i} \Delta \gamma'_i, \\ \Delta \eta_i &= \frac{\delta \eta_i}{\delta \alpha'_i} \Delta \alpha'_i + \frac{\delta \eta_i}{\delta \beta'_i} \Delta \beta'_i + \frac{\delta \eta_i}{\delta \gamma'_i} \Delta \gamma'_i, \\ \Delta \zeta_i &= \frac{\delta \zeta_i}{\delta \alpha'_i} \Delta \alpha'_i + \frac{\delta \zeta_i}{\delta \beta'_i} \Delta \beta'_i + \frac{\delta \zeta_i}{\delta \gamma'_i} \Delta \gamma'_i, \end{aligned}\right\} \quad \ldots \ldots \ldots \quad (N^7.)$$

employing for $\Delta \alpha'_i$ the following new expression,

$$\left.\begin{aligned} \Delta \alpha'_i = \Sigma_{,,} \cdot m_k \Big\{ & \int_0^t \frac{\delta R^{(i,k)}}{\delta \alpha_i} dt + \frac{\delta \alpha'_i}{\delta \alpha_i} \int_0^t \frac{\delta R^{(i,k)}}{\delta \alpha'_i} dt \\ & + \frac{\delta \beta'_i}{\delta \alpha_i} \int_0^t \frac{\delta R^{(i,k)}}{\delta \beta'_i} dt + \frac{\delta \gamma'_i}{\delta \alpha_i} \int_0^t \frac{\delta R^{(i,k)}}{\delta \gamma'_i} dt \Big\} \end{aligned}\right\} \quad \ldots \quad (O^7.)$$

together with analogous expressions for $\Delta \beta'_i$, $\Delta \gamma'_i$, in which the sign of summation $\Sigma_{,,}$ refers to the disturbing masses, and in which the quantity

$$R^{(i,k)} = f^{(i,k)} + \xi_i \frac{\delta f^{(k)}}{\delta \xi_k} + \eta_i \frac{\delta f^{(k)}}{\delta \eta_k} + \zeta_i \frac{\delta f^{(k)}}{\delta \zeta_k} \quad \ldots \ldots \ldots \quad (P^7.)$$

is considered as depending on $\alpha_i \, \beta_i \, \gamma_i \, \alpha'_i \, \beta'_i \, \gamma'_i \, \alpha_k \, \beta_k \, \gamma_k \, \alpha'_k \, \beta'_k \, \gamma'_k \, t$, by the theory of binary systems, while $\alpha'_i \, \beta'_i \, \gamma'_i$ are considered as depending, by the same rules, on $\alpha_i \, \beta_i \, \gamma_i \, \xi_i \, \eta_i \, \zeta_i$ and t.

It may also be easily shown, that

$$\frac{\delta \xi_i}{\delta \alpha'_i} \frac{\delta \alpha'_i}{\delta \alpha_i} + \frac{\delta \xi_i}{\delta \beta'_i} \frac{\delta \alpha'_i}{\delta \beta_i} + \frac{\delta \xi_i}{\delta \gamma'_i} \frac{\delta \alpha'_i}{\delta \gamma_i} = - \frac{\delta \xi_i}{\delta \alpha_i}; \quad \ldots \quad (Q^7.)$$

with other analogous equations: the perturbation of the coordinate ξ_i may therefore be thus expressed,

$$\left.\begin{aligned} \Delta \xi_i = \Sigma_{\prime\prime} . m_k \Big\{ & \frac{\delta \xi_i}{\delta \alpha'_i} \int_0^t \frac{\delta \mathrm{R}^{(i,k)}}{\delta \alpha_i} dt - \frac{\delta \xi_i}{\delta \alpha_i} \int_0^t \frac{\delta \mathrm{R}^{(i,k)}}{\delta \alpha'_i} dt \\ & + \frac{\delta \xi_i}{\delta \beta'_i} \int_0^t \frac{\delta \mathrm{R}^{(i,k)}}{\delta \beta_i} dt - \frac{\delta \xi_i}{\delta \beta_i} \int_0^t \frac{\delta \mathrm{R}^{(i,k)}}{\delta \beta'_i} dt \\ & + \frac{\delta \xi_i}{\delta \gamma'_i} \int_0^t \frac{\delta \mathrm{R}^{(i,k)}}{\delta \gamma_i} dt - \frac{\delta \xi_i}{\delta \gamma_i} \int_0^t \frac{\delta \mathrm{R}^{(i,k)}}{\delta \gamma'_i} dt \Big\}, \end{aligned}\right\} \quad \ldots \quad (R^7.)$$

and the perturbations of the two other coordinates may be expressed in an analogous manner.

It results from the same principles, that in taking the first differentials of these perturbations (R⁷.), the integrals may be treated as constant; and therefore that we may either represent the change of place of the disturbed point m_i, in its relative orbit about m_n, by altering a little the initial components of velocity without altering the initial position, and then employing the rules of binary systems; or calculate at once the perturbations of place and of velocity, by employing the same rules, and altering at once the initial position and initial velocity. If we adopt the former of these two methods, we are to employ the expressions (O⁷.), which may be thus summed up,

$$\left.\begin{aligned} \Delta \alpha'_i &= \Sigma_{\prime\prime} . m_k \frac{\delta}{\delta \alpha_i} \int_0^t \mathrm{R}^{(i,k)} dt, \\ \Delta \beta'_i &= \Sigma_{\prime\prime} . m_k \frac{\delta}{\delta \beta_i} \int_0^t \mathrm{R}^{(i,k)} dt, \\ \Delta \gamma'_i &= \Sigma_{\prime\prime} . m_k \frac{\delta}{\delta \gamma_i} \int_0^t \mathrm{R}^{(i,k)} dt; \end{aligned}\right\} \quad \ldots \quad (S^7.)$$

and if we adopt the latter method, we are to make,

$$\left.\begin{aligned} \Delta \alpha'_i &= \Sigma_{\prime\prime} . m_k \int_0^t \frac{\delta \mathrm{R}^{(i,k)}}{\delta \alpha_i} dt, \ \Delta \alpha_i = - \Sigma_{\prime\prime} . m_k \int_0^t \frac{\delta \mathrm{R}^{(i,k)}}{\delta \alpha'_i} dt, \\ \Delta \beta'_i &= \Sigma_{\prime\prime} . m_k \int_0^t \frac{\delta \mathrm{R}^{(i,k)}}{\delta \beta_i} dt, \ \Delta \beta_i = - \Sigma_{\prime\prime} . m_k \int_0^t \frac{\delta \mathrm{R}^{(i,k)}}{\delta \beta'_i} dt, \\ \Delta \gamma'_i &= \Sigma_{\prime\prime} . m_k \int_0^t \frac{\delta \mathrm{R}^{(i,k)}}{\delta \gamma_i} dt, \ \Delta \gamma_i = - \Sigma_{\prime\prime} . m_k \int_0^t \frac{\delta \mathrm{R}^{(i,k)}}{\delta \gamma'_i} dt. \end{aligned}\right\} \quad \ldots \quad (T^7.)$$

The latter was the method of LAGRANGE: the former is suggested more immediately by the principles of the present essay.

General introduction of the Time, into the expression of the Characteristic Function in any dynamical problem.

23. Before we conclude this sketch of our general method in dynamics, it will be proper to notice briefly a transformation of the characteristic function, which may be used in all applications. This transformation consists in putting, generally,

$$V = t\,H + S, \quad \text{(U}^7.)$$

and considering the part S, namely, the definite integral

$$S = \int_0^t (T + U)\,dt, \quad \text{(V}^7.)$$

as a function of the initial and final coordinates and of the time, of which the variation is, by our law of varying action,

$$\delta S = -H\,dt + \Sigma . m\,(x'\,\delta x - a'\,\delta a + y'\,\delta y - b'\,\delta b + z'\,\delta z - c'\,\delta c). \quad \text{(W}^7.)$$

The partial differential coefficients of the first order of this auxiliary function S, are hence,

$$\frac{\delta S}{\delta t} = -H; \quad \text{(X}^7.)$$

$$\frac{\delta S}{\delta x_i} = m_i x'_i, \; \frac{\delta S}{\delta y_i} = m_i y'_i, \; \frac{\delta S}{\delta z_i} = m_i z'_i; \quad \text{(Y}^7.)$$

and

$$\frac{\delta S}{\delta a_i} = -m_i a'_i, \; \frac{\delta S}{\delta b_i} = -m_i b'_i, \; \frac{\delta S}{\delta c_i} = -m_i c'_i. \quad \text{(Z}^7.)$$

These last expressions (Z^7.), are forms for the final integrals of motion of any system, corresponding to the result of elimination of H between the equations (D.) and (E.); and the expressions (Y^7.) are forms for the intermediate integrals, more convenient in many respects than the forms already employed.

24. The limits of the present essay do not permit us here to develope the consequences of these new expressions. We can only observe, that the auxiliary function S must satisfy the two following equations, in partial differentials of the first order, analogous to, and deduced from, the equations (F.) and (G.):

$$\frac{\delta S}{\delta t} + \Sigma . \frac{1}{2m}\left\{\left(\frac{\delta S}{\delta x}\right)^2 + \left(\frac{\delta S}{\delta y}\right)^2 + \left(\frac{\delta S}{\delta z}\right)^2\right\} = U, \quad \text{(A}^8.)$$

and

$$\frac{\delta S}{\delta t} + \Sigma . \frac{1}{2m}\left\{\left(\frac{\delta S}{\delta a}\right)^2 + \left(\frac{\delta S}{\delta b}\right)^2 + \left(\frac{\delta S}{\delta c}\right)^2\right\} = U_0; \quad \text{(B}^8.)$$

and that to correct an approximate value S_1 of S, in the integration of these equations, or to find the remaining part S_2, if

$$S = S_1 + S_2, \quad \text{(C}^8.)$$

we may employ the symbolic equation

$$\frac{d}{dt} = \frac{\delta}{\delta t} + \Sigma \cdot \frac{1}{m} \left(\frac{\delta S}{\delta x} \frac{\delta}{\delta x} + \frac{\delta S}{\delta y} \frac{\delta}{\delta y} + \frac{\delta S}{\delta z} \frac{\delta}{\delta z} \right); \quad \ldots \quad (D^8.)$$

which gives, rigorously,

$$\frac{d S_2}{dt} = U - U_1 + \Sigma \cdot \frac{1}{2m} \left\{ \left(\frac{\delta S_2}{\delta x} \right)^2 + \left(\frac{\delta S_2}{\delta y} \right)^2 + \left(\frac{\delta S_2}{\delta z} \right)^2 \right\} \quad \ldots \quad (E^8.)$$

if we establish by analogy the definition

$$U_1 = \frac{\delta S_1}{\delta t} + \Sigma \cdot \frac{1}{2m} \left\{ \left(\frac{\delta S_1}{\delta x} \right)^2 + \left(\frac{\delta S_1}{\delta y} \right)^2 + \left(\frac{\delta S_1}{\delta z} \right)^2 \right\}; \quad \ldots \quad (F^8.)$$

and therefore approximately

$$S_2 = \int_0^t (U - U_1)\, dt, \quad \ldots \quad (G^8.)$$

the parts S_1 S_2 being chosen so as to vanish with the time. These remarks may all be extended easily, so as to embrace relative and polar coordinates, and other marks of position, and offer a new and better way of investigating the orbits and perturbations of a system, by a new and better form of the function and method of this Essay.

March 29, 1834.

XVI. *An Investigation of the Laws which govern the Motion of Steam Vessels, deduced from Experiments. By* Peter W. Barlow, *Esq., Civil Engineer. Communicated by* Peter Barlow, *Esq. F.R.S.*

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THE increasing extent of steam navigation, and its importance to the welfare of this country, demand a strict attention, not only to the construction of the vessels, but to the application of the power of steam, in order that the greatest possible useful effect may be produced with a given quantity of coals. The action of paddle-wheels, although a subject strictly mathematical, has hitherto but little engaged the attention of scientific men; in fact, the motion of the vessel being horizontal, while that of the wheel is rotary, there result a certain peculiarity and complication of action which almost defy the theorist to unravel without the aid of a complete set of experiments; for the great commotion of the water in the neighbourhood of a steam-vessel is such, that the results calculated from the usual laws of the resistance of fluids would scarcely be considered satisfactory without having the means of comparing them with practical results. In this respect I have been very fortunately circumstanced; all, or nearly all, His Majesty's vessels are fitted out at Woolwich, and each vessel is submitted to an accurate experiment, to ascertain its speed before it leaves the river, sometimes light and sometimes laden. The exact amount of their cargoes is known, their registered and actual tonnage, area of paddle, and every other particular which can serve as a guide to such inquiry; and I have availed myself of these circumstances, and of my personal acquaintance with many of their officers, and the officers of the yard, to attend several of the experiments myself, and in other cases to obtain an exact record of them. I am in hopes, therefore, that some of the remarks in the following pages may not be without utility as a future guidance in the practice of steam navigation.

Within a few years, several of His Majesty's vessels have been fitted out with wheels of a new construction, in which the floats are so contrived as, by the aid of machinery, to enter and leave the water nearly in a vertical position. This is found to reduce the shock on the engine produced in common wheels by the floats entering the water, and, in cases of deep immersion, to give an increased speed to the vessel.

A wheel in which the paddle should enter the water in a vertical position has long been considered a desideratum to remedy the supposed loss of power from the oblique action of the common wheel; and various methods of effecting this object have been invented, which have, however, been so complicated in their construction, and attended with so much friction and liability to get out of order, that they have not hitherto been brought into general use. The method employed in the wheel above alluded to is certainly the most simple that has appeared: it is, however, attended with a considerable liability to derangement, and consequent expense in repair, which materially lessens its value.

As my object in the present paper is in some measure to make a comparison of the action of this with that of the common wheel, I have added a short description of its construction. It is represented in fig. 1. where *a a*, *a a*, &c., are paddles, which turn upon spindles having a bearing in the frame-work *c*, *c*, *c*, &c., of the wheel, which is of a polygonal figure, having as many sides as it is required to have paddles. The inside frame or polygon is alone attached to the shaft of the engine, which does not continue beyond the side of the vessel, and the outer one has an independent bearing on a centre attached to the paddle-box, so that it receives its motion entirely from the rim or angles of the polygon: by this means the space between the wheels is left quite free. A is a part of the shaft or centre upon which the outer polygon of the wheel revolves, projected in an inclined direction to the middle between the sides, but of course to a point considerably eccentric with the wheel. Each paddle has a crank *b* attached to it at an angle of about 70°, and rods *d*, *d*, &c., connect the extremities of the cranks, with a moveable boss which revolves upon the fixed point A.

It will thus be seen, that in consequence of the point A being situated out of the centre, the paddles will assume different positions during the revolution of the wheel, which positions can be so arranged as to differ very little from the vertical while passing the lower part of the revolution, or that part where the action of the paddle takes place.

Description of the Experiments.

In making the experiments above referred to, the time chosen was generally as nearly as possible that of high water, when there is but little tide; but the effect of this, whatever it may be, was always eliminated by the following equations:

Let t = time in seconds in performing the mile against the tide.

t' = the time with the tide.

v = velocity in miles per hour of the boat.

v' = velocity of the tide water.

n = number of seconds in an hour.

Then $\frac{n}{t'} = v + v'$.

$\frac{n}{t} = v - v'$.

Therefore $\frac{n\,(t+t')}{2\,t\,t'} = v$, the velocity independently of the tide.

The following experiments on His Majesty's steam-vessel Dee will illustrate the formula:

Power, 200 horses. 23 strokes per minute.

			m	s		s
1st Exp.	Time of running a mile against the tide	. .	5	51	or	351
	——— with the tide	. . .	5	33	or	333
2nd Exp.	Time of running a mile with the tide	. . .	4	23	or	263
	——— against the tide	. .	7	58	or	478
3rd Exp.	Time of running a mile with the tide	. . .	4	50	or	290
	——— against the tide	. .	6	35	or	395

1st Exp. $v = \left\{\frac{351+333}{2 \, . \, 351 \, . \, 333}\right\} 3600'' = 10{\cdot}52$ miles per hour.

2nd Exp. $v = \left\{\frac{263+478}{2 \, . \, 263 \, . \, 478}\right\} 3600 = 10{\cdot}58$ ———

3rd Exp. $v = \left\{\frac{290+395}{2 \, . \, 290 \, . \, 395}\right\} 3600 = 10{\cdot}76$ ———

Mean $= 10{\cdot}62$

In this way the actual speeds of the vessels in the annexed Table have been determined.

The remaining columns, not being calculated results, will be sufficiently understood by the heads.

TABLE I. Experiments.

Name of the vessel.	Tonnage.	Horse power.	Quantity of coals in chaldrons.	Quantity of stores.	Diameter of wheel.	Length of paddle-board.	Depth of paddle-board.	Dip of paddle-board.	Strokes per minute.	Speed in English miles.	Diameter of piston.	Length of double strokes.	Number of strokes per minute, full power.	Remarks.
					ft. in.	ft. in.	ft. in.	ft. in.			inches.			
Alban	294	100	14	None.	13 0	9 0	1 6	not known	27	8·84	40	7	30	Government vessel.
Messenger...........	730	200	60	Channel service.	19 4	10 0	2 0	....	20½	9·75	53¾	10	22	Ditto.
Messenger...........	730	200	130	Ditto.	19 4	10 0	2 0	....	18	8·0	....	....	....	Ditto.
Pluto.................	365	100	14	None.	14 4	9 0	1 10	1 9	26½	10·15	40	7	30	Ditto.
Hermes...............	730	140	130	Channel service.	17 6	9 0	2 0	not known	18	6·3	44	9	24	Ditto.
Meteor	296	100	8	Ditto.	13 0	9 0	1 6	1 6	32	9·0	40	7	30	
Firebrand	494	140	10	None.	17 0	9 0	2 0	2 4	24	10·15	44	9	24	
Firebrand Morgan's wheel	494	120	12	Channel service.	14 6 (Polygon)	....	....	2 11½	28	10·55	42	8	27½	Ditto.
Flamer	494	120	15	None.	13 0 (Polygon)	....	....	3 11½	27	10·9	42	8	27½	Ditto.
Flamer Morgan's wheel	494	120	112	Channel service.	13 0 (Polygon)	....	....	5 6	24	9·57	....	....	....	Ditto.
Carron	294	100	8	None.	13 0	9 0	1 6	1 4	28	9·15	40	7	30	Ditto.
Dee *..................	710	200	30	Ditto.	19 4	10 0	2 0	1 6	23	10·62	53¾	10	22	Ditto.
Rhadamanthus......	820	220	46	Ditto.	20 4	9 0	2 6	not known	20	10·39	55½	10	22	Ditto.
Salamander*.........	820	220	210	Channel service.	20 4	9 0	2 6	5 6	15	8·15	55½	10	22	Ditto.
Firefly	550	140	152	Ditto.	17 6	9 0	2 0	3 4	20	8·3	44	9	24	Ditto.
Magnet...............	360	140	6	None.	16 0	10 0	1 6	1 8	29½	11·75	44	9	24	Private.
Phœnix..............	820	220	12	Ditto.	20 4	9 0	2 6	2 6	21	11·7	55½	10	22	Government vessel.
Medea * Morgan's wheel	825	220	15	Ditto.	21 0 (Polygon)	in the Basin		upper edge 4 inches above water line.	12½	one wheel	55½	10	22	Ditto.
Columbia Morgan's wheel	360	100	80	Channel service.	14 0 (Polygon)	....	....	4 10	24	8·5	40	7	30	Ditto.
Firebrand Morgan's wheel	494	120	40	Ditto.	14 6 (Polygon)	....	....	3 7	27	10·1	42	8	27½	Ditto.
Medea Morgan's wheel	825	220	2	None.	21 0 (Polygon)	....	....	3 11	22½	11·33	55½	10	22	Ditto.
Monarch	872	220	....	....	21	10 0	2 0	3 0	20½	10·72	....	10	22	Private.
Monarch	......	......	....	....	....	....	....	....	20⅛	10·50	....	....	....	Ditto.
Monarch	......	......	....	....	....	....	....	....	21	11·02	....	....	....	Additional weight on the safety-valve.

* Since the above experiments, a comparison of the speed of the Medea, Dee, and Salamander was made in the River Medway, at which the Lords of the Admiralty attended: each vessel was laden with nearly her full cargo of coals, stores, &c., amounting in the Medea and Salamander to 200 tons, and in the Dee to a quantity proportional to the tonnage. The exact speed of each was not ascertained for want of a measured mile on the banks of the river; but the result was entirely in favour of the Medea, whose speed amounted to nearly three quarters of a mile per hour beyond that of the other two vessels, their speeds being as nearly as possible the same.

Additional Experiments.

The Medea being moored in the basin at Woolwich, the throttle-valve of her engines quite open, and one wheel only in action, the number of strokes was found to be $12\frac{1}{2}$ per minute. Both wheels being now put in gear, the number of strokes per minute was $8\frac{1}{2}$; so that the resistance on a double surface, at a velocity of $8\frac{1}{2}$, is equal to that on a single surface at a velocity of $12\frac{1}{2}$: from which it follows, (with a very little allowance for the friction attending the working of the paddles,) that the resistance, notwithstanding the violent commotion of the water, is very nearly as the square of the velocity; which law is therefore adopted in the following investigations.

It should be observed, that the engine was scarcely in perfect order when these experiments were made; but being in the same state in both cases, the results are quite comparable.

In an experiment on the Phœnix made subsequently to that in the Table, she was, after being laden, lashed to the Warrior hulk, and her engines started. When her wheels acted with the tide, the number of revolutions per minute was $7\frac{1}{2}$, against the tide $6\frac{1}{2}$, and when free $16\frac{1}{2}$, her speed then being 9·01 miles per hour.

Illustrations of the action of Paddle-wheels in a Vessel in motion.

In order to dispose of the power of an engine to the best advantage, it becomes first necessary to know the manner in which it is at present consumed, and to calculate accurately that portion which is effective in propelling the vessel. When this is fairly understood, the arrangement and proportion of the parts which will effect an improvement will be readily seen.

When a steam-vessel is in motion, the force which opposes the engine is the resistance produced by the paddles moving through the water at a velocity equal to the difference of that of the centre of pressure of the wheel and that of the vessel. The part of this resistance which, when resolved, is in a horizontal direction, is that which is effective: the remaining part of the power is consumed by the resistance opposed to the paddles in a vertical direction, the back water, and other circumstances attending this mode of exerting the power of an engine. Some additional velocity may be given by the tendency of the paddle in its descent to raise the vessel in the water, by diminishing the sectional area of resistance: this, however, if any, is so small as not to be worth consideration; and it may therefore be fairly assumed that the horizontal resistance above mentioned is equal to that opposed to the motion of the vessel.

To make a calculation of these resistances, it becomes necessary to find, with some degree of accuracy, the position of the centre of pressure in the float or paddle, as the calculation is built upon the difference of the velocities of the boat and this centre, which are in some cases so nearly equal that the top of the paddle has no motion through the water.

To find the exact position of this point is, however, a question of very intricate calculation; and as it varies according to the depth of immersion of the paddle or float, the diameter of the wheel, and other circumstances, which vary in different boats, I have contented myself with assuming a point which will meet the ordinary cases, and which I have decided upon from the following considerations.

It is very evident that in every case the resistance upon different parts of the paddle is as the square of the distance from the centre of motion, because the resistance of a fluid varies as the square of the velocity: this ratio is, however, always increased more or less, in consequence of the extremity acting for a greater length of time than the inner part.

In the case of a wheel in motion, in a vessel at rest, if the length of the arc described by the outer extremity of the paddle exceed that described by the inner edge, in the ratio of the large radius to the smaller, the resistance upon any part of the paddle will vary exactly as the square of the radius; but this can only occur when the wheel is either totally immersed or up to the centre of motion: in every other circumstance it is evident that the arc described by the extremity will exceed that of the inner edge in a greater ratio, depending upon the degree of immersion, radius of wheel, &c. Consequently, the resistance upon any part of the paddle will increase in a greater ratio than the square of the distance from the centre of motion. It is, moreover, evident that the position of the centre of pressure will not only vary with every change of immersion, but will continue to ascend from the moment the paddle enters the water until it is immersed below the surface, when it becomes constant, and continues so until the upper part of the paddle again leaves the water.

As these experiments are made entirely with vessels in motion, it is not necessary to enter into a calculation of this precise point. I have merely spoken of the above case with a view to facilitate the investigation of the more complicated question of the centre of pressure of the paddle when the vessel is in motion.

In this case it will be seen, that as the revolution of the paddle resembles a circle rolling on a plane, every part of it will describe a cycloid. That point whose velocity is equal to that of the vessel will move through a simple cycloid, points within that circle in prolate cycloids, and every point without in curtate or contracted cycloids. In fig. 2. is represented the position of the float of a paddle-wheel in different parts of its revolution. The circumference, whose velocity is equal to that of the vessel, is here equal to two thirds of that which passes through the extremity of the paddle, which is about a medium case. It will be readily seen that the effect of the vessel being in motion will be to roll the circle A B C D on the line E F, so that the inner edge of every paddle will move through the cycloid R S T, whilst the extremity moves through the cycloid L K H I M, as shown by the dotted lines in the figure. As the centre of pressure varies at every angle of the paddle, in order to come at the true position it becomes necessary to find the relative velocity of the two extremes of the floats, or the distance moved in the two cycloids, at every instant of time. This would, however, lead to a calculation of greater labour than the nature of the present investigation demands: and as the circumstances upon which such calculations would be founded vary in every experiment, according to the diameter of the wheel, depth of immersion, &c., I have contented myself with assuming two points, one of which is intended to meet the ordinary cases of slightly immersed, and the other that of deeply immersed, paddles. It appears, again, referring to the figure, that whilst the extremity of the paddle is moving through the part of the curtate cycloid below the level of the water, a point, C, in the radius of the wheel, which is situated in the circumference of the rolling circle, has scarcely moved in the simple cycloid N C O. The difference of the curves during the lower part of the motion amounts nearly to what is due to an arc described with a radius equal to the difference of the extreme radius of the wheel and that of the circle of equal velocity with the ship.

I have considered from this cause that the resistance on any part of the float varies nearly as the square of its distance from the rolling circle; and having at the same time taken into consideration the greater length of time of the action of the extremity than of the inner edge of the paddle, I find, from the examination of several experiments, that in the case of slight immersions the assumption of the resistance on any point varying as the cube of the distance from the rolling circle, and in deep immersions as the 2·5 power, will be a sufficiently near approximation for the present purpose.

Having thus assumed the ratio of resistance with respect to the radius, we readily find the position of the centre of pressure by the following equation.

Let r be the difference of the radius of the rolling circle and that of the wheel, n the power of the resistance in relation to the radius, b the depth of the paddle, x any variable distance from its upper edge, and y the distance of the mean centre of pressure, also from the upper edge; then the integral of $(r + x)^n \, d\,x$, will be the sum of all the resistances, and $(r + y)^n \, b$ the expression to which it is to be equal. We have therefore, when $x = b$,

$$\frac{1}{n+1}(r+b)^{n+1} = (r+y)^n \, b,$$

which, when $n = 3$, gives

$$y = \left(\frac{(r+b)^4}{4\,b}\right)^{\frac{1}{3}} - r.$$

And when $r = 2{\cdot}5$,

$$y = \left(\frac{2\,(r+b)^{\frac{7}{2}}}{7\,b}\right)^{\frac{2}{5}} - r.$$

From these equations, the diameters to the centre of pressure of the common wheel (given in column 16 of the following Table) have been calculated.

In the new wheel, the centre of pressure will be nearly in the centre of gravity when the paddle is totally immersed, the motion of the paddle being nearly vertical;

but in consequence of the lower part coming sooner into and continuing longer in action, it must be taken some distance below the centre of gravity.

It is not easy to determine this by calculation; but by a comparison of all circumstances bearing upon this question, I have been induced to make an allowance of one eighth of the paddle on this account.

It may be proper to observe, that in these wheels there is no relation between the diameter of the polygon and the diameter to the centre of pressure, the paddles being differently hung and differently shaped in the several vessels, particulars it has not been thought necessary to introduce into the Table. The remaining calculated columns will, I believe, be sufficiently understood by the heads, except 17 and 18, the former of which exhibits the actual pressure in pounds upon the lower or vertical paddle, as due to the velocity given by the experiment, and the latter the portion of the whole power of the engine which is exerted upon it. The formula for column 17 (V being the velocity of the centre of pressure, v that of the vessel, a the area of the paddle, $62\frac{1}{2}$ the weight of a cubic foot of water in pounds, and $64\frac{1}{3} = 4\,g$, g denoting the force of gravity,) will be $\left(\frac{V - v}{64\frac{1}{3}}\right)^2 \times 62\frac{1}{2} \times a$, the pressure upon a surface moving in a fluid at the velocity $(V - v)$, being equal to the weight of a column of water whose base is the area of the surface, and altitude the height, through which a body must fall to acquire that velocity. This pressure being overcome at a velocity V, the above result, when multiplied by V, will express the power expended upon the vertical paddles; and this number, divided by the whole power of the engine, gives the decimal part of the whole power consumed by the vertical paddle given in column 18. In estimating the part of the power of the engine exerted in any case, the number of strokes made in a minute is compared with the actual number of strokes which ought to be made for the engine to perform its full duty, assuming, as usual, 33000 lbs. raised one foot high per minute to denote the power of one horse.

Table II.

Exhibiting the ratio of the velocity of the Wheel and Vessel, the whole pressure upon the vertical Paddle, and other results calculated from the preceding experiments.

	Name of the vessel.	Tonnage.	Horse power.	Quantity of coals in chaldrons.	Diameter of the wheel.	Length of the paddle-board.	Depth of the paddle-board.	Number of paddles.	Dip of the extremity of the paddle.	Strokes of the engine per minute.	Number of strokes per minute, for full power.	Speed of the vessel in English miles.	Area of paddle per horse power.	Tons burden per horse power.	Velocity of the vessel, that of the wheel being 1.	Diameter of rolling circle.	Diameter to centre of pressure.	Pounds pressure upon the vertical paddle.	Proportion of the power of the engine expended on the vertical paddles.
					ft. in.	ft. in.	ft. in.		ft. in.							ft.	ft.	lbs.	
1.	Messenger......	730	200	60	19 4	10 0	2 0	16	....	20½	22	9·75	·20	3·65	·754	13·31	17·65	844	·154
2.	Messenger......	730	200	130	19 4	10 0	2 0	16	....	18	22	8·00	·20	3·65	·730	12·80	17·53	886	·166
3.	Dee	710	200	30	19 4	10 0	2 0	16	1 6	23	22	10·61	·20	3·55	·732	12·91	18·00	1101	·207
4.	Rhadamanthus	820	220	46	20 4	9 0	2 0	16	....	20	22	10·39	·204	3·66	·791	14·54	18·36	720	·123
5.	Salamander ...	820	220	210	20 4	9 0	2 6	16	5 6	15	22	8·15	·204	3·66	·828	15·21	18·36	268	·047 } mean ·157 (rows 1–9)
6.	Phœnix	820	220	12	20 4	9 0	2 6	16	2 6	26	22	11·7	·204	3·66	·840	15·60	18·57	468	·080
7.	Monarch	872	200	....	21 0	10. 0	2 0	18	3 6	20½	22	10·72	·200	4·36	·748	14·62	19·55	1086	·220
8.	Monarch	......	......	....	....	....	....	......	....	20⅛	......	10·50	....	....	·746	14·60	....	1057	·217
9.	Monarch	......	......	....	....	....	....	......	....	21	......	11·02	....	....	·756	14·69	....	1061	·208
10.	Alban	294	100	14	13 0	9 0	1 6	14	....	27	30	8·84	·27	2·94	·777	9·15	11·77	354	·126
11.	Pluto............	365	100	14	14 4	9 0	1 10	14	1 9	26½	30	10·15	·34	3·65	·823	10·71	13·01	308	·126
12.	Hermes.........	730	140	130	17 6	9 0	2 0	......	....	18	24	6·3	·25	5·21	·626	9·80	15·66	1070	·277
13.	Meteor	296	100	8	13 0	9 0	1 6	......	1 6	32	30	9·0	·27	2·96	·671	7·87	11·70	1083	·370
14.	Firebrand	494	140	10	17 0	9 0	2 0	14	2 4	24	24	10·15	·25	3·51	·772	11·88	15·38	691	·178 } ·193 (rows 10–17)
15.	Firefly	550	140	152	17 6	9 0	2 0	14	3 4	20	24	8·3	·245	3·93	·733	11·60	15·81	684	·178
16.	Magnet........	360	140	6	16 0	10 0	1 6	......	1 8	29½	24	11·75	·214	2·57	·763	11·16	14·62	840	·149
17.	Carron	294	100	8	13 0	9 0	1 6	......	1 4	28	30	9·15	·27	2·94	·777	9·15	11·77	378	·137
18.	Medea	835	220	20	21 0*	4 10†	3 11	11	3 11	22½	22	11·33	·172	3·79	·627	13·79	22·03	3024	·666
19.	Flamer	494	120	13	13 0	5 9	2 9	9	3 11½	27	27½	10·9	·266	4·11	·683	11·30	16·55	1715	·625
20.	Flamer	......	......	112	....	....	....	......	5 6	24	27½	9·57	....	....	·674	11·16	16·55	1441	·526
21.	Firebrand	494	120	12	14 6	4 6½	2 10	9	2 11½	28	27½	10·55	·212	4·11	·667	10·50	15·73	1508	·526 } ·546 (rows 18–23)
22.	Firebrand	......	......	40	....	....	....	......	3 7	27	27½	10·10	....	....	·666	10·48	15·73	1404	·476
23.	Columbia	360	100	80	14 0	3 11	3 0	9	4 10	24	30	8·5	·237	3·6	·654	9·91	15·15	1008	·454
		1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.

* Polygon. † Mean length.

Deductions from the tabular numbers.

The results obtained in Columns 17 and 18 will be seen to differ so much in many cases from each other, as to throw an appearance of doubt upon the accuracy of the experiments. These discrepances are attributable, in a great measure, to the result depending upon the square of the difference of two velocities, so that the slightest error in either of the observations is greatly magnified. As the velocity of the wheel is derived from the number of strokes of the engine, the fraction of a stroke makes a very sensible difference in the relative velocities; and it is to this source that these discrepances may be attributed, for the experiments not having been made with a view to the present investigation, the importance of minute attention to this point was not foreseen.

There is no doubt, however, with so many experiments, made at different times and under different circumstances, that the means obtained in the Table are sufficiently near the truth for practical purposes.

As the Table affords us several observations upon vessels of various tonnage and horse power, I have considered it preferable to make a separate class of the larger and smaller ones, because as the proportions of the wheel, and floats, and other circumstances are different, a slight difference in the laws may exist which would be lost sight of by making a general mean.

The experiments are therefore divided into three classes :—

1st Class. Vessels having the common wheel of greater diameter than nineteen feet.

2nd Class. Vessels having the common wheel of a diameter less than nineteen feet.

3rd Class. Vessels having the new wheels.

To show what is stated above, that the differences in the results in Columns 17 and 18 of the Table may arise from the want of minute attention to the number of strokes of the engine, and the approximation of the general means to the truth, I have calculated what the number of strokes per minute would require to be to give the mean number of each class. And it will be seen how much a fractional part of a stroke per minute affects the numbers, and how little correction is necessary to produce the most accurate agreement in the result.

1st Class. Mean = ·157.

	Number of strokes observed.	Number required to give the Mean.
Messenger	$20\frac{1}{2}$	20·5
Messenger	18	17·8
Dee	23	21·8
Rhadamanthus	20	20·5
Salamander	15	17·2
Phœnix	21	22·2
Monarch	$20\frac{1}{2}$	19·6
Monarch	$20\frac{1}{8}$	19·2
Monarch	21	20·1

2nd Class. Mean = ·193.

	Number of strokes observed.	Number required to give the Mean.
Alban	27	28·3
Pluto	26½	27·5
Hermes	18	16·2
Meteor	32	29·0
Firebrand	24	23·6
Carron	28	29·1
Firefly	20	20·2
Magnet	29½	29·1

3rd Class. Mean = ·546.

Medea	22½	21·9
Firebrand	28	28·2
Firebrand	27	27·1
Flamer	27	26·7
Flamer	24	24
Columbia	24	24·4

It thus appears, that the number of strokes required to give the mean in each class, differs generally but a fraction of a stroke from the registered observations; except in a very few cases, and these can be accounted for by the vessel being particularly light or deeply immersed. At all events, there is no doubt that the mean of each set will approximate very nearly to the truth, the immersion of the paddle being also a mean.

A striking difference is observable between the ratio of the resistance of the paddle in a vertical position to the power of the engine in the common wheels, and in the new wheels; the former being ·157 and ·193 with the large and small boats, and the latter ·546. This difference arises from the nature of their action. In the new wheels the vertical position is the most effective in propelling the vessel, and in the common wheels it is the least so, as may be proved in the following manner.

Let A B, fig. 3, be the position of the paddle-rod of a vessel in motion, V being the velocity of the wheel, and v that of the ship, and ϕ the angle of inclination of the paddle-rod with a vertical line: let C D represent the velocity V at right angles to the paddle, and E C that of the vessel in a horizontal direction. Then it is evident that C F, which is the resultant of these velocities, will represent the velocity and direction of motion of the paddle with respect to still water.

Resolve F C into the two velocities F G, C G, one at right angles to, and the other in the direction of, the paddle, of which the latter is lost, while the former will represent the velocity with which the paddle meets the water in a direction at right angles to its face; then E G or $HF = EF - EH = V - v \cos \phi$. Consequently $(V - v \cos \phi)^2$ will represent the whole resistance which the paddle opposes to the engine at any angle ϕ.

In order to get an expression for the resistance in a horizontal direction, or that part of the power which is effective in propelling the vessel, C G must be resolved into the two resistances G I, C I, of which the former is $(V - v \cos \phi)^2 \cos \phi$; and it is to be shown that a mean resistance which would act uniformly through the arc ϕ, so as to be equal to this variable action, will exceed that of the mean action of the lower paddle; while in the new wheel, the mean resistance is less than that of the lower paddle, and hence the great difference in the mean numbers in the Table.

In the new wheel it has been already stated, that the paddle enters the water nearly in a vertical position; and in order to simplify the investigation, I consider it to be truly vertical in every position, which is so near the truth, in that part of the revolution where the action of the paddle takes place, that the results will be but slightly affected. Let C D, figure 4, be any position of a vertical paddle moving at a velocity V, in the direction F B of a tangent to the circumference. Then by resolving this velocity into two, one at right angles to, and one in the direction of, the paddle, we find the velocity with which it meets the water at right angles to its face, to be $V \cos \phi$, ϕ being as before the angle of inclination of the radius A B with a vertical.

The resistance opposed to the vertical paddle when the ship is in motion with a velocity V, will therefore be $(V \cos \phi - v)^2$, so that in the vertical paddle, when $V \cos \phi$ is equal to v, no resistance is opposed to the engine, and when it is less the paddle opposes a resistance in a contrary direction; and it is sufficiently obvious that the resistance in every position in this case is less than when in its lowest position, while in the old wheel it is everywhere greater, at least within practical limits, which fully accounts for the difference in question.

It is observed above, that the horizontal resistance of the oblique paddle is always greater than in its vertical position within the limits prescribed by practice. Let us examine what the actual limits are, by finding, when with given velocities V and v, $(V - v \cos \phi)^2 \cos \phi$ is a maximum, or when

$$V^2 d \cos \phi - 4 V v \cos \phi \,.\, d \cos \phi + 3 V^2 \cos v^2 \, d \cos \phi = 0.$$

Whence

$$\cos \phi^2 - \frac{4 V \cos \phi}{3 v} = - \frac{V^2}{3 v^2},$$

and

$$\cos \phi = \frac{V}{3 v}.$$

It depends, therefore, on the relative velocities of the wheel and vessel.

When $V = 5, v = 4,$ then $\phi = 65^\circ 33'$

$V = 4, v = 3,$ $\phi = 63 \;\; 37$

$V = 3, v = 2,$ $\phi = 60 \;\; 0.$

These results at once account for the ratio of the power of the engine to that of the resistance on the vertical paddle being greater in the old than in the new wheel. For it appears, contrary to the usual opinion, that not only the total resistance to the paddle increases as it deviates from the vertical, but that the effective horizontal force also increases up to all angles within the limits of the immersion of paddle-wheels.

It should be stated, however, that although an increased propelling power is obtained from the vertical paddle upwards as far as these limits, it is not to be understood that so great an angle is practically advantageous; for the vertical resistance becomes very great, and the shock on the engine by the paddles striking the water at so great an angle is tremendous.

Comparative efficiency of oblique and vertically acting Paddle-wheels.

In order to make a comparison of the efficiency of different wheels, it is necessary to estimate what part of the whole power of an engine is transmitted through them upon the boat, and what part is actually lost. In oblique-acting paddles, a loss of power is sustained in two ways: first, by the resolution of the power of the engine, in which one part is referred to a vertical line, which is wholly inefficient as a propelling power; this part therefore is lost: and of the resolved horizontal force, another part is lost by the motion of the wheel backwards in the water. This may perhaps be best illustrated by the case of a locomotive engine. If the friction between the wheel and the rail is such that the former does not slip, the motion of the carriage is the same as that of the circumference of the wheel; the whole power of the engine is employed in propelling the carriage, and consequently there is no lost power. But if the friction be not sufficient, the wheel will slip back some quantity, the same steam will be consumed in a revolution of the wheel, but the carriage will not be advanced as before, and there will therefore be a loss of power proportional to the skidding or receding of the wheel. So in a steam vessel, all that the centre of pressure actually goes back in the water, or all that its circumferential velocity exceeds that of the vessel (the expense of steam being proportional to the former and the effect to the latter,) may be esteemed lost power; for although the nature of the medium requires some back motion of the wheel to get up the necessary resistance, yet the less there is of this retrogradation, within the limits of practical convenience, the better; because the less power will thus be absorbed by the paddle and the more will be left to act upon the vessel; hence the superiority of one wheel or another will depend upon the quantity of lost power it gives rise to, that wheel being of course to be preferred in which the loss is the least.

The lost power of an engine with the common wheel, or, which is better, the effective part of the power, may be found as follows: First, reduce the variable tangential resistance on the paddle to a mean constant resistance; find also the mean resolved horizontal resistance; then if this mean resolved resistance be supposed to be applied to the circumference of the wheel, the case will exactly resemble that of a locomotive engine, and the part of this force which is to the whole as v to V, will be that part which is employed in propelling the vessel, all the rest will be lost power. The general expression for the tangential resistance on an oblique paddle has been shown to be

$(V - v \cos \phi)^2$ and the integral of $(V - v \cos \phi)^2 d \cdot \phi$, divided by ϕ, will be the mean resistance; or this may be obtained sufficiently near for practical purposes arithmetically. Let this be m, then $V m$ will express the whole power of the engine. Again, the resolved horizontal resistance is expressed generally by $(V - v \cos \phi)^2 \cos \phi$, and the mean value of this is found by dividing the integral of $(V - v \cos \phi) \cos \phi \, d \phi$ by ϕ. Or find the same arithmetically: let this be m', then $V m'$ will be the whole resolved horizontal force, which may be supposed to be applied at the circumference, as in the locomotive engine, and the part of this force expressed by $m' v$, will be the effective force exerted on the vessel; but the whole force is $m V$, therefore $\frac{m' v}{m V}$ will be the proportional part of the power saved, the original engine power being 1. These numbers are computed and arranged within all practical limits in the following little Table.

In the vertical paddle, the mean resistance applied tangentially is the integral of $(V \cos \phi - v)^2 d \phi$ divided by ϕ, which may be all supposed to be applied horizontally as in the locomotive carriage; and the lost power is therefore simply the difference of velocity; that is, the effective horizontal force is $\frac{v}{V}$, the power of the engine being 1; and as in this case the mean velocities are generally as 3 to 2, the part of the whole power which becomes effective is ·666.

In the manner above described, the following Table, exhibiting a comparison of the lost power of the common wheel and that of the new wheel at several states of immersion, has been calculated.

TABLE III.

Angle at which the centre of pressure of the paddle entered the water.	Proportion immersed of the radius of the wheel.	Effective power, that of the engine being 1.		Lost power, that of the engine being 1.		Mean effective resistance, the resistance of the vertical paddle being 1.		Mean resistance opposed to the engine, that of the vertical paddle being 1.		Remarks.
		Common wheel.	MORGAN's wheel.	Common wheel.	MORGAN's wheel.	Common wheel.	MORGAN's wheel.	Common wheel.	MORGAN's wheel.	
35°	·252	·660	·666	·340	·333	1·298	7·02	1·457	·674	Vessel very light, the immersion to the top of the paddle.
44	·350	·645	·666	·355	·333	1·510	5·47	1·750	·522	Mean immersiou of experiments.
50	·430	·620	·666	·380	·333	1·628	5·04	1·971	·482	Mean expressing the ordinary data.
60	·550	·553	·666	·447	·333	1·85	4·25	2·510	·404	Very deep immersion.

Explanation of the manner in which the Power of the Engine is expended in the two Wheels.

Having obtained an expression for the whole resistance opposed to the engine at any angle of the paddle, we may, as before stated, find such a mean resistance, which continuing the same throughout the whole arc, would produce the same effect as the variable resistances expressed in the above formula; and this multiplied by the number of paddles and tangential velocity should be equal to the power of the engine. The depth of immersion not being given in every experiment, the exact angle at which the paddles entered the water is not known; but as the experiments were generally made immediately after the engines were fitted, when the vessel had not taken in her cargo of coals, the paddles were but slightly immersed.

From the preceding data, and inquiries I have made, I am led to assume a dip of three feet six inches, or the water level to be twelve inches above the top of the paddle, as a mean for the first class, which will make the centre of pressure enter the water at 44°.

Then calling $V = 4$ and $v = 3$, which is nearly the mean ratio of the velocities of the common wheel and vessel, I find the mean resistance of the paddle passing through the whole arc to be to the resistance of the vertical one as 1·75 : 1. Now as the whole circumference contains sixteen paddles, and the arc passed through is 88°, we may consider three paddles and a half to be acting; this will make the whole resistance to the engine equal to 6·12 times that opposed by the vertical paddle, or the power of the engine exerted on the vertical paddle = ·163, the whole power being 1; while the mean obtained from the experiments is ·157. In the second class, the paddles, though smaller, (being proportionably immersed,) may be considered to enter the water at the same angle of inclination, so that the same mean resistance will result from it, viz. 1·75. The number of paddles, however, being less in the small wheels, there are not more than three of them effective, which gives the proportion of the power of the engine exerted on the lower or vertical paddle ·190; the mean obtained from the experiments being ·193. We are thus able in the common wheels to account for the power of the engine, which not only proves quite satisfactorily the accuracy of the principles adopted in the preceding calculations, but that the supposed lost power from back-water is very trifling.

We have in the above investigations considered the paddles to be in the direction of the radii from the centre. It is necessary, however, to mention, that in some of the wheels the radii of the paddles are made to proceed from a point on one side of the centre, with a view of reducing the shock produced by the paddle striking the water at too great an angle. But this deviation is not sufficient to make any sensible difference in the amount of the resistance opposed to the engine; for although it is decreased at the commencement of its action by the angle being smaller, it is increased after passing the centre, which resistance observes the same law until the column of water above is less than that due to the square of the velocity.

It now remains to account for the power of the engine in the new wheel, where we have found the horizontal resistance to the paddle to be $(V \cos \varphi - v)^2$. The power of the engine necessary to overcome this resistance will be $(V \cos \varphi - v)^2 \cos \varphi$, as will be readily seen from the following resolutions.

Referring again to figure 4, let G B represent the horizontal resistance or force on the paddle: it is to be ascertained what force in the direction F B will overcome it. Resolve G B into two forces H G, H B, one at right angles to, and one in the direction of, the radius rod. The effect to turn the line A B about the point A will be the force H B alone; the force G H, which is in the direction of the line A B, having no power to turn it, its whole action being on the axle of the wheel. It therefore follows, that the force F B at right angles to the radius rod required to retain the point B in equilibrio, or to exert a force in a horizontal direction equal to G B, is G B $\cos \phi$, because the angle G B H = B A I, and consequently equal to $(V \cos \phi - v)^2 \cos \phi$, as already stated in a preceding page.

Having assumed in this case the same angle of 44° when the paddles begin to act, I find the mean of the horizontal resistances on the paddle from the equation $(V \cos \phi - v)^2$ to be ·547, and the mean of the forces necessary to balance these resistances from the equation $(V \cos \phi - v)^2 \cos \phi$ to be ·522 (the force on the lower paddle being 1), which multiplied by $2\frac{3}{4}$, the number of paddles acting, makes the whole power of the engine employed on the paddles to be 1·436 times that exerted on the vertical paddle, or the proportion of the power of the engine employed on the lower paddle to be ·696; the mean given by the experiments being ·546. There is, therefore, a deficiency of ·150 of the power of the engine to account for, which I suppose partly due to the friction of the wheel, and partly to the deviations of the paddle from the vertical position, which, as before observed, results from the construction.

Consumption of Coals at different speeds.

It may be seen by referring to our first Table of experiments, that in deeply laden vessels the engines make little more than two thirds of the number of strokes due to their full power. Now if this number of strokes required only two thirds the steam necessary to keep the engine in full action, the loss sustained in deeply laden vessels would be simply that due to the oblique action of the wheel; but unfortunately nearly as much steam and as much fuel are required in these cases to procure fifteen strokes, as to make the engine perform its full number of twenty-two strokes under other circumstances. To verify this assertion, which is perhaps a very unexpected one, to those who are not intimately acquainted with the navigation of steam vessels in long voyages, I give the following Table, kindly supplied me by Captain Austin, R.N., the observations having been made by his order and under his own superintendence whilst in command of His Majesty's steamer Salamander. By a reference to this Table it will be seen that there is no proportional relation between the speed of the vessel, or even the speed of the piston, and the consumption of fuel, which may be accounted for in a great measure by the loss of heat from the radiation being constant at all velocities; but from whatever cause it proceeds, it is obviously an object of the greatest importance to the progress of improvement of steam navigation, that some means should be found of enabling an engine to perform its full duty under all degrees of immersion. When a vessel commences a long voyage she is necessarily deeply immersed, and at the end of it, her fuel being consumed, her paddles are not perhaps immersed so deeply by nearly three feet. In the latter case the effective part of the power exerted is ·660, and the power exerted is the whole power of the engine;

while in the former the effective part of the power exerted is only ·553, and this power is itself only two thirds of the whole power; so that when a vessel is deeply laden, not above ·368, or three eighths of the whole power of the engine, is employed effectively, while the fuel expended is nearly the same as when light. It is not proposed in this place to speak of a remedy for this evil, but to point it out, in order, if possible, to obtain some means of improvement.

Table IV.

No. of revolutions in one minute.	Distance run in two hours by log.				Consumption of coals in two hours.			
	Max.	Min.	Mean.	No. of hours' trial.	Max.	Min.	Mean.	No. of hours' trial.
	K. F.	K. F.	K. F.		Bushels.	Bushels.	Bushels.	
6	5 2	3 4	4 03	14	16	9	$10\frac{3}{4}$	28
7	6 6	3 4	4 76	38	23	9	14	64
8	13 0	4 4	9 63	14	30	$9\frac{1}{2}$	18	38
9	13 0	4 0	10 58	22	32	24	$28\frac{1}{4}$	36
10	17 0	4 0	9 80	46	35	14	$26\frac{1}{4}$	62
11	16 0	5 4	5 60	4	37	24	$34\frac{1}{2}$	16
12	15 4	7 0	11 13	30	39	26	$37\frac{3}{4}$	52
13	16 0	5 0	14 06	74	41	34	$36\frac{1}{4}$	102
14	18 6	9 0	14 47	120	46	25	$36\frac{1}{4}$	172
15	17 4	7 2	14 12	110	46	28	$37\frac{3}{4}$	162
16	19 4	8 0	15 14	68	46	28	$37\frac{3}{4}$	98
17	18 0	9 4	15 14	80	41	32	$37\frac{3}{4}$	140
18	19 2	10 0	15 24	68	48	31	$38\frac{1}{2}$	96
19	21 0	14 2	17 68	68	45	29	39	92
20	20 0	14 6	19 80	48	50	39	41	58
21	21 0	20 0	20 50	4	41	32	$38\frac{1}{2}$	14

On the relation between the Diameter of the Wheel, Area of the Paddle, and the Velocity of the Vessel.

When the area of the float of a paddle-wheel is so adjusted to any given diameter that the engine is capable of performing its whole duty, it is evident that the same duty might also be performed with a less paddle and larger wheel, or with a smaller wheel and larger paddle; but the velocity of the vessel will not be the same in the two cases, and the question therefore is to determine what change must be made in the area of the paddle, and what change would take place in the speed of the vessel with a given change in the diameter of the wheel, so that the engine in both cases may perform its whole duty.

Let $d =$ diameter of the first wheel.
$V =$ its circumferential velocity.
$a =$ the area of paddle.
$v =$ the velocity of the vessel.
$r\,d =$ the diameter of the second wheel.
$r\,V =$ the circumferential velocity.

Let $a' =$ the required area of paddle.

$v' =$ the new resulting velocity of the vessel.

All of which quantities are given except a' and v', which may be determined from the following considerations, viz.

1st. That the whole duty of the engine is exerted in both cases; consequently

$$(V - v)^2 V a = (r V - v')^3 r V a'.$$

2nd. That the resistance on the paddle in each case is equal to that of the vessel, and therefore proportional to the squares of the two velocities v' and v, that is,

$$(V - v)^2 a : (r V - v')^2 a' : v^2 : v'^2.$$

From these two equations we find

$$v' = \frac{v}{\sqrt{r}}, \qquad \text{and} \qquad a' = \frac{(V - v)^2}{(r^{\frac{3}{2}} V - v)^2} \times a.$$

From the first it appears that the two velocities are to each other inversely as the square roots of the radii. And by the second the new area of paddle will be found to increase and decrease so rapidly, that generally little practical advantage can be taken of the condition of the first equation.

It appears from the above that there are two different diameters of wheel, with dependent area of paddles, that will allow the full power of the engine to be developed. And when from circumstances of loading, &c. the whole power of the engine cannot develop itself, there are two ways in which this effect can be insured; the one by reducing the paddle, and the other by reducing the diameter of the wheel; by the former it will be seen that the speed of the vessel will remain the same, but by the latter it will be increased inversely as the cube root of the power developed in the two cases.

We have seen that $(V - v)^2 V a$ expresses the whole amount of the power exerted, which in the case we are now supposing, is less than the engine is capable of exerting. Let the amount of power, or, which is the same thing, the number of strokes made in the two cases be as 1 to m.

Now supposing, in the first place, the diameter to remain the same, the velocity V will become m V; and we may find a' and the resulting velocity v' from the equations

$$(V - v)^2 V a : (m V - v')^2 m V a' :: 1 : m,$$

and

$$(V - v)^2 a : (m V - v')^2 a' :: v^2 : v'^2;$$

that is, by making the whole power in the two cases as 1 to m, and the resistances on the paddles as v^2 to v'^2.

From these equations it appears that $v' = v$, or that no increase of velocity will be given to the vessel by reducing the paddle, so as to bring out the full power of the engine.

But if the diameter of the wheel be changed, the paddle remaining the same, both the velocities V and v will be changed. Let the former become p V, and the latter $n\,v$; our equations are therefore

$$(V - v)^2 V a : (p V - n v)^2 p V a : 1 : m,$$

$$(V - v)^2 a : (p V - n v)^2 a : 1 : n^2,$$

which reduced, give $p = n$, and each equal to the $\sqrt[3]{m}$; that is, the velocity of the vessel will be increased in the ratio of the cube root of the powers expended.

We see therefore that when an engine is not able to perform its whole duty, the diameter of the wheel ought to be reduced, and not, as is usually done, the area of the paddle; for in the former case the velocity is increased in the ratio of the cube roots of the number of strokes, while in the former it remains the same as when the less power was developed.

To find the change in the diameter required to produce this effect, we know the circumferential velocities are as V : V $\sqrt[3]{m}$, or as 1 : $\sqrt[3]{m}$; we know also that these velocities are as the number of strokes multiplied by the radii of the wheels; putting therefore r and r' for the two radii, the velocities are as $r : m\,r'$, or $r : m\,r' : 1 : \sqrt[3]{m}$, whence

$$r' = \frac{r}{m^{\frac{2}{3}}},$$

the required radius of paddle.

In the case of the Salamander, from the great immersion of the paddles the engine could only make fifteen strokes instead of twenty, its full duty. We may now find what increase of speed would have been given to the vessel by reducing the wheel so as to allow the engine to perform its whole duty.

We have $m = 1{\cdot}25$, whence $r' = {\cdot}8617\,r$, and $n\,v = 1{\cdot}077\,v$; if therefore the diameter of the wheel of the Salamander had been reduced in the ratio of 1 to ·8617, the speed of the vessel would have been increased in the ratio of 1 to 1·075; that is, by reefing each paddle about fifteen inches, the speed of the vessel would have been increased about two thirds of a mile, and at the same time the consumption of fuel would be increased only in a very small degree, as has been demonstrated by the experiments given in the preceding article.

In these calculations I have assumed a similar action of the paddles with every variation of diameter, which in reality is not strictly true, as every change of the position of the floats will vary the angle at which the centre of pressure enters the water. I find, however, in the greatest extent of reefing ever required, this variation to be so small, that it is not necessary to introduce it into the calculation. As far as its effect extends it is favourable to the reefing, as thereby the obliquity of action is diminished, and consequently the loss of power.

Comparison of the Resistance of a Steam Vessel with that of a Plane Surface.

The resistance of vessels being a subject which has of late much engaged the attention of engineers, I have been induced to add the following comparison of the resistance of a steam vessel with that of the paddles, a calculation which the preceding investigations and experiments have enabled me to arrive at with considerable accuracy. Having obtained, through the kindness of O. LANG, jun. Esq. (to whom I have been highly indebted for many of the preceding data), the sectional immersed area of several of the vessels, I have made the calculations, and obtained the results given in the annexed Table. These have been made in the following manner.

Let V = the velocity of the wheel, v that of the vessel, s its sectional area immersed, and a the area of a paddle whose action is horizontal, and effect equal to that of all the paddles. The resistance being as the square of the velocity, $(V-v)^2 a$, will express the resistance on the paddle, and $v^2 s$ would be the resistance of the vessel if it were a plane surface; but the real resistance being $(V-v)^2 a$, the fraction of the resistance compared with a plane will be $\frac{(V-v)^2 a}{v^2 s}$.

The value of a has been obtained by knowing the depth of immersion, so as to ascertain the angle at which the centre of pressure entered the water, and thence the number of times the whole effective action exceeds that of the vertical paddle; this multiplied into the area of the paddle, gives the whole surface above denoted by a.

In the following Table is given the effective pressure exerted by the engine in every experiment where the dip or immersion of the paddle was known; but the comparison of the resistance of the vessel with a plane, is of course limited to those experiments only in which the area of the immersed section could be ascertained.

TABLE V.

Name of the vessel.	Tonnage.	Horse power.	Effective pressure exerted by the engine.	Velocity of the vessel, that of the wheel being 1.	Velocity of the vertical paddles through the water, that of the wheel being 1.	Area of the paddle-board.	Area of a vertical paddle equal in effect to all the paddles.	Immersed sectional area of the vessel.	Ratio of the resistance of the vessel to that of a plane surface of the same section.
			lbs.			ft. in.			
Medea	835	220	4536	·627	·373	19 0	54·00	263	$\frac{1}{15}$
Flamer	494	120	2814	·683	·317	16 0	52·44	174	$\frac{1}{15}$
Flamer	494	120	2593	·674	·326	16 0	57·60	218	$\frac{1}{16}$
Firebrand..	494	120	2472	·667	·333	12 9	38·56	200	$\frac{1}{19}$
Firebrand..	494	12	2527	·666	·334	12 9	42·00	214	$\frac{1}{18}$
Columbia ..	360	100	1807	·654	·346	12 0	43·10	202	$\frac{1}{18}$ } $\frac{1}{17}$
Salamander	820	220	2180	·833	·167	22 6	398·70	359	$\frac{1}{21}$
Dee	710	200	2531	·732	·268	20 0	69·00	209	$\frac{1}{19}$
Firefly	550	140	3808	·733	·267	18 0	201·00	275	$\frac{1}{11}$
Firebrand..	494	140	2474	·772	·228	18 0	128·61	200	$\frac{1}{17}$
Pluto	365	100	985	·823	·117	16 6	105·23	116	$\frac{1}{24}$
Monarch ..	872	200	7167	·748	·252	20 0			
Monarch ..	872	200	6976	·746	·254	20 0			
Monarch ..	872	200	7002	·756	·244	20 0			
Magnet....	360	140	3672	·763	·237	15 0			
Meteor	296	100	4320	·671	·229	13 6			
Carron	294	100	1731	·777	·323	13 6			
1.	2.	3.	4.	5.	6.	7.	8.	9.	10.

It thus appears, contrary to the results of all experiments hitherto made upon a small scale, that the resistance of a well-shaped vessel does not exceed $\frac{1}{17}$th part that of a plane of the same sectional area.

The above mean being founded on several experiments, I have no doubt is very near the truth, although in each so much error may exist from the want of minute attention to the number of strokes of the engine, as to afford no test of the best-shaped vessel.

As, however, the results are very extraordinary, it may be well to submit them to a totally independent mode of estimation. In the above investigation the mean number of acting paddles with their corresponding velocities and areas, are compared with the sectional area of the vessel and its velocity: but we might have made the calculation in another way, that is, by comparing the force necessary to urge a plane section equal to that of the vessel, with the velocity at which it passes through the water, with the actual power of the engine employed to propel the vessel, which ought to give nearly the same fraction as the other method.

Of the whole power of the engine we have seen that with the vertically acting paddle one third is lost by the retrograding of the wheel. In the Medea therefore the power employed in propelling the vessel is two thirds of 220 = 146 horse powers. Now the velocity of the vessel having been 11·33 English miles per hour, or 16·62 feet per second, the resistance in feet of water is $\frac{(16\cdot62)^2}{64\frac{1}{3}}$ and in pounds $\frac{(16\cdot62)^2}{64\frac{1}{3}} \times 62\frac{1}{2}$ on each square foot. The number of feet in the section is 263, and the velocity in feet per minute is 997. The whole force therefore expended in a minute is 70796970, which divided by 33000 gives 2150 horse power for the force necessary to urge a plane section of 263 feet through the water at the rate of 11·33 miles per hour. But the vessel itself is urged with that velocity by the power of 146 horses. The resistance of a vessel is therefore to that of a plane section of the same area as 146 to 2150, or as 1 to 15 very nearly, which agrees exactly with the number given in the Table. The agreement is equally close in the Flamer; and I find the mean obtained this way from the whole set of experiments, is very nearly the same as that given in the above Table.

Since this Paper was read at the Royal Society, Henry Beaufoy, Esq. has, with a noble generosity, presented to his scientific countrymen one of the most valuable collections of resistances ever published, made by his father, the late celebrated Colonel Beaufoy; and it is very satisfactory to be able to confirm the above extraordinary results on the authority of his tables.

I have found above that to urge a plane section of 263 feet area at the rate of 11·33 English miles, or 9·84 nautical miles per hour through still water, would require 2150 horse powers. According to Colonel Beaufoy's results it would require a power of

2444 horses, which would give a still less fraction than a fifteenth; but compared with a cylinder with flat ends, the number of horse powers is 2275, and the fraction greater than $\frac{1}{16}$th, but less than $\frac{1}{15}$th, a confirmation which I could have scarcely hoped to have obtained.

These results are deduced as below. According to Colonel BEAUFOY's experiments, Table I. Part III., it requires a force of 203·79 pounds to urge a plane of one square foot through still water at the rate of eight nautical miles per hour, or 810 feet per minute: now the Medea moved with a velocity of 11·33 miles per hour, or 966 feet per minute; it would therefore require, according to Colonel BEAUFOY, (the resistance being as the square of the velocity,)

$$810^2 : 996^2 : 203 \cdot 79 \text{ lbs.} : 308 \text{ lbs.}$$

Now the section being 263 feet, the resistance per foot 308 pounds, and the velocity 996 feet per minute,

$$\frac{308 \times 996 \times 263}{33000} = 2444,$$

the number of horse power requisite to urge a plane section of this area at the given rate. But if instead of a mere plane we take Colonel BEAUFOY's experiments for a cylinder with flat ends, which required only 190·78 pounds, we obtain the number of horse power 2275 as above stated.

If I had made use of the results of Colonel BEAUFOY's experiments throughout the preceding investigations, the numbers in Column 17 of Table II. and in Column 4 of Table V. would have been increased by about one seventh; and in estimating the power exerted on the paddles, it would have been found to exceed the nominal power of the engine, which proves that engines work above their nominal power.

General Deductions from the preceding Investigations.

On a general examination of the preceding results, I am led to the following conclusions.

1st. That when the vessels are so laden that the wheel is but slightly immersed, there is little advantage in the vertically acting paddle.

2nd. That in cases of deep immersion it has considerable advantage over the common wheel as at present constructed. It has an advantage, in consequence, in a sea where the degree of immersion is continually varying.

3rd. That in the common wheel, while the paddle passes the lower part of the arc, or when its position is vertical, it not only affords less resistance to the engine, but is less effective in propelling the vessel than in any part of its revolution.

4th. That in the new wheel the paddle while passing the lower part of the arc affords more resistance to the engine, and is more effective in propelling the vessel, than in any part of its revolution.

This property of the vertical paddle is a serious deduction from the value of the wheel; for in consequence of the total resistance to all the paddles being so much less than in the common wheel, much greater velocity is required to obtain the requisite pressure, which is attended with the consumption of an additional quantity of steam, and of course of a proportionate loss of power.

This loss of power is most sensible when the wheel is slightly immersed, as may be seen from the Table; whereas the lost power from the oblique action of the common wheel is then scarcely perceptible. When the vessel is more immersed, and the angle of inclination at which the paddle enters is greater, the proportion of lost power in the common wheel is much increased, while that of the vertical paddle remains nearly constant, so that in cases of deep immersion the vertical paddle has considerably the advantage.

5th. That in any wheel the larger the paddles the less is the loss of power, because the velocity of the wheel is not required to exceed that of the vessel in so high a degree in order to acquire the resistance necessary to propel the vessel.

6th. That with the same boat and the same wheel no advantage is gained by reducing the paddle, so as to bring out, as it is called, the full power of the engine; the effect produced being simply that of increasing the speed of the wheel, and consuming steam to no purpose.

7th. That with the same boat and the same wheel an increase of speed will be obtained by reducing the diameter, or by reefing the paddles, at least within certain limits, viz. as long as the floats remain immersed in the water, and the velocity of the engine does not exceed that at which it can perform its work properly. The increase of speed is in the ratio of the square roots of the radii, or the cube roots of the powers employed.

This result is very important to vessels intended for long voyages, where the great quantity of coals with which they are required to be laden, so much increases the immersion of the paddles, that the engine is not able to exert more than two thirds or three fourths of its full power. In such cases an increase of speed will be given, amounting to nearly one mile an hour, by reducing the diameter of the wheel so as to allow the engine to perform its full duty; and at the same time the consumption of fuel is but little increased, as is shown by Captain Austin's experiments on the Salamander.

8th. That an advantage would be derived from a wheel of large diameter, as far as the immersion of the paddle produced by loading the vessel is concerned, as it would not so sensibly affect the angle of inclination at which it entered the water. This, however, cannot be attained advantageously with an engine of the same length of stroke, because to allow it to make its full number of strokes with the large wheel the size of the paddles must be diminished, which is a much greater evil than a wheel of small diameter with large paddles. To have larger wheels, it is therefore either necessary to have the engines made with longer strokes, or to have the paddle-wheel on a different shaft, in order to diminish their speed. These are both practical inconveniences in sea boats, and I therefore consider the wheels to have gained their greatest limit in point of diameter. In the navigation of rivers, where a much greater speed can be attained, wheels of larger diameter may probably be required.

The ill effect of making the wheels of too large diameter, and the paddles too small, is very sensibly exhibited in the experiments on the Medea; her engines have the same length of stroke as those of the Salamander, Phœnix, and Rhadamanthus; and the wheel is twenty-one feet in diameter to the centre of pressure, while those of the latter vessels are not above eighteen feet five inches, or eighteen feet six inches. The consequence is a considerable loss of power, from the greater velocity of the wheel than of the ship. This loss of power is of course still small compared with that of the common wheel when deeply immersed, so that in the experiment at Sheerness her superiority of speed is perfectly consistent with the preceding calculations; at the same time I have no hesitation in saying, that an increase of speed of half a mile per hour, at least, might be obtained by a smaller wheel and a greater surface of paddle-board.

This view of the case is satisfactorily confirmed by experiments on the Monarch steam vessel. A former experiment on this vessel, not reported in the Table, with a wheel of less diameter and larger floats, gave a speed above eleven miles per hour. In the experiment given in the Table the average velocity is 10·8, so that a sensible diminution of speed was produced by enlarging the diameter of the wheel.

XVII. *On the Generation of the Marsupial Animals, with a Description of the Impregnated Uterus of the Kangaroo. By* RICHARD OWEN, *Esq., M.R.C.S. and Assistant Conservator of the Museum of the Royal College of Surgeons, London. Communicated by Sir* ANTHONY CARLISLE, *F.R.S.*

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THE *Marsupiata*, or *Animalia crumenata*, as the learned SCALIGER designated the few American species which were known in his time, now form in the systems of natural history an extensive series, embracing genera nourished by every variety of food, and exercising in quest of it as many different modes of locomotion as have been observed in other quadrupeds. Their instruments of progression, prehension, and digestion accordingly exhibit corresponding modifications of structure; while in other parts of their organization peculiarities are found to prevail with a degree of uniformity that justifies the consideration of the *Marsupiata* as a distinct group of *Mammalia*.

In all the genera of this group the uterus is double, and the true vagina is separated, either wholly or for a considerable extent, into two lateral canals. Both the digestive and generative tubes terminate within a common cloacal outlet, and the term *Monotremata*, therefore, though confined to the Edentate *Marsupiata*, is so far applicable to the whole of this aberrant division.

As the females approach the Oviparous *Vertebrata* in their separate genital tubes, so also the males resemble them in the peculiar structure and connexions of the intromittent organ; thus, in the *Macropi*, the *Dasyuri* and the *Phalangistæ*, the corpora cavernosa penis have the same position below the pubis, with the same want of ligamentous attachment to the bony pelvis; and the glans has the same bifurcated form and double groove for the transmission of the semen as in the Opossum, in which these peculiarities of the male organs were first accurately described by COWPER*.

In those genera in which the females have an inward fold of integument, or abdominal pouch, the males have an outward duplicature in the corresponding situation for the lodgement of the testes, which are thus placed anterior to the penis; and it is a remarkable fact, that the muscle which surrounds the mammary gland in the one sex is analogous to the suspensory cremaster of the testis in the other.

Both sexes in the marsupial genera manifest also their affinity to the oviparous classes in possessing two superior venæ cavæ, and in the want of the inferior mesenteric artery: and the marsupial bones, so common in the skeletons of reptiles, are

* Philosophical Transactions, vol. xxiv. (1704.) p. 1576.

limited in the mammiferous class to this division, in which alone, from the peculiarly brief period of uterine gestation, and the consequent non-enlargement of the abdomen, their presence might be expected. But these bones serve important purposes in relation to the generative economy of the *Marsupiata.* In the female they assist in producing a compression of the mammary gland necessary for the alimentation of a peculiarly feeble offspring, and they defend the abdominal viscera from the pressure of the young as these increase in size during their mammary or marsupial existence, and still more when they afterwards return to the pouch for temporary shelter. In the males, with the exception of the edentate genera, the marsupial bones, from their relation to the cremaster muscles, which wind round them like pulleys, assist in the compression and retraction of the testes during coition; a process which, from the peculiar position of the scrotum, has been supposed to differ from that of other quadrupeds. A recent opportunity, however, of observing the coitus of the Kangaroo at the Zoological Gardens, proves that there is no difference as to position, which is the same as in the Dog, but that it is chiefly remarkable for the frequent repetition of the act during a long-continued embrace. The peculiar length and tortuosity of the double vagina, for which the bifurcated glans of the male organ is adapted, may render necessary so efficient a process; and as the testes are then retracted entirely out of sight, it would seem that the marsupial bones have the same relation in the male to their secretion as they have in the female to that of the mammary glands.

The minute size of the young of the American Opossum when found in the marsupium, their pendulous attachment to the nipples, and perhaps the mode in which the latter were developed, gave rise among the earlier observers to a supposition that they were originally formed from those parts, and the gemmiparous theory, which has subsequently often been revived, appears to have been prevalent at the time when TYSON first devoted his attention to the subject.

The discovery of the uterus of the Opossum, recorded by TYSON in the twentieth volume of the Philosophical Transactions (p. 139.), was the first step towards a correct explanation of the generative economy of the *Marsupiata.* That learned and accurate anatomist offers a true conjecture as to the parts of their complex uterine apparatus in which the processes of gestation are carried on, which he denominates the *cornua uteri,* and is the first who distinguishes the true vaginæ from the "*common passage,* or *canalis,*" subsequently termed the *urethro-sexual canal.*

The subject of marsupial generation has ever since been regarded as one of peculiar physiological interest, and the labours of HUNTER*, HOME†, GEOFFROY ST. HILAIRE‡,

* Zoological Appendix to WHITE's New South Wales, p. 272.

† Philosophical Transactions, vol. lxxxv. (1795); Lectures on Comparative Anatomy, iii.

‡ 1) Journal Complémentaire du Dictionnaire des Sciences Médicales, tom. iii. p. 193. (1819.) "Si les animaux à bourse naissent aux tétines de leur mère?" 2) Système sexuel des Animaux à bourse, Mém. du Mus. tom. ix. p. 193. (1822.) 3) Anatomie Philosophique, tom. ii. pp. 354, 397. 4) Art. *Marsupiaux,* Dict. des Sciences Nat. tom. xxix. (1823.)

Blainville* and Morgan†, have more especially been devoted to its elucidation; but the attainment of a precise knowledge of the mode in which the embryo was developed,—the more desirable on account of the abovementioned affinities of the *Marsupiata* to the *Ovipara*,—has been prevented by the want of opportunities to examine the impregnated uterus, so as to determine the nature of the relations subsisting between the fœtus and mother.

This deficiency I have the good fortune to be in some degree enabled to supply through the exertions of my friend Mr. George Bennett, F.L.S., who during his recent travels in New South Wales procured the gravid uterus of a female of the large Kangaroo (*Macropus major*, Shaw), and safely transmitted it in spirits to the Museum of the Royal College of Surgeons in London, where it is now preserved, and where I have had the opportunity of examining it for the present communication, through the liberal permission of the President and Board of Curators.

The fœtus was contained in the left uterus (Plate VI. fig. 7. *c'*.), which was three times the diameter of the same part in the unimpregnated state. This uterus measured two inches in length and one inch three lines in diameter, exclusive of the Fallopian tubes. Its parietes varied in thickness from one line to two lines, being in the unimpregnated state about half a line; and this increase was not in the muscular coat, but was chiefly occasioned by the thickening of the internal membrane, which was produced into irregular folds and wrinkles, having, however, a smooth surface when put upon the stretch, and closely resembling the same part in the uterus of the *Ornithorhynchus paradoxus*.

The fœtus had been exposed by a longitudinal incision through the coats of the uterus, and a corresponding one in the most exterior of its envelopes, but had not apparently been otherwise disturbed. It was bent upon itself in the usual manner, with the nose almost touching the thick stem of the embryonic membranes, or umbilical chord. Its whole length in a straight line was seven lines, but when measured along the curve of the back to the extremity of the tail, its length was one inch four lines; the length of the head three and a half lines.

The examination into the nature of the connexion between the mother and fœtus was made in presence of Mr. Clift, for whose kind and valuable assistance I am much indebted.

The edges of the uterus where it had been cut open by Mr. Bennett were carefully examined with the lens, whilst immersed in clear spirit, but no trace of a divided placenta could be detected; the pulpy projections from these edges were satisfactorily seen to be folds of the lining membrane. The only point to which the fœtal membranes seemed to adhere was near the cut margin of the uterus on the right side, but this was found to arise from insinuation of the chorion between folds of the lining

* "Sur les Organes femelles de la Génération et les Fœtus des Animaux didelphes;" Bulletin de la Société Philomathique, 1818, p. 25.

† Transactions of the Linnean Society, vol. xvi. pp. 61, 455.

membrane, which came away when these were separated; and there was not any appearance of a placental structure, or of villi, or a determination of vessels to this point, on either of the opposed surfaces of the chorion or uterus. The greater part of the membranes of the fœtus was collected into a wrinkled mass, which was removable from its position, together with the fœtus, by the slightest pressure of the probe.

The chorion was extremely thin and lacerable; and upon carefully examining its whole outer surface, no trace of villi or of vessels could be perceived. Detached portions were then placed in the field of a microscope, but without the slightest evidence of vascularity being discernible. The next membrane, whose nature and limits will be presently described, was seen extending from the umbilicus to the inner surface of the chorion, and was highly vascular. The fœtus was immediately enveloped in a transparent amnios. The four extremities and tail were very obvious, but the toes of the hind legs were not developed. The nostrils were open and proportionately large; the eyelids were not fully formed, but allowed a little of the eyeball to appear. The tongue projected from the mouth, which, from the imperfect growth of the jaws, appeared more naturally open than in the mammary fœtus. The auditory passages were indicated by slight longitudinal depressions, below which the branchial apertures, one on either side, each about half a line in length, were very conspicuous. On dilating these apertures two passages were seen leading from each of them to the pharynx. The anterior extremities were well set off from the body, and the five toes on each were very apparent, terminated by minute glistening horny claws. The length of each fore leg was two lines, that of the hind leg only one line, terminated by a flattened, undivided, club-shaped mass. The tail was two lines long, thick and strong at the commencement. Impressions of the ribs were visible at the sides of the body. The membranous tube of the spinal marrow was visible along the back between the ununited elements of the vertebral spines. Posterior to the umbilical chord there was a small projecting penis, and behind that, on the same prominence, was the anus, which was pervious.

The external membrane, or chorion (Plate VII. fig. 1. *a.*), corresponded in extent with the enlarged cavity formed by the plication of the lining membrane of the uterus which contained the fœtus. Its texture in every part was delicate and almost friable; it was opake and minutely wrinkled. A reiterated examination was made, with a view to discover any trace of a vascular or villous structure, such as exists in the chorion of the Mare and Sow; but the chorion presented in this respect a similar condition with the membrana putaminis or membrana corticalis of the oviparous embryo.

On turning the chorion away from the fœtus, it was found to adhere to the vascular membrane above mentioned, into which the umbilical stem suddenly expanded. With a slight effort, however, the two membranes could be separated from each other without laceration for the extent of an inch; but at this distance from the umbilicus the chorion gave way on every attempt to detach it from the internal vascular membrane, which here was plainly seen to terminate in a well-defined ridge, formed by the trunk of a blood-vessel.

When the whole of the vascular membrane (Plate VII. fig. 1. *c.*) was spread out, its figure appeared to have been that of a cone, of which the apex was the umbilical chord, and the base the terminal vessel above mentioned. Three vessels could be distinguished diverging from the umbilical chord, and ramifying over it. Two of these trunks contained coagulated blood, and were the immediate continuations of the terminal or marginal vessel; the third was smaller, empty, and evidently the arterial trunk. Besides the extremely numerous ramifications dispersed over this membrane, it differed from the chorion in being of a yellowish tint, which is still perceptible in the preparation. No trace of any other membrane could be seen extending from the fœtus besides the two above described, and the amnios (Plate VII. fig. 1. *b.*), which was reflected from the umbilical chord, and formed, as usual, the immediate investment of the fœtus.

The umbilical chord measured two lines in length and one in diameter. It was found to contain the three vessels above mentioned, with a small loop of intestine; and from the extremity of the latter, a filamentary process was continued to the vascular membrane. The margins of the umbilicus or abdominal opening were very strong, offering much resistance to their division. On tracing the contents of the chord into the abdomen, the two larger vessels with coagulated blood were found to unite; the common trunk then passed backwards beneath the duodenum, and after being joined by the mesenteric vein, went to the under surface of the liver, where it penetrated that viscus: this was consequently an omphalo-mesenteric or vitelline vein. The third vessel passed between the convolutions of the small intestine, along the mesentery, to the abdominal aorta, corresponding to an omphalo-mesenteric or vitelline artery. The membrane, therefore, upon which they ramified answered to the vascular layer of the germinal membrane, which spreads over the yolk in the oviparous animals, or to the umbilical vesicle of the embryo of ordinary *Mammalia*. The filamentary pedicle which connected this membrane to the intestine was given off near the end of the ileum, and not continued from the cæcum *, the rudiment of which was very evident half a line below the origin of the pedicle.

The small intestine above the pedicle was disposed in five folds. The first from the stomach, or duodenum, curved over the vitelline vein, and the remaining folds were disposed around both the vitelline vessels. From the cæcum, which was given off from the returning portion of the umbilical loop of intestine, the large intestine passed backwards to the spine, and then, bent at a right angle, straight down to the anus. The stomach did not present any appearance of the sacculated structure so remarkable in the adult, but had the simple form of a carnivorous stomach.

* The umbilical vesicle, though small, is very conspicuous in the embryo of the Porpesse when two inches in length, but there is no cæcum in this animal; affording, therefore, with the Kangaroo, both negative and positive proof that the cæcum in *Mammalia* is not a remnant of the yolk-duct.

The liver consisted of two equal and symmetrically disposed lobes. The vena portæ was formed by the union of the vitelline with the mesenteric and doubtless the other usual veins, which were, however, too small to be distinctly perceived. The diaphragm was very perfectly formed.

The vena cava inferior was joined, above the diaphragm, by the left superior cava, just at its termination in a large right auricle. The ventricles of the heart were completely joined together, and bore the same proportions to each other as in the adult; a perfection of structure which is not observed in the embryos of ordinary *Mammalia* at a corresponding period of development. The pulmonary artery and aorta were of nearly the same proportionate size as in the adult: the branches given off by the former to the lungs were of considerable size. The ductus arteriosus, on the contrary, was remarkably small. The aorta, prior to forming the descending trunk, dilated into a bulb, from which the carotid and subclavian arteries were given off. This bulb, which is permanent in Fish and perennibranchiate Reptiles, is always a prominent structure in the earlier stages of the embryonic heart in pulmoniferous *Vertebrata*. I was unable to trace the branchial arteries so satisfactorily as the conspicuous nature of the external openings had led me to expect, owing to the long maceration of the specimen.

The lungs were of equal size with the heart, being about a line in length, and nearly the same in breadth: they were of a spongy texture, and of a red colour, like the veins, from the quantity of blood they contained. This precocious development of the thoracic viscera is an evident provision for the early or premature exercise of the lungs as respiratory organs in this animal; and on account of the simple condition of the alimentary canal, the chest exceeds the abdomen in size.

The kidneys had the same form and situation as in the adult. The supra-renal glands were half the size of the kidneys.

The testes were situated below the kidneys, and were one half larger than those glands, the superiority of size depending on their large epididymis. They continue within the abdomen for six weeks after uterine birth.

There was no perceptible trace of an allantois, nor even of an urinary bladder, in this fœtus*.

With respect to the largely developed umbilical vesicle, as it had been laid open before the parts were immersed in spirit, the nature of its contents could not be ascertained; but the quantity of what must be supposed to have been nutritious material had evidently been abundant. As the affinity of the *Marsupiata* to the oviparous *Vertebrata* has been believed to be manifested more particularly in the development of the ovum and ovisac, or corpus luteum, it may not be superfluous to offer a few observations on the differences observable in this respect between *Mammalia* and

* I have subsequently detected the remains of a urachus and of umbilical or vesical arteries in a mammary fœtus of a Kangaroo about a fortnight old, and of a urachus in very small mammary fœtuses of a *Petaurus pygmæus* and of a *Phalangista*.

Aves, in order to show the extent and nature of the correspondence above alluded to, and at the same time to form a probable opinion of the source whence the contents of the umbilical vesicle had been in the present instance derived. In Birds, the material of the yolk is added to the efficacious part of the ovum (the vesicle of Purkinje,) while in the ovary, and the ovum consequently acquires a considerable size from the accumulation of the vitelline matter before it passes into the oviduct, which presents a corresponding capacity for its reception. De Graaf long ago observed, that small as were the perfected ova in the ovaries of quadrupeds, in comparison with those in the ovaries of birds, yet the vesicle which was discovered after impregnation in the Fallopian tube was still more minute than the ovarian vesicle from which he conjectured that it had escaped. Thus, in his celebrated experiments on the Rabbit, he observes of the female organs seventy-two hours after impregnation: "In altero autem testiculo quatuor folliculos invenimus, quorum tres aliquantulum magis lucidi, minorique foramine pertusi videbantur, in quorum etiam medio tantillum limpidissimi liquoris adinvenimus: at quartus folliculus obscurior erat, nec quicquam liquoris in se continebat; quamobrem ovum ex hoc folliculo elapsum suspicabamur, quâ de causâ ejusdem lateris cornu et oviductum perscrutati sumus, ac unicum tantùm ovum in ipso cornu principio deprehendimus, perpusillis alterius lateris ovis simillimum *."

Mr. Cruikshank, who repeated these experiments of De Graaf, succeeded in detecting the ovulum in the Fallopian tube of the Rabbit on the fourth day after impregnation, and believes that he saw it in the corpus luteum, as he terms the Graafian follicle, observing, "The pouting part I believe is the ovum, and stands upon the top of the corpus luteum: it is very vascular, particularly at its basis; but as soon as perfect, or ready for expulsion, carries no red blood: it continues to grow of itself in utero, without adhering to the uterus for two or three days, then takes root and becomes very vascular †."

Any doubts that might still remain as to the pre-existence of the ovulum in the Graafian follicle of the *Mammalia* have been in great measure dispelled by the more recent and accurate observations of Von Baer, who considers it identical with the pellucid vesicle a short time before discovered by Purkinje in the ovary of the Fowl, around which the yolk accumulates to form the ovum prior to its passage into the oviduct.

Escaping then from the ovary without being inclosed within this superadded material, the ovulum in *Mammalia* passes along the oviduct or Fallopian tube, which is consequently of small diameter. It increases in size, according to De Graaf and Von Baer, as it passes along the tube, by imbibition of nutrient material; and this mode of increase goes on rapidly after it has reached the uterus. The granules contained in the so-imbibed fluid accumulate at its periphery, and constitute the germinal membrane; and while the ovum yet floats freely in the uterus, villi, in the ordinary *Mammalia*, are observed to shoot out from the chorion or external membrane. But

* De Graaf, Opera Omnia, p. 400.

† Philosophical Transactions, vol. lxxxvii. p. 206.

notwithstanding the different condition of the ovum of the bird when it reaches the oviduct, it must be observed that the material to be employed in constructing the embryo is derived, as in *Mammalia*, from the oviduct. It is the white of the egg which disappears during incubation, while the greater part of the yolk is inclosed in the abdomen of the chick at the conclusion of that process; so that, although the yolk has a prior existence to the albumen, and is generated in the ovary itself, it is analogous in its function to the milk which nourishes the new-born mammal.

Without, however, entering into the further uses of the yolk in birds, which affords an admirable example of prospective design, it is sufficient for the present purpose to observe, that while it affords the chief differential feature between the oviparous and mammiferous ovum as this is first received into the oviduct, so a corresponding difference is manifested in the structure of the recipient tube as well as in the ovary itself.

Now the true Fallopian tubes of the Kangaroo closely resemble, both in relative size and in structure, those of the ordinary *Mammalia*. The difference is manifested in the greater proportional extent of the fimbriated extremity and its relations to the ovary, which are circumstances in which the ordinary *Mammalia* also differ among each other.

From this accordance, therefore, and from the circumstance of the young being nourished after birth by the secretion of mammary glands, it may be safely concluded that the ovulum in the Kangaroo quits the ovisac in a condition corresponding to that in the ordinary *Mammalia*, and increases in a similar manner as it descends to the uterus.

Additional evidence in favour of a correspondence in the original development of the primordial germ in marsupial and ordinary *Mammalia* is derivable from the structure of the ovary itself, and especially from its appearance after impregnation.

The cellular structure or parenchyma of the ovary, in which the ova are developed, is as dense in the Kangaroo as in the ordinary *Mammalia*. The cavity of the Graafian follicle or ovisac from which the germ of the fœtus above described had escaped, was partly occupied by coagulated caseous substance, and partly by the irregularly thickened membrane of the ovisac; thus forming a corpus luteum* as in other *Mammalia*: this was of a large size in proportion to the rest of the ovary, and the external orifice, probably from the abundance of the secretion as well as from the dense structure of the ovarian capsule, had not become cicatrized. In Birds, on the contrary, owing to the delicate and yielding nature of the stroma ovarii and from the tenuity of the capsule of the ovary, permanent corpora lutea are not formed except under accidental circumstances.

* The corpus luteum in another Kangaroo, six months after impregnation, I found to be composed of a spherical body from two to three lines in diameter, of a pink colour and fleshy substance; the membrane covering this body was vascular, and the cicatrix had nearly disappeared. A corpus luteum, eighteen months after impregnation, was of smaller size, did not project from the ovary, and was of a dark colour and firm texture.

Actual observation can alone, however, be relied upon to establish satisfactorily the precise relations subsisting between the *Marsupiata* and ordinary *Mammalia* at the first stages of their development; but so far as analogous reasons can be deduced from observations on the structures immediately concerned, there appears no ground for concluding that any material difference exists in the formation of the ovum in the ovary, or the condition in which it arrives at the uterus. But in the Kangaroo the uterus is evidently destined, from the great development of the lining membrane, to afford an abundant secretion for the increase of the ovum after it has passed into that cavity; and the chorion, when two thirds of gestation have elapsed, still manifests the same condition as in the earliest period of the ovum in ordinary *Mammalia*. No villi have been put forth from its external surface, no adhesion has taken place between it and the inner membrane of the uterus, nor does it appear to have been organized in any part so as to act as a placenta. Granting, therefore, that the membrane organized by the omphalo-mesenteric vessels is an adequate medium for the transmission of the nutrient material to the embryo, it still remains to be determined how its respiration is effected. It is, however, very probable, that notwithstanding the interposition of the chorion, a chemical combination does take place between the carbon of the fœtal blood distributed over the widely extended umbilical vesicle, and the oxygen of the maternal arterial blood distributed over the highly vascular lining membrane of the uterus; and this interchange may be sufficient for the purposes of a fœtus so imperfect, and during an uterine existence so peculiarly brief, as in the Kangaroo.

In the ova of Fishes, the vascular membrane expanded over the yolk, not being separated from the membrana corticalis by an intervening mass of albumen, suffices for respiration as well as nutrition, until the permanent respiratory organs, the gills, are sufficiently developed. And in the higher Reptiles and Birds, the temporary structure superadded to the vascular covering of the yolk, for the more express purpose of eliminating the effete particles of the growing embryo, does not begin to expand until a late stage of formation.

In the Hunterian series of the incubation of the Gosling, the development of the embryo is seen to have advanced to the formation of the head and eyes, and to the distinct production of the four extremities, whilst the allantois is yet a small vesicle protruding at the posterior extremity of the abdomen; and until this membrane, by its rapid increase and the coextension of the umbilical vessels, has attained to, and spread itself over, the inner surface of the shell, it is still more difficult to explain the mode in which respiration is effected in the embryo of the Bird than in that of the Kangaroo.

But limited as these embryos are in their vital actions to that of simple growth, a more perfect means of respiration would seem unnecessary; and among the inferior animals, the *Entozoa* exhibit to us beings totally excluded from the atmosphere, yet enjoying still greater powers of action, and in the Nematoidean order, even generating by distinct sexes, without the slightest trace in their structure of a respiratory apparatus.

As in the Bird, however, so in the ordinary *Mammalia* there is a period when a temporary respiratory organ is essential to the continuance of intra-uterine existence. But previous to the production of this, it is now ascertained that every mammal is developed by means of omphalo-mesenteric vessels; and it is interesting to observe, that the membrane over which they are spread, or the umbilical vesicle, is largest and most permanent in the order *Glires,* which, next to the *Marsupiata,* exhibit the most striking affinities to Birds, and to which the *Marsupiata* pass by the most natural and unbroken transition.

The temporary organ for the elimination of the effete particles of the mammiferous embryo is at first, as in birds, an allantois, which, extending from the lower part of the intestine, is developed in different proportions in the different orders; but the umbilical vessels coextended with it rapidly seek a more intimate contact with the vascular surface of the womb, and accordingly proceed to organize the chorion, shooting out into villi, either extended over the whole surface, as in the Mare; or disposed in circumscribed tufts, as in the Ruminants; or limited to one particular spot, as in the Human subject and in all the ordinary Unguiculate quadrupeds.

It would appear, indeed, from the examination of the small mammary fœtuses of the Kangaroo, Petaurus, and Phalangista before mentioned, that an allantois and umbilical arteries are developed at a later period of gestation than the uterine fœtus here described had arrived at. None of the above specimens, however, presented any trace of an umbilical vein extending to the liver, and I therefore regard it as improbable that the umbilical arteries spread over the chorion to organize a placenta. As, moreover, the uterine fœtus preserved by Mr. Bennett had attained two thirds of its full size as such, it is not likely that the allantois would afterwards be developed further than to serve as a receptacle of urine; and it is interesting to observe that it is arrested at this point in the Batrachian Reptiles. So far, therefore, as the evidence of this specimen goes, it may be concluded that the chorion does not become organized, and that the *Marsupiata* are essentially ovoviviparous.

In regard to their fœtal membranes and appendages they resemble the embryos of the Fowl and ordinary *Mammalia* at the earliest stages of development; but when growth has extended so far as to render respiration by an umbilical vesicle no longer adequate to the continuance of intra-uterine existence, instead of a temporary structure being specially organized for that purpose, the lungs are precociously developed, and the embryo of the marsupial quadruped quits the uterus prematurely, and breathes the air.

§ 2. *On the Mammary Fœtus of the Marsupiata.*

The period of uterine gestation in the Virginian Opossum, according to Barton, is twenty-six days; the accuracy of which observation is confirmed by Rengger's experiments on an Opossum (*Didelphys Azaræ*, Temm.) of about the same size, in which the uterine gestation lasted twenty-five days.

In the Kangaroo (*Macropus major*, Shaw,) uterine gestation continues thirty-nine days, which seems still more remarkable for its shortness, if we consider the difference in the size of the Opossum and Kangaroo. A longer period has, indeed, been stated to elapse between the impregnation and parturition of the latter animal; but the precautions taken to ensure accuracy in the observations which led to the determination of the period of thirty-nine days were such as to admit of no source of error; and the size of the mammary fœtus figured by Sir Everard Home in the 85th volume of the Philosophical Transactions could hardly be reconciled with the duration of four months assigned to uterine gestation in the Kangaroo by Professor Geoffroy St. Hilaire *.

The period of gestation was determined in a female Kangaroo in the Menagerie of the Zoological Society. She was placed with the male only at such times as they could be watched. She had a young one of the previous year, which had quitted the pouch, but was still sucking occasionally. The coitus was observed on the 27th August 1833, at 1 p.m.; and as it was known that she could not have had intercourse with the male for three months prior to this date, that fact, and the size of the young one when she was selected for the experiment, left no doubt of her being unimpregnated until the date above mentioned. She was separated from the male the same day, and was kept apart in a separate shed and paddock until parturition took place. And here I must express my great obligations to the Council of the Zoological Society for the permission given me to conduct my experiments under circumstances so favourable as are afforded in their noble Vivarium: every arrangement in the building appropriated to the kangaroos which could facilitate my observations on them was promptly effected, and the services of an intelligent keeper were allotted to me.

In order to inure the female to the examinations of the pouch when they should become indispensable, they were commenced six days after the copulation, and were repeated every morning and evening until the 5th October, when, at 7 a.m., the fœtus was discovered in the pouch attached to the left superior nipple. On the preceding day, at the same hour, a greater quantity of the moist brown secretion peculiar to the pouch was noticed, indicating a commencing determination of blood to that part; and at different periods during the day the female was observed to put her head into the pouch and lick off the secretion. When she was again examined at six o'clock in the evening, a slight increase of the secretion was the only perceptible change in the state of the pouch; but there was no appearance in the nipples indicative of the event so soon about to take place.

