assumed. For this purpose two equal threads stretched vertically by weights might be used, and a plane mirror without edge be so adjusted that the reflected piece of the one thread falls in a straight line with the directly seen parts of the other in each position of the eye.

After the position of the image produced by a plane mirror has thus been fixed, the discovery of the position of other subjective pictures will be possible by means of a transparent and at the same time reflecting plane-parallel plate on which a luminous point is reflected. The plate is to be so arranged that the reflected picture of the luminous point covers a point of the image in question. An assistant must move the luminous point until the reflected and refracted image no longer move towards each other in any motion of the eye. The directly measured distance of the reflected point from the plane-parallel plate is equal to the required distance of the subjective image produced by the optical apparatus from the same plate.—Poggendorff's *Annalen*, vol. cxxiii. p. 655.

ON A SIMPLIFIED METHOD OF EXTRACTING INDIUM FROM THE FREIBERG ZINCBLENDES. BY M. WESELSKY.

The roasted and levigated blendes are treated with a mixture of ten parts of hydrochloric and one of nitric acids; the solution, separated from silica and the liberated sulphur, is greatly diluted with water, and carbonate of soda added until a precipitate first begins to form. The solution is boiled, hyposulphite of soda being added until no more sulphurous acid escapes, and the precipitate, which at first is yellowish and flocculent, has become black, when it readily settles down. The solution contains, besides all the iron and zinc, small quantities of arsenic and copper, and also part of the indium. The black precipitate consists of the sulphur-compounds of arsenic, lead, copper, &c., and contains the rest of the indium. Without removing it, freshly-precipitated carbonate of baryta in excess is added to the liquid when it is cold, and the whole allowed to stand for twelve hours. The precipitate, which, besides the above sulphides, contains all the indium and the excess of carbonate of baryta, is well washed, the air being excluded, and is then treated with dilute hydrochloric acid. In this way the carbonate of baryta and the indium are dissolved. To remove a small quantity of sulphides which pass into solution, sulphuretted hydrogen is passed into the acid solution; and baryta is removed by sulphuric acid. Oxide of indium is separated from any possibly adhering oxides of iron or zinc by means of carbonate of baryta.

From experiments with which M. Weselsky is at present occupied, it appears that, under suitable circumstances, indium may be completely precipitated by hyposulphite of soda, by which the application of carbonate of baryta is quite avoided.—*Bulletin der Akademie in Wien*, vol. vii. p. 1869.

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XLVI. *Supplementary Considerations relating to the Undulatory Theory of Light.* *By* Professor CHALLIS, *F.R.S., F.R.A.S.**

ON a review of the arguments by which I have now for a long time maintained that the phenomena of light are referable to the vibrations and pressures, as mathematically determined, of a continuous elastic fluid the pressure of which varies proportionally to its density, I have found that there are certain points of the reasoning which require rectification or confirmation. To discuss these points is the object of the present communication.

(1) By pure reasoning, founded on admitted principles, I have ascertained that the vibratory motion of the supposed elastic fluid is *composite* independently of particular modes of disturbance, and that each component consists of vibrations partly parallel and partly transverse to an *axis*. The former of these results is at once applicable in accounting for the *composition* of light as indicated by prismatic analysis, and the other in the explanation of facts of *polarization*. As the reasoning also showed, independently of arbitrary disturbances, that for small vibrations $udx+vdy+wdz$ is an exact differential, the motion relative, as above stated, to an axis is analytically expressed by the equation

$$(d\,.f\phi)=udx+vdy+wdz,$$

f being a function of x and y only, ϕ a function of z and t only, and the axis of the motion coinciding with the axis of z. In fact this equation gives

$$u=\phi\frac{df}{dx},\quad v=\phi\frac{df}{dy},\quad w=f\frac{d\phi}{dz};$$

* Communicated by the Author.

and we may assume f to be such that, where $x=0$ and $y=0$,

$$f=1, \quad \frac{df}{dx}=0, \quad \frac{df}{dy}=0.$$

The details of the reasoning here referred to are given in the proof of Proposition X. contained in an Article on the Principles of Hydrodynamics in the Philosophical Magazine for December 1852. In the same Article are investigated exact expressions for the functions f and ϕ, and the rate of propagation of the motion along the axis is found to be $a\sqrt{1+\frac{e\lambda^2}{\pi^2}}$, a and λ having the usual significations, and e being a constant such that, where $x=0$ and $y=0$,

$$\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}+4e=0.$$

As e is necessarily a positive quantity, it follows that the rate of propagation, *as determined by hydrodynamics*, is greater than a. It is further evident, putting κa for the rate, that if κ be a numerical constant, its value should be determinable exclusively on hydrodynamical principles. This is what I have attempted to do in a communication to the Philosophical Magazine for February 1853; but having recently discovered that the mathematical reasoning there given requires correction, I propose now to enter upon the discussion of this point.

