XV. Further considerations on the existence of a fourth imponderable

John William Draper M.D.

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XV. *Further considerations on the Existence of a Fourth Imponderable.* By JOHN WILLIAM DRAPER, M.D., Professor of Chemistry in the University of New York*.

IN the Philosophical Magazine for December 1842, I brought forward several facts which had caused me to form the opinion that the chemical rays of the older optical writers constitute in reality a new imponderable substance, which should be placed in the same rank with light, heat and electricity. To the views then given I propose, in this communication, to return again, and furnish further proof of their correctness. An extended examination, which has occupied me several years, has served to deepen my conviction of the truth of this doctrine.

Great changes in the fundamental theories of science ought not to be lightly admitted. It was only after many years of discussion and multitudes of experiments that the doctrine of the unity of air was destroyed, and the theory of the intrinsic differences of gaseous bodies received. This was unquestionably the most important event that ever happened to chemistry.

The imponderable principles are the true LIVING FORCES of chemistry. The circumstance that they do not exhibit the property of weight is only an incidental affair, and ought never to have been regarded as their leading characteristic; they are the regulating forces by which ponderable matter is arranged and grouped. If, then, so great a change occurred in chemistry on more exact views being obtained of its pneumatic department, what may not be expected from the discussions which are arising on the nature of its great controlling forces?

There is another point of view from which these investigations assume a deep interest. I have shown (Phil. Mag., Sept. 1843) that by resorting to prismatic analysis in physiological researches, very remarkable truths appear. The function of digestion, which is carried on during sunshine by the leaves of plants, is under the control of the yellow ray. It is this which causes the decomposition of carbonic acid, furnishes solid food, and gives the green colour. In animals similar results are produced by the agency of a nervous system; and not only so, but all the various operations connected with life are conducted in the same way. There is one class of nerves which gives action to the respiratory apparatus, and another which controls digestion. There is one class which presides over motion, another which is the recipient of sensation, a third which originates all the processes of thought and intellectuality. In the vegetable world the same idea is preserved, de-

* Communicated by the Author.

veloped perhaps in a less elaborate way, but under the guidance of a principle equally æthereal and refined. *The beams of the sun are the true NERVOUS PRINCIPLE of plants.* To the yellow ray is assigned their nutritive processes, to the blue their movements. We can therefore easily understand how it is that botanists who have sought in the interior of plants for indications of a nervous agent have never found them. That agent is external.

By the experiments that have been made for determining the nature of the chemical radiations, the question has been brought down to very narrow limits. There is no author who regards them as connected in any way with radiant heat, nor any, except the wildest speculator, who traces them to electricity. The difficulty is to offer clear and undoubted proof that they are distinct from light. Herschel has directly admitted this distinction and brought forward several experiments (Phil. Trans. 1840, p. 38, &c.) in support of this view. I have given some evidence of the kind, both recently and also several years ago (1837). All these experiments depend on a comparison of tithonographic stains produced by solar spectra that have undergone the action of absorptive media, and the effect of those spectra on the organ of vision; a comparison, in short, of different visible spectra and their tithonographic impressions.

From this comparison we endeavour to prove that invisible rays may be isolated in any part of the spectrum, and, if invisible, we argue that they are not light.

It cannot be concealed, however, that there is a certain degree of imperfection in this species of evidence: an accurate conclusion as to the presence or absence, or quantity of light, is by no means under these circumstances an easy affair. In these distorted spectra, as in the natural one, the terminations shade off gradually, and it is difficult to say where the light in reality ends. On these terminations also, where the light is so dilute and feeble, tithonographic action, although faint, may, by prolonged exposure, become not only perceptible but even prominent. In tithonographic action time enters as an element, in the act of vision it does not. A feeble gleam does not become more bright by constantly looking at it, but a sensitive surface exposed to such a gleam is more and more affected as the time is increased.

Considerations like these demonstrate the necessity of investigating the question in other ways, and more especially since M. Becquerel, one of the ablest writers on these matters, has undertaken to support a doctrine which denies the existence of the chemical rays and imputes the whole action to light (Taylor's *Scientific Memoirs*, vol. iii. part 12).