The nipple in use by the young one of the previous year was the right superior, or anterior one: it was nearly two inches in length, and one third of an inch in diameter, while the other three were about half an inch in length, and about a line in diameter. I took notes of the appearance of the marsupium on the 6th, the 10th, 15th,

* Annales des Sciences Naturelles, tom. ix. p. 340.

21st, 30th, and 38th days of uterine gestation: no material alteration was, however, observable till after the death of the young Kangaroo of the previous year, which took place on the 25th day, when the brown secretion first began to appear, and the nipple that had been in use, to diminish.

As parturition took place in the night, the mode of transmission to the pouch was not observed. No blood or albuminous discharge could be detected on the litter, nor any trace of it on the fur between the vagina and orifice of the pouch; but these might have been removed by the mother. The appearances presented by the little one thus detected within twelve hours after being deposited in the pouch were as follow:—It resembled an earth-worm in the colour and semitransparency of its integument, adhered firmly to the point of the nipple, breathed strongly but slowly, and moved its fore legs when disturbed. Its body was bent upon the abdomen, its short tail tucked in between the hind legs, which were one third shorter than the fore legs, but with the three divisions of the toes now distinct. The whole length from the nose to the end of the tail, when stretched out, did not exceed 1 inch 2 lines.

On the 9th of October I again examined the pouch: the young one was evidently grown, and respired vigorously. I determined to detach it from the nipple for the following reasons: 1st, to decide the nature of the connexion between the fœtus and nipple; 2ndly, to ascertain, if possible, the nature of the mammary secretion at this period; 3rdly, to try whether so small a fœtus would manifest the powers of a voluntary agent in regaining the nipple; and, lastly, to observe the actions of the mother to effect the same purpose, which one might presume would be instinctively analogous to those by means of which the fœtus was originally applied to the nipple.

With respect to the first point, I was aware that the Hunterian dissections as exhibited in the preparations in the Museum of the College, and the observations of Mr. Morgan * and Mr. Collie †, concurred in disproving the theory of a vascular mode of connexion between the mammary fœtus and the nipple; nevertheless, as a discharge of blood had been stated by Geoffroy St. Hilaire to accompany marsupial birth, or the spontaneous detachment of the fœtus from the nipple ‡, and even the anastomoses and distribution of the continuous vessels in the neck of the fœtus had been speculated on by him §, it became desirable to have ocular demonstration of the real state of the facts. The fœtus retained a firm hold of the nipple: when it was detached, a minute drop of whitish fluid, a serous milk, appeared on the point of the

* Transactions of the Linnean Society, vol. xvi. p. 459.

† Zoological Journal, No. xviii.

‡ "Car le sang aperçu à la litière est un indice qu'à ce moment le fœtus s'est detaché de la tétine, et qu'il est né definitivement à la manière des marsupiaux."—Geoffroy St. Hilaire, Annales des Sciences Naturelles, tom. ix. p. 342.

§ "Des vaisseaux nourriciers se repandroient-ils des parties du pharynx le long et entre les lames de la trachée artère pour entrer dans le cœur, et (conjecture de M. Serres,) la gland thyroide seroit-elle le point de leur reunion? J'ai manqué des sujets pour vérifier ses aperçus."—Geoffroy St. Hilaire, Mém. du Muséum, tom. xix. p. 406.

nipple. About half a line of the extremity of the nipple had entered the mouth, which extremity was of smaller diameter than the rest of the nipple, not being as yet so compressed by the contracted orifice of the mouth as to form a clavate extremity, such as it afterwards presents. The young one moved its extremities vigorously after being detached, but did not make any apparent effort to apply its legs to the integument of the mother, so as to creep along, but seemed, in regard to progressive motion, to be perfectly helpless. It was deposited at the bottom of the pouch, and the mother was liberated, and carefully watched for an hour.

She immediately showed symptoms of uneasiness, stooping down to lick the orifice of the vagina, and scratching the exterior of the pouch. At length she grasped the sides of the orifice of the pouch with her fore paws, and drawing them apart, as in the act of opening a bag, she thrust her head into the cavity as far as the eyes, and could be seen moving it about in different directions. During this act she rested on the tripod formed by the tarsi and tail. She never meddled with the pouch while in the recumbent posture, but when stimulated by uneasy sensations, she immediately rose and repeated the process of drawing open the bag and inserting her muzzle, sometimes keeping it there for half a minute at a time. I never observed that she put her fore legs into the pouch; they were invariably employed to widen the orifice. When she withdrew her head, she generally concluded by licking the orifice of the pouch and swallowing the secretion. After repeating the above act about a dozen times she lay down, and seemed to be at ease.

The freedom with which the mother reached with her mouth the orifices both of the genital passage and pouch, suggested at once a means adequate to the removal of the young from the one to the other; while at the same time her employment of the fore paws indicated that their assistance in the transmission of the fœtus need not extend beyond the keeping open the entrance of the pouch while the fœtus was being introduced by the mouth, when it is thus probably conducted to, and held over, a nipple until the mother feels that it has grasped the sensitive extremity of the part from which it is to derive its sustenance.

This mode of transmission is consistent with analogy, the mouth being always employed by the ordinary quadrupeds, as Dogs, Cats, and Mice, for the purpose of removing their helpless offspring. It accords, also, with the phenomena better than those which have been previously proposed; for it is now ascertained, by repeated dissections both of the Kangaroo and Opossum, that there is no internal passage from the uterus to the marsupium; and if the genital outlet can be brought into contact with the orifice of the pouch in the dead Kangaroo by means of great stretching of the relaxed parts, yet such an action has never been witnessed in the living animal*: the tender embryo would be more liable to receive injury from the fore paws; and these, from the absence of a thumb, could not so effectually ensure its passage as the

* This argument is not applicable to those *Marsupiata* which, like *Perameles* and the smaller South American Opossums, have the duplicatures of integument forming the pouch extended close to the cloaca.

lips, which can be opposed to each other. Lastly, the young one did not by any of its actions encourage the idea of its possessing the power of instinctively creeping up to the nipple.

When the female had rested quiet for about half an hour we again examined her, and found the young one not at the bottom of the pouch, but within two inches of the nipple; it was breathing strongly, and moving its extremities irregularly as before. I made an attempt to replace it on the nipple, but without success, and the mother was then released. On an examination two days afterwards, the marsupium was found empty. Every portion of the litter was carefully searched in the hopes of finding the fœtus, but without success. The mother, therefore, owing to the disturbance of the young one, had probably destroyed it. This was a result I had not expected, for the head keeper at the Zoological Farm had twice taken a mammary fœtus from the nipple and pouch of the mother soon after it had been deposited there, and when it did not exceed an inch in length, and it had each time again become attached to the nipple. I afterwards saw this fœtus attached to the nipple, and it continued to grow, without having sustained any apparent injury from the separation, until the death of the mother, when it was nearly ready to leave the pouch. A similar result occurred to Mr. Collie, who, in the letter above quoted*, observes, "I was informed, to my no small delight, that a kangaroo had been caught with its little young in the sac at the teat. This young one, which has not obviously increased since, is of nearly the size of the last and half the middle joint of one's little finger; its integuments are of a flesh colour, and so transparent as to permit the higher coloured vessels and viscera to shine through them, whilst all its extremities seem completely formed; and its muscular power is fully testified by its evident efforts in sucking, during which it puts every part of its body into action. According to the testimony of the person who preserved the mother with this little one for me, the latter by no means passes the whole of its time with the lacteal papilla in its mouth, but has been remarked, more than once, without having hold of it. It has even been wholly removed from the sac to the person's hand, and has always attached itself anew to the teat. Yesterday, on again looking at it, I gently pressed with the tip of my finger the head of the little one away from the teat of which it had hold, and continued pressing a little more strongly for the space of a minute altogether, when the teat, which had been stretched to more than an inch, came out of the young one's mouth, and showed a small circular enlargement at its tip, well adapting it for being retained by the mouth of the sucker. After this I placed the extremity of the teat close to the mouth of the young, and held it there for a short time without perceiving any decided effort to get hold of it anew, when I allowed the sac to close, and put the mother into her place of security. An hour afterwards the young was observed still unattached, but in about two hours it had hold of the teat, and was actively employed sucking."

* Zoological Journal, No. xviii. p. 239.

Mr. Morgan tried a similar experiment with a mammary fœtus about the size of a Norway Rat, which after two hours' separation from the nipple regained its hold, and sustained no injury from the interruption of the supply of nourishment. The evidence, therefore, which has been adduced establishes the fact that the mammary fœtus at a very early period is at least capable of sustaining a separation from the nipple; and although it may not at this stage of growth possess the power of regaining its hold by its own unaided efforts, it is far from being the inert and formless embryo which it has been described to be: it resembles, on the contrary, in its vital powers, the new-born young of the smaller *Mammalia* rather than the uterine fœtus of a larger species when at a period of development at which this corresponds in size to a new-born Kangaroo.

By comparing the new-born Kangaroo with such a fœtus, we find that although in the Kangaroo the ordinary laws of development have been adhered to in the more advanced condition of the anterior part of the body and corresponding extremities, yet that the brain does not present so disproportionate a size; and the same difference is observable in the uterine fœtus of the Kangaroo (Plate VII. fig. 3.), even when compared with the same-sized embryo of an animal of an inferior class (Plate VII. fig. 4.). This difference, I apprehend, is owing to the rapidity with which the heart and lungs acquire their adult structure in the Kangaroo, whereby the passage of the purer and more nutritious blood through the foramen ovale and left auricle to the primary branches of the aorta is arrested. The brain, however, of the mammary fœtus, though exhibiting a low degree of development, yet is of a firmer texture than in a similarly sized fœtus of a Sheep, and attains its ultimate proportions by a more gradual process of growth.

The brain and spinal chord (Plate VII. fig. 9—12.) were taken from a mammary fœtus of the Kangaroo, which measured one inch and a half in length, and which was kindly presented to me by Mr. Allan Cunningham, of Kew.

In this fœtus I first observed the urinary bladder developed, and adhering by its apex to the peritoneum exactly opposite that part of the abdominal integument where a small linear ridge indicated the previous attachment of the umbilical appendage. There were also minute but distinct traces of umbilical arteries running up the sides of the bladder to this point of attachment. As the urinary bladder becomes afterwards expanded, the peritoneum is gradually, as it were, drawn from this part of the abdominal parietes, forming an anterior ligament of the bladder. In a mammary fœtus of the Kangaroo about a month older than the above, there was at the superior part of this duplicature a small projecting point from the bladder, like the remains of a urachus; but the fundus, now developed considerably above this point, was covered with a perfectly smooth layer of peritoneum, and it is this, I apprehend, which has given rise to the belief that there was no trace of urachus or umbilical arteries in the fœtuses of the *Marsupiata*. In the Sloth, the Manis, and the Armadillo, the urachus is continued in the same manner from the middle of the anterior part of the bladder, and not from the fundus.

In neither of the above fœtuses of the Kangaroo was there any corresponding trace of umbilical vein, although there was a distinct ligamentum suspensorium hepatis, formed by a duplicature of the peritoneum descending from the diaphragm to the notch lodging the gall-bladder, and not entering, as usual, the fissure to the left of that notch.

The small intestines in the lesser mammary fœtus, when compared with those of the uterine fœtus above described, were found to have acquired several additional convolutions: the fold to which the umbilical vesicle had been attached was still distinct, but now drawn in to the back of the abdomen *. The cæcum was much elongated, but the remaining large intestines proportionately no more developed than in the uterine fœtus, resembling those of the Viverrine *Carnivora*; the subsequent modification, therefore, of the large intestines seems evidently destined to complete the digestion of the vegetable food.

The stomach was not sacculated, but the division between the cardiac and middle compartments was more marked than in the uterine fœtus. The liver had now ascended in its development beyond the oviparous form which it presented in the uterine fœtus, the right lobe being subdivided into three. The supra-renal glands bore the same proportionate size to the kidneys. The testes were still larger than the kidneys, and were situated below them, not having yet passed out of the abdomen: this takes place when the mammary fœtus is about three inches long from the nose to the root of the tail. The ductus arteriosus was distinct in the small mammary fœtus, but I could not perceive any trace of the thymus gland. Is this gland unnecessary on account of the precocious development of the lungs? or is its absence connected with the mode in which the fœtus in utero is developed? The latter appears the more probable condition of its absence, as in the oviparous and ovoviviparous classes the thymus gland is rudimental or of doubtful existence.

Notwithstanding that the new-born Kangaroo possesses greater powers of action than the same-sized embryo of a Sheep, and approximates more nearly in this respect to the new-born young of the Rat, yet it is evidently inferior to the latter. For although it is enabled by the muscular power of its lips to grasp and adhere firmly to the nipple, it seems to be unable to draw sustenance therefrom by its own unaided efforts. The mother, as Professor GEOFFROY † and Mr. MORGAN ‡ have shown, is therefore provided with a peculiar adaptation of a muscle (analogous to the cremaster) to the mammary gland, for the evident purpose of injecting the milk from the nipple into the mouth of the adherent fœtus. Now it can scarcely be supposed that the fœtal efforts of suction should always be coincident with the maternal

* This process may be compared to that by which the testes are drawn out of the abdomen.

† Mémoires du Muséum, tom. xxv. p. 48.

‡ Transactions of the Linnean Society, vol. xvi. p. 61.

act of injection; and if at any time this should not be the case, a fatal accident might happen from the milk being forcibly injected into the larynx, unless that aperture were guarded by some special contrivance. Professor GEOFFROY first described the modification by which this purpose is effected; and Mr. HUNTER appears to have foreseen the necessity for such a structure, for he has dissected two small mammary fœtuses of the Kangaroo for the especial purpose of showing the relation of the larynx to the posterior nares*. The epiglottis and arytenoid cartilages are elongated and approximated, and the rima glottidis is thus situated at the apex of a cone-shaped larynx, which projects, as in the *Cetacea*, into the posterior nares, where it is closely embraced by the muscles of the soft palate. The air-passage is thus completely separated from the fauces, and the injected milk passes in a divided stream on either side the larynx to the œsophagus.

Thus aided and protected by modifications of structure, both in the system of the mother and in its own, designed with especial reference to each other's peculiar condition, and affording therefore the most irrefragable evidence of creative foresight, the feeble offspring continues to increase from sustenance exclusively derived from the mother for a period of about eight months. The young Kangaroo may then be seen frequently to protrude its head from the mouth of the pouch, and to crop the grass at the same time that the mother is browsing. Having thus acquired additional strength, it quits the pouch, and hops at first with a feeble and vacillating gait, but continues to return to the pouch for occasional shelter and supplies of food till it has attained the weight of ten pounds. After this it will occasionally insert its head for the purpose of sucking, notwithstanding another fœtus may have been deposited in the pouch, for the latter, as we have seen, attaches itself to a different nipple from the one which had been previously in use.

§ 3. *On the Structure and Analogies of the Female Generative Organs in the Marsupiata.*

In the oviparous vertebrate animals the variations of structure which the female generative organs present in the different classes are fewer and of less degree than those observable in the different orders and genera of the *Mammalia*.

The most prevailing characteristic of the oviparous type of the female generative organs is the absence of union in the mesial plane of the lateral efferent portions, which consequently continue separate to their terminations in the excretory outlet.

In Birds the genital apparatus is characterized by the superior, and in the female, as far as function is concerned, exclusive development of the left moiety; and the uniformity in the condition of the excluded ovum in this class corresponds with the sameness which prevails in the structure of the organs concerned in its production.

In Reptiles the ovaries and efferent parts of the genital system are equally developed,

* See Nos. 3731, 3734, 3735 in the Physiological Series of the Hunterian Museum, in which there are evidences that Mr. HUNTER had anticipated most of the anatomical discoveries which have subsequently been made upon the embryo of the Kangaroo.

or nearly so, on both sides. But although a considerable uniformity of structure is found to prevail in this system throughout the different orders of the class, the widest difference obtains both in the place of development of the ovum and the condition in which it quits the mother. No one, e.g., could have predicated from a comparison of the structure of the ovaries and oviducts in poisonous and innocuous Serpents that any difference existed in the structure and development of the ovum, much less that the former were ovoviviparous but the latter oviparous; or, from a comparison of the same organs in *Lacerta crocea* and *Lacerta agilis*, that a like difference should exist in the generative economy of species so nearly allied as for a long time to have been confounded together by naturalists. Yet Von Baer has observed that the young of *Lacerta crocea* are completely developed in the oviduct, and come forth active well-formed lizards.

These and similar examples from other cold-blooded *Ovipara* have led him to the conclusion, that the period of intra-uterine existence and the extent of intra-uterine development depend rather upon the original constitution of the ovum than upon the structure of the generative organs; and they show at least of how little value that opinion of the mode of generation of an animal must be which is founded exclusively upon the structure of the efferent portion of the generative apparatus.

In *Mammalia*, however, in most of the orders of which the connexion of the ovum to the uterus is so much more intimate than in the preceding classes, the variations in the structure of the female sexual organs are more numerous and remarkable; and though it be admitted that the nature of the fœtal coverings and appendages results from the original constitution and properties of the ovum, yet the variations of the uterus have evidently in this class a relation to those differences.

In tracing the female generative apparatus from the human subject through the different orders of *Mammalia*, we find that it approximates to the oviparous type of structure in two ways, viz. by an obliteration of the os tincæ, which is the characteristic separation of the uterus from the vagina in this class, and by a gradually increasing division of the uterus and vagina until they become two separate tubes throughout their entire extent. Both these modes of deviation combine in the Edentate *Marsupiata* to give to their generative apparatus its peculiar resemblance to that of the *Reptilia*. But, for the reasons above mentioned, it would be unwarrantable to conclude from the female organs alone of the Ornithorhynchus that its ovum is excluded, as in Birds, with a hard shell, and a corresponding absence of fœtal development.

In no mammiferous genus do the female organs present that character of unity or concentration, with distinction of parts, which is found in the human subject; for in the lower orders, besides the more essential differences above mentioned, there is always an elongation of the uterus, with a thinning of its parietes, and in general a blending together of the urethral and sexual passages. This latter deviation commences in the *Simiæ*. In the *Lemures* the angles of the uterus begin to elongate and to assume the form of cornua. The mesial cleft increases, and the cornua preponderate in the *Carnivora*, the *Cetacea*, the *Ruminantia*, and the *Pachydermata*; but it is in the *Rodentia*, which present affinities to Birds in other parts of their structure, that the uterus is first found completely divided into two lateral halves. This structure is not, indeed, uniformly met with in all the genera of the order; but besides the Hare and Rabbit, in which the double uterus is allowed to exist by De Graaf and Cuvier*, a similar complete division of the organ obtains in the genera *Sciurus*, *Arctomys*, *Spalax*, *Bathyergus*, *Echimys*, *Eretizon* (F. Cuv.), and *Hydrochœrus*; while in the genera *Mus*, *Cavia*, *Cœlogenys*, and *Dasyprocta* a portion of the true uterus still remains undivided, though this part, to which alone the term corpus uteri can be properly applied, is extremely small or rudimental. Nevertheless, although the corpus uteri exists in these genera, the true vagina is as remarkable for its length and capacity as in those in which the corpus uteri has ceased to exist.

Hitherto the vagina has presented itself under the form of a simple undivided canal, communicating with the urethro-sexual passage, at least after impregnation, by a single aperture. But it is a remarkable and interesting fact that in the Sloth, in the Mare and Ass, in the Pig, in the Cow, and probably also in other Ruminants, the vagina in the virgin state communicates with the urethro-sexual passage by a double aperture, in consequence of being traversed by a narrow vertical septum or chord. This septum has been described by veterinary authors as a hymen in the

* The structure of the female generative organs of the Hare and Rabbit was well known to Daubenton, who has given accurate figures of them; but as he probably regarded the corpus uteri as an essential part of the organ, he describes the true or proper vagina under that name. In speaking, however, of the same parts in the Rabbit, he unconsciously admits the true nature of the latter part, observing, "Chaque corne avançoit dans le vagin de deux lignes de longeur."—Buffon, Hist. Nat., tom. vi. p. 326. And again observes, that in a rabbit ready to bring forth, "les orifices des cornes de la matrice commençaient à dilater pour l'accouchement comme l'orifice interne de la matrice se dilate en pareil cas dans la plûpart des autres animaux."

Supported by the identity of structure in the vagina of the Cavies, in which the true corpus uteri exists, with that of the Hares, and by the authority of Cuvier, I should scarcely have thought it necessary to refer to Daubenton's descriptions had not his views been recently adopted by Geoffroy St. Hilaire and supported by additional arguments, and the same reasoning applied to the determination of the parts of female apparatus in the *Marsupiata*. (See Anatomie Philosophique, pl. 17. fig. 13. pp. 397, 398.) According to this author the cornua uteri and corpus uteri are distinct elements of the efferent portion of the genital apparatus, each being composed of a different substance (tissu), nourished from different arterial branches, and possessing different functions; he proposes therefore to name the former *aduterum, quasi vas vel marsupium ad uterum*. I cannot, however, coincide with this opinion, as I have not been able in any instance to appreciate an essential difference of tissue between the corpus and cornua uteri in those quadrupeds with a partially divided uterus; and I believe such a difference can only be predicated where, as in the Hare and Rabbit, a portion of the vagina is considered as the body of the uterus; and that so far from the corpus uteri not participating in the function of gestation, it is always found traversed by the fœtal membranes in uniparous quadrupeds with the uterus bicornis, as in the Mare, the Deer, and the Porpesse, in all of which I have dissected the pregnant uterus. And Daubenton expressly records an observation he made on the Mouse, in which the corpus uteri is reduced to the smallest proportional size, that in a pregnant female with five young ones in the uterus, two were in the right cornu, two in the left, and one in the corpus uteri.—See Buffon, Hist. Nat., tom. vii. p. 317.

Mare; the analogous part in the human subject also occasionally presents the same structure, and has even been observed in some cases to extend as a mesial partition inwards towards the uterus.

In the *Marsupiata*, where from the small size of the fœtus at birth a similar conformation is permitted to remain as a permanent structure, the vagina is in some genera wholly, and in others partially divided; but the divided portion in the latter is always that which is nearest the urethro-sexual passage.

The true uterus is completely divided in all the genera, and each division is of a simple elongated form, as in the *Rodentia*.

The superadded complications in the female generative organs of the *Marsupiata* are not, then, rightly attributable to the uterus, but to the vagina; and they are of such a nature as to adapt the latter to detain the fœtus, after it has been expelled from the uterus, for a longer period than in other *Mammalia**.

These complications vary considerably in the different marsupial genera. On a comparison of the female organs in *Didelphys dorsigera, Petaurus pygmæus,* and *Petaurus Taguanoides,* in *Dasyurus viverrinus,* in *Didelphys virginiana,* in *Macropus major,* and *Hypsiprymnus Whitei,* or the *Macropus minor* of Shaw, I find that the relative capacity which the uteri bear to the vaginæ diminishes in the order in which the above-named species follow, and that the external pouch has a progressively increasing development, corresponding to that of the vaginæ.

In *Didelphys dorsigera* the uteri rather exceed the unfolded vaginæ in length (Pl. VI. fig. 5.). In most *Marsupiata* the vaginæ at first descend, as if to communicate directly with the urethro-sexual passage, but in this small Opossum, in which the abdominal pouch consists of two slight longitudinal folds, and the young, as is implied by its trivial name, are transported by the mother on her back, each tube, after embracing the os tincæ, is immediately continued upwards and outwards, then bends downwards and inwards, and, after a second turn upwards, descends by the side of the opposite tube to terminate parallel with the extremity of the urethra in the common passage.

In the *Petauri* the vaginæ, when unfolded, are a little longer than the uteri. On examining a specimen of the Pygmy Petaurist which had two very small young in the

* It will thus be seen that the mode of considering the marsupial generative apparatus which I have adopted leads to a conclusion, as to its influence on parturition, diametrically opposite to that which Geoffroy St. Hilaire arrives at. He assigns as the cause of the premature birth of the marsupial generative product, the absence of any constriction between the uterus and vagina analogous to the *cervix uteri* in the ordinary *Mammalia*; but the non-existence of the *cervix* and *os uteri* can only be asserted where a portion of *vagina* is regarded as *uterus*. In the comparative sketch of the forms of the uterus given by Burdach, (Physiologie, Bd. i. pl. iv.) the vaginal is appended to the uterine apparatus in the marsupial or first form, but omitted in the rest: this does not, therefore, express its true relations. In respect of figure, the uterus of *Marsupiata* does not deviate from the perfect or human type in a greater degree than that of *Rodentia*. Burdach (Ibid. p. 130.) considers the vaginæ (*Seitencanalen*) of the *Marsupiata* as the fully developed analogues of the glandular canals described by Malpighi and Gærtner in the female organs of the Ruminants, Pachyderms, &c.; but these canals lead from the urethro-sexual passage, not to the os tincæ, but to the broad ligaments and ovaries.

pouch, I found both the true uteri of three times the diameter of the same in an unimpregnated specimen; but the vaginæ were unaltered in size, indicating that the situation in which gestation takes place in this species is the same as in the Kangaroo. The vaginæ, after receiving the uteri, descend close together half way towards the commencement of the urethro-sexual passage, but do not communicate together in this part of their course. From the upper part of these culs de sac they are continued upwards and outwards, forming a curve like the handles of a vase, then descend, converge, and terminate close together, as in the preceding example.

In *Dasyurus viverrinus* and *Didelphys virginiana*, the mesial culs de sac of the vaginæ descend to the urethro-sexual passage, and are connected to, but do not communicate with it. The septum dividing them from each other is complete, being composed of two layers, which can be separated from each other, and which result, indeed, from the apposition and mutual adhesion of the vaginæ at this part. In order to reach the common passage, each tube is continued outwards from the upper end of the cul de sac, and forming the usual curve, terminates parallel to the orifice of the urethra. The vaginæ in the Dasyures are smaller in proportion to the uteri than in the Virginian Opossum, but of a similar form.

In another species, the *Didelphys Opossum* of LINNÆUS, it would appear from the description and figures of DAUBENTON*, that the septum of the mesial culs de sac of the vaginæ was imperfect; but it is doubtful whether this intercommunication was not the result of parturition, or of an accidental rupture in the specimen examined. If it should prove to be a specific difference of structure, it is an approximation to the type of the female organs as they exist in the Phalangers, the Wombat, and the Kangaroo.

In the latter animal the vaginæ preponderate in size greatly over the uteri; and the septum of the descending cul de sac being always more or less incomplete, a single cavity is thus formed, into which both uteri open; but however imperfect the septum may be, it always intervenes and preserves its original relations to the uterine orifices.

The fœtus has been conjectured to pass into the urethro-sexual cavity by a direct aperture formed after impregnation at the lower blind end of the cul de sac, but I have not been able to discover any trace of such a foramen in two kangaroos which had borne young; and besides, I find that this part of the vagina is not continuous by means of its proper tissue with the urethro-sexual passage, but is connected to it by cellular membrane only; and this structure is agreeable to what is presented in the simpler forms of the marsupial uterus, as in *Didelphys dorsigera*, and the *Petauri*, in which the culs de sac do not even come into contact with the urethro-sexual passage. The evidence of M. RENGGER on the development of the young and the parturition of the *Didelphys Azaræ* is also directly opposed to the theory of a temporary orifice in the mesial cul de sac.

* BUFFON, Hist. Nat., tom. x.

The last form of the marsupial female organs which may be noticed is that which is found in the Kangaroo Rat (*Hypsiprymnus Whitei*), where they present the most extraordinary appearance (Pl. VI. fig. 6.). The type of construction is, however, the same as in the great Kangaroo, but the mesial cul de sac of the vagina attains a still greater development; it not only reaches downwards to the urethro-sexual passage, but also upwards and outwards, dilating into a large chamber, which extends beyond the uteri in every direction. From the sides of this chamber the separated portions of the vagina continue downwards, to terminate, as usual, in the urethro-sexual canal.

In all the preceding genera the structure of the uteri is as distinct from that of the vaginæ as in the *Rodentia*. The fibrous or proper tunic of the uteri is thicker than that of the vaginæ, and the lining membrane is soft and vascular, and disposed in numerous irregular folds, which in section give apparently a still greater thickness to the uterine parietes. The whole extent of the vaginæ, on the contrary, is lined with a thin layer of cuticle, which is readily detachable, even from the middle cul de sac, so generally considered as the corpus uteri in the Kangaroo.

The inner surface of the culs de sac in the Opossum is smooth, but in the lower part of the single cavity in the Kangaroo and Kangaroo Rat it presents a reticulate structure. The lining membrane in the lateral canals in all the genera is disposed in regular longitudinal folds, a disposition which characterizes the true vagina in most of the ordinary quadrupeds. In the Kangaroo, as in the other *Marsupiata*, the lateral canals communicate with the common or urethro-sexual cavity without making a projection; but at the distance of three fourths of an inch from their termination there is a sudden contraction, with a small valvular projection in each, which probably limits the extent to which the bifurcated glans is introduced in coitu. By those who consider the cul de sac and lateral canals as a modification of the corpus uteri, these projections will probably be regarded as severally representing an os tincæ; but as they do not exist in the Opossums and Petaurists, in which there is simply a contraction of the vaginal canals at the corresponding part, and as in both these, as well as the Kangaroo, the true uteri open in the characteristic valvular manner, as in the *Rodentia*, without the slightest appearance of a gradual blending into the vaginal cul de sac, the above structure cannot be regarded as materially affecting a determination supported both by the general texture and connexions of the part in question, as well as by what is now ascertained to be its limited function. Moreover, in the large single vagina of some of the *Rodentia*, as the Hare, Rabbit, and Paca, there are two corresponding valvular folds of membrane near its commencement, a little way above the urethral aperture, which Daubenton consequently regarded as the limits of the corpus uteri.

In endeavouring to trace the purposes answered by the different forms of the female marsupial organs above described, considerable difficulty arises from the want of the necessary evidence which would be afforded by the examination of the pregnant uterus in each of the genera, and by the absence of information as to their respective periods of gestation, and the powers of the new-born fœtus. As far, however, as a conclusion can be drawn from the relative periods of gestation in the Kangaroo and Opossum, the proportionate capacities of the vaginæ to the uteri would appear to be inversely as that period; and that while the vaginæ are calculated to present fewer obstacles to the escape of the fœtus in proportion to the duration of its uterine existence, so a less capacious and complete external pouch is requisite for its ultimate perfection. From Rengger's description of the connexion of the fœtal Opossum to the uterus, it might be concluded that the generation in that animal approximated to the true viviparous mode more nearly than in the Kangaroo; but the determination of this interesting question will require a more exact investigation into the nature of the fœtal vessels and membranes in the genus *Didelphys*. The impregnated uteri of the smaller pouchless Opossums of South America would be objects of peculiar interest and value in the present state of the inquiry.

With respect to the variations of structure in the marsupial female organs, it may also be remarked, that though they are apparently most complicated in the Kangaroos and Phalangers, yet in reality they deviate from the type of the normal *Mammalia* in a minor degree in these *Marsupiata* than in the *Didelphides* and *Petauri*. For the essential difference being a division of the vagina into two canals, we find this to be most complete in the latter genera, while in the Kangaroos the division is only partial, and the complexity arises more from augmented capacity and extent.

Now it is important to observe, that the fission of the efferent tube is not continued, as might naturally be supposed, from the uterus into the vagina, leaving its distal extremity single, but commences at the urethro-sexual cavity, and is arrested near the uteri, the orifices of which thus open into a common canal.

The situation of the rudimentary vaginal septum or hymen in the unimpregnated female organs of the normal *Mammalia* before mentioned corresponds with this formation in the Kangaroo; and in a case where this septum was preternaturally developed in the human subject, it was found to obey the same law of formation, and at the same time to have been coincident with a completely divided uterus.

This malformation, so remarkably analogous to the structure of the marsupial uterus, is described by Dr. Purcell in the sixty-fourth volume of the Philosophical Transactions, and the specimen itself is in the Museum of the Royal College of Surgeons*. The vaginal septum is vertical, commencing at the outlet, and terminating about an inch from the orifices of the uteri; and, as Dr. Purcell accurately describes, it is "not merely membranous, but fleshy, and of a considerable thickness; and like

* I have subsequently witnessed, through the kindness of my friend Dr. Thomas Blundell, an accurate model of a similar malformation of the vagina in the human subject: the vertical septum commenced at the vaginal outlet, and extended backwards for an inch, dividing the passage for that extent into two lateral canals. The individual underwent an operation for its removal. In this case the condition of the uterus could not of course be ascertained; but my friend Dr. Lauth of Strasburg has described and figured two preparations in the Museum of the Faculty of Medicine in that city analogous to the case described by Dr. Purcell. In one of these the uterus was divided internally into two lateral chambers, and the whole of the vagina was

most other mediastina in the human body, consisted of two laminæ combined. Of these, each vagina furnished one; for each had its own constrictor*."

To understand the relations which the female sexual apparatus of the Ornithorhynchus and Echidna bear to those of the ordinary *Mammalia*, it becomes necessary to consider through what families the human or concentrated form of the apparatus degenerates towards the oviparous structure, by the second mode of deviation. The first step in this descent is presented in the Sloths. The uterus is here of a simple, elongated, undivided form; but the distinction between the vagina and uterus by an os tincæ is lost, and so far it resembles an oviduct of a reptile. The uterus presents a similar form in the Nine-banded Armadillo (Plate VI. fig. 4.), but in the Weasel-headed Armadillo the angles are slightly elongated.

Moreover, in these as well as the more decided Edentate genera, as *Manis* and *Myrmecophaga*, the urethro-sexual canal is formed, as in Tortoises, by a continuation of the urethra or urinary bladder, into which the genital tube opens by a small orifice, (but in the Sloth, in the virgin state, by two small orifices,) just as the urethra communicates with the vagina in other *Mammalia*. The vaginal portion of the tube is indicated by the thinness of the parietes in the lower or distal half, by the smoother and less villous structure of the lining membrane, and its disposition in regular longitudinal rugæ; but the change is gradual, and the exact extent of the uterus is not marked by any constriction.

Now the Ornithorhynchus and Echidna, while they present a complete division of the efferent portion of their generative apparatus like the other *Marsupiata*, maintain, in the composition of each lateral moiety and in its mode of termination, their affinity to the Edentate order, in which Cuvier has placed them; and thus, by combining those characters of the oviparous type of the generative system which separately present themselves in other *Mammalia*, there results that affinity to the structure of the same parts in the *Reptilia* which has led to the supposition of their forming a distinct class of animals. The complex scapular apparatus of the *Monotremata*, the mandibles of the Ornithorhynchus, and the structure of the male intromittent organ, which, though perforated by a complete canal, is adapted to transmit the seminal fluid only, form additional deviations from the mammiferous structure. But the single cloacal outlet, the double superior cava, and the absence of the inferior mesenteric artery are approximations to the oviparous type participated by them in common with the other *Marsupiata*; and the whole may be regarded as an aberrant group of *Mammalia* characterized by an ovo-viviparous generation.

A few remarks remain to be added respecting the vaginæ of the Kangaroo.

separated into two canals, each receiving its corresponding os tincæ, as in the Opossum. In the other case the uterus was divided both externally and internally into two lateral compartments, but the vaginal septum commenced a short distance below the uterine orifices, as in Dr. Purcell's case.—See Breschet's Repertoire d'Anatomie, tom. v. p. 99.

* Philosophical Transactions, vol. lxiv. p. 478.

Sir EVERARD HOME* and M. LEUCKART† have both observed these parts of the generative system distended with a gelatinous adhesive matter, with irregular fibrous masses intermixed. One of these substances, which was found in the mesial cul de sac, Sir EVERARD compares to the vertebral column and occipital bone of a fœtus, and has given a figure of it as such. He appears indeed to have been considerably influenced by this circumstance in forming his theory of marsupial generation. M. LEUCKART, who also found several of these bodies, both in the lateral canals and middle cavity, describes them as consisting of a homogeneous fibro-cartilaginous substance, and compares them to a mola, or false conception, but observes that there was nothing in their structure that would permit him to form a conclusion that they were parts of a fœtus. In this instance, the middle cavity and the two thirds of the lateral canals nearest to it were filled with 'a pultaceous yellowish mucus.'

The female organs of the Kangaroo in this condition were also sent over from New South Wales by Mr. G. BENNETT, along with the impregnated uterus described, accompanied with the following note: "Bottle, No. 2. The uterus of a Kangaroo of the common species, the adjacent parts being preserved. This one had the appearance of having just received the male; and we killed a male specimen, having the appearance of being lately with the female, half an hour afterwards on the same range. The cornua uteri?" (vaginal canals) "are evidently diseased, containing a hard cheesy substance. (This has not been hardened by the spirit, for it was about the same consistence when I examined it in the recent state.)"

Mr. BENNETT also observes, that there was no young one in the pouch of this female; but one of the nipples was largely developed, from which he expressed milk. As this is precisely the condition in which the female at the Zoological Gardens was when she received the male, it corroborates Mr. BENNETT's supposition of the same circumstance having recently preceded the death of the female which he examined, and serves to elucidate in some degree the nature and cause of those appearances, which he regarded as the product of disease.

In the vaginæ of this animal, as in those examined by HOME and LEUCKART, portions of dense fibrous substance, varying in length from half an inch to an inch, and from one to three lines in thickness, were inclosed in a thick mucus. The fibrous substances had an irregular surface, and in some instances a rather brittle fracture: they were of a homogeneous texture when cut with the knife, (and such is also the composition of the substance described by Sir EVERARD HOME,) and they most resemble those coagulated masses that are found in the vesiculæ seminales and sometimes in the urethra of the Agouti, Capromys, Guinea-pig, and others of the Rodent order.

Since the dissection of Mr. BENNETT's specimen, I have had the opportunity of observing the female organs of another Kangaroo, which were obligingly submitted to my examination by Professor GREEN: they presented the same appearances as

* Philosophical Transactions, vol. lxxxv. p. 228. † MECKEL's Archiv fur Physiologie, tom. viii. p. 442.

those above described, and having been successfully injected, showed that the vaginæ were highly vascular. The history of this female is not known, but this additional example of the presence of mucus with fibrous masses in the vaginæ would intimate that it is not an uncommon occurrence, and therefore unlikely to be the result of disease.

I had originally intended to limit myself to the description of the preparation which first called my attention more directly to the subject; but the desire of penetrating, if possible, to the final purpose of marsupial generation, induced me to push my inquiries as far as the means at my disposal allowed; and though I am compelled to acknowledge that the end proposed is still to be attained, yet the collateral inquiries instituted with that view have, I hope, tended to render the subject more intelligible, and to point out its real analogies to other known modes of generation.

The conditions of those modifications of structure which relate to the marsupial fœtus after uterine birth are readily appreciable. An offspring prematurely born, and with a great proportion of its growth yet to be accomplished before it attains the power of existing independently, must obviously be incapable of sustaining life with any considerable intermission of sustenance; and since it has no store of nutriment appended to its digestive canal when excluded from the womb, and is therefore dependent for its support solely upon maternal secretion, the period during which the mother must have been confined to a foreign and artificial nest, supposing no other protection to the offspring had been provided, would have been probably too long to be compatible with her own existence. To obviate this inconvenience a natural and portable nest is superadded to her structure, in order that she may resort, without prejudice to the young, to all the places necessary for her own safety and support; while at the same time the young one is enabled to draw an unintermitting supply of nutriment, and has also its own temperature maintained by close contact with the abdominal surface of the parent, in an analogous, though more complete manner than the egg during incubation.

But when we come to consider why the intra-uterine life of the embryo should be such, both in its nature and duration, as to require these modifications, the subject, at present, eludes every attempt at direct explanation. If an unvascular chorion, with the consequent premature birth and after-incubation in a marsupium, were peculiar to the Kangaroo, these might be regarded as the necessary concomitant phenomena of its strange proportions and violent progressive motion; it might then be considered essential that the fœtus should pass the pelvis before the hinder parts had attained their gigantic proportions; it might also be supposed that saltatory progression was incompatible with the safety of the parent or offspring, if the fœtus were attached to the womb by so delicate but vital a medium as a vascular placenta; and that, therefore, while a premature birth obviated the necessity for the formation of a placenta, an unimpeded delivery was equally secured by the same anticipation.

But such explanations fail when the Jerboas of Africa are considered; for in these animals we find similar proportions of the extremities, and consequently the same kind of locomotion as in the Kangaroo, without any external pouch or internal modification of the female apparatus indicative of a difference in their generation from that of the ordinary *Rodentia*. And, on the other hand, the ovoviviparous or marsupial *Mammalia* include the flying Petaurist, the burrowing Wombat, the swimming Cheironectes, the climbing Koala, Opossums with the hinder thumb and prehensile tail, and the Dasyures, with the ordinary proportions and progression of the corresponding carnivorous genera of the placentally developed *Mammalia*; in all of which genera it is obviously impossible to connect marsupial generation with the outward proportions, locomotion, or habits of the parent.

Perhaps it is more philosophical to consider generation as having reference rather to the whole nature of the thing generated, and its relative perfection as compared with other species, than to partial modifications of the structure of the mother.

The whole of the vertebrated animals are recognised as one great division or group in nature, characterized by a plan of formation which, however varied to suit their different spheres and powers of action, has sufficient basal or permanent characters to be recognised as one type, distinguishable from that which pervades any other lower organized group of the animal kingdom.

But the generation most common to the vertebrated group is the same which chiefly prevails in the lower divisions of the animal kingdom, viz. the oviparous, in which the ovum, when once formed, detached, and impregnated, possesses properties that enable it to accomplish all the steps of its future development, without further connexion with the parent. The generation, therefore, which requires a second connexion of the ovum to the parent, as in the placentally developed *Mammalia*, is an exception to the rule of vertebratal reproduction, and we are led to inquire in what essential points these animals deviate from or are superior to the other classes of the division, that in their generation the parent should be subservient in a so much greater degree to the perfect development of the new being.

Now it is in the *Mammalia* that the brain is perfected: we can trace through the different orders the increasing complication of this organ, until we find it in man to have attained that condition which so eminently distinguishes him from the rest of the class. And if the introduction of new powers into an organism necessarily requires a modification in its mode of development, with what other than the perfection of the nervous system can we connect true viviparous or placental generation? for we do not perceive that in their digestion, circulation, respiration, locomotion, or temperature, the Mammiferous *Vertebrata* are in any degree advanced beyond the bird, in consequence of their more complex, or, as it may be termed, more careful generation.

Agreeably to this view, therefore, we should expect to find in those orders in which the umbilical vesicle is largest and most permanent, and the placenta least vascular, a corresponding simplicity of the cerebral organ; and accordingly we do find that the brain in the *Cheiroptera* and *Rodentia* resembles that of the Bird in the smoothness of the cerebral hemispheres and their limited extent, the cerebellum being wholly uncovered by the cerebrum throughout these orders *.

Among the *Marsupiata*, the Opossums and Dasyures present a still simpler form of the brain, the cerebral hemispheres being equally devoid of convolutions as those of the Beaver, and leaving the bigeminal bodies as well as the cerebellum uncovered: the fissure also which separates the olfactory tract from the superimposed cerebral mass, instead of being inferior, as in the *Rodentia*, is here lateral: and, lastly, the proportions which the thickness of the medullary covering bears to the extent of the lateral ventricles is less than in any other mammiferous order.

With respect to the brain of the Kangaroo, it must be observed, that although shortly after birth it resembles in structure the brain of the lowest *Vertebrata*, yet it afterwards assumes a more complex form than that of the Opossums or Dasyures, there being a few symmetrical anfractuosities upon the cerebral hemispheres, which also cover a greater proportion of the bigeminal bodies: the hemispheres are, however, more contracted anteriorly, and have a smaller size, in proportion to the body, than in those of the *Rodentia*.

The inferiority of the brain, then, in connexion with the other points of resemblance to the inferior vertebrate classes which may be traced through the organization of the marsupial quadrupeds, seems at present to be the phenomenon most intimately connected with their generation. Those which I have had the opportunity of observing alive at the Zoological Gardens (and there are at present species of *Dasyurus, Didelphys, Phalangista, Petaurus, Hypsiprymnus, Macropus,* and *Phascolomys,*) are all characterized by a low degree of intelligence; nor can I learn that they ever manifest any sign of recognition of their keepers or feeders. Another character, no less uniformly belonging to them, is the want of a power of uttering vocalized sounds. When irritated they emit a wheezing or snarling guttural sound; that of the *Dasyurus ursinus* is the clearest, and is the nearest approach to a growl. Mr. Harris, however, states, that in addition to this noise, the Ursine Opossum utters a kind of hollow barking. The *Thylacinus cynocephalus*, or large Dog-faced Opossum, he observes, utters "a short guttural cry, and appears exceedingly inactive and stupid, having, like the owl, an almost constant motion with the nictitating membrane of the eye †." The Wombat, when irritated, emits a loud hiss which forcibly reminds one of that of the Serpent. The noise emitted by the Kangaroo under similar circumstances is equally remote from a vocalized sound; the necessary apparatus for producing which Cuvier ‡ long ago observed to be wanting in the larynx of this animal.

* It is also in this order that the double superior cava is most frequently found, after the *Marsupiata*; and the Elephant, whose other affinities to the *Rodentia* Cuvier has especially remarked, resembles them in this respect.

† Linnean Transactions, vol. ix. p. 173.

‡ Leçons d'Anat. Comp. iv. p. 509.

It is interesting to find these analogies to the *Reptilia*; and more might be pointed out if it were not a comparison which merits a separate consideration, and would extend the present communication to an undue length.

There is, however, another order of *Mammalia* which, in addition to certain analogies to the *Reptilia* manifested in their generative and other systems, have the brain nearly as simple as in the Opossums: these are the Edentate *Mammalia*; and the Armadillos, Manises, and Anteaters are more especially characterized by their inferiority in this respect, the Sloths, like the Kangaroo, having a few superficial anfractuosities on the cerebral hemispheres. In order, therefore, to test the degree of relationship which exists between a long intra-uterine and placental development, and the perfection of the brain, it will be requisite to possess an accurate knowledge of the mode of development of the above Edentate genera.

This is an inquiry well deserving attention; and it is to be hoped that the desirable materials, viz. the impregnated uteri of the Edentate and Marsupiate genera, will soon be furnished through the exertions of our scientific countrymen abroad.

Description of the PLATES.

PLATE VI.

Fig. 1. Communication of the true vagina with the urethro-sexual passage by a double orifice, resulting from an occasional formation of the hymen, in the Human subject.

Fig. 2. A section of the urethro-sexual canal, showing a similar mode of communication by a double orifice, resulting from a constant formation of the hymen, in the Sow.

Fig. 3. A similar section of the urethro-sexual passage of the Kangaroo.

In each of the figures, *a* is the urethral, and *b b* the vaginal orifices.

Fig. 4. The female organs of an Armadillo (*Dasypus novem-cinctus,* LINN.).

Fig. 5. The female organs of the Merian Opossum (*Didelphys dorsigera,* LINN.), magnified three diameters.

Fig. 6. The female organs of the Kangaroo Rat (*Hypsiprymnus Whitei,* LESSON).

The same letters indicate the same parts in each of the figures.

a. Ovaries.

b. Fallopian tubes. (In figg. 5. and 6. *Membranous portion of the Fallopian tubes,* HOME.)

c. Uteri. (*Cornua uteri,* TYSON, DAUBENTON. In figg. 5. and 6. *Glandular portions of the Fallopian tubes,* HOME; *Aduterums,* GEOFFROY.)

d. Os tincæ. (In figg. 5. and 6. *Valvular termination of Fallopian tube,* HOME.)

e. Mesial cul de sac of the vagina. (*Corpus uteri,* TYSON, DAUBENTON, GEOFFROY; *Uterus,* HOME.) *e′, e′*. Divided portion of the vagina. (*Uteri re-*

duplicati, and *Vaginæ,* TYSON ; *Vaginæ,* GEOFFROY; *Lateral uterine canals,* HOME.)

f. Urethro-sexual canal. (*'Canalis communis,'* or *common passage from the urethra and the two vaginæ,* TYSON ; *Canal urétro-sexuel,* GEOFFROY ; *Vagina,* HOME.)

g. Urinary bladder.

h. Urethra.

Fig. 7. The impregnated female organs of the Kangaroo (*Macropus major,* SHAW). The gravid uterus c' is laid open, and also the chorion *i,* or membrana corticalis of the fœtus, showing the latter suspended from *k,* the umbilical chord. In addition to the letters above explained, a' is the left ovary, with a large corpus luteum, showing the orifice from which the ovulum escaped not yet cicatrized. * The ovarian ligaments. Bristles are inserted into the Fallopian tubes. The vaginal apparatus e, e' e', not having been preserved along with the impregnated uterus, is here added from another specimen, in which the imperfect septum of the mesial cul de sac (e'') did not extend to the lower end of that cavity, as is usual in the Kangaroo. The cellular membrane which connects the vaginal cul de sac with the urethro-sexual passage has been removed.

PLATE VII.

Fig. 1. The fœtus and membranes of the Kangaroo removed from the uterus. The fœtus magnified two diameters.

a, a. The exterior membrane or chorion laid open.

b, b. The amnion.

c, c. The umbilical vesicle.

d, d. The omphalo-mesenteric veins.

e. The omphalo-mesenteric arteries.

f. The pedicle connecting the umbilical vesicle to the intestinum ileum.

g. The stomach.

h. The duodenum.

i. (Fig. 2.) Convolutions of small intestine.

k. The cæcum.

l. The large intestine.

m. The liver.

n. The kidneys.

o, o. The testes.

p. The bifid rudiment of the penis at the verge of the anus.

q. The diaphragm.

r, r. The lungs.

s. The heart.

t, t. The two superior cavæ.

The pulmonary artery and aorta have the same relative position as in the adult.

u. The rudiments of the posterior extremities.

v. The external orifice of the ear.

w. The branchial orifice.

Fig. 2. The viscera of the preceding fœtus, more magnified: the heart is turned up, to show the auricles, and the whole intestinal canal is seen.

Fig. 3. Outline of the same fœtus, natural size, showing the connexion of the umbilical vesicle.

Fig. 4. Outline of the embryo of the Goose, showing its natural size and state of development when the allantois *x*, is just beginning to expand from the lower part of the intestine. The brain may be observed to be proportionately more developed than in the Kangaroo.

The same letters are used for the different parts as in fig. 1.

Fig. 5. Outline of the Kangaroo about twelve hours after uterine birth, showing its natural size and external development at this period. The elongation of the jaws has reduced the mouth to a simple round anterior orifice, which subsequently becomes even more contracted before the lateral fissures begin to extend backwards. The eye is concealed by the completely formed eyelids. Three divisions are now seen at the posterior extremity. A longitudinal line indicates the separation of the umbilical pedicle.

a. The upper nipple of the left side, to which the above fœtus was attached.

b. The lower nipple of the same side.

Fig. 6. Mammary fœtus of the Pygmy Opossum (*Petaurus pygmæus*), natural size.

6*. The same magnified and dissected, showing,

a. The urinary bladder.

b. The urachus.

Fig. 7. Mammary fœtus of the Kangaroo, about a fortnight old: natural size. The parietes of the abdomen are removed, to show the increased development of the viscera, as compared with the uterine fœtus (fig. 1.); and also the urinary bladder *a*, and its attachment to the abdominal parietes.

Fig. 8. Magnified view of the abdominal viscera of the same mammary fœtus.

a. The urinary bladder.

b. The urachus.

c, c. The umbilical or vesical arteries.

d. The ligamentum suspensorium hepatis, in which there is no trace of umbilical vein.

e. The left lobe of the liver.

f, f. The right lobe now subdivided.

g. The acute fold of the ileum, at the end of which the umbilical vesicle was attached.

h. The elongated cæcum.

i. The large intestine.

k. The supra-renal glands.

l, l. The kidneys.

m. The ureters.

n, n. The testes.

o, o. Epididymis.

p, p. Vas deferens.

Fig. 9. The brain and spinal chord of the same fœtus, from the superior or dorsal aspect. Natural size.

Fig. 10. The same, from the inferior or ventral aspect.

Fig. 11. Superior view of the same brain. Magnified three diameters.

Fig. 12. Side view of the same. Magnified three diameters.

The following letters signify the same parts in each figure:

a. Spinal chord.

b. Medulla oblongata.

c. Cerebellum.

d, e. The medullary mass, from which the bigeminal bodies are developed. The separation of the ganglions, termed Testes *d*, Nates *e*, is faintly visible.

f. Posterior striated bodies.

g. Cerebral hemispheres.

h. Crura cerebri.

i. (Fig. 11.) The ventricle of the right hemisphere laid open.

k. (Fig. 10.) Mammillary body.

The state of development of the brain of this mammary fœtus corresponds to that of the human fœtus at the ninth week.

Fig. 13. The head of a mammary fœtus of a Kangaroo, about eight weeks old, dissected, to show the relation of the larynx to the tongue and posterior nares.

a. The epiglottis, drawn down out of the aperture in the soft palate.

b. The cavity in the tongue for the reception of the nipple.

Fig. 14. The elongated nipple, withdrawn from the mouth: the dotted line shows the extent to which it is grasped; it never extends into the œsophagus or stomach as has been conjectured.

XVIII. *Some Observations on the Structure and Functions of tubular and cellular Polypi, and of Ascidiæ.* *By* Joseph Jackson Lister, *Esq. F.R.S.*

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THE more obscure functions of vitality are of such difficult investigation, and possess at the same time so high an interest, that any one contributing, in however small a degree, to increase our information regarding them, may hope to meet with indulgence.

This consideration encourages me to submit to the Royal Society some observations made during a few weeks spent at Dover and Brighton in the autumn of 1832, and the last summer. I was led to engage in them from having two years before noticed the existence of currents within the tubular stem of a species of *Sertularia*; and their investigation has led me on to additional particulars relating to that family of zoophytes, and other compound animals more or less resembling them, some of which I am willing to hope may be new in physiology.

The facts being only such as presented themselves during a limited stay on the coast, and in part indeed requiring further observations to ascertain their true bearing, the form of the original memoranda is often retained, as being probably the most satisfactory. Though too circumscribed and incomplete to form a ground for new arrangements or theories, they will at least show that the field from which they are culled is hitherto but partially explored, and may perhaps awaken the attention of inquirers more favourably circumstanced.

Of the notices regarding tubular polypi, one on *Tubularia indivisa* relates principally to a peculiar circulation seen within it, and to some circumstances attending its growth, absorption, and decay. Those which follow on *Sertulariæ* describe internal currents of a different kind, a more full observation of growth and absorption, with a case of the development of ova, and some other particulars of this family.

An account is next given of a minute *Ascidia* possessing a character, not I believe before observed in that tribe, of distinct individuals connected by a branching stem and a common circulation, and bearing in these and other points some resemblance to the *Sertulariæ*. The organization and functions of this *Ascidia*, and of a *Polyclinum* allied to it, are stated the more in detail, as the anatomical descriptions of the *Tunicata* by Cuvier, Savigny, and MacLeay appear to be derived almost wholly from dissection.

There are at the conclusion remarks on the natural character of some *Flustræ* and other cellular polypi, which tend to confirm the opinion that they are of a family much nearer to the *Ascidiæ* than to the *Tubulariæ* or *Sertulariæ* with which some of them have been hitherto associated.

It may deserve to be mentioned, that as the species of animals of one type diminish in size, the delicacy of some of the remoter details of their structure does not increase in the same proportion; but the aggregation of the component parts in the smaller species becomes instead more simple. From this cause, as well as from their transparency, most of the objects examined offered peculiar advantages for inspecting their organization while living and in freedom. They were placed in a glass trough with parallel sides, before my achromatic microscope directed horizontally, and the sea water was often changed. Near the end of the observations this was done by two siphons, one of which constantly admitted a fresh supply, while the other carried off the excess; a mode which, had it been earlier adopted, might have rendered some of the results more satisfactory; for the great difficulty, next to procuring a variety of specimens, was to retain them in vigour.

With the exception of two species that grew above the line of low water at spring tides, those obtained were only what the waves threw up; for repeated endeavours to get them, through fishermen, from rocks at sea or from oyster-grounds were unsuccessful.

The drawings in illustration were traced by a camera lucida slid over the eyepiece of the microscope. The facility with which correct graphic records and measurements may be obtained by means of that instrument with a little practice, induces me to recommed its use to other observers. The linear enlargement is marked to each figure.

TUBULAR POLYPI.

Tubularia.

The specimen of *T. indivisa* figured Plate VIII. fig. 1., was one among a broken mass of tubes, most of them larger than itself, which was found at Dover, October 1832, freshly cast ashore, and was kept several days, on another account, before the polypus terminating it was observed. It is a very small example of the species to which it is considered to belong. The arms had no ciliæ.

When magnified about one hundred times, a current of particles was seen within the tube, that strikingly resembled, in its steady continued flow, the circulation in plants of the genus *Chara*. The general course of the stream was parallel to the slightly spiral lines of irregular spots on the tube, and in the directions marked by the arrows. On the greater part of the side first viewed (that shown in the drawing), it set as from the polypus; but on reversing the glass trough so as to show the other side, the flow was there towards the polypus; each current thus occupying half of the circumference. The particles had no dancing motion among themselves like those which will be hereafter mentioned in *Sertularia*, but floated evenly on at a uniform rate. They were various in size; some very small, others apparently aggregations of smaller ones; a few were nearly globular, but in general they were of no regular form. The tube had between the lines of more conspicuous spots a granulated appearance, and beneath this the currents ran; but I could not, by altering the focus of the microscope, detect the opposite current on the further side, as may be done in *Chara*, owing to the interposition, in most parts, of a grumous matter. Some of the larger particles of this were like those in circulation, and by close attention might be seen slowly to change their relative positions.

At the nodous parts *c d e* were slight vortices in the current: at *c* near the end of the tube it came over from the opposite side, and I could not at this time succeed in detecting the passage of any particles between the tube and the stomach of the polypus.

Between the stomach *b*, and the mouth *a*, a remarkable action went on, wholly different from that in the tube. The mouth became swollen by a flow into it from the stomach, to the shape shown at *a* 3: this flow continued for about a minute. The contents of the mouth were then squeezed back into the stomach, which expanded as the mouth contracted (*a b*). During this reflux the connecting orifice was seen distinctly open, and it continued so on the return of the flow to the mouth (*a* 2), till the stomach became nearly emptied. The orifice then closed gradually, preparatory to the effort of forcing the fluid back to the stomach. The intervals between that act were very evenly eighty seconds.

Two currents were continually going on, both in the mouth and stomach, one flowing always down the sides in the direction *a b*, and an opposite one in the axis; except that the latter was suspended at the time of the close contraction of the stomach and of the discharge from the mouth into it.

The creature was observed at intervals throughout the day from ten in the morning; its appearance continuing much the same. The front arms sometimes spread themselves a little, and at one time a cloud of particles hung before them in the water like those within the mouth, and which seemed to have been recently ejected. Though the swelling and contracting continued, no motion was seen in the ejected particles; proving that in the ordinary course of this action the mouth was not opened.

The next morning the polypus was found much altered (*a* 4); the hinder arms, or those of the neck, were shrunk to an indistinct mass, appearing to be partly absorbed and partly thrown off, and a stream of particles was drifting away from them. The least imperfect of these arms had a rapid action going on in its granular substance, which ended in a current within it towards its root. The terminating arms appeared as perfect as the day before.

Rudiments of the horny shell had extended to the neck from *f*, its end when it was first observed. The currents in the intermediate space *b* were now plainly connected with those in the tube and had lost their former even course, the particles flowing about irregularly as the division between *b* and *c* became by degrees broken away. The currents in the tube were unaltered.

Later in the day the imperfect new piece of shell was folded in, the stomach *b* had disappeared and the end of the soft part within the tube was shrunk to a conical form, all dilatation and contraction (of which there were some remains in the morning) having ceased; another discharge had taken place from the mouth, and a considerable one from the neck, when the tube acquired a defined end (*a* 5); and between the shell and the conical soft part a vacant space had been gradually left. Into this was now admitted from the neck some granulated fluid matter, which flowed over the conical surface, and after some agitation of the particles, formed a rounded covering to it; semitransparent at first, but soon becoming more opake, so as not to be distinguished from what it covered (*a* 6). The circulating particles of the tube now flowed into the end and returned from it, as at a septum of *Chara*.

Afterwards rapid action of large granulations and stillness alternating took place within the neck; and in the evening the polypus was completely separated from the soft matter of the tube, and dead; its substance breaking away in a stream of disengaged particles. The following morning some slimy matter about the end of the tube was all that remained of it; but the circulation within the tube continued as before.

The specimen was now immersed in spring water. The first effect was, instead of the two currents, one flow, towards the end of the tube, of roundish particles, some of which escaped into the water: after this had continued a short time, the whole internal contents of the tube began to move forwards together, and became protruded at the end like thin paste; then for a while a clear current with few particles in it flowed back between the shell and the moving mass, and showed the shell to be transparent, and that the mottledness and granulations, as well as the irregular lines of spots which were before seen in it, were wholly belonging to the soft matter; another circumstance analogous to what is met with in *Chara*.

This single observation on *Tubularia* was prolonged in the hope that the destruction of the hinder arms might have proved to be part of a process of lengthening the tube. Some of the later appearances were evidently the effect of disease, and the action between the mouth and stomach during the first day seemed unnaturally violent; yet the currents there will be seen to bear a resemblance to those which accompanied vigorous growth in *Sertularia*. The instance of deposition recorded stands at present by itself; that of absorption will be fully corroborated. The circulation in the tube exhibits a character hitherto, I believe, only observed in the vegetable kingdom.

SERTULARIÆ.

Each of the divisions *Tubulariæ* and *Sertulariæ*, as adopted by CUVIER, includes widely differing genera, while they separate others that are closely allied. Some which I think should be placed among cellular polypi, will be noticed hereafter;

others I have not seen; but *Plumularia, Sertularia,* and *Campanularia* belong to one very beautiful natural family, for which the old name of *Sertulariæ* may perhaps be well retained, and these are the subject of the observations under the present head. The specimens figured include some principal variations in the form and position of the cells.

Sertularia pluma, Plate VIII. fig. 2. (Ellis, pl. vii. b B. *Plumularia pluma,* Lamarck), was found at Dover, October 1832, on the ropes of *Fucus siliquosus.* It consisted of a minute horny tube, creeping along and round the *Fucus* with frequent anastomosis, (a usual property of the *Sertulariæ,*) and sending out offsets at short intervals. These had each a main stem, feathered with jointed branches directed to each side alternately in close succession: every joint was elaborately formed of thin transparent shell, of which one part continued the branch, and the rest formed a cell for a polypus; all the cells on a side branch taking one direction. Each plume might comprise from 400 to 500 polypi. It was neglected to be drawn till the polypi were shrunk: when living, their arms spread widely, their body scarcely projecting beyond the edge of the shell.

All the polypi were connected together by a soft matter, of a pulpy or finely granulated appearance, which was a continuation of their substance, and extended throughout the interior of the branches, the stem, and the creeping tube or root. When a magnifying power of 100, and still more clearly when one of 300, was used, a current of particles, various in size and of irregular form, was observed running along the axis of this soft matter. It flowed in one channel, alternately backwards and forwards, through the main stem and lateral branches of a plume, and through the root as far as the opacity admitted of its being traced: sometimes it was seen to continue into the cells. The stream was throughout in one direction at one time: it might be compared to the running of sand in an hour-glass, and was sometimes so rapid in mid-tide that the particles were hardly distinguishable; but it became much slower when near the change. Sometimes it returned almost without a pause; but at other times it was quiet for a while, or the particles took a confused whirling motion for a few seconds, the current afterwards appearing to set the stronger for this suspension. The whirling or starting motion took place sometimes at one sometimes at another part of the stem and branches during the intervals of the currents. Five ebbs and five flows occupied fifteen minutes and a half, the same average time being spent in the ebb as in the flow. The longest continued stream was two minutes and a quarter; the longest suspension, half a minute.

When the connexion of a plume with the root was interrupted by bending its stem, the stream running down the middle was observed to continue its flow up one of the lower and stronger lateral branches, and then to return down that branch and up the main stem, the course of the current in most of the other side branches being still the same as in the middle one.

On a stem being cut off below the commencement of the side branches, a few seconds passed before anything exuded from the stump. A small stream of particles then issued, soon succeeded by a flow of viscid matter: this stopped a while, and then went on again, but ceased altogether in about five minutes. It hung like honey about the end, and on its gradually clearing away, the wound appeared healed. The successive flow, slight agitation, and reflux of particles, continued in the stump as before the separation. The alternate currents in the axis of the soft matter were seen in all the *Sertulariæ* that were examined, and appear to be an essential character of this family. Further particulars regarding them will be noticed as they were observed in other species.

I am not aware that any writer of our own country has mentioned the existence of these currents, unless it be Dr. Fleming, in an account of *Sertularia gelatinosa* given in the Edinburgh Philosophical Journal, 1820 *; but his description, if accurate, must refer to a different action. Some of the continental works which were the most obvious to be consulted are silent regarding them ; but on further search, in Cavolini's "Memorie per servire alla Storia de Polipi Marini," published at Naples in 1785, the general character of the motion was found very clearly and explicitly given.

It is extraordinary that so singular a phenomenon as the flow and reflux of the circulation in one and the same channel, announced evidently by a careful and able investigator, should have remained, as it appears to have done, almost unnoticed for nearly half a century. I am glad to corroborate these observations by a quotation from his treatise, which serves also to explain some of his opinions †.

* "When in an active state, I have observed the water taken in at the mouth descend, for the space of several seconds, through the gelatinous parenchyma of the body and footstalk, and again return to be ejected. The fluid thus circulating did not seem to move in a body through tubular vessels, but to be divided into minute globules, which permeated a cellular structure."—Edinburgh Philosophical Journal, vol. ii. p. 85.

† "Un fenomeno assai singolare nell' economia delle Sertolare è un movimento che si osserva nell' interiore del corpo come in un proprio tubo. L' esteriore corneo invoglio, ordinariamente transparente, chiude e veste il corpo molle dell' animale, il quale corpo si vede essere formato come di un amasso granelloso. In mezzo di questo corpo per una linea alungo si vede che una simile granellatura venga transportata con moto vorticoso di un fluido che non si arriva a distinguere: mercè di questa agitazione si vede che quelle briciolette di materia ora vengono portate in giro, ora in una corrente salgono in sopre or discendono; e questo fenomeno accade così nel tronco principale che nei rami fino a toccare gli organi polipiformi e dura ciò finchè vive la sertolara anchorchè i suoi organi siano strettemente ritirati."—p. 121.

"Questo corrente si vede estendersi per tutto il tratto del corpo di esso polipo (*Sertularia Dichotoma*) e fino alla testa dei polloni che si sviluppano nel mezzo dei divisati calici; si arresta poi nel piede di questo quando si sono sviluppati in organi polipiformi. . . . . Questo canale altro non può essere che il cuore; in fatti quando i polloni si sviluppano in organi polipiformi siccome questi devono predare e digerire il cibo non può il cuore più apartenere a loro per che devono serbare un organo ministro del cuore. Ma è cosa degna di considerazione che per questo cuore discendano briciolette medesime che sembrano entrare nella composizione del corpo dell' animale . . . . e se potrebbe dire che queste briciolette di materia son quelle che ricevuta avendo certa alterazione dall' azione del cuore, si animalizino, cioè passino nella formazione del corpo stesso dell' animale. E queste briciolette dovranno necessariamente al cuore venire dai ventrigli, i quali posti nel fondo degli organi polipiformi triturano gli animaluzzi infusorj che dalli tentacoli sono acchiappati."—p. 197.

Cavolini saw the currents in all the *Sertulariæ* observed by him, but he did not detect their continuation into the stomach of the expanded polypi; a circumstance equally belonging to every species that I have met with. Had he done so, he must, it would seem, either have included the stomachs, with the branches and the root, under the name of *heart*, or probably would have relinquished a term which, on several accounts, appears to be misapplied. His not having perceived the stream of communication may be accounted for by its being generally much smaller and less conspicuous in a short space near the base of the cells, as well as less regular in its periods, than that which runs along the stems and footstalks.

Sertularia pumila, Plate VIII. fig. 3. (Ellis, pl. v. a, A.), was gathered growing above low water mark at Dover, October 1832. Each of its cells is divided near its base by a partition *a*, on which the polypus is fixed, and which may be seen to be pierced with an oval hole *b*, if a section of the shell is made a little above or below it. In the complicated cell of *S. pluma*, last described, this perforated septum was not detected; but I believe it to exist throughout the family. *Sertularia abietina* has the aperture long and narrow. In *Campanularia* it seems to be round, and in the middle of the septum, which in that genus is very conspicuous.

The connexion between the stomach and the stem, by the aperture, takes an angular course in the present species, and could not always be perceived. One polypus had within its mouth revolution of darkish substances, which after a while it disgorged; in the stomach of the same there was irregular motion of particles, and an action seemed to be going on between the two; the upper part of the neck sometimes swelling, and the cavity of the mouth extending down that portion in a tubular form. Within other expanded polypi, no motion could be detected.

In the substance of the necks of the polypi, transverse lines were visible, bearing a resemblance to those characteristic of voluntary muscle in the higher animals. I have observed the same appearance on a band forming the edge of the bag of a *Lucernaria*, and also very distinctly in the axis of its numerous knobbed tentacula.

Sertularia setacea, Plate VIII. fig. 4. (Ellis, pl. xxxviii. 4 D. *Plumularia setacea*, Lamarck,), was found at Brighton on flag, with which the shore was strewn after a storm, July 1833. It was distinguished by its subconical cell, so short as commonly to shelter only a part of the stomach, and by its spinous ovaries, differing from those in Ellis's figure, and mostly sessile on the creeping root. The ova within were opake and yellow. Its polypi had from sixteen to nineteen arms, and when they were full-blown it was an object of remarkable beauty. From its transparency, and the smaller number of its moving particles, their individual quivering motions and the course of its currents were more conspicuous than in the former two species. The stream sometimes extended only to the pulp below the septum, and sometimes mounted into the stomach; and in whichever part it terminated, agitation took place there on the ceasing of the upward flow. The soft part within the branches, which adhered generally to one side of the tube, had the look of a slimy matter, inclining to granular, and held together by greater tenacity at its outside. Nothing like muscular contraction was seen in the pulp of this or any other species.

As a little globular animalcule was driving rapidly past one of the expanded polypi, it instantaneously seized it, and brought it to its mouth by contracting its arms. They gradually opened again, except one, that remained a while doubled, with its end on the animalcule. The mouth indistinctly seemed filled with hairs or tentacula, that closed over the prey; and after a few seconds it was carried slowly down, in the manner of the *Actiniæ*, the mouth contracting and the neck enlarging, into the stomach: here it was uncertainly seen, and soon disappeared. Agitation of particles in the stomach followed the swallowing, and then the currents between the stomach and the branch went on again as usual.

One polypus was supported on a stalk that, probably owing to injury, was entirely empty of pulp; yet it was still open and vigorous.

The minute and delicate species, Plate VIII. fig. 5, found at Brighton July 1833, resembled *Campanularia* in having its cells placed on footstalks, and in the branching of its stem, but was differently jointed, and the cells and polypi were nearer to *Sertularia pluma*. The polypi had sixteen arms. The currents, which generally extended into the stomach of each, set more strongly into the side appendage, which they all possessed, ending in two small ears, and looking like a continuation of the footstalk. All the shell of the cells, when dry, gave the colours of thin plates, owing to its extreme tenuity.

The only ovary, *a*, in the specimen was enormously large compared with the cells, depressed at the end, and transparent. It had an opake ovate substance within, of a dirty yellow colour, that was connected with the base and with the extremity of the ovary by a column of soft matter running nearly in the axis. Within this, between the base and the ovate body, currents were seen like those in the stem, and sometimes a motion of particles was observed in the adhesion to the shell at *b*. No separate ova, like those to be noticed in *Campanularia*, were visible.

Several species of *Campanulariæ* that were observed agreed very nearly in the form of their bell-shaped cells; with a distinct septum, and a thin column of soft matter between it and the base of the cell. The branches also of all had annular indentations round them, more or less numerous, which form a simple and beautiful provision for giving flexibility combined with strength.

The specimen figured in Plate IX., or that in Plate X. fig. 1, may equally serve, I believe, as an example of *Sertularia dichotoma*, (taking that to be the name of ELLIS's species, pl. XII. a, c, C. and XXXVIII. 3, B.); but the former of the two was much the stouter in its growth. Its cells were from ·018 to ·02 inch in length, and the arms of its polypi commonly thirty; the other had about twenty-six arms and cells not exceeding ·014 inch.

The young branch, Plate IX., furnished an instance of the progressive production of a polypus; for having been drawn (August 19, 1833,) on account of the singular form of the cell *b*, the sprout *c* was found to have lengthened during the tracing, and this led me to watch its growth at intervals during that and the following day. About five hours were spent in the completion of the footstalk, and twenty-seven more in that of a cell and its polypus.

The branch sprang from a creeping tube that had been recently broken at a little distance beyond. This, like the pruning of a tree, may have accelerated the growth; and from the same cause as was supposed, the internal currents were very irregular. Dancing of particles sometimes took place along the whole stem, sometimes at parts of it only. The set of the stream was sometimes up or down *a b* and *c* altogether, sometimes out of *a* into *c*, or the contrary. Before a current in the stem became strong, some particles would for a while appear to struggle on against it. The sprout *c*, which looked like a continuation of the stem, was at first quite full of soft matter, and the motion of the currents in it was inconsiderable; but they increased so as soon to become greater by far there than in any other place, and its soft part was then dilated and contracted alternately, except at its end, which was always full. The time of the stream's running into *c* was, on an average of seven alternations, six minutes; of the reflux four minutes. The longest time of the former was eight minutes and a half, the shortest of the latter two minutes. In about two hours the sprout had assumed the appearance *c* 2. Particles of larger size were now seen, maintaining a continual circulation within, except that all was still for a short time when the pulp was most shrunk: when it was dilated they had great agitation. Their flow was always downwards along the axis, and upwards along the sides of a defined cavity, being in a direction opposite to that observed in the polypus of *Tubularia*; and the manner of the flow, and the abundance of fluid, more resembled the circulation of *Chara* than anything I had before seen in *Sertularia*; but still the dancing motion distinguished them. Some of the particles were nearly round, and transparent, other larger ones seemed like masses of the pulp brought into circulation.

The part of the branch towards the end was more opake than the rest: slight currents were occasionally seen in the axis there at long intervals; and the outer portion of this part, which was in the act of growth, had not a granulated appearance, but was marked with radiating lines nearly parallel to each other in front, and more diverging on the sides, so as to make always a considerable angle with the surface; and among them no current or motion was seen.

In six hours more the branch had the shape of *c* 3, the rudiments of a cell *f* appearing at the end: its commencement was about three hours previously. The peculiar circulation down the axis and up the sides was now only during the latter part of the influx; the dilatations were lessened, and a part of the shell at *d* was never filled; a separate agitation had begun to show itself at *e*, and a faint one at *f*.

After being left for a night, the incomplete cell had grown, in twelve hours from its beginning, to the appearance *c* 4. The soft matter of the branch was now wholly detached from the shell on the left side, but lay close along it on the right: the currents in that part were become scarcely perceptible, very slight dilatation and shrinking being sometimes the only indications of them. The two tracings *c* 5 give alternations in the soft matter of the cell three hours later: the striæ indicative of the forming process, smaller than those seen during the growth of the branch, still remained near the end: when viewed as an opake object, the part *f* was a brownish orange, and *g* milky. In a short time rudiments of arms appeared; and *c* 6 represents them when the cell seemed completed after sixteen hours' growth: its end, however, was closed, and the polypus, by slow motions backwards, forwards, and sidewise, was releasing the yet imperfect arms from their adhesion to the side. The tracings *c* 7 and *c* 8 show their continued progress.

At twenty-six hours from the commencement of the cell the arms, apparently fully formed and folded over each other, had been pressing against the end of the shell, which seemed still to inclose them but had something of a ragged look, when at length several were slowly raised beyond the cell. On their being drawn in again, a little transparent film was seen projecting, the remains, as appeared, of what had covered the end. The arms were then again protruded as at *c* 9. The stomach enlarged and contracted, but seemed coated with an orange-coloured matter in irregular masses.

In one hour more the branch was terminated by a fine perfect polypus, fully expanded, with twenty-eight arms, which was discharging at the mouth the opake orange-coloured matter that had lined the stomach.

The mouth, when open, varied from the form of a saucer to that of an upright cup; no hairs were detected within it.

The pulp in the stalk had now many fresh adhesions to the shell on the left (*c* 9), and must therefore have swollen since the tracing *c* 4, and contracted again.

For some time after this the polypi of *b* and *c* continued expanded and, to appearance, healthy; but that of *a*, which on the first day gave proof of its vigour by seizing and swallowing an animalcule, shrivelled greatly during the night, and was found in the morning to be evidently in the course of absorption. A downward flow from it of particles that were mostly small and a few orange-coloured, took place every eight or ten minutes, continuing for three or four: the influx in the intervals was scarcely or not at all visible. The progress and completion of this absorption are shown in the tracings *a* 2, *a* 3, *a* 4, *a* 5.

Another *Campanularia*, resembling the above, after being kept some days, though fresh sea-water was often supplied gave the following symptoms of decay. The polypi which remained were all contracted, with great agitation of particles in their interior, and increase of the currents. Next, the continuity of the soft matter which had connected them with the stem became broken, and no further motion was seen beyond the separation. Still later the current extended from the root along a part only of the line of pulp in the stem; a vortex was established where the stream ended, and the pulp beyond that part assumed a larger granulation; the root being the place where the remains of life were latest retained.

The drawings of Plate X. fig. 1. may, like the last, be referred to ELLIS's pl. xxxviii. 3 B. He there represents young polypi as emerging from ovaries; and states, in the French edition of his work, that they appeared evidently to spread their tentacula; and that some, becoming detached, sank to the bottom of the glass of water in which they were placed, where they began to move and extend, like the freshwater polypi. CAVOLINI, on the other hand, watched in vain to witness the exclusion of the ova; but he produces experiments and arguments, from which he infers that the young polypi of ELLIS were imaginary, and that the ova, when first given out, have no external organs or sensibility, but resemble the seeds of plants, with a scabrous surface to enable them to adhere to bodies *.

When my specimen was subjected to the microscope (July 20, 1833), a kind of tube or hollow cord of granular matter could be seen, more or less distinctly, extending through each of its ovaries from the base, generally along the axis like the columella in plants, and having the ova attached to it. These, in some ovaries (as *a*,) were small, and the cord spread out into a substance that filled up the end, indicating, by its appearance, that the shell was there not yet completed; in others (*b*,) the substance at the end was shrunk, and the ova were grown larger; in others, again, the foremost ova reached the end, and had an appearance of maturity; those behind were always less advanced.

The ova were roundish, and consisted of two portions; the outer and more transparent, that might be called the white, inclosing an inner bag filled with particles in fluid like those in the currents of the stem and connected with them by the cord. The current and agitation were seen in the inner bags only, and the flow into and from them alternately along the cord was strongly marked. When near the end of the cell, the ova became more opake, which hid the changes that might be taking place within them. The number in a full ovary was about seven. Their expulsion took place from several with much the same circumstances, of which the ovary marked *b* may give an example. In two days after that tracing was made they had filled the shell to the end, and began to emerge in succession at an average interval of six hours. The protrusion (*b* 2, *b* 3) took about a quarter of an hour, and was commonly preceded by a transparent projection, like torn membrane, before the end of the ovary, and a few active particles in the water.

Each young polypus was at first an oval body on a very short pedicle, and appeared dark from being filled with opake particles, generally smaller than those in circulation, which were in great and continual agitation and seemed to have no

* Dr. GRANT, in his observations on the ova of various zoophytes, (Edinburgh New Philosophical Journal, vol. i. p. 150,) concurs in the opinion that ELLIS was in error.

channel of communication with the ovary. Rudiments of arms were at first scarcely discernible, but gradually grew more distinct. In an hour or two the mouth opened, and the imprisoned particles rushed out with the vivacity and rapid motions of bees swarming (*b* 4). Their action could not at all be referred to currents in the water, and was very different from the dancing of inorganic molecules; such, indeed, that it was difficult not to believe them possessed of vitality. They darted about in all directions, some sweeping off at once, some flying to a distance and returning as quickly, others traversing hastily the surface of the polypus, and all by degrees dispersed in the surrounding water. The mouth closed slowly; agitation continued in those particles that remained within; after a while it opened again, and more escaped, and thus in about an hour the cavity became nearly emptied of them and filled with clear water: a soft substance occupied its bottom (*b* 5). The mouth continued to open and close slowly at intervals: the arms, which were about twelve in number, lengthened, and the bag contracted. This action of the mouth, slow motions of the arms, and slight inflection of the pedicle, were the only signs of vitality in the young polypi. Afterwards, to my surprise, they gradually shrunk away; and I found that though their internal communication with the ovary seemed cut off till some time after they had discharged their particles, agitation subsequently began in the substance at the bottom of the bag; and currents at long intervals succeeded, between it and the top of the ovary, which continually subtracted matter from the young polypus. The latter became more rugose, and at length was altogether absorbed. Thus *b* 6 gives the appearance of the same ovary when the fifth ovum was about to emerge; of the first there remained but a doubtful vestige; the second had diminished by degrees to a small knob; the third was fast dwindling, and the fourth was emptied of particles. The cord, with its side attachments to the ovary (see *c* 2), continues till the last ovum is gone; when it is itself absorbed, leaving the shell empty. Apparently the disappearance of every young polypus was caused in the manner described. In no instance could I discover one to be detached, as I entertained no doubt that they would have been if left in their natural situation in the sea, and thus my hopes of tracing their further economy were disappointed.

A small *Campanularia*, which was thought to be in an early stage of its existence, Plate X. fig. 2, grew on a cell of the *Sertularia pluma*: it consisted only of a knob or bulb by which it was fixed, a simple stem, and one cell containing a polypus. The usual currents alternated every few minutes between the stomach and the bulb, the fluid continuing longest in the latter, and the greatest agitation of the particles being there. It remained mostly expanded for two days, during which no growth or material change took place in it.

The annular strictures along the stem of the *Coryne*, Plate X. fig. 3, and the shape of its small cell, gave to this zoophyte a sort of resemblance to *Campanularia*. The stem seemed capable of a slight motion, and the head and arms were constantly moving slowly about. The ova that grew naked among the scattered arms had a central opake portion, more distinguished from the surrounding albumen than in those of the ovaries just described. A stream of particles flowed through the short pedicle of the ova, alternately into and out of their central part; which swelled with the inward flow, and shrunk with the returning one; the flux and reflux were about two minutes each. The alternate current was seen in the axis of the cell, but the other parts of the line were too opake to show it: there appeared to be none in the transparent albumen, nor in the arms.

The examples given exhibit the circulating fluid of the *Sertulariæ* under a variety of circumstances. It appears from them to be the great agent in absorption, and to perform a prominent part in the obscure processes of growth; and its flow into the stomach of the polypi seems to indicate that in the very simple structure of this family it acts also as a solvent of the food.

The particles carried by it present an analogy to those of the blood in the higher animals on one side, and of the sap of vegetables on the other. Some of them appear to be derived from the digested food, and others from the melting down of parts absorbed; but it would be highly interesting to ascertain distinctly how they are produced, and what is the office they perform, as well as the true character of their remarkable activity and seemingly spontaneous motions; for the hypothesis of their individual vitality is too startling to be adopted without good evidence.

I could not satisfy myself as to the immediate cause of the currents. Preceded as they usually are by agitation of the particles, and in the absence of all appearance of muscular contraction of the soft matter in the tubes, the explanation of this question may perhaps depend on that of the former. The alternate swelling and shrinking of the pulp supposes either a filtration of water through the parietes of the tube, or a current of animal fluid, which I could not perceive, flowing between the tube and the pulp in a direction opposite to that in the axis; for it is evident that (in *Campanularia*, at least, and the same may be inferred throughout,) there is no interruption at the seeming joints to the continuity of the shell.

Along the arms of the *Sertulariæ* there are at intervals short projections like blunt hairs, single or in tufts, and generally more numerous towards the ends; and it seems to be by their means that the polypi attach with a touch, or release at will, substances that drift within their reach. In *Coryne* there is a similar provision on a knob at the end of the arms. I have never seen in either the least appearance of ciliæ, nor any of those currents in the water near the polypi which are so conspicuously produced by other tribes of zoophytes. Must we infer that this family is furnished with no means of respiration? or (adopting one of the suppositions suggested above) may the exposure of the soft matter to water, passing in and out through pores in the shell, supply its place?

In the processes of growth, the shell must evidently be thickened at some places, and at others softened or dissolved; but it is not altered by absorption of the soft parts.

ASCIDIÆ.

The small *Ascidia*, Plate XI., fig. 1 to 7, was not unfrequent at Brighton, in August 1833, on pieces of *Conferva elongata* that were washed ashore, and had to the naked eye the look of minute lumps of pellucid jelly with a spot of orange and grey. It does not, I believe, come within either of the descriptions of subgenera of Savigny or MacLeay.

It occurs in groups that consist of several individuals; each having its own heart, respiration, and system of nutrition, but fixed on a peduncle that branches from a common creeping stem, and all being connected by a circulation that extends throughout. Their parts are of such transparency that their interior is easily seen. Their external shape is that of a pouch compressed at the sides, and fixed at the hind part of its base upon the peduncle.

Its two openings are in the form of very short tubes; that of the mouth *g* at the top of the pouch, and that of the funnel *f'* in front*. The longest diameter, from the peduncle to the space between the openings, is about ·085 inch.

The outer covering is a tough coat, *a*, a continuation of the peduncle, more pliable near the openings; lined internally with a soft substance or mantle *b*, in which a ramifying circulation is very distinct. A great part of the interior is occupied by the branchial sac *c*, which is subcylindrical, flattened at the sides, and has its axis vertical; its cavity terminating upwards in the oral opening, and being closed at the bottom. It is united to the envelope or to the mantle above and behind; the juncture, *c' c'* beginning in front of the oral opening, extends backwards on each side of it, and then downwards in two lines: between these, along the middle of the back, is a vertical compound stripe *d* (fig. 4), that seemed to me cartilaginous. At the bottom the sac appears to be enveloped by the soft substance of the mantle, but at its sides and front a vacant space is left between them, that ends in the opening of the funnel. The branchial sac is more compressed towards its lower part; and here are placed, externally to it, the heart *m* on the left, and the stomach *i* and other viscera on the right side, the vent *k* opening upwards at the front into the funnel. On its sides and front the sac is perforated by four rows of narrow, vertical, irregularly oval holes or spiracles, about sixteen in each row, placed at less than the diameter of one apart from each other. Through these the water, which flows constantly in at the mouth when its orifice is open, appears to be conveyed to the vacant space *f* between the sac and mantle, and it then escapes at the funnel. The sac seems extremely thin between the spiracles; but their edges are thickened, as if cartilaginous; and they are lined with closely set ciliæ, which, by their motion, cause the current of water. When these are in full activity (fig. 7), the effect upon the eye is that of delicately-toothed

* The terms back and front as they stood in my memoranda are here interchanged, to accord with the designation of the parts given by Savigny and Cuvier.

oval wheels revolving continually, in a direction ascending on the right and descending on the left of each oval, as viewed from without; but the ciliæ themselves are very much closer than the apparent teeth, and the illusion seems to be caused by a fanning motion given to them in regular and quick succession, which will produce the appearance of waves; and each wave here answers to a tooth. The spaces between the rows of spiracles are of much more substance than the intervals of the spiracles: some ligaments f'' are stretched from them across the side cavities to the mantle, that seem intended to keep the branchial sac expanded. These spaces also support finger-like processes *e*, about eight in a row, that project nearly at right angles into the central cavity.

The central cavity I shall venture to call the mouth, though the mouth is said by Cuvier to lie at its bottom. The large short tube at its opening ends in five or six obscure indentations; it can be drawn in and closed at the will of the animal, as can the opening of the funnel. At the bottom of the tube the entrance of the mouth is guarded by simple tentacula g', some longer, some shorter, ranged subalternately: their number was not ascertained. Whatever little substances, alive or inanimate, the current of water brings, flow in unless stopped by the tentacula—and they do not appear fastidious,—to the mouth; and lodge somewhere on the sides of it. A lively animalcule will sometimes disengage himself by struggling, and dart about in the cavity till he lodges on some other part; or if a morsel is found unsuitable, it is ejected by the funnel's being closed, and the branchial sac suddenly contracted vertically. Mostly, however, whatever part the food lodges on, it travels from thence horizontally with a steady slow course towards the front of the cavity, where it reaches a downward stream of similar materials *h*; and they proceed together, receiving accessions from both sides, and enter at last, at the bottom, the œsophagus h': this is a small flattened tube which carries them, flowing on in the same way, without any effort of swallowing, towards the stomach: the tube takes a sharp curve upwards and backwards before arriving there.

It is extraordinary that these particles pass along in the mouth just behind the spiracles, when the ciliæ are in full activity, without being at all affected by them. I have in some positions seemed to catch a glimpse of a membrane suspended within, too transparent to be commonly seen. One may imagine the water to pass to the spiracles strained through the meshes of such a membrane, and the food to be carried along it by invisible villi; but this is mere conjecture. The projecting fingers have the effect (whether intended for such a purpose or not) of detaining some prisoners more bulky than the usual food of the animal; for in several individuals I met with small shrimp-like crustacea confined between the rows: one escaped during an observation; another after three days seemed as lively as when first swallowed.

The stomach (*i* and *i* 2) runs backward horizontally; its fore part had an inflated look when seen from the side (fig. 2), and when from below (fig. 5) that of possessing two lateral lobes. The food after accumulating here was observed to be pressed onward to the hinder portion, leaving a narrow opake line of connexion with the œsophagus; the rest of the fore part, of which the apparent volume was nearly as before, having an ochreous tint: this was inferred to be the liver, enveloping the stomach above and on the sides, and accords with its place in other Ascidiæ and Mollusca. The line is continued by the intestinal canal, that rises and then bends forward, taking the form of a reversed S, and terminates in an ascending rectum and sphincter *k*. The fæces are considerable, as might be expected where the food is taken with so little discrimination. Transparent vessels, that may be supposed lacteals, *l*, ramify along a part of the intestine, and meet at a collection of globular bodies, from whence in the individual, fig. 2, two flattish lobes extend backward; in others these were wanting. From the meeting of the vessels two branches ran, one downwards and backwards, which was lost under the stomach, the other forwards; and from the direction it took, I supposed it might communicate with a main stream of blood near the heart. Some individuals had not the projection above the vent observable in fig. 2.

But the part that struck me as most remarkable in this creature was the circulation, of which a good view can be obtained through the transparent coat; for the particles of the blood are numerous, and though not uniform in size or shape, are mostly between ·00025 and ·0002 inch in diameter, and approaching to globular. They are easily measured, as in the intervals between the spiracles they pass mostly but one at a time (fig. 7).

The creeping tube, which unites the individuals of a group, is the channel for two separate currents of blood, an upward and a downward one, that are flowing at one and the same time, and that send off each a branch to every peduncle: the blood thus passes into the animal by one current, while another carries it back. One of these canals communicates at the termination of the peduncle with the heart; which is placed, as has been mentioned, near the bottom of the branchial sac on the left side, and consists of a transparent ventricle, or boyau, running forward and a little sloping downward, in a channel hollowed to contain it. Along the whole length of the boyau a part on one side of its axis seems fixed to the channel, the rest free and contractile.

When the blood entered the heart from the peduncle, contraction began at the middle of the ventricle, impelling onward the contents of the fore part; and the contraction of the back part followed in the same direction, so as for the whole to have the effect of one pulsation: the heart was then filled again by a flow from the peduncle. The intervals of the pulse were pretty regular in the same individual; but in different ones they varied from two seconds to one and a half second. Part of the blood thus impelled formed a main upward stream along the front of the branchial organ, branching off at each of the horizontal passages between the rows of spiracles, and at one above them on a line with the junction to the mantle on each side. All these again united, and formed a downward current behind. The horizontal channels were connected also by the smaller vertical passages between the spiracles; the set of the current in the latter being upwards for the two lower rows, and downwards for the two upper ones.

Another large portion of the blood, on leaving the heart, immediately divided into many ramifications, that spread like a network over the stomach and intestines, and the soft substance of the mantle. Of these a part ran into the horizontal passages above the branchial sac, a part into the descending back stream; a large proportion, after leaving the intestines, took a short course, and collecting into one channel, flowed into that stream near the bottom; and all, united, then entered the peduncle, and constituted the returning current that went to circulate in other animals of the group.

After this circulation had gone on for a while, the pulsations became fainter for a few beats, and the flow slower; and suddenly, with but slight pause, the whole current in all its windings was reversed. The heart gave the opposite impulse; the channel in the peduncle that before poured in the blood, now carried it back, and the other the contrary; and every artery became a vein. These changes continued to succeed each other alternately; the average time of the currents being the same in both directions, but the period of each varying within a single observation as much as from thirty seconds to two minutes. The phenomenon, like the currents in the *Sertulariæ*, was invariably met with in every animal of the species that came under my notice.

Sometimes when the creeping tube or the peduncle has been injured, the circulation of an individual is in consequence insulated, but without appearing to impair any of its functions. I severed one at the part where it joined the peduncle; when for a few seconds the pulsation ceased; it then began irregularly and with considerable pauses, and increased in steadiness as it went on. At first the impulse given by the heart was towards the front; and the downward back stream, instead of flowing out at the wound, was poured into the hinder end of the ventricle, at *n*, fig. 2; but when the current was reversed, part of the blood was driven for a time through the stump of the peduncle into the water: however, it soon staunched, and all the vital actions went on as before the separation, except that at the beginning of every pulsation there was a slight recoil.

In one case, where the circulation did not extend to another animal, one channel, and only one, was open in the peduncle, and in this a small current ran to or fro according to the direction of the impulse given by the heart. Some animals, which had probably been injured but were still connected with other vigorous ones, seemed to be in course of absorption. One was observed in which the soft parts were so shrunk as to occupy a small part only of the tunic; the currents of its peduncle extended into this mass, but no heart or motion of branchiæ was visible. Upon looking at the same the next day the tunic was empty, the soft matter and circulation reaching only to the end of the peduncle. I also once noticed a flux and reflux of the blood in a creeping stem, where the current did not communicate with any animal.

In some of the last-mentioned particulars this *Ascidia* bears a resemblance to the *Sertulariæ,* and like them it increases by sprouts: the two streams of the stem run through the bud before its organs are developed. The production of its young from ovaries did not fall under my observation. No proper motion was seen in the particles of its blood, like that of the *Sertulariæ.*

In a sessile *Ascidia,* nearly half an inch in length, of which the coat was too rough and opake to allow an inspection of the branchiæ, the circulation was distinctly visible in the mantle near the openings, and the particles in the blood were only of about the same size as in the above.

A *Polyclinum* (Plate XII. fig. 1.) was met with abundantly at the same time, occurring upon similar *Algæ,* like a grey slimy crust speckled with white and black. The individuals that composed the groups were placed as if promiscuously, and without arrangement. Their branchial sacs were rather smaller than those of the branching *Ascidia* described, and considerably resembled them in general form and in having four rows of spiracles with the same action of ciliæ round them; but instead of being each covered by a proper envelope, they were connected by one common coat that stretched over them all, and was joined to them only round the oral orifices, which projected externally with a large opening and six distinct indentations: within were simple tentacula, like those of the other. The vent *c* was placed near the base of the branchial sac, with the heart *b* on one side, and on the other some viscera, that were not well defined. Into the cavity formed by the coat the pellets of fæces were discharged, and were carried away by a current that was constantly flowing in through the spiracles of the branchiæ, and running out at a common funnel.

Some of the funnels rose into swellings and tubes of considerable size compared with that of the component animals; and they contracted on being touched, showing the coat to possess irritability. Opake whitish spots studded it here and there, and encircled the openings of the funnels, and more thickly those of the mouths. The thinness and transparency of the coat in the specimen drawn, were such as to show distinctly within, the particles of the blood running between the spiracles of the branchiæ, and in one instance in the heart. They were much fewer than in the *Ascidia* before described; and on the coat itself I could detect no circulation whatever.