The determination of the constant e depends on the integration of the equation

$$\frac{d^2f}{dr^2}+\frac{df}{rdr}+4ef=0,$$

r being any distance from the axis of motion. The integral is not obtainable in a finite form, but it may readily be shown that the following series for f satisfies it, viz.

$$f=1-er^2+\frac{e^2r^4}{1^2.2^2}-\frac{e^3r^6}{1^2.2^2.3^2}+\&c.$$

For finding e it is required to ascertain the large values of r that make f vanish. This problem is solved by Sir W. Hamilton in a memoir on Fluctuating Functions in the Transactions of the Royal Irish Academy (vol. xix. p. 313), and by Professor Stokes in the Transactions of the Cambridge Philosophical Society (vol. ix. part 1. p. 182). I hesitated to accept the equation (52) in the latter memoir, because it contains quantities R and S representing series that are convergent for a certain number of terms and then become divergent, which yet are employed as if they were wholly convergent. Whatever be the answer to this objection, it is certain that the equation obtained by Professor Stokes for large values of r, viz.

$$f=(2\pi r\sqrt{e})^{-\frac{1}{2}}(\cos 2\sqrt{e}r+\sin 2\sqrt{e}r),$$

approaches the true integral in proportion as r is larger. In fact this equation is the exact integral of the differential equation

$$\frac{d^2f}{dr^2}+\frac{df}{rdr}+4ef=\frac{f}{r^2},$$

which evidently differs less from the foregoing differential equation as r is larger. Hence it appears that the consecutive large values of r which cause f to vanish, increase by the common difference $\frac{\pi}{2\sqrt{e}}$. By peculiar reasoning applied to the infinite roots of the equation $f=0$ (Phil. Mag. for February 1853), I found the common difference to be ultimately $\frac{1}{\sqrt{e}}$. But from the preceding argument it must be concluded that that reasoning is not legitimate, and that some error is involved in the treatment of the infinite roots. It will therefore be necessary to determine the rate of propagation by a new investigation, employing for the purpose the above expression for f. This I proceed to do by a course of reasoning analogous to that which was followed in the previous investigation.

In the article already cited, containing Proposition X., the following equations to the first approximation are obtained:—

$$\phi=m\cos\frac{2\pi}{\lambda}(z-\kappa at+c), \quad \kappa=\sqrt{1+\frac{e\lambda^2}{\pi^2}}, \quad a^2\sigma+f\frac{d\phi}{dt}=0$$

Also if w and ω be respectively the velocities parallel and transverse to the axis of z, we have $w=f\frac{d\phi}{dz}$ and $\omega=\phi\frac{df}{dr}$. The foregoing expression for f may be put under the form

$$(4\pi r\sqrt{e})^{-\frac{1}{2}}\cos\left(2\sqrt{e}r-\frac{\pi}{4}\right);$$

and as r is assumed to be very large, it may be supposed to have the constant value r_0 outside the cosine, and the general value r_0+h under the cosine, h being always very small compared to r_0. Then putting, for brevity, f_0 for the constant coefficient, and c' for $2\sqrt{e}\,r_0-\frac{\pi}{4}$, we shall have

$$f=f_0\cos(2\sqrt{e}h+c'), \quad \frac{df}{dr}=-2\sqrt{e}f_0\sin(2\sqrt{e}h+c').$$

Z 2

Consequently

$$w = -\frac{2\pi m f_0}{\lambda} \cos(2\sqrt{e}h + c') \sin\frac{2\pi}{\lambda}(z - \kappa a t + c),$$

$$\omega = -2m\sqrt{e} f_0 \sin(2\sqrt{e}h + c') \cos\frac{2\pi}{\lambda}(z - \kappa a t + c),$$

$$\sigma = -\frac{2\pi m \kappa f_0}{\lambda a} \cos(2\sqrt{e}h + c') \sin\frac{2\pi}{\lambda}(z - \kappa a t + c).$$