This doctrine, however, will not, I am persuaded, stand the test of criticism: there are facts, and those very imposing ones, which make it utterly untenable. It is my object in this paper to set forth that evidence and offer further and clearer proof of the physical independence of the tithonic rays and light, and indirectly establish the existence of a new imponderable.

The true issue of the question, as has been said, rests in proving a clear distinction between light and the tithonic rays; the other imponderables may be left out of the argument. The mechanical properties of the two agents are so closely alike, reflexion, refraction, polarization, interference, &c. taking place under the same laws for both, that the discussion necessarily becomes one of quantity and measure. Will a given ray of light, disturbed by the action of absorptive media, change its luminous and chemical relations *pari passu*? or can we alter the one and leave the other untouched? or, changing both by any process of treatment, do both change to the same extent?

The final decision of this question obviously rests in obtaining accurate measures for the rays of light and for the tithonic rays. It is the comparison of those measures which is to settle the point.

In this Journal (Phil. Mag., Dec. 1843) I have described an instrument, under the name of the tithonometer, which gives indications by the production of muriatic acid from the union of chlorine and hydrogen. This instrument is affected chiefly by the indigo rays, or more correctly speaking, by those rays which extend over the blue, indigo, and violet spaces of the spectrum, having their maximum in the indigo. It is important that the reader should keep this fact in mind.

Optical writers have been greatly embarrassed for want of a photometrical instrument which can measure the intensity of light: the chief difficulty in the way is the impossibility of contrasting together lights that differ in colour. By all it is admitted, that the eye is able to judge of the amount of illumination of white surfaces, or the depth of shadows within small limits of error, provided the rays compared are nearly of the same tint.

But in the discussion on which I am now entering this very difficulty is increased a hundred-fold. We are required to measure the intensity of light which has passed through all sorts of absorbent media, and therefore has become excessively coloured. How shall we compare together the rays which have gone through sulphocyanate of iron, and are of a deep blood-red, with those that have passed through sulphate of copper and are of a bright blue?

Nevertheless this problem is capable of a complete solution,

and a photometer can be obtained which gives results comparable with those of the tithonometer: such an instrument I have constructed; it is exceedingly simple, as the following considerations prove.

We are to remember that the tithonometer gives indications which are expressive of the intensity of the blue rays generally; the blue tithonic rays are the rays which it measures. In using the term *blue*, it will be understood to comprehend the blue, indigo and violet, or the more refrangible rays generally. It is obvious, therefore, that the photometer which is to be used with it must measure the same blue rays, or in other words, the tithonometer and the photometer must be affected by rays comprehended between the same limits of refrangibility.

This can be effected by interposing in the photometer some absorbent medium which will admit no rays to pass it, except such as are in the limits of refrangibility with which the tithonometer is engaged. It is fortunate, as I have found, that such a medium occurs in a solution of sulphate of copper and ammonia.

Let a wooden box, six inches long, two wide and two deep, be provided; in the centre of its top an aperture three quarters of an inch in diameter is to be made; the box must be blackened interiorly, and a rectangular prism of wood be placed in the box, with its right angle in such a position that its edge bisects as a diameter the circular aperture: over this wooden prism a piece of clean white paper should be pasted, care being taken that where it bends over the right angle of the prism it is folded sharp. So far the reader will recognise in this Ritchie's photometer, as described in the *Annals of Philosophy*. Upon the aperture in the top of the box a glass trough is placed; it is made by drilling a circular hole, an inch in diameter, in a piece of plate glass one-third of an inch thick, and then placing on each side of it a thin piece of plate glass. This forms a circular trough, in which a strong solution of sulphate of copper and ammonia may be inclosed; over the trough a conical tube six or eight inches long is placed, so that the eye may see distinctly through the aperture in the top of the box the disc of paper, and more especially its dividing diameter.

Two small lamps are then prepared, of such dimensions that when set opposite the open ends of the box their rays may illuminate the paper; they are supposed to be adjusted so as to shine with equal intensity.

On looking through the tube a circle of blue light is seen, and, if the lamps are shining equally, its two halves are equally bright. At the commencement of every experiment this pre-

liminary observation should be made, and, if necessary, the proper adjustments secured.