Instead of the finger-like bodies projecting into the central cavity of the branchial sac in the other, this had within, a thin ledge between each row of spiracles; and in front there were three tapering moveable prominences, one connected with each ledge, that were sometimes stretched forward horizontally into the cavity, at others bent downwards with a spiral curve (*a*, *a*). These seemed to suspend a generally invisible vertical membrane, and to assist in giving the food its direction towards the stomach; for it moved horizontally along the sides of the cavity, as in the other *Ascidia,* and when it reached the front took a *spiral* motion downwards. The branchial sacs occasionally contracted forcibly to reject what had been stopped by the tentacula, or found unfit for food. The oral opening, instead of projecting, was then drawn down below the level of the coat, and depressed it; the ciliæ being also at such times closely stretched across the openings of the spiracles (*d*). Whenever the ciliæ stopped their action, they were seen to be very numerous and appeared almost as a continuous membrane.

The conveyance of the food from the different parts of the mouth to the stomach by an even progress, and without any muscular act of swallowing, remained as mysterious as before.

No note was taken of the existence of alternations in the circulation of this polyclinum ; and I cannot now assert it as a fact, though I believe it to be so. It would be interesting to trace the limits of that phenomenon in the animal kingdom*.

No indication of a nervous system was noticed in either of these *Ascidiæ*; and not being previously prepared for the investigation by reading the labours of others, it did not occur to me to look for it.

CELLULAR POLYPI.

It may assist towards establishing the place of *Cellularia* and *Flustra* to add some general remarks on such species of those genera as came under my observation. These all appeared to belong to one natural family, far higher in its organization than the tubular polypi, with which some of its members are even yet associated. They show nothing of the internal currents which in the *Sertulariæ* connect the different parts of the zoophyte; nor indeed have I succeeded in any instance in detecting their circulation. Each animal when retracted is inclosed entirely by its cell; through a valve in which, the arms and mouth are sent out. A short sheath mostly precedes them, from whence the arms rise straight together, and then open to a funnel- or bell-shaped figure of beautiful regularity. Though radiating like those of the *Sertulariæ,* they are organs of a different kind ; not extended motionless, waiting for such food as may be drifted to them, nor rough with irregular projections for attaching it, but uniformly fringed with a row of ciliæ on each side; of which the lively action is so identical with that on the spiracles of the *Ascidiæ* described, that I cannot doubt them to serve, equally with those, the double purpose of drawing food to the mouth by currents, in the water, and of respiration. In all cases which

* Since the reading of this Paper, an extract from one of the letters of KUHL and VAN HASSELT has been pointed out to me in the Bulletin des Sciences Naturelles, tom. ii. p. 212, which is dated in Java 1821, and announces their discovery of the same kind of circulation in the *Biphoræ*. CUVIER in his Règne Animal, edit. 1830, does not notice this; and in 1824 BLAINVILLE, in the 32nd volume of the Dictionnaire des Sciences Naturelles, p. 114, only refers to the account as being not understood.

It ought also to be stated, that in the 60th volume of that work, published 1830, article 'ZOOPHYTES,' the currents in the medullary part of the *Sertulariæ* are mentioned as characterizing that family, and are considered to be an oscillation analogous to what is seen in some plants. In the same article the arms of their polypi are described as ciliated.

were noticed, the seeming revolution of the teeth was upwards on the right, and downwards on the left of each arm when viewed at the back; and several times a small globule of food lodged on an arm and travelled down it to the mouth, while the arm remained expanded and the ciliæ in full play.

From the back of the arms in most species a few fine pointed hairs were seen projecting singly or in pairs, and nearly at a right angle, as if to give notice of anything coming within their touch. What appear strong muscles, characterized by cross hatchings, run longitudinally from the insertion of the arms downwards. Within these, in the mouth, food commonly collects, and has a revolving motion there till swallowed or sometimes rejected. The act of swallowing is distinct and energetic. Plate XII. fig. 2 represents one of the animals of *Flustra pilosa*, both closed and expanded; from a specimen that rose like a small leaf from the fucus it encrusted: and fig. 3 gives those of *F. papyracea* when drawn in. Both species, in the latter position, have the arms, *c* 2, lying lengthwise and extending to the valve *b*, the mouth in the same line, and the neck folded back. At its other end the neck is joined to the side of a sort of pouch, which is indistinctly spotted, and of a reddish ochreous colour, closed at the bottom, and with a small circumscribed space in front containing particles in constant revolution, their motion having the same axis as the vessel: continuing forwards from this part, it becomes greatly contracted, and then assumes a sudden enlargement, generally filled with opake matter. The mouth and neck are placed indifferently on either side of the other parts. The whole animal of each species is moveable within the cell, and filaments are seen attaching it to the bottom and sides.

When the arms and mouth of *Flustra pilosa* were protruded, a vacancy was left in the sheath *b'* on one side; the neck was drawn forwards into the same line as the mouth; the enlarged end of the vessel in front of the pouch was also carried forwards, and its opake contents were in several instances expelled in pellets through the opening at the side of the sheath, and then again accumulated; proving this to be the anal orifice.

The course of the food swallowed was not made out beyond the neck, nor were the viscera which the pouch might contain. It seems analogous to the part marked by SAVIGNY as ovaries in several figures of his excellent work on *Ascidiæ*, but from its position we may be allowed to suppose, that at least a stomach and liver have a place in it.

Between the animals of *Cellulariæ* and those of *Flustræ*, no line of distinction was detected, though the number of arms and other details vary according to the species. In most that were inspected, the cells from their position or opacity did not allow a sight of the interior; but the general strong resemblance of the parts exposed led to the inference that a structure like that described, with a separate termination of the intestine, extended throughout.

This was plainly observable in *Serialaria lendigera*, a common species, with eight ciliated arms, which has hitherto been placed among the *Sertulariæ*; where the subgenus is characterized by the distribution of its cells in groups, at parts of the branches; and it may serve to show how secondary are characters derived from the mode of growth, in a natural arrangement of zoophytes, that a cell and animal which appear identical with this, are met with as a crust only, on the stalks of fuci.

Anguinaria anguina, fig. 4, evidently requires to be transferred from the *Tubulariæ* to the present family; and also the subgenus *Tibiana*, or at least the elegant little species, fig. 5, which has the spot of revolution visible, the mottled sac, and the filaments between the animal and the bottom of the cell.

A zoophyte allied to the above was found upon the same marine plants, which seems to be that imperfectly represented by Ellis, pl. xxxviii. 5. F.

It consists of a creeping tube and a number of stems branching from it, each ending in an animal that is shown (not very distinctly) at fig. 6. The stems, though commonly still, have free power of motion; and when one is disturbed it bends quickly to and fro, so as to strike one or two more: these again strike upon others, and thus for a few seconds all are in action; but they soon return to quietness, and the arms, which during the commotion had been doubled in, open again.

The arms are placed on the edge of a pretty transparent tunic, and have granulations on their back. They are fringed with ciliæ possessing the same action as those of *Ascidiæ* and *Flustræ*; and in the specimen drawn, small substances were occasionally seen carried downwards along them. As in *Flustra*, a part of the intestine had within it a revolution of particles and dark matter round its axis, and this part communicated with an ascending rectum. The arms at the part of the circle opposite to the rectum appeared to be continued below the edge of the tunic, and the current produced in the water and the food it brought flowed into a cavity there, at the bottom of which was active indistinct motion as if of filaments. A connexion was thought to exist between that part and the place where the revolution was going on, but no act of deglutition was perceived.

No current of blood was visible in the stem, nor any circulation either in the body or the arms. Much of the space within the tunic was occupied by a darkish appearance, the nature of which was not ascertained. I had not opportunity to inspect other individuals, but the species seemed to be intermediate between such animals of *Flustra* as I had met with, and the pedunculated compound *Ascidia*; more nearly related to the former, but approaching the latter in the form of the lower part of the body, the position of the rectum, and the absence of all apparent effort of swallowing: and if with the help of imagination we could connect the ciliated arms together by cross bands at intervals and unite their ends in a circle, extending the tunic to meet that circle, and leaving an opening for the funnel where the rectum is placed, the organ would not be unlike the branchiæ of some *Ascidiæ*. Indeed the affinity appeared to me not very distant between *Ascidia* and *Flustra*; while, to the *Sertu-*

lariæ, except in the resemblance given by their projecting arms, I can discover no more analogy in the *Flustræ* than in the *Ascidiæ* themselves.

In concluding this desultory paper, I must express my obligation to several of my friends, whose kindness has enabled me to compare my observations in a department of natural history previously little known to me, with other researches to which I should hardly have found my way alone.

Explanation of the Plates.

The numbers with the sign × prefixed denote the linear enlargement.

Plate VIII.

Fig. 1. *Tubularia indivisa* (page 366.). The arrows show the direction of the currents within. *c.* The place of their return in the tube.

d, e. Nodous parts.

a. The mouth of the polypus, pressing its fluid contents into the stomach *b.*

a 2. The same on the flow returning from the stomach.

a 3. The mouth swollen, and the stomach emptied.

f. Original end of the shell.

a 4. The tube and shell extending to the hinder arms, which are in the process of destruction.

*a*5, *a*6. Appearances at the end of the tube at two later periods.

Fig. 2. *Sertularia pluma* (page 369.).

× 1. Anastomosis of the creeping tube and manner of growth.

× 6. A plume. *a.* Side of the cells. *b.* Back of the same.

Fig. 3. *Sertularia pumila* (page 371.). *a.* Septum in the cells.

a 2. The same, seen from above by section of the shell.

b. The aperture in the septum, by which the polypus is connected with the tube.

c. Mouth of the polypus. *d.* Stomach. *e.* Empty ovary.

Fig. 4. *Sertularia setacea* (page 371.). The soft matter occupies one side of the stem, with a current of particles flowing in its axis.

a. Septum in the small cell.

b. Spiny ovaries, with the ova indistinctly seen.

Fig. 5. The minute *Sertularia* described page 372, with the appendage on the side of its cells.

a. Its large ovary, containing an opake mass, connected with the base and end by a column, and with the sides by strings *b.*

Plate IX.

A young stem of *Campanularia dichotoma*? with its annular strictures (page 373.).

a. A branch terminated by a perfect polypus.

b. Another, of which the polypus, with a deformed shell, is recently completed.

× 200. The ends of two of its arms, more enlarged.

c. A sprout in the act of growth.

c 2. The same sprout six hours later. The arrows show the course of the currents within.

c 3. The same after six hours more: rudiments of a shell forming.

c 4. Further growth of the same in eight hours. The diverging lines at the end of *c*, *c* 2, *c* 3, &c. accompany the forming process.

c 5 to 9. Progress of the imperfect cell and polypus towards completion.

a 2 to 5. Stages in the absorption of the polypus *a*.

Plate X.

Fig. 1. *Campanularia dichotoma*? with ovaries in different states (page 375.).

a. An immature one, of which the shell appears to be in progress of growth at the end. *b*. One more advanced.

c. Another, which has begun to protrude its ova.

b 2, *b* 3. Exclusion of the first ovum from the ovary *b*.

b 4. The young polypus discharging its active particles.

b 5. Its appearance after their discharge, before the emergence of the second ovum.

b 6. The same ovary after the first and second young polypus had been absorbed, when the third was become rugose, and the fourth had just discharged its particles.

c 2. The ovary *c* with the columella remaining, before discharging its last ovum.

Fig. 2. The simple *Campanularia* described page 376.

Fig. 3. *Coryne* (page 376.).

a. Its small cell, in the axis of which the alternate current was seen.

b. The naked ova, in the peduncles of which the same current appeared.

Plate XI.

Animals of the *Ascidia* described page 378. The interior is seen through the transparent coats.

Fig. 1. Manner of their growth.

Fig. 2. The right side of one supported on its peduncle.

Fig. 3. The left side of another.

Fig. 4. The back.

Fig. 5. The front and base.

Fig. 6. A sprout ending in the rudiment of an animal.

The letters refer to the same parts in all. The small arrows denote the course of the circulation when the forward impulse is given by the heart; the large arrows at the orifices, the influx and efflux of water.

a. The envelope. *b*. The mantle.

c. The branchial sac, with its four rows of spiracles.

c′ c′ c′. Its line of junction with the mantle above and behind.

d. Vertical dorsal stripe. (fig. 4.)

e, e. Finger-like processes projecting from the branchial sac into the cavity of the mouth within. (fig. 2, 4.)

f. Cavity between the sac and mantle, ending in the funnel *f′*.

f″. Ligaments stretched between the sac and mantle. (fig. 4, 5.)

g. The oral opening. *g′*. Its tentacula. (fig. 2, 4.)

h. The downward stream of food which flows into the œsophagus *h′*. (fig. 2, 5.)

i & *i* 2. The stomach, its fore part enveloped by the liver? (fig. 2, 5.)

k. The vent terminating the intestinal canal.

l. Lacteals? uniting near a mass of transparent globular bodies, with which two lobes are connected. (fig. 2.) The latter are wanting in some individuals.

m. The heart.

n. Point where the back stream of blood communicates with the heart when the peduncle is severed. (fig. 2.)

Fig. 7. A portion of the branchial sac, more magnified, to show the ciliæ surrounding the spiracles and the particles of the blood.

Plate XII.

Fig. 1. *Polyclinum* (page 382.), with a portion of the same more magnified. The branchial sacs, &c. are seen through the transparent common coat. The arrows pointing inwards indicate the oral openings; those pointing outwards, the common funnels.

a, a. Ledges on the interior of the branchial sac, each ending at a moveable spiral process in front, seen in two of the animals.

b. The heart of one of them.

c. Vent of the same, with other viscera imperfectly seen.

d. Appearance of a spiracle when the ciliæ are closed.

Fig. 2. *Flustra pilosa*, encrusting a fucus, and single animals of the same, seen through the shell and coat (page 384.). *a*. The shell.

b. Valve in the coat, through which the ciliated arms *c*, and the mouth are protruded. *b′*. The short sheath.

c 2. The arms when drawn in, with the neck *d* folded back.

e. Pouch (containing the stomach, liver, &c.?).

f. Place of gyration of particles in the intestine. *g*. Rectum.

h. Ligaments or muscles between the animal and the base of the cell.

Fig. 3. *Flustra papyracea* (page 384.). The animals folded in their cells. The letters in this and the following figures refer to the same parts as in the last.

Fig. 4. *Anguinaria anguina* (page 385.).

Fig. 5. *Tibiana* (page 385.).

Fig. 6. A zoophyte described page 385.

XIX. *On the Nervous System of the* Sphinx ligustri, Linn., (Part II.) *during the latter stages of its Pupa and its Imago state; and on the Means by which its Development is effected. By* George Newport, *Esq. Communicated by* P. M. Roget, *M.D. Sec. R.S.*

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In a former paper * I have described the anatomy of the nervous system of the *Sphinx ligustri,* Linn., and the changes it undergoes during the larva and the earlier stages of the pupa states. In the paper which I now have the honour of laying before the Society, these changes will be followed through the remaining stages, until the insect has arrived at its full development, and I shall endeavour to show the manner in which they are effected.

I. 1. *Of the Pupa.*

We have seen that the nervous system of this insect, during the larva state, is composed of two cerebral ganglia which lie above the œsophagus and dorsal vessel, and eleven ganglia, connected by intervening cords, disposed along the median line of the body, below the œsophagus and alimentary canal. These ganglia and cords undergo considerable changes, both in number, situation, and form, when the insect has entered its pupa state of existence. After these changes have been carried to a certain extent, they appear to be suspended for several weeks, during which the insect remains in a state of hybernation. At the expiration of that period the changes again proceed, and are continued uninterruptedly until the insect has arrived at the perfect state.

In the month of March, when the pupa is becoming more active, all the ganglia of the body are very distinct, and the optic nerves, which proceed from the supra-œsophageal ganglia, and which are soon to equal them in size, are beginning to be enlarged at their base. [Plate XIII. figg. 1 and 2.] The ganglia of the head and thorax have undergone the most alteration. If the nervous system be closely examined at this period, it will be seen that these ganglia and nerves give evidence of still further change. The nerves which supply the wings, and which, up to this period, are each formed by two roots,—one derived from the cord, and one from the ganglion attached to it, as shown in the larva state in my former paper,—are increasing in size, particularly at their base [fig. 2. b.]; while the anterior pair of nerves from the second ganglion, which unite with the second pair from the same ganglion, now originate from the cords, preparatory to the subsequent change in situation of the ganglion itself.

* Philosophical Transactions, 1832, p. 383.

By the middle of April, great progress has been made in the changes which are taking place in all parts of the pupa. The respiratory organs have undergone much alteration; the tracheæ which ramify among the muscles of the thorax being extended in calibre, while some of those from the spiracles along the sides of the abdomen have been gradually developed into pulmonary sacs, which are now of considerable size. Of these there are four upon each side of the body, the anterior ones being much the largest, while the tracheæ in the succeeding segments are also enlarging. The dorsal vessel [Plate XIV. figg. 11. 12. and 13. (*a*, *a*, *a*)] has become a much firmer structure; its valves (*b*), the muscles attached to it, and the many vessels which enter it laterally, and carry the circulatory fluid, are more distinct, and its division into several arterial trunks at its termination, anterior to the cerebral ganglia, [fig. 12. (*f*),] is now more easily traced. The muscles of the thorax are also more developed, and certain processes which at first are soft and delicate, and which during the previous months have been in the progress of formation, have now become hard and of a dark colour, like the exterior of the pupa-case. Four of these processes exist along the under surface of the thorax [Plate XIV. fig. 1. (*t*, *u*, *v*, *x*)]. They are developed, as we shall hereafter see, from the duplicatures, or folds, into which the integuments of the thoracic segments are thrown at the period of assuming the pupa state. The anterior one forms the division between the head and collar; the second, [fig. 1. (*v*),] between the collar and thorax, and is posterior to the first pair of legs; the third traverses the thorax, [fig. 1. (*u*),] and serves as an attachment for some of the principal muscles; and the fourth, [fig. 1. (*t*),] or posterior one, is that which in future will constitute the division between the thorax and abdomen, and even at this period of development, before there has been much deposition of earthy matter to form the covering of the future insect, it is continued around the whole segment, and is of firm but transparent texture. By means of these processes we may clearly indicate the situation of the thoracic ganglia at this stage of development. The second subœsophageal ganglion lies between the first two processes, immediately anterior to the first pair of legs. The third ganglion is opposite to the second pair of legs, and anterior to the third process. The fourth and fifth ganglia lie between the inferior pair of wings (*i*) and third pair of legs (*k*), and constitute one mass, which is situated just behind the third process. The sixth ganglion (*m*) is separated from the fifth only by a short extent of cord, is very much decreased in size, and is altered from a circular to an oval form. It is situated upon or immediately behind the division between the thorax and abdomen. The nerves from this ganglion are still disposed irregularly, as at the period of transformation, and the longitudinal cords, which are continued from it into the abdomen, have the ganglia situated as in the previous stages of the pupa.

By the second week in May the future exterior of the perfect insect begins to be formed beneath the common covering of the pupa-case, and in each segment of the abdomen, along the upper surface, on both sides of the dorsal vessel, there is a little deposit of gelatinous pink-coloured matter, precisely in the situation of the red bands which encircle the body of the perfect insect; but no distinct traces of organization, in the form of scales, can be detected in it, nor have the black bands as yet begun to make their appearance. The nervous system is now about to undergo its final change. There is an evident alteration in the appearance of the ganglia, although they retain the same situation, both in the thorax and abdomen, as in the month of April. The second ganglion is much decreased in size, has become of an oval form, and is not very distinct from the cords themselves. The third, fourth, and fifth ganglia are approaching nearer together, and are tending to form the two portions of the large thoracic mass which exists in the perfect insect, [Plate XIII. fig. 6.] and from which the nerves to the legs and wings are distributed. But the sixth ganglion still lies upon the division between the thorax and abdomen, and its nerves are still disposed in an irregular manner, in consequence of the change that has taken place in the direction of the muscles of the segment to which they are distributed. The other ganglia remain in the same segments of the abdomen as in the previous stages of development. The transverse series of nerves [fig. 1, 2. (*e*, *h*, *o*, *o*), fig. 6. (*o*, *p*)] have a little shifted their position, and instead of remaining, as in the larva, almost closely attached to the anterior part of the abdominal ganglia, they have moved forwards, and lie nearly equally distant between them; have become more uniform in size through their whole length; and have lost the ganglionic appearance they exhibited during the earlier stages of the pupa state. The cerebral ganglia continue very distinct from each other, while the optic nerves, which proceed from them laterally, are extending in every direction, and are nearly as large as the ganglia from which they are developed. The enlargement has taken place chiefly upon the anterior surface, outwards, forwards, and downwards; but these parts of the nervous system are still very far from being completed. The patch of gelatinous dark-coloured substance which is seen upon the base of the optic nerves, close to the cerebral ganglia, immediately after the Sphinx has entered its pupa state, although up to the present period it has not been increased or extended, has now assumed a distinctly organized appearance, its outlines being clearly defined. Its outer margin is smooth, and continuous with an exceedingly delicate transparent membrane covering the whole surface of the nerve. The interior margin is more distinct, and is corrugated and folded upon itself so as to resemble a partially closed sphincter. It is now removed a little further from the cerebral ganglia, preparatory to its subsequent expansion over the extremity of the visual organ, of which it seems destined to constitute the choroid. It exhibits exactly the same appearance during the development of *Papilio Urticæ*, LINN., [Plate XV. fig. 31. (*c*)] in which I have watched it even more attentively than in the Sphinx.

It is at this period of development that we are enabled to trace with the greatest precision the distribution of the vagus nerve [Plate XIII. fig. 3. (E. *e*)] to the œsophagus (*f*, *k*) and dorsal vessel (*h*, *h*), and its connexions with the anterior lateral ganglia (*g*, C.); and the connexions of these with the cerebral ganglia (*b*, A.), the antennæ (D. *a*), the manducatory nerves (*d*), and the transverse or involuntary series (*c*). At a later period this becomes almost impracticable, owing to the completion of structures among which the nerves of connexion are distributed. Indeed, after this period it becomes more difficult to trace any of the nerves in the anterior part of the body, the whole insect being rapidly approaching to its perfect condition.

By the first week in June the processes which form the division between the thorax and abdomen, and the attachment for the great muscles of the body, are completed. The muscles themselves have acquired a consistency and strength which they have not before possessed; the exterior of the perfect insect is nearly completed beneath the pupa-case; the pink-coloured deposit is extended in the form of little scales over the upper and anterior part of each abdominal segment, and a similar deposit in the form of a black band, but with fewer traces of scales, exists also upon the posterior part of each segment; while the whole under surface of the body is covered with a thick, semi-opake, greyish-coloured fluid, in which traces of minute scales are very evident. The antennæ, the legs, and the wings, folded in their envelopes beneath the thorax and first segments of the abdomen, are still exceedingly delicate and vascular, and are covered also with scales. The nervous system has now arrived at the last stage of development. During the period between this and the previous stage the optic nerve has been greatly enlarged and extended, and the dark-coloured patch has been expanded over its extremity, and seems now to constitute the choroid membrane. But the eye is not yet completed, although the optic nerve seems very nearly to have arrived at its maximum of development. The exterior portion of the organ, next the pupa-case, the cornea, is still of a transparent gelatinous substance, and the lenses appear to be the last parts of the eye which are completed. The cerebral ganglia are now extended transversely, and form, with the first subœsophageal ganglion, and the enlarged crura which connect them, one continuous mass around the œsophagus and anterior part of the dorsal vessel [Plate XIII. fig. 6. A.]. The second ganglion has entirely shifted its position, and receded towards the middle of the thorax, and has coalesced with the third, which has entirely disappeared, and seems to have joined in part with both the second and fourth, and the intervening cords. This aggregation of ganglia and cords [Plate XIII. and XIV. figg. 6. and 8, (2, 3, 4, 5.)] is situated in the middle of the thorax, and supplies all the muscles in that part of the body. The longitudinal cords are continued from the hinder part of the fifth ganglion, and just before leaving the thorax to enter the abdomen they give off the nerves which formerly belonged to the sixth ganglion [fig. 6. (6)], which is now entirely obliterated. The cords then descend into the abdomen, and immediately give off the nerves that belonged to the seventh ganglion, [fig. 6, 7.], which, with part of the cord that existed between the sixth and seventh ganglia, is also obliterated. The cords are then continued in a direct line along the abdomen, the eighth, ninth, tenth, and eleventh ganglia being situated as in the previous stages.

Such is the state of the nervous system at the period antecedent to the development of the perfect insect, which usually takes place about the middle or latter end of June. The time which the *Sphinx ligustri* remains in the pupa state is thus shown to be at least forty-two or forty-three weeks, as nearly as we are able to ascertain, since owing to the eggs not being all deposited in the same week or month, and consequently the larva not produced at exactly the same time, some broods are two, three, or even four weeks later than others.

A few days before the insect is ready to burst from the pupa-case, it becomes exceedingly restless and active, and writhes and turns in its cell repeatedly. That cell—the little hermitage it had constructed in the earth with much assiduity in the preceding autumn, by moistening the soil that was to form the inclosing walls with fluid from its silk-bags, and smoothing and moulding it into shape by rolling and turning its body while the material was yet in a moist condition, and afterwards lining the whole interior with a tissue of silky hangings—is no longer necessary to protect the feeble and delicate pupa from the intrusion of enemies. The insect is now vigorous, and its hard coriaceous covering being scarcely susceptible of injury, it makes a powerful effort to force the walls of its cell, and emerge from its subterranean abode. It gradually works its way upwards to the surface of the ground by repeated contortions of its abdominal segments, assisted by the pointed extension of the twelfth segment of the pupa-case, which serves as a lever against which the power of the insect is exerted. The depth in the ground at which the cell is situated is generally from six to eight inches, very rarely more, so that the insect has not far to travel. But it does not all at once arrive at the surface; its progress is slow and gradual until within a short time before it is ready to burst forth, which is generally in the early part of the day.

During the three days preceding its actual appearance, an alteration, which has for some time been taking place in the exterior of the pupa-case, becomes very evident. The coverings of the eyes, of the antennæ, the legs, and the wings are more convex and prominent, particularly of the latter, which extend on each side the thorax; and the union of the sutures of the approximated coverings of the limbs appear as if about to separate. A few hours previously to the liberation of the insect the coverings of the wings lose their solidity, and upon slight pressure are elastic and yielding, like dried membrane. This is also the case with other parts of the body, but in a much less degree; while the abdominal segments are elongated beyond their original extent. This occurs from the abdominal portion being the first part of the insect that is entirely freed from its attachment within the pupa-case. After this the thoracic portion of the pupa-case becomes fissured along its dorsal line, as well as transversely, behind the head and second segment, and the new-born insect gradually pressing itself through the opening, and carefully withdrawing its limbs from their respective coverings, comes forth with its wings rumpled and small, as if atrophied, but like its whole body completely covered with scales. The insect immediately seeks a shady situation, where it may suspend itself perpendicularly at rest against a wall or the stem of a tree, and remain unmolested during their complete expansion. This occupies but a short period: in the Butterfly it seldom exceeds more than a few minutes; but in the Sphinx, whose wings are larger and stronger, it is not completed, and the wings fit for flight, in less than two or three hours.

2. *Of the perfect Insect.*—The nervous system of the Sphinx in its perfect condition offers many interesting points for consideration, although it differs but little in its general arrangement from the last stage of the pupa. Thus, there is no further alteration in the cerebral ganglia, nor in those which constitute the thoracic mass, from which nerves to the organs of locomotion are distributed; but the whole are covered in by a new structure, and do not lie, as in the larva, in the open cavity of the thorax. The cords and ganglia are now inclosed on each side between the muscles, from which they are separated by a semi-opake membrane of a fibrous texture. This extends over their upper surface, and protects them from the œsophagus, which passes along above them. Before the last change in the pupa, in the beginning of June, we can readily trace the distribution of nerves and obliteration of ganglia, but after this period there is considerable difficulty. A very remarkable change, the obliteration of a ganglion in the thorax, occurs just before this period; but it is so rapidly effected, that I have never yet been able to observe it at the actual moment of its occurrence, and hence am unable to state, from positive observation, whether it be the second or third ganglion that disappears. A similar change occurs in *Papilio Urticæ*, Linn., and in this insect it takes place between the forty-eighth and fifty-eighth hour after entering the pupa state; but I have not been more successful in detecting it even in this instance. At first we might suppose it to be the second ganglion that disappears, as we have seen that a change is taking place in it. But this, I believe, is not the case, because the nerves to the two pairs of wings, which originally were each formed by two roots, the first pair [Plate XIII. fig. 1. (*f*)] by a root from the cord between the second and third ganglion, and another from the third ganglion itself, and the second [fig. 1. (*i*)] pair by one root from the fourth ganglion, and one from the cord between the fourth and the third, become now united. Now were it the second ganglion that becomes obliterated, the origin of the nerves to the first pair of wings would necessarily be anterior to the whole thoracic mass. But instead of there being the third ganglion and cord between the origins of these nerves, as in the larva state, they are now united into two roots, and arise from those portions of the thoracic mass which pass on each side of the central attachment for the muscles [Plates XIII. and XIV. figg. 6. and 8. (*w*)] of the thorax. Each root passes diagonally outwards, the first in a direction backwards [fig. 8. (*a*)], the second in like manner forwards [fig. 8. (*b*)], until meeting each other they unite and form a small plexus [fig. 8. (*c*)], and then again dividing are distributed, the one [fig. 6. (*a*)] to the anterior, the other [fig. 6. (*d*)] to the posterior pair of wings. The reason for this curious union and complexity in the distribution of these nerves to the wings, is not at first very evident, but a little reflection

will show us that it is regulated by evident design, and is one of those beautiful provisions in the animal economy by which harmony in the functions of every part of the body is preserved. The wings, the organs of the most varied and rapid motion, endowed with an equal degree of sensibility and power, are required at every effort of the insect to act in the most perfect unison, and hence must be supplied with their energy from the same centre. That this is the reason for the union of these nerves, is, I think, apparent, from the fact that in the Bee, the Ichneumon, and other hymenopterous insects remarkable for their velocity and power of flight, the nerves to the wings originate almost precisely in the same manner as in the Sphinx and its affinities; while in others, as in the *Panorpa communis*, LINN., or Scorpion Fly, they originate by double-rooted pairs, just as in the larva of the Sphinx; and the insect is neither remarkable for its velocity nor equability of motion. And it may be further stated, that in winged coleopterous insects, in which the wing-covers are merely elevated, and are motionless during flight, the wings alone being actively employed, the nerves to the two organs are not always united, but often originate separately from the great nervous centres, and are continued to their distribution as separate trunks, like the nerves to the legs or the antennæ.

The cords in the abdomen of the Sphinx in its perfect state, like those of the thorax, are covered in by a curious structure, of the exact nature of which it is difficult to form a conclusion. It is spread over the whole like a broad riband [Plate XIV. fig. 9. (*a*)], from their commencement in the first, to their termination in the antepenultimate segment, and seems to bind down and protect the cords and ganglia in their course along the abdomen, whatever other office it may be thought to perform. The ganglia, and the nerves distributed from them, scarcely differ from those of the pupa, excepting only the two anterior pairs from the terminal ganglion. These, in the female Sphinx, are very much elongated, and are enfolded around the ovarial tubes and organs of generation, among which they are distributed. With a little care they can be easily separated from them. The terminal pair of nerves, as in other insects, is distributed conjointly to the rectum and organs of generation.

Besides the nerves and ganglia which constitute the symmetrical parts of the system, there are others, including those of the head and mouth, that require more particular notice. They are arranged in two classes: 1. *Nerves of the senses*; 2. *Nerves of involuntary functions.*

II. 1. *Nerves of the Senses.*

a. Nerves of the Antennæ.—These, in the Sphinx and other *Lepidoptera*, originate each by a single root from the anterior part of the cerebral ganglia, close to the base of the optic nerves. After entering the base of the antennæ they give off a considerable number of branches; but the real nature of the organs themselves is yet undetermined. It is evident that they are endowed with the sense of touch, and are used by many insects, Grasshoppers, Beetles, &c., as cerebral feelers. The structure of the antennæ in the Sphinx, in which the under surface of each joint is encircled by a double ring of exceedingly delicate elastic cilia [Plate XIV. figg. 17, 18. (*a*)], seems more fitted for *feeling*, or perceiving the vibrations of the atmosphere, and thereby for performing a function analogous to that of hearing, as has been suggested by Bonsdorf, Camparetti, and other naturalists, than for any other with which we are acquainted. The sense of touch is evidently the primary endowment of the antennæ in articulated animals, as seen in the Myriapods; but this cannot be their use in many insects, *Libellulæ, Diptera*, &c., in which they are short and immoveable, nor in those *Coleoptera* in which they are terminated by lamellæ; while their structure in almost every class is totally incompatible with the function of smelling. But there is no class in which their structure could incapacitate them for feeling the pulsations of the atmosphere, and thereby performing a function analogous to that of hearing.

b. Nerves of Vision.—The compound eyes of insects are parts of great interest, but of difficult investigation. Professor Muller, Straus-Durckheim, and others have carefully examined them, but there is still a difference of opinion respecting their real structure. I have not yet sufficiently examined them to offer an opinion, my attention having been confined chiefly to the development of the optic nerves themselves, during the transformations of the insect. This can only be shown in those insects which have simple sessile eyes in their larva state, and numerous compound ones in their perfect, as in *Lepidoptera, Hymenoptera*, and some other genera. In the larva of the Sphinx the optic nerves are only two diminutive trunks, extending from the sides of the cerebral ganglia, and dividing each into eight filaments, given to the eight minute eyes on each side of the head. At the period of changing to the pupa state there is a deposit of dark pigment, very slightly organized, at the base of each nerve. As the changes of the insect advance, the optic nerves gradually enlarge at their base; and when this enlargement has gone on to a considerable extent, the dark pigment is carried forwards from the base of the nerves, and exhibits a corrugated appearance around its interior margin. When the changes have further advanced, the optic nerves are extended, of a pear-like form, from the sides of the cerebral ganglia, which they then equal in diameter. The enlargement of the nerves seems to be occasioned by the shortening of the cords which connect the cerebral with the subœsophageal ganglia, and the extension forwards of the nervous substance of the cords within the investing theca, the effect of which is not to enlarge the cerebral ganglia in a corresponding degree, but to develop the optic nerves, by the gradual extension and expansion of the nervous substance within them, in the form of successive series of purse-like layers of fibres [Plate XV. fig. 31. B.] one within the other. When the outer layer has arrived at its maximum of extension, it seems to become perforated at a point corresponding to the central part of the membrane, which is carried forward to become the choroid [fig. 31. (*c*).]. The next layer advances, and then the next in succession from within outwards, so that the central portion of the nerve is the last part developed. The fibres of each series, from being bent like the segment of an arc, gradually assume a more lineal direction, and diverging from the axis of the eye, the whole nerve, when completed, forms a series of flattened pear-shaped cones, one within the other, the apices of which constitute the base or origin of the nerve next the cerebral ganglia. The eye of the Sphinx, when perfect, being convex, the outer or first completed series of fibres is the shortest, while each succeeding series is longer and less earlier perfected in proportion to its distance from the circumference, so that the central fibres are the longest and last developed. Thus the same law which regulates the development of the osseous structure, as shown by DUTROCHET in the vertebræ of the frog, regulates that of the ganglia and nerves. The common covering of the optic nerve is formed of an extension of the theca which covers the cerebral and subœsophageal ganglia and nerves, and through which the ramifications of tracheal vessels penetrate in considerable abundance. In the optic nerve, in particular, they are very numerous; and I have never yet been able to detect their terminations, or to discover any other description of vessels in the nerves or ganglia, although there is scarcely a doubt that others do really exist.

It is difficult to observe the radii of the optic nerve in the Sphinx, owing to the size and opacity of the part; but the sacculi of nervous matter are beautifully seen in the nerve of the eye of *Papilio Urticæ*, LINN., at about forty-eight hours after changing to the pupa state. It is necessary to remove the nerve with the cerebral ganglia from the head of the insect, and view it with a good lens by intense transmitted sun-light [Plate XV. fig. 31.].

c. Nerves of the Mouth and its parts.—These originate from the first subœsophageal ganglion, and from the crura which unite it with the cerebral ganglia. In the larva they supply the mandibles, palpi, and pharyngeal region; and in the perfect insect the two halves of the flexible and delicate proboscis, the structure and muscles of which, in order to show the arrangement of its nerves, I must briefly notice. This organ in the perfect *Lepidoptera* has recently been described by Mr. NEWMAN* in his Letters on the External Anatomy of Insects, and is shown to be analogous to the maxillæ, or lesser jaws. It is situated, in the larva, beneath the strong mandibles, which in the perfect state are obsolete, and exist only as very minute parts on each side its connexion with the head. It is an elongated, tapering, flexible organ, composed of two symmetrical halves, placed laterally together, convex on their external, and concave on their internal surface [Plate XIV. fig. 15. *a*, *b*.], and by their approximation forming a tube to the mouth, which is nearly of the same size through its whole length, excepting at the tip, where it is a little smaller. Each half is slightly ciliated externally, and along the whole anterior margin of its concave surface is furnished with a row of minute hooks, and near the tip, along its anterior and external surface, with a number of little elongated papillæ, which, probably, are organs of taste. In a state of rest, the proboscis is rolled up spirally between the labial palpi;

* Entomological Magazine, Part VI., January 1834.

and at its base, in some *Lepidoptera,* there are small maxillary palpi. It is connected above with the triangular arch or palate, the epipharynx*, which forms the roof of the mouth, and below with the hypopharynx, or analogue of the tongue, which forms the floor of the mouth, and conducts to the œsophagus. The mouth is only a dilated cavity between the proboscis and commencement of the œsophagus [fig. 15. *c.*]. Each half of the proboscis is furnished with two kinds of muscles, longitudinal and transverse, acting as flexors and extensors. The transverse muscles consist of many short semicircular fibres, which encircle the exterior of the proboscis [fig. 16. (*c. c*),] and are attached along the margin of the inner or grooved surface of the organ, which by their contraction they tend to elongate. These muscles are exceedingly small and numerous, and amount to at least one thousand in each half of the organ. They are assisted in their action as extensors by one of the longitudinal muscles (*b*), which arises within the anterior of the cranium, and is attached by a multitude of fibres, inserted at very acute angles along the anterior margin of the groove. This muscle, in conjunction with the circular ones, acts as a powerful elongator of the proboscis at the instant of taking food. The other two longitudinal muscles are flexors (*a, a*). One of these, the direct antagonist of the first, arises from the under surface of the head, and is inserted along the inferior margin of the groove, and assists in rolling up the organ. The other, a more powerful flexor than the last, is the largest of the three longitudinal muscles. It arises from the lateral and under surface of the head, and is attached to the inner surface of the exterior, or most convex part of the organ, by many long fibres inserted at very acute angles into a slight tendinous ridge, so as to compose one large penniform muscle [fig. 16. (*a*).]. Each half of the proboscis is also supplied with one large and one small tracheal vessel [Plate XV. fig. 19. (*f. h*)], derived from those of the head (*g*). These extend from one end of the organ to the other, giving off numerous branches, and gradually decrease in size, and distribute longer, and a greater number of branches the nearer they approach the end of the organ, so that, as in other parts of the body, they are lost in the surrounding structures. The nerves of the proboscis extend along the course of the tracheal vessels (*a, b, c, d, e*). In the larva we have seen that the nerves to the mandibles come from the anterior of the subœsophageal ganglion [Plate XV. fig. 34. (1.*f*).]; and the same is the case with the nerves of the proboscis in the perfect insect [Plate XIV. fig. 10. (*a*. 1).]. The nerves of this organ in the larva are of inferior size; in the perfect insect they are largely developed. They originate on each side the subœsophageal ganglion as single trunks, one to each half of the organ. Immediately the nerve has entered the hinge anterior to the mouth, it is divided into four branches. One of these (*b*) passes backwards, apparently to the palpus, and others forwards into the organ. The innermost branch (*c*) is small, and gives off very minute filaments. It passes in a direct line immediately beneath the grooved or mucous surface between it and the large trachea, and does

* Newman.

not appear to give filaments to the muscles; while the main branch (*d*) of the nerve passes exterior to the tracheal vessels, and seems to be given chiefly to the flexor muscles, which lie along the exterior of the organ. The course of the fourth nerve (*c*) I have not distinctly traced; it appears to run along the smaller flexor. Now there are two pairs of nerves which exist in the larva, and come from the crura, near the base of the cerebral ganglia, just below the origin of the pneumogastric, which I have been unable to discover in the perfect insect. I suspect, therefore, that these nerves, during the development of the insect, have united with the mandibular and maxillary to form the large trunk to the proboscis. This appears probable, as I shall presently show that coalescence of nerves actually does take place, and that the nerves belonging to the subœsophageal ganglion are forced upwards during the development, so as to appear as if coming from the lower part of the crura [Plate XIII. fig. 7. (1)] on each side the pharynx. If this be correct, a question arises, What are their functions? The larger branch given to the muscles, chiefly the flexors, is clearly analogous to the great mandibular nerve of *Vertebrata*; and it is not unreasonable to suppose that the small branch which passes along the groove of the proboscis, where an exquisite sensibility of taste is required, may be analogous to the gustatory, and in the larva be one of those nerves which are distributed around the mouth and palate. This opinion is further supported by the nerves originating in the larva just below the pneumogastric, and above the subœsophageal, ganglion, which ganglion, in the perfect insect, gives the nerve to the proboscis. Now this is in perfect accordance with, and beautifully illustrates, the philosophic views of Sir Charles Bell, who has shown that every portion of an organized being is supplied with an additional set of nerves for every additional function it is required to perform. In the larva, the mandibles are hard and powerful, requiring, probably, little more than simple sensation and motion. But in the perfect insect, the proboscis is delicate and flexible, and, so far as we are enabled to judge, highly susceptible of impressions, one of which, doubtless, is taste.

II. 2. *Nerves of Involuntary Function.*

a. The Vagus, or Pneumogastric.—This nerve, the *recurrent* of Lyonet, originates immediately above those nerves which seem to have united with the manducatory. It has been shown to arise by two roots, [Plate XIII. fig. 3. (*e*),] one from each crus. These, after passing forwards and uniting in a ganglion in the middle line above the palate, run backwards, as a single trunk, [*e*, *f*,] beneath the cerebral ganglia, the brain, between the dorsal vessel and œsophagus. Just at entering, and for a short distance within the thorax, it gives off filaments to the dorsal vessel [fig. 3. (*h*, *h*)], while the main trunk passes along the middle of the œsophagus, unto which it distributes filaments, until it arrives at the cardiac portion of the stomach, [fig. 3. (*i*),] where it gives a few filaments to the air-bag, or crop, and then divides into three branches, which run along the middle and sides of the stomach, and are again subdivided and distributed around it. The ganglion at the union of the roots of this nerve distributes a few filaments from its anterior surface, forwards upon the palate, and apparently also to the extremity of the dorsal vessel, which, after passing along the œsophagus beneath the brain, here divides into several trunks. Two of these pass downwards, one on each side the œsophagus, to the proboscis, others outwards, and others upwards, to the eyes, antennæ, and front part of the head. Behind the brain, within the region of the head, the vagus is connected by a branch on each side with the anterior lateral ganglia (*g*), which are also connected with the superadded or transverse series (*c*). The constancy of its existence, and the situation and distribution of the vagus, in insects, are points of deep interest for consideration. I have never found it in any other situation than that which it occupies in the *Sphinx ligustri*. These are positive indications of the analogy it bears to the great pneumogastric nerve in *Vertebrata* *. It is clear that it ministers to a very important function, the involuntary motions of the stomach and alimentary canal, which are as distinct and as constant as in the *Vertebrata*. Yet we find an exceedingly large ganglion at its origin (E); and the remains of this ganglion may be traced upwards through fishes, reptiles, and mammalia, to man himself, in whom we have it remaining only as a slight enlargement. What, then, is the office of this ganglion? Does it communicate sensation to the parts, or is the ganglion merely a great centre of nervous energy, ministering to the involuntary functions of the alimentary canal, the place of which in the higher animals is probably supplied by a more perfect development of the sympathetic system? The interest of this consideration is increased, from the circumstance, that even while the insect is in some of its earlier stages, before there is a complete approximation of the lateral cords and ganglia of the body, and even previous to the development of the organs of locomotion, as in the Bee, the ganglion of the vagus is nearly as perfect in form as when the insect has passed through all its changes. In the maggot or larva of the Wild Bee, (*Anthophora retusa*, KIRBY,) where the whole of the nervous system is exceedingly transparent, the ganglion is as complete as in the more organized and active caterpillars, and the same is the case in the larva of *Chrysomela tenebricosa*, LINN., and other species. In the latter insect it distributes several branches posteriorly to the sides of the œsophagus and pharynx, [Plate XIII. fig. 4. (*c*, *c*),] besides the nervous trunk (*d*), which passes backwards to the stomach, and which in its course becomes somewhat enlarged (*b*). In the perfect insect of the same species [fig. 5.], its form and situation are the same. In the same insect we have also remaining the nerves of taste (*e*, *e*). The general figure of the ganglion of the vagus is heart-shaped, or triangular, with the apex directed backwards; but in the ground beetle, *Carabus*, LINN., it is elongated oval, lying transversely above the palate. It is interesting to remark that the vagus always originates from the crura, immediately below the cerebral ganglia or brain; and even in *Crustacea* we find it still arising from the crura [Plate XVII. fig. 40. (*d*, *d*)]. Now from this uniformity of origin, its possessing a ganglion, and its distribution to an organ endowed

* Philosophical Transactions, 1832, p. 386.

with involuntary motion, there is reason to believe that it is a compound structure, and partakes both of the motor and sensitive principle, but of an involuntary nature, and not therefore belonging to the symmetrical system.

b. Anterior lateral Ganglia.—The size of these ganglia, relatively to that of the cerebral ganglia, is very considerable [Plate XIII. fig. 3. (*a*)]; and hence, doubtless, Straus-Durckheim was induced to call them "accessories of the brain." In the larva and pupa of the Sphinx, they are situated behind the brain, one on each side of the upper part of the œsophagus, anterior to a pair of large constrictor muscles [Plate XIV. fig. 14. (*h*)], which are attached to the lateral posterior part of the head. Their connexions are remarkable: they occupy an intermediate situation between all the different nerves in this part of the body. A large nerve on each side the head connects the ganglia with the brain, and a small branch which passes transversely connects this nerve with the pneumogastric. Another nerve passes direct from the ganglion, and connects it with the transverse or superadded series. Other small filaments pass outwards laterally from the ganglion to the surrounding structures; and, lastly, there is a nerve which runs forward from the ganglion beneath the optic lobes, and forms connexions with the nerves to the antennæ and proboscis. Here, then, we have a series of connexions which seem to indicate the real nature of the ganglia, and their analogy with the superior cervical ganglia of the sympathetic system in vertebrated animals. Indeed, it is in these highly organized *Invertebrata* that we might expect to find a distinct sympathetic system, seeing that as we ascend in the scale of creation, from the *Polypifera*, or half-vegetative beings, to the most perfect animals, in proportion to the number, variety and importance of the functions to be performed, the number, extent, and complexity of structures are increased, and are more dependent upon each other, and every part of the body is less and less capable of maintaining for itself a separate existence.

c. The Transverse, Superadded, or Respiratory Nerves.—These nerves have for a long time engaged the attention of naturalists, and have been delineated by Lyonet, Heroldt, and others; but their true function has never been established *. There is a point of interest attaching itself to these nerves greater than to any others in the whole system of the insect. Hitherto there has been no distinct analogy shown between the nervous system of the vertebrated and that of the invertebrated classes in the possession of two series of nerves, the one for motion and the other for sensation; and it has been imagined by some that these transverse nerves may perhaps be analogous to the motor, while the longitudinal cords and ganglia are analogous to the gangliated sensitive system. Others believe the transverse nerves of insects are analogous to the true visceral or sympathetic. Perhaps I may be excused, therefore, for entering somewhat at length upon their distribution and structure.

* I have called these nerves *transverse*, from the direction of their principal branches; *superadded*, from their being nerves given to muscles, in addition to nerves from the moto-sensitive or spinal cords; and *respiratory*, from their distribution being chiefly to muscles which appear to be most concerned in respiration.

In the larva and pupa of the Sphinx, we have seen that these transverse nerves divide and distribute their branches anterior to every ganglion. In the thoracic segments, some of their branches [Plate XIII. fig. 2. (*e, h*)] unite with nerves which are already formed by two roots (*f, i*), one from the cords and the other from a ganglion, and which are destined for the future wings. In the abdomen, after giving some very small filaments to the nerves from the gangliated cords, they are distributed to the muscles of the segments, in addition to the nerves derived from the gangliated cords, and which, there is reason to believe, are compound nerves, and communicate both sensation and motion. I am therefore inclined to regard the transverse as superadded nerves, analogous to the respiratory nerves of the higher animals. In my former paper upon the Sphinx in its larva state, these nerves were believed to be arranged in distinct series, originating separately from the posterior part of each ganglion. Subsequent examinations have convinced me that the whole form one continuous system [Plate XVI. fig. 35. (*c*)], and do not originate separately by single tracts from the ganglia, but, as suspected and suggested to me by Professor GRANT, pass over the ganglia (*h*), and are continued along the median line between the cords (*k*) until they divide (*c, c*), to be distributed to the tracheæ and muscles. They are formed of three series of fibres, two of which are closely approximated, so as to look like a single tract [Plate XVI. fig. 35. (*k*)]. This comes down between the cords until it arrives just before a ganglion (*h*), where it divides nearly at right angles, and unites with the third series, which runs transversely across the body of the insect. A filament from each division (*h, h*) passes over the outer margin of the upper surface of the ganglion; then, converging again to the middle line, meets with its fellow from the opposite side: and these two filaments unite, and form one tract, after each filament has received a few fibres (*i, i*) from the upper or motor surface of the cords. The fibres thus united pass along the groove formed between the cords until they arrive at the next ganglion, where they divide, and distribute again as before. Each transverse series, besides the filament which passes over the ganglion, gives also a filament to the great or moto-sensitive nerve (*g, f*), which comes from the gangliated cords, and is distributed to the different parts of the segment. The terminal pair of nerves of this series is always distributed to the rectum, near its termination, in addition to the last pair of compound nerves from the last great ganglion.

I have found these nerves taking nearly the same origin and course in the abdomen of the large green Grasshopper (*Gryllus viridissimus*, LINN.). After the united filaments in this insect have passed along the median line, or groove, between the cords, [Plate XVI. fig. 39.] and arrived above a ganglion, they gradually diverge at an acute angle, and not abruptly as in the larva of the Sphinx. Each division gives a filament to a small nerve, which runs to the diagonal muscles of the segment (*c*), and which does not originate from the ganglion, but from the upper surface of the cord, or motor tract, which is passing over it, and is probably a motor nerve. The filaments then converge, as in the Sphinx; and, in passing along the cords, gather a few filaments from the motor tract of each, and after uniting in the middle line pass backwards to the next ganglion, to be distributed as before. In this species it is curious that the superadded nerves do not seem to unite with the great moto-sensitive nerve, but only with the small nerve behind it, which is given to muscles that, acting diagonally, seem to be much concerned in the function of respiration. But there are facts which might at first incline us to believe that these transverse nerves constitute the visceral or true sympathetic system in insects. Thus, their union with most of the nerves of the body; their connexion with the anterior lateral ganglia; the manner in which they receive additional filaments from the cords; and the existence of a ganglion upon the terminal filament in the *Gryllus viridissimus*, Linn. [Plate XVI. fig. 39. (*c*, *b*),]; and, above all, the existence of clearly defined ganglia at each distribution in the *Carabi* [fig. 38. (*c*),], or ground Beetles, and in the Mole Cricket, *Gryllotalpa*, in which the ganglia are very distinct, and situated above the great ganglions of the cords. On the other hand, it is only in a few genera of insects that these ganglia exist; and it has not yet been proved that respiratory nerves must necessarily be without ganglia. Indeed, it is not improbable that we may hereafter find a much closer connexion between the respiratory and sympathetic systems in the higher animals than has hitherto been imagined. Now the existence of ganglia upon these nerves, although in but a few genera, seems very decidedly to prove that they are not analogous to the simple motor nerves of the body, while their distribution being almost entirely to muscle, and but sparingly to the viscera, seems as clearly to show that they are not analogous to the sympathetic or visceral nerves of *Vertebrata*.

To prove more directly that these nerves are not simply those of motion, but are for the involuntary function of respiration, we must examine the means by which respiration in insects is performed. Nine pairs of spiracles, or breathing orifices, are placed in the larva along the sides of the body. Eight of these are in regular succession. They all communicate with longitudinal tracheæ, from which several large ramifying branches pass off transversely, nearly opposite to each spiracle. The longitudinal tracheæ extend from one end of the body to the other, and communicate freely with the spiracles. A similar arrangement of the tracheæ and spiracles exists in the perfect insect, but with this difference,—in the anterior part of the abdominal region, those parts which in the larva are ramifying tracheal tubes, are now altered in structure, some of them being developed into pulmonary sacs or bags, while in the thorax the tracheæ themselves are larger than those in the abdomen, and the spiracles are larger, and of a different form. In the thorax, and first pair in the abdomen, they are either elongated, semioval, or straight. The remainder of the abdominal ones are oval. The spiracles are acted upon by two sets of muscles, the one diagonal, and the other oval. The latter act the part of sphincters, the former are connected with the muscles of the segment. Reaumur, Bonnet, and others have clearly proved that the anterior pairs of spiracles in the larva (those of the collar and thorax in the perfect state) are of the greatest importance to the insect; since if these be closed, the insect soon becomes axphyxiated when placed under water; but if the anterior spiracles be left exposed while the remaining ones are submerged, it will live, and remain active for a great length of time. Their importance is also shown in their form and size in the perfect insect. It is in the anterior, this very part of the body, that we find the greatest abundance and complexity of respiratory nerves, even in the larva, where there scarcely seems more occasion for a greater number of respiratory nerves than in the abdominal segments. It is around the spiracles in this part that the respiratory nerves divide, and pass to many muscles which are associated with those of the spiracles in the function of respiration in the perfect insect; and these are muscles which are to act upon the future wings. Now the action of these very muscles, which elevate and expand the wings at the instant of flight, is an act of inspiration, during which the spiracles are opened, and the air, rushing into them, fills the air-bags and tracheæ over the whole body, just the same as in birds, as remarked by Professor Grant, the muscles of the chest and wings are associated simultaneously in action with those of the glottis and tracheæ, and exactly the same as the arms and muscles of the chest in man, and the anterior extremities and muscles of the chest in quadrupeds, are influenced at the instant of making any sudden or great exertion.

It is also remarkable that these nerves in general appear to be developed in size in proportion to the quantity of respiration of the insect. Thus, in the larva of the Blood Beetle, *Chrysomela tenebricosa*, Linn., and in the *Carabi*, they are exceedingly small; while in the Sphinx and other insects that are capable of powerful and long-continued flight, respire large quantities of atmospheric air, and have the organs of respiration exceedingly large, they have arrived at their maximum of development.

During the development of the Sphinx, the respiratory nerves undergo a curious change of situation, which certainly indicates that they are not simply nerves of motion, but are for an especial function. In the larva they are situated very close to the anterior part of the ganglia, but in the perfect insect they have moved forwards very nearly half way between the ganglia. Now it is well known that during development there is a tendency in nerves to approach and unite with each other, the lateral cords and ganglia are more closely approximated, and the ganglia in the anterior part of the body approach and coalesce into one mass. But instead of all the transverse nerves uniting with the nerves from the ganglia, which, had they been simple nerves of motion, we should expect they would have done, we find them in the abdomen, carried forwards in the segments, and distributed separately to the same muscles as those from the ganglia.

Another striking fact indicative of their separate function is their being distributed largely, even in the larva, to the double rooted nerves for the future wings, and but slightly, so far as can be discovered, to the primary organs of locomotion, the legs. The nerves to the legs come directly from the gangliated cords, and communicate both the motor and sensitive influence; the legs themselves are but little concerned in the function of respiration, and consequently are but slightly supplied with nerves from the superadded series. But the nerves to the wings, being already formed of two roots, could hardly require an additional one, were it not for some especial purpose, and did not each root confer a distinct endowment.

In addition to all that has yet been stated respecting the superadded nerves, there is a curious fact relating to the terminal pair, which seems further to prove that they are not simply nerves of motion. These nerves in the Sphinx, and all other insects, and in *Crustacea*, [Plate XVII. fig. 40. (*r*)] are given, as before stated, to the rectum, in addition to the terminal pair from the last great ganglion of the cords, and end in the sphincter muscles. Now if these were simple motor nerves, we should expect that they would be approximated to the terminal pair, which come directly from the last great ganglion of the cords, instead of merely passing along parallel with them, and ending separately, although in the same structures, viz. the sphincter and levator muscles of the anus. But it may be said that this does not prove them to be other than simple motor nerves, or that the nerves from the ganglion communicate both sensation and motion. This objection is clearly answered, and the terminal nerves from the last great ganglion of the cords are shown to communicate motion as well as sensation, from their distribution in the male of the Wild Bee before noticed, *Anthophora retusa*, Kirby. This insect, which I have taken with its partner in coitu, has the male organ of generation terminating in a forcipated claw, which passes out beneath the anus by the same orifice. With this claw the male firmly seizes and attaches himself to the vagina of the female during the period of coition, which lasts only for a few seconds. The organ must therefore be endowed both with sensation and motion. Now the terminal pair of nerves from the ganglion, after passing backwards for some distance, divide into two branches, one of which ascends, and is given to the rectum, and levator, and sphincter muscles of the anus, which also receive in addition the terminal pair of nerves from the superadded series, while the other branch is entirely distributed to the male organ, and appears to be the only large nerve which is given to that part; so that the last pair of nerves from the terminal ganglion are directly proved to communicate both sensation and motion, and therefore must be of compound structure; while the last pair of the transverse series are as clearly shown to be superadded nerves.

II. 3. *Structure of the Cords, Nerves, and Ganglia.*

a. It has been admirably proved by Sir Charles Bell, in his series of experiments upon the nervous system in vertebrated animals, detailed in the papers submitted by him to the Royal Society, that, as regards the physical condition of the being, different parts of the spinal column are endowed with different properties, and minister to different functions—volition, sensation, and involuntary motion. The same train of reasoning which led that distinguished philosopher to the discovery of these facts in the higher animals, must long ago have taught us, That since the laws of nature are simple and uniform, the same principle exists through the whole series of animated beings;—that however altered in arrangement or appearance in different parts of the series, structures corresponding to those which are endowed with especial properties in Man, and his immediate affinities, exist in every organized creature having the powers of locomotion and sensation. Yet however certain this principle must have appeared to every reflecting mind, we have not until recently been able to distinguish in invertebrated animals the particular structures from one another, and to show their analogy with similar structures of the nervous system in the vertebrated. Some, therefore, have imagined that the gangliated cords of the *Invertebrata* are simple structures, communicating both sensation and motion. This, however, I shall endeavour to show is not the case.

It was during the early part of the summer of 1833 that I first had an opportunity of conversing with Sir Charles Bell respecting the nervous system of insects, when he suggested a closer examination of the cords than I had then made, to ascertain whether a double nervous column, one portion for sensation, and the other for motion, exists in the *Invertebrata*, as in the higher animals. He at the same time pointed out one of the *Crustacea*, the Lobster (*Astacus marinus*, Leach), as perhaps the most eligible for the inquiry. At that time I had no hopes of succeeding in demonstrating the parts by dissection, although I believed they really did exist. In the month of August, after many dissections and examinations of the animal in its recent state, I began first to hope for success; and in the beginning of September completed a preparation of the nervous system of the Lobster, which I still possess, that appeared to show the two motor and sensitive columns, and I immediately communicated the circumstance to my friend Dr. Marshall Hall. Early in October a second preparation was completed, which showed these columns far more distinctly than the first. Fearing the possibility of mistake, I showed the preparation to Dr. Hall, a few weeks afterwards to Professor Grant, and many others: it is now in the possession of Sir Charles Bell.

The nervous system of *Crustacea* has been examined by many anatomists, Edwards, Carus, Home, and others. In the Lobster it is formed upon the same general plan as that of insects. It consists of two longitudinal cords, corresponding to the two halves of the body, united at certain distances by ganglia [Plate XVII. fig. 40. (1 to 14.)]. These cords are double, each being composed of two tracts, lying one over the other [fig. 42. (*u*, *v*)], analogous to the motor and sensitive tracts in the spinal column of *Vertebrata*. These tracts, however, are not readily distinguished until after the cords have been kept for a short time in alcohol, when they become very evident even to the naked eye. The ganglia [figg. 40, 41, 42. (*u*, *v*)] are fourteen in number, one cerebral (A), and thirteen subœsophageal (C, D). Seven of these are thoracic (C), and the remainder are post-abdominal or caudal ganglia (D). They all belong entirely to the sensitive tract, which lies nearest to the under or exterior surface of the animal. The tracts are in close apposition until they arrive at a ganglion. The motor then becomes more distinct, and passes over the ganglion without uniting with it, and immediately afterwards is again closely approximated to the sensitive. A distinct line between the two tracts extends along the whole lateral surface of each cord, and is more or less evident in different parts of its course. It will thus be seen that the ganglia are situated almost entirely along the under surface of the cords, and it is from these that the sensitive* portion of the double or symmetrical nerves (*o, o, o, o, o,*) of the body take their origin. The manner in which the nerves from the motor tract unite with those from the ganglia of the sensitive in the Lobster, to form these symmetrical nerves, is not at first very apparent. Upon close examination it seems to be by fibres coming off laterally from the motor tract, just above the anterior margin of each ganglion, passing backwards and outwards, and immediately uniting with those from the ganglion into distinct trunks. The ganglia in the thorax are rounder, larger, and closer together than the caudal or post-abdominal ones, and give nerves to the true organs of motion, the legs; to the claws, mandibles, and feelers; to the glandular structures, and the circulatory vessels in the branchiæ and thorax. The caudal ganglia are of a much smaller size, and are of an oval shape. Each of these gives off two pairs of nerves (*o, o*), which again divide into two branches, and pass outwards close to the under surface of the body, supplying the large trunks of circulatory vessels which pass along the same course with them, and the external layer of muscles. The posterior division of the second pair from each ganglion is larger than the others, in consequence of its again dividing into two branches as soon as it reaches the lateral margin of the body. The largest of these branches (*p, p*) descends to supply the muscles of the false feet, the other ascends to those of the lateral surface of the segments. This is analogous to the means by which the false feet are supplied in the larva of *Sphinx ligustri*, and other Lepidopterous insects, in all of which they are supplied from the ganglia in the abdominal region, which are analogous to the post-abdominal of the Lobster. The terminal ganglion [Plate XVII. fig. 40. (14.)] is the largest, and gives off four pairs of large nerves, and, as in insects, was originally formed of two ganglia. The two terminal nerves (*s*) from this ganglion, which has coalesced longitudinally, pass on each side the rectum, and divide each into two branches. The terminal branch supplies, and is entirely lost, in the rectum and sphincter ani, and the other supplies the muscles which elevate and expand the anus

* While engaged upon the anatomy of the Lobster I obtained a large living specimen, which, although apparently vigorous and healthy, appeared to suffer but very little pain when pricked or pinched, and was of a much lighter colour than usual, its whole covering being quite blue, instead of the usual blackish purple. Upon killing the animal and examining its spinal cords, the motor columns and nerves were of the usual size and appearance, but all the ganglia of the sensitive columns, particularly those in the post-abdominal region, were exceedingly small, and each inclosed only a very small nodule of grey matter. May we not infer from this fact, that the degree of sensation in the nerves belonging to the spinal column very much depends upon the size of the ganglia and the quantity of grey matter they contain?

in the expulsion of fæces, and the middle lamella of the tail, in which the anus is situated. The remaining pairs of nerves (*w*) are given to the other lamellæ of the tail.

All the nerves I have now described in the Lobster belong to what Sir Charles Bell calls the regular or symmetrical, and come directly from ganglia; but there are others [fig. 40. (*q*, *q*, *q*)], which come directly from the upper surface of the cords, unconnected with those from ganglia. In the caudal region there are two sets of these posterior to each ganglion. They arise from the tracts by single trunks, each dividing into five or six branches, that ramify in every direction, and are given entirely to the muscles. Although at first sight they appear to form ganglionic enlargements (*q*, *q*) before dividing into branches, there are no ganglia upon them. This appearance is occasioned by the approximated fibres which constitute the trunk being spread out, instead of rounded like a cord. The two last of these nerves (*r*) originate singly from the tracts, and are given to the under surface of the rectum. In the thoracic region they come from the tracts [Plate XVII. figg. 40. and 42. (*l*, *l*)] immediately above the posterior part of the ganglia, and are given to the muscles of the branchiæ.

The detection of a double spinal column in the Lobster has since led me to examine more closely the nervous system of the Scorpion, one of the *Arachnida* (*Scorpio europæus*, Linn.). Upon showing my dissection of the Lobster to Professor Grant, he directed my attention to a structure observed in the Scorpion by Professor Müller, of Bonn, which has been thought to be the motor tract. This structure I had not at that time observed. It consists of a straight narrow slip, or riband, extending along the median line, above the cords and ganglia, from the great thoracic mass, over which it is expanded, to the last caudal ganglion, and is nearly of uniform size through its whole length. It is connected by some exceedingly small fibres with the nerves, while passing over the ganglia, and is, I believe, analogous to the transverse or involuntary nerves of insects. The true motor tract appears to be closely adherent to the sensitive in the Scorpion, the same as in the Lobster, and is scarcely observable even where it passes over a ganglion. It is nearly equal in size to the sensitive, with which it is connected. The nerves given off unite with those from the ganglia, just the same as in the Lobster.

The double structure of the nervous cords is more distinctly seen in one of the *Myriapoda* (*Scolopendra morsitans*, Linn.) [Plate XVII. fig. 43.] than in the Scorpion. The two longitudinal cords are united by twenty-three ganglia, and are composed of two tracts [figg. 44. and 45. (*a*, *b*)], lying one over the other, as in the Lobster and Scorpion. The ganglia are entirely on the under surface of the cords [Plate XVII. fig. 47. (*a*)], and the existence of the motor tract is very evident after it has been for some time in alcohol. It is marked by a line [fig. 47. (*d*)] which passes laterally over the ganglia, and is continued along the lateral surface of the cords. The nerves from the motor tract come off as filaments, anterior to those from the ganglion, with which they immediately unite. Four pairs of nerves are given off from each ganglion, and a fifth pair passes off from the motor tract [fig. 46. (*e*)] immediately posterior to each ganglion, and is given to the internal series of muscles. A narrow slip or riband [figg. 44. and 45. (*c*)], about one third the diameter of each cord, extends along the median line above the cords from one end of the body to the other, as in the Scorpion. This, like the tract in the Scorpion, has been thought to be the motor tract, but is, I think, analogous to the transverse or involuntary nerves of insects. In passing over each ganglion this tract is connected with the nerves by four pairs of very minute filaments. It is interesting to remark the existence of distinct ganglia, from which the antennal nerves originate, situated upon and forming portions of the cerebral ganglia [fig. 48. (D)], just the same as the ganglia upon the antennal feelers in the Lobster [fig. 40. (*b, c, d*)] ; and also to compare the size of the antennal nerves in *Scolopendra* with the optic nerves in the same animal, which are now gaining much importance in the animal series, and begin to share the cerebral ganglia nearly equally with the cerebral prolongations of the cords—the antennæ. In the *Scolopendra* we have thus a clear proof that the anterior or cerebral portion of the nervous system is formed originally by the coalescence of at least two pairs of ganglia, the antennal and optic ganglia, just the same as the caudal ganglion is formed by the ganglia of the penultimate united with the ganglion of the terminal segment of the body. The motor root of the great mandibular nerve is very distinct from the sensitive [E].

Although a double nervous column was thus proved to exist in *Crustacea, Arachnida*, and *Myriapoda*, it was not until lately that I have been able to identify and to distinguish the motor and sensitive columns from each other in insects. Their actual existence, therefore, could only be inferred from the discovery of them in other *Articulata*. It has been shown in another part of this paper, that the transverse series of nerves in insects cannot be analogous to the true motor nerves, from their having ganglia upon them in several genera. It was in the *Carabus*, LINN., the very insect in which the ganglia of the transverse nerves are most distinct [fig. 38. (*c*)], that I first identified the double structure of the cords in insects, and clearly distinguished the motor from the sensitive column (*a, b*). The motor roots are given off, and unite with the sensitive from the ganglia to form the symmetrical nerves, exactly the same as in the Lobster. The motor, sensitive, and transverse or involuntary nerves are all very distinct in the Green Grasshopper, *Gryllus viridissimus*, LINN. [Plate XVI. fig. 39. (*a, b, c*)]. Indeed they are so distinct under the microscope as to have been readily seen by a friend who was with me when examining the specimen. But it is in Lepidopterous insects, *Papilio, Phalœna*, and *Sphinx*, that the detection of the three kinds of nerves, motor, sensitive, and transverse or involuntary, has given me most satisfaction; because it is in these genera that the transverse nerves, from their large size and from the apparent absence of any other motor column, have been believed to be analogous to the motor nerves of vertebrated animals.

In the larva of the *Sphinx ligustri*, soon after it has entered its last skin, the three kinds of nerves are more distinct than at a subsequent period. The two sensitive columns, or gangliated portions of the cords, lie close to the under surface of the body, and consist each of a column of fibres, which at certain distances inclose a nodule of granulated, opake, grey matter, which constitutes a chief part of the ganglion. A few fibres of the sensitive column pass on that side of the nodule which lies to the median line of the body of the insect; while the larger portion of the column passes on the outer side of the nodule, from which the nerves are given off, and the two portions of the column uniting again behind the nodule thus constitute a ganglion. As the development of the nervous system proceeds, the ganglion thus formed in the sensitive column of one cord is closely applied to, and firmly united in, the median line, with a corresponding ganglion in the sensitive column of the other; and the two thus combined form a double ganglion of the spinal or symmetrical system. The motor column [Plate XVI. fig. 35. (*b*)], consisting entirely of a series of longitudinal fibres, giving off nerves at certain distances, lies upon, and is closely approximated to the sensitive (*a*), which it very nearly equals in diameter, and is only clearly distinguished from it while passing over the ganglia, and by a line which runs along the sides of each cord. The motor nerves are given off from the column at the anterior margin of each ganglion (*h*), along which they pass diagonally outwards, until they reach the nerve from the ganglion (*f*), with which they immediately unite. In the caudal ganglion of the Sphinx [fig. 36.], which at this period consists of two double ganglia, the motor column (*b*), after being thus distributed to the first, passes on to the next, and terminates in each half of the column dividing upon the middle of the ganglion into two portions (*b b*), that unite with the terminal nerves which are given to the rectum and generative organs. In the thoracic part of the insect, the double-rooted nerves to the wings are formed, first by the anterior root, which is derived entirely from the motor column, and next by the posterior, which is formed by one part from the motor and one from a ganglion of the sensitive column. In addition to these, the nerves of the wing receive several large nerves from the transverse or respiratory series, the anatomy of which has been described in a former part of this Paper*.

In the *Papilio urticæ*, Linn. [fig. 37.], and *P. Iö*, Linn., the ganglia are exceedingly large compared with the size of the cords. When examined with a very strong light, the motor column may be seen from the under surface of the cords through the ganglia quite distinct from the sensitive, and it continues so along the sides of the cords into the nerves of the wings. This is an interesting fact, on account of the wings being supplied with nerves in *Papilio*, Linn., directly from the cords, and not as in *Sphinx* from the cords and ganglia. The motor nerves pass around the exterior of the ganglia, and the column itself passes over them, exactly the same as in other *Articulata*.

We have thus a series of facts which distinctly show the existence of a nervous system analogous to that of vertebrated animals through all the higher *Articulata*; and it cannot be doubted that the same structure exists throughout all the articu-

* Page 402.

lated classes. The motor tract, as we should naturally expect would be the case, is equal in size to the sensitive, the power of motion being evidently the primary endowment of organized beings, and existing where sensation can hardly be expected to be found, and where there is only the simplest form of the nervous tissue, entirely without ganglia.

If the nerves and cords be examined immediately after removal from the body of an insect, they exhibit a fibrous appearance; but if macerated a few hours in water, they then look as if formed of series of globules, or rather of disintegrated, irregular parts, as remarked by Dr. Hodgkin in the nerves of the higher animals. It is very certain that the large fibres exchange or interweave a few filaments with each other, to constitute the two tracts of the cord; and this is also the structure of the nerves in general. It is by approximation of several fibres that the large nervous trunks are formed during the development of the Sphinx; the transverse nerves unite first with those from the motor root which comes from the cord, and next with those from the ganglion. This union begins, first by a shortening of the nervous columns in a longitudinal direction; and this is followed by the transverse nerves, and motor root to the wing, becoming greatly thickened, and gradually approximated from the distal extremity inwards to the middle line of the body. This approximation continues until these are united in like manner with the nerve from the ganglion, so that the development of the nerves to the wings takes place from the periphery to the centre, exactly the same as in the lateral development of the cords, as observed by M. Audouin and Dr. Milne Edwards in the smaller *Crustacea*. The nerves to the wings are thus formed of three series of fibres, which are traceable as distinct tracts along a great part of the whole nerve; although closely approximated nerves do not coalesce, but only interchange filaments. The nerves of other *Articulata* exhibit the same appearance as those of insects. The fibrous texture is best seen, and is very distinct, in some of the *Crustacea*. In the Sphinx, and other insects, after coagulation in alcohol, the nerves are contracted in diameter.

b. The terminations of nerves are very difficult to distinguish. They appear to end in, and unite with, the tissues unto which they are distributed. In the Wild Bee I have traced some of the extremities of nerves from the last ganglion, apparently into the very substance of the exterior, or hard covering of the segments of the abdomen. In the larva of the Blood Beetle, *Chrysomela tenebricosa*, Linn., I have traced some of the filaments from nerves of the third ganglion into the cellular texture of the vesicles, or bags, which inclose masses of adipose matter; but I could not discover that any of the filaments entered the fatty masses. They appeared to terminate in the texture of the vesicles. I have found them distributed likewise over the tracheal vessels, and once succeeded in tracing some filaments from a large nerve on the internal side of the posterior thighs of *Gryllus viridissimus*, Linn., to the chief tracheal vessel along which it runs. The filaments were expanded over the vessel until they appeared lost in its texture. The same is the case with some filaments from the transverse or respiratory system in the *Sphinx* and in *Cossus Ligniperda*, Steph. The respiratory nerve divides and passes on each side the ramifying tracheal vessels as they come from the longitudinal one; the posterior division forms a very minute plexus at their base, and both distribute some filaments upon the ramifying branches, which appear to be lost in their substance. While the respiratory nerves pass on both sides of these tracheæ, a large branch from the gangliated cords, the symmetrical system, passes only on the posterior, and gives off a few filaments to the surrounding muscles in its way round the side of the body to the dorsal muscles.

c. The ganglia, when just removed from the recently killed insect, are of a more opake colour than the nerves. When placed in alcohol they do not contract in size, but become still more opake, and appear, therefore, in their chemical composition, more analogous to coagulated albumen, while the nerves, which remain nearly transparent, seem more analogous to fibrin. There is as much uncertainty respecting the ultimate structure of ganglia as of nerves. When macerated in water for a few hours they readily decompose; the cerebral ganglia much sooner than the others. From this circumstance some have supposed that cerebral ganglia contained ventricles, but I have been unable to discover any, although I have searched for them very closely. From the appearance of the ganglia in *Papilio Iö*, LINN., before noticed, it seems probable that a few fibres pass through the ganglia, both longitudinally and transversely, to the body of the insect, and that ganglia are in reality composed of a nodule of grey matter intermingled with, and inclosed among the fibres of the sensitive column. This is further supported by the entire disappearance of ganglia, as in the sixth and seventh, during the transformations of the insect, while the nerves which come from these ganglia remain, and then come from the cords. Whatever be the ultimate structure of ganglia, there seems to be some modification of their chemical composition different from that of nerve. As the optic nerves, which are developed during the pupa state, are formed of fibres, there certainly appears reason to suppose that the structure of the cerebral, and consequently of other ganglia, is to a certain extent fibrous, whatever be the peculiar arrangement or interchanging of the fibres.

All the cords and ganglia, but particularly the latter, are profusely supplied with exceedingly minute tracheal vessels, which penetrate the nerves and most internal part of the ganglion. The minuteness of these extremities is such that I have failed to detect them even with a powerful triplet. I have in general used a triplet, or WOLLASTON'S doublet, in examining these minute structures.

Having traced the nerves of the Sphinx through all their changes, and examined their distribution and structure as compared with other *Articulata,* it now remains to show the manner in which the changes which take place in them are induced and effected.

III. *Development of the Nervous Columns.*

a. During the time I was most engaged in watching the development of the Sphinx, in the spring of 1832, considerable difficulties presented themselves, and many things were not sufficiently explained, owing partly to want of specimens, and partly to the uncertainty of the period at which the changes take place in different individuals. I determined, therefore, to repeat my observations upon another Lepidopterous insect, of a different genus, and for this purpose chose the commonest of our British species, the Nettle Butterfly, *Papilio urticæ*, Linn. Heroldt has accurately noticed the changes in *Papilio brassicæ*, Linn.

I selected for my observations a large number of the larva of the Nettle Butterfly, and fed them in breeding cages until they suspended themselves preparatory to changing to the pupa state. The moment of throwing off the old skin was carefully watched, and the precise time of its occurrence noted. By these means an adequate number of specimens was collected, and the time the insects had remained in the pupa state accurately known, and the specimens were then dissected at stated periods. The manner in which the insect prepares to undergo its change, and the mode of its occurrence, are known to every naturalist; I shall therefore confine myself to the changes of the nervous system, in illustration of what takes place in the *Sphinx ligustri*. The nervous system of *P. urticæ*, Linn., very closely resembles that of the Sphinx, and has the same number of ganglia.

Two hours after the insect has suspended itself to undergo its transformation, a considerable change in the arrangement of the nervous system takes place. The cerebral ganglia are distinct from each other, but are not yet enlarged. When viewed from above, each presents a pear-shaped appearance, the anterior part of the lateral surface being elongated forwards and gives origin to the antennal and optic nerves. At the base of the optic nerves, even at this early period, there is the same appearance of dark pigment as in the *Sphinx ligustri*, from which it is clear that this is deposited in the earliest stages of transformation, both in the Butterfly and Moth. The subœsophageal ganglion is nearly twice its original size, and the crura which connect it to the cerebral ganglia are considerably shorter, as well as the cords that connect the second, third, fourth, and fifth ganglia. The two last are separated only by a short interval, and are slightly enlarged. The fifth, sixth, and seventh ganglia are closer together, the cords between them disposed irregularly, and the longitudinal position of the ganglia altered. The ganglia from the seventh to the eleventh remain as in the active larva.

By unremittingly watching a number of larva through all their preparatory states for changing, we can easily judge, within a very short period, when the transformation will take place. Just before throwing off the old skin there is much activity throughout the whole insect, and if it be dissected about *half an hour* [Plate XV. fig. 21.] before this occurs, the nerves for the future wings, and the cerebral, and second, third, fourth, and fifth ganglia are all slightly enlarged, and the first ganglion very considerably. The cords which connect them diverge from each other, while those between the fifth, sixth, and seventh ganglia are more folded than in any other part of the body.

Immediately after the insect has assumed the pupa state [Plate XV. fig. 22.], all the ganglia are brought closer together, and the cords are disposed more irregularly than at any other period, in consequence of the shortening which has taken place in every segment of the body, by which the cords have been rendered too long to lie in a direct line. Those cords which connect the first five ganglia are somewhat increased in size. It is at this period there is the greatest activity, and sometimes irregularity in the progress of the changes. The fourth and fifth ganglia, and their intervening cords (which are those parts in which the first great changes commence), are often nearer together and have more coalesced at this period in some specimens, than in others at five or six hours later. This coincides with what occurs in the *Sphinx ligustri,* in which the precise period when the coalescence of ganglia takes place cannot positively be stated, since it varies a little in different specimens, and depends probably upon the temperature of the atmosphere, and upon the vigour of the insect at the time of changing,

One hour after transformation [Plate XV. fig. 23.], the cerebral ganglia have become more closely united, the nerves to the antennæ more distinct, and the rudiments of the optic nerves more developed at their base. The fourth and fifth ganglia are still approaching each other, and the cords are larger in diameter at their connexion with the fifth, the anterior part of which has become less distinct, and seems about to coalesce with them. The distance between the remaining ganglia is still decreasing, and the investing membranes, or exterior surface of the cords, exhibit a corrugated appearance, as if in the act of becoming shortened. In the *Sphinx ligustri,* besides the longitudinal cords and ganglia, and nerves given directly from them, we have seen there are others lying upon them,—the transverse or superadded nerves. There are like series in *Papilio urticæ,* L., the distributions of which are nearly similar. The first series begins immediately behind the first subœsophageal ganglion (*b*), where the nerves run directly outwards, along the course of the trachea, which are distributed over the first ganglion, and come directly from the first spiracle. Some of the branches unite with nerves from the second ganglion (*d*), while the main branch of this segment runs in the course of the muscles at the back part of the head. Behind the second ganglion, branches unite with the large nerve which comes from the cord between the second and third ganglion to supply the first pair of wings (*f*), and which is apparently single, and does not originate, as in the Sphinx, one root from the cord and the other from a ganglion. Behind the third ganglion, the nerve from the cord to the second pair of wings (*i*) receives a branch from the third series (*h*), while the greater number of the nerves pass outwards to the muscles. A series of these transverse nerves exists, as in the Sphinx, just anterior to each of the remaining ganglia (*o, o, o*), unto the nerves of which they give a few filaments, while their main branches are distributed separately among the tracheæ and muscles, excepting only those of the fourth, fifth, and sixth series, which become approximated to the nerves from the corresponding ganglia, and in the development of the Butterfly at this period, afford us an instance of the commencement of an interesting fact before alluded to, the formation of nervous trunks by the approximation and union of many fibres. The series anterior to the fifth ganglion [5. (*o*)] is now greatly diverging, and the ganglion and nerves are passing forwards and becoming united with it.

Seven hours after changing [Plate XVI. fig. 24.] there is still an enlargement of the cerebral ganglia, optic nerves, five first ganglia, and their intervening cords. The fourth and fifth have advanced closer together, and the very short cords which connect them are so much increased in diameter as to resemble a separate ganglion (*x*): the distance between the fifth and sixth is diminished, and all the remaining ganglia are slightly enlarged. The cords between them, just anterior to each ganglion, are also slightly enlarged, and are less irregularly disposed than in the previous stages. The transverse nerves are beginning to assume their temporary ganglionic appearance (*o, o, o*), and the terminal nerves from the last ganglion are enlarging for the supply of the developing organs of generation.

At *twelve hours* [Plate XV. fig. 25.] the fifth ganglion, by its coalescence with the cords that united it to the fourth, has assumed a triangular appearance, the broadest part being posteriorly. The transverse series, anterior to the fifth ganglion, which at seven hours was beginning to be united to the nerves from this ganglion, is now so completely joined to them as almost to have disappeared, there being only a triangular elevation upon the anterior part of the ganglion to indicate its previous existence [5. (*o*)], thus affording us a further proof of the adhesion of contiguous parts, and of the manner in which nervous trunks are formed.

At *eighteen hours* [Plate XV. fig. 26.] all the parts have become more concentrated; the ganglia, cords, and nerves, particularly those to the wings, are more enlarged; and the transverse nerves, although continuing separate, give filaments to the nerves from the ganglia, and themselves exhibit at their division more the appearance of ganglia; while the fourth and fifth ganglia and cords have now so completely coalesced as to appear like an irregular elongated mass. The cords in the abdomen lie more in a direct line, but just anterior to each ganglion are still a little enlarged.

At *twenty-four hours* [Plate XV. fig. 27.] the fourth and fifth ganglia have advanced still closer together; the fifth is slightly larger than the fourth. The cords just before the sixth ganglion are dilated, and the transverse nerves of the thorax are enlarged, keeping pace with, or rather preceding, the development of the respiratory organs.

At *thirty-six hours* [Plate XV. fig. 28.] the optic nerves have attained a size almost equal to that of the cerebral ganglia, and after this period become very little larger; and the first subœsophageal ganglion has coalesced with the cerebral ganglia, and forms with them a complete ring around the œsophagus. The fifth ganglion has now decreased in size, and is smaller than the fourth, while in some specimens the nerves which were given from it now come from the cords immediately behind it, and thereby seem to indicate that part of the nervous substance of the ganglion has been transmitted forwards. The sixth ganglion, which at twenty-four hours is decreased in size, has disappeared; and the nerves which came from that also come now from the cords, very near to those of the fifth ganglion, and thus further show that the substance of the ganglion has been transmitted forwards. The seventh ganglion is decreased in size.

At *forty-eight hours* [Plate XVI. fig. 29.] the whole of the cords have regained the longitudinal direction, so that there must have been either consolidation, absorption, or elongation forwards of the nervous substance, for the purposes of development. The seventh ganglion has now entirely disappeared.

At *fifty-eight hours* [Plate XVI. fig. 30.] a further change is effected. The second and third ganglia approach and coalesce, and the double ganglion thus formed is only separated from the larger thoracic mass, composed of the fourth and fifth ganglia, and part of the sixth, by very short but much enlarged cords. The transverse plexus are united with the nerves to the wings, and the whole mass of ganglia and nerves have been carried forwards, and lie more in the middle of the thorax. The optic and antennal nerves have nearly attained their full development, and the plexus of nerves and ganglia in the thorax, which in the larva exhibit an intricate arrangement, are now united, and form only a few large trunks, which can hardly be recognised as the same structures. The arrangement of the whole nervous system is nearly the same as exists in the perfect insect. Yet all this has taken place at a comparatively early period of the pupa, three days not having elapsed since the insect underwent its metamorphosis. It is interesting to observe that while the nervous system has been thus rapidly progressing, the alimentary canal, generative system, and other organs are still very far from completion, and, as compared with the nervous system, have made but little progress. It therefore seems as if necessary that the nervous system should be first completed.

These observations upon the Butterfly were made in June 1832, when the length of time that the insect remained in the pupa state was generally thirteen days and a few hours. They were carefully repeated in the following August, when the temperature of the season was considerably higher, and then the insect seldom continued more than nine, and often not more than eight days in pupa; thus clearly proving the decided influence which increased temperature exerts over development in accelerating the latter stages, since I could not discover that the earlier period, during which the changes in the nervous system were taking place, was very much accelerated by it.

These observations coincide with those upon the Sphinx. But it is interesting to remark the difference in the length of time which the changes occupy in the two insects, relatively to the length of time which they pass in the pupa state. The Butterfly, during the summer, is scarcely fourteen days, and often not more than eight in the pupa, and very nearly completes its changes in the nerves in three days. The Sphinx, on the other hand, passes nearly nine months in the pupa state, during more than eight of which its nervous system is undergoing change. But it may still be remarked that the greatest rapidity and extent of change in the Sphinx occurs during a short period immediately subsequent to its becoming a pupa. This is in perfect accordance with the changes in the Butterfly.

b. The means by which the development of the two insects take place are similar. They depend chiefly upon a shortening of the longitudinal and diagonal muscles of the body, when the parts of the future insect which have been forming in the larva have arrived at the greatest development they are capable of in that condition, and, as in the fœtus of vertebrated animals, at the completion of the full term of utero-gestation, induce a necessity for change. When this is to take place in the Sphinx, the larva ceases to eat, becomes restless and active, and after forming a cell in the earth lies at rest with its body coiled up, and soon loses the power of locomotion. During this time a contraction of all the longitudinal and diagonal muscles of the body is taking place, particularly of the fourth, fifth, and sixth segments; and the minute vessels which connect the old skin of the larva to the new one of the pupa beneath it are ruptured, and a fluid is effused which greatly assists in separating the old from the new covering. The body of the insect is considerably shortened. This contraction occasions a permanent shortening of the longitudinal muscles, which then gain new attachments, by which portions of each segment of the body, now soft and delicate, are drawn one beneath the other, forming broad duplicatures of the external teguments. This contraction and shifting of the muscles is carried to such an extent in the fourth, fifth, and sixth segments, as to form a large duplicature around the whole body, and to constitute the future division between the thorax and abdomen. The fifth segment is almost lost in the fourth, and the sixth, the first of the abdomen, is greatly diminished. The third segment is not at all decreased along its dorsal surface. It constitutes the greater portion of the thorax.

By these changes in the tegumentary and muscular structure of the body the ganglia of the cords are brought nearer together, and confined in their respective places in the segments by the nerves running transversely from them. The cords, from being too long to lie in a direct line, are folded irregularly between the ganglia. The greatest folding and irregularity of the cords is between the fourth, fifth, and sixth ganglia, where, from the almost entire obliteration of two segments, it might justly be expected. A disposition is induced in the first five ganglia to become aggregated into one mass, by their impingement upon each other, occasioned by the approximation and union of segments to form the thorax, which is assuming a fixed condition, and becoming the centre of development. It is in this manner that the nervous structure appears to be elongated forwards for the enlargement of particular parts. The cords in the abdomen recover their original direction, but are not much increased in diameter, and the sixth and seventh ganglia entirely disappear, while the ganglia and nerves in the thorax are enlarged, and aggregated into two masses; the crura of the cerebral ganglia are much shortened, and the optic nerves are at the same time proportionably developed. From these facts we may conclude, that it is by an elongation forwards and outwards in every direction,—by the approximation of nervous trunks already formed,—and by the interweaving, exchange, and recombination of filaments into new trunks, that the development of the nervous system in insects is completed.

Description of the Plates.

Plate XIII.

Fig. 1. Nervous system of *Sphinx ligustri,* as seen in the pupa state in the month of April, exhibiting the relative situation of the nerves and ganglia, and the manner in which they are distributed to, and pass under the longitudinal muscles.

A. A portion of the exterior of the dorsal surface of the pupa case, reflected to show the muscles and nerves. Magnified $2\frac{1}{2}$ diameter.

Fig. 2. The cerebral and thoracic ganglia and nerves, magnified ten diameters. The letters of figg. 1. and 2. correspond with each other.

A. Cerebral ganglia.
B. Optic nerves developing.
C. Nerves which connect the anterior lateral ganglia with the antennal nerves.
D. The nerves to the antennæ.
E. The vagus or pneumogastric ganglion and nerve.
a. Anterior lateral ganglia.
b. First series of respiratory nerves.
c. Pair of small nerves from the cord.
2 . Second ganglion of the cords.
d. Nerve to first pair of legs.
e. Second respiratory nerves.
f 3. Double-rooted nerve to first pair of wings.
g. To second pair of legs.
h. Third respiratory nerves.
i. To second pair of wings.
k. To third pair of legs.
l. Nerves of fifth ganglion, which sends branches to the dorsal muscles of eighth segment.
m. Nerves from the sixth ganglion.
n. Symmetrical nerves, which, after passing under the longitudinal abdominal muscles, pass up to the dorsal.
o, o, o, o, o. Respiratory nerves of the abdomen.

p, p, p, p, p. Extremities of the respiratory nerves, which, after passing over the longitudinal abdominal muscles, divide and pass on each side of the spiracles

q. Dorsal longitudinal muscles.

r. Abdominal longitudinal muscles.

s. Duplicatures of the segments.

t. Division between thorax and abdomen.

u. Between third and fourth segments.

v. Between the first and second segments.

w. Anterior spiracles.

Fig. 3. The supra-œsophageal ganglia and nerves: magnified fifteen diameters.

A. Cerebral ganglia.

B. Optic nerves developing.

C. Nerves of connexion.

D. Nerves of the antennæ.

E. The vagus nerve.

a. Anterior lateral ganglia.

b. Nerves which connect them with the cerebral ganglia.

c. With the first respiratory or transverse nerves.

d. With the antennæ.

g. With the vagus or pneumogastric.

e. The ganglion and nerve of the vagus.

f. Its trunk after passing beneath the cerebral ganglia.

h, h, h. Branches given to the aortal portion of the dorsal vessel, or heart, which runs immediately above the vagus.

i. Division of the vagus at the cardiac extremity of the stomach.

k, k. Branches given to the œsophagus, along which the vagus runs.

Figg. 4 and 5. Nerves and ganglia of the head in the larva and perfect state of the common Blood Beetle, *Chrysomela tenebricosa*, LINN. Fig. 4. Larva. Fig. 5. Perfect insect.

A. Cerebral ganglia in the larva, scarcely at all united, and exactly as seen in the early stages of the larva in the Bee and Sphinx.

A. Cerebral ganglia.

B. Optic nerves.

C. Anterior lateral ganglia.

D. First subœsophageal ganglion.

a. The two nerves or origins of the vagus, forming between them the ganglion.

b. A ganglionic enlargement on the trunk of the nerve.

c. Nerves given to the sides of pharynx and œsophagus.

d. A division of the vagus at the commencement of the stomach, as in the larva of the Sphinx.

e, e. The pharyngeal and nerves of *taste*.

Fig. 6. Nervous system of the perfect insect *Sphinx ligustri.*

A. Cerebral ganglia.

B. Optic nerves. The figures refer to the number of the ganglia.

o, o, o. Respiratory nerves.

p, p, p. Their division at the spiracles.

Fig. 7. Lateral view of the cerebral and thoracic part of the nervous system in the perfect state of the Sphinx, magnified two and a half diameters.

Plate XIV.

Fig. 8. The thoracic ganglia and nerves of Sphinx in the imago state. Figures as before.

a. Nerve to the first pair of wings.

b. The double-rooted origins of this nerve.

c. A plexus or ganglion formed at the union of the two roots.

d. The nerves to second pair of wings.

e. Some separate filaments which are given to the muscles.

f. Nerves to the second pair of legs.

g, h. Tracheal vessels.

Fig. 9. The anterior portion of the abdominal nerves and columns, covered by the investing structure (*a*), seen only in the perfect state of the insects.

b. Respiratory nerves.

c. The moto-sensitive or symmetrical nerves.

Fig. 10. The cerebral ganglia and nerves of the proboscis, magnified fifteen diameters.

a. The great nerve to the proboscis.

b. Its entrance into the organ.

c. The external branch.

d. The main branch given to the muscles.

e. The internal, or branch which runs along the grooved or internal side of the organ.

B. Optic nerve.

D. Antennal nerve.

Figg. 11, 12, 13. Exhibit vertical sections of the larva, pupa, and imago states of *Sphinx ligustri*, Linn.; showing the relative situation of the circulatory, alimentary, and nervous systems in the three stages, and also the duplicatures of the external integument as occasioned by the contractions and re-attachments of the muscles at the period of changing from the larva to the perfect state. The silk-vessels and part of the organs of generation, &c. are omitted. The figures refer respectively to the number of the segments. Magnified two and a half diameters.

a, b. The dorsal vessel with its appendages.

The alimentary canal.

c. The œsophagus.

d. The stomach.

e. The small intestines.

f. The biliary vessels.

g. The cæcum.

h. The colon and rectum.

i. The testis.

Fig. 14. Œsophageal portion of the alimentary canal in larva of Sphinx, exhibiting the constrictor (*h*) muscles of the pharynx (*d*), aortal portion of the dorsal vessel (*a*, *b*), cerebral ganglia, and optic nerves developed from them (*c*); anterior lateral ganglia (*e*, *f*) and ganglion and nerve of the vagus in situ, seen from above.

Fig. 15. Internal view of part of the proboscis of the perfect insect.

a. The transverse muscles.

b. The groove.

c. The œsophagus.

Fig. 16. Longitudinal and transverse muscles of the proboscis of Sphinx.

a. Flexors.

b. Longitudinal extensors.

c. Transverse extensors.

Fig. 17. Lateral view of four joints of the antennæ *Sphinx Elpenor*, Linn.

Fig. 18. View of the articulating surface of the antenna.

Plates XV. and XVI.

Fig. 19. Tracheæ of the proboscis, showing their relative situation with that of the nerves. Letters *a* to *e* as in fig. 10.

f, *h*. The tracheæ.

g. Their united origin.

Figg. 20. to 30. Plates XV. and XVI. Exhibit the gradual change and development of the nervous system of *Papilio urticæ*, magnified 12 diameters. The figures and letters refer as in figg. 1. and 2. of *Sphinx ligustri*.