Hence it follows that $w = \frac{a\sigma}{\kappa}$. Suppose now another series of waves, exactly equal to the first, to be propagated in the contrary direction; and let z_0 be the coordinate of a position at which the velocity parallel to the axis is constantly zero. Then if in general $z = z_0 + l$, and if w', ω', and σ' be respectively the resulting velocities and condensation, we shall have

$$w' = -\frac{2\pi m f_0}{\lambda} \cos(2\sqrt{e}h + c')\left(\sin\frac{2\pi}{\lambda}(l - \kappa a t) + \sin\frac{2\pi}{\lambda}(l + \kappa a t)\right),$$

$$\omega' = -2m\sqrt{e} f_0 \sin(2\sqrt{e}h + c')\left(\cos\frac{2\pi}{\lambda}(l - \kappa a t) + \cos\frac{2\pi}{\lambda}(l + \kappa a t)\right),$$

$$\sigma' = -\frac{2\pi m k f_0}{\lambda a} \cos(2\sqrt{e}h + c')\left(\sin\frac{2\pi}{\lambda}(l - \kappa a t) - \sin\frac{2\pi}{\lambda}(l + \kappa a t)\right).$$

From the first and second of these equations it appears that

$$\frac{\omega'}{w'} = \frac{\sqrt{e}\lambda}{\pi} \cdot \tan(2\sqrt{e}h + c') \cot\frac{2\pi l}{\lambda}.$$

Let now the distance r_0 apply to positions at which the transverse velocity is always zero; and in order to get rid of the negative signs, and to avoid double signs, let $c' = (2n+1)\pi$. Then supposing h and l to represent very small *equal* distances from a point (r_0, z_0), where the velocity is constantly zero, we have ultimately

$$\frac{\omega'_0}{w'_0} = \frac{\sqrt{e}\,\lambda}{\pi} \cdot \frac{\tan 2\sqrt{e}h}{\tan\frac{2\pi l}{\lambda}} = \frac{e\lambda^2}{\pi^2}.$$

This result informs us that the changes of condensation produced by the flow of the fluid to or from any point of no velocity are due to the longitudinal and the transverse motions in a constant ratio. It hence follows that the changes of condensation at *any* given point of a single series of waves are due at each instant to the longitudinal and transverse motions in the same ratio. In fact, from the foregoing values of w and ω, it will be seen that the ratio of $\frac{\delta\omega}{\delta h}$ to $\frac{\delta w}{\delta l}$ is that of e to $\frac{\pi^2}{\lambda}$. Hence if $\delta h = \delta l$, we have $\frac{\delta\omega}{\delta w} = \frac{e\lambda^2}{\pi^2}$, which is clearly the ratio in which the two velocities contribute to the changes of condensation.

By substituting $\frac{\omega'_0}{w'_0}$ for $\frac{e\lambda^2}{\pi^2}$ in the value of κ, we obtain for the velocity of propagation $a\sqrt{1 + \frac{\omega'_0}{w'_0}}$; which shows that the excess of the velocity above the value a is caused by the transverse velocity, and that, because the changes of condensation are due to the transverse as well as the longitudinal velocity, they are more rapid than they would be if due to the latter alone, and the rate of propagation of a given state of density is consequently accelerated.

Reverting now to the expressions for ω' and σ', if $(2n+1)\pi$ be substituted for c', $\frac{2\pi}{\lambda'}$ for $2\sqrt{e}$, and κ' for $\frac{\kappa\lambda'}{\lambda}$, the following equations may be obtained:—

$$\omega' = \frac{2\pi m f_0}{\lambda'} \cos\frac{2\pi l}{\lambda}\left(\sin\frac{2\pi}{\lambda'}(h - \kappa' a t) + \sin\frac{2\pi}{\lambda'}(h + \kappa' a t)\right),$$

$$\sigma' = \frac{2\pi m \kappa' f_0}{\lambda' a} \cos\frac{2\pi l}{\lambda}\left(\sin\frac{2\pi}{\lambda'}(h - \kappa' a t) - \sin\frac{2\pi}{\lambda'}(h + \kappa' a t)\right).$$

At the same time

$$w' = \frac{2\pi m f_0}{\lambda} \cos\frac{2\pi h}{\lambda'}\left(\sin\frac{2\pi}{\lambda}(l - \kappa a t) + \sin\frac{2\pi}{\lambda}(l + \kappa a t)\right),$$

$$\sigma' = \frac{2\pi m \kappa f_0}{\lambda a} \cos\frac{2\pi h}{\lambda'}\left(\sin\frac{2\pi}{\lambda}(l - \kappa a t) - \sin\frac{2\pi}{\lambda}(l + \kappa a t)\right).$$