Suppose now it were required to know how much blue light is transmitted by a given solution. A trough is to be provided, which may be formed by drilling a hole two and a half inches in diameter through a thick piece of plate glass; on each side of this a thin piece of similar glass is laid, the trough having been filled with the solution under investigation. Troughs made in this manner never leak; they completely answer their purpose, and are easily washed and refilled.

Let the substance under trial be a concentrated solution of bichromate of potash. Having adjusted the lamps and filled the trough, set it before one of them, supporting it by a proper foot. On looking through the tube of the photometer, if the absorbent cell of the copper solution has been previously removed, the circle of light will be seen very brightly illuminated; one of its halves of a yellow tint, but not much less luminous than the other. The copper cell being now restored to its place, on looking again through the tube there is a striking contrast, one half of the circle is of a bright blue, but the other seems totally black: with solutions which cut off the blue rays less perfectly this blackness is of course less intense; in these cases the lamps are to be moved into such positions that the two halves of the circle are equally illuminated, and its dividing diameter invisible. As the eye is not disturbed with any difference of colour, the observation can be made within small limits of error. The calculation of the relative intensities can then be made by the common photometrical law.

As this photometer is affected by rays of the same refrangibility as those which affect the tithonometer, it is clear that if it be the rays of light which are operative in the union of chlorine and hydrogen, the results given by the two instruments should correspond within certain small limits of error.

With respect to the tithonometer, I have improved this instrument considerably; by shading it with a glass case, which cuts off thermometric disturbance; by taking the observations through a small telescope, which avoids parallax; by having the scale moveable so as to slide along the tube; by making one charge of gas last for a great number of experiments; by completing the tithonization before commencing; by altering the position of the adjusting wire, so as to bring it nearly down to the end of the tube. The detail of these changes would, however, detain me now too long; they will be given at some suitable time hereafter.

I may, however, record as a striking fact, that so great is the sensitiveness of chlorine and hydrogen, that a mixture will actually explode by the rays of an electric spark. In this

Journal (Dec. 1843, p. 402) I have already stated that silent combination would take place under these circumstances, but more recently I have had instruments repeatedly destroyed by explosions resulting in that way.

It being understood that the indications of the photometer and the tithonometer correspond, that they are affected by rays of the same refrangibility, we are enabled to proceed to the direct solution of the question and the determination of M. Becquerel's hypothesis.

A transparent medium, which absorbs to a greater or lesser extent the more refrangible rays, being selected, it is required to determine whether, when a given ray has passed through it, the chemical effect diminishes as the intensity of the more refrangible rays of light diminishes. According to M. Becquerel, the effect should be in direct proportion to the quantity of light; if, for example, the ray lost one half of its blue light, the chemical effect should diminish one half, &c.

We require, therefore, two observations: first with the photometer, to ascertain how much of the light escapes the absorptive action of the medium under trial; second, with the tithonometer, to ascertain what quantity of the tithonic rays escapes absorption. If the whole effect is due to light, the two observations should give the same result.

Before giving the results, which I have obtained in a tabular form, in order that I may be clearly understood, I will give a particular example. I took some naphtha, the colour of which was slightly yellow, and placing it in the glass trough before described, proceeded to determine its relation for the more refrangible rays of light. This was done by adjusting the two lamps of the photometer till they coincided, then interposing the trough between one of the lamps and the end of the photometer. On looking through the tube a great diminution of the intensity of the light on the corresponding semicircle of paper was observed; the other lamp was now removed until the paper disc was uniformly illuminated; the distance of the two lamps from the centre of the box was now measured, they were respectively twelve and fifteen inches; but the intensity of the light is proportional to the square of the distance.

(1.) For the light in the unobstructed beam 225 or 100.

(2.) For the light in the absorbed beam ... 144 or 64.

Supposing now that the value of the unobstructed beam be represented by 100, and we calculate the amount of light which passes through the naphtha, we find it is represented by 64. Consequently, of every hundred rays of blue light which fell upon this naphtha, sixty-four escaped absorption.