Fig. 31. Posterior view of the cerebral and œsophageal ganglia of *Papilio urticæ*, L., as seen during the change at forty-eight hours after change to the pupa state.

B. The optic nerves, developing and showing the sacculi of fibres and development of the choroid membrane (*c*).

Fig. 32. One of the abdominal ganglia of *P. urticæ*, viewed from below by means of transmitted light, and showing the two inclosed approximated nodules of grey matter, and also the transverse nerves, as seen at twenty-four hours after change to the pupa state, magnified 30 diameters; the transverse nerves (*c*), the spinal cords (*f*), the compound symmetrical nerves (*h*), the small diagonal nerves (*g*).

Fig. 33. Abdominal ganglion at forty-eight hours after changing, seen from its upper surface by transmitted light. References the same as fig. 32.

Fig. 34. A lateral view of the supra-œsophageal or cerebral ganglia and nerves, and of the first and second subœsophageal of the larva of *Sphinx ligustri.* The letters and figures from A to F as in fig. 1.

G. The lateral cords which connect the cerebral with the subœsophageal ganglia, and pass on each side the œsophagus. (*h*) The nerve which is given to the side of the mouth, and is perhaps the nerve of *taste.*

d. Nerves to the first pair of legs.

e. Some of the transverse nerves which pass round on each side of the cardiac part of the stomach.

Fig. 35. One of the abdominal ganglia of the Sphinx, with the portion of cords and transverse nerves (*c*), showing the motor columns of the cords (*b*) passing over the double ganglion of the sensitive (*a*), and the manner in which the double tract of the transverse or respiratory nerves (*c*) divides at right angles before a ganglion (*d*), and sends on it filaments (*e*) which, after uniting with the transverse portion of the nerve (*d*), and with the moto-sensitive nerves (*f*), converge and join again in the median line above the cords, having first derived a few filaments from the motor column (*g*).

Fig. 36. The double terminal ganglion of the larva of the Sphinx, showing the transverse nerves (*c*), and division and termination of the motor column (*b*, *b*).

Fig. 37. View of the under surface of the posterior thoracic nerves and ganglia in *Papilio Iö,* LINN., showing the transverse (*c*), motor (*b*) and sensitive tracts (*a*).

Fig. 38. The same in the abdominal parts of cords in *Carabus monilis,* L., in which the ganglia of the transverse nerves are very distinct (*c*).

Fig. 39. The same in *Gryllus viridissimus,* LINN., but without ganglia on the transverse nerves.

PLATE XVII.

Fig. 40. The nervous system of the Lobster, (*Astacus marinus,* LEACH,) natural size.

1 to 14. Ganglia.

A. Cerebral ganglia.

B. Passage for the œsophagus between the crura.

C. The subœsophageal thoracic ganglia.

D. The post abdominal or caudal ganglia and nerves.

E, E. The origins of the vagus.

a. Optic nerves.

b. Nerves from a distinct ganglion to the large antennæ.

c. Nerves from another ganglion, anterior to the last, to the small antennæ. These four ganglia to the four antennæ are situated anteriorly, and a little lateral to the cords, and are connected with the sensitive column (*d*).

e, e. Origins of the vagus and of a nerve, as in insects, distributed to the sides of the mouth.

f. The continuation of the vagus along the dorsal surface of the stomach, and in connexion with the anterior distribution of the anterior aortal vessel, as in insects.

g. The glosso-pharyngeal nerve.

h. Mandibular nerves.

i, i. Nerves to the inferior lip and palpi.

k. To the large claws.

l, l, l, l. Nerves derived from the upper surface of the cords to the branchiæ.

m. To the circulatory vessels.

n. From the ganglia.

o. Moto-sensitive or symmetrical nerves from the ganglia.

p. Their division to the post-abdominal feet.

q. Nerves from the upper surface of the cords.

r. The terminal pair to the rectum.

s. Terminal nerves from the cords and ganglia.

t, v, w. To the lamellæ of the tail.

Fig. 41. The under surface of the 7th, 8th, and 9th thoracic ganglia, exhibiting the compound structure of the ganglia, which are situated entirely on the under surface.

Fig. 42. The same portion of the nervous system viewed from the upper surface, and exhibiting the two halves of the motor column passing over the ganglia.

Fig. 43. The nervous system of *Scolopendra morsitans*, Linn., of natural size.

Fig. 44. A portion of the same magnified, and showing the involuntary or respiratory tract (*c*) passing in the median line above the motor column (*b*), which is seen distinct from the ganglia of the sensitive (*a*).

Fig. 45. A lateral view of the same.

Fig. 46. The motor surface of a ganglion (*a*), and motor (*b*) and involuntary tracts (*c*).

Fig. 47. A lateral view of the same, showing the ganglia on the under surface, and the line (*d*) which separates the motor from the sensitive columns; the involuntary tract passing above them (*c*).

Fig. 48. The cerebral and first subœsophageal ganglia of *Scolopendra*.

A. Cerebral ganglia.

B. Optic nerves.

D. Antennal nerves with large ganglia at their base.

E. The subœsophageal ganglion giving origin to the great mandibular nerves, and exhibiting their double origin.

XX. *Experimental Researches in Electricity.—Eighth Series.* *By* MICHAEL FARADAY, *D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.*

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§. 14. *On the Electricity of the Voltaic Pile; its source, quantity, intensity, and general characters.* ¶ i. *On simple Voltaic Circles.* ¶ ii. *On the intensity necessary for Electrolyzation.* ¶ iii. *On associated Voltaic Circles, or the Voltaic Battery.* ¶ iv. *On the resistance of an Electrolyte to Electrolytic action.* ¶ v. *General remarks on the active Voltaic Battery.*

¶ i. *On simple Voltaic Circles.*

875. THE great question of the source of electricity in the voltaic pile has engaged the attention of so many eminent philosophers, that a man of liberal mind and able to appreciate their powers would probably conclude, although he might not have studied the question, that the truth was somewhere revealed. But if in pursuance of this impression he were induced to enter upon the work of collating results and conclusions, he would find such contradictory evidence, such equilibrium of opinion, such variation and combination of theory, as would leave him in complete doubt respecting what he should accept as the true interpretation of nature: he would be forced to take upon himself the labour of repeating and examining the facts, and then use his own judgment on them in preference to that of others.

876. This state of the subject must, to those who have made up their minds on the matter, be my apology for entering upon its investigation. The views I have taken of the definite action of electricity in decomposing bodies (783.), and the identity of the power so used with the power to be overcome (855.), founded not on a mere opinion or general notion, but on facts which, being altogether new, were to my mind precise and conclusive, gave me, as I conceived, the power of examining the question with advantages not before possessed by any, and which might compensate, on my part, for the superior clearness and extent of intellect on theirs. Such are the considerations which have induced me to suppose I might help in deciding the question, and be able to render assistance in that great service of removing *doubtful knowledge.* Such knowledge is the early morning light of every advancing science, and is essential to its development; but the man who is engaged in dispelling that which is deceptive

in it, and revealing more clearly that which is true, is as useful in his place, and as necessary to the general progress of the science, as he who first broke into the intellectual darkness, and opened a path into knowledge before unknown to man.

877. The identity of the force constituting the voltaic current or electrolytic agent, with that which holds the elements of electrolytes together (855.), or in other words with chemical affinity, seemed to indicate that the electricity of the pile itself was merely a mode of exertion, or exhibition, or existence of *true chemical action*, or rather of its cause; and I have consequently already said that I agree with those who believe that the *supply* of electricity is due to chemical powers (857.).

878. But the great question of whether it is originally due to metallic contact or to chemical action, i. e. whether it is the first or the second which *originates* and determines the current, was to me still doubtful; and the beautiful and simple experiment with amalgamated zinc and platina, which I have described minutely as to its results (863, &c.), did not decide the point; for in that experiment the chemical action does not take place without the contact of the metals, and the metallic contact is inefficient without the chemical action. Hence either might be looked upon as the *determining* cause of the current.

879. I thought it essential to decide this question by the simplest possible forms of apparatus and experiment, that no fallacy might be inadvertently admitted. The well known difficulty of effecting decomposition by a single pair of plates, except in the fluid exciting them into action (863.), seemed to throw insurmountable obstruction in the way of such experiments; but I remembered the easy decomposibility of the solution of iodide of potassium (316.), and seeing no theoretical reason, if metallic contact was not *essential*, why true electro-decomposition should not be obtained without it, even in a single circuit, I persevered and succeeded.

880. A plate of zinc, about eight inches long and half an inch wide, was cleaned and bent in the middle to a right angle, fig. 1 *a*. Plate XVIII. A plate of platina, about three inches long and half an inch wide, was fastened to a platina wire, and the latter bent as in the figure *b*. These two pieces of metal were arranged together as delineated, but as yet without the vessel *c*, and its contents, which consisted of dilute sulphuric acid mingled with a little nitric acid. At *x* a piece of folded bibulous paper, moistened in a solution of iodide of potassium, was placed on the zinc, and was pressed upon by the end of the platina wire. When under these circumstances the plates were dipped into the acid of the vessel *c*, there was an immediate effect at *x*, the iodide being decomposed, and iodine appearing at the *anode* (663.), i. e. against the end of the platina wire.

881. As long as the lower ends of the plates remained in the acid the electric current continued, and the decomposition proceeded at *x*. On removing the end of the wire from place to place on the paper, the effect was evidently very powerful; and on placing a piece of turmeric paper between the white paper and zinc, both papers being moistened with the solution of iodide of potassium, alkali was evolved at the

cathode (663.) against the zinc, in proportion to the evolution of iodine at the *anode*. Hence the decomposition was perfectly polar, and decidedly dependent upon a current of electricity passing from the zinc through the acid to the platina in the vessel *c*, and back from the platina through the solution to the zinc at the paper *x*.

882. That the decomposition at *x* was a true electrolytic action, due to a current determined by the state of things in the vessel *c*, and not dependent upon any mere direct chemical action of the zinc and platina on the iodide, or even upon any *current* which the solution of iodide might by its action on those metals tend to form at *x*, was shown, in the first place, by removing the vessel *c* and its acid from the plates, when all decomposition at *x* ceased, and in the next by connecting the metals, either in or out of the acid, together, when decomposition of the iodide at *x* occurred, but in a *reverse order*; for now alkali appeared against the end of the platina wire, and the iodine passed to the zinc, the current being the contrary of what it was in the former instance, and produced directly by the difference of action of the solution in the paper on the two metals. The iodine of course combined with the zinc.

883. When this experiment was made with pieces of zinc amalgamated over the whole surface (863.), the results were obtained with equal facility and in the same direction, even when only dilute sulphuric acid was contained in the vessel *c* (fig. 1.). Whichever end of the zinc was immersed in the acid, still the effects were the same: so that if, for a moment, the mercury might be supposed to supply the metallic contact, the reversion of the amalgamated piece destroys that objection. The use of *unamalgamated zinc* (880.) removes all possibility of doubt.

884. When, in pursuance of other views (930.), the vessel *c* was made to contain a solution of caustic potash in place of acid, still the same results occurred. Decomposition of the iodide was effected freely, though there was no metallic contact of dissimilar metals, and the current of electricity was in the *same direction* as when acid was used.

885. Even a solution of brine in the glass *c* could produce all these effects.

886. Having made a galvanometer with platina wires, and introduced it into the course of the current between the platina plate and the place of decomposition *x*, it was affected, giving indication of currents in the same direction as those shown to exist by the chemical action.

887. If we consider these results generally, they lead to very important conclusions. In the first place they prove, in the most decisive manner, that *metallic contact is not necessary for the production of the voltaic current.* In the next place they show a most extraordinary mutual relation of the chemical affinities of the fluid which *excites* the current, and the fluid which is *decomposed* by it.

888. For the purpose of simplifying the consideration, let us take the experiment with amalgamated zinc. The metal so prepared exhibits no effect until the current can pass: it at the same time introduces no new action, but merely removes an influence which is extraneous to those belonging either to the production or the

effect of the electric current under investigation (1000.); an influence also which, when present, tends only to confuse the results.

889. Let two plates, one of amalgamated zinc and the other of platina, be placed parallel to each other (fig. 2.), and introduce a drop of dilute sulphuric acid, *y*, between them at one end: there will be no sensible chemical action at that spot unless the two plates are connected somewhere else, as at P Z, by a body capable of conducting electricity. If that body be a metal or certain forms of carbon, then the current passes, and, as it circulates through the fluid at *y*, decomposition ensues.

890. Then remove the acid from *y*, and introduce a drop of the solution of iodide of potassium at *x* (fig. 3.). Exactly the same set of effects occur, except that when the metallic communication is made at P Z, the electric current is in the opposite direction to what it was before, as is indicated by the arrows, which show the courses of the currents (667.).

891. Now *both* the solutions used are conductors, but the conduction in them is essentially connected with decomposition (858.) in a certain constant order, and therefore the appearance of the elements in certain places *shows* in what direction a current has passed when the solutions are thus employed. Moreover, we find that when they are used at opposite ends of the plates, as in the last two experiments (889. 890.), metallic contact being allowed at the other extremities, the currents are in opposite directions. We have evidently, therefore, the power of opposing the actions of the two fluids simultaneously to each other at the opposite ends of the plates, using each one as a conductor for the discharge of the current of electricity, which the other tends to generate; in fact, substituting them for metallic contact, and combining both experiments into one (fig. 4.). Under these circumstances there is an opposition of forces: the fluid, which brings into play the stronger set of chemical affinities for the zinc, (being the dilute acid,) overcomes the force of the other, and determines the formation and direction of the electric current; not merely making that current pass through the weaker liquid, but actually reversing the tendency which the elements of the latter have in relation to the zinc and platina if not thus counteracted, and forcing them in the contrary direction to that they are inclined to follow, that its own current may have free course. If the dominant action at *y* be removed by making metallic contact there, then the liquid at *x* resumes its power; or if the metals be not brought into contact at *y*, but the affinities of the solution there weakened, whilst those active at *x* are strengthened, then the latter gains the ascendancy, and the decompositions are produced in a contrary order.

892. Before drawing a *final* conclusion from this mutual dependence and state of the chemical affinities of two distant portions of acting fluids (916.), I will proceed to examine more minutely the various circumstances under which the reaction of the decomposed body is rendered evident upon the action of that body, also in the act of decomposition, which produces the voltaic current.

893. The use of *metallic contact* in a single pair of plates, and the cause of its great

superiority above contact made by other kinds of matter, become now very evident. When an amalgamated zinc plate is dipped into dilute sulphuric acid, the force of chemical affinity exerted between the metal and the fluid is not sufficiently powerful to cause sensible action at the surfaces of contact, and occasion the decomposition of water by the oxidation of the metal, although it *is* sufficient to produce such a condition of the electricity (or the power upon which chemical affinity depends) as would produce a current if there were a path open for it (916. 956.); and that current would complete the conditions necessary, under the circumstances, for the decomposition of the water.

894. Now the presence of a piece of platina touching both the zinc and the fluid to be decomposed, opens the path required for the electricity. Its *direct communication* with the zinc is effectual, far beyond any communication made between it and that metal, (i. e. between the platina and zinc,) by means of decomposable conducting bodies, or, in other words, *electrolytes,* as in the experiment already described (891.); because, when they are used, the chemical affinities between them and the zinc produce a contrary and opposing action to that which is influential in the dilute sulphuric acid; or if that action be but small, still the affinity of their component parts for each other has to be overcome, for they cannot conduct without suffering decomposition; and this decomposition is found *experimentally* to react back upon the forces which in the acid tend to produce the current (904. 910. &c.), and in numerous cases entirely to neutralize them. Where direct contact of the zinc and platina occurs, these obstructing forces are not brought into action, and therefore the production and the circulation of the electric current and the concomitant action of decomposition are then highly favoured.

895. It is evident, however, that one of these opposing actions may be dismissed, and yet an electrolyte be used for the purpose of completing the circuit between the zinc and platina immersed separately into the dilute acid; for if, in fig. 1, the platina wire be retained in metallic contact with the zinc plate *a*, at *x*, and a division of the platina be made elsewhere, as at *s*, then the solution of iodide placed there, being in contact with platina at both surfaces, exerts no chemical affinities for that metal; or if it does, they are equal on both sides. Its power, therefore, of forming a current in opposition to that dependent upon the action of the acid in the vessel *c*, is removed, and only its resistance to decomposition remains as the obstacle to be overcome by the affinities exerted in the dilute sulphuric acid.

896. This becomes the condition of a single pair of plates where *metallic contact* is allowed. In such cases, only one set of opposing affinities are to be overcome by those which are dominant in the vessel *c*; whereas, when metallic contact is not allowed, two sets of opposing affinities must be conquered (894.).

897. It has been considered a difficult, and by some an impossible, thing to decompose bodies by the current from a single pair of plates, even when it was so powerful as to heat bars of metal red hot, as in the case of HARE's calorimeter, arranged as a

single voltaic circuit, or of WOLLASTON's powerful single pair of metals. This difficulty has arisen altogether from the antagonism of the chemical affinity engaged in producing the current with the chemical affinity to be overcome, and depends entirely upon their relative intensity; for when the sum of forces in one has a certain degree of superiority over the sum of forces in the other, the former gains the ascendancy, determines the current, and overcomes the latter forces so as to make the substance exerting them yield up its elements in perfect accordance, both as to direction and quantity, with the course of those which are exerting the most intense action.

898. Water has generally been the substance, the decomposition of which has been sought for as a chemical test of the passage of an electric current. But I now began to perceive a reason for its failure, and for a fact which I had observed long before (315. 316.) with regard to the iodide of potassium, namely, that bodies would differ in facility of decomposition by a given electric current, according to the condition and intensity of their ordinary chemical affinities. This reason appeared in their reaction back upon the affinities tending to cause the current; and it appeared probable, that many substances might be found which could be decomposed by the current of a single pair of zinc and platina plates immersed in dilute sulphuric acid, although water resisted its action. I soon found this to be the case, and as the experiments offer new and beautiful proofs of the direct relation and opposition of the chemical affinities concerned in producing and in resisting the stream of electricity, I shall briefly describe them.

899. The arrangement of the apparatus was as in fig. 5. The vessel *v* contained dilute sulphuric acid; Z and P are the zinc and platina plates; *a*, *b*, and *c* are platina wires; the decompositions were effected at *x*, and occasionally, indeed generally, a galvanometer was introduced into the circuit at *g*: its place only is here given, the circle at *g* having no reference to the size of the instrument. Various arrangements were made at *x*, according to the kind of decomposition to be effected. If a drop of liquid was to be acted upon, the two ends were merely dipped into it; if a solution contained in the pores of paper was to be decomposed, one of the extremities was connected with a platina plate supporting the paper, whilst the other extremity rested on the paper, *e*, fig. 12: or sometimes, as with sulphate of soda, a plate of platina sustained two portions of paper, one of the ends of *a* and *c* resting upon each piece, *c*, fig. 14. The darts represent the direction of the electric current (667.).

900. Solution of *iodide of potassium*, being placed in moistened paper at the interruption of the circuit at *x*, was readily decomposed. Iodine was evolved at the *anode*, and alkali at the *cathode*, of the decomposing body.

901. *Protochloride of tin*, when fused and placed at *x*, was also readily decomposed, yielding perchloride of tin at the *anode* (779.), and tin at the *cathode*.

902. Fused chloride of silver, placed at *x*, was also easily decomposed; chlorine was evolved at the *anode*, and brilliant metallic silver, either in films upon the surface of the liquid, or in crystals beneath, evolved at the *cathode*.

903. Water acidulated with sulphuric acid, solution of muriatic acid, solution of sulphate of soda, fused nitre, and the fused chloride and iodide of lead were not decomposed by this single pair of plates, excited only by dilute sulphuric acid.

904. These experiments give abundant proofs that a single pair of plates can electrolyze bodies and separate their elements. They also show in a beautiful manner the direct relation and opposition of the chemical affinities concerned at the two points of action. In those cases where the sum of the opposing affinities at x was sufficiently beneath the sum of the acting affinities in v, decomposition took place; but in those cases where they rose higher, decomposition was effectually resisted and the current ceased to pass (891.).

905. It is, however, evident, that the sum of acting affinities in v may be increased by using other fluids than dilute sulphuric acid, in which latter case, as I believe, it is merely the affinity of the zinc for the oxygen already combined with hydrogen in the water that is exerted in producing the electric current (919.): and when the affinities are so increased, the view I am supporting leads to the conclusion, that bodies which resisted in the preceding experiments would then be decomposed, because of the increased difference between their affinities and the acting affinities thus exalted. This expectation was fully confirmed in the following manner.

906. A little nitric acid was added to the liquid in the vessel v, so as to make a mixture which I shall call diluted nitro-sulphuric acid. On repeating the experiments with this mixture, all the substances before decomposed again gave way, and much more readily. But besides that, many which before resisted electrolyzation now yielded up their elements. Thus, solution of sulphate of soda, acted upon in the interstices of litmus and turmeric paper, yielded acid at the *anode* and alkali at the *cathode*; solution of muriatic acid tinged by indigo yielded chlorine at the *anode* and hydrogen at the *cathode*; solution of nitrate of silver yielded silver at the *cathode*. Again, fused nitre and the fused iodide and chloride of lead were decomposable by the current of this single pair of plates though they were not by the former (903.).

907. A solution of acetate of lead was apparently not decomposed by this pair, nor did water acidulated by sulphuric acid seem at first to give way (973.).

908. The increase of intensity or power of the current produced by a simple voltaic circle, with the increase of the force of the chemical action at the exciting place, is here sufficiently evident. But in order to place it in a clearer point of view, and to show that the decomposing effect was not at all dependent, in the latter cases, upon the mere capability of evolving *more* electricity, experiments were made in which the quantity evolved could be increased without variation in the intensity of the exciting cause. Thus the experiments in which dilute sulphuric acid was used (899.) were repeated, using large plates of zinc and platina in the acid; but still those bodies which resisted decomposition before, resisted it also under these new circumstances. Then again, where nitro-sulphuric acid was used (906.), mere wires of platina and zinc were immersed in the exciting acid; yet, notwithstanding this change, those

bodies were now decomposed which resisted any current tending to be formed by the dilute sulphuric acid. For instance, muriatic acid could not be decomposed by a single pair of plates when immersed in dilute sulphuric acid; nor did making the sulphuric acid strong, nor enlarging the size of the zinc and platina plates immersed in it, increase the power; but if to a weak sulphuric acid a very little nitric acid was added, then the electricity evolved had power to decompose the muriatic acid, evolving chlorine at the *anode* and hydrogen at the *cathode,* even when mere wires of metals were used. This mode of increasing the intensity of the electric current, as it excludes the effect dependent upon many pairs of plates, or even the effect of making any one acid stronger or weaker, is at once referable to the condition and force of the chemical affinities which are brought into action, and may, both in principle and practice, be considered as perfectly distinct from any other mode.

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909. The direct reference which is thus experimentally made in the simple voltaic circle of the *intensity* of the electric current to the *intensity* of the chemical action going on at the place where the existence and direction of the current is determined, leads to the conclusion that by using selected bodies, as fused chlorides, salts, solutions of acids, &c., which may act upon the metals employed with different degrees of chemical force; and using also metals in association with platina, or with each other, which shall differ in the degree of chemical action exerted between them and the exciting fluid or electrolyte, we should be able to obtain a series of comparatively constant effects due to electric currents of different intensities, which would serve to assist in the construction of a scale so as to supply the means of determining relative degrees of intensity accurately in future researches.

910. I have already expressed the view which I take of the decomposition in the experimental place, as being the direct consequence of the superior exertion at some other spot of the same kind of power as that to be overcome, and therefore as the result of an antagonism of forces of the *same* nature (891. 904.). Those at the place of decomposition have a reaction upon, and a power over, the exerting or determining set proportionate to what is needful to overcome their own power; and hence a curious result of *resistance* offered by decompositions to the original determining force, and consequently to the current. This is well shown in the cases where such bodies as chloride of lead, iodide of lead, and water would not decompose with the current produced by a single pair of zinc and platina plates in sulphuric acid (903.), although they would with a current of higher intensity produced by stronger chemical powers. In such cases no sensible portion of the current passes (967.); the action is stopped: and I am now of opinion that in the case of the law of conduction which I described in the Fourth Series of these Researches (413.), the bodies which are electrolytes in the fluid state cease to be such in the solid form, because the attractions of the particles by which they are retained in combination and in their relative position, are then too powerful for the electric current. The particles retain their places; and as decompo-

sition is prevented, the transmission of the electricity is prevented also; and although a battery of many plates may be used, yet if it be of that perfect kind which allows of no extraneous or indirect action (1000.), the whole of the affinities concerned in the activity of that battery are at the same time also suspended and counteracted.

911. But referring to the *resistance* of each single case of decomposition, it would appear that as these differ in force according to the affinities by which the elements in the substance tend to retain their places, they also would supply cases constituting a series of degrees by which to measure the initial intensities of simple voltaic or other currents of electricity, and which, combined with the scale of intensities determined by different degrees of *acting force* (909.), would probably include a sufficient set of differences to meet almost every important case where a reference to intensity would be required.

912. According to the experiments I have already had occasion to make, I find that the following bodies are electrolytic in the order in which I have placed them, those which are first being decomposed by the current of lowest intensity. These currents were always from a single pair of plates, and may be considered as elementary *voltaic forces.*

Iodide of potassium (solution).
Chloride of silver (fused).
Protochloride of tin (fused).
Chloride of lead (fused).
Iodide of lead (fused).
Muriatic acid (solution).
Water, acidulated with sulphuric acid.

913. It is essential that in all endeavours to obtain the relative electrolytic intensity necessary for the decomposition of different bodies, attention should be paid to the nature of the electrodes, and the other bodies present which may favour secondary actions (986.). If in electro-decomposition one of the elements separated has an affinity for the electrode, or for bodies present in the surrounding fluid, then the affinity resisting decomposition is in part balanced by such power, and the true place of the electrolyte in a table of the above kind is not obtained: thus, chlorine combines with a positive platina electrode freely, but iodine scarcely at all, and therefore I believe it is that the chloride stands first in the preceding Table. Again, if in the decomposition of water not merely sulphuric but also a little nitric acid be present, then the water is more freely decomposed, for the hydrogen at the *cathode* is not ultimately expelled, but finds oxygen in the nitric acid, with which it can combine to produce a secondary result; the affinities opposing decomposition are in this way diminished, and the elements of the water can then be separated by a current of lower intensity.

914. Advantage may be taken of this principle to interpolate more minute degrees into the scale of initial intensities already referred to (909. 911.) than is there supposed; for by combining the force of a current *constant* in its intensity, with the use

of electrodes consisting of matter, having more or less affinity for the elements evolved from the decomposing electrolyte, various intermediate degrees may be obtained.

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915. Returning to the consideration of the source of electricity (878, &c.), there is another proof of the most perfect kind that metallic contact has nothing to do with the *production* of electricity in the voltaic circuit, and further, that electricity is only another mode of the exertion of chemical forces. It is, the production of the *electric spark* before any contact of metals is made, and by the exertion of *pure and unmixed chemical forces*. The experiment, which will be described further on (956.), consists in obtaining the spark upon making contact between a plate of zinc and a plate of copper plunged into dilute sulphuric acid. In order to make the arrangement as elementary as possible, mercurial surfaces were dismissed, and the contact made by a copper wire connected with the copper plate, and then brought to touch a clean part of the zinc plate. The electric spark appeared, and it must of necessity have existed and passed *before the zinc and the copper were in contact.*

916. In order to render more distinct the principles which I have been endeavouring to establish, I will restate them in their simplest form, according to my present belief. The electricity of the voltaic pile (856. note) is not dependent either in its origin or its continuance to the contact of the metals with each other (880. 915.). It is entirely due to chemical action (882.), and is proportionate in its intensity to the intensity of the affinities concerned in its production (908.); and in its quantity to the quantity of matter which has been chemically active during its evolution (869.). This definite production is again one of the strongest proofs that the electricity is of chemical origin.

917. As *volta-electro-generation* is a case of mere chemical action, so *volta-electro-decomposition* is simply a case of the preponderance of one set of chemical affinities more powerful in their nature, over another set which are less powerful; and if the instance of two opposing sets of such forces (891.) be considered, and their mutual relation and dependence borne in mind, there appears no necessity for using, in respect to such cases, any other term than chemical affinity, (though that of electricity may be very convenient,) or supposing any new agent to be concerned in producing the results; for we may consider that the powers at the two places of action are in direct communion and balanced against each other through the medium of the metals (891.), fig. 4, in a manner analogous to that in which mechanical forces are balanced against each other by the intervention of the lever (1031.).

918. All the facts show us that that power commonly called chemical affinity, can be communicated to a distance through the metals and certain forms of carbon; that the electric current is only another form of the forces of chemical affinity; that its power is in proportion to the chemical affinities producing it; that when it is deficient in force it may be helped by calling in chemical aid, the want in the former being made up by an equivalent of the latter; that, in other words, the forces termed chemical affinity and electricity are one and the same.

919. When the circumstances connected with the production of electricity in the ordinary voltaic circuit are examined and compared, it appears that the source of that agent, always meaning the electricity which circulates and completes the current in the voltaic apparatus, and gives that apparatus power and character (947. 996.), exists in the chemical action which takes place directly between the metal and the body with which it combines, and not at all in the subsequent action of the substance so produced with the acid present*. Thus, when zinc, platina, and dilute sulphuric acid are used, it is the union of the zinc with the oxygen of the water which determines the current; and though the acid is essential to the removal of the oxide so formed, in order that another portion of zinc may act on another portion of water, it does not, by combination with that oxide, produce any sensible portion of the current of electricity which circulates; for the quantity of electricity is dependent upon the quantity of zinc oxidized, and in definite proportion to it: its intensity is in proportion to the intensity of the chemical affinity of the zinc for the oxygen under the circumstances, and is scarcely, if at all, affected by the use of either strong or weak acid (908.).

920. Again, if zinc, platina, and muriatic acid are used, the electricity appears to be dependent upon the affinity of the zinc for the chlorine, and to be circulated in exact proportion to the number of particles of zinc and chlorine which unite, being in fact an equivalent to them.

921. But in considering this oxidation, or other direct action upon the METAL itself, as the cause and source of the electric current, it is of the utmost importance to observe that the oxygen or other body must be in a peculiar condition, namely, in the state of *combination*; and not only so, but limited still further, to such a state of combination, and in such proportions as will constitute an *electrolyte* (823.). A pair of zinc and platina plates cannot be so arranged in oxygen gas as to produce a current of electricity, or act as a voltaic circle, even though the temperature may be raised so highly as to cause oxidation of the zinc far more rapidly than if the pair of plates were plunged into dilute sulphuric acid, for the oxygen is not part of an electrolyte, and cannot therefore conduct the forces onwards by decomposition, or even as metals do by itself. Or if its gaseous state embarrass the minds of some, then liquid chlorine may be taken. It does not excite a current of electricity through the two plates by combining with the zinc, for its particles cannot transfer the electricity active at the point of combination, across to the platina. It is not a conductor of itself, like the metals; nor is it an electrolyte, so as to be capable of conduction during decomposition, and hence there is simple chemical action at the spot, and no electric current†.

* WOLLASTON, Philosophical Transactions, 1801, p. 427.

† I do not mean to affirm that no traces of electricity ever appear in such cases. What I mean is that no electricity is evolved in any way, due or related to the causes which excite voltaic electricity, or proportionate to them. That which does appear occasionally is the smallest possible fraction of that which the acting matter could produce if arranged so as to act voltaically, probably not the one hundred thousandth, or even the millionth part, and is very probably altogether different in its source.

922. It might at first be supposed that a conducting body, not electrolytic, might answer as the third substance between the zinc and the platina; and it is true that we have some such capable of exerting chemical action upon the metals. They must, however, be chosen from the metals themselves, for there are no bodies of this kind except those substances and charcoal. To decide the matter by experiment, I made the following arrangement. Melted tin was put into a glass tube bent into the form of the letter V, fig. 6, so as to fill the half of each limb, and two pieces of thick platina wire, *p*, *w*, inserted, so as to have their ends immersed some depth in the tin; the whole was then allowed to cool, and the ends *p* and *w* connected with a delicate galvanometer. The part of the tube at *x* was now reheated, whilst the portion *y* was retained cool. The galvanometer was immediately influenced by the thermo-electric current produced. The heat was steadily increased at *x*, until at last the tin and platina combined there; an effect which is known to take place with strong chemical action and high ignition; but not the slightest additional effect occurred at the galvanometer. No other deflection than that due to the thermo-electric current was observable the whole time. Hence, though a conductor, and one capable of exerting chemical action on the tin, was used, yet, not being an *electrolyte*, not the slightest effect of an electrical current could be observed (947.).

923. From this it seems apparent that the peculiar character and condition of an electrolyte is *essential* in one part of the voltaic circuit; and its nature being considered, good reasons appear why it and it alone should be effectual. An electrolyte is always a compound body: it can conduct, but only whilst decomposing. Its conduction depends upon its decomposition and the *transmission of its particles* in directions parallel to the current; and so intimate is this connexion, that if their transition be stopped, the current is stopped also; if their course be changed, its course and direction changes with them; if they proceed in one direction, it has no power to proceed in any other than a direction invariably dependent on them. The particles of an electrolytic body are all so mutually connected, are in such relation with each other through their whole extent in the direction of the current, that if the last is not disposed of, the first is not at liberty to take up its place in the new combination which the powerful affinity of the most active metal tends to produce; and then the current itself is stopped; for the dependencies of the current and the decomposition are so mutual, that whichever be originally determined, i. e. the motion of the particles or the motion of the current, the other is invariable in its concomitant production and its relation to it.

924. Consider, then, water as an electrolyte and also as an oxidizing body. The attraction of the zinc for the oxygen is greater, under the circumstances, than that of the oxygen for the hydrogen; but in combining with it, it tends to throw into circulation a current of electricity in a certain direction. This direction is consistent (as is found by innumerable experiments) with the transfer of the hydrogen from the zinc towards the platina, and the transfer in the opposite direction of fresh oxygen from

the platina towards the zinc; so that the current *can pass* in that one line, and, whilst it passes, can consist with and favour the renewal of the conditions upon the surface of the zinc, which at first determined both the combination and circulation. Hence the continuance of the action there, and the continuation of the current. It therefore appears quite as essential that there should be an electrolyte in the circuit, in order that the action may be transferred forward, in a *certain constant direction*, as that there should be an oxidizing or other body capable of acting directly on the metal; and it also appears to be essential that these two should merge into one, or that the principle directly active on the metal by chemical action should be one of the *ions* of the electrolyte used. Whether the voltaic arrangement be excited by solution of acids, or alkalies, or sulphurets, or by fused substances (476.), this principle has always hitherto, as far as I am aware, been an *anion* (943.); and I anticipate, from a consideration of the principles of electric action, that it must of necessity be one of that class of bodies.

925. If the action of the sulphuric acid used in the voltaic circuit be considered, it will be found incompetent to produce any sensible portion of the electricity of the current by its combination with the oxide formed, for this simple reason, it is deficient in a most essential condition: it forms no part of an electrolyte, nor is it in relation with any other body present in the solution which will permit of the mutual transfer of the particles and the consequent transfer of the electricity. It is true, that as the plane at which the acid is dissolving the oxide of zinc formed by the action of the water, is in contact with the metal zinc, there seems no difficulty in considering how the oxide there could communicate an electrical state, proportionate to its own chemical action on the acid, to the metal, which is a conductor without decomposition. But on the side of the acid there is no substance to complete the circuit: the water, as water, cannot conduct it, or at least only so small a proportion that it is merely an incidental and almost inappreciable effect (970.); and it cannot conduct it as an electrolyte, because an electrolyte conducts in consequence of the *mutual* relation and action of its particles; and neither of the elements of the water, nor even the water itself, as far as we can perceive, are *ions* with respect to the sulphuric acid (848.)*.

926. This view of the secondary character of the sulphuric acid as an agent in the production of the voltaic current, is further confirmed by the fact, that the current generated and transmitted is directly and exactly proportional to the quantity of water decomposed and the quantity of zinc oxidized (868. 991.): and is the same as that required to decompose the same quantity of water. As, therefore, the decomposition of the water shows that the electricity has passed by its means, there remains no other electricity to be accounted for or to be referred to any action other than that of the zinc and the water on each other.

* It will be seen that I here agree with Sir HUMPHRY DAVY, who has experimentally supported the opinion that acids and alkalies in combining do not produce any current of electricity. Philosophical Transactions, 1826, p. 398.

927. The general case (for it includes the former one (924.),) of acids and bases, may theoretically be stated in the following manner. Let *a*, fig. 7. be supposed to be a dry oxyacid, and *b* a dry base, in contact at *c*, and in electric communication at their extremities by plates of platina *p p*, and a platina wire *w*. If this acid and base were fluid, and combination took place at *c*, with an affinity ever so vigorous, and capable of originating an electric current, the current could not circulate in any serious degree; because, according to the experimental results, neither *a* nor *b* could conduct without being decomposed, for they are either electrolytes or else insulators, under all circumstances, except to very feeble and unimportant currents (970. 986.). Now the affinities at *c* are not such as tend to cause the *elements* either of *a* or *b* to separate, but only such as would make the two bodies combine together as a whole; the point of action is, therefore, insulated, the action itself local (921. 947.), and no current can be formed.

928. If the acid and base be dissolved in water, then it is possible that a small portion of the electricity due to chemical action may be conducted by the water without decomposition (966. 984.); but the quantity will be so small as to be utterly disproportionate to that due to the equivalents of chemical force; will be merely incidental; and, as it does not involve the essential principles of the voltaic pile, it forms no part of the phenomena at present under investigation*.

929. If for the oxyacid a hydracid be substituted (927.),—as one analogous to the muriatic, for instance,—then the state of things changes altogether, and a current due to the chemical action of the acid on the base is possible. But now both the bodies act as electrolytes, for it is only one principle of each which combine mutually,—as, for instance, the chlorine with the metal,—and the hydrogen of the acid and the oxygen of the base are ready to traverse with the chlorine of the acid and the metal of the base in conformity with the current and according to the general principles already so fully laid down.

930. This view of the oxidation of the metal, or other *direct* chemical action upon it, being the sole cause of the production of the electric current in the ordinary voltaic pile, is supported by the effects which take place when alkaline or sulphuretted solutions (931. 943.) are used for the electrolytic conductor instead of dilute sulphuric acid. It was in elucidation of this point that the experiments without metallic contact, and with solution of alkali as the exciting fluid, already referred to (884.), were made.

931. Advantage was then taken of the more favourable condition offered, when metallic contact is allowed (895.), and the experiments upon the decomposition of bodies by a single pair of plates (899.) were repeated, solution of caustic potassa

* It will, I trust, be fully understood, that in these investigations I am not professing to take an account of every small, incidental, or barely possible effect, dependent upon slight disturbances of the electric fluid during chemical action, but am seeking to distinguish and identify those actions on which the power of the voltaic battery essentially depends.

being employed in the vessel *v*, fig. 5. in place of dilute sulphuric acid. All the effects occurred as before: the galvanometer was deflected; the decompositions of the solutions of iodide of potassium, nitrate of silver, muriatic acid, and sulphate of soda ensued at *x*; and the places where the evolved principles appeared, as well as the deflection of the galvanometer, indicated a current in the *same direction* as when acid was in the vessel *v*; i. e. from the zinc through the solution to the platina, and back by the galvanometer and decomposing agent to the zinc.

932. The similarity in the action of either dilute sulphuric acid or potassa goes indeed far beyond this, even to the proof of identity in *quantity* as well as in *direction* of the electricity produced. If a plate of amalgamated zinc be put into a solution of potassa, it is not sensibly acted upon; but if touched in the solution by a plate of platina, hydrogen is evolved on the surface of the latter metal, and the zinc is oxidized exactly as when immersed in dilute sulphuric acid (863.). I accordingly repeated the expement before described with weighed plates of zinc (864. &c.), using however solution of potassa instead of dilute sulphuric acid. Although the time required was much longer than when acid was used, amounting to three hours for the oxidizement of 7·55 grains of zinc, still I found that the hydrogen evolved at the platina plate was the equivalent of the metal oxidized at the surface of the zinc. Hence the whole of the reasoning which was applicable in the former instance applies also here, the current being in the same direction, and its decomposing effect in the same degree, as if acid instead of alkali had been used (868.).

933. The proof, therefore, appears to me complete, that the combination of the acid with the oxide, in the former experiment, had nothing to do with the production of the electric current; for the same current is here produced when the action of the acid is absent, and the reverse action of an alkali is present. I think it cannot be supposed for a moment, that the alkali acted chemically as an acid to the oxide formed; on the contrary, our general chemical knowledge leads to the conclusion, that the ordinary metallic oxides act rather as acids to the alkalies: yet that kind of action would tend to give a reverse current in the present case, if any were due to the union of the oxide of the exciting metal with the body which combines with it. But instead of any variation of this sort, the direction of the electricity was constant, and its quantity also directly proportional to the water decomposed, or the zinc oxidized. There are reasons for believing that acids and alkalies, when in contact with metals upon which they cannot act directly, still have a power of influencing their attractions for oxygen (941.); but all the effects in these experiments prove, I think, that it is the oxidation of the metal necessarily dependent upon, and associated as it is with, the electrolyzation of the water (921. 923.), that produces the current; and that the acid or alkali merely act as solvents, and by removing the oxidized zinc, allow other portions to decompose fresh water, and so continue the evolution or determination of the current.

934. The experiments were then varied by using solution of ammonia instead of

solution of potassa; and as it, when pure, is a bad conductor, like water (554.), it was occasionally improved in that power by adding sulphate of ammonia to it. But in all the cases the effects were the same as before; decompositions of the same kind were effected, and the electric current producing these was in the same direction as in the experiments just described.

935. In order to put the equal and similar action of acid and alkali to stronger proof, arrangements were made as in fig. 8.; the glass vessel A contained dilute sulphuric acid, the corresponding glass vessel B solution of potassa, P P was a plate of platina dipping into both solutions, and Z Z two plates of amalgamated zinc connected with a delicate galvanometer. When these were plunged at the same time into the two vessels, there was generally a first feeble effect, and that in favour of the alkali, i. e. the electric current tended to pass through the vessels in the direction of the arrow, being the reverse direction of that which the acid in A would have produced alone: but the effect instantly ceased, and the action of the plates in the vessels was so equal, that, being contrary, because of the contrary position of the plates, no permanent current resulted.

936. Occasionally a zinc plate was substituted for the plate P P, and platina plates for the plates Z Z; but this caused no difference in the results: nor did a further change of the middle plate to copper produce any alteration.

937. As the opposition of electro-motive pairs of plates produces results other than those due to the mere difference of their independent actions (1011. 1045.), I devised another form of apparatus, in which the action of acid and alkali might be more directly compared. A cylindrical glass cup, about two inches deep within, an inch in internal diameter, and at least a quarter of an inch in thickness, was cut down the middle into two halves, fig. 9. A broad brass ring, larger in diameter than the cup, was supplied with a screw at one side; so that when the two halves of the cup were within the ring, and the screw was made to press tightly against the glass, the cup held any fluid put into it. Bibulous paper of different degrees of permeability was then cut into pieces of such a size as to be easily introduced between the loosened halves of the cup, and served when the latter were tightened again to form a porous division down the middle of the cup, sufficient to keep any two fluids on opposite sides of the paper from mingling, except very slowly, and yet allowing them to act freely as one *electrolyte*. The two spaces thus produced I will call the cells A and B, fig. 10. This instrument I have found of most general application in the investigation of the relation of fluids and metals amongst themselves and to each other. By combining its use with that of the galvanometer, it is easy to ascertain the relation of one metal with two fluids, or of two metals with one fluid, or of two metals and two fluids upon each other.

938. Dilute sulphuric acid, sp. gr. 1·25, was put into the cell A, and a strong solution of caustic potassa into the cell B; they mingled slowly through the paper, and at last a thick crust of sulphate of potassa formed on the side of the paper next to the

alkali. A plate of clean platina was put into each cell and connected with a delicate galvanometer, but no electric current could be observed. Hence the *contact* of acid with one platina plate, and alkali with the other, was unable to produce a current; nor was the combination of the acid with the alkali more effectual (925.).

939. When one of the platina plates was removed and a zinc plate substituted, either amalgamated or not, a strong electric current was produced. But, whether the zinc were in the acid whilst the platina was in the alkali, or whether the reverse order were chosen, the electric current was always from the zinc through the electrolyte to the platina, and back through the galvanometer to the zinc, the current seeming to be strongest when the zinc was in the alkali and the platina in the acid.

940. In these experiments, therefore, the acid seems to have no power over the alkali, but to be rather inferior to it in force. Hence there is no reason to suppose that the combination of the oxide formed with the acid around it has any direct influence in producing the electricity evolved, the whole of which appears to be due to the oxidation of the metal (919.).

941. The alkali, in fact, is superior to the acid in bringing a metal into what is called the positive state; for if plates of the same metal, as zinc, tin, lead, or copper, be used both in the acid or alkali, the electric current is from the alkali across the cell to the acid, and back through the galvanometer to the alkali, as Sir Humphry Davy formerly stated*. This current is so powerful, that if amalgamated zinc, or tin, or lead be used, the metal in the acid evolves hydrogen the moment it is placed in communication with that in the alkali, not from any direct action of the acid upon it, for if the contact be broken the action ceases, but because it is powerfully negative with regard to the metal in the alkali.

942. The superiority of alkali is further proved by this, that if zinc and tin be used, or tin and lead, whichever metal is put into the alkali becomes positive, that in the acid being negative. Whichever is in the alkali is oxidized, whilst that in the acid remains in the metallic state, as far as the electric current is concerned.

943. When sulphuretted solutions are used (930.) in illustration of the assertion, that it is the chemical action of the metal and one of the *ions* of the associated electrolyte that produces all the electricity of the voltaic circuit, the proofs are still the same. Thus, as Sir Humphry Davy† has shown, if iron and copper be plunged into dilute acid, the current is from the iron through the liquid to the copper; in solution of potassa it is in the same direction, but in solution of sulphuret of potassa it is reversed. In the two first cases it is oxygen which combines with the iron, in the latter sulphur which combines with the copper, that produces the electric current; but both of these are *ions*, existing as such in the electrolyte, which is at the same moment suffering decomposition; and, what is more, both of these are *anions*, for they leave

* Elements of Chemical Philosophy, p. 149; or Philosophical Transactions, 1826, p. 403.

† Elements of Chemical Philosophy, p. 148.

the electrolites at their *anodes,* and act just as chlorine, iodine, or any other *anion* would act which might have been previously chosen as that which should be used to throw the voltaic circle into activity.

944. The following experiments complete the series of proofs of the origin of the electricity in the voltaic pile. A fluid amalgam of potassium, containing not more than a hundredth of that metal, was put into pure water, and connected through the galvanometer with a plate of platina in the same water. There was immediately an electric current from the amalgam through the electrolyte to the platina. This must have been due to the oxidation only of the metal, for there was neither acid nor alkali to combine with, or in any way act on, the body produced.

945. Again, a plate of clean lead and a plate of platina were put into *pure* water. There was immediately a powerful current produced from the lead through the fluid to the platina: it was even intense enough to decompose solution of the iodide of potassium when introduced into the circuit in the form of apparatus already described (880.), fig. 1. Here no action of acid or alkali on the oxide formed from the lead could supply the electricity: it was due solely to the oxidation of the metal.

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946. There is no point in electrical science which seems to me of more importance than the state of the metals and the electrolytic conductor in a simple voltaic circuit *before and at* the moment when metallic contact is first completed. If clearly understood, I feel no doubt it would supply us with a direct key to the laws under which the great variety of voltaic excitements, direct and incidental, occur, and open out various new fields of research for our investigation.

947. We seem to have the power of deciding to a certain extent in numerous cases of chemical affinity, (as of zinc with the oxygen of water, &c. &c.) which of *two modes of action of the attractive power* shall be exerted (996.). In the one mode we can transfer the power onwards, and make it produce elsewhere its equivalent of action (867. 917.); in the other, it is not transferred, but exerted wholly at the spot. The first is the case of volta-electric excitation, the other ordinary chemical affinity: but both are chemical actions and due to one force or principle.

948. The general circumstances of the former mode occur in all instances of voltaic currents, but may be considered as in their perfect condition, and then free from those of the second mode, in some only of the cases; as in those of plates of zinc and platina in solution of potassa, or of amalgamated zinc and platina in dilute sulphuric acid.

949. Assuming it sufficiently proved, by the preceding experiments and considerations, that the electro-motive action depends, when zinc, platina, and dilute sulphuric acid are used, upon the mutual affinity of the metal zinc and the oxygen of the water (921. 924.), it would appear that the metal, when alone, has not power enough, under the circumstances, to take the oxygen and expel the hydrogen from the combination; for, in fact, no such action takes place. But it would also appear that it has power so far to act, by its attraction for the oxygen of the particles in contact

with it, as to place the similar forces already active between these and the other particles of oxygen and the particles of hydrogen in the water, in a peculiar state of tension or polarity, and probably also at the same time to throw those of its own particles which are in contact with the water into a similar but opposed state. Whilst this state is retained, no further change occurs; but when it is relieved, by completion of the circuit, in which case the forces determined in opposite directions, with respect to the zinc and the electrolyte, are found exactly competent to neutralize each other, then a series of decompositions and recompositions takes place amongst the particles of oxygen and hydrogen constituting the water, between the place of relief and the place where the zinc is active; these intervening particles being evidently in close dependence upon and relation to each other. The zinc forms a direct compound with those particles of oxygen which were, immediately before, in divided relation to both it and the hydrogen: the oxide is removed by the acid, and a fresh surface of contact between the zinc and water is presented, to renew and repeat the action.

950. Practically, the state of tension is best relieved by dipping a metal which has less attraction for oxygen than the zinc, into the dilute acid, and making it also touch the zinc. The force of chemical affinity, which has been influenced or polarized in the particles of the water by the dominant attraction of the zinc for the oxygen, is then transferred, in a most extraordinary manner, through the two metals, so as to re-enter upon the circuit in the electrolytic conductor, which cannot convey or transfer it without decomposition as the metals can; or rather, probably, it is exactly balanced and neutralized by the force which at the same moment completes the combination of the zinc with the oxygen of the water. The forces, in fact, of the two particles which are acting towards each other, and which are therefore in opposite directions, are the origin of the two opposite forces, or directions of force, in the current. They are of necessity equivalent to each other. Being transferred forward in contrary directions, they produce what is called the voltaic current: and it seems to me impossible to resist the idea that it must be preceded by a *state of tension* in the fluid, and between the fluid and the zinc; the *first consequence* of the affinity of the zinc for the oxygen of the water.

951. I have sought carefully for indications of a state of tension in the electrolytic conductor; and conceiving that it might produce something like structure, either before or during its discharge, I endeavoured to make this evident by polarized light. A glass cell, seven inches long, one inch and a half wide, and six inches deep, had two sets of platina electrodes adapted to it, one set for the ends, and the other for the sides. Those for the *sides* were seven inches long by three inches high, and when in the cell were separated by a little frame of wood covered with calico; so that when made active by connexion with a battery upon any solution in the cell, the bubbles of gas rising from them did not obscure the central parts of the liquid.

952. A saturated solution of sulphate of soda was put into the cell, and the electrodes connected with a battery of 150 pairs of 4-inch plates: the current of electricity

was conducted across the cell so freely, that the discharge was as good as if a wire had been used. A ray of polarized light was then transmitted through this solution, directly across the course of the electric current, and examined by an analysing plate; but though it penetrated seven inches of solution thus subject to the action of the electricity, and though contact was sometimes made, sometimes broken, and occasionally reversed during the observations, not the slightest trace of action on the ray could be perceived.

953. The large electrodes were then removed, and others introduced which fitted the *ends* of the cell. In each a slit was cut, so as to allow the light to pass. The course of the polarized ray was now parallel to the current, or in the direction of its axis (517.); but still no effect, under any circumstances of contact or disunion, could be perceived upon it.

954. A strong solution of nitrate of lead was employed instead of the sulphate of soda, but the results were equally negative.

955. Thinking it possible that the discharge of the electric forces by the successive decompositions and recompositions of the particles of the electrolyte might neutralize and therefore destroy any effect which the first state of tension could by possibility give, I took a substance which, being an excellent electrolyte when fluid, was a perfect insulator when solid, namely, borate of lead, in the form of a glass plate, and connecting the sides and the edges of this mass with the metallic plates, sometimes in contact with the poles of a voltaic battery, and sometimes even with the electric machine, for the advantage of the much higher intensity then obtained, I passed a polarized ray across it in various directions, as before, but could not obtain the slightest appearance of action upon the light. Hence I conclude, that notwithstanding the new and extraordinary state which must be assumed by an electrolyte, either during decomposition (when a most enormous quantity of electricity must be traversing it), or in the state of tension which is assumed as preceding decomposition, and which might be supposed to be retained in the solid form of the electrolyte, still it has no power of affecting a polarized ray of light; for no kind of structure or tension can in this way be rendered evident.

956. There is, however, one beautiful experimental proof of a state of tension acquired by the metals and the electrolyte before the electric current is produced, and *before contact* of the different metals is made (915.); in fact, at that moment when chemical forces only are efficient as a cause of action. I took a voltaic apparatus, consisting of a single pair of large plates, namely, a cylinder of amalgamated zinc, and a double cylinder of copper. These were put into a jar containing dilute sulphuric acid*, and could at pleasure be placed in metallic communication by a copper wire adjusted so as to dip at the extremities into two cups of mercury connected with the two plates.

* When nitro-sulphuric acid is used, the spark is more powerful, but local chemical action can then commence, and proceed without requiring metallic contact.

957. Being thus arranged, there was no chemical action whilst the plates were not connected. On *making* the connexion, a spark was obtained*, and the solution was immediately decomposed. On breaking it, the usual spark was obtained, and the decomposition ceased. In this case it is evident that the first spark must have occurred before metallic contact was made, for it passed through an interval of air, and also that it must have tended to pass before the electrolytic action began; for the latter could not take place until the current passed, and the current could not pass before the spark appeared. Hence I think there is sufficient proof, that as it is the zinc and water which by their mutual action produce the electricity of this apparatus, so these, by their first contact with each other, were placed in a state of powerful tension (951.), which, though it could not produce the actual decomposition of the water, was able to make a spark of electricity pass between the zinc and a fit discharger as soon as the interval was rendered sufficiently small. The experiment demonstrates the direct production of the electric spark from pure chemical forces.

958. There are a few circumstances connected with the production of this spark by a single pair of plates, which should be known, to ensure success to the experiment. When the amalgamated surfaces of contact are quite clean and dry, the spark, on making contact, is quite as brilliant as on breaking it, if not even more so. When a film of oxide or dirt was present at either mercurial surface, then the first spark was often feeble, and often failed, the breaking spark, however, continuing very constant and bright. When a little water was put over the mercury, the spark was greatly diminished in brilliancy, but very regular both on making and breaking contact. When the contact was made between clean platina, the spark was also very small, but regular both ways. The true electric spark is, in fact, very small, and when surfaces of mercury are used, it is the combustion of the metal which produces the greater part of the light. The circumstances connected with the burning of the mercury are most favourable on breaking contact; for the act of separation exposes clean surfaces of metal, whereas, on making contact, a thin film of oxide, or soiling matter, often interferes. Hence the origin of the general opinion that it is only when the contact is broken that the spark passes.

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959. With reference to the other set of cases, namely, those in which chemical affinity is exerted (947.), but where no transference of the power to a distance takes place, and where no electric current is produced, it is evident that forces of the most intense kind must be active, and in some way balanced in their activity, during such combinations; these forces being directed so immediately and exclusively towards each other, that no signs of the powerful electric current they can produce become apparent, although the same final state of things is obtained as if that current had passed. It was Ber-

* It has been universally supposed that no spark is produced on making the contact between a single pair of plates. I was led to expect one from the considerations already advanced in this paper. The wire of communication should be short; for with a long wire, circumstances strongly affecting the spark are introduced.

ZELIUS, I believe, who considered the heat and light evolved in cases of combustion as the consequences of this mode of exertion of the electric powers of the combining particles. But it will require a much more exact and extensive knowledge of the nature of electricity, and the manner in which it is associated with the atoms of matter, before we can understand accurately the action of this power in thus causing their union, or comprehend the nature of the great difference which it presents in the two modes of action just distinguished. We may imagine, but such imaginations must for the time be classed with the great mass of *doubtful knowledge* (876.) which we ought rather to strive to diminish than to increase; for the very extensive contradictions of this knowledge of itself shows that but a small portion of it can ultimately prove true.

960. Of the two modes of action in which chemical affinity is exerted, it is important to remark, that that which produces the electric current is as *definite* as that which causes ordinary chemical combination; so that in examining the *production* or *evolution* of electricity in cases of combination or decomposition, it will be necessary, not merely to observe certain effects dependent upon a current of electricity, but also their *quantity*: and though it may often happen that the forces concerned in any particular case of chemical action may be partly exerted in one mode and partly in the other, it is only those which are efficient in producing the current that have any relation to voltaic action. Thus, in the combination of oxygen and hydrogen to produce water, electric powers to a most enormous amount are for the time active (861. 873.); but any mode of examining the flame which they form during energetic combination, which has as yet been devised, has given but the feeblest traces. These therefore may not, cannot, be taken as evidences of the nature of the action; but are merely incidental results, incomparably small in relation to the forces concerned, and supplying no information of the way in which the particles are active on each other, or in which their forces are finally arranged.

961. That such cases of chemical action produce no *current of electricity*, is perfectly consistent with what we know of the voltaic apparatus, in which it is essential that one of the combining elements shall form part of, or be in direct relation with, an electrolytic conductor (921. 923.). That such cases produce no *free electricity of tension*, and that when they are converted into cases of voltaic action they produce a current in which the opposite forces are so equal as to neutralize each other, prove the equality of the forces in the opposed acting particles of matter, and therefore the equality of electric power in those quantities of matter which are called *electro-chemical equivalents* (824.). Hence another proof of the definite nature of electro-chemical action (783. &c.), and that chemical affinity and electricity are forms of the same power (917. &c.).

962. The direct reference of the effects produced by the voltaic pile at the place of experimental decomposition to the chemical affinities active at the place of excitation (891. 917.), gives a very simple and natural view of the cause why the bodies or *ions* evolved pass in certain directions; for it is only when they pass in those directions

that their forces can consist with and compensate (in direction at least) the superior forces which are dominant at the place where the action of the whole is determined. If, for instance, in a voltaic circuit, the activity of which is determined by the attraction of zinc for the oxygen of water, the zinc move from right to left, then any other *cation* included in the circuit, being part of an electrolyte, or forming part of it at the moment, will also move from right to left; and as the oxygen of the water, by its natural affinity for the zinc, moves from left to right, so any other body of the same class with it (i. e. any other *anion*), and under its government for the time, will move from left to right.

963. This I may illustrate by reference to fig. 11, the double circle of which may represent a complete voltaic circuit, the direction of its forces being determined by supposing for a moment the zinc *b* and the platina *c* as representing plates of those metals acting upon water, *d, e,* and other substances, but having their energy exalted so as to effect several decompositions by the use of a battery at *a* (989.). This supposition may be allowed, because the action in the battery will only consist of repetitions of what would take place between *b* and *c,* if they really constituted but a single pair. The zinc *b,* and the oxygen *d,* by their mutual affinity, tend to unite; but as the oxygen is already in association with the hydrogen *e,* and has its inherent chemical or electric powers neutralized for the time by those of the latter, the hydrogen *e* must leave the oxygen *d,* and advance in the direction of the arrow head, or else the zinc *b* cannot move in the same direction to unite to the oxygen *d,* nor the oxygen *d* move in the contrary direction to unite to the zinc *b,* the relation of the *similar* forces of *b* and *e,* in contrary directions, to the *opposite* forces of *d* being the preventive. As the hydrogen *e* advances, it, on coming against the platina *c, f,* which forms a part of the circuit, communicates its electric or chemical forces through it to the next electrolyte in the circuit, fused chloride of lead, *g, h,* where the chlorine must move in conformity with the direction of the oxygen at *d,* for it has to compensate the forces disturbed in its part of the circuit by the superior influence of those between the oxygen and zinc at *d, b,* aided as they are by those of the battery *a*; and for a similar reason the lead must move in the direction pointed out by the arrow head, that it may be in right relation to the first moving body of its own class, namely, the zinc *b.* If copper intervene in the circuit from *i* to *k,* it acts as the platina did before; and if another electrolyte, as the iodide of tin, occur at *l, m,* then the iodine *l,* being an *anion,* must move in conformity with the exciting *anion,* namely, the oxygen *d,* and the *cation* tin *m* move in correspondence with the other *cations b, e,* and *h,* that the chemical forces may be in equilibrium as to their direction and quantity throughout the circuit. Should it so happen that the anions in their circulation can combine with the metals at the *anodes* of the respective electrolytes, as would be the case at the platina *f* and the copper *k,* then those bodies becoming parts of electrolytes, under the influence of the current, immediately travel; but considering their relation to the zinc *b,* it is evidently impossible that they can

travel in any other direction than what will accord with its course, and therefore can never tend to pass otherwise than *from* the anode and *to* the cathode.

964. In such a circle as that delineated, therefore, all the known *anions* may be grouped within, and all the *cations* without. If any number of them enter as *ions* into the constitution of *electrolytes*, and, forming one circuit, are simultaneously subject to one common current, the anions must move in accordance with each other in one direction, and the cations in the other. Nay, more than that, equivalent portions of these bodies must so advance in opposite directions; for the advance of every 32·5 parts of the zinc *b* must be accompanied by a motion in the opposite direction of 8 parts of oxygen at *d*, of 36 parts of chlorine at *g*, of 126 parts of iodine at *l*; and in the same direction by electro-chemical equivalents of hydrogen, lead, copper and tin, at *e*, *h*, *k*, and *m*.

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965. If the present paper be accepted as a correct expression of facts, it will still only prove a confirmation of certain general views put forth by Sir Humphry Davy in his Bakerian Lecture for 1806*, and revised and re-stated by him in another Bakerian Lecture, on electrical and chemical changes, for the year 1826†. His general statement is, that "*chemical and electrical attractions were produced by the same cause, acting in one case on particles, in the other on masses, of matter; and that the same property, under different modifications, was the cause of all the phenomena exhibited by different voltaic combinations*‡." This statement I believe to be true; but in admitting and supporting it, I must guard myself from being supposed to assent to all that is associated with it in the two papers referred to, or as admitting the experiments which are there quoted as decided proofs of the truth of the principle. Had I thought them so, there would have been no occasion for this investigation. It may be supposed by some that I ought to go through these papers, distinguishing what I admit from what I reject, and giving good experimental or philosophical reasons for the judgement in both cases. But then I should be equally bound to review, for the same purpose, all that has been written both for and against the necessity of metallic contact,—for and against the origin of voltaic electricity in chemical action,—a duty which I may not undertake in the present paper §.

¶ ii. *On the Intensity necessary for Electrolyzation.*

966. It became requisite, for the comprehension of many of the conditions attending voltaic action, to determine positively, if possible, whether electrolytes could

* Philosophical Transactions, 1807. † Ibid. 1826, p. 383. ‡ Ibid. 1826, p. 389.

§ I at one time intended to introduce here, in the form of a note, a table of reference to the papers of the different philosophers who have referred the origin of the electricity in the voltaic pile to contact, or to chemical action, or to both; but on the publication of the first volume of M. Becquerel's highly important and valuable Traité de l'Electricité et du Magnétism, I thought it far better to refer to that work for these references, and the views held by the authors quoted. See pages 86, 91, 104, 110, 112, 117, 118, 120, 151, 152, 224, 227, 228, 232, 233, 252, 255, 257, 258, 290, &c.—July 3rd, 1834.

resist the action of an electric current if beneath a certain intensity? whether the intensity at which the current ceased to act would be the same for all bodies? and also whether the electrolytes thus resisting decomposition would conduct the electric current as a metal does, after they ceased to conduct as electrolytes, or would act as perfect insulators?

967. It was evident from the experiments described (904. 906.) that different bodies were decomposed with very different facilities, and apparently that they required for their decomposition currents of different intensities, resisting some, but giving way to others. But it was needful, by very careful and express experiments, to determine whether a current could really pass through, and yet not decompose an electrolyte (910.).

968. An arrangement (fig. 12.) was made, in which two glass vessels contained the same dilute sulphuric acid, sp. gr. 1·25. The plate *z* was amalgamated zinc, in connexion, by a platina wire *a*, with the platina plate *e*; *b* was a platina wire connecting the two platina plates P P′; *c* was a platina wire connected with the platina plate P″. On the plate *e* was placed a piece of paper moistened in solution of iodide of potassium: the wire *c* was so curved that its end could be made to rest at pleasure on this paper, and show, by the evolution of iodine there, whether a current was passing; or, being placed in the dotted position, it formed a direct communication with the platina plate *e*, and the electricity could pass without causing decomposition. The object was to produce a current by the action of the acid on the amalgamated zinc in the first vessel; to pass it through the acid in the second vessel by platina electrodes, that its power of decomposing water might, if existing, be observed; and to verify the existence of the current at pleasure, by decomposition at *e*, without involving the continual obstruction to the current which would arise from making the decomposition there constant. The experiment, being arranged, was examined, the existence of a current shown by the decomposition at *e*, and then left with the end of the wire *c* resting on the plate *e*, so as to form a constant metallic communication there.

969. After several hours, the end of the wire *c* was replaced on the test paper at *e*: decomposition occurred, and *the proof* of a passing current was therefore complete. The current was very feeble compared to what it had been at the beginning of the experiment, because of a peculiar state acquired by the metal surfaces in the second vessel, which caused them to oppose the passing current by a force which they possess under these circumstances (1040.). Still it was proved, by the decomposition, that this state of the plates in the second vessel was not able entirely to stop the current determined in the first, and that was all that was needful to be ascertained in the present inquiry.

970. This apparatus was examined from time to time, and an electric current always found circulating through it, until twelve days had elapsed, during which the water in the second vessel had been constantly subject to its action. Notwithstand-

ing this lengthened period, not the slightest appearance of a bubble upon either of the plates in that vessel occurred. From the results of the experiment, I conclude that a current *had* passed, but of so low an intensity as to fall beneath that degree at which the elements of water, unaided by any secondary force resulting from the capability of combination with the matter of the electrodes, or of the liquid surrounding them, separated from each other.

971. It may be supposed, that the oxygen and hydrogen had been evolved in such small quantities as to have entirely dissolved in the water, and finally to have escaped at the surface, or to have reunited into water. That the hydrogen can be so dissolved was shown in the first vessel; for after several days minute bubbles of gas gradually appeared upon a glass rod, inserted to retain the zinc and platina apart, and also upon the platina plate itself, and these were hydrogen. They resulted in this way. Notwithstanding the amalgamation of the zinc, the acid exerted a little direct action upon it, so that a small stream of hydrogen bubbles was continually rising from its surface; a little of this hydrogen gradually dissolved in the dilute acid, and was in part set free against the surfaces of the rod and the plate, according to the well known action of such solid bodies in solutions of gases (623. &c.).

972. But if the gases had been evolved in the second vessel by the decomposition of water, and had tended to dissolve, still there would have been every reason to expect that a few bubbles should have appeared on the electrodes, especially on the negative one, if it were only because of its action as a nucleus on the solution supposed to be formed; but none appeared even after twelve days.

973. When a few drops only of nitric acid were added to the vessel A, fig. 12., then the results were altogether different. In less than five minutes bubbles of gas appeared on the plates P′ and P″ in the second vessel. To prove that this was the effect of the electric current (which by trial at *e* was found at the same time to be passing,) the connexion at *e* was broken, the plates P′ P″ cleared from bubbles and left in the acid of the vessel B, for fifteen minutes: during that time no bubbles appeared upon them; but on restoring the communication at *e*, a minute did not elapse before gas appeared in bubbles upon the plates. The proof, therefore, is most full and complete, that the current excited by dilute sulphuric acid with a little nitric acid in vessel A, has intensity enough to overcome the chemical affinity exerted between the oxygen and hydrogen of the water in the vessel B, whilst that excited by dilute sulphuric acid alone has *not* sufficient intensity.

974. On using a strong solution of caustic potassa in the vessel A, to excite the current, it was found by the decomposing effects at *e*, that the current passed. But it had not intensity enough to decompose the water in the vessel B; for though left for fourteen days, during the whole of which time the current was found to be passing, still not the slightest appearance of gas appeared on the plates P′ P″, nor any other signs of the water having suffered decomposition.

975. Sulphate of soda in solution was then experimented with, for the purpose of

ascertaining with respect to it, whether a certain electrolytic intensity was also required for its decomposition in this state, in analogy with the result established with regard to water (974.). The apparatus was arranged as in fig. 13.; P and Z are the platina and zinc plates dipping into a solution of common salt; *a* and *b* are platina plates connected by wires of platina (except in the galvanometer *g*) with P and Z; *c* is a connecting wire of platina, the ends of which can be made to rest either on the plates *a*, *b*, or on the papers moistened in solutions which are placed upon them; so that the passage of the current without decomposition, or with one or two decompositions, was under ready command, as far as arrangement was concerned. In order to change the *anodes* and *cathodes* at the places of decomposition, the form of apparatus fig. 14. was occasionally adopted. Here only one platina plate, *c*, was used; both pieces of paper on which decomposition was to be effected were placed upon it, the wires from P and Z resting upon these pieces of paper, or upon the plate *c*, according as the current with or without decomposition of the solutions was required.

976. On placing solution of iodide of potassium in paper at one of the decomposing localities, and solution of sulphate of soda at the other, so that the electric current should pass through both at once, the solution of iodide was slowly decomposed, yielding iodine at the *anode* and alkali at the *cathode*; but the solution of sulphate of soda exhibited no signs of decomposition, neither acid nor alkali being evolved from it. On placing the wires so that the iodide alone was subject to the action of the current (900.), it was quickly and powerfully decomposed; but on arranging them so that the sulphate of soda alone was subject to action, it still refused to yield up its elements. Finally, the apparatus was so arranged under a wet bell-glass, that it could be left for twelve hours, the current passing during the whole time through a solution of sulphate of soda, retained in its place by only two thicknesses of bibulous litmus and turmeric paper. At the end of that time it was ascertained by the decomposition of iodide of potassium at the second place of action, that the current was passing and had passed for the twelve hours, and yet no trace of acid or alkali from the sulphate of soda appeared.

977. From these experiments it may, I think, be concluded, that a solution of sulphate of soda can conduct a current of electricity, which is unable to decompose the neutral salt present; that this salt in the state of solution, like water, requires a certain electrolytic intensity for its decomposition; and that the necessary intensity is much higher for this substance than for the iodide of potassium in a similar state of solution.

978. I then experimented on bodies rendered decomposable by fusion, and first on *chloride of lead*. The current was excited by dilute sulphuric acid without any nitric acid between zinc and platina plates, fig. 15., and was then made to traverse a little chloride of lead fused upon glass at *a*, a paper moistened in solution of iodide of potassium at *b*, and a galvanometer at *g*. The metallic terminations at *a* and *b* were of platina. Being thus arranged, the decomposition at *b* and the deflection at *g*

showed that an electric current was passing, but there was no appearance of decomposition at *a*, not even after a *metallic* communication at *b* was established. The experiment was repeated several times, and I am led to conclude that in this case the current has not intensity sufficient to cause the decomposition of the chloride of lead; and further, that, like water (974.), fused chloride of lead can conduct an electric current having an intensity below that required to effect decomposition.

979. *Chloride of silver* was then placed at *a*, fig. 15., instead of chloride of lead. There was a very ready decomposition of the solution of iodide of potassium at *b*, and when metallic contact was made there, very considerable deflection of the galvanometer needle at *g*. Platina also appeared to be dissolved at the anode of the fused chloride at *a*, and there was every appearance of a decomposition having been effected there.

980. A further proof of decomposition was obtained in the following manner. The platina wires in the fused chloride at *a* were brought very near together (metallic contact having been established at *b*), and left so; the deflection at the galvanometer indicated the passage of a current, feeble in its force, but constant. After a minute or two, however, the needle would suddenly be violently affected, and indicate a current as strong as if metallic contact had taken place at *a*. This I actually found to be the case, for the silver reduced by the action of the current crystallized in long delicate spiculæ, and these at last completed the metallic communication; and at the same time that they transmitted a more powerful current than the fused chloride, they proved that electro-chemical decomposition of that chloride had been going on. Hence it appears, that the current excited by dilute sulphuric acid between zinc and platina, has an intensity above that required to electrolyze the fused chloride of silver when placed between platina electrodes, although it has not intensity enough to decompose chloride of lead under the same circumstances.

981. A drop of *water* placed at *a* instead of the fused chlorides, showed as in the former case (970.), that it could conduct a current unable to decompose it, for decomposition of the solution of iodide at *b* occurred after some time. But its conducting power was much below that of the fused chloride of lead (978.).

982. Fused *nitre* at *a* conducted much better than water: I was unable to decide with certainty whether it was electrolyzed, but I incline to think not, for there was no discoloration against the platina at the *cathode*. If sulpho-nitric acid had been used in the exciting vessel, both the nitre and the chloride of lead would have suffered decomposition like the water (906.).

983. The results thus supplied of conduction without decomposition, and the necessity of a certain electrolytic intensity for the separation of the *ions* of different electrolytes, are immediately connected with the experiments and results given in § 10. of the Fourth Series of these Researches (418. 423. 444. 449.). But it will require a more exact knowledge of the nature of intensity, both as regards the first origin of the electric current, and also the manner in which it may be reduced,

or lowered by the intervention of larger or smaller portions of bad conductors, whether decomposable or not, before their relation can be minutely and fully understood.

984. In the case of water, the experiments I have as yet made, appear to show, that, when the electric current is reduced in intensity below the point required for decomposition, then the degree of conduction is the same whether sulphuric acid, or any other of the many bodies which can affect its transferring power as an electrolyte, are present or not. Or, in other words, that the necessary electrolytic intensity for water is the same whether it be pure, or rendered a better conductor by the addition of these substances; and that for currents of less intensity than this, the water, whether pure or acidulated, has equal conducting power. An apparatus, fig. 12, was arranged with dilute sulphuric acid in the vessel A, and pure distilled water in the vessel B. By the decomposition at *e*, it appeared as if water was a *better* conductor than dilute sulphuric acid for a current of such low intensity as to cause no decomposition. I am inclined, however, to attribute this apparent superiority of water to variations in that peculiar condition of the platina electrodes which is referred to further on in this Series (1040.), and which is assumed, as far as I can judge, to a greater degree in dilute sulphuric acid than in pure water. The power, therefore, of acids, alkalies, salts, and other bodies in solution, to increase conducting power, appears to hold good only in those cases where the electrolyte subject to the current suffers decomposition, and loses all influence when the current transmitted has too low an intensity to effect chemical change. It is probable that the ordinary conducting power of an electrolyte in the solid state (419.) is the same as that which it possesses in the fluid state for currents under the due electrolytic intensity.

985. Currents of electricity, produced by less than eight or ten series of voltaic elements, can be reduced to that intensity at which water can conduct them without suffering decomposition, by causing them to pass through three or four vessels in which water shall be successively interposed between platina surfaces. The principles of interference upon which this effect depends, will be described hereafter (1009. 1018.), but the effect may be useful in obtaining currents of standard intensity, and is probably applicable to batteries of any number of pairs of plates.

986. As there appears every reason to expect that all electrolytes will be found subject to the law which requires an electric current of a certain intensity for their decomposition, but that they will differ from each other in the degree of intensity required, it will be desirable hereafter to arrange them in a table, in the order of their electrolytic intensities. Investigations on this point must, however, be very much extended, and include many more bodies than have been here mentioned before such a table can be constructed. It will be especially needful in such experiments, to describe the nature of the electrodes used, or, if possible, to select such as, like platina or plumbago in certain cases, shall have no power of assisting the separation of the *ions* to be evolved (913.).

987. Of the two modes in which bodies can transmit the electric forces, namely, that which is so characteristically exhibited by the metals, and that in which it is accompanied by decomposition, the first appears common to all bodies, although it occurs with almost infinite degrees of difference ; the second is at present distinctive of the electrolytes. It is, however, just possible that it may hereafter be extended to the metals ; for their power of conducting without decomposition may, perhaps justly, be ascribed to their requiring a very high electrolytic intensity for their decomposition.

987½. The establishment of a certain electrolytic intensity being necessary before decomposition can be effected, is of great importance in all those considerations which arise regarding the probable effects of weak currents, such for instance as those produced by natural thermo-electricity, or natural voltaic arrangements. For to produce an effect of decomposition or of combination, a current must not only exist, but have a certain intensity before it can overcome the quiescent affinities opposed to it, otherwise it will be conducted, producing no permanent effects. On the other hand, the principles are also now evident by which an opposing action can be so weakened by the juxtaposition of bodies not having quite affinity enough to cause direct action between them (913.), that a very weak current shall be able to raise the sum of actions sufficiently high, and cause chemical changes to occur.

988. In concluding this division *on the intensity necessary for electrolyzation,* I cannot resist pointing out the following remarkable conclusion in relation to intensity generally. It would appear that when a voltaic current is produced, having a certain intensity, dependent upon the strength of the chemical affinities by which that current is excited (916.), it can decompose a particular electrolyte without relation to the quantity of electricity passed, the *intensity* deciding whether the electrolyte shall give way or not. If that conclusion be confirmed, then we may arrange circumstances so that the *same quantity* of electricity may pass in the *same time,* in at the *same surface,* into the *same decomposing body in the same state,* and yet differ in intensity, *decomposing in one case and in the other not.* For taking a source of too low an intensity to decompose, and ascertaining the quantity passed in a given time, it is easy to take another source having a sufficient intensity, and reducing the quantity of electricity from it by the intervention of bad conductors to the same proportion as the former current, and then all the conditions will be fulfilled to produce the result described.

¶ iii. *On associated Voltaic circles, or the Voltaic battery.*

989. Passing from the consideration of single circles (875. &c.) to their association in the voltaic battery, it is a very evident consequence, that if matters are so arranged that two sets of affinities, in place of being opposed to each other as in figg. 1, 4. (880. 891.), are made to act in conformity, then, instead of either interfering with the

other, it will rather assist it. This is simply the case of two voltaic pairs of metals arranged so as to form one circuit. In such arrangements the activity of the whole is known to be increased, and when ten, or a hundred, or any larger number of such alternations are placed in conformable association with each other, the power of the whole becomes proportionably exalted, and we obtain that magnificent instrument of philosophic research, the *voltaic battery*.

990. But it is evident from the principles of definite action already laid down, that the *quantity* of electricity in the current cannot be increased with the increase of the *quantity of metal* oxidized and dissolved at each new place of chemical action. A single pair of zinc and platina plates throws as much electricity into the form of a current, by the oxidation of 32·5 grains of the zinc (868.) as would be given by the same alteration of a thousand times that quantity, or nearly five pounds of metal oxidized at the surface of the zinc plates of a thousand pairs placed in regular battery order. For it is evident, that the electricity which passes across the acid from the zinc to the platina in the first cell, and which has been associated with, or even originated by, the decomposition of a definite portion of water in that cell, cannot pass from the zinc to the platina across the acid in the second cell, without the decomposition of the same quantity of water there, and the oxidation of the same quantity of zinc by it (924. 949.). The same result recurs in every other cell; the electro-chemical equivalent of water must be decomposed in each, before the current can pass through it; for the quantity of electricity passed, and the quantity of electrolyte decomposed, *must* be the equivalents of each other. The action in each cell, therefore, is not to increase the quantity set in motion in any one cell, but to aid in urging forward that quantity, the passing of which is consistent with the oxidation of its own zinc; and in this way it exalts that peculiar property of the current which we endeavour to express by the term *intensity*, without increasing the *quantity* beyond that which is proportionate to the quantity of zinc oxidized in any single cell of the series.

991. To prove this, I arranged ten pairs of amalgamated zinc and platina plates with dilute sulphuric acid in the form of a battery. On completing the circuit, all the pairs acted and evolved gas at the surfaces of the platina. This was collected and found to be alike in quantity for each plate; and the quantity of hydrogen evolved at any one platina plate was in the same proportion to the quantity of metal dissolved from any one zinc plate, as was given in the experiment with a single pair (864. &c.). It was therefore certain, that, just as much electricity and no more had passed through the series of ten pair of plates as had passed through, or would have been put into motion by, any single pair, notwithstanding that ten times the quantity of zinc had been consumed.

992. This truth has been proved also long ago in another way, by the action of the evolved current on a magnetic needle; the deflecting power of one pair of plates in a battery being equal to the deflecting power of the whole, provided the wires used be sufficiently large to carry the current of the single pair freely; but the *cause*

of this equality of action could not be understood whilst the definite action and evolution of electricity (783. 869.) remained unknown.

993. The superior decomposing power of a battery over a single pair of plates is rendered evident in two ways. Electrolytes held together by an affinity so strong as to resist the action of the current from a single pair, yield up their elements to the current excited by many pairs; and that body which is decomposed by the action of one or of few pairs of metals, &c., is resolved into its *ions* the more readily as it is acted upon by electricity urged forward by many alternations.

994. Both these effects are, I think, easily understood. Whatever *intensity* may be, (and that must of course depend upon the nature of electricity, whether it consist of a fluid or fluids, or of vibrations of an ether, or any other kind or condition of matter,) there seems to be no difficulty in comprehending that the *degree* of intensity at which a current of electricity is evolved by a first voltaic element, shall be increased when that current is subjected to the action of a second voltaic element, acting in conformity and possessing equal powers with the first: and as the decompositions are merely opposed actions, but exactly of the same kind as those which generate the current (917.), it seems to be a natural consequence, that the affinity which can resist the force of a single decomposing action shall be unable to oppose the energies of many decomposing actions, operating conjointly, as in the voltaic battery.

995. That a body which can give way to a current of feeble intensity should give way more freely to one of stronger force, and yet involve no contradiction to the law of definite electrolytic action, is perfectly consistent. All the facts and also the theory I have ventured to put forth, tend to show that the act of decomposition opposes a certain force to the passage of the electric current; and that this obstruction should be overcome more or less readily, in proportion to the greater or less intensity of the decomposing current, is in perfect consistency with all our notions of the electric agent.

996. I have elsewhere (947.) distinguished the chemical action of zinc and dilute sulphuric acid into two portions; that which, acting effectually on the zinc, evolves hydrogen at once upon its surface, and that which, producing an arrangement of the chemical forces throughout the electrolyte present, (in this case water,) tends to take oxygen from it, but cannot do so unless the electric current consequent thereon can have free passage, and the hydrogen be delivered elsewhere than against the zinc. The electric current depends altogether upon the second of these; but when the current can pass, by favouring the electrolytic action it tends to diminish the former and increase the latter portion.

997. It is evident, therefore, that when ordinary zinc is used in a voltaic arrangement, there is an enormous waste of that power which it is the object to throw into the form of an electric current; a consequence which is put in its strongest point of view when it is considered that three ounces and a half of zinc, properly oxydized, can circulate enough electricity to decompose nearly one ounce of water, and cause

the evolution of about 2400 cubic inches of hydrogen gas. This loss of power not only takes place during the time the electrodes of the battery are in communication, being then proportionate to the quantity of hydrogen evolved against the surface of any one of the zinc plates, but includes also *all* the chemical action which goes on when the extremities of the pile are not in communication.

998. This loss is far greater with ordinary zinc than with the pure metal, as M. De la Rive has shown *. The cause is, that when ordinary zinc is acted upon by dilute sulphuric acid, portions of copper, lead, cadmium, or other metals which it may contain, are set free upon its surface; and these, being in contact with the zinc, form small but very active voltaic circles, which cause great destruction of the zinc and evolution of hydrogen, apparently upon the zinc surface, but really upon the surface of these accidental metals. In the same proportion as they serve to discharge or convey the electricity back to the zinc, do they diminish its power of producing an electric current which shall extend to a greater distance across the acid, and be discharged only through the copper or platina plate which is associated with it for the purpose of forming a voltaic apparatus.

999. All these evils are removed by the employment of an amalgam of zinc in the manner recommended by Mr. Kemp †, or the use of the amalgamated zinc plates of Mr. Sturgeon (863.), who has himself suggested and objected to their application in galvanic batteries; for he says, "Were it not on account of the brittleness and other inconveniences occasioned by the incorporation of the mercury with the zinc, amalgamation of the zinc surfaces in galvanic batteries would become an important improvement; for the metal would last much longer, and remain bright for a considerable time, even for several successive hours; essential considerations in the employment of this apparatus ‡."

1000. Zinc so prepared, even though impure, does not sensibly decompose the water of dilute sulphuric acid, but still has such affinity for the oxygen, that the moment a metal which, like copper or platina, has little or no affinity, touches it in the acid, action ensues, and a powerful and abundant electric current is produced. It is probable that the mercury acts by bringing the surface, in consequence of its fluidity, into one uniform condition, and preventing those differences in character between one spot and another which are necessary for the formation of the minute voltaic circuits referred to (998.). If any difference does exist at the first moment, with regard to the proportion of zinc and mercury, at one spot on the *surface*, as compared with another, that spot having the least mercury is first acted on, and, by solution of the zinc, is soon placed in the same condition as the other parts, and the

* Quarterly Journal of Science, 1831, p. 388; or Bibliotheque Universelle, 1830, p. 391.

† Jameson's Edinburgh Journal, October 1828.

‡ Recent Experimental Researches, p. 42, &c. Mr. Sturgeon is of course unaware of the definite production of electricity by chemical action, and is in fact quoting the experiment as the strongest argument *against* the chemical theory of galvanism.

whole plate rendered superficially uniform. One part cannot, therefore, act as a discharger to another; and hence *all* the chemical power upon the water at its surface is in that equable condition (949.), which, though it tends to produce an electric current through the liquid to another plate of metal which can act as a discharger (950.), presents no irregularities by which any one part, having weaker affinities for oxygen, can act as a discharger to another. Two excellent and important consequences follow upon this state of the metal. The first is, that the *full equivalent* of electricity is obtained for the oxidation of a certain quantity of zinc; the second, that a battery constructed with the zinc so prepared, and charged with dilute sulphuric acid, is active only whilst the electrodes are connected, and ceases to act or be acted upon by the acid the instant the communication is broken.

1001. I have had a small battery of ten pairs of plates thus constructed, and am convinced that arrangements of this kind will be very important, especially in the development and illustration of the philosophical principles of the instrument. The metals I have used are amalgamated zinc and platina, connected together by being soldered to platina wires, the whole apparatus having the form of the couronne des tasses. The liquid used was dilute sulphuric acid of sp. gr. 1·25. No action took place upon the metals except when the electrodes were in communication, and then the action upon the zinc was only in proportion to the decomposition in the experimental cell; for when the current was retarded there, it was retarded also in the battery, and no waste of the powers of the metal was incurred.

1002. In consequence of this circumstance, the acid in the cells remained active for a very much longer time than usual. In fact, time did not tend to lower it in any sensible degree; for whilst the metal was preserved to be acted upon at the proper moment, the acid also was preserved almost at its first strength. Hence a constancy of action far beyond what can be obtained with the use of common zinc.

1003. Another excellent consequence was the renewal, during the interval of rest, between two experiments of the first and most efficient state. When an amalgamated zinc and a platina plate, immersed in dilute sulphuric acid, are first connected, the current is very powerful, but instantly sinks very much in force, and in some cases actually falls to only an eighth or a tenth of that first produced (1036.). This is due to the acid which is in contact with the zinc becoming neutralized by the oxide formed; the continued quick oxidation of the metal being thus prevented. With ordinary zinc, the evolution of gas at its surface tends to mingle all the liquid together, and thus bring fresh acid against the metal, by which the oxide formed there can be removed. With the amalgamated zinc battery, at every cessation of the current, the saline solution against the zinc is gradually diffused amongst the rest of the liquid; and upon the renewal of the contact with the electrodes, the zinc plates are found most favourably circumstanced for the production of a ready and powerful current.

1004. It might at first be imagined that amalgamated zinc would be much inferior in force to common zinc, because of the lowering of its energy, which the mercury might be supposed to occasion over the whole of its surface; but this is not the case. When the electric currents of two pairs of platina and zinc plates were opposed, the difference being that one of the zincs was amalgamated and the other not, the current from the amalgamated zinc was most powerful, although no gas was evolved against it, and much was evolved at the surface of the unamalgamated metal. Again, as Davy has shown*, if amalgamated and unamalgamated zinc be put in contact, and dipped into dilute sulphuric acid, or other exciting fluids, the former is positive to the latter, i. e. the current passes from the amalgamated zinc, through the fluid, to the unprepared zinc. This he accounts for by supposing that "there is not any inherent and specific property in each metal which gives it the electrical character, but that it depends upon its peculiar state—on that form of aggregation which fits it for chemical change."

1005. The superiority of the amalgamated zinc is not, however, due to any such cause, but is a very simple consequence of the state of the fluid in contact with it; for as the unprepared zinc acts directly and alone upon the fluid, whilst that which is amalgamated does not, the former (by the oxide it produces) quickly neutralizes the acid in contact with its surface, so that the progress of oxidation is retarded, whilst, at the surface of the amalgamated zinc, any oxide formed is instantly removed by the free acid present, and the clean metallic surface is always ready to act with full energy upon the water. Hence its superiority (1037.).

1006. The progress of improvement in the voltaic battery and its applications, is evidently in the contrary direction at present to what it was a few years ago; for in place of increasing the number of plates, the strength of acid, and the extent altogether of the instrument, the change is rather towards its first state of simplicity, but with a far more intimate knowledge and application of the principles which govern its force and action. Effects of decomposition can now be obtained with ten pairs of plates (417.), which required five hundred or a thousand pairs for their production in the first instance. The capability of decomposing fused chlorides, iodides, and other compounds, according to the law before established (380. &c.), and the opportunity of collecting certain of the products, without any loss, by the use of apparatus of the nature of those already described (789. 814. &c.), render it probable that the voltaic battery may become a useful and even economical manufacturing instrument; for theory evidently indicates that an equivalent of a rare substance may be obtained at the expense of three or four equivalents of a very common body, namely, zinc: and practice seems thus far to justify the expectation. In this point of view I think it very likely that plates of platina or silver may be used instead of plates of copper with advantage, and that then the evil arising occasionally from solution of the copper, and its precipitation on the zinc, (by which the electro-motive power of the zinc is so much injured,) will be avoided (1047.).

* Philosophical Transactions, 1826, p. 405.

¶ iv. *On the Resistance of an Electrolyte to Electrolytic Action, and on Interpositions.*

1007. I have already illustrated, in the simplest possible form of experiment (891. 910.), the resistance established at the place of decomposition to the force active at the exciting place. I purpose examining the effects of this resistance more generally; but it is rather with reference to their practical interference with the action and phenomena of the voltaic battery, than with any intention at this time to offer a strict and philosophical account of their nature. Their general and principal cause is the resistance of the chemical affinities to be overcome; but there are numerous other circumstances which have a joint influence with these forces (1034. 1040. &c.), each of which would require a minute examination before a correct account of the whole could be given.

1008. As it will be convenient to describe the experiments in a form different to that in which they were made, both forms shall first be explained. Plates of platina, copper, zinc, and other metals, about three quarters of an inch wide and three inches long, were associated together in pairs by means of platina wires to which they were soldered, fig. 16, the plates of one pair being either alike or different, as might be required. These were arranged in glasses, fig. 17, so as to form Volta's crown of cups. The acid or fluid in the cups never covered the whole of any plate; and occasionally small glass rods were put into the cups, between the plates, to prevent their contact. Single plates were used to terminate the series and complete the connexion with a galvanometer, or with a decomposing apparatus (899. 968. &c.), or both. Now if fig. 18 be examined and compared with fig. 19, the latter may be admitted as representing the former in its simplest condition; for the cups i, ii, and iii of the former, with their contents, are represented by the cells i, ii, and iii of the latter, and the metal plates Z and P of the former by the similar plates represented Z and P in the latter. The only difference, in fact, between the apparatus, fig. 18, and the trough represented fig. 19, is that twice the quantity of surface of contact between the metal and acid is allowed in the first to what would occur in the second.

1009. When the extreme plates of the arrangement just described, fig. 18, are connected metallically through the galvanometer *g*, then the whole represents a battery consisting of two pairs of zinc and platina plates urging a current forward, which has, however, to decompose water unassisted by any direct chemical affinity before it can be transmitted across the cell iii, and therefore before it can circulate. This decomposition of water, which is opposed to the passage of the current, may as a matter of convenience be considered as taking place either against the surfaces of the two platina plates which constitute the electrodes in the cell iii, or against the two surfaces of that platina plate which separates the cells ii and iii, fig. 19, from each other. It is evident that if that plate were away, the battery would consist of two pairs of plates and two cells, arranged in the most favourable position for the production of a current. The platina plate therefore, which being introduced as at *x*, has oxygen evolved at one surface and hydrogen at the other (that is, if the decomposing current passes), may be considered as the cause of any obstruction arising from the decomposition of water by the electrolytic action of the current; and I have usually called it the interposed plate.

1010. In order to simplify the conditions, dilute sulphuric acid was first used in all the cells, and platina for the interposed plates; for then the initial intensity of the current which tends to be formed is constant, being due to the power which zinc has of decomposing water; and the opposing force of decomposition is also constant, the elements of the water being unassisted in their separation at the interposed plates by any affinity or secondary action at the electrodes (744.), arising either from the nature of the plate itself or the surrounding fluid.

1011. When only one voltaic pair of zinc and platina plates were used, the current of electricity was entirely stopped to all practical purposes by interposing one platina plate, fig. 20, i. e. by requiring of the current that it should decompose water, and evolve both its elements, before it should pass. This consequence is in perfect accordance with the views before given (910. 917. 973.). For as the whole result depends upon the opposition of forces at the places of electric excitement and electro-decomposition, and as water is the substance to be decomposed at both before the current can move, it is not to be expected that the zinc should have such powerful attraction for the oxygen, as not only to be able to take it from its associated hydrogen, but leave such a surplus of force as, passing to the second place of decomposition, should be there able to effect a second separation of the elements of water. Such an effect would require that the force of attraction between zinc and oxygen should under the circumstances be *at least* twice as great as the force of attraction between the oxygen and hydrogen.

1012. When two pairs of zinc and platina exciting plates were used, the current was also practically stopped by one interposed platina plate, fig. 21. There was a very feeble effect of a current at first, but it ceased almost immediately. It will be referred to, with many other similar effects, hereafter (1017.).

1013. Three pairs of zinc and platina plates, fig. 22, were able to produce a current which could pass an interposed platina plate, and effect the electrolyzation of water in cell iv. The current was evident, both by the continued deflexion of the galvanometer, and the production of bubbles of oxygen and hydrogen at the electrodes in cell iv. Hence the accumulated surplus force of these plates of zinc, which are active in decomposing water, is more than equal, when added together, to the force with which oxygen and hydrogen are combined in water, and is sufficient to cause the separation of these elements from each other.

1014. The three pairs of zinc and platina plates were now opposed by two intervening platina plates, fig. 23. In this case the current was stopped.

1015. Four pairs of zinc and platina plates were also neutralized by two interposed platina plates, fig. 24.

1016. Five pairs of zinc and platina, with two interposed platina plates, fig. 25, gave a feeble current; there was permanent deflexion at the galvanometer, and decomposition in the cells vi and vii. But the current was very feeble; very much less than when all the intermediate plates were removed and the two extreme ones only retained; for when they were placed six inches asunder in one cell, they gave a powerful current. Hence five exciting pairs, with two interposed obstructing plates, do not give a current at all comparable to that of a single unobstructed pair.

1017. I have already said that a *very feeble current* passed when the series included one interposed platina and two pairs of zinc and platina plates (1012.). A similarly feeble current passed in every case, and even when only one exciting pair and four intervening platina plates were used, fig. 26, a current passed which could be detected at x, both by chemical action on the solution of iodide of potassium, and by the galvanometer. This current I believe to be due to electricity reduced in intensity below the point requisite for the decomposition of water (970. 984.); for water can conduct electricity of such low intensity by the same kind of power which it possesses in common with metals and charcoal, though it cannot conduct electricity of higher intensity without suffering decomposition, and then opposing a new force consequent thereon. With an electric current under this intensity, it is probable that increasing the number of interposed platina plates would not involve an increased difficulty of conduction.

