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> "Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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THE
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[FOURTH SERIES.]

JULY 1864.

I. *On the Extra Current of the Induction Current.*
By J. C. Poggendorff*.

In my latest investigation laid before the Royal Academy in August last, on the thermal action of electrical sparks, in order to test the view propounded by Reitlinger, that this action is proportional to the intensity of the current, I had interposed very long wires, in the form of coils, in the path of an induction current. I thereby observed that the sparks, notwithstanding the considerable enfeeblement in intensity produced by this interposition, lost scarcely any of their heat, just as they have long been known to become thereby more fully luminous, but to lose little or none of their striking-distance.

I observed on this occasion that when the circuit of the inductorium is completely metallic through such accessory coils, the current, in spite of this metallic circuit, and in spite of the great enfeeblement which it experiences through the resistance of a very long and thin wire, possesses such tension that extremely piercing sparks are obtained if the wires are merely touched in one point with the hand.

I further found that the free electricity at the end of the accessory coil turned towards the *positive* pole of the inductorium is *positive*, and *negative* at the other end; and that if two such accessory coils are placed end to end, the wire joining them exhibits far less tension than the two wires leading from the coils to the inductorium, from which I concluded that in the long metallic circuit there must be a zero point of tension. Finally, I convinced myself, by individual interruptions of the voltaic ex-

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* Translated from the *Monatsbericht der Berliner Akademie*, November 1863, by Dr. E. Atkinson.

citing current, that the tension observed only belongs to the current when it is opened.

These phenomena are certainly surprising when it is considered that the poles of the inductorium lose their tension completely when they are joined by a simple, by no means short, thick metallic wire. It is indeed stated that they retain even then a trace of tension; but with my apparatus I could not observe it, either by the gold-leaf electrometer or with the tongue. Even when I had approached the poles of a powerful apparatus till they produced sparks abundantly, I could, by means of 100 feet of a platinum wire 0·1 millim. in diameter, which was bent backwards and forwards in the air, and was equal in resistance at least to 1000 feet of the induction wire, entirely take away not merely the sparks, but every perceptible trace of tension at the poles, when I connected them with the two ends of this wire. But these phenomena of tension were at once distinctly observed, though in a less degree, even with a copper wire of 0·66 millim. in thickness and not more than 400 feet long which was rolled in the form of coil.

Non-metallic, relatively bad conductors alone exhibited a different deportment in this respect. A hemp string, for instance, moistened with feebly acid water, showed not merely tension in the electrometer, but gave just as delicate sparks as a large coil of wire, even when its length was only an inch or less.

It differed remarkably from metallic wires (in which, as is well known, only very feeble heating is perceptible) by the fact that it became considerably heated—so much so, that a thermometer round whose cylinder it was coiled once, rose in a minute from 40° to 50°, that it smoked visibly, gradually dried, then carbonized, and finally disappeared with sparks.

Although it was not impossible that even straight wires of great thickness and length, simply in virtue of their resistance, might exhibit a certain tension when united with the poles of the inductorium, I was yet convinced that the phenomena mentioned were not phenomena of resistance, but arose from an induction which might indeed occur in a feeble degree in straight wires, and I designated it even then as the result of such a process, without expressing an opinion as to the particular manner.

Continued occupation with the subject has confirmed me in the original view, and leads me to consider it as not doubtful that the tension observed proceeds from an induction current, which the current of the inductorium, the current of opening, produces in the spires of the accessory coil in the opposite direction to its own.

As it is usual to designate the current of induction, which the voltaic current can produce in its own wire on opening and closing, as an extra current, I do not hesitate to designate the current in question as the *extra current of the induction current.*

To test this view more accurately, it appeared best to investigate the deportment of various induction apparatus to each other.

I first combined two apparatus of this kind, as similar as possible, in opposite directions, by joining in a corresponding manner the two external divisions of an inductorium with three divisions, by bright copper wires of small length. Although each of these divisions only contained 1200 feet of copper wire of 0·25 millim. in thickness, and, both united in the same direction, exhibited no perceptible trace of tension, yet when united in opposite directions their junction wires gave painful sparks.

There was no current in this; for an electrical egg interposed in one of the joining wires showed no luminous phenomena.

Thereupon I combined this inductorium as a whole, in which it contained about 3800 feet of wire, in an opposite direction with another large apparatus whose induction coil consisted of 23,000 feet of wire $\frac{1}{7}$th of a millim. in thickness, in which I passed one and the same voltaic current through the inducing coils of both, and interrupted them by one and the same hammer.

I began by testing the induction coils singly, by causing one to give sparks at the micrometer while the other was closed by metal. As was to be presupposed, the larger apparatus was the more powerful; the striking-distance of its sparks was more than double that of the sparks of the smaller.

When they were now combined in an opposite direction (that is, the similar poles of both coils united) by bright copper wires of no great length, a current moved in the direction of the stronger apparatus accompanied by a tension upon the joining wires, which yielded even more piercing sparks than those in the former case*.

Both these results serve doubtless as a support of my view, in so far as they show that when, in a closed circuit of wire, two induction currents (whether equally or unequally strong is quite immaterial) act in opposite directions to each other, free electricity occurs.

It was now necessary to furnish a proof that in the original experiment the current of the inductorium evoked an *opposite current* (*Gegenstrom*) in the accessory wire. This was effected in two ways.

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* I will not omit to mention that even when both apparatus were joined in the same direction, a tension was indeed observed on the wires, though somewhat feeble. This, however, I can only consider as abnormal, arising from some not yet explained circumstance. For, if it were normal, it must occur in each individual inductorium, as that may always be supposed to consist of two unequal instruments acting in the same direction.

First by a bundle of iron wire. If such a bundle is inserted in the accessory coil, tension and sparks are observed in the junction wires to a surprising degree. The supposition is most natural that this result arises from a strengthening of the opposite current; and if this supposition is confirmed, the inducing induction current must be enfeebled by the bundle of iron. This is in fact the case.

I was convinced of this in the following manner. I joined an inductorium having 10,000 feet of wire to an accessory coil of 23,000 feet, interposing at the same time a galvanometer and a well-exhausted electrical egg—the latter for the double reason of observing the luminous phenomena in it, and removing the closing current, so that the hammer might be used; for indidividual openings and closings of the battery of six elements had produced no result.

By means of this arrangement, and without iron wire in the accessory coil, I obtained a deflection of about 20° in the galvanometer, and the well-known luminosity in the egg. When the iron was interposed in the coil, the deflection sank at least to half its amount, and the light in the egg disappeared almost completely, being reduced to a few irregular sparks.

Hence the strengthening of the opposite current, and therefore its existence, cannot be subject to any doubt.

In what manner soft iron strengthens the opposite current does not come into consideration. I will, however, remark that an induction current magnetized the iron in the same direction as that in which it deflects a magnetic needle, that it gives therefore to Ampère's molecular currents the same direction as its own; while in an adjacent wire, according to the observations of Henry and others, it produces an induction current of the second order in an opposite direction*. By its magnetization the iron reacts on the magnetizing induction wire, and as it produces in it an opposite current, I conclude that this current is the product of the commencing magnetism, and the disappearing acts little, or not at all. If in the momentary magnetization which the soft iron experiences by the induction current, resulting from the opening of the voltaic current, both elements, the increase and decrease of the magnetism, were of equal influence upon that current, it could not be perceptibly affected.

A closed coil inserted in the accessory coil acts differently, that is, enfeebles the opposite current, and therewith the tension;

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* It is clear that an induction current, since in its transitory career it increases and decreases, must induce two currents as well in its own as in an adjacent wire, one of opposite and one of the same direction. But according to all observations the first is, in galvanic induction, the stronger; hence I have only spoken of it, and called it *opposite current.*

for the inducing induction current produces in it a current of the same direction as the opposite current which reacts upon this. But I had no coil of a sufficient number of windings, which I might have inserted in the accessory coil, so as in this way completely to destroy the opposite current.

The inductorium itself gives the second proof of the existence of the supposed opposite current. For to produce the often mentioned phenomena of tension it is unnecessary to use an accessory coil; the inductorium itself is quite sufficient.

Nothing more is necessary than that, after the poles of the instrument at work have been united with one another by a short wire, the inducing coil together with its iron core be partially withdrawn. On commencing, free electricity appears in the junction wire, and it increases until about two-thirds or three-fourths of the coil are withdrawn from the induction coil. It is true that in this case the empty part of the induction coil represents the position of the accessory coils in the earlier experiments, and so far this result is not surprising.

If now the inducing coil is slowly reinserted, free electricity begins to decrease, and continues to entire disappearance when the coil has been restored to its original position. This also is natural.

But, it may be asked, what happens in this second process? Obviously nothing else than that, in the coils of the empty part of the induction coil, the original induction, partly or entirely removed, is reproduced. This induction destroys the earlier condition. But what destroys an induction can be only an induction, and one of opposite direction; hence by this experiment the existence of the opposite current is proved. I do not suppose that anything well founded can be urged against these simple conclusions.

I will only add that the phenomenon of tension in question, if purely one of resistance, could never entirely disappear, but rather, even with the very best conduction between the poles of the instrument, must occur in full force; for each partial current which is induced in an individual spire of the induction coil has to traverse the sum of all the other spires of the coil, and hence to overcome a resistance which would be quite sufficient to cause free electricity to appear if this were merely evoked by resistance.

From all this I consider the origin of free electricity in the circuit of a metallic closed inductorium to be sufficiently established, and hence I think myself justified in passing over other experiments which I have undertaken in this direction.

Yet I cannot help discussing an objection which seems to follow from the statement that the striking-distance of the induction spark undergoes no enfeeblement from a wire circuit introduced into the path of the current. The striking-distance stands obviously in a direct ratio to the electromotive force; and when this latter is enfeebled by an opposite current, it can scarcely be otherwise than that the striking-distance should also be diminished. Although my earlier investigations seem to speak against this, I believe that such a diminution actually occurs whenever the interposed wire is used in the form of coil, and that it is only on the one hand the indefiniteness of the striking-distance, and on the other the weakness of the partial current, which may have prevented this diminution from being perceived. It does not here depend so much on the absolute length of the coil added, as upon its ratio to the length of the wire of the inductorium. With a certain ratio, the opposite current, or its enfeebling influence upon the inducing current, is most strongly developed, and then the diminution also naturally follows. Some experiments which I made in this respect were favourable to this view, although a repetition of them with greater means than those at my command would not have been superfluous. For straight wires, the above statement, though not perhaps with the utmost rigidity, applies with tolerable approximation.

In the foregoing I have only spoken of the developments of the opposite current in free air; its occurrence is extremely striking when part of the circuit is in a rarefied space.

If under the receiver of an extra plate of an air-pump which is provided with the necessary insulated conductors, a bright copper wire is stretched, and the air is adequately exhausted, and if the arrangement is placed in the circuit of an inductorium provided with its accessory coil, as soon as the instrument is set to work the wire is seen to become brightly luminous, and to send bright rays towards the bell. The phenomenon is improved by clothing the bell externally with a strip of tinfoil corresponding to the wire, which is placed in connexion with the ground; and still more by bringing a small piece of phosphorus under the bell.

In general the wire does not become continuously, but partially luminous; these luminous parts are in continual motion, run backwards and forwards on the wire, and send glimmering rays towards the tinfoil, which also becomes luminous on the inside, so that the whole, since at the same time the dark parts, by contrast, appear to emit dark rays, has an appearance like that of the aurora borealis.

I could see nothing of a stratification in this luminous phenomenon, although I had first with this view allowed phosphorus to evaporate under the bell; the formation of stratification is probably suppressed or concealed by the great mobility of the light. Nor could I perceive any material difference in the appearance and colour of the light when the receiver was alternately touched on the positive and on the negative side of the apparatus. Its colour is whitish throughout.

This *side light*, as I will call it, as it is obviously analogous to the lateral emissions which have long been known in powerful electric discharges in free air, was most intense when the inductorium was made to give sparks in the air, and at the same time the air surrounding part of the joining partially exhausted. The further apart the poles are moved, the more intense is the light, and of course the tension upon the wires. It is, on the other hand, relatively feeble if the poles of the apparatus are connected with the armatures of a Leyden jar, in which, as is known, *recurrent* currents are formed, and the tension upon the wires is feebler.

The side light is a useful indicator of the degree of free electricity on the wires.

If, for example, an induction coil is placed in metallic connexion with a larger one, and an induction current is alternately produced in the first and in the second, while the other is used empty as an accessory coil, even the sensation of feeling shows that the tension in the wire is stronger when the current is induced in the smaller coil, although the current is feebler in that case than in the other; but this is shown much more convincingly by the side light. The strengthening action produced by introducing a bundle of iron wire into the accessory coil cannot be more surprisingly shown than by the side light.

I must in conclusion mention that in 1859, Koosen*, on the occasion of another investigation, made observations which are closely allied to mine, but do not quite coincide with them. He observes the phenomenon in a form in which it is essentially a phenomenon of partial currents. He offers, that is, two paths for the induction current, one through air and one through metal, by letting the poles of the instrument give sparks, and joining them at the same time by a very long wire, in which he finds that the striking-distance of the sparks, in spite of this metallic lateral circuit, is either not at all or not perceptibly diminished. The free electricity in the wire has not indeed escaped him; but since he only states that the wire has a *certain tension* which can be shown by the gold-leaf electrometer, while he does not mention the sparks and their piercing action, he has probably not seen the phenomenon in its full development, perhaps because he used covered and varnished wires, perhaps because he only studied it at one branch of the current. Finally, he does not dwell upon the cause of the phenomenon. Although he has in all probability used the wire in the form of coil, yet he does not say

* Pogg. *Ann.* vol. cvii. p. 211.

so, but always speaks of the length and resistance of the wire. So that I do not consider the publication of my experiments to be superfluous.

I have, moreover, repeated the observations in the manner described by Koosen, and found them confirmed in the main point, as was to be expected; yet I have also observed that it depends on the relation between the length of wire in the accessory coil and in the inductorium. An accessory coil of 10,000 feet took away completely the sparks of an inductorium of 23,000 feet, while it left untouched those of a small instrument of 3000 feet. The phenomenon is best seen as one of partial currents and of resistance when a hemp thread 5 or 6 feet in length, moistened with spring-water, which is fixed insulated in the air, once backwards and forwards, and connected with the poles of the spark micrometer. By moving a wire bridge laid across, it can be shortened at pleasure for the current, and it may be observed that the first action upon the sparks consists in an attenuation of them.

II. *On the Discrepance between the English and French Barometer-Scales; and on the Corrections necessary in reducing the Readings to the Freezing-point.* *By* Charles Packe, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

HAVING brought out with me a barometer graduated with a double scale, reading millimetres on one side and inches on the other, I have been at some pains to investigate the slight apparent anomaly existing between the French and English scales as compared with the boiling-point, which I think may be satisfactorily accounted for in the following manner:—

Boiling-point.	Barometer.		
212° F., or = 100 C.	in. 29·905 or mm. 759·58, as given by the Kew Committee of the British Association.	=	in. 29·922 or mm. 760, as given by Regnault in his Tables of the Elasticity of Vapour.

The larger portion of this discrepance arises from the difference of the standard temperature of the scales of the English and French barometers; the remainder is accounted for by the difference of latitude producing a variation of gravity.

First as to the discrepance arising from the standard temperatures. That of the English barometer being 30° F. higher than that of the French scale, when the mercurial column is reduced to the freezing-point, the scale of the French barometer is also reduced to the freezing-point, but the scale of the English one is only reduced to the temperature of 62° F.

The consequence is that the French barometer, when reduced, will always read higher than the English barometer.

Let A be the height of the barometer observed;

B the linear expansion of brass for 1° F. = ·0000104344, as given by Laplace and Lavoisier, or ·000018782 for 1° C.

The French barometer, when reduced, will, on account of the difference of standard temperatures, read higher than the English barometer by an amount = 30 AB; *e. g.*,

Let the height of the barometer be = the boiling-point 212°, *i. e.* 29·905 inches.

$$\begin{array}{lll} B = \cdot 0000104344 & = \log \bar{5}\cdot 018467 & \\ A = 29\cdot 905 & = \log 1\cdot 475743 & \\ \qquad 30 & = \log 1\cdot 477121 & \\ \cline{2-2} & \quad\ \bar{3}\cdot 971331 & = \cdot 009361 \text{ inch excess of the French reading.} \end{array}$$

By changing A it will be evident that we get the excess for any height of the barometer, but for the average height at the sea-level it may be taken as ·009 inch.

For exact observation, therefore, it is useless to have a barometer marked with a double scale,—the French and English: they *cannot* be made to coincide; *e. g.*

Let the barometer read 29 inches = 736·59 millims. (temp. 62° F. = 16°·67 C.). In the English scale at 62° (the temperature of the standard) no correction is made for the brass scale. The only correction is for the expansion of the mercury — ·087,

$$\begin{array}{rr} & \text{in.} \\ & 29 \\ - & \cdot 087 \\ \cline{2-2} \text{reduced} & 28\cdot 913 = 734\cdot 38 \text{ millims.} \end{array}$$

But in the French scale, the temperature of the standard being 32° F., the correction to be made is for the expansion of the mercury — the expansion of the scale:

$$\begin{array}{lr} & \text{mm.} \\ \text{Expansion of mercury for } 16°\cdot 67 \text{ C.} = & 2\cdot 212 \\ \text{Expansion of brass scale for} \quad ,, \quad = & -\cdot 231 \\ \cline{2-2} & 1\cdot 981 \end{array}$$

$$\begin{array}{rr} & \text{mm.} \\ & 736\cdot 59 \\ - & 1\cdot 981 \\ \cline{2-2} \text{reduced} & 734\cdot 61 = 28\cdot 9224 \text{ inches.} \end{array}$$

To reduce the barometer to the freezing-point, we have the following formulæ:—

Let M = the cubic expansion of mercury for the number of degrees Centigrade or Fahrenheit, by which the observed temperature differs from the freezing-point.

B = the linear expansion of the brass scale for the number of degrees by which the observed temperature differs from the standard.

H = the observed height of the barometer in inches or millimetres.

The formula for the reduction of the English barometer will be

Above 62° F.	$-(M-B)\times H$
Below 62°, above 32° . .	$-(M+B)\times H$
Below 32°	$+(M-B)\times H$

For the reduction of the French barometer,

Above the freezing-point .	$-(M-B)\times H$
Below the freezing-point .	$+(M-B)\times H$

By the difference in the standard temperatures we can thus account for ·00936 inch out of the ·017 by which the Kew equivalent to the boiling-point differs from that of Regnault. The remainder is almost exactly accounted for by the difference of gravitation.

The increase of gravitation, *i. e.* gravity as diminished by the centrifugal force, is from the equator to the poles =·0052005, or ·00260 to the 45th degree of latitude.

Adopting the law that it increases as the square of the sine of the latitude, we find that in 51° 30′, the latitude of London, the increase of gravity is =·0031852; in latitude 49°, that of Paris, the increase of gravity ·0029621.

Let D be the difference of these two gravities =·0002231;

B' the height of the mercurial column at London, equivalent to a given pressure;

B the corresponding height of the mercurial column at Paris.

B will be equal $(B'+B'D)$: *e. g.*

Gravity in lat. 51° 30′ = ·0031852
Gravity in lat. 49° = ·0029621

$$\text{Difference} = \cdot0002231 = \log \bar{4}\cdot348305 = D$$
$$29\cdot905 = \log 1\cdot475744 = B'$$
$$\bar{3}\cdot824049 = \cdot006688 = B'$$

To compare, therefore, the barometric column at London with that representing a corresponding pressure at Paris, we must add two corrections—one for the difference of the temperature of standard, and a second for the decrease of gravity. The corrections will be as follows:—

Barometer at London observed and reduced to freezing-point		in. 29·905
Correction for temperature of standard	+	·009361
Correction for decrease of gravity .	+	·006688
		29·921049 equivalent pressure at Paris.

This is very close to the equivalent pressure which Regnault gives in his Tables, 760 millims. =29·922 inches*.

Of course in a lower latitude the accurate correction for gravity will be greater; but practically all Tables are made to correspond with that of London or Paris; or else the barometer is supposed to be reduced to the mean gravity of lat. 45° = ·0026003. I am somewhat astonished to find that in all the Tables given for the reduction of the barometer to the freezing-point, the only elements taken into consideration in computing the dilatation of the mercurial column are the linear expansion of the brass scales, and the cubic expansion of mercury. Surely for real accuracy the superficial expansion of the glass tube (*i. e.* increase of its capacity) should also be allowed for, as was done by Mr. Stewart in his experiment to determine the melting-point of mercury (see Phil. Trans. vol. cliii. p. 430).

Let the expansion in volume of mercury (*i. e.* cubic) be, as determined by Regnault, =·00018153 for each degree Centigrade above the freezing-point, and the coefficient of linear expansion of the glass tube be =·0000086130, as determined by Dulong and Petit; then the coefficient of the superficial expansion (*i. e.* capacity) of the glass tube will be =·000017226.

Take M for the cubic expansion of the mercury, and G for the

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* I have taken the increase of gravity adopted by Guyot in his Tables $=0{\cdot}00520048$, which assumes the ellipticity of the earth $\frac{1}{298}$.

If we take the ellipticity of the earth as given by the British Ordnance Survey, $\frac{1}{294}$ (see Phil. Mag. vol. xxiv. p. 413), we shall get the increase of gravity from the equator to the pole $={\cdot}00527919$, and this will give the difference between that of Paris and London

$$
\begin{aligned}
&= {\cdot}00023642 = \log \bar{4}{\cdot}373684 \\
&29{\cdot}905 = \log \underline{1{\cdot}475743} \\
&\qquad 3{\cdot}849427 = {\cdot}007070, \text{ the amount to be added for gravity.}
\end{aligned}
$$

superficial expansion of the glass tube, for the number of degrees observed above or below the freezing-point.

The dilatation of the mercurial column will be =M−G, or

$$\begin{array}{l} \cdot 00018153 \\ \underline{-\cdot 000017226} \\ =\cdot 000164304 \text{ for each degree Centigrade,} \\ =\cdot 000091280 \text{ for each degree Fahrenheit.} \end{array}$$

The Tables, French and English, give as the correction,

$$\cdot 0001614 \text{ for } 1^\circ \text{ Centigrade} = \cdot 00008967 \text{ for } 1^\circ \text{ Fahrenheit;}$$

but they all take Dulong and Petit's coefficient for the cubic expansion of mercury, which is lower than that of Regnault, being only ·000180180.

When the column is thus reduced for the expansion of the mercury, the additional correction for that of the scale has to be applied.

I am, Gentlemen,
Yours faithfully,
Charles Packe.

Gavarnie, Hautes Pyrénées,
April 22, 1864.

III. *On the Probability of Testimony and Arguments.* By T. K. Abbott, *M.A., Fellow and Tutor of Trinity College, Dublin*[*].

THERE are certain questions respecting the probability of testimony and argument which, although of considerable practical importance, have been either neglected or erroneously treated by writers on the subject. It is the purpose of the present paper to consider briefly a few of these, keeping in view not so much theoretical generality or completeness, as the correspondence of the conditions assumed with those which occur in ordinary experience.

The case of testimony is represented in the ordinary treatises as follows:—Suppose a witness, A, affirms that a certain event has occurred, the antecedent probability of which was p, and let the witness's credibility be a. Then, it is said, there are only two possible cases. Either he is right, the chance of which is

$$a;$$

and the event has occurred, the chance of which is]

$$p;$$

∴ the chance of this coincidence is

$$ap.$$

---

[*] Communicated by the Author.

Or he is wrong,

$$1-a;$$

and the event has not occurred,

$$1-p;$$

∴ the chance of this coincidence is

$$\overline{1-a}\,\overline{1-p}.$$

The sum of these gives the common denominator; and hence the resulting probability, P, that the event has actually happened is

$$\mathrm{P}=\frac{ap}{ap+\overline{1-a}\,\overline{1-p}}.$$

Now let there be two or more witnesses who agree in the same assertion, their respective credibility being a_1, a_2, &c. Then either they are together right, and the event has occurred, the chance of which is

$$a_1\,a_2\ldots a_n p;$$

or they are together wrong, and the event has not occurred, the chance of which is

$$\overline{1-a_1}\,\overline{1-a_2}\ldots\overline{1-a_n}\,\overline{1-p}.$$

Hence the resulting probability of the event is

$$\mathrm{P}=\frac{a_1\,a_2\ldots a_n p}{a_1\,a_2\ldots a_n p+\overline{1-a_1}\,\overline{1-a_2}\ldots\overline{1-p}}.$$

From the first formula it follows that, if $a=\frac{1}{2}$, $\mathrm{P}=p$; that is to say, if this witness attests an event, of the chances of which we know nothing, his affirmation is enough to make the odds on it even; but if we have any other means of estimating the chances, his testimony goes for nothing. This is certainly not practically true. But the second formula leads to a result more obviously absurd. Suppose $a_1=a_2=\ldots=a_n=\frac{1}{2}$; then in this case again $\mathrm{P}=p$; that is to say, if any number of witnesses, supposed to be wholly independent, give coincident testimony, it adds nothing to the probability of the event, unless each separately is sufficiently credible to turn the odds in its favour. If the odds are against the truth of each witness separately, then the greater their number the less credit is due to their testimony. The weight usually attributed to the coincidence of independent witnesses is inconsistent with the formula, according to which agreement in truth is just as unlikely as agreement in falsehood. A calculation which yields such results as these must be erroneous, or imply conditions very different from those of ordinary experience. In fact we have taken no account of the datum that error is manifold, while truth is one.

Laplace analyzes the case of single witnesses as follows. His method generally is, having stated the chances of every possible case, to multiply each by the chances of the hypothesis in that particular case. Thus, suppose an urn contains 100 balls numbered consecutively, and one is drawn. The witness A, who knows how many balls there are, affirms that the ball drawn was No. 25. Let us suppose his general credibility to be $\frac{5}{6}$; we have the following cases:—

Hypothesis.—A affirms No. 25.

Either he is right $=\frac{5}{6}$, and it was drawn $=\frac{1}{100}$. In this case the hypothesis is certain, therefore the chances of this supposition $=\frac{5}{6}\frac{1}{100}=\frac{1}{120}$.

Or he is wrong $=\frac{1}{6}$, and No. 25 was not drawn, $=\frac{99}{100}$; but this must now be multiplied by the chance of the hypothesis in this case, *i. e.* the chance that when A is wrong, his false testimony will be borne to No. 25. There being 99 balls not drawn, any of which might be asserted falsely, this chance is therefore $\frac{1}{99}$, and the total chances of this last supposition $=\frac{1}{6}\frac{99}{100}\frac{1}{99}=\frac{1}{600}$.

The chances of the two suppositions respectively are therefore $\frac{1}{120}$ and $\frac{1}{600}$; and as these exhaust all possible cases, the total probability of the event affirmed is $\frac{\frac{1}{120}}{\frac{1}{120}+\frac{1}{600}}=\frac{5}{6}$, *i. e.* it is the same as the witness's general credibility. The erroneous formula first quoted would have given as the resulting probability $\frac{\frac{1}{120}}{\frac{1}{120}+\frac{99}{600}}=\frac{5}{104}$.

Laplace, it will be seen, has taken account of the diversity of error; yet his conditions do not correspond perfectly with those of actual experience. In fact the resulting probability in the case supposed was found to depend solely on the credibility of the witness, not at all on the antecedent probability of the event. This is a consequence of the supposition that the witness knows the number of balls in the urn; so that the number of possible errors is 99, and the odds against any particular ball are also 99 to 1. In fact the chance of any ball being named at random is the same as the chance of its being drawn. But there is a further condition implied in the mode of stating the problem. Let it be supposed that there are 2 white balls, 3 yellow, 5 red, in all 10. The witness asserts that white is drawn, his credibility being still supposed $=\frac{5}{6}$. The chances in favour of his assertion are

$$\frac{5}{6}\cdot\frac{2}{10}=\frac{1}{6}.$$

If he is wrong, either yellow was drawn or red. The chances of the former are $\frac{3}{10}$; and as there are 7 balls not of this colour, of which 2 are white, and the witness knows this, the chances of white being falsely named in this case $=\frac{3}{10}\frac{2}{7}=\frac{3}{35}$. Similarly, the chances of a false assertion of white when red is drawn $=\frac{5}{10}\frac{2}{5}=\frac{7}{35}$. Total false assertions of white $=\frac{10}{35}$, to be multiplied by the chance that A speaks falsely $=\frac{1}{6}$.

Hence, finally, the probability of A's assertion is

$$\frac{\frac{1}{6}}{\frac{1}{6}+\frac{1}{6}\frac{10}{35}}=\frac{7}{9}.$$

In general, on this supposition, if there are n cases whose probabilities are p_1, p_2, &c., the sum of these being Σ, the chances of a false assertion of p_1 are

$$\frac{p_1}{\Sigma}\left(\frac{p_2}{\Sigma-p_2}+\frac{p_3}{\Sigma-p_3}+\ldots\right).$$

Call the quantity within the brackets C; then if the credibility of the witness be a, and he asserts the event p_1, the probability that he is right is

$$\frac{a}{a+(1-a)\mathrm{C}}.$$

If we suppose that the witness knows the colours of the balls, but not the number of each colour, the case is different. Suppose, with the same numbers, he affirms white. Either he is right, and it is drawn

$$=\tfrac{1}{6}.$$

Or he is wrong, and selects white from the *two* colours not drawn,

$$=\tfrac{1}{6}\tfrac{8}{10}\tfrac{1}{2}=\tfrac{1}{15}.$$

Resulting probability of white

$$=\frac{\frac{1}{6}}{\frac{1}{6}+\frac{1}{15}}=\frac{5}{7}.$$

In general, if there be n cases p_1, p_2, &c. as above, and the witness, knowing only the number of possible cases, asserts the event p_1, the probability that he is right is

$$\frac{(n-1)ap_1}{(n-1)ap_1+\overline{1-a}\,(\Sigma-p_1)}.$$

Thus it is clear that on either hypothesis the case assumed by Laplace is not general. The condition involved in it, stated generally, will be seen to be, that, if the antecedent probability of the event named be p, the chances of its being falsely named when it has not occurred are

$$\frac{p}{1-p}.$$

Now let us see how Laplace treats the case of a witness attesting a fact of given antecedent probability.

Suppose there are 100 balls, of which one is white and the rest black. The witness whose credibility is $\frac{5}{6}$ asserts that the ball drawn was white. The cases are:—

Hypothesis.—A affirms white. Either he is right $=\frac{5}{6}$, and white was drawn $=\frac{1}{100}$;

chances in favour of this supposition $=\frac{5}{600}$.

Or he is wrong $=\frac{1}{6}$, and black was drawn $=\frac{99}{100}$;

chances in favour of this supposition $=\frac{99}{600}$.

For on this supposition the hypothesis is certain, inasmuch as A, if he is wrong, must bear witness to white. Hence the total probability of white is

$$\frac{5}{5+99}=\frac{5}{104}.$$

In general if the whole number of balls be n, of which one only is white, the rest being black, and the witness whose credibility is a asserts that white was drawn, the probability of this event is evidently

$$\frac{a}{a+(1-a)(n-1)}.$$

The probability that the assertion is not true is

$$\frac{(n-1)(1-a)}{a+(n-1)(1-a)}.$$

If n is very large, this will not differ much from unity, *i. e.* certainty, if we suppose even a slight chance of mistake or deception on the part of the witness. Now this is the case of an extraordinary event; and the result, adds Laplace, confirms the judgment of common sense, that such an event requires far stronger evidence to prove it than an ordinary one.

On the same conditions, if several (say r) witnesses join in attesting the drawing of white, then if their credibility is the same, the resulting probability is

$$\frac{a^r}{a^r+(1-a)^r(n-1)}.$$

This corresponds with the formula quoted at the beginning of this paper, and the same consideration shows that it is not applicable to such cases as are met with in ordinary experience. Suppose, for instance, the number of balls is 10, and three independent witnesses testify that white was drawn. If the credibility of each is $\frac{2}{3}$ (*i. e.,* it is 2 to 1 that his unsupported testimony is true), then the three together do not make it an even chance that white was drawn. The odds are still 9 to 8 against it; and, as before noticed, if the testimony of these is supported by a number of independent witnesses whose average credibility is only $\frac{1}{2}$, it does not become a whit more probable. Common sense shows that this is to allow too much to the extraordinary character of the event, and that some condition must be involved in the calculation which does not apply to the events of ordinary life.

One such condition was introduced by the assumption that when the witness is mistaken or deceived, there is no diversity of error possible. There are only two colours, and he knows this; therefore if black is drawn and he is wrong, it can only be by testifying to white. In order to apply such a formula to the evidence for an extraordinary event in general, we must assume that, supposing it not to have occurred, any error on the part of the witnesses must have led to its being reported. Of course whenever this can be shown, the circumstance detracts immensely from the weight of the testimony; but it is very far indeed from being generally the case. Before examining the problem more generally, it is worth while to show that Laplace's formula applies only to the particular case which he has selected; and that the extension of it by subsequent writers, who substitute p (the antecedent probability) for $\frac{1}{n}$, is fallacious, even when we frame our conditions in perfect analogy with those of Laplace. Thus suppose, besides the 99 black and one white ball, we put into the urn 99 blue and one yellow, 99 red and one green. The drawing of white is exactly as extraordinary as before, and the chances against it greater, viz. $\frac{299}{300}$. Nevertheless when it is affirmed its probability becomes (the witness's credibility being $\frac{5}{6}$)

$$\frac{5}{5+\frac{299}{5}}=\frac{25}{324},$$

or more than half as much again as in the former case. We must therefore investigate the problem under more general conditions.

Suppose then that the witness A does not know anything about the colours or number of the balls in the urn; his affirmations, therefore, may range over all possible colours, say m. Now if he states that the ball drawn was white, we have the following cases:—

Hypothesis.—A affirms white. Either he is right and white is drawn,

$$\frac{a}{n};$$

or he is wrong and black is drawn,

$$(1-a)\left(\frac{n-1}{n}\right);$$

on which supposition the probability of the hypothesis, *i. e.* the chance that he affirms white out of the $m-1$ colours which are not drawn, is

$$\frac{1}{m-1}.$$

The chances in favour of this supposition therefore are

$$\overline{1-a}\,\frac{n-1}{n}\,\frac{1}{m-1}.$$

Hence the probability of the event affirmed

$$=\frac{(m-1)a}{(m-1)a+(1-a)(n-1)}.$$

If there are several (r) independent witnesses who give the same evidence, then, supposing their credibility to be the same, the probability of the event is

$$\frac{(m-1)^r a^r}{(m-1)^r a^r+(1-a)^r(n-1)}.$$

In the example above given of 10 balls and 3 witnesses each of credibility $\frac{2}{3}$, if we suppose only four colours capable of being mentioned, this formula gives as the probability of the event $\frac{24}{25}$; that is, the odds in favour of the event are 24 to 1 instead of being only 8 to 9.

If now we return to the case put by Laplace, it may be asked, is it possible that the circumstance of the witness knowing or not knowing that there are only two colours in the urn can make such an enormous difference in the credibility of his testimony? I answer no; and for this reason, that a does not represent the same quantity in both cases. This will be at once obvious from the following consideration. When there are 100 numbered balls, a man whose announcements are made altogether at random will be right only once in a hundred times; but a man whose credibility is $\frac{1}{2}$, *i. e.* who is right fifty times in a hundred, is a good witness. Two such witnesses agreeing would be equivalent to one whose credibility is $\frac{99}{100}$. But if there are fifty black and fifty white balls, so that there are only two possibilities to choose from, and the chances of these are equal, the random speaker will be right fifty times in a hundred. In this case, therefore, the credibility represented by $\frac{1}{2}$ is no credibility at all, and two such witnesses are accordingly of no more value than one; so that the same degree of credibility is represented in the one case by $\frac{1}{100}$, and in the other by $\frac{1}{2}$. The credibility, therefore, which was represented by $\frac{1}{2}$ in the first case ought to be represented by a greater fraction in the second; the witness has not lost all his credibility because the event itself has become more probable. It is clear, then, that a represents not the antecedent credibility of the witness, but the chances of his announcement being right under the particular conditions supposed.

To consider the matter practically:—the chances of the witness being mistaken in the colour of a black ball are the sum of the chances of his taking it for white, for yellow, for red, and the rest of the m colours. Now if it is known that there are only two colours in the urn, he is excluded from $m-2$ of the $m-1$ possible mistakes, but we have no reason to suppose that when he is disposed to mistake black for red for example, he has no choice but to affirm white if he knows that red is not present. If we know nothing further about the case, we must suppose that in all such instances (*i. e.* where he mistakes black or white for a colour known not to be present) he has no reason for choosing either black or white, and therefore divides his assertions equally between them. We have then the following cases:—

Hypothesis.—He affirms white, the probability of which is p, and his credibility a.

Case 1. He believes that white is drawn, and it is so;

$$\text{chances in favour of this supposition} = ap.$$

Case 2. White is drawn, and he mistakes it for some colour known to be absent;

$$\text{chances} = p\,\frac{m-2}{m-1}(1-a).$$

In half of these instances he affirms black, and the other half white; hence

$$\text{chances that he affirms white on this supposition} = (1-a)p\frac{m-2}{2m-2}.$$

Case 3. Black is drawn, and he mistakes it for white;

$$\text{chances} = (1-a)(1-p)\frac{1}{m-1}.$$

Case 4. Black is drawn, and he mistakes it for an absent colour,

$$(1-p)\frac{m-2}{m-1}(1-a),$$

in half of which instances he affirms white; hence

chances that white is affirmed on this supposition

$$=(1-a)(1-p)\frac{m-2}{2m-2}.$$

On the whole, therefore, he affirms white in

$$ap+(1-a)p\frac{m-2}{2m-2}+(1-a)(1-p)\frac{m-2}{2m-2}\text{ cases,}$$

of which he is right in $ap+(1-a)p\frac{m-2}{2m-2}$ cases. Hence the probability that white was drawn is

$$\frac{ap+(1-a)p\frac{m-2}{2m-2}}{ap+(1-a)p\frac{m-2}{2m-2}+(1-a)(1-p)\frac{m-2}{2m-2}};$$

or putting $a+(1-a)\frac{m-2}{2m-2}=\alpha$, it is

$$=\frac{\alpha p}{\alpha p+(1-\alpha)(1-p)}.$$

His credibility in these particular circumstances, therefore, is not a, but α. When m is considerable, α nearly $=\frac{a+1}{2}$; *i. e.* the odds in his favour, instead of a to $1-a$, are now $1+a$ to $1-a$, or more than double.

The result will be the same if we introduce the supposition that the witness intends to deceive, since the motives which usually affect his veracity cannot be supposed to operate uniformly in favour of the assertion of white when the remaining colours are excluded. Indeed we ought rather to suppose that in the absence of these colours a proportionate number of the motives to deception disappears; so that if his veracity be v and his judgment r, he now affirms white truly in $ap(1-v)\frac{m-2}{m-1}$ cases, in which, if the other colours were present, he would be induced to affirm some one of them falsely.

But it is useless to pursue this hypothesis any further. Enough has been said to show that it is altogether unfit to furnish a general formula. It would be quite as reasonable to neglect the probability of the event, and limit ourselves to that of its assertion, as to neglect the latter probability altogether. The general formula already given is independent of any hypothesis with respect to the knowledge of the witness, or the proportion between extraordinary and ordinary events in general. It may be stated somewhat differently thus:—A witness whose credibility is a, announces an event, the antecedent probability of which is p, and the chance of its being announced without reason r; then the resultant probability is

$$\frac{ap(1-r)}{ap(1-r)+(1-a)(1-p)r}.$$

If complete generality is desired, we must introduce separately the chances of the witness being mistaken k, and of his intending to deceive f, and also the chances of the event being invented i, or being erroneously supposed to have happened s. It may be worth while to exhibit the formula in this shape. It is obvious that when the event has actually happened, it may or may not be that the causes which would lead to its being either invented or falsely believed have also existed, i. e. pi and ps are possible cases. This consideration simplifies the formula, which will be found to give for the resulting probability of the event announced

$$\frac{p\{(1-f)(1-k)+fki\}}{p\{(1-f)(1-k)+fki\}+(1-p)\{(1-f)ks+fi\}}.$$

When $r=p$, the probability just given is reduced to a, and accordingly it frequently happens that, although the event announced is extraordinary, the chances of fiction or mistake may be proportionately small, and in such cases we are satisfied with ordinary testimony.

Mr. J. S. Mill has some useful observations on Laplace's formula in the chapter on the "Grounds of Disbelief" in his 'System of Logic.' He draws attention to the absolute identity supposed by hypothesis to exist between the 99 black balls, which renders the case unlike that of real events. I have not referred to this, because in fact it results from the nature of the problem to be solved, in which we compare events of different degrees of antecedent probability; the 99 black balls are not intended to represent 99 similar events, but one and the same event. For the purposes of calculation, the chances in favour of an event must be treated as representing so many cases of its occurrence. When we say that the chances are 9 to 1 in favour of a certain horse A beating another horse B, there are only two events conceivable, and only two sets of motives, &c. possible: we do not conceive A as made up of 9 parts, each having an equal chance of victory with B. We may speak, indeed, of 10 trials in which A will win 9 times; but in each trial both horses and both sets of motives are equally present. To express degrees of probability, then, the most convenient method is to suppose a proportionate number of identical events, as Laplace has done.

It is important, however, to consider that testimony, true or false, is not given without motives; and therefore it may seem more proper that the formula should represent the operation of these. Let the probability of the event be p; the probability that its occurrence would move the witness to announce it truly $=t$. The probability of the event not happening is $1-p$; let the chances that its non-occurrence would move the witness to deny it truly $=d$. This may not be the same as t.

Now let the chances that a motive to lie exists be α, and the chances of A's yielding to it be ϕ, *i. e.* the chances of his lying $=\alpha\phi=v$. Then, supposing that A announces the event p, which is one of the possibilities:

Either the event occurs and moves him to speak the truth, the motives to lying or to mistake not operating $=pt(1-v)$.

Or the event does not occur, which non-occurrence has no effect on the witness, the motives to lying operating $=(1-p)(1-d)v$; but this must be multiplied by the chances of the hypothesis, *i. e.* of A fixing on this particular event, on this supposition, namely $\frac{1}{m}$. This gives

$$(1-p)(1-d)\frac{v}{m}.$$

Resulting probability that the event announced has occurred

$$=\frac{pt(1-v)}{pt(1-v)+(1-p)(1-d)\frac{v}{m}}.$$

If there are r witnesses, and the quantities t, d, v the same for all, we have

$$\frac{pt^r(1-v)^r}{pt^r(1-v)^r+(1-p)(1-d)^r\frac{v^r}{m^r}}.$$

In this formula t may represent, for example, the interest which the witness would have in truly reporting the event if it happened, and d his interest in truly reporting its non-occurrence.

The case of arguments conspiring or opposed is somewhat different from that of testimony; since an argument may be fallacious, and yet the conclusion true. In the case of arguments establishing the same conclusion with the probability a, b, &c. respectively, the resulting probability is clearly

$$1-(1-a)(1-b)\ldots$$

If two arguments are opposed, we have these cases:

the argument a is invalid, b invalid $=a(1-b)$;
a invalid, b valid . . . . . . $=b(1-a)$;
neither is valid . . . . $=(1-a)(1-b)$;
Sum . . $=1-ab$.

Hence the probability of the conclusion to which a leads

$$=\frac{a(1-b)}{1-ab}.$$

Now the case of arguments suggests an important question. It often happens that when a proposition has been established on probable evidence, it is found to lead to a further inference which admits of being tested directly to some extent. Thus the proposition A may assert that a certain narrative is the work of a credible witness, and B that a particular circumstance contained in it is true. Our hypothesis is, If A is true, B is true; and from this we infer conversely, If B is false, A is false. Logically these two propositions are convertible; yet there lurks a great fallacy in the inference when we have to do, not with certain, but with probable propositions. For example, if the truth of B does not follow with certainty from that of A, we have the following cases possible:—

A true and B true; let the probability of this be λ,
A true and B false; " " $=\mu$,
A false and B true; " " $=\nu$,
A false and B false; " " $=\rho$,

where

$$\lambda+\mu+\nu+\rho=1.$$

Then if A is true, the probability that B is true is

$$=\frac{\lambda}{\lambda+\mu}.$$

If B is false, the probability that A is false is

$$=\frac{\rho}{\rho+\mu}.$$

The former expression represents the probability of the statement, If A is true, B is true: the latter that of the converse, If B is false, A is false; and it appears at once that these are really independent. Suppose it to be known that the third case is impossible, or nearly so, i. e. $\nu=0$; then, in order to determine the probability of the second inference, we must know besides that of the first, the absolute probability of A (or B). Let the probability of A be $v(=\lambda+\mu)$; then the probability of the inference of the truth of B from that of A

$$=\frac{\lambda}{v},$$

and that of the converse

$$=\frac{1 \quad v}{1-\lambda}.$$

Let us analyze the problem more in detail. Suppose the probability of A as proved directly

$$=w,$$

the probability of the inference by which B is deduced from A

$$=y,$$

and the probability of the disproof of B=X, where, if x_1, x_2, &c. be separate arguments against B,

$$X=1-(1-x)(1-x_2)\ \ldots\ldots$$

Then calling the inference C, we have the following cases:—

1. A, B, and C all true; probability $wy(1-X)$.
2. A true, B and C false „ $w(1-y)X$.
3. A and B true, C false „ $w(1-y)(1-X)$.
4. A false, B and C true „ $(1-w)y(1-X)$.
5. A and B false, C true „ $(1-w)yX$.
6. A and C false, B true „ $(1-w)(1-y)(1-X)$.
7. All three false „ $(1-w)(1-y)X$.

Sum $= 1-wyX$.

the only supposition which is necessarily impossible being, that A and C are true, and B false. Hence on the whole the probability of A

$$=\frac{w(1-yX)}{1-wyX};$$

of the inference C

$$=\frac{y(1-wX)}{1-wyX};$$

of B

$$=\frac{1-X}{1-wyX}.$$

The odds in favour of A are $w-wyX$ to $1-w$; that is, they are diminished in proportion to the strength of y and X combined; the odds in favour of B are $1-X$ to $X-wyX$; these, therefore, are increased similarly. We deduce the important consequence that, as regards the evidence of A, it is precisely the same thing whether we strengthen the arguments or objections against B, or strengthen the force of the inference of B from A. If it be held that this inference is certain, *i. e.* that $y=1$, then the odds in favour of A are $w(1-\mathrm{X})$ to $1-w$, and every argument against B tells with equal force against A. Let us take a numerical illustration.

Let $w=\frac{99}{100}$, $y=\frac{1}{2}$, and $\mathrm{X}=\frac{9}{10}$; then $wy\mathrm{X}=\frac{891}{2000}$, and the probability of A is $\frac{1089}{1109}$, *i. e.* the odds in its favour are less than 55 : 1. With this value of y, the probability of A cannot be reduced below $\frac{99}{101}$, its value when $\mathrm{X}=1$, the odds being then $49\frac{1}{2}$ to 1. But if $y=\frac{9}{10}$, then $wy\mathrm{X}=\frac{8019}{20000}$, and the odds in favour of A are reduced to 1881 to 100, less than 19 to 1. Whatever be the value of w, if $y=\frac{1}{2}$ and $\mathrm{X}>\frac{1}{2}$, then the common denominator is $<1-\frac{1}{4}w$, and the odds in favour of A are less than $\frac{3}{4}w$ to $1-w$. Thus if $y=\frac{3}{4}$, and $\mathrm{X}=\frac{2}{3}$, the odds are only $\frac{1}{2}w$ to $1-w$.

IV. *Notes on Scientific History.* By JOHN TYNDALL, *F.R.S.*[*]

I.

1. TWO years ago, in a Friday evening discourse at the Royal Institution[†], I drew attention to the scientific labours of Dr. Julius Robert Mayer, and since that time the knowledge of his writings has been widely diffused by the publication in English of four of the five memoirs which he completed before his health gave way. A translation of Mayer's first paper (date 1842) will be found in the Philosophical Magazine, S. 4. vol. xxiv. p. 371. A *résumé* of this paper, written by myself, appears in the Philosophical Magazine, S.4. vol. xxv. p. 378. A translation of his third paper (date 1848) will be found in the Philosophical Magazine, S. 4. vol. xxv. pp. 241, 387, 417. A translation of his fourth paper (date 1851) in the Magazine, S. 4. vol. xxv. p. 493. No translation of his second paper (date 1845) has yet been published. Circumstances have recently compelled me to refer to this Essay; and pending its full translation, I would ask permission to make such a *résumé* of its contents as will give the readers of this journal some notion of its merits. The extracts will show the relationship of its author to other writers with whom he has been recently compared. From the works of these writers, moreover, I shall extract the portions on which their claims mainly rest, and thus the public will be enabled to form an independent estimate of this passage in scientific history.

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[*] Communicated by the Author.

[†] Proceedings of the Royal Institution, June 1862. Phil. Mag. vol. xxiv. p. 57.

2. Mayer was led from the contemplation of organic nature to the publication of his first paper "On the Forces of Inorganic Nature." An observation made in 1840, on the blood of a patient in a tropical climate, was the origin of his scientific writings. It led him to the consideration of those physical forces on which the phenomena of vitality depend. This, if the laws of life were ever to become amenable to scientific investigation, he knew must be his starting-point. The paper now under consideration may be broadly divided into two parts, in the first of which he deals with the law of the conservation of energy* as it manifests itself in inorganic nature, and in the second of which he applies the law to the phenomena of life.

3. At the outset of this paper he announces, as he had previously done in that of 1842, the indestructibility of force, its convertibility, and its quantitative constancy. Chemistry, he says, teaches the qualitative changes which matter undergoes under different circumstances, the *form* of the matter and not its *amount* being changed. What chemistry does for matter, physics must do for force; the force is as unalterable as the matter, and the function of physics is to study force in its forms, and to ascertain the conditions of its metamorphoses. This is the sole problem with which natural philosophy has any concern; for as to the creation or annihilation of force, either act lies as much beyond the range of human thought as of human power.

4. For thousands of years men have employed the powers of inorganic nature to obtain mechanical effects. But to the forces of moving air and of falling water a new force has been added in modern times—the force of *heat*, which may be converted into mechanical effect. Supposing that to a train weighing 100,000 lbs. a velocity of 30 feet a second is to be imparted; this may be done by the expenditure of ordinary mechanical force—by permitting, for example, the train to roll down an incline until the required velocity has been obtained. Trains, however, in general move without this exercise of gravitating force, and, despite the friction of their parts, they are kept in motion. Let this friction be supposed equivalent to a rise of 1 in 150, then with a velocity of 30 feet a second the weight of the train will be lifted 720 feet in an hour, which corresponds to the work of about forty-five horses. This large quantity of generated motion implies the expenditure of an equal amount of force. The force expended in the case of the locomotive is *heat*.

The quantity of heat taken up by the steam employed to work the engine is greater than that which can be obtained from the recondensation of the steam. The difference between both

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* Rankine's terminology.

is the heat usefully applied; that is to say, this difference expresses the heat which has been converted into mechanical effect. The more perfect the machine, the less will be the amount of heat obtainable from the condensation of the steam. The best engines give a difference of about 5 per cent.; that is to say, 100 lbs. of coal, burnt in such a machine, give no more heat than 95 lbs. which are burnt without doing any work.

(Considering the sort of criticism to which he has been repeatedly subjected*, the manner in which Mayer establishes the result last mentioned is worthy of particular attention. It will be observed that he deliberately chooses a substance which experiment proves to be suited to his purpose—a substance, that is, in which *the whole* of the heat rendered latent is consumed in *exterior* work.)

5. To prove this important proposition, we must investigate the relationship of elastic fluids to heat and to mechanical work. Gay-Lussac has proved by experiment, that when an elastic fluid passes from one vessel into a second one of the same size, but exhausted, the vessel from which the elastic fluid issues is cooled, while that into which it enters is warmed by exactly the same number of degrees. This experiment, which is distinguished for its simplicity, shows that a given weight and volume of an elastic fluid may expand to double, quadruple, &c. of its previous volume without experiencing any change of temperature, or, in other words, that for the simple expansion of the gas no expenditure of heat is necessary.

6. Let a cubic inch of air at 0°† and under a pressure of 28 inches of mercury, be heated to 274°, and let the quantity of heat required to warm the air be x. When it streams into another exhausted recipient of the same volume, the air will retain its temperature of 274°; the medium surrounding the vessels will undergo no change of temperature. Again, let a cubic inch of air, not at *constant volume* but under a *constant pressure* of 28 inches of mercury, be heated from 0° to 274°, a greater quantity of heat is now needed than before: let the quantity be $x+y$. If the air be permitted to cool in the two cases, it will give back the heat communicated to it. The air which is not followed by a pressure will, on cooling from 274° to 0°, give out the heat x; that which cools under a constant pressure will yield the heat $x+y$.

7. Steam in the engine, where it expands under the piston,

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* As an example, see 'Good Words,' October 1862, p. 604, note:—"Mayer's statements imply its indiscriminate application to all bodies in nature, whether gaseous, liquid, or solid." Not what Mayer's words "imply," but what they *are* is stated in the text.

† All through his papers Mayer uses Centigrade degrees.

behaves like the air under constant pressure. The heat necessary to the expansion of the steam is X+Y. When the steam is cooled, the pressure of the piston is absent, or it is exercised in a greatly diminished degree: hence, in cooling, the heat given out will be X. With every stroke of the piston, therefore, there is the loss of heat Y; that is to say, *with the action of the engine a consumption of heat is inseparably connected.*

8. From the quantity of fuel consumed in an engine, the total expenditure of heat may be calculated. The loss by radiation, transmission, and convection being subtracted, the remainder is the usefully applied heat. As, however, by far the greater part of the unused heat can be but roughly estimated, only an approximate result can be thus obtained. More sharply and more simply the problem may be solved by calculating the quantity of heat rendered latent when a gas expands under pressure. Let the amount required to heat a gas at constant volume 1° be x; to produce the same elevation of temperature under constant pressure the heat necessary will be $x+y$. Let the weight raised in the latter case be P, and the height to which it is raised h; then we have

$$y=\mathrm{P}\times h.$$

A cubic centimetre of atmospheric air at 0° and 76 millims. barometric pressure weighs 0·0013 of a gramme; warmed 1° under constant pressure, it expands $\frac{1}{274}$th of its volume, and lifts a mercury column 76 centimetres long and of a square centimetre basis to a height of $\frac{1}{274}$th of a centimetre.

The weight of this column is 1033 grammes; the specific heat of air, according to De la Roche and Berard, is 0·267; hence the heat communicated to our cubic centimetre of air in order to raise its temperature 1° is equal to that which would raise the temperature of $0{\cdot}0013\times0{\cdot}267=0{\cdot}000347$ of a gramme of water 1°.

According to Dulong, the specific heat at constant pressure is to that at constant volume as 1·421 : 1; therefore the quantity required to raise the temperature of our cubic centimetre of air at constant volume 1° would be sufficient to heat

$$\frac{0{\cdot}000347}{1{\cdot}421}=0{\cdot}000244$$

of a gramme of water 1°.

Hence the difference $(x+y)-x$, or

$$y=0{\cdot}000347-0{\cdot}000244=0{\cdot}000103$$

thermal units, by which a weight $\mathrm{P}=1033$ grammes is raised to a height $=\frac{1}{274}$th of a centimetre. Reducing these numbers, we find

1 thermal unit (1 gramme of water heated 1° C) = 1 gramme

$$\text{raised to a height of}\begin{cases}\text{367 metres}\\ \text{1130 Par. feet.}\end{cases}$$

This is Mayer's calculation of the mechanical equivalent of heat. He first published the result in 1842, making use of the specific heat of air as determined by De la Roche and Berard. Substituting for it the subsequent and more accurate determination of M. Regnault, and changing in no particular the method of calculation, Mayer's equivalent, instead of 367, becomes 426 kilogrammetres; Joule's equivalent is 425.

II.

It has been many times affirmed that, in the calculation of the mechanical equivalent of heat, M. Séguin had anticipated Dr. Mayer by three years—that he had, in fact, pursued the same method and published the "same result." M. Séguin's book is in but few hands; I shall therefore give, in his own language, the details of his calculation*.

9. "Supposons donc que l'on ait renfermé dans un cylindre ABCD, ayant un mètre de section, un mètre cube de vapeur à 100°, et que cette vapeur soit contenue par un piston CD, dont le poids équivaut à un kilogramme par centimètre carré, et derrière lequel on a fait le vide; ce qui représente, à peu de chose près, une pression égale à celle que l'atmosphère exerce sur tous les corps au niveau de la mer. L'appareil, d'ailleurs, étant disposé de telle sorte qu'il ne puisse ni céder ni recevoir du dehors aucune portion de calorique.

10. "Si l'on augmente la charge du piston CD, en y ajoutant successivement des poids pour comprimer la vapeur, jusqu'à ce que sa température se soit élevée de 20°, son ressort fera alors équilibre à une pression de 2 kil. par centimètre carré; et, considérant que son volume augmente de 0·00375 de ce qu'il était à 100° par chaque degré de température, l'espace ABFE qu'elle occupera sera exprimé par

$$\frac{1+1\times 20\times 0{\cdot}00375}{2}=0{\cdot}5375.$$

On pourra donc considérer l'effet comme sensiblement représenté par la moyenne de toutes les pressions exercées par la vapeur depuis DC jusqu'en EF multipliée par l'espace parcouru DE.

11. "La pression étant de 1 kil. en DC et de 2 kil. en EF,

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* *Sur l'Influence des Chemins de Fer* (Paris, 1839), pp. 385–389 inclusive.

et croissant en progression géométrique, en désignant par S la somme des termes, par n le nombre des termes, le dernier, a le premier, et q la raison, P la pression moyenne; faisant $n=100$, ce qui suffit pour obtenir une valeur de P assez approchée, et observant que la valeur de l ou la pression de la vapeur en EF, est égale à 2 kil. par centimètre carré, et celle de a qui se rapporte à CD, égale à 1 kil., nous aurons pour déterminer q

$$l=aq^{n-1}, \quad q=\sqrt[99]{2}=1{\cdot}007,$$

$$\mathrm{P}=\frac{a(q^n-1)}{n(q-1)}=\frac{1(1{\cdot}007^{100}-1)}{100(1{\cdot}007-1)}=1{\cdot}43.$$

Multipliant cette valeur par l'espace DE parcouru par le piston, égal à $\mathrm{AD}-\mathrm{AE}=1-0{\cdot}5375=0{\cdot}4625$ et par 10,000 qui représente le nombre de centimètres carrés contenus dans un mètre carré, on obtient

$$1{\cdot}43\times0{\cdot}4625\times10{,}000=6613 \text{ kil.};$$

ce qui nous indique que l'effet théorique obtenu par la détente d'un mètre cube de vapeur comprimée par un poids de 2 kil. par centimètre carré, qu'on laisse répandre dans un espace qui répond à une pression de 1 kil. et à un abaissement de température de 20°, est représenté par un poids de 6·613 kil. élevé à un mètre, ou par 6°·613.

12. "En faisant un calcul analogue pour connaître les espaces qu'occupe la vapeur, lorsqu'on augmente sa pression de manière à faire élever sa température de 20 en 20 degrés, on trouvera

"1. Que pour 140° la pression en $\mathrm{GH}=3^{\text{kil}}{\cdot}61$,

$$\mathrm{ABHG}=\frac{1+1\times40\times0{\cdot}00375}{3{\cdot}61}=0^{\text{m}}{\cdot}319.$$

$$\mathrm{GE}=0{\cdot}537-0{\cdot}319=0^{\text{m}}{\cdot}218; \quad \mathrm{P}=2^{\text{kil}}{\cdot}83;$$

et pour l'effet total,

$$2{\cdot}83\times0{\cdot}218\times10{,}000=6170 \text{ kil.}$$

"2. Pour 160° la pression en IK $6^{\text{kil}}{\cdot}15$,

$$\mathrm{ABKI}=\frac{1+1\times60\times0{\cdot}00375}{6{\cdot}15}=0^{\text{m}}{\cdot}199,$$

$$\mathrm{IG}=0{\cdot}319-0{\cdot}199=0^{\text{m}}{\cdot}120, \quad \mathrm{P}=4^{\text{kil}}{\cdot}82;$$

et pour l'effet total,

$$4{\cdot}82\times0{\cdot}128\times10{,}000=5780 \text{ kil.}$$

"3. Enfin, pour 180° la pression $\mathrm{LM}=9^{\text{kil}}{\cdot}93$,

$$\mathrm{ABLM}=\frac{1+1\times80\times0{\cdot}00375}{9{\cdot}93}=0^{\text{m}}{\cdot}131,$$

$$LI = 0{\cdot}199 - 0{\cdot}131 = 0^{\text{m}}{\cdot}068, \quad P = 8^{\text{kil}}{\cdot}00;$$

et pour l'effet total,

$$8{\cdot}00 \times 0{\cdot}068 \times 10{,}000 = 5440 \text{ kil.}$$

13. "Si nous supposons ensuite que lorsque la vapeur pousse le piston devant elle, et que la chaudière est en communication avec le cylindre, sa température s'abaisse d'une quantité proportionnelle à l'effet dynamique produit, nous trouverons que la vapeur s'introduisant dans le cylindre à 100°, et perdant 20° pendant le mouvement du piston, la température, à la fin de la course, sera de 80°, et la pression de $0^{\text{kil}}{\cdot}485$. La pression moyenne $0^{\text{kil}}{\cdot}727$. Soit pour l'effet total,

$$0{\cdot}727 \times 1 \times 10{,}000 = 7270 \text{ kil.},$$

valeur qui se trouve à peu près classée suivant la même loi que les autres quantités auxquelles nous sommes parvenus, en considérant l'effet produit par la vapeur à des températures et à des pressions plus élevées.

14. "En réunissant tous ces résultats, et en les comparant à l'élévation de température qui leur correspond, nous formerons le tableau suivant:—

Pressions en kilogrammes.	Températures réelles.	Effet produit en kilogrammes élevés à 1 mètre.	Différences.	Températures correspondantes à l'effet produit.	Différences.
0·48	80°	7270	....	20°	
1	100	6613	657	18·20	1·80
2	120	6170	443	16·97	1·23
3·61	140	5780	390	15·90	1·07
6·15	160	5540	240	15·24	0·66"
9·93	180				

15. We have now the means of comparing Séguin's alleged calculation of the mechanical equivalent of heat with that of Mayer. With reference to the foregoing Table, Mr. Joule writes thus (Phil. Mag. August 1862):—"In page 389 he [M. Séguin] gives a Table of the quantity of mechanical effect produced corresponding to the loss of temperature of steam on expanding. From this it appears that 1° Cent. corresponds with 363 kilogrammes raised to the height of 1 metre. . . . . . Mayer discourses to the same effect as Séguin, but at greater length, with greater perspicuity, and more copious illustration. He adopts the same hypothesis as the latter philosopher, viz. that the heat evolved on compressing an elastic fluid is exactly the equivalent of the compressing force, and thus arrives at the same equivalent, viz. 365 kilogrammes per 1° Cent."

16. In the Philosophical Magazine for April 1863, Prof. William Thomson of Glasgow, and Prof. Tait of Edinburgh, express themselves thus:—"Does Prof. Tyndall know that Mayer's paper has *no claims to novelty or correctness* at all, saving this, that by a lucky chance he got an approximation to a true result from *an utterly false analogy*; and that even on this point he had been anticipated by Séguin, who, three years before the appearance of Mayer's paper, had obtained and published the same result from the same hypothesis." I have nowhere in this paper introduced italics into quotations; wherever they occur they are in the original.

17. And in reference to the same subject, a more recent anonymous northern writer* expresses himself thus:—"Of Séguin and Mayer, it seems not very difficult to estimate the claims, so far as the true theory or the mechanical equivalent of heat is concerned. Séguin in 1839, and Mayer in 1842, gave as values of the mechanical equivalent: the first 363 kilogrammetres, or, in terms of the ordinary British units, 660 foot-pounds; the second the almost identical number 365 or 663. It is curious also to observe that the methods employed are almost identical."

18. Did the reputation of Dr. Mayer depend on his calculation of the mechanical equivalent of heat, and were the statements here quoted correct, his right to the recognition which I have thought his due might fairly be questioned. But let us inquire whether this is really the case. The Table on which the claim for M. Séguin is founded is now before the reader; and on referring to it, two columns will be seen, the one headed "Effet produit en kilogrammes élevés à 1 mètre," and the other headed "Températures correspondantes à l'effet produit." The first number in the first of these columns is 7270 kilogrammetres, and the "température correspondante" is 20 degrees. Hence, dividing 7270 by 20, we have the quotient 363 as the number of kilogrammetres corresponding to a single degree. And so of the other pairs of numbers, which give 363, or thereabouts, as the mechanical effect due to a single degree. All this seems very plain; and did no text accompany the Table, and had not M. Séguin in that text explicitly defined his own terms, we might be justified in assuming that he meant the number 363 to stand for the mechanical equivalent of heat, in the same sense as Dr. Mayer meant the number 365 to stand for it. *It is only necessary, however, to read the foregoing pages to see that Mayer and Séguin are speaking of two totally different things; that the degrees of the one are not the degrees of the other; that the "températures correspondantes" of the latter, which refer*

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* Not my Edinburgh reviewer, who, while he writes as a critic, knows how to preserve the style of a gentleman.

to his compressed steam, are not thermal units at all, and that there is no determination whatever of the mechanical equivalent of heat in the above Table.

19. The number 363 has been found *for* M. Séguin, not *by* him: he never made the division which results in this quotient. In 1847, for the first time, and without giving any description of his method*, M. Séguin gives his results "reduced to the type of 1 gramme elevated 1 metre, and corrected with reference to the specific heat of water and vapour." His equivalent there given varies from 395 to 529 kilogrammetres (*Comptes Rendus*, vol. xxv. p. 420). The data, moreover, on which M. Séguin founded this last calculation were subsequently declared erroneous by himself: the experiments of M. Regnault, he states, "defeated the calculations" (*Cosmos*, vol. vi. p. 684); and Mr. Grove has shown that when the correct specific heat of steam, as determined by Regnault, is introduced into the calculations, M. Séguin's equivalent becomes 1666 kilogrammetres instead of 363 (Proceedings of the Royal Institution, vol. ii. p. 155). We have already seen that in Mayer's case, when the correct specific heat of air is employed, his result is almost identical with that derived from the mean of all the best experiments of Mr. Joule. The one is 426, the other is 425†.

III.

20. After going formally through the calculation of the mechanical equivalent of heat, Mayer proceeds to determine the useful effect in steam-engines, and finds it to be about 5 per cent. of the consumed fuel. He then determines the useful

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* Such a description would be a desirable addition to our knowledge.

† I have already drawn attention to these facts (Phil. Mag. vol. xxv. p. 385), but have been met, not by explanation, but by iteration. This, I trust, will now cease. It is no compliment to the scientific public to think that mere hardihood of assertion can decide this question.

To illustrate the difficulty of satisfying rival claimants, I may remark that in 1862 I withdrew the name of Dr. Mayer from the list of candidates for the Copley medal, out of deference to an eminent man—*not* Mr. Joule—who thought his own claims prior to those of Mayer. What I have had to endure at the hands of two northern critics for my supposed depreciation and suppression of the claims of Mr. Joule is at least partially known to the scientific public. Again, a writer in M. Séguin's periodical, *Le Cosmos*, while declaring that Mr. Joule must be entirely put aside, the question of priority resting solely between Séguin and Mayer, charges me with having manifested a wholly insufficient appreciation of M. Séguin. I should be a mere intellectual quicksand if I allowed myself to be swayed by such criticisms. I have, judging from the facts, steered through these rival claims with the best light that I possess, and not one of my censors appears to have gone to one-tenth of the trouble that I have incurred to inform myself of the rights of the question.

effect in the case of gunpowder, and finds in certain cases that 9 per cent. of the force of the consumed charcoal is expended on the projectile. He gives various illustrations of the generation of heat by mechanical power, and describes some observations of his own, made in a paper-mill, in which were four pulping machines, each containing about 80 lbs. of paper and 1200 lbs. of water. The surrounding temperature being 15° C., the pulp rose in thirty-two minutes from 14° to 16°. The highest observed temperature, which remained uniform for several hours, was 30°. Assuming that in one minute a horse can raise 27,000 lbs. a foot high, the heating of 1280 lbs. of water 1 degree in sixteen minutes (not taking into account the heat communicated to the apparatus) is equivalent to 3·16-horse power. The estimate in the factory was, that the pulping machines were worked with 5-horse power. Does the mechanical action of the five horses become nothing in the machine? Fact replies, *it becomes heat.*

21. He then goes on to show the relationship of mechanical work to electricity and magnetism, and passes to the consideration of chemical processes as compared with mechanical operations. A weight at such a distance from the earth that the attraction is insensible, he regards as in a state of *mechanical separation*; the falling of the weight to the earth as a case of *mechanical combination.* Such a weight would reach the earth's surface with a velocity of 34,440 feet a second, and the heat generated by its collision would raise the temperature of an equal weight of water 17,356°. Chemical combination is in principle the same. The chemical combination of 1 gramme of carbon and 2·6 grammes of oxygen is *equivalent* to the mechanical combination of a weight of ½ a gramme with the earth. The chemical combination of 1 gramme of hydrogen with 8 grammes of oxygen is *equivalent* to the mechanical combination of a weight of 2 grammes with the earth. The heat here developed is equal to 34,700 thermal units*.

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* In 1843 Mr. Joule wrote the following remarkable passage:—"I had before endeavoured to prove that when two atoms combine together, the heat evolved is exactly that which would have been evolved by the electric current due to the chemical action taking place, and is therefore proportional to the chemical force causing them to combine. I now venture to state more explicitly that it is not precisely the attraction of affinity, but rather the mechanical force expended by the atoms in falling towards one another, which determines the intensity of the current, and consequently the heat evolved" (Phil. Mag. 1843, vol. xxiii. p. 442). I cite this as one of the points of osculation between these two remarkable men. They thus touched each other repeatedly, Joule being in advance sometimes, and Mayer sometimes. But their main achievements lie in distinct fields; and these are, in my opinion, so balanced as to render them a kind of "double star, the light of each being, in a certain sense, complementary to that of the other." (Phil. Mag. S. 4. vol. xxvi. p. 67.)

The manner in which Mayer expands his conceptions from the union of atoms to the union of worlds is a remarkable illustration of his generalizing power. After discoursing thus, he goes on to say :—

22. "The earth moves in its orbit with a mean velocity of 93,700′. To produce this motion by the combustion of coal, fifteen times the earth's weight of coal would have to be consumed, and the heat produced would be competent to raise the temperature of a quantity of water equal to the earth in weight 128,000°. A small portion therefore of the force with which the earth moves in its orbit would suffice to dissolve all mechanical connexion among its parts. Supposing a mass equal to the earth in weight to lie at rest on the surface of the sun, to raise that mass, place it at the earth's distance from the sun (215 times the sun's radius), and to impart to it there the velocity of 93,700′, would require 429 times the above quantity of coal, or a quantity 6435 times the weight of the earth."

23. (In a letter published in the Philosophical Magazine for August 1862 Mr. Joule writes as follows :—"In 1847, in a popular lecture published in the 'Manchester Courier,' I explained the phenomena of shooting-stars, and also stated that the effect of the earth falling into the sun would be to increase the temperature of that luminary." The foregoing passage, giving the amount of the heat that would result from the falling of the earth into the sun, was published by Mayer in 1845 *.)

24. Mayer next briefly considers the case of the voltaic battery and the gas battery. He then draws out a scheme of the five principal forms of energy which he has been examining, and under five-and-twenty separate heads he states their relations and mutual conversions. "Preconceived notions," he says, "sanctioned by time and diffusion, and not the phenomena of Nature, are opposed to the propositions here laid down. While," he adds, "we ascribe substantiality to *motion*, we must entirely deny materiality to heat and electricity. I know quite well that we have against us here the most deeply rooted convictions—hypotheses canonized by the greatest authorities. With the theory of imponderables we banish from science the last remains of the mythology of Greece; but we know that Nature in her simple truth transcends in glory the devices of the human phantasy, as much as she excels the operations of the human hand."