The naphtha trough was now carried to the tithonometer; first, it was determined how many seconds it took a given

beam of light, coming from an Argand lamp, burning steadily, to move the index through one division.

(3.) For the tithonic ray of the unobstructed beam 31^s .

The trough was then interposed in the column of light, and the number of seconds required to make the index move through one division determined.

(4.) For the tithonic ray in the absorbed beam ... 65^s .

But as the lamp might have varied in intensity, or the tithonometer in sensitiveness, the first observation was repeated; it gave

(5.) For the tithonic ray of the unobstructed beam 31^s .

This process of repetition was uniformly resorted to, and the mean of the two taken. It may be proper to remark, that there was rarely any perceptible difference.

It follows, that a ray which could effect the union of a given quantity of chlorine and hydrogen in 31 seconds, required after passing through the naphtha 65 seconds.

Calculating on these principles how many of the tithonic rays passed through the naphtha, as before, we find,

(6.) For the tithonic rays in the unobstructed beam 100.

(7.) For the tithonic rays in the absorbed beam ... 48.

Now, comparing this result (6.) and (7.) with the result (1.) and (2.) for light, we find that the absorptive action of naphtha of a slightly yellow tint is very much greater for the tithonic rays than for the luminous rays.

Consequently it follows, that it is not the light comprehended between the extreme blue and extreme violet rays which brings about the union of chlorine and hydrogen, but another and invisible class of rays, which is absorbed by naphtha under a different law for that for its action on light.

The foregoing example shows the mode by which I have obtained the results of the following table:—

Table showing the absorptive action of certain media for the luminous and tithonic rays.

Name.	Number of rays which escaped absorption.	
	Tithonic rays.	Luminous rays.
Naphtha.....	48	64
Sulphocyanate of iron ...	66	58
Nitrate of iron	53	64
Red prussiate of potash...	12	15
Turpentine.....	20	38
Copaiva balsam.....	24	42
Iodide of starch	40	33
Stevens's blue ink.....	30	33
Litmus water.....	16	23

From this table, therefore, we gather that the chemical effect produced by a given ray has no relation to the quantity of light which is in it; that a satisfactory explanation of the phænomena can only be given by assuming the existence and presence of another agent besides the light, and to which agent the chemical effect is due; that media are known, which in their absorptive action bear relations which are totally different for these two agents; and, finally, that, as prismatic analysis has also previously shown, no explanation can be given of these results by imputing them to the agency of light, we are forced to admit the existence of another imponderable principle, the same as that which passes in these papers under the name of tithonic rays.

In addition to the results obtained from the foregoing quantitative experiments, there are other phænomena of a very novel and interesting kind, from which we may draw an argument of overwhelming force. The discussion of these I shall now take up.

Early during the last century the remarkable appearance of phosphorescence, excited in the Bolognean stone and calcined oyster-shells, attracted the attention of chemists. Dufay, in France, wrote several papers upon it; and the experiments of Wilson in England are perhaps as fine a specimen of philosophical investigation as those early times can furnish. These results, which few are now acquainted with, deserve to be republished.

To Becquerel we are indebted for one of the most remarkable discoveries in connexion with this subject. He found that the rays of an electric spark, which had passed through glass, no longer preserved the quality of exciting phosphorescence; but when they passed through quartz, they retained that power unimpaired. This result is very strikingly shown by placing a piece of colourless glass and a piece of quartz on a surface covered with sulphuret of lime (oyster-shells calcined with sulphur), and discharging a Leyden phial a little distance off. The sulphuret will glow as brilliantly on the part covered by the quartz as on the uncovered spaces, but under the glass it will remain dark.

Nevertheless, this same sulphuret, carried into the sunshine, phosphoresces powerfully *under glass*, apparently showing that there is a difference between the phosphorogenic emanation of the sun, for thus M. Becquerel terms it, and that of an electric spark.

On this radiation, as it comes from the sun, M. Becquerel has treated in his paper, of which a translation is given in

Taylor's Scientific Memoirs (vol. iii. part 12). He determines the place of the phosphorogenic rays in the spectrum, after they have passed through a glass prism, and shows that the fixed lines which occur in the chemical spectrum, occur also among these phosphorogenic rays.