1018. In order to obtain an idea of the additional interfering power of each added platina plate, six voltaic pairs and four intervening platinas were arranged as in fig. 27; a very feeble current then passed (985. 1017.). When one of the platinas was removed so that three intervened, a current somewhat stronger passed. With two intervening platinas a still stronger current passed; and with only one intervening platina a very fair current was obtained. But the effect of the successive plates, taken in the order of their interposition, was very different, as might be expected; for the first retarded the current more powerfully than the second, and the second more than the third.

1019. In these experiments both amalgamated and unamalgamated zinc were used, but the results generally were the same.

1020. The effects of retardation just described were altered altogether when changes were made in the *nature of the liquid* used between the plates, either in what may be called the *exciting* or the *retarding* cells. Thus, retaining the exciting force the same, by still using pure dilute sulphuric acid for that purpose, if a little nitric acid were added to the liquid in the *retarding* cells, then the transmission of the current was very much facilitated. For instance, in the experiment with one pair of exciting plates and one intervening plate (1011.), fig. 20, when a few drops of nitric acid were added to the contents of cell ii, then the current of electricity passed with considerable strength (though it soon fell from other causes (1036. 1040.),) and the same good effect was produced by the nitric acid when many interposed plates were used.

1021. This seems to be a consequence of the diminution of the difficulty of decomposing water when its hydrogen, as in these cases, instead of being absolutely expelled, is transferred to the oxygen of the nitric acid, producing a secondary result at the *cathode* (752.); for in accordance with the chemical views of the electric current and its action already advanced (913.), the water, instead of opposing a resistance to decomposition equal to the full amount of the force of mutual attraction between its oxygen and hydrogen, has that force counteracted in part, and therefore diminished by the attraction of the hydrogen at the *cathode* for the oxygen of the nitric acid which surrounds it, and with which it ultimately combines instead of being rendered in its free and independent state.

1022. When a little nitric acid was put into the exciting cells, then again the circumstances favouring the transmission of the current were strengthened, for the *intensity* of the current itself was increased by the addition (906.). When therefore a little nitric acid was added to both the *exciting* and the *retarding* cells, the current of electricity passed with very considerable freedom.

1023. When dilute muriatic acid was used, it produced and transmitted a current more easily than pure dilute sulphuric acid, but could not compete with nitric acid. As muriatic acid appears to decompose more freely than water (765.), and as the affinity of zinc for chlorine is very powerful, it might be expected to produce a current more intense than that from the use of dilute sulphuric acid; and also to transmit it more freely by undergoing decomposition at a lower intensity (912.).

1024. In relation to the effect of these interpositions, it is necessary to state that they do not appear to be at all dependent upon the size of the electrodes, or their distance from each other in the acid, except that when a current *can pass*, changes in these facilitate or retard its passage. For on repeating the experiment with one intervening and one pair of exciting plates (1011.), fig. 20, and in place of the interposed plate P using sometimes a mere wire, and sometimes very large plates (1008.), and also changing the terminal exciting plates Z and P, so that they were sometimes wires only and at others of great size, still the results were the same as those already obtained.

1025. In illustration of the effect of distance, an experiment like that described with two exciting pairs and one intervening plate (1012), fig. 21, was arranged so that the distance between the plates in the third cell could be increased to six or eight inches, or diminished to the thickness of a piece of intervening bibulous paper. Still the result was the same in both cases, the effect being no greater, sensibly, when the plates were merely separated by the paper, than when a great way apart; so that the principal opposition to the current does not depend upon the *quantity* of intervening electrolytic conductor, but on the *relation of its elements to the intensity of the current*, or to the chemical nature of the electrodes and the surrounding fluids.

1026. When the acid was sulphuric acid, *increasing its strength* in any of the cells, caused no change in the effects; it did not produce a more intense current in the exciting cells (908.), or cause the current produced to traverse the decomposing cells more freely. But if to very weak sulphuric acid a few drops of nitric acid were added, then either one or other of those effects could be produced; and, as might be expected in a case like this, where the exciting or conducting action bore a *direct* reference to the acid itself, increasing the strength of this (the nitric acid), also increased its powers.

1027. The *nature of the interposed plate* was now varied to show its relation to the phenomena either of excitation or retardation, and amalgamated zinc was first substituted for platina. On employing one voltaic pair and one interposed zinc plate, fig. 28, there was as powerful a current, apparently, as if the interposed zinc plate was away. Hydrogen was evolved against P in cell ii, and against the side of the second zinc in cell i; but no gas appeared against the side of the zinc in cell ii, nor against the zinc in cell i.

1028. On interposing two amalgamated zinc plates, fig. 29, instead of one, there was still a powerful current, but interference had taken place. On using three intermediate zinc plates, fig. 30, there was still further retardation, though a good current of electricity passed.

1029. Considering the retardation as due to the inaction of the amalgamated zinc upon the dilute acid, in consequence of the slight though general effect of diminished chemical power produced by the mercury on the surface, and viewing this inaction as the circumstance which rendered it necessary that each plate should have its tendency to decompose water assisted slightly by the electric current, it was expected that plates of the metal in the unamalgamated state would probably not require such assistance, and would offer no sensible impediment to the passing of the current. This expectation was fully realized in the use of two and three interposed unamalgamated plates. The electric current passed through them as freely as if there had been no such plates in the way. They offered no obstacle, because they could decompose water without the current; and the latter had only to give direction to a part of the forces, which would have been active whether it had passed or not.

1030. Interposed plates of copper were then employed. These seemed at first to occasion no obstruction, but after a few minutes the current almost entirely ceased. This effect appears due to the surfaces taking up that peculiar condition (1040.) by which they tend to produce a reverse current; for when one or more of the plates were turned round, which could easily be effected with the couronne des tasses form of experiment, fig. 18, then the current was powerfully renewed for a few moments, and then again ceased. Plates of platina and copper, arranged as a voltaic pile with dilute sulphuric acid, could not form a voltaic trough competent to act for more than a few minutes, because of this peculiar counteracting effect.

1031. All these effects of retardation, exhibited by decomposition against surfaces for which the evolved elements have more or less affinity, or are altogether deficient in attraction, show generally, though beautifully, the chemical relations and source of the current, and also the balanced state of the affinities at the places of excitation and decomposition. In this way they add to the mass of evidence in favour of the identity of the two; for they demonstrate, as it were, the antagonism of the *chemical powers* at the electromotive part with the *chemical powers* at the interposed parts; they show that the first are *producing* electric effects, and the second *opposing* them; they bring the two into direct relation; they prove that either can determine the other, thus making what appears to be cause and effect convertible, and thereby demonstrating that both chemical and electrical action are merely two exhibitions of one single agent or power (916. &c.).

1032. It is quite evident that as water and other electrolytes can conduct electricity without suffering decomposition (986.), when the electricity is of sufficiently low intensity, it may not be asserted as absolutely true in all cases, that whenever electricity passes through an electrolyte, it produces a definite effect of decomposition. But the quantity of electricity which can pass in a given time through an electrolyte without causing decomposition, is so small as to bear no comparison to that required in a case of very moderate decomposition; and with electricity above the intensity required for decomposition, I have found no sensible departure as yet from the law of *definite electrolytic action* developed in the preceding series of these Researches (783. &c.).

1033. I cannot dismiss this division of the present Paper without making a reference to the important experiments of M. Aug. De la Rive on the effects of interposed plates*. As I have had occasion to consider such plates merely as giving rise to new decompositions, and in that way only, causing obstruction to the passage of the electric current, I was freed from the necessity of considering the peculiar effects described by that philosopher. I was the more willing to avoid for the present touching upon these, as I must at the same time have entered into the views of Sir Humphry Davy upon the same subject†, and also those of Marianini‡ and Ritter§, which are connected with it.

¶ v. *General Remarks on the active Voltaic Battery.*

1034. When the ordinary voltaic battery is brought into action, its very activity produces certain effects, which re-act upon it, and cause serious deterioration of its power. These render it an exceedingly inconstant instrument as to the *quantity* of effect which it is capable of producing. They are already, in part, known and understood; but as their importance, and that of certain other coincident results, will be more evident by reference to the principles and experiments already stated and

* Annales de Chimie, tom. xxviii. p. 190; and Mémoires de Génève.

† Philosophical Transactions, 1826, p. 413.

‡ Annales de Chimie, tom. xxxiii. pp. 117, 119, &c.

§ Journal de Physique, tom. lvii. pp. 349, 350.

described, I have thought it would be useful, in this investigation of the voltaic pile, to notice them briefly here.

1035. When the battery is in action, it causes such substances to be formed and arrayed in contact with the plates as very much weaken its power, or even tend to produce a counter current. They are considered by Sir Humphry Davy as sufficient to account for the phenomena of Ritter's secondary piles, and also for the effects observed by M. A. De la Rive with interposed platina plates*.

1036. I have already referred to this consequence (1003.), as capable, in some cases, of lowering the force of the current to one eighth or one tenth of what it was at the first moment, and have met with instances in which its interference was very great. In an experiment in which one voltaic pair and one interposed platina plate were used with dilute sulphuric acid in the cells (fig. 31.), the wires of communication were so arranged, that the end of that marked 3 could be placed at pleasure upon paper moistened in the solution of iodide of potassium at *x*, or directly upon the platina plate there. If, after an interval during which the circuit had not been complete, the wire 3 were placed upon the paper, there was evidence of a current, decomposition ensued, and the galvanometer was affected. If the wire 3 were made to touch the metal of *p*, a comparatively strong sudden current was produced, affecting the galvanometer, but lasting only for a moment; the effect at the galvanometer ceased, and if the wire 3 were placed on the paper at *x*, no signs of decomposition occurred. On raising the wire 3, and breaking the circuit altogether for a while, the apparatus resumed its first power, requiring, however, from five to ten minutes for this purpose; and then, as before, on making contact between 3 and *p*, there was again a momentary current, and immediately all the effects apparently ceased.

1037. This effect I was ultimately able to refer to the state of the film of fluid in contact with the zinc plate in cell i. The acid of that film is instantly neutralized by the oxide formed; the oxidation of the zinc cannot, of course, go on with the same facility as before; and the chemical action being thus interrupted, the voltaic action diminishes with it. The time of the rest was required for the diffusion of the liquid, and its replacement by other acid. From the serious influence of this cause in experiments with single pairs of plates of different metals, in which I was at one time engaged, and the extreme care required to avoid it, I cannot help feeling a strong suspicion that it interferes more frequently and extensively than experimenters are aware of, and therefore direct their attention to it.

1038. In considering the effect in delicate experiments of this source of irregularity of action in the voltaic apparatus, it must be remembered that it is only that very small portion of matter which is directly in contact with the oxidizable metal which has to be considered with reference to the change of its nature; and this portion is not very readily displaced from its position upon the surface of the metal (582. 605.), especially if that

* Philosophical Transactions, 1826, p. 413.

metal be rough and irregular. In illustration of this effect, I will quote a remarkable experiment. A burnished platina plate (569.) was put into hot strong sulphuric acid for an instant only: it was then put into distilled water, moved about in it, taken out, and wiped dry: it was put into a second portion of distilled water, moved about in it, and again wiped: it was put into a third portion of distilled water, in which it was moved about for nearly eight seconds; it was then, without wiping, put into a fourth portion of distilled water, where it was allowed to remain five minutes. The two latter portions of water were then tested for sulphuric acid; the third gave no sensible appearance of that substance, but the fourth gave indications which were not merely evident, but abundant for the circumstances under which it had been introduced. The result sufficiently shows with what difficulty that portion of the substance which is in *contact* with the metal leaves it; and as the contact of the fluid formed against the plate in the voltaic circuit must be as intimate and as perfect as possible, it is easy to see how quickly and greatly it must vary from the general fluid in the cells, and how influential in diminishing the force of the battery this effect must be.

1039. In the ordinary voltaic pile, the influence of this effect will occur in all variety of degrees. The extremities of a trough of twenty pairs of plates of Wollaston's construction were connected with the volta-electrometer, fig. 11. (711.), of the Seventh Series of these Researches, and after five minutes the number of bubbles of gas issuing from the extremity of the tube, in consequence of the decomposition of the water, noted. Without moving the plates, the acid between the copper and zinc was agitated by the introduction of a feather. The bubbles were immediately evolved more rapidly, above twice the number being produced in the same portion of time as before. In this instance it is very evident that agitation by a feather must have been a very imperfect mode of restoring the acid in the cells against the plates towards its first equal condition; and yet imperfect as the means were, they more than doubled the power of the battery. The first effect of a battery which is known to be so superior to the action which the battery can sustain, is almost entirely due to the favourable condition of the acid in contact with the plates.

1040. A *second* cause of diminution in the force of the voltaic battery, consequent upon its own action, is that extraordinary state of the surfaces of the metals (969.) which was first described, I believe, by Ritter*, to which he refers the powers of his secondary piles, and which has been so well experimented upon by Marianini, and also by A. De la Rive. If the apparatus, fig. 31. (1036.), be left in action for an hour or two, with the wire 3 in contact with the plate *p*, so as to allow a free passage for the current, then, though the contact be broken for ten or twelve minutes, still, upon its renewal, only a feeble current will pass, not at all equal in force to what might be expected. Further, if P^1 and P^2 be connected by a metal wire, a powerful momentary current will pass from P^2 to P^1 through the acid, and therefore in the

* Journal de Physique, lvii. p. 349.

reverse direction to that produced by the action of the zinc in the arrangement; and after this has happened, the general current can pass through the whole of the system as at first, but by its passage again restores the plates P^2 and P^1 into the former opposing condition. This, generally, is the fact described by Ritter, Marianini, and De la Rive. It has great opposing influence on the action of a pile, especially if the latter consist of but a small number of alternations, and has to pass its current through many interpositions. It varies with the solution in which the interposed plates are immersed, with the intensity of the current, the strength of the pile, the time of action, and especially with accidental discharges of the plates by inadvertent contacts or reversions of the plates during experiments, and must be carefully watched in every endeavour to trace the source, strength, and variations of the voltaic current. Its effect was avoided in the experiments already described (1036. &c.), by making contact between the plates P^1 and P^2 before the effect dependent upon the state of the solution in contact with the zinc plate was observed, and by other precautions.

1041. When an apparatus like fig. 26. (1017.) with several platina plates was used, being connected with a battery able to force a current through them, the power which they acquired, of producing a reverse current, was very considerable.

1042. *Weak and exhausted charges* should never be used at the same time with *strong and fresh ones* in the different cells of a trough, or the different troughs of a battery: the fluid in all the cells should be alike, else the plates in the weaker cells, in place of assisting, retard the passage of the electricity generated in, and transmitted across, the stronger cells. Each zinc plate so circumstanced has to be assisted in decomposing power before the whole current can pass between it and the liquid. So that, if in a battery of fifty pair of plates, ten of the cells contain a weaker charge than the others, it is as if ten decomposing plates were opposed to the transit of the current of forty pairs of generating plates (1031.). Hence a serious loss of force, and hence the reason why, if the ten pairs of plates were removed, the remaining forty pairs would be much more powerful than the whole fifty.

1043. Five similar troughs, of ten pairs of plates each, were prepared, four of them with a good uniform charge of acid, and the fifth with the partially neutralized acid of a used battery. Being arranged in right order, and connected with a volta-electrometer (711.), the whole fifty pairs of plates yielded 1·1 cubic inch of oxygen and hydrogen in one minute: but on moving one of the connecting wires so that only the four well-charged troughs should be included in the circuit, they produced with the same volta-electrometer 8·4 cubical inches of gas in the same time. Nearly seven eighths of the power of the four troughs had been lost, therefore, by their association with the fifth trough.

1044. The same battery of fifty pairs of plates, after being thus used, was connected with a volta-electrometer (711.), so that by quickly shifting the wires of communication, the current of the whole of the battery, or of any portion of it, could be made to pass through the instrument for given portions of time in succession. The whole of the battery evolved 0·9 of a cubic inch of oxygen and hydrogen in half a minute; the forty plates evolved 4·6 cubic inches in the same time; the whole then evolved 1 cubic inch in the half minute; the ten weakly charged evolved 0·4 of a cubic inch in the time given: and finally the whole evolved 1·15 cubic inch in the standard time. The order of the observations was that given: the results sufficiently show the extremely injurious effect produced by the mixture of strong and weak charges in the same battery*.

1045. In the same manner associations of *strong and weak* pairs of plates should be carefully avoided. A pair of copper and platina plates arranged in *accordance* with a pair of zinc and platina plates in dilute sulphuric acid, were found to stop the action of the latter, or even of two pairs of the latter, as effectually almost as an interposed plate of platina (1011.), or as if the copper itself had been platina. It, in fact, became an interposed decomposing plate, and therefore a retarding instead of an assisting pair.

1046. The *reversal*, by accident or otherwise, of the plates in a battery has an exceedingly injurious effect. It is not merely the counter action of the current which the reversed plates can produce, but their effect also in retarding even as indifferent plates, and requiring decomposition to be effected upon their surface, in *accordance* with the course of the current, before the latter can pass. They oppose the current, therefore, in the first place, as platina interposed plates would do (1011—1018.); and to this they add a force of opposition as counter-voltaic plates. I find that, in a series of four pair of zinc and platina plates in dilute sulphuric acid, if one pair be reversed, it very nearly neutralizes the power of the whole.

1047. There are many other causes of reaction, retardation, and irregularity in the voltaic battery. Amongst them is the not unusual one of precipitation of copper upon the zinc in the cells, the injurious effect of which has before been adverted to (1006.). But their interest is not perhaps sufficient to justify any increase of the length of this paper, which is rather intended to be an investigation of the theory of the voltaic pile than a particular account of its practical application.

Note.—Many of the views and experiments in this Series of my Experimental Researches will be seen at once to be corrections and extensions of the theory of electro-chemical decomposition, given in the Fifth and Seventh Series of these Researches. The expressions I would now alter are those which relate to the independence of the evolved elements of the poles or electrodes, and the reference of their evolution to powers entirely internal (524. 537. 661.). The present paper fully shows my present views; and I would refer to paragraphs 891. 904. 910. 917. 918. 947. 963. 1007. 1031. &c., as stating what they are. I hope this note will be considered as sufficient

* The gradual increase in the action of the whole fifty pairs of plates was due to the elevation of temperature in the weakly charged trough by the passage of the current, in consequence of which the exciting energies of the fluid within were increased.

in the way of correction at present; for I would rather defer revising the whole theory of electro-chemical decomposition until I can obtain clearer views of the way in which the power under consideration can appear at one time as associated with particles giving them their chemical attraction, and at another as free electricity (493. 957.).—M. F.

Royal Institution,
March 31, 1834.

XXI. *On the Functions of some parts of the Brain, and on the relations between the Brain and Nerves of Motion and Sensation. By Sir* Charles Bell, *F.R.S.*

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The difficulties which attend the investigation of the structure and functions of the brain are shown by the ineffective labours of two thousand years; and the first endeavour of the author is to remove the idea of presumption that attaches to the very title of this paper. Perhaps the enumeration of some of the sources of error which have retarded discovery may be the best introduction and apology.

The first impediment to success is in the nature of the inquiry, since extraordinary and contradictory results must be expected from experimenting on an organ so fine as that must be which ministers to sensibility and motion, and which is subject to change on every impression conveyed through the senses. This remarkable susceptibility is exemplified in what we often witness; extraordinary results, such as violent convulsions and excruciating pain, from causes which appear quite inadequate. For example, the presence of a minute spicula of bone which has penetrated to the brain, will at one time be attended with no consequence at all; at another it will occasion a deep coma, or loss both of sensibility and motion. Nay, symptoms apparently as formidable will be produced by slight irritation on remote nerves. Seeing these contradictory effects, is it reasonable to expect constant and satisfactory results from experiments in which deep wounds are inflicted on the brain of animals, or portions of it torn away?

Other circumstances evince the slight varieties in the causes which produce the most extraordinary effects. Water in the brain, which has free access to all the cavities of the brain, and which to all appearance both presses equally, and if it irritate must irritate equally, will have the effect of rendering one side of the body paralytic and of convulsing the other with incessant motion.

Another source of error, especially to the experimenter on the brain, is the disturbance of its circulation; for the brain depends more directly than any other organ on the condition of the circulation within it. We may see this in the provisions for the free and equable supply of the blood within the head, as well as for its unimpeded exit. Now by raising the skull, a necessary preliminary to most experiments on the substance of the brain, there is an immediate disturbance of the circulation, which of itself may be attended with insensibility or convulsions.

The most frequent source of error, perhaps, is the obscurity which hangs over the whole subject; for although the brain be divided naturally into distinct masses, not one of these grand divisions has yet been distinguished by its function. There is not even an opinion as to their relative importance. Hence it has followed that the experimenter has not known what to seek, or how to plan his experiment; and hence have been derived the weakest fancies that have ever obscured any science. Another difficulty meets the inquirer at every step if he be not critically guarded. Whole masses of the brain may be destroyed by disease, or actually removed with impunity; that is to say, without any immediate influence on the mind, or on the power of motion or of sensibility; yet the very slightest general impression on the brain will in the instant deprive the individual both of sense and motion.

It will not be denied that the most unequivocal proof of the little success which has attended the efforts made to improve this part of physiology, is the failure of all attempts to explain the phenomena which attend injury of the brain; it is neither said why in disease of the brain sensation and motion should be lost together, nor why one faculty should sometimes be imperfect and the other entire. There is no satisfactory reason given for the most common occurrence in practice, the loss of motion and sensation on the side of the body opposite to that side of the brain which has received the injury; nor has the condition of the face as associated with that of the body been accounted for. When circumstances so remarkable present themselves daily, consequent upon accident or disease affecting the brain, without our teachers succeeding in offering a satisfactory reason for them, it is obvious that we are in a state of profound ignorance of the most interesting functions of the animal body, notwithstanding the innumerable experiments which have been made upon the brains of animals.

These are probably the reasons why ingenious men have failed to make us acquainted with the distinct functions of the divisions of the brain, and countenance us in advancing to the inquiry in a manner altogether different. If the real intricacy of the brain, and the disappointments met with, have inclined many to consider it as an inextricable labyrinth, we may well doubt whether the thread which is to lead us through has been properly selected. This term is not altogether metaphorical, since it is our design to follow the course of the natural filaments discernible in the nervous matter of the brain. The investigation into the substance of the brain must be made in a manner different from common dissection; there is a new element to conquer. Every part of the brain is closely united and pent up within the skull, for the protection of its delicate substance. This compactness of structure guards the brain against impulse from within as well as from external injury; but whether the whole of this structure be essential and of primary importance, or whether some part may not perform the merely accessory office of packing and joining together the more delicate parts, and so securing the finer filaments which run through it, is even up to the present time matter of conjecture. However, it is to the filamentous and striated texture that we attach importance, as leading in the right path, and as marking the relations which exist between the parts of the brain, and the connexions of these with the nerves distributed over the body. The advantage with which we now enter on this inquiry is obvious, for instead of seeking, by injuring the substance of the brain, to discover the effects on remote parts of the nervous system, we commence the inquiry with a knowledge of that system.

It being now universally allowed that nerves have distinct functions, and not a common quality, and that the sensitive and motor roots of the nerves spring from different sources, it must appear a very natural mode of inquiry to follow these nerves into the brain, and to observe the tracts of nervous matter from which they take their origin. It is surely an easy, as well as a natural proceeding, to follow these tracts, and to mark the portions of the brain to which they ultimately tend; finally, to inquire what is the effect of the diseases of these parts, what the accompanying symptoms, and to compare the symptoms with the anatomical details.

On this plan I now propose to demonstrate that sensibility and motion belong to the cerebrum,—that two columns descend from each hemisphere,—that one of these, the anterior, gives origin to the anterior roots of the spinal nerves, and is dedicated to voluntary motion,—and that the other (which from its internal position is less known) gives origin to the posterior roots of the spinal nerves, and to the sensitive root of the fifth nerve, and is the column for sensation.

Further, I propose to show that the columns of motion which come from different sides of the cerebrum join and decussate in the medulla oblongata,—that the columns of sensation also join and decussate in the medulla oblongata. Finally, that these anterior and posterior columns bear in every circumstance a very close resemblance to one another,—that is to say, the sensorial expansions of both are widely extended in the hemispheres: they pass through similar bodies towards the base of the brain, and both concentrate and decussate in the same manner, thus agreeing in every respect, except in the nervous filaments, to which they give origin.

Of the striated Septa in the Medulla Oblongata and Pons Varolii.

We can have no hesitation in giving superior importance to those tracts of striated matter which descend from the brain to the spinal marrow, since they are obviously the lines of communication between the organ of the mind and the frame of the body. But these longitudinal tracts are separated by certain plates of fibrous matter which go directly transverse, are very regular, very easily demonstrated, and although no doubt important in themselves, are particularly useful to us in our present view, as establishing the natural distinctions or boundaries between the columns which descending from the encephalon constitute the medulla oblongata and the spinal marrow.

I shall first name parts that are familiar, as being noticed in systematic works, and proceed to others which I conceive have been overlooked. Of the former class are the superficial transverse fibres of the pons or nodus cerebri, which passing across terminate in the crura cerebelli. When this part of the pons Varolii is raised, and with it the longitudinal striated matter which passes from the crus cerebri and is prolonged to the corpus pyramidale, a very distinct layer or septum of transverse fibres is seen crossing from the one hemisphere of the cerebellum to the other. This septum is best seen from behind, when the tracts which descend from the cerebrum and from the corpora quadrigemina are taken away, for then its appearance (as in Plate XX. fig. 1. A. A.) much resembles the plates now to be described.

As to those septa which I conceive have hitherto been neglected, the most remarkable is that which forms a plane in the median line, resting with its edge upon the last-named transverse septum, and extending its fibres directly backwards, so as to form a striated leaf, separating the two great longitudinal tracts which pass between the medulla oblongata and the thalami nervorum opticorum (Plate XX. fig. 1. B.).

If we separate the corpus restiforme (meaning by that term the mass which passes between the cerebellum and the medulla oblongata,) from the corpus olivare, we shall find a layer of delicate fibres which constitute a pellicle much resembling the fibrous layer, which might be peeled from the bark of the birch-tree, and this is a septum (fig. 1. c. c.).

Another septum of the same kind intervenes between the two anterior corpora pyramidalia. So accurately are the extreme anterior fibres of this septum attached to the corpora pyramidalia, that if we separate these bodies the fibres will alternately adhere to the right and left column, so as to present an appearance as if there was an actual commissure between them; and authors have mistaken this, describing that, which truly is a septum of separation, as a bond of union. And so on the back part of the medulla oblongata, when we push aside the restiform bodies, or those columns which have sometimes been called the posterior pyramidal bodies, and open the central slit, we have the same appearance of minute commissures, which, however, is only the separation of the fibres of the plate or septum; and these fibres, instead of running in a direction to be a lateral bond of union or commissure, run from before backwards, and intervene between the longitudinal columns.

These layers not only distinguish in a natural way the columns which are descending from the cerebrum to form the spinal marrow, but they are necessary as leading us to the true points of union between the longitudinal columns, where their fibres actually decussate, and where these septa are deficient to permit the union.

The Pons Varolii, or nodus cerebri, is undoubtedly an intricate part of the brain; but until this intricacy be explained, we can have no hope of making a correct arrangement of the course of the filaments in the brain, and which pass through this body. We shall therefore take it as a key to the composition of the brain.

The pons has with seeming correctness been considered as the commissure of the cerebellum. In this, its capacity of joining opposite parts, we have to notice its two transverse laminæ of fibres above alluded to, one superficial and the other deep-seated. We observe also an oblique lateral process which passes from the cerebellum to the crus cerebri. These septa intersect and distinguish the grand fasciculi or tracts of nervous matter, which, coming down from the cerebrum, seem to flow under the bridge and converge in the medulla oblongata*.

We commence our investigation with parts that are familiar. We trace the corpora pyramidalia of the medulla oblongata upwards from the point of their decussation towards the brain. They enter the pons by two distinct arches. The superficial layer of transverse fibres stretching from the crura cerebelli is over them, and the deeper septum is under them. On raising the superficial layer of the pons, we see the fibres of the corpora pyramidalia passing quite through to the crus cerebri; and now in one view we see a great portion of the grand tract which furnishes the nerves of motion (Plate XIX. A. B. C.).

Let us divide these tracts by a transverse incision where the corpora pyramidalia enter the pons, and lift them up. We keep close to the deeper transverse septum, which we shall find as distinct and smooth as a floor, and now directed by this septum we distinguish the portion of fibrous matter which is anterior to it; and if we follow this up into the crus cerebri, we shall come upon the corpus nigrum, and find that the crus is not a simple texture of filaments, but that it is compound, and that we are lifting that anterior division of it which belongs to motion, and which we shall find spreads over the tract of nervous matter which comes up behind the deeper-seated septum.

We may complete our view of this motor tract, by making sections of the cerebrum, and pursuing the diverging fibres, first into the corpus striatum, and thence, as they proceed onwards, spreading into the hemisphere of the cerebrum and diverging to the cineritious convolutions.

Thus we have already found, that the crus cerebri is not simple, but consists of parts easily and naturally divided. Returning then to the pons as furnishing us with the means of making the natural distinctions of these tracts, we take the deep septum or posterior set of transverse fibres again as our guide, and trace

The posterior Tract.

To obtain a distinct view of the whole extent of the posterior tract, we require to have the parts carefully prepared †. It will be very convenient to have the crura, pons,

* The terms pons and nodus are sufficiently intelligible and harmless, as implying no theory; I retain the old names unless the new ones be countenanced by the just eminence of the authors who have invented them. This is the proper check against the mutiplication of terms in anatomy. In describing the course of the fibres, the expressions I employ are used in their anatomical sense, as implying the direction in which the hand and eye are following the line, and not in reference to the course in which I may suppose the energy to pass in the performance of their functions.

† It will be in vain for the anatomist to attempt demonstrating these facts in the recent brain; but he will find it easy if he take some old preparation of the brain, which has been for some years in spirits.

and medulla oblongata detached from the great masses of the cerebrum and cerebellum, so that they may lie before us. We should first mark out and trace the columns of the spinal marrow; observing the corpora restiformia as they come down from the cerebellum, we may split them at the posterior fissure and fold them aside.

We now survey the extent of the fourth ventricle. On each side of the calamus scriptorius are two pyramidal columns*. To trace these upwards we must cut into the iter ad tertiam ventriculam, by dividing the corpora quadrigemina, and then we can trace them up into the thalami nervorum opticorum. By a section we may trace them through that body, and then diverging into the hemispheres of the cerebrum.

Having followed these columns upwards, we next trace them downwards, and find that they join, intermingle, and decussate, and again separate, and proceed down the spinal marrow (Plate XX. fig. 2. B. C.).

From no part of this column does any nerve of motion take its origin; its relations to the sensitive nerves will be seen on further dissection.

The corpus striatum and the thalamus lie very curiously together: the thalamus forms a nucleus round which the corpus striatum bends, and when their respective layers of striæ make their exit beyond these bodies to form the great fan- or solar-like expansion into the hemisphere of the cerebrum, their rays mingle together. A rude representation of these two tracts of the cerebrum, as we have traced them, may be made with the hands. If I place my wrists together, parallel, and closing one hand, embrace it with the other, I represent the two portions of one crus. The closed fist is the thalamus, and the other is the corpus striatum. If I then extend my fingers, interlacing their points, I represent the final distribution of the portions of the nervous matter which are dedicated to sensation and volition.

But before proceeding further, we must distinguish a certain portion of the great tract of fibrous matter that lies behind the septum of the pons, which does not belong to sensibility, but to a different order of parts. If we dissect round the corpus olivare, we find it easy to separate this body from the column of motion on the fore part, and the column of sensation behind. Following then the fibrous portion of matter which ascends from it, we find that it runs close upon the back of the septum of the pons, and that a part of it goes off to the corpora quadrigemina, whilst a part runs directly into the crus cerebri.

On tracing the column which descends from the corpus olivare, we find that it is very soon attached to the columns both of motion and of sensation, and becomes incorporated with them as it passes downwards (Plate XXI. G.).

We have now traced three great tracts or courses of fibres into the crus cerebri;

* In fact all the columns which form the medulla oblongata converge downwards and are pyramidal. We have the anterior pyramidal bodies, the posterior pyramidal bodies or corpora restiformia, and those deeper columns, whose form might authorize the term, as they are more especially counterparts of the true anterior pyramidal bodies.

an anterior one for motion, a posterior one for sensation, and a middle one, which for the present we may call the tract of the corpus olivare.

After these dissections, it is impossible for us to consider the medulla oblongata as the mere commencement of the spinal marrow: it has a peculiar structure and distinct functions; it is the body formed by the convergence of the great tracts of the cerebrum, where these tracts respectively meet and decussate; in it the tract of the corpus olivare is joined to those of motion and sensation.

Below the medulla oblongata the spinal marrow commences, or rather is prolonged from it, but it is constituted with a distinct arrangement of its columns. On each side it receives three columns from the cerebrum, besides those which come down from the cerebellum, under the name of corpora restiformia, to form its posterior part, and these columns enter into relations which do not exist above.

Decussation of the Posterior or Sensitive Part.

We have noticed a fact of more than ordinary importance as reconciling the occurrence of symptoms, with our knowledge of anatomy. Where the posterior tract, descending from the cerebrum, has reached the point of the medulla oblongata, just opposite to the decussation of the corpora pyramidalia on the fore part, we described a coalescence. We have already stated, that when we proceed to separate the columns on the sides of the slit called calamus scriptorius, we see small, neat, and regular filaments, as it were, interlacing and joining the two columns. But when we examine further, we perceive that these filaments belong to a plate of fibrous texture which passes in the central plane from before backwards (Plate XX. fig. B.). This striated septum stops or is interrupted by the union of the columns of sensation; and now attending to the fibres of these two columns, we find them to decussate with an interweaving as distinct as that of the corpora pyramidalia or anterior columns (Plate XX. fig. 2. C.). After this union and decussation has taken place we may trace the nervous matter downwards in the two lateral portions of the spinal marrow, covered by the columns, which are the most posterior of all, and which descend from the cerebellum under the name of corpora restiformia.

Before tracing the origin of the sensitive roots of the spinal nerves, and that of the fifth nerve, in their relations to these tracts, we may review their course. We cannot fail to observe the remarkable correspondence in the structure and course of the two grand tracts or divisions of the crus cerebri, which descending, form so large a portion of the spinal marrow. Tracing them from the brain, we find both converging from the periphery of the hemisphere; both entering masses of cineritious matter, emerging alike, and approaching, but not absolutely joining; both contracting into narrow pyramidal columns; both having corresponding decussations, and only distinguishable at last by one of them giving origin to the motor nerves, and the other to the sensitive.

The origin of the posterior roots of the Spinal Nerves, and their relation to the decussation of the Posterior Column.

The brain being before us so as to present its posterior aspect, and the back part of the spinal marrow, we raise the cerebellum and tear the pia mater, so as to expose the fourth ventricle. We may divide the processes of the cerebellum and take that body away. Having the parts thus prepared, we attend more particularly to the posterior series of roots of nerves which run towards the uppermost spinal nerve.

If we trace the line where the posterior roots of the spinal nerves arise, we find that the posterior columns of the spinal marrow are behind these roots; and if we trace these posterior columns upwards, we see them diverging under the name of corpora restiformia to the cerebellum. We strike a level by following the posterior roots of the spinal nerves into the spinal marrow. In doing this we shall find it necessary to lift the posterior column, and then, being able to trace the roots of the nerves, we shall find them connected with a course of longitudinal filaments; and these, on further investigation, will be found to be continued from the point immediately below the decussation of the posterior column of sensation, which I have described above (Plate XX. fig. 2. c.).

Thus it will be found, that the posterior roots of the first, and consequently of all the spinal nerves, are derived from that posterior column which descended from the posterior division of the crus cerebri, and that they are thus placed in the same relation as the anterior roots with respect to the decussation of the prolonged medullary matter of the cerebrum.

The origin of the sensitive root of the Fifth Nerve, and its relation to the Spinal Marrow.

In former papers I have proved the fifth nerve of the head, according to the arrangement of Willis, to be the nerve of sensation to the head and face, thus distinguishing it from the nine nerves of the encephalon, and from the appropriate nerves of the senses to the nose, and eye, and ear.

I gave my reasons, at the same time, for distinguishing it as the nerve of mastication, and showed, in short, that it had all the characteristics of a spinal nerve. It becomes now a subject of interest to observe in what respect it further resembles the spinal nerves, and to inquire how its relations with the brain are formed. It is a happiness in this inquiry, that although it be difficult to trace the motor roots of nerves, owing to the delicacy of their connexions with the brain, the sensitive root is followed with ease into the brain or spinal marrow.

We commence the dissection of the fifth nerve by distinguishing its grand divisions as they emerge from the side of the pons, separated by a transverse band of fibres (Plate XX. fig. 6, 7.).

Leaving, for the present, the scattered roots of the motor portion which pass between the transverse cords of the pons, we shall proceed to follow the other in a retrograde direction towards its origin. For this purpose, with a small and fine knife, we cut into the substance which surrounds the sensitive root, to the depth of a twelfth of an inch, and then lay aside the knife and take the curette, and perhaps the ivory handle of the knife *. With these we push aside the substance of the brain, in doing which there is no difficulty in distinguishing the smooth, flat, and ribbon-like white nerve. Continuing to press aside the matter of the pons, and, when separated, to cut it away, we find the nerve taking a course backwards and downwards into the medulla oblongata, making a considerable angle. Here we are interrupted by the crossing of the portio mollis of the seventh nerve. We observe in passing, that the portio mollis has two roots; that besides that usually described passing round the processus ad cerebellum to the anterior part of the fourth ventricle, it has a round root, which enters anteriorly to that process. But by attention and much neat dissection we may preserve these roots of the seventh nerve, and, recovering the tract of the fifth nerve below, trace it downwards. We are again interrupted by the origins of the eighth pair of nerves; and here, too, it will be found, on careful dissection, that this nerve does not correspond with the description in systematic works. But to proceed with our proper subject. Some part of the root of the fifth may be seen to deviate in a direction towards the calamus scriptorius; but the main tract descends behind the fasciculus of the corpus olivare, by the side of the great fasciculus of fibres which we have already traced down from the cerebrum. Disregarding this association, and following still the root of the fifth nerve, we find it continued to the roots of the superior spinal nerves; and in tracing it thus far, we must conclude that its relations are with the spinal marrow rather than directly with the brain, and that it joins the posterior column below the decussation of that sensitive tract or column. It remains a proper subject of inquiry to determine how far the deviation of a part of the sensitive tract of this nerve corresponds with its complex function in being the source of taste as well as of common sensibility.

It has been observed by diligent anatomists from time to time, that the nerves of the encephalon come off in a direction ascending from the spinal marrow. There can be no doubt that the sensitive root of the fifth ascends, and that it has its origin in the spinal marrow rather than in the brain. Without at present inquiring into the minute anatomy of the other nerves, we may draw very important conclusions from what is before us.

It is rather surprising, that from what was known of the anatomy of the brain, pathologists should have so agreed in their explanation of the phenomenon of injury of one side of the brain producing its effects on the opposite side of the body. Their opinion was founded on the decussation of the anterior columns, or pyramidal bodies, and those only; but great misconception must have prevailed as to the anatomy, when such an explanation could be satisfactory; and, at all events, it must

* If we order dissecting instruments, there is no end to the trouble of procuring them fine enough. The operating case of the oculist, however, furnishes at once all that is necessary for delicate anatomy.

have been believed that the posterior roots of the spinal nerves were the same, in function, with the anterior roots. When, however, it is understood that the anterior column of the medulla oblongata gives off only filaments of motion, the rationale of decussating fibres fails, or rather is imperfect; for in injury of the brain, both motion and sensation are lost on the opposite side of the body. We perceive how important it was, in order to understand this symptom, that the posterior or sensitive part of this column should be shown to descend from the cerebrum, and decussate at a point, corresponding to that at which the decussation of the pyramidal bodies takes place.

I have observed, that the corpus striatum is the part in which most frequently rupture of the cerebral vessels occurs; and the observations of authors correspond with this opinion. In such cases we can readily believe that the power of motion will be most injured; whilst such derangement in the hemisphere must, at the same time, more or less affect the sensibility.

Certain circumstances essential to the study of the pathology of the brain are explained through this part of anatomy; first, that motion and sensation should, in by far the greater number of cases, be lost together, in disease of the brain; because the sensorial extremities of both columns are in the hemisphere of the cerebrum; secondly, it is seen why it is that the sensibility, as well as the power of motion, is injured on the opposite side of the body when the hemisphere of the cerebrum is hurt or diseased, for both columns decussate; in the third place, the anatomy of the origin or root of the fifth nerve explains very satisfactorily why, in palsy, the privation of sensibility of the side of the face corresponds with that of the body.

My paper should perhaps have terminated here, with these demonstrable facts, but I am tempted to reach a little further.

Further examination of the relation between the Brain and Spinal Marrow.

Other questions will be suggested in reference to the symptoms of disease in the brain. When the side of the body is paralytic, how far are the nerves affected which appear to have their origin above the decussations? Does the ninth or lingual, or the portio dura of the seventh nerve, correspond with the spinal nerves? Do the third nerve and the muscles of the eye partake of the condition of the body?

As there is no decussation above the apparent origin of these nerves, and as the commissures of the brain do not serve to explain this phenomenon, we are directed in our inquiries to the spinal marrow.

The spinal marrow has much resemblance to the brain, in the composition of its cineritious and medullary matter, and in the union of its parts. In short, its structure declares it to be more than a nerve, that is, to possess properties independently of the brain. Another consideration presses upon us. Where are the many relations existing between the different parts of the frame, and necessary to their combined actions, established? There must be a relation between the four quarters of an animal. If the muscles of the arm or of the lower extremities are combined through the plexus of nerves in the axilla, and in the loins, what combines the muscles of the trunk, and more especially what joins the extremities together in sympathy? That these combined motions and relations are not established in the brain, the phenomena exhibited on stimulating the nervous system of the decapitated animal sufficiently evince. They must therefore depend on an arrangement of fibres somewhere in the spinal marrow. Comparative anatomy countenances this idea, since the motions of the lower animals are concatenated independently of a brain, and independently of the anterior ganglion, which in some respects gives direction to the volition of these animals.

It comes next to be inquired what use there can be in a decussation, by which one side of the brain is made to serve the opposite side of the body. Ingenuity can offer no reason for such an arrangement; the object must surely be an interchange of fibres, and consequently a correspondence in the movements of the sides of the body and of the extremities. And on this subject it must be admitted, that although in nine out of ten cases the side of the body opposite to that which is diseased in the brain is affected with paralysis, it is not always so, and very often a certain debility is perceptible in the side which is least affected. Again, when a man is seized with paralysis, he is sometimes at the instant affected with pain in the other side. These irregularities tend to countenance the belief that the decussations of the sensitive and motor spinal columns are rather intended to effect combination and sympathy between every part of the frame, than that one half of the brain should belong to the opposite half of the body, for no apparent object, and without producing any harmony of action.

Such arguments induce me to believe that the brain does not operate directly on the frame of the body, but through the intervention of a system of nerves whose proper roots are in the spinal marrow, and that the decussation, or rather the arrangement of the fibres, takes place at the point where the columns descending from the brain join the spinal marrow, and consequently in effect above the origin of all the nerves, excepting those of the four senses. This supposition would furnish an explanation of the whole of one side of the body, limbs, face, and head, being similarly affected in paralysis. It would also explain the appearance, which all the nerves of motion and sensibility have, of coming in a direction upwards from the spinal marrow, rather than directly outwards from the brain, as the nerves of the proper organs of sense do.

In reflecting on the origins of the nerves of the encephalon, it appears that neither nerves of sense nor of motion arise from the cerebellum or its processes. It further appears that the restiform bodies or processes form no union or decussation similar to those which we have described in the columns of motion and sensation which descend from the cerebrum.

Those descending processes of the cerebellum, however, form a large portion of the spinal marrow; and we must thence infer that the cerebellum operates through the system of the spinal marrow.

The symptoms attributed to disease of the cerebellum do not remove the obscurity which invests this part of anatomy. We know that sometimes the whole hemisphere of the cerebellum is destroyed by suppuration, without loss either of sense or of motion. Moreover, when symptoms do attend disease of the cerebellum, its juxtaposition to the medulla oblongata inclines us to suspect that the effects are produced through the latter body. The substance of the cerebellum is not of diameter sufficient to have a large clot of blood in it, or a large abscess, without blood or matter communicating with either the fourth ventricle, or bursting out upon the surface. The influence thus becomes general on the nervous system, and a confusion in the symptoms is the necessary result. We have no distinct and well-marked cases of disease in the substance of the cerebellum, such as we possess of disease in the cerebrum; and on the whole it does not appear to stand in direct relation to the motions of the frame, or to the common sensibility.

Explanation of the Plates.

Plate XIX.

The figure in this Plate represents the great anterior column which gives off the nerves of motion.

A, A. The fibrous texture of the hemisphere, concentrating to form the anterior portion of the crus cerebri.

B. The anterior column where it is passing the pons Varolii.

C. The right pyramidal body; a little further down is the point of decussation.

D. The remaining part of the pons Varolii, a portion having been dissected off to expose B.

1. The olfactory nerve in outline.

2. The union of the optic nerves.

3, 3. The third nerve.

4, 4. The fourth nerve.

5, 5. The fifth nerve, trigeminus.

6, 6. The muscular division of the fifth nerve.

7. The sensitive root of the fifth nerve.

8. The sensitive root rising from the posterior part of the medulla oblongata.

9. The sixth nerve.

10. The portio mollis of the seventh nerve, or auditory nerve.

11. The portio dura of the seventh nerve, or facial nerve.

12. The eighth nerve, viz. par vagum, glosso-pharyngeal nerve, and spinal accessory nerve.

13. The ninth nerve, or lingual nerve.

14. Spinal nerves.

15. Spinal accessory nerve of the right side.

Plate XX.

Fig. 1. Represents the plates of fibres which pass across the pons and medulla oblongata, and which divide those great columns of medullary matter which we trace down from the cerebrum into the spinal marrow.

A, A. The posterior transverse septum of the pons, seen from the back part.

B. The septum, which rising perpendicularly from the septum of the pons, divides the great tracts of nervous matter which descend from the cerebrum, viz. the posterior divisions of the crura cerebri.

C, C. The lateral septum of the medulla oblongata, which separates the corpus olivare from the anterior or muscular tract.

Fig. 2. In this figure the posterior or sensitive tract is shown. They are separated so as to exhibit the posterior transverse septum of the pons.

A. The pons Varolii, with the transverse fibres of the septum (fig. 1. A, A.).

B, B. The sensitive tract dissected and separated.

C. The union and decussation of the posterior tract.

D, D. The posterior root of the spinal nerve continued with the posterior tract below the decussation.

E, E. The sensitive roots and tracts of the fifth pair of nerves.

Plate XXI.

This figure presents a lateral view (slightly oblique) of the two columns, with a section of the pons and crus cerebri.

A. Fibrous texture of the anterior tract as it converges from the left hemisphere of the cerebrum.

B. A section of the left crus cerebri.

C. The motor tract in its course through the pons Varolii.

D. The corpus pyramidale of the left side.

E, E. The posterior or sensible tracts.

F. Their union and decussation.

G. The corpus olivare, hanging by the tract or column, where it is united to the anterior column D and the posterior column E.

H. The superior part of the tract of the corpus olivare, running up into the corpora quadrigemina.

I. Corpora quadrigemina.

Note.—These views of the brain have been taken from dissections made of the parts after they have been preserved a long time in proof spirits.

XXII. *On the repulsive Power of Heat. By the Rev.* BADEN POWELL, *M.A. F.R.S. Savilian Professor of Geometry in the University of Oxford.*

Received April 10,—Read June 19, 1834.

THE expansion of bodies by heat seems to imply a mutual repulsion of their particles; and it is a question naturally suggested, whether such a power of repulsion may not generally belong to heat, or be excited by it, between particles or masses of matter, at sensible as well as insensible distances.

But however obvious the suggestion of such an inquiry, it is not of a nature easy to be pursued or decided. The subject has been partially investigated by Signor LIBRI and by MM. FRESNEL and SAIGEY; but their researches do not seem to have attracted much attention, and their results have even been regarded with considerable doubt. Very recently, however, Professor FORBES, of Edinburgh, has revived the inquiry, by referring to the same principle to account for the singular phenomena presented in certain vibrations of heated metallic bars, first noticed by Mr. TREVELYAN, and since fully investigated by himself*. In a different form the subject had occupied my attention before I was acquainted with Professor FORBES's investigations; but on reading his paper, a new interest attached to the inquiry, and in pursuing it, I have obtained some results which appear to me decisive on a question, at once of importance in the analogies of physical action, and which has been hitherto regarded as involved in considerable uncertainty.

Signor LIBRI, I believe in 1824, examined the influence of heat on capillary attraction, and found that a drop of water suspended on a wire, when the wire was heated at one part, moved away from that part, both when the wire was horizontal and even when inclined upwards from the heated part. This he inferred was due to repulsion produced by the heat between the wire and the particles of the water.

M. FRESNEL† employed discs of foil and of mica fixed vertically at the extremities of a delicately suspended magnetic needle in vacuo, placed so little out of the meridian that it just produced a pressure of the disc against another fixed disc. On heating either of the two with the sun's rays, concentrated by a lens, a sensible repulsion was produced. He showed that the effect was not occasioned by any current of the little air remaining, as it was not increased on the admission of more air; that it bore no relation to magnetic or electric conditions; and did not increase, but gene-

* Transactions of the Royal Society of Edinburgh, vol. xii.

† Annales de Chimie, vol. xxix. pp. 57, 107. (1825).

rally diminished, with thicker discs. He mentions other points on which his results had not been equally decisive, but allows that the whole subject requires further examination. The completion of this interesting inquiry is doubtless one of the numerous benefits of which science was deprived by his early loss.

M. Saigey*, in the course of a series of experiments on the development of magnetism in certain metallic bodies, notices some effects of repulsion, which (after examining every ordinary cause likely to have occasioned them,) he concludes by referring to heat. He tried the effect by means of a needle of lead finely suspended at different distances from a bar of copper, and found the number of oscillations in a given time decrease with the distance; that is, the needle more rapidly assumed the position of parallelism to the heated bar in which repulsion would tend to place it.

Signor Libri's result is remarkable as contradicting the statement of Laplace†, who speaks of the "repulsive force of heat" as subsisting among the particles of a fluid; but observes that *experiment shows* it has no other effect on capillary attraction than what results from its diminishing the density of the fluid.

In trying to repeat Libri's experiment, I have never been able to succeed, except in producing a slight apparent motion in the drop, which seems explicable from the mere effect of evaporation on the side next the heat.

I have observed a drop of oil, contained in a glass tube of about one tenth of an inch bore, move away from the part where heat is applied, evidently from the expansion of the glass, which renders the tube slightly conical, when the drop moves towards the narrower end. I have applied heat to capillary tubes till the suspended liquid has boiled, without producing any effect; to inclined glasses between which a drop of oil was advancing, without in the least affecting its motion; and to a plate of glass from the under side of which a globule of mercury remained suspended, without overcoming the attraction.

With regard to repulsion at greater distances, on employing an arrangement somewhat similar to Fresnel's, when the discs were two small plates of glass with truly plane surfaces, I found that if in the first instance they were pressed together, so as to adhere, heat always overcame the attraction, and the moveable disc sometimes receded to a sensible distance. But this effect (and perhaps also that in Fresnel's experiment) appeared to me in a great measure due to another cause than repulsion, viz. the slight curvature which will be given to the plate of glass by the greater expansion of the more heated surface, producing a convexity towards the heat.

The amount to which this takes place may be easily calculated from the known dilatation of glass, the difference of temperatures of the two surfaces, and the thickness.

In some cases, the two glasses were pressed so hard together that the *colours of thin plates* appeared between them. On the application of heat, these colours in-

* Bulletin Mathématique, tom. xi. No. 167.

† Mécanique Céleste, Supp., livr. x. p. 75.

stantly descended in the scale, and soon vanished. These tints, then, may be employed to furnish an exact indication of the most minute changes of distance between the surfaces, by whatever cause they may be produced; and the effect due to curvature by heat (or rather, in this case, the restoration of the bent glass to a plane figure,) might be calculated, and compared with the effect observed. I made many experiments in this way, and satisfied myself that the change of figure was *insufficient* to account for the *whole* observed effect, and that the separation indicated by the descent of the tints in the scale was therefore, in part, due *to a real repulsion.*

But I do not detail these experiments, because it is immediately evident that the use of *lenses* would afford a simple mode of deciding the question, divested of all influence of change of figure, without any calculation. It is evident, that if the rings be formed between a convex surface and one which is either convex, plane, or even concave of less curvature, heat applied outside of either glass will tend, by the change of figure in every case in the first instance, to *diminish the angle of contact*; that is, (if no other cause interfere,) to make the rings *enlarge,* without altering the central tint, until the curvature become equal to that of the convex surface.

In this form of the experiment I have invariably found that, *from the first moment, the rings regularly* CONTRACT, *and the central tint descends in the scale, till the whole vanishes.*

There are, however, several precautions necessary to be attended to. If the glasses be more than very slightly convex, the portion of the surface, throughout which they approach sufficiently close for the repulsion to act, is very small: this may render the total effect of the repulsive force too weak to overcome the weight of the upper glass, or even its inertia, though placed vertically. This difficulty I found with surfaces which gave the first bright ring, when the centre was a point of maximum brightness, about 0·1 inch diameter. Even here the rings never enlarged. But with surfaces of less curvature, which gave a diameter of 0·2 or 0·3 inch, the effect never failed to be exhibited, most decidedly, on bringing a red hot iron over the glasses when laid one on the other, without pressure.

The experiments, though simple in principle, certainly require some care: but with all precautions, and after the most careful consideration of all causes which can have tended to produce or affect the result, it appears to me that the separation of the glasses through the extremely small but finite and known spaces, whose changes are indicated by the degradation of the tints, can only be due to *the real action of a repulsive power, produced or excited between the surfaces of the glasses by the action of heat.*

There are many questions relating to the nature and properties of this repulsive power which are immediately suggested, and some of which appear capable of solution by variations of the same method.

The *distance* at which the repulsive power can act is shown by these experiments to extend beyond that at which the most extreme visible order of NEWTON's tints is formed. But I have also repeated the experiment successfully with the colours formed under the base of a prism placed upon a lens of very small convexity; and according to the analysis of these colours given by Sir JOHN HERSCHEL*, the distance is here about the 1100dth of an inch.

Beyond these small distances other methods must be resorted to. But the certainty of the result within these limits, confirms its probability at greater distances, as inferred by FRESNEL and SAIGEY.

I have tried many experiments with the view of ascertaining the relations of the repulsion by heat to different *substances and conditions of surface*. There are obvious difficulties in the way of such experiments, except in a few cases. I have found that the effect is produced not only between two surfaces of glass, but between glass and metal. I applied heat at the back of a plate of speculum metal, with a highly polished surface, on which the rings were formed with a convex lens; and on comparing the effect in this case with that similarly produced when a plate of glass of the same thickness was employed, the effect was decidedly less with the metal, notwithstanding its better conducting power: but its highly polished surface rendered it a much worse radiator.

In attempting to repeat the experiment with a coated or roughened surface, there is the radical difficulty of rendering the rings visible. There are obvious objections to coating the polished surface, and leaving a small space clear in the centre to form the rings, owing to inequality of surface and contact. But I have found that this is not absolutely necessary. The rings may be formed if the central part of the coating be only slightly rubbed, and particles of the coating left adhering. I have formed the rings when such particles are seen in the middle of them. With this precaution I tried many comparative experiments. The metal plate coated with China ink gave a greater effect than when plain. With a plate of glass coated with China ink, the smoke of a candle, and leaf-gold, the effect with the smoke and China ink was greater than with the leaf-gold, which accords with the greater radiating power of those surfaces.

But with all these coatings the effect was greater than with the plain glass. Whereas, according to Sir JOHN LESLIE, both the China ink and the leaf-gold have lower radiating powers than the glass. This difference I ascribe to the better contact which the lens has with the softer and more yielding surface of the coating against which it is pressed.

These comparative experiments were made by placing the plate, with the lens resting on it, at an invariable height above the flame of a spirit lamp.

From these experiments, then, though we may infer that, *cœteris paribus*, the better *radiating* power of the surface increases the effect, yet there are other circumstances which affect the result more powerfully, and these seem to be, in general, *whatever may tend to the more rapid communication of heat.*

* On Light, p. 641.

This is still more conspicuous when the rings are formed in a thin plate of *water* between the lenses. The effect is here even greater than in air, and, as we may presume, independent of radiation*.

We may then conclude, upon the whole, that the repulsive effect depends upon the *amount of heat communicated to the second surface by whatever means.*

Also, according to what was shown at first, viz. that heat cannot overcome capillary attraction, it follows that in the case of an interposed liquid the heat must be supposed to act by exciting repulsion directly between the two surfaces themselves, through the fluid, and not by weakening the attraction of the liquid for either of them.

* This variation of my experiment was first tried by Professor Forbes of Edinburgh, on receiving an account which I communicated to him of my experiments in January 1834.

[illegible]

XXIII. *On the Equilibrium of a Mass of Homogeneous Fluid at liberty.* By James Ivory, *K.H. M.A. F.R.S., Instit. Reg. Sc. Paris. Corresp., et Reg. Sc. Gottin. Corresp.*

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Of the questions to which the publication of the Principia gave rise, none has been attended with greater difficulty than that which relates to the figure of the planets. In this research it is required to determine the figure of equilibrium of a mass of fluid consisting of particles that mutually attract one another at the same time that they are urged by a centrifugal force caused by a rotation about an axis. Geometers have long ago adopted a theory of the equilibrium of fluids which is said to be perfect, and to leave only mathematical difficulties to be surmounted in every problem: but it must be admitted that the utility of this theory amounts to very little; for it has failed in solving the fundamental problem for determining the figure of equilibrium of a homogeneous planet in a fluid state. This is the more remarkable, because Maclaurin, soon after the origin of such inquiries, demonstrated with accuracy and elegance, that a planet supposed fluid would be in equilibrium if it had the figure of an oblate elliptical spheroid. To every one that reflects, the question, not easily answered, must occur, Why has it been found impossible to deduce the discovery of Maclaurin from the analytical theory? If we suppose that the theory is physically correct, and that mathematical difficulties alone oppose its successful application, there is great probability that these would have yielded, as in other instances, to the repeated attempts of geometers.

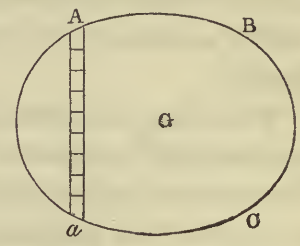
But if Clairaut's theory of the equilibrium of fluids be examined attentively and without prejudice, other difficulties of greater moment will present themselves. In a homogeneous fluid at liberty, if the forces in action be such as to make the problem possible, the equilibrium, according to the theory, requires only one condition, namely, that the forces urging every particle in the surface be directed perpendicularly towards that surface. The solution is thus made to depend entirely upon the differential equation of the surface, and seems to demand that this equation be determinate, and explicitly given: for if the equation be indeterminate, or not explicitly given, how can it be said that the problem is solved? If the forces which urge the particles of the fluid are explicit functions of the coordinates of the point on which they act, so that when the values of the coordinates are assigned, the algebraic expressions are completely ascertained, there is no doubt that the equation of the fluid's surface will be known, and the figure of equilibrium will be determined. With respect to such problems, the theory of Clairaut is therefore perfect, and it possesses all the elegance which might be expected from the talents of the author. On the other hand, if the forces in action depend upon the very figure to be found, as must always happen when the particles attract one another, the equation of the surface will not be explicitly known, because the differential coefficients are derived, in part at least, from the unknown figure of the fluid. Since quantities which depend entirely upon what is sought are not eliminated from the final equation, the ordinary rules of mathematical investigation would lead us to infer, either that the problem is not solved, or that it is indeterminate, and admits of many solutions. It is allowed on all hands that there is a mutual connexion between the figure of a mass of fluid and the attractions it exerts upon its particles: the relation which these two things, alike unknown, must bear to one another in the case of equilibrium, is expressed by the equations of the upper surface and of the interior level surfaces; and therefore it seems hardly possible to deny that these equations are indeterminate. What is wanting to complete the solution of the problem cannot possibly be supplied by any abstract or mathematical properties which the indeterminate equations may possess; and hence arises a suspicion that there is an imperfection of the theory, proceeding, probably, from some necessary condition having been overlooked.

None of the observations that have been made go the length of charging with inaccuracy any of the properties of Clairaut's theory, or any of the equations which express those properties. An equilibrium of a mass of fluid entirely at liberty cannot exist, unless all the conditions of that theory be fulfilled. The question is, whether those conditions be sufficient to determine completely the figure of equilibrium in all hypotheses respecting the forces. It is no small imperfection that the principal points of this theory have not been deduced from the nature of an equilibrium in a manner independent of opinion or arbitrary assumptions. If a strict mode of investigation had been followed, we should have been in possession of a just criterion for ascertaining in any particular case, whether all the conditions required for an equilibrium were fulfilled or not. But in solving problems of this kind, it is often thought sufficient to prove some enumerated properties, or to obtain certain algebraic equations, which unavoidably introduces obscurity and occasions a want of evidence; since it can hardly be supposed that the same properties, or the same equations, will bear alike upon a great variety of problems differing from one another in the nature of the forces urging the particles of a fluid.

Is not the principle, that the equilibrium of a mass of fluid is in all cases secured when every individual particle is pressed equally by all the canals issuing from it and terminating in the surface, an opinion or an assumption? That the property is general, no one will doubt. But when the fluid consists of attracting particles, the forces urging the particles and the pressures of the canals both vary when the upper surface of the fluid is made to change: and may it not be alleged that the variation of the figure of the mass may be such that the pressures of all the canals may still continue to be equal? Thus it may be possible that the assumed principle may be fulfilled in the same body of fluid under different forms.

The difficulties which must be overcome before this subject can be freed from inaccurate and insufficient reasoning, have occurred in problems relating to fluids of uniform density; and for this reason homogeneous fluids are alone treated of in what follows.

1. Suppose that A B C represents a mass of homogeneous fluid entirely at liberty, the particles of which are urged by accelerating forces; let all the forces which act upon any element of the mass, as dm, be reduced to the directions of three rectangular coordinates x, y, z; and put X, Y, Z for the sums of the partial forces respectively parallel to x, y, z. Now, if A a be an infinitely slender prism of the fluid parallel to x, passing completely through the mass, and divided in its whole length into elementary portions, it is obviously a condition necessary to the equilibrium of the body of fluid, that the forces X, acting upon all the elements of A a, mutually destroy one another.



What has been enunciated of a prism parallel to x, must hold equally of prisms parallel to y and z.

Any element dm may be conceived as formed by the intersection of three slender prisms parallel to x, y, z; and, as the pressures in the whole extent of each prism balance another, the element will be at rest, having no tendency to move parallel to x, or to y, or to z. But no proof is required to show that an elementary portion of a fluid in equilibrium must be pressed equally on all sides.

The forces which act upon the elements at the ends of any prism, A a, passing completely through the mass parallel to x, are necessarily directed inward, and have opposite directions; wherefore the force X, in varying through the whole length of A a, must first decrease, then become equal to zero, and afterwards changing its sign, increase in approaching the other surface of the fluid. Thus, in every slender prism parallel to x, there is a point at which the force X is equal to zero; and if the whole body of fluid be divided into such prisms, all the zero points will form a continuous surface stretching completely through the mass. In like manner there will be two other internal surfaces containing all the points at which the forces Y and Z are evanescent. The intersection of the three surfaces will determine a point G within the body of fluid at which all the three forces X, Y, Z, vanish, and which may be called the centre of the mass in equilibrium.

In considering the equilibrium of a mass of fluid entirely at liberty, it is obvious that we may abstract from any motion common to all the particles, and from any forces acting upon them all with equal intensity in the same direction. The forces that must be balanced and rendered ineffective to produce motion, are such only as tend to change the relative position of the particles with respect to one another;

which supposes that the centre of gravity of the whole body of fluid continues at rest and free from the action of any forces. Thus it appears that G, the only point of a fluid in equilibrium not acted upon by any force, is no other than the centre of gravity of the mass.

2. The equilibrium of a fluid entirely at liberty will not be disturbed by a pressure of the same intensity applied to all the parts of the exterior surface.

By the intensity of a pressure is meant the amount of it when applied to some given surface, most conveniently to the unit of surfaces. A constant pressure, or one acting uniformly with the same intensity, is proportional to the surface to which it is applied.

This being understood, what is affirmed above is an immediate consequence of the fundamental property of an incompressible fluid to transmit a pressure exerted upon its surface in all directions without any loss of intensity. The inward pressure upon any part of the surface thus produces an equivalent outward pressure upon every other part, which is balanced by the contrary pressure supposed to act over the whole surface. Wherefore if a mass of fluid be in equilibrium, it will continue in equilibrium, supposing a pressure of the same intensity to be applied to all parts of the surface.

3. The action of the forces upon the particles in the interior parts of the body of fluid is next to be considered.

Take any point $(x\,y\,z)$ of the mass, and draw through it in any direction a plane surface w infinitely small and of any figure; from the same point $(x\,y\,z)$ draw the infinitely short line δs perpendicular to w, and construct an upright prism upon the base w with the height δs. The forces acting upon a particle at the point $(x\,y\,z)$ being represented as before by X, Y, Z, and the coordinates of the end of δs being $x + \delta x, y + \delta y, z + \delta z$, we shall have this identical equation,

$$\left(X\frac{\delta x}{\delta s} + Y\frac{\delta y}{\delta s} + Z\frac{\delta z}{\delta s}\right) \times \delta s \times w = (X\,\delta x + Y\,\delta y + Z\,\delta z) \times w;$$

or by introducing a new symbol,

$$F = X\frac{\delta x}{\delta s} + Y\frac{\delta y}{\delta s} + Z\frac{\delta z}{\delta s},$$

$$F \times \delta s \times w = (X\,\delta x + Y\,\delta y + Z\,\delta z) \times w.$$

Now $\frac{\delta x}{\delta s}$, $\frac{\delta y}{\delta s}$, $\frac{\delta z}{\delta s}$, are the cosines of the angles which the directions of the forces make with δs: wherefore $X\frac{\delta x}{\delta s}$, $Y\frac{\delta y}{\delta s}$, $Z\frac{\delta z}{\delta s}$, are the partial forces urging the particle $(x\,y\,z)$ in the direction of δs; and the whole accelerating force in the same direction is equal to F. The density being constant, and represented by unit, the mass of the prism will be equal to $\delta s \times w$; and as this may be as small as we please, we may assume that every particle of it is urged by the same force F; so that $F \times \delta s \times w$ is the effort of the prism to move from the point $(x\,y\,z)$ in the direction of δs. Let p, a function of x, y, z, represent the intensity of pressure at the point $(x y z)$, and $p + \delta p$ will be the intensity at the other end of δs: the external pressures acting upon the opposite ends of the prism are therefore $p \times w$ and $(p + \delta p) \times w$; and the difference of these, or $\delta p \times w$, is the impulse causing the prism to move towards the point $(x y z)$ in the direction of δs. Now, the prism being at rest, the impulses $F \times \delta s \times w$ and $\delta p \times w$, which tend to move it in opposite directions, must be equal; wherefore, taking the foregoing value of $F \times \delta s \times w$, and suppressing the factor w, which is common to the equal quantities, the non-effect of the opposite forces requires this equation,

$$- \delta p = X \delta x + Y \delta y + Z \delta z,$$

which expresses that the effort of the accelerating forces to move the prism in any direction is counterbalanced by the contrary action of the pressure. The equation must hold at every point of the mass, without any relation being supposed between the infinitely small quantities $\delta x, \delta y, \delta z$; which condition requires that

$$X \delta x + Y \delta y + Z \delta z$$

be the variation of a function in which the three variables x, y, z, are independent of one another. If this function be represented by $\varphi' (x, y, z,)$, so that

$$\int (X\, d x + Y\, d y + Z\, d z) = \varphi' (x, y, z,),$$

we shall have

$$C - p = \varphi' (x, y, z,).$$

The forces respectively parallel to x, y, z, are now thus expressed:

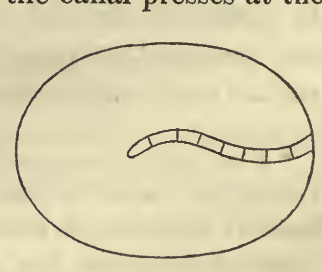
$$X = \frac{d \,.\, \varphi' (x, y, z,)}{d x}, \quad Y = \frac{d \,.\, \varphi' (x, y, z,)}{d y}, \quad Z = \frac{d \,.\, \varphi' (x, y, z,)}{d z}.$$

The differentials of $\varphi' (x, y, z,)$ vanishing at the centre of gravity, the function will increase on every side in receding from that point; and when it becomes equal to C, we shall have

$$C = \varphi' (x, y, z,),$$

which is the equation of the surface of the fluid, the pressure p being equal to zero at all the points of that surface.

If an infinitely narrow canal of any figure be extended from the point $(x y z)$ to the surface of the fluid, the intensity with which all the fluid in the canal presses at the point $(x y z)$ will be equal to the function p. Let the whole length of the canal be divided into small parts, $\delta s, \delta s', \delta s''$, &c.; and at every point of division draw the sections w, w', w'', &c., perpendicular to the sides of the canal, which will thus be divided into an infinite number of small prisms, to every one of which the foregoing investigation will apply. Wherefore, the variation of the intensity of pressure, or δp, in the length of any prism, will be just equal to the action of the accelerating forces upon the particles of the prism; and the intensity with which all the fluid in the



canal presses at the point $(x\,y\,z)$ will be equal to the sum of all the variations of the function p in the whole length of the canal, that is, to the difference between the value of p at the point $(x\,y\,z)$ and at the surface of the fluid. Now the value of p at the surface of the fluid is equal to zero; wherefore, the intensity with which all the fluid in the canal presses at the point $(x\,y\,z)$ is equal to the value of p at that point.

It follows from what has been proved, that every narrow canal drawn from any point $(x\,y\,z)$, and terminating in the surface of the fluid, will press at that point with equal intensity. Hence, if an infinitely small mass of the fluid, such as a sphere, or a cube, &c., be situated at the point $(x\,y\,z)$, it will have no tendency to move by the action of the surrounding fluid; for it will be equally pressed by all the narrow canals standing upon different portions of its surface, and extending to the surface of the fluid. This property is perfectly general and necessary; and it may become a question, whether it be not alone sufficient to secure an equilibrium. Without entering upon the discussion of this question, we here confine our attention strictly to what has been demonstrated, namely, that in a fluid in equilibrium, every infinitely small portion is pressed with equal intensity by all the narrow canals issuing from it, and terminating in the surface of the fluid *.

4. According to what has been shown, the forces which urge the particles of a fluid in equilibrium, and the consequent pressures, depend upon one function $\varphi'(x, y, z,)$, varying in its value as the coordinates change their place from the centre of gravity to the surface of the fluid. The same function likewise determines the figure of the mass; for, the fluid being at liberty, the surface will contain all the points at which there is no pressure. If p denote the pressure at any interior point $(x\,y\,z)$, this equation has been investigated, viz.

$$C - p = \varphi'(x, y, z);$$

and if we make $p = 0$, the result, viz.

$$C = \varphi'(x, y, z)$$

must be verified at all the points of the surface. But it is to be observed, that instances may occur in which the function $\varphi'(x, y, z)$ in passing from a point within the fluid to a point in the surface, undergoes a modification in the form of its expression. It may happen that the quantities which it contains acquire particular relations at the surface; and on this account the function may put on a *singular form*, distinguished

* If the mathematical principle of the property respecting the canals be stated abstractly, it will be found to lie in the nature of the function p, which must be a maximum at the centre of gravity, the point of greatest pressure; and continually decreasing in receding from that point, it must have the same value at all points of the surface of the fluid. Now it is not impossible but, in some problems, there may be more than one function that will satisfy the two conditions; and, should this be the case, the figure of the fluid remaining the same, the property respecting the canals would be verified in more than one supposition respecting the pressure and the forces in action.

in some respects from the original expression as it exists in the interior parts. We may suppose that $\varphi' (x, y, z)$ changes into $\varphi (x, y, z)$ at the surface of the fluid; insomuch that $\varphi' (x, y, z)$ and $\varphi (x, y, z)$ are identical for all the points in the surface, but are different from one another when the coordinates of any other point are substituted. The pressure at any interior point being determined by the expression

$$C - p = \varphi' (x, y, z);$$

the equation of the fluid's surface will be

$$C = \varphi (x, y, z);$$

the first formula being identical with the second at the surface, or when $p = 0$.

The hypothesis of which we have been speaking is not an imaginary one, for a homogeneous planet in a fluid state is an example in point. In this case the forces in action are partly the attraction of the mass upon a particle; and as the fluid has a spheroidical form, the attraction upon a particle in the surface is more simple in its expression, and depends upon fewer quantities than the like force upon a point within the surface. Although it is true universally that the forces urging a particle in the surface of a fluid in equilibrium are deducible from the general expressions of the forces in the interior parts, yet in such cases as that mentioned it does not hold conversely that the latter forces are deducible from the former. This distinction, which is important, is not attended to in CLAIRAUT's theory, which tacitly assumes that the forces are invariably expressed by the same functions without any change of form, whether the point of action be in or below the surface of the fluid.

It appears from what has been said, that in solving problems of equilibrium it is necessary to begin with inquiring in what manner the forces at the surface, which always depend upon the equation of the surface, are connected with the forces supposed to act upon the particles within the surface. A twofold division must be distinguished. In the first and more simple class of problems, it is assumed that the function $\varphi' (x, y, z)$ from which the forces are deduced, undergoes no modification at the surface, but retains immutably the same form of expression at every point of the mass. In the other class of problems the function $\varphi' (x, y, z)$ is supposed to undergo some modification at the surface of the fluid; so that the forces in the interior parts admit of a twofold expression, one derived from the original function $\varphi' (x, y, z)$, and another from the particular form $\varphi (x, y, z)$, which that function assumes at the surface. In such cases the equilibrium will depend upon two different algebraic expressions, and not upon one only, as in the first division, or in CLAIRAUT's theory.

5. The following theorem contains all that concerns the equilibrium in the first and more simple hypothesis, namely, when the functions of the coordinates which express the forces undergo no change of form in passing from a point in the surface of the fluid to a point within the surface.

Theorem.

If a body of homogeneous fluid at liberty have for the equation of its surface,

$$C = \varphi(x, y, z),$$

the mass will be in equilibrium, supposing that every particle $(x\,y\,z)$ is urged by the forces X, Y, Z, respectively parallel to the rectangular coordinates x, y, z, and equal to the partial differential coefficients of $\varphi(x, y, z)$, that is,

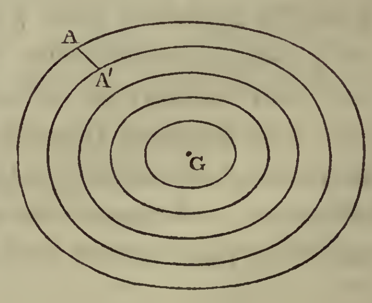
$$X = \frac{d\,.\,\phi(x,y,z)}{dx},\quad Y = \frac{d\,.\,\phi(x,y,z)}{dy},\quad Z = \frac{d\,.\,\phi(x,y,z)}{dz}.$$

The origin of the coordinates being placed at the centre of gravity of the mass, the theorem must be supposed to assume further, that the expressions of the forces, that is, the differential coefficients of $\varphi(x, y, z)$, vanish when the coordinates are all equal to zero; for without this condition the equilibrium of the mass of fluid would be impossible. From this it follows that the value of $\varphi(x, y, z)$ will increase continually as the point $(x\,y\,z)$ recedes from the centre and approaches the surface of the fluid on any side. If C° denote the value of $\varphi(x, y, z)$ at the centre of gravity, $C - C^{\circ}$ will be the whole increase in varying from that point to the surface of the fluid; and as every gradation of magnitude is passed through, an interior surface may be found that will satisfy the equation

$$C' = \varphi(x, y, z),$$

provided C' be any quantity between the limits C and C°. Wherefore if $C - C^{\circ}$ be divided into an infinite number of elementary portions, each equal to δp, there will exist a series of curve surfaces gradually contracting in their dimensions round the centre, and the last containing a drop of fluid, which may be as small as we please; of which successive curve surfaces, beginning with the upper surface of the mass, these are the respective equations:

$$C = \varphi(x, y, z), \text{ or } C = \varphi,$$
$$C - \delta p = \varphi,$$
$$C - 2\,\delta p = \varphi,$$
$$C - 3\,\delta p = \varphi, \text{ \&c.}$$



From A in the upper surface draw A A′ perpendicular to the surface next below; put $k = $ A A′, the thickness of the stratum; and let w denote any infinitely small portion of the curve surface at A′; then $k \times w$ will be the portion of the stratum insisting on the small surface w. The coordinates of the point A′ being x, y, z, the forces in action and respectively parallel to the coordinates will be

$$\frac{d\phi}{dx},\ \frac{d\phi}{dy},\ \frac{d\phi}{dz};$$

and by these forces we may suppose that every particle in the small mass $k \times w$ is urged. Now let

$$F = \sqrt{\left(\frac{d\Phi}{dx}\right)^2 + \left(\frac{d\Phi}{dy}\right)^2 + \left(\frac{d\Phi}{dz}\right)^2};$$

and by a well-known property, the cosines of the angles which the coordinates of the lower surface of the stratum make with the normal of the same surface, will be respectively equal to

$$\frac{1}{F} \times \frac{d\Phi}{dx}, \quad \frac{1}{F} \times \frac{d\Phi}{dy}, \quad \frac{1}{F} \times \frac{d\Phi}{dz};$$

and hence the sum of the partial forces acting in the direction of k, will be equal to

$$\frac{1}{F} \cdot \left(\frac{d\Phi}{dx}\right)^2 + \frac{1}{F} \cdot \left(\frac{d\Phi}{dy}\right)^2 + \frac{1}{F} \cdot \left(\frac{d\Phi}{dz}\right)^2 = F:$$

wherefore, $F \times k \times w$ will be the impulse or pressure exerted by the small mass $k \times w$ upon the small surface w, on which it insists. Again, the coordinates of the end of k in the upper surface of the stratum, are

$$x + \frac{k}{F} \cdot \frac{d\Phi}{dx}, \quad y + \frac{k}{F} \cdot \frac{d\Phi}{dy}, \quad z + \frac{k}{F} \cdot \frac{d\Phi}{dz};$$

and as the equations of the two curve surfaces are

$$C = \varphi \cdot \left(x + \frac{k}{F} \cdot \frac{d\Phi}{dx}, \ y + \frac{k}{F} \cdot \frac{d\Phi}{dy}, \ z + \frac{k}{F} \cdot \frac{d\Phi}{dz}\right),$$

$$C - \delta p = \varphi \cdot (x, y, z);$$

we deduce,

$$\delta p = \frac{k}{F} \cdot \left\{\left(\frac{d\Phi}{dx}\right)^2 + \left(\frac{d\Phi}{dy}\right)^2 + \left(\frac{d\Phi}{dz}\right)^2\right\} = k \times F.$$

Wherefore, the pressure $F \times k \times w$ of the mass $k \times w$ upon the small surface w, will be equal to $\delta p \times w$; which proves that the incumbent stratum exerts a constant pressure upon the surface passing through A′, the intensity at every point being equal to δp. The like demonstration may be employed to show, that any other stratum exerts a constant pressure upon the fluid below it; and hence it follows, that all the fluid above any of the interior surfaces, whatever be the number of strata it consists of, presses with the same intensity at every point of the surface. Now the forces urging the particles of the fluid decrease continually in approaching the centre of gravity, at which point they are evanescent: wherefore the infinitely small mass, or drop, contained within the surface nearest the centre, may be considered as free from the action of any accelerating forces; and, its surface being subjected to the constant pressure of all the incumbent strata, these pressures, the directions of which ultimately pass through the centre of gravity, will balance one another without any tendency to produce either progressive or rotatory motion.

If n be the number of strata above any of the interior surfaces, the intensity of pressure at all the points of the surface will be $n \times \delta p$; and the equation of the surface being

$$C - n \times \delta p = \varphi (x, y, z),$$

if $p = n \times \delta p$, we shall have

$$C - p = \varphi (x, y, z),$$

which equation ascertains the pressure at any point $(x y z)$, and determines the surface containing all the points at which the same pressure prevails. This agrees with what was investigated in No. 3.

The interior surfaces at all the points of which the pressure is constant have been called *level surfaces*; and a stratum of the fluid lying between two level surfaces is called a *level stratum.*

A property common to all the level surfaces, and to the upper surface of the fluid, consists in this, that the resultant of the forces acting upon a particle contained in any of these surfaces is directed perpendicularly towards it. Take two points, $(x y z)$ and $(x + \delta x, y + \delta y, z + \delta z)$, infinitely near one another in the surface of which the equation is

$$C - p = \varphi (x, y, z);$$

and put δs for the short line between the two points; by differentiating, $C - p$ being constant, we get

$$\frac{d\phi}{dx} \cdot \frac{dx}{ds} + \frac{d\phi}{dy} \cdot \frac{dy}{ds} + \frac{d\phi}{dz} \cdot \frac{dz}{ds} = 0;$$

or, which is equivalent,

$$X \frac{dx}{ds} + Y \frac{dy}{ds} + Z \frac{dz}{ds} = 0:$$

Now $\frac{dx}{ds}, \frac{dy}{ds}, \frac{dz}{ds}$ are the cosines of the angles which the directions of the forces make with the line ds: wherefore the algebraic expression in the last equation is the sum of the partial forces which act in the direction of ds; and as this sum is equal to zero in all positions of that line round the point $(x y z)$, the forces will produce no effect in the plane touching the curve surface, and will exert their whole action at right angles to the surface.

From what is here investigated, we may derive this general property: If the forces X, Y, Z, which vary from point to point, be always perpendicular to a surface, they must satisfy this equation,

$$X\,dx + Y\,dy + Z\,dz = 0,$$

the coordinates being made to vary in the surface. For if the equation be divided by $ds = \sqrt{dx^2 + dy^2 + dz^2}$, the result will be

$$X \frac{dx}{ds} + Y \frac{dy}{ds} + Z \frac{dz}{ds} = 0,$$

which expresses, as is shown above, that the whole action of the forces is perpendicular to the surface.

It may be observed further with respect to the level surfaces, that in forming their equations, nothing is supposed to change in the general equation

$$C - p = \varphi(x, y, z),$$

except the quantity $C - p$, which is constant in every individual surface, and the values of the coordinates, the form of the function $\varphi(x, y, z)$, and all the coefficients it contains, remaining immutably fixed. Every particular surface has, therefore, its independent equation, which is completely defined when the value of its constant is ascertained: and, as the equation of the upper surface determines the equilibrium of the whole mass of fluid, so, for the very same reasons, the equation of any interior level surface will determine the separate equilibrium of the fluid within it, supposing the constant pressure of the incumbent stratum to be taken off or annihilated.

The foregoing theorem, which is equivalent to the theory of Clairaut, cannot possibly be attended with any difficulty. But if the simplicity of the matter conduces to make it clear, it also greatly narrows its application. The theorem is sufficient for determining the equilibrium when the forces are explicit functions of the coordinates of the point of action; that is, such functions as are entirely known when the values of the coordinates are assigned. In this case, the differential equation of the surface must first be formed; and, this being integrated, we obtain the equation of the figure which the fluid must assume.

But the theorem is not sufficient for determining the equilibrium when a fluid consists of particles that mutually attract one another; because, in this case, the forces, varying with the figure of the fluid, are not explicit functions of the coordinates of the point of action; and because the expressions of the forces for a point in the surface of the fluid are in some respects different from the like expressions for a point within the surface, which is contrary to the hypothesis of the theorem. The problem thus assumes a new aspect, and further researches are required for its solution.

6. In the second division of problems, if the equation of the surface of a mass of fluid be

$$C = \varphi(x, y, z) \text{ or } C = \varphi,$$

the forces which urge the particles within the surface are expressed by the differential coefficients, viz.

$$\frac{d\varphi'}{dx}, \frac{d\varphi'}{dy}, \frac{d\varphi'}{dz},$$

of a function $\varphi'(x, y, z)$, which is different from $\varphi(x, y, z)$, for all the points within the surface, and identical with it for the points in the surface. The equilibrium requires that the forces acting upon the interior particles, or the differentials of $\varphi'(x, y, z)$, vanish at the origin of the coordinates in the centre of gravity; and this will not take place if $\varphi'(x, y, z)$ contain any terms such as Ax, By, Cz, the coefficients A, B, C being constant quantities. And since $\varphi'(x, y, z)$ is changed into $\varphi(x, y, z)$ when the coordinates have particular values, it follows that $\varphi(x,y,z)$ will contain no terms such as $A x$, $B y$, $C z$; and consequently that its differential coefficients, viz.

$$\frac{d\,\varphi}{d\,x}, \frac{d\,\varphi}{d\,y}, \frac{d\,\varphi}{d\,z},$$

will vanish at the centre of gravity. Wherefore, in all problems of this class, the foregoing theorem may be applied to the equation of the surface of the fluid, since the necessary conditions are fulfilled.

Now attending solely to the equation of the surface, viz.

$$C = \varphi(x,y,z),$$

it has been shown that the expressions

$$\frac{d\,\varphi}{d\,x}, \frac{d\,\varphi}{d\,y}, \frac{d\,\varphi}{d\,z},$$

represent forces respectively parallel to the coordinates, the resultant of which is directed perpendicularly towards the surface. If it be supposed that every particle of the fluid is urged by forces expressed by substituting its coordinates instead of the coordinates of the surface in the same functions $\frac{d\,\varphi}{d\,x}, \frac{d\,\varphi}{d\,y}, \frac{d\,\varphi}{d\,z}$, it is proved in the theorem that the mass will be in equilibrium, and may be divided by an infinite number of level surfaces into thin strata that exert a constant pressure upon one another. We have, therefore, now to inquire how the equilibrium which takes place when $\frac{d}{d\,x}, \frac{d\,\varphi}{d\,y}, \frac{d\,\varphi}{d\,z}$ are the forces in action, is to be preserved when, instead of these, the other forces, $\frac{d\,\varphi'}{d\,x}, \frac{d\,\varphi'}{d\,y}, \frac{d\,\varphi'}{d\,z}$, are substituted. These latter forces may be considered as produced by additions made to the first, and they may be thus written,

$$\frac{d\,\varphi}{d\,x} + \left(\frac{d\,\varphi'}{d\,x} - \frac{d\,\varphi}{d\,x}\right), \quad \frac{d\,\varphi}{d\,y} + \left(\frac{d\,\varphi'}{d\,y} - \frac{d\,\varphi}{d\,y}\right), \quad \frac{d\,\varphi}{d\,z} + \left(\frac{d\,\varphi'}{d\,z} - \frac{d\,\varphi}{d\,z}\right):$$

and supposing the whole body of fluid to be divided, as in the theorem, into thin level strata, to which the joint action of the forces $\frac{d\,\varphi}{d\,x}, \frac{d\,\varphi}{d\,y}, \frac{d\,\varphi}{d\,z}$ is at every point perpendicular, it is evident that the equilibrium will be destroyed when the additional forces come into action, unless their resultant, urging any particle, be perpendicular to the level surface in which the particle is contained; but if the resultant be perpendicular to the level surface, the equilibrium will not be disturbed, because the thin strata will still continue to exert a constant pressure upon one another in like manner as before the new forces were introduced *. However the additional forces

$$\left(\frac{d\,\varphi'}{d\,x} - \frac{d\,\varphi}{d\,x}\right), \quad \left(\frac{d\,\varphi'}{d\,y} - \frac{d\,\varphi}{d\,y}\right), \quad \left(\frac{d\,\varphi'}{d\,z} - \frac{d\,\varphi}{d\,z}\right)$$

* It is by means of this very general principle that we pass from the equilibrium of a homogeneous fluid to that of one in which the density, being constant at all the points of the same level surface, varies, according to any law, from one level surface to another.

be supposed to vary in passing from one level surface to another, there will be no tendency to destroy the equilibrium, provided their action be directed perpendicularly to every such surface. The perpendicularity of the resultant of the additional forces to a level surface is expressed by this equation,

$$\left(\frac{d\,\varphi'}{d\,x} - \frac{d\,\varphi}{d\,x}\right) d\,x + \left(\frac{d\,\varphi'}{dy} - \frac{d\,\varphi}{dy}\right) dy + \left(\frac{d\,\varphi'}{d\,z} - \frac{d\,\varphi}{d\,z}\right) d\,z = 0;$$

or more simply by this,

$$d\,.\,\varphi'\,(x, y, z) - d\,.\,\varphi\,(x, y, z) = 0,$$

the coordinates varying in the level surface.

We can now assign the conditions necessary for the equilibrium of a mass of homogeneous fluid at liberty, the particles of which are urged by the forces $\frac{d\varphi}{d\,x}$, $\frac{d\varphi}{dy}$, $\frac{d\varphi}{d\,z}$, at the surface, and by the forces $\frac{d\,\varphi'}{d\,x}$, $\frac{d\,\varphi'}{dy}$, $\frac{d\,\varphi'}{d\,z}$, within the surface; the functions $\varphi\,(x,y,z)$ and $\varphi'\,(x,y,z)$ being identical for all the points in the surface, and different from one another for all the points within the surface: first, the resultant of the forces in action at the surface must be directed perpendicularly towards that surface; and secondly, supposing the coordinates to vary from point to point of the same level surface, the differential equation

$$d\,.\,\varphi'\,(x, y, z) - d\,.\,\varphi\,(x, y, z) = 0$$

must be verified at all the points of the level surface.

In the hypothesis respecting the forces under consideration, there are two independent pressures at every interior point of the fluid; one caused by the forces $\frac{d\varphi}{d\,x}$, $\frac{d\varphi}{dy}$, $\frac{d\varphi}{d\,z}$, deduced from the equation of the upper surface of the fluid; and the other by the additional forces

$$\left(\frac{d\,\varphi'}{d\,x} - \frac{d\,\varphi}{d\,x}\right), \left(\frac{d\,\varphi'}{dy} - \frac{d\,\varphi}{dy}\right), \left(\frac{d\,\varphi'}{d\,z} - \frac{d\,\varphi}{d\,z}\right):$$

and the equilibrium of the fluid will be impossible unless the mass can be partitioned by an infinite number of surfaces, in every one of which the two pressures are both constant *. Now the pressure caused by the forces $\frac{d\,\varphi}{d\,x}$, $\frac{d\,\varphi}{dy}$, $\frac{d\,\varphi}{d\,z}$, is constant in all the surfaces called level surfaces in the theorem; and as these surfaces depend solely on the equation of the figure of the fluid, it is obvious that no figure can be induced on the mass that will secure the equilibrium, unless the pressure caused by the additional forces be likewise perpendicular to the same level surfaces. But if both pressures be constant at all the points of every level surface, which is the condition expressed by the equation

$$d\,.\,\varphi'\,(x, y, z) - d\,.\,\varphi\,(x, y, z) = 0,$$

* In no other way is it possible that the pressures propagated through the mass can balance and sustain one another.

the equilibrium of the fluid will obviously be a consequence of the theorem. It is therefore demonstrated, with respect to problems of the second class, that the equation of the upper surface of the fluid is not sufficient by itself to determine the equilibrium of the mass.

In the theorem, the term level surface is liable to no ambiguity; but in the more complex state of the forces that occurs in the second division of problems, two different systems of surfaces in which the pressure is constant require attention; for the pressure caused by the forces $\frac{d\varphi}{dx}, \frac{d\varphi}{dy}, \frac{d\varphi}{dz}$, is constant in all interior surfaces determined by the equation

$$d \,.\, \varphi\,(x, y, z) = 0;$$

and the pressure caused by the forces $\frac{d\varphi'}{dx}, \frac{d\varphi'}{dy}, \frac{d\varphi'}{dz}$, is constant in all surfaces of which the general equation is

$$d \,.\, \varphi'\,(x, y, z) = 0.$$

It will therefore conduce to clearness if the meaning of a level surface be restricted, by adding to the two properties of being perpendicular to the resultant of the forces acting on the particles contained in it, and being pressed at all its points with the same intensity, the further condition of being deduced by varying the constant in the equation of the upper surface of the fluid. The effect of the equation

$$d \,.\, \varphi'\,(x, y, z) - d \,.\, \varphi\,(x, y, z) = 0$$

is to verify the two differential equations above mentioned at all the points of the same surface: it implies that the two systems of surfaces of constant pressure are blended in one; and as this is a necessary condition of equilibrium, it distinguishes from all other figures those which are alone susceptible of an equilibrium.

7. The general theory of the equilibrium of homogeneous fluids at liberty having been explained at sufficient length, it is next to be applied to some of the principal problems.

Problem I.

To determine the equilibrium of a homogeneous fluid at liberty, the particles attracting one another with a force inversely proportional to the square of the distance, at the same time that they are urged by a centrifugal force caused by revolving about an axis.

The mass of fluid being in equilibrium, the centre of gravity will be free from the action of any forces; and as the attractive forces balance one another at that point, there must be no centrifugal force at the same point; that is, the axis of rotation must pass through it.

The origin of the coordinates being placed in the centre of gravity, let x, y, z, denote the rectangular coordinates of a particle of the fluid, and x', y', z', those of a molecule dm of the mass, the two coordinates x and x' being parallel to the axis of rotation: and f being the distance between the assumed particle and the molecule dm, we shall have

$$f = \sqrt{(x - x')^2 + (y - y')^2 + (z - z')^2}.$$

The attraction of spheres, according to the law supposed in this problem, being the same as if all the matter were collected in the centre, we may adopt for the unit of mass a sphere of the fluid having its radius equal to the unit of distance; and for the unit of force, the attraction of the sphere upon a point in its surface; then the direct attraction of the molecule $d\,m$ upon the particle at the distance f, will be $\frac{d\,m}{f^2}$; and the partial attractions urging the particle inward in the directions of x, y, z, will be respectively equal to

$$\frac{d\,m}{f^2} \cdot \frac{x - x'}{f},\ \frac{d\,m}{f^2} \cdot \frac{y - y'}{f},\ \frac{d\,m}{f^2} \cdot \frac{z - z'}{f}.$$

Now if we observe that

$$\frac{x - x'}{f} = \frac{df}{d\,x},\ \frac{y - y'}{f} = \frac{df}{d\,y},\ \frac{z - z'}{f} = \frac{df}{d\,z},$$

it will readily appear that the sums of the attractive forces, with which all the molecules of the mass urge the particle inward in the respective directions of x, y, z, may be thus commodiously expressed:

$$-\frac{d\,.\int\frac{d\,m}{f}}{d\,x},\ -\frac{d\,.\int\frac{d\,m}{f}}{d\,y},\ -\frac{d\,.\int\frac{d\,m}{f}}{d\,z},$$

the integral extending to all the molecules of the mass.

The attraction of the sphere at its surface being represented by unit, the velocity communicated by that force in the infinitely short time $d\,t$, will be $1 \times d\,t$; and if the time of one entire revolution about the axis of rotation be denoted by T, the velocity generated by the centrifugal force at the distance of unit from the axis in the time $d\,t$, will be $\frac{4\,\pi^2}{T^2} \times d\,t$; wherefore the centrifugal force acting at the distance of unit from the axis of rotation, and estimated in parts of the unit of force, will be equal to

$$\frac{4\,\pi^2}{T^2} = \varepsilon.$$

At the distance of $\sqrt{y^2 + z^2}$ from the axis, the centrifugal force will therefore be $\varepsilon \times \sqrt{y^2 + z^2}$; and the resolved parts of it which urge the particle in the prolongations of y and z, will be equal to $\varepsilon \times y$ and $\varepsilon \times z$.