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* It has been said that in the application of the dynamical theory of heat to shooting-stars, "and some other points of celestial dynamics," Mr. Joule had "at least one year's priority." (Phil. Mag. vol. xxv. p. 431.) The "some other points" shrink, if I mistake not, to *the* point referred to in the text, and the year's priority is, in reality, two years' *posteriority*. Mr. Joule's remarks on shooting-stars shall be quoted further on.

IV.

Having cleared his way through the powers of inorganic nature, he turns to vital phenomena, and at once fixes the attention of his readers upon the sun.

25. Measured by human standards, the sun is an inexhaustible source of physical energy. This is the continually wound-up spring which is the source of all terrestrial activity. The vast amount of force sent by the earth into space in the form of wave motion would soon bring its surface to the temperature of death. But the light of the sun is an incessant compensation. It is the sun's light, converted into heat, which sets our atmosphere in motion, which raises the water into clouds, and thus causes the rivers to flow*. The heat developed by friction in the wheels of our wind- and water-mills was sent from the sun to the earth in the form of vibratory motion.

(The reader cannot fail to remark the insight implied in this last utterance. But a still higher order of thought immediately reveals itself.)

26. Nature has proposed to herself the task of storing up the light which streams earthward from the sun—of converting the most volatile of all powers into a rigid form, and thus preserving it for her purposes. To this end she has overspread the earth with organisms, which, living, take into them the solar light, and by the consumption of its energy generate incessantly chemical forces.

27. These organisms are *plants.* The vegetable world constitutes the reservoir in which the fugitive solar rays are fixed, suitably deposited, and rendered ready for useful application. With this prevision the existence of the human race is also inseparably connected. The reducing action of the sun's rays on inorganic and organic substances is well known; this reduction takes place most copiously in full sunlight, less copiously in the shade, and is entirely absent in darkness, and even in candle-light. The reduction is a conversion of one form of force into another—of mechanical effect into chemical tension.

28. The time does not lie far behind us when it was a subject of contention whether, during life, plants did not possess the power of changing the chemical elements, and indeed of creating them. Facts and experiments seemed to favour the notion, but a more accurate examination has proved the contrary. We now know that the sum of the materials employed and excreted is equal to the total quantity of matter taken

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* This, and much more, was stated by Sir John Herschel in 1833 (Outlines of Astronomy), but Mayer was the first to show the relation of all these actions to the law of the conservation of energy.

up by the plant. The tree, for example, which weighs several thousand pounds, has taken every grain of its substance from its neighbourhood. In plants a *conversion* only, and not a *generation* of matter, takes place.

29. Plants consume the force of light, and produce in its place chemical tensions. Since the time of Saussure, the action of light has been known to be necessary to the reduction. In the first place we must inquire whether the light which falls upon living plants finds a different application from that which falls upon dead matter; that is to say, whether, *cæteris paribus,* plants are less warmed by solar light than other bodies equally dark-coloured. The results of the observations hitherto made on a small scale seem to lie within the limits of possible error. On the other hand, every-day experience teaches us that the heating action of the sun's rays on large areas of land is moderated by nothing more powerfully than by a rich vegetation, although plants, on account of the darkness of their leaves, must be able to absorb a greater quantity of heat than the bare earth. If, to account for this cooling action, the evaporation from the plants be not sufficient, then the question above proposed must be answered in the affirmative.

30. The second question refers to the cause of the chemical tension produced in the plant. This tension is a physical force. It is equivalent to the heat obtained from the combustion of the plant. Does this force, then, come from the vital processes, and without the expenditure of some other form of force? The creation of a physical force, of itself hardly thinkable, seems all the more paradoxical when we consider that it is only by the help of the sun's rays that plants can perform their work. By the assumption of such a hypothetical action of the "vital force" all further investigation is cut off, and the application of the methods of exact science to the phenomena of vitality is rendered impossible. Those who hold a notion so opposed to the spirit of science would be thereby carried into the chaos of unbridled phantasy. I therefore hope that I may reckon on the reader's assent when I state, as an axiomatic truth, *that during vital processes only a conversion of matter, as well as of force, occurs, and that a creation of either the one or the other never takes place.*

(To the philosophy of vegetable life here so firmly sketched, nothing to my knowledge has been added since. It will be immediately seen that Mayer's power does not relax when he treats of animal life and energy.)

V.

31. The physical force collected by plants becomes the property of another class of creatures—of animals. The living

animal consumes combustible substances belonging to the vegetable world, and causes them to reunite with the oxygen of the atmosphere. Parallel to this process runs the work done by animals. This work is the end and aim of animal existence. Plants certainly produce mechanical effects, but it is evident that for equal masses and times the sum of the effects produced by a plant is vanishingly small, compared with those produced by an animal. While, then, in the plant the production of mechanical effects plays quite a subordinate part, the conversion of chemical tensions into useful mechanical effect is the characteristic sign of animal life.

32. In the animal body chemical forces are perpetually expended. Ternary and quaternary compounds undergo during the life of the animal the most important changes, and are, for the most part, given off in the form of binary compounds—as burnt substances. The magnitude of these forces, with reference to the heat developed in these processes, is by no means determined with sufficient accuracy; but here, where our object is simply the establishment of a principle, it will be sufficient to take into account the heat of combustion of the pure carbon. When additional data have been obtained, it will be easy to modify our numerical calculations so as to render them accordant with the new facts.

33. The heat of combustion of carbon I assume with Dulong to be 8550°. The mechanical work which corresponds to the combustion of one unit of weight of coal corresponds to the raising of 9,670,000 units to a height of 1 foot.

If we express by a weight of carbon the quantity of chemical force which a horse must expend to perform the above amount of work, we find that the animal in one day must apply 1·34 lb.; in an hour 0·167 lb.; and in a minute 0·0028 lb. of carbon to the production of mechanical effect.

According to current estimates, the work of a strong labourer is $\frac{1}{7}$th of that of a horse. A man who in one day lifts 1,850,000 lbs. to a height of a foot must consume in the work 0·19 lb. of carbon. This for an hour (the day reckoned at eight hours) amounts to 0·024 lb.; for a minute it amounts to 0·0004 lb. = 3·2 grains of carbon. A bowler who throws an 8-lb. ball with a velocity of $30'$ consumes in this effort $\frac{1}{10}$th of a grain of carbon. A man who lifts his own weight (150 lbs.) 8 feet high, consumes in the act 1 grain of carbon. In climbing a mountain 10,000 feet high, the consumption (not taking into account the heat generated by the inelastic shock of the feet against the earth) is 0·155 lb. = 2 ozs. 4 drs. 50 grs. of carbon.

34. If the animal organism applied the disposable combustible material solely to the performance of work, the quan-

tities of carbon just calculated would suffice for the times mentioned. In reality, however, besides the production of mechanical effects, there is in the animal body a continuous generation of heat. The chemical force contained in the food and inspired oxygen is therefore the source of *two* other forms of power, namely, mechanical motion and heat; and the *sum* of these physical forces produced by an animal is the equivalent of the contemporaneous chemical process. Let the quantity of mechanical work performed by an animal in a given time be collected, and converted by friction or some other means into heat; add to this the heat generated immediately in the animal body in the same time, we have then the exact quantity of heat corresponding to the chemical processes that have taken place.

35. In the active animal, continues Mayer, the chemical changes are much greater than in the resting one. Let the amount of the chemical processes accomplished in a certain time in the resting animal be x, and in the active one $x+y$. If during activity the same quantity of heat were generated as during rest, the additional chemical force y would correspond to the work performed. In general, however, more heat is produced in the active organism than in the resting one. During work, therefore, we shall have x plus a portion of y for heat, the residue of y being converted into mechanical effect.

36. I must now prove that the extra quantity of combustible matter consumed by the working animal contains the necessary force for the performance of the work. A strong horse, not working, is amply nourished on 15 lbs. of hay and 5 lbs. of oats a day. If the animal performed daily the work of lifting a weight of 12,960,000 lbs. 1 foot high, it could not exist on the same nutriment. To keep it in good condition we must add 11 lbs. of oats. The 20 lbs. of nutriment first mentioned is the quantity which we have named x, and contains, according to Boussingault, 8·074 lbs. of carbon. The additional 11 lbs. of oats, our quantity y, contains, according to the same authority, 4·734.

According to Boussingault, also, the carbon introduced is to that excreted in a combustible form as 3938 : 1364·4. Calculating from these data, we find x, or the quantity of carbon burnt by the resting animal, 5·2766 lbs., and $y=3{\cdot}094$ lbs. The quantity consumed in mechanical effect is 1·34 lb., which we will call z.

37. We have therefore the following relations:—1. The mechanical effect is to the total consumption as $z : x+y=0{\cdot}16$. 2. The mechanical effect is to the surplus consumption of the working animal as $z : y=0{\cdot}43$. 3. The generation of heat at rest is to the generation of heat while working as $x : x+y-z=0{\cdot}75$.

38. In the same way Mayer, taking the data furnished by Liebig regarding the prisoners and soldiers at Giessen, determines the following relations for a man. 1. The mechanical effect is to the total consumption as $95{\cdot}7 : 540 = 0{\cdot}177$. 2. The mechanical effect is to the surplus consumption of the man at work as $957 : 285 = 0{\cdot}336$. 3. The generation of heat in the resting man to that in the working man as $255 : 540 - 95{\cdot}7 = 0{\cdot}57$.

39. In these calculations, he continues, I have confined myself to the consumed carbon. If the heat of combustion be set equal to the carbon + the hydrogen, the additional heat of the hydrogen may be regarded as nearly $= \frac{1}{4}$th of that of the carbon. According to the individual constitution and habits of life, the labour and the consumption must be liable to considerable variations. The above results, however, serve to demonstrate the following propositions:—

(1.) The surplus nutriment consumed in the working organism completely suffices to account for the work done.

(2.) The maximum mechanical effect produced by a working mammal hardly amounts to $\frac{1}{5}$th of the force derivable from the total quantity of carbon consumed. The remaining $\frac{4}{5}$ths are devoted to the generation of heat.

VI.

40. In order to enable them to convert chemical force into mechanical work, animals are provided with specific organs, which are altogether wanting in plants. These are the muscles.

41. To the activity of a muscle two things are necessary:— 1. The influence of the motor nerves as the determining condition; and 2. the material changes as the cause of the mechanical effect.

42. Like the whole organism, the organ itself, the muscle, has its psychical and its physical side. Under the former we include the nervous influence, under the latter the chemical processes.

43. The motions of the steamship are performed in obedience to the will of the steersman and engineer. The spiritual influence, however, without which the ship could not be set in motion, or, wanting which, would go to pieces on the nearest reef, guides, but moves not. For the progress of the vessel we need physical force—the force of coal; in its absence the ship, however strong the volition of its navigator, remains dead.

VII.

Thus does this remarkable man, at a time when the writings of the most celebrated scientific professors were beset with mysticism as regards the operations of the vital force, pour light upon the darkness, and bring the processes of the animal body into harmony with the great law of conservation, which he himself, alone and unaided, had thought out.

44. Here follow a few of Mayer's remarks on muscular motion. In the first part of this memoir, the part played by combustion in inorganic apparatus in the steam-engine, for instance was, in its main characters, explained. Our present problem is to consider the phenomena of vitality in connexion with their physical causes, and thus give to the propositions of physiology the basis of exact science.

45. It has been already stated that an active working man converts in a day 0·19 lb. of carbon into mechanical effect. The weight of the whole muscles of such a man, who weighs 150 lbs., is 64 lbs.; and, subtracting 77 per cent. of water, 15 lbs. of dry combustible material remains. Let it be assumed (though not granted) that the heat-giving power of this mass (with 40 per cent. of nitrogen and oxygen) is equal to that of an equal mass of pure carbon; then, if the work were done at the expense of the muscles themselves, the whole of the muscles must be oxidized and consumed in mechanical effect in eighty days.

46. This arithmetical deduction becomes still more evident if we confine our attention to the work performed by a single muscle—the heart. I assume, with Valentin, the quantity of blood in the left ventricle to be at every systole on an average 150 cubic centimetres. The hydrostatic pressure of the blood in the arteries is, according to Poiseuille, equal to the pressure of a column of mercury 16 centimetres in height. The mechanical work done by the left ventricle during a systole may be calculated from these data. It is equal to the raising of a column of mercury 16 centimetres long, and with a base of a square centimetre to a height of 150 centimetres. The weight of the mercury amounts to 217 grammes. The mechanical effect of a systole therefore is

$$=\left\{\begin{array}{lll}325\cdot 6 \text{ grammes} & \text{raised} & 1 \text{ metre,}\\ \quad 2 \text{ lbs.} & \quad ,, & 1 \text{ foot,}\end{array}\right.$$

which is equivalent to 0·887 of a thermal unit, or equivalent to the combustion of 0·0001037 of a gramme of carbon. Taking for a minute 70 strokes, and for a day 100800 strokes of the pulse, the work done by the left ventricle in a day is equivalent to the raising of 202000 lbs. to a height of one foot. This is equal to 89428 thermal units, which is equal to the combustion of $\left\{\begin{array}{l}10\cdot 45 \text{ grms.}\\ 168\cdot 3 \text{ grs.}\end{array}\right\}$ of carbon. According to Valentin, the work done by the right ventricle is half that done by the left. The work of both chambers in a single day is therefore equal to the raising of 303000 lbs. 1 foot high $=134143$ thermal units $=\left\{\begin{matrix}15\cdot67 \text{ grms.}\\ 252\cdot4 \text{ grs.}\end{matrix}\right\}$ of carbon.

47. Assuming the weight of the whole heart to be 500 grammes, and deducting from this 77 per cent. of water, we have remaining 115 grammes of dry combustible material. Assuming this material to be equal to that of pure carbon, it would follow that the entire organ, if it had to furnish the matter necessary to its action, would be oxidized in eight days. Taking the weight of the two ventricles alone as 202 grammes, under the same conditions the complete combustion of this muscular tissue would be effected in 3½ days.

VIII.

48. This partial *résumé* of half of Mayer's second memoir is now ended. It embraces only the first 56 pages of an essay which contains 112 pages. Mayer began, as has been stated, with the question of vital dynamics. The observation which led to his scientific labours was made on a patient at Java in 1840, and in 1842 he published his first paper. He informs us that he had put it briefly together to secure himself against casualties*; and having done this, he continued his inquiries, and in 1845 published the memoir from which the foregoing extracts are taken. He did this in the intervals of a laborious profession, "ohne äussere Ermunterung," as he himself touchingly observes. The full translation of the essay can alone give an adequate idea of the research which it implies. Mayer probably had not the means of making experiments himself, but he ransacked the records of experimental science for his data, and thus conferred upon his writings a strength which mere speculation can never possess. From the extracts which I have given, the reader may infer his strong desire for quantitative accuracy, the clearness of his insight, and the firmness of his grasp. Regarding the recognition which will be ultimately accorded to Dr. Mayer, a shade of trouble or of doubt has never crossed my mind. Individuals may seek to pull him down, but their efforts will be unavailing as long as such evidence of his genius exists, and as long as the general mind of humanity is influenced by considerations of justice and of truth†.

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* Phil. Mag. vol. xxv. p. 501.

† The paucity of facts in Mayer's time has been urged as if it were a reproach to him, but it ought to be remembered that the quantity of fact necessary to a generalization is different for different minds. "A word to the wise is sufficient for them," and a single fact in some minds bears fruit that a hundred cannot produce in others. Mayer's data were comparatively scanty, but his genius went far to supply the lack of experiment, by

IX.

49. There are a few points remaining, which, to preserve the events of scientific history in their true relationship, ought to be referred to here. It has been asserted mildly (as is his wont) by Mr. Joule, less mildly (as is their wont) by his northern supporters, that, as regards vital dynamics, he anticipated Mayer by two years. In a letter published in the Philosophical Magazine for August 1862, Mr. Joule writes thus :—"Permit me to remark, that I applied the dynamical theory of heat to vital processes in 1843." Let all justice be done to Mr. Joule regarding his application of the theory. In a postscript to a paper in the December Number of the Philosophical Magazine for 1843 he writes thus :—

50. "On conversing a few days ago with my friend Mr. John Davies, he told me that he had himself a few years ago attempted to account for that part of animal heat which Crawford's theory had left unexplained, by the friction of the blood in the veins and arteries, but that, finding a similar hypothesis in Haller's 'Physiology,' he had not pursued the subject further. It is unquestionable that heat is produced by such friction, but it must be understood that the mechanical force expended in the friction is a part of the force of affinity, which causes the venous blood to unite with the oxygen, so that the whole heat of the system must still be referred to the chemical changes. But if the animal were engaged in turning a piece of machinery, or in ascending a mountain, I apprehend that, in proportion to the muscular effort put forth for the purpose, a *diminution* of the heat evolved in the system by a given chemical action would be experienced."

This citation embraces, I believe, every word that was written on "vital dynamics" by Mr. Joule, before the appearance of Mayer's paper on organic motion. It consists of a conjecture, the sagacity of which is in accordance with the insight always manifested by Mr. Joule. Let the reader compare it with sections 4 to 7 of this *résumé,* and make the deductions which he deems right from his estimate of Mayer's work.

51. In 1852 Prof. Wm. Thomson wrote on the subject of "vital dynamics," and it will be instructive to compare what he has done with what had been done by Mayer seven years earlier.

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enabling him to see clearly the bearing of such facts as he possessed. They enabled him to think out the law of conservation, and his conclusions received the stamp of certainty from the subsequent experimental labours of Mr. Joule. In reference to their comparative merits, I would say that, as Seer and Generalizer, Mayer, in my opinion, stands first,—as Experimental Philosopher, Joule.

The whole paper of Prof. Thomson, published in the Philosophical Magazine for 1852, vol. iv., may be compared with the writings of Dr. Mayer now before the reader. I, however, will limit myself here to the section "On the Power of Animated Creatures over Matter" (p. 258).

"A principal object of the present communication is to point out the relation of this theory [that of animal heat and motion] to the dynamical theory of heat. It is remarked, in the first place, that both animal heat and weights raised or resistance overcome, are *mechanical* effects of the chemical forces which act during the combination of food with oxygen. The former is a dynamical mechanical effect, being thermal motions excited; the latter is a mechanical effect of the statical kind. The whole mechanical value of these effects, which are produced by means of the animal mechanism in any time, must be equal to the mechanical value of the work done by the chemical forces. Hence, when an animal is going up-hill or working against resisting force, there is less heat generated than the amount due to the oxidation of the food, by the thermal equivalent of the mechanical effect produced. From an estimate made by Mr. Joule [in 1846, Phil. Mag. vol. xxviii. p. 454], it appears that from $\frac{1}{4}$ to $\frac{1}{6}$ of the mechanical equivalent of the complete oxidation of all the food consumed by a horse may be produced from day to day as weights raised [Mayer published the same result a year previous to Mr. Joule, see 39]. The oxidation of the whole food consumed being, in reality, far from complete [see Mayer, 36 to 39], it follows that a less proportion than $\frac{5}{6}$, perhaps even less than $\frac{3}{4}$ of the heat due to the whole chemical action that actually goes on in the body of the animal, is given out as heat. An estimate, according to the same principle, upon very imperfect data, however, is made by the author, regarding the relation between the thermal and the non-thermal mechanical effects produced by a man at work [Mayer made the same estimate, see 38]; by which it appears that probably as much as $\frac{1}{6}$ of the whole work of the chemical forces arising from the oxidation of his food during the twenty-four hours may be directed to raising his own weight, by a man walking up-hill for eight hours a day; and perhaps even as much as $\frac{1}{4}$ of the work of the chemical forces may be directed to the overcoming of external resistances by a man exerting himself for six hours a day in such operations as pumping. In the former case there would not be more than $\frac{5}{6}$, and in the latter not more than $\frac{3}{4}$ of the thermal equivalent of the chemical action emitted as animal heat, on the whole, during twenty-four hours, and the quantity of heat emitted during the time of working would bear much smaller proportions respectively than these to the thermal equivalents of the chemical forces actually operating during those times" *.

Comparing the foregoing remarks with what Mayer had written seven years earlier, the reader will draw his own conclusions as to their comparative completeness. In his writings upon vital dynamics Prof. Thomson never once mentions the name of Mayer; he is not, I presume, to be blamed for this omission; for when he wrote in 1852 he knew nothing about Mayer's most important labours. I state this because the opposite supposition is too unpleasant to be entertained. But in 1862 I gave him the titles of Mayer's memoirs, and requested him and others to refer to them, and correct me if I had erroneously estimated the merits of their author. To my great regret, Prof. Thomson, without giving himself the trouble of consulting the documents to which I had referred him, sanctions the publication of the statement, that, as far back as 1851, he had given to Mayer "the full credit which his scientific claims can possibly be admitted to deserve" †.

52. In the paper from which I have just quoted, Prof. Wm. Thomson also refers to the deoxidation of carbon and hydrogen from carbonic acid and water, effected by the action of solar light upon the green leaves of plants, as a mechanical effect of radiant heat. This action, he says, was pointed out by Helmholtz in 1847.

53. The words of Helmholtz are as follows (Erhaltung der Kraft, p. 69 ‡):—"There remains to us of known natural processes those of organic beings. In plants the processes are principally chemical, and besides this, in some of them, at least, a slight generation of heat takes place. Of foremost importance is the fact, that in them a great quantity of chemical forces is deposited, the equivalent of which we obtain as heat on the combustion of the plant. The only *vis viva* which, according to our present knowledge, is absorbed during the growth of the plant, consists of the chemical rays of the sun. Results are, however, still wanting to enable us to make a sure and strict comparison of the forces which here disappear and appear. For animals we have, however, some grounds of comparison. They take in the complicated oxidizable compounds which are produced by plants and oxygen, and return them for the most part burnt as carbonic acid and water; partly, however, they are excreted, reduced to sim-

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* On first reading this passage I thought it might be an abstract of a fuller statement, but I have been unable to find anything more complete.

† Phil. Mag. vol. xxv. p. 264.

‡ This excellent essay was translated by myself many years ago, and published in the last volume of the Scientific Memoirs. (Taylor and Francis, Red Lion Court, Fleet Street.)

pler combinations. They therefore consume a quantity of chemical forces, and generate in their place heat and mechanical forces. As the last represent a comparatively small amount of work in comparison with the amount of heat, the question of the conservation of force reduces itself to this:—Do the combustion and the change of materials in the nutriment generate an equal quantity of heat to that yielded up by the animal? According to the experiments of Dulong and Despretz, this question can, at least approximately, be answered in the affirmative."

54. Helmholtz made this statement independently of Mayer, for when he wrote he did not know what had been published two years before at Heilbronn*; and clearly as the above paragraph illustrates his insight on this momentous point, it could not be accepted as an adequate abstract of Mayer's previous writings on the same subject. In a lecture of varied excellence, translated by myself, and published in the Philosophical Magazine, 1856, vol. ii., Helmholtz expresses in clear and beautiful language the relation of animals to vegetables, and of both to the sun. The lecture was given at Königsberg on the 7th of February 1854; and if the reader wishes to realize fully the extent to which Mayer had occupied this field, and the scantiness of the additions made to our knowledge of vital dynamics during the nine years following the publication of Mayer's essay, he may compare pp. 509, 510, and 511 of Helmholtz's lecture with sections 4, 5, 6, and 7 of this *résumé*.

55. One word more on this subject, which shall have as slight a personal tinge as I can under the circumstances impart to it. In a religious periodical, which we are informed numbers 120,000 readers, Prof. Thomson undertook to give an accurate account of the discovery, nature, and development of the law of the conservation of energy, professedly with the view of correcting the errors which he believed me to be disseminating regarding Mayer. In that article he charged me with depreciation and suppression, and these bad words rest unretracted in the pages of 'Good Words' to this hour. After dealing with various questions relating to the "conservation of energy," Prof. Thomson comes at length to the "grandest question of all," and states it thus:—"Whence do we derive the stores of potential energy which we employ as fuel and food? What produces the potential energy of a loaf or a beefsteak? What supplies the coal and the water power, without which our factories would stop?" And the answer to this question is "the sun." Prof. Thomson can now name the man who answered this question seventeen years before he called it the grandest of all—the man

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* "I myself, without being acquainted with either Mayer or Colding," &c.—Phil. Mag. S. 4. vol. ii. p. 409.

whose name, to my deep regret, he has never yet named in connexion with this question, though Mayer's relation to it has been now for two years known to him. But this is not all. In public and in private—in articles which are so far manly as to bear their author's names, and in an article which bears no name, but which has been recently mentioned with commendation in the pages of this Magazine—I have been attacked for my support of Dr. Mayer in language which I would not stoop to characterize. While here, from the pen of the man who, in equal ignorance both of me and of the facts, instituted this ungentle crusade against me, I extract testimony to the greatness of Mayer's work, stronger than I have ever uttered in attempting to vindicate his claims.

X.

56. Sir W. Herschel had called the maintenance of solar heat "The Great Secret." Mayer endeavoured to solve it, and published his essay on Celestial Dynamics in 1848. It will be seen, however, in a foregoing page that the idea of a meteoric source of solar heat was at his hand in 1845. His calculation of the quantity of heat which would be generated by the mechanical combination of the earth and sun proves this (see 22). But in 1848 he published a complete development of his theory, and his essay is now readily accessible since its translation by Dr. Debus for the Philosophical Magazine*. There could scarcely be a closer coincidence between two independent scientific memoirs than that subsisting between the essay of Dr. Mayer and the paper of Prof. Thomson, published six years subsequently†. Thomson considers and rejects the assumption that the sun is a heated body, losing heat; Mayer did the same. Thomson considers and rejects the assumption that the heat of the sun is due to chemical action; so did Mayer. Thomson considers and embraces the theory that meteors falling into the sun give rise to his heat; so did Mayer. Thomson arrives at the conclusion that the main source of solar light and heat is the zodiacal light. This was also Mayer's conclusion. Their calculations run parallel, and their deductions from them are the same. As an instance of coincidence in detail the following is worthy of notice:—"A dark body," writes Prof. Thomson in 1854, "of dimensions such as the sun, in any part of space, might, by entering a cloud of meteors, become incandescent as intensely in a few seconds, as it could in years of continuance of the same meteoric circumstances, and again getting to a position in space comparatively free from meteors, it might almost as suddenly become dark again. It is far from improbable that this is the explanation of the appear-

* Vol. xxv. pp. 241, 387, 417.

† Phil. Mag. vol. viii. p. 409.

ance and disappearance of bright stars, and the strange variations of brilliancy of others which have caused so much astonishment." (Phil. Mag. vol. viii. p. 415). Three years previous to the publication of the above paragraph Dr. Mayer wrote thus :—" It is more than probable that the earth has come into existence in some such way, and that in consequence of this process our sun, as seen from the distance of the fixed stars, exhibited at that epoch a transient burst of light. But what took place in our solar system perhaps millions of years ago, still goes on at the present time here and there among the fixed stars; and the transient appearance of certain stars, which in some cases, like the celebrated star Tycho, have at first an extraordinary degree of brilliance, may be satisfactorily explained by assuming the falling together of previously invisible double stars." (Bemerkungen ü. d. mech. Aequiv. d. Wärme, p. 56; Phil. Mag. vol. xxv. p. 521.)

57. At the commencement of his paper "On the Mechanical Energies of the Solar System," Prof Thomson states that this theory was never brought forward in any definite form, so far as he was aware, "until Mr. Waterston communicated to the British Association at Hull a remarkable speculation on cosmical dynamics (*Dynamik des Himmels*), in which he proposed the theory that solar heat is produced by the impact of meteors." Mayer is here definitely ignored; and I assume the reason to be the same that I have assigned for Prof. Thomson's silence regarding Mayer's writings on vital dynamics. But he was not left without information; in my lecture in June 1862 I referred to those writings in the following words :—" In 1853 Mr. Waterston proposed independently the meteoric theory of the sun's heat, and in 1854 Prof. Wm. Thomson applied his admirable mathematical powers to the development of the theory; but six years previously the subject had been handled in a masterly manner by Mayer, and all that I have said on this question has been derived from him." I do not see how I could have stated the truth in more considerate terms. Instead, however, of making himself acquainted with the essay of Mayer and nobly bidding him welcome, Prof. Thomson permits himself to sanction the following language towards me. " Prof. Tyndall is most unfortunate in the possession of a mental bias, which often prevents him (as, for instance, in the case of Rendu and glacier-motion)* from recognizing the fact that the claims of individuals whom he supposes

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* I have asked Prof. Thomson to point out the passages in my writings which justify this language, but he has not done so. The readiness of Prof. Thomson to make such statements and to neglect their proof has excited attention in other quarters, and will assuredly furnish him with its harvest of results.

to have been wronged, have, before his intervention, been fully ventilated, discussed, and settled by the general award of scientific men." Such rashness is rare in a man occupying so responsible a position. The fact really is that if it could be shown that Prof. Thomson had, "before my intervention," been aware of what Mayer had done, he would at the present moment be in as unenviable a position as could possibly be occupied by a scientific man. I may add that he has never yet rendered to Dr. Mayer the credit which belongs to him.

58. How far Mr. Joule's lecture on shooting-stars (23) affects the essay of Dr. Mayer on Cosmical Dynamics the reader must himself determine. Here is the extract from the 'Manchester Courier' (12th May 1847) referred to by Mr. Joule and printed in the Philosophical Magazine. "You have no doubt frequently observed what are called *shooting-stars,* as they appear to emerge from the dark sky at night, pursue a short and rapid course, burst, and are dissipated in shining fragments. From the velocity with which these bodies travel there can be little doubt that they are small planets, which in the course of their revolution round the sun are attracted and drawn to the earth. Reflect for a moment on the consequences which would ensue if a hard meteoric stone were to strike the room in which you are assembled with a velocity sixty times as great as a cannonball. The dire effects of such a collision are effectually prevented by the atmosphere surrounding our globe, by which the velocity of the meteoric stone is checked, and its living force converted into heat, which at last becomes so intense as to melt the body and dissipate it in fragments, too small probably to be noticed in their fall to the ground. Hence it is that, though multitudes of shooting-stars appear every night, few meteoric stones have been found, those few corroborating the truth of our hypothesis by the marks of intense heat they bear on their surfaces." Those who have read Mayer's essay will never forget it, and will be able to judge how far its character could be affected by the above extract; even had it been under the eyes of Mayer from the moment of its publication. Those who have not read Mayer, will find him translated in the Phil. Mag. vol. xxv. pp. 241, 387, 417. In my book on Heat I have given Mr. Joule due credit for the above hypothesis*.

59. More than a year ago I addressed a letter to Prof. W. Thomson which gave me great pain to write. I wrote it partly in defence of Dr. Mayer, partly in defence of my own character. It was, I am told, vigorously expressed, but it has never been intimated

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* The hypothesis of the cosmical origin of meteorolites is due to Chladni, the protective power of the atmosphere and its sufficiency to dissipate meteors being the point brought forward by Mr. Joule.

to me that it contained a single ungentlemanly term. It brought me expressions of approval and sympathy from some of the most eminent men in Europe; and I was so content with this, that I willingly—some thought, tamely—let the discussion drop. It was quite natural that my style and matter should be the reverse of agreeable to Prof. Thomson; and, as might be expected, he expressed himself to this effect. He complained of the liberties which I had taken with his name; of the liberties I had taken with other names. In short, he considered my whole tone "unprecedented in scientific discussion," and he declined having anything to do with me. On one technical question, moreover, he made a complaint, to which, as it involves a point of personal courtesy, I am anxious to reply. He complained that I had printed my letter in the Philosophical Magazine without sending him the original. I am informed by good authority that the course I pursued was the usual and proper one. Had it been customary to send the original in such a case, I should certainly not have failed in this act of courtesy to Prof. Thomson. Had I even known his personal views on the matter, I should have sent him the original, regardless of the general practice. Nor should I have allowed myself to be in any degree influenced by the fact that Prof. Thomson had inserted in 'Good Words' expressions injurious to my character, which circulated unknown to me among the 120,000 readers of that periodical, until their accidental discovery by my assistant gave me an opportunity of demonstrating their baselessness. I would at the same time remind him that, though there is a dignity in silence when exercised at the proper time and in the proper way, it is *not* dignity, nor even manliness as defined in England, that permits a man to make an unwarranted accusation, and prevents him from retracting it after its injustice has been exposed.

It is with great reluctance that I refer to these topics; and were I alone concerned, I should give the world no further opportunity to animadvert on the dissensions of those among whom, in the interest of their common vocation, brotherly kindness ought to reign. But silence is scarcely becoming on my part when I see the reputation of a man, in whom the finest intellectual qualities are associated with the most shrinking modesty of character, made the target of anonymous reviewers. I never had an interest in this controversy apart from the desire to do him justice. To me Dr. Mayer is personally unknown, and my own scientific labours, unlike those of my chief censor, are entirely unaffected by anything that he has done. I may add that all that I had seen or known of Mr. Joule, previous to this discussion, had served to inspire me with respect and attachment for him. Personal liking and what has been called "patriotism" added themselves to the exalted value which I attached to his researches. I held, and still hold, him to be one of the noblest workers of this age; and my estimate of his labours is not a shade lowered by this other conviction, that he must, in the history of science, accept Dr. Mayer as his scientific brother,—that the Thinker and Generalizer is fit to stand, and will be caused to stand, beside the Experimental Philosopher. To permit Dr. Mayer to remain in the position in which I found him, would be to fasten on myself the guilt of that neglect of which the plea of ignorance alone acquits his contemporaries. In every sentence that I have written in his favour I have felt that strength which perfect single-mindedness can alone impart, and, fearless alike of his fate and of my own, I now commit his reputation, and my conduct concerning it, to the impartial judgment of mankind.

Royal Institution, June 1864.

V. *On a Modification of* Watt's *Parallelogram.*

By P. TCHÉBYCHEF*.

THE mechanism known as Watt's parallelogram furnishes a solution of the following practically important problem: *To produce, to a sufficient degree of approximation, rectilineal motion by a combination of circular motions.*

The degree of precision attainable by contrivances of this kind depends obviously upon the number of its disposable elements, and in this point of view the parallelogram of Watt is far from being satisfactory. In its structure, for instance, two rods more are employed than in the mechanism *à fléau*, and yet the motion produced is the same. In attempting to produce approximate rectilineal motion by means of either of these two contrivances, the motion really attained is an oval one, which has at most five elements in common with the desired rectilineal motion. Now this degree of approximation is unquestionably small when we take into consideration the degree of complication presented by Watt's parallelogram. The latter, as is well known, possesses four disposable elements, each of which, as therein employed, furnishes two arbitrary parameters—its length and its direction. Seeing, therefore, that, on the whole, eight parameters are involved, we are justified in seeking a contrivance of the same degree of complexity as Watt's parallelogram, but capable of furnishing a much more rectilineal motion—one, in fact,

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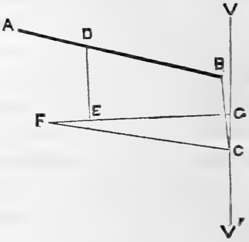
[*] From the *Bulletin de l'Acad. Imp. des Sciences de St. Pétersbourg*, vol. iv. p. 433.

which has with the desired motion eight, instead of five common elements.

We have done this, and found that the approximation in question may be attained by articulating, with each other and with the beam, the four rods of Watt's parallelogram in the following manner. In this figure A B represents the semi-beam upon which it is required to construct a mechanism capable of producing approximately rectilineal motion along the vertical line V V′, passing through the extremity B of the beam when the latter has a horizontal position. B C, D E, C F, F G are the four rods composing this mechanism; C is the point whose motion is to be considered; and F G, turning around a fixed axis G, represents the counter-beam, as in Watt's parallelogram. These rods are articulated with each other and with the beam in the same manner as in Watt's parallelogram, with the sole difference that the rods D E and F C, instead of being connected with each other, are articulated with the counter-beam F G at two different points E and F. The lengths and distances adopted are the following:—

Fig. 1.



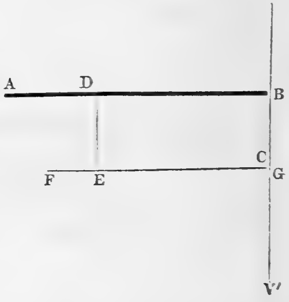
$$CF = FG = \frac{\sqrt{5}+1}{4} AB,$$

$$BD = EG = \frac{\sqrt{5}-1}{2} AB.$$

Consequently BD is a mean proportional between AB and AD, and EF is the half of AD. The rods BC and DE have the same length; and provided the latter do not sensibly exceed the semi-path of the point C, it may be arbitrarily chosen. The centre of oscillation G of the counter-beam FG is chosen so that, when the beam is horizontal, the rods BC and DE may be vertical, and at the same time the rods CF and FG may have the same horizontal position, as seen in fig. 2.

Such is the composition of the mechanism which, with the same number of rods as Watt employed, will give a motion having eight elements in common with the desired rectilineal one. This fact may be very easily verified by determining, as a function of the inclination of the beam, the variable distance

Fig. 2.



of the point C from the vertical line VV′ (fig. 1)*. It will then be at once seen that the vertical VV′ is a tangent, at the point corresponding to the horizontal position of the beam, to the curve described by C; further, that in the neighbourhood of this point the curve has seven elements in common with its tangent, and, lastly, that it cuts the same at a distance from G less than BC; so that, within the space described by C, the curve and the vertical have necessarily an eighth common element.

We see from this with what extreme rapidity the deviations of the point C from the vertical line V V′ (fig. 1) increase with the amplitude of the oscillations of the beam, the distances being of the seventh order with respect to the inclination of the beam. In ordinary practical cases, where the inclination is never great, the working of this mechanism would, as far as precision is concerned, be greatly superior to that of Watt. Take, as an example, the case treated by Prony in his well-known "Note sur le parallélogramme du balancier de la machine à feu"†, where the length of the semi-beam A B is 2·515 metres, that of the rod B C being 0·762 of a metre, and the greatest inclination of the beam 17° 35′ 30″. With the improved mechanism the deviations from the vertical would be less than 0·05 of a millimetre, whereas, according to Prony, the deviations with Watt's parallelogram would amount to 2 millimetres,—a quantity forty times the above, and far from being insignificant in the working of a machine of this description.

Hitherto, in seeking to approach as closely as possible to a

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* This variable distance is expressed by the formula

$$\frac{\sqrt{5}+1}{4}.\,\mathrm{AB}\,(\cos\psi-\cos\phi),$$

wherein the angles ψ and ϕ are functions of the inclination α of the beam which satisfy the two equations

$$\left(1-\frac{3-\sqrt{5}}{2}\cos\alpha-\frac{\sqrt{5}-1}{2}\cos\phi\right)^2$$
$$+\left(\frac{\mathrm{BC}}{\mathrm{AB}}-\frac{3-\sqrt{5}}{2}\sin\alpha+\frac{\sqrt{5}-1}{2}\sin\phi\right)^2=\frac{\mathrm{BC}^2}{\mathrm{AB}^2},$$

$$\left(1-\cos\alpha+\frac{\sqrt{5}+1}{4}\cos\phi-\frac{\sqrt{5}+1}{4}\cos\psi\right)^2$$
$$+\left(\frac{\mathrm{BC}}{\mathrm{AB}}-\sin\alpha+\frac{\sqrt{5}+1}{4}\sin\phi+\frac{\sqrt{5}+1}{4}\sin\psi\right)^2=\frac{\mathrm{BC}^2}{\mathrm{AB}^2}.$$

The approximate expression for the distance in question is consequently given by the series

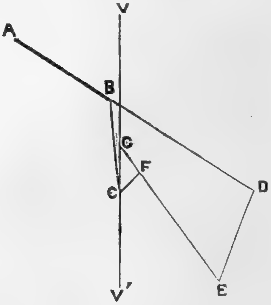
$$\frac{7-3\sqrt{5}}{32}\,\frac{\mathrm{AB}^2}{\mathrm{BC}}\,\alpha^7+\frac{\sqrt{5}-2}{16}\,\frac{\mathrm{AB}^3}{\mathrm{BC}^2}\,\alpha^8+\ldots$$

† *Annales des Mines*, vol. xii.

vertical motion, we have only considered how *many* elements were common to the vertical and to the curve described by the point C; the degree of approximation of the two, however, depends also very essentially upon the position of these elements. We have already examined this question in the first part of a memoir entitled "Théorie des mécanismes connus sous le nom des parallélogrammes"*, and therein proposed methods for rendering such an approximation as perfect as possible. By applying these methods to the case under consideration, we should be led to introduce certain small changes in the values of the several parameters, in order to render the mechanism as perfect as possible. By means of these corrections, the deviations of the point C would be reduced in the proportion of about 1 to 2^7 (see § 5 of the memoir cited). But since, in practical cases, these deviations, as we have seen, are themselves very small—amounting at most to some hundredths of a millimetre,—it is evident that by the application of the above corrections the theoretical precision of the apparatus might be carried to a limit unattainable by the mechanician. For ordinary practical purposes, therefore, there is no inducement to seek a mechanism capable of giving rectilineal motion with greater accuracy. This improved mechanism is the more worthy of attention, since, as we have shown, the utmost desirable precision is attained by employing the same number of pieces as in Watt's parallelogram, whose practical defects are often experienced.

We may observe, lastly, that the adjoining new form of the mechanism is obtained by changing the signs of the radicals in the foregoing values of the elements.

Fig. 3.



In this new form, where

$$CF = FG = \frac{\sqrt{5}-1}{4} AB,$$

$$BD = EG = \frac{\sqrt{5}+1}{2} AB,$$

the degree of working precision is theoretically the same as before; its construction, however, would necessitate the prolongation of the beam, and thus be attended with great disadvantages.

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* *Mémoires de Savants Etrangers,* vol. vii.

VI. *On the Barometer as an Indicator of the Earth's Rotation and the Sun's Distance.* *By* PLINY EARLE CHASE*.

THE existence of daily barometric tides has been known for more than a hundred and fifty years, but their cause is still a matter of dispute. It is evident that they cannot be accounted for by variations of temperature, for (1) their regularity is not perceived until all the *known* effects of temperature have been eliminated; (2) they occur in all climates, and at all seasons; (3) opposite effects are produced at different times, under the same average temperature. Thus at St. Helena the mean of three years' hourly observation gives the following average barometric heights:—

	h		h	in.		h		h	in.
From	0	to	12	28·2801	From	18	to	6	28·2838
From	12	to	0	28·2861	From	6	to	18	28·2784

The upper lines evidently embrace the coolest parts of the day, and the lower lines the warmest. Dividing the day in the first method, the barometer is highest when the thermometer is highest; but in the second division the high barometer prevails during the coolest half of the day.

On account of the combined effects of the earth's rotation and revolution, each particle of air has a velocity in the direction of its orbit, varying at the equator from about 65,000 miles per hour at noon, to 67,000 miles per hour at midnight. The force of rotation may be readily compared with that of gravity by observing the effects produced by each in twenty-four hours, the interval that elapses between two successive returns of any point to the same relative position with the sun. The force of rotation producing a daily motion of 24,895 miles, and the force of terrestrial gravity a motion of 22,738,900 miles, the ratio of the former to the latter is $\frac{24895}{22739000}$, or ·00109. This ratio represents the proportionate elevation or depression of the barometer above or below its mean height that should be caused by the earth's rotation, and it corresponds very nearly with the actual disturbance at stations near the equator.

From 0^h to 6^h the air has a forward motion greater than that of the earth, so that it tends to fly away; its pressure is therefore diminished, and the mercury falls. From 6^h to 12^h the earth's motion is greatest; it therefore presses against the lagging air, and the barometer rises. From 12^h to 18^h the earth moves away from the air, and the barometer falls; while from 18^h to 24^h the increasing velocity of the air urges it against the earth, and the barometer rises.

If the force of rotation at each instant be resolved into two components, one in the direction of the radius vector, and the

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* From Silliman's American Journal for May 1864.

other parallel to the earth's orbit, it will be readily perceived that whenever the latter tends to increase the aërial pressure, the former tends to diminish it, and *vice versâ*. Let B= the height of the barometer at any given instant; M= the mean height at the place of observation; $\theta-90^\circ=$ the hour-angle; C= the earth's circumference at the equator; $t=24$ hours; $g=$ the terrestrial gravity; $l=$ the latitude: and a simple integration gives the theoretical formula

$$B=M\left(1+\frac{\sin\theta\cos\theta\cos l}{R^3}\cdot\frac{2C}{gt^2}\right)^{*}.$$

This formula gives a maximum height at 9^{h} and 21^{h} and a minimum at 3^{h} and 15^{h}. The St. Helena observations place the maximum at 10^{h} and 22^{h} and the minimum at 4^{h} and 16^{h}, an hour later in each instance than the theoretical time. This is the precise amount of retardation caused by the inertia of the mercury, as indicated by the comparisons with the water barometer of the Royal Society of London.

Aërial currents, variations of temperature, moisture, and centrifugal force, solar and lunar attraction, the obliquity of the ecliptic, and various other disturbing causes, produce, as might be naturally expected, great differences between the results of theory and observation. But by taking the grand mean of a series of observations, sufficiently extended to balance and eliminate the principal opposing inequalities, the two results present a wonderful coincidence.

According to our formula, the differences of altitude at 1, 2, and 3 hours from the mean, should be in the respective ratios of ·5, ·866, and 1. The actual differences, according to the mean of the St Helena observations, are as follows:—

Differences of Barometer.				Ratios.		
Difference of time	1 h.	2 h.	3 h.	1 h.	2 h.	3 h.
Before 1h.	·0166	·0298	·0365	·455	·816	1
After 1h.............	·0159	·0266	·0298	·534	·893	1
Before 7h.	·0122	·0202	·0243	·502	·831	1
After 7h.............	·0135	·0239	·0297	·455	·805	1
Before 13h..........	·0136	·0248	·0284	·479	·873	1
After 13h.	·0131	·0215	·0227	·577	·947	1
Before 19h..........	·0161	·0287	·0348	·463	·825	1
After 19h.	·0150	·0265	·0286	·524	·927	1
Mean	·0145	·0252	·0293	·495	·860	1

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* $\frac{C}{gt^2}$ represents the effective ratio of an entire day. But there is in each day a half day of acceleration, and a half day of retardation, and the ratio for each half day is $\frac{C}{2}\div\frac{gt^2}{4}=\frac{2C}{gt^2}$.

The mean of the above differences varies from the theoretical mean less than $\frac{1}{5000}$ of an inch. If we take the mean of the ratios instead of the ratios of the means of the observed differences, the coincidence is still more striking.

Difference of time...............	1 h.	2 h.	3 h.
Means of observed ratios	·498625	·864625	1·000000
Theoretical means	·500000	·866025	1·000000

The calculated time for the above-observed means differs less than 20″ from the actual time.

Observed means.................	·498625	·864625	1·000000
Theoretical difference of time .	59′ 48″	119′ 40″	180′
Observed difference of time ...	60 0	120 0	180

The varying centrifugal force to which the earth is subjected by the ellipticity of its orbit, must in like manner produce annual tides. The disturbing elements render it impossible to determine the average monthly height of the barometer with any degree of accuracy, from any observations that have hitherto been made. We may, however, make an interesting approximation to the annual range, still using the St. Helena records, which are the most complete that have yet been published for any station near the equator. Comparing the mean daily range as determined by the average of the observations at each hour, with the mean yearly range as determined by the monthly averages, we obtain the following results:—

Year.	Daily range. in.	Annual range. in.	Ratio.	Approximate solar distance. m.
1844	·0672	·1650	2·4553	137,070,000
1845	·0646	·1214	1·8793	80,300,000
1846	·0670	·1214	1·8120	74,650,000
	3)·1988	3)·4078	3)6·1466	
	·0663	·1359	2·0489	95,446,000
Mean	·0663	·1290	1·9457	86,056,000
	2)·1326	2)·2649	2)3·9946	
	·0663	·1324	1·9973	90,702,000

The approximate estimates of the solar distance are based on the following hypothesis:—

Let e = effective ratio of daily rotation to gravity.
a = arc described by force of rotation in a given time t.
r = radius of relative sphere of attraction, or distance through which a body would fall by gravity during the disturbance of its equilibrium by rotation.
A = area described by radius vector in time t.

Let e', a', r', A' represent corresponding elements of the annual revolution. Then

$$A : A' :: ar : a'r' :: e^2 : e'^2.$$

But the forces of rotation and revolution are so connected that a differs but slightly from a'.

$$\left.\begin{array}{r} e^2 : e'^2 :: r : r', \\ \therefore\ r' = \dfrac{e'^2 r}{e^2} \end{array}\right\} \text{very nearly.}$$

It may be interesting to observe how nearly r (22,738,900 miles) corresponds with Kirkwood's value of $\frac{D}{2}$ (24,932,000 miles). A more thorough comprehension of all the various effects of gravity and rotation on the atmosphere, would probably lead to modifications of our formulæ that would show a still closer correspondence.

There is a great discrepancy between the determinations of the solar distance that are based on the records of 1844 and 1846; but it is no greater than we might reasonably have anticipated. On the other hand, it could hardly have been expected that any comparisons based on the observations of so short a period as three years, would have furnished so near an approximation to the most recent and most accurate determination of the earth's mean radius vector. In order to obtain that approximation, it will be seen that I took, 1st, the mean of the ranges and ratios for three successive years; 2nd, the ranges and ratios of the mean results of the three years; 3rd, the grand mean of these two primary means. I could think of no other method which would be so likely to destroy the effects of changing seasons, and other accidental disturbances.

The following Table exhibits the effects of latitude on the aërobaric tides. The differences between the theoretical and observed ranges may be owing partly to the equatorial-polar currents, and partly to insufficient observations.

Station.	Latitude.	Mean height.	Mean range.	Ratio.	Theoretical ratio.
	° ′	in.	in.		
Arctic Ocean ……	78 37	29·739	·012	·000404	·000527
Girard College …	39 58	29·938	·060	·002004	·002046
Washington ……	38 53	30·020	·062	·002065	·002079
St. Helena ………	15 57	28·282	·066	·002344	·002567
Equator …………	0	30·709	·082	·002670	·002670

The theoretical ratios are determined by multiplying the equatorial ratios by $\frac{\cos l}{R}$. The formula $\rho=\frac{\cos l}{R}\cdot\frac{2C}{gt^2}$ (ρ indicating the ratio of the mean range to the mean height) gives—

		Theoretical ratio.	Observed ratio.
Latitude	0° 0′	·002190	·002670
Latitude	78 37	·000432	·000404

showing that the ratio is less near the pole and greater near the equator than our theory indicates, a natural consequence of the centrifugal force at the equator and the cold surface currents that produce the trade-winds.

The revolution of the sun around the great Central Sun must also cause barometric fluctuations that may possibly be measured by delicate instruments and long and patient observation. The Torricellian column may thus become a valuable auxiliary in verifying or rectifying our estimates of the distances and masses of the principal heavenly bodies.

VII. *Notices respecting New Books.*

Manual of the Metalloids. *By* James Apjohn, *M.D., F.R.S., M.R.I.A., Professor of Chemistry in the University of Dublin.* London: Longmans, 1864, pp. viii and 596.

THIS work forms one of the series of Scientific Manuals issued under the auspices of Professors Galbraith and Haughton of Trinity College, Dublin. With regard to its intended scope and aim, the author says, "In preparing it my wish has been to produce a condensed, but at the same time tolerably comprehensive treatise, in which no topic of importance should be omitted, while all would be discussed with as much brevity as is consistent with clearness. It is intended as a Handbook in Chemistry for students in Medicine and Engineering, ..."

In books intended for the use of students, completeness in relation to matters of detail is unattainable, and is not even to be desired; but it is very important that such books should be as free as possible from errors, and that the knowledge gained by the study of them—though necessarily limited in extent—should be accurate, and should serve as a firm foundation for further acquisitions. In such works, even slight mistakes often amount to serious faults; and they are the less excusable, since the author, not being called upon to enter upon the more abstruse parts of the science, may generally ensure accuracy, upon the subjects of which it is desirable that he should treat, by exercising a moderate degree of care. In the present volume, errors in regard to the simplest matters of fact are, unfortunately, by no means rare, nor are they all of small importance. It would be tedious to the reader were we to quote all the passages upon which this assertion is founded: the following must suffice as specimens of many more that might be given.

At page 68 we read, "The compound, for example, generally known under the name of Dutch liquor, $Ȼ_2 H_4 Cl_2$, reacts upon water in the following manner:—

$$Ȼ_2 H_4 Cl_2 + H_2 Θ = 2 Cl H + Ȼ_2 H_4 Θ.$$

Now, viewing this latter substance, $Ȼ_2 H_4 Θ$, as a derivative of water, it is clear that $C_2 H_4$, now called ethylene, has replaced two atoms of hydrogen, and is therefore a binatomic body." What a capital process this is for preparing oxide of ethylene—upon paper, and what a pity that it does not answer equally well in sealed tubes!

On page 232 we are told that "the compounds of nitrogen with hydrogen are three in number, viz. amidogen, ammonia, and ammonium, and have their composition represented by the following formulæ:—

Amidogen	NH_2
Ammonia	NH_3
Ammonium	NH_4."

A more misleading statement than this could not easily be put before the student; for not only are the first and last of the three substances named completely unknown, but the whole analogy of the ascertained combining properties of nitrogen is against even the possibility of their existence, the most characteristic of all these properties being that, with elements of the hydrogen-class, nitrogen unites in only one proportion, that namely of which ammonia is an example. But our author habitually pays little regard to fine-drawn distinctions between substances which are well known as having a material existence, and those which are at best the convenient fictions of past or present theoretical systems. For instance (p. 256), he tells us that "There are seven known oxides of sulphur, all of which possess the properties of acids. The name and atomic composition of each is given in the subjoined Table:—

Sulphurous acid	SO_2
Sulphuric acid	SO_3
Hyposulphurous acid	$S_2 O_2$
Dithionic acid (Hyposulphuric)	$S_2 O_5$
Trithionic acid	$S_3 O_5$
Tetrathionic acid	$S_4 O_5$
Pentathionic acid	$S_5 O_5$.

The hyposulphurous and pentathionic acids are instances of polymeric bodies, or of such as have the same percentage composition, but different atomic weights." At page 478 occurs another example of the same kind: we there read, "The oxides of carbon are six in number, and of these all but the first are possessed of acid characters. They are—

Carbonic oxide	CO
Carbonic acid	CO_2
Oxalic acid	$HO, C_2 O_3 + 2 HO$
Rhodizonic acid	$3 HO, C_7 O_7$
Croconic acid	$HO, C_5 O_4$
Mellitic acid	$HO, C_4 O_3$."

From these instances it will be seen that Dr. Apjohn's lists of "known compounds" include many substances with which other chemists are by no means well acquainted: hence it is natural that he should require to make room for them by ignoring substances which often receive a considerable share of attention. Thus (p. 502) the usual list of hydrocarbons is very much curtailed: we are told that "The number of compounds of carbon and hydrogen is very great. Those at present known are reducible to three groups:—Those whose general formula is $C_n H_n$, those represented by $C_n H_{n+1}$, and those by $C_n H_{n+2}$, n being always an even number." Again (pp. 463, 464), "The only known compounds of boron with the metalloids are the teroxide, tersulphide, terchloride, and terfluoride." Surely Dr. Apjohn has heard of *nitride* of boron. No similarly distinct assertion is made of the non-existence of hydride of silicon and of the whole series of compounds corresponding with an oxide containing half as much oxygen as silica, discovered a few years ago by Wöhler; but, from the absence of the slightest allusion to any of these interesting substances, we must suppose that their existence is not yet recognized by our author.

If, from the enumeration of the compounds of the various elements, we turn to the detailed description of their properties and reactions, we find no greater accuracy. We will quote but one passage in illustration of this remark. It occurs on pages 561 and 562, and refers to the volumetric process for estimating cyanogen, in presence of excess of potash, by means of a standard solution of nitrate of silver. "The free potash will develop oxide of silver; but this is immediately taken up by the cyanide of potassium, with a view [*sic*] to the formation of the double cyanide; so that, as long as there is uncombined cyanide of potassium, there will be no permanent precipitate. But when the cyanide of potassium is altogether converted into the double cyanide of potassium and silver, if any additional quantity of the nitrate be added, the oxide of silver separated from it by the potash will appear as a permanent precipitate." It is difficult to suppose that the writer of this passage has ever performed the operation he professes to describe, otherwise he could hardly have failed to notice that in reality it is the white cyanide of silver, and not the brown-grey oxide which "*appears* as a permanent precipitate."

But the most remarkable portions of Dr. Apjohn's work are those in which he has occasion to refer to the past history of the science, or to record his opinion of the works of his contemporaries. At page 124, for instance, the relation of Lavoisier to the antiphlogistic system of chemistry is placed in a new light. "Stahl conceived that combustible bodies, such as carbon, sulphur, phosphorus, and iron, included a fiery principle, which he called phlogiston; and that, when they underwent combustion, the fiery principle is evolved. This phlogistic theory is at the present day only interesting in connexion with the history of chemistry. It held, however, its ground for a long time, and was only abandoned when it was shown by Ray and Mayow that bodies in burning, instead of becoming lighter, augment in weight.

"Lavoisier put forward on this subject a very plausible theory, which was founded on the well-known fact that, if a gas be compressed, heat will be developed." This passage not only contains a totally inadequate, and therefore erroneous statement of Lavoisier's "plausible theory," but implies what is directly contrary to facts well known to all who have paid any attention to the history of chemistry—namely, that the phlogistic theory held its ground long after it had been discovered that combustible bodies increase in weight when burned, and that this observation first came to be regarded as a serious objection to the theory when it was shown by Lavoisier to be connected with the disappearance of part of the atmosphere in which combustion takes place.

Every one knows that the discovery of the composition of water is attributed by some authorities to Cavendish, and by others to James Watt: according to Dr. Apjohn, similar rival claims have been put forward to the discovery of hydrogen itself. He says (p. 130), "Hydrogen . . . . . was first distinguished by Cavendish in 1766, and to him the credit of its discovery is usually given, though in modern times it has been claimed for Watt."

Further on (p. 471) we are told that Lavoisier and De Morveau burned the diamond in oxygen (discovered by Priestley in August 1774) about the year 1764. On page 212, the "difficulty of procuring absolute nitric acid, NO_5, now called nitric anhydride," is stated to have "been recently overcome by Naeterer," a chemist whose name we do not remember to have met with before; while nothing is said about M. H. Sainte-Claire Deville as having had anything to do with the matter.

After these specimens of Professor Apjohn's historical accuracy, the reader will not be surprised at slight peculiarities of spelling in the names of foreign chemists, such as Schonbein for Schönbein, Schrotter for Schrötter, or Lassaign for Lassaigne; but unless he is very well acquainted with the Professor's style, he may be a little at a loss on reading Bertholon (p. 255, and repeated in the index) instead of Berthelot, or on being told (p. 407) that Lavoisier, instead of Le Verrier, investigated oxide of phosphorus (or at least a substance so called).

We draw attention to these matters, not because the exact spelling of a chemist's name is of much importance to a student who is beginning the study of chemistry, but because they illustrate the inaccuracy and carelessness which pervade the whole book and give it throughout a slovenly air. Scrupulous accuracy in the statement of scientific facts and theories need not be expected from an author who thus wrongly names his authorities, or allows such examples of English composition as the following to go forth under his name:—

Page 183. "We now come to consider the relative proportions of the oxygen and nitrogen of which the atmosphere is chiefly composed. This is always done by condensing the oxygen of a known volume of atmospherical air, and measuring the nitrogen which is left."

Page 248. "Schlossing has ascertained that distillation by heat is not necessary; and that a solution of ammoniacal salt, to which a little hydrate of potash has been added, if placed for twenty-four hours under a glass bell with a cup of dilute sulphuric acid, passes completely from the alkaline to the acid liquid."

Page 417. "TERIODIDE OF PHOSPHORUS, $PI_3=285$.—This is obtained when into a thin test-tube, or small flask, containing phosphorus, and from which the air has been displaced by dry carbonic acid, twelve times its weight of iodine is introduced."

Page 297. "Such a result indeed is always obtained when the nitric acid is concentrated, and that the bottle or flask (it should be a strong one) is immediately closed by the pressure of the thumb after the acid has been introduced."

In conclusion, we can conscientiously say, after a careful examination of the book before us, that we have been unable to discover any one respect in which it is superior to the average of elementary works on chemistry, while, as we have pointed out, it frequently falls below the average. If such a work was to be published at all, it is to be regretted that it was issued as one of a series of educational works which have already acquired a certain reputation for general excellence, and are "recommended by the Committee of Council on Education."

VIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxvii. p. 542.]

February 25, 1864.—Major-General Sabine, President, in the Chair.

THE following communication was read:—

"On the supposed Identity of Biliverdin with Chlorophyll, with remarks on the Constitution of Chlorophyll." By G. G. Stokes, M.A., Sec.R.S.

I have lately been enabled to examine a specimen, prepared by Professor Harley, of the green substance obtained from the bile, which has been named biliverdin, and which was supposed by Berzelius to be identical with chlorophyll. The latter substance yields with alcohol, ether, chloroform, &c., solutions which are characterized by a peculiar and highly distinctive system of bands of absorption, and by a strong fluorescence of a blood-red colour. In solutions of biliverdin these characters are *wholly wanting*. There is, indeed, a vague minimum of transparency in the red; but it is totally unlike the intensely sharp absorption-band of chlorophyll, nor are the other bands of chlorophyll seen in biliverdin. In fact, no one who is in the habit of using a prism could suppose for a moment that the two were identical; for an observation which can be made in a few seconds, which requires no apparatus beyond a small prism, to be used with the naked eye, and which as a matter of course *would* be made by any chemist working at the subject, had the use of the prism made its way into the chemical world, is sufficient to show that chlorophyll and biliverdin are quite distinct.

I may take this opportunity of mentioning that I have been for a good while engaged at intervals with an optico-chemical examination of chlorophyll. I find the chlorophyll of land-plants to be a mixture of four substances, two green and two yellow, all possessing highly distinctive optical properties. The green substances yield solutions exhibiting a strong red fluorescence; the yellow substances do not. The four substances are soluble in the same solvents, and three of them are extremely easily decomposed by acids or even acid salts, such as binoxalate of potash; but by proper treatment each may be obtained in a state of very approximate isolation, so far at least as coloured substances are concerned. The *phyllocyanine* of Fremy* is mainly the product of decomposition by acids of one of the green bodies, and is naturally a substance of a nearly neutral tint, showing however extremely sharp bands of absorption in its neutral solutions, but dissolves in certain acids and acid solutions with a green or blue colour. Fremy's *phylloxanthine* differs according to the mode of preparation. When prepared by removing the green bodies by hydrate of alumina and a little water, it is mainly one of the yellow bodies; but when prepared by hydrochloric acid and ether, it is mainly a mixture of the same yellow body (partly, it may be, decomposed) with the product of decomposition by acids of the second green body. As the mode of preparation of *phylloxantheine* is rather hinted at than described, I can only conjecture what the substance is; but I suppose it to be a mixture of the second yellow substance with the products of decomposition of the other three bodies. Green seaweeds (*Chlorospermeæ*) agree with land-plants, except as to the relative proportion of the substances present; but in olive-coloured sea-weeds (*Melanospermeæ*) the second green substance is replaced by a third green substance, and the first yellow substance by a third yellow substance, to the presence of which the dull colour of those plants is due. The red colouring-matter of the red sea-weeds (*Rhodospermeæ*), which the plants contain in addition to chlorophyll, is altogether different in its nature from chlorophyll, as is already known, and would appear to be an albuminous substance. I hope, before long, to present to the Royal Society the details of these researches.

March 3.—Major-General Sabine, President, in the Chair.

The following communication was read:—

"On the Spectra of Ignited Gases and Vapours, with especial regard to the different Spectra of the same elementary gaseous substance." By Dr. Julius Plücker, of Bonn, For. Memb. R.S., and Dr. S. W. Hittorf, of Munster.

In order to obtain the spectra of the elementary bodies, we may employ either flame or the electric current. The former is the more easily managed, but its temperature is for the most part too low to volatilize the body to be examined, or, if it be volatilized or already in the state of gas, to exhibit its characteristic lines. In most cases it is only the electric current that is fitted to produce these lines; and

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* Comptes Rendus, tom. l. p. 405.

the current furnished by a powerful induction coil was what the authors generally employed.

In the application of the current, different cases may arise. The body to be examined may be either in the state of gas, or capable of being volatilized at a moderate temperature, such as glass will bear without softening, or its volatilization may require a temperature still higher.

In the first two cases the body is enclosed in a blown-glass vessel consisting of two bulbs, with platinum wires for electrodes, connected by a capillary tube. In the case of a gas, the vessel is exhausted by means of Geissler's exhauster, and filled with the gas at a suitable tension. In the case of a solid easily volatilized, a portion is introduced into the vessel, which is then exhausted as highly as possible, and the substance is heated by a lamp at the time of the observation. In the third case the electric current is employed at the same time for volatilizing the body and rendering its vapour luminous. If the body be a conductor, the electrodes are formed of it; but the spectrum observed exhibits not only the lines due to the body to be examined, but also those which depend on the interposed gas. This inconvenience is partly remedied by using hydrogen for the interposed gas, as its spectrum under these circumstances approaches to a continuous one. If the body to be examined be a non-conductor, the metallic electrodes are covered with it. In this case the spectrum observed contains the lines due to the metal of which the electrodes are formed, and to the interposed gas, as well as those due to the substance to be examined.

Among the substances examined, the authors commence with nitrogen, which first revealed to them the existence of two spectra belonging to the same substance. The phenomena presented by nitrogen are described in detail, which permits a shorter description to suffice for the other bodies examined.

On sending through a capillary tube containing nitrogen, at a pressure of from 40 to 80 millimetres, the direct discharge of a powerful Ruhmkorff's coil, a spectrum is obtained consisting, both in its more and in its less refrangible part, of a series of bright shaded bands: the middle part of the spectrum is usually less marked. In each of the two parts referred to, the bands are formed on the same type; but the type in the less refrangible part of the spectrum is quite different from that in the more refrangible. In the latter case the bands have a channeled appearance, an effect which is produced by a shading, the intensity of which decreases from the more to the less refracted part of each band. In a sufficiently pure and magnified spectrum, a small bright line is observed between the neighbouring channels, and the shading is resolved into dark lines, which are nearly equidistant, while their darkness decreases towards the least refracted limit of each band. With a similar power the bands in the less refrangible part of the spectrum are also seen to be traversed by fine dark lines, the arrangement of which, however, while similar for the different bands, is quite different from that observed in the channeled spaces belonging to the more refrangible region.

If, instead of sending the *direct* discharge of the induction coil through the capillary tube containing nitrogen, a Leyden jar be interposed in the secondary circuit in the usual way, the spectrum obtained is totally different. Instead of shaded bands, we have now a spectrum consisting of brilliant lines having no apparent relation whatsoever to the bands before observed. If the nitrogen employed contains a slight admixture of oxygen, the bright lines due to oxygen are seen as well as those due to nitrogen, whereas in the former spectrum a slight admixture of oxygen produced no apparent effect.

The different appearance of the bands in the more and in the less refracted portion of the spectrum first mentioned suggested to the authors that it was really composed of two spectra, which possibly might admit of being separated. This the authors succeeded in effecting by using a somewhat wider tube. Sent through this tube, the direct discharge gave a golden-coloured light, which was resolved by the prism into the shaded bands belonging to the less refrangible part of the spectrum, whereas with a small jar interposed the light was blue, and was resolved by the prism into the channeled spaces belonging to the more refrangible part.

By increasing the density of the gas and at the same time the power of the current, or else, in case the gas be less dense, by interposing in the secondary circuit at the same time a Leyden jar and a stratum of air, the authors obtained lines of dazzling brilliancy which were no longer well defined, but had become of appreciable breadth, while at the same time other lines, previously too faint to be seen, made their appearance. The number of these lines, however, is not unlimited. By the expansion of some of the lines, especially the brighter ones, the spectrum tended to become continuous.

Those spectra which are composed of rather broad bands, which show different appearances according as they are differently shaded by fine dark lines, the authors generally call *spectra of the first order*, while those spectra which show brilliant coloured lines on a more or less dark ground they call *spectra of the second order*.

Incandescent nitrogen accordingly exhibits two spectra of the first, and one of the second order. The temperature produced by the passage of an electric current increases with the quantity of electricity which passes, and for a given quantity with the suddenness of the passage. When the temperature produced by the discharge is comparatively low, incandescent nitrogen emits a golden-coloured light, which is resolved by the prism into shaded bands occupying chiefly the less refrangible part of the spectrum. At a higher temperature the light is blue, and is resolved by the prism into channeled bands filling the more refrangible part of the spectrum. At a still higher temperature the spectrum consists mainly of bright lines, which at the highest attainable temperature begin to expand, so that the spectrum tends to become continuous.

The authors think it probable that the three different spectra of the emitted light depend upon three allotropic states which nitrogen assumes at different temperatures.

By similar methods the authors obtained two different spectra of sulphur, one of the first and one of the second order. The spectrum of the first order exhibited channeled spaces, like one of the two spectra of that order of nitrogen; but the direction in which the depth of shading increased was the reverse of what was observed with nitrogen, the darker side of each channeled space being in the case of sulphur directed towards the red end of the spectrum.

Selenium, like sulphur, shows two spectra, one of the first and one of the second order.

Incandescent carbon, even in a state of the finest division, gives a continuous spectrum. Among the gases which by their decomposition, whether in flame or in the electric current, give the spectrum of carbon, the authors describe particularly the spectra of cyanogen and olefiant gas when burnt with oxygen or with air, and of carbonic oxide, carbonic acid, marsh-gas, olefiant gas, and methyle rendered incandescent by the electric discharge; they likewise describe the spectrum of the electric discharge between electrodes of carbon in an atmosphere of hydrogen. The spectrum of carbon examined under these various conditions showed great varieties, but all the different types observed were represented, more or less completely, in the spectrum of cyanogen fed with oxygen. The authors think it possible that certain bands, not due to nitrogen, seen in the flame of cyanogen, and not in any other compound of carbon, may have been due to the undecomposed gas.

The spectrum of hydrogen, as obtained by a small Ruhmkorff's coil, exhibited chiefly three bright lines. With the large coil employed by the authors, the lines slightly and unequally expanded. On interposing the Leyden jar, and using gas of a somewhat higher pressure, the spectrum was transformed into a continuous one, with a red line at one extremity, while at a still higher pressure this red line expanded into a band.

The authors also observed a new hydrogen spectrum, corresponding to a lower temperature, but having no resemblance at all to the spectra of the first order of nitrogen, sulphur, &c.

Oxygen gave only a spectrum of the second order, the different lines of which, however, expanded under certain circumstances into narrow bands, but very differently in different parts of the spectrum.

Phosphorus, when treated like sulphur, gave only a spectrum of the second order.

Chlorine, bromine, and iodine, when examined by the electric discharge, gave only spectra of the second order, in which no two of the numerous spectral lines belonging to the three substances were coincident. The authors were desirous of examining whether iodine would give a spectrum of the first order the reverse of the absorption-spectrum at ordinary temperatures. The vapour of iodine in an oxyhydrogen jet gave, indeed, a spectrum of the first order, but it did not agree with what theory might have led us to expect.

In the electric discharge, arsenic and mercury gave only spectra of the second order. The metals of the alkalies sodium, potassium, lithium, thallium show, even at the lower temperature of Bunsen's lamp, spectra of the second order.

Barium, strontium, calcium in the flame of Bunsen's lamp show bands like spectra of the first order, and in each case a well-defined line-like spectra of the second order. On introducing chloride of barium into an oxyhydrogen jet, the shading of the bands was resolved into fine dark lines, proving that the band spectrum of barium is in every respect a spectrum of the first order.