In passing, I may mention that at the time I published my account of these fixed lines (Phil. Mag., May 1843), I had no idea that any other chemist had seen them. It of course soon appeared that M. Becquerel had some months previously given an account of them to the French Academy. My result was wholly independent and without any knowledge of his. On comparing the two papers, it will be seen that there is a strong coincidence, not only in the manner of the experiment, but even in the very phrases of description. It is this which has drawn these passing remarks from me. Men who are pursuing the same object, and using the same resources, will employ even words that are alike, though they speak languages that are different and live thousands of miles apart.

On examining the plate given in M. Becquerel's paper, I was struck with the close resemblance between the phosphorogenic spectrum and that tithonographic spectrum on iodide of silver of which Sir J. Herschel has given an elaborate account (Phil. Mag., Feb. 1843). As far as the eye could judge they seemed perfectly alike; the tithonographic spectrum in question was obtained by me in Virginia. This coincidence was so striking that it appeared almost certain that the phosphorogenic emanations of Becquerel were the same as my tithonic rays: there was the upper spectrum commencing at the line G, and going beyond the furthest confines of the violet, exerting a positive action; there was also the lower spectrum, commencing at the line F and going below the red ray, and exerting a negative action; a phænomenon absolutely the same as that traced on the Daguerreotype plate.

The phosphorogenic rays that come from the sun have the same place in the spectrum, or are dispersed by the prism exactly in the same way as the tithonic rays. To all appearance these may be expressions for the same agent.

We must remember, however, that the phosphorogenic rays of the sun differ from those of an electric spark. Glass to the former is transparent, to the latter it is not.

Before, therefore, I can carry this argument to the point on which I design it to bear, it is necessary to ascertain the index of refraction of the rays of an electric spark to which glass is impervious.

This I proceeded to determine in the following way:—At the distance of six inches from the terminations of two blunt

wires, between which the spark from a Leyden phial was caused to pass, I placed a lens of quartz, the focus of which for parallel rays was six inches, and then intercepted the resulting beam by a diaphragm with a circular aperture in it, one-third of an inch in diameter. I had caused an equiangular prism of quartz to be cut and polished from a large and perfectly faultless rock crystal; it was cut transverse to the axis. This prism I placed in such a way that in dispersing the beam that came through the circular aperture I got rid of double refraction and obtained only one spectrum; this was received on a metal plate, which, having been washed over with gum water, and sulphuret of lime dusted on it, offered an uniform phosphorescent surface, which might be set in a vertical plane. *When the spark passed I saw that the plate was phosphorescing on those portions where the more refrangible rays had fallen.*

But the transient light of a Leyden spark did not last long enough, nor was the phosphorescence it produced powerful enough to enable me to conduct the experiment in a way entirely satisfactory. I resorted, therefore, to the brilliant light which is obtained when a piece of metal, or what is far better, the hard variety of carbon which is obtained from the interior of gas retorts, is lowered upon mercury entirely filling a very small open porcelain cup, and the continuous discharge of a voltaic battery passed. The battery used contained fifty pairs of Grove's cells, but a smaller number would probably have been amply sufficient. All the remainder of the arrangement was as just described.

As soon as the light was emitted, I marked on the sulphuret of lime the beginning of the red, the centre of the yellow, and the termination of the visible violet ray. Then, stopping the current, I examined on what parts the plate was phosphorescing. The commencement of the glow was between the indigo and the blue; towards the blue it extended far beyond the visible boundaries of the spectrum; I could not see any divisions or points of maxima on it. The surface of the plate shone all over, except in the region of the less refrangible rays, and there were the traces of the negative action which M. Becquerel has so well illustrated in the case of the solar emanations; rays which, however, were first observed in the last century.

It is necessary to remark, that the rays from the voltaic discharge resemble those from an electric spark in their inability to traverse glass. On this observation all the value of the foregoing experiment depends.