Hence the transverse motion and condensation may be represented by equations exactly analogous to those which represent the longitudinal motion and condensation, and the two motions are correlative to each other. If $\frac{\pi^2}{\lambda'^2}$ be substituted for e in the value of κ, we obtain $\kappa = \sqrt{1 + \frac{\lambda^2}{\lambda'^2}}$. Also $\kappa' = \frac{\kappa\lambda'}{\lambda} = \sqrt{1 + \frac{\lambda'^2}{\lambda^2}}$. Thus the velocities of propagation κa and $\kappa' a$ are each greater than a, because, as already explained, the transverse and longitudinal motions both contribute to the changes of condensation. But the *ratio* of these velocities is that of λ to λ', as evidently should be the case, since the propagations over these breadths occupy necessarily the same time.

There remains another consideration which must be brought to bear on the determination of the velocity of propagation. We found above the general relation $w=\frac{a\sigma}{\kappa}$ between the longitudinal velocity and condensation in a single series of waves. The analogous relation obtained by the process of reasoning that has been usually adopted in questions of this kind is $w=a\sigma$. But that process does not take into account that the total motion is composed of separate longitudinal and transverse motions relative to *axes*. The factor $\frac{1}{\kappa}$ is wholly due to the *lateral spreading* which accompanies the condensations and rarefactions propagated along and parallel to the axis of motion, which has the effect of diminishing the rate of change of the density in the direction of propagation, and thus making the effective elasticity of the fluid, *cæteris paribus*, less than the actual in the ratio of $\frac{a^2}{\kappa^2}$ to a^2. But just in the proportion in which the effective elasticity is caused by lateral spreading to be less than the actual in the direction of propagation, it must, by a reciprocal action, be made greater than the actual in the transverse direction, and accordingly be increased in the ratio of κ^2a^2 to a^2. Thus the ratio of the latter effective elasticity to the other is κ^4, and the ratio of the corresponding velocities of propagation is κ^2. Now we have proved that this ratio is $\frac{\lambda'}{\lambda}$. Hence, substituting in the expression for κ, we have

$$\kappa=\sqrt{1+\frac{1}{\kappa^4}}, \text{ or } \kappa^6-\kappa^4=1.$$

Consequently the numerical value of κ^2 is obtained by the solution of a cubic equation which has one real positive root and two imaginary roots. The value of κ will be found to be 1·2106. Hence, taking $a=916{\cdot}322$ feet, the resulting velocity of propagation is 1109·3 feet. The value by observation, as given by Sir J. Herschel in the *Encyclopædia Metropolitana*, is 1089·7 feet. The difference 19·6 feet might be lessened in some degree by calculating the corrections of the observations for temperature according to Regnault's coefficient of expansion. But probably the principal part of the difference is due to the circumstance that the theoretical reasoning assumes the fluid to be *perfect*, and it may be that atmospheric air is not strictly such. It seems hardly to be accounted for that a course of reasoning involving considerations so various and peculiar as those which have been gone through above, should have conducted to a result differing

from observation by no larger amount, unless the principles of the reasoning are fundamentally correct. I may here state that the point of no velocity might have been taken on the axis of motion instead of being at a great distance from it, inasmuch as the motion contiguous to the axis may be supposed to consist of two equal sets of longitudinal and transverse motions; and each set might be treated independently of the other. By conducting the reasoning in this way I obtained the same results as by the other method.

If it be objected that when the effect of the development of heat on the rate of propagation is taken into account the mathematical result is contradicted by experiment, I reply as follows:—It is evident, from the mutual relation of the longitudinal and transverse motions above described, that we have had under consideration a case of *free* expansions and contractions due to successive generations and fillings up of a partial vacuum. Now it is admitted, I believe, that experiment has decided that in such a case there is no change of temperature. Consequently the rate of propagation remains unaffected. The case of development or absorption of heat when air is suddenly let into, or abstracted from, *closed* spaces, and when, in consequence, *work* is done, has no analogy to this. Upon the whole I seem entitled to conclude that I have at length succeeded in solving the difficult problem of determining mathematically the rate of propagation in a continuous elastic fluid. The results obtained are essential to the undulatory theory of polarization.