Spectra of the first order were observed in the case of only a few of the heavy metals, among which may be particularly mentioned lead, which, when its chloride, bromide, iodide, or oxide was introduced into an oxyhydrogen jet, gave a spectrum with bands which had a channeled appearance in consequence of a shading by fine dark lines.

Chloride, bromide, and iodide of copper gave in a Bunsen's lamp, or the oxyhydrogen jet, spectra with bands, and besides a few bright lines. The bands in the three cases were not quite the same, but differed from one another by additional bands. Manganese showed a curious spectrum of the first order. When an induction discharge passed between electrodes of copper or of manganese, pure spectra of these metals, of the second order, were obtained.

March 17.—Major-General Sabine, President, in the Chair.

The following communication was read:—

"Remarks on Sun Spots." By Balfour Stewart, M.A., F.R.S., Superintendent of the Kew Observatory.

In the volume on Sun Spots which Carrington has recently published, we are furnished with a curve denoting the relative frequency of these phenomena from 1760 to the present time. This curve exhibits a maximum corresponding to 1788·6. Again, in Dalton's 'Meteorology' we have a list of auroræ observed at Kendal and Keswick from May 1786 to May 1793.

The observations at Kendal were made by Dalton himself, and those at Keswick by Crosthwaite. This list gives—

For the year 1787	27 auroræ,	For the year 1790	36 auroræ;
1788	53 „	1791	37 „
1789	45 „	1792	23 „

showing a maximum about the middle, or near the end of 1788. This corresponds very nearly with 1788·6, which we have seen is one of Carrington's dates of maximum sun spots.

The following observation is unconnected with the aurora borealis. In examining the sun pictures taken with the Kew Heliograph under the superintendence of Mr. De la Rue, it appears to be a nearly universal law that the faculæ belonging to a spot appear to the left of that spot, the motion due to the sun's rotation being across the picture from left to right.

These pictures comprise a few taken in 1858, more in 1859, a few in 1861, and many more in 1862 and 1863, and they have been carefully examined by Mr. Beckley, of Kew Observatory, and myself. The following Table expresses the result obtained:—

Year.	No. of cases of facula to left of spot.	No. of cases of facula to right of spot.	No. of cases of facula equally on both sides of spot.	No. of cases of faculæ mostly between two spots.
1858	2	0	0	0
1859	18	0	0	3
1861	9	1	3	0
1862	64	4	7	3
1863	47	0	9	2
1864	18	1	2	1

April 7.—Major-General Sabine, President, in the Chair.

The following communication was read:—

"Description of a train of Eleven Sulphide-of-Carbon Prisms arranged for Spectrum Analysis." By J. P. Gassiot, F.R.S.

The principles which should regulate the construction of a battery of prisms have been alluded to in the description of the large spectroscope now at Kew Observatory, which has a train of nine dense glass prisms with refracting angles of 45°*.

While for purposes of exactitude, such as mapping out the solar spectrum, flint glass stands unrivalled; yet when the greatest amount of dispersion is the desideratum, prisms filled with bisulphide of carbon present obvious advantages, on account of the enormous dispersive power of that liquid—the difference of its indices of refraction for extreme rays being, according to Sir David Brewster, as 0·077 against 0·026 for flint glass.

In the fluid prisms of the ordinary construction, the sides are cemented on with a mixture of glue and honey. This cement, on hardening, warps the sides, and confusion of the spectral lines is the consequent result. To obviate this source of error, it has been proposed to attach an additional pair of parallel sides to such prisms, a thin film of castor-oil being interposed between the surfaces. The outer plates are then secured by means of sealing-wax, or some cement, at the corners. In the battery of prisms now about to be described, Mr. Browning has dispensed with this attachment at the corners, which is likely to prove prejudicial, and has secured the second sides in their proper position by extremely light metal frames which clasp the plates only on their edges.

Thus arranged, the frames exert no pressure on the surfaces of the plates, and are quite out of the field of view, and they can be handled without any fear of derangement.

On account of the lower refractive power of bisulphide of carbon, as compared with flint glass, a refractive angle of 50° was given to the fluid prisms. Eight such prisms would cause a ray of light to travel more than a circle, and would be the greatest number that could be employed had the ordinary arrangement been adopted.

In place, however, of giving to the fluid prisms two pairs of parallel sides, Mr. Browning, taking advantage of the difference between the refractive and dispersive properties of crown glass and bisulphide of carbon, has substituted a prism of crown glass having

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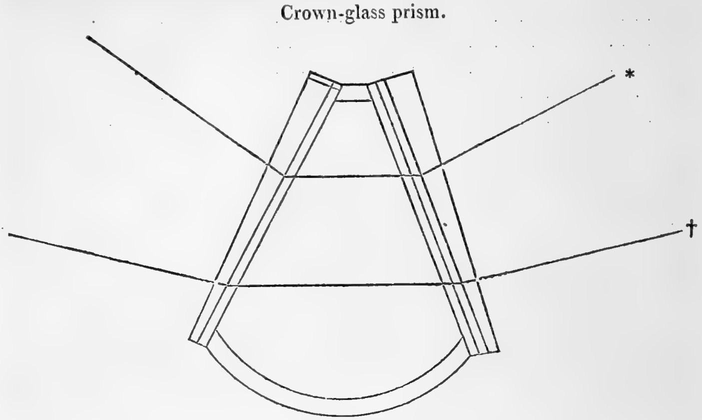
* Phil. Mag., vol. xxvii. p. 143.

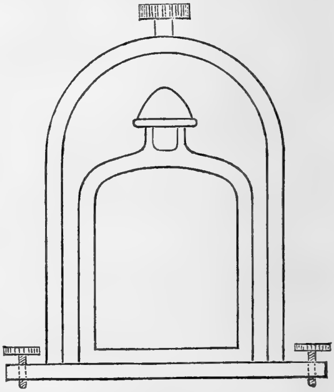
a refracting angle of 6° for one of the outer plates of each prism—the base of this crown-glass prism being brought to correspond with the apex of the fluid prism, thus :—



By this means the angle of minimum deviation of the prisms is so much decreased, that eleven of them thus constructed can be used in a circle instead of eight. An increase of dispersive power, due to refracting angles of 150° of the bisulphide of carbon, is thus gained, minus only the small amount of dispersion counteracted owing to the dispersive power of the crown-glass prisms being employed in the contrary direction.

From the well-known low dispersive power of this medium, however, this loss is inconsiderable, amounting to scarcely more than a fifteenth of the power gained. Owing to the minimum angle of deviation being lowered, the further advantage is also secured of a larger field of view being presented to the telescope by the first and last prism of the train.

Each prism, in addition to the light metal frame referred to, has a separate stand, furnished with screws for adjusting the prisms, and securing them at the angle of minimum deviation for any particular ray. The prism stands within a stirrup furnished with a welled head. By this arrangement the prisms can be removed and replaced without touching their sides—a matter of some importance, as all fluid prisms show different results with every change of temperature.



For the sake of simplicity, the

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* Direction of ray as it would pass through two pair of parallel sides.

† Direction of ray as altered by interposing the crown-glass prism.

metal framing of the prisms, and the various adjusting-screws, have been omitted in the last sketch.

The very unfavourable state of the weather prevented any observations being made on the solar spectrum with these prisms until Saturday the 12th inst. The results then obtained may probably not be considered devoid of interest. They are as follows:—

The prisms were arranged so as to enable that portion of the spectrum to be observed in which the well-defined D line of Fraunhofer is situated. This line, long since resolved as double, presented an angular separation of 3′ 6″, measured from the centre of one to that of the other principal line, this measurement being made by Mr. Balfour Stewart by means of the micrometer attached to the telescope; the value of the divisions of the micrometer he had previously determined relatively to the divided circle of the spectroscope. A centre line (clearly defined and figured in Kirchhoff and Bunsen's map) was distinctly visible, and nearly equidistant from the centre towards the violet; five clearly defined lines were perceptible, as also two faint lines on each side of the principal lines, between the centre line of Kirchhoff towards the red. Several faint lines were also perceptible.

The lines as represented in the diagram were drawn by Mr. Whipple, one of the assistants in the Observatory, as they were observed by him about 3.45 P.M Some of these may possibly be due to the earth's atmosphere, but the five most refrangible lines were observed at an earlier period of the day by Mr. Stewart, Mr. Browning, and myself.

The great angular separation of the double D line to 3′ 6″ is a proof of the power of this arrangement of the sulphide-of-carbon prisms, and offers the means of mapping out the entire solar spectrum on a scale not hitherto attained.

Note.—Since the preceding observations were recorded, an inspection has been made of the region of the spectrum towards the refrangible side of double D; and, from the comparisons made with a map of lines obtained by means of the battery of glass prisms with that given by those of the sulphide-of-carbon prisms, many new lines are produced in addition to those observable by the former, while the battery of glass prisms itself gives a number of additional lines to those that are depicted in Kirchhoff's map.

GEOLOGICAL SOCIETY.

[Continued from vol. xxvii. p. 545.]

March 23, 1864.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "On some new Fossils from the Lingula-flags of Wales." By J. W. Salter, Esq., F.G.S., A.L.S.

Since the author's paper last session, on the discovery of *Paradoxides* in Britain, the researches of Mr. Hicks have brought to light so many new members of the hitherto scanty fauna of the Primordial zone, that Mr. Salter was now enabled to describe two new genera of Trilobites and a new genus of Sponges, and to complete the description of *Paradoxides Davidis*. He also remarked that the fauna of the Lingula-flags shows an approximation, in some of its genera, to Lower Silurian forms, and some (the Shells and a Cystidean) are of genera common to both formations; but the Crustacea, which are the surest indices of the age of Palæozoic rocks, are of entirely distinct genera; and their evidence quite outweighs that of the other fossils. The Primordial zone is moreover in Britain, separated from the Caradoc and Llandeilo beds by the whole of the Tremadoc group, at least 2000 feet thick.

2. "On the Millstone-grit of North Staffordshire, and the adjoining parts of Derbyshire, Cheshire, and Lancashire." By E. Hull, Esq., B.A., F.G.S., and A. H. Green, Esq., M.A., F.G.S.

In this paper the Millstone-grit series was described, from the eastern edge of the Lancashire Coal-field southwards to the Coalfields of North Staffordshire.

After giving a general sketch of the Geology of the district, and defining the upper and lower limits of the Millstone-grit, the authors explained a series of sections, running from east to west, at intervals, across the country. In the most northerly of these the group consists of five thick gritstone-beds, separated by seams of shale, and attains a thickness of more than 2000 feet; while on the extreme south all but two of these beds have thinned away, and the whole thickness is there not more than 300 or 400 feet.

Between the base of the Millstone-grit and the Carboniferous Limestone lies a group of shales and sandstones, with thin earthy limestones towards the bottom, which seem to hold the place of the Yoredale Rocks of Yorkshire. The mineral character of these beds was described, and their place noted on the sections.

A short notice was also given of two small inliers of Carboniferous Limestone, namely, at Moxon, east of Leek, and at Astbury, near Congleton.

April 13.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "On the Geology and Mines of the Nevada Territory." By W. Phipps Blake, Esq.

In describing the physical features of the country, the author observed that it is an elevated semi-desert region, composed of a succession of longitudinal mountain-ranges with intermediate valleys and plains, the most abundant rocks being Metamorphic and Igneous; but Tertiary strata and Carboniferous Limestone also occur.

The author then described the hot springs, which are extended along a line of fissure in a granitic rock, and parallel to the mountains, and which deposit silica in an amorphous and a granular state, sulphur being also seen in the cracks and cavities of the siliceous deposit. He considered these phenomena to illustrate the formation of a quartz-vein in a fissure.

Mr. Blake then gave an account of certain mineral veins in porphyry, which yield sulphurets of silver (including crystals of Stephanite, but very little ruby silver) and a little gold; also galena, copper pyrites, iron pyrites, and a little native silver, the veinstone being a friable quartz. The prevailing direction of the veins was stated to be nearly north and south; and the author remarked that they were richer in gold near the surface than at greater depths.

2. "On the Red Rock in the Section at Hunstanton." By Harry Seeley, Esq., F.G.S., of the Woodwardian Museum, Cambridge.

The physical structure of the rock was first considered, and it was shown to be divisible into three beds, the uppermost of which is of a much lighter colour than the rest, the middle being concretionary in structure, and the lower sandy. These three beds, with the overlying white sponge-bed, were considered to belong to one formation, and were treated of in this paper as the Hunstanton Rock; but the thin band of red chalk some distance above was considered, though of similar colour, to be quite distinct, as also was the Carstone below.

Mr. Seeley then showed that near Cambridge the Shanklin Sands and the Gault have both become very thin, so that there is a great probability of the latter being unconformable to the beds above as well as to those below. He considered the lower part of the Carstone to be of the age of the Shanklin sands; and as the Chalk is not unconformable to the Hunstanton Rock, he concluded that the latter could not be the Gault, but must be the Upper Greensand,—a conclusion which he afterwards showed was supported by the evidence of the fossils, and the occurrence of phosphate of lime.

The seam of soapy clay which separates the Hunstanton Rock from the Chalk was supposed to have resulted from the disintegration of a portion of the former, the red colour of which the author endeavoured to show was due to Glauconite.

The upper part of the red rock of Speeton was thought to be possibly newer than that of Hunstanton, and perhaps to represent the time which elapsed between the formation of the latter and that of the band of red chalk.

In conclusion, Mr. Seeley remarked that as the phosphate of lime is confined to Bed No. 2, and as many individuals of Gault species occur in Bed No. 3, while others of a Chalk character are met with in Bed No. 1, it is very probable that the Hunstanton Rock is a more typical example of the Upper Greensand than is seen at Cambridge, and may represent also those periods which separate that formation from other divisions of the Cretaceous system.

April 27.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read :—

1. " On the Geology of Arisaig, Nova Scotia." By the Rev. D. Honeyman, F.G.S.

A careful examination of the country in the neighbourhood of Arisaig enabled the author to construct three sections and a map showing the geological constitution of the district. Two of these sections were nearly parallel to one another, running from N. to S., and taken some distance apart, while the third was nearly at right angles to the other two; thus a tolerably accurate idea of the geology of the country could be obtained. The author described each of these sections in detail, giving lists of the fossils found in the different beds, which proved them to be of Upper Silurian age; and he further considered that they justified the adoption for the subdivisions of these Nova-Scotian Silurians of the terms May-hill, Lower Ludlow, Aymestry, and Tilestones, the first and third of which had been used for them previously by Mr. Salter. Besides Silurian rocks, there occurs in the western part of this district a conglomerate of Lower Carboniferous age, while trap-rocks occur on the north and south.

2. " On some Remains of Fishes from the 'Upper Limestone' of the Permian Series of Durham." By J. W. Kirkby, Esq.

The object of this paper was to record the discovery of Fish-remains in the upper Magnesian Limestone of the Permian formation, which is higher in that series than any vertebrate remains had been previously known to occur. The strata exposed in the quarries were described in detail, especially the bed from which most of the Fishes were obtained, and which is known as the "flexible limestone."

The author stated that at least nine-tenths of the specimens belong to *Palæoniscus varians*, the remainder belonging to two or three species of the same genus, and to a species of *Acrolepis*. Detailed descriptions of the different species of Fishes were given, as also were short notices of the species of Plants sometimes found associated with them, one of which he believed to be *Calamites arenaceus*, a Triassic species. The occurrence of *Palæonisci* with smooth scales was stated to be antagonistic to Agassiz's conclusion that the Permian species of that genus have striated, and the Coal-measure species smooth scales. In conclusion Mr. Kirkby remarked that the fauna of the period appeared to have an Estuarine facies, and he expressed his opinion that the Fishes were imbedded suddenly, as a result of some general catastrophe.

3. " On the Fossil Corals of the West Indian Islands.—Part 3. Mineral Condition." By P. Martin Duncan, M.B. Lond., Sec. G.S.

The results of the process of fossilization, as seen in the West Indian fossil Corals, being very remarkable, and having much obscured their specific characters, thus rendering their determination extremely difficult, Dr. Duncan found it necessary to thoroughly examine their different varieties of mineralization, and to compare their present condition with the different stages in the decay and fossilization of recent Corals as now seen in progress. Thus the author was enabled to show the connexion between the destruction of the minuter structures of the Coral by membrane decomposing and certain forms of fossilization in which those structures are imperfectly preserved; and he likewise stated that the filling-up of the interspaces by granular carbonate of lime and other substances, as well as the induration of certain species, during a "prefossil" and "post-mortem" period, gave rise to certain varieties of fossilization, and that the results of those operations were perpetuated in a fossil state.

The forms of mineralization described by Dr. Duncan are—(1) Calcareous; (2) Siliceous; (3) Siliceous and Crystalline; (4) Siliceous and Destructive; (5) Siliceous Casts; (6) Calcareo-siliceous; (7) Calcareo-siliceous and Destructive; (8) Calcareo-siliceous Casts.

In describing these forms, especial reference was made to those in which the structures were more or less destroyed during the replacement (by silica) of the carbonate of lime which filled the interspaces, and during that of the ordinary hard parts of the Coral.

In explaining the nature and mode of formation of the large casts of calices from Antigua, the author drew attention to the fact that the silicification is more intense on the surface and in the centre of the corallum than in the intermediate region; and, when examined microscopically, it could be seen that the replacement of the carbonate of lime began by the silica appearing as minute points in the centre of the interspaces and of the sclerenchyma, and not on their surface. In conclusion, the relation of hydrated silica to destructive forms of fossilization was discussed, together with the influence of all the forms enumerated above in the preservation of organisms, and as one cause of the incompleteness of the geological record.

IX. *Intelligence and Miscellaneous Articles.*

REMARKS ON THE DISTILLATION OF SUBSTANCES OF DIFFERENT VOLATILITIES. BY M. CAREY LEA.

SOME experiments which have been recently published by M. Berthelot recall to me a similar and remarkable case which attracted my attention several years ago.

M. Berthelot distilled 92 parts of alcohol and 8 of water, and found that the distillate at the beginning, middle, and end of the operation contained equal quantities of water and of alcohol.

He distilled also a mixture containing a large quantity of sulphide of carbon and a small quantity of alcohol, and found that the least volatile body, the alcohol, passed over with the first portions of the distillate, so that toward the end of the operation the retort contained sulphide of carbon almost pure.

To these facts, which tend to cast the greatest doubt on all the results obtained by the laborious process of fractional distillation, I now add the following.

When a mixture containing the chlorides of ethylamine, diethylamine, and triethylamine is distilled with caustic alkali, we should, according to received ideas, expect to find the ethylamine, which is a gas at ordinary temperatures, distil over first. Triethylamine, which is at ordinary temperatures and pressures a liquid, separates as such when a strong solution of its chloride is treated with caustic alkali, and, floating on the surface, as I have before pointed out, we would naturally expect to find it principally in the latter stages of the distillation. The contrary is, however, the case when the less substituted ammonias predominate in quantity. Almost the whole of the triethylamine passes over in the first portions of the distillate, and subsequent ones, though rich in ethylamine and diethylamine, scarcely contain a trace of triethylamine.—Silliman's *American Journal*, May 1864,

NOTE ON THE RESIDUAL CHARGE OF ELECTRICAL CONDENSERS.

BY M. J. M. GAUGAIN.

When after having discharged a Leyden jar it is left to itself and after some time a new metallic connexion is established between its armatures, we all know that a second spark is obtained, less strong than the first. This fact, generally known as the *secondary discharge*, has been designated by Mr. Faraday the *residual charge*. I have adopted this latter name, slightly modifying the sense, to designate, not the quantity of electricity which passes in a second discharge, but all that remains after the original discharge, a quantity which may give rise to a multitude of successive secondary discharges.

The existence of the residual charge is generally explained by saying that part of the electricity of the armatures penetrates slowly into the interior of the dielectric when the condenser is charged, and that this portion, slowly absorbed, is restored with equal slowness. But this explanation can certainly not apply to the experiments of which I am about to speak; for these experiments have been made in such conditions that the electricity of the armatures could not communicate itself to the dielectric, and yet the residual charge formed in certain cases more than three-quarters of the total charge.

I worked, as in my former researches, on small fulminating panes, with moveable armatures: in certain cases the armatures were applied directly to the dielectric; in other cases they were separated by small layers of air of uniform thickness. The general results were the same in either case.

In a first series of researches I proposed to ascertain according to what law the residual charge varies when the duration of the charge varies—that is, the time during which the condenser is in connexion with the electrical source. I suppose the tension of this source to be invariable, as is the duration of the discharge. This was always a fraction of a second, the same in all the experiments. The observations were made in the following manner.

The lower armature of the fulminating pane on which I worked being in connexion with the ground, I connect for a definite time the upper armature with a source of constant tension; the condenser once charged, I detach the upper armature, and measure its total charge by the method which I have called *gauging*.

Secondly, after this first operation, and when the dielectric has reverted to the neutral state, I charge the condenser again for the same time as at first, then I discharge it immediately by connecting for an instant the armatures; that done, I remove the higher armature, and gauge the quantity of electricity which it retains; this quantity represents the residual charge according to the definition given above.

When this double series of operations is performed on the same condenser, giving successively different values to the duration of the discharge, this very simple result is attained—that the difference between the total and the residual charge is constant. This difference, which represents the quantity of electricity which has disappeared in an instantaneous discharge, is precisely equal to the total instantaneous charge. I denote in this manner the quantity of electricity which the influencing armature would receive if the condenser, completely discharged, were put in connexion with the source of electricity during a small interval of time equal to that taken for the discharge. This law was verified by a great number of experiments, and on very different dielectrics. I worked successively on disks of shellac, of stearic acid, and of gutta percha, and on a cake made of flour of sulphur moistened by salad oil. I give the results obtained by a series of experiments made with the latter substance:—

Duration of the charge.	Total charge.	Residual charge.	Difference.
Fraction of a second	26	..	26
2 minutes	44	18	26
4 minutes	49	23	26
8 minutes	55	28	27
16 minutes	59	33	26

The difference between the total and the residual discharge was sensibly the same for all durations of the charge, and equal to 26, a number which exactly represents the total instantaneous charge.

Although observation alone would have enabled me to ascertain this relation, it is easy to see *à priori* that it must exist if the bodies called insulating are generally formed, as I have been led to admit, of many elements of very different conductibilities.

In the experiments I have just cited, the condenser was charged for a more or less long time, but always discharged immediately after being separated from the electric source. In another series of researches the condenser was always charged for the same time, and discharged during the same fraction of a second; but the discharge was separated from the charge by longer or shorter intervals. This kind of observation appeared to me very suitable for putting in evidence the true origin of the residual charge.

I shall cite the results of a series of experiments in which the duration of the charge was limited, like that of the discharge, to a fraction of a second; the dielectric was a disk of shellac 6 millims. in thickness.

1. The condenser was charged and gauged immediately after: the total charge was 45.

2. The condenser, after being charged, was left to itself for 15 minutes, and gauged at the end of this time: the total charge was again 45.

3. The condenser was discharged immediately after being charged: the residual charge was zero.

4. Lastly, the condenser was discharged 15 minutes after the charge: the residual charge was 27.

Experiments 1 and 2 prove clearly that in the interval of 15 minutes the armature gauged loses nothing of its charge, and that therefore no appreciable absorption is exerted by the shellac; and yet it follows, from experiments 3 and 4, that in this time of 15 minutes the residual charge rose from zero to 27. This increase of residual charge can only depend on a different arrangement of electricity in the interior of the dielectric. When the charge has only been maintained for an instant, the parts of the dielectric which possess a great conductibility alone participate in the transmission of the influence; and as an instant is sufficient to polarize them, an instant is sufficient to restore them to the neutral state. If, on the contrary, the apparatus has been charged for a sufficiently long time, the elements endowed with a feeble conductibility come into play; and as they cannot be restored to the neutral state in a very short time, they retain after the discharge almost all the electricity they had before. This electricity retains a portion of the electricity of opposite kind which is accumulated on the armature.

The residual charge is thus seen not to depend on a property of absorption specially belonging to insulating bodies; it depends simply on electrical movements which take place in the interior of these bodies in virtue of their conductibility.—*Comptes Rendus*, May 2, 1864.

ON THE BOILING OF WATER, AND ON THE EXPLOSION OF STEAM-BOILERS. BY M. L. DUFOUR OF LAUSANNE.

In the experiments which I have had the honour of communicating to the Academy, I have shown that the boiling-point of water and of other liquids may experience considerable retardation when these liquids are heated in the body of another liquid of the same density and without touching the sides of the vessels. In this mode of heating the liquids, it cannot be said that their boiling is produced at a fixed point; the change of state becomes possible when the temperature can give to the vapour an elastic force equal to the external pressure; but this change takes place only very rarely at the exact point at which its possibility commences.

With a view to the study of ebullition, I have undertaken a great number of experiments; and among others, I have endeavoured to study ebullition by arriving at this phenomenon rather by a change of pressure, which the liquid undergoes, than by an increase of its temperature. The apparatus resembles, with certain modifications, that which M. Regnault used in studying the elastic force of aqueous vapour. A sheet-iron vessel communicates, by suitable tubes, (1) with an air-pump, (2) with a mercury manometer, (3) with a glass retort. In this retort were placed the liquids experimented upon, and a thermometer with a small reservoir plunged in the interior. By means of stopcocks, suitably arranged, the various parts of the apparatus could be connected. An observation of the manometer and of an external barometer obviously gave, at any moment, the external pressure of the apparatus.

Studied under these circumstances, the boiling of water presents some characters worthy of attention. In the case of distilled water, it is soon seen that after a first heating to 100°, boiling obtained by diminution of pressure is only produced at the temperature which the known law requires. Water remains liquid although the pressure is far below the tension of aqueous vapour for the temperature in question. When boiling commences, it is produced with tremulous violence, and usually part of the liquid is carried into the tubes with the first burst of vapour. These retardations are then more pronounced the more frequently water has been raised to a high temperature. They are more considerable when the water has been alternately heated to 110° and then cooled in the apparatus a certain number of times before being submitted to the test of diminution of pressure. The following are some examples in which are noted in three successive columns, (1) the temperature of the liquid when boiling commences; (2) the pressure at this time; (3) the temperature at which normal ebullition would take place for this pressure:—

°	mm.	°
71	175	64
57	75	46
66	108	53·5
90·5	335	78·7
53	37	33

Retardations of 7°, 11°, 11°·8, 20°, &c. are thus seen; that is, far more considerable than those observed for water in glass vessels when ebullition is attained by reheating.

Taking ordinary water, not distilled, and even tolerably calcareous, the same facts are observed; but it is necessary that the water should have been two or three times heated to boiling, then cooled in the vessel, or submitted to a very prolonged ebullition before being submitted to diminutions of pressure. Normal ebullition is less rare than with distilled water; but nevertheless very frequent retardations of 10°, 15°, and more are seen, as in preceding experiments.

The presence of platinum, and in general of metallic substances, is known to have the reputation of hindering retardations of ebullition in glass vessels; and for a long time platinum wires have been used in concentrating liquids to prevent them from jumping over. Platinum wires placed in distilled water hinder, in fact, these retardations from being produced when, after having been heated once or twice to 100°, water is subjected to a diminution of pressure. But if the liquid containing platinum wire is heated to boiling for several times, and then allowed to cool—if especially platinum is for some days in contact with water at the bottom of the vessel, it is soon seen that the metal has become inactive, and then delays are observed as considerable as if water alone were in the retort. If ordinary water is taken abundantly carbonated, if along with it various solid bodies are introduced, facts are observed analogous to that which has been mentioned in the case of platinum. I have tried pieces of iron, lead, tin, zinc, copper, &c.; fragments of chalk, of wood, of quartz, paper, &c. In the first reheating, the presence of these bodies prevents any retardation, and ebullition takes place at the exact point at which the temperature of the liquid imparts to the vapour an elastic force equal to the superficial pressure. But if they are left for some time in contact with water, heated four or five times to ebullition, the contact of all these bodies appears to have become indifferent, and the liquid furnishes then very frequent examples of the retardation of ebullition. The following are some examples in which the retort contained ordinary water, with fragments of iron, platinum, lead, chalk, and wood:—

°	mm.	°
74	217	68·5
85	171	63·2
67	71	45
72	87	49.

These correspond to retardations of 5°·5, 21°·8, 22°, 23°. Ebullition then commenced, sometimes spontaneously, sometimes by a blow given to the vessel; it was always very tumultuous and violent, almost explosive.

These facts and others relative to other liquids, show that the ordinary law regarding the boiling-point of a liquid in reference to its pressure can only apply when ebullition is arrived at by a change in pressure rather than by a variation of temperature. These facts show, moreover, that water is susceptible of presenting great retardation in its ebullition, even when in contact with any metals and solid substances. Glass and porcelain vessels form by no means an exception. Lastly, it is seen that the contact of solids is sometimes active and sometimes indifferent; and by analyzing the experiments of which I have given extracts, it is soon seen that the very probable cause of this change of influence is the presence or absence round these solids of a more or less condensed gaseous atmosphere. —*Comptes Rendus*, May 30, 1864.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

AUGUST 1864.

X. *On the Absorption and Radiation of Heat by Gaseous and Liquid Matter.*—Fourth Memoir. *By* JOHN TYNDALL, *F.R.S., &c.*[*]

THE Royal Society has already done me the honour of publishing in the Philosophical Transactions three memoirs on the relations of radiant heat to the gaseous form of matter. In the first of these memoirs[†] it was shown that for heat emanating from the blackened surface of a cube filled with boiling water, a class of bodies which had been previously regarded as equally, and indeed, as far as laboratory experiments went, perfectly diathermic, exhibited vast differences both as regards radiation and absorption. At the common tension of one atmosphere the absorptive energy of olefiant gas, for example, was found to be 290 times that of air, while when lower pressures were employed the ratio was still greater. The reciprocity of absorption and radiation on the part of gases was also experimentally established in this first investigation.

In the second inquiry[‡] I employed a different and more powerful source of heat, my desire being to bring out with still greater decision the differences which revealed themselves in the first investigation. By carefully purifying the transparent elementary gases, and thus reducing the action upon radiant heat, the difference between them and the more strongly acting compound gases was greatly augmented. In this second inquiry, for example, olefiant gas at a pressure of one atmosphere was

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[*] From the Philosophical Transactions, Part II. 1864, having been read at the Royal Society June 18, 1863.

[†] Phil. Trans. February 1861; and Phil. Mag. September 1861.

[‡] Phil. Trans. January 1862; and Phil. Mag. October 1862.

shown to possess 970 times the absorptive energy of atmospheric air, while it was shown to be probable that, when pressures of $\frac{1}{30}$th of an atmosphere were compared, the absorption of olefiant gas was nearly 8000 times that of air. A column of ammoniacal gas, moreover, 3 feet long, was found sensibly impervious to the heat employed in the inquiry, while the vapours of many of the volatile liquids were proved to be still more opake to radiant heat than even the most powerfully acting permanent gases. In this second investigation, the discovery of dynamic radiation and absorption is also announced and illustrated, and the action of odours and of ozone on radiant heat is made the subject of experiment.

The third paper* of the series to which I have referred was devoted to the examination of one particular vapour, which, on account of its universal diffusion, possesses an interest of its own —I mean of course the vapour of water. In this paper I considered all the objections which had been urged against my results up to the time when the paper was written; I replied to each of them by definite experiments, removing them one by one, and finally placing, as I believe, beyond the pale of reasonable doubt the action of the aqueous vapour of our atmosphere. In this third paper, moreover, the facts established by experiment are applied to the explanation of various atmospheric phenomena.

I have now the honour to lay before the Royal Society a fourth memoir, containing an account of further researches. Hitherto I have confined myself to experiments on radiation through gases and vapours which were introduced in succession into the same experimental tube, the heat being thus permitted to pass through the same thickness of different gases. A portion of the present inquiry is devoted to the examination of the transmission of radiant heat through different thicknesses of the same gaseous body. The brass tube with which my former experiments were conducted is composed of several pieces, which are screwed together when the tube is to be used as a whole; but the pieces may be dismounted and used separately, a series of lengths being thus attainable, varying from 2·8 inches to 49·4 inches. I wished, however, to operate upon gaseous strata much thinner than the thinnest of these; and for this purpose a special apparatus was devised, and with much time and trouble rendered at length practically effective.

The apparatus is sketched in fig. 1. C is the source of heat, which consists of a plate of copper against the back of which a steady sheet of flame is caused to play. The plate of copper forms one end of the chamber F (the "front chamber" of my

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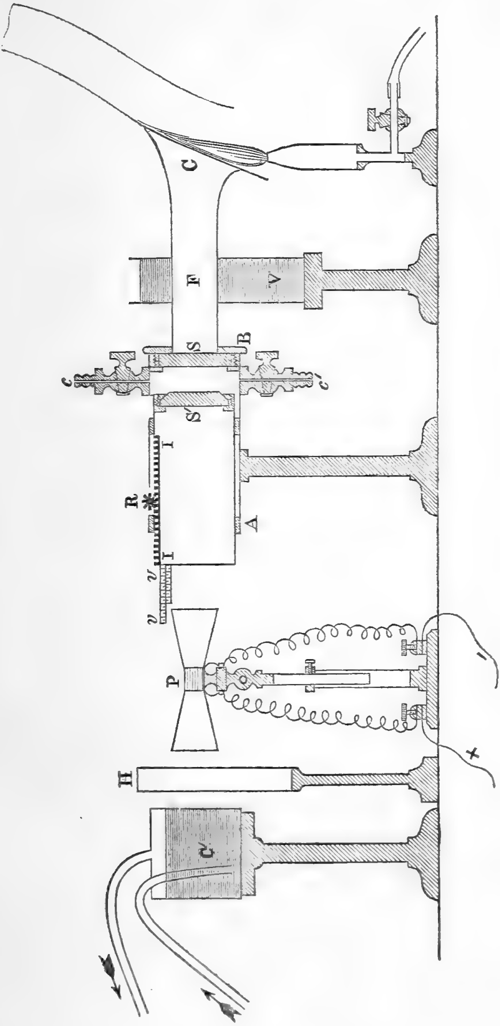
* Phil. Trans. December 1862; and Phil. Mag. July 1863.

former memoirs). This chamber, as in my previous investigations, passes through the vessel V, through which cold water continually circulates, entering at the bottom and escaping at the top. The heat is thus prevented from passing by conduction from the source C to the first plate of rock-salt S. This plate forms the end of the hollow cylinder A B, dividing it from the

Fig. 1.



front chamber F, with which the cylinder A B is connected by suitable screws and washers. Within the cylinder A B moves a second one, I I, as an air-tight piston, and the bottom of the second cylinder is stopped by the plate of rock-salt S′. This plate projects a little beyond the end of the cylinder, and thus can be brought into flat contact with the plate S. Fixed firmly to A B is a graduated strip of brass, while fixed to the piston is a second strip, the two strips forming a vernier, *v v*. By means of the pinion R, which works in a rack, the two plates of salt may be separated, their exact distance apart being given by the vernier. P is the thermo-electric pile with its two conical reflectors; C′ is the compensating cube, employed to neutralize the radiation from the source C. H is an adjusting screen, by the motion of which the neutralization may be rendered perfect, and the needle brought to zero under the influence of the two opposing radiations. The graduation of the vernier was so arranged as to permit of the employment of plates of gas varying from 0·01 to 2·8 inches in thickness. They were afterwards continued with the pieces of the experimental tube, already referred to, and in this way layers of gas were examined which varied in thickness in the ratio of 1 : 4900.

In my former experiments the chamber F was always kept exhausted, so that the rays of heat passed immediately from the source through a vacuum; but in the present instance I feared the strain upon the plate S, and I also feared the possible intrusion of a small quantity of the gas under examination into the front chamber F, if the latter were kept exhausted. Having established the fact that a length of 8 inches of dry air exerts no sensible action on the rays of heat, I had no scruple in filling the chamber F with dry air. Its absorption was *nil*, and it merely had the effect of lowering in an infinitesimal degree the temperature of the source. The two stopcocks *c* and *c′* stand exactly opposite the junction of the two plates of salt S, S′ when they are in contact, and when they are drawn apart these cocks are in communication with the space between the plates.

After many trials, the following mode of experiment was adopted:—The gas-holder containing the gas to be examined was connected by an india-rubber tube with the cock *c′*, the other cock *c* being at the same time left open. The piston was then moved by the screw R until the requisite distance between the plates was obtained. This space being filled with dry air, the radiations on the two faces of the pile were equalized, and the needle brought to zero. The gas-holder was now opened, and by gentle pressure the gas from the holder was forced first through a drying apparatus, and then into the space between the plates of salt. The air was quickly displaced, and a plate of the gas substituted for it. If the layer of gas possessed any sensible absorbing power, the equilibrium of the two sources of heat would be destroyed; the source C′ would triumph, and from the deflection due to its preponderance the exact amount of heat intercepted by the gas could be calculated.

When oxygen, hydrogen, or nitrogen was substituted for atmospheric air, no change in the position of the galvanometer-needle occurred; but when any one of the compound gases was allowed to occupy the space between the plates, a measurable deflection ensued. The plates of rock-salt were not so smooth, nor was their parallelism so perfect as entirely to exclude the gas when they were in contact. The contact was but partial, and hence a stratum of gas sufficient to effect a sensible absorption could find its way between the plates even when they touched each other. On this account the first thickness in the following Tables was really a little more than 0·01 of an inch. The first column in each contains the thickness of the gaseous layer, while the second column contains the absorption expressed in hundredths of the total radiation. The first layer of carbonic oxide, for example, absorbed 0·2, and the second layer 0·5 per cent. of the entire heat.

Table I.—Carbonic Oxide.

Thickness of gas.	Absorption in hundredths of the total radiation.
0·01 of an inch	0·2
0·02 „	0·5
0·03 „	0·7
0·04 „	0·9
0·06 „	1·4
0·1 „	1·6
0·3 „	3·0
0·4 of an inch	3·5
0·5 „	3·8
0·6 „	4·0
1·0 „	5·1
1·5 „	6·1
2·0 „	6·8

Table II.—Carbonic Acid.

0·01 of an inch	0·86
0·02 „	1·2
0·03 „	1·5
0·04 „	1·9
0·05 „	2·1
0·06 „	2·3
0·1 „	3·3
0·2 „	4·1
0·3 „	4·8
0·4 of an inch	5·3
0·5 „	5·7
0·6 „	5·9
0·7 „	6·0
0·8 „	6·1
0·9 „	6·2
1·0 „	6·3
1·5 „	7·0
2·0 „	7·6

TABLE III.—Nitrous Oxide.

Thickness of gas.	Absorption in hundredths of the total radiation.	Thickness of gas.	Absorption in hundredths of the total radiation.
0·01 of an inch	1·48	0·4 of an inch	10·20
0·02 ,,	2·33	0·5 ,,	11·00
0·03 ,,	3·80	0·6 ,,	11·70
0·04 ,,	4·00	0·8 ,,	12·17
0·05 ,,	4·20	1·0 ,,	12·80
0·1 ,,	6·00	1·5 ,,	14·20
0·2 ,,	7·77	2·0 ,,	15·7

TABLE IV.—Olefiant Gas.

0·01 of an inch	1·80	0·5 of an inch	23·30
0·02 ,,	3·08	1·0 ,,	26·33
0·05 ,,	5·37	2·0 ,,	32·80
0·1 ,,	9·14		

We here find that a layer of olefiant gas only 2 inches in thickness intercepts nearly 33 per cent. of the radiation from our source. Supposing our globe to be encircled by a shell of olefiant gas only 2 inches in thickness, this shell would offer a scarcely sensible obstacle to the passage of the solar rays earthward, but it would cut off at least 33 per cent. of the terrestrial radiation and in great part return it. Under such a canopy, trifling as it may appear, the surface of the earth would be kept at a stifling temperature. The possible influence of an atmospheric envelope on the temperature of a planet is here forcibly illustrated.

The only *vapour* which I have examined with the piston apparatus is that of sulphuric ether. Glass fragments were placed in a U tube and wetted with the ether. Through this tube dry air was gently forced, whence it passed, vapour-laden, into the space between the rock-salt plates S S′. The following Table contains the results.

TABLE V.—Air saturated with the Vapour of Sulphuric Ether.

Thickness of vapour.	Absorption in hundredths of the total radiation.	Thickness of vapour.	Absorption in hundredths of the total radiation.
0·05 of an inch	2·07	0·8 of an inch	21·0
0·1 ,,	4·6	1·5 ,,	34·6
0·2 ,,	8·7	2·0 ,,	35·1
0·4 ,,	14·3		

Comparing these results with those obtained with olefiant gas, we find for thicknesses of 0·05 of an inch and 2 inches respectively the following absorptions :—

Olefiant gas.		Sulphuric ether.	
Thickness of 0·05 . .	5·37	Thickness of 0·05 . .	2·07
Thickness of 2 inches .	32·80	Thickness of 2 inches.	35·1

Sulphuric ether vapour, therefore, commences with an absorption much lower than that of olefiant gas, and ends with a higher absorption. This is quite in accordance with the result established in my second memoir*, that in a short tube the absorption effected by the sparsely scattered molecules of a vapour may be less than that of a gas at a tension of an atmosphere, while in a long tube the gas may be exceeded by the vapour. The deportment of sulphuric ether indicates what mighty changes of climate might be brought about by the introduction into the earth's atmosphere of an almost infinitesimal amount of a powerful vapour. And if *aqueous vapour* can be shown to be thus powerful, the effect of its withdrawal from our atmosphere may be inferred.

§ 2. The experiments with the piston apparatus being completed, greater thicknesses of gas were obtained by means of the composite brass experimental tube already referred to. The arrangement adopted, however, was peculiar, being expressly intended to check the experiments, which were for the most part made by my assistants. The source of heat and the front chamber remained as usual, a plate of rock-salt dividing, as in my previous investigations, the front chamber from the experimental tube. The distant end of the tube was also stopped by a plate of salt; but instead of permitting the tube to remain continuous from beginning to end, it was divided, by a third plate of rock-salt, into two air-tight compartments. Thus the rays of heat from the source had to pass through *three* distinct chambers, and through three plates of salt. The first chamber was always kept filled with perfectly dry air, while either or both of the other chambers could be filled at pleasure with the gas or vapour to be examined. For the sake of convenience I will call the compartment of the tube nearest to the front chamber the *first* chamber, the compartment nearest to the pile the *second* chamber, the term 'front chamber' being, as before, restricted to that nearest to the source. The arrangement is sketched in outline in fig. 2.

The entire length of the tube was 49·4 inches, and this was maintained throughout the whole of the experiments. The only change consisted in the shifting of the plate of salt S′ which formed the partition between the first and second chambers. Commencing with a first chamber of 2·8 inches long, and a second chamber 46·4 inches long, the former was gradually augmented, and the latter equally diminished. The experiments were executed in the following manner:—The first and second chambers were thoroughly cleansed and exhausted, and the needle

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* Phil. Trans. part 1, 1862; and Phil. Mag. vol. xxiv. p. 343.

brought to zero by the equalization of the radiations falling upon the opposite faces of the pile. Into the first chamber the gas or

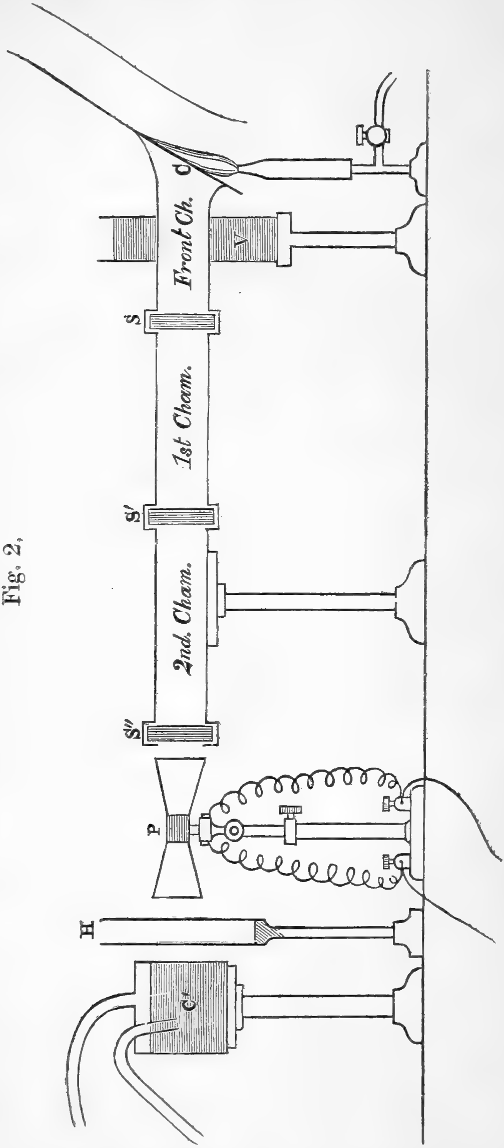

Fig. 2.

vapour to be examined was introduced, and its absorption determined. The first chamber was then cleansed, and the gas or vapour was introduced into the second chamber, its absorption there being also determined. Finally the absorption exerted by the two chambers acting together was determined, both of them being occupied by the gas or vapour.

The combination here described enabled me to check the experiments, and also to trace the influence of the first chamber on the quality of the radiation. In it the heat was more or less sifted, and it entered the second chamber deprived of certain constituents which it possessed on its entrance into the first. On this account the quantity absorbed in the second chamber when the first chamber is full of gas, must always be less than it would be if the rays had entered without first traversing the gas of the first chamber. From this it follows that the sum of the absorptions of the two chambers, taken separately, must always exceed the absorption of the tube taken as a whole. This may be briefly and conveniently expressed by saying that the sum of the absorptions exceeds the absorption of the sum.

TABLE VI.—Carbonic Oxide.

Length.		Absorption per 100.		
1st chamber.	2nd chamber.	1st chamber.	2nd chamber.	Both chambers.
2·8	46·6	6·8	12·9	12·9
8·0	41·4	9·6	12·2	12·9
12·2	37·2	10·7	12·2	12·9
15·4	34·0	10·9	12·2	13·4
17·8	31·6	11·1	12·0	13·3
36·3	13·1	12·6	10·3	13·4

TABLE VII.—Carbonic Acid.

2·8	46·6	8·6	13·8	13·3
8·0	41·4	9·9	12·7	13·0
12·2	37·2	11·0	11·4	13·0
15·4	34·0	11·8	12·1	13·9
23·8	25·6	11·7	11·4	13·1
23·8	25·6	11·2	11·2	12·6
23·8	25·6	10·4	10·5	12·0
36·3	13·1	11·6	10·0	12·3

Various causes have rendered these experiments exceedingly laborious. Could I have procured a sufficiently large quantity of gas in a single holder for an entire series of experiments it would not have been difficult to obtain concurrent results, but the slight variations in quality of the same gas generated at different times tell upon the results and render perfect uniformity extremely difficult to obtain. The approximate constancy of the numbers in the third column is, however, a guarantee that the determinations are not very wide of the truth. Irregularities, however, are revealed. Some remarkable ones occur in the case of carbonic acid, with the chambers 23·8 and 25·6,—the absorptions in the first chamber varying in this instance from 11·7 to 10·4, and in the second chamber from 11·4 to 10·5, and in both chambers from 13·1 to 12·0. The gas which gave the largest of these results was generated from marble and hydrochloric acid; the next was obtained from chalk and sulphuric acid, and the gas which gave the smallest result was obtained from bicarbonate of soda and sulphuric acid. The slight differences accompanying these different modes of generation made themselves felt in the manner recorded in the Table.

Table VIII.—Nitrous Oxide.

Length.		Absorption per 100.		
1st chamber.	2nd chamber.	1st chamber.	2nd chamber.	Both chambers.
2·8	46·6	16·1	32·9	33·9
12·2	37·2	23·1	30·0	32·0
15·4	34·0	23·6	29·6	32·0
17·8	31·6	26·2	29·6	32·7

The differences arising from different modes of generation are most strikingly illustrated by the powerful gases. My friend Dr. Frankland, for example, was kind enough to superintend for me the formation of a large holder of olefiant gas by the so-called "continuous process," in which the *vapour* of alcohol is led through sulphuric acid diluted with its own volume of water; the following results were obtained:—

Table IX.—Olefiant Gas.

Length.		Absorption per 100.		
1st chamber.	2nd chamber.	1st chamber.	2nd chamber.	Both chambers.
2·8	46·6	34·6	66·1	67·7
8·0	41·4	44·2	65·3	67·5
15·4	34·0	53·6	62·3	67·0

Considering the difficulty of the experiments, the agreement of the absorption of both chambers, the sum of which was the constant quantity of 49·4 inches, must be regarded as satisfactory. This is the general character of the results as long as we adhere to the same gas. Olefiant gas generated by mixing the *liquid* alcohol with sulphuric acid and applying heat to the mixture, gave the results recorded in the following Table:—

Table X.—Olefiant Gas.

Length.		Absorption per 100.		
1st chamber.	2nd chamber.	1st chamber.	2nd chamber.	Both chambers.
12·2	37·2	54·8	70·0	76·3
15·4	34·0	59·1	72·7	77·1
19·8	29·6	67·8	70·4	77·0
23·8	25·6	69·2	70·2	77·6
36·3	13·1	72·8	60·3	78·8

The absorptions of both chambers in this Table are almost exactly 10 per cent. higher than those found with the gas generated under Dr. Frankland's superintendence.

A few remarks on these results may be introduced here. In the case of carbonic oxide (Table VI.), we see that while a length of 2·8 inches of gas is competent, when acting alone, to intercept 6·8 per cent. of the radiant heat, the cutting off of this length from a tube 49·4 inches long, or, what is the same, the addition of this length to a tube 46·6 inches long, makes no sensible change in its absorption. The second chamber absorbs as much as both. The same remark applies to carbonic acid, and it is also true within the limits of error for nitrous oxide and olefiant gas. Indeed it is only when 8 inches or more of the column have been cut away that the difference begins to make itself felt. Thus, in carbonic oxide, the absorption of a length of 41·4 being 12·2, that of a chamber 49·4, or 8 inches longer, is only 12·9, making a difference of only 0·7 per cent., while the same 8 inches acting singly on the gas produces an absorption of 9·6 per cent. So also with regard to carbonic acid; a tube 41·4 absorbing 12·7 per cent., a tube 49·4 absorbs only 13·0 per cent., making a difference of only 0·3 per cent. As regards olefiant gas (Table IX.), while a distance of 8 inches acting singly effects an absorption of 44 per cent., the addition of 8 inches to a tube already 41·4 inches in length raises the absorption only from 65·3 to 67·5, or 2·2 per cent. The reason is plain. In a length of 41·4 the rays capable of being absorbed by the gas are so much diminished, so few in fact remain to be attacked, that an additional 8 inches of gas produces a scarcely sensible effect. Similar considerations explain the fact that, while by augmenting the length of the first chamber from 2·8 inches to 15·4 inches we increase the absorption of olefiant gas nearly 20 per cent., the shortening of the second chamber by precisely the same amount effects a diminution of barely 4 per cent. of the absorption. All these results conspire to prove the heterogeneous character of the radiation from a source heated to about 250° C.

The sum of the absorptions placed side by side with the absorption of the sum exhibits the influence of sifting in an instructive manner. Tables VI., VII., VIII., IX., and X., thus treated, give the following comparative numbers:—

TABLE XI.—Carbonic Oxide.

Length of chambers.		Sum of absorptions.	Absorption of sum.
2·8	46·6	19·7	12·9
8·0	41·1	21·8	12·9
12·2	37·2	22·9	12·9
15·4	34·0	23·1	13·4
17·8	31·6	23·1	13·3
36·3	13·1	22·9	13·4
	Means	22·3	13·1

TABLE XII.—Carbonic Acid.

2·8	46·6	22·4	13·3
8·0	41·4	22·6	13·0
12·2	37·2	22·4	13·0
15·4	34·0	23·9	13·9
23·8	25·6	23·1	13·1
36·3	13·1	21·6	12·3
	Means	22·6	13·1

TABLE XIII.—Nitrous Oxide.

2·8	46·6	49·0	33·9
12·2	37·2	53·1	32·0
15·4	34·0	53·2	32·0
17·8	31·6	55·8	32·7
	Means	52·8	32·7

TABLE XIV.—Olefiant Gas.

2·8	46·6	100·7	67·7
8·0	41·4	109·5	67·5
12·2	37·2	109·4	65·0
15·4	34·0	115·9	67·0
	Means	108·9	66·8

TABLE XV.—Olefiant Gas.

12·2	37·2	124·8	76·3
15·4	34·0	131·8	77·1
19·8	29·6	138·2	77·0
23·8	25·6	139·4	77·6
36·3	13·1	133·1	78·8
	Means	133·4	77·3

The conclusion that the sum of the absorptions is greater than the absorption of the sum is here amply verified. The Tables also show that the ratio of the sum of the absorptions to the absorption of the sum is practically constant for all the gases. Dividing the first mean by the second in the respective cases, we have the following quotients:—

Carbonic oxide	1·70
Carbonic acid	1·72
Nitrous oxide	1·61
Olefiant gas (mean of both) . .	1·68

The sum of the absorptions ought to be a maximum when the two chambers are of equal length. Supposing them to be unequal, one being in excess of half the length of the tube, let us consider the action of this excess singly. Placed after the half-length, it receives the rays which have already traversed that half; placed after the shorter length, it receives the rays which have traversed the shorter length. In the former case, therefore, the excess will absorb less than in the latter, because the rays in the former case have been more thoroughly sifted before the heat reaches the excess. From this it is clear that, as regards absorption, more is gained by attaching the excess to the short length of the tube than to the half-length; in other words, the sum of the absorptions, when the tube is divided into two equal parts, is a maximum. This reasoning is approximately verified by the experiments. Supposing, moreover, one of the lengths constantly to diminish, we thus constantly approach the limit when the sum of the absorptions and the absorption of the sum are equal to each other, the former being then a minimum. The effect of proximity to this limit is exhibited in the first experiment in each of the series; here the lengths of the compartments are very unequal, and the sum of the absorptions is, in general, a minimum.

After the absorption by the permanent gases had been in this way examined, I passed on to the examination of vapours. They were all used at a common pressure of 0·5 of an inch of mercury, or about $\frac{1}{60}$th of an atmosphere. The liquid which yielded the vapour was enclosed in the flasks described in my previous memoirs, and the pure vapour was allowed to enter the respective compartments of the experimental tube without the slightest ebullition. The following series of Tables contains the results thus obtained.

Table XVI.—Bisulphide of Carbon. Pressure 0·5 of an inch.

Length.		Absorption per 100.		
1st chamber.	2nd chamber.	1st chamber.	2nd chamber.	Both chambers.
2·8	46·6	3·6	7·6	7·6
8·0	41·4	4·4	7·3	7·6
15·4	34·0	5·7	6·0	7·5
17·8	31·6	5·8	6·4	7·5
23·8	25·6	6·7	6·0	7·8

Table XVII.—Chloroform. Pressure 0·5 of an inch.

2·8	46·6	5·5	15·9	16·3
8·0	41·4	9·2	15·6	16·8
12·2	37·2	10·5	14·8	17·1
15·4	34·0	11·6	14·1	16·9
23·8	25·6	15·0	14·0	18·4
36·3	13·1	15·6	10·9	17·2

Table XVIII.—Benzole. Pressure 0·5 of an inch.

2·8	46·6	4·0	20·0	20·6
8·0	41·4	8·4	17·3	20·4
12·2	37·2	9·8	16·5	19·0
17·8	31·6	11·9	15·7	20·1
23·8	25·6	14·3	15·1	21·0

Table XIX.—Iodide of Ethyle. Pressure 0·5 of an inch.

2·8	46·6	7·1	23·5	25·4
8·0	41·4	9·1	21·1	23·3
12·2	37·2	12·8	20·5	25·2
15·4	34·0	14·6	20·8	25·2
17·8	31·6	15·8	20·0	25·5

Table XX.—Alcohol. Pressure 0·5 of an inch.

2·8	46·6	11·7	46·1	46·1
8·0	41·4	18·5	43·6	47·0
12·2	37·2	26·0	44·1	47·5
15·4	34·0	32·1	41·1	47·0
17·8	31·6	32·4	40·0	47·6

Table XXI.—Alcohol. Pressure 0·1 of an inch.

8·0	41·4	8·0	22·2	24·9
15·4	34·0	12·1	20·0	24·7
17·8	31·6	13·1	19·7	25·7
23·8	25·6	14·8	18·4	25·2
36·3	13·1	19·1	13·8	25·1

Table XXII.—Sulphuric Ether. Pressure 0·5 of an inch.

Length.		Absorption per 100.		
1st chamber.	2nd chamber.	1st chamber.	2nd chamber.	Both chambers.
2·8	46·6	14·8	50·0	51·6
8·0	41·4	23·9	51·0	53·9
12·2	37·2	30·9	48·8	53·6
15·4	34·0	34·0	47·8	53·1

Table XXIII.—Acetic Ether. Pressure 0·5 of an inch.

2·8	46·6	17·0	60·2	62·9
8·0	41·4	30·7	58·1	64·6
12·2	37·2	41·6	55·1	64·2
15·4	34·0	44·4	55·5	62·4
23·8	25·6	50·9	52·7	64·7
36·3	13·1	58·1	42·6	64·8

Table XXIV.—Formic Ether. Pressure 0·5 of an inch.

2·8	46·6	17·4	63·0	64·4
8·0	41·4	33·3	59·1	63·4
17·8	31·6	40·0	48·4	60·3
23·8	25·6	45·6	47·2	60·2

I have already compared the sum of the absorptions for gases with the absorption of the sum; in the following Tables the same comparison is made for the vapours.

Table XXV. Bisulphide of Carbon, 0·5 inch.

Length of chambers.		Sum of absorptions.	Absorption of sum.
2·8	46·6	11·2	7·6
8·0	41·4	11·7	7·6
15·4	34·0	11·7	7·5
17·8	31·6	12·2	7·5
23·8	25·6	12·7	7·8
	Means	11·9	7·6

Table XXVI.—Chloroform, 0·5 inch.

2·8	46·6	21·4	16·3
8·0	41·4	24·8	16·8
12·2	37·2	25·3	17·1
15·4	34·0	25·2	16·9
23·8	25·6	29·0	18·4
36·3	13·1	26·5	17·2
	Means	25·36	17·1

Table XXVII.—Benzole, 0·5 inch.

Length of chambers.		Sum of absorptions.	Absorption of sum.
2·8	46·6	24·0	20·6
8·0	41·4	25·7	20·4
12·2	37·2	26·3	19·0
17·8	31·6	27·6	20·1
23·8	25·6	29·4	21·0
	Means	26·6	20·2

Table XXVIII.—Iodide of Ethyle, 0·5 inch.

2·8	46·6	30·6	25·4
8·0	41·4	30·2	23·3
12·2	37·2	33·3	25·2
15·4	34·0	35·4	25·2
17·8	31·6	35·8	25·2
	Means	33·1	24·9

Table XXIX.—Alcohol, 0·5 inch.

2·8	46·6	57·8	46·1
8·0	41·4	62·1	47·0
12·2	37·2	70·1	47·5
15·4	34·0	73·2	47·0
17·8	31·6	72·4	47·6
	Means	67·1	47·0

Table XXX.—Alcohol, 0·1 inch.

8·0	41·4	30·2	24·9
15·4	34·0	32·1	24·7
17·8	31·6	32·8	25·7
23·8	25·6	33·2	25·2
36·3	13·1	32·9	25·1
	Means	32·2	25·1

Table XXXI.—Sulphuric Ether, 0·5 inch.

2·8	46·6	64·8	51·6
8·0	41·4	74·9	53·9
12·2	37·2	79·7	53·6
15·4	34·0	81·8	53·1
	Means	75·3	53·05

TABLE XXXII.—Formic Ether, 0·5 inch.

Length of chambers.			Sum of absorptions.	Absorption of sum.
2·8	46·6		80·4	64·4
8·0	41·4		82·4	63·4
17·8	31·6		88·4	60·3
23·8	25·6		92·8	60·2
		Means	86·0	62·07

TABLE XXXIII.—Acetic Ether, 0·5 inch.

2·8	46·6		77·2	62·9
8·0	41·4		88·8	64·6
12·2	37·2		96·7	64·2
15·4	34·0		99·9	62·4
23·8	25·6		103·6	64·7
36·3	13·1		100·7	64·8
		Means	94·5	63·9

An inspection of the foregoing Tables discloses the fact that, in the case of vapours, the difference between the sum of the absorptions and the absorption of the sum is, in general, less than in the case of gases. This resolves itself into the proposition that for equal lengths, within the limits of these experiments, the sifting power of the gas is greater than that of the vapour. The reason of this is that the vapours are examined in a state of tenuity which is only $\frac{1}{60}$th of that possessed by the gases. Thus, no matter how powerful the individual molecules may be, their distance asunder renders a thin layer of them a comparatively open screen.

§ 3. The entrance of a gas into an exhausted vessel is accompanied by the generation of heat; and the gas thus warmed, if a radiator, will emit the heat generated. Conversely, on exhausting a vessel containing any gas, the gas is chilled, and thus an external body, which prior to the act of exhaustion possessed the same temperature as the gas within the vessel, becomes, on the first stroke of the pump, a warm body with reference to the gas remaining in the vessel; and if the external body be separated from the cooled gas by a diathermic partition, it will radiate into the gas and become chilled by this radiation. It was shown in my second memoir* that this mode of warming and of chilling a gas or vapour furnished a practical means of determining, without any source of heat external to the gaseous body itself, both its radiative and absorptive energy. For the sake of con-

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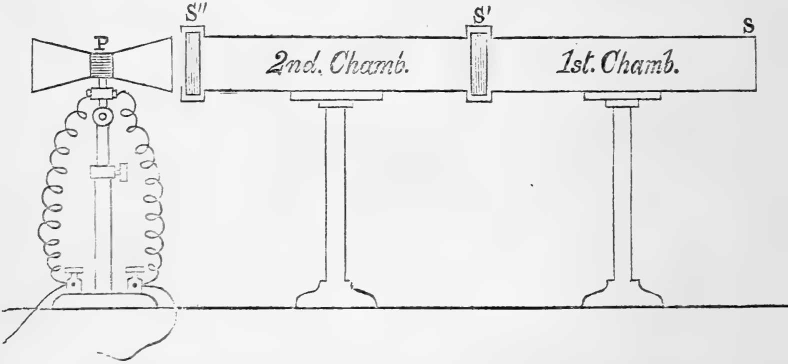
* Phil. Trans. part 1, 1862; and Phil. Mag. vol. xxiv. p. 337.

venience I have called the radiation and absorption of a gas or vapour thus dynamically heated and cooled, dynamic radiation and dynamic absorption.

In illustration of the manner in which dynamic radiation may be applied in researches on radiant heat, I have had made, during the last half-year, a considerable number of experiments, some of which I will here describe. In the first place, the experimental tube was divided into two compartments, as in the experiments described in the foregoing section. The source of heat was abolished, and one end of the experimental tube was stopped by a plate of polished metal; the other end was stopped by a transparent plate of rock-salt, while the space between the ends was divided into two compartments by a second plate of rock-salt. The thermo-electric pile occupied its usual position at the end of the tube, the compensating cube, however, being abandoned. For the sake of convenient reference, I will call the compartment of the tube most distant from the pile the first chamber, and that adjacent to the pile the second chamber. An outline sketch of the arrangement is given in fig. 3.

Fig. 3.



The experiments were conducted in the following manner:—Both compartments being exhausted and the needle at zero, the gas was allowed to enter the first chamber through a gauge-cock which made its time of entry 40 seconds. The second chamber was preserved a vacuum; the gas on entering the first chamber was dynamically heated, and radiated its heat to the pile through the vacuous second chamber; the needle moved, and the limit of its excursion was noted. The first chamber was then exhausted and carefully cleansed with dry air. The second chamber was filled with the same gas, not with a view to determine its dynamic radiation, but to examine its effect upon the heat radiated from the first chamber. The needle being at zero, the gas was again permitted to enter the first chamber exactly as in the first experiment,—the only difference between the two experiments being, that in the first the heat passed through a vacuum to the pile, while in the second it had to pass through a column of the same kind of gas as that from which it emanated. In this way the absorption exerted by any gas upon heat radiated from the same gas, or from any other gas, may be accurately determined. Finally, the apparatus being cleansed and the needle at zero, the gas was permitted to enter the second chamber, and its dynamic radiation from this chamber was determined. The intermediate plate of salt S′ was shifted, as in the former experiments, so as to alter the lengths of the two chambers, but the sum of both lengths remained constant as before.

In the following Tables the three columns bracketed under the head of "Deflection," contain the arcs through which the needle moved in the three cases mentioned; (1) when the radiation from the gas in the first chamber passed through the empty second chamber; (2) when the radiation from the first chamber passed through the occupied second chamber; and (3) when the radiation proceeded from the second chamber.

Dynamic Radiation of Gases.

TABLE XXXIV.—Carbonic Oxide.

Length.		Deflection.		
1st chamber.	2nd chamber.	By 1st chamber, 2nd chamber empty.	By 1st chamber, gas in 2nd chamber.	By 2nd chamber.
2·8	46·6	1°·0	0°·0	28°·0
15·4	34·0	3·8	2·1	24·4
36·3	13·1	13·7	6·3	16·6

TABLE XXXV.—Carbonic Acid.

2·8	46·6	1·0	0·0	33·6
15·4	34·0	3·7	1·25	23·3
36·3	13·1	16·8	6·6	17·5

TABLE XXXVI.—Nitrous Oxide.

2·8	46·6	1·0	0·2	44·5
15·4	34·0	4·3	1·2	31·7
36·3	13·1	19·5	6·2	22·0

TABLE XXXVII.—Olefiant Gas.

15·4	34·0	11·9	1·0	68·0
23·8	25·6	22·8	3·0	
36·3	13·1	59·0	10·4	65·0

The gases, it will be observed, exhibit a gradually increasing power of dynamic radiation from carbonic oxide up to olefiant gas. This is most clearly illustrated by reference to the results obtained in the respective cases with the first length of the second chamber. They are as follows:—

	°
Carbonic oxide	28·0
Carbonic acid	33·6
Nitrous oxide	44·5
Olefiant gas	68·0

Its proximity to the pile, and the fact of its having to cross but one plate of salt, makes the action of the second chamber much greater than that of the first.

Each of the Tables exhibits the fact that as the length of the chamber increases the dynamic radiation of the gas contained in it increases, and as the length diminishes the radiation diminishes. We also see how powerfully the gas in the second chamber acts upon the radiation from the first. With carbonic oxide, the presence of the gas in the second chamber reduces the deflection from 13°·7 to 6°·3; with carbonic acid it is reduced from 16·8 to 6·6; with nitrous oxide it is reduced from 19·5 to 6·2. Now this residual deflection, 6°·2, is not entirely due to the transparency of the gas, to heat emitted *by the gas*. No matter how well polished the experimental tube may be, there is always a certain radiation from its interior surface when the gas enters it. With perfectly dry air this radiation amounts to 8 or 9 degrees. Thus the radiation is composite, in part emanating from the molecules in the first chamber, and in part emanating from the surface of the tube. To these latter, the gas in the second chamber would be much more permeable than to the former; and to these latter, I believe, the residual deflection of 6 degrees, or thereabouts, is mainly due. That this number turns up so often, although the radiations from the various gases differ considerably, is in harmony with the supposition just made. In the case of carbonic oxide, for example, the deflection is reduced from 13°·7 to 6°·3, while in the case of nitrous oxide it is reduced from 19°·5 to 6°·2; in the case of olefiant gas it is reduced from 59° to 10°·4, while in other experiments (not here recorded) the deflection by olefiant gas was reduced from 44° to 6°.

As may be expected, this radiation from the interior surface augments with the tarnish of the surface, but the extent to which it may be increased is hardly sufficiently known. Indeed the gravest errors are possible in experiments of this nature if the influence of the interior be overlooked or misunderstood. An experiment or two will illustrate this more forcibly than any words of mine.

A brass tube 3 feet long, and very slightly tarnished within, was used for dynamic radiation. Dry air on entering the tube produced a deflection of 12 degrees. The tube was then polished within, and the experiment repeated; the action of dry air was instantly reduced to 7·5 degrees.

The rock-salt plate at the end of the tube was then removed, and a lining of black paper 2 feet long was introduced within it. The tube was again closed, and the experiment of allowing dry air to enter it repeated. The deflections observed in three successive experiments were

80°, 81°, 80°.

This result might be obtained as long as the lining continued within the tube.

The plate of rock-salt was again removed, and the length of the lining was reduced to a foot; the dynamic radiation on the entrance of dry air in three successive experiments gave the deflections

76°, 74°, 75°.

The plate was again removed and the lining reduced to 3 inches; the deflections obtained in two successive experiments were

66°, 65°.

Finally the lining was reduced to a ring only 1½ inch in width; the dynamic radiation from this small surface gave in two successive trials the deflections

56°, 56°·5.

The lining was then entirely removed, and the deflection instantly fell to

7°·5.

A coating of lampblack within the tube produced the same effect as the paper lining; common writing-paper was almost equally effective; a coating of varnish also produced large deflections, and the mere oxidation of the interior surface of the tube is also very effective.

In the above experiments the lining was first heated, and it then radiated its heat through a thick plate of rock-salt against the pile. The effect of the heat was enfeebled by distance, by reflexion from the surfaces of the salt, and by partial absorption. Still we see that the radiation thus weakened was competent to drive the needle almost through the quadrant of a circle. If, instead of being thus separated from the lining, *the face of the pile itself* had formed part of the interior surface of the tube, receiving there the direct impact of the particles of air, of course the deflections would be far greater than the highest of those above recorded. Indeed I do not doubt my ability to cause the needle of my galvanometer to whirl, by the dynamic heating of the surface of my pile, through an arc of 1000 degrees. Assuredly an arrangement subject to disturbances of this character cannot be suitable in experiments in which the greatest delicacy is necessary.

Experiments on dynamic radiation, similar to those executed with gases, were made with vapours. The tube was divided into two compartments as before. Both compartments being exhausted, vapour was permitted to enter the first chamber. Dry air was afterwards permitted to enter the same chamber; the air was heated, it warmed the vapour, and the vapour radiated its heat against the pile. The heat passed in the first experiment through a vacuous second chamber, and in the second experiment through the same chamber when it contained 0·5 of an inch of the same vapour as that from which the rays issued. A third experiment was made to determine the dynamic radiation from the second chamber. The following Tables contain the results:—

Dynamic Radiation of Vapours.

Table XXXVIII.—Bisulphide of Carbon, 0·5 inch.

Length.		Deflection.		
1st chamber.	2nd chamber.	By 1st chamber, 2nd chamber empty.	By 1st chamber. Vapour in 2nd chamber.	By 2nd chamber.
15·4	34·0	2°·4	1°·6	14°·2
36·3	13·1	9·75	5·5	9·0

Table XXXIX.—Benzole, 0·5 inch.

15·4	34·0	3·0	1·1	34·0
36·3	13·1	21·6	11·9	15·1

Table XL.—Iodide of Ethyle, 0·5 inch.

15·4	34·0	3·4	2·7	38·8
36·3	13·1	25·4	13·8	19·0

Table XLI.—Chloroform, 0·5 inch.

15·4	34·0	4·5	2·1	41·0
36·3	13·1	22·3	10·0	19·0

Table XLII.—Alcohol, 0·5 inch.

15·4	34·0	4·9	2·0	53·8
36·3	13·1	33·8	16·9	34·9

TABLE XLIII.—Alcohol, 0·1 inch.

Length.		Deflection.		
1st chamber.	2nd chamber.	By 1st chamber, 2nd chamber empty.	By 1st chamber. vapour in 2nd chamber.	By 2nd chamber.
15·4	34·0	2°·0	1°·3	35°·7
36·3	13·1	21·8	16·2	11·5

TABLE XLIV.—Boracic Ether, 0·1 inch.