But it can nevertheless be easily proved, that although glass is impervious to the phosphorogenic emanation coming from

the voltaic deflagration of any metallic bodies, the observation applies to transient discharges only. A voltaic light, which lasts but a moment, fails to cause phosphorescence through glass in the same way that an electric spark does; but if the discharge is continued, the surface presently begins to glow; and if maintained for several minutes, it shines as brightly as though a piece of quartz had been used.

The inability of an electric spark to cause phosphorescence is connected with its transient duration. The voltaic light enables us at pleasure to imitate the effects of an electric spark or those of the sun.

The phosphorogenic rays, whether they originate in an electric spark or from the sun, occupying thus the same place in the spectrum, and even exhibiting the same peculiarities as the tithonic rays on iodide of silver, we have next to determine whether this is an apparent or a positive identity.

Prof. Henry of Princeton read a paper before the American Philosophical Society in May 1843, in which he discussed all the leading mechanical properties of the phosphorogenic rays, and among other important experiments made some with a view of determining this particular question. A Daguerreotype plate and some sulphuret of lime were simultaneously exposed to the sky; the plate was stained, but no effect was produced on the lime. A Daguerreotype plate and some sulphuret of lime were exposed to the light of an electric spark; the lime was observed to glow, but no impression was produced on the plate. When the plate was exposed to a succession of sparks for ten minutes, with a sheet of mica interposed, an impression was made. Lime exposed to the moon did not phosphoresce, but a sensitive plate under the circumstances is said to be stained. In view of these different facts, Prof. H. observes, "These experiments, although not sufficiently extensive, appear to indicate that the phosphorogenic emanation is distinct from the chemical, and that it exists in a much greater quantity in the electric spark than either the luminous or chemical radiation."

From Wilson's experiments, it appears that he was aware that when the phosphorescent surface is warmed, so as to hasten the disengagement of light, the moonbeams may be found to have left traces of action upon it; feeble it is true, but nevertheless very apparent. We have seen also that the peculiarity of an electric spark is due to its transient duration. Before therefore a final decision can be obtained on this point, we are required to examine the effect of the tithonic rays and phosphorogenic emanation, under circumstances which are precisely similar as to intensity and time.

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For the transient rays of an electric spark, quartz is transparent and glass is nearly opaque. Having prepared a bromo-iodized silver plate, so as to be exceedingly sensitive, I set in front of it, at the distance of about one-third of an inch, a disc of quartz and one of crown glass, of equal thickness; and between a pair of copper wires, the interval of which was three-eighths of an inch, I passed the spark of a Leyden phial fifteen times; the distance between this spark and the sensitive plate was about two inches. On mercurializing, the plate was deeply whitened all over, equally so through the glass, through the quartz, and on the uncovered spaces; but a spot of sealing-wax, which I had put on the glass, left its shadow on the plate beautifully depicted; as were also the edges of the glass and the quartz. The two discs overlapped one another to a certain extent, but the corresponding portion of the silver plate was as deeply stained there as anywhere else.

Next I put a surface of sulphuret of lime in the place of the Daguerreotype plate, everything remaining as before. On passing fifteen sparks the lime phosphoresced powerfully under the quartz, but not under the glass, so that the difference between its shadow and that of the spot of wax could not be distinctly seen.

For these reasons therefore I adopt the view expressed by Prof. Henry, that the phosphorogenic emanation and the tithonic rays are distinct. Under the same circumstances, glass to the one is transparent, to the other it is opaque.

Now upon what sort of evidence is it that M. Melloni is universally admitted to have established the physical independence of light and heat? was it not by showing that rock-salt is perfectly transparent to calorific rays, that glass is much less so, that Rochelle salt, alum, and sulphate of copper are almost opaque? It is surely impossible to confound the phosphorogenic emanation of an electric spark with its rays of light; the latter pass perfectly through glass, the former do not. So far as the eye can distinguish, an electric spark, the rays of which have passed through glass, differs in no respect from one the rays of which are received directly into the eye. If we consider the constitution of such a ray, previous to and after its passage through glass, the eye can discover no difference; but, as respects the phosphorogenic emanation, there was something existing in that ray at the first of these epochs which had ceased to exist in it at the second; a something not having the quality of communicating any impression to the organ of vision; and that which we cannot see, surely no man will acknowledge to be light.