Now if X, Y, Z represent the total forces tending inward and urging the assumed particle in the directions of the coordinates, we shall have

$$X = -\frac{d.\int\frac{d\,m}{f}}{d\,x},\ Y = -\left(\frac{d.\int\frac{d\,m}{f}}{d\,y} + \varepsilon\,y\right),\ Z = -\left(\frac{d.\int\frac{d\,m}{f}}{d\,z} + \varepsilon\,z\right);$$

wherefore, p denoting the intensity of pressure, we obtain

$$dp = \frac{d.\int\frac{dm}{f}}{dx}\,dx + \frac{d.\int\frac{dm}{f}}{dy}\,dy + \frac{d.\int\frac{dm}{f}}{dz}\,dz + \varepsilon\,(y\,dy + z\,dz);$$

and by integrating,

$$p = \int\frac{dm}{f} + \left(\frac{\varepsilon}{2}\,y^2 + z^2\right) - \mathrm{C}: \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

and from this the equation of the surface of the fluid is derived by making $p = 0$, viz.

$$0 = \left[\int\frac{dm}{f}\right] + \frac{\varepsilon}{2}\,(y^2 + z^2) - \mathrm{C}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

the brackets signifying that the inclosed integral is deduced from an attracted particle in the surface of the fluid.

The integral $\int\frac{dm}{f}$ in the last equations is the sum of all the molecules of the fluid mass divided by their respective distances from the attracted point. In equation (1.) the pressure p varies through all gradations of magnitude, from zero to the maximum value at the centre of gravity. The exact import of this equation is therefore attended with no difficulty.

The integral for a point or particle in the surface of the fluid, distinguished by brackets in equation (2.), is a particular or singular value of the general integral. When the attracted point is within the surface, the value of the integral depends not only upon the coordinates of that point, but also upon the limits of the integrations, which are determined by the equation of the surface of the fluid; but when the attracted point is in the surface, the expression of the integral is more simple, because it involves only the coordinates of the surface. The particular integral is obtained from the general one by substituting the coordinates of the surface; but the integral for a point within the surface cannot be derived by any change of coordinates, from the modified and singular form which the expression assumes at the surface. From this it follows that the level surfaces deduced from the equation of the upper surface of the fluid, are different from the interior surfaces determined by making p constant in equation (1.).

Relatively to the linear dimensions of the mass of fluid $d\,m$ is a quantity of three dimensions, and therefore the integral $\int\frac{dm}{f}$ being extended to all the molecules of the mass, is only of two dimensions. Let x, y, z, R represent the coordinates of an attracted point in the surface of the fluid, and the radius drawn from the same point to the centre of gravity; further, assume

$$a = \frac{x}{\sqrt{x^2 + y^2 + z^2}}, \quad b = \frac{y}{\sqrt{x^2 + y^2 + z^2}}, \quad c = \frac{z}{\sqrt{x^2 + y^2 + z^2}},$$

and the quantities a, b, c will depend only upon the angles that determine the direction of R: then $\left[\int \frac{dm}{f}\right]$ being a function of two dimensions, in which x, y, z are the only variables, $\frac{1}{R^2} \times \left[\int \frac{dm}{f}\right]$ will be a quantity of no dimensions; it will, therefore, be a function of $\frac{x}{R}, \frac{y}{R}, \frac{z}{R}$, or of a, b, c; so that we shall have

$$\left[\int \frac{dm}{f}\right] = R^2 \times F(a, b, c), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

F being the mark of a function. The same value may be expressed by means of the coordinates, viz.

$$\left[\int \frac{dm}{f}\right] = (x^2 + y^2 + z^2) \times F\left(\frac{x}{\sqrt{x^2 + y^2 + z^2}}, \frac{y}{\sqrt{x^2 + y^2 + z^2}}, \frac{z}{\sqrt{x^2 + y^2 + z^2}}\right).$$

If the value just found be substituted in equation (2.), the result will be,

$$C = (x^2 + y^2 + z^2) \times F\left(\frac{x}{\sqrt{x^2 + y^2 + z^2}}, \frac{y}{\sqrt{x^2 + y^2 + z^2}}, \frac{z}{\sqrt{x^2 + y^2 + z^2}}\right) + \frac{\varepsilon}{2} \times (y^2 + z^2):$$

which proves that the forces in action at the surface of the fluid are not sufficient to determine the equilibrium of the mass. For the equation of the figure of the fluid at which we have arrived, containing an arbitrary function, is indeterminate; and, on examination, it will be found to comprehend the ellipsoid and innumerable other figures*.

If for x, y, z we substitute their values Ra, Rb, Rc, the equation of the surface will assume this form,

$$C = R^2 \times \left\{ F(a, b, c) + \frac{\varepsilon}{2} (b^2 + c^2) \right\}.$$

The equation of a level surface is deduced from the equation of the upper surface by changing the constant, and substituting the coordinates of the level surface for those of the upper surface: now, supposing that r, in the same straight line with R, is a radius of a level surface, the coordinates of the point in that surface at the extremity of r will be ra, rb, rc, because r and R have the same direction: wherefore, by substituting ra, rb, rc for x, y, z in the equation of the upper surface, and denoting the new constant by C′, the equation of the level surface of which r is the radius will be

$$C' = r^2 \times \left\{ F(a, b, c) + \frac{\varepsilon}{2} (b^2 + c^2) \right\}.$$

The comparison of this equation with that of the upper surface of the fluid leads to this result,

$$\frac{r^2}{R^2} = \frac{C'}{C};$$

* In a particular examination of CLAIRAUT's theory that occurs in the sequel of this Paper, it is proved from different principles, that the equation of the figure of the fluid deduced from the forces in action at the surface is indeterminate, and admits of innumerable solutions.

from which it follows, that every interior level surface is similar to the upper surface, and similarly posited about the centre of gravity.

The expression of the integral in equation (3.) is evidently true in all similar spheroids, without any change in the function F; for F, being of no dimensions, contains only the proportions of the linear dimensions of the geometrical figures, and these proportions are the same when the figures are similar. And, since all the level surfaces are similar to the upper surface, it is obvious that the equation of a level surface may be thus expressed,

$$C' = \left[\int \frac{dm}{f}\right] + \frac{\varepsilon}{2} \cdot (y^2 + z^2):$$

because the integral between the brackets, which stands for the sum of all the molecules within the level surface divided by their respective distances from a point $(x\,y\,z)$ in that surface, is equal to the part of the equation of the level surface which contains the function F. Now the equilibrium of the mass of fluid will be impossible, unless the pressure determined by the equation (1.) be constant at all the points of the same level surface; which requires that the equation

$$d \cdot \left\{ \int \frac{dm}{f} + \frac{\varepsilon}{2} (y^2 + z^2) \right\} = d \cdot \left\{ \left[\int \frac{dm}{f}\right] + \varepsilon\, (y^2 + z^2) \right\}$$

be verified, the coordinates of the attracted point varying in any level surface*. This differential equation will be fulfilled if the equation

$$\text{constant} = \int \frac{dm}{f} - \left[\int \frac{dm}{f}\right]$$

hold at all the points of every level surface. And as the integral without brackets is the sum of all the molecules of the whole mass of fluid, divided by their respective distances from the attracted point in the level surface; and the integral with brackets is the like sum relatively to all the molecules within the level surface; the last equation may be expressed more simply thus,

$$\text{constant} = \int \frac{dm}{f},$$

the integral being extended to all the molecules of the stratum between the level surface and the upper surface of the fluid. In the figures which verify this equation there will exist in the interior parts no surfaces of constant pressure except the level surfaces, which is a necessary condition of equilibrium; and the intensity of pressure in every level surface will be determined by the equation (1.), as required in the problem.

We have next to investigate the figures which verify this last equation. Let s represent the distance of dm from the centre of gravity; and, r being drawn to an

* That is, of every surface determined by varying the constant in the equation of the upper surface, according to the definition in No. 6.

attracted point in a level surface, put θ and θ' for the angles which r and s make with the axis of rotation; and ϖ and ϖ' for the angles which determine the positions of the projections of r and s upon a plane passing through the centre of gravity perpendicular to the same axis: then ψ being the angle between the two lines r and s, and f the distance of $d\,m$ from the attracted point, we shall have

$$\gamma = \cos\psi = \cos\theta\cos\theta' + \sin\theta\sin\theta'\cos(\varpi - \varpi'),$$
$$f = \sqrt{s^2 - 2\,s\,r\,.\,\gamma + r^2}.$$

Again, if the plane of the two lines r and s describe the small angle $d\,\sigma$ by revolving about r, the extremity of s will describe the short line $s\cos\psi\,d\,\sigma$ perpendicular to the revolving plane: further, supposing that the arc ψ increases to $\psi + d\,\psi$, the extremity of s will move through the short line $s\,d\,\psi$ in the plane of the arc ψ; now the short lines $s\cos\psi\,d\,\sigma$ and $s\,d\,\psi$ being perpendicular to one another and to s, the molecule $d\,m$ may be considered equal to $s\cos\psi\,d\,\sigma \times s\,d\,\psi \times d\,s$; or, which is the same thing, we may assume

$$d\,m = -\,d\,\gamma\,d\,\sigma\,.\,s^2\,d\,s.$$

By substituting the values of $d\,m$ and f, the integral under consideration will be thus expressed:

$$\int\frac{d\,m}{f} = \iint -\,d\,\gamma\,d\,\sigma\int\frac{s^2\,d\,s}{\sqrt{s^2 - 2\,s\,r\,.\,\gamma + r^2}},$$

the integrations being extended to all values from $\gamma = 1$, $\sigma = 0$, to $\gamma = -1$, $\sigma = 2\,\pi$, and from $s = r'$, to $s = R'$, r' and R' being two radii in the same straight line, the first of a level surface, and the other of the upper surface of the fluid. The radical quantity must now be expanded in a series of the powers of $\frac{r}{s}$, viz.

$$\frac{1}{s} + \frac{r}{s}\,.\,C^{(1)} + \frac{r^2}{s^3}\,.\,C^{(2)} + \frac{r^3}{s^3}\,.\,C^{(3)} +, \&c.,$$

the coefficients being determined by the formula

$$C^{(i)} = \frac{1}{2^i} \times \frac{d^i\,(\gamma^2 - 1)^i}{1\,.\,2\,.\,3\,\ldots\,i\,d\,\gamma^i}:*$$

and having substituted this series, and effected the integrations with respect to $d\,s$ between the assigned limits, the result will be

$$\int\frac{d\,m}{f} = \frac{1}{2}\iint -\,d\,\gamma\,d\,\sigma\,(R'^2 - r'^2)$$
$$+\,r\iint -\,d\,\gamma\,d\,\sigma\,(R' - r')\,C^{(1)}$$
$$+\,r^2\iint -\,d\,\gamma\,d\,\sigma\,\log\frac{R'}{r'} \times C^{(2)}$$

* Vide Appendix.

$$+ r^3 \iint - d\gamma\, d\sigma \left(\frac{1}{r'} - \frac{1}{R'}\right) C^{(3)}$$

$$+ \frac{r^4}{2} \iint - d\gamma\, d\sigma \left(\frac{1}{r'^2} - \frac{1}{R'^2}\right) C^{(4)}$$

$$\vdots$$

$$\frac{r^i}{i-2} \iint - d\gamma\, d\sigma \left(\frac{1}{r'^{i-2}} - \frac{1}{R'^{i-2}}\right) C^{(i)}.$$

Because every level surface is similar to the upper surface, and similarly posited about the centre of gravity, and that r and R, as well as r' and R$'$, are radii of the two surfaces in the same straight line, we have

$$r = \alpha \,.\, R, \quad r' = \alpha \,.\, R',$$

α being a fraction of unit, which is the same for all the points of the same level surface; wherefore, by substituting the values of r and r', and leaving out the term

$$r^2 \iint - d\gamma\, d\sigma \log \frac{R'}{r'} \times C^{(2)} = R^2 \alpha^2 \log \frac{1}{\alpha} \,.\, \iint - d\gamma\, d\sigma\, C^{(2)},$$

which is equal to zero, we get

$$\int \frac{dm}{f} = \frac{1-\alpha^2}{2} \iint - d\gamma\, d\sigma \,.\, R'^2$$

$$+ (\alpha - \alpha^2)\, R \iint - d\gamma\, d\sigma\, C^{(1)} \,.\, R'$$

$$+ (\alpha^2 - \alpha^3)\, R^3 \iint \frac{- d\gamma\, d\sigma\, C^{(3)}}{R'}$$

$$- \frac{(\alpha^2 - \alpha^4)\, R^4}{2} \,.\, \iint \frac{- d\gamma\, d\sigma\, C^{(4)}}{R'^2}$$

$$\vdots$$

$$\frac{(\alpha^2 - \alpha^i)\, R^i}{i-2} \iint \frac{- d\gamma\, d\sigma\, C^{(i)}}{R'^{i-2}}.$$

Such is the expression of the integral under consideration, the attracted point being the intersection of R, with the level surface of which α R is a radius; and the value of the integral must be constant at all the points of the same level surface, that is, it must be the same when α is the same in whatever direction R be drawn.

In the first place, if the figure of the fluid be a sphere, the expression of $\int \frac{dm}{f}$ is reduced to its first term, which is constant in every spherical surface concentric with the upper surface; because by the nature of the functions $C^{(i)}$ all the integrals vanish when R' is constant. But the supposition of a sphere requires that ε be equal to zero in the equations (1.) and (2.), or that there be no centrifugal force.

But if the radius R vary as it changes its direction, $\int \frac{dm}{f}$ cannot be of the same quantity at every point of the same level surface, except when all the terms after the first are separately equal to zero, that is, except the expression of R' be such that

$$\iint \frac{-d\gamma\, d\sigma\, C^{(i)}}{R^{i-2}} = 0,$$

for all values of i from 1 to ∞.

The investigation will be greatly facilitated by the following theorem:

If $a' = \cos \theta'$, $b' = \sin \theta' \cos \varpi'$, $c' = \sin \theta' \sin \varpi'$, the integral

$$\iint - d\gamma\, d\sigma\, C^{(i)}\, a'^{m}\, b'^{m'}\, c'^{m''},$$

extended to all values of γ from 1 to -1, and of σ from 0 to 2π, will be equal to zero in all cases when $m + m' + m''$ is less than i*.

It is obvious that $\frac{1}{R'^2}$ is a function of the three quantities a', b', c'; and if we assume

$$\frac{1}{R'^2} = U^{(0)} + U^{(1)} + U^{(2)},$$

$U^{(0)}$ being a constant, and $U^{(1)}$, $U^{(2)}$ functions such that a', b', c' rise to one dimension in all the terms of $U^{(1)}$, and to two dimensions in all the terms of $U^{(2)}$, the highest sum of the indexes in the combinations of a', b', c', contained in the expressions of $\frac{1}{R'^4}$, $\frac{1}{R'^6}$, $\frac{1}{R'^8}$, &c., will not exceed 4, 6, 8, &c.: wherefore, by the theorem, the assumed value of $\frac{1}{R'^2}$ will succeed in making all the terms of $\int \frac{dm}{f}$ vanish in which i is an even number, and it is evidently the most general assumption for $\frac{1}{R'^2}$ that will answer the same end.

When i is an odd number, we have

$$\iint \frac{-d\gamma\, d\sigma\, C^{(i)}}{R'^{i-2}} = \iint \frac{-d\gamma\, d\sigma\, C^{(i)}}{(U^{(0)} + U^{(1)} + U^{(2)})^{\frac{i-2}{2}}}.$$

In this case $C^{(i)}$, being an odd function of γ, is the same in quantity, but changes its sign, when for θ' and ϖ' we substitute $\theta' + \frac{\pi}{2}$ and $\varpi' + \pi$: wherefore the whole integral will be equal to zero, if the denominator retain the same positive value when θ' and ϖ' are changed into $\theta' + \frac{\pi}{2}$ and $\varpi' + \pi$, the increase of the integral being, on

* Vide Appendix.

this supposition, exactly compensated by the decrease. But this requires that $U^{(1)}$ be exterminated, because this function varies its sign when θ' and ϖ' are changed into $\theta' + \frac{\pi}{2}$ and $\varpi' + \pi$. Wherefore, leaving out $U^{(1)}$, if we assume

$$\frac{1}{R'^3} = U^{(0)} + U^{(2)},$$

it will follow from what has been said, that all the terms of $\int \frac{dm}{f}$ after the first, both those in which i is even and those in which it is odd, will vanish, so that we shall have

$$\int \frac{dm}{f} = \frac{1 - \alpha^2}{2} \int\int - d\gamma\, d\sigma\, R'^2,$$

which is constantly of the same value at all the points of the same level surface.

Taking the most general expression of $U^{(2)}$, and observing that the constant

$$U^{(0)} = U^{(0)} a'^2 + U^{(0)} b'^2 + U^{(0)} c'^2,$$

may be blended with $U^{(2)}$, we shall have

$$\frac{1}{R'^3} = A\, a'^2 + B\, b'^2 + C\, c'^2 + D\, a'\, b' + E\, a'\, c' + F\, b'\, c' :$$

but x', y', z' being the coordinates of R' in the surface of the fluid, we have

$$\frac{x'}{R'} = a', \ \frac{y'}{R'} = b', \ \frac{z'}{R'} = c' :$$

and these values being substituted, the result will be

$$1 = A\, x'^2 + B\, y'^2 + C\, z'^2 + D\, x'\, y' + E\, x\, z' + F\, y'\, z',$$

which is the equation of an ellipsoid, the coordinates x', y', z' being parallel to three diameters intersecting at right angles. It is therefore demonstrated, that the ellipsoid comprehends all the figures that will make the integral $\int \frac{dm}{f}$, taken between the assigned limits, of the same value at all the points of the same level surface, that is, at all the points of any interior surface similar to the upper surface, and similarly posited about the centre.

The foregoing reasoning is independent of the centrifugal force; but by attending to the rotatory motion which causes that force, it is easy to prove that the axis about which the fluid revolves, or the diameter parallel to the coordinate x', must coincide with one of the axes of the geometrical figure. For, there being no centrifugal force at the poles of the axis of rotation in the surface of the fluid, the only force in action at these points is the attraction of the mass. But the resultant of the forces urging every particle in the surface of a fluid in equilibrium must be perpendicular to the surface: and as there are no points on the surface of an ellipsoid at which the attraction of the mass is perpendicular to the surface, except the extremities of the three axes, it follows that, with one or other of these, the axis of rotation of the fluid in equilibrium must coincide. The diameter parallel to x' being thus proved to be an axis of the ellipsoid, we may assume that the other two coordinates are parallel to the remaining axes of the geometrical figure, in consequence of which the equation of the surface will become more simple, viz.

$$1 = \frac{x^2}{k^2} + \frac{y^2}{k'^2} + \frac{z^2}{k''^2},$$

the three semiaxes being k, k', k'', of which k is the axis of rotation.

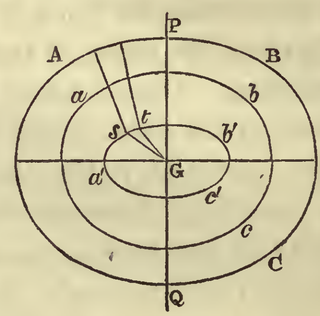
Further, the figure of the fluid in equilibrium can be no other than a spheroid of revolution. Draw a plane through the axis of rotation and any point $(x\ y\ z)$ in the surface of the fluid. This plane will contain that part of the attraction of the spheroid which is parallel to the axis of rotation, or to the coordinate x: it will also contain the centrifugal force directed at right angles from the axis of rotation. The same plane will also contain the resultant of the attractions parallel to y and z; for if it did not, the resultant might be resolved into two forces, one contained in the plane, and the other perpendicular to it; and the force perpendicular to the plane would partly act in a direction touching the surface of the spheroid, which is inconsistent with the equilibrium of the fluid. Wherefore, the whole attractive force at any point in the surface of the spheroid is contained in a plane passing through the point and the axis of rotation; which obviously excludes ellipsoids with three unequal axes, and limits the figures of equilibrium to spheroids formed by the revolution of an ellipsis about the axis of rotation; and as the centrifugal force necessarily causes the equatorial diameter to be longer than the polar axis, it follows that the figure of the fluid in equilibrium can be no other than an oblate elliptical spheroid of revolution, of which the equation is

$$k^2 = x^2 + \frac{k'^2}{k^2}(y^2 + z^2),$$

the fluid turning about k, the less axis.

By the foregoing investigation, the problem for determining the equilibrium of a homogeneous planet in a fluid state is reduced to solving the equation of the upper surface, which is an expression of a known form, as the figure of the fluid is ascertained. The equation of the upper surface adjusts the oblateness of the spheroid to the quantity of the centrifugal force. It is only this part of the problem which, if we judge rightly, is fairly made out in the modes of investigation usually adopted; for in all these it is assumed that the figure of the fluid is an oblate elliptical spheroid, but, except in Maclaurin's demonstration, the equilibrium is not proved on satisfactory grounds. D'Alembert first observed, that in general more spheroids than one may be in equilibrium with the same centrifugal force, or with the same velocity of rotation; and it is now well known that there may be two such spheroids, or one only, or that no spheroid of the proposed matter can be found that will be in equilibrium with the given quantity of centrifugal force. All this is pure mathematical deduction from an algebraic equation; it is attended with no difficulty, and is very fully discussed by all the authors who have written on the figure of the earth; it would, therefore, be superfluous to treat of it here; but it may not be improper to add a few words for the purpose of explaining in what manner the number of solutions of the problem is limited by the nature of the equilibrium.

Let A B C represent an oblate elliptical spheroid of homogeneous fluid in equilibrium by revolving about the axis P Q; and *a b c*, an interior level surface, which is therefore similar to the upper surface A B C, and similarly posited about the centre: the stratum between the two surfaces will act upon the fluid within the level surface in two ways, namely, by pressure and by attraction. From the nature of the spheroid, the attraction of the stratum upon every particle within the level surface is zero; and the pressure of the exterior fluid acts upon every point of the same surface with equal intensity: wherefore, the whole mass A B C being in equilibrium, if the stratum be taken off, the remaining body of fluid *a b c* will be in equilibrium separately. But another spheroid, $a' b' c'$, of a different form, may be traced within A B C, the less axes and the equators of the two figures coinciding, such that it will remain in equilibrium separately, upon abstracting the exterior fluid. Every small portion *s t* of the surface $a' b' c'$ is pressed inward by the exterior fluid; it also sustains a pressure from within outward, caused by the attraction of the fluid on the outside of the surface $a' b' c'$ upon the particles within that surface. Now, although each of the two contrary pressures varies from one point of the surface to another, yet the spheroid may be so determined, that their joint action, or their difference, shall be the same at every point of the surface. When the spheroid $a' b' c'$ has this figure, it will be in equilibrium with respect to the action of the exterior fluid; and, if that be abstracted, it will be in equilibrium separately, because the whole mass A B C is in equilibrium. What has been said may easily be proved by calculation; for the spheroid A B C being given, we know the pressure of the exterior fluid upon *s t*; we know also the attraction of the exterior fluid upon a particle of the spheroid $a' b' c'$, for it is equal to the difference of the attractions of the spheroids A B C and $a' b' c'$ upon the particle: and hence it is easy to deduce, that the relation between the oblateness and the centrifugal force is expressed by the same equation in the spheroid $a' b' c'$ and in the level surfaces.



It thus appears, that in general there are two spheroids of the same matter, but not more than two, which will be in equilibrium with the same rotatory velocity. If the oblateness of A B C increase, that of $a' b' c'$ will decrease; and the two spheroids continually approaching the same figure, they will ultimately coincide in a limit at which there is only one form of equilibrium. On the other hand, as A B C becomes more nearly spherical, $a' b' c'$ will be more and more flattened; so that, the centrifugal force being zero and A B C a perfect sphere, $a' b' c'$ will be an infinitely thin circle of fluid particles in the plane of the equator.

The problem that has been solved leads to a consideration which it is important to notice, because it relates to a principle of equilibrium that has been very generally adopted. It has been shown that the equation of the surface (2.) is indeterminate, and admits of innumerable solutions; but in every figure which satisfies that equation, the other equation (1.), viz.

$$p = \int \frac{dm}{f} + \frac{\varepsilon}{2}(y^2 + z^2) - C,$$

will hold at every interior point $(x\,y\,z)$ of the mass of fluid. In this latter equation, p is the pressure of any canal issuing from the point $(x\,y\,z)$ and extending to the surface of the fluid; and therefore, in every figure which satisfies the equation of the surface, every such canal will exert the same pressure upon a molecule placed at the point $(x\,y\,z)$. Now of the innumerable figures that satisfy the equation of the surface there is only one that is in equilibrium; and thus it is proved, that a mass of fluid, without being in equilibrium, may assume many figures in which every interior particle is pressed with equal intensity by all the canals issuing from it and terminating in the surface. And as neither the equation of the surface, nor the equal pressure of all the canals extending from a molecule to the surface, is sufficient to secure the equilibrium except when the forces are explicit functions of the coordinates; so neither of the two properties can be employed in any other hypothesis respecting the forces, to verify an equilibrium, that is, to prove that a proposed figure will be in equilibrium.

8. In the following problem the forces in action are known functions of the coordinates, and the solution is deduced immediately from the theorem in No. 5.

Problem II.

To determine the figure of equilibrium of a fluid at liberty, the particles being supposed to attract one another with a force directly proportional to the distance, at the same time that they are urged by a centrifugal force caused by revolving about an axis.

As the attractions of the particles balance one another at the centre of gravity, in order to free that point from the action of any forces the axis of rotation must pass through it.

Let x, y, z denote the coordinates of an attracted particle, and x', y', z' those of an element dm of the mass, the origin being at the centre of gravity, and x, x' being parallel to the axis of rotation; adopting for the unit of mass the whole given mass of fluid, and for the unit of force the attraction of the whole mass collected in a point upon a particle at the distance 1, the attraction of dm upon the assumed particle at the distance f will be $f\,dm$; and the cosines of the angles which f makes with x, y, z being

$$\frac{x-x'}{f}, \frac{y-y'}{f}, \frac{z-z'}{f},$$

the partial attractions, directed inward, and parallel to x, y, z, will be

$$dm\,(x-x'),\ dm\,(y-y'),\ dm\,(z-z');$$

and, by integrating, the sums of the like attractions of all the molecules of the mass are obtained, viz.

$$x\int dm - \int x'\,dm,\ y\int dm - \int y'\,dm,\ z\int dm - \int z'\,dm.$$

Now, by the property of the centre of gravity, we have

$$\int x'\,dm = 0,\ \int y'\,dm = 0,\ \int z'\,dm = 0:$$

wherefore, the attractions of the whole mass respectively parallel to x, y, z will be equal to

$$x\int dm,\ y\int dm,\ z\int dm,$$

or simply to x, y, z, because $\int dm$ is the unit of mass.

Let ε denote the centrifugal force at the distance 1 from the axis of rotation, and estimated in parts of the unit of force; then the action of this force urging the particle in the prolongation of y and z will be equal to εy and εz.

Now, if X, Y, Z denote the whole accelerating forces acting parallel to x, y, z, we shall have

$$X = x,\ Y = (1-\varepsilon)\,y,\ Z = (1-\varepsilon)\,z;$$

which forces are therefore known functions of the point of action. Representing the intensity of pressure by p, we obtain

$$-dp = x\,dx + (1-\varepsilon)\,.\,(y\,dy + z\,dz);$$

and, by integrating,

$$C - p = \frac{x^2}{2} + (1-\varepsilon)\,.\,\frac{y^2+z^2}{2},$$

which equation determines the pressure at the interior points of the fluid. The equation of the figure of the mass in equilibrium is obtained by making $p = 0$, viz.

$$C = \frac{x^2}{2} + (1-\varepsilon)\,.\,\frac{y^2+z^2}{2}.$$

Supposing, therefore, that ε is less than 1, or that the centrifugal force at the distance 1 from the axis of rotation is less than the attraction of the mass collected in a point at the same distance, the fluid in equilibrium will have the form of an oblate elliptical spheroid of revolution.

As this problem is different from the first only in the law of attraction, it may be alleged that the methods of solution should be similar. There would be no difficulty in applying to it the same investigation employed in the first problem; but in whatever manner we proceed, the distinction between the two cases will remain unchanged.

In the second problem, the forces acting upon a particle within the surface are the same functions of the coordinates as the like forces acting upon a particle in the surface; because the forces which urge a particle in any situation depend only on the whole mass of fluid, and the distance of the particle from the centre of gravity. But in the first problem, if we except the particular class of figures susceptible of an equilibrium, the finding of which is an additional condition to be investigated, the forces urging a particle within the surface are not deducible from the forces at the surface merely by changing the coordinates of the point of action.

9. To complete the theory in this paper, it would be necessary to determine the figure of equilibrium of a revolving mass of homogeneous fluid, on the supposition that the particles attract one another with a force varying as any power of the distance. The solving of this problem would enable us to decide whether the equilibrium be possible in any other law of attraction but the direct proportion of the distance, or the inverse proportion of the square of the distance. The principles that have been laid down are sufficient to solve the problem enunciated in this general manner; but the application of them would require mathematical discussions too extensive to be entered upon at present. To conclude this paper, some observations will be made that seem to be called for by the notions that prevail on the subject of which it treats.

On Maclaurin's *Demonstration of the Equilibrium of the oblate elliptical Spheroid.*

In treating of the figure of the earth, Newton begins with observing that a homogeneous mass of fluid, supposing its particles urged only by their mutual attraction, would arrange itself in a form perfectly spherical. If this sphere acquire a revolving motion about one of its diameters, a new force will be impressed on its particles, causing them to recede from the axis of rotation; and, in obedience to this force, the fluid will subside at the poles and dilate itself in the direction parallel to the equator. Newton assumes, without alleging any reason in support of his assumption, that the revolving fluid will permanently settle in an oblate elliptical spheroid. Admitting tacitly that this is the figure of equilibrium, he proves that the relative dimensions of the spheroid depend upon the proportion of the centrifugal force to gravity at the equator; and this proportion being ascertained by experiment in the case of the earth, he finds that the equatorial diameter is to the polar axis as 230 to 229. The whole of this speculation, when published in the Principia, was entirely new; it involves many points of difficult investigation; and the ability has always been admired by which the difficulties are either overcome or evaded by ingenious approximations sufficiently exact and requiring the least possible calculation. But this splendid theory was incomplete till it should be proved that a fluid sphere turning upon an axis must assume the form of an elliptical spheroid. The attention of geometers was therefore turned to this point. The subject was treated by Mr. James

Stirling in 1735, and by Clairaut in 1737, but only on the supposition of a spheroid little different from a sphere; and the results obtained by these geometers perfectly coincided with the determination of Newton. In a dissertation on the tides, which shared the prize of the Academy of Sciences of Paris in 1740, Maclaurin made a great addition to the Newtonian theory, by proving that any proposed elliptical spheroid of homogeneous fluid would be in equilibrium if it revolved about its less axis with a certain rotatory velocity, and by introducing in his demonstration accurate notions respecting the conditions required for the equilibrium of a fluid entirely at liberty.

If an oblate elliptical spheroid of homogeneous fluid revolve about the less axis, the equilibrium of the mass will be secured if the resultant of the attractive and centrifugal forces acting upon a particle in the surface be directed perpendicularly towards the surface. In order to prove this, suppose that innumerable surfaces are described within the spheroid, similar to the upper surface, and similarly posited about the centre, and it will be easy to prove with respect to a particle in any of the interior surfaces, that the resultant of its centrifugal force, and of the attraction upon it of all the matter within the surface in which it is placed, is perpendicular to that surface. Now it is proved in the Principia that all the matter between the upper surface and any of the interior surfaces exerts no attraction upon a particle either in or within that surface; and hence it follows that the resultant of the centrifugal force of a particle, and the attraction upon it of all the matter of the spheroid, is perpendicular to the interior surface passing through the particle. The interior surfaces are therefore the true level surfaces of the spheroid, and the equilibrium of the revolving mass is establishsd by the reasoning in the theorem in No. 5. From this demonstration it would appear that the Newtonian property, according to which the matter of a homogeneous stratum bounded by two similar and concentric elliptical surfaces does not attract a particle within the stratum, is not merely accidental to the equilibrium, but a condition necessary to its existence.

The equilibrium of the oblate spheroid may be made out by a different process. The attraction of the mass upon one of its particles may be investigated; and, when this done, it is found that the attractions parallel to the equator and perpendicular to the same plane, are proportional to the respective distances of the particle from the axis of rotation and from the equator. It thus appears that the forces urging any particle are known expressions of the coordinates of the point of action; and therefore the solution of the problem is immediately deduced from the theorem in No. 5. Now in this procedure there is no direct mention made of the Newtonian property; and hence it may, perhaps, be alleged that it is not essential to the equilibrium, although it is a principal step in the former demonstration. But a little reflection will show that the property in question is a condition no less necessary in this than in the former investigation; for it is by means of it that the forces acting upon a particle are disengaged from the upper surface of the fluid, the boundary of the attracting mass, and are brought to depend entirely upon the situation of the particle with respect to the equator and the axis of rotation. This second investigation, therefore, concurs with the first, in proving that the Newtonian property is necessary to the equilibrium of the spheroid, and not merely accidental.

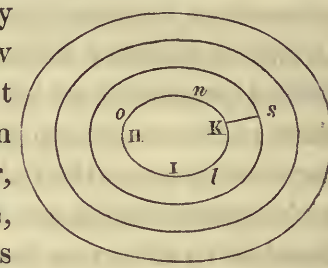
Maclaurin's demonstration is different in some respects from either of the two investigations that have been mentioned. He requires three separate conditions for the equilibrium: first, the resultant of the centrifugal force and the attraction of the mass, must be perpendicular to the surface of the spheroid; secondly, every particle must be pressed equally in all directions; thirdly, all the columns reaching from the centre to the upper surface must balance and sustain one another. Now if the first of these conditions be fulfilled, and that too whether the mass of fluid be an elliptical spheroid or have any other figure, the other two will follow as necessary consequences. It may be observed further, that a demonstration proceeding on an arbitrary enumeration of properties, which may not be complete, makes a vague impression, and falls short of the conviction produced by a proof that rests on determinate principles bearing directly upon the point to be investigated. The conditions essential to Maclaurin's demonstration are only these two: first, the attraction upon a particle proportional to its distances from the equator and the axis of rotation, which is peculiar to ellipsoids, and necessarily connected with the Newtonian property; secondly, the perpendicularity to the upper surface of the resultant of the forces acting upon a particle contained in that surface: and notwithstanding the beautiful train of reasoning employed by the author, his demonstration would gain in precision and clearness by omitting all that relates to the superfluous properties.

Clairaut's *Theory*.

To Clairaut belongs an important part of the theory of the figure of the earth. He was the first that entertained correct notions respecting the effect to alter the form of the terraqueous globe, produced by heterogeneity in its structure. At present we confine our attention to his general equations of the equilibrium of fluids, and their application to the case of a homogeneous planet. His theory is constructed with great analytical skill, and is seducing by its conciseness and neatness. From the single expression of the hydrostatic pressure are derived the equations of all the level surfaces, and of the upper surface of the fluid. But these equations are not sufficient in all cases to solve the problem. They are sufficient to solve it when the forces are known algebraic expressions of the coordinates of the point of action: they are not sufficient when the forces are not explicitly given, but depend, as in a homogeneous planet, on the assumed figure of the fluid. In this latter case, the solution of the problem requires, further, that the equations be brought to a determinate form by eliminating all that varies with the unknown figure of the fluid.

In the theory of Clairaut it is tacitly assumed that the forces urging the interior particles are derived from the forces at the upper surface merely by changing the coordinates of the point of action *. Now there are cases, and the homogeneous planet is one, in which the forces acting on the interior particles are not deducible, in the manner supposed, from the forces at the surface; and with respect to such problems, the theory is silent, and has provided no means of solution.

But it will be satisfactory, and it is not difficult, to acquire just notions respecting Clairaut's theory, by a careful examination of the principles as they are laid down by the author, for whose great abilities and high pretensions as a discoverer in science we entertain the sincerest respect, although we dissent from him on some points. The French geometer assumes for the foundation of his superstructure a mass of fluid, H K I, in equilibrium †. If f represent the force perpendicular to the surface of H K I, at any point K, and k the thickness K s of an additional stratum $o\,n\,l$; and if the stratum be so determined that $k \times f$ shall have constantly the same value at all the points of the surface; it will follow that the pressure of the stratum upon the surface on which it lies, is constant; and hence the body composed of the stratum and the original mass will be in equilibrium. In like manner, if a second stratum be added to the new body in equilibrium, the thickness being determined by the same condition as before, a third body of fluid in equilibrium will be obtained, consisting of two strata and the central mass. By adding more strata indefinitely, the dimensions of the mass of fluid may be enlarged to any extent, at the same time that the conditions of equilibrium are continually preserved. In all this it is evidently supposed that no change in the figure of the successive surfaces is effected by the strata laid upon them; for without this admission the procedure would be nugatory, and could lead to no determinate conclusion.



The investigation of Clairaut is very elegant and geometrical, and carries with it the clearest evidence. It is entirely consonant to the theorem in No. 5. When it is not extended beyond its proper assumptions, it leads to a sure, and in truth to the only satisfactory principle of the equilibrium of a mass of fluid at liberty. It assumes that the pressure of every new stratum upon the surface on which it is laid, is caused solely by the forces in action at that surface, these forces being supposed to exert the same energy on all the particles of the infinitely small thickness of the stratum, and the thickness being so determined as to make the pressure constant. The procedure is agreeable to the usual rules of mathematical investigation, according to which the forces are conceived, not to flow continuously as the coordinates increase, but to vary from surface to surface by infinitely small gradations. Now this is very

* When the forces acting upon the interior particles assume *singular forms* of expression at the surface, Clairaut's theory fails; and this makes the distinction in the text necessary. But the whole theory, founded on an assumed principle, or upon an algebraic equation which determines the effect of the forces upon a particle taken individually, is so loosely delivered that it is difficult to speak of it with precision.

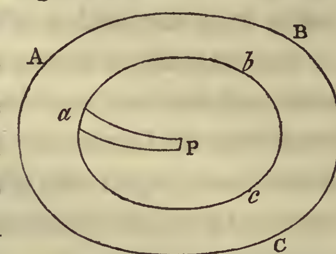
† Théorie de la Figure de la Terre, Première Partie, § xxi.

satisfactory when no cause of motion emanates from the fluid itself, and all the forces in action depend merely on the place of a particle. But if the fluid in question consist of attracting particles, will there not come into play the attraction of every additional stratum upon all the fluid contained within it, of which force no mention is made by Clairaut? The only cause assigned for the pressure of the stratum upon the fluid below it, is the action of forces foreign to the matter of the stratum; the attraction of the stratum is inherent in that matter; the two causes of motion are distinct from one another, and their different effects ought to be separately considered. The procedure of Clairaut, although it is unexceptionable when the forces in action depend only upon the position of a particle, seems chargeable with omission when applied to fluids consisting of particles that act upon one another by attraction or repulsion.

The initial body of fluid H K I is assumed to be in equilibrium; the equilibrium will not be disturbed by the pressure of the stratum *o n l*, which acts with equal intensity at every point of the surface H K I; but if the fluid consist of attracting particles, the attraction of the stratum *o n l* upon all the particles contained within it may alter the form of the mass H K I, and the equality of pressure upon the changed figure no longer existing, the equilibrium will be destroyed. This argument has greater weight, because in the procedure of Clairaut it is not the attraction of one stratum only which is neglected, but the sum of the attractions of all the successive strata, that is, no account is made of the attraction of a stratum of a finite thickness upon the particles within it.

It may perhaps be alleged that the attraction of a stratum upon the interior fluid is incomparably smaller than the forces which urge the particles of the stratum itself, and therefore that the first force may be accounted as nothing in respect of the other. Now the question is not about a comparison of forces different in degree, but whether the stratum attracting the particles within it in all directions, has power to move them and thereby to cause an alteration of figure. The procedure of Clairaut, by making every stratum exert a constant pressure upon the fluid below it, leaves every particle of that fluid at liberty to obey the smallest impulse; and an equilibrium cannot subsist unless the attraction of the stratum be either absolutely zero, or cause a pressure urging every particle with equal intensity in all directions. If the stratum be bounded by concentric spherical surfaces, or by elliptical surfaces that are similar to one another and similarly posited, Newton has proved that the attraction of the stratum has no power to move the particles within it. Must these important propositions be extended, tacitly and without examination, to all strata, whatever be the bounding surfaces? If one bounding surface be spherical and the other elliptical, or if both be elliptical but dissimilar, will the attraction of the stratum be ineffective to move the interior particles? The plain truth is that Clairaut has not attended to the attraction of the stratum, and consequently the application of his theory is limited to fluids consisting of particles that have no action upon one another. The inadvertence with which the investigation of CLAIRAUT is chargeable, seems not to have been noticed, at least it is not remedied, by any of the authors who have subsequently handled the subject.

The attraction of the stratum being admitted, its effect becomes a subject for mathematical investigation. We may suppose a stratum of homogeneous fluid bounded by two surfaces of any figure, A B C and *a b c*; and we may estimate the pressure tending to move an interior particle P in any direction, which is caused by the attraction of the stratum upon the contained fluid. Let dm represent an elementary portion of the stratum; x', y', z', the coordinates of dm; x, y, z those of P; and f the distance between P and dm.



The direct attraction of dm on P is equal to $\frac{dm}{f^2}$; and the partial attractions tending inward parallel to x, y, z, are respectively

$$\frac{dm}{f^2}\cdot\frac{x-x'}{f},\quad \frac{dm}{f^2}\cdot\frac{y-y'}{f},\quad \frac{dm}{f^2}\cdot\frac{z-z'}{f},$$

or, which is the same,

$$\frac{dm}{f^2}\cdot\frac{df}{dx},\quad \frac{dm}{f^2}\cdot\frac{df}{dy},\quad \frac{dm}{f^2}\cdot\frac{df}{dz}:$$

and the total partial attractions on P of all the matter of the stratum will be

$$\frac{d.\int\frac{dm}{f}}{dx},\quad \frac{d.\int\frac{dm}{f}}{dy},\quad \frac{d.\int\frac{dm}{f}}{dz},$$

the sign of integration extending to all the molecules of the stratum. Now if p represent the intensity of pressure, we shall have

$$dp = \frac{d.\int\frac{dm}{f}}{dx}dx + \frac{d.\int\frac{dm}{f}}{dy}dy + \frac{d.\int\frac{dm}{f}}{dz}dz;$$

and by integrating,

$$p = \int\frac{dm}{f} - \text{C}.$$

In this formula, $\int\frac{dm}{f}$ is the sum of all the molecules of the stratum divided by their respective distances from P, and C is the like sum at any arbitrary point which may be assumed in the inner surface of the stratum at *a*, and may be joined to P by a narrow canal having any direction: and if we write $\left[\int\frac{dm}{f}\right]$ for C, that is, for the sum of all the molecules of the stratum divided by their respective distances from *a*, the value of p in the formula

$$p = \int\frac{dm}{f} - \left[\int\frac{dm}{f}\right]$$

will be equal to the intensity of pressure urging the particle P in the direction of the canal. It appears, therefore, that the effect of the attraction of the stratum to move the particle P is not infinitely little; it is expressed by the difference of two definite integrals; and, however small in degree the pressures urging P on different sides may be supposed, yet, if they be unequal, the particle must move in the direction in which the force is greatest. By omitting the attraction of the stratum, the procedure of Clairaut is evidently defective, and applicable only to such fluids as consist of particles that have no action upon another.

But the investigation of Clairaut, although limited as it is laid down by the author, when it is stated with all the generality of which it is susceptible, will be found on due reflection to contain the only true and satisfactory principle of the equilibrium of a mass of fluid at liberty*. To render it perfectly general, nothing is wanting but to take into account all the forces necessary to complete the equilibrium at every separate stage of the procedure. The original mass H K I being supposed in equilibrium, the stratum *o n l* must be adjusted as Clairaut directs, so as to exert a constant pressure; but a new condition must be added, that the body of fluid H K I be in equilibrium by the attraction of the stratum, that is, the pressures caused in the mass H K I by the attraction of the stratum, must urge every particle of it with the same intensity on all sides. When these conditions are fulfilled, the body of fluid, consisting of H K I and the stratum *o n l*, will be in equilibrium, and its upper surface will be stable as was that of H K I, and capable of supporting additional strata. A new mass in equilibrium will be formed by adding a second stratum, so as to fulfill the same conditions as the first, that is, it must press with the same intensity at all points of the surface below it, and its attraction must have no power to move the particles contained within it. Continuing the same procedure and adding more strata indefinitely, a body of fluid of any dimensions will be formed, which is in equilibrium, all the forces in action being taken in account.

If we now examine a mass of fluid constructed by the foregoing process, so as to be in equilibrium, it is obvious that all the successive surfaces are deduced in the same manner from the forces acting on the particles contained in them. If the forces be explicit functions of the coordinates of their points of action, the condition that every surface must be pressed with the same intensity at all its points, determines the general equation of all the surfaces, nothing varying from one surface to another but the magnitude of pressure, as in the theorem in No. 5. The upper surface contains all the points of the fluid at which there is no pressure, and its equation alone ascertains the figure of equilibrium. This is the theory of Clairaut in its full extent, and it is comprised in the theorem alluded to: but if the forces in action are not explicit functions of the coordinates, but depend upon the very figure to be investigated, the condition that the pressure must be constant in every successive surface, leads to an

* It is obvious that all the steps of Clairaut's procedure must be perfectly similar. As the central body H K I is supposed in equilibrium, so the addition of every stratum must produce a body in equilibrium, all the causes capable of moving a particle being taken into account; if not, the process cannot be continued, or will fall into error.

equation that merely expresses a relation of two things alike unknown, namely, the figure of the fluid which is sought, and the forces resulting from that figure; and in this case it is necessary to take into account some other properties peculiar to the problem for the purpose of completing the solution. When the fluid consists of attracting particles, the equilibrium requires that the attraction of a stratum on the outside of any of the interior surfaces have no power to move the particles within that surface. Now it has been shown that the attraction of the stratum on the outside of the surface $a\,b\,c$, causes a pressure, p, urging an interior particle at P, in the direction of a canal reaching from P to a point a in the surface $a\,b\,c$, the quantity of which pressure is determined by the formula

$$p = \int \frac{d\,m}{f} - \left[\int \frac{d\,m}{f}\right]:$$

and it is obvious that p will be the same to whatever point of the surface $a\,b\,c$ the canal is drawn, and consequently that the particle will have no tendency to move in any direction, if $\left[\int \frac{d\,m}{f}\right]$ have constantly the same value at all points of that surface. On the other hand, if $\left[\int \frac{d\,m}{f}\right]$ have different values at different points of the surface $a\,b\,c$, the pressures upon P will be unequal, and the fluid will not be in equilibrium. Wherefore, in order to secure the equilibrium we must add to the constant pressure at all the points of every interior surface, as required by CLAIRAUT, or to the equation common to all these surfaces, this other condition, that the sum of the molecules of any stratum divided by their respective distances from a point in the inner surface of the stratum have constantly the same value at all the points of the surface. These conditions are the same with what has been investigated in the first part of this Paper; and, by means of the analysis in No. 7, they demonstrate that the figure of equilibrium of a homogeneous planet can be no other than an oblate elliptical spheroid of revolution.

In order fully to illustrate the investigation of CLAIRAUT, and to bring it completely within the power of the understanding, some further discussion is still required. The French geometer sets out with assuming, that the central mass H K I is in equilibrium; upon this all his inferences are grounded; but, in drawing the conclusion, he dismisses the first assumption, and substitutes for it the supposition that the central body of fluid is infinitely small. It may therefore be made a question, whether the results obtained are modified in any manner by the shifting of the original hypothesis.

The successive strata being so adjusted that the forces urging their particles are perpendicular to their surfaces, it is obvious that, upon every addition, the forces in action at the upper surface will be directed perpendicularly towards that surface, saving an abatement that must be made for the inequality of pressure upon the central mass, when that is not in equilibrium. But if the central mass be infinitely small, whether it be in equilibrium or not, will depend upon the action of very small forces, and the effect of these to vary the direction of the forces in action at the successive upper surfaces from exact perpendicularity, will continually become less and less, and may be ultimately neglected. No objection can therefore be made to substituting, for the equilibrium of the central mass, the supposition that it is infinitely small, in so far at least as it is purposed to construct a body of fluid such that the forces in action at the upper surface shall be perpendicular to that surface.

If we suppose that the forces urging the particles of the fluid are expressed by known and explicit functions of the coordinates of their point of action, the body of fluid, as it acquires finite dimensions, will likewise approach continually to a known figure; for the equation of the surface, deduced from the perpendicularity of the forces, has a determinate form, which ascertains the figure of the mass when its volume is given. In this case, too, all the forces acting upon every individual stratum being taken into account, and the strata exerting a constant pressure upon one another, the equilibrium of a mass of fluid will be fulfilled simultaneously with the condition of the perpendicularity of the forces to the upper surface.

It remains to examine what will be the result when the central body H K I, supposed infinitely small and of any figure, consists of attracting particles. In this case there is no question about an equilibrium; because, although the forces at the successive upper surfaces are exactly estimated, Clairaut has neglected the attraction of every stratum upon the body of fluid to which it is added, an omission which is fatal to an equilibrium of the mass. But as the procedure of that geometer always induces a figure which fulfills the condition of the perpendicularity of the forces to the upper surface, it is interesting to inquire whether, in the case of an attraction between the particles, the resulting figure is determinate and invariable, or indeterminate and varying with the figure of the small central body. Assume any body of finite dimensions similar to the small central mass H K I, and consisting of the same fluid; and supposing, for the sake of simplicity, that the law of attraction is that of nature, it is easy to prove, that the attractive forces acting in similar directions at similar points of the surfaces of the two bodies have constantly the same proportions as the linear dimensions of the bodies: and if the two bodies revolve with the same rotatory velocity about axes similarly placed, the centrifugal forces acting in similar directions at similar points of the surfaces, will likewise be to one another as the linear dimensions of the bodies. It appears, therefore, that the forces perpendicular to the surface of the central body H K I, although they are infinitely small, yet being proportional to the like forces at the surface of the finite body, they have given and finite proportions. Now upon the proportion of these forces depend the relative thickness and figure of the first additional strata at least; and as no limit can be assigned when this influence will cease, the conclusion undoubtedly is, that the ultimate surface will vary with the figure of the central mass. And thus the form induced by the procedure of Clairaut upon a mass of fluid consisting of attracting particles is indeterminate, and susceptible of being varied indefinitely.

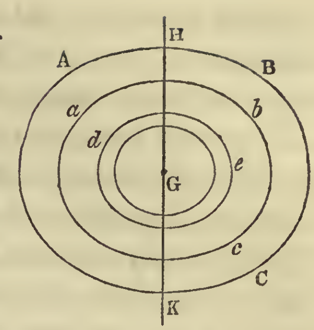
What has been said is well elucidated by the investigation that has been given of the exact figure of equilibrium, when all the forces in action are taken into account. Assuming that the problem is possible, it has been found that the supposition is verified, and all the conditions of equilibrium fulfilled, when that body is an oblate elliptical spheroid, and only when it has that figure. If the body H K I, whether its dimensions be finite or infinitely small, have the figure mentioned, and if the centrifugal and attractive forces be so adjusted that their resultant is, at every point, perpendicular to the surface of the spheroid, the procedure of CLAIRAUT will generate a series of figures all similar to one another, and all in equilibrium ; but, as this proposition is exclusive, if we substitute for H K I a body of a different form, supposed infinitely small, none of the successive figures will be in equilibrium, although in the long run, when they have acquired finite dimensions, they will fulfill the condition of the perpendicularity of the forces to the upper surface.

The discussion in which we have been engaged is of importance, because it shows the insufficiency of the methods usually employed for determining the equilibrium of a homogeneous fluid consisting of attracting particles. In this problem an equilibrium is not sufficiently established by making the upper surface perpendicular to the resultant of the forces acting upon the particles contained within it, nor by proving that all the narrow canals diverging from an interior particle, and terminating in the surface, press with equal intensity ; nor can the problem be solved by attending solely to the forces that act upon the particles individually*.

On the Method of Investigation followed in the Paper published in the Philosophical Transactions for 1824.

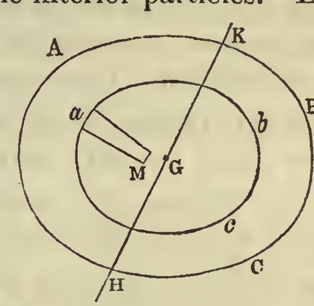
The equilibrium of a homogeneous planet may likewise be investigated by the method employed in my first paper on this subject, published in the Philosophical Transactions for 1824. As this method admits of being treated in few words, and will contribute to illustrate the principles on which the solution of the problem depends by placing them in a new light, I am induced to add a short explanation of it, more especially as it will give me an opportunity of stating clearly what is really liable to objection in that paper.

* In a Memoir published in 1784, LEGENDRE has arrived at this conclusion, that the elliptical spheroid is exclusively the figure of equilibrium of a homogeneous planet. To the mathematical processes employed by that eminent geometer, no objections can be made. But, on examination, it will appear that the grounds on which his investigation really rests, are these two : first, the equation of the upper surface of the fluid, which is a necessary condition of equilibrium ; secondly, an expression for the radius of the spheroid assumed arbitrarily and without reference to an equilibrium. Such a procedure can never be admitted as a complete and an *à priori* solution of the problem, unless it were first proved that every figure that can possibly fulfill the conditions of equilibrium is necessarily included in the expression assumed for the radius of the spheroid. No particular spheroid can be deduced from the equation of the upper surface alone, without first making a supposition respecting the expression of the radius : and this is an evident proof, that the equation is indeterminate and comprehends many different figures.



Let A B C represent a body of homogeneous fluid which revolves about the axis H K, passing through the centre of gravity G; and describe an interior surface *a b c*, similar to the upper surface A B C, and similarly posited about the point G: if we suppose that the mass A B C is in equilibrium by the action of the centrifugal force, and the attraction of its particles in the inverse proportion of the square of the distance, it is a property derived from the particular law of attraction and the nature of a centrifugal force, that every other body of the same fluid, as *a b c*, similar to A B C, similarly posited about the common centre of gravity G, and revolving about the same axis K H, will be separately in equilibrium by the centrifugal force of its particles and the attraction of its own mass. It would be superfluous to repeat the demonstration of this proposition here, as it is attended with no difficulty, and has not been contested. And because the body of fluid *a b c* is separately in equilibrium with respect to the centrifugal force of its particles and the attraction of its mass, it must likewise be in equilibrium with respect to the other forces that act upon it: for if it were not so, the whole body of fluid A B C would not be in equilibrium.

Now the only force external to the mass *a b c*, and tending to change the figure of that mass, is the attraction of the exterior stratum upon the interior particles. Let M be any particle within the stratum: the several forces which act upon it are, first, the centrifugal force; secondly, the attraction of the mass *a b c*; and, thirdly, the attraction of the exterior stratum. On account of the separate equilibrium of the mass *a b c*, the combined action of the two first forces has no tendency to move the molecule M; and therefore the equilibrium of the whole mass A B C requires that the attraction of the exterior stratum be ineffective to move the same molecule. Thus every molecule M within the surface *a b c* must be urged equally by the pressures which the attraction of the stratum causes in all canals originating at the molecule, and terminating in the surface *a b c*. This is the same condition to which every other mode of investigation has led; and as the mathematical application of this property to determine the figure of equilibrium has already been fully detailed, it need not be repeated here.



In order to leave nothing unexplained, it will be proper to remark, that the interior surface *a b c* is a level surface, that is, it is perpendicular at every point to the resultant of all the forces which act on a particle contained in it; for the centrifugal force of a particle at *a*, and the attraction upon it of the mass *a b c*, have their resultant perpendicular to the surface *a b c*, because the body of fluid *a b c* is separately in equilibrium: and the attraction of the stratum upon the particle at *a* is perpendicular to the surface *a b c*, because the sum of all the molecules of the stratum, divided by their respective distances from any point in the surface $a\,b\,c$, has the same invariable quantity. It follows from what is now proved, that the exterior fluid presses with the same intensity at every point of the interior surface $a\,b\,c$.

The least attention to the internal pressures at the surface $a\,b\,c$, and to the forces by which these pressures are caused, will show that the equilibrium of the mass A B C is secured by these two conditions: first, the resultant of the forces in action at the exterior surface must be directed perpendicularly towards that surface; and secondly, the level surfaces, that is, the interior surfaces, which are perpendicular to the resultant of all the forces acting upon the particles contained in them, must be similar to the outer surface, and similarly posited about the centre of gravity. These conditions of equilibrium, although enunciated in different terms, it will readily appear are not inconsistent with those before laid down, but are equivalent to them, and must necessarily bring out the same result.

The same things that have just been proved were investigated in the paper on this subject published in the Philosophical Transactions for 1824. There is no inaccuracy in that paper in deducing the conditions which the equilibrium requires to be fulfilled. These are, the perpendicularity to the upper surface of the resultant of the forces in action at that surface, and the immobility of a particle by the attraction of a stratum within which it is placed, and which is bounded by two surfaces similar and similarly posited to the upper surface. What is really exceptionable in that paper consists in the manner in which the second of the true conditions of equilibrium is conceived to be fulfilled. It is supposed in the paper that every individual particle within the stratum is attracted by the matter of the stratum so as to be drawn in all directions with equal intensity, which no doubt fulfills what is required, and is exact in particular figures; but being deficient in generality, it is an improper foundation on which to place the determination of the figure of equilibrium. To correct this misconception, it must be observed that the stratum, by attracting the particles within it, produces pressures in every part of the interior mass; and the immobility of a particle requires that it be pushed by the surrounding fluid with equal force in all directions. The difference between the two modes of action will be stated with most precision in mathematical language.

Assume a particle within the stratum, f being its distance from $d\,m$, a molecule of the stratum; the condition that the particle be attracted by the stratum equally in all directions, requires that the integral $\int \frac{d\,m}{f}$, extended to all the molecules of the stratum, have constantly the same value at all the points within the stratum; and the condition that the particle be at rest by the equal pressure of the surrounding fluid, requires that the same integral $\int \frac{d\,m}{f}$ have a constant value at all the points of the lower surface of the stratum. The second determination, which admits the integral, although it must be constant in any one surface, to vary in any manner in passing from one to another, is perfectly general; it embraces the full extent of the problem, and comprehends the first mode of action as a particular case. It happens that either of the two ways of rendering the attraction of a stratum ineffective to move the particles contained within it, leads precisely to the same final results in determining the figure of equilibrium of a homogeneous planet, which, although it does not excuse the misconception, makes the correcting of it less difficult. In conclusion, what is exceptionable in the paper of 1824 has already been explained publicly; and the paper in the Philosophical Transactions for 1831 is not liable to the same reproach.

APPENDIX, *containing the Investigation of some Algebraic Formulas.*

1. Development of $\frac{1}{\sqrt{s^2 - 2 s r \gamma + r^2}}$, used in No. 7.

If we assume

$$s - r z = \sqrt{s^2 - 2 s r \gamma + r^2},$$

the value of z will be

$$z = \gamma + \frac{1}{2} \cdot \frac{r}{s} (z^2 - 1):$$

now considering z as a function of γ, and applying the theorem of LAGRANGE, we deduce

$$z = \gamma + \frac{1}{2} \cdot \frac{r}{s} (\gamma^2 - 1) + \frac{1}{1 . 2} \cdot \frac{1}{2^2} \cdot \frac{r^2}{s^2} \cdot \frac{d (\gamma^2 - 1)}{d \gamma} +, \&c.;$$

and by substituting this value in the assumed formula, we obtain

$$\sqrt{s^2 - 2 s r \gamma + r^2} = s - r \gamma - \frac{1}{2} \cdot \frac{r}{s} (\gamma^2 - 1) - \frac{1}{1 . 2} \cdot \frac{1}{2^2} \cdot \frac{r^2}{s^2} \cdot \frac{d (\gamma^2 - 1)}{d \gamma} -, \&c.;$$

then by differentiating with respect to γ, and dividing by $s r$, we finally obtain

$$\frac{1}{\sqrt{s^2 - 2 s r \gamma + r^2}} = \frac{1}{s} + \frac{1}{2} \cdot \frac{r}{s^2} \cdot \frac{d (\gamma^2 - 1)}{d \gamma} + \frac{1}{1 . 2} \cdot \frac{1}{2^2} \cdot \frac{r^2}{s^2} \cdot \frac{d d (\gamma^2 - 1)}{d \gamma} +, \&c.$$

This expression of the development is investigated differently in the Philosophical Transactions for 1824.

2. Demonstration of the theorem used in No. 7.

It is obvious that θ and θ' are the two sides of a spherical triangle, $\varpi - \varpi'$ being the included angle, ψ the third side, and σ the angle opposite to θ'; wherefore, because $\gamma = \cos \psi$ and $\sqrt{1 - \gamma^2} = \sin \psi$, we have by the known properties of spherical triangles,

$$\cos \theta' = \cos \theta . \gamma + \sin \theta . \sqrt{1 - \gamma^2} \cos \sigma$$

$$\sin \theta' \sin (\varpi - \varpi') = \sqrt{1 - \gamma^2} \sin \sigma$$

$$\sin \theta' \cos (\varpi - \varpi') = \sin \theta . \gamma - \cos \theta . \sqrt{1 - \gamma^2} \cos \sigma.$$

For the sake of brevity put $\cos\theta = a$, $\sin\theta\cos\varpi = \sqrt{1-a^2}\cos\varpi = b$, $\sin\theta\sin\varpi = \sqrt{1-a^2}\sin\varpi = c$; and from the last expressions we readily deduce

$$a' = \cos\theta' = a\,.\,\gamma + \sqrt{1-a^2}\,.\,\sqrt{1-\gamma^2}\cos\sigma$$

$$b' = \sin\theta'\cos\varpi' = b\,.\,\gamma - a\cos\varpi\,.\,\sqrt{1-\gamma^2}\cos\sigma + \sin\varpi\sqrt{1-\gamma^2}\sin\sigma$$

$$c' = \sin\theta'\sin\varpi' = c\,.\,\gamma - a\sin\varpi\,.\,\sqrt{1-\gamma^2}\cos\sigma - \cos\varpi\,.\,\sqrt{1-\gamma^2}\sin\sigma.$$

If these values be substituted in

$$a'^m\, b'^{m'}\, c'^{m''},$$

and the several powers be expanded and reduced to terms containing the sines and cosines of the multiples of the arc σ, the result will be of this form:

$$\Gamma^{(0)} + (1-\gamma^2)^{\frac{1}{2}}\,.\,\Gamma^{(1)}\cos\sigma + (1-\gamma^2)^{\frac{2}{2}}\,.\,\Gamma^{(2)}\cos 2\sigma +, \&c.$$

$$+ (1-\gamma^2)^{\frac{1}{2}}\,\Delta^{(1)}\sin\sigma + (1-\gamma^2)^{\frac{2}{2}}\,\Delta^{(2)}\sin 2\sigma +, \&c.,$$

the expressions $\Gamma^{(i)}$ and $\Delta^{(i)}$ being integral functions of γ; and it is to be observed that the index of the highest power of γ in $\Gamma^{(0)}$ cannot exceed $m + m' + m''$. If we now multiply by $d\sigma$ and integrate between the limits $\sigma = 0$ and $\sigma = 2\pi$, we shall get

$$\int a'^m\, b'^{m'}\, c'^{m''}\,.\,d\sigma = 2\pi \times \Gamma^{(0)}.$$

Wherefore, attending to the expression of $C^{(i)}$, we have

$$\int -\,d\gamma\, d\sigma\, C^{(i)}\, a'^m\, b'^{m'}\, c'^{m''} = \frac{1}{2^i}\,.\,\frac{2\pi}{1.2.3\ldots i}\int -\,d\gamma\,\Gamma^{(0)}\,\frac{d^i(\gamma^2-1)^i}{d\gamma^i}.$$

Now it is easy to prove that

$$\int -\,d\gamma\,.\,\gamma^n\,.\,\frac{d^i(\gamma^2-1)^i}{d\gamma^i} = 0,$$

when n is less than i, the integral being taken between the limits $\gamma = +1$ and $\gamma = -1$: and since the highest power of γ in $\Gamma^{(0)}$ does not exceed $m + m' + m''$, it must be less than i; and hence it follows that the integral under consideration is equal to zero.

XXIV. *Observations on the Torpedo, with an account of some additional Experiments on its Electricity. By* John Davy, *M.D. F.R.S. Assistant Inspector of Army Hospitals. Communicated by Sir* James McGrigor, *Bart. F.R.S. Director General of the Army Medical Department.*

Received May 15,—Read June 19, 1834.

1. *On the Fœtal Development of the Torpedo.*

THE accounts we possess by different naturalists of the mode of generation of this fish are so discordant and perplexing, that I have been induced to investigate the subject afresh, and I now propose to submit to the Society the results of my observations.

It may be advisable to premise a few particulars respecting the generative organs of the Torpedo. The female, like those Rays and Squali which are considered ovoviviparous, has two ovaria, a common oviduct and two uterine cavities. The ovaria, one on each side of the spine, are attached to and enveloped in a fold of the peritonæum, just above the liver and a very little below the common infundibulum, or opening of the oviduct. The oviduct passes round on each side under the liver, and ends in an enlargement, one over each kidney, which from its function may be called a uterine cavity, formed, like the duct itself, of a villous inner membrane and of a peritonæal outer coat, connected together by loose filamentous tissue, and opening into the lower part of the intestine or cloaca by a common mouth, a little posterior to the minute papilla, the termination of the ureters. In the oviduct, just above its enlargement into the uterine cavity, there is only a slight trace of a glandular structure, in which respect the Torpedo appears equally to differ from the different species of Squalus and of Ray; all those which I have examined of either genus being possessed of a large glandular body in the situation mentioned.

The male generative organs consist of two firm oval testes, occupying the same situation as the ovaria in the female, and not very different in appearance; of vasa deferentia without vesiculæ seminales; and of a papilla in the cloaca, the common termination of the seminal and urinary passages, near the verge of the intestine.

Like the Squali and Rays in general, the male Torpedo is provided with two appendices, one on each side of the anus, composed of articulated bones, of muscles, of cartilages, and a glandular structure.

The eggs of the Torpedo I have never found in the oviduct in their passage, but only in the ovaria, or attached to the ovaria, or in the uterine cavities. When mature and attached to the ovaria, they are covered with a vascular membrane, through which they break to enter the infundibulum. In the uterine cavity they are destitute of white; they are covered, before the appearance of the embryo, with a most delicate membrane or pellicle, and consist entirely of yolk. The number of eggs varies very much with the size of the fish: in the smallest pregnant fish that I have examined, I have never found fewer than four in the two cavities; and in the largest, not more than seventeen. Their size, too, varies;—their average weight is about 182 grains; the largest of eighteen eggs which I have weighed, taken from five different fish, before the embryo appeared, was equal to 210 grains, the smallest to 129. Though without a distinct white, there is, in the uterine cavity common to all of them, a little fluid, generally milky, more rarely glairy, and sometimes bloody, which on evaporation affords crystals of common salt and a very little animal matter, composed chiefly of albumen.

In describing the fœtal development of the Torpedo, I shall confine myself strictly to what I have actually observed.

In the first stage in which I have witnessed the embryo, it appeared as represented in Plate XXII. fig. 1., about seven tenths of an inch long, without fins, or electrical organs, or any distinct appearance of eyes, with very short external branchial filaments*, not yet carrying red blood, and with a red spot in the situation of the heart (probably the heart itself) communicating, by red vessels in the umbilical cord, with the vascular part of the egg.

In the next stage in which I have observed it, it appeared as in Plate XXII. fig. 2., not quite an inch long, nor a quarter of an inch wide; the ventral fins visible, and also the dorsal and the inferior portion of the great pectoral fins; the branchial cartilages distinct and naked, the electrical organs not having yet appeared; the external branchial filaments longer than in the preceding, but still comparatively short; some of them tipped with red blood, others carrying it.

The next stage of advance I have seen is represented by fig. 3. Plate XXII. This embryo was about an inch and one tenth long and four tenths of an inch wide where widest, and it weighed just five grains. Its electrical organs were beginning to appear. The external branchial filaments were about six tenths of an inch long and contained red blood. The heart was distinct and large, as were also the two lobes of the liver. The stomach was small, apparently empty, smaller than the intestine: the intestine was large and white. The vitello-intestinal canal was distinct; it appeared as a very slender thread, connected with the upper part of the intestine, and, like the intestine itself, it contained no yolk. The eyes were apparent. There was a vesicle on the head distended with a colourless fluid, and the cavity of the cranium was full of a similar fluid. The roots of the electrical nerves were visible, but no brain.

The next stage in which I have observed the embryo is represented by fig. 4.

* These filaments, variable in length and appearance, are constant in containing each a blood-vessel, which makes the circuit of the filament.

Plate XXII. It was advanced only a little beyond the preceding; the principal differences were in the electrical organs being a little larger, the branchial filaments considerably longer (about an inch long), and the brain and spinal cord apparent.

In the next stage in which I have seen it, as represented by fig. 5. Plate XXII., there was a very considerable advance. The fœtus was about two inches and a half long, and one inch and three quarters wide. The electrical organs were distinct, the pectoral fins entire, the external branchial filaments very long. The stomach was still small, and empty, whilst the intestine was distended with yolk. The external yolk was covered with a vascular membrane, and not partially, as in the preceding, but entirely. The vitello-intestinal canal freely communicated with the intestine, and was yet very little enlarged where it joins itself to the intestine at the commencement of its valvular portion.

The next stages which have come under my observation are represented by figg. 6 and 7. Plate XXII., and fig. 1. Plate XXIII. The cavity of the abdomen is shown laid open in figg. 6 and 7, in order to exhibit the external yolk in progress of diminution, and the internal yolk contained in a membranous bag, as it were, a lateral extension of the vitello-intestinal canal, in progress of accumulation*. The branchial filaments have almost entirely disappeared.

I shall notice only two stages more of the young Torpedo, represented by fig. 4. Plate XXIII., and fig. 1. Plate XXIV.; the one of a fish six weeks old, in which the internal yolk was considerably diminished in bulk, its connexion with the umbilicus almost absorbed, the intestine full of yolk, the stomach empty but considerably developed: the other of a fish six months old, in which only a very small portion of the internal yolk remained, and the connexion of the inner yolk-bag with the umbilicus was absorbed, a vestige only of the canal of communication remaining.

These fish, at about the full period of utero-gestation, were extracted from a Torpedo just after she had been caught, were instantly put into salt water, and were preserved alive. I shall have occasion to revert to them in another part of my paper.

I may remark generally, that I have never found in any of the gravid Torpedos which I have examined in different stages, any membrane investing the fœtus, as is

* At this period of fœtal development the yolk has two distinct membranes, an external transparent one and an internal vascular one. The former is of great delicacy generally, excepting where the egg joins the abdomen; there it is very thick and strong, and slightly opake, serving in a manner the part of the sheath of the umbilical cord of the Mammalia: it is connected with, and appears to end in the cutis of the abdomen. The latter, the membrana umbilicalis, or chorion, if it may be so called from its great vascularity, passes into the cavity of the abdomen, and terminates in the vitello-intestinal canal, from whence the internal yolk-vesicle proceeds. Two large vessels (the trunks of the vessels of the chorion) enter the cord-like termination of the egg: one of them terminates in the vena portæ; the termination of the other I have not ascertained in a satisfactory manner; I believe it corresponds in function to the umbilical arteries, and brings blood from the fœtus to the egg, the other vessel returning it.

the case with the fœtus of some of the Squali*. Neither have I found any fluid in the uterine cavity at any period, excepting that already mentioned†.

The facts I have stated relative to the development of the fœtus of the Torpedo, though amply sufficient to demonstrate that this fish is not oviparous, are not incompatible with its being ovoviviparous, as it is considered by the naturalists who have paid most attention to the subject; yet I believe it is not strictly so, and that it is more correct to say that it is intermediate between ovoviviparous and viviparous, the fœtus, as I believe, deriving its support in part from the ovum, and in part from the parent. The principal fact on which I found this belief is, that the mature fœtus is very much heavier than the egg. In the three following Tables I shall give the statical results substantiating this fact.

The first Table will relate to the ovum just after it has entered the uterine cavity, or before the appearance of the embryo; the second, to the ovum after the fœtal development has commenced, but has made little progress; and the third, to the fœtus when mature, or nearly mature, indicated by the total disappearance of the external yolk, or its being reduced externally to a very small bulk. In noticing the kind of Torpedo, I shall use the popular names by which they are designated at Rome, reserving for another place the consideration of its species. The exact time when the fish was caught will be given, with a view to endeavour to determine its breeding season and period of utero-gestation.

Table I.

Kind.	When caught.	Number of Eggs.	Weight of each Egg tried.
			grains.
Tremola.	March 30.	8	200
			200
Tremola.	May 21.	5	185
			185
			198
			188
			188
Tremola.	May 24.	9	193
			193
			193
			200
			210
Tremola.	April 29.	5	167
			167
Tremola.	May 31.	5	129
			140
			170
			165

* The fœtus of the *Squalus Acanthias*, at a very early period, is contained in a delicate membrane, which at a more advanced period, near the full time, disappears. The fœtus of the *Squalus Squatina* seems to be analogous to that of the Torpedo without a membrane; that of the *Squalus galeus* has a membrane, even in its advanced stage, appearing to be, as it were, a link between the Torpedo and the oviparous Rays, whose eggs, inclosed in a thick strong horny shell (*Mus marinus, Pulvinar marinum* of the older naturalists), are hatched out of the body.

† I have in vain sought in the uterine cavity of the Torpedo for lithic acid, which is so abundantly secreted

TABLE II.

Kind.	When caught.	Number of Eggs.	Weight of each Egg tried.	Weight of each Embryo attached tried.
			grains.	grains.
Tremola.	June 13.	13	166	2
			140	2·5
			101	2
			156	2
			134	
			111	
			147	2
			174	3
			131	
			164	
			102	
Tremola.	June 26.	14	79	12
			79	14
			115	12
			107	13
			108	11
			77	13
Tremola.	June 28.	9	215	5
Occhiatella.	June 29.	4	120	1
			119	
			114	

TABLE III.

Kind.	When caught.	Number of fœtal Fish.		Weight of each tried.	
		Male.	Female.	Male.	Female.
				grains.	grains.
Tremola.	September 6.	2	3	540	580
Occhiatella.	September 12.	2	4	503	505
Tremola.	September 15.	1	4	435	457
					420
					460
					471
Tremola.	September 29.	6	7	481	514
				487	495
				464	533
				452	506
				485	521
				428	519
					500

The mean of the results contained in these Tables is, that the weight of the egg before any appearance of the embryo is 182 grains; and after its appearance, in-

by the kidneys of the chick in ovo; nor have I succeeded in detecting urea in the fluid it contains, a substance which I have found in a notable quantity in the fluid of the uterine cavity of the *Squalus Squatina,* and in abundance in the liquor amnii of the Dog about the fifth week of pregnancy, and have also detected in the human liquor amnii at the full period. In the cloaca of very young Torpedos I have sometimes seen a transparent fluid, probably urine, but in too small quantity for examination. The nature of the urinary secretion of the adult Torpedo I have not yet been able to ascertain; I suspect that it is liquid, and that it is voided almost as rapidly as it is secreted, the fish being without a urinary bladder, and its cloaca of narrow dimensions.

cluding the weight of the embryo, about 177 grains; whilst the weight of the mature fœtal fish is about 479 grains; proving an augmentation of weight in the mature fœtus more than double that of the egg, and in this respect differing remarkably from the fœtal chick, which at its full time weighs considerably less than the original yolk and white from which it is formed, owing in part to the evaporation of water through the shell, and in part to the excretions going on, especially of lithic acid derived from the kidneys.

How is this augmentation of weight to be accounted for? Is there, as in the majority of the Mammalia, any connexion between the fœtus of the Torpedo and the parent through the medium of a vascular and cellular structure? Or has the fœtal fish in utero, like the fœtus of the Sepiæ in the egg, the power of feeding by the mouth, and of taking food into the stomach? Or does the uterine cavity of the parent fish secrete or pour out a fluid which is absorbed by and in part nourishes the fœtus?

The first and second query I must answer in the negative. Nothing that I have observed indicates any connexion such as that supposed in the first query, between the parent and fœtus. I have carefully examined the gravid uterus under water, thinking it possible that the villi of the uterine cavity might inosculate with the branchial filaments, but I could not detect the slightest union of them, or even apposition. I have carefully examined, too, the stomach of the fœtus in its different stages, and I have always found it empty. Admitting, then, that the augmentation is effected by absorption (the only way apparently remaining to account for it), another question arises, How is the absorption accomplished? Is the whole surface of the fœtus an absorbing surface, as in the instance of some of the Mammalia which are destitute of a placenta, and whose fœtus do not appear to be connected with the uterus, as that of the Opossum and Kangaroo? Or are the branchial filaments the principal absorbing organs?

It appears not improbable that both the general surface and the filaments are concerned in the operation. The late Dr. Monro, who observed these filaments in the fœtus of the common Skate, supposes that they perform the same function as the gills, and are a substitute for them, like the branchial appendices of the Tadpole; and the same view has been taken by others of analogous filaments belonging to the fœtus of most of the Squali. This function they may perform in common with the surface; and at the same time they may convey nourishment and material for growth. If I may hazard a conjecture, I would suggest that the matter which may be absorbed by the surface, may enter into the composition of the body generally; whilst that which may be absorbed by the branchial filaments, may be chiefly employed in forming the electrical organs, and perhaps the branchiæ and the adjoining mucous glands. I shall notice a few circumstances which appear to me favourable to this conjecture.

1. The branchial filaments are most numerous and of greatest length whilst the electrical organs are forming, appearing just before these organs begin to be deve-

loped, and being removed when they are tolerably complete. Now it seems more reasonable to suppose that this associated progress of the two is in the relation of cause and effect, than to imagine that the filaments are solely designed as a substitute for the branchiæ; especially as the blood in the vessels of the yolk membrane seems to be as well adapted to receive the influence of any little air which may be contained in the fluid in the uterine cavity, as the blood circulating in the vessels of the filaments.

2. In none of the Squali the fœtus of which I have had an opportunity of examining at different periods, have I found the same elaborate apparatus of branchial filaments: they have been less numerous and very much shorter. Does not this greater elaborateness indicate that they are intended, in the Torpedo, for a special purpose? And when we consider the nature of the electrical organs abounding in fluid, as well as their peculiar office, does it not seem accordant that there should be such a peculiar provision as that in question for their formation?

3. In one instance I found a large fasciculus, as represented in Plate XXIV. fig. 2. unconnected with the branchial apertures, attached to the head, anterior to the eyes, in the situation of the principal cluster of mucous glands in the adult fish, between the anterior portions of the two electrical organs. May not this be considered an instantia crucis, both as showing that the branchial filaments are not solely designed as a substitute for the gills, and rendering it highly probable that they are concerned, not only in the development of the electrical organs, but also of the mucous glands?

It is not necessary to discuss the other two modes in which the fœtus of the Torpedo is nourished, analogous to what is witnessed in the chick in ovo, first by means of vessels conveying blood, passing from the yolk membrane, and afterwards, in addition, by the direct passage of the substance of the yolk into the intestine of the fœtus, through the vitello-intestinal canal.

Whether the fœtus of those Squali and Rays which are considered ovoviviparous are only nourished in these two ways, or also in the additional manner of the fœtus of the Torpedo, is a subject for inquiry. From what I have observed, I am rather disposed to think that they are nourished in the latter manner, though in a less degree, and without excepting even those which are contained in a closed membrane.

From the facts given in the preceding tables, and from others which I have observed, it may be inferred that the Torpedo does not bear young more than once a year; that the breeding season is the latter end of autumn and the beginning of winter*; and that the period of utero-gestation is from nine to twelve months †.

I have alluded, some pages back, to the fœtal Torpedo at its full term. Since I

* According to ARISTOTLE, it brings forth in autumn. In my former paper I supposed erroneously that the principal breeding season is the spring, from the circumstance that the fish at that time abound in ova of a large size.

† I say from nine to twelve months, because I suspect the period of utero-gestation is not precisely fixed, but that it varies with circumstances favourable or unfavourable to bringing forth. Thus, I have had young Tor-

have been in Malta, though I have examined more than two hundred Torpedos, I have found five only in which the young were arrived at, or near, this stage. Of these, three were brought alive. I shall give some particulars, chiefly of their broods, as they may not be considered uninteresting in themselves, and as they may tend to illustrate the slow growth and some of the peculiarities of this extraordinary fish.

The first live Torpedo that I obtained in this state was an Occhiatella, on the 12th of September. It was fourteen inches long, and eight inches and a half wide; and after the extraction of the fœtal fish it weighed one pound three ounces. It had been caught rather more than an hour, and was in a small bucket full of salt water. I immediately set about preparing an apparatus to try its electricity, which occupied me about five minutes: but it was too late—the fish was then motionless. As soon as the apparatus was ready, I opened the cavity of the abdomen, hoping that if gravid, as asserted by the fisherman, the young might be still alive. From each uterine cavity three fish were extracted, but they were all dead; neither in air nor in salt water did they show the slightest signs of irritability, though they had no appearance of being bruised or in any way injured. The size of each was nearly the same; the only difference I could perceive was a little variation in the magnitude of the external yolk-bag: in two it had all but disappeared—it was smaller than a barleycorn; in the other two it was a little larger, and in the two others perhaps a little larger still. The internal yolk was very large, and about the same size in all. Their organization generally appeared to be complete, even to their teeth.

The next fish that I obtained near its full time was a Tremola, on the 29th of September. It was eighteen inches long and thirteen wide. It had been caught an hour or two before, and was in a very languid state, having been put into a vessel containing only just sufficient water to cover it. It was tried on the multiplier, but it did not affect the needle. When moribund, the abdomen was opened, and I extracted with the hand, without experiencing any shock, from the two uterine cavities, twelve fœtal fish; and one which had been expelled before, and was alive and swimming about, made thirteen. They were all nearly of the same size; and of all of them, the external yolk-bag had very nearly disappeared, the portion remaining being less than a small pea. Most of them appeared inanimate; two or three only moved their tails very slightly, and the margins of the pectoral fins. They were as soon as possible transferred to fresh sea-water. After about two minutes, one or two of them began to move their water-valves *. I was now called away, and rather more than four hours elapsed before I returned. On my return I found them all freely respiring,

pedos brought me, caught in the sea, in which the internal yolk-vesicle was large; and, in one instance, I found them in utero, with this vesicle greatly reduced in size, so as to suggest the idea, which ARISTOTLE adopted, that the young of the Torpedo, after birth, return at will into the uterus; an idea which cannot be held, on account of its anatomical impossibility. (ARISTOTLE, Hist. Animal., vi. cap. 10.)

* I apply this term to the valves which are situated at the openings behind the eyes, the office of which appears to be, to force water into the gullet, to supply the branchiæ; which water, in regular respiration, passes

and moving about actively. They imparted smart shocks to the fingers or finger pressing the upper surface, and another of the same hand the under surface of the electrical organ: they distinctly affected the galvanometer, and feebly magnetized needles through the medium of a spiral. These trials were made at 2 P.M. At 10 P.M. all the fish were alive and vivacious; an hour after I found them all dead.

The third fish I have to notice was a Tremola, seventeen inches long and twelve and a quarter broad. When brought, on the evening of the 6th of November, in a vessel of salt water, soon after it had been caught, it was tolerably vivacious; yet it did not affect the galvanometer. Before it was quite dead, the abdomen was opened, and the fœtal fish were extracted. They were ten in number, all of them about the same size. In all of them the outer yolk-bag had disappeared, as represented in Plate XXIII. figg. 2, 3, 4. Touching them with the hand, in the act of removing them from the uterine cavity, I received a distinct shock, sharp though not strong. Put into fresh sea-water, as they were extracted, some of them immediately, and in a few seconds all of them, were active and swam about: and making trial of one of them instantly, (the apparatus being in readiness,) it powerfully affected the galvanometer, and made a needle slightly magnetic. To ascertain the state of the internal yolk-vesicle, one of these fish was killed by putting it into fresh water, about half an hour after its extraction. It immediately became very restless, and endeavoured to escape: then, in less than a minute, it became quiet, and its water-valves ceased to act. Two or three times, at intervals, it was again restless: in about twenty minutes it was motionless and dead *. The appearance of its internal yolk-vesicle is represented in Plate XXIII. fig. 3.

Three of these fish remained alive till the 22nd of May, in sea water, which was changed daily, or every second day. Of the others, one only died a natural death; the rest were killed at intervals, for the purpose of examining the size of the internal yolk-vesicle, which very slowly diminished during this period, and, as well as I could judge, in a very regular manner; supposing, when first extracted, that in all, the internal yolk was nearly of the same size. Plate XXIV. fig. 1. shows the diminution it had undergone on the 22nd of May, when the three residual fish died, apparently from the carelessness of a servant giving them turbid salt water, and weaker in salt than they had been accustomed to.

During the whole of this period of five months and more, they ate nothing, though very small fish, both dead and alive, were put into the water. They retained, and indeed increased in activity, and even in their electrical energies, of which I made

out through the branchial apertures, but occasionally is discharged (the latter being closed) in considerable quantity through the superior apertures. BLOCH erroneously supposes that these latter are the normal outlets. He says, "Ils servent à l'animal à rejetter l'eau qu'il avale, soit en prenant sa proie, soit celle qui entre par l'ouverture des ouïes."—Histoire Naturelle des Poissons, tom. iii. p. 667.

* Is it found in the Nile, as asserted by some authors? The above fact would seem to indicate that the Torpedo cannot exist in fresh water.

occasional trials. They also became of rather firmer consistence, and of a darker colour, and perhaps contracted a little in dimensions. The weighing of those first killed was neglected; of the three which died last, two (males) weighed 510 grains each; the other (a female) 560 grains. Their stomachs were pretty largely developed, but empty: in the intestine there was a small quantity of yolk remaining, coloured greenish yellow in the inferior part, from the admixture of bile *.

All these facts seem to show a very slow development, and are in accordance with a long period of utero-gestation; and I may add, in favour of the same, that the ova in the ovaria of all the three parent fish were very small, the largest of them not exceeding a pea, and the majority of their minute vesicles containing a transparent fluid.

Other inferences might be drawn from these details, especially in favour of the branchial filaments being absorbent organs, rather than supplying the place of gills (the gills being apparently useless in utero when formed); but I am afraid of trespassing further on the time of the Society on a subject of limited interest.

In the beginning of this paper, I have alluded to the discrepancy which exists amongst writers on natural history relative to the mode of generation of the Torpedo. Aristotle always describes this fish as viviparous; so does Lorenzini, who wrote in the middle of the seventeenth century. On the contrary, Blumenbach, generally an accurate writer, though he quotes Lorenzini, gives the Torpedo as an example of the oviparous cartilaginous fish, laying a few large eggs, protected by a horny shell. And even Cuvier appears to have fallen into the same error, at least in his Règne Animal he has not corrected it; and from his general account of the generation of the Rays, both in this work and in his Histoire Naturelle des Poissons, it is to be inferred. It is most probable that analogy and want of confidence in Aristotle and Lorenzini were the cause of this mistake. No doubt, had these able men enjoyed an opportunity of investigating the subject themselves, they would not have failed in ascertaining the truth. Even in Malta, the inquiry is of considerable difficulty, requiring much time and patient waiting, owing to the great rareness of the gravid fish. Some idea of this may be formed, when I mention, that after I had begun the pursuit, more than twelve months elapsed before I could procure a fish with young, though I examined a very large number in hope of finding one, and though I offered to pay the fishermen above fifteen times the market price of the fish.

2. *On the Species of Torpedo in the Mediterranean.*

Respecting the number of species of Torpedo found in the Mediterranean, naturalists have been much divided in opinion; some, as Rondelet, followed by Risso, admitting four species; some, as Bellon, and latterly Rudolphi, limiting them to

* I have never found the stomach of the fœtal fish, or of these fish, which were so long without eating, softened or corroded; a change which I have several times observed in the stomach of the adult fish, killed when there was food in it in process of digestion.

two; and others, as LINNÆUS and BLOCH, with WILLOUGHBY, RAY, and ARTEDI, admitting only one*.

That there are two distinct species in the Mediterranean, namely, the Occhiatella and the Tremola, as the two kinds are vulgarly called at Rome, the spotted and non-spotted of BELLON, there does not appear to me to be a doubt. But it appears more than doubtful if any other true species exist in this sea. I draw this conclusion from multiplied observations made both at Rome and in Malta.

That these fish, the Occhiatella and Tremola, are distinct species, admits of satisfactory proof. They differ not only in their colour and general appearance, but also somewhat in their form. The Occhiatella is more gracefully made than the Tremola; its fins are larger, especially its dorsal fins; its water-valves are larger and different in shape, and the openings behind the eyes to which they belong are guarded by much smaller projections than protect those passages in the Tremola†. And, internally, there is a remarkable difference in the structure of the villous coat of the uterine cavity: in the Occhiatella the villi are filamentous and detached, as represented in Plate XXIV. fig. 4.; in the Tremola they are continuous delicate plates or laminæ, as shown in Plate XXIV. fig. 3.‡ These characters are constant in all the different specimens which I have examined.

To these two well characterized species, it appears to me that all the varieties of the Torpedo, at least those known hitherto in the Mediterranean, may be referred: the *T. unimaculata* of RISSO, and the second species of RONDELET§, to the Occhiatella; and the varieties with dark irregular spots, or without spots, to the Tremola.

CUVIER, in the last edition of his Règne Animal, and RUDOLPHI, have so considered the first-mentioned variety, the *T. unimaculata*, as it differs only in having one eye-spot instead of five, the most common number. But it is not more uncommon to meet with it having three or four spots than one; and that this is purely accidental

* Both those who have adopted four species and those who have allowed only one, appear to have followed RONDELET, in the latter instance critically, in the former literally, in proof of which the following passage may be adduced. "Torpedinum genera quatuor facimus, tria earum quæ maculis notatæ sunt, quartum ejus quæ maculis caret. Quæ genera omnia viribus et corporis specie non differunt, sed maculis tantum. Quare quæ de unius facultatibus et partium tum internarum tum externarum descriptione dicuntur, eadem etiam reliquis convenire existimato." (G. RONDELETII Libri de Piscibus, &c., p. 358. fol. Lugduni, 1554.)

† CUVIER, in the edition of his Règne Animal of 1830, (for an extract from which, as well as for one from RUDOLPHI's Grundriss der Physiologie, relative to the Torpedo, I am indebted to my friend Dr. ALLEN THOMSON,) distinguishes the Occhiatella by its spots, and by the absence "de dentelures charnues au bord de ses évents." This does not hold good of any of the specimens which I have examined. The cartilaginous projections (which they really are) covered with cutis, I have found only smaller in the Occhiatella, not absent.

‡ The villi increase in size during the period of pregnancy, and then contain a large quantity of blood. In each filament, in the instance of the uterus of the Occhiatella, there is a blood-vessel reflected on itself, circulating blood of a bright scarlet hue; and in the lamellar structure of the uterus of the Tremola there is a similar appearance of blood-vessels in loops.

§ "Secunda Torpedinis species à primâ differt, quòd maculas nigras, rotundas, circulis non distinctas habeat, sed eadem pentagoni figurâ dispositas. Est etiam primæ concolor."—RONDELET, p. 362.

is proved by the circumstance, that in a brood of several fœtal fish, of which all but one resembled the parent in having five spots, the exception had three. The Occhiatella has been seen even with seven eye-spots.

The varieties of the Tremola are the *T. marmorata* of Risso and the *T. Galvanii*, which Rudolphi, and, I believe, latterly Cuvier, has considered identical in species. This appears to me to be proved by their general character being the same, their water-valves, fins, and uterine structure; and further, by the circumstance, that between the two varieties (the former marked with black spots or patches irregularly distributed, the latter without spots,) there is a complete gradation or intermixture, both the spotting and colouring varying infinitely, so that it is difficult to find two fish exactly similar in either respect. How much this variation is owing to locality and other circumstances, it is difficult to decide. As the spotted fish is most frequently caught where the bottom is sandy, and the other variety where it is muddy, light may be concerned in the difference, and the spots may be produced like freckles, by the action of light: and in process of time they may become hereditary, the fœtuses generally, even of these varieties, resembling in appearance the parent fish.

Rudolphi has given to the Occhiatella species the Italian name of *T. ocellata*; perhaps the Latin word *oculata** may be preferable. The other species he designates as the *T. marmorata*, for which might be substituted the term *diversicolor*, being applicable to all the varieties of it, and descriptive of its quality of variableness of appearance.

For the information of travellers who may visit Malta, and wish to investigate the electricity of the Torpedo, I may mention that this fish (both the *T. oculata* and *diversicolor*) is called by the Maltese Haddayla, derived from a verb in their language signifying to benumb or paralyze, and consequently that it should be inquired for by this name, not by that of Torpedo, which is generally unknown here. I may add further, by way of caution, that the Torpedo in Malta is often difficult to be procured, partly owing to its being little sought after for the table, being used as an article of food only by the indigent, and partly, I believe, from the uncertainty and irregularity of its coming into shallow water. However, by paying well the fishermen, it may be obtained at all seasons; and the longest time to wait may be a fortnight or three weeks.

3. *An account of some additional Experiments on the Electricity of the Torpedo.*

Mr. Faraday in the Third Series of his Experimental Researches on Electricity, states that he has little or no doubt, were Harris's electrometer applied to the Torpedo, the evolution of heat would be observed †. I have made very many experiments

* Pliny, by Bloch and others, is supposed to have applied this term to the Torpedo. As in the only passage in which I am aware he has used it (Hist. Nat., lib. xxxii. cap. ii.—the passage is little more than a list of fish,) the word Torpedo is also employed, as if applied to a different fish, the justness of their supposition is doubtful.

† Philosophical Transactions, 1833, p. 46.

on this subject, completely establishing Mr. FARADAY's anticipation. The instrument employed was similar to that described by Mr. HARRIS in the Philosophical Transactions of 1827, differing merely in the wire passed through the small globe being exceedingly fine, and of platina, formed after Dr. WOLLASTON's method*; in having a small stop-cock for regulating the height of the spirit in the stem; and in using as small a quantity of spirit as possible†. The delicacy of this instrument was so great, that the spirit was not only moved by a single spark of the electrical machine, but even very distinctly by the electricity of a single voltaic combination, composed of a copper and zinc wire, the former $\frac{1}{25}$th of an inch in diameter, the latter $\frac{1}{50}$, excited by dilute sulphuric acid.

This instrument was strongly affected by active fish, and even distinctly by weak ones; indeed, occasionally, when it has formed part of a circle in connexion with a galvanometer, I have seen it affected alone, the galvanometer affording no indication of the passage of the electricity. Using two air thermometers of the same construction, each connected with the wires for contact at one end, and with a galvanometer at the other, the heating effect of the electricity of the Torpedo has been apparently diminished, and even more distinctly diminished on adding to the circle another link of very fine platina wire. And at the same time its influence on the galvanometer has been diminished, and its power of imparting permanent magnetism to a needle placed in a spiral, both forming part of the circle.

When heat has been applied to the extra link of platina by means of a spirit lamp, so as to render it red hot, the diminution of effect has disappeared; and equally so, as well as I could judge from many experiments, whether acting on the thermometer, the galvanometer, or the needle in the spiral.

It appeared not improbable that a short portion of a very fine platina wire might be ignited in the passage of the electricity of the Torpedo. I have made several experiments to ascertain this, but have never witnessed the effect, even in perfect darkness, and using fish, the discharge of whose electricity at the same time converted a needle into a tolerably powerful magnet, the needle having been put into a spiral connected with the fine wire, so as to afford a test of the strength of the electricity. This want of ignition may at first view seem contrary to the effect on the thermometer; but perhaps it ought not to be considered so, taking into account the rapid manner in which the heat evolved in the fine platina wire must be carried off by the adjoining compound wire of platina and silver.

The experiments detailed in my former paper were demonstrative that the electricity of the Torpedo is capable of acting like voltaic electricity in effecting chemical

* Philosophical Transactions, 1813, p. 114.