15·4	34·0	6·3	2·1	61·0
36·3	13·1	29·1	15·7	31·6

TABLE XLV.—Formic Ether, 0·5 inch.

15·4	34·0	6·3	2·5	68·0
36·3	13·1	46·0	23·8	41·0

TABLE XLVI.—Sulphuric Ether, 0·5 inch.

15·4	34·0	5·6	2·5	68·0
36·3	13·1	45·3	22·4	36·5

TABLE XLVII.—Acetic Ether, 0·5 inch.

15·4	34·0	5·7	1·0	73·9
36·3	13·1	49·1	22·0	41·0

Collecting the radiations from the second chamber for the lengths 34 inches and 13·1 inches together in a single Table, we see at a glance how the radiation is affected by varying the length.

TABLE XLVIII.

	Dynamic radiation of various vapours at 0·5 inch pressure and a common thickness of	
	34 inches.	13·1 inches.
Bisulphide of carbon	14°·2	9°·0
Benzole	34·0	15·1
Iodide of ethyle	38·8	19·0
Chloroform	41·0	19·0
Alcohol	53·8	34·9
Sulphuric ether	68·0	36·5
Formic ether	68·0	41·0
Acetic ether	73·9	41·0
	At a pressure of 0·1 of an inch.	
Alcohol	35·7	11·5
Boracic ether	61·0	31·6

The extraordinary energy of boracic ether as a radiant may be inferred from the last experiment. Although attenuated to $\frac{1}{300}$th of an atmosphere, its thinly scattered molecules are able to urge the needle through an arc of 61 degrees, and this merely by the warmth generated on the entrance of dry air into a vacuum.

Arranging the gases in the same manner, we have the following results :—

TABLE XLIX.

	Dynamic radiation of gases at 1 atm. pressure and a common thickness of	
	34 inches.	13·1 inches.
Carbonic oxide	24°·4	16°·6
Carbonic acid	23·3	17·5
Nitrous oxide	31·7	22·0
Olefiant gas	68·0	65·0

The influence of tenuity which renders the vapour at 0·5 of an inch a more open screen than the gas at 30 inches is here exhibited. In the case of the vapour, a greater length is available for radiation than in the case of the gas, because the radiation from the hinder portion of the column of vapour is less interfered with by the molecules in front of it than is the case with the gas. By shortening the column we therefore do more injury to the vapour than to the gas; by lengthening it we promote the radiation from the vapour more than that from the gas. Thus while a shortening of the gaseous column from 34 inches to 13·1 causes a fall in the case of carbonic oxide only from 23°·3 to 17°·5, the same amount of shortening causes benzole vapour to fall from 34° to 15°·1,—a much greater diminution. So also as regards olefiant gas, a shortening of the radiating column from 34 inches to 13·1 inches causes a fall in the deflection only from 68° to 65°; the same diminution produces with sulphuric ether a fall from 68° to 36°·5; and with acetic ether from 73°·9 to 41°. In the long column acetic ether vapour beats olefiant gas, but in the short column the gas beats the vapour.

One of the earliest series of experiments of this nature which were executed last autumn, though not free from irregularities, is nevertheless worth recording. The experiments were made with a brass tube, slightly tarnished within, the tube being 49·4 inches long, and divided into two equal compartments, each 24·7 inches in length, by a partition of rock-salt placed at the centre of the tube.

TABLE L.—Dynamic radiation of Vapours.

	Deflection.		
	By 1st chamber, 2nd chamber empty.	By 1st chamber, vapour in 2nd chamber.	By 2nd chamber.
	°	°	°
Bisulphide of carbon	8·2	5·8	21·2
Benzole	20·0	12·4	45·9
Chloroform	24·3	10·9	55·2
Iodide of ethyle	27·5	14·7	55·3
Alcohol	42·7	22·3	69·0
Sulphuric ether	46·3	21·7	80·5
Formic ether	47·5	19·8	79·5
Propionate of ethyle	49·8	25·0	82·3
Acetic ether	53·3	30·0	82·1

To ascertain whether the absorption by the vapours bears any significant relation to the absorption by the liquids from which these vapours were derived, the transmission of radiant heat through those liquids was examined. The open flame of an oil-lamp was used, and the liquids were enclosed in rock-salt cells. Thus the total radiation from the lamp, with the exception of the minute fraction absorbed by the rock-salt, was brought to bear upon the liquid.

In the following Table the liquids are arranged in the order of their powers of transmission.

TABLE LI.

Name of liquid.	Transmission in hundredths of the radiation.
Bisulphide of carbon	83
Bisulphide of carbon saturated with sulphur	82
Bisulphide of carbon saturated with iodine	81
Bromine	77
Chloroform	73
Iodide of methyle	69
Benzole	60
Iodide of ethyle	57
Amylene	50
Sulphuric ether	41
Acetic ether	34
Formic ether	33
Alcohol	30
Water saturated with rock-salt	26

These results are but approximate, but they are not very far from the truth; and it is impossible to regard them without feeling how purely the act of absorption is a *molecular* act, and that when a liquid is a powerful absorber the vapour of that liquid is sure also to be a powerful absorber.

To experiment with water, it was necessary to saturate it with the salt of which the cell was formed, but the absorptive energy is due solely to the water. We might infer from this alone, were no experiments made on the aqueous vapour of the atmosphere, that that vapour must exert a powerful action upon terrestrial radiation. In fact, in all the statements that I have hitherto made I have underrated its action.

The deportment of the elements sulphur and iodine, dissolved in bisulphide of carbon, is in striking harmony with all that we have hitherto discovered regarding the action of elementary bodies. The saturation of the bisulphide by sulphur scarcely affects the transmission, while a quantity of iodine sufficient to convert the liquid from one of perfect transparency to one of almost perfect opacity to light, produces a diminution of only two per cent. of the radiation. This shows that the heat really used in these experiments consists almost wholly of the obscure rays of the lamp. It is worth remarking that the obscure rays of a luminous source have a much greater power of penetration in the case of the liquids here examined than the rays from an obscure source, however close to incandescence. The deportment of bromine is also very instructive. The liquid is very dense, and so opake as to cut off the luminous rays of the lamp; still it transmits 77 per cent. of the total radiation. It stands in point of diathermancy above every compound liquid in the list, except bisulphide of carbon. This latter substance is the rock-salt of liquids.

Before a strict comparison can be made between vapours and liquids, they must be examined by heat of the same quality, and I have already made arrangements with which I hope to obtain more complete and accurate results than those above recorded.

XI. *On the relative Amounts of Heat produced by the Chemical Combination of Ordinary and Ozonized Oxygen.* *By* THOMAS WOODS, *M.D.*[*]

THE difference between the physical conditions of ordinary oxygen and oxygen in its more active state, or ozone, does not seem to be known. Clausius considers the atoms of the former to be grouped in a binary arrangement, and those of the latter to be either isolated, or so joined to the molecules of ordi-

[*] Communicated by the Author.

nary oxygen as not to occupy any volume of their own; whilst Von Babo and others think an exactly opposite disposition of the atoms is the true one.

The interest that attaches itself to ozone, both from its peculiar actions and the extraordinary constitution it must possess, as shown by the researches of Andrews and Tait, make it most desirable that more should be known about it. I have endeavoured to do something towards fixing a knowledge of its constitution. I have tried to find which opinion, Clausius's or Von Babo's, is the correct one; that is, whether ozone is composed of combined or isolated atoms.

The manner in which I have endeavoured to solve the problem is founded on the principle I proved in this Magazine in October 1851, and now universally admitted, viz. that the decomposition of compounds absorbs heat. I thought that, supposing ozone to differ from oxygen in the arrangement of its atoms, less heat should be produced by it than by oxygen from union with another body if the atoms were in a combined state, because a certain amount of the heat of combination would be absorbed by the decomposition or disuniting of the compound molecule,—and that an opposite result might be expected from a different arrangement. I therefore determined to cause both ordinary and active oxygen to unite with a like body under similar circumstances, to find which, if either, produced the greater amount of heat. I accordingly mixed a measured quantity of ozonized air over water with a known volume of deutoxide of nitrogen; and with the same quantity of ordinary air I mixed a like volume of the deutoxide as in the first experiment, and marked the rise of temperature with a thermometer in both instances. I took every possible precaution to ensure uniformity of circumstance in the two series of trials, and I repeated them frequently, but I found that no difference in the rise of temperature produced was apparent. The increase of temperature in both amounted to 9° F.

The ozonized air was obtained by putting a mixture of sulphuric acid and permanganate of potash, according to Böttger's plan, under an inverted funnel, the narrow end of which entered a bottle, so as to keep any ozonized air that was produced. After twelve hours the starch- and iodide-of-potassium papers were turned blue. I made ozone also by leaving phosphorus partly covered with water in a large bottle. I could not, however, compare the air thus ozonized with common air, because the former contained a variable quantity of oxygen. After twenty-four hours' partial immersion of the phosphorus in water nearly all the oxygen was removed, having formed phosphoric acid; the quantity that remained was not known, and therefore could not be compared with ordinary air.

I also compared ozonized *oxygen* (not mixed with nitrogen as in the air) with ordinary oxygen as to its power of producing heat by combining with deutoxide of nitrogen. I mixed, as in the previous experiments, a measured volume of each with a similar quantity of the deutoxide over water, but no difference in amount of increase of temperature resulted. The ozonized oxygen was obtained by transmitting a current from a Daniell's battery (ten in series) through water acidulated with sulphuric and chromic acids.

It would only be occupying space unnecessarily, to give more fully the details of the experiments, as the results were entirely negative, *no difference whatever in the amount of heat produced by the combination in equal volumes of plain and ozonized oxygen was found.* If, therefore, any arrangement of particles exist in one oxygen which does not in the other, these different states of aggregation do not at all events require any force to transmute them.

Parsonstown, July 1864.

XII. *On a Physical Analysis of the Human Breath. By* W. F. Barrett, *Assistant in the Physical Laboratory of the Royal Institution*[*].

In a memoir by Professor Tyndall, read before the Royal Society on the 17th of March 1864, it was shown that when a carbonic oxide flame is used as a source of heat, the absorption by carbonic acid is extremely large, even exceeding that by olefiant gas, which with other sources is by far the most powerful absorbent. Thus, of the radiation from heated lampblack, olefiant gas absorbs six times as much as carbonic acid; whilst of the heat emitted by a carbonic oxide flame, the absorption by carbonic acid at small tensions is more than twice that effected by olefiant gas at the same pressure.

This very large absorption of heat by carbonic acid, suggested to me the possibility of this method of experiment being applied to the determination of that gas in certain cases where it existed in small quantities. By the desire and under the direction of Professor Tyndall I have made, with this view, several experiments upon air and upon the human breath. As Professor Tyndall is now in Switzerland, I publish the results at his request, but without his supervision, and upon my own responsibility.

The apparatus used in this investigation consisted of the same instruments (with the exception of the source of heat and "front

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[*] Communicated by the Author.

chamber"), and was arranged in the same manner as that figured in the Philosophical Magazine for September 1861, and described by Professor Tyndall in his memoirs "On the Absorption and Radiation of Heat by Gaseous Matter." The source of heat was a small flame of carbonic oxide. It is well known in what a capricious and unstable manner carbonic oxide burns in the open air, as it is extremely difficult to keep a small flame of this gas from being extinguished when unguarded from the currents of air present in a room. The first consideration was therefore to provide a means of protecting the source, so as to bring it under control and render it at once steady and uniform. The following method was finally adopted and adhered to throughout the investigation.

A glass globe 4 inches in diameter had its upper part drawn into a short chimney, and its lower into a neck which fitted tightly into a brass gallery, this being held at right angles by a brass stem. At a short distance from the gallery the stem was bent perpendicularly downwards, and then passed into a suitable stand in which it could be raised or lowered at pleasure, thus enabling the lamp to be fixed at any desired elevation. The globe was pierced by a third opening midway between the chimney and the neck. Through this aperture, which formed a tubulure 1·1 inch in diameter, the radiation from the source passed unchanged in quality into the experimental tube. Part of the interior surface of the globe opposite the central opening was thickly coated with silver-leaf, which by reflexion increased the total radiation without sensibly altering its character. Through the neck of the lamp passed a brass tube terminating in a small jet, by means of which any gas could be conveyed and burnt in the centre of the globe.

Carbonic oxide gas, contained in a large air-holder, was then forced by gentle pressure through a regulator, and finally caused to issue from the burner fixed in the lamp. The gas burnt with a small blue flame about half an inch in length, which could be adjusted by means of stopcocks or by slightly altering the regulator. And thus a source of heat nearly constant from day to day, and extremely steady, was obtained. In front of the lamp was mounted the brass experimental tube, 49·4 inches long and 2·4 inches in diameter, its ends being stopped by polished plates of rock-salt. As the diameter of the tube was greater than that of the opening in the lamp, a diaphragm was made of double polished brass having a central aperture of the same size as the opening. When fitted to the end of the tube next the source, the stop completely suppressed all radiation from the heated sides of the glass globe.

The radiation from the flame, after passing through the experimental tube, fell on the anterior face of a thermo-electric pile, and produced a certain deflection in a delicate galvanometer; this deflection was neutralized by a compensating cube containing water heated by steam and placed before the opposite face of the pile. The needle of the galvanometer was brought precisely to zero by the adjustment of a double metal screen placed between the cube and the pile. Another double screen of plated metal was now introduced between the tube and the source; a deflection of the needle took place indicating the amount of heat falling on the posterior face of the pile; and as this amount exactly neutralized the radiation from the flame, it corresponded to that radiation, and consequently gave the total heat emitted by the source. This deflection was carefully noted; the double silvered screen was now removed; the needle then descended to zero, and everything was ready for the first experiment.

After taking the total heat in the above manner, the brass adjusting screen before the compensating cube was left untouched, the slight variations which occurred throughout the day being remedied by altering, within small limits, the size or distance of the source.

The experimental tube having been exhausted, and the needle of the galvanometer standing at zero, air from the laboratory was drawn first over a U-tube filled with marble moistened with a solution of potash, and then over sulphuric acid into the experimental tube. Whilst the air was passing into the tube the needle was closely watched through a telescope, and it was found that the only effect of 30 inches of this pure air was slightly to augment the amount of heat passing through the tube, this action being due to the feeble dynamic radiation of the air. After the tube had been exhausted, air direct from the laboratory, and retaining therefore its carbonic acid and aqueous vapour, was allowed to enter; the needle instantly moved in the direction of absorption, and when the tube was filled, a deflection of 9°·3 was observed; this deflection corresponds to an absorption of 15 per cent. of the total radiation. The tube was again exhausted, and air was drawn into it after passing over sulphuric acid; this dry air gave a deflection of 8°·7, or 13·8 per cent. absorption. The small amount of carbonic acid present in the air could in this case have been the only agent which intercepted nearly 14 per cent. of the whole radiation.

To be assured of this remarkable result, fragments of solid potash were placed in a glass tube about 4 inches long, and common air passed over the potash alone into the tube, when a deflection of only 4°, or an absorption of 6·4 was obtained. This seems to establish the fact that, with a source such as a carbonic oxide flame, where the chief radiating body is heated carbonic acid, the absorption by the minute amount of carbonic acid in our atmosphere was easily measurable, and far exceeded the absorption by the aqueous vapour. The quantity of this latter gas is about 35 times the quantity of carbonic acid, and yet the generally feeble carbonic acid intercepts nearly 8 per cent. more of the radiation from a carbonic oxide flame. Taking the average state of our atmosphere as containing by volume 4 parts of carbonic acid and 140 parts of aqueous vapour in every 10,000 parts of air, and estimating the quantities of these gases present in the laboratory at the time of experiment at double this amount, the absolute quantities contained in the experimental tube and producing the above absorptions are found to be ·18 cubic inch of carbonic acid and 6·3 cubic inches of aqueous vapour, the capacity of the tube itself being 225 cubic inches.

In the memoir by Professor Tyndall referred to at the commencement, it was stated that the degree of accord between the oscillating periods of the molecules of a source of heat and those of a body placed in the path of its rays, determines the amount of absorption which those rays will undergo in passing through the interposed substance. This statement is illustrated in the foregoing experiments, whilst it was confirmed by substituting for the flame of carbonic oxide a small flame of hydrogen. Here, as before, air passed over potash and sulphuric acid showed only slight dynamic radiation; common air sent direct from the laboratory into the tube gave a deflection of 6°, or an absorption of 10 per cent. In the next experiment the air was dried by passing over sulphuric acid; the needle in this case showed no absorption whatever, the same amount of heat reaching the pile when the tube was filled with dry air as when the tube was exhausted. To complete the comparison, air was drawn into the tube after being deprived of its carbonic acid; here a deflection of 2°·5 was found, or 4·4 per cent. absorption.

The following Table shows these results placed side by side:—

Table I.

	Source:			
	Carbonic oxide flame.		Hydrogen flame.	
	Deflection.	Absorption.	Deflection.	Absorption.
Air passed over potash and sulphuric acid	0°·0	0·0	0°·0	0·0
Air direct into tube	9·3	15·0	6·0	10·0
Air *minus* aqueous vapour	8·7	13·8	0·0	0·0
Air *minus* carbonic acid	4·0	6·4	2·5	4·4

The day on which these measurements were made was remarkably fine and dry, so that the absorptions are certainly below those that might be expected under other circumstances.

On different days the absorption of the heat from a carbonic oxide flame by common air was found to be as follows:—

TABLE II.—Source: small carbonic oxide flame.

Common air.	Absorption per 100.
On March 5	18·7
On April 13	15·0
On April 28	18·1
On May 3	19·3

It is here evident that the variations in the amount of carbonic acid in the atmosphere can be detected by this mode of experiment: such a variation was strikingly shown by comparing air obtained from Brighton Downs with air taken from the laboratory. In both cases the air was dried, and the determinations made without altering the position of the source or experimental tube. The results are:—

	Absorption per 100.
Air from Brighton Downs	13·5
Air from the laboratory	17·8

The foregoing experiments led me to examine the absorption by the carbonic acid contained in our breath.

An india-rubber bag fitted with a stopcock was filled with air from the lungs. The experimental tube was exhausted and the needle brought to zero, where it steadily remained. Breath from the bag was now passed over sulphuric acid and into the tube; the needle moved quickly on the side of cold, and finally showed an absorption of 30°·8, or 50 per cent. Half the entire radiation from the carbonic oxide flame was thus cut off by the carbonic acid mixed with the air from the lungs.

Smaller pressures of breath from the same bag were next allowed to enter the tube, the absorptions found are given in the following Table:—

TABLE III.—Source: Carbonic oxide flame.

	Tension, in inches.	Deflection. °	Absorption per 100.
Carbonic acid of breath	1	7·2	12·0
	3	15·0	25·0
	5	20·0	33·3
	30	30·8	50·0

The bag used as a reservoir was put to a direct test, and found to have no influence on the absorption.

A platinum spiral, heated to whiteness by a voltaic current, was substituted for the carbonic oxide flame,—the object of this being to compare the relative absorption, by air and breath, of the heat emitted from the incandescent spiral and from the flame. With the spiral as source, 30 inches of common air absorbed 5 per cent. of the total radiation, while an equal quantity of dried air from the lungs gave almost exactly the same absorption.

On April 26, experiments with the carbonic oxide flame were renewed, for the purpose of making numerous measurements of the absorption by air from the lungs taken at different periods, and also to determine the amount of pure carbonic acid necessary to produce the same absorption. The percentage of carbonic acid in breath could then be calculated; and the amount found by this means, compared with an accurate chemical analysis, would show the reliability and advantages of this novel mode of experiment.

The experiments were conducted in precisely the same manner as that already described.

Two large vulcanized india-rubber bags, provided with suitable stopcocks and connexions, were thoroughly cleansed, by dry air, from any possible impurity. These bags were taken home, and No. I. filled with air from the lungs about half an hour after rising, and No. II. filled from the lungs of the same person about 10 minutes after breakfast. A third and smaller bag was also filled with air from the lungs after a brisk walk.

Dr. Frankland very kindly undertook to analyze these different specimens of breath,—the amount of carbonic acid in each being determined by him with that precision which his own delicate apparatus and high experimental skill enabled him to command.

The same day on which the chemical analyses were made, the absorption by the breath contained in each bag was ascertained, and is given in the next Table.

TABLE IV.—Carbonic oxide flame.

	Tension, in inches.	Absorption per 100.
Bag No. I. . . .	1	10·2
	15	47·2
	30	50·6
Bag No. II. .	1	10·75
	15	. .
	30	52·8
Bag No. III. .	1	11·8
	15	48·7
	30	53·7

The deflections are not given in this Table, because the absorptions are the mean of two or more experiments. The first bag was now emptied of the remaining breath it contained and cleansed with dry air; it was then filled from the lungs by Dr. Frankland, after he had undergone considerable exertion. The absorption by this breath is given in the fifth column of Table V., a chemical analysis of it being also made by Dr. Frankland. In the next Table the absorptions by air from the four bags are collected together and can be seen at a glance.

Table V.

Tension, in inches.	Bag No. I.	No. II.	No. III.	No. IV.
1	10·2	10·7	11·8	12·1
15	47·2	..	48·7	50·0
30	50·6	52·8	53·7	54·0

The analysis made by Dr. Frankland gave the following results as the percentage amount of carbonic acid contained in the several bags:—

Table VI.

	Carbonic acid per cent. in	Air from the lungs taken
Bag No. I.	4·311	before breakfast.
„ II.	4·556	after breakfast.
„ III.	4·061	after walking.
„ IV.	5·212	after severe exertion.

By comparing in each case the absorption given in Table V. with the analysis in Table VI., relative correctness is evident with one exception, namely that of bag No. III. An error from some cause must have arisen here, as an inspection of the two Tables shows that, whereas in all the other cases increase in absorption accompanies increase in the amount of carbonic acid, this bag is found to have a medium absorption, but, according to Dr. Frankland, contained the least amount of carbonic acid. The error may have been caused by the substance of the bag itself, which was different from, and newer than, the other two that were used. This bag was therefore rejected to avoid in future any chance of error, and in subsequent experiments the two other bags, which had been repeatedly tested, were adhered to.

A series of experiments was now commenced with a view of making quantitative measurements of the carbonic acid in breath.

This was thought to be easily accomplished by admitting into the exhausted tube a known quantity of pure carbonic acid until a deflection was obtained corresponding to that produced by the breath. As the absorptions in the two cases were equal, the percentage of carbonic acid in the breath could be readily found. The following Table shows how far experiment corroborated theory:—

TABLE VII.—Source: Carbonic oxide flame.

Pure carbonic acid gas.	Absorption per 100.
2·5 inches	54·5
4·0 inches	57·7
30·0 inches	70·0

Referring to Table V., we see that 30 inches of breath from bag No. IV. absorbed 54 per cent. of the whole radiation. The nearest approach to this absorption is the first in the above Table, and from this we may roughly assume that about 2·4 inches of pure carbonic acid gives about the same absorption as 30 inches of dried breath. The following proportion should then give the amount of carbonic acid in the breath from bag No. IV.,

$$30 : 100 :: 2{\cdot}4 : 8,$$

from which it appears that the breath in bag No. IV. contained 8 per cent. of carbonic acid. Chemical analysis shows us that there is but 5·2 per cent. of carbonic acid in this specimen of breath. Here is at once a formidable difficulty. It would be impossible to doubt the correctness of Dr. Frankland's analysis, for 8 per cent. is far beyond the average amount of carbonic acid found in expired air, yet at the same time the absorption was most accurately determined and confirmed by repeated experiments. Could any of the conditions of experiment in the two cases have been different? It was just possible that the burner in the lamp might have been moved; and if raised, the radiation from the heated jet would probably come into play and thus slightly alter the character of the source. The burner was therefore lowered a quarter of an inch, and in this position the following observations were made:—

TABLE VIII.—Pure carbonic acid gas.

Tension, in inches.	Deflection.	Absorption per 100.
1·0	28°·2	45·5
1·5	31·5	50·8
2·0	33·0	53·1
2·5	34·5	55·6
3·0	35·5	57·2
Total heat	45·0	100·0

We have here raised the absorption by the pure gas, but the calculated percentage is still very wide of the mark. Theoretically, nearly 1·6 inch of the pure carbonic acid should give the same absorption as 30 inches of breath from bag No. IV. The discrepancy was evidently not owing to any external cause; and the only difference was, that in the case of breath the carbonic acid was mixed with air, whilst with the pure gas no such medium was present. Possibly the air might have had an effect upon the carbonic acid somewhat similar to that which it is known to have upon aqueous vapour, by preventing any partial condensation of the gas. The following is the result of an experiment made to decide this question:—

Table IX.—Pure carbonic acid gas and dry air.

Tension, in inches.	Deflection.	Absorption per 100.
	°	
1·0	29·8	44·4
1·3	32·0	47·7
Tube now filled with dry air .	34·2	53·8
Total heat	46·1	100·0

Here carbonic acid at 1·3 inch tension absorbed nearly 47·7 per cent. of the heat from the carbonic oxide flame; but when 28 inches of dry air, perfectly inert when alone, were added to the gas in the tube, the absorption was raised to 53·8, or more than 6 per cent. This singular effect is most probably owing to the cause above mentioned, namely, that carbonic acid at small tensions is partially condensed on the polished surface of the interior of the tube: the entrance of dry air removes the film, and consequently throws a larger amount of gas in the path of the rays from the source. Many experiments were made to put this result beyond doubt, and to determine with certainty the absorption by carbonic acid at different tensions when it was mixed with dry air. It was found to be the best mode of experiment to admit into the tube a certain quantity, say 20 inches, of dry air first, observing its absorption, if any, and when the needle was at zero, adding a definite amount of pure carbonic acid to the air in the tube. The quantity of gas admitted was accurately found by observing, through a magnifying lens, the depression of the barometer-gauge attached to the air-pump, and which was in communication with the experimental tube. The following Table contains some of the results:—

TABLE X.—Dry air and carbonic acid gas.

	Deflection.	Absorption per 100.
20 inches of dry air, admitted first, gave	0°·0	0·0
1·2 inch of carbonic acid	34·5	51·0
1·5 " "	35·5	53·4
2·0 " "	37·0	57·3
Total heat	47·5	100·0
20 inches of dry air	0·0	0·0
1 inch of carbonic acid	31·5	48·4
1·1 " "	32·1	49·4
1·2 " "	32·7	50·7
1·3 " "	33·2	52·0
1·4 " "	33·6	52·9
1·5 " "	33·9	53·5
2·0 inches "	35·0	56·1
5·0 " "	37·8	64·0
Total heat	46·0	100·0

I next tried to determine the absorption by carbonic acid at lower pressures than one inch, but found that when the gas was added to the air in the tube, it required to obtain a certain tension before absorption became manifest. Although one-tenth of an inch of gas can be easily measured when added to one or two inches of carbonic acid already in the tube, yet one, two, or even three tenths, when admitted into an exhausted tube, show scarcely an appreciable absorption.

TABLE XI.—Dry air and carbonic acid gas.

	Deflection.	Absorption per 100.
21 inches of dry air first	0°·0	0·0
0·1 inch of carbonic acid	0·5	0·7
0·2 " "	0·5	0·7
0·3 " "	0·5	0·7
0·5 " "	21·5	32·1
1·0 " "	32·3	48·2
1·2 " "	33·2	50·4
1·3 " "	33·4	50·9
1·4 " "	33·8	51·8
1·7 " "	35·0	54·5
2·0 inches " "	35·9	56·8
Remaining 7 inches of dry air	36·0	57·0
Total heat	46·6	100·0

It will be seen in the above Table that the absorption suddenly rises at five-tenths of an inch: it will also be observed that the effect of filling the tube with 7 inches of dry air adds scarcely anything to the previous absorption.

These results, with a few others not before recorded, are placed together in the following Table, in which the first column shows the absorption by carbonic acid alone, and the second the absorption by the same amounts of carbonic acid added to about 20 inches of dry air, the absorption by the latter being *nil*:—

TABLE XII.

Tension, in inches.	Absorption per 100 by	
	Carbonic acid gas alone.	Dry air and carbonic acid.
0·5	...	32·1
1·0	45·0	48·4
1·0	44·8	48·2
1·1	...	49·4
1·2	...	51·0
1·2	...	50·7
1·2	...	50·4
1·3	47·7	52·0
1·4	...	52·9
1·4	...	51·8
1·5	50·8	53·4
1·5	...	53·5
1·7	51·2	54·5
2·0	53·1	57·3
2·0	53·0	56·8
2·0	53·0	56·1
2·5	54·9	...
2·5	54·1	...

The slight discrepancy between some of the repeated observations is probably the result of a small chemical difference in the carbonic acid prepared at different periods.

This Table gives us the power of calculating the percentage of carbonic acid in the different samples of breath; to obtain greater accuracy, omissions in Table XII., and the intervals between the tenths of an inch, are calculated and given with the mean experimental results in the annexed Table.

TABLE XIII.

Tension of gas, in parts of an inch.	Absorption per 100 by carbonic acid and dry air.
0·5	32·1
1·0	48·3
1·05	48·8*
1·1	49·4
1·15	50·0*
1·2	50·7
1·25	51·3*
1·3	52·0
1·35	52·3*
1·4	52·6
1·45	53·0*
1·5	53·4
1·55	53·7*
1·6	54·0
1·65	54·3*
1·7	54·5
1·75	54·9*
1·8	55·2*
1·9	55·9*
2·0	56·5

Referring to the absorption by 30 inches of breath in bag No. II. given in Table V. and found to be 52·8 per cent., we next compare this absorption with the nearest approach to it seen in the above Table; this is at 1·4 of an inch tension; this amount of carbonic acid, when mixed with 20 inches of dry air, absorbs 52·6 per cent. of the entire radiation from a carbonic oxide flame. We may therefore conclude that the absorption by 30 inches of breath in this case is approximately the same as by 1·4 inch of carbonic acid; from which we obtain 4·66 as the percentage of carbonic acid in the breath of bag No. II. Let us now turn to the analysis made by Dr. Frankland of the same breath. In Table VI. we find he states this expired air to contain 4·556 of carbonic acid. Our determination is therefore a remarkably close approximation, considering the novelty of this analysis by physical means, and the difficulties attending these first experiments.

A number of other specimens of breath taken at different periods were examined; their absorption and the percentage of carbonic acid are given in the following Table. Dr. Frankland

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* The absorptions thus marked have been intercalated.

having analyzed only the first four, the other determinations remained unchecked.

TABLE XIV.

Bag No.	Absorption per 100		Tension of CO^2, in parts of an inch.	Percentage of carbonic acid found	
	By 30 inches of breath.	By pure carbonic acid and dry air.		By absorption.	By chemical analysis.
I.	50·6	50·7	1·2	4·00	4·311
II.	52·8	52·6	1·4	4·66	4·556
III.	53·7	53·7	1·55	5·16	4·061
IV.	54·0	54·0	1·6	5·33	5·212
V.	50·0	50·0	1·15	3·83	
VI.	52·7	52·6	1·4	4·66	
VII.	50·0	50·0	1·15	3·83	
VIII.	52·1	52·0	1·3	4·33	

The percentage of carbonic acid found in bags No. II. and IV. only vary 0·1 from the chemical analysis; even this small difference will disappear in extended and repeated experiments. In bag No. I. the difference amounts to 0·3 per cent.: this would be considerable in a chemical analysis; but bearing in mind the small data upon which the physical determinations are made, it is sufficiently near to prove the correctness of the principle and the general accuracy of the observations. Bag No. III. shows a difference of upwards of 1 per cent. between the physical and chemical analyses. This anomaly has already been referred to, and may be accounted for by supposing that the material of which the bag is composed had imparted to the breath contained in it an impurity which could not be detected by chemical analysis, but which strongly influenced the more delicate mode of experiment.

In order to make these preliminary experiments complete, and to imitate as nearly as possible the condition of the breath as it entered the experimental tube, the following experiment was made.

A glass bolt-head, having rather a larger capacity than the brass tube, was fitted with a cap and stopcock and well exhausted. It was connected with the gauge of the air-pump, and also with drying-tubes, leading to a gas-holder containing carbonic acid. A stopcock was now carefully opened, and 1·4 inch of dry carbonic acid allowed to enter the bolt-head. The stopcock was then promptly closed, the drying-tubes and gas-holder removed, and pure dry air caused to fill the glass vessel. Thus a mixture was obtained containing 1·4 part of carbonic acid, and 28·6 parts of pure air, this being about the average composition of breath. The bolt-head was next connected with the experimental tube, and the latter thoroughly exhausted, the source of heat and the tube remaining the same as in previous experiments. Communication between the bolt-head and tube was then made, and closed when the barometer-gauge had sunk 15 inches. The needle showed a deflection of 28°·1, or an absorption of 43·3 per cent. When the whole contents of the bolt-head were allowed to diffuse themselves between the vessel and the tube, it was found that the mercury of the gauge had sunk nearly 17 inches. The absorption found in the last observation was therefore due to rather less than half the quantity of gas admitted into the bolt-head.

After the tube had been cleansed and exhausted, 0·7 inch of carbonic acid was allowed to enter: this quantity of gas gave an absorption of 34·7 per cent.; but when 14·3 inches of dry air were added to the gas in the tube, the absorption rose to 43·6 per cent. The absorption of the mixture from the bolt-head, containing rather less than 0·7 inch of carbonic acid, was found to be 43·3 per cent. Thus no difference exists between the absorption when carbonic acid and dry air are admitted into the tube together, or when they are allowed to enter successively.

In the first section of this paper some experiments are recorded which determine the absorption by the carbonic acid in the atmosphere. The amount of carbonic acid producing this absorption is so extremely small, that it is impossible to measure anything like its quantity with the barometer attached to the air-pump. A tension of only ·012 of an inch of the pure gas should theoretically be equal to 30 inches of common air. As yet, therefore, this estimation cannot be made; though possibly it might be accomplished by making use of the rectangular barometer invented by Cassini and Bernoulli, and also by admitting the carbonic acid after it has been mixed with air in a large receiver.

Royal Institution,
July 1864.

XIII. *On the Physical Cause of the Change of Climate during Geological Epochs.* *By* James Croll*.

NO fact in geological science is better established than that in former periods of our earth's history great changes of climate, in so far at least as the northern portions are concerned, must have taken place. But although there is universal agreement among geologists in regard to the fact of those changes having taken place, yet there is the greatest diversity of opinion

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* Communicated by the Author.

regarding their cause and origin. The great diversity and extreme character of those changes, as indicated by the remains of ancient *floræ* and *faunæ*, are such as to render it difficult to find any possible cause adequate for the effect.

The point regarding which the greatest difficulty has been felt, is in accounting for the extreme cold of the glacial epoch, and the warm and tropical character of the carboniferous.

Before entering on the consideration of a cause which appears in a great measure to have been overlooked by geologists, we shall briefly refer to a few of the more prominent theories which have been advanced to account for those changes.

The warm character of the climate during the Silurian, Carboniferous and other periods of the Palæozoic age, was at one time generally referred by geologists to the influence of the earth's internal heat. But it has been proved by Professor William Thomson* that the general climate of our globe could not have been sensibly affected by internal heat at any time more than 10,000 years after the commencement of the solidification of the surface. And Mr. Hopkins has concluded† that the present effect of internal heat is only about $\frac{1}{20}$th of a degree on the mean superficial temperature. Professor W. Thomson, from calculations based upon more correct data, has lately found that it only amounts to about $\frac{1}{75}$th of a degree‡. Professor Phillips, however, is still of opinion that the warm climates of ancient epochs may have been due to the influence of internal heat. He does not question the correctness of the calculations made by Thomson, Fourier, Poisson, and others, but thinks that they have overlooked the fact that the condition of the earth's atmosphere, as regards its power of conducting heat, might have been different in former ages from what it is at present. "The state of the atmospheric mantle," he says, "which envelopes the terraqueous globe, mitigates solar heat and stellar radiation, and, like the clothing of a steam cylinder, prevents excessive waste of the warmth treasured within"§. It is quite true, as Professor Phillips suggests, that a diminution in the conductivity or in the diathermancy of the earth's atmosphere, and an increase in its height, would increase the influence of internal heat on the climate. But when we reflect that under the present condition of the atmosphere the internal heat could not even sensibly affect the climate after the short period of 10,000 years from the commencement of solidification of the earth's surface, it appears very improbable that our atmosphere could have ever

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* Phil. Mag. for January 1863.

† Journal of the Geological Society, vol. viii.

‡ Proceedings of the Royal Society of Edinburgh for March 21, 1864.

§ Life on the Earth, p. 163.

been so far different from what it is at present, that by means of it the internal heat could have produced and maintained that high temperature of climate which is supposed by some to have prevailed during the long Palæozoic ages of the earth's history. And besides, the important fact is overlooked that any change in the condition of the atmosphere which would prevent the dissipation of the earth's internal heat into surrounding space, such as an increase in the quantity of aqueous vapour contained in the atmosphere, would at the same time tend to lower the temperature of the earth's surface by diminishing the quantity of radiant heat reaching the surface from without. Such a state of things would no doubt equalize, to a certain extent, the extremes of summer and winter temperature, but would not very sensibly increase the mean annual temperature of the climate. In fact it would rather have an opposite tendency.

Some have attempted to account for the change of climate by assuming that the earth's axis of rotation may have shifted its position in consequence of the uprising of large mountain masses on some part of the earth's surface between the equator and the poles. But it has been shown by Professor Airy* and others, that the earth's equatorial protuberance is such, that no geological change on its surface could ever possibly alter the position of the axis of rotation to an extent which could at all sensibly affect the climate.

Others, again, have tried to explain the change of climate by supposing, with Poisson, that the earth during its past geological history may have passed through hotter and colder parts of space. This, to say the least of it, is not a very satisfactory hypothesis. There is no doubt a difference in the quantity of force in the form of heat passing through different parts of space; but space itself is not a substance which can possibly be either cold or hot. If we adopt this hypothesis, we must therefore assume that the earth during the hot periods must have been in the vicinity of some other great source of heat and light besides the sun. But the proximity of a mass of such magnitude as would be sufficient to affect to any great extent the earth's climate would, by its gravity, seriously disarrange the mechanism of our solar system. Consequently if our solar system had ever during any former period of its history really come into the vicinity of such a mass, the orbits of the planets ought at the present day to afford some evidence of it. But again, in order to account for a cold period, such as the glacial epoch, we have to assume that the earth must have come into the vicinity of a

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* Athenæum for September 22, 1860.

cold body. Astronomical science affords not the slightest evidence in favour of such an hypothesis *.

A theory has lately been propounded by Prof. Frankland†, wherein the changes of climate experienced by our earth during past epochs is referred to a difference in the influence of internal heat on the sea and on land. He concludes that the cooling of the floor of the ocean would not proceed so rapidly as if it had been freely exposed to the air. And hence it would continue at a comparatively high temperature long after the surface of the dry land had reached its present mean temperature. And as heat is transmitted from the bottom to the surface of the ocean, not by conduction, but by convection, viz. by the warm stratum of water in contact with the bottom rising to the surface, the temperature of the ocean would consequently be higher than the mean temperature of the earth's surface. He concludes that this state of things satisfactorily accounts for the glacial epoch. "The sole cause of the phenomena of the glacial epoch," he says, "was a higher temperature of the ocean than that which obtains at present."

The high temperature of the ocean, he believes, would give rise to augmented atmospheric precipitation. This would produce such an accumulation of snow during winter months as would defy the heat of summer to melt. The overcast sky during summer, caused by the great amount of evaporation, would intercept the sun's rays, and thus reduce the summer's temperature.

While admitting the general correctness of Prof. Frankland's theory, we, however, fear that it does not altogether harmonize with the facts of geology. There is no evidence to support the conclusion that the ocean was warmer during the glacial epoch than at present; but, on the contrary, we have geological evidence to conclude that it must have been much colder than at present. For example, on examining the fauna of the marine drift of that period, we find that it is decidedly of an arctic character, indicating the low temperature of the seas during that epoch. These beds show, for example, that our British seas during that period contained in abundance numerous species of shells which are now only to be found in more northern latitudes ‡. In the glacial drift of Scotland alone there have been found the following species of an arctic character, and which are

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* See Mr. Hopkins's remarks on this theory, Journal of the Geological Society, vol. viii.

† Phil. Mag. for May 1864.

‡ This important fact was first noticed by Mr. Smith of Jordan Hill, and communicated to the Wernerian Society in the early part of 1839. See his Collected Papers published by John Gray, Glasgow.

now extinct in our British seas, viz. *Mya Uddevallensis, Saxicava rugosa, Tellina proxima, Thracia myopsis, Astarte arctica, Leda minuta, Leda truncata, Leda oblonga, Pecten Grœnlandicus, Pecten Islandicus, Margarita cinerea, Turritella erosa, Scalaria Grœnlandica, Natica clausa, Velutina undata, Buccinum ciliatum, Fusus carinatus, Cylichna alba.* Other arctic shells, such as the *Panopæa Norvegica, Puncturella Noachina, Nucula tenuis, Trophon clathratus, Trophon scalariformis, Natica pusilla, Natica helicoides, Trichotropis borealis, Saxicava arctica, Hypothyris psittacea, Margarita undulata, Margarita helicina, Buccinum Humphreysianum, Cyprina Islandica,* existed during the glacial epoch in great abundance in our British seas, but are now fast dying out in the deep and cold recesses of the ocean where they have retreated in order to find a temperature more congenial to their nature*.

We freely admit that a warmer sea and a colder land would tend to produce an accumulation of snow and ice such as prevailed during the glacial period. And did the facts of geology and the principles of physical science favour the idea that the sea during that period was warmer than at present, we should assuredly admit the warm sea to be at least one of the causes of the glacial epoch. But when the very same result may be conceived to follow upon the contrary supposition, which agrees better with the evidence of geology—that the sea was actually colder during the glacial period than at present—we feel inclined to refer the cold of that period to some other cause than to a warm sea. That a reduction of the mean temperature of both land and sea in our latitude would produce the same effect, will be obvious to all who will but reflect that at the present day there exists in places where the mean annual temperature is not much above the zero of the Fahrenheit scale, glaciers in magnitude equal to, if not greater than any which covered our valleys during the glacial epoch. At the present day there are glaciers upwards of 50 miles in breadth, and 2000 feet in depth, merging into the cold and frozen seas around the north of Greenland †. Greenland at the present day is probably a representation of what our island was during the glacial period.

Of late, evidence of the most conclusive character has been adduced by Prof. Ramsay and others of the existence of a glacial epoch in this country during the far back Palæozoic age.

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* See a valuable paper "On the Glacial Drift of Scotland," by Archibald Geikie, F.R.S.E., F.G.S. John Gray, Glasgow, 1863. See also a paper by Prof. E. Forbes "On the Connexion between the Distribution of the existing Fauna and Flora of the British Isles, and the Geological Changes which have affected their area during the period of the Northern Drift" (Memoirs of the Geological Survey, vol. i.).

† See Dr. Kane's 'Second Expedition,' vol. i. chap. xviii.

But the existence of glaciers in such an early age is certainly inconsistent with Prof. Frankland's theory, and in fact with every possible theory based upon the principle of internal heat.

Prof. Frankland accounts for the glaciers of the Permian period as follows :—

"I have already argued," he says, "that perpetual snow would first tip the mountain peaks, and then slowly and gradually descend to the sea-level. But it must be borne in mind that during the whole of the pre-glacial period the atmospheric precipitation was even greater than during that period, and consequently wherever the land rose well above the snow-line, glaciers, on a scale far surpassing any of the present time, would be the inevitable consequence"[*].

But glaciers on the sides of elevated mountains will not explain the facts of the Permian breccia. These breccias afford conclusive evidence that in that early age our British mountains were not only covered with perpetual snow, but must have had glaciers stretching into the sea, and breaking up and floating away as icebergs in a manner similar to what we find occurring in Greenland at the present day. We cannot do better than state the matter in Prof. Ramsay's own words.

"These breccias are chiefly formed of the moraine-matter of glaciers, drifted and scattered in the Permian sea by the agency of *icebergs* . . . They were therefore deposited in water with considerable regularity, and, as we have seen, over a large area. It is altogether unlikely that the stones were poured into the sea by rivers in the manner in which some conglomerates are formed on steep coasts where mountain-ridges nearly approach the shore, 1st, because the fragments, being derived almost exclusively from the Longmynd country, if the sea then washed its old shores, no river-currents passing out to sea could carry such large fragments from thirty to fifty miles beyond their mouths and scatter them promiscuously along an ordinary sea-bottom; and, 2ndly, if the rivers merely passed from the Longmynd across a lower land to the sea, transporting stones and blocks of various size, these would have been waterworn on their passage seaward after the manner of all far-transported river-gravels, whereas many of the stones are somewhat flat, like slabs, and most of them have their edges but little rounded"[†].

If we adopt the theory that the climate of our globe has been gradually becoming colder during all past ages, in consequence of the gradual diminution of the influence of internal heat, how are we to account for the glaciers and icebergs of the Permian

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[*] Phil. Mag. for May 1864.

[†] Journal of the Geological Society, vol. xi. p. 198.

period, following as they really do immediately after the warm coal-period?

Passing beyond the coal-period to the earlier age of the Old Red Sandstone, we again meet with the evidence of glacial action*.

It was long ago suggested by Agassiz, that the ancient climate was subject to alternate depressions and risings of temperature, coinciding with great destruction and renewal of life. Modern geological investigation seems to favour this conjecture. "Thus, looking," says Mr. Page, "at the Cambrian strata of the northern hemisphere—their angular grits and conglomerates, their extreme paucity of fossil forms, and other features—we are at once reminded of the action of ice and the presence of ungenial conditions. This is followed over the same areas by the more genial and exuberant period of Siluria; which is in turn succeeded by the Old Red Sandstone, whose grits and bouldery conglomerates, as well as paucity of vegetable forms, once more suggest the recurrence of colder influences. Following the Old Red, we have the exuberant flora and fauna of the coal-period, again to be succeeded by the scanty life-forms and grits and conglomerates of Permia. Again the trias and oolite of the northern hemisphere are characterized by life-forms that betoken warm and genial conditions; while the chalk that succeeds imbeds water-worn blocks of granite and lignite, which would seem to imply the presence of ice-drift and deposit in seas that were open to boreal influences. Next the early tertiaries occur over the same areas, marked by plants and animals that indicate a warm and genial climate; and this in turn gives place to the well-known glacial or boulder-drift epoch, once more to be succeeded by the milder influences of the post-tertiary or current era"†.

The principal cause why geologists have been so slow in admitting the existence of cold epochs during the earlier ages of our earth's history, is the idea still entertained by some, that, owing to the influence of internal heat, the climate of our globe was then very much warmer than at present, and that, ever since, it has been gradually growing colder and colder in consequence of the decrease of internal heat. But, as we have already seen, the notion is quite erroneous. If the climate in former ages was warmer than at present, the cause must be sought for elsewhere.

Some have referred the change of climate to a difference in the distribution of land and sea. It has been supposed by some,

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* See 'The Past and Present Life of the Globe.' By David Page, F.G.S., p. 91; and 'Advanced Text-Book,' p. 132.

† The Past and Present Life of the Globe, p. 190.

for example, that the cold of the glacial epoch might result from the absence of the Gulf-stream during that period.

Mr. Hopkins calculates that the absence of the Gulf-stream would lower the mean temperature of January 24° F. in the north of Scotland, but could have no sensible influence on the July temperature of London, or places in western Europe further to the south*.

Mean annual temperature due to the Gulf-stream.

Iceland	18° F.
North of Scotland . . .	12·25
Snowdon	7·5
Alps	3

Were the indications of ancient glaciers confined to the western parts of Europe, the absence of the Gulf-stream might to a considerable extent account for the phenomena of the glacial period. But we know that the glaciation extended over the greater part of northern Europe and northern America. It is perfectly evident that the absence of the Gulf-stream in our seas could not, for example, greatly lower the temperature of the climate of North America. Neither would a decrease of 3° F. in the mean annual temperature of the Alps, account for the enormous glaciers which we know existed there during that period.

Sir Charles Lyell supposes that, were the land all collected round the poles while the equatorial zones were occupied by the ocean, the temperature of the climate would be lowered to an extent that would account for the glacial epoch. And on the other hand, were the land all collected along the equator, while the poles were covered with sea, a temperature such as existed during the coal-period might be produced. Professor Phillips admits that if the land were all collected round the poles, the temperature of the globe would be lowered; but he remarks, truly, that this supposition does not agree with the observed facts regarding the glacial deposits, for these require deep sea over much of what is now in circumpolar zones. Professor Phillips appears to doubt very much whether the collecting of the land along the equator would sensibly increase the temperature of the globe; but suggests that the rise of temperature might result from the land being divided into low islands scattered over the area of the globe, amidst large breadths of water. But he brings forward no geological evidence in favour of such a state of distribution of land and sea during the warm epochs.

It is perfectly evident that if the great changes of climate

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* Journal of the Geological Society, vol. viii.

which have been experienced by our globe are to be attributed to differences in the distribution of sea and land, changes on the earth's surface of the most extravagant and unlikely character must be assumed to have taken place. Another objection which we have to all these hypotheses which have come under our consideration is, that every one of them is irreconcileable with the idea of a regular succession of colder and warmer cycles.

The recurrence of colder and warmer periods evidently points to some great, fixed, and continuously operating cosmical law.

We have already referred to the hypotheses of our system passing through colder and hotter parts of space, and of the shifting of the earth's axis of rotation, and have shown that they receive no support whatever from the known facts and principles of physical science. The true cosmical cause must be sought for in the relations of our earth to the sun.

There are two causes affecting the position of the earth in relation to the sun, which must, to a very large extent, influence the earth's climate; viz., the precession of the equinoxes and the change in the excentricity of the earth's orbit. If we duly examine the combined influence of these two causes, we shall find that the northern and southern portions of the globe are subject to an excessively slow secular change of climate, consisting in a slow periodic change of alternate warmer and colder cycles.

In a paper read before the Geological Society in 1830 *, Sir John Herschel directed attention to the probable influence of the change in the excentricity of the earth's orbit as a cause of the change of climate during geological eras. But as no trustworthy calculations had then been made regarding the superior limit of excentricity, he was unable to arrive at any positive results on the subject. It is true that Lagrange had investigated the subject, and had arrived at results which were afterwards found to be almost correct; but as this geometer had assigned very erroneous values to the masses of the smaller planets, not much confidence could be placed in his results.

Owing to his not having taken fully into consideration certain conditions which greatly affect climate, Sir John Herschel seems to have been of opinion that the general climate of our globe cannot be much affected by the change in the excentricity of its orbit, and this perhaps is the reason which has led geologists in general to take for granted that the changes in ancient climate cannot be attributed to this cause.

Both the superior and the inferior limit of excentricity have now been determined by M. Leverrier, and it may be well to examine to what extent climatic changes may be referable to this cause.

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* Transactions of the Geological Society, 2nd series, vol. iii. p. 295.

According to the calculations of Leverrier, the superior limit of the earth's excentricity is ·07775, and the inferior limit ·003314. The excentricity is at present diminishing, and will continue to do so during 23,980 years from the year 1800*.

The change in the excentricity of the earth's orbit may affect the climate in two different ways; viz., it may affect the climate by either increasing or diminishing the mean annual amount of heat received from the sun, or it may affect it by either increasing or diminishing the difference between summer and winter temperature.

Let us consider the former case first. The total quantity of heat received from the sun during one revolution is inversely proportional to the minor axis.

The difference of the minor axis of the orbit when at its maximum and its minimum state of excentricity is as 997 to 1000. This small amount of difference cannot therefore sensibly affect the climate. Hence we must seek for our cause in the second case under consideration.

When the excentricity is at a maximum, the distance of the sun from the earth, when the latter is in the aphelion of its orbit, is no less than 102,256,873 miles; and when in the perihelion it is only 87,503,039 miles. The earth is therefore 14,753,834 miles further from the sun in the former position than in the latter. The direct heat of the sun being inversely as the square of the distance, it follows that the amount of heat received by the earth when in these two positions will be as 19 to 26. According to the determinations of Hansen regarding the present excentricity of the earth's orbit, the earth during winter, when nearest to the sun, is 93,286,707 miles distant. Suppose now that, according to the precession of the equinoxes, winter in our northern hemisphere should happen when the earth is in the aphelion of its orbit, at the time when the orbit is at its greatest excentricity; the earth would then be 8,970,166 miles further from the sun in winter than at present. The direct heat of the sun would therefore be one-fifth less during that season than at present; and in summer one-fifth more than at present. The difference between the heat of summer and winter in this case would be two-fifths greater than at present. This enormous difference would affect the climate to a very great extent. But if winter under these circumstances should happen when the earth is in the perihelion of its orbit, the earth would then be 14,753,834 miles nearer the sun in winter than in summer. In this case the difference between winter and summer in the latitude of this country would be almost annihilated. But as the winter in the one hemisphere

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* *Connaissance des Temps* for 1843 (Additions).

corresponds with the summer in the other, it follows that while the one hemisphere would be enduring the greatest extremes of summer heat and winter cold, the other would be enjoying a perpetual spring.

It is quite true that whatever may be the excentricity of the earth's orbit, the two hemispheres must receive equal quantities of heat per annum; for proximity to the sun is exactly compensated by the effect of swifter motion. The total amount of heat received from the sun between the vernal and autumnal equinoxes is the same in both halves of the year, whatever the excentricity of the orbit may be. For example, whatever extra heat the southern hemisphere may at present receive from the sun during its summer months in consequence of greater proximity to the sun, is exactly compensated by a corresponding loss arising from the shortness of the season; and, on the other hand, whatever deficiency of heat we in the northern hemisphere may at present have during our summer half year in consequence of the earth's distance from the sun, is exactly compensated by a corresponding length of season.

But the surface temperature of our globe depends as much upon the amount of heat radiated into space as upon the amount derived from the sun. It will be observed, however, that this compensating principle holds only true in regard to the heat directly received from the sun. In the case of the heat lost by radiation the reverse takes place. The southern hemisphere, for example, has not only a colder winter than the northern, in consequence of greater distance from the sun, but it has also a longer winter. And this extra loss of heat from radiation is not compensated by its nearness to the sun during summer months; for, as we have already seen, it gains nothing in consequence of proximity. And on the same principle our winter in the northern hemisphere, in consequence of our proximity to the sun, is not only warmer than that of the southern hemisphere, but is also at the same time shorter. Consequently our hemisphere is not cooled to such an extent as the southern. It follows therefore, other things being equal, that the mean temperature of the winter half year, as well as the intensity of the sun's heat, must be inversely as the square of the sun's distance. But it is not on this change in the mean winter temperature, as we shall presently see, that the change of climate chiefly depends.

The Climate of the Carboniferous Epoch.

It is the generally received opinion among both geologists and botanists that the *flora* of the coal-period does not indicate the existence of a tropical, but a moist, equable, and temperate climate. "It seems to have become," says Sir Charles Lyell, "a more and more received opinion that the coal-plants do not, on the whole, indicate a climate resembling that now enjoyed in the equatorial zone. Tree-ferns range as far south as the southern parts of New Zealand, and Araucarian pines occur in Norfolk Island. A great preponderance of ferns and lycopodiums indicates moisture, equability of temperature, and freedom from frost, rather than intense heat"*.

Mr. Robert Brown considers that the rapid and great growth of many of the coal-plants showed that they grew in swamps and shallow water of equable and genial temperature.

"Generally speaking," says Mr. Page, "we find them resembling equisetums, marsh-grasses, reeds, club-mosses, tree-ferns, and coniferous trees; and these in existing nature attain their maximum development in warm, temperate, and subtropical, rather than in equatorial regions. The Wellingtonias of California and the pines of Norfolk Island are more gigantic than the largest coniferous tree yet discovered in the coal-measures"†.

The coal-period was not only characterized by a great preponderance over the present in the quantity of ferns growing, but also in the number of different species. Our island possesses only about 50 species, while no fewer than 140 species have been enumerated as having inhabited those few isolated places in England over which the coal has been worked. And Humboldt has shown that it is not in the hot, but in the mountainous, humid, and shady parts of the equatorial regions that the family of ferns produces the greatest number of species.

"Dr. Hooker thinks that a climate warmer than ours now is would probably be indicated by the presence of an increased number of flowering plants, which would doubtless have been fossilized with the ferns; whilst a lower temperature, *equal to the mean of the seasons now prevailing*, would assimilate our climate to that of such cooler countries as are characterized by a disproportionate amount of ferns"‡.

The enormous quantity of the carboniferous flora shows also that the climate under which it grew could not have been of a tropical character, or it must have been decomposed by the heat. Peat, so abundant in temperate regions, is not to be found in the tropics.

The condition most favourable to the preservation of vegetable remains, at least under the form of peat, is a cool, moist, and equable climate, such as prevails in the Falkland Islands at the present day. "In these islands," says Mr. Darwin, "almost

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* Elementary Geology, p. 399.

† The Past and Present Life of the Globe, p. 102.

‡ Memoirs of the Geological Survey, vol. ii. part 2. p. 404.

every kind of plant, even the coarse grass which covers the whole surface of the land, becomes converted into this substance"*.

From the evidence of geology we may reasonably infer that were the difference between our summer and winter temperature nearly annihilated, and were we to enjoy an equable climate equal to, or perhaps a little above the present mean annual temperature of our island, we should then have a climate similar to what prevailed during the Carboniferous epoch.

But we have already seen that such must have been the character of our climate at the time that the excentricity of the earth's orbit was at a maximum, and winter occurred when the earth was in the perihelion of its orbit. For, as we have already shown, the earth would in such a case be 14,753,834 miles nearer to the sun in winter than in summer. This enormous difference would almost extinguish the difference between summer and winter temperature. The almost if not entire absence of ice and snow, resulting from this condition of things, would probably tend to raise the mean annual temperature of the climate higher than it is at present.

The Climate of the Glacial and other Cold Epochs.

In this country the greater portion of the moisture of the air is precipitated in the form of rain, and what happens to fall during winter as snow disappears in the course of a few weeks at most. But were the winter temperature very much reduced, it is obvious that what now falls during that season as rain, would then fall as snow. Under such circumstances it would be very doubtful whether the heat of summer would be sufficient to melt the snow of winter. Whether this would be the case or not would depend upon the character of the summer. Under a cloudless sky, the direct rays of the summer-sun would, in our latitude, be more than sufficient to remove the winter's accumulation of ice and snow. But if from thick fogs or an overcast sky the direct rays of the sun were prevented from penetrating to the earth, the heat of summer would not in such a case be sufficient to remove the snow and ice; and the formation of glaciers would be the inevitable result. Some may at first sight suppose that the rain of summer would be sufficient of itself to melt the snow of winter, but such would not be the case; for it takes nearly eight tons of water at 50° F. to melt one ton of snow, even when the latter is already in a thawing condition. It is therefore perfectly evident that all the rain of summer would not be sufficient to melt more than one-eighth part of the snow of winter†. Prof. Forbes found that not more than one-

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* Journal of Researches, chap. xiii.

† Phil. Mag. for May 1864.

fiftieth part of the snow of Norway is liquefied by the rain of summer.

The conditions necessary to the formation of glaciers would be secured by a state of things the reverse of what produced the climate of the coal-period, viz., by winter occurring when the earth was in the aphelion of its orbit, at the time of greatest excentricity.

We have already seen that the direct heat during winter would, under these conditions, be nearly one-fifth less than at present. This would no doubt bring our mean winter temperature below the freezing-point. The low temperature of the winter would not only prevent the melting of the ice and snow, but would cause the entire moisture of the air to be precipitated, not in the form of rain as at present, but as snow. It is also more than probable that a diminution of one-fifth in the total quantity of heat received from the sun during the winter months, would lower the temperature to such an extent as would freeze our British seas. It is quite true that the direct heat of the sun during the summer would be one-fifth greater than at present. But it is questionable whether the summers would on this account be warmer than they are at present. The temperature of the summer is not always proportionate to the quantity of heat directly received from the sun. In the Straits of Magellan, in 53° S. lat., where the direct heat of the sun ought to be as great as in the centre of England, MM. Churruca and Galeano have seen snow fall in the middle of summer; and though the day was eighteen hours long, the thermometer seldom rose above 42° or 44° F., and never above 51°*.

The great strength of the sun's rays during summer, due to its nearness at that season, would, in the first place, tend to produce an increased amount of evaporation. But the presence of snow-clad mountains and an icy sea would chill the atmosphere and condense the vapour into thick fogs. The thick fogs and cloudy sky would effectually prevent the sun's rays from reaching the earth, and the consequence would be that the snow would remain unmelted during the entire summer. In fact we have this very condition of things exemplified in some of the islands of the southern ocean at the present day. Sandwich Land, which is in the same parallel of latitude as the north of Scotland, is covered with ice and snow the entire summer. And in the island of South Georgia, which is in the same parallel as the centre of England, the perpetual snow descends to the very sea-beach. The following is Capt. Cook's description of this dismal place:—"We thought it very extraordinary," he says, "that an island between the lat. of 54° and 55° should, in the very

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* Edinburgh Philosophical Journal, vol. iv. p. 266.

height of summer, be almost wholly covered with frozen snow, in some places many fathoms deep. . . . The head of the bay was terminated by ice-clifts of considerable height; pieces of which were continually breaking off, which made a noise like a cannon. Nor were the interior parts of the country less horrible. The savage rocks raised their lofty summits till lost in the clouds, and valleys were covered with seemingly perpetual snow. Not a tree nor a shrub of any size were to be seen. The only signs of vegetation were a strong-bladed grass, growing in tufts, wild burnet, and a plant-like moss, seen on the rocks. . . . We are inclined to think that the interior parts, on account of their elevation, never enjoy heat enough to melt the snow in such quantities as to produce a river, nor did we find even a stream of fresh water on the whole coast"*.

This rigorous climate chiefly results from the rays of the sun being intercepted by the dense fogs which envelope the island during the entire summer; and the fogs, again, are due to the air being chilled by the presence of the snow-clad mountains, and the immense masses of floating ice which come from the antarctic seas. A reduction of one-fifth in the amount of heat received from the sun during winter would, in this country, produce a state of things as bad as, if not worse than that which at present exists in South Georgia.

Some may be apt to suppose that the presence of the Gulf-stream would under such a condition still prevent our British seas from freezing during winter. We may, however, remark that it is not necessary for us to assume that our seas were frozen during the glacial epoch. We know that the seas around Sandwich Land, and the island of South Georgia, are never frozen, and yet the perpetual snow descends to a lower level than it does in Greenland or in Spitzbergen. All that seems necessary would be the presence of immense masses of floating ice during summer months, and this we should then no doubt certainly have, notwithstanding the presence of the Gulf-stream.

But if we examine the matter fully, we shall find that the Gulf-stream as well as the climate is affected by the change in the excentricity of the earth's orbit.

It is now generally admitted that the cause of the great oceanic currents is the constant impulse of the trade-winds on the surface of the ocean. The trade-winds, on the other hand, owe their existence to the difference of temperature between the equatorial and polar regions of the globe. Now any cause which tends to increase or diminish this difference will, other things being equal, tend also to increase or diminish the strength of these aërial currents. The general tendency of the under currents of

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* Capt. Cook's Second Voyage, vol. ii. pp. 232, 235.

the atmosphere is to pass from the polar to the equatorial regions. We have already seen that, as the excentricity of the earth's orbit increases, the severity of the winter in the one hemisphere is augmented, while that in the other hemisphere is diminished. The glacial epoch, as we have already found, probably occurred in Europe at the time when the winters in the northern hemisphere were at their severest, and those in the southern hemisphere at their mildest condition. The great accumulation of ice and snow in the northern regions, arising from the severity of the seasons, and its comparative absence in the southern hemisphere, would tend to keep the air in the northern regions of the globe much colder than the air in the southern hemisphere, and the consequence would be that the aërial currents from the north would be stronger than those from the south. The general effect of this state of things would be to diminish the Gulf-stream, as we shall presently see.

According to Captain Duperrey, the constant ocean currents, of which the Gulf-stream is one, seem all to take their rise from three great currents of cold water from the south pole. We have first the great equatorial current of the Pacific, taking its rise in a cold current from the south pole. A portion of this equatorial current passes through the Asiatic archipelago and joins a second cold stream flowing into the Indian Ocean from the south. The current then flows westward, passes round the Cape of Good Hope, where it joins the third southern current, which passes along the western coast of Africa, and then takes a westerly direction, forming what is called the equatorial current of the Atlantic. On approaching Cape St. Roque, this current divides itself into two portions; the principal portion flowing into the Gulf of Mexico, and forming what is known as the Gulf-stream, the other portion directing its course to the south along the coast of Brazil.

The diminution of the aërial currents in the southern hemisphere would tend in the first place to reduce the great currents of cold water from the south pole which feed the equatorial currents; and this in turn would diminish the equatorial current of the Atlantic, the feeder of the Gulf-stream. The equatorial current being reduced, the Gulf-stream would also be reduced. But there is another way in which the Gulf-stream is affected by this state of things. At present the S.E. trades of the Atlantic blow with greater force than the N.E. trades, and the consequence is that the S.E. trades sometimes extend to 10° or 15° N. lat., whereas the N.E. trades seldom blow south of the equator. But during the glacial epoch the very reverse must have occurred. Hence the great equatorial current of the Atlantic must during that period have been driven considerably to the south of its present position. And in this case, the configuration of the land being supposed to have been similar to what it is at present, a greater proportion of the current would be turned into the southern branch along the Brazilian coast, and of course a correspondingly smaller proportion would flow into the Gulf of Mexico. The Gulf-stream would consequently be greatly diminished, if not altogether stopped.

If a relation could be properly established between geological epochs and changes of climate, due to the change in the excentricity of the earth's orbit, we might then have some hope of being able to arrive, at least approximately, at a knowledge of the positive ages of the various strata composing the earth's crust. The total age of the crust itself, as we have already noticed, can be determined by other means. Taking the temperature of melting rock at 7000° F., Prof. William Thomson has calculated, from principles of cooling established by Fourier, the probable age of the earth's crust to be about 98,000,000 years*. The entire geological history of our globe must therefore be comprehended within this period.

As yet no calculations have been made regarding the time when the excentricity was at a maximum, or of the time required to pass from the maximum to the minimum state of excentricity. In the *Annales* of the Paris Observatory, vol. ii. p. 29, there is a Table giving ·0473 for the excentricity at 100,000 years before the year 1800, and ·0189 for the excentricity 100,000 years after 1800. There are subordinate maxima and minima in that interval of 200,000 years; but the principal maximum I have been informed does not fall within that period. We may therefore safely conclude that it is considerably more than 100,000 years since the glacial epoch.

XIV. *On the Dispersion of Light by Quartz, owing to the Rotation of the Plane of Polarization.* *By* Prof. STEFAN†.

THERE are only two possible forms of dispersion; to each colour in white light may be assigned either a particular velocity of propagation or a particular direction of vibration. The first kind of colour-dispersion occurs in refraction and diffraction; the second kind when light passes through a substance which turns its plane of polarization, inasmuch as the rotation has a different magnitude for each colour.

A spectrum resulting from the alteration of the plane of po-

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* Phil. Mag. for January 1863.

† Translated by Prof. Wanklyn from the papers issued by the Kaiserliche Akademie der Wissenschaften in Wien, 1864, No. 15.

larization may be exhibited in the following manner:—Polarized light is passed through the rotating medium, made to fall upon a conical mirror, which serves as an analyzer, and projected upon a screen placed perpendicular to the axis of the mirror. The white light falling upon the cone appears spread out into a coloured fan. Or a plate of calcareous spar is introduced into a polarizing apparatus, so that the ring-like figures appear small and near the centre of the field, whilst the black cross is spread over the whole field. If the pencil of rays, where it consists of parallel rays, be passed through a plate of quartz cut perpendicular to the axis, then the black cross just mentioned will be transformed into a coloured fan.

The occurrence of dispersion through refraction, or through alteration of the plane of polarization, leads to the conclusion that in the one case the refractive index, and in the other the angle of rotation is a function of the length of the undulations of a colour. Each colour is determined by the length of the undulations, also by the refractive index, or by the angle of rotation in a given substance. There must therefore be a connexion between the two last-named quantities. This connexion may be disclosed by a prismatic analysis of the light as it leaves the polarizing apparatus.

The rotation of the plane of polarization is proportional to the thickness of the plate of quartz. When the latter is considerable, then the amount of rotation for the different colours is equal to several complete revolutions. When the polarizer and analyzer are placed parallel, the latter removes from the light coming through the quartz all coloured rays which have undergone rotations which are odd multiples of 90°. In the places of these colours, dark bands appear in the spectrum. In order to arrive at the number of the bands, the thickness of the plate in millimetres should be multiplied by $\frac{1}{6}$ and $\frac{5}{9}$; the number of odd integers between the two products is the number of bands.

For the purpose of getting the bands as sharply defined as possible, the following rule may be given:—Place the prism so that it gives with a mean ray a minimum of deviation, and the quartz plate so that the bands in the fixed spectrum have the maximum deviation. The latter is the sign that the rays pass through the quartz parallel to the optic axis

On making the analyzer rotate, the bands pass from the red towards the violet end, or the reverse, according as the analyzer moves in the sense of the rotation of the plane of polarization or the contrary. Thereby the number of bands may be altered by a unit.

The relative position of the bands is dependent upon the nature of the substance forming the prism, and upon the thickness of the rotating plate. For a prism made of crown glass the following propositions may be deduced from the measurements:—

1. The dark bands of the spectrum are equidistant.
2. The distance between two contiguous bands is inversely proportional to the thickness of the quartz plate employed.
3. The bands move regularly and correspondingly on turning the analyzer.

Since the dark bands answer to colours of which the angles of rotation differ by a constant quantity, it follows that the distances of the colours in the spectrum are proportional to the differences of their angles of rotation.

By the refractions in the prism, the directions of propagation of the coloured rays, and by rotation in the quartz, their directions of vibration are spread out so as to form a fan. The arrangement of the colours follows the same law in both fans.

On calculating the refractive indices of the individual dark bands, the following law is arrived at:—Equal differences of refractive index correspond to equal differences of rotation. Angle of rotation and refractive index are therefore in linear connexion; consequently both are similar functions* of the length of the undulations.

Making the reciprocal squares of the lengths of the undulations abscissæ and the indices of refraction ordinates, then, according to Cauchy's law of dispersion, the terminal points of the latter will lie in a straight line. The dispersion through rotation in quartz follows therefore the same law. Biot's law, that the angle of rotation is inversely proportional to the square of the length of the undulations, cannot be maintained. The line drawn for the angles of rotation cuts the axis of ordinates, not at the origin, but on the negative side. If this line is also correct for the ultra-red rays, then for rays of a certain length of undulation, a right-rotating quartz may become left-rotating, and *vice versâ*.

The investigation of the spectrum of flint glass led to the same laws. For the spectra of water and quartz, it was found that the dark bands lay nearer to one another towards the violet end. A corresponding departure of the refraction produced by this substance from Cauchy's law was therefore inferred, and found to be supported by direct observation.

Moreover a direct way was devised for finding the dependence of the angle of refraction upon the length of the undula-

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* If by "similar functions" be meant functions differing merely by a constant multiplier, as the reasoning in the next paragraph seems to imply, the statement is clearly wrong. The author seems to have forgotten the necessity of introducing an arbitrary constant after integration. The experimental results stated are in perfect harmony with Biot's law.—G. G. S.

tions. The light proceeding from the analyzer was sent through a fine grating (*Gitter*) instead of through a prism; the dark bands appeared in the spectra produced by diffraction. The bands are not equidistant, but approach one another quite close at the violet end. If the reciprocal squares of the sines of the deviations of the bands be taken, they will be found to be in arithmetical progression. The former law is thereby afresh supported.

This opportunity was also taken to measure the lengths of undulation of the following Fraunhofer's lines, viz., A, a, B, C, D, E, b, F, G; and the following values in millionths of a millimetre were found: 759·8, 717·8, 687·2, 655·8, 589·4, 525·3, 518·7, 484·3, 430·2.