But the reader may inquire, what has all this discussion of

the characters of the phosphorogenic emanation to do with the existence of the tithonic rays as a fourth imponderable? A few words will show. From these considerations and experiments we have arrived at the conclusion, that there exist in the beams of an electric spark *invisible rays, which are therefore totally distinct from light*. They occupy the same spectrum region as the tithonic rays which decompose iodide of silver; their leading character is, that glass which is transparent to the rays of light is opaque to them.

But the admission of this fact breaks down at once the doctrine of a trinity of imponderables, and compels us to enlarge our list of those living forces of chemistry. The great obstacle which is in the way of admitting the tithonic rays as a fourth imponderable, is in the circumstance that it would impress a very serious change on that science, and apparently afford an argument of weight against the mathematical theory of light. I believe that some great generalization will hereafter prove that all these imponderables are modifications of one primordial principle. I also believe that some capital experiment will hereafter show that the forty different metals we are acquainted with are merely modifications of one or two more simple forms; but these are things that we are unable to deal with now; and viewing the experiments which have been made in the last few years, not as mathematicians, but as chemists, all men must acknowledge that our prevailing doctrines of the nature and number of the imponderables are liable, before long, to undergo a very serious modification.

The admission that the phosphorogenic emanation and light are principles differing from the tithonic rays and from each other, relieves us of much difficulty in increasing our list of imponderables. If these principles differ thus intrinsically from one another, the question comes home to us—what are they? If electricity and heat and light are three recognised imponderables, are not the tithonic rays a fourth, and the phosphorogenic emanation a fifth?

In view of this, I would suggest the propriety of ceasing to call these last by the epithet of *emanations*, and of giving them the more appropriate name of PHOSPHOROGENIC RAYS.

And now what appears to become of M. Becquerel's hypothesis, that all the different effects we have been considering are due to light, and are presented to us under different aspects because everything depends on the nature of the receiving surface; that it is the same principle which affects the eye as light, decomposes chloride of silver as a tithonic ray, and makes sulphuret of lime shine as a phosphorogenic ray; that the difference is not in the radiant principle, but in the surface

on which it is received? To go no further in a discussion which has already extended this paper too much, if the agent is the same in all cases, and the difference perceived is due to the receiving surface, how is it that a ray of light which has passed through a piece of transparent glass can no longer excite phosphorescence in the sulphuret of lime? Can we escape the conclusion that the ray has had something removed from it, or has had some modification impressed on it, or, in short, that something invisible to the eye has been taken away?

To my mind these considerations are conclusive, *and I therefore regard the tithonic rays as constituting a fourth imponderable, and the phosphorogenic rays as a fifth.*

University of New York,
April 12, 1844.

XVI. *Remarks on the Review of a work entitled, "An Inquiry into the Nature of the Simple Bodies of Chemistry," inserted in the Philosophical Magazine for April 1844. By DAVID LOW, Esq., F.R.S.E., Professor of Agriculture in the University of Edinburgh.*

To Sir David Brewster, K.H., &c. &c. &c.

DEAR SIR,

ALTHOUGH I have not the pleasure of a personal acquaintance with you, I will venture to address you as if I had, on the subject on which I am now to trouble you.

I send you a little work published about six months ago, together with the April Number of the Philosophical Magazine, in which a criticism on the work appears. I call it a criticism, but you will perceive that it is nothing but an attack, and a very virulent one, on the author.

An opinion entertained by many chemists, it is well known, is, that the simple bodies of chemistry, so called, are not really simple, or that they can only be termed simple with relation to the present state of our knowledge and our means of analysis. I have ventured to enunciate the proposition somewhat more precisely, and endeavoured to prove that these bodies cannot be separated in their nature and functions from the great class of bodies which we know to be compound. Thus chlorine cannot be separated as one of a distinct order of natural bodies from cyanogen, with which it exhibits the closest parallelism in its chemical actions and relations.

To establish and illustrate my argument, I have ventured to inquire whether we cannot reasonably suppose the simple bodies, so called, to be resolvable into other bodies of the same class. I assume, in the first place, that all these bodies may be resolved into three, hydrogen, carbon and oxygen,