† I mention these circumstances because I have not been able to refer to Mr. HARRIS's later account of his instrument, published in the Philosophical Transactions of the Royal Society of Edinburgh. I should add that the bulb of the thermometer was defended from the variable temperature of the surrounding air by being included in a wooden box.

decompositions; and several other trials which I have made are amply confirmatory of this. In these latter experiments I have not, as in the former, coated the wires introduced into the fluids with sealing-wax, leaving the points only exposed. Though the wires were naked, and in every instance introduced more than a quarter of an inch into the fluid, and the distance between them was at least a tenth of an inch, yet satisfactory results were obtained. Using either a saturated solution of common salt, or a mixture of equal parts of sulphuric acid of commerce and water, and platina or gold wires, gas was given off round each wire under the influence of the discharge of the electricity of an active fish, one contact wire being applied to the under surface, and the other to the upper surface of the Torpedo. When steel needles were used with the salt water, then gas was disengaged only from the one in connexion with the under surface of the fish, the other needle becoming oxidated. Using a strong solution of nitrate of silver and gold wires, silver was precipitated only on that in connexion with the under surface; employing strong nitric acid and platina wires, gas was given off from one only, that in connexion with the upper surface; and using a solution of iodide of potassium and starch*, the iodine in combination with the starch, as indicated by the discolouration, was precipitated round the same wire.

Even the decomposition of water has been effected when the circle has been interrupted by four portions of the solution of common salt, contained in small tubes with two needles in each, the needles in one connected with those in the other, and at the same time with the galvanometer, a spiral holding an unmagnetized needle and an air thermometer. And simultaneous with the chemical decomposition, the needle in the galvanometer has been moved, and the spirit in the air thermometer has been raised, and the needle in the spiral has been magnetized.

The tests or indications of the electricity of the Torpedo at present known are six in number, namely, the physiological effect, as the sensation it imparts is sometimes called; the chemical effects, as the precipitation of iodine, the decomposition of water, &c.; its effect on the thermometer, on the galvanometer, and on steel in the spiral. These different tests, in point of delicacy, I am inclined to believe are in the order in which they are enumerated. That the two first should be placed highest, and that sensation should have the precedence, the experiments which I have made appear to prove, independently of all analogy.

When the human body has formed part of a circle of communication between the two opposite surfaces of a Torpedo, and also a chemical apparatus with platina wires and the solution of iodide of potassium and starch, the shock experienced by the hands has been strong, and the chemical effect either null or slight, no gas appearing when a strong solution of salt has been used, and no precipitation of iodine occurring unless the platina points were very nearly in contact, and the fish energetic.

* When starch in powder is added to a saturated or nearly saturated solution of the iodide of potassium, a transparent gelatinous mass is formed. This I have used in my experiments; a single combination of copper and zinc wire, acted on by a very dilute acid, occasions in this compound a precipitation of iodine.

When besides the human body and the chemical apparatus the galvanometer has been introduced into the circle with the air thermometer and spiral, the shock has been experienced as if it had been received direct from the fish, but I have never witnessed at the same time any other effect.

Not taking the human body into the circle in trials on fish of very feeble electricity, I have witnessed the precipitation of iodine when neither the air thermometer, nor a delicate galvanometer with a double needle, has been affected.

The same kind of evidence has been obtained of the thermometrical test being next in point of delicacy, in as much as I have seen the air thermometer affected by a fish which had no influence on the galvanometer in connexion with the wire of the thermometer.

That the needle in the spiral is the least delicate test, does not require to be insisted on. The electricity of a Torpedo, of almost feeble energy, has been equal to produce all the effects alluded to at once, excepting sensation, as already explained, and excepting the imparting permanent magnetism to the steel needle. The last effect, as might be expected, has required greater force; a moderate force, however, has been sufficient when a very slender needle has been used, and a spiral of fine wire closely coiled, only just capable of receiving the needle*.

It having been stated on high authority that a spark has been obtained from the *Gymnotus electricus*†, I have thought it right to renew the attempt to procure a spark from the Torpedo. I have tried the method which it is said succeeded with Mr. Walsh in the instance of the Gymnotus, namely, dividing with a pen-knife gold-leaf attached to glass, and connecting the divided parts with the contact wires. Using an active fish in this way I could neither observe a spark in the dark, nor in the light detect the slightest indications of the passage of electricity, either by the galvanometer or the more delicate test of the sensation or shock. I have been equally unsuccessful using an electroscope formed on the principle of Coulomb's, which displayed sparks when touched either by a small rod of glass slightly excited, or of sealing-wax; even when the Torpedo was taken out of water and all adhering moisture removed, no effect could be obtained, not even the slightest indications of attraction. I have varied the trials, using highly rarefied air at ordinary temperatures, and also condensed air deprived of moisture, with the same negative result. And I have been equally unsuccessful in substituting flame: unless the metallic points were in contact in the flame of the spirit-lamp, the passage of the electricity

* The spiral I have latterly used is of fine copper wire gilt, having about 300 convolutions to the inch: an inch of it weighs four tenths of a grain.

† Mr. Walsh is said to have written to M. le Roi to the above effect; and also that Sir John Pringle and M. Magellan assured M. le Roi they had witnessed the result repeatedly. Vide Bloch's Ichthyologie, p. 1020. Bloch refers for his information to Rozier's Journal, Ann. 1774. M. de Humboldt (Annales de Chimie et de Physique, tom. xi. 427.) states that the same result has been observed by M. Fahlberg. He refers to Vetensk. Acad. ny. quart. 2. (1801.)

appeared to be completely interrupted. In very many experiments, employing the most active fish, if there were any visible space between the ends of the red-hot platina wire, I never witnessed the galvanometer in connexion with one wire affected, nor could obtain a shock. Reasoning on the subject, this perhaps is what might be expected, considering that the surface of the fish is a better conductor than air. One fact, however, which I had observed, afforded some encouragement to persist in the trials, the fact that the torpedinal electricity passes through distilled water, which is a worse conductor of it than its own skin.

I thought it possible that by insulating the Torpedo on a plate of dry glass, and wiping its circumference dry and smearing it with oil, that either a spark might be procured, or that the galvanometer might be affected. But in this, too, I have been disappointed; not even in flame, when the interruption of the circle has been only just visible, has any effect on the instrument been produced, or any chemical effect, using the delicate test of solution of iodide of potassium and starch.

In a few experiments on metallic conductors, the effect of the electricity of the Torpedo on the galvanometer appeared to be much the same, whatever metals were used, and whether rusty or bright, provided the junctions were bright. The mass of metal appeared to have more influence; the effect, as might have been expected, diminishing with the increase of the mass; thus, when a poker weighing about two pounds formed a part of the circle, the effect on the electrometer, though distinct, was less powerful than when it was omitted; and when a large copper coal-scuttle was substituted for it, the effect was still more diminished, the deviation of the needle being only just visible. Extension of surface, as in the instance of increased length of wire, had a sensible modifying effect; thus, in an experiment in which about 1000 feet of wire were used (formed of three pieces, two about $\frac{1}{50}$th of an inch in diameter, the third piece considerably finer), the motion of the needle was decidedly slower than when a short length of wire was employed, though the space traversed was not perceptibly different.

The few experiments which I have made on the Torpedo, analogous to those instituted by Mr. Todd, described by him in the Philosophical Transactions for 1816, have afforded very similar results. When the brain has been divided longitudinally, the fish has continued to give shocks; when the brain has been entirely extracted, the fish instantly lost this power, though the muscles generally continued to act powerfully; nor could any shock be procured in this instance, either by puncturing with a sharp instrument the electrical nerves, where they quit the cavity of the cranium, or where they enter the electrical organs, just after passing between the branchial cartilages. On one occasion, however, it may be mentioned, that when a small portion of brain was accidentally left, contiguous to the electrical nerves of one side, and with which they were connected, then the fish, on being irritated, gave a shock to an assistant who grasped the corresponding electrical organ.

M. de Humboldt states that the shock of the Torpedo may be procured by touching, with the finger or hand, one surface only of the fish *. The experiments which I have made expressly on this point have led me to a different conclusion, namely, that it is requisite to touch the opposite surfaces of the electrical organs or organ, or a conductor or conductors connected with them, to receive a shock. In very many instances that I have irritated Torpedos by pressing with the finger on different parts of the back, as the upper surface of the electrical organs, and on the margin of the pectoral fins, however much the fish were irritated, I never had any sensation excited by the electricity, which there was reason to believe was discharged; though immediately after, on touching the two surfaces, irritating only the upper, shocks were received. On some few occasions, I have perceived a shock, when apparently only one surface of the fish was touched; but I believe in these instances the discharge took place through the water †. In corroboration, I may mention, that in experiments in which one surface only has been touched and irritated, the fish themselves appear to make an effort to bring, by muscular contraction, the border of the under surface (the upper being pressed on) in contact with the offending body. And this I have witnessed as distinctly in the fœtal fish as in the adult; clearly showing that the effort is instinctive ‡.

The conductor, which I suppose to be necessary for conveying the electricity when a shock is felt without immediate contact, exists in salt water. The galvanometer has been affected when the two extremities of it have been brought in contact, one with the back of the fish, and the other with the water, two or three inches from the fish. And in one instance I experienced a shock, although I touched the water alone, close to a Torpedo; it was in removing an active fish, by means of an earthenware dish, from one vessel into another: the hand that received the shock grasped the wet margin of the dish just as the Torpedo entered it.

I believe that the Torpedo has the power of discharging its electricity in any direction it chooses. This inference is drawn from finding that when one hand, in contact with the opposite surfaces of the fish, is receiving shocks, the other hand, immersed in the water close by, has received no shock. And, in confirmation of this, I may mention, (and at the same time to show how the discharge is connected with the volition of the animal,) that when I have applied to the opposite surfaces of a Torpedo copper plates, merely gently touching, joined together by a copper wire, and

* Annales de Chimie et de Physique, tom. xi. p. 430.

† The most remarkable example of the kind of which I have any note, was that of a young Torpedo, which gave slight shocks to the hand on which it was supported, whether just under the surface of the water, or just after it was taken out of the water into the air.

‡ In my former paper, I have supposed that the mucus with which the Torpedo is lubricated may be a conducting medium between the two opposite surfaces. This was an erroneous view; it may serve that purpose to the surfaces individually. Seeing the error theoretically, I was led to examine the margins of the fins, and they have appeared to me to have less mucus adhering to them than any other part of the fish, as if intended partially to insulate the electrical organs.

then irritated the fish with the contact wires in the usual manner, the galvanometer attached to the contact wires has been distinctly affected.

In my former paper I have stated my inability to account for my brother, the late Sir Humphry Davy, not having obtained any positive results in his experiments on the Torpedo. After reconsidering the subject, I am disposed to think it might have been owing to his using large fish, without the means of ascertaining their electrical activity, excepting by the shock. And we have seen, that when the human body forms part of the circle of communication with a galvanometer, the latter is not affected in the passage of the electricity producing the shock, which may serve to explain his not having witnessed any effect on the instrument at Trieste. As regards the electrical energies of large Torpedos, nothing is more uncertain. There appears to be no relation between the muscular and electrical power. I have seen strong vivacious fish, which made great muscular exertions in the water, almost or entirely destitute of electrical power; and, on the contrary, I have seen others languid and moribund, which have exerted considerable electrical power. Small fish are almost always active electrically, and they are greatly to be preferred as subjects for experiment. Mr. Walsh noticed, in the fish on which he experimented at Rochelle and the Isle of Ré, a retraction of the eyes of the Torpedo at the instant it exercised its electrical function. This I have not witnessed in the Torpedos of the Mediterranean; nor, indeed, have I been able to associate any visible sign, any apparent movement of the fish, with the electrical discharge.

The electricity of the Torpedo, theoretically considered, offers a wide field for speculation. Is it, it may be asked, merely a form or variety of common electricity, or a distinct kind, or not a single power, but a combination of many powers?

The first opinion, which is commonly received, and which has been ably advocated recently by Mr. Faraday, is supported by the majority of the facts adduced in this paper. The circumstance principally hostile to it, at least in appearance, is the interruption of the torpedinal electricity by the smallest quantity of air, and its want of the power of attraction and repulsion in the air.

These peculiarities are seemingly in favour of the second opinion, that the electricity of the Torpedo is specific and peculiar. But, till the opposite surfaces of the electrical organs can be perfectly insulated, so that no easier mode of communication is afforded than through air, they can hardly be considered as deserving of much weight*. The origin of the electricity of this fish perhaps offers a stronger argument in favour of its specific nature; being, apparently, peculiar and distinct from any known mode of electrical excitement, independent, as far as we can judge, of chemical action, or change of temperature, or change of form. But this argument may be

* In the experiments in which I attempted to insulate the surfaces by means of oil, the probability is, that I failed, and that a communication continued, if not by the outer surface of the skin, at least by its inner; indeed, the attempt to insulate these organs in the manner desired seems to be almost hopeless.

put aside by referring torpedinal electricity to animal secretion, the cause and nature of which are still a mystery *.

The third opinion may be indulged in as an hypothesis, and, as a guide to research, it may not be useless. It applies, however, almost as much to other varieties of electricity as to that of the Torpedo; all of which, it is possible, may be compounded, or owe their various effects to the union of several powers or ethereal fluids, and their peculiarities, compared one with another, to the predominance in various degrees of these fluids. What is known of the solar ray is not unfavourable to such an opinion; and the history of physical science, in relation to elementary ponderable matter, may rather give encouragement to the notion.

Malta,
March 4th, 1834.

Explanation of the Plates.

Plate XXII.

The figures in this plate are intended to show the progress of the embryo, the increase and decrease of the branchial filaments, and the decrease of the external and increase of the internal yolk-vesicle.

Fig. 1. Fœtal Torpedo and yolk-bag, at the period when the branchial filaments are very short, and do not carry red blood.

Fig. 2. Fœtal Torpedo and yolk-bag, at the period when the branchial filaments are beginning to carry red blood.

Fig. 3. Front view of the fœtal Torpedo, with the yolk-bag, showing the further increase of the branchial filaments, and the commencement of the development of the electrical organs.

* In examining the structure of the Torpedo, I have found that the skin covering the electrical organs above is not only more coloured, but also thicker than below, and more vascular, and surrounded by more powerful muscles, and supplied with a greater quantity of mucus; whilst the under surface appears to have a larger proportion of subcutaneous nerves. This difference of structure in the two surfaces of the electrical organs is probably somehow connected with their opposite electrical states.

I may here notice another peculiarity of organization common to both species of Torpedo, which came under my observation in seeking, though unsuccessfully, for the great sympathetic or the analogous ganglionic nerves, which Cuvier asserts exist in the cartilaginous fish (Histoire Naturelle de Poissons, tom. i. p. 438.). The peculiarity alluded to is represented by Plate XXIV. fig. 5. It has very much the appearance of a nervous ganglion, but is in reality a blood-vessel, enlarged into a little bulb, lined with a reddish substance like muscular fibre, giving the idea of a small heart. It is situated one on each side of the aorta, from whence it proceeds, just below the great plexus of nerves which supplies the pectoral fin; and the arterial branch derived from it is lost in this fin. If it be muscular, as it appears, its function may be to aid in propelling the blood into the pectoral fin, and perhaps into the electrical organ.

Fig. 4. Front view of the fœtal Torpedo, showing a more advanced stage in the development of the branchial filaments and electrical organs.

Fig. 5. Front view of the fœtal Torpedo at a more advanced stage, with the abdomen laid open to show,

A. The vitello-intestinal canal, connecting the yolk and intestine previous to the appearance of the internal yolk-vesicle;

D. The intestine distended with yolk.

Figg. 6 and 7. Fœtal Torpedos further advanced, with the cavity of the abdomen laid open, showing the internal yolk-bag, B, progressively augmenting, as the external yolk-bag, A, is diminishing.

Fig. 7. D. The intestine laid open below the stomach, E, showing the entrance of the vitello-intestinal canal.

Plate XXIII.

The figures in this plate are intended to show the diminution and disappearance of the external yolk-vesicle, and, with fig. 1. Plate XXIV., the diminution of the internal yolk-vesicle.

Fig. 1. Front view of a young Torpedo, showing a further diminution of the external yolk-bag.

Fig. 2. Ditto, showing the total disappearance of the external yolk-bag.

Fig. 3. Ditto, with the abdomen laid open, showing,

B. The internal yolk-bag, of large size.

Fig. 4. A young Torpedo, six weeks old; the cavity of the abdomen laid open, to show,

B. The internal yolk-bag, considerably diminished in bulk, and its connexion with the umbilicus almost absorbed;

D. The intestine, full of yolk;

E. The stomach considerably developed, but empty.

Plate XXIV.

Fig. 1. A Torpedo, six months old; the cavity of the abdomen laid open to show the internal yolk-bag, B, almost entirely absorbed, and with a mere vestige of the canal of communication remaining; D, the intestine; E, the stomach.

Fig. 2. Back view of a fœtal Torpedo, showing the accessory fasciculus of branchial filaments, described at p. 537.

Fig. 3. A. The glandular structure in the oviduct, just above the uterine cavity of the *T. diversicolor*.

Fig. 4. The uterine organs of the *T. oculata*, the infundibulum, ovaries, oviducts, uterine cavities (one laid open), and their common aperture in the cloaca.

Fig. 5. The bulbous vessels (supposed to be auxiliary hearts) connected with the aorta.

XXV. *Some Remarks in reply to Dr.* Daubeny's *Note on the Air disengaged from the Sea over the Site of the recent Volcano in the Mediterranean. By* John Davy, *M.D. F.R.S. Assistant Inspector of Army Hospitals.*

Received May 19,—Read May 29, 1834.

In the second part of the last volume of the Philosophical Transactions, at the request of the Council of the Royal Society, Dr. Daubeny has stated his objections to the explanation which I have proposed of the origin of the air disengaged over the sunken remains of the volcano of 1831, as described in my last paper on the subject *.

I am induced to reply to these objections on the ground that they do not appear to me well founded, nor compatible with the facts which I have brought forward.

In the paper alluded to, after having noticed the composition of the air, which I had found to consist of about 80 per cent. azote and 10 oxygen, I remarked that two views might be taken of its origin; one, that it was of volcanic source; the other, that it was derived from the sea water, and merely disengaged by the heat of the volcano.

The first view, that which Dr. Daubeny advocates, I could not adopt, as it appeared to me least probable. The minuteness of the quantity of air observed in its ascent by Captain Swinburne, rising "in small silver threads of bubbles," (this is his expression,) seemed very unfavourable to the idea of a deep volcanic source; and the admixture of oxygen with the azote seemed to me to demonstrate that its source could not be deep. For the sake of argument, let us suppose, with Dr. Daubeny, that the volcano had a submarine communication either with Malta, more than 100 miles distant, or with the nearest parts of Sicily, at least 20 miles distant. Supposing it possible that air could penetrate so far, taking the shortest distance of 20 miles, it appears incredible that it should not be deprived of its oxygen in its passage, especially considering the nature of the matter thrown up by the volcano, containing elements possessing an attraction for oxygen. Whether the whole intermediate tract is imagined to be similarly composed, or only the volcanic region, seems immaterial, as a few feet thickness of such material may be supposed sufficient to deprive of its oxygen a very large quantity of atmospheric air. Had the volcano been supplied with atmospheric air through channels of communication with the land, some indications of such a supply, it might be expected, would have been witnessed during the period of its active eruption. But, as I have stated in my former paper, no indications of the kind occurred: the iron contained in the ejected ashes was generally in the state of protoxide, and the cinders generally contained traces of sulphur, and the gaseous products appeared to be very inconsiderable, and such as might be fairly attributed

* Philosophical Transactions, 1833, Part I.

to the effect of heat in expelling carbonic acid from carbonate of lime and magnesia, and the burning of the sulphur thrown out in coming into the atmosphere.

The second view, that which Dr. Daubeny opposes, appears to me to be free from all serious difficulties, and not liable to the objections which Dr. Daubeny has brought against it. Very minute streams of air, differing only from common air, or the air contained in water, in having less oxygen, are observed rising from a bed of volcanic ashes and scoriæ a few fathoms below the surface of the sea; and where they rise, the cinders are not black, as they are elsewhere, but of an ochry hue. These were the circumstances of the occurrence. The inference I drew was, that the air was expelled from the water by the heat of the bottom, and that it was deprived of part of its oxygen by the attraction of the black oxide of iron, and its conversion into peroxide. If we suppose that the spots from whence the air rose were the mouths of fissures through which steam ascended, the results, it appears to me, were precisely those which might be expected. What the exact state of the bottom was in regard to temperature, it is impossible to decide from that of the surface; but that it might have been what I have supposed, is most easy of belief. When the volcanic island was last visited, just before its disappearance, and its crumbling masses falling to pieces, from the pressure of the hand or foot, eluding the grasp, and suggesting to the illustrious individual who last landed on it the notion of a magician of the old romance, as I heard Sir Walter Scott relate on his arrival in Malta, even then its sides were still warm, and in some places so hot that Miss Scott's shoes were burnt. If so, four months after its eruption had ceased, there is no difficulty in the idea that the shoal it formed seven months after submersion might, in relation to temperature, be what I have imagined.

Dr. Daubeny in his remarks omits to notice the cause I have considered in special operation for the removal of a portion of the oxygen of the air, namely, the peroxidation of the iron. He combats chiefly the opinion I have expressed, that, generally, air in descending from the surface to the depths of the ocean, will be deprived of oxygen by the action of living and dead matters swimming or suspended in the water. He is of opinion that this is not the case; that it is contrary to the analogy of nature; that it is disproved by the existence at great depths of algæ of an intensely green colour. Were this a well authenticated fact, I should consider it a decisive proof; but I am doubtful of the fact. I have never heard of sea weed having been brought up by the lead from great depths in sounding; in no charts which I have ever consulted is such bottom noticed. And there are certain facts which are hardly in accordance with it; such as the state of iron cannon which have been sunk in deep water during a long period, and have been converted, as it were, into plumbago; such as the preservation of wood under sea water for many years, and indeed for many centuries, in the bed of the sea, but occasionally brought to light and thrown up by storms, sweeping away the incumbent layer of sand. The depths of the ocean, as well as its breakers, may be intended in the economy of nature for other purposes than those of animal or vegetable life. As on those shores on which the waves dash with greatest violence, sandstone barriers are forming from the deposition, amongst the sand thrown up, of cementing carbonate of lime, set free from its solution by the disengagement of the carbonic acid gas, owing to the agitation ; so, in the greatest depths, deprived of oxygen, vegetable matter may be tranquilly subsiding, and in process of conversion into beds of coal, for which the temperature of the water at its maximum density, judging from the formation of peat, appears to be most favourable.

Dr. Daubeny objects to the disengagement of air from water under the circumstances supposed, subjected to a high temperature under pressure. He says : "Either in this situation the pressure of the superincumbent mass of fluid is sufficient to prevent the conversion of the lower strata of water into steam, or it is not. If it be, this same pressure will enable the water to retain in solution its original quantity of air, or at least the greater proportion of it. If, on the contrary, there be not sufficient pressure for the purpose, then no doubt the water will rise up in the form of steam, through the superincumbent mass, along with the air which it had contained ; but, as the temperature of the sea round about the volcano, at least near its surface, is stated by Dr. Davy not to be higher than that of the atmosphere, it is plain that all the steam must become rapidly condensed, and when it returns to its liquid state, there seems no reason why it should not exert its affinity for the air intermixed, and combine with it as before." The greater part of this reasoning relative to the effect of pressure does not appear to me applicable to the phenomenon in question. The volcanic shoal over which the air rose, as already mentioned, was only a few fathoms deep : the circumstances may be considered somewhat analogous to those of an experiment in the laboratory for the expulsion of air by boiling water in a retort connected with a pneumatic apparatus. In this latter instance, the air is disengaged and the steam is condensed, and yet the air is not reabsorbed ; nor does there appear to me more reason why it should be reabsorbed in the instance under consideration. Supposing a portion of sea water to be converted into steam in the fissures of the shoal, the instant it comes in contact with the cold water it will be condensed, like the steam from the retort, and will be diffused by mingling with adjoining water : but the air disengaged at the same time will not be reabsorbed ; owing to its comparative lightness, it will immediately ascend, and passing through water already saturated with air, reach the surface.

Dr. Daubeny supposes that the quantity of air emitted, as observed by Captain Swinburne, bore some relation to that evolved from chalybeate springs ; from whence he infers that "so constant a supply could hardly be derived from such a source as sea water." Were not the quantity very minute, "in small silver threads of bubbles," and varying from time to time in the same place, as Captain Swinburne relates, the objection would have weight. But Dr. Daubeny admits that this minute quantity is very much less than occurs in most thermal springs ; consequently the argument loses its force. Relative to the analogy alluded to by Captain Swinburne between the bottom from whence the air rose and a chalybeate spring, it appears to me that

Dr. Daubeny has mistaken his meaning, and that the similarity he wished to point out existed not in the bubbling of the gas (that not being peculiar to a chalybeate spring), but in the change of colour of the bottom; for he says where the air came up most plentifully, there "the cinders (elsewhere quite black) had a rusty appearance."

As to the inferior degree of interest which Dr. Daubeny connects with my manner of explaining the phenomenon, it is of little importance. Truth, of course, or an approximation to it, is the end of inquiry; and the explanation which I offered had the preference, because it appeared most in accordance with the facts. I would not wish to undervalue Dr. Daubeny's speculations; but in attaching so much interest to his opinion, that the atmosphere is the source of the gas disengaged, derived through subterranean channels, I cannot help thinking he has had recourse to a difficult hypothesis of little usefulness; for what facts are there in favour of the idea that volcanic fires are fed like ordinary fires, or in any way dependent on the atmosphere for their activity?

That azote is often a product, and an abundant one, of extinct volcanos is certain; but it does not follow that it is also a product of active volcanos. Probably the ammoniacal salts which form in such abundance in certain solfataras, of which that of the island of Volcano is the most remarkable, is owing to a complicated play of chemical affinities, in which atmospheric air, sulphur, alumine, and steam are the elements chiefly concerned.

Dr. Daubeny concludes his note by expressing the wish that the quantity of gas evolved by thermal springs should be ascertained. This is a scientific desideratum; but some caution is required how the knowledge so obtained is made a test of the truth of any theory of the origin of the air in such springs. Atmospheric air may be carried down not only dissolved in water, as in the rains feeding springs, as I have dwelt on in my former paper, but it may be also forced down mechanically in froth by the impetus of a descending stream of water, and, so entangled, may find its way to a great distance; and according to the nature of the strata and channels through which it passes, it may either lose oxygen by the attraction of metallic bodies, or have its oxygen converted into carbonic acid by the action of carbonaceous matter, or it may ascend unaltered. A remarkable instance of this last-mentioned condition presents itself in the springs of the Sava, about a quarter of a mile above Wurzen. It is recorded in my brother's Journal, in which, on the 27th of August 1828, he has written: "Admired the sorgente Sava,—a number of deep circular holes, with air bubbling through them, and large jets of water, which is beautifully clear." And on the 30th he added: "Examined this evening the air disengaged in such large quantities where the Sava rises. It appeared to me to possess all the characters of *common air*, was not absorbable by water, and supported flame in the same manner as common air."

Malta April 20, 1834.

XXVI. *On the Ova of the* Ornithorhynchus paradoxus. *By* Richard Owen, *Esq., Assistant Conservator of the Museum of the Royal College of Surgeons in London. Communicated by Sir* Anthony Carlisle, *F.R.S.*

Received May 15,—Read June 19, 1834.

THE modes of generation of which the ultimate result is the birth of young endowed with powers of action and liberated from the fœtal coverings, are usually comprehended under the terms viviparous and ovoviviparous. But the processes by which the requisite development of the fœtus is effected in the first of these modes, vary remarkably; and so far as they have been investigated in the different orders of *Mammalia*, to which true viviparous or placental generation is peculiar, a very regular gradation has been traced towards the oviparous or ovoviviparous modes, in which the exterior covering of the ovum never becomes vascular.

As lactation has been generally regarded as exclusively associated with a true viviparous generation, the arguments adduced in favour of the mammary nature of the abdominal glands of the Ornithorhynchus have been supposed to imply a necessary belief in the accordance of its mode of generation with that of the higher orders of *Mammalia*. They have consequently been objected to most strenuously by those physiologists who maintain the oviparous nature of this animal*: and various explanations have been offered, with a view to reconcile the lately ascertained facts respecting the mammary glands with the oviparous theory of the *Monotremata*, and their supposed position in the natural system as a distinct class of *Vertebrata*. The reasonableness or necessity of these objections would have been more apparent if the essential dependence of lactation on placental development had first been demonstrated: for with respect to the observations† against which they were directed, these were confined to the elucidation of a single disputed and doubtful point in the economy of the *Monotremata*; the uterine apparatus being considered so far only as was necessary to determine the correspondence of its periodical changes with those of the mammary glands; while the objections to the oviparity of the Ornithorhynchus extended only to the theory which maintained that the ovum was expelled with a calcareous covering, and that embryonic development took place after exclusion by a process of incubation.

In proceeding now to the more immediate consideration of the structure of the

* Geoffroy St. Hilaire, in the Gazette Médicale de Paris for January and February, 1833; Revue Encyclopédique for July and August, 1833.

† Philosophical Transactions, 1832, p. 517.

ovary of the Ornithorhynchus, with a view to determine its exact relations with that of the normal *Mammalia*, I believe myself in no way biassed by the proof of the mammiferous nature of the Ornithorhynchus, which has been afforded by the concurrent testimonies of several scientific observers who have themselves witnessed the lacteal secretion; since it is obvious that in order for the milk, the elaboration of which is determined by the derivation of blood from the generative system, to meet with a due recipient after the cessation of the uterine functions, it is only necessary that the offspring should possess the capability of receiving the maternal secretion, and not that it should have gained that power by any particular mode of development, or through the agency of any given system of vessels.

From an examination of the internal structure of the mammary fœtus of the marsupial animals, there appeared, indeed, previously to the present inquiry, to be grounds for entertaining a belief that lactation might coexist with a mode of generation essentially similar to that of the Viper and Salamander; and a subsequent examination of the uterine fœtus in the Kangaroo has gone far to establish the truth of this supposition*.

In the specimen of the female organs of the Ornithorhynchus figured in Plates XVI. and XVII. of the Philosophical Transactions for 1832, two ovisacs, or Graafian follicles, had taken on the action of preparation for the male influence; and, it is probable, from a comparison of these ovisacs with the corpora lutea of specimens hereafter to be described, in which the ova had recently passed into the uterus, that they had nearly if not quite attained their full development. This opinion is further corroborated by the circumstance of the uterus to which the ovisacs in question belonged having increased almost to the size of that of the impregnated female figured at Plate XXV. fig. 2. in illustration of the present communication. In a female

* LATREILLE, in his Familles Naturelles du Règne Animal, 8vo, 1825, excludes the *Monotremata* from the Mammiferous series, at the conclusion of which he observes, "tous ceux dont nous traiterons désormais sont ovipares ou ovovipares, et par conséquent dépourvus de mamelles." (p. 66.) ARISTOTLE says, "No oviparous animal has an epiglottis:" and there are perhaps few generalizations in the Historia Animalium that testify more strongly to the extent of his anatomical researches. This remarkable proposition has stood the test of ages of subsequent research, and is adopted by CUVIER without any modification in the Règne Animal (Nouv. Ed. tom. i. p. 300.); yet it must always have been difficult to suppose that the relation which subsisted between a small part of the larynx and any given mode of generation could be other than that of simple coincidence; and it now appears that, in the sense in which CUVIER defines oviparous generation (Ibid. p. 300.), the *Marsupiata* do form an exception to the Aristotelian rule. The Ornithorhynchus also possesses a large and well-formed epiglottis, and it certainly has mammary glands. From this we may be led to conclude that an epiglottis is formed not because the fœtus is developed by a placenta, but because it is to be nourished after birth by a lacteal secretion. The larynx requires, at that feeble period, an extraordinary protection, for which the young bird or reptile has no need: and it is worthy of remark, that the epiglottis is proportionately developed as the young mammal is prematurely born. Having therefore, as its peculiar coexistence with lactation would show, an especial reference to the earlier periods of life, we can better understand why the epiglottis should be of secondary importance to the adult, in which both accident and experiment have shown that it is not essential to safe deglutition.

Ornithorhynchus preserved in the Military Museum at Chatham*, I subsequently found three ovisacs developed in the ovary to a similar but not greater extent. Mr. Hill†, who first detected the ovarian ova in the recent animal, also states that they were not larger than a small pea; and they have never exceeded that size in any of the specimens examined by Sir Everard Home.

A knowledge of the size which the ovarian ovum of the Ornithorhynchus acquires before it finally escapes, is of great consequence in forming a judgement as to the ultimate mode of development of the embryo, as a direct deviation from the generation of the ordinary Mammal, and a proportional approximation to that of the Bird, would be manifested in the ratio of the accumulation of the vitelline matter in the ovisac. Now it has very recently been supposed that the ovulum of the Ornithorhynchus attains a greater diameter than in other *Mammalia* before passing from the ovary; and Professor De Blainville‡ adduces in favour of this opinion the size of the ova discovered by Lieutenant Maule, and the corresponding capacity of the orifice of the Fallopian tube. But the expression of the zealous officer just alluded to, leads to the belief that the eggs of the size of a large musket-ball which he saw, were in the uterus rather than in the ovary; and the size of the orifice of the Fallopian tube in *Mammalia* is in relation with that of the entire ovary which it embraces, and not with that of the ovum which it is destined to transmit to the uterus.

The two ovarian ova or ovisacs § were two lines and a half in diameter, and adhered to the ovary by about one third part of their whole circumference.

In the specimen in the Chatham Museum the three ovisacs which had attained nearly the same size, were attached to the ovary by a smaller portion of their circumference, but were still sessile, and not appended, as in the Bird, by a distinct pedicle. In both specimens smaller ovisacs of different sizes projected to a greater or less extent from the surface of the ovary.

The clustered form of the ovary, which results from this position of the ovisacs, is not, however, peculiar to the Ornithorhynchus among *Mammalia*, but obtains in a greater or less degree throughout the Marsupial and Rodent orders. De Graaf long ago figured the ovary of the Rabbit as composed, when prepared for impregnation, like that of the Ornithorhynchus, of a cluster of spherical ovisacs or folliculi ||; and Daubenton, in describing the ovaria of the Black Rat¶ and of the Water Rat**, particularly notices their tubercular or racemose figure, and the yellow colour of the

* My thanks are especially due to G. J. Guthrie, Esq. F.R.S., and to the officers in charge of the Museum at Fort Pitt, for the facilities there afforded me in the examination of this specimen.

† Transactions of the Linnean Society, vol. xiii. p. 623.

‡ Nouvelles Annales du Muséum, tom. ii. p. 405.

§ Philosophical Transactions, 1832. Plate XVI.

|| De Mulierum Organis, Tab. xxv. p. 412. Opera Omnia, Lugd. Batav. 1677.

¶ Buffon, Hist. Nat., tom. vii. Plate xxxviii. p. 293.

** Ibid., Plate xlvi. p. 357.

larger ova; the affinity, therefore, which the Ornithorhynchus and some other Marsupial animals manifest in this particular to the class of Birds, obtains also in those orders of the more normal *Mammalia,* of which the fœtus is characterized by the magnitude and persistence of the vitelline or umbilical vesicle.

The structure of the ovary of the Ornithorhynchus exhibits all the essential characters of the mammiferous type of this organ; its fibrous coat is strong and inelastic, and the cellular substance in which the ovisacs are imbedded is dense, and cannot be stretched without much force. On making a section through an ovisac or Graafian follicle, it is found to be implanted more or less deeply in condensed layers of this cellular substance, to which its theca closely adheres. The innermost layer of the theca is less vascular, thinner, and smoother on the inner surface than in the corresponding coat in the human ovisac.

In the Ornithorhynchus the theca of the expanded ovisac at the most prominent part is very thin and transparent; and the capsule of the ovary is either wanting, or blended with its peritoneal covering, which is slightly protruded, as though the pressure of the contained follicles had stimulated the absorbents to remove the resisting laminæ, as in the progress of an abscess to the surface of the skin. In the true *Ovipara,* as in the Fowl and Tortoise, &c., this partial thinning of the capsula ovarii surrounding the mature ovarian ovum, is less perceptible on account of its general tenuity; and the part where the ovum is about to escape is indicated chiefly by the extremities of the vessels, which converge to it from all parts of the capsule; it is also of a linear form; while in the Ornithorhynchus and other *Mammalia* it is a circular protuberance.

The contents of the ovisacs of the Ornithorhynchus consist of minute granules, which in the larger ovisacs are applied in a condensed state to the inner surface of the containing membrane, and there form a granular stratum. I have opened with great care many ovisacs of the Ornithorhynchus, of different sizes, under the microscope, separating with the point of a needle the clusters of granules as they escaped, and have examined the inner surface of the capsule, especially opposite the mammillary projection, but never succeeded in detecting the vesicle described by Purkinje and Von Baer as existing in the ovarian ova of other animals: the long maceration of the parts in spirit may, however, have destroyed this delicate but essential part of the ovum; and the coagulation of the albumen, which is mixed with the granules, adds greatly to the difficulty of this delicate investigation.

The contents of the larger ovisacs above described varied both in colour and consistency: in the smaller of the two taken from the first specimen, the fluid in which the granules were immersed was more abundant, and bore a slight straw-coloured tinge; in the other ovisac from the same animal, and in those of the Chatham specimen, the granules were more numerous, the contents having a caseous consistency, and being of a deep yellow hue.

De Graaf describes a similar variety in the consistence and colour of the contents of the ovarian follicles or ovisacs in the Rabbit,—a difference which he considers to be dependent on impregnation; the ovisacs then becoming denser and of a redder tinge until the ova escape, which takes place the third day after the coitus.

From a comparison of the ovarian ovum of the Ornithorhynchus with the mature ovarian ova of the Rabbit, the Sow, and the Ewe, the principal difference consists in the greater proportion of granules in the contained fluid, and in the more coherent nature of the external granular stratum, which however appears not to possess the necessary consistence to be, with its contents, expelled entire from the ovisac. In the Fowl, on the contrary, where there is no adhesion between the ovarian ovum and the calyx, the former passes unbroken into the oviduct. Thus in every essential particular the monotrematous ovum up to this period of development is the same as that of the ordinary *Mammalia*; and its structure is in exact physiological correspondence with the mode of nourishment of the young animal.

Soon after the preceding observations had been made, three uteri of the Ornithorhynchus, containing ova of different sizes, were transmitted from New South Wales to the Museum of the Royal College of Surgeons, by my friend George Bennett, Esq. F.L.S.*

The Board of Curators having liberally granted me permission to describe and figure these interesting preparations, I am enabled to resume the subject of the previous observations, and proceed with the description of the ovaries as they appear after the impregnation and the escape of the ova into the uterus.

In each of these specimens, the left ovary only had taken on the sexual actions, but did not exceed in size the same parts in the unimpregnated specimens above described. The right ovary had, however, become enlarged; it measured half an inch in length, a third of an inch in breadth, and was about half a line in thickness: a few ovisacs, about the size of a small pin's head, projected from the surface.

The left ovary in each of the specimens was concealed by the thin membrane, forming the expanded orifice of the oviduct. In one of these it was with some difficulty it could be withdrawn from the Fallopian aperture, owing to the adhesion which was produced by what appeared to be a coagulated secretion; a circumstance which must have effectually ensured the passage of the ovum into the oviduct.

In two of the specimens, the left ovary presented two empty ovisacs, or corpora lutea (Plate XXV. fig. 2. *b.*), corresponding with the number of ova found in the uterus. In the third specimen, the left ovary presented two ovisacs still uncicatrized, but only one ovum was contained in the uterus. In a fourth specimen, three similar ovisacs were present, but the ova had been removed from the uterine cavity.

The discharged ovisacs were of an elongated flask-shaped form, about three lines

* Natural history owes much to this gentleman; he discovered what had so long been a desideratum in science, the animal of the Pearly Nautilus, and first transmitted to this country the impregnated uterus of the Kangaroo; and now his indefatigable exertions have materially contributed to elucidate the still more obscure subject of the generation of the Edentate *Marsupiata*.

in length, and two in diameter, with the margins of the orifice, through which the ovum and granular substance had passed, everted, with a slight contraction, resembling the neck of a flask, below the aperture. On compressing these ovisacs, small portions of coagulated substance escaped. When longitudinally divided, they were found to consist of the same parts as the ovisac before impregnation, with the exception of the granular contents and granular stratum; but the theca, or innermost parietes of the sac, was much thickened, and encroached irregularly upon the empty space, so as to leave only a cylindrical passage to the external opening.

De Graaf's accurate figure of the corpora lutea in the Rabbit is given at Plate XXV. fig. 11, to show the close correspondence between the two animals in the appearance of these parts; and their structure is essentially the same.

The undischarged ovisacs of the left ovary, in the impregnated Ornithorhynchi, were numerous and of a globular form, but did not exceed a line and a half in diameter; a circumstance which corroborates the opinion before expressed relative to the size of the mature ovisacs. For if these parts really attained, prior to the escape of the ovum, much greater dimensions than those in Plate XXV. fig. 1., it might be expected that the other ovisacs would at least have exhibited some proportional degree of increase.

The impregnated Ornithorhynchus, in the uterus of which the two smallest-sized ova (Plate XXV. fig. 3.) were found, was shot on the evening of the 6th of October 1832, in the Yas River, Murray County, New South Wales. These ova were of a semitransparent white colour when recent, but had lost that appearance when examined at the Museum, to which they had been transmitted, *in situ*, with the uterus and surrounding parts well preserved in spirits. The ova were situated at the upper part of the left uterus, and at the distance of about a line from each other. Each ovum was spherical in form, and measured two lines and a half in diameter: they were of a deep yellow colour, with a smooth and polished surface, and had not the slightest adherence to the uterine parietes.

The specimen containing the two ova next in size (Plate XXV. figg. 2 and 4.) was shot in the same locality on the 7th of October. These ova measured each three lines in diameter, and were situated a little below the middle of the left uterus: they were of a spherical form, but had evidently been slightly compressed in the uterine cavity. They were of a lighter colour than the preceding; a circumstance which was specially evident at the upper part, from the subsidence of the contained vitelline mass. Externally they were smooth, and rolled freely out of the position where they were lodged, like those of the preceding specimen.

The third specimen, in the uterus of which the largest ovum was contained, was shot on the evening on which the first specimen was obtained. This ovum had the same spherical form, smooth exterior surface, and freedom from connexion with the uterus, as the preceding, but was of a much lighter colour, owing to the increased quantity of its fluid contents, to which its greater size was chiefly attributable. It measured three lines and a half in diameter, and had been situated in a depression or cell a little below the middle of the left uterus. The lining membrane of the uterus was highly vascular in the recent state in each of the above specimens.

In all these ova the contents could be seen, through the cortical or outer membrane, to be of two kinds, viz. a greyish subtransparent fluid, and a yellowish denser mass, which varied in their relative proportions as above mentioned, the denser substance always subsiding to the lowest part of the ovum, whichever way it was turned.

In the largest ovum, the yellow mass or yolk occupied about one third of its cavity, while in the smallest it constituted four fifths of the whole mass.

The chorion or cortical membrane of these ova (Plate XXV. fig. 6. *a*.) offered a moderate degree of resistance when torn open with the forceps, and yielded equally in every direction when separated from the yolk, the rent margins curling inwards like the coat of an hydatid. This membrane was of a dull greyish colour, inclining to brown, slightly transparent, and more polished upon its inner than upon its outer surface: it resembles the cortical membrane of the ovum of the Salamander, but is of a more delicate texture. The fluid contents occupied the space between the cortical and vitelline membranes, a situation analogous to that of the albumen in the egg of the Fowl, but had not become coagulated by the action of the spirit in which it had been so long immersed.

The yellow matter, or yolk, was seen to be invested by its proper capsule (Plate XXV. fig. 6. *b*.), which, when reflected under the microscope, was found to consist of an extremely thin, smooth, and transparent outer layer, which I regard as the *membrana vitelli* (Plate XXV. fig. 7. *a*.), with a thicker granular membrane immediately lining it, analogous to the *blastoderma* or germinative membrane (Plate XXV. fig. 7. *b*.)

The contents of the above investments, or substance of the yolk, consisted of innumerable minute opake granules, similar in size and regularity of form to those contained in the ovarian follicles; and with these granules were mingled larger transparent globules of yellow-coloured oil. There was not the slightest trace of chalazæ attached to the vitelline membrane, as, from analogy, we should expect to be the case had the ovum been destined to have been perfected by incubation. I was unable to detect any rudiments of the embryo: an opake streak was discernible on one part of the yolk, but not sufficiently definite to be satisfactorily recognised as a cicatricula; it is indeed, probable, from the observation of Lieutenant Maule, that the ova attain a greater size by the imbibition of nutrient material before the lineaments of the fœtus become visible.

The ova of the Rabbit figured by De Graaf the seventh day after the coitus, agree in size with the largest of the ova of the Ornithorhynchus: in Mr. Cruikshank's plate* they are represented somewhat smaller. According to both authors no trace of fœtal development is visible at this period; but it is probable that the formative actions have commenced, as the ova of the Rabbit have now contracted

* Philosophical Transactions, 1797, Plate IV. p. 204.

an adherence to the parietes of the uterus. On the sixth day, when the ova of the Rabbit nearly correspond in size to the smallest ova of the Ornithorhynchus above described, they are equally devoid of any adherence to the uterine walls. As, however, the differences between the ova of these animals are so obviously manifested in the greater strength of the outer, or cortical membrane, of the ova of the Ornithorhynchus, and in the magnitude which they are already known to attain before any distinct development of the fœtus can be perceived, there can be little doubt that the generation of this species proportionately approximates towards the oviparous mode.

On comparing the ovum of the Rabbit with that of the Bitch, it is seen to attain in the former to a considerably larger size before it contracts an adhesion to the uterus, which appears to have relation to the greater share which the umbilical vesicle has in the development of the embryo; since in the Kangaroo, in which the umbilical vesicle fulfills the functions of the placenta, the chorion remains unattached to the uterus, and unvascular when the fœtus is almost fully formed. And as the quantity of vitelline granules accumulated in an ovum is indicative of the size and persistence of the umbilical or vitelline vesicle, we may infer that, in the Ornithorhynchus, the latter will play an important part in the development of the embryo.

The changes which the impregnated uteri of the Ornithorhynchus had undergone, as compared with the same part in the quiescent state, were greater than those which have been observed to take place in the Kangaroo. The uterus containing the two smallest-sized ova measured seven lines in diameter, but was much firmer and denser than in the unimpregnated specimens; and having also increased in length, was thrown into more abrupt curves on either side of the ovarian ligament. The uterus which had contained the largest ovum measured an inch in diameter; and that containing those of the second size was of nearly the same size (Plate XXV. fig. 2.). The right uterus in all the specimens had become sympathetically affected, being firmer in texture and thicker in its coats.

The parietes of the impregnated uteri were from three to four lines in thickness; an increase which was principally occasioned by the extension of small vascular folds between the fibrous and internal coats, which were so placed at right angles to these tunics as to present an appearance very similar to that of the second cavity of the stomach of the Porpesse. The fibrous coat was slightly thickened near the cervix, and the serous covering was separated from it by the ramifications of numerous large and tortuous uterine vessels.

There was not the slightest trace of a decidual or adventitious membrane in the cavity of the womb; and especial attention was directed to this circumstance in consequence of the office assigned to it in a recent work*, as ministering support to the ova in the higher *Mammalia*, at a period when, like those of the Ornithorhynchus, they have no attachment to the uterine parietes†.

* Breschet, Etudes de l'Œuf Humain.

† In the recent specimens Mr. Bennett noticed besides the ova only a "moisture" in the uterus.

It may, however, be said that the deciduous membrane is here represented by the cortical or outer covering of the ovum: but this membrane, though of a denser structure and without villi, is certainly analogous to the outer tunic of the uterine ovum of the Rabbit and Bitch, which in them is gradually separated from the vitelline membrane by the imbibition of albuminous fluid. Now the relative proportion of the fluid interposed between the cortical and vitelline membranes in the small and large ova of the Ornithorhynchus, shows that the mutual recedence of the two membranes is effected in the same way.

The form, the structure, and the detached condition of the ova of the Ornithorhynchus, may still be regarded as compatible with, and perhaps favourable to, the opinion that they are excluded as such, and that the embryo is developed out of the parent's body. But the following objections present themselves to this conclusion;—the only part of the efferent tube of the generative apparatus which can be compared in structure or relative position to the shell-secreting uterus of the Fowl, is the dilated terminal cavity in which, in all the specimens above described, the ova were situated; and upon the oviparous theory it must be supposed either that the parietes of this cavity, after having secreted the requisite quantity of soft material, suddenly assume a new function, and complete the ovum by providing it with the calcareous covering necessary to enable it to sustain the superincumbent weight of the mother during incubation; or that this is effected by a rapid deposition from the cuticular surface of the external passages; or lastly, according to a more recent, but still more improbable supposition, by a calcareous secretion of the abdominal glands poured out upon the ovum after its exclusion.

But granting that the egg is provided in any of these ways with the necessary external covering, yet from the evidence afforded by the specimens under consideration, the ovum is still deficient in those parts of its organization which appear to be essential to successful incubation, viz. a voluminous yolk to support the germinal membrane, and the mechanism for bringing the cicatricula into contiguity with the body of the parent. Add to this, that such a mode of development of the fœtus requires that all the necessary nutritive material be accumulated in the ovum prior to its exclusion. Now the bony pelvis of the bird is expressly modified to allow of the escape of an egg, both large from the quantity of its contents, and unyielding from its necessary defensive covering; but whatever affinities of structure may exist in other parts of the Ornithorhynchus, it is most important to the question of its generation to bear in mind that it manifests no resemblance to the bird in the disposition of the pubic bones.

Again, as we have seen that the ova of the Ornithorhynchus have attained a diameter of little more than two lines after having traversed the whole of the Fallopian tube, the length of which is six inches, and the internal secreting surface increased by numerous folds, it may be reasonably inferred from the analogy of the Rabbit and other *Mammalia*, that the ovum was of much smaller dimensions when first received into the oviduct. But the yolk in Birds and oviparous Reptiles is invariably the product of the ovary, and derives no appreciable increase from the secretions of the efferent tube, which supply only the albuminous part of the egg, or the material for the first formation of the chick. If, therefore, the gestation of the Ornithorhynchus terminates by the exclusion of an egg, as in the Bird or Tortoise, the preparatory steps in the formation of the ovum are widely different, for the parts concerned manifest the essential characters of the mammiferous type, and the germ itself has a corresponding structure.

These facts, it is agreeable to find, are in exact accordance with the now ascertained functions of the abdominal glands; for since the yolk in the Bird, besides its uses in the course of the fœtal development, is intended as an after-substitute for a mammary secretion, remaining, as it does, but little diminished at the close of incubation, it might have been concluded, from *à priori* physiological deduction, that the Ornithorhynchus, in which no such substitute is required, would approximate the other *Mammalia* in the small size of the ovarian ovum.

The nature or amount of subsequent deviations from a true viviparous generation, can be determined only by future examinations of more advanced ova. From the structure of the cortical membrane it is probable that they do not become organized, and that the *Monotremata*, like the *Marsupiata*, are essentially ovoviviparous. Since, however, the female Ornithorhynchus has no tegumentary pouch to protect a prematurely born offspring, it must be presumed that the fœtus acquires greater proportional bulk* and more mature strength by a longer continuance within the uterus. In this case it may be doubted whether the vitelline vesicle will suffice for nourishment and respiration through the whole period of development, and the allantois and umbilical vessels will probably be more or less developed for that purpose.

The means of prosecuting this inquiry are the more likely to be afforded, since, through the exertions of Mr. Bennett, the period when the pregnant female may be procured is now ascertained. Had not a specimen supposed to be in this condition, which my friend had preserved alive, unfortunately escaped from its confinement, he would, there is little doubt, have ascertained the true nature of the generative product, and the probable duration of gestation.

With reference to the latter point, Mr. Bennett observes, that two months after the capture of the female specimen with the smallest ova, viz. on the 8th of December 1832, he succeeded in laying open one of the burrows of the *Ornithorhynchi*, on the banks of the Murrumbidgee River, in which three living young ones were found: they were naked, and measured only one inch and seven eighths in length, and he considers them to have been recently brought forth. Not having any means of preserving these specimens, and being at a great distance from Sidney, they were lost.

* In reference to this point it may be observed, that the kidneys are not lodged low down in the pelvis, as in the true *Ovipara*, but occupy the position characteristic of the mammiferous type of structure, which allows free space for the enlargement of the uterus during pregnancy.

The nest was most carefully scrutinized by Mr. BENNETT, but not the slightest trace of egg-shell could be perceived in it.

The principal points, therefore, in the generative economy of this paradoxical species which still remain to be determined by actual observation, are,

1st. The manner of copulation.

2nd. The season of copulation. (This is probably at the latter end of the month of September, or beginning of October.)

3rd. The period of gestation.

4th. The condition of the ovum both before and immediately after it has quitted the ovisac.

5th. The nature and succession of the temporary structures developed for the support of the fœtus during gestation.

6th. The exact size, condition, and powers of the young at the time of birth.

7th. The act of suckling.

8th. The period during which the young requires the lacteal nourishment.

9th. The age at which the animal attains its full size.

Description of the PLATE.

Fig. 1. The ovary and expanded extremity of the Fallopian tube of an Ornithorhynchus preserved in the Military Museum at Chatham, in which three ovisacs, or Graafian follicles, had taken on the sexual actions.

Fig. 2. The pelvis and surrounding parts of a female Ornithorhynchus, with the urinary and genital organs *in situ*: the left uterus contains two ova.

a. The left, *a'* the right, ovary.

b. The two discharged ovisacs or corpora lutea, from which the ova, *c*, had escaped.

d. The expanded orifice of the oviduct.

e. The left uterus, showing its thickened parietes, and the depressions in the vascular internal membrane, in which the ova were lodged. *e'* the right uterus.

f, *f*. The convoluted oviducts, or Fallopian tubes.

g, *g*. The ovarian and uterine ligaments.

h, *h*. The kidneys.

h', *h'*. The supra-renal glands.

i. The urinary bladder, turned down.

k. The rectum.

l. The external oblique muscle.

m. The internal oblique muscle.

n, n. The recti abdominis.

o, o. The pyramidales.

p. The cloacal passage. (The letter is placed on the retractor muscle.)

q. The common outlet.

Fig. 3. The two smaller-sized ova of the *Ornithorhynchus paradoxus*. (p. 560.)

Fig. 4. The two ova next in size, seen *in situ* in fig. 2.

Fig. 5. The larger ovum taken from the third specimen.

Fig. 6. The same ovum magnified three diameters, with the cortical membrane torn open, showing the vitelline membrane and its contents.

Fig. 7. A small portion of the vitelline membrane, *a*, more highly magnified, with part of the germinal membrane, or membrana granulosa, *b*, adhering to its inner surface.

Fig. 8. A portion of the ovary of an impregnated Ornithorhynchus, magnified.

a. The capsule of the ovary.

b. The laminated cellular substance, or stroma of the ovary.

c, c. The theca of a discharged ovisac, thickened, and encroaching upon the cavity from which the ovum had been expelled, forming a corpus luteum.

d. A small ovisac filled with its coagulated granular substance.

e. Part of a larger ovisac, with the granular substance removed, but the external granular stratum remaining.

f. Ovisacs artificially emptied, showing the state of the theca before the discharge of the ovum.

Fig. 9. The ovary of a Kangaroo six months after parturition, showing the cavity of the ovisac obliterated by its thickened parietes, nat. size.

Fig. 10. The ovary of a Rabbit two days after the coitus, showing its racemose structure, and the papillæ of the Graafian follicles.

Fig. 11. The ovary of a Rabbit three days after the coitus, laid open. It is thus described by De Graaf: "Testiculus in quo tertio à coitu die folliculorum crassities et cavitates, in quibus Ova delituerunt, apparent."—De Mulierum Organis, p. 412. tab. xxv., from which fig. 10. and 11. are taken.

XXVII. *Observations on the Motions of Shingle Beaches.* *By* Henry R. Palmer, *Esq. F.R.S. Civil Engineer.*

Received March 12,—Read April 10, 1834.

The extraordinary prevalence of tempestuous weather during the last autumn having occasioned numerous disasters on our coast, the public attention was directed in an unusual degree to the imperfections of many of the harbours, and more particularly to those which are encumbered with accumulations of shingle. The access to harbours thus circumstanced is generally uncertain, and in tempestuous weather is frequently dangerous, or even impossible.

The action of the sea, which gives motion to the shingles and produces the evils complained of, has long been a subject of speculation; but I have not found that it has been systematically investigated. Indeed, the contrariety of opinions advanced upon the subject, sufficiently indicates an entire absence of that satisfactory mode of inquiry which is essential to the foundation of a safe and practical deduction.

Very little has been written upon the subject; and such facts as have been mentioned have only been referred to incidentally, or with a view to geological science. My present object is exclusively practical in its nature, and my observations have been limited to such facts as would assist in establishing certain and fixed rules for controuling the motions of the beach, so far as to enable us to preserve a clear channel through it in all seasons, and in every variety of weather; and to accumulate and preserve the shingles, where it is needful to do so.

The subject at first sight appears greatly complicated; and were it necessary to discuss minutely all the modifications arising from the variety of forms and local circumstances, it would perhaps be too much so for general description. I have, however, limited my investigation to those simple and unvarying laws to which nature always adheres; and therefore the following observations must be considered as restricted only to certain general principles, subject to a variety of modifications.

The principles which I propose to illustrate will (under similar circumstances) at all times exhibit the same phenomena, but for the sake of perspicuity I shall now only refer to the coasts of Kent and Sussex.

Section 1.

That the pebbles which compose the shingle beaches on these coasts are kept in continual motion by the action of the sea, and that their ultimate progress is in an easterly direction, are facts long known and commonly observed. The following observations are chiefly directed to the particular manner in which the motions are produced.

From a general view of the effects that I have noticed, it appears that the actions of the sea upon the loose pebbles are of three kinds: the first heaps up, or accumulates the pebbles against the shore; the second disturbs, or breaks down the accumulations previously made; and the third removes, or carries forward the pebbles in a horizontal direction.

For convenience I propose to distinguish these by the following terms, viz. the first, the accumulative action; the second, the destructive action; the third, the progressive action.

All the consequences resulting from these various actions are exclusively referrible to two causes. The one is to the current, or the motion of the general body of the water in the ebbing and flowing of the tides; the other to the waves, or that undulating motion given to the water by the action of the winds upon it; and it is of considerable importance to the present inquiry that the effects resulting from each specific cause be separately considered.

The motion of the shingles along the shore is commonly attributed to the currents, the action of the waves being considered only as a disturbing force. That such a notion is erroneous will, I apprehend, presently appear; although I have to regret that I have not had the opportunity of obtaining such satisfactory information relating to the velocities of the currents in the channel as would have enabled me to include every form of argument upon the subject. The absence of such information has also prevented me from deciding satisfactorily as to the sources from whence the whole body of shingle is derived, which, although not necessary for the practical purposes I have in view, would have given more interest to the subject, and would have rendered the elucidation more complete. I must, therefore, for the present, be content to pursue the motions of the beach after it is found lying along or near the shore; observing only that the materials of which it is composed are those of the various strata in the vicinity of the coasts, together with the ordinary sea sand, and such small particles as may have been brought to the shore by the floods of the various rivers.

That the current is not the force which moves the pebbles along the coast, will appear from the following reasons:

1st. If it were so, the direction of the motion of the pebbles would be determined by that of the currents; but while the direction of the currents will vary with the changes of the tides, we find that the direction of the pebbles may remain unaltered; and also that the motion of the pebbles is continued where no current exists.

2nd. Although the velocities of the currents may not have been ascertained with precision, yet it is known that the velocities generally along this coast, which can possibly act on the shingles, are not sufficient to give motion to pebbles of every dimension, which are in fact carried forward.

3rd. The motion of a current will not produce that order in which the pebbles are found to lie, which order (as will be hereafter shown) may easily be distinguished as the effect of the motion of the waves only.

The direction of the waves is determined principally by the wind, the prevailing direction of which on the coasts referred to is from the westward. Every breaker is seen to drive before it the loose materials which it meets; these are thrown up the inclined plane on which they rest, and in a direction corresponding generally with that of the breaker. In all cases we observe that the finer particles descend the whole distance with the returning breaker, unless accidentally deposited in some interstices; but we perceive that the larger pebbles return only a part of the distance; and upon further inspection we find that the distance to which each pebble returns bears some relation to its dimensions. This process is an indication of the accumulative action.

But under some circumstances, depending on the wind, it is found that pebbles of every dimension return with the breakers that forced them up the plane, and that these are accompanied also by others, which had been previously deposited, but which are in such cases disturbed by the waves; and by a continued repetition of the breakers acting in this manner, the whole of the shingle previously accumulated is immersed below the surface of the water. This process is an indication of the destructive action.

The particulars of the accumulative action, *combined with that of progression*, are explained as follows. (See Plate XXVI. fig. 1.)

Let A B C D be an inclined plane, representing that on which the loose pebbles move. Suppose the wind to blow in such a direction as to cause a wave to strike a pebble at A, in the direction of A *a*, and to the distance (*a*) up the plane, that point being the extent to which the force can reach. Now here the wave breaks partly into spray, and is dispersed in all directions; is partly absorbed, and descends in a shallow form, which rapidly diminishes in its depth, so that the pebble is soon left exposed, and therefore does not return the whole distance with the water, but is left at rest at (a'), being at a higher level than that from whence its motion commenced.

With the rise of the tide the striking force is also elevated; and by the repetition of the operation described through the different heights in succession, the further motion of the pebble will be represented by a' b' b' b', &c., the distance in each step of its descent being something less than in that of its ascent, until it has reached the summit (*f*) determined by the height of the tide. Now if we suppose a pebble of less dimensions than the former to be struck from the same point, we shall find it raised as before; but because its surface is greater in proportion to its weight, and because from its less bulk it remains longer immersed in the declining wave, it will descend further, and follow the line (*a g*, &c.), and will not be left at rest till it has reached (*o*).

If, then, we suppose a pebble whose dimensions are less than either of the former, it will be evident that the point at which that will arrive on the highest level will be more distant still; hence it follows that the distance travelled horizontally by the pebbles during a tide will be in some proportion to their bulk, the specific gravities being the same.

(The pebbles do not in reality move in straight lines, but in a succession of curves; the straight lines are assumed here, and in other parts of this paper, to simplify the description.)

I trust it is only necessary to remark, that if the wind continue to blow in the same direction during the ebbing of the tide as through the flowing of it, the direction in which the waves will strike the shore will be nearly the same, and the progress of the pebbles will be urged by a similar action, and therefore their direction will also be the same.

In this action we observe a constant tendency to heap up and accumulate the shingles; and it is an interesting fact, that when the action has continued equally through a tide, the pebbles are left *in regular order, according to their dimensions,* the largest being uppermost, and the smallest at the bottom of the plane. I do not mean to state that all the largest are at the top, or that all the smallest are at the bottom, for it is evident that some of every size will be found at every level; but that if an equal measure (say half a peck) be taken from the different levels, the average of each specimen will exhibit in regular order the various dimensions.

The order in which the pebbles are thus found is, then, that by which the effect of the waves is distinguished from that of a current, the effect of the latter consisting only in its influence on the direction of the impinging and recoiling motions of the waves, by which the motion of the beach may in a small degree be accelerated or retarded.

Section 2.

In the illustration of that action of the sea which breaks down and removes an accumulation, I propose referring to my observations in the order in which they were made. My attention was first directed to this part of the subject in the neighbourhood of Sandgate in October last.

The accumulative action had been continued for a considerable time. The numerous groins erected near Folkestone to impede the progress of the beach, for the protection of the cliffs, had collected a bank of pebbles, which in some parts was five feet in height. The wind had so much abated as to be scarcely perceptible, but the sea had a motion denominated a *ground swell.*

The waves approached the shore nearly at right angles with it; but although in rapid succession, their forces were very moderate. These circumstances continued through five tides, by which time nearly the whole of the loose shingle had disappeared, including all that had been collected by the groins at Folkestone. The water being particularly clear, I was enabled to perceive distinctly the action upon the pebbles, and their motion downwards. I observed, that although every wave became broken and dispersed as usual, yet they followed in such rapid succession, that each wave rode over its predecessor while on its return, and thus produced a continual downward current, which carried with it the pebbles that were disturbed. That the pebbles were not removed far from the line of low water, would appear from the fact, that on the subsiding of the swell, it being succeeded by a light breeze of wind from the westward, the accumulation immediately commenced, and was restored to its former quantity by the action of four tides. I have subsequently had some favourable opportunities for making other observations on the effects produced by different rates of succession of the waves, and particularly at Dover, during the late gales, where the same actions were noticed. There I watched for an opportunity of witnessing that rate of succession which exhibited the destructive and accumulative actions in their smallest degrees; and I observed, that when ten breakers arrived in one minute, the destructive action was but just evinced; and that when only eight breakers arrived in the same period, the pebbles began to accumulate; which facts harmonized with my observations made at Sandgate and Folkestone, viz. *that the difference between the two actions was determined by the rapidity in succession of the waves upon the shore.*

In the description of the accumulative action, I have assumed the forces to be directed obliquely with the line of coast, and have therefore necessarily included the progressive motion; but it remains to be explained in what manner the shingles are carried forward while the destructive action is going on.

It is known that the action and reaction of the waves give to the whole body of the water, within a certain distance from the shore, an undulating motion. The direction of this motion, when approaching the shore, will, to a certain degree, correspond with that of the waves upon the surface, and the direction of the recoil will also be affected in like manner; therefore the pebbles that have been carried down by the destructive action are moved forward through an angular course *beneath the water*, until, by the excess of the impinging forces over those of the recoil, they are again raised by the action of the water, and deposited where the destructive action has ceased, or where, from local circumstances, it cannot occur. The circumstances which are most unfavourable to the destructive action are those which least admit of the constant downward under-current,—an inlet, or narrow arm of the sea, for example. If we suppose a wave rolling through the mouth of an inlet, carrying with it a charge of shingles, it does not break as upon an inclined plane, but is dispersed in the general body of the water, which is comparatively quiescent; and there being no returning force, the shingle becomes deposited, and a bank is formed: and although the destructive process would act upon that bank if it could attain a certain height, yet the attainment of that height is prevented by the waves passing over it, and carrying with them, in succession, the shingles with which they are charged.

Section 3.

In Plate XXVII. is represented a section of the beach formed along the outside of Folkestone Harbour. This section was taken with great accuracy, after the ground swell before referred to had removed most of the loose pebbles from it; so that the section may be considered as representing the plane upon which the progressive motion of the pebbles is carried on. Its slope is in the proportion of 1 to 9, nearly, and (with the exception of that part near the summit where there remained a bank of pebbles beyond the reach of the previous tides,) the surface of the plane corresponds very nearly with a straight line, which, considering that it is a natural formation, is a fact worthy of notice.

I think this plane may be considered as representing the average dimensions and inclinations of the surfaces over which the beach travels along this coast, and I have therefore generally assumed such an one for the present purposes. Upon such an inclination, the loose pebbles are in contact with each other; and although their depth upon the plane is constantly varying, yet, for the sake of conveying a general idea, we may assume the average to be about six inches, extending between high- and low-water marks. When, however, the plane is less inclined, the same quantity of beach is spread over a larger surface, and its depth is diminished; and the pebbles are in some places so far separated as to exhibit the appearance of a diminished quantity. In Plate XXVI. fig. 2. this is illustrated geometrically.

Let A B represent a plane on which all the pebbles are in contact, C B a plane considerably more inclined. If, from the centre of each pebble on the plane A B, a horizontal line be drawn to the plane C B, the position of the pebbles on the latter will be respectively at the various points of intersection.

Section 4.

There are numerous points on the coast at which the line of beach is apparently intercepted and its continuity destroyed, and the rock washed bare. Having sufficient evidence that the motion of the beach was continuous, I thought it important to ascertain in what manner the pebbles escaped past those places, and was happy in finding, upon investigation, that a valuable deduction could be made.

In the description of the accumulative action, it was remarked that the waves having struck the pebbles upwards, became dispersed, and were incapable of returning them to the level from which they were forced. But I now observed that the surface of the rock, being very irregular, constituted numerous channels; so that the waves, instead of returning in a dispersed and weakened form, moved back in columns, which were of sufficient power to return every pebble that had been thrown up; and as these channels offered no impediment to the angular progressive motion of the pebbles, it was more rapid than on the ordinary plane surface. Here, then, was pointed out by nature a principle on which the shingles might be hastened forward, and their accumulation about any particular place prevented; and by simply reversing that principle, a method of accumulating or retaining the shingles, where they are wanted, is also suggested, viz. by the reduction of the descending force of the breakers.

The effect of confining the retiring breakers to a column is also exemplified in another manner, when the waves are driven directly upon the beach by a moderate wind, or such as would produce the accumulative action. A succession of waves, acting over the same lines of the beach, soon forms a slight depression, which continues increasing until it becomes a definite channel. The whole line of beach being thus acted upon, it assumes the form of a series of banks parallel with each other. The waves do not then recoil in a dispersed form, but, having broken, are again collected and returned through the channels, and remove all loose matter from them. While in this state, the beach has no progressive motion, but continues (to use a military term) "marking time," until, from the change of wind, an oblique direction is given to the motion of the waves.

Section 5.

The progressive motion of the beach may be easily traced along the coast as far as the bay called Sandwich Flats. See Plate XXVIII. The general character of the motion during its progress is that which is most favourable, under every circumstance, to the chances of becoming securely deposited. Every part of the coast is attempted by every variety of motion in its turn, until a place of final security is discovered.

The locality of Romney Marsh appears to have afforded the sought-for shelter, and now exhibits an extraordinary example of the accumulation, which, having been combined with sand, silt, and vegetable soil derived from other sources, has long been considered an acquisition to our surface of considerable value.

Although this tract has continued increasing to the present day, yet a great quantity of the beach travels past it, and we do not find any other accumulation of much extent between that and Sandwich Flats, beyond which there is no further trace of the shingle which we have so far followed, the pebbles to the northward of these flats being evidently those derived from the cliffs near about them.

On the approach of the shingle to the Sandwich Flats, it becomes gradually dispersed, owing to the increasing inclination of the plane, until it seems to disappear. A considerable extent of these flats has attained a height very little inferior to that of the high-water mark of spring tides; and it is so nearly horizontal, that the water does not partake of that undulating motion upon it which has before been adverted to.

On the Sandwich Flats there is a continual deposit of soil and silt, brought there from the interior of the country by the river Stour, and which, after its exposure to salt water, is particularly suitable for permanently uniting all the coarser or larger fragments with which it may become intermixed. So much of the materials which have composed the beach as may be conveyed to the higher parts of these flats are not likely to be again disturbed, because many days may intervene before another tide may reach them; and they thus become united to the surface on which they rest, and gradually contribute to its height.

The greatest motion of the pebbles being where they are exposed to the action of the greatest number of waves, we must look to the lower levels of these flats to trace the further course of the greater portion of the shingle. But even the slope of the surface of the lower levels is so very gradual, that the undulating motion of the water is proportionally diminished; *the action of the water then becomes greatest in the direction of the land.* While, then, we bear in mind the nature of the soil over which it acts, we find an almost insurmountable impediment to the further progress of the shingle, and are enabled to account for the rapid extension of the Sandwich Flats towards the sea, which, in fact, is only the continuation of that process which has been for ages in operation, and which has formed a large portion of those extensive marshes between the Isle of Thanet and the main land of Kent.

Section 6.

Having described those chief principles which regulate the motion of the shingles on this coast, and having traced their progress to a final destiny, I shall now proceed with some further general remarks referring to the application of the foregoing observations.

So much effect has been attributed to the motion of the tidal currents, that vast sums have been expended in attempts to divert the motion of the shingles to a distance from the general line of the shore, from whence, by the increased depth and velocity of the current, it has been expected they would be carried past a particular spot, through which a permanently open channel has been required. Such attempts have been made at various periods during upwards of two centuries at Dover, and more recently at Folkstone in the same neighbourhood. It is hardly necessary to observe, that such attempts have not been successful, and from the principles which I have laid down, their failure may be easily accounted for.

If a wall or pier be extended from the shore into the sea, it is evident that such erection will in the first instance impede and prevent the progressive motion. It is also evident, that the progressive is not necessarily combined with the accumulative action, but, on the contrary, where the former is impeded the latter is assisted. The accumulative action, therefore, continues until the angle formed by the pier and the line of the shore is occupied, and the pier being no longer an impediment to the progressive motion, that motion is again restored, and the general mass proceeds as if no impediment had existed.

The most perspicuous evidence of these results is exemplified at the harbour of Folkestone. (See Plate XXVII.)

Previously to the commencement of this exclusively artificial work, the beach travelled along the line of cliff in the ordinary way.

By extending the walls a sufficient distance into the sea, it was expected that a commodious harbour would be formed, and the shingles diverted so far into deep water, that they could not again appear above the surface until they were removed beyond the harbour's mouth.

The accumulation, however, immediately commenced, and continued as the work advanced until it became apparent that no other effect was produced upon it than a comparatively slight change of direction. The entrance of the harbour being much encumbered with shingle, an additional pier or jetty was erected, and extended about two hundred feet further into the sea without having approached the effect intended. It is true that some advantage was derived from the extended pier, by increasing the distance between the most violent action of the breakers and the still water of the harbour. The shingles, therefore, pass the mouth in a more dispersed form than they originally did, and hence they do not so readily form a barrier, neither does its perpendicular height become so great.

Much valuable information on this part of the subject is recorded in LYON's History of Dover, which, as it may at any time be consulted, is not repeated here. I shall only remark, that from the succession of experiments made at that place, the general result has been in a considerable acquisition of new land, which, although valuable in itself, is not the object intended to be obtained.

If, then, it be admitted that projecting piers will not prevent the encumbrance about the mouth of a harbour, situated as those referred to in the tract of the restless beach, it remains to be seen how far such works may be otherwise injurious.

While the accumulative action is going on, every abrupt projection from the coast is an impediment to the progressive motion of the beach until its angle is filled up. Such abrupt projections offer no protection against the destructive action; when, therefore, by the increase of wind, the action of the sea becomes violent, an accumulation previously caused by a projecting pier is rapidly removed, and again is rapidly deposited where it is not resisted. And there is perhaps no combination of circumstances less capable of resisting, or more favourable to the deposition of, the shingle, than is found in artificial harbours, shielded by an *abrupt* weather pier in a line of beach.

With a long continuance of violent winds from the same quarter, every accumulation of loose shingle is broken down, and is hurried forward, while it unremittingly appears to seek protection. During the recent gales every inlet within the tract of the beach was seriously encumbered with it; commenced with the heap accumulated by the very pier that was intended to prevent such an effect (where such existed), and increased by the successive arrivals of those more remote, together with that quantity commonly passing along the sloping plane, but now brought down by the destructive action and forced along with accelerated motion.

The ordinary state of the beach at Folkestone harbour is represented in Plate XXVII.

the additional extent of the beach on the east side occasioned by the heavy gales is represented by the dotted lines.

The plan of Dover harbour, in the same plate, represents the state of the beach in June 1833, after the wind had blown rather strong for a few days. (This is drawn from a survey made by Mr. J. S. Tucker, of the Hydrographer's Office in the Admiralty.) The dotted lines to the eastward of the piers represent the general outline of the addition to the former by the recent gales, which having formed a barrier across the harbour's mouth, extended about seven hundred feet beyond it.

Many very interesting facts might be mentioned concerning the effects produced by the continued gales at various places on the coast, but I find that the description of them in sufficient detail to make them useful would extend this paper much beyond the limits assigned: I, however, trust that a reference to two of the most remarkable cases will be found sufficient to illustrate the principles attempted to be explained.

Section 7.

The only natural power by which the channels through the beach are retained, is the returning force of the water, which on this coast is generally scanty. And it is obvious, that however judiciously that force may be employed, it is but *remedial in principle*, and necessarily implies a previous evil. So long, therefore, as the cause continues to act, the remedy is prevented, and the harbour becomes inaccessible when protection is most required.

If on inspection of the great bank recently thrown up at Dover (as represented upon the plan), we imagine it to be dispersed over several miles of the sloping plane, and assume the whole to be in continued and equable motion, it will immediately be inferred, that the quantity that would be passing a given spot at one time would be comparatively insignificant; and hence, since we have no reason to suppose that there will be a limit to the quantity, and since it has been shown that its motion cannot be prevented, it follows that the great objects in view must be attained, first, by securing permanently such accumulations as are necessary for the protection of land from the action of the sea, or useful by their addition to its surface; and secondly, by facilitating and inciting the progressive motion of that superfluous quantity from whence the evils complained of are derived: and therefore the uninterrupted and permanent welfare of the numerous harbours which communicate with the sea, through the extensive tract of the shingle beach, is dependent more on a *system of management along the coast*, than upon particular devices adapted exclusively to each separate case.

18, *Fludyer Street, Whitehall.*

XXVIII. *Analysis of the Moira Brine Spring near Ashby-de-la-Zouche, Leicestershire; with Researches on the Extraction of Bromine.* By Andrew Ure, *M.D. F.R.S.*

Received June 5,—Read June 19, 1834.

THE Moira coal mines are intersected by so many faults and slips, that they afford a very limited supply of water. The chief portion of the fresh water is drawn from within three hundred feet of the surface, by a pump barrel of nine inches diameter and six feet stroke, working four or five hours a day. It is raised at the rate of about seven strokes per minute, and amounts in this time to a volume of ninety-one gallons. There is a cistern at the bottom of the basset shaft connected with a reservoir cut in coal, which holds four or five days' drainage of water. The engine is employed in raising the fresh water not more than nine hours in a week.

The shafts for working the coal vary in depth from seven to eleven hundred feet. One shaft is 252 yards (or 756 feet) deep, and contains four lifts of pumps. The uppermost of these pumps delivers the water, which is altogether saline, into a cistern at the bottom of the basset shaft, whence it is raised from time to time. The average quantity of salt water alone employs the engine about eighty minutes daily, or ten hours in the week. The topmost lift of all the pumps delivers the water into a cistern about ten yards down the shaft: if the water comes from the fresh reservoir, it is allowed to run over at the top of the cistern down the drain into the brook; if from the salt reservoir, it is forced into the bath cistern by a forcing pump. The salt water is pumped up at the rate of five strokes and a half per minute, constituting seventy-one gallons per minute, whilst working, or about ninety hogsheads in a day.

In working the main coal, a little salt water oozes out; but this transudation, or bleeding as it is called, ceases after a time. In some few places small dribblings continue to issue, which collectively throughout the whole range of the Moira mines do not, in the course of twenty-four hours, exceed fifty hogsheads, and are conducted to the common reservoir.

The transudation of salt water generally appears in any adit in the coal as soon as driving commences. It slowly bleeds, but never spirts or springs forth as if from pressure; but its oozing is invariably accompanied with a faint hissing noise, as if air were escaping at the same time. The liquid proceeds chiefly from small crevices (pin-cracks), and seems associated with inflammable air, which separates as it trickles down the face of the coal. The gas is occasionally abundant enough to admit of being fired. In driving an adit in the solid coal to any distance, not so much as a dram of water is found at any one point, and very little oozes from the roof or floor of the opening. When a lump of coal is detached, however, water soon afterwards begins to exude in drops from the crevices of the seam.

Immediately over the coal measures from which the saline water issues, there is a stratum of remarkably fine fire-clay, a shale free from iron and lime, called by the miners tow, about eighteen inches thick. This slate-clay is impermeable to water. Immediately under the coal lies a stratum of soft clay eight inches thick, which rests on a layer of compact slate-clay, several feet thick, also impermeable to water.

The bed of coal, although it contain pin-cracks which seldom extend many inches, has also the partings called slines, and those called cleavings in the direction of the bed; yet the coal is so little penetrable by water laterally, that it can confine by a wall a few yards thick the water of old workings.

When a fault has been perforated, water is seldom or never observed, so long as the confusion of strata occasioned by the break continues to exist. But from the parallel strata, the coal yields this saline water at almost every pore. The fault might have been a rent of an immense depth, but the line of slip is filled up and glazed, so to speak, by the incumbent pressure: hence in the greater number of instances where salt water is formed and continues to flow, the source of this fluid cannot be traced to the faults; for although near some of these the water may be abundant, yet generally the borders of the faults and the faults themselves are quite destitute of water, acting as barriers to it in every direction.

In consequence of the uniform distribution of saline matter through this coal, the potters are unable to employ it in their kilns, for it gives their earthenware the well known glaze due to the action of the vapour of chloride of sodium.

Saline water is found in one or more of the sandy rocks of the strata above the coal, but in very small quantity, and much less strongly impregnated than that which issues from the coal.

The brine-spring water is used for baths both at Moira and Ashby-de-la-Zouche as a medicinal application. These are celebrated for their sanatory powers in rheumatic, paralytic, and scorbutic diseases. Its internal administration, in small doses frequently repeated, is said to accelerate the discussion of scrofulous swellings, and of broncho-cele; results which have been latterly ascribed to the combined agency of the bromides of sodium and magnesium with chloride of calcium which are found in the water.

A considerable quantity of this water was sent to me for analysis in bottles well corked and sealed*. Its taste is simply but strongly saline. It has no smell. It is pellucid and colourless. Its specific gravity at 60° Fahr. is 1·04647. A glass balloon being filled with it, the orifice was shut with a tight cork, from which a narrow bent glass tube proceeded, so as to dip under the mercury of a pneumatic trough. The balloon and tube were entirely filled with the water, to the exclusion of air. On the application of heat, gradually increased till the water began to boil, the air contained

* By Edward Mammatt, Esq., superintendent of the mines and baths.

in it was disengaged, and received in a graduated tube over mercury. On examination it proved to be common air, with a slight excess of azote, equivalent altogether to only four and a half cubic inches in the gallon of water, or about one sixtieth part of the volume.

This quantity is not two thirds of the amount found in river water, nor more than half that in the waters of ordinary springs. The deficiency may be ascribed to the agency of saline matter in expelling air from water, in the process of solution, a fact particularly exposed in my paper on Nitric Acid published in the Journal of Science for January 1819.

One thousand grains of the water evaporated to dryness on a steam bath, afforded a group of saline crystals, which, after gentle ignition in a covered platinum capsule, weighed sixty-two and a half grains. During the ignition of the mother-water salts, a faint odour, resembling that of muriatic acid mixed with the hydrobromic, is perceptible.

As this water has its transparency hardly disturbed by nitrate of barytes, it obviously contains no appreciable quantity of sulphuric salts. In its concentrated state it does not affect solution of muriate of platinum, and therefore seems to be free from salts of potash.

Oxalate of ammonia indicates the presence of lime in notable quantity; and phosphate of soda applied to the liquid after the separation of the lime, detects magnesia by the peculiar aspect of the ammonia-magnesian phosphate. Tincture of galls shows the presence of a trace of iron; but ammonia added to the water occasions no appreciable precipitate of either that metallic oxide or alumina. From the slowness with which the iron is indicated by the galls, and the non-action of ferrocyanate of potash on the water even faintly acidulated, the iron is obviously in the state of protoxide. The quantity of chlorine present in a given weight of the water was determined by solution of nitrate of silver; and this amount was found to coincide with the weights of ignited chloride of sodium obtained by evaporation of the lime-free water, and of chlorides of calcium and magnesium inferred from the lime and magnesia got in the analysis. From the proportion of chlorine estimated by nitrate of silver, a small deduction must, however, be made on account of the quantity of bromine present, as determined by subsequent researches.

The following is the general result of the analysis of one gallon:

	Grains.
Bromides of sodium and magnesium	8·
Chloride of calcium	851·2
Chloride of magnesium	16·0
Chloride of sodium	3700·5
Protoxide of iron	a trace
Solid saline contents in one gallon	4575·7

The above eight grains of bromides are equivalent to six grains of bromine.

Since the bromine is probably the most important ingredient of the Moira saline water, and since it is the one of which the quantity is most difficult to determine, I now proceed to offer some remarks on its elimination.

As bromine is always associated with chlorine in the waters from which it has been hitherto extracted, the first object of the analyst is to remove the chlorides as far as possible by crystallization. With this view the salts of lime and magnesia, which are usually present, ought to be decomposed at the outset by a due addition of carbonate of soda, so that the mother water obtained by evaporation may contain no deliquescent chlorides, but consist eventually of the chloride and bromide of sodium. By this precaution, also, none of the hydrobromic acid will be dissipated in the first process, as happens when the bromides of calcium and magnesium are present.

It is stated by Macquer that chloride of sodium is insoluble in alcohol of specific gravity ·840. I find, on the contrary, that a less aqueous alcohol, that of specific gravity 0·830, will dissolve at ordinary temperatures one twentieth of its weight of pure chloride of sodium; and that the same alcohol will dissolve fully five times as much bromide of sodium. On this difference of solubility in alcohol I sought to establish a simple method of separating the chlorides and bromides of sodium in the mother liquor of saline springs. On triturating with alcohol of 0·830 the saline mass obtained by evaporating the said mother liquor to dryness, a solution was obtained of specific gravity 0·985, which contained nearly one fifth its weight of saline matter, consisting chiefly of chloride of sodium. Thus it appears that a small proportion of bromide of sodium present in the alcohol, enables it to dissolve a large proportion of chloride. The separation of these two salts by alcohol is therefore impracticable.

The process which I eventually adopted for analysis, was to transmit through the mother liquor of the soda salts a current of chlorine gas till it communicated the maximum golden yellow tint, and then to pour in sulphuric ether, and agitate. The well known reddish yellow stratum of ether, combined with bromine and chlorine, soon rises to the surface of the saline solution. If the mother liquor has been submitted in its most concentrated state to the action of chlorine gas, the quantity of ether should be small in proportion to the bulk of the liquor; for if too much be added, it will hydrogenate the bromine, and cause the whole mass to become immediately colourless. If, on the other hand, the mother liquor be too dilute, it will absorb a quantity of chlorine proportional to its volume, whereby much ether will be decomposed. Distilled water made yellow by chlorine gas affords, on agitation with ether, a yellow supernatant stratum, not dissimilar to that produced by a minute portion of bromine treated in a similar way. It is therefore obvious that the reddish yellow ethereous stratum obtained from the mother liquors of bromic waters, is always a combination of chlorine and bromine with ether, in proportions more or less uncertain.

Desfosses or Berzelius * prescribes barytes as an agent for separating chlorine

* Berzelius, Traité de Chimie, i. 294.

from bromine, from the supposed property of chloride of barium being insoluble, and bromide of barium being soluble, in concentrated alcohol. I have digested in 60 times its weight of alcohol, sp. gr. 0·830, a mixture of chloride and bromide of barium, made by saturating the ethereous stratum with pure water of barytes, evaporating and igniting, and yet I found afterwards that much of the bromide had remained undissolved, evolving its ruddy vapour when acted on by manganese and sulphuric acid. Hence I cannot recommend this process to the analytical chemist.

Chemical research has two objects; the discovery of truth, and the improvement of the useful arts. The first object may, in the present instance, be attained with great precision by the followed method.

Having impregnated the mother liquor with chlorine gas, and agitated the dehydrogenated fluid with ether, separate the ruddy ethereous stratum by a funnel. More chlorine gas may now be transmitted through the liquor, and more ether may then be agitated with it, when usually a second layer of chloro-bromic ethereous fluid will be obtained, to be added to the former. Saturate exactly the ethereous liquid with a weak solution of pure soda, which may be done with extreme delicacy, in consequence of the loss of colour which instantaneously occurs whenever the neutral point is reached. In a few seconds a faint yellow tinge may reappear in the ether, which must be removed by a drop or two of the soda. The colour of bromine is as sensible a test of alkali as litmus. Separate the ether by a funnel, evaporate the subjacent saturated solution to dryness, and ignite carefully in a covered platina capsule. Weigh the mixed chlorides and bromides of sodium, and decompose ten grains, or any definite weight, of the mixture by solution of nitrate of silver. From the weight of the silver precipitate the relative proportions of the chloride and bromide present may be determined on the following principles:

10 grains of chloride of sodium are equivalent to 24·46 of chloride of silver.

10 grains of bromide of sodium are equivalent to 18·39 of bromide of silver.

Hence, if the silver precipitate from 10 grains be altogether a chloride, it will weigh 24·46 grains; and if altogether a bromide, it will weigh only 18·39 grains. The annexed Table will show that the differences of weight are sufficiently great for every analytical purpose.

Chloride of Sodium. Grains.	Bromide of Sodium. Grains.	Weight of Silver Precipitate.
10	+ 0	24·46
9	+ 1	23·85
8	+ 2	23·24
7	+ 3	22·64
6	+ 4	22·03
5	+ 5	21·42
4	+ 6	20·82
3	+ 7	20·21
2	+ 8	19·60
1	+ 9	19·00
0	+10	18·39

By a skilful application of analytical resources, differences ten times smaller than any represented by these numbers may be certainly appreciated in practice; that is, the result may be found within one part in a hundred.

If 10 grains of the mixed salts be used for analysis by nitrate of silver, the following arithmetical rule will be found sufficiently accurate. From the number 24·46 deduct the weight of the silver precipitate, (perfectly dry of course,) and divide the remainder by 0·6; the quotient will denote the proportion of bromide of sodium present in the 10 grains of the mixed salts. Thus, supposing that 22·9 grains of silver precipitate have been obtained from 10 grains of a mixed chloride and bromide of sodium, the difference between 24·46 and 22·90 is 2·46, which, divided by 0·6, gives for a quotient the number 4·1, indicating four grains and one tenth of a grain of bromide of sodium. According to Berzelius, bromide of sodium has for its prime equivalent the number 101·7, hydrogen being unity, and consists of 78·39 bromine + 23·31 sodium.

I have been in the practice of solving many problems in analytical chemistry by the application of an arithmetical process analogous to the one above stated.

The chloride and bromide of silver are both soluble in water of ammonia, and cannot therefore be separated by this agent.

The best experimental mode of effecting the complete separation of bromine and chlorine in analysis may possibly be by converting the mixture of those two elements into perchloride and perbromide of mercury, and decomposing the perchloride by sulphuric acid. The perbromide is said to resist this powerful agent; with what truth I have not ascertained. Red oxide of mercury dissolves when agitated along with water in the ethereous solution of bromine and chlorine, and affords a colourless solution.

XXIX. *An Account of some Experiments to measure the Velocity of Electricity and the Duration of Electric Light. By* Charles Wheatstone, *Esq. Professor of Experimental Philosophy in King's College, London. Communicated by* Michael Faraday, *Esq. F.R.S. &c.*

Received and Read June 19, 1834.

§ 1.

THE path of a luminous or an illuminated point in rapid motion, it is well known, appears as a continuous line, in consequence of the after duration of the visual impression. There is nothing, however, in the appearance of such a line by which the eye can determine either the direction or the velocity of the motion which generates it. It occurred to me some years since, that if the motion which described the line in these cases were to be compounded with another motion, the direction and velocity of which were known, it would be easy, from an inspection of the resultant straight or curved line, to determine the velocity and direction of the former. Following up this idea, I made a series of experiments relating to the oscillatory motions of sonorous bodies, too numerous, and not sufficiently connected with the subject of the present communication, to be detailed in this place. The satisfactory results thus obtained made me desirous to ascertain whether, by similar means, some information might not be gained respecting the direction and velocity of the electric spark: the method by which I then proposed to effect this purpose was first announced in a lecture delivered by Dr. Faraday at the Royal Institution in June, 1830. My attention was again drawn to the subject at the commencement of last year, and I attempted to realize the idea in the following manner.

Fig. 1 represents the apparatus employed, which was screwed at *a* to the spindle of a whirling machine, so that a rapid rotatory motion might be given to it. The upper and lower parts, which were all of brass except the wooden disc *b c*, were insulated from each other by a stout glass rod *d e*; a slip of tinfoil connected the ball *h* with *a*, and the upper ball *g* was capable of adjustment to various distances from the lower one *h*. When the ball *f* was placed within striking distance of the prime conductor of an electric machine, a spark passed between them, and also between the balls *g* and *h*, which could be separated to the distance of four inches, so as to exhibit a spark of that length. It is obvious, that if the angular motion of the balls were in any sensible proportion to the velocity of electricity, there would be a deviation between the upper and lower terminations of the line. The instrument revolving from left to right, if the motion of the spark be downwards, the deflection of the line should be as in fig. 2; and if its motion be upwards, it should be deflected as in fig. 3.

When the apparatus was made to revolve rapidly, the sparks passed in the same manner as when it was at rest, and no deviation of the extremities of either of the two sparks from the same vertical line was observed. The apparatus revolved fifty times in a second, and as a difference of the twentieth part of the circumference described by the balls could have easily been observed had it existed, we may safely conclude that the spark passed jointly through the air and the metallic conductor in less time than the thousandth part of a second.

§ 2.

Having failed to observe any deflection of the spark by the means just mentioned, I found it necessary, if I would continue the inquiry, to contrive some more effectual means of prosecuting it. It occurred to me that the motion of the reflected image of the electric spark in a plane mirror would answer all the purposes of the motion of the apparatus itself connected with the spark. Several advantages, it was evident, would result from this substitution; the apparent motion of the reflected image in a small moving mirror would be equal to an extensive motion of the object itself; the same mirror might be presented to any object to be examined, thus forming, with its moving machine, an independent and universally applicable instrument; and many experiments might be tried, which, without this expedient, would be difficult or impossible to perform, from the size or immobility of the apparatus.

The most convenient form of the revolving mirror is represented in fig. 4; it rotates on a vertical axis, and in its motion successively assumes every vertical plane. If a luminous point, the flame of a candle for instance, be placed at any distance before this revolving mirror, the successive places of its reflected image will describe a circle, the radius of which is equal to the perpendicular distance between the luminous point and the axis of rotation. The angular velocity of the image is twice that of the mirror; the entire circle is consequently described while the mirror makes a semi-revolution; and if the back of the mirror be also a reflecting surface, the image will describe two entire circles during one revolution of the mirror. If the motion exceed a certain rapidity, the successive images leave their impressions on the retina, and the eye, properly placed, takes in the view of a perfectly continuous line of light, being an arc of the circle described, which arc is larger in extent in proportion to the proximity of the eye to the mirror.

If now, while the mirror is in motion, the luminous point be moved in a direction parallel to the axis of rotation, the composition of the two motions of the image, the one depending on the motion of the object, the other on the motion of the mirror, will give rise to a diagonal resultant; and if the number of rotations made by the mirror in a given time are known, the direction and velocity of the moving point may be calculated.

By screwing the axis of the mirror to a machine with multiplying wheels, I was enabled to cause it to revolve fifty times in a second. The reflected image of a luminous point, therefore, passed over half a degree in the 72,000dth part of a second, the angular velocity of the image being, as before noticed, double that of the mirror. An arc of half a degree is easily estimated by the eye, and is equal to about an inch seen at the distance of ten feet. Supposing this to be the limit of distinct observation, though perhaps a much smaller arc might be distinguished even by the unassisted eye, we might expect, when a line of electric light is placed parallel to the axis of the revolving mirror, to ascertain two things: first, the duration of the light at each point where it appears; and secondly, the time which elapses between the appearance of the light in two successive points of its path; provided that the time, in either case, be not less than the 72,000dth part of a second. The first would be indicated by the horizontal elongation of the reflected image, and the second by the distance between two lines drawn from the images perpendicular to the horizontal plane. If the duration and velocity were both rendered sensible by the mirror, the reflected image would appear as a deflected band of light.

I successively presented to the mirror, sparks four inches in length drawn from the prime conductor of a powerful electrical machine; the explosions of a charged jar; a glass tube four feet in length, exhibiting a spiral of electric sparks passing between dots of tinfoil; an exhausted glass tube six feet in length, through which the spark passed, and produced an unbroken line of attenuated electric light; various pictures, such as birds, stars, &c., formed of electric sparks. But in all these cases, when the reflected images occurred within the field of view, they appeared perfectly unaltered, and precisely as they would have done had they been reflected from the mirror while at rest.

When sparks were made to follow each other quickly, several reflected images were simultaneously seen in different positions, owing to the images having been renewed before the visual impression caused by the first had disappeared. The exhausted tube being held near a prime conductor, when looked at directly, will sometimes appear to gleam with a continuous light; but examined in the mirror, this apparent continuity is seen to be owing to a rapid succession of transient flashes.

§ 3.

For some experiments another position of the revolving mirror is preferable to that just described. Fig. 5 represents the reflecting surface inclined to the axis of rotation, and nearly perpendicular to it. If a luminous point be placed anywhere in the prolongation of the axis, its images, successively reflected from different parts of the mirror, form together a circle, the whole circumference of which may be seen at once. In this form of the experiment the angular velocity of the image is equal to that of the mirror, and both move in the same direction; whereas in the former case the image moved with double the velocity of the mirror, and in the opposite direction. The visual magnitude of the circle described increases with the distance of the object and inclination of the mirror. The flame of a candle presented to it appears as a broad luminous ring; the image of the sun is converted into a magnificent fiery belt, &c.