For the angles of rotation of the lines B, C, D, E, F, G, H, these values were obtained: 15·55, 17·22, 21·67, 27·46, 32·69, 42·37, 50·98 degrees. The constant part in the dispersion-formula is −1·697*, the part divided by the square of the length of undulation is +8·1088.

The foregoing phenomena are well adapted for exhibition by projection. The following arrangement answers:—Heliostat, slit in window-shutter, polarizing prism, quartz column, analyzing Nicol, lens of 1¹ metre focus, prism in minimum deviation, or grating directly against the lens, distance of the latter from the slit 3 metres, screen where the image is distinct.

XV. *On Earth Currents, and their relation to Electrical and Magnetic Phenomena.* *By* Father Secchi†.

A RECENT communication by M. Matteucci on Earth Currents, in which he mentions my researches on the same subject, affords me an opportunity of presenting the results which I have obtained by comparing observations of magnetized bars and of the atmospheric electricity. The limits of this Note prevent my entering upon the details of these observations, and I shall confine myself to the principal results.

But before presenting these comparative results, I think it well to resolve some difficulties on the origin of these currents. M. Matteucci's investigation has proved directly that they are not the effect of the chemical action of the terminal plates. I have arrived at the same conclusion in an indirect manner by changing the terminal plates, and finding that the current of the plates, which is strong enough for short circuits, becomes very feeble for the resistance of conductors when the circuit is long enough, and that in other cases the direction is often opposite to that of the electromotive force of the plates. My researches, moreover, were not directed towards the absolute value of these

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* There must be some mistake. The constant term ought assuredly to be positive.—G. G. S.

† *Comptes Rendus*, June 27, 1864.

currents, but simply to their variations; so that a constant current of any origin would have no influence on the results.

But in these researches there is a source of error which could not be neglected; that is, the influence of temperature on the line-wires. This may be regarded from a twofold point of view: (1) the current itself may be considered to be thermo-electric in virtue of the solar action on the wire; (2) a variation of resistance in these wires, due to the temperature, may make a constant current appear periodical. Thus the current from Rome to Anzio, although constant, might appear variable.

To resolve this difficulty, it seemed to me that the use of two wires at right angles, one in the meridian and the other in the parallel, would give sufficient elements; for, these two wires being subjected to almost the same thermal variations, the resulting difference ought to have the same phases. Only having at my disposal the meridional wire, I requested M. Jacobini, Inspector of Telegraphs, to be good enough to use his leisure and the intervals of inactivity of a line directed towards the east, and to compare it simultaneously with another directed in the meridian to see if they presented notable differences.

M. Jacobini commenced then a series of observations between Rome and Arsoli, a station to the east of Rome, 50 kilometres distant, in the Apennines, and which is at right angles to the magnetic meridian. Observations were simultaneously made on the line from Rome to Anzio, 52 kilometres in length and on the magnetic meridian, the same line as is used for the Observatory. After several preliminary trials, a regular system of observations was arranged towards the end of May; I give here the results of the first half of June from the 1st to the 16th, excluding, however, the days 7, 8, 9, and 10; for on these days there was a strong magnetic disturbance, and currents in all directions traversed the wire in a very abnormal manner, to which I shall afterwards revert.

Currents observed on the Telegraphic Wire from Rome to Arsoli and to Anzio.

	6 A.M.	7.	8.	9.	10.	11.	Mid-day.	1 P.M.	2.	3.
Arsoli, E...	17°·0	24°·0	26°·0	18°·6	16°·2	14°·0	11°·6	14°·0	17°·0	19°·5
Anzio, S...	20·0	18·0	20·5	21·0	22·0	22·4	22·0	20·5	20·3	18·0

	4 P.M.	5.	6.	7.	8.	9.	10.	11.	Mid-night.
Arsoli, E...	18°·0	18°·0	19°·0	16°·0	16°·0	19°·0	20°·0	19°·0	23°·0
Anzio, S....	17·5	17·0	18·0	20·0	19·0	19·0	20·0	20·5	20·5

The conclusions to be drawn from this Table are obviously the following:—

(1) The fluctuation of the current in the direction of the vertical (or equatorial) is greater than in the direction of the meridian.

(2) The maximum of the one corresponds to the minimum of the other, so that the two periods are almost complementary; thus the maximum of the equatorial is about 8 o'clock, and the minimum of the meridian is at 7 o'clock, or 7.30; the equatorial minimum is midday, and the meridional maximum between 11 o'clock and midday. I say between these limits, because as the observations were not always made exactly at the hour, in the mean these fractions of an hour were reduced to the nearest entire hour, which is adequate in this matter.

(3) Besides the principal maxima and minima, there are, after midday, other but more feeble secondary maxima and minima, in which the same law of complement prevails as that observed in the morning.

(4) During night the current is almost constant, but higher.

From the nature of these results, it appeared impossible to attribute these currents to thermal actions, from whatever point of view they are regarded. But these results give a valuable clue to the discovery of the source of these variations, and show that it is sufficient to compare the period of the current in one direction to obtain that of the other; and thus our researches may be utilized although made solely in the direction of the meridian.

The three following Tables condense the observations made during one year in the meridional terrestrial current from Rome to Anzio, and I compare it with those of the bifilar and of atmospheric electricity. I do not add the periods of the declinometer and of the vertical, because they are simpler and better known. The minimum of the vertical is between 11 o'clock and midday; that of the declinometer according to well-known laws. The maxima of the vertical are the morning and the evening, after which there is a feeble nocturnal minimum. What most affects the bifilar is that its period changes with the season, so that in winter the minimum after midday disappears.

TABLE I.—Mean Values of the Terrestrial Currents observed between Rome and Anzio at the Collége Romain.

Date.	Time.									
	7 A.M.	9.	10.30.	Mid-day.	1.30 P.M.	3.	5.	7.	9.	10.
1863.										
May 25 to 28	4·63	6·0	7·5	6·20	5·53	3·85	4·3	4·13	4·33	4·70
Aug. 10 to 31	7·31	9·12	9·91	8·90	7·16	6·98	6·58	7·00	7·37	7·86
Sept. 1 to 30	6·90	7·80	9·9	8·3	6·78	6·43	6·92	6·67	6·76	7·11
Oct. 1 to 18	8·1	8·42	9·26	8·17	7·74	6·28	6·25	6·10	6·27	
Nov. 6 to 30	15·62	14·15	13·77	13·87	9·31	14·52	15·13	11·31	11·8	15·30
Dec. 1 to 17	13·84	11·3	11·93	12·15	11·45	11·57	10·15	11·48	11·86	
1864.										
Jan. 1 to 31	8·79	8·95	8·85	8·97	8·42	7·78	8·15	8·56	8·65	
Feb. 1 to 29	7·56	8·2	8·57	8·81	7·98	7·21	7·20	6·73	7·35	
Mar. 1 to 31	6·87	8·93	10·5	9·55	6·65	6·21	6·72	7·75	7·95	
Apr. 1 to 24	5·42	8·6	8·75	7·21	5·55	5·34	4·45	6·34	6·52	

By this long series of observations, the diurnal period with its principal minimum in the morning between 7 and 9 o' clock, and the maximum near midday, is seen to be confirmed; but the influence of seasons tells, for there is anticipation in summer, and retardation in winter.

Although the absolute value of the current does not enter into our discussion, the enormous increase which is attained during the last quarter of 1863, and especially in the month of November, must not be overlooked.

It is interesting to compare these variations with those of the horizontal force.

TABLE II.—Indications of the Bifilar.

Date.	Time.										Therm.
	7 A.M.	9.	10.30.	Mid-day.	1.30. P.M.	3.	5.	7.	9.	10.30.	Fahrenheit.
1863.											°
May	134·4	133·7	136·9	136·0	137·4	135·4	136·5	135·4	137·4	136·7	66·95
Aug.	125·4	123·0	124·6	126·9	127·9	127·4	127·5	128·1	128·7	129·7	79·30
Sept.	127·2	124·7	126·5	127·9	129·9	128·2	124·5	128·9	129·3	128·4	76·42
Oct.	139·0	135·9	134·7	135·7	136·9	135·7	137·7	137·3	138·7	......	69·63
Nov.	149·9	147·3	146·9	146·3	144·4	144·3	144·8	145·9	147·4	148·8	62·92
Dec.	160·7	155·7	157·9	157·2	156·2	155·4	156·1	157·1	157·3	158·6	53·12
1864.											
Jan.	96·6	94·2	93·9	92·2	93·3	92·6	92·9	93·3	93·7	......	47·67*
Feb.	96·8	94·1	92·2	91·7	92·9	92·4	92·4	93·3	94·2	......	49·47
Mar.	87·5	84·9	83·6	83·9	84·8	84·4	85·6	85·6	85·8	......	55·84
Apr.	85·7	82·4	83·7	86·7	87·0	85·9	85·6	86·9	87·4	86·5	53·98

* The scale changes at the beginning of the year.

Here the last column contains the mean temperature of each month, and a proportional correction of 0·9 must be applied for each degree of thermometric variation. But even after this correction the months of November and December are signalized by an enormous increase in the absolute value of the horizontal component, which I can also verify in the observations of Lisbon, and which I think general, and which has obliged us to change the scale.

As regards the diurnal variation, the minimum is seen to correspond to the maximum of the equatorial current, and in the summer season the two instruments exhibit after midday a secondary oscillation which disappears in winter.

Lastly, I shall adduce the results of the atmospheric electricity obtained with the moveable conductor in the same period of observations. I must first remark that for their absolute value the numbers must be referred to the unit of measure in the last column, dividing them by this number. M. Volpicelli, in a Note printed in the *Comptes Rendus,* has said that our apparatus contains a long wire covered with gutta percha, which being agitated might falsify the indication. That is not correct; for the communication between the conductor and the electrometer is effected by a very short wire, not more than a metre, and which is naked, having only been varnished long after it was ascertained that that had no appreciable influence.

TABLE III.—Mean Monthly Values of the Atmospheric Statical Electricity.

Date.	Time.										Unit.
	7 A.M.	9	10	Mid-day.	1·35	3	5	7	9	10.30	
1863.											
May	6·41	7·53	......	4·89	4·57	4·81	4·60	5·24	7·03	6·91	7·09
August	6·12	7·44	......	5·62	4·49	4·72	4·32	6·15	8·10	7·59	6·96
September ...	4·72	5·31	......	4·73	4·17	4·00	5·00	5·81	6·22	4·44	5·00
October	3·97	3·81	......	5·05	4·15	3·98	5·65	5·29	3·98	......	6·15
November ...	4·46	3·85	......	4·72	5·58	4·48	8·77	7·85	4·61	6·70	5·20
December ...	4·37	7·00	......	8·13	6·75	6·69	8·76	8·79	7·09	6·10	5·29
1864.											
January	3·66	6·82	6·00	7·13	5·96	6·76	6·32	8·30	5·79	......	4·87
February......	2·75	3·81	4·42	3·89	2·97	2·56	2·74	5·19	3·64	......	4·25
March	3·88	3·85	3·63	3·32	3·12	2·72	3·18	5·17	5·69	......	4·46
April	4·13	3·35	3·49	2·64	2·59	2·78	3·46	4·64	4·03	......	4·50

It is seen by this Table that in general the electricity has a double maximum and minimum, especially in summer. The first maximum corresponds in the morning to 1 o'clock, the time of the maximum of the current; and it is the same with the maximum of the evening. But there is this difference, that while the maximum of the morning is the principal in the current, it is only the secondary in statical electricity.

The general conclusion to be drawn from all this is, that the variations in the currents of magnetized bars and of atmospheric electricity may be derived from the same principle in motion; that this action cannot be confounded with that of solar heating; and that rather a kind of diurnal electrical flux and reflux must be assumed allied to the solar action, but whose energy in this transformation is manifested in a different manner to that of direct heat and light. The opinion already enunciated by M. De la Rive, that the different variations of magnetized bars may be derived from the atmospheric electricity, appears thus to acquire great probability.

I shall conclude with a word on the extraordinary magnetic variations. Observations made during the 7th, 8th, 9th, and 10th of June, when there was a great magnetic disturbance, have once more shown that these motions of the bars are connected with motions of the currents; but a profound discussion cannot find a place here. I may, however, simply remark that the existence of irregular earth currents in telegraphic wires has become to M. Jacobini a very marked sign of approaching bad weather and surrounding storms; and I think this might be utilized in other telegraphic lines for predicting the weather. This is also a new and unsought-for confirmation of the connexion between storms and magnetic variations.

XVI. *Mineralogical Notes.* *By* Professor N. S. Maskelyne *and* Dr. Viktor von Lang, *of the British Museum.*

[With a Plate.]

[Continued from vol. xxvi. p. 139.]

On some Combinations of Gadolinite. *By* Viktor von Lang.

THE researches of A. E. Nordenskjöld[*] and Th. Scheerer[†] have shown that the crystals of Gadolinite belong to the prismatic system. To the same conclusion I was led by the examination of the specimens of Gadolinite in the mineralogical collection of the British Museum, although several of the crystals I examined were of a decidedly oblique habit. But as we find that on such apparently oblique crystals sometimes the longer, sometimes the shorter diagonal of the prism comes to simulate the axis of symmetry, we are justified in considering that fact only another proof of the prismatic character of these crystals.

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[*] *Öfversigt. af Kongl. Vetenskap Akademiens Förhandlingar,* 1859, No. 7, p. 287.

[†] *Neues Jahrbuch für Min.* &c. 1861, p. 134.

The planes hitherto known on Gadolinite are*

100, 001, 110, 101, 102, 111, 112.

In addition to these I have found the new planes

210, 014, 211, 322, 423,—

fig. 9, Plate III. representing the poles of all these planes.

From the angles measured by Nordenskjöld with only the hand-goniometer, we deduce the elements†

$$a:b:c=1:0{\cdot}6249:1{\cdot}3780.$$

They represent also my measurements (which on the crystals of Gadolinite can never be made with great accuracy) sufficiently well, and with them are calculated the angles of the following Table:—

	100.	010.	001.	110.
110	58° 0′	32° 0′	90° 0′	0° 0′
210	38 40	51 20	90 0	19 20
201	19 57	90 0	70 3	60 7
101	35 58	90 0	54 2	64 36
102	55 26	90 0	34 34	72 30
014	90 0	61 8	28 52	65 50
111	60 21	37 41	68 58	21 2
112	65 10	47 46	52 26	37 34
211	41 18	46 10	74 11	24 47
322	49 31	53 3	71 42	21 20
423	44 3	54 54	66 59	29 43

The following observed combinations are represented in Plate III.

Fig. 1.—001, 110, 101, 102, 111, 112.
Fig. 3.—100, 001, 110, 210, 101, 210, 211.
Fig. 4.—100, 010, 110, 210, 101, 102, 111.

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* In the 'Handbook of Mineralogy' by Brooke and Miller, a plane, *e*, parallel to the axis *b* is given with an angle $ee'=33°\ 34'$, so that the symbol of that plane would become nearly (014). But as the crystal measured by Miller is the same as that investigated by Phillips, a comparison of the two measurements shows that the above angle is very probably to be taken as the inclination *e*, 100. As the calculated angle $102\,.\,100=34°\ 34'$ comes near to the former angle, the symbol of the plane *e* is certainly (102).

† The elements which I had deduced from my measurements before I became acquainted with Nordenskjöld's memoirs are

$$a:b:c=1:0{\cdot}6289:1{\cdot}3746.$$

These forms were observed on large crystals from Ytterby, Sweden. The crystals are of an opake green material, which is traversed by white veins: they may perhaps be only pseudomorphous, after the forms of real Gadolinite. The planes are here and there brilliant.

I found on the crystals figs. 1 and 4,

110.010 = 32° 15′ 32° 0′ calc.
210.110 = 21 appr. 19 20 „
102.001 = 34 28 34 34 „
102.101 = 19 10 19 28 „
110.102 = 72 31 72 30 „
110.111 = 21 25 21 2 „
111.112 = 17 appr. 16 32 „

The crystal fig. 3, the right half of which only was developed, could only be measured with the hand-goniometer, and gave

101.10$\bar{1}$ = 74° 71° 56′ calc.
110.21$\bar{1}$ = 25 24 47 „

2 1 $\bar{1}$ being in the zone [1 1 0, 1 0 $\bar{1}$].

Fig. 2.—1 0 0, 0 0 1, 1 1 0, 2 0 1, 1 0 1, 1 0 2.

A large black crystal engaged in reddish felspar from Ytterby. I found with the hand-goniometer,

001.102 = 34° 34° 34′ calc.
102.101 = 19½ 19 58 „
101.201 = 16 16 1 „

Fig. 5.—0 0 1, 1 1 0, 2 1 0, 1 0 2, 1 1 1, 1 1 2, 0 1 4, 3 2 2.

A detached black crystal, of which fig. 6 gives a vertical projection. The symbols of the new planes 0 1 4 and 3 2 2, the latter in the zone [1 1 0, 1 0 2], are deduced from the following approximate measurements:—

0 $\bar{1}$ 4 0 0 1 = 29° 28° 52′ calc.
1 1 0 3 2 2 = 22 21 20 „
1 1 0 2 1 0 = 20½ 19 20 „

Fig. 7.—0 0 1, 1 1 0, 2 1 0, 1 0 1, 1 0 2, 1 1 1, 4 2 3.

The half of a smaller black crystal, which in fig. 8 is represented in vertical projection. The faces reflect the light very badly. The following angles are therefore only very rough approximations:—

110.210 = 17–18° 21° 20′ calc.
111.423 = 16 18 1 „

4 2 3 being in the zone [1 1 1, 1 $\bar{1}$ 0].

Notices of Aërolites. By Nevil Story Maskelyne.

Kusiali, Kumaon.

A small but very interesting fragment of an aërolite was sent to me a few weeks since by my friend Dr. Oldham, the Director of the Geological Survey of India. It weighs 63 grains. It was accompanied by the following statement from Dr. Oldham:—

"Small fragment of meteorite that fell close to Kusiali village, in the district of British Gurhwal (say 30° N. lat., 79° E. long.). There are stated to have been eight or ten explosions nearly half an hour before the stone fell, to have been a strong light like burning gunpowder in the track of the stone, which came (apparently to observers) from the north-north-west to south-south-east.

"The mass is described to have fallen on an open surface of hard gneiss-rock, and to have been shattered into fragments, none of which were larger than the piece I send, most of them much smaller."

The fall took place a few minutes before 5 o'clock A.M., on the 16th of June 1860.

Dr. Oldham adds that there was only one other specimen preserved, and that it has unfortunately been lost. The rest of the fragments were eagerly seized by the natives, who attribute to these sky-stones healing properties, that recall to our minds superstitions that have raised minerals into talismans, from the remotest time down to our own days. It may not be impossible, however, that the iron that is present in these bodies in a state of such minute division may have been found to act medicinally as a tonic.

The strange statement that the fall of the aërolite was preceded, by so long a period as half an hour, by a series of explosions, is one so irreconcilable with any intelligible explanation that should connect the two phenomena, that we may fairly hold ourselves relieved from the attempt to supply such an explanation.

The Kusiali stone is a chondrite, but it belongs to the group of this class which is least charged with spherules.

It is very full of the opake white flocculence which is so abundant in some aërolites and entirely absent from others; and in the interstices of this confused aggregate of mineral are seen crystalline granules that are sometimes complete crystals, but oftener without any geometrical form that can be traced. These generally seem to be olivine, but they are also sometimes in bars, and have occasionally the divergent structure, features which I take to be, in these as in similar cases, characteristic of the augitic and felspathic ingredients of the aërolite. The Labradorite crystals in some Melaphyrs, and especially in a specimen from Darmstadt (among several that M. Kesselmeyer was so good as to forward to me), present a remarkable similarity to some of these bacillary aërolitic minerals, and are similarly associated with a white nearly opake body.

The section of the Kusiali meteorite is sprinkled with small iron-particles, and by Troïlite in a somewhat smaller amount.

Kaee, Oude.

Among the recent acquisitions made to the Collection of Meteorites in the British Museum is a stone presented by Thomas Maclear, Esq., Astronomer Royal at the Cape of Good Hope.

This unique little aërolite was accompanied by a document in Persian, which was "a copy of the original in the intelligence department of the district of Sandee in the Elakar or country of Aga Mirza (in the kingdom of Oude), dated 2nd of Zeekad, 1253 hizree, corresponding with January 29, 1838."

The stone, and the record of its fall, were sent by the King of Oude to Col. Caulfield, Acting Resident at the Oude Court, to be forwarded to the Resident, Col. Low, C.B. Col. Low gave them to the Astronomer Royal at the Cape, to whose liberality the British Museum has been indebted for them, as on previous occasions it had been for specimens of the Cold Bokkeveldt Meteorite.

The certified translation of the Persian document runs as follows:—"To-day, after sunset, though there were no clouds, thunder was heard, and the men were astonished! It was afterwards ascertained that in the village of Kaee, held in nakar (rent-free) by Hidayut Allee, the Taalookdar of the village of Kugtalee, &c., a stone fell from the sky. The aforesaid Taalookdar sent that stone to the reporter of intelligence; its colour is black, and it weighs 17 tollahs and 6 massahs."

This "sky-stone," as it is called in the Persian superscription, is a small complete aërolite, presenting the well-known appearance of an irregular solid, bounded by planes comparatively flat with their edges rounded. The crust had been broken away along two of the edges, and one of these has been worked so as to exhibit a considerable polished surface, in which the characters of the stone may be seen. The weight of the stone is 7 oz. 160 grs. In external appearance the Kaee stone much resembles the aërolites of Doroninsk, Ohaba, and Grünberg, and is, like them, a member of the large Chondritic class of Rose. They present on their polished faces much meteoric iron, in small but thickly sown and pretty equably disseminated grains, rather showing a tendency to agglutination or to being strung together. Spherules remarkably round, and, in section, very dark and brilliant, are plentifully distributed through the mass of the stone, and are associated with others of a lighter hue. The aërolite is very compact and takes a good polish. Its specific gravity is 3·63. It contains Troïlite in about an equal proportion in bulk to the iron, and it is similarly disseminated. In the microscope some of the spherules are seen to consist of the grey mineral, with a fan-like structure radiating from an edge or a point external to the spherule, apparently belonging to one of the oblique systems. There are also the usual kinds of spherule, in which the clear olivine-like mineral is seen forming a sort of breccia, and engaged in a flocculent aggregate of confusedly crystalline matter, from which a ray of polarized light emerges without any definite plane of polarization. Similar ingredients fill the interspherular space, and the aspect of the aërolite in the microscope is that of a pretty uniform mixture of this flocculent aggregate and fragmentary but clear crystals. There are likewise a few crystals or broad clear bars of crystal, unlike the ordinary olivine in their habit, but with their plane of polarization parallel or perpendicular to the direction of the bar. It is not likely to be augite seen in a section parallel to the plane 1 0 0 (and perpendicular to the axis of symmetry); and it is difficult to resist suggesting the probability of its being enstatite, cut probably nearly parallel to one of its planes of cleavage. It is not a rare ingredient of aërolites.

XVII. *Note on the History of the Dynamical Theory of Heat.* *By* James P. Joule, *LL.D., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

SOME observations in Professor Tyndall's "Notes on Scientific History" call for an early notice on my part. After the perusal of this article, I freely admit that I erroneously took the degrees in the fifth column of Séguin's Table for thermal units in kilogrammes. I regret this oversight the more particularly, as it seems to have misled others who have since written on the subject. But I must still express my conviction that in the statement of his hypothesis Séguin anticipated Mayer. To prove this, I will give the following extracts from the *Chemins de Fer* :—

" . . . . . il me paraît plus naturel de supposer qu'une certaine quantité de calorique disparaît dans l'acte même de la production de la force ou puissance mécanique, et réciproquement; et que les deux phénomènes sont liés entre eux par des conditions qui leur assignent des relations invariables. Il résulterait, comme conséquence de cette manière d'envisager les faits, que si l'on fait passer directement de la vapeur d'eau, de la chaudière qui la produit à travers une masse d'eau dans laquelle elle se condense, cette vapeur élèvera plus la température de l'eau que si on la faisait servir préalablement à mettre en jeu une machine à vapeur, dans laquelle elle perdrait une partie de son ressort." (p. 382.)

"L'abaissement de température qui accompagne l'expansion de tout fluide aériforme dans un espace plus grand que celui qui répond au degré de tension où il était d'abord, et le phénomène opposé, ou la production de chaleur qui est toujours la suite de sa compression, me paraissent deux circonstances qui viennent à l'appui de cette assertion." (p. 383.)

"Je vais donc raisonner dans l'hypothèse que l'abaissement de température de la vapeur, lorsqu'elle se dilate, représente exactement la quantité de puissance qui apparaît alors." (p. 384.)

"Enfin l'abaissement subit de température de la vapeur lorsqu'elle s'échappe dans l'air, circonstance mise à profit de nos jours pour utiliser son ressort et sa puissance, montre que, dans ce cas, l'effort qu'elle exerce en recul contre les appareils qui la laissent échapper, ou la vitesse qu'elle communique à l'air ambiant, forment un équivalent de la perte de chaleur qu'elle éprouve." (p. 394.)

Séguin gives data from which the mechanical equivalent of heat may be readily deduced on his hypothesis, the result being too great in consequence of the thermal effect of the compression of vapour being understated. Neither in Séguin's writing of 1839, nor in Mayer's paper of 1842, were there such proofs of the hypothesis advanced as were sufficient to cause it to be admitted into science without further inquiry. I believe that the experiment attributed to Gay-Lussac was not referred to by Mayer previously to the year 1845. Mayer appears to have hastened to publish his views for the express purpose of securing priority. He did not wait until he had the opportunity of supporting them by facts. My course, on the contrary, was to publish only such theories as I had established by experiments calculated to commend them to the scientific public, being well convinced of the truth of Sir J. Herschel's remark, that "hasty generalization is the bane of science."

I applied the dynamical theory to steam-engines, to electro-magnetic engines, to vital processes, and to chemistry in 1843. In the postscript alluded to in § 50 of Mr. Tyndall's article, I intended the word "apprehend" to express the meaning applied to it in Johnson's Dictionary, viz. "to conceive by the mind," not "to conjecture*". My popular lecture will show that the outlines of cosmical and other applications of the theory were so

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* [With respect to this passage, we had, before receiving Dr. Joule's communication, been requested by Prof. Tyndall, in a letter dated Pontresina, July 18, to substitute the word "statement" for "conjecture".—W. F.]

familiar to my mind, and such obvious deductions from what I had established, that I did not deem them of sufficient novelty to present them to our own local scientific Society.

I believe that no one who has the advantage of acquaintance with Prof. Thomson will for a moment doubt his eagerness to give due credit to M. Mayer's just claims. But has justice been done to Thomson himself, who has done far more than any other individual towards the application, development, and promulgation of the dynamical theory of heat? I fear not.

I am, Gentlemen,
Yours very respectfully,
James P. Joule

XVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 71.]

May 26, 1864.—Major-General Sabine, President, in the Chair.

THE following communication was read:—

"On the Spectra of some of the Fixed Stars." By W. Huggins, F.R.A.S., and William A. Miller, M.D., LL.D.

After a few introductory remarks, the authors describe the apparatus which they employ, and their general method of observing the spectra of the fixed stars and planets. The spectroscope contrived for these inquiries was attached to the eye end of a refracting telescope of 10 feet focal length, with an 8-inch achromatic object-glass, the whole mounted equatorially and carried by a clock-movement. In the construction of the spectroscope, a plano-convex cylindrical lens, of 14 inches focal length, was employed to convert the image of the star into a narrow line of light, which was made to fall upon a very fine slit, behind which was placed an achromatic collimating lens. The dispersing portion of the arrangement consisted of two dense flint-glass prisms; and the spectrum was viewed through a small achromatic telescope with a magnifying power of between 5 and 6 diameters. Angular measures of the different parts of the spectrum were obtained by means of a micrometric screw, by which the position of the small telescope was regulated. A reflecting prism was placed over one half of the slit of the spectroscope, and by means of a mirror, suitably adjusted, the spectra of comparison were viewed simultaneously with the stellar spectra. This light was usually obtained from the induction spark taken between electrodes of different metals. The dispersive power of the apparatus was sufficient to enable the observer to see the line *Ni* of Kirchhoff between the two solar lines D; and the three constituents of the magnesium group at *b* are divided still more evidently*. Minute details of the methods adopted for testing the exact coincidence of the corresponding metallic

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* Each unit of the scale adopted was about equal to $\frac{1}{1800}$th of the distance between A and H in the solar spectrum. The measures on different occasions of the same line rarely differed by one of these units, and were often identical.

lines with those of the solar and lunar spectrum, are given, and the authors then proceed to give the results of their observations.

Careful examination of the spectrum of the light obtained from various points of the moon's surface failed to show any lines resembling those due to the earth's atmosphere. The planets Venus, Mars, Jupiter, and Saturn were also examined for atmospheric lines, but none such could be discovered, though the characteristic aspect of the solar spectrum was recognized in each case; and several of the principal lines were measured, and found to be exactly coincident with the solar lines.

Between forty and fifty of the fixed stars have been more or less completely examined; and tables of the measures of about 90 lines in Aldebaran, nearly 80 in α Orionis, and 15 in β Pegasi are given, with diagrams of the lines in the two stars first named. These diagrams include the results of the comparison of the spectra of various terrestrial elements with those of the star. In the spectrum of Aldebaran coincidence with nine of the elementary bodies were observed, viz. sodium, magnesium, hydrogen, calcium, iron, bismuth, tellurium, antimony, and mercury; in seven other cases no coincidence was found to occur.

In the spectrum of α Orionis five cases of coincidence were found, viz. sodium, magnesium, calcium, iron, and bismuth, whilst in the case of ten other metals no coincidence with the lines of this stellar spectrum was found.

β Pegasi furnished a spectrum closely resembling that of α Orionis in appearance, but much weaker: only a few of the lines admitted of accurate measurement, for want of light; but the coincidence of sodium and magnesium was ascertained; that of barium, iron, and manganese was doubtful. Four other elements were found not to be coincident. In particular, it was noticed that the lines C and F, corresponding to hydrogen, which are present in nearly all the stars, are wanting in α Orionis and β Pegasi.

The investigation of the stars which follow is less complete, and no details of measurement are given, though several points of much interest have been ascertained.

Sirius gave a spectrum containing five strong lines, and numerous finer lines. The occurrence of sodium, magnesium, hydrogen, and probably of iron, was shown by coincidence of certain lines in the spectra of these metals with those in the star. In *α Lyræ* the occurrence of sodium, magnesium, and hydrogen was also shown by the same means. In *Capella* sodium was shown, and about twenty of the lines in the star were measured. In *Arcturus* the authors have measured about thirty lines, and have observed the coincidence of the sodium line with a double line in the star-spectrum. In *Pollux* they obtained evidence of the presence of sodium, magnesium, and probably of iron. The presence of sodium was also indicated in *Procyon* and *α Cygni*.

In no single instance have the authors ever observed a star-spectrum in which lines were not discernible, if the light were sufficiently intense, and the atmosphere favourable. Rigel, for instance, which some authors state to be free from lines, is filled with a multitude of fine lines.

Photographs of the spectra of Sirius and Capella were taken upon collodion; but though tolerably sharp, the apparatus employed was not sufficiently perfect to afford any indication of lines in the photograph.

In the concluding portion of their paper, the authors apply the facts observed to an explanation of the colours of the stars. They consider that the difference of colour is to be sought in the difference of the constitution of the investing stellar atmospheres, which act by absorbing particular portions of the light emitted by the incandescent solid or liquid photosphere, the light of which in each case they suppose to be the same in quality originally, as it seems to be independent of the chemical nature of its constituents, so far as observation of the various solid and liquid elementary bodies, when rendered incandescent by terrestrial means, appears to indicate.

June 16.—Major-General Sabine, President, in the Chair.

The following communications were read:—

"Aërial Tides." By Pliny Earle Chase, A.M., S.P.A.S.

The remarkable coincidence which I have pointed out* between the theoretical effects of rotation and the results of barometrical observations, has led me to extend my researches with a view of defining more precisely some of the most important effects of lunar action on the atmosphere. The popular belief in the influence of the moon on the weather, which antedates all historical records, has received at various times a certain degree of philosophical sanction. Herschel and others have attempted partially to formulate that influence by empirical laws, but the actual character of the lunar wave that is daily rolled over our heads, appears never to have been investigated.

Major-General Sabine has shown that the moon produces a diurnal variation of the barometer, amounting to about ·006 of an inch at St. Helena, which is nearly equivalent to $\frac{1}{10}$ of the average daily variation (Phil. Trans. 1847, Art. V.). This would indicate a tidal wave of rather more than 1 ft. for each mile that we ascend above the earth's surface, or from 3 to 6 ft. near the summits of the principal mountain-chains. It is easy to believe that the rolling of such a wave over the broken surface of the earth may exert a very important influence on the atmospheric and magnetic currents, the deposition of moisture, and other meteorological phenomena. As the height of the wave varies with the changing phases of the moon†, its effects must likewise vary in accordance with mathematical laws, the proper study of which must evidently form an important branch of meteorological science.

Besides this daily wave, there appears to be a much larger, but hitherto undetected, weekly wave. M. Flangergues‡, an astronomer at Viviers in France, extended his researches through a whole lunar cycle, from Oct. 19, 1808 to Oct. 18, 1827, and he inferred from his observations—

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* See Proceedings of Amer. Philos. Soc. vol. ix. p. 283.

† The height at St. Helena appears to fluctuate between about ·9 and 1·6 ft.

‡ Bib. Univ., Dec. 1827.

1. That in a synodical revolution of the moon, the barometer rises regularly from the second octant, when it is the lowest, to the second quadrature, when it is the highest, and then descends to the second octant.

2. That the varying declination of the moon modifies her influence, the barometer being higher in the northern lunistice than in the southern.

The more recent and more complete observations at St. Helena give somewhat different results, which serve to confirm the natural *à priori* conviction that there are two maxima and two minima in each month. The means of three years' hourly observations, indicate the existence of waves which produce in the first quarter a barometric effect of +·004 in., in the second quarter of —·016 in., in the third quarter of +·018 in., and in the fourth quarter of —·006 in.—results which appear to be *precisely* accordant, in their general features, with those which would be naturally anticipated from the combination of the cumulative action of the moon's attraction, with the daily wave of rotation, and the resistance of the æther.

One peculiarity of the lunar-aërial wave deserves attention, for the indirect confirmation that it lends to the rotation theory of the aërobaric tides, and the evidence it furnishes of opposite tidal effects, which require consideration in all investigations of this character. When the daily lunar tides are highest, their pressure is greatest, the lunar influence accumulating the air directly under the meridian, so as to more than compensate for the diminished weight consequent upon its "lift." But in the general aërial fluctuations, as we have seen heretofore, and also in the weekly tides which we are now considering, a high wave is shown by a low barometer, and *vice versâ*. The daily blending of heavy and light waves produces oscillations which are indicated by the alternate rise and fall of the barometer and thermometer at intervals of two or three days.

M. Flangergues's observations at perigee and apogee seem to show that a portion of the movement of the air by the moon is a true lift, which, like the lift of rotation, must probably exert an influence on the barometer. On comparing the *daily* averages at each of the quadratures and syzygies, I found the difference of temperature too slight to warrant any satisfactory inference, but a similar comparison of the *hourly* averages, at hours when the sun is below the horizon, gave such results as I anticipated; as will be seen by a reference to the following

Table of Barometric and Thermometric Means at the Moon's Changes.

Moon's Phase.	Average Height of Barometer, in inches.	Height of Lunar Weekly Tides.	Height of Lunar Daily Tides.	Daily Height of Thermometer.	Thermometer at 12 P.M.	Thermometer at 4 A.M.
		in.	in.	°	°	°
Full	28·270	—·0115	·0054	67·67	60·22	59·787
Third Quarter	28·289	+·0065	·0087	61·68	60·41	59·824
New	28·282	+·0005	·0064	61·65	60·31	59·716
First Quarter	28·286	+·0044	·0047	61·63	60·37	59·823

In obtaining the above averages, I was obliged to interpolate for such changes as took place on Sundays or holidays, when no observations were taken. The interpolation, however, does not change the general result, and on some accounts the Table is more satisfactory than if the observations had been made with special reference to the determination of the lunar influences, accompanied, as such a reference would very likely have been, by a bias to some particular theory.

The thermometric and barometric averages show a general correspondence in the times of the monthly maxima and minima,—the correspondence being most marked and uniform at midnight, when the air is most removed from the direct heat of the sun, and we might therefore reasonably expect to find the strongest evidences of the relations of temperature to lunar attraction.

By taking the difference between the successive weekly tides, we readily obtain the amount of barometric effect in each quarter. The average effect is more than three times as great in the third and fourth quarters as in the remaining half-month,—a fact which suggests interesting inquiries as to the amount of influence attributable to varying centrifugal force, solar conjunction or opposition, temperature, &c.

Although, as in the ocean tides, there are two simultaneous corresponding waves on opposite sides of the earth, those waves are not of equal magnitude, the barometer being uniformly higher when the moon is on the inferior meridian, and its attraction is therefore exerted in the same direction as the earth's, than when it is on the superior meridian, and the two attractions are mutually opposed. Some of the views of those who are not fully satisfied with the prevailing theory of the ocean-tides, derive a partial confirmation from this fact.

I find, therefore, marked evidences of the same lunar action on the atmosphere as on the ocean, the combination of its attraction with that of the sun producing both in the air and water, spring tides at the syzygies, and neap tides at the quadratures; and I believe that the most important normal atmospheric changes may be explained by the following theory:—

The attraction- and rotation-waves, as will be readily seen, have generally opposite values, the luni-solar wave being

Descending, from 0° to 90°* and from 180° to 270°,
Ascending, from 90° to 180° and from 270° to 0°;

while the rotation-wave is

Ascending, from 330° to 60° and from 150° to 240°,
Descending, from 60° to 150° and from 240° to 330°.

From 60° to 90° and from 240° to 270°, both waves are descending, while from 150° to 180° and from 330° to 360° both are ascending. In consequence of this change of values, besides the principal maxima and minima at the syzygies and quadratures, there should be secondary maxima and minima† at about 60° in advance of those points.

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* Counting θ from either syzygy.

† The secondary maxima and minima should correspond with the daily maxima and minima, which occur at St. Helena at about 4^h and 10^h A.M. and P.M., giving $\theta=60°$ a maximum, and $\theta=150°$ a minimum.

The confirmation of these theoretical inferences by the St. Helena observations appears to me to be quite as remarkable as that of my primary hypothesis. If we arrange those observations in accordance with the moon's position, and take the average daily height of the barometer, we obtain the following

Table of the Lunar Barometric Tides.

Moon's Position.	Mean Daily Height of the Barometer at St. Helena, 28 inches + the numbers in the Table.			
	1844.	1845.	1846.	1844–6. Average.
0°	·2621	·3020	·2701	·2781
15	·2650	·3058	·2693	·2800
30	·2707	·3153	·2707	·2856
45	·2691	·3165	·2688	·2848
60	·2625	·3077	·2688	·2797
75	·2682	·3093	·2783	·2853
90	·2667	·3184	·2800	·2884
105	·2593	·3170	·2721	·2828
120	·2595	·3124	·2686	·2802
135	·2677	·3099	·2691	·2822
150	·2712	·3118	·2715	·2848
165	·2710	·3104	·2735	·2850
180	·2621*	·3020	·2701	·2781

This Table shows—

1. That the average of the three years corresponds *precisely* with the theory, except in the secondary maximum, which is one day late.
2. That the primary maximum occurred at the quadratures in 1845 and 1846, and one day earlier in 1844.
3. That the primary minimum occurred at the syzygies in 1844 and 1845, and one day later in 1846.
4. That 1846 was a disturbed year; and if it were omitted from the Table, each of the remaining years, as well as the average, would exhibit an entire correspondence with theory, except in the primary maximum of 1844.
5. That 1845 was a normal year, the primary and secondary maxima and minima all corresponding with theory, both in position and relative value.

"On the Microscopical Structure of Meteorites." By H. C. Sorby, F.R.S., &c.

For some time past I have endeavoured to apply to the study of meteorites the principles I have made use of in the investigation of terrestrial rocks, as described in my various papers, and especially in that on the microscopical structure of crystals (Quart. Journ. Geol. Soc. 1858, vol. xiv. p. 453). I therein showed that the presence in crystals of "fluid-, glass-, stone-, or gas-cavities" enables us to determine in a very satisfactory manner under what conditions the crystals were formed. There are also other methods of inquiry still requiring much investigation, and a number of experiments must

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* Since the tabular numbers represent the *semiaxes* of the barometric curve, and not the simple *ordinates*, the values for 0° and 180° are the same.

be made which will occupy much time; yet, not wishing to postpone the publication of certain facts, I purpose now to give a short account of them, to be extended and completed on a subsequent occasion*.

In the first place it is important to remark that the olivine of meteorites contains most excellent "glass-cavities," similar to those in the olivine of lavas, thus proving that the material was at one time in a state of igneous fusion. The olivine also contains "gas-cavities," like those so common in volcanic minerals, thus indicating the presence of some gas or vapour (Aussun, Parnallee). To see these cavities distinctly, a carefully prepared thin section and a magnifying power of several hundreds are required. The vitreous substance found in the cavities is also met with outside and amongst the crystals, in such a manner as to show that it is the uncrystalline residue of the material in which they were formed (Mezö-Madaras, Parnallee). It is of a claret or brownish colour, and possesses the characteristic structure and optical properties of artificial glasses. Some isolated portions of meteorites have also a structure very similar to that of stony lavas, where the shape and mutual relations of the crystals to each other prove that they were formed *in situ,* on solidification. Possibly some entire meteorites should be considered to possess this peculiarity (Stannern, New Concord), but the evidence is by no means conclusive, and what crystallization has taken place *in situ* may have been a secondary result; whilst in others the constituent particles have all the characters of broken fragments (L'Aigle). This sometimes gives rise to a structure remarkably like that of consolidated volcanic ashes, so much, indeed, that I have specimens which, at first sight, might readily be mistaken for sections of meteorites. It would therefore appear that, after the material of the meteorites was melted, a considerable portion was broken up into small fragments, subsequently collected together, and more or less consolidated by mechanical and chemical actions, amongst which must be classed a segregation of iron, either in the metallic state or in combination with other substances. Apparently this breaking up occurred in some cases when the melted matter had become crystalline, but in others the forms of the particles lead me to conclude that it was broken up into detached globules whilst still melted (Mezö-Madaras, Parnallee). This seems to have been the origin of some of the round grains met with in meteorites; for they occasionally still contain a considerable amount of glass, and the crystals which have been formed in it are arranged in groups, radiating from one or more points on the external surface, in such a manner as to indicate that they were developed after the fragments had acquired their present spheroidal shape (Aussun, &c.). In this they differ most characteristically from the general type of concretionary globules found in terrestrial rocks, in which they radiate from the centre; the only case that I know at all analogous being that of certain oolitic grains in the Kelloways rock at Scarborough, which have undergone a secondary crystallization. These facts are all quite independent of the fused black crust.

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* The names given thus (Stannern) indicate what meteorites I more particularly refer to in proof of the various facts previously stated.

Some of the minerals in meteorites, usually considered to be the same as those in volcanic rocks, have yet very characteristic differences in structure (Stannern), which I shall describe at greater length on a future occasion. I will then also give a full account of the microscopical structure of meteoric iron as compared with that produced by various artificial processes, showing that under certain conditions the latter may be obtained so as to resemble very closely some varieties of meteoric origin (Newstead, &c.).

There are thus certain peculiarities in physical structure which connect meteorites with volcanic rocks, and at the same time others in which they differ most characteristically,—facts which I think must be borne in mind, not only in forming a conclusion as to the origin of meteorites, but also in attempting to explain volcanic action in general. The discussion of such questions, however, should, I think, be deferred until a more complete account can be given of all the data on which these conclusions are founded.

GEOLOGICAL SOCIETY.

[Continued from p. 75.]

May 11, 1864.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "On a Section with Mammalian Remains near Thame." By T. Codrington, Esq., F.G.S.

A railway-cutting through a hill between Oxford and Thame having exposed a section of certain gravel-beds, from which many Mammalian remains were collected, the author now gave a short description of the section and a list of the bones he had obtained from it. The hill is nearly surrounded by the Thame and two small tributaries, and consists of Kimmeridge clay capped by a bed of coarse gravel overlain by sandy clay. The gravel consists of chalk-flints, pebbles derived from the Lower Greensand, and fragments of mica-schist, &c., indicating a northern-drift origin; it contained many bones of Elephant, Rhinoceros, Horse, Ox, and Deer, and a single phalanx of a small carnivore, but no flint implements were discovered.

2. "On a Deposit at Stroud containing Flint Implements, Land and Freshwater Shells, &c." By E. Witchell, Esq., F.G.S.

In the construction of a reservoir near the summit of the hill above the town of Stroud, the author observed, about two feet from the surface, a deposit of tufa containing Land-shells, with a few freshwater Bivalves; in it he subsequently discovered several flint-flakes of a primitive type, and in the overlying earth a few pieces of rude pottery.

3. "On the White Limestone of Jamaica, and its associated intrusive rocks." By A. Lennox, Esq., F.G.S., late of the Geological Survey of Jamaica.

The White Limestone of Jamaica was described as including a basement series of sandstones and shales, a hard white limestone, a yellowish limestone, and an uppermost member consisting of dark-red marl; it was estimated to be at least 2500 feet thick; and the author stated that, at the junction of the calcareous rock with the granite, the former was often more or less altered; and this appeared to be the best proof of the Tertiary age of the latter.

Mr. Lennox then adverted to a diagram-section of the rock-formations of Jamaica by the late Mr. Barrett (Quart. Journ. Geol. Soc. vol. xix. p. 515), which he considered erroneous on the following grounds:—(1) he knows no section in Jamaica in which the relation of the White Limestone to the Hippurite-limestone is seen; (2) the White Limestone he believes to be of Miocene age; and (3) the shaly and sandy beds represented in the section as overlying the White Limestone he considers to be undoubtedly in infraposition.

The author then discussed the question of the age of the White Limestone, first on physical grounds, and afterwards palæontologically, inferring that it was decidedly of Miocene date; and in conclusion he remarked that the White Limestone had probably been deposited slowly in a tranquil sea, and discussed its relation to the Tertiary beds of the other West Indian Islands.

4. "Facts and Observations connected with the Earthquake which occurred in England on the morning of the 6th of October, 1863." By Fort-Major T. Austin, F.G.S.

Earthquakes in the British Isles attract usually but little notice, owing probably to the mild form in which they generally occur; but the one treated of in this paper, owing to its greater violence, aroused attention to the subject. The disturbance was said to extend from a point in St. George's Channel forty or fifty miles to the north-west of Pembrokeshire to Yorkshire, and the focus of the disturbance to be situated near the former spot. The author brought forward a number of facts for the purpose of proving the intensity of the shock, the time at which it occurred, the number of vibrations, their direction (which was considered to be from W.N.W. to E.S.E.), and the occurrence of incidental phenomena, and concluded by passing in review the natural causes competent to produce these and other characteristics of earthquakes.

XIX. *Intelligence and Miscellaneous Articles.*

APJOHN'S 'MANUAL OF THE METALLOIDS.'

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

HAVING been many years since an occasional, though, I fear, a not very valuable contributor to the pages of the Philosophical Magazine, or rather of its predecessor, the Annals of Philosophy, I trust you will permit me to make a few remarks on the review of my volume on the Metalloids which has appeared in the last Number of your Journal. In these remarks you will, I hope not, find anything uncandid, or exceeding the just limits of scientific discussion. I am quite aware how unreasonable it would be to occupy your pages with a lengthened paper on questions chiefly relating to myself, and shall therefore make my notice of the review as brief as possible.

In page 68 of my book, with a view to a brief explanation for the Tyro in chemistry of what is meant by a binatomic body, a brief statement is made which is certainly not correct. In point of fact the reaction described between Dutch liquor and water has not been observed; but, assuming it to take place, we have a simple explanation of what is meant by a triatomic organic radical; and what was in my mind when writing this passage was, to speak hypothetically of the reaction, though this I find has not been done. I can assure the reviewer that I was not ignorant of the series of processes by which biniodide of ethylene is convertible into glycol, and the latter into the biniodide of ethylene; but the description of these would have been quite unintelligible to the junior student, and, at the same time, altogether unsuited to my introductory chapter.

In pages 561 and 562 I give an explanation of Liebig's well-known method for the estimation of hydrocyanic acid, in relation to which the reviewer has the following passage:—"It is difficult to suppose that the writer of this passage has ever performed the operation he professes to describe, otherwise he could hardly have failed to notice that in reality it is the white cyanide of silver, and not the brown-grey oxide which appears as a permanent precipitate." In reply to this commentary, I beg to state that the inference suggested would be quite erroneous. The experiment in question is one which, as a teacher of chemistry, I am obliged repeatedly to perform; and I may add that, in doing so, I have not failed to observe the white precipitate of which he makes mention. This, however, or rather the portion of it which first falls, is not cyanide, as alleged in the review, but chloride of silver, arising from the alkaline chlorides invariably present in the ordinary solutions of soda and potash. If the solution of caustic potash was absolutely pure, it appeared to me a probable conclusion that the precipitate would be oxide of silver, and hence it has been described as such; but I am now by actual experiment aware that this is not the case, and that, after the whole of the hydrocyanic acid has been converted into the soluble double cyanide, it may be thrown down as insoluble cyanide of silver by adding just as many measures (chloride being supposed absent) of the volumetric solution of nitrate of silver as have been already employed. The ignorance on this latter point, which I freely avow, the reviewer no doubt considers as quite unpardonable; but I am not without hope that the candid critic will feel himself at liberty to pronounce a more lenient judgment, the more especially as a knowledge of the nature of the permanent precipitate which appears in this volumetric process is not necessary with a view to the accuracy of the experiment. Chemical reactions cannot be always predicted with certainty; and the charge against me simply amounts to this, that, not having made the trial, I did not know that the cyanide of potassium of the double cyanide would be precipitated by nitrate of silver.

The next charge preferred against me is one of rather a paradoxical nature, being actually that I have stated the compounds of nitrogen with hydrogen to be three in number, viz. amidogen, ammonia, and ammonium,—of which he says that "a more misleading statement could not easily be put before the student." As a further illustration of my trangressions in this direction, the offence is imputed to me of enumerating seven oxides of sulphur, and six of carbon. On these points I decline to enter upon any defence, further than to say that, in the best works on chemistry recently published, precisely the same enumeration of these compounds has been made. If I have sinned against scientific accuracy, of which I must say I am quite unconscious, precisely the same offence has been committed by modern chemists of great eminence, such as Pelouze and Fremy, Regnault, Miller, and several others; and I am at a loss to understand why so humble an individual as myself should be singled out for especial censure, while my distinguished contemporaries are permitted to pass unscathed. Many of the atomic groups here referred to have certainly not as yet been insulated, and are therefore to a certain extent hypothetic; but this does not appear to me to constitute any sufficient reason why their constitution and properties, as far as these can be deduced from the known compounds in which they occur, should not be considered and discussed.

Of a somewhat similar character are the remarks upon my mode of dealing with the compounds of carbon and hydrogen. In page 502 I state that these numerous bodies form properly a part of organic chemistry, but that there are a few of them, in particular $C_4 H_4$ and $C_2 H_4$, to which the student should pay an early attention. Notwithstanding this distinct announcement of the plan which I intended to pursue, and the fact that in all almost all treatises on chemistry olefiant gas and marsh-gas alone are discussed in connexion with carbon, the reviewer does not hesitate to use the following language:—"From these instances it will be seen that Dr. Apjohn's lists of known compounds include many substances with which other chemists are by no means well acquainted: hence it is natural he should require to make room for them by ignoring substances which often receive a considerable share of attention. Thus (page 502) the usual list of hydrocarbons is much curtailed." Of the tone of this extract, and there are several other passages in the review on a level with it, I do not complain. Some persons think that in every kind of controversy a sarcasm or a sneer constitutes the most effective weapon, and therefore naturally resort to them. I would suggest, however, that there is no sufficient excuse for the misstatement or distortion of facts exemplified in the passage which I have just quoted.

In the remainder of the review there is a good deal of criticism of a hostile nature, upon the details of which it is not my intention to enter. I am quite prepared to admit that in my book there are some omissions, and that in particular the interesting compounds of silicon with hydrogen and with oxygen, discovered by Wöhler and Buff, have not been noticed. This was not an intentional omission; for the manuscript was prepared, but, through some accident, not forwarded to the printer. There are also other errors which I have no disposition to deny or justify. It is, for example, quite true that the rival claims of Watt and Cavendish had reference, not to the discovery of hydrogen, but to a kindred question—the discovery of the composition of water; and that the combustion of the diamond by Lavoisier and Morveau was effected, not seventy, but about eighty years after the analogous experiment of the Florentine academicians. I am also quite aware that the spelling of the names of foreign chemists is sometimes erroneous, and that in a few instances, as a consequence of the omission of certain words, the composition has been rendered faulty. These errors, however, are, I believe, of small consequence; nor is their number unusually great, considering that the volume extends to about 500 pages, and that, from much preoccupation of time by other duties, I was unfortunately unable to pay sufficient attention to the correcting of the press. Such as they are, they, or at least the majority of them (some will no doubt escape detection), have been carefully noted by myself and others (I have had, for example, for a considerable time on my list of *errata* all those, with a single exception, animadverted on by the reviewer, to which I attach any importance); and from the next edition, which will, I presume, shortly appear, as the book is very nearly out of print, they will of course be excluded.

Though very unwilling to add to the length of this communication, I must not omit adverting briefly to the criticism which has been passed on the few remarks made by me in page 124 on the origin of the heat and light attendant on combustion. These remarks are pronounced by the reviewer to contain not only "a totally inadequate and therefore erroneous statement" of Lavoisier's views, but to imply "what is directly contrary to facts well known to all who have paid any attention to the history of chemistry—namely, that the phlogistic theory held its ground long after it had been discovered that combustible bodies increase in weight when burned, and that this observation first came to be regarded as an objection to the theory when it was shown by Lavoisier to be connected with the disappearance of part of the atmosphere in which combustion takes place."

In reply to this, I beg to say that the experiments of Ray and Mayow constituted in my mind a full refutation of the Stahlian theory; for, after their publication, it became impossible to maintain it except upon the absurd assumption that a principle existed in inflammable bodies which conferred upon them levity instead of weight. The abandonment indeed of a prevalent theory is, I am aware, a slow process; and it is quite possible that, notwithstanding its obvious absurdity, the phlogistic hypothesis continued to be employed by some chemists until oxygen, and the part which it plays in all ordinary cases of combustion, were discovered by Lavoisier. It should, however, be recollected that I did not contemplate anything like a complete discussion of the theories of combustion. This is a topic merely glanced at incidentally in my 'Manual;' and my method of handling it may possibly be, with justice, described as "inadequate." I deny that it is "erroneous." I also allege that my statement of Lavoisier's views is substantially accurate; but, instead of bandying contradictions with my anonymous adversary, I would solicit the attention of the reader to the following extract from vol. i. p. 184, of the eighth edition of Turner's 'Chemistry,' brought out under the supervision of Liebig and Gregory:—

"To account for the production of heat and light during combustion, Lavoisier had recourse to Black's theory of latent heat. Heat is always evolved when a substance, without change of form, passes from a rarer into a denser state, and also when a gas becomes liquid or solid, or a liquid solidifies, because a quantity of heat previously combined or latent within it is then set free. Now this is precisely what happens in many instances of combustion. Thus water is formed by the burning of hydrogen, in which case two gases give rise to a liquid; and in forming phosphoric acid with phosphorus, or in oxidizing metals, oxygen is condensed into a solid. When the product of the combustion is gaseous, as in the burning of charcoal, the evolution of heat is ascribed to the circumstance that the oxidized body contains a smaller quantity of combined heat, or has a smaller specific heat than the substance by which it is produced."

This passage, combined with that which immediately succeeds it, and which I abstain from quoting merely because of its length, enunciates very distinctly the views which I have ascribed to Lavoisier in relation to the origin of heat and light which accompany combustion. If they are mistaken views, I have, at all events, the consolation of having fallen into error in good company.

I may, in conclusion, observe that I fear I have suffered much in the estimation of the reviewer by continuing to employ the *dualistic* * methods of explanation, and the atomic weights long in use among chemists, instead of those of the unitary system, of which, I make no doubt, he is a zealous advocate. In my introductory chapter I have explained my motives for adhering, at least for the present, to the equivalents and the language of Berzelius and Davy, and I regret that before entering on the details of the work, he did not, as it were, lay the axe to the root, and point out the errors I may have committed in such fundamental discussion. If convicted of any inaccuracies, either of statement or inference, in relation to the system of the illustrious Gerhardt, I trust I have given sufficient proof that I would be prepared to admit them, and, as far as possible, atone for them.

James Apjohn, M.D., F.R.S., M.R.I.A.,
Professor of Chemistry in the University of Dublin.

Out of deference to Professor Apjohn's wishes, we have adopted the somewhat unusual course of publishing his reply to the criticisms on his work entitled 'A Manual of the Metalloids,' which appeared in the last Number of this Journal. Upon this reply, the reviewer claims the right of adding the few following remarks:—

Professor Apjohn's work is intended for the use of junior students in chemistry, and comes before them, not only with the prestige conferred upon it by the distinguished position of its author, but with the additional advantage of being published as one of a series

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* This phrase is at present sometimes used as one of reproach by gentlemen who have studied chemistry in a German school, and who are quite satisfied with the fictions of the unitary system.

of educational manuals which, as we have already stated, enjoys the recommendation of the Committee of Council on Education. On examining the book, we found that it fell very far short of what an introductory text-book of Chemistry ought, in our judgment, to be; and we felt it needful to express our opinion of it all the more emphatically, since we thought it probable that the circumstances of its publication would secure for it a considerable circulation, quite independently of its intrinsic merits. Dr. Apjohn's statement that the first edition is already nearly out of print, confirms us in this supposition. We did not, however, condemn the work in vague and general terms. We stated as clearly as we were able the grounds of our objection to it, quoting literally several passages in support of our charges, and accompanying the quotations with precise references to the pages where they might be found in the original. But notwithstanding all our care and our anxiety to quote fairly and correctly, Professor Apjohn brings against us the charge of "misstatement or distortion of facts." This charge is made with reference to what is said (p. 61 of the review) respecting Dr. Apjohn's treatment of the compounds of carbon and hydrogen. In the review it is stated that "the usual list of hydrocarbons is very much curtailed" by our author; and this passage is followed by a quotation from the book under review, which we conceive fully bears out our assertion. Professor Apjohn now states that, in writing that passage, he had in view only those compounds of carbon and hydrogen which are usually treated of among inorganic compounds. In answer to this, we can only say that, whatever the author's intention may have been, nothing of this kind is apparent in the book itself: the passage we have quoted is the opening passage of the section devoted to these compounds, and is immediately preceded by the heading CARBO-HYDROGENS in small capitals. The following is the whole of the first paragraph of this section, and we leave the reader to judge whether a charge of "misstatement or distortion" in connexion with it is most applicable to Professor Apjohn or to ourselves:—

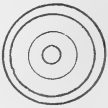
"The number of compounds of carbon and hydrogen is very great. Those at present known are reducible to three groups:—Those whose general formula is $C_n H_n$, those represented by $C_n H_{n+1}$, and those by $C_n H_{n+2}$, n being always an even number. The subject of these hydrocarbons belongs properly to organic chemistry, but there are a few of them to which the student must direct an early attention. Those which will be considered here are only two in number, and belong, one to the first, the other to the third group. The former is called olefiant, the latter marsh-gas."

Professor Apjohn also takes exception to what we have said in reference to the history of the antiphlogistic theory of combustion. In reply, we have only to state that we were concerned, not with what *he* regards as "a full refutation of the Stahlian theory," but with what was regarded as such a refutation by the contemporaries of Ray and Mayow and of Lavoisier; and that we objected to his statement of Lavoisier's theory of combustion, not because we conceived that he has wrongly represented Lavoisier's views with respect to the production of light and heat, but because it seems to us to be implied in the passage that we quoted that this theory extended to the explanation of the light and heat of combustion only.

We do not think it needful to add anything in defence or explanation of our other criticisms, since Dr. Apjohn himself admits their justice; but with regard to his statement that he had detected several of the faults which we have pointed out, and that they will be corrected in a new edition, we may remind him that, had this statement accompanied the copy of his book sent for review, we should probably have contented ourselves with laying it before our readers and recommending any who intended purchasing the work to wait for the second edition. And since we now understand that a new edition is likely soon to appear, we may further remind him that his having had on his list of *errata* all the errors, "with a single exception," pointed out in our review, is no proof of the completeness of his revision, for we made no attempt to enumerate all the faults we had detected: our quotations were, as we have already said, intended merely as specimens to prove that our criticisms were not made without good reason; and in case of need we are quite prepared to produce at least as many more of very similar quality.

ON THE MEASUREMENT OF THE CHEMICAL BRIGHTNESS OF VARIOUS PORTIONS OF THE SUN'S DISK. BY THOMAS WOODS, M.D.

Professor Roscoe's paper "On the Measurement of various portions of the Sun's Disk," read to the Royal Society, an abstract of which appears in the May Number of this Magazine, is extremely interesting. The chemically active rays decrease in intensity from the centre to the circumference, which the Professor found by exposing a prepared paper in a camera to the action of the sun's picture, and comparing the shade of tint produced thereby at the centre and at the circumference with a certain standard. I would, however, suggest the plan I described in this Magazine in July 1854. It consists in exposing the prepared paper to the sun's picture in the camera for a period so short that the centre or most active rays only have time to act on it; then for the next impression to leave the paper exposed for a somewhat longer time, so that a somewhat larger picture is obtained; and so on until the entire picture is given. For instance, suppose the sun's picture is divided into zones by concentric circles thus, and suppose the centre rays could affect the prepared paper in *one* second, the second zone in *two* seconds, the third in *three* seconds, and the circumference in *four*; then by exposing the paper for these periods of time a corresponding amount of the disk would be obtained; the size of the impression produced would be in proportion to the time of exposure; and the intensity of the rays from any part of the disk would be more accurately fixed by once getting the *time* required for their action, and more permanently, I fancy, than by the use of the standard tints. This was the plan I adopted in 1854 to show the identity



of the sun's action on a photographic surface with that of flame, the centre rays of the latter being also more intense in chemical action than those at the circumference.

Parsonstown, July 1864.

ON THE COLOURING-MATTER OF EMERALDS.

BY MM. WÖHLER AND G. ROSE.

Vauquelin, after having discovered oxide of chrome in the emerald, attributed to this oxide the green colour of the stone. In 1858 M. Lewy published very interesting researches on the deposit, the formation, and the composition of the emeralds of Muso in New Granada. His conclusion is that the colour is due to an organic substance, the existence of which he proved by very accurate experiments. Thus he says that the green colour disappears when the emeralds are heated to redness. As we were unable to verify this assertion in the blowpipe experiments to which we subjected the emerald, M. Rose and myself exposed a piece of Muso emerald, weighing 7 grammes and of a deep green colour, for an hour to the temperature of melting copper. The colour did not disappear; the specimen simply became opake. Yet it had lost 1·62 per cent. of its weight, which agrees closely with the numbers given by M. Lewy. On analysis the above specimen gave 1·186 per cent. of its weight of oxide of chrome. M. Lewy thinks that such a small quantity of oxide is insufficient to communicate so pronounced a tint to emerald.

To settle this question, we melted 7 grammes of colourless glass with 13 milligrammes of oxide of chrome. We thus obtained a transparent homogeneous glass of a green colour, identical with that of the emerald analyzed. Hence it seemed proved that 13 parts of oxide of chrome are sufficient to communicate a very deep green colour to 7000 parts of a silicate, and we do not hesitate to assume that the colour of emerald is due to oxide of chrome, without, however, contesting the existence of an organic substance in this mineral. —*Comptes Rendus*, June 27, 1864.

RESEARCHES ON THE RESPIRATION OF FLOWERS.

BY M. CAHOURS.

The author gives the following summary of the results arrived at in the course of his researches:—

1. Every flower left in a confined space of normal atmospheric air consumes oxygen and produces carbonic acid in variable proportions, whether the flower has odour or not.

2. The circumstances in which this takes place being the same, the proportion of carbonic acid increases as the temperature rises.

3. That generally for flowers gathered on the same plant, and whose weights are virtually equal, the quantity of carbonic acid produced is somewhat more considerable when the apparatus in which the experiment is made is exposed to light, than when placed in

complete darkness; that nevertheless in certain cases the proportion is virtually the same under the two conditions.

4. When normal air is replaced by pure oxygen, the differences observed become much more marked.

5. That the flower whose development is commencing, disengages a little more carbonic acid than that which has attained its complete development, which may be explained by a more powerful vital action.

6. Every flower left in an inert gas disengages small quantities of carbonic acid.

7. We see, in conclusion, that, of the various elements constituting the flower, the pistil and the stamina, in which the greatest vitality resides, are those which consume the greatest quantity of oxygen, and produce the largest proportion of carbonic acid.—*Comptes Rendus*, June 27, 1864.

ON THE SPECTRAL RAY OF THALLIUM. BY M. J. NICKLÈS.

I have found that there are compounds of thallium which do not possess the property of colouring the flame green, and of developing the characteristic spectral ray; these are compounds with sodium, and especially with chloride of sodium. By its flame and its yellow rays this chloride completely hides the green ray.

Although chloride of thallium is insoluble in cold water, it is not so in water saturated with chloride of sodium. Thus, on pouring a solution of the latter into acetate of thallium, a precipitate of chloride of thallium is indeed formed, but the mother-liquors retain a considerable quantity of the latter without colouring the flame green. If, then, among the rays of the solar spectrum that characteristic of thallium has not been found, nothing proves that this metal does not exist in the sun; for if it has not been found, sodium has, the paralyzing action of which, when present in a certain proportion, I announce in this Note.

This incompatibility between the ray of sodium and that of thallium ought to be taken into account in toxicological or medico-legal researches directed upon thallium; for when it is present in animal tissues or liquids, it may be accompanied by sodium-compounds in sufficient quantity to annul its action on the flame, and thus lead to the supposition that this poisonous metal is absent.

Thus, also, if thallium is to be sought in mineral waters or mother-liquors, and generally in saline waters containing excess of chloride of sodium, it must first be disengaged from the sodium compounds, either by displacing it by means of pure zinc, or by means of the battery, or by precipitation by hydrosulphate of ammonia or iodide of potassium.

In regard to the latter, I have assured myself that liquids containing chloride or bromide of thallium in solution are precipitated by iodide of potassium, which gives rise to an iodide of thallium of a beautiful yellow colour, insoluble in the precipitating iodide but fairly so in distilled water.—*Comptes Rendus*, Jan. 11, 1864.

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XX. *On the Spectra of Compounds and of Simple Substances.* *By* ALEXANDER MITSCHERLICH*.

[With Two Plates.]

IN a previous paper I have shown that compounds of the metals have other spectra than the metals themselves. This fact appeared to me of great importance, because by the observation of the spectra a new method is found of recognizing the internal structure of the hitherto unknown elements, and of chemical compounds. Hence I have investigated the spectra of all the metals which I could procure, and of many of their compounds, and have found that compounds of the first order, in so far as they are volatile and remain undecomposed when adequately heated, always exhibit spectra which completely differ from those of the metals. The observations which I have recently made in this respect are communicated in the following research.

In the spectra of the compounds of the metals, to which I shall subsequently recur in detail, a regularity is most observed in the arrangement of the bright and obscure parts; thus, for example, in the spectrum of chloride, bromide, and iodide of copper. (See the delineations of the spectra.)

In these spectra, lines occur, or appear under special circumstances, which do not seem to me to belong to the spectra, because they contravene their regularity; such lines occur, for instance, in the spectra of copper- and of bismuth-compounds. It was obvious to suppose that these lines belonged to previously unknown metals; for the spectra produced by the introduction of compounds of oxide of copper or bismuth into the flame, and

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* Translated from Poggendorff's *Annalen*, No. 3, 1864, by Dr. E. Atkinson.

those which, presupposing a deportment analogous to the alkalies, must belong to the metals, exhibit a regular shading by which otherwise only the spectra of the compounds are characterized. But the number of these supposed new metals would soon have reached that of those at present known. The discovery of such metals appeared easiest in bismuth compounds. Two lines occurred—one which was soon ascertained to be that of the already known thallium, and a second near the division 77 of the scale. By precipitations, sublimations, reductions, and oxidations conducted in the most careful manner, I could not separate from bismuth the metal belonging to the line 77. Subsequent investigations showed that, according to the manner in which the spectrum was prepared, this line in the spectra of bismuth compounds occurred or disappeared; it always occurred when deoxidizing gases came in contact with the metal at a high temperature, and disappeared with oxidizing gases. The line in question can be obtained quite pure when metallic bismuth is volatilized in hydrogen and the latter is ignited. From these observations it follows that this line belongs to bismuth as metal.

I have investigated most bodies and compounds in which a spectrum might be supposed, and by different methods, in order to avoid the chance of obtaining mixed or erroneous spectra by foreign influences. As a source of light, I used flame and the electrical discharge. I will briefly adduce the methods which I used for obtaining the spectra, and designate them by numbers for convenience of subsequent reference.

1. A method which I have described in the memoir mentioned above. From a tube closed at the top and provided with a very small aperture at its lower bent end, a solution continuously flows, by means of a wick of very fine platinum wire, into a flame of coal-gas, or of hydrogen; the gas emerges from a narrow aperture. The liquid in question brought by the wick rapidly evaporates in the flame. Carbon does not separate in the coal-gas; it is prevented from doing so by the presence of water, by which hydrogen and carbonic oxide are formed.

2. The substances are brought into a coal-gas flame which burns in oxygen. An oxyhydrogen burner is used, from whose middle aperture coal-gas, and from whose external ring oxygen issues. Either by regulating the oxygen or the position of the substances, they can be introduced into a reducing or oxidizing flame. The spectrum of carbon which would here be formed disappears almost entirely, in consequence of the formation of carbonic oxide, which again, in virtue of the feeble intensity of its light, does not lessen the purity of the phenomena when a substance to be volatilized is brought into the flame.

3. The same arrangement as the above is used, but with chlorine instead of oxygen, and hydrogen instead of coal-gas in the burner. The spectrum which might be formed by the union of these bodies is too feeble to be observed; only a very feeble brightness in the green is perceptible.

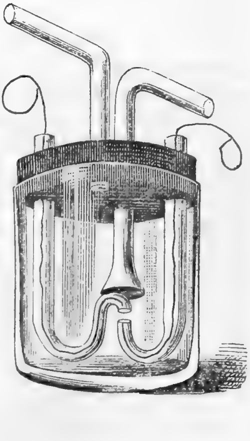
4. Bromine or iodine is evaporated in hydrogen, and this is burnt in air or in oxygen, and the substances volatilized in them.

5. If the substances to be investigated are combustible gases, they are allowed to emerge out of the middle aperture of the oxyhydrogen burner instead of coal-gas, and can thus be burned in air or in oxygen; if they are not combustible, they are mixed with a combustible gas such as carbonic oxide or hydrogen. The light produced by the combustion of carbonic oxide and of hydrogen is so feeble that it may be quite disregarded in considering the formation of other spectra by its means.

6. The solid substances are introduced into a tube, one end of which is connected with a Rose's hydrogen-apparatus, or with a gas-holder filled with carbonic oxide. By heating, the substance is volatilized in the gases, and kindled at the other end of the tube. If the substances are volatile, a small bulb is blown in the tube, into which the substance is brought.

In the above six methods the light is produced by a flame, while in the two following the substance to be investigated is volatilized by the discharge of a small Stöhrer's induction-apparatus.

7. In a vessel closed by a cork, I caused the electrical spark to pass either between two wires of the metal investigated, or between salts fastened on the ends of wires. The vessel was provided with two glass tubes, by which it could be filled with any given gas and closed, or by which a current of gas could be passed during the discharge.