(2) I proceed, in the next place, to advert to a communication I made to the Philosophical Magazine for January 1857, entitled "On the Transmutation of Rays of Light." In the course of the article I have enumerated various inferences relating to phenomena of light, which had been deduced by means of the analysis I had applied to the undulatory theory; and to one of these, which is numbered (4) in the order of the series, I wish now to call attention. That deduction is expressed in the following terms:—"When the æther in motion suffers disturbance by encountering atoms actually or relatively at rest, and the original motion is a simple series of vibrations of the usual type, or is compounded of several such motions with parallel axes and different values of m, λ, and c, the result of the disturbance may in either case consist of an indefinite number of separate motions having their axes in various directions, and having values of m, λ, and c altogether different from the values of these quantities in the original motion." Further on I remark that "when the circumstances of the disturbance are as supposed in (4), light may produce new light, which may differ from the original light in intensity, colour composition, and direction of

propagation. This effect I have called a 'Transmutation of Rays;' and I beg it may be understood that in making use of these terms I mean only to express a result deduced from the mathematical theory." In writing the last sentence I had in mind the statement made by Professor Stokes, that change of refrangibility always took place from a greater to a less refrangibility. As there was nothing in my theory of Transmutation which pointed to such a limitation, and as the experimental evidence for it appeared to be only negative, I preferred stating the theoretical results in all their generality, without citing any experiments bearing upon them. But now that the experiments of Dr. Tyndall have shown that this law of transmutation applies to the less refrangible as well as to the more refrangible rays, and that there may be change from less to greater refrangibility, I feel at liberty to say that the theory is in complete accordance with these experimental results. I take this occasion to remark that the term "Transmutation of Rays," which has acquired special interest since Dr. Tyndall's experiments have shown that it expresses a law of nature, was originated by me, on purely theoretical grounds, in the communication here referred to, published more than eight years ago, and has since been adopted without any reference to its occurrence in that communication.

(3) In my Theory of the Composition of Colours, contained in the Philosophical Magazine for November 1856, I have endeavoured, under section (5), to give reasons for a distinction between "terrestrial light," that is, light which has been reflected, refracted, or generated by terrestrial substances, and direct solar light. I was induced to do this by the persistent assertion of experimenters that a composition of yellow and blue solar rays does not produce a green colour, whereas the composition of such rays emanating from yellow and blue terrestrial substances undoubtedly produces green. More recently, an experiment by Sir J. Herschel, described in the 'Proceedings of the Royal Society' (vol. x. No. 35, p. 82), has led me to infer that the distinction I sought to account for does not really exist. This experiment renders it very probable that in cases in which green is not perceived to result from a mixture of yellow and blue solar rays, the rays are of too great intensity for the eye to distinguish the colour. At least Sir J. Herschel found, after concentrating a solar spectrum by an achromatic lens, so as to bring the yellow, green, and blue spaces pretty close together, that, on *diminishing the intensity of the light*, the green appeared to be so diffused as to encroach greatly on the yellow and blue spaces. When making the experiment of covering white paper with alternating parallel spaces, not inconsiderable in breadth, of yellow and blue colours made by chalk pencils, I constantly found that even when the eye was near enough to distinguish the spaces easily, the whole appeared to be suffused by a tinge of green. Now, although this diffusion in both kinds of experiment may be referable to the manner in which the organ of sight is acted upon by the rays, it proves not the less that a combination of yellow and blue has the same effect in producing green, whether the light come directly from the sun, or is what I called terrestrial light. For this reason I withdraw the distinction I endeavoured to establish between solar light and terrestrial light.

With reference to the same subject, I take this opportunity to state that I have made experiments for showing the effects of combining colours by means of revolving disks, the disks being divided into spaces covered alternately with the two colours to be compounded. The apparatus I used was professedly made according to directions contained in Professor Maxwell's paper on this subject, and among the different sets of colours was one which was intended to show that yellow and blue combined do not produce green. The result in this instance was certainly a dirty white; but according to my sight the blue and the yellow had scarcely any resemblance to prismatic blue and yellow. On substituting for them the very same chalk colours that I used in the above-mentioned experiment of parallel spaces, I found that the result was decidedly green. It may be that the colours I used were not pure colours; but the fact that one appeared blue and the other yellow was owing to the predominance of blue or yellow solar rays, and the predominant tint of the compound was determined accordingly.

For these reasons, drawn, it will be seen, in part from personal observations, I hold that sunlight and terrestrial light are not essentially different, and that, in accordance with the mathematical theory of the composition of colours given in the above-cited article, combinations of yellow and blue, with either kind of light, have the effect of producing green.