A series of minute sparks made to pass between two points, or between a point and the prime conductor of a machine, presents to the eye, from the rapidity of their succession, the appearance of a permanent star of light. When this star is placed in the prolongation of the axis of the revolving mirror, the successive sparks of which it consists are reflected to the eye each from a different part of the surface, and they are exhibited arranged at regular distances in a circle. When the intermissions are rapid the appearance is extremely beautiful.

The brush of light which appears on a point when presented at some distance from the conductor, is also by this means shown to be an intermitting action, notwithstanding its permanent appearance; its reflected images present, however, this remarkable peculiarity, they are elongated in the direction of the motion, proving that a brush is not so transient as a spark, and that the emissions which constitute it last during an interval of time measurable by the motion of the mirror.

But this instrument is not confined to observe merely the intermittences of electric light; whenever a rapid succession of alterations occurs in an object which does not change its place, they may be separately examined by this means. Vibrating bodies afford many instances for investigation; one among these is perhaps worthy to be mentioned. A flame of hydrogen gas burning in the open air presents a continuous circle in the mirror; but while producing a sound within a glass tube, regular intermissions of intensity are observed, which present a chain-like appearance, and indicate alternate contractions and dilatations of the flame corresponding with the sonorous vibrations of the column of air.

§ 4.

Experiments have frequently been made with a view to determine the velocity of the transmission of electricity through conducting bodies. In all the recorded trials of this kind it was attempted to measure the interval of time supposed to occur between two discharges made at opposite extremities of the wire, which were brought near each other so that they might be seen at the same time. In one experiment, performed at Shooter's Hill in 1747 under the superintendence of Dr. Watson, the circuit was four miles in extent, two miles through wire, and two miles through the ground; but the discharges appeared, as in all similar experiments, to be perfectly simultaneous. Nor need we feel surprised at this result, when we know that the eye is unable to distinguish the succession of luminous objects which follow at the interval of the eighth or tenth of a second, from their simultaneous appearance; and that, therefore, with a circuit even of four miles extent, the velocity of a few miles per second would be the utmost observable by such means.

I determined, therefore, to repeat a similar experiment, substituting for the imperfect judgement of the eye a revolving mirror, but more rapid in its motion and accurate in its indications than any I had previously employed. The instrument I am about to describe will, unless there be some error in the estimate which I have not been able to perceive, measure beyond the millionth of a second; and this degree of minuteness may be yet far surpassed by more costly instruments and more careful observations.

But as it is only on the hypothesis of an actual transfer of a fluid from one end of the wire to the other that a difference of time between the two sparks at its opposite extremities might be expected to be observed, in order to render the proposed experiment independent of this theoretical view, I took the necessary precaution of bringing a third spark, formed by disconnecting the middle of the wire, near to and in a line with the extreme sparks. For on the supposition of the transfer of two fluids in opposite directions, the extreme sparks would be simultaneous, but the middle spark later in its occurrence; the same appearances would also accord with the theory of one electricity, if we admit that a disturbance of electric equilibrium is simultaneously propagated from each end, arising in the one case from successive additions to, and in the other from successive subtractions from, the neutral quantity in the conducting wire.

The experiment was tried at the Gallery in Adelaide Street. The insulated wire, the total length of which was half a mile, was disposed as in fig. 6. The parallel portions of the wire were each 120 feet in length, and six inches apart, and were tied to the balustrade with silk loops six inches long. The swagging of the wire was prevented by silk cords extending across the gallery; and to keep the lengths at their proper distances apart they were tied to the cords wherever they crossed them. The ends of the wire marked 2, 3, 4, 5, were continued to the similarly marked wires of the spark-board, fig. 7, which was so fixed against the wall beneath the gallery, that the balls between which the sparks were to pass were in the same horizontal line. The striking-distance between each spark was the tenth of an inch, and the spark-board itself was three inches and a half in diameter. The conducting wire I employed was of copper, and its thickness the fifteenth of an inch.

Fig. 8. represents the measuring instrument with its appendages; and fig. 10. shows in a more distinct manner some of its essential parts. A B C D is a solid board of well baked mahogany one foot in length, and eight inches in breadth. E is a circular mirror of polished steel one inch in diameter, so fixed to the horizontal axle F G, that the axis of rotation is in the plane of the mirror. The pivots of the axle work in the uprights of the brass frame H I. Motion is communicated from the wheel K to the axle by means of a thread passing over grooves made on the circumferences of both; and a band passing over the wheel L, on the same axis with K, may be attached to the wheel of any machine capable of giving to it a rapid motion. In the experiments I have made with this instrument the train of wheels was so arranged that the axle carrying the mirror would have made 1800 revolutions

while the wheel to which the motion was first communicated was turned round once, had there been no retardation to have been taken into consideration arising from the slipping of the bands. M is a small Leyden jar, the inner coating of which is to be constantly supplied, through the chain N, with electricity, either positive or negative, from a machine; the bent wire proceeding from the inner coating of the jar is in immediate contact with the fixed discharger O P, and the spontaneous discharge of the jar is to be regulated by varying the distance between the two balls. The wire 1 in connexion with the outer coating of the jar, and the wire 6 attached to the knob of the brass frame, are continued to the similarly numbered wires of the spark-board. When the jar is fully charged, and the arm Q, revolving with the axle, is brought opposite the knob of the discharger, the discharge of electricity, or disturbance of electric equilibrium, passes through the entire circuit, and the three sparks appear perfectly simultaneous to the eye. When the face of the mirror is level with and turned towards the spark-board, and is so adjusted as to form an angle of 45° with the horizontal plane, the eye looking directly downwards sees the reflected images of the three sparks. The plane glass or lens R is for the purpose of preventing the eye approaching too near the mirror, and for accommodating the vision of long- or short-sighted observers. The arm Q is so placed that the circuit may be completed when the mirror is in the position just described; the other arm serves merely as a counterpoise. To obviate the inaccuracy which would result from discharges taking place when the arm is in different positions with respect to the knob of the discharger, a plate of mica, S, is interposed, having a very small horizontal slit exactly opposite the axis of the discharger; this fixes within narrow limits the occurrence of the discharge, and, with whatever rapidity the mirror moves, the sparks are generally within the field of view.

It was a point of essential importance to determine the angular velocity of the axle carrying the mirror. No confidence could be placed in the result obtained by calculating the train of wheels, as in such rapid motion many retarding causes might operate and render the calculation uncertain: it was necessary, therefore, to devise a means independent of these sources of error, and which should immediately indicate the ultimate velocity. Nothing appeared more likely to effect this purpose than to attach a small syren to the instrument, the plate of which should be carried round by the axle of the mirror. T is a small hollow box an inch in diameter, into which wind was conveyed through a tube placed to the aperture *u*. On the face of this box a number of equidistant apertures were arranged in a circle, and a disc moving before it having the same number of apertures, periodically intercepted the issuing current, and produced a sound corresponding to the frequency of the impulses. It is obvious that the number of revolutions would be ascertained by dividing the number of vibrations in a second, corresponding to the sound, by the number of apertures. I at first employed ten apertures: when the motion was slow, the sound could be easily determined; but on augmenting the velocity it became inappreciable. I then reduced the number of apertures to five, but with no better success, and ultimately to two; but the sound was then so feeble, compared with the accompanying noises, that it could not be distinctly heard.

The difficulty was at last overcome by employing the arm Q itself to produce the sound. A small slip of paper was held to it; and as at every revolution a blow was given to the paper, its rapid recurrence gave rise to a sound the pitch of which varied with the velocity of the motion. When the machinery was put in motion with the maximum velocity I employed in my experiments, the sound G♯[4] was obtained, indicating 800 revolutions of the mirror in a second. I am not aware that anything can have interfered with the accuracy of this result; the same sound was heard when different pieces of paper or card were used; and on moderating the velocity, the sound descended through all the degrees of the scale below it, until distinct percussions were perceived*.

Let us now consider what is the shortest duration of the electric light, and the greatest velocity of transmission through the wire, that can be detected by means of the instrument I have described. The mirror revolves 800 times in a second; and during this time the image of a stationary point would describe 1600 circles: the elongation of a spark through half a degree, a quantity obviously visible, and equal to one inch seen at the distance of ten feet, would therefore indicate that it exists the 1,152,000dth part of a second. The deviation of half a degree between the two extreme sparks, the wire being, as above stated, half a mile in length, would indicate a velocity of 576,000 miles in a second. This estimated velocity is on the supposition that the electricity passes from one end of the wire to the other: if, however, the two fluids in one theory, or the disturbances of equilibrium in the other, travel simultaneously from the two ends of the wire, the two external sparks will keep their relative positions, the middle one will be alone deflected, and the velocity measured will be only half that in the former case, viz. 288,000 miles in a second.

Repeated experiments gave the following results. In all cases, when the velocity of the mirror exceeded a certain limit, the three sparks were elongated into three parallel lines, and the lengths became greater as the velocity of the motion was increased. The greatest elongation observed was about 24°, indicating a duration of about the 24,000dth of a second. The lines did not always commence at the same places; sometimes they appeared immediately below the eye, sometimes to the right, at other times to the left, and occasionally they were out of view altogether. This indetermination, it has already been explained, is owing to the arm not always taking the spark at the same distance from the discharger: several discharges are therefore required to be made before the eye can distinctly observe the appearances. When

* Since this paper was read, a registering apparatus has been attached to the instrument; it consists of an index, communicating with the axis by a light train of wheels, and making one complete revolution while the mirror revolves 10,000 times. The number of revolutions of the mirror indicated by this means, did not, in consequence of the increased resistance to the motion, exceed 600 in a second.

the velocity was low, the terminating points appeared to be exactly in the same vertical line; but when the velocity was considerable, and the mirror revolved towards the right, the lines assumed this appearance, ; when it revolved towards the left, they appeared thus, . In no case did I see them thus, , or thus, , as required in the hypothesis of the actual transfer of a single fluid. I found it convenient to place at the side of and near the spark-board, the flame of a taper or candle, to serve as a guide to the eye: the lines of electric light in the mirror were immediately above and parallel to the constant line formed by the reflection of this flame, and thus the eye could be more readily directed to them: it also served to keep the focal distance of the eye properly adjusted. The spark-board was in all the experiments placed at the distance of ten feet from the mirror.

The deviation between the extreme sparks and the middle one could not, I am tolerably certain, have exceeded half a degree.

Having obtained a considerable elongation of these sparks, I expected also to be able to elongate the sparks or widen the lines in some of the various arrangements of electric light described in § 2; but even with the extraordinary velocity now attained, no alteration whatever could be observed in them; they were still reflected as distinct and unaltered as the objects themselves when directly looked at. The elongation of the sparks at the interruptions of the wire above noticed were no doubt owing to this circumstance,—that the diameter of the wire was not sufficiently great to allow the charge of the jar to pass through it except in a successive manner. The duration of the discharge in the cases of these sparks appeared to be longer than the time required for the electricity to pass through many miles of wire.

The sparks from the great magnet constructed by Mr. SAXTON, which is at the Gallery in Adelaide-street, were considerably elongated even when the mirror was moving with a comparatively low velocity.

§ 5.

For the purpose of increasing the chances of observing sparks, &c., when their appearance cannot be commanded at the moment the mirror is in the proper position to reflect them to the eye, I propose to employ a mirror with polygonal faces symmetrically placed with respect to the axis of rotation, a hexagon for instance, fig. 9, where *a*, *b* is the moving axis, and *c*, *d*, *e* three of the reflecting surfaces. During one rotation of the axis, if the object be continuously luminous, six luminous arcs will be successively presented to the eye, all occupying the same position; and if the light be transient, we shall have six times the number of chances of observing its reflection than if one reflecting surface only were employed. It is true the arcs are not circular ones, but the difference is scarcely noticeable when the radius of the polygonal section is very small compared with the distance of the luminous object, which would be the case in all our experiments.

I have also proposed various modifications of parts of the instrument, § 4, to suit particular experiments, and to ensure additional accuracy in the repetition of those already made; but not having yet put these to the test of experiment, it would be premature at present to describe them.

§ 6.

The instantaneousness of the light of electricity of high tension, rendered evident by the preceding investigations, affords the means of observing rapidly changing phenomena during a single instant of their continued action, and of making a variety of experiments relating to the motions of bodies when their successive positions follow each other too quickly to be seen under ordinary circumstances.

A few obvious instances will at present suffice. A rapidly moving wheel, or a revolving disc on which any object is painted, seems perfectly stationary when illuminated by the explosion of a charged jar. Insects on the wing appear, by the same means, fixed in the air. Vibrating strings are seen at rest in their deflected positions. A rapid succession of drops of water, appearing to the eye a continuous stream, is seen to be what it really is, not what it ordinarily appears to be, &c.

§ 7.

The preceding experiments having been directed rather to detect elongations and deviations than to measure them, I am not prepared to state the results with numerical accuracy. I shall endeavour to supply this deficiency in further investigations, but must at present content myself with stating the following general conclusions, deduced from the appearances which I have observed, though, I must allow, more accurately performed experiments are required before they can be considered as fully established. 1st, The velocity of electricity through a copper wire exceeds that of light through the planetary space. 2ndly, The disturbance of electric equilibrium in a wire communicating at its extremities with the two coatings of a charged jar, travels with equal velocity from the two ends of the wire, and occurs latest in the middle of the circuit. 3rdly, The light of electricity in a state of high tension has a less duration than the millionth part of a second. 4thly, The eye is capable of perceiving objects distinctly, which are presented to it during the same small interval of time.

By prosecuting these researches with instruments of higher power, and of greater accuracy in their indications, numerical laws may be established for a large class of phenomena, the relations of which we have had hitherto no means of observing. The relative velocities of electricity in different metallic wires; the modifications in the velocity of electricity in different states of tension when passing through the same conductor, if any such differences exist; the duration of the electric spark under different circumstances of tension and quantity, &c., will be among these objects of investigation.

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TO THE

PHILOSOPHICAL TRANSACTIONS

FOR THE YEAR 1834.

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METEOROLOGICAL JOURNAL,

KEPT BY THE ASSISTANT SECRETARY,

AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF

THE PRESIDENT AND COUNCIL.

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge .... =83 feet 2¼ in.

—————————————— above the mean level of the Sea (presumed about) ...... =95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House .... =79 feet.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR JANUARY AND FEBRUARY, 1834.

1834.	9 o'clock, A.M. Barom.	9 o'clock, A.M. Attach. Therm.	3 o'clock, P.M. Barom.	3 o'clock, P.M. Attach. Therm.	Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer. Fahrenheit. 9 A.M.	Fahrenheit. 3 P.M.	Self-registering. Lowest.	Self-registering. Highest.	Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
JANUARY W 1	29.850	46.7	29.906	48.0	33	39.7	43.3	38.3	43.3	.027	WNW	Cloudless—haze.
T 2	30.251	44.9	30.366	45.5	32	38.2	40.7	36.8	42.7		NNW	Cloudless—light haze and wind.
F 3	30.081	44.4	29.915	46.5	40	44.1	49.3	35.3	49.7		SW	Overcast—light rain and fog.
S 4	30.010	48.7	30.150	48.6	45	49.2	45.0	43.5	49.2		WNW	Fair—light clouds and haze.
⊙ 5	30.058	48.3	30.012	51.1	43	47.3	49.0	41.4	49.0		WNW	Lightly cloudy.—A.M. Light fog. P.M. Light wind.
M 6	29.814	48.9	29.642	50.2	42	46.8	48.5	45.4	48.5		SSW	Dark and overcast.—Rain, at night.
T 7	29.574	47.8	29.629	48.4	38	42.7	45.0	39.6	45.0		WSW	A.M. Lightly cloudy—light wind. P.M. Fine—light clouds.
W 8	29.210	48.3	29.237	49.6	44	45.2	46.9	40.7	46.9	.025	ESE	Overcast.—A.M. Rain, early. P.M. Light rain.
● T 9	29.395	48.4	29.309	48.2	40	42.2	43.2	41.3	43.6	.008	E	A.M. Fog and deposition. P.M. Overcast. Night, rain.
F 10	29.102	48.3	29.164	49.7	43	42.9	44.4	40.7	45.4		S	A.M. Drizzling rain. P.M. Fine and clear—light wind and clouds. Evening, rain.
S 11	29.410	49.5	29.427	51.0	45	45.4	48.5	42.2	49.2		S	Overcast.—At 2½ h. p.m. heavy rain with hail.
⊙ 12	29.120	50.8	29.067	52.0	48	48.7	49.8	44.8	49.8	.050	SSE	Overcast—light drizzling rain.
M 13	29.635	49.7	29.677	52.5	47	47.7	51.4	42.8	51.4	.006	S	Overcast.—Rain, a.m. At night, strong, unsteady wind.
T 14	29.663	51.7	29.627	53.3	45	47.9	50.5	47.2	50.5		SSE	A.M. Rain, early. P.M. Fair—light clouds and wind. Evening, clear.
W 15	29.318	51.3	29.641	52.9	47	47.7	47.6	44.6	48.3	.033	SSE	A.M. Rain, early. P.M. Fine and cloudless. At night, strong, unsteady wind.
T 16	29.609	51.6	29.639	55.2	47	48.4	52.5	43.7	52.7	.011	SSW	Fine.—A.M. Cloudless—light haze and deposition. P.M. Light clouds and wind.
F 17	29.419	55.8	29.355	56.7	52	52.7	51.0	47.7	52.3	.025	SW	A.M. Boisterous wind, with light rain. P.M. Fair—lightly cloudy.
S 18	29.563	52.9	29.691	53.7	43	45.3	48.8	44.2	48.8	.022	WSW	A.M. Fine and cloudless. Noon, hail-storm. P.M. Fine—light clouds.
⊙ 19	29.687	51.8	29.574	52.3	41	43.7	45.9	41.4	48.4	.028	WSW	Lightly overcast.—A.M. Light fog, and deposition. Evening, light shower.
M 20	29.980	49.3	30.035	51.4	38	39.8	47.0	37.8	47.7	.006	WSW	Fair.—A.M. Cloudless—light haze. P.M. Lightly overcast.
T 21	29.941	51.8	29.867	53.3	48	48.7	52.0	39.3	52.0		WSW	Lightly cloudy and overcast.
W 22	29.645	54.3	29.694	54.3	50	51.2	50.5	48.3	52.3		SSW	Light rain from 10 a.m. to 2 p.m.—A.M. Light unsteady wind. P.M. Fair—calm—overcast.
T 23	29.792	54.6	29.833	55.7	52	52.8	55.2	44.6	55.2	.061	W var.	Overcast.—Light rain, a.m.
F 24	29.999	57.3	29.870	58.3	53	54.6	55.7	52.8	55.2	.011	WSW	Lightly cloudy.—A.M. Clear. P.M. Overcast.
○ S 25	30.136	56.3	30.196	57.0	48	48.9	49.7	47.7	50.7		NW	Fine.—A.M. Cloudless—haze. P.M. Clear—light clouds.
⊙ 26	29.878	56.3	29.879	57.3	51	51.6	54.8	45.3	54.8	.004	SW	Overcast—light unsteady airs.
M 27	29.870	55.2	29.816	55.3	48	48.5	52.2	48.3	52.3	.006	NW	Light fog.—Rain, early a.m.
T 28	29.877	56.3	29.311	57.6	52	52.9	51.7	47.9	54.7	.005	SW	Overcast—deposition.—Light rain at 2 p.m.
W 29	30.234	46.6	30.344	46.7	26	34.6	38.3	34.1	38.3	.017	N	Fine and cloudless—light haze.
T 30	30.333	44.9	30.238	47.7	29	36.8	43.3	32.5	44.8		WSW	Overcast—light haze.
F 31	30.138	48.3	30.134	50.7	42	44.8	46.8	35.9	47.7		SSE	A.M. Overcast—deposition and light fog. P.M. Fine—light clouds.
MEANS..	29.761	50.7	29.750	52.0	43.6	46.2	48.3	42.5	49.0	Sum. .345	Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr.	9 A.M. 29.701 3 P.M. 29.686
FEBRUARY S 1	30.170	46.2	30.122	47.9	33	39.2	42.2	36.8	44.3		ESE	A.M. Overcast—light haze. P.M. Fine—light clouds.
⊙ 2	29.995	42.5	29.916	44.6	33	35.2	39.8	32.0	43.2		E	Lightly cloudy.—Fine, p.m.
M 3	30.015	43.8	29.992	46.6	40	41.1	46.7	34.3	46.7		SE	Very fine and cloudless—light haze.—Deposition, a.m.
T 4	29.871	45.3	29.826	47.7	39	41.7	47.2	38.4	47.2		ESE	Lightly cloudy—light haze.
W 5	29.854	46.8	29.816	49.7	43	43.4	49.2	39.7	49.7		SSE	A.M. Fine—nearly cloudless. P.M. Lightly overcast. Night, light rain.
T 6	30.024	47.2	30.038	49.7	40	40.9	46.2	38.8	46.7		WSW	Fine and cloudless—light haze.—Deposition, a.m.
F 7	30.142	45.4	30.147	46.4	37	37.4	40.6	34.8	40.6		SW	A.M. Strong haze. P.M. Lightly cloudy. Night, strong fog.
● S 8	30.152	42.4	30.144	43.6	31	33.2	39.8	31.0	39.8		ESE	Strong haze.
⊙ 9	30.305	41.3	30.388	42.4	34	34.4	38.7	31.4	38.7		SSE	A.M. Fog and light deposition. P.M. Lightly cloudy.
M 10	30.388	40.3	30.334	43.4	30	34.7	40.8	30.2	42.6		SSW	Lightly cloudy.
T 11	30.110	43.6	30.080	45.4	34	43.7	45.3	33.6	45.3		SSW	A.M. Very light rain—light fog. P.M. Fine—light clouds and haze.
W 12	29.636	45.6	29.715	47.4	42	44.3	44.8	38.1	45.7		SE	A.M. Light continued rain. P.M. Fine—light clouds.
T 13	30.020	42.7	30.107	45.6	34	37.2	44.3	33.7	44.7		WSW	Fair—lightly cloudy—haze.—Light hoar frost, a.m.
F 14	30.302	43.7	30.288	46.4	35	39.8	45.7	36.1	45.7		NNW	Morning and evening hazy. Noon, fine. Night, light rain.
S 15	30.288	45.4	30.275	46.7	40	42.1	45.0	38.7	45.0		W	A.M. Fog—light deposition. P.M. Lightly cloudy.
⊙ 16	30.400	42.8	30.369	45.3	32	37.6	42.3	34.8	42.7		NNE	A.M. Fair—light clouds and haze. P.M. Fine and clear—light clouds.
M 17	30.289	40.3	30.235	44.0	30	32.3	42.6	29.3	43.2		SSW	Fine and cloudless—haze.—Hoar frost, a.m.
T 18	30.156	44.7	30.103	46.4	43	44.2	46.8	31.4	46.8		SW	Overcast—hazy.
W 19	29.962	47.2	29.946	49.8	41	46.3	50.6	43.3	50.7		WSW	Lightly cloudy and overcast—light haze.
T 20	30.185	47.8	30.136	50.7	42	42.7	48.3	40.5	49.2		WSW	A.M. Hazy. P.M. Fine—light clouds.
F 21	30.075	48.7	30.185	50.3	39	43.8	47.3	41.5	47.3		NNW	Fine.—A.M. Cloudless—haze. P.M. Clear—light clouds.
S 22	30.403	45.6	30.396	48.7	38	38.4	46.3	34.3	46.7		WSW	Fine.—A.M. Cloudless—haze. P.M. Light clouds.
○⊙ 23	30.283	48.8	30.226	51.4	44	46.8	51.2	37.2	51.7		SSW	Lightly cloudy and overcast.
M 24	30.114	49.8	30.045	52.6	47	48.8	53.4	46.2	54.2		SSW	Lightly cloudy and overcast.—A.M. Light wind. P.M. Light haze.
T 25	30.472	48.6	30.487	50.9	35	41.3	48.0	38.5	48.0		WSW	Fine and cloudless.—A.M. Haze. P.M. Clear.
W 26	30.448	46.7	30.324	50.3	41	42.7	49.7	34.8	49.7		SSW	A.M. Overcast—light wind and haze. P.M. Clear and cloudless.
T 27	30.150	49.3	30.138	52.3	47	48.6	53.8	40.2	54.7		SSW	Overcast.
F 28	30.157	52.9	30.394	52.3	53	53.2	46.3	47.8	53.2		WSW	Fog and light rain.
MEANS ..	30.156	45.6	30.149	47.8	38.5	41.2	45.8	36.7	46.6	*	Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr.	9 A.M. 30.115 3 P.M. 30.102

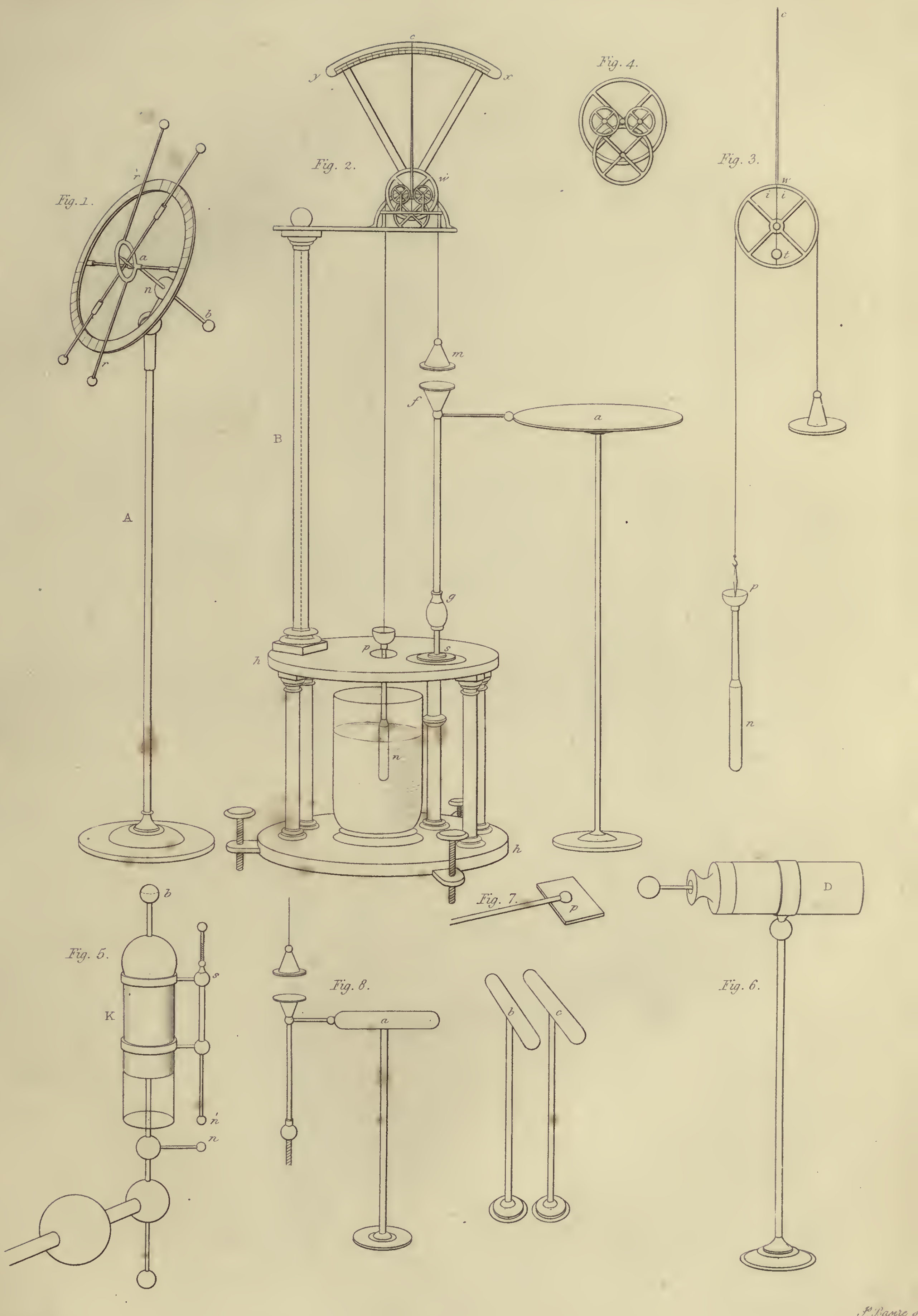
* The Rain Gauge is, in every state of the weather, invariably examined every morning at 9 o'clock, and the result set down in the appropriate column; while, on the other hand, any *sensible exhibition* of rain is equally noticed under the Remarks on the Weather in the last column, independently of any reference to the indications of the Gauge. During the present month, the amount of rain appears to have been too small to become appreciable in the Rain Gauge employed.—J.H.

METEOROLOGICAL JOURNAL FOR MARCH AND APRIL, 1834.

1834.	9 o'clock, A.M.		3 o'clock, P.M.		Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer.				Rain in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
						Fahrenheit.		Self-registering.				
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		9 A.M.	3 P.M.	Lowest.	Highest.			
MARCH												
S 1	30.468	51.7	30.414	54.2	50	50.0	54.6	44.3	54.7		S	Deposition—light clouds and fog.
⊙ 2	30.310	53.3	30.305	56.4	48	50.3	55.3	47.7	55.3		SSW	Lightly cloudy and overcast.
M 3	30.406	51.3	30.337	54.1	46	46.2	52.7	43.3	52.8		SE	Overcast—light fog.
T 4	30.160	53.3	30.063	55.7	46	49.3	50.0	44.7	55.6		SSW	A.M. Fine and clear. P.M. Lightly cloudy.
W 5	29.780	54.7	29.748	56.2	53	53.3	55.0	48.7	55.0		SSW	Lowering—light brisk wind.—At night, high wind.
T 6	29.994	52.8	30.041	55.2	43	45.2	52.7	40.7	52.7		W	Fine and cloudless—light haze.
F 7	30.209	53.6	30.229	56.3	47	49.4	55.2	44.3	57.1		WSW	Fine.—A.M. Cloudless—light breeze. P.M. Light clouds.
S 8	30.328	53.9	30.303	57.4	49	50.4	57.6	44.8	57.8		WSW	Fine—light clouds.
⊙ 9	30.485	54.6	30.464	57.3	49	50.7	56.5	46.2	57.2		WSW	Fine and clear—light clouds.
● M 10	30.369	53.3	30.330	56.4	47	47.8	56.4	43.3	56.7		W	Fine.—A.M. Lightly overcast. P.M. Clear—light clouds.
T 11	30.477	53.2	30.414	54.9	42	47.4	53.2	45.3	53.2		ENE	Haze.—Light wind, p.m.
W 12	30.536	51.8	30.482	54.9	43	45.2	54.6	40.7	54.6		ESE	Hazy.—A.M. Lightly cloudy. P.M. Strong deposition.
T 13	30.426	52.4	30.380	53.2	46	46.7	47.7	44.7	47.7		ESE	A.M. Light rain—fog. P.M. Haze.
F 14	30.386	48.7	30.380	51.6	35	41.8	50.5	35.7	50.5	.022	N	A.M. Cloudless—light haze. P.M. Cloudy—light wind.
S 15	30.542	48.7	30.507	51.3	38	44.5	50.0	40.8	50.0		N	Light wind.—A.M. Cloudy. P.M. Fine—light clouds.
⊙ 16	30.547	46.7	30.475	49.5	38	40.8	49.5	36.3	49.7		N	Fine.—A.M. Cloudless. P.M. Light clouds and wind.
M 17	30.485	47.6	30.483	48.4	43	44.0	46.0	39.8	46.0		NNE	Light haze.—A.M. Cloudy. P.M. Hazy—light wind.
T 18	30.562	45.3	30.542	47.4	29	40.3	43.6	37.3	43.8		ESE	A.M. Overcast and Hazy. P.M. Fine—light clouds.
W 19	30.561	42.2	30.509	46.5	36	37.8	44.3	30.8	44.3		E	Fine.—A.M. Lightly cloudy—light wind. P.M. Nearly cloudless—haze.
T 20	30.476	44.2	30.434	46.8	36	41.7	45.5	36.3	46.5		NNE	Lightly overcast.—A.M. Haze. P.M. Light wind.
F 21	30.427	43.8	30.360	46.0	33	40.6	43.7	35.1	43.7		NE	Overcast—haze.
S 22	30.202	46.3	30.095	50.0	33	43.7	50.9	39.6	51.6		WSW	Fine—lightly cloudy.—Evening, light rain.
⊙ 23	30.019	47.9	29.830	51.7	40	44.8	51.8	36.8	53.4	.017	W	Lightly cloudy—light unsteady wind.
M 24	29.786	50.2	29.808	51.3	38	48.7	48.0	44.3	48.7	.010	NNW	Light brisk wind.—A.M. Fine—cloudy. P.M. Lightly overcast. Hail storm at 2½ h.
○ T 25	29.882	46.6	29.940	47.7	27	39.2	44.2	35.3	44.3	.021	NNW	Fine.—A.M. Cloudless. P.M. Light clouds. Evening, clear.
W 26	30.188	43.7	30.162	46.7	29	37.8	45.2	30.7	45.2		N	Fine and cloudless—light haze.
T 27	30.116	46.8	30.043	50.3	39	45.4	55.0	36.8	55.0		W	Cloudy and overcast.—Evening, light rain.
F 28	29.776	50.7	29.571	52.3	46	48.8	49.8	44.8	50.6	.008	SSW	Overcast—light showers and unsteady wind.
S 29	29.683	50.3	29.679	52.8	38	46.5	50.6	40.4	53.3	.006	W	Fine and nearly cloudless—light breeze.—Clear, p.m.
⊙ 30	29.923	48.8	29.898	52.3	38	43.4	53.3	35.3	53.3		W	Fine—light haze—light showers at intervals. A.M. Cloudless. P.M. Cloudy.
M 31	29.874	47.7	29.943	50.8	37	42.7	48.7	37.2	49.1	.222	NNW	Fine—light clouds and breeze.
MEANS..	30.238	49.6	30.199	52.1	40.7	45.3	50.7	40.4	51.3	Sum. .306	Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr.	9 A.M. 30.186 — 3 P.M. 30.139
APRIL												
T 1	30.116	48.2	30.188	51.7	38	43.8	51.8	35.8	51.8	.014	N	Fine—light clouds, haze, and wind.
W 2	30.274	50.2	30.244	51.6	46	47.3	50.7	43.2	51.5		S	Overcast—light rain and fog.
T 3	30.299	53.4	30.352	55.2	50	51.6	54.6	46.7	56.3		NNW	Light wind.—A.M. Cloudy. P.M. Fine—light clouds.
F 4	30.489	50.7	30.394	54.7	41	46.3	54.8	39.6	54.6		NNW	Fine.—A.M. Cloudless—haze. P.M. Light clouds.
S 5	30.352	50.7	30.279	55.2	45	48.9	56.0	45.6	56.9		N	Fine—lightly cloudy.—Haze, a.m.
⊙ 6	30.390	52.6	30.370	55.4	44	49.7	54.5	45.3	54.7		NNE	Lightly cloudy and hazy.
M 7	30.355	52.7	30.279	55.3	42	48.0	56.4	40.2	56.8		SSW	Fine and cloudless—haze.—Calm, p.m.
T 8	30.374	52.8	30.348	53.7	37	46.3	49.0	42.3	49.2		E	Light wind.—A.M. Cloudy. P.M. Fine—light clouds.
● W 9	30.362	48.3	30.307	50.3	31	43.2	46.2	38.9	46.2		ENE	Light wind.—A.M. Overcast—light haze. P.M. Cloudy.
T 10	30.305	47.2	30.237	49.8	31	42.6	46.5	32.6	47.6		N	Fine, and nearly cloudless—light wind.—Hail at 4 h. 22 m. p.m.
F 11	30.200	46.3	30.134	49.0	33	42.0	44.3	32.3	45.4		N	Cloudy.—Light unsteady wind, a.m.; and hail and rain at 5 p.m.
S 12	30.002	44.8	30.023	47.9	36	41.3	44.8	34.2	46.5		N	A.M. Clear—light clouds and wind. P.M. Hail and thunder storm at 3 o'clock.
⊙ 13	30.233	46.5	30.242	48.6	37	44.4	46.3	34.2	47.3	.011	N	Light soft clouds.—Fine, a.m.
M 14	30.382	45.7	30.358	48.2	36	44.7	49.5	33.2	49.5		E	Fine.—A.M. Light soft clouds. P.M. Cloudless.
T 15	30.416	47.2	30.374	50.2	38	46.2	53.3	35.2	53.8		E	Fine.—A.M. Light clouds. P.M. Cloudless.
W 16	30.360	48.8	30.307	51.6	41	46.9	52.4	37.3	52.7		ESE	A.M. Lightly cloudy. P.M. Fine and cloudless.
T 17	30.268	48.5	30.204	52.4	43	46.7	55.2	38.2	55.3		NE	Fine and cloudless—haze and light wind.
F 18	30.199	51.7	30.182	54.5	45	50.9	58.0	40.8	59.1		ENE	Fine—light wind.—A.M. Light clouds. P.M. Nearly cloudless.
S 19	30.243	53.2	30.214	56.4	46	50.8	62.3	39.7	62.8		N	Fine.—A.M. Cloudless—light wind. P.M. Light clouds.
⊙ 20	30.295	52.3	30.245	55.8	45	47.2	57.5	42.4	58.0		N	A.M. Overcast—light wind. P.M. Fine and clear.
M 21	30.339	51.2	30.297	55.0	43	44.2	54.8	38.8	54.8		NE	A.M. Overcast. P.M. Fine—light clouds.
T 22	30.313	51.3	30.227	54.7	40	47.2	55.9	37.7	56.7		N	Fair—light clouds and wind.
W 23	30.224	53.7	30.220	56.3	44	49.7	54.3	41.0	54.3		N	Lightly overcast and cloudy.—Light haze and wind, a.m.
○ T 24	30.330	53.6	30.289	54.3	37	47.8	50.2	42.0	50.3		N	Lightly overcast and cloudy.
F 25	30.164	50.3	30.128	53.6	37	44.6	52.5	37.0	52.6		W	Overcast—haze.
S 26	30.091	53.3	30.004	56.3	35	48.7	54.2	40.2	57.2		E	Fine and clear—light clouds.
⊙ 27	29.697	55.8	29.516	57.4	38	52.7	62.6	39.0	63.6		ESE	Clear and cloudless.—Evening, overcast.
M 28	29.322	57.6	29.344	61.0	51	57.2	62.5	52 7	64.3		S	Lowering—light wind.—Light rain, early a.m.
T 29	29.418	60.3	29.398	61.3	53	58.3	58.1	51.4	62.0		SSW	Overcast—slightly lowering.—Occasional light rain.
W 30	29.519	59.3	29.582	61.3	53	54.4	58.0	52.3	58.7		WSW	Overcast—light fog.
MEANS..	30.178	51.3	30.143	54.0	41.2	47.8	53.6	40.3	54.4	Sum. .025	Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr.	9 A.M. 30.120 — 3 P.M. 30.077

METEOROLOGICAL JOURNAL FOR MAY AND JUNE, 1834.

1834.	9 o'clock, A.M.		3 o'clock, P.M.		Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
						Fahrenheit.		Self-registering.				
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		9 A.M.	3 P.M.	Lowest.	Highest.			
MAY T 1	29.677	58.9	29.744	62.2	55	55.4	60.3	50.9	61.2		S	Light wind.—A.M. Lowering—rain, early. P.M. Fine—light clouds.
F 2	29.924	62.2	29.875	62.8	52	58.1	62.2	46.7	63.7	.019	S	Lightly overcast.
S 3	29.989	61.7	29.994	64.9	52	59.7	66.5	51.3	68.4		SW	Fine—light clouds.—Clear, a.m.
☉ 4	29.990	63.8	29.973	66.3	53	64.9	71.8	50.3	73.4		S	Fine and clear—light clouds.
M 5	29.962	64.8	30.010	65.9	58	64.2	64.2	61.7	67.0	.017	S	A.M. Continued rain—light fog. P.M. Fair—cloudy.
T 6	30.330	66.9	30.352	67.7	53	61.6	69.8	49.4	70.3	.080	WSW	Fine and cloudless—light cloudiness.
W 7	30.469	68.4	30.420	69.5	53	63.2	73.2	51.5	75.0		WSW	Fine and cloudless.—Light cloudiness, a.m.
● T 8	30.340	65.6	30.204	68.9	56	60.3	72.8	54.8	74.7		WSW	Fine.—A.M. Lightly overcast. P.M. Nearly cloudless.
F 9	29.894	68.2	29.810	70.6	52	65.6	72.7	57.3	73.3		SSW	Fine—lightly cloudy.
S 10	29.983	68.2	29.958	68.5	46	60.6	65.6	51.8	67.7		NNE	Fine and clear—light clouds.
☉ 11	29.860	67.2	29.760	68.4	48	61.9	70.4	50.0	72.3		SSE	Fine and clear.—A.M. Light cloudiness. P.M. Light clouds.
M 12	29.741	64.7	29.744	68.4	55	61.1	66.8	57.8	67.8	.083	SW	Cloudy.—A.M. Rain, early. P.M. Fine. Evening, light rain.
T 13	29.617	63.8	29.602	65.7	54	59.8	58.5	54.6	62.7	.036	SSW	A.M. Cloudy—light wind. P.M. Light rain. Evening, clear.
W 14	29.783	64.4	29.819	65.8	51	58.8	62.8	50.5	64.6	.011	SW	Fine—cloudy.—Clear, a.m.
T 15	29.858	63.5	29.893	66.2	53	61.8	67.9	54.8	69.8		ESE	A.M. Fair—lightly cloudy. P.M. Fine—light clouds.
F 16	29.950	65.6	29.869	67.5	55	60.2	68.2	53.5	70.8		NNE	A.M. Fair—lightly cloudy. P.M. Cloudy.
S 17	29.533	68.4	29.431	66.5	53	65.4	57.3	54.7	67.3		SW	A.M. Fine—light soft clouds. P.M. Light rain.
☉ 18	29.454	67.3	29.499	65.4	43	58.8	59.8	44.7	62.8	.103	WSW	Fine—cloudy.—Clear—light wind, p.m.
M 19	29.808	66.4	29.889	64.9	44	60.4	63.4	46.9	65.8		WSW	Fine and clear—light clouds.—Light shower about noon.
T 20	30.329	66.2	30.350	64.7	46	60.0	68.2	46.0	69.2	.081	WSW	Fine and cloudless.—Faint cloudiness, a.m.
W 21	30.524	67.7	29.980	66.2	48	62.2	67.0	50.5	68.7		NNE	Fine and cloudless.—Faint cloudiness, a.m.
○ T 22	30.461	66.2	30.372	64.4	49	59.6	63.8	47.3	64.3		ESE	Fine—light unsteady wind.—A.M. Lightly cloudy. P.M. Cloudless.
F 23	30.305	64.7	30.285	65.4	53	63.8	68.2	49.2	68.2		ENE	Fine and cloudless—light unsteady wind.—Light cloudiness, a.m.
S 24	30.388	67.3	30.378	67.9	53	63.7	70.8	48.3	70.8		NNE	Fine and cloudless.—A.M. Light cloudiness. P.M. Light wind.
☉ 25	30.468	62.4	30.392	63.3	42	58.1	62.0	49.7	62.0		NNE var.	A.M. Fair—light clouds—light unsteady wind. P.M. Clear and cloudless—light wind.
M 26	30.334	58.8	30.313	61.5	39	54.9	61.7	44.8	62.7		NE var.	Fine—light wind.—A.M. Lightly cloudy. P.M. Cloudless.
T 27	30.301	62.6	30.239	62.5	44	57.9	66.6	44.4	66.7		NNE	Fine and cloudless—light wind.—Light cloudiness, a.m.
W 28	30.228	65.3	30.176	63.2	43	58.4	63.4	47.7	63.7		NNE	Fine and cloudless.—Light wind and cloudiness, a.m.
T 29	30.155	64.6	30.085	63.0	40	56.3	67.0	43.0	68.5		NE	Fine.—A.M. Cloudless—light cloudiness. P.M. Light clouds.
F 30	30.134	66.7	30.156	66.3	49	60.2	64.5	53.4	66.9		NNE	Fine.—A.M. Light cloudiness. P.M. Light clouds and wind.
S 31	30.278	67.2	30.239	65.4	45	60.2	66.3	47.3	68.3		ESE	Fine—lightly overcast.
MEANS ..	30.067	65.2	30.026	65.8	49.6	60.6	65.9	50.5	67.7	Sum. .430	Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr.	9 A.M. 29.967 — 3 P.M. 29.924
JUNE ☉ 1	30.360	70.7	30.296	67.7	48	67.0	74.0	51.6	75.1		W	Fine.—A.M. Lightly overcast. P.M. Cloudless—streaked cloudiness.
M 2	30.238	71.4	30.165	69.3	50	72.7	76.2	54.3	77.6		SSW	Fine and clear.—A.M. Light clouds. P.M. Cloudless.
T 3	30.095	71.3	30.077	70.0	54	68.7	67.5	61.2	73.6		W	Fair—lightly cloudy.—Light shower, p.m. Evening, clear.
W 4	30.027	73.3	29.913	70.8	49	66.2	66.5	53.2	70.8		SSW	A.M. Fine—light broken clouds and cloudiness. P.M. Cloudy. Evening, rain.
T 5	29.744	72.6	29.829	68.9	49	64.1	62.6	52.2	67.2	.069	WNW	Cloudy.—A.M. Clear. P.M. Light brisk wind. Evening, clear.
F 6	30.124	69.6	30.128	68.5	47	61.9	65.4	48.7	67.7		N	Fine—light clouds.—Cloudiness, a.m.
S 7	30.174	69.3	30.081	68.2	51	62.8	67.0	50.4	70.3		N	Fine.—A.M. Cloudless—light cloudiness. P.M. Light clouds.
● ☉ 8	29.967	72.3	29.889	71.0	46	64.2	69.7	48.6	71.8		N	Fine and cloudless.—A.M. Light cloudiness. P.M. Clear.
M 9	29.796	73.3	29.739	70.8	52	68.8	64.7	54.4	76.6		SSW	Fine and cloudless—light cloudiness.—Evening, clear.
T 10	29.707	67.7	29.651	69.3	52	65.7	66.9	56.4	71.0		SSW	A.M. Clear—cloudy. P.M. Dark and lowering—light wind. Shower at 3½ h.
W 11	29.732	72.3	29.706	68.2	50	62.0	61.6	51.2	67.4	.025	S	Heavy showers—light wind.—Fine and lowering, alternately.
T 12	29.816	72.4	29.764	68.0	46	63.3	65.5	47.3	68.7	.333	SW var.	A.M. Clear—cloudy. P.M. Fine—light clouds. Evening, rain.
F 13	29.777	68.8	29.847	67.7	50	62.0	65.2	50.7	67.7	.125	WNW	Cloudy.—A.M. Fine. P.M. Light continued rain.
S 14	29.954	67.0	29.897	68.6	55	65.0	69.8	58.8	71.8		S	Light wind and clouds.—A.M. Cloudy. P.M. Fine. Thunder storm at 8½ h.
☉ 15	29.935	73.2	29.899	70.9	51	66.9	70.3	57.7	72.8	.014	W	Fine—lightly cloudy.—Clear—light fresh wind, p.m. and evening.
M 16	29.702	69.4	29.671	69.6	49	65.1	65.2	59.4	68.7		W	A.M. Clear—cloudy. P.M. Lightly overcast. Heavy shower and brisk wind at 11, a.m.
T 17	29.766	65.1	29.823	67.3	49	60.3	65.6	52.8	67.4	.031	W	Clear—cloudy.—Light brisk wind, p.m. Evening, fine—lowering—light shower.
W 18	30.092	72.6	30.043	68.7	51	65.7	65.0	50.9	71.2	.011	SW	A.M. Fine and clear—cloudy. P.M. Light rain. Evening, lowering—light wind—deposition.
T 19	30.137	67.8	30.116	70.6	61	67.3	72.8	61.2	75.2		WSW	A.M. Fair—cloudy. P.M. Fine and clear—light soft clouds—light wind.
F 20	30.152	74.3	30.049	72.0	57	68.2	78.2	54.6	82.2		SW	Fine.—A.M. Lightly cloudy. P.M. Cloudless.
○ S 21	29.910	80.0	29.879	76.2	61	80.4	84.4	65.3	86.7		SSW	Fine.—A.M. Clear and cloudless. P.M. Light clouds. Midnight, heavy rain.
☉ 22	29.932	72.7	29.996	75.3	62	67.2	72.6	63.2	74.4	.222	SSW	Cloudy.—Fair, p.m. Evening, fine—light clouds.
M 23	30.295	76.4	30.271	73.8	47	65.3	73.3	55.3	74.4		WSW	Fine.—A.M. Cloudiness. P.M. Clear—light clouds.
T 24	30.364	75.3	30.330	72.0	50	68.2	70.0	53.9	73.2		WSW	Fine—light clouds.—Clear, p.m.
W 25	30.357	67.8	30.328	71.8	58	63.2	72.5	59.8	74.2		SSW	Overcast.—Evening, fine—light high clouds.
T 26	30.241	71.6	30.164	72.9	54	66.7	72.0	60.7	75.7		W	A.M. Fine and clear—light clouds and wind. P.M. Overcast. Evening, lowering, Night, rain.
F 27	30.119	69.8	30.192	70.4	55	64.7	62.7	58.6	68.3	.097	NW	Lightly cloudy—haze.
S 28	30.285	70.0	30.194	70.3	45	62.6	70.0	50.6	71.7		N	A.M. Fair—lightly cloudy. P.M. Fine and clear—light clouds.
☉ 29	30.225	71.4	30.266	70.3	50	64.2	67.2	56.3	69.3		E	A.M. Lightly cloudy—light unsteady wind. P.M. Fine and clear—light wind.
M 30	30.435	73.7	30.374	70.3	45	65.1	69.9	50.7	71.7		NNE	Fine.—A.M. Clear—light clouds—light unsteady wind. P.M. Nearly cloudless. Evening, clear.
MEANS ..	30.049	71.4	30.019	70.3	51.5	65.9	69.1	55.0	72.5	Sum. .927	Mean of Barometer, corrected for Capillarity and reduced to 32° Fahr.	9 A.M. 29.930 — 3 P.M. 29.903



G. Wightwick, del.

J. Basire sc.

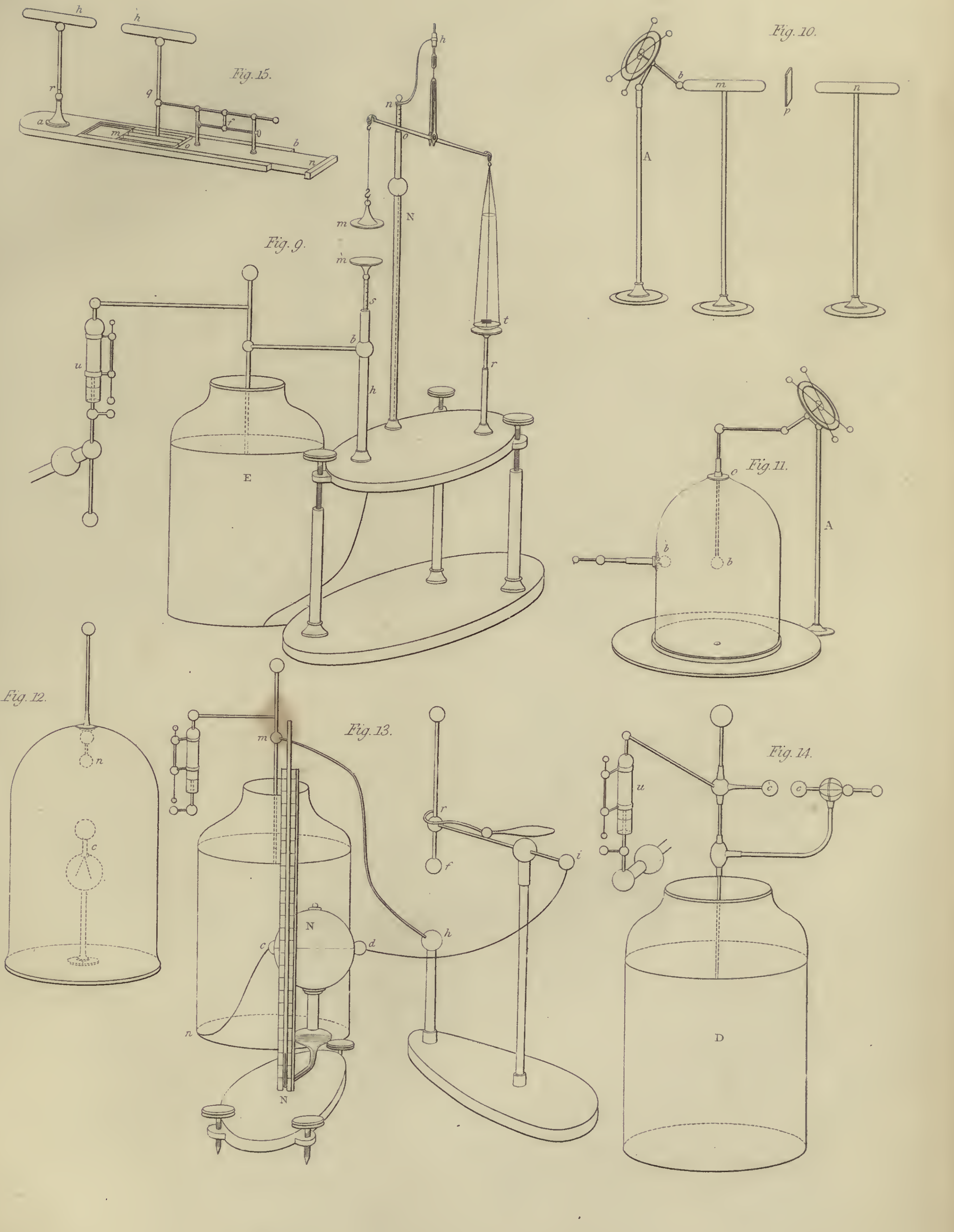


Fig. 9.
Fig. 10.
Fig. 11.
Fig. 12.
Fig. 13.
Fig. 14.
Fig. 15.
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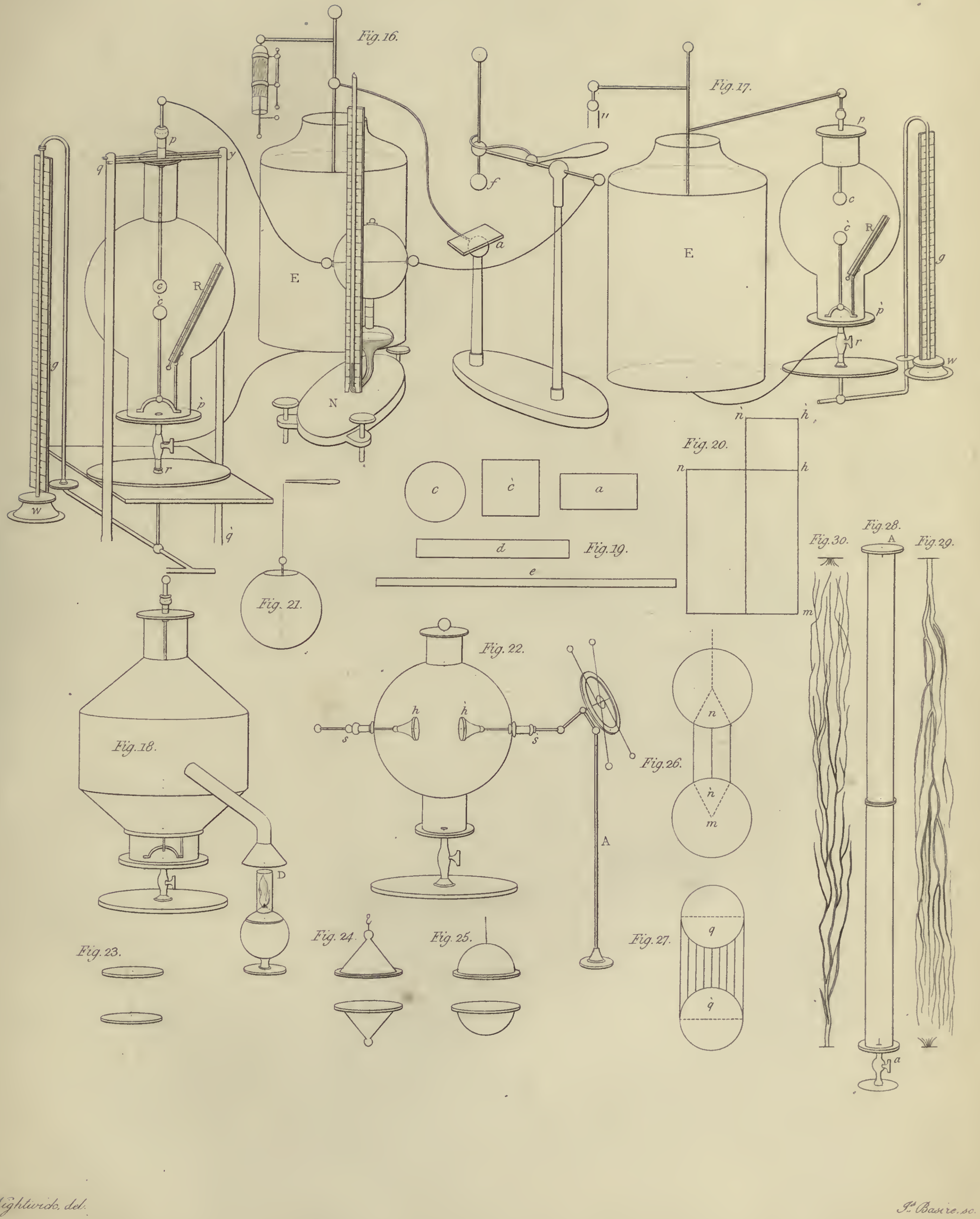
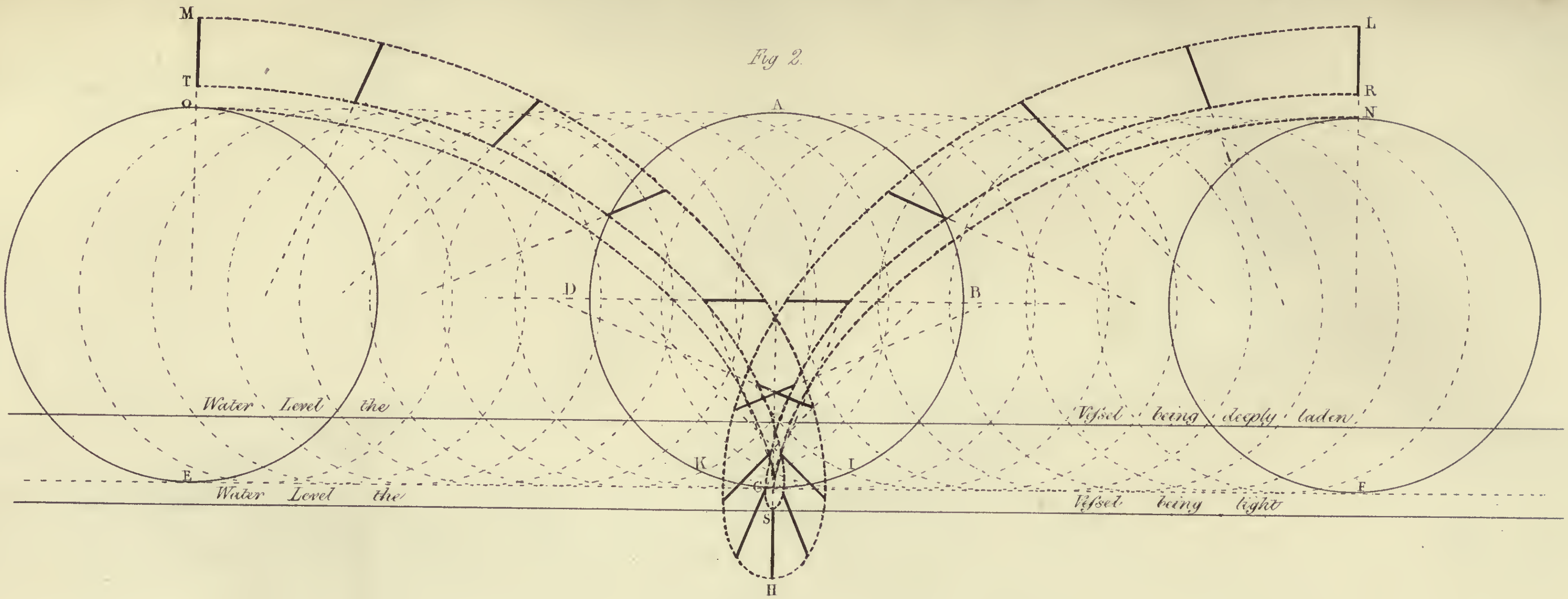


Wightwick, del. Ja. Basire, sc.

Position of the Floats of a Paddle Wheel when in motion.

Fig 2.



Morgan's Paddle Wheel.

Fig 1.

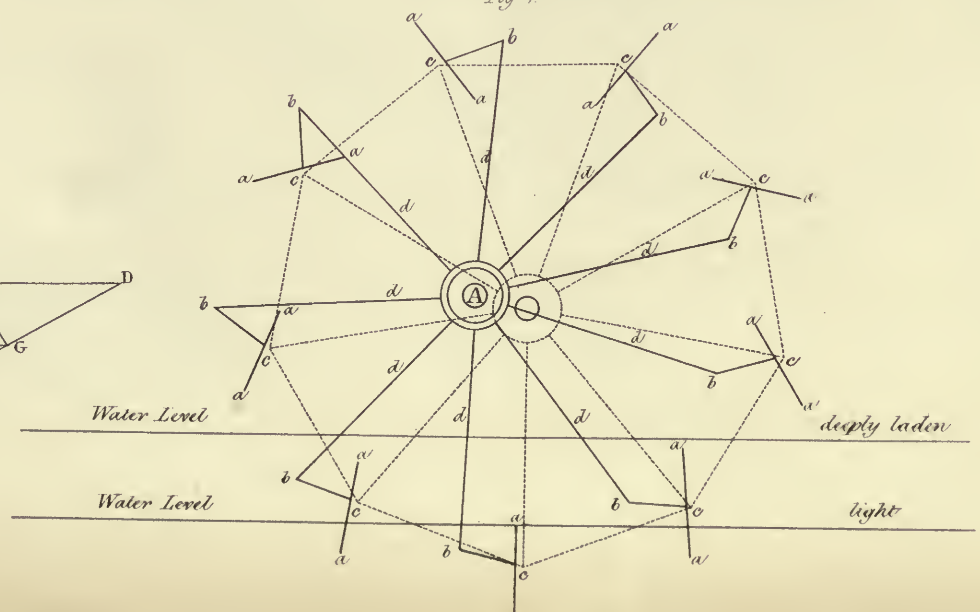


Fig. 3.

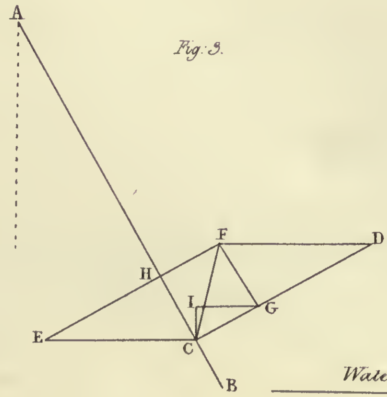
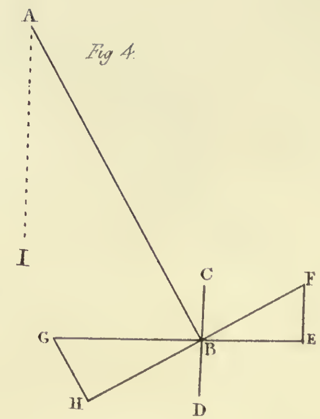


Fig 4.



P. W. Barlow del.

J. Basire lithog.

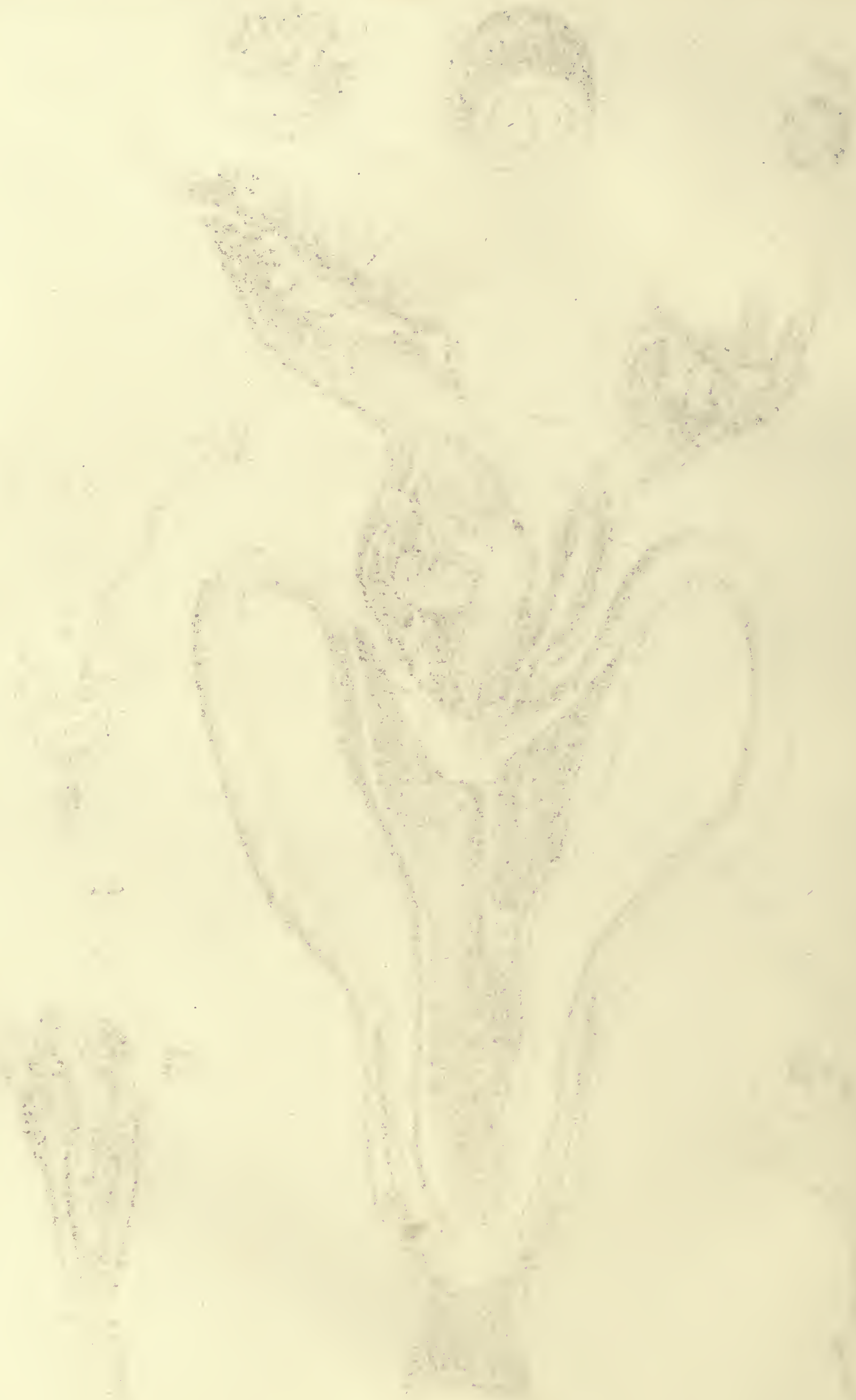


Fig. 1

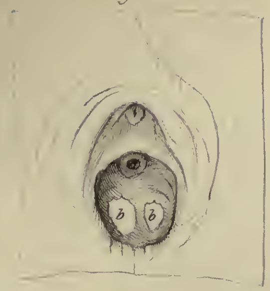


Fig. 2

Fig. 3

Fig. 7

Fig. 5

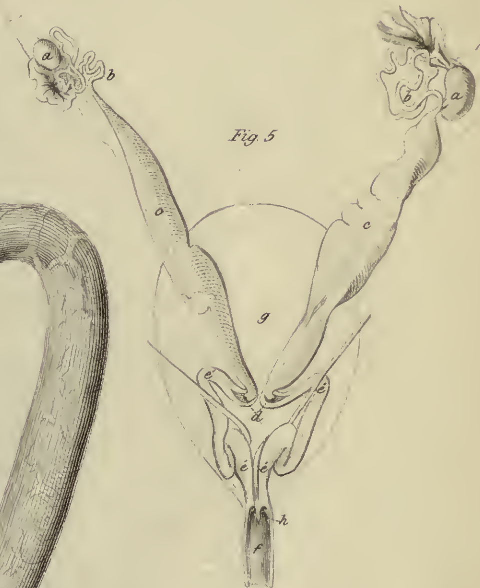


Fig. 6

Fig. 4

Richd. Owen, del.

J. Basire sc.


Fig. 3
Fig. 5
Fig. 4
Fig. 11
Fig. 1
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Fig. 9
Fig. 6*
Fig. 12
Fig. 2
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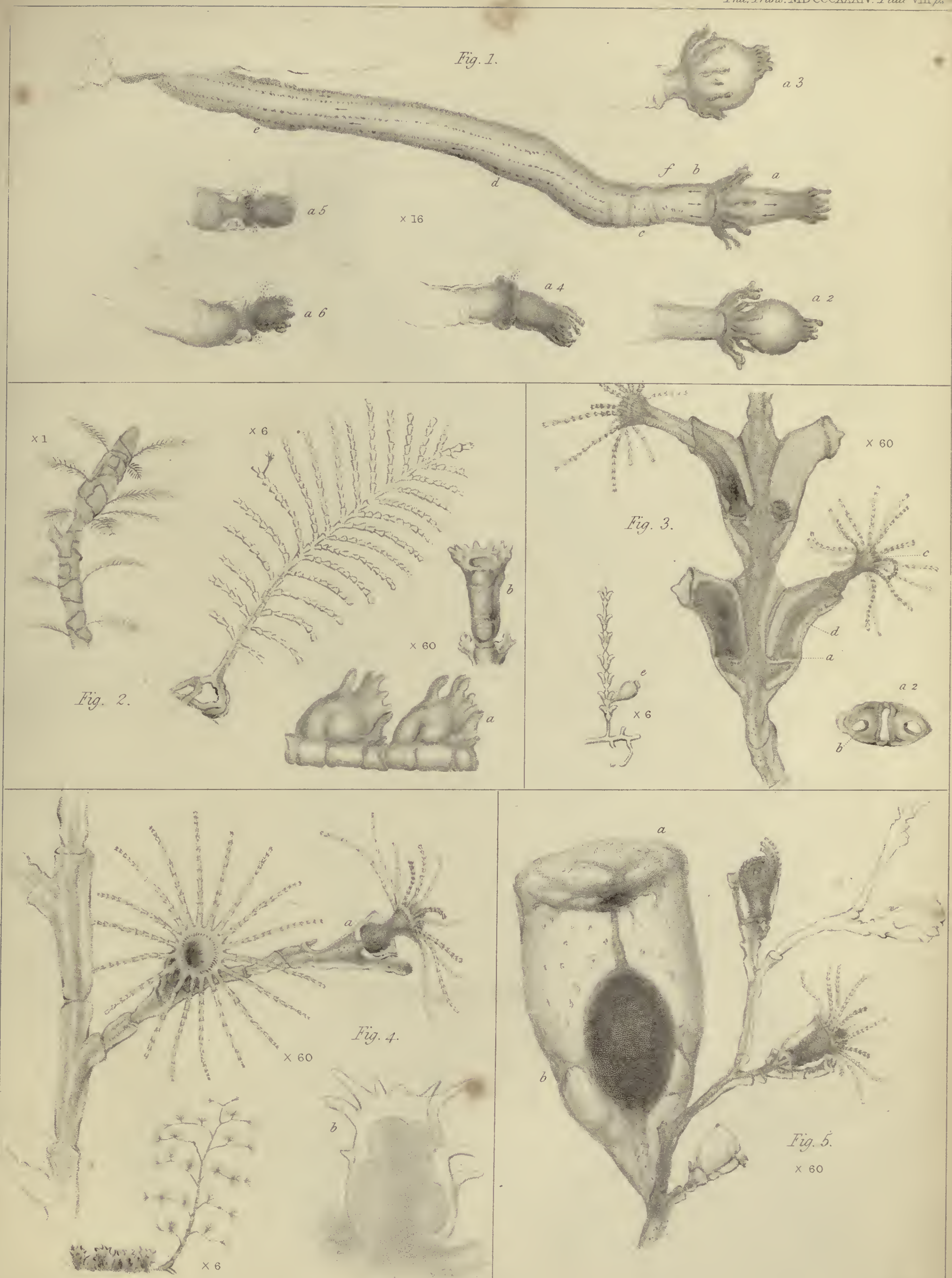
Fig. 8
Fig. 13
Fig. 14



J. J. Lister del.

J. Basire sc.

J. J. Lister del.

J. Basire sc.

J. J. Lister del.

J. Basire sc.

Fig. 2.

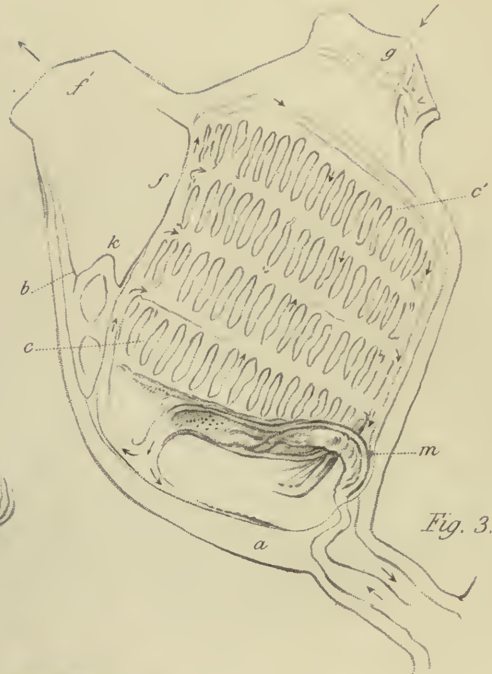


Fig. 3.

Fig. 1.

Fig. 6.

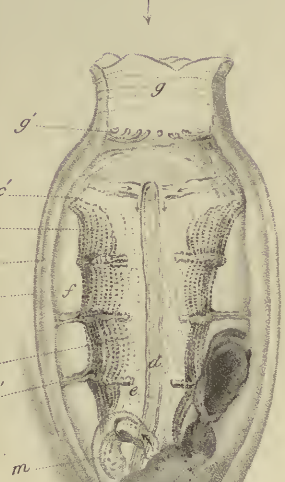


Fig. 4.

× 30

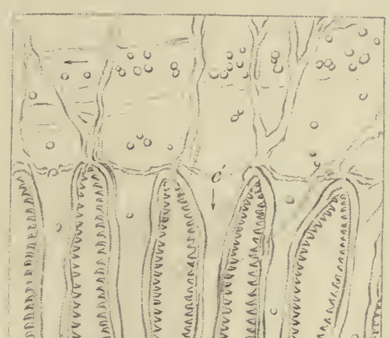
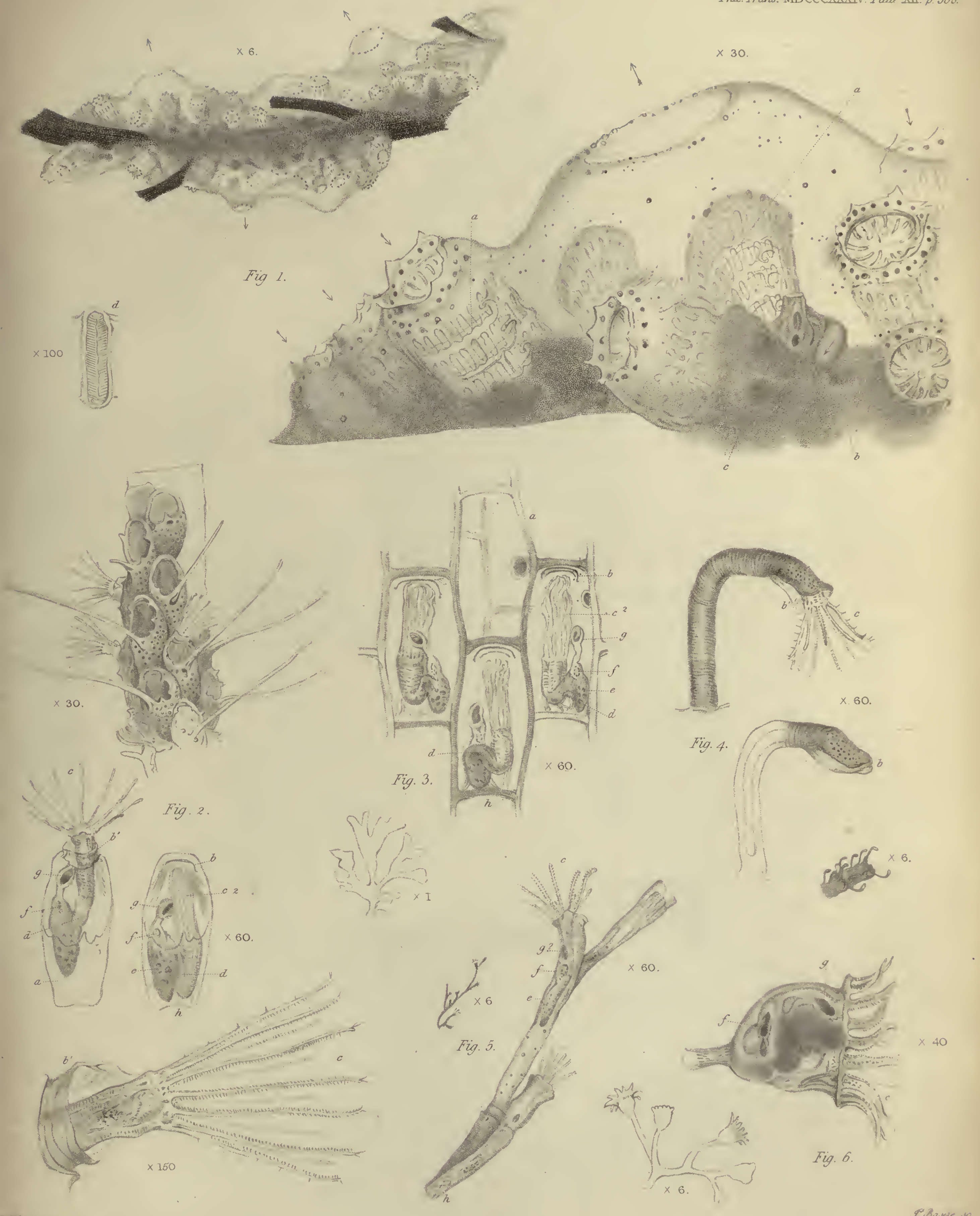


Fig. 7.

Fig. 5.

× 30

T. Foster del. J. Basire sc.



J. J. Lister del.

J. Basire sc.

Fig. 3.

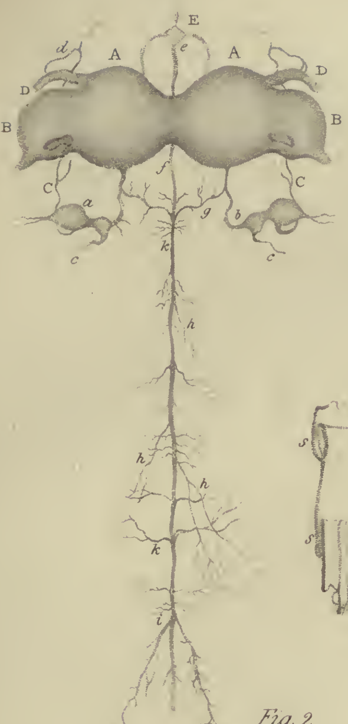


Fig. 1.

Fig. 4.

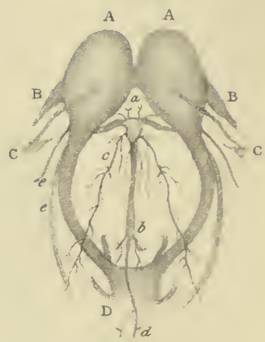


Fig. 5.

Fig. 2.

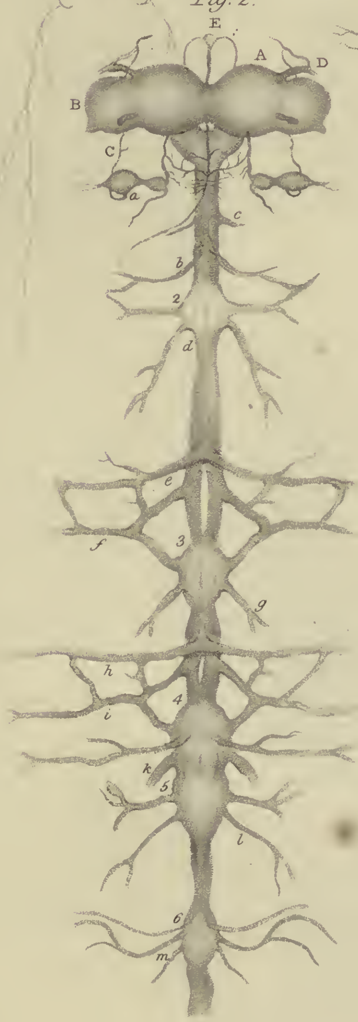


Fig. 6.

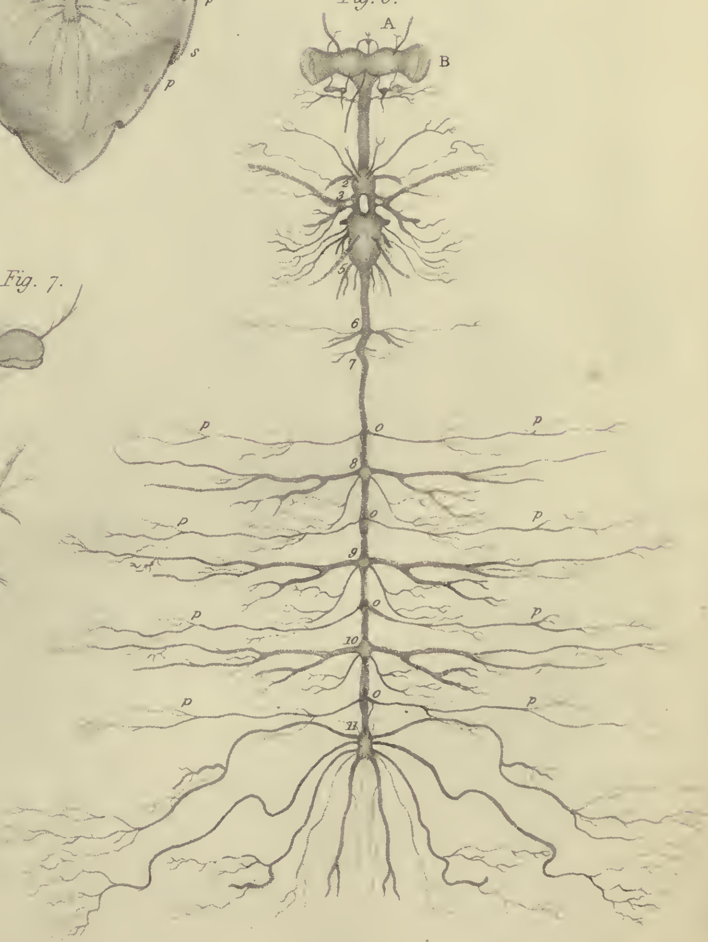
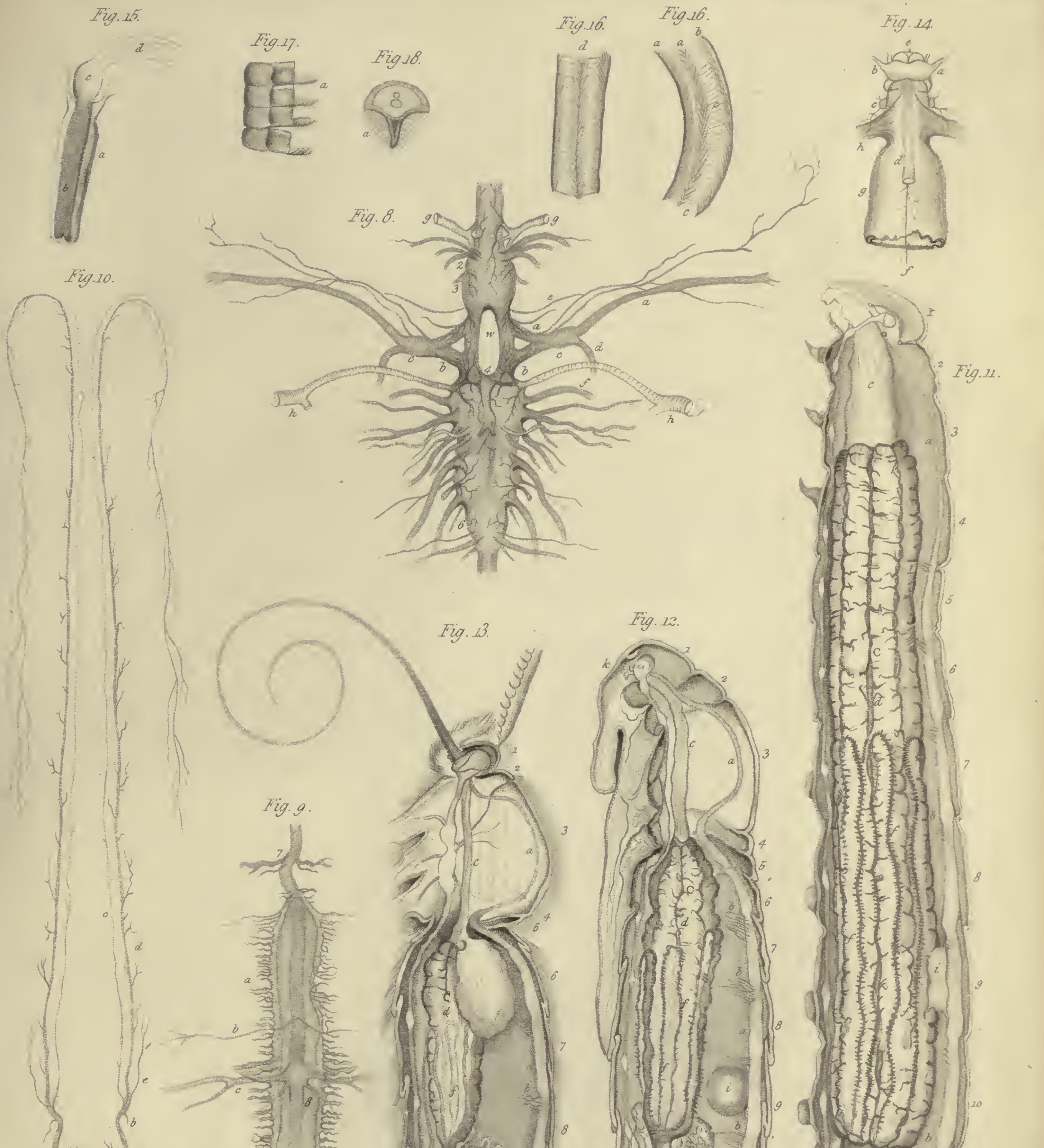


Fig. 7.

G. Newport. del. 1833.

Js Basire sc.



G. Newport del. 1833.

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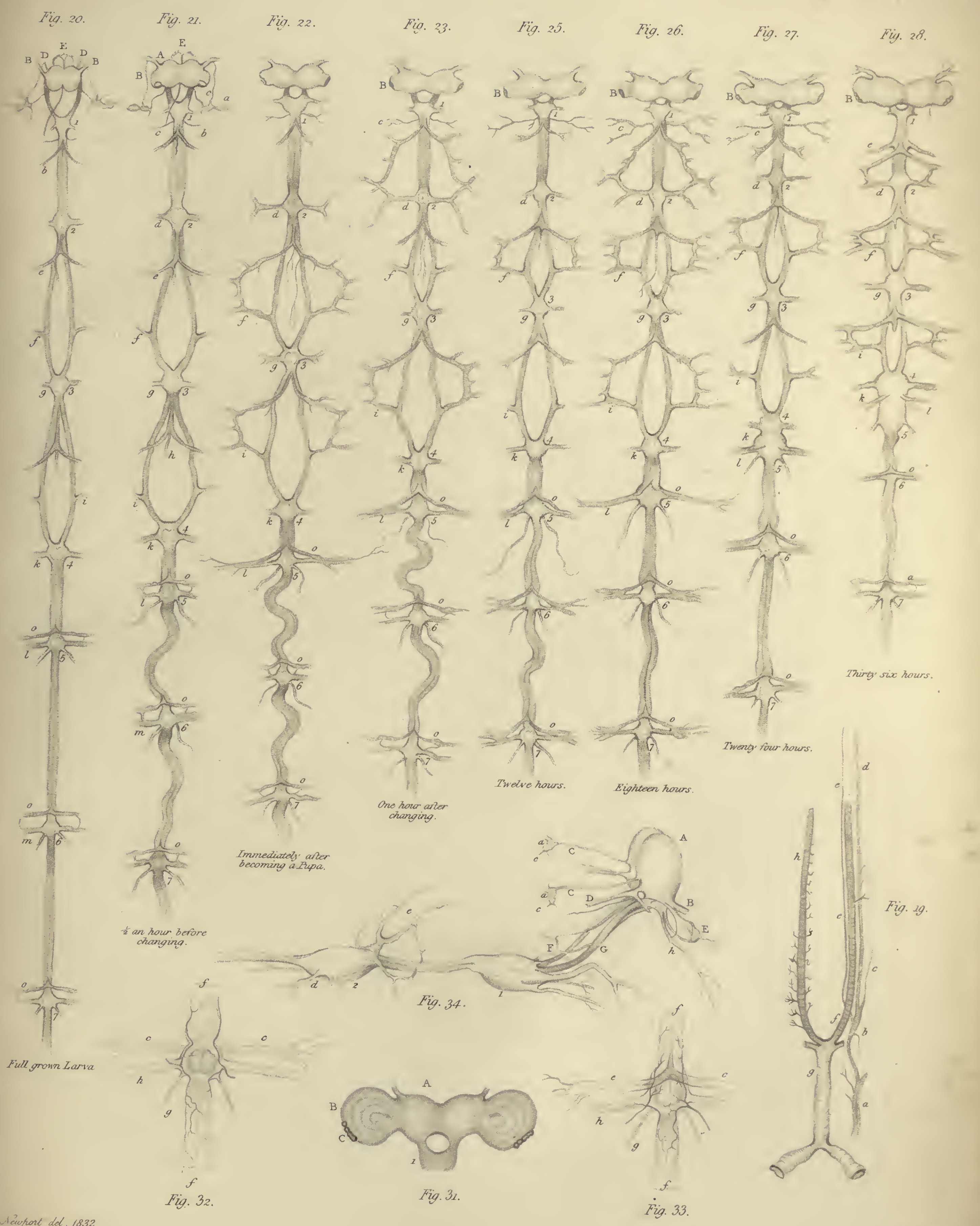
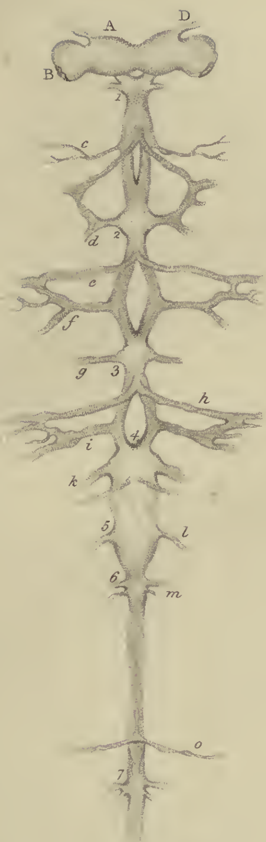


Newport del. 1832.

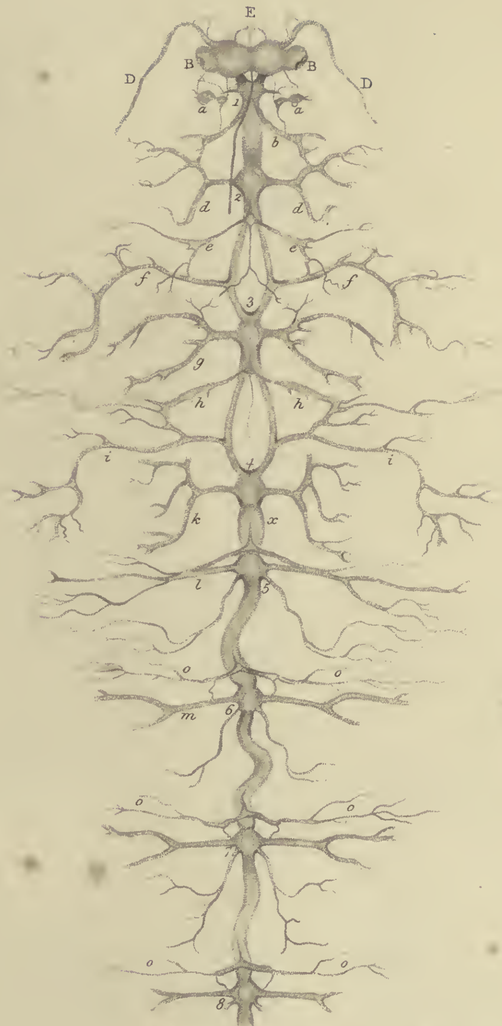
J. Basire sc.

Fig. 29.



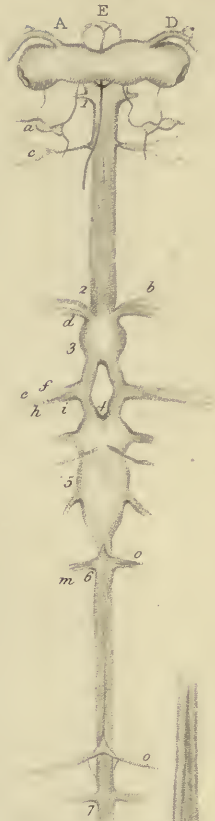
Forty eight hours.

Fig. 24.



Seven hours

Fig. 30.



Fifty eight hours

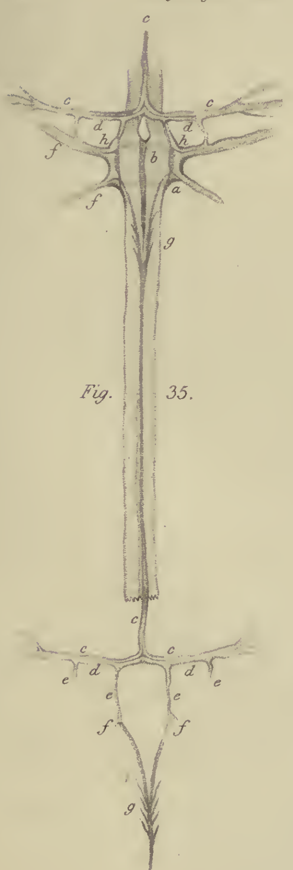


Fig. 35.

Fig. 37.

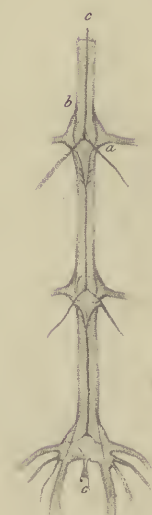


Fig. 39.

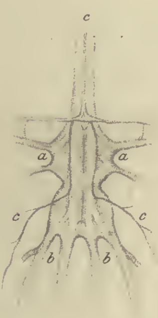


Fig. 36.

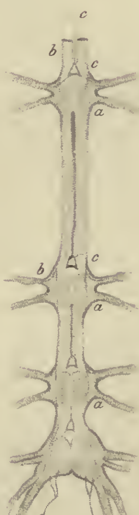


Fig. 38.