8. In order to be able to use liquids as well as solids for electrodes, two tubes, bent as shown in the annexed figure, were fixed in the cork of the vessel and filled with liquid till this reached the lower aperture. Two thin platinum wires in connexion with the induction-apparatus were brought in both tubes to near the aperture. The spark then passes from liquid to liquid. By means of tubes fixed in the vessel, as in the other method, the experiment may be made in different gases. It is observed that if the spark only passes from liquid to liquid, the gases in the vessel are without action upon the spectrum, and that

therefore the temperature of the discharge is so lowered by the liquid electrodes that the gases are not ignited sufficiently to produce luminous phenomena. The latter mode of experiment is important, because spectra of the metals and of their compounds can thus be produced free from any admixture with spectra of the gases.

The spectra produced by the methods described are mostly depicted in Plates I. and II.; the luminous parts are expressed in as accurate shading as is possible by printing-ink. The lines produced by method 8 (liquid electrodes) are designated by *e* in the Plates. As regards the investigations themselves I remark as follows.

The spectra of the compounds of copper and of bismuth can be very conveniently obtained pure. Chloride, bromide, and iodide of copper, and salts of oxide of copper, such as the acetate and nitrate, were investigated by method 6 (volatilization in the glass tube). The spectra of chloride, bromide, or iodide of copper are obtained perfectly pure and very distinct by methods 3 (Cl and H) and 4 (Br, or I, and H), using even any salts of copper. The spectrum of fluoride of copper is best obtained by method 2 (H, or coal-gas, and O), using fluoride of copper and fluoride of ammonium. If in the latter method but little oxygen is admitted to the burning gases, a new line is formed in the same place in all spectra of the copper compounds. If the temperature is increased by copious addition of oxygen, the spectra disappear almost entirely, and instead of it the above line appears very bright with a few other feebler ones. This line might perhaps be due to a degree of oxidation of the copper, were it not that it is formed, as I shall subsequently state, when oxygen is completely excluded; hence it must belong to copper as metal.

It follows therefrom that the spectrum which is only obtained from the oxides of copper, and which I formerly considered to be that of the metal, belongs to an oxide of copper. There occur, therefore, in this, as in other copper compounds, at high temperatures the lines of metallic copper. Hence at a very high temperature of ignition all copper compounds are decomposed into their constituents, whereas these compounds remain undecomposed at a lower temperature—that, for example, of the hydrogen flame.

The spectrum of metallic copper produced by electricity contains, besides the lines obtained by the flames, other lines of different luminous intensity. This is also the case with other metals, with the sole difference that the additional lines in these are of less luminosity. I cannot decide how these new lines are formed, since the greater intensity of the light of the electrical spectrum could only explain the occurrence of the feebler of them.

The compounds investigated by method 8 (liquid electrodes) were more or less decomposed according to the magnitude of the affinity of their constituents at this temperature,—which is evident from the spectra; the spectrum of chloride of copper is seen very distinctly, while the spectra of bromide and of iodide of copper are more difficult to recognize.

Metallic copper investigated by method 7 (solid electrodes), in chlorine, bromine, iodine, oxygen, and nitrogen, and in other gases, always gives the spectrum of copper very distinctly, and, further, that of the kind of gas, and in chlorine the spectrum of chloride of copper very feebly.

Hence the temperature produced by the passage of the spark between solid electrodes is much higher than that between liquids, because, in the first, iodide and bromide of copper remain partially undecomposed, which in the latter are completely decomposed.

New lines occur in these spectra which neither belong to the metal nor to the gas, and whose formation and origin I have not yet investigated; for the lines are mostly so obscure that a certain result could not be deduced from them. Sulphate and nitrate of copper, and iodide, bromide, and chloride of copper, examined in the different gases as solid salts by method 7, behave like metallic copper.

Bismuth in the formation of its spectrum exhibits almost entirely the same phenomena as copper, excepting that the compounds are much more easily decomposed, when the spectrum of the metal appears. The spectra of chloride, bromide, and iodide of bismuth are obtained by method 6 (volatilization in the glass tube). But if the tube is strongly heated, the compounds are partially decomposed, even at this temperature, in the manner described. The spectrum of an oxide of bismuth readily occurs along with that of the haloid salt when water is present in the latter or is formed in appreciable quantity.

The spectrum of the metal is best obtained if any bismuth compound or the metal itself is brought into the reducing flame in method 2 (hydrogen, or coal-gas, and O); that of the oxide, if in the same method bismuth is held in the strongly oxidizing flame.

In whatever way the spectrum of metallic bismuth is obtained, whether by electricity or by flame, it is under all circumstances the same.

The compounds of calcium, of strontium, and of barium give almost as many spectra as do those of copper and bismuth. But to obtain them pure is more difficult than with the above metals. The spectra of barium and of strontium by the flame, I could only obtain, free from those of their compounds, in method 2 (H, or coal-gas, and O), by using a very small quantity of fluoride of barium or fluoride of strontium. The spectrum of calcium I could never obtain by the flame quite free from those of the oxide or of the haloid salt. By method 7 (solid electrodes) the latter is obtained free from admixtures by the use of hydrogen and metal; but at this high temperature there occur, along with the lines which are produced in the flame, a great number of more feebly luminous lines. The lines which could only be obtained by this method are not depicted in the spectra given.

In the case of barium, I could free the spectrum of the oxide from that of the metal in the same way as with bismuth; I even succeeded, when I investigated nitrate of baryta or iodide of barium by method 1 (wick of platinum wire), in producing the spectrum of baryta in individual parts of the flame. In order to obtain the spectra of the oxides of the other alkaline metals, which indeed can usually only be obtained simultaneously with the spectra of their metals, I have used in method 1 a mixture of nitrate of ammonia with the nitrates or a solution of the iodides; the latter are decomposed at high temperatures, and give the spectra of the oxides very beautifully. In these three metals I succeeded in obtaining the spectra of the haloid salts free from the spectra of the oxides, and but seldom mixed with those of the metals. The chlorine compounds have been investigated by method 1 (wick of platinum wire), and by method 3 (H and Cl). It is to be observed that in the hydrochloric flame itself they give no light at all, not even if the compound volatilized by the hydrogen burning in air enters the flame. This can only be explained by assuming that the temperature produced by the union of hydrogen and chlorine is not high enough to bring chlorine compounds to luminosity. The spectra of these compounds are obtained pure if there is more hydrogen than is necessary for union with chlorine, which free hydrogen burning in the air produces a higher temperature.

The spectra of bromine compounds were prepared by method 4 (Br and H), using the bromides in solutions, which were introduced into the flame by method 1.

The iodine compounds are decomposed, as stated; the iodide-of-barium spectrum is only obtained if the iodine compound is volatilized in a hydrogen flame which contains much iodine vapour,—although truly it is then not free from that of the oxide, so that the feebler lines of the iodide-of-barium spectrum cannot be recognized; but the lines depicted occur with all their sharpness.

Fluorine compounds were investigated by method 2 (H, or coal-gas, and O), using a mixture of the oxide with fluoride of ammonium in abundance.

It is not possible to obtain the spectrum of the sulphur compounds of these metals, which is also the case with those of copper and bismuth, as all sulphur compounds appear to be completely non-volatile. I made the experiment by bringing the metallic sulphide into hydrogen saturated with sulphide of carbon, or into sulphuretted hydrogen, which burned in an oxygen atmosphere (method 5). In the latter experiment no spectrum was seen; in the former frequently a spectrum which was produced by the decomposition of the sulphur compounds. Nor did I succeed in obtaining the spectra of cyanogen compounds; if the salts of the above metals are brought into burning cyanogen, spectra are formed as in ordinary flame.

As regards the compounds of lead, gold, iron, and manganese, I could only obtain spectra in a very few cases. Chloride of lead by method 6 (volatilization in the glass tube), or acetate of lead by method 2 in the oxidizing part of the flame, always give the spectrum of the oxide. By chloride of lead or acetate of lead, heated according to method 2 (H and O) in the reducing part of the flame, the spectrum of the metal could never be obtained free from that of oxide of lead.

By method 8 (liquid electrodes) it is obtained free from that of the oxides. The lines which are seen in this method are denoted by the letter *e* in the spectra given; it is probable that they are also contained in the spectrum of lead which is obtained by the flame, and that they are only obscured by the brightness of the oxide spectrum. By method 3 (H and Cl), the chloride-of-lead spectrum is formed even with the most varied lead compounds, but somewhat obscurely. Iodide of lead, bromide of lead and fluoride of lead, examined by method 6 (volatilization in the glass tube), gave always only the oxide-of-lead spectrum, formed by the burning of lead to oxide of lead at this high temperature.

Chloride of gold, by method 6 (volatilization in the glass tube), gives the spectrum of this compound. In no other way could I obtain a spectrum of a gold compound, neither by using hyposulphite of protoxide of gold and soda, nor potassio-iodide of gold or potassio-cyanide of gold. The spectrum of gold itself is prepared with chloride of gold by method 8 (liquid electrodes).

Of iron I could only obtain the spectrum of an oxide and that of the metal. The former is produced if the chloride or iodide are used by method 6 (volatilization in the glass tube), and very distinctly with sesquichloride of iron. By method 2 (H, or coal-gas, and O), according as the volatile iron salt is brought into the reducing or oxidizing flame, this spectrum is obtained simultaneously with that of iron or without it. By method 8 (liquid electrodes), using concentrated solution of sesquichloride of iron, the spectrum of the metal is obtained almost quite free from that of the oxide; it is only obtained perfectly so by method 7 (solid electrodes), using iron and iron compounds.

Chloride of manganese, investigated by method 6 (volatilization in the glass tube), gives the spectrum of the oxygen compound pure; it is obtained much more beautifully simultaneously with the spectrum of the metal by method 2 (H and O). By the beauty of its colours and the sharpness of its lines, this spectrum is about the most beautiful of all. By this method I could only detect one line which belonged to the metal, probably because the bright parts of the spectrum of the oxygen compounds obscure that of the metal; investigated by method 8 (liquid electrodes), the manganese compounds give moreover a few bright and several feebly luminous lines, of which the bright are depicted.

The spectrum of an oxide of tin, obtained by method 2 (H, or coal-gas, and O), using oxide of tin, and that of an oxygen or chlorine compound of chromium, using chloride of chromium, by method 8, I have not drawn, as both were too obscure. The spectra of metallic tin and chromium were prepared by method 2 (H, or coal-gas, and O) and by method 8 (liquid electrodes).

From the investigations adduced, it follows, as I have already expressed in an earlier paper, *that every compound of the first order which is not decomposed, and is heated to a temperature adequate for the production of light, exhibits a spectrum peculiar to this compound, and independent of other circumstances.*

The decomposition of compounds may be caused by the gases of flame, or even by the high temperature alone, independent of the gases: thus, for instance, chloride of bismuth, if its solution is used instead of the electrodes in the electrical discharge in chlorine, is decomposed by the high temperature of the electrical discharge alone. Individual compounds resist even this temperature—as, for instance, bromide of copper and iodide of copper, and others, which show the spectrum of the metal together with that of the compound. But if, instead of the solutions which produce a diminution of temperature by their rapid evaporation, the salts themselves are used as solid electrodes, at the then higher temperature most salts are decomposed (as, for instance, these copper salts), and but few compounds remain undecomposed.

With a great number of metals such a decomposition takes place even under the temperature at which a luminous appearance is observed; hence in this case it has as yet been impossible to observe a direct spectrum of the compounds; and therefore a comparison of the spectra of all compounds has hitherto been impossible.

The metals whose compounds are decomposed at such a low temperature, and hence only give the spectrum of the metal itself, are potassium, sodium, lithium, magnesium, zinc, cadmium, silver, and mercury. Potassium, sodium*, lithium, as metal, cyanides or chlorides, investigated by method 6 (volatilization in the glass tube) or by method 2 (H and O), give, especially in the spectra produced by the first method, several lines in addition to those depicted by other observers.

By method 2 (H and O) there are further obtained the spectra of magnesium, zinc, cadmium, silver, mercury—by the use of chloride of magnesium and chloride of ammonium, of chloride of zinc, of carbonate of cadmium, of cyanide of silver, and of cyanide of mercury. By the use of other mercury compounds, such as chloride and sulphate of mercury, no lines can be obtained by this method. In burning magnesium, the same spectrum is obtained as in the methods adduced.

By method 8 (liquid electrodes) these spectra can also be more or less well obtained with the use of various solutions. In the spectra obtained by this method other lines occur, which, however, are for the most part more feebly luminous than those formed by the flame. If zinc is burned in iodine, a brightness only is observed in the spectrum, doubtless produced by ignited particles of iodide of zinc.

That in the case of sodium compounds the metal actually gives the spectrum, I have shown in the paper already mentioned. I have found the same when I obtained this spectrum with the use of the metal by method 7 (solid electrodes), excluding oxygen. That the spectrum found with the use of the compounds of magnesium, zinc, cadmium, silver, and mercury is in each case the spectrum of the metal itself, I proved in the same manner †.

From the fact that in sodium compounds the metal gives the spectrum, I thought myself justified, in the above paper, in expressing the opinion that in the oxygen compounds of barium, strontium, and calcium the spectrum is also produced by the metal itself. This opinion, as I have already stated, has not

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* Brightness without any shading, as formed in the sodium and potassium spectra, and which I could only ascribe to the ignited solid particles in the flame, I have omitted in the spectra depicted.

† The objection urged against me in an English paper ('The Photographic News') after the publication of my former paper, that spectra produced by the volatilization of carbonate of soda, chloride of sodium, &c. which are brought into heated tubes could not have been formed, because the temperature is too low for the volatilization of these salts, does not hold; for I have myself observed the vapours.

held good, inasmuch as each of the spectra is composed of the spectrum of the metal and that of the oxide.

In the same paper, by the experiment in which when chloride of potassium is brought into a flame with much chloride of ammonium no spectrum is perceived, I have shown that under certain circumstances metallic compounds of the first order, even when volatile, may give no spectra. This is confirmed by the fact that even the yellow colour of the sodium flame almost disappears if a flame containing sodium is brought over strongly volatilizing sal-ammoniac,—and further by the fact that if any sodium, lithium, or potassium compound is investigated by method 3 (H and Cl), not even a coloured flame, and still less a spectrum, is formed. The result is similar if compounds of the alkalies are volatilized in burning sulphuretted hydrogen; it can be distinctly observed that the interior core retains its colour perfectly if these compounds are volatilized in it, but that in the outer core, where the sulphur is already burned, a feeble colour is formed. Under certain circumstances, metals, even when volatilizing in the flame, may show no spectrum—thus, for instance, in the case of any of the compounds of mercury, excepting the cyanide, and excepting mercury itself, by method 1 (wick of platinum wire), 2 (H, or coal-gas, and O), or by method 6 (volatilization in the glass tube). If the mercury compounds are heated higher, for instance by the electrical spark, or if the cyanide is investigated by method 2 (H, or coal-gas, and O), the spectrum is observed very distinctly.

Nickel, cobalt, and aluminium, investigated in the most varied compounds by method 2 (H and O) and method 6 (evaporation in the glass tube), gave no perceptible spectrum; this was also the case with the compounds of titanium, tungsten, vanadium, molybdenum, uranium, platinum, and palladium investigated by method 6 (volatilization in the glass tube) and other methods. When aluminium was burned in bromine, a continuous brightness only could be produced, as was the case with the combustion of zinc in iodine. Arsenic and antimony, examined as chlorides by method 3 (H and Cl) and by method 6 (volatilization in the glass tube), in the form of other compounds and as metal, showed a luminosity without brighter or darker lines in the spectrum. Only by method 7 (solid electrodes) could lines be found in the methods adduced, which I thus investigated. I have not investigated the still rarer metals and their compounds.

From the decompositions which, according to these investigations, take place in the flame, it would follow that if, for instance, metallic copper is heated with common salt, the chlorine which is liberated at a high temperature by the decomposition of the salt unites with copper and forms a chloride-of-copper spectrum, since chloride of copper is not decomposed at this temperature, —and further, that if oxide of copper is mixed with chloride of sodium, the free chlorine would also partially combine with copper, as cupric salts investigated by method 3 (H and Cl) give the chloride-of-copper spectrum. These decompositions do in fact take place. If, for instance, chloride of sodium is fused upon a polished copper plate at a high temperature, the bright copper is attacked; and if it is more strongly heated, chloride of copper is readily observed in the flame by the spectrum-apparatus.

Chemical processes at high temperatures may be studied by means of the spectra. The most beautiful method is by the sunlight. For compounds which give a more continuous spectrum, such a study would be more difficult, but very easy for the haloid compounds of barium, strontium, and calcium. I think of shortly making such a set of experiments.

As I supposed in my former memoir, we may by these decompositions compare, in an interesting manner, the temperatures which give rise to developments of light of various kinds—thus, for instance, the temperature of electrical discharges with the temperature of light produced by combustion, or with that of the solar light. It follows from the experiments adduced, especially from those with copper compounds, that the electrical sparks have a lower temperature when they come from liquid electrodes, that this temperature is about that of the oxyhydrogen blowpipe, but that the temperature resulting from the passage of sparks between solid electrodes is much higher than that of any flame.

The spectra of the metalloids and of their compounds with one another are not very numerous.

The spectra of hydrogen, oxygen, nitrogen, and chlorine only result from the electrical spark; they are known, just as are those of iodine and bromine, which can be prepared by the electrical spark or by the absorption of white light.

If iodine is examined by method 6 (volatilization in the glass tube), the spectrum depicted is obtained; but if very much iodine is volatilized, the absorption spectrum is produced. I shall recur to these phenomena subsequently. I have not succeeded in obtaining a spectrum of chlorine or of bromine in a similar manner, not even with bromine when hydrogen, in which bromine vapours were volatilized, burnt in oxygen; a continuous brightness was all that could be perceived.

The spectrum of sulphur was prepared by method 7 (solid electrodes), those of selenium and tellurium by method 6 (volatilization in the glass tube).

The spectrum of phosphorus obtained by method 6 (volatilization in the glass tube) is only visible if very much hydrogen is burned with traces of phosphorus; if more phosphorus is used, much phosphoric acid is formed, which separates as ignited solid substance and, like all such substances, gives a continuous spectrum, which by its brightness conceals the spectrum of phosphorus. This experiment gives a clue to the affinity at high temperatures of hydrogen to oxygen as compared with that of phosphorus to the same substance; for phosphorus first burns with a green flame, probably to phosphorous acid; subsequently hydrogen burns to water, and phosphorous to phosphoric acid.

Carbon compounds, examined by methods 5 (combustion of gases) and 6 (volatilization in the glass tube), give different spectra according to the nature of the body combined with carbon. Hydrocarbons and chlorides of carbon always show the well-known spectrum of coal-gas flame, which arises from the carbon as such; by method 7 (solid electrodes) the spectrum of hydrogen is simultaneously seen, and for the most part a separation of carbon. But if the carbon is combined with oxygen or sulphur, as in carbonic oxide and sulphide of carbon, a continuous brightness is observed on burning, in which I did not succeed in discovering dark or bright lines. The metalloid which is united with carbon (for instance chlorine, bromine, iodine, and sulphur) can never, with very few exceptions, be recognized by the flame when investigated by methods 1 to 6. With nitrogen this is not the case; if the compound is not very rich in oxygen, it can be recognized by the formation of a spectrum of ammonia.

I obtained the spectra of silicon and fluorine by effecting the electrical discharge in silicofluoride of hydrogen and in hydrofluoric acid. The spectrum of fluorine which I obtained with that of hydrogen alone, was deducted from that of fluoride of silicon, and thus the latter recognized. Both spectra consist of individual lines. These spectra, like the rest which are only formed by the passage of the sparks from dry electrodes, I have not depicted. Silicon and fluorine, investigated by method 5 (combustion of gases), using fluoride of silicon with hydrogen, give only luminosities in which no shadings are perceptible.

The spectrum of boron was prepared with the use of boracic acid by methods 6 (evaporation in the glass tube) and 8 (liquid electrodes), and with fluoride of boron by method 7; in both cases I obtained the same spectrum.

Spectra of the compounds of the metalloids with one another I could only observe in small number. Most compounds give a spectrum which, from the small intensity of its light, cannot be investigated—thus, for instance, hydrogen and hydrochloric acid; others give a continuous one, as, for instance, sulphuretted hydrogen, sulphide of carbon, sulphide of nitrogen, carbonic oxide. I never succeeded in recognizing dark or bright lines in the spectra of these compounds. Of the compounds of the metalloids with one another, I could only recognize the spectra of cyanogen and ammonia: the first is already known, but not in a state of purity; the lines of the carbon spectrum which result from the decomposition of cyanogen when burnt in hydrogen have been assigned to the cyanogen spectrum; the spectrum of ammonia has recently, during my research, been depicted by M. Dibbits. It is interesting to recognize again in the spectra of both compounds the properties of the simple substances resembling them. While ammonia always appears with more or less intensity in the spectra of its compounds and thus behaves like the metals, cyanogen, like iodine, bromine, &c., loses in its compounds the property of producing a spectrum; thus hydrocyanic acid, sulphocyanogen, cyanic acid, &c. give no recognizable spectrum.

The spectrum of ammonia can best be depicted by method 5 (combustion of gases), by the use of oxygen; it is obtained of feebler luminosity by method 6 (evaporation in the glass tube), from ammonia compounds, or best by urea.

The decomposition and formation of cyanogen at high temperatures, which can easily be followed by the spectra, is interesting. For if the electrical discharge passes in cyanogen, the gas is decomposed, very dense carbon being deposited in characteristic curves, but if much carbonic oxide is added it remains undecomposed; but if the electrical spark be allowed to strike for a short time through air which contains a hydrocarbon, cyanogen is formed. Hence at the same temperature the compound is formed and decomposed.

If hydrogen is brought to burning cyanogen, a change of colour is speedily observed in the flame, and the formation of ammonia is readily shown by the spectrum. If carbonic oxide is admitted to cyanogen, no change, as already observed, is produced. Hence at this high temperature hydrogen has a greater affinity for nitrogen than carbon.

Of the compounds of the metalloids, I further examined by method 6 (volatilization in the glass tube), or method 5 (combustion of gases), protoxide of nitrogen, binoxide of nitrogen, nitrous acid, nitric acid, chloride of sulphur, oxychloride of phosphorus, and, further, sulphuretted hydrogen and sulphide of carbon mixed with chlorine; in all cases I only obtained luminosities without shadings. By a decomposition of the compounds, selenious and selenic acids give the selenium spectrum, and chloride of iodine the iodine spectrum.

If the spectra are compared with one another, it is found that the metallic spectra consist of individual sharp lines, and that those of their compounds with the metalloids (excepting the haloid salts of calcium, strontium, and barium, whose spectra consist of individual lines) are composed of broad luminosities with narrow dark lines which recur at definite intervals.

By means of that property, the spectra of the haloid compounds of calcium, strontium, and barium are readily compared with each other; and it is at once observed that individual characteristic lines recur in the spectra of one and the same metal, which, according to the halogens, are more or less distant from one another, by which means the metal is easily recognized in the spectra of its compounds. The fluorides form an exception to this.

If this phenomenon is investigated in the case of the barium compounds by the spectra depicted, it is found that the distances of the two prominent lines in the various spectra are to each other as the atomic weights of these compounds. This relation gives rise to further interesting conclusions. Thus from an observed distance of such lines, the distance of the corresponding lines in the spectra of other barium compounds may be calculated; further, from the distance and one known atomic weight that of the other compounds may be determined.

If we start from the chloride-of-barium lines, which are most easily procured, and if we form from the atomic weights of the compounds and the distance of the chief lines of the chloride-of-barium spectrum, which is expressed by 3·9 degrees of the scale, the equation for the distance of these lines in the iodide-of-barium spectrum, we obtain, taking the atomic weight of iodide of barium at 195·5, that of chloride of barium at 104, $\frac{3{\cdot}9}{x}=\frac{104}{195{\cdot}5}$, from which $x=7{\cdot}3$: the drawing gives 7·3.

Just as these equations may be stated for the lines of iodide of barium from those of chloride of barium, so in like manner we get for bromide of barium, whose atomic weight is 148·5, the equation $\frac{3{\cdot}9}{x}=\frac{104}{148{\cdot}5}$, from which $x=5{\cdot}5$; according to the drawing it is 5·2.

Of course the atomic weights of the barium compounds mentioned may be calculated from the spectra, as well as the spectra from the atomic weights. Other equations may also be obtained which express the relations of bromide of barium to iodide of barium.

The feebler lines of the chloride- and bromide-of-barium spectra may also be compared like the strong lines; but in the case of iodide of barium they could not be drawn, and in bromide and chloride of barium they had not the precision and distinctness necessary for making calculations from them.

In the spectra of those haloid salts of barium whose light consists of more strongly refracted rays, the distances of the individual lines from each other are less than in the spectrum of each haloid salt whose corresponding lines are produced by light of greater wave-length. For this relation I have also succeeded in finding an expression.

The distance of the more strongly refracted bright line of the chloride of barium from a definite point of the scale, is to the distance of the other bright line from this same point as the distances from the same starting-point of the corresponding lines of the spectra of iodide and bromide of barium. In order to find the starting-point, whose distance from that of the most strongly refracted line of the chloride-of-barium spectrum is expressed by y, I formed from the spectra of chloride of barium and bromide of barium the following equation, in which the unit is a degree of the scale:—

$$\frac{\text{Distance of the first chloride-of-barium line}}{\text{Distance of the second chloride-of-barium line}}\ \frac{y}{y+3{\cdot}9}$$
$$=\frac{\text{Distance of the first bromide-of-barium line}}{\text{Distance of the second bromide-of-barium line}}\ \frac{y+3}{y+8{\cdot}2},$$

from which $y=9{\cdot}0$.

From the spectra of chloride of barium and iodide of barium the equation is obtained,

$$\frac{y}{y+3{\cdot}9}=\frac{y+9}{y+16{\cdot}5}, \text{ from which } y=9{\cdot}5.$$

From those of bromide of barium and iodide of barium,

$$\frac{y+9}{y+16{\cdot}5}=\frac{y+3}{y+8{\cdot}2}, \text{ from which } y=10{\cdot}5.$$

From the above equations we obtain for the position of the common starting-point $96{\cdot}1-y$, that is, $96{\cdot}1-9{\cdot}7=86{\cdot}4$ degrees*.

From these and the preceding equations, it follows that if two spectra of the chloride, bromide, or iodide of barium are known by the first equations, the distances from each other of the principal lines in the spectrum of the third compound may be calculated from them. From the latter equations, which have given the starting-point, the position of these lines may be calculated.

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* I have designated the starting lines in the spectra of the metals by the letter *a*.

These relations cannot be established in the case of fluoride of barium.

Comparing the spectra of the haloid compounds of calcium, which mostly consist of three lines, two very near each other, the third somewhat distant, with the atomic weights of these compounds, it is found that *the distances from each other of corresponding lines are inversely proportional to the atomic weights.*

Taking for the atomic weight of iodide of calcium 147, of bromide of calcium 100, and of iodide of calcium 55·5, we obtain the following equation for calculating the distance of the most divergent lines of bromide of calcium (which I denote by x), from the distance of the chloride of calcium lines, which, according to calculation is 6·5 for the latter compound:—

$$\frac{x}{6{\cdot}5}=\frac{55{\cdot}5}{100}, \text{ from which } x=3{\cdot}6.$$

From the drawing of the bromide-of-calcium spectrum the same result is obtained, $x=3{\cdot}6$.

From the observed position of the lines of both compounds, the equation for the starting-point of the spectra of the calcium compounds is the following, in which y is the distance of the least refracted lines of the chloride-of-calcium spectrum from the starting-point, which in this case is opposite to the starting-point found in the spectra of the barium compounds:—

$$\frac{\text{Distance of the first chloride-of-calcium line}}{\text{Distance of the second chloride-of-calcium line}}\;\frac{y}{y+6{\cdot}5}$$
$$=\frac{\text{Distance of the first bromide-of-calcium line}}{\text{Distance of the second bromide-of-calcium line}}\;\frac{y-1{\cdot}2}{y+2{\cdot}2},$$
$$\text{from which } y=2{\cdot}6.$$

The starting-point lies near 129·8 of the division.

In iodide of calcium I have not succeeded in observing a spectrum; I shall calculate this from the distance of the chief lines of chloride of calcium and from the atomic weights, taking the starting-point found as a basis. In accordance with this, the equation for the two lines of iodide of calcium which have the greatest difference is

$$\frac{x}{6{\cdot}5}=\frac{55{\cdot}5}{147}, \text{ from which } x=2{\cdot}5.$$

The equation for the position of the lines of iodide of calcium is as follows, if the distance of the line nearest the starting-point from the starting-point is $9=y$:—

$$\frac{\text{Distance of the first chloride-of-calcium line from the starting-point}}{\text{Distance of the first iodide-of-calcium line from the starting-point}}\quad\frac{2{\cdot}5}{y}$$

$$=\frac{\text{Distance of the second chloride-of-calcium line from the starting-point}}{\text{Distance of the second iodide-of-calcium line from the starting-point}}\quad\frac{9}{y+2{\cdot}5},$$

from which $y=1$.

Hence the distance of the first iodide-of-calcium line would be near the division 128·8, and the other near 126·3. The position of the third line close to 128·8 might be directly determined from the position of the line corresponding to it in the chloride-of-calcium spectrum; it must be near 128·6.

The observations of the fluoride-of-calcium spectrum do not agree with the calculations. By calculation from the same equations as in the case of iodide of calcium, the distance, for instance, of the two extreme lines of the fluoride of calcium would amount to 9 degrees, while from observation it is 22 degrees. From calculation from other equations these lines must be about 117 and 126; in the spectrum observed they are, on the contrary, at 102 and 124.

In the spectra of the haloid salts of strontium there is the same behaviour as in those of calcium.

Calculating from the distance of the extreme sharp lines of bromide of strontium those of the corresponding lines of chloride of strontium, taking the atomic weights of chloride of strontium at 79·6, of bromide of strontium 123·8, the distance of these bromide-of-strontium lines from one another being 6·5, the equation obtains $\frac{6{\cdot}5}{x}=\frac{79{\cdot}6}{123{\cdot}8}$, from which $x=10{\cdot}1$.

From the drawing x also $=10$.

Proceeding as with calcium, the starting-point calculated from both spectra gives the following equation:—

$$\frac{\text{Distance of the first chloride-of-strontium line from the starting-point}}{\text{Distance of the first bromide-of-strontium line from the starting-point}}\quad\frac{y+0{\cdot}5}{y}$$

$$=\frac{\text{Distance of the second chloride-of-strontium line from the starting-point}}{\text{Distance of the second bromide-of-strontium line from the starting-point}}\quad\frac{y+10{\cdot}5}{y+6{\cdot}5}$$

from which $y=0{\cdot}9$.

Hence the starting-point lies near the division 138·4.

If, as in the case of the calcium compounds, the distance of the lines in the spectrum of fluoride of strontium is calculated from that of bromide of strontium, we obtain the equation

$$\frac{x}{6{\cdot}5}=\frac{123{\cdot}8}{62{\cdot}8}, \text{ from which } x=12{\cdot}8.$$

From observation $x=18$. Hence, as in the case of fluoride of calcium and fluoride of barium, this does not agree with calculation.

The iodide-of-strontium, like the iodide-of-barium spectrum, I could not observe. According to the following equations, which are formed as in the case of barium, it has been calculated:—

$$\frac{x}{6{\cdot}5}=\frac{123{\cdot}8}{170{\cdot}8}, \text{ from which } x=4{\cdot}7.$$

From the distances and from the position of the starting-point, we obtain the following equation for the position of the lines for iodide of strontium:—

$$\frac{\text{Distance of the first bromide-of-strontium line from the starting-point}}{\text{Distance of the first iodide-of-strontium line from the starting-point}}\;\frac{1}{y}$$

$$=\frac{\text{Distance of the second bromide-of-strontium line from the starting-point}}{\text{Distance of the second iodide-of-strontium line from the starting-point}}\;\frac{7}{y+4{\cdot}7},$$

from which $y=0{\cdot}67$.

Hence the first line of iodide of strontium is at the division 137·3, and the other at 132·6. The position of the other bright lines of the iodide of strontium which lie between the two has been calculated in the same way.

Hence for the haloid compounds of barium, excepting fluoride of barium, it follows that the distances of corresponding lines in their spectra are directly proportional to the atomic weight, and that for the haloid compounds of calcium and strontium, excepting the fluorides, these are inversely proportional to the atomic weights. Further, that the relation of the line nearest the starting-point to the same, to the distance of the furthest line, is the same for the same metal in the case of the haloid compounds of barium, strontium, and calcium. Here also the fluorides are to be excepted.

I hope I shall hereafter succeed in finding the reason for this abnormal relation of the fluorides.

The fact appears important to the significance of the starting point on the barium spectrum, that I could observe a distinct line in the spectrum of this metal. This line only becomes visible by methods 6 and 7 (electricity), or by bringing barium compounds into the flame of cyanogen burning in oxygen.

Such a relation, as in the case of calcium, strontium, and barium compounds, could not be discovered in the other metals. Similarity in the spectra of the compounds of a metal is easily seen in the case of copper—for instance, in the violet part of the bromide- and chloride-of-copper spectrum, and in the green part of the chloride-, bromide-, and iodide-of-copper spectrum; but no relation could be established between these spectra and the other properties of the compounds.

As there are resemblances in the spectra between individual compounds of a metal, so there are such between compounds of different metals with oxygen. Most striking is the resemblance between the spectra of lime and of strontia; the individual parts correspond to each other, but the lime spectrum is more extended than that of strontia.

The spectra of baryta and oxide of lead are also very similar in certain respects. This similarity is difficult to express in the drawings, while it is very striking to the sight.

Relations between these spectra and the atomic weights I have not as yet been able to establish, but it may be expected that such exist.

I have not been able to find properties of the spectra of the metals which enable a connexion of the metals *with one another* to be recognized: similarity of individual spectra together with similar properties of the metals seems to point at such a connexion; thus, for instance, the spectrum of zinc is very like that of cadmium.

The metalloids show the same spectra, provided with regular shading, as the metallic oxides; and if, for instance, the spectrum of the oxygen compounds of bismuth is compared with the spectrum of iodine obtained by method 6 (volatilization in the glass tube), a resemblance is observed corresponding to that which I have noticed as existing between the spectrum of oxide of lead and of baryta. The oxides of both these metals are decomposed by high temperatures, and show individual lines as spectra. Iodine also, when investigated by method 7 at a high temperature, exhibits an entirely different spectrum, consisting of individual lines like the spectra produced at high temperatures from these metallic oxides; and from the phenomenon that iodine shows two spectra, from their similarity with the spectra of the metallic oxides and those of the metals, and from its analogous deportment with the first at high temperatures, the opinion might be expressed that ordinary iodine is a compound body.

From this it would follow that iodine at ordinary temperatures, and iodine at the temperature of hydrogen flame, must be conceived as two different compounds, because the spectrum of iodine formed at ordinary temperatures is different from that produced by the hydrogen flame. A compound of hydrogen with iodine in this flame cannot be the cause of it, because the same spectrum is obtained when iodine is in a carbonic-oxide flame; from the ready decomposability of oxygen compounds of iodine, the presence of one of these cannot well be the cause.

Spectra resembling those of iodine I could not detect by the flame in the case of chlorine and bromine. If the preceding supposition is correct, bromine must be regarded as a compound body; as it has two spectra, one formed by absorption, and the other by the electrical spark.

If we compare with the flame spectrum of iodine the spectra of the metalloids as formed by the flame of selenium, tellurium, phosphorus, and those of sulphur and nitrogen, resulting from feeble electrical charges, it is found that all these metalloids have, in their spectra, the character of this iodine spectrum, and would thus, if the above-expressed supposition be confirmed, be compound bodies.

In comparing these spectra, several peculiarities are observed: thus in the distinctly marked part of the sulphur and selenium spectra, the number of luminosities appears to be inversely as the atomic weights; a similar relation appears to obtain to that which exists between the spectra of the haloid compounds of barium. I must, however, add here that the spectrum of sulphur, like that of tellurium, could not by the methods which I have hitherto used be obtained with the distinctiveness which the spectra of other bodies exhibited.

These communications on the connexion of the spectra with one another and with the atomic weights can only be regarded as precursory. I shall continue the investigations with more accurate apparatus, and in due time make further communications.

Appendix.

In conclusion I will adduce some observations which refer to the flames giving spectra.

The flames giving spectra are in most cases produced by the luminous gases of the volatilized bodies,—thus, for instance, in burning cyanogen, and in the metals and metallic compounds which are brought into the flame.

If the bodies are contained in the flame in such large quantity that they cannot volatilize completely, or if they are not at all volatile, they become white-hot and give the spectra of all ignited bodies, that is, a perfectly continuous one, without dark or bright lines. The former is the case if phosphorus is investigated by method 6 (volatilization in the glass tube, and this somewhat strongly warmed); for the latter, the carbon separated from hydrocarbons is the best example.

If such a body, which is brought in excess into the flame and hence does not evaporate sufficiently, has only few lines in the spectrum (so that an absorption spectrum of it is otherwise easily obtained, since in the middle there is a brightly luminous body, and round about the vapours of this body), its absorption spectrum is obtained. This is the case if sodium is burned, or much sodium volatilized in a hydrogen flame. This phenomenon can best be investigated if more or less iodine is brought into the hydrogen flame. If there is only little iodine, the new spectrum which I have found is obtained; but if there is much iodine, the absorption spectrum is obtained. The middle of the flame is white-hot; the white light must pass through iodine vapours and these absorb a part, as is the case if a candle-light is viewed through iodine vapours.

It is not every volatile body which, introduced into the flame, gives a spectrum. For particular bodies, a very high temperature is necessary to heat them so strongly that they disengage light. This is best seen with mercury; for mercury salts, excepting cyanide of mercury, investigated by methods 1 (wick of platinum wire), 2 (H, or coal-gas, and O), and 6 (volatilization in the glass tube), hence at a lower temperature, give no spectrum; while, when heated by methods 7 (solid electrodes) and 8 (liquid electrodes), they produce a very bright spectrum. This is further observed in the case of oxygen, nitrogen, and other gases, which only by the electrical discharge are sufficiently heated to give a spectrum.

To investigate whether solid bodies did not also produce spectra, I caused white light to pass through a gold-leaf transparent with blue light, or through gold which was precipitated in extremely dilute solution, but I could not observe either dark or bright lines in either experiment.

Berlin, February 1864.

XXI. *Upon the Quartz from Euba, and on the Biaxial character of Pyramidal and Rhombohedral Crystals.* *By* Professor A. Breithaupt*.

In volume cxx. of Poggendorff's *Annalen* Prince Salm-Horstmar alludes to a quartz from Euba, near Chemnitz in Saxony, and makes mention of its inferior hardness and low specific gravity, and observes also that it is optically biaxial. I conceive that it is I who may claim to have been the first to discover these peculiarities, for there was assuredly no one who, prior to my doing so, had called attention to them.

What first of all interested me was the felspar that is associated with this quartz, of which, under the name of Paradoxite, I shall shortly publish a description. I accidentally found, on separating the two, that this quartz is of less hardness than is usually attributed to this mineral, and I was thence led to address myself to M. Steeg to prepare me some slices thereof for optical purposes. He on that wrote me to say that he too, when cutting slices at right angles to the principal axis, found this quartz to be only of the hardness of adularia. On scratching it I find, however, that it is somewhat, but very little, harder than adularia. According to my scale, its hardness is from 8 to 8¼; at the free summits of the crystals it even attains a hardness of 8½, taking that of the Zinnewald smoky quartz, or the transparent quartz of St. Gotthard, or from Graubündten, as equal to 9.

Not knowing whether anyone besides myself has determined its specific gravity, I may state that it fluctuates between 2·578 and 2·632, and that it so far goes hand in hand with the hardness, that fragments of the crystals on the ends where they were seated are of the lowest specific gravity, while fragments of the free ends were of the highest, yet never attaining that of other varieties of quartz that I have examined.

In the optical sections prepared for me by M. Steeg, I discovered at once the very distinct biaxial character of the mineral. The hyperbolas are not, however, black, but appear bluish. M. Jenzsch, the Councillor of Mines, has met with both left-handed and right-handed individuals.

Although it is stated in the notice referred to that this quartz is cloudy, this is only partially the case, for some of the crystals met with are quite transparent.

This quartz possesses also another peculiarity, being more liable to be weathered than any other with which I am acquainted. I cannot say how many thousand times I have noticed quartz lying on the surface of the ground, but I have never met with

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* Communicated by W. G. Lettsom, Esq. From Poggendorff's *Annalen der Physik*, vol. cxxi. p. 326.

any that is as much weathered as this Euba quartz now and then is. It may be assumed for a fact that it is more liable to become so than is its neighbour the paradoxite.

My friend M. Reich, the Superior Councillor of Mines, had the kindness to examine this quartz chemically for me, and found therein nothing but silica, with traces of oxide of iron amounting to $\frac{1}{4}$ per cent. Professor Wöhler has been so good as to say he will prepare some silicon from it and submit it to a further examination.

This quartz occurs at Euba in veins, which are four in number in all. Three of them do not exceed 1 inch or 2 inches in width, but the fourth is above 2 feet thick. In the former, quartz and paradoxite alone occur; but in the latter, fragments of porphyry are met with, as also fluor in considerable quantity, and some calcite and mica.

As I was familiar with paradoxite elsewhere only in connexion with tin-veins, and as the blue fluor, as it occurs here, is specially associated with veins of that kind, I caused large fragments of the general mass of the veins to be broken up, stamped, and washed, when, lo, tin-ore was obtained by washing, from which beautiful metallic tin was got before the blowpipe with oxalate of potash upon charcoal. The tin-vein formation, therefore, which hitherto has been held to be one of the oldest, is a very recent one, for the veins in question occur in the lower new red sandstone.

There are also other varieties that may be held to belong to this quartz, especially, for instance, the so-called *star-quartz* from the vicinity of Bautzen in Saxony, and from the neighbourhood of Hohenelbe in Bohemia. These are of the hardness of adularia. They consist of wedge-shaped, bacillary, aggregated pieces radiating in a stellar form; they are, however, quite cloudy, and for the most part somewhat weathered. The Euba quartz assumes partially the same structure; but it is rarely stellar, it is commonly fascicular. I am acquainted also with another star-quartz from the Hill of Molignon in the Tyrol.

As I have for the last four years occupied myself to my utmost with optical studies, I will here mention, quite in a preliminary way, and as concisely as I can, some of the principal results at which I have arrived.

The grossular garnet from Siberia is in one tetragonal axis uniaxial, as is the case with essonite and almandine. The garnets whose specific gravity is the highest, namely the manganese garnets, are optically isotropic.

In the *pyramidal system*, I have found that all transparent minerals to which I have had access are, with one single exception, *optically biaxial.* The species that exhibit this character the most clearly are Cromfordite (muriocarbonate of lead), Scheelites of various localities, hyacinth-zircon from the neighbourhood of Schandau in Saxony, different Wulfenites, and mellite. The biaxial character is exhibited strongest in a meionite from Monte Somma (in others it is comparatively very weak), and in the yellowish zircon from Ceylon. That apophyllites are optically biaxial was established by Sir David Brewster more than twenty years ago. The observations upon mellite were made by M. Reich, Superior Councillor of Mines, and by M. Jenzsch, Councillor of Mines, quite independently of each other.

The one single exception is Matlockite, which is optically uniaxial. There is also a green uranite, which is so feebly biaxial that many an observer might take it for uniaxial.

In the *rhombohedral system* pretty much the same holds good. Tamarite (copper mica), dioptase, the majority of calcites, Smithsonite, most apatites, nepheline, most quartzes (probably all), beryl, phenakite, spartalite, Greenockite, and so forth, are optically biaxial. Mimetite from Johanngeorgenstadt is as clearly and strongly so as many a biaxial mica is (Muscovite).

That biaxial quartzes are met with was already known, and the same is the case with respect to beryl.

As far as I have proceeded as yet in the determination of the angles of the crystals, not only in the optically uniaxial crystals of the cubic system, but also in the optically biaxial crystals of the pyramidal and rhombohedral systems, those deficiencies of symmetry may be shown to occur to which attention was directed by me some years ago.

When in the course of last year M. de Kokscharow witnessed in so marked a manner, while with me, the biaxial character of various idocrases, he, of his own accord, observed that he would repeat his measures of that class of crystals. The manganesian idocrase from St. Marcel in Piedmont is optically the most remarkable.

Freiburg, January 1864.

XXII. *On Molecular Physics.* *By* Prof. W. A. Norton[*].

IT is proposed in the present paper to give a general exposition of a Physical Theory of Molecular phenomena, based upon the highest generalizations, and the most reliable physical conceptions to which the progress of science has hitherto con-

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[*] From Silliman's American Journal for July 1864.

ducted*. A theory so comprehensive in its scope can be completely substantiated only by undertaking a thorough discussion of the minute details of special phenomena in the several departments of physical science, and subjecting it at all practicable points to the rigid test of numerical calculation. But before any detailed discussions can be entered upon, we must deduce from the fundamental conceptions adopted the general principles of molecular action, or the laws of the molecular forces, note the characteristic features of the different provinces into which the entire field of research is naturally divided, and trace out the general relations which they bear to each other, or, in other words, recognize the mutual dependence and essential correlation of special physical forces.

The established truths and generally received ideas which form the basis of the theory are as follows:—

1. All the phenomena of material nature result from the action of force upon matter.

2. All the forces in operation in nature are traceable to two primary forces, viz. attraction and repulsion.

3. All bodies of matter consist of separate indivisible parts, called atoms, each of which is conceived to be spherical in form.

4. Matter exists in three different forms, essentially different from each other. These are (1) ordinary or gross matter, of which all bodies of matter directly detected by our senses either wholly or chiefly consist. (2) A subtile fluid, or æther, associated with ordinary matter, by the intervention of which all electrical phenomena originate or are produced. This *electric æther*, as it may be termed, is attracted by ordinary matter, while its individual atoms repel each other. (3) A still more subtile form of æther, which pervades all space and the interstices between the atoms of bodies. This is the medium by which light is propagated, and is called the *luminiferous æther*, or the *universal æther*. The atoms, or "atomettes," of this æther mutually repel each other; and it is attracted by ordinary matter, and is consequently more dense in the interior of bodies than in free space.

5. Heat, in all its recognized actions upon matter, manifests itself as a force of repulsion.

The corner stone of a physical theory of molecular phenomena must consist in the conception that is formed of the essential constitution of a single molecule—understanding by a molecule an atom of ordinary matter endued with the properties and invested with the arrangements which enable it to exert forces of

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* The principal features of the general theory here propounded have, with few exceptions, been advocated by the author before the Connecticut Academy of Arts and Sciences, at various meetings of the Academy during the last six years.

attraction and repulsion upon other molecules. In seeking for this, the most philosophical course that can be pursued is to follow out to their legitimate conclusions the general principles already laid down. We have admitted the existence of a subtile æther, attracted by all bodies, and pervading their interstices; now if bodies attract this æther, the atoms of which they are composed must exert an attractive action upon it. Every atom must therefore be surrounded with an *æthereal atmosphere,* condensed upon its surface, and extending indefinitely outward. Again, it is conceded that the electric æther or fluid, if it exists, must be attracted by ordinary matter; but if this attraction subsists it must be exerted by the individual atoms, and therefore every atom must also be surrounded with an atmosphere of electric æther—an *electric atmosphere,* as it may be termed. We must suppose that the interstices between the atoms of this electric atmosphere will be pervaded by the more subtile æthereal atmosphere. We are thus led to conceive of a molecule as consisting of an atom surrounded with two atmospheres, æthereal and electric—the former being the more attenuated, and pervading the other. We may suppose either that these two æthers exercise no direct action upon each other, or, what is more probable, that the electric atoms attract the æthereal, and are therefore surrounded, like the central atoms of the molecules, with æthereal atmospheres. To this supposed fact we may attribute the mutual repulsion subsisting between the electric atoms, and thus restrict the fundamental property of repulsion to the atoms of the universal æther.

The conception here formed of a molecule involves the idea of the operation of the two forces of attraction and repulsion: a force of attraction is exerted by the atom upon each of the two atmospheres surrounding it, and a force of mutual repulsion between the atoms of each atmosphere. These we regard as the *primary forces* of nature, from which all known forces are derived. They determine primarily the physical relations of the atom to its atmospheres. In seeking for the molecular actions that may result from their operation, there are two different routes that may be taken. We may conceive that the atmospheres surrounding each atom are naturally in a condition of statical equilibrium, and that the primary forces with which the molecule is invested take effect at all distances, without the intervention of any medium, and unobstructedly through all intervening matter; or we may conceive the natural equilibrium of the molecular atmospheres to be a dynamical one, and that, as a necessary consequence, recurring impulses, both attractive and repulsive, are propagated outward by the surrounding æther from each molecule, and take effect upon other molecules. Here, as before, we shall follow the indications of existing science, which, as will be generally conceded, point to a dynamical origin of the molecular forces. The ideas we have thus been led to form with regard to the real nature and mode of action of these forces are as follows.

The *molecular forces* consist of—

1. A repulsive action of the electric atmosphere of a molecule exerted primarily upon the electric æther immediately exterior to it. This force of repulsion is made up of recurring impulses, which are propagated in waves through the circumambient electric æther. These impulses fall upon the electric atmospheres of contiguous molecules, and are thence propagated down to the surfaces of the central atoms, and take effect upon these as a force of repulsion.

2. An attractive action exerted by the central atom of the molecule upon the electric æther surrounding it; originating a series of successive contractions of this atmosphere, and thus of inward-acting impulses, which are propagated outward and form a set of attractive waves. These are received, like the repulsive impulses, upon the surfaces of contiguous electric atmospheres, and propagated to the central atoms, upon which they take effect as an attractive force. The recurring contractions of the atmosphere here supposed do not necessarily imply that the force which produces them acts by impulses, for every such contraction must develope a resistance, which will occasion a subsequent expansion; and, at the same time, recurring expansions should result from the similar impulses propagated from surrounding molecules. The electric atmospheres that envelope the atoms of bodies may accordingly be in a perpetual dynamical condition of alternating contractions and expansions, or of alternating inward and outward movements of their atoms, although the primary forces acting upon these atoms should be continuous in their action.

But if we confine our attention to the action of a single atom upon its electric atmosphere, it will be seen that the expansions, which of necessity follow the contractions, must be of less extent than the contractions; for a part of the contractile force is expended in impelling a portion of the universal æther compressed upon the surface of the central atom normally outward from this surface. To the extent that this takes place will the contraction of the atmosphere exceed the expansion which immediately follows it, and an effective attractive force be propagated through the surrounding electric æther. We are thus led to recognize the existence of a third molecular force, viz. a force of repulsion originating in the attractive action exerted by the atom of the molecule upon its electric atmosphere.

3. A third molecular force, then, consists of a series of repulsive or outward-acting impulses imparted to the universal æther at the surface of the atom of a molecule by the contractile force exerted by the atom upon its electric atmosphere. This repulsion is equal, at its origin, to the attraction which developes it. It is propagated in waves which, unlike the waves conveying the other molecular forces, proceed through the universal æther. These waves, if each contraction of the atmosphere were not followed by a partial expansion, would be of the character of "waves of translation," and would convey only outward-acting impulses; they are, in fact, oscillatory waves, in which the outward predominate over the inward acting-impulses.

The force thus originating may be regarded as the primary force of *heat*, and may be termed *heat repulsion*. The other two molecular forces may be designated as the forces of *electric attraction* and *electric repulsion*. But they should not be confounded with the special electric forces that come into play whenever the natural quantity of electric æther associated with atoms, or present on different sides of atoms, experiences any material increase or diminution—which will be considered in another connexion.

The molecular forces that have now been specified might be otherwise characterized as follows: (1) A repulsion of the one electric atmosphere for the other, operating through the intervention of the electric æther posited between the two. (2) An attraction of the gross atom of the one molecule for the electric atmosphere of the other, also taking effect by means of the intervening electric æther. (3) A repulsion exerted by the atom of the one molecule upon that of the other, through the intervening universal æther, and originating in the attraction just mentioned. These forces consist of recurring impulses propagated in waves through the æthereal media, which take effect ultimately as attractive or repulsive impulses upon the central atoms of molecules. The law of diminution of the propagated forces is that of the inverse squares.

If the *æthereal* as well as the electric atmospheres of particles be conceived to be in a state of dynamical equilibrium, their alternate contractions and expansions should originate oscillatory waves that would be propagated indefinitely onward through the æther of space. If we admit, with Professor Challis, that such purely oscillatory waves, when they fall upon particles, will give rise to an attraction or a repulsion, according to the breadth of the waves in comparison with the diameter of the particles, and that the force of gravitation may be conveyed by such waves, we have in the supposed dynamical condition of æthereal atmospheres of particles a possible origin of waves of gravitation, which cannot be found, primarily, in any supposed motion of gross atoms in an isolated condition. It should be observed, too, that the dynamical condition of atmospheres here considered is really a necessary consequence of the first operation of the force of attraction of atoms upon the surrounding æther, if the elasticity of the æther be perfect.

A different view of the possible nature and origin of the molecular forces from that which has been given, may be obtained by changing our stand-point.

We may conceive the same three forces, viz. one of attraction, and two of repulsion, to be in operation, but we may replace the forces of electric attraction and repulsion by equivalent forces propagated through the universal æther. This may be realized as a physical conception by regarding the atoms of the molecules and those of the surrounding electric æther, each encompassed by its æthereal atmosphere, as being, or rather their atmospheres, in the dynamical condition of alternate contraction and expansion, and thus as being centres from which proceed oscillatory waves, and that, as the result, in accordance with the general theory so ably advocated by Professor Challis, the electric atoms of two atmospheres may repel each other, and the central atoms which they surround may also repel each other; the general result being that similar atoms repel, and dissimilar attract. Upon this view the forces we have deduced from the dynamical state of the *electric* atmospheres, which must still be in operation, must be overshadowed by those now considered. Upon the former idea it is the forces now derived from the dynamical state of the *æthereal* atmospheres that must be overshadowed by the others. A discussion of phenomena can alone decide which of these two general views should be adopted. In the present memoir we shall chiefly occupy the ground first taken.

Among the physicists of the present day there seems to be a growing inclination to discard the notion of an electric fluid as distinct from the æther of space, and attempts have been made by Challis, Tyndall, and others, to frame a consistent dynamical theory of molecular forces and phenomena based upon the supposed existence of only two forms of matter, viz. gross matter, and the æther of space. The fundamental position taken by these distinguished physicists is that the molecular forces, including heat, are conveyed by purely oscillatory waves, and originate in a vibratory motion of the ultimate particles of bodies. Against this idea, however plausible it may seem, and however admirable may be the ingenuity and skill with which it has been sustained, many serious objections may be urged. One or two of these may be briefly stated.

1. No possible mode of explaining the phenomena of electri-

city and magnetism has yet been indicated by the advocates of this theory. The electric fluid is expelled by them from the vast field it has hitherto occupied, but all attempts to supply its place have proved futile.

2. Another obvious objection is, that vibratory motions of gross atoms are supposed to originate the forces by which such atoms are primarily aggregated into masses, whereas it is essential to the possibility of such vibrations that contiguous atoms should exercise a mutual action upon one another—that is, be previously aggregated. We must suppose, then, the existence originally of other forces, to bring isolated atoms together and make the supposed forces due to vibratory motions of the atoms possible; that is, these latter forces become possible only when there is no longer any further occasion for them. We have seen that another possible origin may be ascribed to such oscillatory waves that does not involve the physical impossibility just referred to, from which those who seek for the key to all molecular phenomena in the motions of gross atoms can hardly escape.

3. The notion advocated by Tyndall in his admirable work on 'Heat considered as a Mode of Motion,' that heat and light originate in a vibratory motion of ordinary atoms, involves the supposition that these atoms are capable of vibrating at the astonishing rate of six hundred trillion vibrations in a second, while the most rapid vibration of atoms, or of a collection of atoms, known to take place in the production of sound, does not exceed 24,000 per second. It may be conjectured that this immense chasm may be spanned by the idea that the ultimate particles of bodies are immeasurably smaller than any collection of atoms which may be simultaneously vibrating when bodies emit sound; and that since a musical string vibrates more rapidly in proportion as it is shorter, a single particle may vibrate at an inconceivably rapid rate by reason of its exceeding minuteness. But the analogy here supposed does not exist as a physical fact, and no such inference can be drawn from it; for the rate of vibration of the string depends upon the distance between its two fixed points, but in no proper sense can it be said that two particles between which another is situated, are fixed, so as to be incapable of taking on the motion imparted to the intermediate one. So far from this being the case, the displaced particle can only vibrate by reason of the reaction of the contiguous particles to the action which it exercises upon them; and in receiving this action the motion must be transmitted. If, to remove the difficulty, we conceive the particle to oscillate as if it were wholly isolated, in union with an oscillatory wave falling upon it, we then fall upon the second objection stated above, and seek in vain through the universe for the vibratory

motion of atoms of ordinary matter in which this wave conveying such wonderfully rapid vibrations can have originated. We at the same time remove the necessary foundation for the explanation of a variety of special facts and phenomena, which require the assumption of special rates of vibration, proper to the particles of different bodies—as the different colours of bodies, &c.

Again, if the rates of vibration of ultimate particles depend upon the mutual actions subsisting between the displaced particle and those adjacent to it, the vibrations in which the heat-force is supposed to consist, should be propagated from particle to particle, just as any mechanical force is; in other words, heat should be conducted after the same manner, essentially, and at the same rate, that sound is conducted by the same medium.

By ascending to the reservoir of primary force, from which all the different streams of force flow, as has been attempted in this communication, we may avoid some of the difficulties attending the rejection of the idea of the existence of an electric æther; and in many portions of the field of physical science the part played by the electric æther is so similar to that which we may suppose would be performed by the universal æther under similar circumstances, that the suspicion at times arises that all the offices now attributed to the former will eventually be found to be discharged by the latter. If so, the processes of operation will not of necessity be changed, but only the agent or medium.

Admitting that the molecular forces consist of two forces of repulsion and one of attraction, as characterized on pp. 195–6, let us proceed to inquire into the variations that may occur in the effective action of two similar molecules, separated by various intervals of distance. Let $x=$ the distance between two molecular atmospheres; $r=$ the radius of either atmosphere; $m=$ the constant of electric repulsion, that is, the force of electric repulsion exerted upon either atom when $x=1$; and $n=$ the constant of the electric attraction, which will also be the constant of the equal force of repulsion propagated from the surface of the atom through the universal æther. Also let $u=$ the force of electric repulsion, and $v=$ the excess of the attractive force over the æthereal repulsion developed by the attraction,—all the forces being considered as taking effect upon the central atom. The effective action exerted by either molecule upon the other will be the difference between the values of u and v. Denote it by f; then $f=v-u$. When the calculated value of f is positive the action will be attractive; when it is negative the action will be repulsive. We have for the force of attraction the general expression $\frac{n}{(r+x)^2}$; for the æthereal repulsion the expression $\frac{n}{(2r+x)^2}$; and for the electric repulsion $\frac{m}{x^2}$. Then

$$v=\frac{n}{(r+x)^2}-\frac{n}{(2r+x)^2}=\frac{n(3r^2+2rx)}{(r+x)^2(2r+x)^2}, \quad . \quad . \quad (1)$$

$$u=\frac{m}{x^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$f=v-u=\frac{n(3r^2+2rx)}{(r+x)^2(2r+x)^2}-\frac{m}{x^2}. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

As the two forces of electric attraction and repulsion have an entirely different origin, we have no reason to suppose that $n=m$; nor have we any means of ascertaining, on *à priori* grounds, their comparative values. But we can assume that $u=v$ for some supposed value of x, determine the ratio of n to m on this supposition, and calculate the values of f for various values of x, both greater and less than that for which we have taken $f=0$.

TABLE I.

$f=0$ when $x=20r$.		$f=0$ when $x=10r$.		$f=0$ when $x=5r$.		$f=0$ when $x=3r$.		$f=0$ when $x=2r$.	
$n=12·410m$.		$n=7·576m$.		$n=5·428m$.		$n=4·938m$.		$n=5·143m$.	
x.	*f*.	*x*.	*f*.	*x*.	*f*.	*x*.	*f*.	*x*.	*f*.
0·5*r*	−0·470 *k*	0·5*r*	−1·8450*k*	0·5 *r*	−2·4560 *k*	2·0*r*	−0·00996*k*	1·8*r*	−0·00881*k*
0·6	+0·2341	0·7	−0·4586	1·0	−0·2461	2·5	−0·00075	1·9	−0·00361
0·7	+0·5510	0·9	−0·0368	1·5	−0·01906	2·7	−0·00001	1·95	−0·00164
0·9	+0·7275	1·0	+0·0522	1·672	0·00000	2·8	+0·00009	2·0	0·00000
1·0	+0·7236	1·2	+0·1310	1·7	+0·00207	2·9	+0·00008	2·1	+0·00246
1·3	+0·6146	1·3	+0·1447	2·0	+0·01386	3·0	0·00000	2·2	+0·00408
1·4	+0·5709	1·4	+0·1497	2·2	+0·01576	3·2	−0·00034	2·3	+0·00508
1·5	+0·5360	1·5	+0·1492	2·25	+0·01585	3·5	−0·00102	2·4	+0·00563
2·0	+0·3533	1·7	+0·1398	2·3	+0·01584	4·0	−0·00215	2·5	+0·00586
5·0	+0·0514	2·0	+0·1183	2·5	+0·01506	5·0	−0·00360	2·7	+0·00568
10·0	+0·0064	4·0	+0·0301	2·75	+0·01322	6·0	−0·00416	3·0	+0·00460
15·0	+0·00109	5·0	+0·0158	3·0	+0·01102	7·0	−0·00421	4·0	+0·00036
20·0	0·00000	7·0	+0·00444	4·0	+0·00384	8·0	−0·00404	4·1	+0·00003
25·0	−0·000265	10·0	0·00000	5·0	0·00000	9·0	−0·00377	4·5	−0·00109
30·0	−0·000317	15·0	−0·00107	6·0	−0·00181	10·0	−0·00349	5·0	−0·00211
35·0	−0·000306	20·0	−0·00097	7·0	−0·00280	15·0	−0·00224	7·0	−0·0036
40·0	−0·000280	30·0	−0·00061	8·0	−0·00289	20·0	−0·00150	8·0	−0·0036
80·0	−0·000110	40·0	−0·000413	9·0	−0·00292	40·0	−0·000487	10·0	−0·0032
		80·0	−0·000128	10·0	−0·00283	80·0	−0·000138	15·0	−0·00215
				15·0	−0·00202			20·0	−0·00146
				20·0	−0·00141			40·0	−0·000481
				40·0	−0·00047			80·0	−0·000137
				80·0	−0·000136				

The preceding Table contains the results of numerous calculations made after this manner, in which k stands for $\frac{m}{r^2}$.

From these results it appears that for values of $\frac{n}{m}$ greater than 4·938, or thereabouts, there are *two alternations of the effective force* f *as the distance between the molecular atmospheres increases indefinitely from zero.* The first is from a repulsion to an attraction; the second is from an attraction to a repulsion. The repulsion which becomes effective beyond the limit of the attraction at first increases, and then decreases, extending to an indefinite distance. If the ratio $\frac{n}{m}$ be less than about 4·938, the effective action of the two molecules upon each other will be repulsive at all distances. It will be observed also that the range of distance within which an attractive force takes effect is greater in proportion as the value of $\frac{n}{m}$ is greater, and that this becomes reduced nearly to zero when this ratio is equal to 4·938; also that in all cases in which an effective attraction manifests itself at any distance whatever between the molecules (that is, in the case of every known solid and liquid), the effective repulsion within the limit of the attraction obtains at less distances between the electric atmospheres of the molecules than about $3r$,—that is, than once and a half the diameter of either atmosphere.

For the more accurate determination of the least value of the ratio $\frac{n}{m}$, we have the following results of computation:—

For $f=0$ when $x=3r$, $\frac{n}{m}=4·93827$; for $f=0$ when $x=2·9r$, $\frac{n}{m}=4·93449$; for $f=0$ when $x=2·8r$, $\frac{n}{m}=4·934409$; for $f=0$ when $x=2·7r$, $\frac{n}{m}=4·93847$. If then the ratio $\frac{n}{m}$ be greater than 4·9344, the two alternations of effective molecular force above mentioned will have place; if the ratio be less than 4·9344, the effective action of the one molecule upon the other will be repulsive at all distances.

It is assumed in the foregoing calculations that the surface of each molecular atmosphere which receives the impulses, whether attractive or repulsive, propagated from the other through the intervening electric æther, may be regarded as the same as that from which the electric repulsion proceeds outward; but it will be readily seen that they may be supposed to differ within certain limits, without vitiating the result that for certain values of

$\frac{n}{m}$ two alternations of the effective force will subsist. The forces may also experience losses, to a certain extent, in their propagation, and this general principle still hold good.

It should also be observed that when two particles are remote from each other, as in the case of a particle of cometic matter repelled by the sun, we must suppose the intervening space to be occupied by the universal æther only. In such a case, then, both the attractive action and the *electric* repulsion will be wanting, and the only force remaining will be the *æthereal* or *heat-repulsion*; which should operate at indefinite distances, according to the law of inverse squares. A discussion of the phenomena of evolution of cometary envelopes, and of the outstreaming of jets of nebulous matter from particular parts of the surface of the nucleus, must be had before we can decide how far the *electric* repulsion may be in operation in the processes of ejection of cometic matter from the nucleus*.

The general law of the variations of the force of effective molecular action is graphically represented by the curve $r\ a\ m\ c\ n$ in fig. 1. The abscissas represent the comparative distances between the electric atmospheres of the two molecules, and the ordinates the intensities of the effective force corresponding to these distances. When the ordinate lies above the axis of abscissas, the force is attractive; when it lies below, the force is repulsive. The two axes are asymptotes to the curve. The curve has been constructed from the calculated results obtained on the supposition that $f=0$ when $x=5r$.

There are four points, marked a, b, c, d, to be especially noted. a and c, where $f=0$, represent positions of equilibrium, a being a position of stable, and c of unstable equilibrium. When the atmospheres are separated by the distance O b the attraction has its maximum value, $b\,m$; and when they are at the distance O d, the repulsion, beyond the outer limit of the attraction, has its maximum value, $d\,n$. In order that two particles may unite

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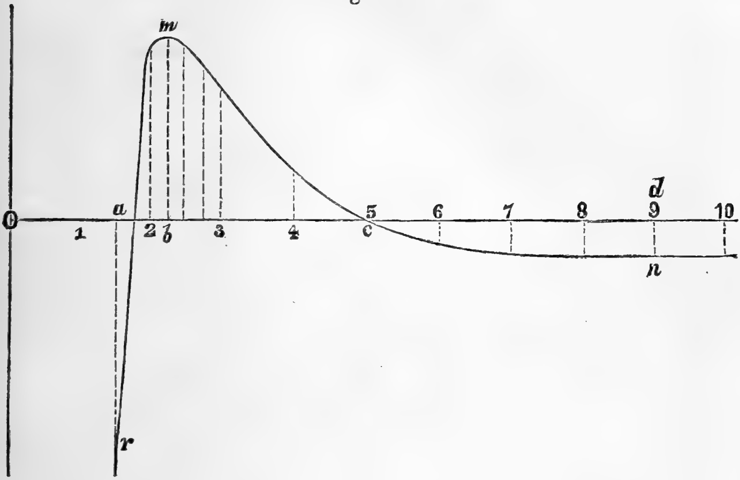
* In the memoir by the author "On the Theoretical Determination of the Dimensions of Donati's Comet," published in No. 87, vol. xxix., and in No. 94, vol. xxxii. of Silliman's American Journal, the conclusion was reached, as one result of the computations, that "the repulsion exerted by the sun, and also by the nucleus (of the comet), is not a property belonging to all the particles of the mass, like the attraction of gravitation; and is probably therefore a force emanating from the surface of the body, or from a portion only of its mass." We now see that the existence of such a force is also a legitimate deduction from the theory of molecular forces under consideration, and that it consists in the force of æthereal repulsion, which we have denominated *heat-repulsion*. Its impulses constitute the entire force of radiant heat given off by the body into free space, and vary in intensity or amount with the temperature.

when influenced by their own proper forces only, the distance between their atmospheres must be less than O*c*. But if they are subject to an external pressure urging them toward each other, it suffices that this pressure should exceed the maximum repulsion *d n*, at the greater distance O*d*. To separate the particles, the force in operation must exceed in intensity the maximum molecular attraction, *b m**.

Fig. 1.



If heat be imparted to the two particles under consideration, it will obviously tend to depress the entire curve of molecular action, and diminish the range *a c* of the attractive force. If the amount be continually increased, the distances between the two positions of equilibrium, *a* and *c*, will eventually be reduced to zero, and the curve thrown entirely below the axis of abscissas, or the effective force become a mutual repulsion at all intervals of distance between the particles. This would be the necessary tendency of the introduction of new repulsive impulses into the system, even if the original forces continued in full

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* To guard against misapprehension, it may be well to observe here that the resisting force of cohesion which is brought into play when a body is ruptured by a pulling force is not necessarily proportional to the intensity of the maximum force of attraction between two of its particles. For it must depend, not only upon the intensity of this attraction, but also on the number, distance, and position of the other particles that oppose, by their attractive action, the separation of the two. Incidentally the displacement these particles may experience, from the action of the rupturing force, comes in as a modifying cause.

operation as before; but heat, by expanding the molecular atmospheres, should also tend to diminish the ratio of n to m, and therefore to modify in a similar manner the natural curve of molecular action.

It is easy to see that a similar curve will also serve to represent the mutual actions of dissimilar molecules, which obtain when a chemical union is formed between two different substances. But in this case a force of electric attraction not yet considered may come into play.

The general principle of two alternations of the effective action of molecules, to which we have been theoretically conducted, is distinctly recognized as a physical fact in the three different states of aggregation of matter—in the molecular attraction and repulsion manifest in solids and liquids, and in the mutual repulsion subsisting between the same particles when widely separated in the vaporous condition. It will be seen hereafter that the law of molecular action, as portrayed in the curve shown in fig. 1, furnishes, in the probable variations of the ratio $\frac{n}{m}$, an adequate general cause for the varied results of this action exhibited in the different properties of substances; and at the same time reveals the probable explanation of many physical and chemical phenomena occurring at surfaces of contact, and of the dependence of these phenomena upon temperature and other circumstances, as in oxidation, combustion, &c.

From the stand-point now taken, new views open up to us on all sides. The more conspicuous of these we will proceed to sketch, in general outline, under the several heads of the *Molecular Constitution and Mechanical Properties of Bodies, Heat, Light, Electricity, Magnetism,* and *Chemical Action.* The intimate relations subsisting between the phenomena occurring in these different departments of Nature, and between the special agents by which they are produced, or the "correlation of the physical forces," will be seen to be deducible from the fundamental conceptions adopted. It will be observed that all these varied phenomena are but different results of the action of the primary forces, which consists in an attraction of atoms for their atmospheres, and in a mutual repulsion between the atoms of these atmospheres; that they are, primarily, movements or disturbances produced in these subtile atmospheres, from which æthereal waves of impulses and motions of molecules and masses may result.

[To be continued.]

XXIII. *On the Operating Symbol of Differential Covariants. By* The Honourable Chief Justice COCKLE, *President of the Philosophical Society of Queensland**.