(4) It having been suggested to me to employ Ångström's values of λ, given in Poggendorff's *Annalen* for November 1864, for testing my Theory of the Dispersion of Light contained in the Supplementary Number of the Philosophical Magazine for December 1864, I have calculated as follows for this purpose. It was considered sufficiently accurate to obtain the values of λ for the rays C, D, F, and G from those for the rays B, E, and H, and the given values of μ by mere interpolation, and to regard the *differences* of the results deduced from the old and the new values of λ as the same that would have

been obtained by calculating strictly according to the theoretical formula (θ). According to this principle, it is only necessary to apply these differences to the values of λ previously calculated from Fraunhofer's data, to obtain the values that would be given by Ångström's data. These calculations having been gone through, the comparison, to the third place of decimals, of the observed and calculated values of λ for the two sets of data stands as follows :—

Ray.	λ by Fraunhofer.	Excess of calculation.		λ by Ångström.	Excess of calculation.	
		Flint-glass No. 13.	Oil of Cassia.		Flint-glass No. 13.	Oil of Cassia.
B	2·541	0·000	0·000	2·5397	0·000	0·000
C	2·422	+0·003	+0·006	2·4263	−0·002	+0·001
D	2·175	−0·001	−0·001	2·1786*	−0·003	−0·003
E	1·945	0·000	0·000	1·9484	0·000	0·000
F	1·794	+0·002	−0·003	1·7973	+0·003	−0·001
G	1·587	+0·005	−0·004	1·5923	+0·004	−0·003
H	1·464	0·000	0·000	1·4672	0·000	0·000

It appears from this comparison that the excesses of calculation are somewhat smaller with Ångström's values than with Fraunhofer's, especially in the case of oil of cassia, the more refractive substance.

With this communication I conclude the series of arguments by which I maintain that the Undulatory Theory of Light rests legitimately on no other than a hydrodynamical basis.

Cambridge, April 22, 1865.

XLVII. *On the Reversal of the Spectra of Metallic Vapours.* *By* H. G. Madan, *F.C.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen, April 11, 1865.

May I be allowed to mention a simple and convenient method of illustrating one of the most important points in Bunsen and Kirchhoff's spectrum discoveries, viz. the reversal of the spectra of metallic vapours; the most familiar example of which is the reversal of the sodium-line D?

I have tried most of the various methods proposed for effecting this object, but none have appeared to me so easy and effective as the following.

* This value applies to the middle of the double line.

It consists simply in directing the spectroscope upon a fragment of sodium burning in oxygen gas. The incandescent metal gives, of course, a continuous spectrum; but the rays, in passing through the cooler atmosphere of sodium-vapour which surrounds the metallic nucleus, are selectively absorbed, and the dark double line D, or Na α, appears with great distinctness on the bright spectrum. As, however, the fragment of sodium is soon consumed, I have used an apparatus resembling that employed for making phosphoric anhydride, by which pellets of sodium may be added as often as required.

A moderate-sized deflagrating-jar is placed in a dish of sand. In it is suspended a shallow iron cup, and through the same cap which carries the latter is passed a short wide glass tube, so as to be directly over the centre of the cup. Through the sand and under the lower edge of the jar passes a bent glass tube, connected with a caoutchouc bag of oxygen, and serving to introduce a continuous slow stream of the gas to replace that consumed by the sodium. The spectroscope should first be adjusted as to position and focus by bringing it to bear on a candle placed on the opposite side of the jar, so that its flame may just be seen over the edge of the iron cup. Then, while the jar is filling with gas, the cup may be withdrawn, a pellet of sodium placed in it and heated over a spirit-lamp until it begins to burn, and lastly immersed in the jar. Fragments of sodium may be added as required through the glass tube, and will readily burn if the mass of soda in the cup be not allowed to cool below dark redness. The experiment may thus be carried on as long as desired; and, of course, two or more spectroscopes may be arranged round a single deflagrating jar.

I have not yet tried whether the spectrum thus produced can be thrown on a screen, the deflagrating-jar being enclosed in a Duboscq's lantern; but, from its brilliancy in the spectroscope, I have little doubt that it could be thus shown. The idea of this method occurred to me about a year ago, and I have shown it to many in Oxford; but it seemed so obvious an expedient that I thought it must have been already described. As, however, I have been unable to find any account of it, I venture now at any rate to bear my testimony to its efficacy.

The same method is of course available in the case of other volatile and oxidizable metals, as lithium, zinc, magnesium.

I remain,
Yours, &c.,
H. G. Madan.

Queen's College, Oxford.