CONSIDERING the complete form of differential cubic, the operating symbol Δ, of my paper "On Differential Covariants" in the last March (1864) Number of this Journal, will become

$$\delta - a\frac{d}{db} - 2b\frac{d}{dc} - 3c\frac{d}{df};$$

or rather we ought to say

$$\Delta = \delta - \left[a\frac{d}{db}\right] - 2\left[b\frac{d}{dc}\right] - 3\left[c\frac{d}{df}\right],$$

in order to mark the indissoluble nature of the bond which connects the elements, or constituents, of the constituents or elements of the operator. Moreover a, b, c, f, and x being treated as independent, we have

$$\Delta\frac{d}{dx} = \frac{d}{dx}\Delta,$$

or, more fully,

$$\Delta\mathrm{K}' = \Delta\frac{d\mathrm{K}}{dx} = \frac{d}{dx}\,.\,\Delta\mathrm{K}.$$

Illustration will probably serve all the purposes of a more laboured exposition, and the use of the brackets in giving clearness and precision to the operations will appear from the following transformation:

$$\left[b\frac{d}{dc}\right](bc') = b\left[b\frac{d}{dc}\right]c' = b\left[b\frac{d}{dc}\right]\frac{dc}{dx} = b\frac{d}{dx}\left[b\frac{d}{dc}\right]c = b\frac{db}{dx} = bb'.$$

If $\Delta\mathrm{K}$ vanishes, the commutative property of $\frac{d}{dx}$ and Δ enables us to perceive that $\Delta\mathrm{K}'$ vanishes also. That $\Delta\mathrm{K}'_2$ vanishes may be shown directly, thus:

$$\begin{aligned}\Delta\mathrm{K}'_2 &= \Delta(2bb' - a'c - c'a + ab'' - ba'')\\ &= -2ab' - 2ba' + 2a'b + 2ab' - aa'' + aa''\\ &= 0.\end{aligned}$$

If we employ differentials instead of differential coefficients, the operator for

$$(a, b, c, f \between d, dx)^3 y$$

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* Communicated by the Author.

is

$$dx\,.\,\delta-\left[a\frac{d}{db}\right]-2\left[b\frac{d}{dc}\right]-3\left[c\frac{d}{df}\right].$$

In these last expressions dx is to be treated as if it were an ordinary symbol of quantity, and δ is now defined by the equation

$$\delta d^m y = m d^{m-1} y.$$

Brisbane, Queensland, Australia,
May 30, 1864.

XXIV. *On the Conditions of Stability of thin Films of Liquids; a Report by* Professor PLATEAU *on a Memoir by* Professor LAMARLE *of Ghent**.

IN the Sixth Series of my researches "On the Figures of Equilibrium of a Liquid Mass without Weight"†, I have established, in part experimentally and in part theoretically, the laws which relate to films terminating in a common liquid edge, and to liquid edges which meet at the same liquid point. From these laws I drew the conclusion, which I have tried to confirm by experiment, that every equilibrated system of films in which they are not fulfilled is an unstable system, and I ended this series by saying:—

"I will return once more to systems of films, in order to consider the theory of them from a more general point of view. In fact, as I have already observed, the liquid films of which they are made up may be compared to stretched membranes, and hence it will be seen that each system will arrange itself so that the sum of the surfaces of all its films will be a minimum. But I reserve this point for a future Series."

In expressing myself thus, my intention simply was to take as examples certain particular systems of films, which from their simplicity are capable of being made the subjects of direct calculation, and to show that the sum of the surfaces of the films is a minimum relatively to some one mode of deformation; but I had no intention of treating the problem in all its generality, for I did not consider it practicable to do so. I perceived that there must exist a necessary connexion between the principle of the minimum sum of the areas and my laws; but I could not seize the precise nature of this connexion, and it seemed to me next to impossible to do so. All these difficulties, however, have been

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* Communicated by Professor Plateau, from the *Bulletin de l'Académie oyale de Belgique*, 2^{e} sér. vol. xvii. No. 6.

† Phil. Mag. S. 4. vol. xxiv. p. 128.

overcome with wonderful sagacity and rare good fortune by M. Lamarle.

He begins by establishing, more definitely than I had done, the above-mentioned principle of the minimum; and then, starting from this, he discusses the case of films meeting at the same liquid edge. He supposes any number of plane films starting from solid edges and all uniting in one common liquid edge, and imagines the whole cut by a plane perpendicular to this latter. The section consists of straight lines starting respectively from fixed points and all meeting at the same point; and he shows first, by elementary geometrical considerations, that if the straight lines are three in number, their sum will be a minimum when they make equal angles with each other. If the straight lines are more numerous, he shows, by considerations equally simple, that, in order that their sum may be the smallest possible, the single point of junction must be replaced by several points connected together by additional straight lines in such a way that at each of these points there are only three straight lines making equal angles with each other. Lastly, the diminution in the sum of the straight lines beginning with the commencement of these modifications (that is to say, for example, in the case of there being more than three straight lines, as soon as the point of junction divides so as to give rise to new straight lines and points), it follows that the demonstration applies equally to curved lines, for these may always be replaced by their tangents in the immediate neighbourhood of the point of junction. M. Lamarle then shows that all these results extend to the films themselves, all of which, whether plane or curved, are cut by the plane in question; that is to say, the minimum of the sum of the areas requires that these films should meet three by three under equal angles in each liquid edge.

My first law—namely, that in every stable system of liquid films more than three films never meet in the same edge, and that these always make equal angles with each other—is thus completely demonstrated, and appears as a consequence of the principle of the minimum.

M. Lamarle next considers the question of liquid edges meeting at the same liquid point. In dealing with it, he supposes a number of plane liquid films all meeting at the same point in the interior of the system, and he investigates the conditions which these films must satisfy in order that they may meet three by three, forming equal angles with each other, conformably with the foregoing law. He considers the point which is common to all the planes as the centre of a sphere, which is therefore cut by them along arcs of great circles: we have thus a number of hollow pyramids whose summits lie in the same point, and whose bases are spherical polygons having angles of 120° exclusively. M. Lamarle points out in the first place that these polygons can only be either triangles, four-sided figures, or pentagons, and hence he derives one analytical relation between the respective numbers of these several kinds of polygons and the total number of films; a second relation he finds in the fact that the sum of the surfaces of all the polygons must make up the whole surface of the sphere; lastly, these polygons must all be in simple juxtaposition, without encroaching upon each other in some places and leaving empty spaces at other places. From these three conditions, M. Lamarle deduces that there are only seven possible combinations of films starting from the same point, and joined together three by three under equal angles.

If, in each of these combinations, the sides of the spherical polygons are replaced by their chords, we have the edges of a complete polyhedron, and the seven polyhedrons thus formed are,—the regular tetrahedron; the right triangular prism with equilateral base, the height being in a determinate ratio to the length of the side of the base; the cube; the right pentagonal prism with regular base, the height being in a determinate ratio to the length of the side of the base; two peculiar polyhedrons made up of four-sided figures and pentagons; and, lastly, the regular dodecahedron. In these polyhedrons the numbers of liquid edges are 4, 6, 8, 10, 12, 16, and 20 respectively.

Now M. Lamarle proves that for each of these systems of films, except that of the regular tetrahedron, it is possible to conceive such a mode of displacement that, from its commencement up to a certain limit, a diminution of the sum of the areas of the films will result from it: the system of the regular tetrahedron, in which not more than four liquid edges meet at the same liquid point, and make equal angles with each other, is therefore the only one of these which can be stable. Hence, when the films are plane, the liquid edges which meet at any one liquid point are necessarily four in number and make equal angles with each other. Lastly, M. Lamarle shows that the same conclusion applies to curved films, and, by consequence, to curved edges; there is, in fact, no limit to the smallness which the above-mentioned sphere may be conceived to have, and hence we are free to conceive of it as so minute that the portions of the films contained within it may be regarded as plane.

My second law—namely, that in every stable system of liquid films the number of liquid edges meeting in any one liquid point is always four, and that they make equal angles with each other —is thus demonstrated by M. Lamarle as completely as the first, and like it, is deduced from the principle of the minimum.

It may be added that the modes of displacement supposed by

M. Lamarle, and which, by an ingenious way of treating them, he is able to refer all to the same principle are exactly those which lead to the real results obtained—to those, that is, given by the experiments with the skeletons of iron wire.

To sum up, M. Lamarle has solved questions which seemed extremely difficult, and his investigation is an important contribution towards a complete theory of liquid films.

XXV. *On some Effects produced by a Fluid in Motion.*

By George F. Rodwell, *F.C.S.*[*]

[With a Plate.]

No. II. *On the Trompe.*

A STREAM of water falling through a tube may, under certain conditions, be caused to carry down air with it, which air, when removed from the direct influence of the stream, is able in virtue of its small specific gravity compared with water, to separate itself therefrom, and may be collected and employed for any of the purposes for which a blast of air is requisite. An arrangement for producing a blast by the above means has been in use for the last 200 years, and is known as a *trompe*.

I.

Baptista Porta is said to have invented a machine for producing wind by the flow of water; but among the numerous hydraulic machines which he figures in his work, entitled 'I tre Libri de' Spiritali' (1606), I am unable to find one which bears the least resemblance to a trompe; neither, so far as I am aware, is the trompe mentioned in the 'Mechanica Hydraulico-Pneumatica' of Gaspar Schottus, nor in the 'Mundus Subterraneus' of Athanasius Kircher, although the latter was well acquainted with the discovery.

Grignon[†] states that the trompe was discovered in Italy about the year 1640, but he gives no authority for the assertion. The earliest account of the invention which I have been able to find is in a work by Father Jean François, published in 1655[‡], in which there is a section entitled "Du Meslange des Eaux avec l'Air, et d'une invention pour exciter un vent impetueux." In this section François states that it had been *recently* discovered that water is capable not only of dragging along with it terrestrial bodies, but also air; and that an arrangement for supply-

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[*] Communicated by the Author.

[†] *Mémoires de Physique sur l'art de fabriquer le fer, d'en fondre et forger des canons d'artillerie,* &c. Paris, 1775.

[‡] *Les Sciences des Eaux, qui explique en quatre parties leur formation, communication, mouvemens, et meslanges.*

ing furnaces with air by this means had been invented, and was extensively used. Although a new invention, it was at this time (1655) used in Germany and Italy, and in more than a hundred places in Dauphiny.

The machine described by François consisted of a channel or trough in which water was caused to flow in a horizontal direction: from the bottom of the trough two vertical tubes proceeded, and the lower extremity of each passed into a wooden tub furnished with an outlet for water, and a tube near its upper part for the exit of air: within each tub, and immediately beneath the lower extremity of the tube which entered it, a large convex stone was placed in order to break the fall of the water; the upper extremity of each tube near its juncture with the trough was enlarged and of a conical form, but in some forms of the machine the tubes had holes pierced in their circumference for the purpose of giving freer access to the air. François mentions that Kircher assured him that he had seen more than forty of these machines with tubes in an inclined position.

In the first volume of the Philosophical Transactions (1665) there is a short description*, accompanied by a figure of a water-blowing machine used in Italy. A stream of water is represented flowing through a large, vertical, rectangular tube, midway between the top and bottom of which there is a pipe for conveying the blast to a furnace; but the machine is evidently wrongly figured and described, for it is impossible that a current of air could be produced by such an arrangement.

In the 43rd volume of the Philosophical Transactions there is a paper (read March 1744–45) by a Mr. James Stirling, entitled "A Description of a Machine to blow fire by the fall of Water." This machine was a modified form of that described by François; it consisted of a large funnel 5 feet high, from the apex of which proceeded a tube from 14 to 16 feet long; the lower extremity of the tube passed into a vessel 5½ feet diameter, provided with a tube for the exit of the air carried down, and also with an outlet for water; four orifices were made in the circumference of the tube just beneath its juncture with the funnel.

From the fact of this paper having been read before the Royal Society, it would appear that the trompe was but little known in England even a hundred years after its invention; but that this was the case is scarcely to be wondered at when we remember that such a machine can only be used with advantage in

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* Extract of a letter lately written from Venice by the learned Doctor Walter Pope, to the Reverend Dean of Rippon, Doctor John Wilkins, concerning the Mines of Mercury in Friuli; and a way of producing wind by a fall of water.

mountainous countries, where small streams of water with a conconsiderable fall abound. The trompe described by Mr. Stirling was used in a Scottish lead mine.

I am not aware when the water-blowing machine was first called a *trompe*; in neither the account given by François, nor in Dr. Pope's letter is the word employed, and nearly a hundred years later Mr. Stirling gives no name to the machine; but it was called a trompe in France long before his paper was published.

Grignon* writes as follows: "Les trompes ont tiré leurs noms de ces météores qui portent le même nom, et dont il y a deux sortes, l'une marine et l'autre terrestre: celle qui se forme sur mer est une colonne d'eau immense, enlevée par la violence du vent; et celle que l'on voit sur terre est formée par une tourbillon de vent dont le mouvement est determiné par les montagnes; ce tourbillon enveloppe un nuage, le comprime et en forme une colonne composée d'air et d'eau qui se précipite sur la surface de la terre ou se brise contre un rocher."

The Italian *tromba*, French *trombe*, signifies a pump, an elephant's trunk, and a trumpet, as well as a waterspout: the Spanish *trompa* signifies a trumpet, an elephant's trunk, and a large top. Prof. Max Müller informs me that *trompe* is the old French form of *trombe*, and that Dier, in his 'Lexicon Etymologicum,' derives trombe in the sense of trumpet from the Latin *tuba*, the insertion of the *r* being warranted by the analogy of *tronar* instead of *tonar*.

Landais, in his 'Dictionnaire des Dictionnaires,' derives *trombe* in the sense of waterspout from *στρόμβος*. Now *στρόμβος* means a top, a spindle, a round shell, and is derived from *στρέφω*, to turn, twist, revolve. I will not pretend to decide whether (adopting Dier's derivation) a trumpet and a waterspout were called by the same word on account of their being tubiform, or whether the trumpet was so named from the fact of its being the first musical instrument with a straight tube†, and the waterspout afterward snamed from its trumpet-shape; or whether (adopting Landais's derivation, and this seems to me the most plausible) the waterspout was first called a trombe (from *στρέφω*); the trumpet and elephant's trunk from their being shaped like a waterspout; the top from its rotatory motion; and the pump from the fact of water being apparently drawn up in it, in the same way that it is drawn up in a waterspout.

I conceive the water-blowing machine received the same name as a waterspout, either because of the mixed column of air and water produced in certain forms of the machine, or, more pro-

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* In the work cited above.

† *Tuba* meant originally a *straight* tube, as opposed to *cornu*.

bably, from the whirling motion of the water around a conical cavity, which, as we shall see hereafter, is produced in the trough of machines similar to that mentioned by François, and in all machines with inclined tubes: in both these respects there is a resemblance to a waterspout.

Several modifications of the trompe have been constructed since its first invention, the main difference consisting in the way in which air is allowed to enter the tube: thus there is the trompe without orifices for the admission of air, as in that described by François, and in the trompe mentioned by Mariotte (in his *Traité du Mouvement des Eaux*), in which latter modification a blast is produced by a stream of water falling from a height of 10 or 12 feet into a funnel which communicates by a vertical tube with a vessel for collecting the air carried down; or, again, there may be orifices in the tube, as in the trompe described by Mr. Stirling, and in that now used; or air may be admitted by two small tapering tubes which enter the trompe tube just below its juncture with the water-cistern, and pass upwards through the water therein till they reach the air: this latter arrangement was much used formerly.

The modern trompe consists of a large cistern in which there is a constant depth of from 4 to 6 feet of water; from the bottom of the cistern proceed two tubes from 20 to 30 feet long, the lower extremities of which pass into a wooden wind-chest furnished with an arrangement for keeping the water at a certain level, so that no air can escape except by a blast pipe in the upper part of the chest: beneath the lower extremity of each tube there is a flat iron plate to break the fall of the descending water. The upper part of each tube is contracted at the point where it joins the cistern, and immediately beneath the contracted part four holes are made in the circumference of the tube. When water is allowed to flow from the cistern into the air-chest, a quantity of air is carried down with it, and a perfectly regular and constant current of air issues from the blast pipe.

II.

I have endeavoured in this paper to ascertain the most favourable conditions under which air is carried down by a stream of water, and to arrive at a satisfactory explanation of the cause of the descent of air in the different modifications of the trompe.

In a former paper * I have given an account of some experiments made with a view of determining the quantity of air carried down by a known amount of water; but as the method there employed was scarcely so satisfactory as could be desired, I

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* "On some Effects produced by a Fluid in Motion," No. I., Philosophical Magazine for January 1864.

subsequently devised the apparatus of which the following is a description.

A, Plate III. fig. 1, is a conical glass vessel closed above and below by corks; through the upper cork passes a glass tube B, 24 inches long and $\frac{21}{60}$ths of an inch internal diameter; 3 inches of B are within the vessel A, and a scale of inches is attached to that part of B outside A; a tube C, $\frac{2}{15}$ths of an inch diameter, communicating with the interior of A, is bent twice at right angles, and passes into a circular glass vessel D, which has a scale of inches attached to it, and is provided with a stopcock E. A tube F of small diameter communicates with the interior of A both above and below; a scale is attached to it, and by it the absolute level of water in A is shown; through the lower cork of A passes a stopcock G. H is a glass tube communicating with the water-cistern by means of a caoutchouc tube, I I I; a similar caoutchouc tube, K K, is connected with the stopcock G, and the two tubes are brought together and can be closed or opened simultaneously by the spring clip L. A half-litre flask, M, collects the air which is carried down by the descending stream. The dotted lines *a, b, c, d, e* show the different levels at which water was caused to remain constant in A during separate experiments.

The water-cistern was about 8 feet from the apparatus, and the water within it was kept at a constant level of 2 feet above the orifice of the delivery-tube H. The caoutchouc tube, I I I, was of such diameter that it delivered half a litre of water in 16 seconds at the height of the tube H.

In order to prepare the apparatus for an experiment, water is placed in the vessels A and D to the level required in each; the flow from H is determined, and the orifice of H is then brought on a level with that of the tube B, and so placed that when water is flowing, it passes exactly down the axis of B. The efflux from H remaining constant, the efflux from A is made equal to it; this is done by keeping the eye on the scale of F and turning the stopcock G until the water-column in F remains perfectly immoveable at the desired level; the caoutchouc tubes I I I and K K are then brought together at one point, and are closed by the spring clip L; finally, the half-litre flask M is filled with water, and inverted over the orifice of the tube C. A watch, suspended behind a large lens, is placed in such a position that the eye can observe both it and the level of water in D with as little difficulty as possible. Immediately before an experiment, the spring clip L is removed from the two caoutchouc tubes, and they are kept closed by the thumb and forefinger of the left hand so as to be capable of instant and simultaneous release; the right hand is placed on the stopcock

E, and the eyes are fixed on the second hand of the watch; at a certain second the caoutchouc tubes are released at L, E is simultaneously turned, and the level kept constant in D during the experiment; when the half-litre flask M is full of air, the caoutchouc tubes are simultaneously closed and the exact second noted. By repeating this several times, we determine the time to a part of a second required by a stream of water flowing at a known rate to carry down half a litre of air under the conditions of the experiment.

It is obviously necessary that the level in A should remain perfectly constant during an experiment—that is to say, that the influx and efflux should be equal; for if the efflux from A be greater than the influx, less than the real amount of air carried down will make its appearance in M; if, on the contrary, the influx be greater than the efflux, more than the real amount of air carried down will appear in M: it is also necessary to keep the level in D constant, otherwise the pressure on the orifice of C, and consequently on the air in A, will vary. It is difficult to cause the descending current of water to pass exactly down the axis of the tube B, and a very slight variation from that position greatly diminishes the amount of air carried down; when perfectly in the axis, a peculiar roughness is added to the roaring sound produced; but in order to remove any possible source of error from this cause, the amount of air carried down by the stream from each delivery-tube, under certain known conditions, when the stream passed absolutely down the axis of B was determined, and before each set of experiments, the same conditions were made to obtain, and the delivery-tube adjusted until the standard amount of air, previously found to be carried down under those conditions, was carried down, when (the stream being known to be in the exact axis of B) any other conditions, as to pressure &c., not involving the removal of the delivery-tube could be made to obtain. Great care was taken that the whole apparatus should be perfectly vertical before each set of experiments, and the whole was made as immoveable as possible.

It may be considered unnecessary to give these details of experiment, but I am well aware that results are often doubted because their author has neglected to state the exact method by which he obtained them.

Before giving the results obtained by the apparatus described above, it will be as well to consider some facts connected with a water-jet.

A jet of water moving with considerable velocity from above downwards is observed to widen gradually as it gets further from a point just below the orifice from which it issues; and at a certain distance from the orifice the velocity of the particles becomes so greatly accelerated by gravity, that the cohesion of two contiguous cross sections of the jet is destroyed, and the jet, ceasing to be continuous, breaks up into separate masses of water. We observe, moreover, that when the continuity begins to be broken, particles of water are thrown out from the centre stream and move downwards in a gradually diverging line inclined at a small angle to the main stream. Now when such a stream passes downwards through a long vertical tube, the area of the cross section of which is not many times as great as that of the orifice from which the stream issues, we find that at a certain distance from the orifice the quantity of spray thrown off from the stream which reaches the sides of the tube is so great that, by the adhesion of the water-particles to the glass, the whole stream is dragged to the sides of the tube; it is not, however, dragged to *one* side of the tube, as would be the case if the jet were not perfectly vertical, but it is evenly and equally spread out in a dome shape, and for the rest of its course flows as a tube of water. If the lower orifice of the tube through which it flows is perfectly free, the water as it passes out forms a long conical bag filled with air.

Above the point where the jet is spread out by the adhesion of the glass there is a good deal of spray; and although it is insufficient to drag the jet to the sides of the tube, it accumulates; the tube is thus narrowed at certain points, and disks of water are formed which are pushed down by the stream above and force down air beneath them: at the moment of the formation of a disk the conical bag of water at the end of the tube is expanded laterally by the air forced into it, and bursts, delivering up its contents; this bursting occurs with great rapidity under certain conditions.

The pressure on the lower orifice of the vertical tube through which the stream passes also tends to the formation of disks, because the descending masses of water will have a tendency to be flattened out by the air beneath them; the greater the pressure, the higher within certain limits will disks be formed; but the quantity of air forced down will be less, because the chance of the rupture of the disks will be increased; and when this occurs, the air beneath them escapes upwards between the descending stream and the sides of the tube. The formation of disks can only happen below a certain point, because the stream above that point ceases to be sufficiently broad to allow of their being produced; the point varies with the nature of the stream, the relative dimensions of the tube and of the orifice from which the stream flows, and with the pressure on the lower orifice of the tube.

If a stream is very small compared with the tube through which it passes, or if there is sufficient pressure on the orifice of the tube to support a column of water at a height in the tube above that at which the stream is sufficiently broad to allow disks to be formed, the stream meets the water surface in direct collision, and air is then undoubtedly carried down, for the reason proposed by Prof. Magnus*. This physicist observed that small solid bodies, if allowed to fall into water, produce a cavity of greater or less depth, into which air enters; and the water, if the particles at its surface possess any motion (which they must do from the disturbance caused by the falling body), unites over the cavity and thus air is enclosed. Magnus conceives that a water-jet falling into water acts in a precisely similar manner, the detached masses of water forming cavities like solid bodies. We have a frequent example of this action of detached fluid masses: if we wish to pour out beer so that it shall have no froth, we pour it down the side of the glass, the adhesion of which flattens the stream into a ribbon, and it enters the fluid in the glass slowly: there is no falling of detached masses here, consequently no air is carried down; on the other hand, if we wish to produce froth, we pour out the beer from as great a height as possible, and the masses detached by the accelerating force of gravity carve out channels in the liquid, into which air enters and is carried down, and afterwards rises to the surface in bubbles.

Richard† states that a M. Mercadier constructed artificial trompes in which sand, wheat, rye, millet, salt, mercury, and lead shot were severally caused to fall into water in place of a descending stream of that liquid, and a considerable blast of air was obtained thereby.

The quantity of air carried down by a solid body falling into water is very surprising. I took small lead shot about $\frac{1}{12}$th of an inch diameter, and weighing ·072 gramme apiece, and threw them one at a time from a height of about a foot and a half into a vessel containing water to a depth of 6 inches; in order to collect the air, a wide-mouthed vessel was filled with water, inverted, and placed with its mouth near the water-surface in the vessel into which the shot was thrown. By throwing in a shot at an angle of about 60 degrees, and as near as possible to the edge of the inverted vessel, the air carried down could be collected, and I thus found that the lead shot caused no less than 190 times its own bulk of air at 18° C. to penetrate beneath the water. The same shot falling by its own weight from a height of 4 feet into 6 inches of water appeared to carry down quite as much air, but in this case it is obvious the amount could not be determined.

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* "On the Motion of Fluids," Phil. Mag. for January 1851, p. 8.

† *Etudes sur l'art d'extraire immédiatement le Fer de ses Minerais sans convertir le métal en fonte.* Paris, 1838.

No one who has fired a gun into water can have failed to observe the numberless bubbles of air which rise to the surface for a few seconds after the penetration of the shot. Each shot ploughs for itself a channel, into which air enters, is enclosed by the water, and penetrates to a greater or less depth according as the velocity of the shot is considerable or otherwise.

Let us now pass on to the experiments made with the apparatus described above.

The number of seconds required by half a litre of water to flow from the delivery-tube H, and the number of seconds required by that water to carry down half a litre of air, being determined in the manner stated above, the number of cubic centimetres of air carried down by half a litre of water could obviously be readily calculated. Each of the results given below is the mean of several determinations—generally of ten, except in those cases in which four or five consecutive determinations gave absolutely similar results.

Circular delivery-tube (H, fig. 1) $\frac{3}{20}$ths of an inch internal diameter. (The comparative dimensions of the delivery-tube and the vertical tube B are shown full size at A, fig. 2.)

Flow = half a litre of water in 24 seconds.

Level of water maintained constant in A during the experiment.	Depth of water maintained constant in D during the experiment.	Order of experiments.	Quantity of air carried down by half a litre of water when the orifice of the tube delivering the air (*c*, fig. 1) = $\frac{8}{60}$ths of an inch in diam.	Order of experiments.	Quantity of air carried down when the orifice of the tube delivering the air (*c*, fig. 1) = $\frac{1}{60}$th of an inch in diam.
At 2 inches below the orifice of B (*a*, fig. 1)	1 inch.	1.	615 cub. cent.	16.	136 cub. cent.
At 1 inch below the orifice of B (*b*, fig. 1)	,,	2.	615 ,,	17.	136 ,,
At the orifice of B (*c*, fig. 1)	,,	3.	545 ,,	18.	137 ,,
At 1 inch above the orifice of B (*d*, fig. 1)	,,	4.	201 ,,	19.	132 ,,
At 2 inches above the orifice of B (*e*, fig. 1)	,,	5.	174 ,,	20.	182 ,,
At *a*, fig. 1	inches.	6.	153 ,,	21.	122 ,,
,, *b*, ,, ...	,,	7.	153 ,,	22.	122 ,,
,, *c*, ,, ...	,,	8.	167 ,,	23.	113 ,,
,, *d*, ,, ...	,,	9.	157 ,,	24.	178 ,,
,, *e*, ,, ...	,,	10.	125 ,,	25.	171 ,,
,, *a*, ,, ...	5 inches.	11.	130 ,,	26.	171 ,,
,, *b*, ,, ...	,,	12.	130 ,,	27.	171 ,,
,, *c*, ,, ...	,,	13.	125 ,,	28.	171 ,,
,, *d*, ,, ...	,,	14.	113 ,,	29.	166 ,,
,, *e*, ,, ...	,,	15.	118 ,,	30.	165 ,,

In the first and second experiments, in which the resistance was least, we see that half a litre of water carried down 615 cubic centims. of air, a quantity (with one exception) more than three times as great as that carried down in any of the other experiments given above. The air escaped from C with perfect regularity, and the results were very constant. In experiment 3 the orifice of the tube B (fig. 1) was caused to touch the water-surface in the vessel A, and we find a diminution of 70 cubic centims. in the amount of air carried down; on further increasing the resistance by causing B to dip one inch under the water-surface (exp. 4), the quantity of air was at once diminished to 201 cubic centims.; the air-disks were now formed higher in the tube, and the air escaped from C irregularly, proving that the disks were constantly broken, and that the air beneath them escaped upwards; the highest of the disks formed at 15 inches from the orifice of H; the results were less constant than before. In experiment 5 the air came over still more irregularly; in other words, the disks were more frequently broken. In the 6th and 7th experiments we have obviously the same amount of resistance as in exp. 5, but the orifice of B was free. In experiment 8 we have precisely the same conditions as in exp. 5, and nearly the same amount of air was carried down. In experiment 5 the resistance was = 3 inches of water, for B dipped 2 inches under water, and there was 1 inch of water in D. So also in experiment 8 the resistance was = 3 inches, because there were 3 inches of water in D, and the orifice of B touched the water-surface. In experiment 9 there were 4 inches of water resistance, and in experiment 10, 5 inches. In experiments 11 and 12 the resistance was obviously the same as in experiment 10, but the orifice of B was free. In experiment 13 we have precisely the same conditions as in the 10th experiment, and the same amount of air was carried down. In experiments 14 and 15 there was still greater resistance; the highest of the disks formed at 13 inches from the orifice of H.

The conditions were now somewhat changed, the resistance to the descent of air into A being greatly increased in consequence of the air having to escape from an orifice one-eighth the size of that used in the first fifteen experiments. In the 16th, 17th, 18th, and 19th experiments the resistance, as judged of from the effects produced in the tube, was about equal to that in the 11th experiment. In experiment 20 the pressure was sufficient to keep a column of water suspended in B at an unvarying height of 11 inches from the orifice of H, and a persistent collision between the descending stream and the water-surface took place at that height: the air escaped from C in a perfectly regular manner, and the results were very concordant, not the least variation in the quantity of air carried down being observable in a number of consecutive experiments. In experiments 21, 22, and 23 the resistance was insufficient to keep a column of water suspended in B, and the disk action returned, the air escaping from C very irregularly. In experiment 24 there was persistent collision at about 11 inches from the orifice of H, and we get nearly the same result as in experiment 20. In experiments 25, 26, 27, and 28 the collision was persistent at 10 inches from the orifice of H, in experiment 29 at 9 inches, and in experiment 30 at 8 inches. We see, therefore, that in the above 30 experiments, air was carried down by disk action in 22 (viz. experiments 1 to 19, and 21 to 23); and by direct collision, according to the theory of Magnus, in 8 (viz. experiments 20 and 24 to 30).

The nearer the collision approached the orifice from which the jet issued, the less was the quantity of air carried down; and when the water in B touched the orifice of H, air only entered at intervals, and was seen to force its way through the water into the line of the descending jet. This latter effect is due to the diminution of pressure in the line of a moving fluid,—an effect which may be shown by introducing a small mercury gauge into the line of a current of liquid flowing in any direction; or in a more striking manner if we allow a liquid to flow from a delivery-tube A (fig. 3), dipping beneath the surface of water in a vessel B, and place in the line of the current the lower orifice of a tube, C, open at both ends. Air will now be found to force its way into the current against the pressure of the column of water D E.

Experiments with delivery-tubes of various shapes, a constant depth of 1 inch of water being maintained in the vessel D. Orifice of the tube delivering the air (C, fig. 1) = $\frac{8}{60}$ths of an inch diameter. The comparative dimensions of the tube B (fig. 1) and the delivery-tubes employed are shown in fig. 2.

I. Circular delivery-tube $\frac{15}{120}$ths of an inch in diameter (B, fig. 2).

Flow = half a litre of water in 39 seconds.

Level of water maintained constant in A during the experiment.	Quantity of air carried down by half a litre of water.
At 1 inch below the orifice of B (*b*, fig. 1).	No air was carried down.
At the orifice of B (*c*, fig. 1).	211 cubic centims.
At 1 inch above the orifice of B (*d*, fig. 1).	147 „ „

No air was carried down in the first experiment, because there was not sufficient water in the stream to allow of the formation of a conical bag at the bottom of the tube B, and the air consequently escaped upwards between the sides of the tube and the descending stream; but immediately the orifice of B touched the water-surface, B was virtually closed, and the air being unable to escape upwards was carried over into M.

II. Rectangular delivery-tube $\frac{3}{10}$ths by $\frac{11}{60}$ths of an inch (C, fig. 2).
Flow = half a litre of water in 16 seconds.

Level of water maintained constant in A during the experiment.	Quantity of air carried down by half a litre of water.
At *b*, fig. 1.	615 cubic centims.
„ *c*, „	500 „ „
„ *d*, „	246 „ „

III. Rectangular delivery-tube $\frac{1}{6}$th by $\frac{1}{17}$th of an inch (D, fig. 2).
Flow = half a litre of water in 31·5 seconds.

Level of water maintained constant in A during the experiment.	Quantity of air carried down by half a litre of water.
At *b*, fig. 1.	No air carried down.
„ *c*, „	298 cubic centims.
„ *d*, „	140 „ „

IV. Square delivery-tube $\frac{23}{120}$ths of an inch in the side (E, fig. 2).
Flow = half a litre of water in 16·25 seconds.

Level of water maintained constant in A during the experiment.	Quantity of air carried down by half a litre of water.
At *b*, fig. 1.	677 cubic centims.
„ *c*, „	665 „ „
„ *d*, „	427 „ „

V. Triangular delivery-tube $\frac{1}{4}$th of an inch in the side (F, fig. 2).
Flow = half a litre of water in 16·5 seconds.

Level of water maintained constant in A during the experiment.	Quantity of air carried down by half a litre of water.
At *b*, fig. 1.	733 cubic centims.
„ *c*, „	673 „ „
„ *d*, „	402 „ „

III.

When a fluid is flowing from an orifice in the bottom of a vessel, it sinks steadily until within a short distance of the bottom, when the particles of water immediately over the orifice begin to move circularly around a centre, and a conical cavity is formed extending from the water-surface to a greater or less depth into the issuing stream. This cavity, according to M. Laroque*, begins to be formed "quand la vitesse angulaire est devenue suffisamment grande pour que la force centrifuge qu'elle engendre puisse vaincre la pression hydrostatique et la cohésion du liquide."

If water possesses rotatory motion at the time of its efflux from the bottom of a vessel, the conical cavity is formed much sooner than it otherwise would be, and extends to a far greater depth into the liquid.

A stream of water entering water at right angles to its surface does not produce rotation; but if it enters at an angle differing ever so little from a right angle, rotation in the direction in which the stream flows is at once communicated to the water into which it flows.

A discharge-tube 2 inches long by $\frac{1}{4}$th of an inch diameter was fitted into the bottom of a cylindrical vessel 10 inches high by $4\frac{3}{4}$ inches diameter; water was placed in the vessel until it stood at a height of 9 inches from the upper orifice of the discharge-tube; it was allowed to come to perfect rest, and the orifice was then opened: the water sank steadily till within $\frac{1}{4}$th of an inch of the bottom, when a cavity formed over the orifice.

The same experiment was repeated; but before the commencement of efflux, rotatory motion was given to the water in the vessel; when efflux commenced, the conical cavity formed at 8 inches from the orifice of the discharge-tube and extended into the issuing jet. When a discharge-tube of double diameter was substituted, the cavity appeared at half an inch from the orifice of the discharge-tube when the water was perfectly at rest prior to efflux. When rotatory motion was given to the water, the cavity appeared at 8 inches from the orifice and extended into the issuing stream; air entered through the cavity and expanded the water as it issued from the vessel into an ellipsoid 3 inches in its longer diameter by 1 inch in its shorter. The rate of efflux was three times as slow as when no rotatory motion was given to the water.

If when the conical cavity has extended to some distance into the issuing stream the latter is caused to enter water, the air which enters through the cavity is seen to penetrate but a very

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* *Ann. de Chim. et de Phys,* March 1864, "Recherches Hydrauliques."

short distance into the water; but if we make such an arrangement that we have a vessel perpetually emptying itself by a long tube, air enters by the conical cavity, is detached from the apex of the cone, and passes down the tube with the descending water.

In order to determine the amount of air carried down by this means, I fitted a small funnel N, fig. 1, into the upper orifice of the tube B, and placed the delivery-tube H in such a position that the water which it delivered impinged against the side of the funnel. This being the case, a hollow cone of water possessing rapid rotatory motion was formed in the funnel, and air entered through its centre: the air and water on issuing from the funnel formed alternate disks in the tube B, and these rapidly descended into the vessel A. A mercury gauge communicating with the interior of A showed that the pressure remained perfectly constant, and precisely the same amount of air came over during a number of successive experiments.

The amount of air carried down was very large: when the circular delivery-tube $\frac{3}{20}$ths of an inch diameter (used in some of the previous experiments) was employed, no less than 1200 cubic centims. of air were carried down by half a litre of water, the water-surface in A being 1 inch below the orifice of B (viz. at *b*, fig. 1). The flow from the delivery-tube was = half a litre of water in 24 seconds.

With the smaller circular delivery-tube, $\frac{13}{120}$ths of an inch diameter, and delivering half a litre of water in 39 seconds, 1392 cubic centims. of air were carried down by half a litre of water; the water-surface in A being, as before, 1 inch below the orifice of B. Stated in other words, a little stream of water, about $\frac{1}{10}$th of an inch in diameter at the orifice from which it issues, with a head of water of 2 feet, carries down in one hour 128 litres of air. By this method we see, therefore, that 1 part of water carries down more than $2\frac{3}{4}$ times its own bulk of air—the largest quantity which I have found under any circumstances to be carried down by water.

The mouth of the funnel N was covered air-tight with a thick sheet of caoutchouc, and a small vibrating tongue, such as is used in Wheatstone's concertinas, was fitted into the caoutchouc; the delivery-tube was then passed through the cover, and so placed that it delivered water, as before, against the side of the funnel: when water flowed, the tongue was readily vibrated by the entering air. A second tongue was now added, and it was found that the flow from the delivery-tube might be diminished from half a litre of water in 24 seconds to the same quantity in 40 seconds, and yet the tongues were readily vibrated.

A musical instrument might thus be constructed in which the air could be twice used before leaving the instrument; for the air rushing in to penetrate through the conical cavity, and that same air after having been carried down by the water and collected, might be employed separately to produce vibrations in vibrant bodies according to any of the usual methods.

IV.

There is one other way in which water passing through a tube may carry down air.

Suppose a continuous vertical tube with water flowing through it. If now the water is caused to cease flowing, it will remain suspended in the tube by atmospheric pressure; and such would of course be the case with any tube under 33 feet in length; but let an orifice be made anywhere in the circumference of the tube, and the whole column beneath that orifice will instantly fall, because it has no longer the weight of the atmosphere on its lower end alone, but also at the orifice; the influence of the atmosphere is consequently annulled below the orifice, and the column falls by its own weight.

If water be flowing through such a tube, the column below the orifice will have greater velocity than that above it; hence rupture of the column will ensue at the orifice, and air will enter; but as water is continually flowing, the orifice is quickly closed, and we thus obtain an intermittent entry of air.

In order to see how much air might be carried down by this means, a short glass tube O, fig. 1, $\frac{3}{20}$ths of an inch diameter, was fitted into the upper orifice of the tube B by means of a cork; a piece of caoutchouc tubing, P, was adapted to the tube O, and a circular delivery-tube H, $\frac{3}{20}$ths of an inch diameter, was connected with the upper part of the caoutchouc tube. H delivered half a litre of water in 24 seconds. An orifice $\frac{1}{30}$th of an inch diameter was made in the circumference of the caoutchouc tube midway between the orifices of H and O; thus a perfectly continuous tube, with the exception of an orifice $\frac{1}{30}$th of an inch diameter, intervened between the water-cistern and the vessel A.

Matters being thus arranged, water was allowed to flow from H, and it was found that half a litre of water carried down 218 cubic centims. of air.

When a caoutchouc tube with four orifices in its circumference was substituted for that with one orifice, half a litre of water carried down 320 cubic centims. of air: in both instances the water-surface in A was 1 inch below the orifice of B (viz. at *b*, fig. 1).

V.

We see from the above that there are four modes by which air can be carried down by a stream of water falling through a tube.

1. If the area of the cross section of the tube through which the water falls be not much greater than that of the orifice from which the water flows, disks will be formed in the tube, and, being pushed down by the descending stream, will force down the air beneath them.

2. If the area of the cross section of the tube through which the water falls be much greater than that of the orifice from which the water flows, so that disk action is prevented, or if the pressure on the lower end of the tube be competent to support a column of water in the tube at such a distance from the orifice from which the water flows that the descending stream has not widened sufficiently to allow of the formation of disks, air will be carried beneath the water-surface on account of the formation of cavities, according to the theory of Magnus.

3. If there is not a great depth of water in the vessel which supplies the descending stream, or if (the depth not of necessity being small) rotatory motion is from any cause imparted to the water, air will enter through a cavity formed above the orifice from which the descending stream issues, and extending into the descending stream.

4. If the area of the cross section of the orifice from which the water flows be as great, or nearly as great, as that of the tube through which the water falls, and if at the same time the orifices for the admission of air do not exceed a certain area compared with that of the orifice from which the water flows, air will enter at the rupture of the stream, produced at the orifices by the accelerated motion of the water below those orifices.

The cause of the descent of air in the different modifications of the trompe is not due to any one action of a stream of water; air is carried down by all four of the modes of action mentioned above.

Generally only one mode obtains in one form of the machine; but there may be two modes acting simultaneously, the particular mode or modes being determined (*a*) by the relation of the area of the cross section of the trompe-tube to that of the orifice from which the stream flows; (*b*) by the head of water above the orifice from which the stream flows; (*c*) by the fact of whether there are causes which induce rotatory motion in the water before it leaves the cistern; (*d*) by the form of the orifice from which the stream flows; (*e*) by the manner in which air is allowed access to the interior of the tube; and lastly, (*f*) by the amount of pressure on the lower orifice of the tube.

The first and fourth modes least seldom obtain; the second obtains in the generality of modern trompes; and the third obtains in the trompe described by François, in trompes with very shallow cisterns, in trompes in which the water before leaving the cistern receives rotatory motion, either by the stream which supplies the cistern entering at an angle to the water-surface, or by some other cause, and in all trompes with inclined tubes (of which, as stated above, Kircher had seen forty prior to the year 1655).

I consider the most economical, and in every way the most efficient form of trompe to be the old form, in which there are no air-holes, and the air enters by a conical cavity in the water above the orifice from which the descending stream issues. It will be seen from the above experiments that by this method we obtain nearly double the amount of air obtainable by other means. The construction of such a trompe, moreover, is comparatively easy; there is no need to have the tubes perfectly vertical, and less spray is carried into the furnace than by the form of trompe now in use.

20 Great Marlborough Street, London,
July 28, 1864.

XXVI. *Chemical Notices from Foreign Journals.*

By E. Atkinson, *Ph.D., F.C.S.*

[Continued from vol. xxvii. p. 506.]

MÜLLER* has investigated the action of marsh-gas on several metallic oxides at high temperatures. The marsh-gas was prepared by heating in a charcoal fire a mixture of acetate of soda, potash, and lime in a coated retort, by which almost the theoretical quantity of gas was obtained.

Pure sesquioxide of iron, $Fe^2 O^3$, heated in a hard glass tube in a stream of the gas, soon became black, while a large quantity of water was formed. Subsequent investigation and quantitative analysis of the altered substance showed that it was protosesquioxide of iron, $Fe^3 O^4$. When sesquioxide of iron placed in a porcelain tube, through which passed a current of marsh-gas, was heated in a charcoal fire, protosesquioxide was obtained, which contained, however, a larger quantity of protoxide.

Protosesquioxide of manganese was easily attacked by the gas and reduced to protoxide of manganese.

The oxide of cobalt, $Co^6 O^7$, by heating in a current of marsh-gas, was reduced to metallic cobalt with formation of water. Oxide of copper was likewise reduced to metallic copper, while oxide of tin and oxide of zinc remained unchanged.

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* Poggendorff's *Annalen*, May 1864.

Peroxide of lead, even when gently heated in a current of the gas, was reduced with explosive violence to protoxide of lead. Oxide of bismuth was slowly, but at last completely reduced to the metallic state.

The result of these experiments showed that the action of the gas was in all cases reducing; in no case could any carbon be detected in the substances formed. And they further show that at a red heat the affinity of metals for carbon is inconsiderable.

The author also subjected the above metallic oxides to the action of olefiant gas. When sesquioxide of iron was heated in this gas, at first a formation of water ensued, which gradually became smaller. The decrease in weight, as ascertained by several successive weighings, never amounted to 30 per cent., which would have been required by a reduction of the sesquioxide to metallic iron. The maximum decrease was about 20 per cent.; while there was subsequently an increase of weight. Hence something more than reduction had taken place: either some oxygen was left with the iron, or for a portion of the disengaged oxygen some carbon had been substituted. That this latter was the case, was proved by subsequently heating the residue in hydrogen, when neither was any water formed nor was there any change in weight. When the black residue was treated with hydrochloric acid it dissolved with effervescence, and at the same time the characteristic odour perceived in the solution of cast iron was observed, while carbon was set free. Hence part of the carbon was combined and part in a state of mixture; and the amount of the former appeared to be more considerable than the quantity present in cast iron. The author is of opinion that a partial reduction of the sesquioxide first takes place, and that then this lower oxide, losing some further oxygen, takes up carbon at the same time.

Protosesquioxide of manganese is at first reduced to greenish protoxide, which afterwards increases in weight, doubtless owing to the separation of carbon. Oxide of copper, heated in a current of gas, is at first reduced to metallic copper; and when this is complete, a separation of carbon takes place.

Brunner* has examined the action of hydrogen on the solutions of certain metallic salts. When pure hydrogen was passed through moderately strong neutral solution of nitrate of silver it became turbid, and on continuing the action for several hours a slight grey precipitate was formed, which under the microscope was seen to consist of metallic silver. But the quantity was very small, and did not seem to increase even with several weeks' action.

* Poggendorff's *Annalen*, May 1864.

When hydrogen was passed through a neutral solution of bichloride of platinum it soon became turbid, and partly a black pulverulent and partly a metallic lustrous precipitate was formed. At the same time the solution became clearer, and was finally free from platinum. On this deportment Brunner bases a method for separating platinum from its solutions, the details of which he describes; and he indicates the possibility of a technical application of this method for separating platinum from its ores.

Palladium is as readily reduced from its solutions as platinum; while the reduction of chloride of iridium is quite insignificant, and chloride of gold is unchanged. The same is the case with solutions of chloride of mercury if the hydrogen is under the ordinary pressure. But Brunner found, as Beketoff had also done, that under a pressure of 100 atmospheres this substance is reduced and mercury deposited in distinct globules.

Davy referred the action of his safety-lamp to the cooling action of the wire gauze. Dissatisfied with the inadequacy of this, Krönig proposes* the following:—

"Although experiment shows that a wire gauze can cool the gaseous products of combustion present in a flame to a point below the temperature at which they ignite, the question arises, on what does this action depend. Several things are possible. A cold wire gauze introduced into the flame can take away heat. But the cooling thus produced is less the higher the temperature of the gauze rises; and a continuous cooling of the flame by the wire gauze is only possible when the wire gauze loses on the outside the heat it receives from the flame. Such a loss can occur either by conduction or by radiation. If the flame is small, heat may be conducted from the middle parts of the heated wire gauze; but this conduction must be less the greater the flame. Hence it is probable that the wire gauze loses heat more by radiation than by conduction.

"The assumption that metal gauze radiates more heat than the gaseous flame is a matter of course, for we know that ignited solid bodies radiate more light than gaseous bodies at the same temperature."

This opinion, says Krönig, has become a certainty since the publication of Magnus's interesting experiments in his paper "On the Constitution of the Sun"†. For not only does he show that the introduction of a disk of platinum into a non-luminous gas-flame causes it to radiate more heat, but also that this radiation experiences a further increase when the platinum is soaked in carbonate of soda. This observation appears completely to ex-

* Poggendorff's *Annalen,* May 1864.

† Phil. Mag. S. 4. vol. xxvii. p. 376.

plain the statement of Graham*, that the wire gauze of the safety lamp soaked with solution of alkali becomes much more impervious to the flame.

Buchner nine years ago gave a method for freeing sulphuric acid from arsenic, which was based on the ready conversion of arsenious acid into the more volatile chloride of arsenic by means of hydrochloric acid, and which simply consisted in passing a current of HCl through heated sulphuric acid. Bussy and Buignet, and, independently of them, Bloxam†, found that sulphuric acid could not be purified in this way from arsenic. On going into the experiments of Bussy and Buignet, Buchner found the cause to be that the arsenic was present as arsenic, and not as arsenious acid. Now arsenic acid in solution is but imperfectly converted into chloride of arsenic. Souchay, under Fresenius's direction, has recently found that from a boiling mixture of arsenic and hydrochloric acids very little arsenic is volatilized; and indeed it is on this fact that Fresenius and Babo's method for removing, by means of chlorate of potash and hydrochloric acid, organic matters from solutions containing arsenic is based.
Buchner‡ accordingly proposes, as a modification of his method, to convert any arsenic acid which may be present first into arsenious acid. This is best effected by heating the acid to be purified with a few pieces of charcoal; sulphurous acid is liberated, which in a few minutes reduces the arsenic acid so completely to arsenious acid that every trace may be subsequently removed by passing a current of hydrochloric acid through.

Delafontaine§ has determined the atomic weight of thorium, and communicated some considerations on the formula of thoria. Berzelius, in 1829, had found a series of numbers for the atomic weight of thorium agreeing but imperfectly with each other, and the mean of which (calculated according to the modern atomic weights for sulphur, barium, and potassium; for Berzelius's determination was made with the sulphate of thoria and potash) is 59·38. Delafontaine used for the preparation of thoria the minerals thorite and orangite; and the method he used is that employed by Marignac for the treatment of cerite. The powdered mineral was moistened with water and made into a semifluid paste with sulphuric acid, by which so much heat was produced as to expel most of the excess of sulphuric acid. The residual mass was heated until all acid was driven off. The residue was then dissolved in *cold* water and filtered. The solution was next con-

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* Poggendorff's *Annalen*, vol. xxxvii. p. 367.

† Journal of the Chemical Society, vol. xv. p. 52.

‡ Liebig's *Annalen*, May 1864.

§ *Archives des Sciences Physiques et Naturelles*, vol. xviii. p. 343.

centrated and heated in the water-bath to 100°, by which a sulphate of thoria little soluble in hot water was deposited. This operation was repeated several times, and the precipitate was considered to be pure when, on heating, it left a perfectly white residue. The mother-liquor from all these crystallizations, on the addition of sulphate of potash, yielded the thoria in them in the form of double salt.

The sulphate of thoria thus obtained is heavy, white, and caseous; it consists of a large number of very small interlaced needles, which gave to it a porcelain-like appearance. In no case could distinct crystals be seen; but when on this salt was poured a quantity of water insufficient for its solution, it became converted into clear colourless crystals in the form of 6 to 8-sided prisms with pointed ends.

These two compounds differ in the quantity of water they contain. The water in them was determined by heating them from 400° to 450°, and the thoria by heating the salt to redness until the weight remained constant. To determine the sulphuric acid, the thoria was first precipitated as oxalate, and in the filtrate the sulphuric acid was precipitated by chloride of barium, hydrochloric acid having been previously added.

The results of Delafontaine's analysis show that if thoria is to be considered as a base, ThO, the salts must have respectively the improbable formulæ $4\mathrm{RO\,SO^3}+9\mathrm{Aq}$ and $2\mathrm{ThO\,SO^3}+9\mathrm{Aq}$. But some time ago* Nordenskiöld and Chydenius showed that crystallized thoria prepared in the dry way had the same form and angles as stannic and titanic acids. This, then, seems to show that thoria, like zirconia, which it so much resembles, consists of one atom of metal and two of oxygen; if, then, this is the case, the above salts have the formulæ

$$2\mathrm{ThO^2\,SO^3}+9\mathrm{Aq}$$

and

$$\mathrm{ThO^2\,SO^3}+9\mathrm{Aq}.$$

The objection that the water in the first salt is expressed by a fraction, it shares with the sulphates of uranium, cadmium, yttrium, and didymium. The atomic weight of thorium is then 231·5.

St.-Claire Deville and Troost† have made the following experiment, which shows that iron is permeable to hydrogen gas at a high temperature. A tube of cast steel was taken containing so little carbon that it could not be hardened, and so soft that it could be drawn out in the cold to a tube of 3 to 4 millimetres in

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* Phil. Mag. vol. xx. p. 375.

† *Comptes Rendus,* vol. lvii. p. 966. Liebig's *Annalen,* May 1864.

diameter. To the ends of this tube, which was thus constructed without soldering, two thinner copper tubes were soldered, and in these glass tubes were cemented. The whole tube was placed in a porcelain tube in a furnace, and one of the ends connected with a hydrogen-apparatus, while the other was provided with a long vertical delivery-tube which dipped under mercury. While the tube was kept at a very high temperature, hydrogen was passed through until its action on the sides of the tube must have been terminated and all atmospheric air and moisture completely expelled. When now the hydrogen-tube was sealed, the mercury rapidly ascended in the vertical tube at the rate of 3 to 4 centimetres in a minute, and more rapidly according as the heat of the furnace was increased. Hence in the interior of the apparatus an almost vacuous space was formed in consequence of hydrogen passing through the tube in opposition to the external atmospheric pressure.

Deville has continued* on these thick tubes of iron the experiments which, in conjunction with Troost†, he had previously made on porous earthen tubes, and has arrived at some unexpected results.

To the two ends of an iron tube about 3 millims. in thickness were soldered two very fine copper tubes, by which it communicated on the one hand with a source of nitrogen, and on the other with a manometer. Two good stopcocks (immersed for safety's sake in cold water) were cemented to the ends of these copper tubes; by one of them a current of nitrogen could be introduced or stopped at will, and by the other, which was a three-way stopcock, the interior of the tube could be connected either with a manometer or with a mercury or a water pneumatic trough to collect the gases and analyze them.

The iron tube was introduced into an impermeable porcelain tube very slightly longer than itself. This was closed at each end by a cork perforated so as to permit the copper tube to pass, and at each end a glass tube was fitted so as to allow a current of any gas to enter the annular space between the porcelain and the iron tube. The middle of this apparatus was fastened firmly in a furnace fed by gas-coke, and by a ventilator which renders the operator entirely master of variations in the temperature.

In this manner there could be introduced into the iron tube, and into the annular space which surrounds it, two separate currents of gas isolated by a metallic disphragm several millimetres in thickness.

At first pure nitrogen was passed into the iron tube and into

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* *Comptes Rendus*, July 18, 1864.

† Phil. Mag. vol. xxii. p. 61.

the annular space; heat was applied, and kept pretty constant. This is indicated by the manometer, which ought not to vary appreciably when the stopcock, by which the nitrogen enters the iron tube, is closed. At this moment (the nitrogen-stopcock being closed) a current of hydrogen was passed into the annular space. It was thus seen that, in proportion as hydrogen displaced nitrogen in the annular space, the mercury ascended in the manometer and attained such a level as to indicate that the internal pressure had been more than doubled. This is pure hydrogen, which, penetrating the sides of the iron tube, adds its pressure to that of the nitrogen, which does not escape in any appreciable quantity.

After some hours the pressure attains a maximum; the pressure could be calculated from the height of the mercury in the manometer; the three-way stopcock was then worked so as to allow the gas in the interior of the tube to be collected and analyzed. Thus there were all the elements for obtaining the pressure of each gas in the inside of the iron tube. Throughout the experiments a constant current of hydrogen traversed the annular space.

After this first experiment, one or more may be made by placing in its original position the three-way cock, which simply places the iron tube in communication with the manometer (the nitrogen stopcock is not touched, and thus new quantities of gas are not introduced). This second *heating* gives rise to a new maximum pressure, less, however, than the first. And in the same way a third and fourth experiment may be made.

M. Deville then gives a tabular statement of his results, from which he draws the following conclusions.

"Hydrogen passing into the annular space at the atmospheric pressure tends to enter the iron tube by traversing its pores. (1) At a temperature but little elevated, hydrogen has both inside and outside the iron tube exactly the same pressure (that of the atmosphere) as if there were no nitrogen in the interior. Hence the law of the diffusion of gases, whether into liquid or into gases themselves, is verified. (2) If the temperature is very high, the pressure of hydrogen in the iron tube is much higher than the pressure in the exterior. These results are in complete contradiction with all facts known regarding the diffusion of gases.

"The only two circumstances which might serve for their explanation are as follows:—

"1. In the inside of the tube a mixture of nitrogen and hydrogen acts like a homogeneous substance, drawing to itself pure hydrogen from the outside as if part of the physical properties of the hydrogen were destroyed by the presence of the nitrogen.

But in the present state of science it would be difficult to admit this explanation.

"2. In the inside the gases are at rest; outside, hydrogen is in motion. If it were to this difference alone that the phenomenon had to be attributed, I might draw important conclusions in support of the mechanical theory of heat, of the new ideas on the constitution of gases, and of the hypothesis of Mr. Graham. But before this I desire to examine carefully the conditions of the experiment, which might have escaped me, and discuss them again in all their parts."

M. Debray has made a communication on the production of some crystallized phosphates and arseniates*. The phosphates and arseniates obtained by precipitating metallic solutions by soluble phosphates are gelatinous, or at least amorphous; but the precipitates formed in solutions of magnesia or cobalt by phosphate of ammonia are rapidly transformed into small crystals of definite composition. Debray finds now that this transformation of phosphates is more general than had been supposed, and that there are few which do not finally pass, under favourable circumstances, from the amorphous state into that of well-defined crystals. He explains this as follows. The amorphous precipitates produced by soluble phosphates and metallic solutions are not quite insoluble in the saline, acid, or alkaline liquors in which they are formed. Hence if by any diminution of temperature their solubility diminishes, a portion crystallizes either on the sides of the glass or on the amorphous substance; an increase, on the contrary, dissolves a part of the amorphous substance, which is more readily soluble than the crystals; so that by a series of changes in the solvent power of the liquid, as feeble as may be desired, but continuous, the amorphous substance ought to be entirely changed into crystals.

This transport of matter from the amorphous to the crystalline state is analogous to the phenomena discovered by M. Deville†, in which amorphous oxides were converted into crystalline oxides under the influence of a current of hydrochloric acid. Here the acid acting on the amorphous oxide gives a chloride and water, between which the inverse action takes place. But the oxide thus formed is crystalline, and much more difficult of attack than the amorphous, which is thus acted upon until it is entirely changed.

M. Debray describes the production of a series of oxides belonging to the magnesian group. At the ordinary temperature he obtains with an excess of phosphate, in two or three days,

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* *Comptes Rendus*, July 4, 1864.

† Phil. Mag. vol. xxii. p. 515.

and often sooner, the following new salts in well-defined crystals:—

Phosphate of ammonia and nickel .	$2NiO, NH^4O, PO^5+12HO$
Phosphate of ammonia and zinc . .	$2ZnO, NH^4O, PO^5+12HO$
Phosphate of ammonia and manganese	$2MnO, NH^4O, PO^5+2HO$
Phosphate of ammonia and iron . .	$2FeO, NH^4O, PO^5+2HO$

At a temperature of about 80° C. ammoniacal double salts are obtained with magnesia, cobalt, nickel, manganese, and iron in nacreous crystals, which have the general formula

$$2RO, NH^4O, PO^5+2HO.$$

Zinc forms an exception; it produces an anhydrous phosphate.

Chancel's phosphate of ammonia and cobalt,

$$2CoO, NH^4O, PO^5+12HO,$$

when left in contact with a concentrated acid solution of phosphate of ammonia for seven or eight days, is transformed into tolerably large rose-coloured crystals of a phosphate which is also insoluble, and which has the composition

$$CoO, NH^4O, HO, PO^5+4HO.$$

Phosphate of iron undergoes a similar change.

In very acid liquids no precipitates are formed, but beautiful crystals are obtained on spontaneous evaporation.

With phosphate of ammonia and magnesian salts in excess no ammoniacal phosphates are obtained, and the products vary with the temperature. Thus the salts of magnesia and of manganese give phosphates in the form of fine rhomboidal octahedra, of the following composition:

$$2MnO, HO, PO^5+6HO \text{ and } 2MgO, HO, PO^5+6HO.$$

At 100° C. manganese gives a tribasic phosphate,

$$3MnO, PO^5+3HO,$$

which has the form of *Hureaulite*, and may be considered as a variety of this mineral free from iron.

Arseniate of ammonia gave precipitates which could not be transformed at the ordinary temperature; but kept for several days at 100°, well-defined crystals were obtained. Those of zinc and of manganese are as follows:—

$$2MnO, HO, AsO^5+2HO; \quad 2ZnO, HO, AsO^5+2HO.$$

With phosphate of soda in excess and salts of the magnesian group the products vary with the nature of the salt employed. The following are the principal ones obtained:—

Phosphate of magnesia . . .	$2MgO, HO, PO^5+14HO$
Phosphate of zinc	$3ZnO, PO^5+4HO$
Phosphate of iron	$3FeO, PO^5+8HO$

The latter is *Vivianite* in small crystals quite like those of Commentry. It has never before been artificially prepared.

Phosphate of nickel and soda, $2NiO, NaO, PO^5+14HO$
Phosphate of cobalt and soda, $3CoO, PO^5+2NaO, HO, PO^5 +8HO$.

The latter is in small crystals of a magnificent blue colour.

M. Damour* has communicated a note on the density of zircon. In the course of his researches on the density of minerals, he had been led to observe the great variations in the density of zircon, which oscillates between 4·04 and 4·67; and he set himself to ascertain whether this arose from a difference in composition, or was due to a special molecular state. He analyzed a specimen from Ceylon, of the specific gravity 4·183, and found that the analytical results corresponded with the formula of zircon, $ZrO^2 SiO^2$. Results very closely agreeing with these were obtained by Berzelius for a zircon of Epailly which had the specific gravity 4·667.

The difference did not arise therefore from chemical composition. Damour accordingly endeavoured to modify the molecular state. A zircon from Ceylon, whose density was 4·183, was heated to dull redness, without, however, producing any loss of weight or alteration in density. But heated to an incipient white heat its density rose to 4·534, there being little or no loss. The same experiment, repeated five times on different specimens, gave always the same result—an increase of $\frac{1}{12}$ to $\frac{1}{16}$ in the density.

A temperature of white redness produced by a turpentine flame fed by air is inadequate to melt zircon. By the oxyhydrogen blowpipe, however, the surface becomes fused and covered with a thin layer of white enamel.

Damour compared the refractive indices of the specimens differing in their density. Senarmont had found for a specimen from Ceylon, whose density was 4·636, the numbers

$$\left.\begin{array}{ll}\text{For the ordinary ray} \quad . & \omega=1{\cdot}92 \\ \text{For the extraordinary ray} & \epsilon=1{\cdot}97\end{array}\right\}\text{ for red rays.}$$

And Damour found for another specimen from Ceylon, which had the density 4·210, the numbers

$$\left.\begin{array}{ll}\text{For the ordinary ray} \quad . & \omega=1{\cdot}85 \\ \text{For the extraordinary ray} & \epsilon=1{\cdot}86\end{array}\right\}\text{ for red rays.}$$

Hence the refractive index increases with the density.

It is doubtless to the allotropic state of zirconia that these different physical properties of zircon are due. Zirconia, as ob-

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* *Comptes Rendus*, January 18, 1864.

tained in the form of hydrate, exhibits a brisk incandescence when exposed to a temperature of dull redness, and its physical properties are found to be greatly modified. Denoting these two conditions of the earth by Z̈r (*a*) and Z̈r (*b*), it is easy to suppose that their mixture in varying proportions will produce numerous variations in the physical characters of the compound of which this substance forms part.

The author gives, finally, a list of the densities of a number of zircons.

Lemoine* has found that when sulphur is combined with red phosphorus in excess, a new sulphide of phosphorus, P^2S^3, is obtained. When 1 equivalent of sulphur is mixed with 1 of red phosphorus and the mixture heated to 100°, a violent action is set up accompanied by great evolution of heat. The residue consists of sesquisulphide of phosphorus mixed with excess of hposphorus. The two bodies are most readily separated by solution in bisulphide of carbon. Whatever excess of red phosphorus is taken, the substance formed is the same; but if the sulphur is in excess, tersulphide of phosphorus is obtained.

Sesquisulphide of phosphorus crystallizes from sulphide of carbon or from chloride of phosphorus in right rhomboidal prisms.

It melts at 142°, and distils between 300° and 400°. At 260° it volatilizes completely in a current of dry carbonic acid. The sublimate thus obtained does not colour polarized light, which circumstance, together with its aspect and the mode of its grouping, place it in the regular system. It is therefore dimorphous. It is almost unalterable in the air and in cold water.

XXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 159.]

June 16, 1864.—Major-General Sabine, President, in the Chair.

THE following communication was read:—

"Description of a New Mercurial Gasometer and Air-pump." By T. R. Robinson, D.D., LL.D., F.R.S., &c.

In some experiments on the electric spectra of metal and gases, I felt the want of a mercurial gasometer for working with such of the latter as are absorbable by water. That of Pepys is on too large a scale for my requirements, and it seemed better to contrive one more easily manageable, which I saw could also be made to act as a mercurial air-pump. In this I have succeeded to my satisfaction; and I hope that a description of it may be useful to those who are engaged in similar researches.

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* *Comptes Rendus*, May 16, 1864.

There have been several attempts made to exhaust by means of mercury, the chief of them with which I am acquainted being those of Close (Nicholson's Journal, 4to, iii. p. 264), Edelcrantz (Nicholson, 8vo, vii. p. 188), Traill and Children (Nicholson, xxi. pp. 63 & 161), and that of Geisler, which he uses in preparing the beautiful vacuum-tubes which bear his name. In all the principle is the same. A vessel is filled with mercury, which is made to descend from it, leaving in it a Torricellian vacuum; this vessel may be made to communicate with a receiver, and abstract from it a portion of the gas which fills it; and by repeating the process the receiver can be exhausted as by successive strokes of an air-pump. In the two first instruments to which I have referred, the descent of the mercury is produced by lifting a plunger which fills one leg of an inverted siphon, the vacuum vessel being at the top of the other leg. On depressing the plunger, the mercury is again forced up to fill that vessel; and of course both legs must be longer than the barometric column. In the two next, the receiver itself is filled with mercury, which, by opening a cock, falls through a tube of sufficient length into a cistern below. Here the stroke (so to call it) cannot be repeated. In Geisler's the bend of the siphon is of vulcanized caoutchouc, so that one leg can be inclined down to a horizontal position, and thus allow the metal to fall from the other, or when raised to the vertical position fill it again. This I believe acts well, but it must be rather unwieldy; and my practical acquaintance with the working of tubes of that material has made me suspicious of their tightness and permanence under such circumstances.

As in all these cases the mercury is supported in the vacuum-vessel by atmospheric pressure, it is obvious that its descent will be produced by removing in any way that pressure; and an effective means of doing this is supplied by the common air-pump; more tedious certainly than the mechanical means above mentioned, but far more manageable; and as any mercurial pump must be slow in its working, while it is only required for special purposes, this defect is not of much importance, and moreover is compensated by some special advantages.

But besides bringing down the mercury, means must be provided for raising it again. My first plan was to do this by condensed air, the same syringe which made the exhaustion having its action reversed by a well-known arrangement. It worked extremely well, was lighter, and required less mercury than the contrivance which I finally adopted; but it is less convenient for gasometric work, as the syringe must be worked while gas is delivered. The machine in its present form is shown in fig. 1. Its base is a stout piece of mahogany, 21 inches by 10·5, with a rim round it 0·5 deep to prevent the loss of any spilled mercury, and handles at the ends by which it can be transported. To this is firmly fixed the iron stand B, 3·5 high, 4 in diameter above; its upper surface is carefully trued to a flanch, in which is cemented the vacuum-bell A, so that when the touching surfaces are lightly smeared with a mixture of lard and wax and screwed together by the six screws (some of which appear in the figure), the joint is air-tight. The bell A is 2 inches in diameter and 6·5 high; it has a tubulure at the top, in which is ground a glass cock C, whose construction is shown in fig. 2. The key of it is pierced from its bottom to a level with the bore, with which this perforation communicates occasionally by a lateral opening. In the position of the figure, it will be seen that the bell communicates with the branch *a*; if the key be turned half round, it is connected with the branch *r*; and in an intermediate position it is completely shut off. These glass cocks have this great advantage over those of metal, that it can always be ascertained if they are air-tight; their transparency permits us to see if the key and shell are in optical contact; and the slightest air-way there is at once detected. They should not be lubricated with oil, which *grips* and may perhaps find its way into the bell and soil its interior. I find the best material to be castor oil with rosin dissolved in it. A hole is drilled down the axis of B, which communicates by a tube (sunk in the wood and therefore not visible in the figure) with the cast-iron cylinder D. This is 13 inches high and 3·2 in internal diameter;

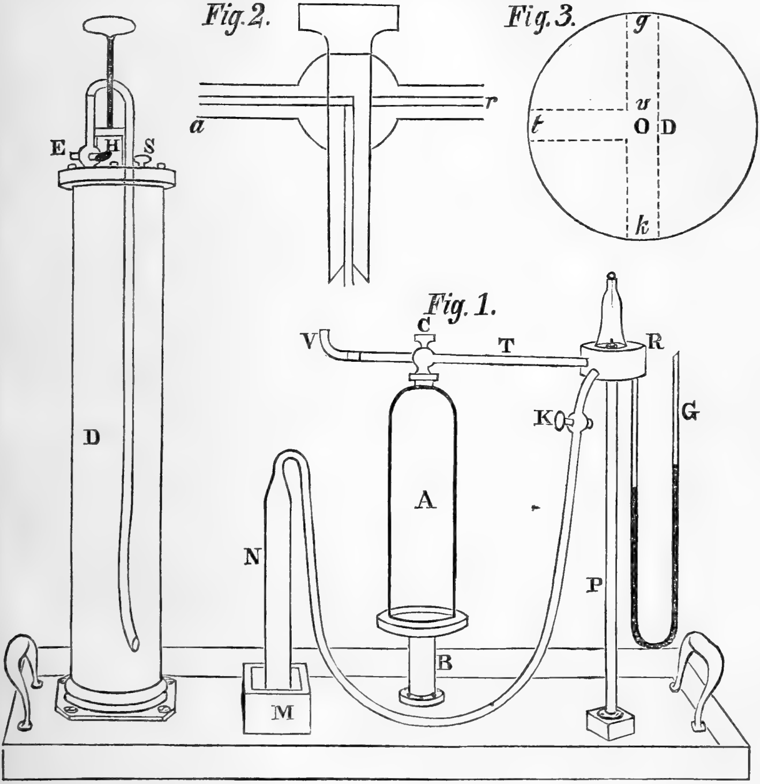
Fig. 2.

Fig. 3.

Fig. 1.

its top and bottom are secured to it air-tight by screws; in it works a plunger of boxwood well varnished 10·4 high, and moving so loosely that mercury may pass it easily. The plunger is wrought by a rod passing through the collar of leather H. In the top of the cylinder is a stopcock E, to which is fixed a tube of vulcanized caoutchouc (varnished with a solution of caoutchouc in benzidine), which is shown hanging down; it has a coupling to connect it with an ordinary air-pump. There is also in the top a screw S for admitting air. One end of the bell's cock communicates with the atmosphere, the other with the receiver-plate R. This is of glass 2 inches in diameter, 0·75 thick, and is cemented on the top of the iron pillar P. Through it are drilled the passages shown in fig. 3; in *t* is ground the glass tube, shown in fig. 1 by T, the end of which is in contact with the cock, and their junction made air-tight by a tube of Para caoutchouc; in *g* and *k* are similarly ground the siphon-gauge G and the glass cock K. These all communicate with the receiver by the passage *v*, and by removing the tubes can be easily dried or cleaned. The cock K is connected by elastic tube with the catch-jar N, which is supported in a small mercurial trough M.

The operation of this machine as an air-pump is as follows:—The receiver being placed on R, open the screw S, press down the plunger nearly to the bottom of the cylinder, remove the key of the bell-cock, and pour through the opening which it leaves as much mercury as will fill the bell to the bore of the cock. In this one 10 lbs. are required. Raise the plunger to the top, and the metal will subside from the bell till only 0·3 of an inch remains on the top of B, filling the space left vacant in D by the rising of the plunger. The length of the plunger and the height of B must be adjusted to this condition. Replace the key; turn it to communicate with the atmosphere (which position I call (*a*)), and depress the plunger. The mercury will rise again in the bell, filling it, and expelling the air from it, till at last a little mercury will appear in the bore of the cock. To prevent this from being splashed about, a bit of bent tube *v* is ground on the end of the cock, which receives it, and when it has too much is removed and emptied into D through S. Secondly, turn the key to shut off the bell (position (*o*)); draw up the plunger, close S, open E, and couple it to an air-pump, with which exhaust D. This pump may be of the commonest description, for an exhaustion of one or two inches is quite sufficient. The mercury will sink in the bell, leaving above it a Torricellian vacuum. Close E, and turn the key to communicate with the receiver (position (*r*)); its air or gas will expand into the bell.

These three operations form the cycle of operation, and must be repeated till the required exhaustion be obtained, with one modification of the first one. In it, at the second and all subsequent strokes, the key is to be at (*o*) and S opened; thus the atmospheric pressure will raise the mercury and do much of the plunger's work; that must then be depressed and the key set at (*a*); the other two steps are as at first.

When the instrument is to be used as a gas-holder, either the receiver must be in its place, or the opening of R must be closed by a piece of flat glass; the bell must be filled by the plunger, and made, by (*r*) and by opening *k*, to communicate with the jar N. The mercury will rise in that to its neck, and sink in A; fill A again, pass gas into N, and, by carefully working the key, draw it into A till that is full. As this gas will be mixed with the air of the vessels and passages, it must be expelled, and A refilled till its purity is certain. If it be noxious, it must be conducted into some absorbent fluid by an elastic tube, slipped on the *a* end of the cock; which will also convey the gas to any vessel.

If it be required to fill a receiver for experiments in an atmosphere of gas either at common pressure or a less one, it may either be exhausted by an air-pump connected with K, and filled from A, or exhausted by A and filled from N. The former can only be done with gases which have no action on brass.

These operations seem complicated when described with so much detail, but in practice they are very easy, and their result is good. Some precautions, however, are required to ensure it. The bottom of the bell-cock and of its key must be ground, so as to leave no shoulder or hollow in which air may be entangled when the bell is filled. Every part of the metal work must be air-tight; this can only be secured by covering, not only its joints, but its whole surface with several coats of varnish-paint—best of white lead. When the first coat is applied, on exhausting the apparatus, every hole or pore is revealed by an opening in the paint (often almost microscopic), which must be filled up as it forms till all is tight. It is almost needless to mention that the whole must be perfectly dry. If the bell be filled a few times with undried air, enough of moisture will adhere to its walls to prevent an exhaustion of more than 0·1 inch. In such a case it must be dried by drawing air into it through sulphuric acid, and this repeatedly. Moisture also occasionally finds its way into a part still more troublesome, into the passage which connects the bell and cylinder; it is probably condensed there when the mercury is colder than the atmosphere. I remove this by connecting the tube of K with a desiccator; setting C to (*r*), opening K and E, and by working the air-pump drawing a stream of dry air into D, which bubbles up through the mercury in the passage, and at last sweeps away all trace of water and its vapour. In this operation it is necessary to remove a portion of the mercury, as otherwise it would be sucked into the pump; indeed this mischief might occur in ordinary work by some mistake in the manipulation—for instance, by leaving E open with (*a*). To prevent the possibility of this, D is connected with the pump by a mercury trap, easily imagined, which intercepts any of that metal that might come over. And lastly, the interior of the bell must be *perfectly clean* if the highest degree of exhaustion is required. This state is obtained by washing it with strong nitric acid, then with distilled water, and when quite dry wiping it with linen, from which all traces of soap or starch have been removed by boiling it in rain-water. Thus we reduce to a minimum the film of air which adheres to the bell even when filled with mercury, and lessens its vacuum. When all these precautions were taken, I found that with a receiver containing 3·7 inches, the fifth operation brought the gauge (which had been similarly cleaned and *carefully* boiled) down to 0·01. The sixth brought it still lower, but my present means of measurement* are not sufficient to determine the precise amount. In this machine the old air-pump theorem ought to hold, and by it, with the fraction $\frac{3\cdot7}{17}$, I find that the fifth *should* give 0·007, and the sixth 0·0014; so that the presence of adhering air is still sensible, though very slight. So high a power, however, is not long maintained; for by use, and especially with oxygen, which (probably from the presence of ozone) has a peculiar tendency to dirty mercury, the bell becomes soiled; but it continues to give a vacuum of 0·02, which is quite sufficient for ordinary objects. At common pressure and temperature, the electric discharge through the receiver shows no evidence of the presence of mercurial vapour; but at 0·02 it is otherwise; the discharge is greenish white, and the spectrum shows little except the lines of mercury. If the gauge were detached, perhaps this vapour might be absorbed by gold-leaf.

The apparatus acts well as a mercurial gas-holder, and can deliver 18·5 inches. Like all other contrivances for confining gaseous matter by mercury, it is liable to have its contents contaminated with air by diffusion between the metal and the vessel which contains it; but I expected that in this arrangement the defect would be little felt. In order that it may take place, the air must pass a distance of 17·2 inches, of which 14·6 is a tube only 0·125 in diameter, and the rest is in a vertical direction against the pressure of 2·6 inches of mercury. A single experiment will show how far this avails. The bell was filled with dry hydrogen, which was found to contain 0·901 of the pure gas; it was left for ten days exposed to considerable changes of temperature, and was then found to have 0·854; it was therefore contaminated at the rate of 0·005 per day. I am not aware of similar measures with ordinary mercurial apparatus; nor is this amount of error very important; but it may I believe be corrected by a means long since announced by the late Professor Daniell which has been strangely neglected. He proposed it to prevent the infiltration of air into barometers. If the liquid metal adhered to the surface which it touches, as water would, this action could not occur; now it *wets*, if I may use the word, several metals, as copper or silver, but it also dissolves them, and becomes less fluid. Daniell, however, found that it does *wet* platinum without acting on it in any injurious degree; and advised that a ring of platinum wire should be fused round the tube where it dips into its cistern. On inquiring of his friend and fellow-labourer, Dr. W. A. Miller, I learn that it was completely successful, but was not taken up by the opticians, and passed out of memory. It is obvious that if a bit of platinum tube were cemented in the vertical passage below D, it would effectually bar the diffusion. I do not like to undo the joint there, which is now perfectly tight; but I will certainly, when the opportunity offers, try the experiment.

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* A micrometer microscope put in the place of the telescope of my theodolite.

GEOLOGICAL SOCIETY.

[Continued from p. 160.]

May 25, 1864.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "On the Geology of part of the North-western Himalayas." By Capt. Godwin-Austen. With Notes on the Fossils, by T. Davidson, Esq., F.R.S., F.G.S., R. Etheridge, Esq., F.G.S., and S. P. Woodward, Esq., F.G.S.

The geological formations occurring in these regions were stated to be (1) a fluvio-lacustrine series, (2) a Siwalik series, (3) Nummulitic Limestone, (4) Jurassic rocks, and (5) a Palæozoic series. In reference to the fluvio-lacustrine strata, the author gave a *résumé* of the conclusions respecting their physical features and mode of formation at which he had arrived in a former paper, and in addition gave some details respecting their position and stratigraphical characters, especially describing the mode of occurrence in them of some land and freshwater Shells, which were referred to in a Note by Mr. S. P. Woodward. The lakes in which the lacustrine deposits were formed were supposed by Capt. Godwin-Austen to have been produced in consequence of the mouths of valleys, into which rivers run, becoming blocked up by means of glaciers and otherwise, as now often happens in the same region. Stratigraphical details of the other series of rocks were then given, the Jurassic formation being supposed to belong to the Middle division of the Oolites, and the Palæozoic limestone being described as Carboniferous Limestone, both of which determinations were confirmed by Messrs. Etheridge and Davidson in Notes on the Fossils. The age of the clay-slate and mica-slate was stated to be very doubtful, and the author concluded by describing the localities in which granitic rocks occur, but chiefly as forming the axis of the North-western Himalayas. In Notes appended to the paper, Mr. Davidson described species of Brachiopoda from three deposits, one of Carboniferous age, one of Jurassic, and one of unknown date; Mr. Etheridge described the remaining fossils from the Jurassic strata; and Mr. Woodward noticed the Shells from the fluvio-lacustrine series. While the latter were stated to be nearly all recent British species, Mr. Etheridge remarked on the great affinity of the Jurassic fossils to those of the same age (Middle Oolite) in England, and Mr. Davidson observed that the fossiliferous limestone of the Carboniferous series bore a great resemblance, lithologically and in its fossils, to deposits of a similar age in Great Britain.

2. "On the Cetacean Fossils termed *Ziphius* by Cuvier, with a notice of a new species (*Belemnoziphius compressus*) from the Red Crag." By Prof. T. H. Huxley, F.R.S., F.G.S.

The genius *Ziphius*, as originally constituted by Cuvier, contained three species described by him, namely, *Z. cavirostris, Z. plani-*

rostris, and *Z. longirostris*; but it is probable that each of these really belongs to a distinct genus—the first to *Ziphius*, the second to *Choneziphius*, and the last to the author's genus *Belemnoziphius*. More recently M. Gervais has established a new species—*Ziphius Becanii*—from a specimen formerly considered to belong to *Z. longirostris*; and this species, with that described in this paper, and Professor Owen's MS. species, were also considered referable to *Belemnoziphius*.

Besides the foregoing conclusions respecting the affinities of the fossil *Rhynchoceti*, Professor Huxley discussed the relations of the recent genera and species belonging to the same group, including the cetacean of Aresquiers, which was considered by Gervais to belong to the genus *Ziphius*. He exhibited these relations in a tabular form, and concluded by stating that he had arrived at the following results:—

1. Unless the cetacean of Aresquiers be identical with *Ziphius cavirostris*, all the *Ziphii* of Cuvier belong to Cetacea generally distinct from those now living.

2. If the cetacean of Aresquiers be identical with *Ziphius cavirostris*, it is not certain that the latter is truly fossil; nor, if it be so, have we any knowledge of its stratigraphical position.

3. Of the certainly fossil *Ziphii*, the stratigraphical position of *Belemnoziphius longirostris* is unknown; but all the other species of that genus, and *Choneziphius planirostris*, are derived from the English or Antwerp Crag, and are not known to occur out of it.

4. So that at present we are justified in regarding *Belemnoziphius* and *Choneziphius* as true Crag Mammals.

June 8, 1864.—W. J. Hamilton, Esq., President,
in the Chair.

The following communications were read:—

1. "On the Rhætic Beds and White Lias of Western and Central Somerset, and on the Discovery of a new Fossil Mammal in the Grey Marlstones beneath the Bone-bed." By W. Boyd Dawkins, Esq., B.A., F.G.S.

After describing the sections in the district, and showing the palæontological relations of the White Lias to the *Avicula contorta* series and the zone of *Ammonites planorbis*, the author enunciated the following conclusions:—(1) That the true position of the White Lias is immediately above the *Avicula contorta* zone of Dr. Wright, and at the base of the Lower Lias shales; (2) that it is entirely distinct from the Rhætic beds, lithologically and palæontologically; and (3) from the discovery of Rhætic fossils in the Grey Marls below the Bone-bed, that the latter belong to the Rhætic formation. He then proceeded to describe a two-fanged mammalian tooth, which he had found in the Grey Marlstones below the Bone-bed, and which he considered to be the analogue of the trenchant four-ridged premolar of *Hypsiprymnus*, of the section to which *H. Hunteri* belongs. Until additional remains be found, its affinities to

Microlestes or to *Plagiaulax* cannot be determined; Mr. Dawkins has therefore named it provisionally *Hypsiprymnopsis Rhæticus.* In conclusion he traced the range of the Marsupials in space and time, showing that of the six families into which Van der Hoeven divides the existing Marsupials, two—the entomophagous and sarcophagous Dasyurina, and the phytophagous Macropoda—had been represented in England during the interval between the deposition of the Purbeck beds and that of the Rhætic Marlstones below the Bone-bed.

2. "On the Geological Structure of the Malvern Hills and adjacent District." By Harvey B. Holl, M.D., F.G.S.

The object of this communication was threefold, namely (1) to discuss the structure and origin of the crystalline rocks of the Malvern Hills, (2) to give the results of an examination of the superposed Palæozoic strata, (3) to state the chronological relationship of the several events in their geological history.

The geological structure of these hills was described in detail, and it was concluded that the rocks hitherto treated of as syenite, and supposed to form the axis of the range, are in reality of metamorphic origin, consisting of gneiss (both micaceous and hornblendic), mica-schist, hornblende-schist, &c., all invaded by vein of granite and trap-rocks. It was then shown that the Hollybush Sandstone is the equivalent of the Middle Lingula-flags, and that the overlying black shales correspond with the Upper Lingula-beds, the whole being overlain, as in Wales, by Dictyonema-shales. These rocks, on the east of the Herefordshire Beacon, are altered by trap-dykes, which were shown to be of later date than those traversing the crystalline rocks before alluded to. Allusion was next made to the Upper Llandovery strata which overlie unconformably the primordial rocks just noticed, after which the several faults in the district were described in detail.

Dr. Holl concluded with some remarks on the general relations of the rocks of the Malvern Hills with those of the surrounding districts, describing the successive physical changes supposed to have been consequent upon their deposition and their subsequent elevations and depressions.

XXVIII. *Intelligence and Miscellaneous Articles.*

ON THE ROTATORY POWER OF ACTIVE LIQUIDS AND OF THEIR VAPOURS. BY M. D. GERNEZ.

WHEN Biot, in 1815, was accidentally led to the discovery of rotatory polarization in liquids, he soon recognized in this remarkable phenomenon all the characters of a property depending on the individual form of the molecules. Among the experiments which he devised to exhibit this phenomenon, the best consisted in volatilizing an active liquid (oil of turpentine) and causing a ray of polarized light to traverse the vapour. After many fruitless attempts, Biot finally succeeded in establishing the existence of the rotatory power of the vapour of the oil, when an explosion and a fire destroyed his apparatus. Either because this experiment presented too many dangers, or because its arrangement appeared too difficult, it has not been repeated since 1818. It remained, however, to inquire if the rotating power is the same in magnitude and in direction in the vapour and in the liquid which has produced it; nor was it uninteresting to determine the law of dispersion of the planes of polarization of rays of various colours under these two different conditions. Such was the object of my researches, which doubtless I could not have executed without the kind encouragement of MM. Pasteur and Verdet, and the resources which the laboratory of the École Normale Supérieure presented.

Some preliminary experiments made with the vapours of essence of turpentine and of camphor, by means of a tube 15 metres in length, heated by a series of gas jets, showed that the rotatory power prevails in vapours in the same direction as in liquids. The magnitude of the rotation was sufficient to allow me to reduce the length of the tube to 4 metres, and to arrange it so that the temperature was uniform from one end to the other. But working upon liquids of considerable rotatory power, I could see that the numbers representing the molecular rotatory powers of the vapours were much less than those corresponding to liquids condensed at the ordinary temperature; I was thus led to investigate whether the molecular rotatory power of these essences did not vary with the temperature.

These liquids were examined at various temperatures, with special apparatus, and by methods the description of which will be found elsewhere. I will merely remark that I decided to work on essences as homogeneous as possible; and as liquids always slightly alter when kept for several hours at high temperatures, I commenced these determinations at lower temperatures.

Representing by (α) the molecular rotatory power at the temperature t, the determinations made up to 160° are contained in the following formulas :—

Rays.	Essence of orange values of (α).	Essence of bigarade values of (α).	Essence of turpentine values of (α).
C	$9\overset{\circ}{0}{\cdot}45-0{\cdot}0893t-0{\cdot}000054t^2$	$9\overset{\circ}{2}{\cdot}79-0{\cdot}1041t-0{\cdot}000106t^2$	$2\overset{\circ}{8}{\cdot}29-0{\cdot}003187t$
D	$115{\cdot}91-0{\cdot}1237t-0{\cdot}000016t^2$	$118{\cdot}55-0{\cdot}1175t-0{\cdot}000216t^2$	$36{\cdot}61-0{\cdot}004437t$
E	$148{\cdot}82-0{\cdot}1585t-0{\cdot}000028t^2$	$153{\cdot}81-0{\cdot}1667t-0{\cdot}000198t^2$	$46{\cdot}29-0{\cdot}006187t$
F	$180{\cdot}67-0{\cdot}1979t-0{\cdot}000001t^2$	$186{\cdot}89-0{\cdot}2162t-0{\cdot}000152t^2$	$55{\cdot}00-0{\cdot}007000t$
G	$241{\cdot}20-0{\cdot}2331t-0{\cdot}000181t^2$	$249{\cdot}33-0{\cdot}2638t-0{\cdot}000403t^2$	$71{\cdot}01-0{\cdot}008437t$

The molecular rotatory power may thus be expressed as a function of the temperature by the parabolic formula $a-bt-ct^2$, a being very small for the essences of orange and bigarade, and virtually zero for essence of turpentine.

If the values of (α) are compared for the same temperature and for the different rays of the spectrum, it is seen that the essences of orange and of bigarade diverge much more than essence of turpentine from the law of the inverse ratio of the square of the wave-length; the product $(\alpha)\lambda^2$ varies in fact from the ray C to the ray G by about one-seventh of its value in the case of essences of orange and of bigarade, while the variation is only one-fifteenth in the case of essence of turpentine.

The ratio of the rotatory powers for the same ray is taken at any two temperatures; it is found to be the same whatever ray of the spectrum be considered; it is thus easily deduced that the law of the dispersion of the planes of polarization of rays of different colours is the same for all temperatures.

The preceding liquids, like camphor, were brought to the state of vapour in a tube 4 metres in length, surrounded by a jacket, which could be raised to any temperature by means of a series of gas-jets. I measured the rotations produced by this column of vapour, and I compared them with those that would be produced by a certain length of the liquid arising from the condensation of the vapour. The following Table refers to this series of experiments:—

Rays.	Essence of orange.			Essence of bigarade.			Essence of turpentine.			Camphor.		
	Vapour.	Liquid.	Ratio.	Vapour.	Liquid.	Ratio.	Vapour.	Liquid.	Ratio.	Vapour.	Liquid.	Ratio.
	↗	↗		↗	↗		↖	↖		↗	↗	
	°	°		°	°		°	°		°	°	
C	......	......	......	7·74	36·08	4·66	4·36	13·03	2·99	6·89	19·43	2·82
D	12·10	45·97	4·14	9·83	46·06	4·69	5·57	16·59	2·98	9·44	26·71	2·83
E	14·39	59·57	4·15	12·75	59·54	4·67	7·09	21·08	2·97	13·36	37·82	2·83
F	17·46	72·24	4·14	15·37	71·61	4·66	8·36	24·96	2·99	17·7	50·12	2·82
G	23·44	97·19	4·15	20·75	96·63	4·66	10·96	32·68	2·98	28·11	79·62	2·83

The ratio of the rotations for the same ray in the two conditions is the same for all rays of the spectrum, the difference being less than the errors of observation; hence it may be concluded that the law of dispersion is independent both of the temperature and of the condition of the body.

It remained to follow the variation of the molecular rotatory power after the change of condition. For this determination, which necessitates experimental precautions that I cannot here enumerate, I used the observation of the sensible tint, the use of which is justified by the preceding. The measurement being made when the tube was saturated with vapour at a known temperature and pressure, the vapour was expelled by a current of carbonic acid; it was condensed, and the molecular rotatory power of the liquid determined at various temperatures. For essence of turpentine and for camphor, the molecular rotatory power of the vapour is almost exactly the same as that of the liquid supposed to be at the same temperature; for the essences of orange and of bigarade it is a little less, and the curve which represents it continues to approach the axis of the temperatures in the part which corresponds to the state of vapour.

In fine, the rotatory power of substances which I have studied for a definite ray of the spectrum is not a constant; it varies regularly with the temperature, and changes neither direction, nor virtually intensity, when the liquid passes into the state of vapour.

For rays of different colours, the law of the deviation of the planes of polarization is independent of the temperature and of the condition of the substance.

If, then, it be admitted that the rotatory power of active substances depends on their molecular structure, it may be concluded from the preceding that the liquid molecules vaporize without any modification taking place in their form.—*Comptes Rendus,* June 13, 1864.

INVERSION OF THE ABSORPTION BANDS IN THE SPECTRUM OF DIDYMIUM. BY R. BUNSEN.

By an experiment which I have made at the instance of Professor Kirchhoff, I have succeeded in changing the dark lines which the absorption spectrum of solutions of oxide of didymium shows into bright lines. If a small quantity of oxide of didymium is heated with microcosmic salt at a red heat until the mass is free from gas-bubbles and has become transparent, an amethyst-coloured glass is obtained on cooling, which, held between the slit of the spectrum-apparatus and the source of light, produces the characteristic absorption spectrum of didymium compounds. As source of light, an ignited platinum wire as fine as a hair is used, the image of which, by means of a small lens of short focal distance, is made to fall upon the slit in a suitable manner. If then the didymium glass melted in a platinum spiral is brought between the source of light and the lens, the stronger absorption lines of didymium, and more especially the chief band αDi near Fraunhofer's line D, are seen in the spectrum-apparatus with perfect distinctness. If the didymium glass in the spiral (which issustained by a holder) is gradually heated by a non-luminous flame held below it, so long as a red heat has not been attained the band αDi is seen to become gradually broader. If the temperature is raised to a continually increasing red heat, the dark band diminishes more and more in darkness, and finally entirely disappears. If now the platinum wire which serves as hindermost source of light is removed, a spectrum of the fused ignited didymium glass appears, which has exactly, in the position of the dark band αDi in the absorption spectrum, a similarly shaped bright line on a dark ground. Indications of a similar inversion may also be perceived in the other absorption lines of the same spectrum.

Professor Bahr in Upsala made two years ago the interesting observation that the salts of the metals occurring along with yttrium, and designated by Mosander erbium and terbium, give remarkable absorption spectra which are wanting in solutions of pure yttria.

Among the lines of these spectra first observed by Bahr, and used as tests, there is *one* of great intensity, which is readily changed into a bright band in the manner described.—Liebig's *Annalen*, August 1864.

ON A NEW POLARIZING PRISM. BY PROF. H. W. DOVE.

This contrivance consists of an isosceles right-angled prism of calc-spar, one of whose equal sides is perpendicular and the other parallel to the optic axis of the crystal, and therefore the hypothenuse-side at an angle of 45° with it. This rhombohedron-surface occupies the axis of the polarizing-apparatus previously constructed by the author, instead of the Nicol's prism which is otherwise placed there, so that the light of a lamp, concentrated by a condensing lens, arrives at the analyzing-apparatus after having suffered two refractions at the equal surfaces of the prism and one total reflexion at the hypothenuse-surface. The large quantity of light admitted by the apparatus renders practicable the employment of the most deeply coloured glasses, in order to obtain the perfectly definite separation of the various homogeneous systems of rings. It can also be used with advantage in the polarizing microscope, and for the production of the systems of rings on a white screen by means of solar or the electric light. This prism, which acts like a Nicol, has been ground according to my directions by M. Langhoff, the optician.—Poggendorff's *Annalen*, vol. cxxii. p. 18.

ON THE OPTICAL PROPERTIES OF CARTHAMINE.
BY PROF. H. W. DOVE.

In the Proceedings of the Berlin Academy for 1857, page 209, I have described a method of combining the visual impressions made upon the two eyes simultaneously so as to produce the appearance of a vivid chromatic lustre in substances which, when illuminated in the usual way, show no trace of it. The method consists in holding a piece of differently coloured glass before each eye, and looking through both at a picture in which the colours of the two pieces of glass are so combined that a figure is executed in one colour upon a ground of the other colour. This reminded me that the so-called *shot* silks, in which the warp and weft are of different colours, as well as the wings of certain beetles, and, lastly, the dichroic platinum-compounds examined by Haidinger, especially those which are of a bright green by reflected light, and appear deep red by transmitted light, produce the impression of a lustre which approaches very closely to that of a metal. In like manner a metallic lustre already possessed by a surface is distinctly heightened by combination with another colour, as is clearly shown when the surface becomes covered with a film of oxide, or is covered with a galvanic deposit thin enough to exhibit interference tints. To the same class of phenomena belong also the favourable changes in our impression of the colour of newly cast statues when they are allowed to stand exposed to the atmosphere. If these changes take place quickly, they soon lose in beauty through the darkening of the surface; and hence we see that the colours must cooperate in a definite ratio in order to excite the most favourable impression.

It is well known that the imperfectly pure carthamine, which occurs in commerce in *pink saucers,* has a yellowish metallic lustre when allowed to dry in a saucer, and that in time it becomes greenish at the surface. Carthamine spread upon glass plates exhibits a bronze-like iridescence which afterwards disappears. Dr. Stahlschmidt very kindly prepared for me a series of glass plates upon which pure carthamine was spread with the greatest possible evenness. On looking through one of these, the whole plate appears of a deep red. On looking at a plate, coated on one side only, by reflected light, the coated side being below, the glass plate appears uniformly green. But if the plate is reversed, so that the daylight, to which the back of the plate is turned, is reflected by the layer of carthamine, the observer would fancy he was looking at a plate of polished brass. If carthamine is spread upon a plate of blue, yellow, red, or green glass, the green colour seen by reflexion in the first case disappears, but the metallic lustre seen in the second case remains unchanged. This latter therefore is produced by the combination of the reflected green light with the internally dispersed red light.

The reflected green light appears with increased intensity if the plate, placed in such a position that the side coated with carthamine is below, is looked at through a Nicol's prism or through one of the polarizing prisms above described (p. 247). This acts in the plane of reflexion like a Nicol's prism perpendicularly to the principal section. The cause of heightening of the colour thus occasioned is, that the light polarized in the plane of reflexion, by the outer surface of the glass, is got rid of. On the contrary, if the layer of carthamine is uppermost, the yellowish light gradually becomes more and more nearly green as the Nicol is turned round. Carthamine very slightly depolarizes transmitted polarized light, and, when the light is incident at a very oblique angle, renders it elliptically polarized.

I may take this opportunity of remarking that the polarizing prism described by me is especially well adapted for experiments with radiant heat. I have often exposed it to the concentrated heat of the sun until the cork into which it was fitted began to burn, without the prism itself being in any way injured. In using a Nicol's prism in saccharimetrical experiments, the prism is often injured, if a flame is brought very close to it, by the connecting layer of Canada-balsam becoming blistered. The same applies to experiments on the polarization of radiant heat. When employed as analyzing-apparatus, a Nicol's prism has the advantage that the object remains erect, while in mine the reflected image rotates. The latter, however, possesses the recommendation for a polarizing-apparatus of offering a large field of view. It is preferable to Foucault's prism, since in this moisture is apt to be precipitated in the separating stratum and to render the surface dull, whereas in mine the surfaces can at all times be easily cleaned.—Poggendorff's *Annalen,* vol. cxxii. p. 454.

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XXIX. *On the Photographic Use of a Silvered-Glass Reflecting Telescope. By* Henry Draper, *M.D., Professor of Natural Science in the University of New York*[*].

SINCE the introduction of mirrors of silvered glass for astronomical uses by Foucault and Steinheil, they have continually increased in favour, and have now assumed very considerable importance. As a testimonial to their efficiency, I may state that one of 15½ inches aperture has furnished me the means of producing photographs of the moon 50 inches in diameter, well defined, and of good general effect.

My attention was first directed to them in 1860, owing to some remarks made by Sir J. F. W. Herschel to my father. I had at that time a reflector of speculum-metal of 15 inches aperture and 12 feet focal length, mounted in an observatory which was described in a paper read at the Oxford Meeting of the British Association in 1860. Soon afterwards the speculum was replaced by a silvered-glass mirror of 15½ inches aperture and 12½ feet focal length, which I had ground, polished, and silvered. Since then more than a hundred mirrors have been prepared in my workshop, in order to secure two of the highest perfection. The full account of these operations, illustrated by forty-seven woodcuts, has just been published by the Smithsonian Institution at Washington, in the 'Contributions to Science.'

In the past four years many opportunities have presented themselves for learning the qualities of these instruments, what their defects and advantages are, and the best means of constructing them. When the eye has become experienced in

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[*] Communicated by the Author.

judging of faults, and the hand quick at correcting them, a short time suffices to bring the glass concave to a state of perfection. If the rough grinding be completed, and the convex tool put into good condition, a single day should finish the surface.

In the beginning of the experiments, one of the most serious difficulties encountered was that arising from the irregular distribution of heat through the mass of the disk of glass. When, for instance, a mirror of 15½ inches is polished by a pitch tool of the same size, it is hard to avoid the production of rings of varying focal length, owing to the overplus of heating towards the middle. Where the tests are applied at the centre of curvature, and the operator does not have to depend on indications derived from telescopic observation, these and similar imperfections are much more easily detected and avoided than where they have to be disentangled from atmospheric disturbances.

Another obstacle, which proved to be formidable at first, was the unequal amount of compression that disks of glass and speculum-metal suffer when supported on different parts of their edges. In all the large disks examined, there has been a diameter of minimum compressibility, which ought to be kept always vertical. If set horizontally, the mirror immediately gives double images.

As regards the best machine for producing parabolic surfaces, the experience of six years on several different ones, including those of Lord Rosse and Mr. Lassell, has brought me to the conclusion that the most satisfactory results are to be obtained only by employing polishers of much less diameter than the surface to be produced—a method of working first published by M. Foucault. A better mirror can be made by small local polishers moved by the hand, than by a full-sized polisher moved by any machine that I have tried. This is due to the complete control that the operator gains over the distribution of heat and moisture, and to the power of rubbing away only those parts which are necessary to be removed in order to extricate the parabolic surface below. The method resembles that of scraping used by mechanics to produce true planes. At the same time a thorough knowledge of the appearance of spherical, elliptical, parabolic, hyperbolic, and other surfaces at the centre of curvature must be gained. I have also polished several mirrors which could only bring oblique pencils to a sharp focus, and which were suitable for the Herschelian construction, the amount of the obliquity, 2° 30′, just carrying the image to the edge of the tube. Some of the best lunar photographs were taken when the diagonal mirror of the Newtonian was 6 inches out of centre in the 16-inch tube.

Many different processes for silvering glass have been proposed, some requiring the aid of heat, others doing as well cold. Those of M. Cimeg and Professor A. Martin have given the most satisfactory results. The reducing agents are respectively Rochelle salt and cane-sugar (*interverti*). As the formulæ may be of use to those interested in the matter, they are appended. For silvering a $15\frac{1}{2}$-inch mirror, about 22 ounces of liquid are needed. In Cimeg's process, 800 grains of nitrate of silver must be dissolved in 4 ounces of water, and ammonia added until the precipitated oxide of silver is almost redissolved. In another vessel, 560 grains of tartrate of potash and soda are to be placed with 17 or 18 ounces of water. The concave surface of the glass must be thoroughly cleaned with nitric acid and water, and, when dry, coated with uniodized collodion, and the film polished off with cotton flannel. The liquids being mixed are then to be poured into a shallow vessel of hard india-rubber, and the glass immersed face downwards, the back standing out of the liquid and being freely exposed to daylight or sunlight. The same precautions are necessary to avoid streaks as in the case of a collodion negative. The silver, when polished, should be about $\frac{1}{200,000}$ of an inch thick, and should show the sun by transmitted light as a light-blue disk.

In Professor Martin's process only 100 grains of nitrate of silver are required. The formula is to dissolve the nitrate in water, add ammonia till the brown oxide redissolves completely, then pour in slowly 80 grains of caustic soda in solution. If this produces a precipitate, the quantity of ammonia must be increased. The reducing agent is procured by boiling $12\frac{1}{2}$ parts of white sugar in 100 parts of water with 1 part of nitric acid, and adding water to make 500 parts with 50 parts of alcohol. Two ounces and a half of this liquid are to be mixed with the previous solution just before the glass is immersed. The drawback to this process, as compared with Cimeg's, is that there are often many minute holes in the silver, particularly if the solutions have been freshly prepared. They cannot be avoided by previous filtration.

The durability of silver films under favourable circumstances is quite surprising. When not exposed to sulphuretted hydrogen, they do not show any disposition to tarnish, and, all things considered, are more lasting than the polished specula that are in the observatory. These latter are apt to accumulate a yellow film, which interferes as seriously with their photographic power as does the reddish colour so strongly seen in Gregorians and Newtonians when the copper and tin are incorrectly proportioned. The silvered surfaces however, are, occasionally liable to an accident which does not affect the others. A series of minute fissures will spread all over them, and the silver seem to lose its adhesion to the glass. This appears to arise from continued exposure to dampness, and may be obviated by covering the concave surface, when not in use, with a plate of flat glass, the edge of the concave being ground flat. The diagonal mirror of my Newtonian is not subject to this difficulty, owing to the free ventilation and greater warmth in its neighbourhood. The lower story of the observatory was contrived to keep the large metal speculum at a uniform temperature, and is excavated out of the solid rock. Being cool, it communicates to objects put in it a tendency to condense moisture from the warmer air that enters. It is obvious that an observatory for a silvered-glass instrument should be altogether above ground, and not cooler than the adjacent air.

The vapours which arise from fresh paint exercise a prejudicial effect, in depositing upon the surface a somewhat greasy film. After repainting the interior of the buildings, it became necessary to expose the large concave several times to the sun, and repolish it to keep it in working order. In the course of three months the trouble sensibly disappeared, the present mirror not having been taken off its air-sac since spring, nor will it require to be moved for a long time.

The reflecting power of silver films varies considerably with the method of preparation. When deposited from alcoholic solutions in the manner recommended by M. Foucault, they have frequently a leaden appearance, while the two processes just described give surfaces of velvety blackness in oblique positions, and certain to show objects in their natural colours. This dulness results probably from the presence of foreign matters incorporated with the silver, as is shown by the fact that when such a film is dissolved off a piece of glass with nitric acid, an insoluble reddish powder is left.

For the purposes of celestial photography, a silvered-glass telescope offers, without doubt, the greatest advantages. Not only is it less difficult to manage than a speculum, but its higher reflecting power materially shortens the time of exposure of the sensitive plate. The superiority of any reflector to an achromatic is of course too well known to require mention. I have no doubt that they will in future be constructed of much larger size than any speculum yet made, the intrinsic difficulties being much less. The glass need only weigh one-eighth as much as the metal. They are also more permanent; for if by accident the silver should be injured, it is only a morning's work to dissolve away the old film and replace it by a new one, which will copy the glass below with so much accuracy that the most refined tests (such as that with an eclipsing screen at the centre of curvature) will fail to indicate any change in figure. The glass covered by metal is even more durable than a lens.

However, unless the astronomer can have access to a much steadier atmosphere than prevails at Hastings near New York, where my observatory is, there would be but slight inducement to build a very large instrument. In the nine months' interval between March and December 1863, only three really fine nights occurred, when the best lunar negatives could be taken. Up to the present date, September 1864, there has not been a single occasion this year on which the results of the past one could be exceeded. The good nights have been during the absence of the moon. If the instrument could be transported to the Peruvian plateaus, 15,000 feet above the sea, or somewhere near the equator on the rainless west coast of South America, it could accomplish more.

Notwithstanding these impediments, I have succeeded, as has already been stated, in making some photographs of the moon 50 inches in diameter, and many of smaller sizes. In order to take advantage of a steady night when it does occur, it is necessary to get photographs whenever it is clear, so as to keep the chemicals and clock in the best order. For this reason about 1500 original negatives were made in 1862 and 1863. A singular cause has this summer led to the loss of a great deal of time. Owing to an excessive drought, the woods have been on fire in many places, and have communicated to the atmosphere the power of stopping the larger portion of the chemical rays. The moon of June 19 required an exposure of ten minutes to secure an impression, which was not more vigorous than one of that phase taken in $\frac{1}{200}$th of the time usually is. On the next night two seconds were sufficient. In the meantime there had been a heavy dew, but no rain. These smoky atmospheres give great prominence to the lines in the less refrangible parts of the spectrum about A. The diminution of the light on another occasion was so great that the eye could look without inconvenience upon the meridian sun, which, as it declined to the west, was gradually extinguished, and this though there were no clouds. These phenomena are by no means confined to small areas, but extend over large tracts of country. When my regiment was stationed at Harper's Ferry in Virginia in 1862, a similar condition of atmosphere prevailed there and at the observatory 200 miles distant. The yellowness was at that time attributed to dust in the air.

In developing and enlarging photographs, care is necessary to preserve the proper relation of light and shade. In the case of the full moon, for instance, there is a tendency to flatness and an indistinct appearance. If, however, the negative, instead of being developed with sulphate of iron, is brought out with pyrogallic acid, it assumes a much more brilliant aspect, the contrast of light and dark parts is greater, and the picture more lively. In enlarging such a photograph, by treating the reverse or positive made from the original negative with iodide of mercury dissolved in iodide of potassium, the contrast can be increased at pleasure, the dark parts of the collodion film gaining more than their due proportion of strength. The eventual paper proof, instead of being flat, is whiter in the high lights and darker in the deep shades than it should be. Such a result shows the uncertainty of any method of comparing the actinic force of various parts of the moon together, and attempting to deduce therefrom arguments as to their composition. The nature of the collodion and sensitizing compounds used makes also a material difference. I have found that, as a rule, it is better to use sulphate of iron alone in those phases where the moon is about one-half illuminated, and graduate the strength of the solution so as to gain the most desirable result. The parts of the surface on which the sunlight is falling nearly perpendicularly, are only too prone to become overdark as compared with those on which the light strikes obliquely. The contrast requires restraint rather than encouragement. The negatives should present the appearance of overdone positives to enlarge well. Even with the best system of development, it is impossible to secure a good picture of the entire portion of the moon visible in the first and third quarters; for either the larger portion must be overdeveloped and overexposed to secure the extreme edge, or else the most obliquely illuminated part must be left undeveloped. It may be possible to overcome this difficulty in the future, by diaphragms so adjusted as to cut off the brighter light while the fainter parts continue to act.

In any operation of forcing or intensifying, it is necessary to bear in mind that the deposit constituting the image is not a continuous film, but is more or less granular. Most of the processes of redeveloping tend to increase the size of the silver grains, and, if practised injudiciously upon photographs that have subsequently to be magnified, may impair them seriously. Sulphate of iron employed alone for educing a picture seems to give the finest granulations, particularly if the sensitive plate, previous to exposure to light, has been washed in water so as to remove most of its free nitrate of silver. It requires, however, to be redipped in the nitrate-of-silver bath before being treated with the sulphate. The free nitrate of silver necessary to the reduction cannot be advantageously supplied otherwise; and the sensitiveness would be decreased.

No photograph that has to be magnified should be varnished.

Apart from the fact that it is entirely unnecessary, it is sure to produce some markings or imperfections. Dust and little hairs adhere to the film while sticky, and when dry it is certain to be either finely corrugated, or else dotted with little transparent elevations, which act like lenses toward the bright beam of light subsequently used. In order to be secure against accidents, I always keep two or three reverses of the finest negatives. These, being copied by contact in rays diverging from a point, are just as valuable as the original.

With regard to the means used for enlarging, they were at first single and compound achromatic combinations. But these present two defects. The visual and actinic foci are made to coincide, even approximately, with difficulty; and when in addition a flat field, sometimes 5 feet in diameter, is required, such lenses as could be obtained failed entirely. It then occurred to me to supply their place by a concave mirror of suitable figure, and both difficulties have in consequence been surmounted. The field is flat, as is shown by examination on ground glass, and in the produced photograph. As for the sharpness, I have made a picture of a scale of *Lepisma saccharina* under a power of 289, by taking a photograph magnified seventeen times, and then magnifying that. It shows the characteristic markings almost as well as a microscope. With this contrivance, the whole interior of the observatory, 27 feet long, can be used as a camera obscura.

University, New York,
September 1, 1864.

XXX. *On the Conformation of the Alps.*

By JOHN TYNDALL, *F.R.S., &c.*[*]

TO the physical geologist the conformation of the Alps, and of mountain-regions generally, constitutes one of the most interesting problems of the present day. To account for this conformation, two hypotheses have been advanced, which may be respectively named the hypothesis of *fracture* and the hypothesis of *erosion*. Those who adopt the former maintain that the forces by which the Alps were elevated produced fissures in the earth's crust, and that the valleys of the Alps are the tracks of of these fissures. Those who hold the latter hypothesis maintain that the valleys have been cut out by the action of ice and water, the mountains themselves being the residual forms of this grand sculpture. To the erosive action here indicated must be added that due to the atmosphere (the severance and detachment of

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[*] Communicated by the Author.

rocks by rain and frost), as affecting the forms of the more exposed and elevated peaks.

I had heard it stated that the Via Mala was a striking illustration of the fissure theory—that the profound chasm thus named, and through which the Hinter-Rhein now flows, could be nothing else than a crack in the earth's crust. To the Via Mala this year I therefore went, to instruct myself by actual observation upon the point in question. The gorge commences about a quarter of an hour above the town of Tusis; and, on entering it, the conclusion which first gains credence is that it must be a fissure. This conclusion in my case was modified as I advanced. Some distance up the gorge I found upon the slopes to my right quantities of rolled stones, evidently rounded by water-action. Still further up, and just before reaching the first bridge which spans the chasm, I found more rolled stones associated with sand and gravel. Through this mass of detritus, fortunately, a vertical cutting had been made, which exhibited a section showing perfect stratification. There was no agency in the place to roll these stones, and to deposit these alternating layers of sand and pebbles, but the river which now rushes some hundreds of feet below them. At one period of the Via Mala's history the river must have run at this high level. Other evidences of water-action soon revealed themselves. From the parapet of the first bridge I could see the solid rock 200 feet above the bed of the river scooped and eroded. It is stated in the guide books, that the river, which usually runs along the bottom of the gorge, has been known almost to fill it during violent thunderstorms; and it may be urged that the marks of erosion which the sides of the chasm exhibit are due to those occasional floods. In reply to this, it may be stated that even the existence of such floods is not well authenticated, and that, if the supposition were true, it would be an additional argument in favour of the cutting-power of the river. For if floods operating at rare intervals could thus erode the rock, the same agency, acting without ceasing upon the river's bed, must certainly be competent to excavate it. I proceeded upwards, and from a point near another bridge (which of them, I did not note) had a fine view of a portion of the gorge. The river here runs at the bottom of a cleft of profound depth, but so narrow that it might be leaped across. That this cleft must be a crack is the impression first produced; but a brief inspection suffices to prove that it has been cut by the river. From top to bottom we have the unmistakeable marks of erosion. This cleft was best seen by looking downwards from a point near the bridge; looking upwards from the bridge itself, the evidence of aqueous erosion was equally convincing.

The character of the erosion depends upon the rock as well as upon the river. The action of water upon some rocks is almost purely mechanical; they are cut away, sometimes in sensible masses. In other cases the action is chemical as well as mechanical. Water, in passing over limestone, charges itself with carbonate of lime without damage to its transparency; the rock is dissolved in the water; and the gorges cut by water in such rocks often resemble those cut in the ice of glaciers by glacier streams. To the solubility of limestone is probably to be ascribed the fantastic forms which this rock usually assumes, and also the grottos and caverns which interpenetrate limestone formations. A rock capable of being thus dissolved will expose a smooth surface after the water has quitted it; and in the case of the Via Mala it is the polish of the surfaces, and also the curved hollows scooped in the sides of the gorge, which assure us that the chasm has been the work of the river.

About four miles from Tusis, and not far from the little village of Zillis, the Via Mala opens into a plain which is bounded by high terraces, evidently cut by water. It occurred to me the moment I saw it, that the plain had been the bed of an ancient lake; and a farmer, who was my temporary companion, immediately informed me that such was the tradition of the neighbourhood. This man conversed with intelligence, and as I drew his attention to the rolled stones, which rest not only above the river but above the road, and inferred that the river must have been there to have rolled those stones, he saw the force of the evidence perfectly. In fact in former times, and subsequent to the retreat of the great glaciers, a rocky barrier crossed the valley at this place, damming the river which came from the residual glaciers higher up. A lake was thus formed which poured its waters over the barrier. Two actions were here at work, both tending to obliterate the lake—the raising of its bed by the deposition of detritus, and the cutting of its dam by the river. In process of time the cut deepened into the Via Mala; the lake was drained, and the river now flows in a definite channel through the plain which its waters once totally covered.

From Tusis I crossed to Tiefenkasten by the Schien Pass, and thence over the Julier Pass to Pontresina. There are three or four ancient lake-beds between Tiefenkasten and the summit of the Julier. They are all of the same type—a more or less broad and level valley-bottom, with a barrier in front through which the river has cut a passage, the drainage of the lake being the consequence. These lakes were sometimes dammed by barriers of rock, sometimes by the moraines of ancient glaciers. An example of this latter kind occurs in the Rosegg valley, about twenty minutes below the end of the Rosegg glacier, and about an hour from Pontresina. The valley here is crossed by a pine-covered moraine of the noblest dimensions: in the neighbourhood of London it might be called a mountain. That it is a moraine, the inspection of it from a point on the Surlei slopes above it will convince any person possessing an educated eye. Where, moreover, the interior of the mound is exposed, it exhibits moraine-matter—detritus pulverized by the ice, with boulders entangled in it. It stretched quite across the valley, and at one time dammed the river up. But now the barrier is cut through, the stream leaving about one-fourth of the moraine to its right, and the remaining three-fourths to its left. Other moraines of a more resisting character hold their ground as barriers to the present day. In the Val di Campo, for example, about three-quarters of an hour from Pisciadello, there is a moraine composed of large boulders, which interrupt the course of a river and compel the water to fall over them in cascades. They have in great part resisted its action since the retreat of the ancient glacier which formed the moraine. Behind the moraine is a lake-bed, now converted into a meadow, which is quite level, and rests on a deep layer of mould.

At Pontresina a very fine and instructive gorge is to be seen. The river from the Morteratsch glacier rushes through a deep and narrow chasm which is spanned at one place by a stone bridge. The rock is not of a character to preserve smooth polishing; but the larger features of water-action are perfectly evident from top to bottom. Those features are in part visible from the bridge, but still better from a point a little distance from the bridge in the direction of the upper village of Pontresina. The hollowing out of the rock by the eddies of the water is here quite manifest. A few minutes walk upwards brings us to the end of the gorge; and behind it we have the usual indications of an ancient lake, and terraces of distinct water origin. From this position the genesis of the gorge is clearly revealed. After the retreat of the ancient glacier which filled this valley, a transverse ridge of comparatively resisting material crossed the valley at this place. Over the lowest part of this ridge the river flowed, rushing steeply down to join at the bottom of the ridge the stream which issued from the Rosegg glacier. On this incline the water became a powerful eroding agent, and finally cut its channel to its present depth. Geological writers of reputation assume at this place the existence of a fissure, the "washing out" of which resulted in the formation of the gorge. Now no examination of the bed of the river ever proved the existence of this fissure; and it is certain that water can cut a channel through unfissured rock—that cases of deep cutting can be pointed out where the clean bed of the stream is exposed, the rock which forms the floor of the river not exhibiting a trace of fissure. An example of this kind occurs near the Bernina Gasthaus, about two hours from Pontresina. A little way below the junction of the two streams from the Bernina Pass and the Heuthal the river flows through a channel cut by itself, and 20 or 30 feet in depth. At some places the river-bed is covered with rolled stones; at other places it is bare, but shows no trace of fissure. The abstract power of water (if I may use the term) to cut through rock is demonstrated by such instances. But if water is competent to form a gorge without the aid of a fissure, why assume the existence of such in cases like that at Pontresina? It seems far more philosophical to accept the simple and impressive history written on the walls of those gorges by the agent which produced them.

Numerous cases might be pointed out, varying in magnitude, but all identical in kind, of barriers which crossed valleys and formed lakes having been cut through by rivers, narrow gorges being the consequence. One of the most famous examples of this kind is the Finsteraarschlucht in the Valley of Hasli. Here the ridge called the Kirchet seems split across, and the river Aar rushes through the fissure. Behind the barrier we have the meadows and pastures of Imhof resting on the sediment of an ancient lake. Were this an isolated case, one might reasonably conclude that the Finsteraarschlucht was produced by an earthquake, as some suppose it to have been; but when we find it to be a single sample of actions which are frequent in the Alps—when probably a hundred cases of the same kind, though different in magnitude, can be pointed out—it seems quite unphilosophical to assume that in each particular case an earthquake was at hand to form a channel for the river. As in the case of the barrier at Pontresina, the Kirchet, after the retreat of the Aar glacier, dammed the waters flowing from it, thus forming a lake, on the bed of which now stands the village of Imhof. Over this barrier the Aar tumbled towards Meyringen, cutting, as the centuries passed, its bed ever deeper, until finally it became deep enough to drain the lake, leaving in its place the alluvial plain through which the river now flows in a definite channel.

But it may be urged that it is not necessary to assume the operation of a special earthquake to split each particular barrier. The broad view taken by the advocates of the fracture theory is that the valleys are the tracks of the fissures produced by the upheaval of the land, and the cracks across the barriers to which I have referred are in reality portions of the great cracks which formed the valleys. Such an argument, however, would virtually concede the theory of erosion as applied to the *valleys* of the Alps. These narrow channels, often not more than 20 or 30 feet across, sometimes even narrower, frequently occur at the bottom of broad valleys. Such a fissure might enter into the list of accidents which gave direction to the real erosive agents which scooped the valley out; but the formation of the valley, as it now exists, could no more be ascribed to it than the motion of a railway train could be ascribed to the finger of the engineer which turns on the steam.

These deep gorges occur, I believe, for the most part in limestone strata; and the effects which the merest driblet of water can produce on such rocks are quite astonishing. It is not uncommon to meet chasms of considerable depth produced by small streams the beds of which are dry for a large portion of the year. Right and left of the larger gorges such secondary chasms are usually to be found. The idea of *time* must, I think, be more and more included in our reasonings on these phenomena. Happily the marks which the rivers have, in most cases, left behind them, and which refer, geologically considered, to actions of yesterday, give us ground and courage to conceive what may be effected in geologic periods. Thus the modern portion of the Via Mala throws light upon the whole. Near Bergün in the Valley of the Albula there is also a little Via Mala which is not less significant than the great one. The river flows here through a profound limestone gorge; but to the very edges of the gorge we have the evidences of erosion. The most striking illustration of water-action upon limestone rock which I have ever witnessed is, I think, furnished by the gorge at Pfäffers. Here the traveller passes along the side of the chasm midway between top and bottom. Whichever way he looks, backwards or forwards, upwards or downwards, towards the sky or towards the river, he meets everywhere the irresistible and impressive evidence that this wonderful fissure has been sawn through the mountain by the waters of the Tamina.

I have thus far confined myself to the consideration of the gorges formed by the cutting through of the rock barriers which frequently cross the valleys of the Alps; as far as I have examined them they are the work of erosion. But the larger question still remains, To what action are we to ascribe the formation of the valleys themselves? This question includes that of the formation of the mountain ridges; for were the valleys wholly filled, the ridges would disappear. Possibly no answer can be given to this question which is not beset with more or less of difficulty. Special localities might be found which would seem to contradict every solution which refers the conformation of the Alps to the operation of a single cause. Still the Alps present features of a character sufficiently definite to bring the question of their origin within the sphere of close reasoning. That they were in whole or in part once beneath the sea will not be disputed. They are in great part composed of sedimentary rocks which must have required a sea to form them. Their present elevation above the sea is due to one of those local changes in the shape of the earth which have been of frequent occurrence throughout geologic time, and which in some cases have depressed the land, and in others caused the sea-bottom to protrude beyond its surface. Considering the inelastic character of its materials, the protuberance of the Alps could hardly have been pushed out without dislocation and fracture; and this conclusion gains in probability when we consider the foldings, contortions, and even reversals in position of the strata in many parts of the Alps. Such changes in the position of beds which were once horizontal could not have been effected without dislocation. Fissures would be produced by these changes; and such fissures, the advocates of the fracture theory contend, mark the positions of the valleys of the Alps.

Imagination is necessary to the man of science, and we could not reason on our present subject without the power of presenting mentally a picture of the earth's crust, cracked and fissured by the forces which produced its upheaval. Imagination, however, must be strictly checked by reason and by fact. That dislocations occurred cannot, I think, be doubted, but that the valleys of the Alps are thus formed is a conclusion not at all involved in the admission of dislocations. I never met with a precise statement of the manner in which the advocates of the fissure theory suppose the forces to have acted,—whether they assume a general elevation of the region, or a local elevation of distinct ridges; or whether they assume local subsidences after a general elevation, or whether they would superpose upon the general upheaval minor and local upheavals. In the absence of any distinct statement, I will assume the elevation to be general—that a swelling out of the earth's crust occurred here, sufficient to place the most prominent portions of the protuberance three miles above the sea-level. To fix the ideas, let us consider a circular portion of the crust, say one hundred miles in diameter, and let us suppose, in the first instance, the circumference of this circle to remain fixed, and that the elevation was confined to the space within it. The upheaval would throw the crust into a state of strain; and if it were inflexible, the strain must be relieved by fracture. Crevasses would thus intersect the crust. Let us now inquire what proportion the area of these open fissures is likely to bear to the area of the unfissured crust. An approximate answer is all that is here required; for the problem is of such a character as to render minute precision unnecessary. No one, I think, would affirm that the area of the fissures would be $\frac{1}{100}$th the area of the land. For let us consider the strain upon a single line drawn over the summit of the protuberance from a point on its rim to a point opposite. Regarding the protuberance as a spherical swelling, the length of the arc corresponding to a chord of 100 miles and a versed sine of 3 miles is 100·24 miles; consequently the surface to reach its new position must stretch 0·24 of a mile, or be broken. A fissure or a number of cracks with this total width would relieve the strain; that is to say, the sum of the widths of all the cracks over the length of 100 miles would be 420 yards. If, instead of comparing the width of the fissures with the length of the lines of tension, we compared their areas with the area of the unfissured land, we should of course find the proportion much less. These considerations will help the imagination to realize what a small ratio the area of the open fissures must bear to the unfissured crust. They enable us to say with certainty, for example, that to assume the area of the fissures to be $\frac{1}{10}$th of the area of the land would be quite absurd, while that the area of the fissures could be one-half or more than one-half that of the land would be in a proportionate degree unthinkable. If we suppose the elevation to be due to the shrinking or subsidence of the land all round our assumed circle, we arrive equally at the conclusion that the area of the open fissures would be altogether insignificant as compared with that of the unfissured crust.

To those who have seen them from a commanding elevation, it is needless to say that the Alps themselves bear no sort of resemblance to the picture which this theory presents to us. Instead of deep cracks with approximately vertical walls, we have ridges before us running into peaks, and gradually sloping to form valleys at angles which, I imagine, would average less than 40 degrees, many of them certainly not reaching 30. Instead of a fissured crust we have a state of things closely resembling the surface of the ocean when agitated by a storm. The valleys, instead of being much narrower than the ridges, occupy the greater space. A plaster cast of the Alps turned upside down, so as to invert the elevations and depressions, would exhibit blunter and broader mountains, with narrower valleys between them, than the present ones. The valleys that exist cannot, I think, with any correctness of language be called fissures. It may be urged that they originated in fissures: but even this is unproved, and, were it proved, would still make the fissures play the subordinate part of giving direction to the agents which are to be regarded as the real sculptors of the Alps.

The fracture theory, then, if it regards the elevation of the Alps as due to the operation of a force acting throughout the entire region, is, in my opinion, utterly incompetent to account for the conformation of the country. If, on the other hand, we are compelled to resort to local disturbances, the manipulation of the earth's crust which will be necessary to obtain the valleys and the mountains will, I imagine, bring the difficulties of the theory into very strong relief. Indeed an examination of the region from many of the more accessible eminences—from the Galenstock, the Grauhaupt, the Pitz Languard, the Monte Confinale—or, better still, from Mont Blanc, Monte Rosa, the Jungfrau, the Finsteraarhorn, the Weisshorn, or the Matterhorn, where local peculiarities are toned down, and the operations of the powers which really made this region what it is are alone brought into prominence, must, I imagine, convince every physically minded man of the inability of any fracture theory to account for the present conformation of the Alps. A correct model of the mountains, with an unexaggerated vertical scale, produces the same effect upon the mind as the prospect from one of the highest peaks. We are apt to be influenced by local phenomena which, though insignificant in view of the general question of Alpine conformation, are, with reference to our customary standards, vast and impressive. In a true model those local peculiarities disappear; for on the scale of a model they are too small to be visible; while the essential facts and forms are presented to the undistracted attention.

A minute analysis of the phenomena strengthens the conviction which the general aspect of the Alps fixes in the mind. We find, for example, numerous valleys which the most ardent plutonist would not think of ascribing to any other agency than erosion. That such is their genesis and history is as certain as that erosion produced the Chines in the Isle of Wight. From these indubitable cases of erosion—commencing, if necessary, with the small ravines which run down the flanks of the ridges, with their little working navigators at their bottoms—we can proceed, by almost insensible gradations, to the largest valleys of the Alps; and it would perplex the plutonist to fix upon the point at which, in his opinion, fracture begins to play a material part. In ascending one of the larger valleys, we enter it where it is wide and where the eminences are gentle on either side. The flanking mountains become higher and more abrupt as we ascend, and at length we reach a place where the depth of the valley is a maximum. Continuing our walk upwards we find ourselves flanked by gentler slopes, and finally emerge from the valley and reach the summit of an open col, or depression in the chain of mountains. This is the common character of the large valleys. Crossing the col, we descend along the opposite slope of the chain, and through the same series of appearances in the reverse order. If the valleys on both sides of the col were produced by fissures, what prevents the fissure from prolonging itself across the col? The case here cited is representative; and I am not acquainted with a single instance in the Alps where the chain has been cracked in the manner indicated. The cols are simply depressions; and in the case of many of them the unfissured rock can be traced from side to side.

The typical instance just sketched follows as a natural consequence from the theory of erosion. Before either ice or water can exert great power as an erosive agent, it must collect in sufficient mass. On the higher slopes and plateaus—in the region of cols—the power is not fully developed; but lower down tributaries unite, erosion is carried on with increased vigour, and the excavation gradually reaches a maximum. Lower still the elevations diminish and the slopes become more gentle; the cutting-power gradually relaxes, until finally the eroding agent quits the mountains altogether, and the grand effects which it produced in the earlier portions of its course entirely disappear.

I have hitherto confined myself to the consideration of the broad question of the erosion theory as compared with the fracture theory; and all that I have been able to observe and think with reference to the subject leads me to adopt the former. Under the term erosion I include the action of water, of ice, and of the atmosphere, including frost and rain. Water and ice, however, are the principal agents, and which of these two has produced the greatest effect it is perhaps impossible to say. Two years ago I wrote a brief note "On the Conformation of the Alps"*, in which I ascribed the paramount influence to glaciers. The facts on which that opinion was founded are, I think, unassailable; but whether the opinion fairly follows from the facts may be regarded as an open question. The arguments which have been thus far urged against the opinion appear to me to be far from conclusive. Indeed the idea of glacier erosion appeared so daring that its boldness was deemed by many its sufficient refutation. It is, however, to be remembered that a precisely similar position was taken up by many respectable people when the extension of ancient glaciers was first mooted. The idea was considered too hardy to be entertained; and the evidences of glacial action were sought to be explained by reference to almost any process rather than the true one. Let those who so wisely took the side of "boldness" in that discussion beware lest they place themselves, with reference to the question of glacier erosion, in the position formerly occupied by their opponents. Looking at the little glaciers of the present day—mere pigmies as compared to the giants of the glacial epoch—we find that from every one of them issues a river more or less voluminous, charged with the matter which the ice has rubbed from the rocks. Where the

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* Phil. Mag. vol. xxiv. p. 169.

rocks are of a soft character, the amount of this finely pulverized matter suspended in the water is very great. The water, for example, of the river which flows from Santa Catarina to Bormio is thick with it. The Rhine is charged with this matter, and by it has so silted up the Lake of Constance as to abolish it for a large fraction of its length. The Rhone is charged with it, and tens of thousands of acres of cultivable land are formed by it above the Lake of Geneva. In the case of every glacier we have two agents at work,—the ice exerting a crushing force on every point of its bed which bears its weight, and either rasping away this point in powder or tearing it bodily from the rock to which it belongs; while the water which everywhere circulates upon the bed of the glacier continually washes the detritus away and leaves the rock clean for further abrasion. Confining the action of glaciers to the simple rubbing away of the rocks, and allowing them sufficient time to act, it is not a matter of opinion, but a physical certainty, that they will scoop out valleys. But the glacier does more than abrade. Rocks are not homogeneous; they are intersected by joints and places of weakness, which divide them into virtually detached masses. A glacier is undoubtedly competent to root such masses bodily away. Indeed the *à priori* consideration of the subject proves the competence of a glacier to deepen its bed. Taking the case of a glacier 1000 feet deep (and some of the older ones were probably three times this depth), and allowing 40 feet of ice to an atmosphere, we find that on every square inch of its bed such a glacier presses with a weight of 375 lbs., and on every square yard of its bed with a weight of 486,000 lbs. With a *vertical* pressure of this amount the glacier is urged down its valley by the pressure from behind. We can hardly, I think, deny to such a tool the power to excavate.

While writing these remarks, I have refreshed my memory by reference to the paper of Mr. John Ball, published in the 25th volume of the Philosophical Magazine (Feb. 1863). Mr. Ball's great experience of the Alps is sure to render anything he writes regarding them interesting. I have read his paper and attended to his suggestions, but I confess I do not see the cogency of his arguments. An inspection of the map of Switzerland, with reference to the direction of its valleys, suggests to my mind no objection to the theory of erosion. The perusal of the paper has assured me that Mr. Ball has paid attention to the formation of ancient lakes. He deems their beds a prominent feature of Alpine valleys; and he considers the barriers which dammed them up, and which were not removed by the ancient glaciers, as "a formidable difficulty in the way of Prof. Tyndall's bold hypothesis." "Looking at the operation as a whole," writes Mr. Ball, "it is to me quite inconceivable that a glacier should be competent to scoop out valleys a mile or more in depth, and yet be unable to remove the main inequalities from its own channel." Assuredly a glacier *is* competent to remove such barriers, and they probably have been ground down in some cases thousands of feet. But being of more resisting material than the adjacent rock, they are not ground down to the level of that rock. Were its bed uniform in the first instance, the glacier would, in my opinion, *produce* the inequalities which Mr. Ball thinks it ought to remove. I have recently had the pleasure of examining some of these barriers in the company of Mr. Ball; and to me they represented nothing more than the natural accidents of the locality. It would, I think, be far more wonderful to find the rocks of the Alps perfectly homogeneous, than to find them exhibiting such variations in point of resistance as are actually observed.

The question of lake-basins is in competent hands; and on its merits I will at present offer no opinion. But I cannot help remarking that the dams referred to by Mr. Ball furnish a conclusive reply to some of the arguments which have been urged against Prof. Ramsay's theory. These barriers have been crossed by the ice, and many of them present steeper gradients than Prof. Ramsay has to cope with in order to get his ice out of his lake-basins. An inspection of the barriers shows that they were incompetent to embay the ice: they are scarred and fluted from bottom to top. When it is urged against Prof. Ramsay that a glacier cannot drop into a hole 2000 feet deep and get out again, the distance ought to be stated over which these 2000 feet have to be distributed. A depression 2000 feet deep, if only of sufficient length, would constitute no material obstacle to the motion of a great glacier. With a suitable pressure from behind, the glacier would assuredly scrape along its bed. The retardation of a glacier by its bed is often referred to as proving its incompetence as an erosive agent; but this very retardation is in some measure an expression of the magnitude of the erosive energy. Either the bed must give way, or the ice must slide over itself; and to make ice slide over itself requires great power. We get some idea of the crushing pressure which the moving glacier exercises against its bed from the fact that resistance, and the effort to overcome it, are such as to make the upper layers of a glacier move bodily over the lower ones—a portion only of the total motion being due to the progress of the entire mass of the glacier down its valley.

The sudden bend in the valley of the Rhone at Martigny has been regarded as conclusive evidence against the theory of erosion. Why, it has been asked, did not the glacier of the Rhone go straight forward instead of making this awkward bend? But if the valley be a crack, why did the crack make this bend?

The crack, I submit, had at least as much reason to prolong itself in a straight line as the glacier had. A statement of Sir John Herschel with reference to another matter is perfectly applicable here:—"A crack once produced has a tendency to run—for this plain reason, that at its momentary limit, at the point on which it has just arrived, the divellent force on the molecules there situated is counteracted only by half the cohesive force which acted when there was no crack, viz. the cohesion of the uncracked portion alone" (Proc. Roy. Soc. vol. xii. p. 678). To account for the bend, the adherent of the fracture theory must assume the existence of some accident which turned the crack at right angles to itself; and he surely will permit the adherent of the erosion theory to make a similar assumption. The influence of small accidents on the direction of rivers is beautifully illustrated in glacier streams, which, on slopes of equal inclination, cut either straight or sinuous channels, the determining causes being apparently of the most trivial character. In his interesting paper "On the Lakes of Switzerland," M. Studer refers to the bend of the Rhine at Sargans in proof that the river must there follow a pre-existing fissure. I made a special expedition to the place this year; and though I felt that M. Studer had good grounds for the selection of this spot, I was unable to arrive at his conclusion as to the necessity of a fissure.

In the interesting volume recently published by the Swiss Alpine Club, M. Desor informs us that the Swiss naturalists who met last year at Samaden visited the end of the Morteratsch glacier, and there convinced themselves that a glacier had no tendency whatever to imbed itself in the soil. I scarcely think that the question of glacier erosion, as applied either to lakes or valleys, is to be disposed of so easily. My experience regarding the Morteratsch glacier shall now be recounted. I this year took with me a theodolite to Pontresina, and while there had to congratulate myself on the invaluable aid of my friend Mr. Hirst, who in 1857 did such good service upon the Mer de Glace and its tributaries. We set out three lines across the Morteratsch glacier, one of which crossed the ice-stream near the well-known hut of the painter Georgei, while the two others were staked out, the one above the hut and the other below it. Calling the highest line A, the line which crossed the glacier at the hut B, and the lowest line C, the following are the mean hourly motions of the three lines, deduced from observations which extended over several days. On each line eleven stakes were fixed, which are designated by the figures 1, 2, 3, &c. in the Tables.

Morteratsch Glacier, Line A.

No. of stake.	Hourly motion.
1.	0·35 inch.
2.	0·49 ,,
3.	0·53 ,,
4.	0·54 ,,
5.	0·56 ,,
6.	0·54 ,,
7.	0·52 ,,
8.	0·49 ,,
9.	0·40 ,,
10.	0·29 ,,
11.	0·20 ,,

As in all other measurements of this kind, the retarding influence of the sides of the glacier is manifest: the centre moves with the greatest velocity.

Morteratsch Glacier, Line B.

No. of stake.	Hourly motion.
1.	0·05 inch.
2.	0·14 ,,
3.	0·24 ,,
4.	0·32 ,,
5.	0·41 ,,
6.	0·44 ,,
7.	0·44 ,,
8.	0·45 ,,
9.	0·43 ,,
10.	0·44 ,,
11.	0·44 ,,

The first stake of this line was quite close to the edge of the glacier, and the ice was thin at the place, hence its slow motion. Crevasses prevented us from carrying the line sufficiently far across to render the retardation of the further side of the glacier fully evident.

Morteratsch Glacier, Line C.

No. of stake.	Hourly motion.
1.	0·05 inch.
2.	0·09 ,,
3.	0·18 ,,
4.	0·20 ,,
5.	0·25 ,,
6.	0·27 ,,
7.	0·27 ,,
8.	0·30 ,,
9.	0·21 ,,
10.	0·20 ,,
11.	0·16 ,,

Comparing the three lines together, it will be observed that the velocity diminishes as we descend the glacier. In 100 hours the maximum motion of the three lines respectively is as follows:—

Maximum Motion in 100 *hours.*

Line A	56 inches.
„ B	45 „
„ C	30 „

This deportment explains an appearance which must strike every observer who looks upon this glacier from the Pitz Languard, or from the new Bernina Road. A medial moraine runs along the glacier, commencing as a narrow streak high up; but towards the end the moraine extends in width, and finally quite covers the terminal portion of the glacier. The cause of this is revealed by the foregoing measurements, which prove that a stone on the moraine where it is crossed by the line A, approaches a second stone on the moraine where it is crossed by the line C with a velocity of 26 inches in 100 hours. The moraine is in a state of longitudinal compression. Its materials are more and more crowded together, and must consequently move laterally and render the moraine at the terminal portion of the glacier wider than above.

The motion of the Morteratsch glacier, then, diminishes as we descend. The maximum motion of the third line is 30 inches in 100 hours, or 7 inches a day—a very slow motion; and had we run our lines nearer to the end of the glacier, the motion would have been slower still. At the end itself it is nearly insensible. Now I submit that this is not the place to seek for the scooping power of a glacier. The opinion appears to be prevalent that it is the snout of a glacier that must act the part of ploughshare; and it is certainly an erroneous opinion. The scooping power will exert itself most where the weight, and consequently, other things being equal, the motion is greatest. A glacier's snout often *rests upon* matter which has been scooped from the glacier's bed higher up. I therefore do not think that the inspection of what the end of a glacier does or does not accomplish can decide this question.

The snout of a glacier is potent to remove anything against which it can fairly abut; and this power, notwithstanding the slowness of the motion, manifests itself at the end of the Morteratsch glacier. A hillock, bearing pine trees, was in front of the glacier when Mr. Hirst and myself inspected its end; and this hillock is being bodily removed by the thrust of the ice. Several of the trees are overturned; and in a few years, if the glacier continues its reputed advance, the mound will certainly be ploughed away.

I will here add a few measurements executed on the Rosegg glacier: the line was staked out across the trunk formed by the junction of the Rosegg glacier proper with the Tschierva glacier, a short distance below the rocky promontory called Agaliogs.

Rosegg Glacier.

No. of stake.	Hourly motion.
1.	0·01 inch.
2.	0·05 „
3.	0·07 „
4.	0·10 „
5.	0·11 „
6.	0·13 „
7.	0·14 „
8.	0·18 „
9.	0·24 „
10.	0·23 „
11.	0·24 „

This is an extremely slowly moving glacier; the maximum here found hardly amounts to 7 inches a day. Crevasses prevented me from continuing the line quite across the glacier.

To return to the question of Alpine conformation,—it stands, I think, thus:—We have, in the first place, great valleys, such as those of the Rhine and the Rhone, to which we might conveniently give the name of valleys of the 1st order. The mountains which flank these main valleys are also cut by lateral valleys which run into the main one, and which may be called valleys of the 2nd order. When these latter are examined, smaller valleys are found running into them, which may be called valleys of the 3rd order. Smaller ravines and depressions, again, join the latter, which may be called valleys of the 4th order, and so on until we reach streaks and cuttings so minute as not to merit the name of valleys at all. At the bottom of every valley we have a stream, diminishing in magnitude as the order of the valley ascends, carving eternally at the earth and carrying its materials to lower levels. We find moreover that the larger valleys have been filled for untold ages by glaciers of enormous dimensions, and that these glaciers were always moving, grinding down and tearing away the rocks over which they past. We have, moreover, on the plains which extend at the feet of the mountains, and in enormous quantities, the very matter derived from the sculpture of the mountains themselves. The plains of Italy and Switzerland are cumbered by the débris of the Alps. The lower, wider, and more level valleys are also filled to unknown depths with the materials derived from the higher ones. In the vast quantities of moraine-matter which cumber many of the valleys we have also suggestions as to the magnitude of the erosion which has taken place. This moraine-matter, moreover, is only in part derived from the falling of rocks from the eminences upon the glacier; it is also in great part derived from the grinding and the ploughing-out of the glacier itself. This accounts for the magnitude of many of these ancient moraines, which date from a period when almost all the mountains were covered with ice and snow, and when consequently the quantity of moraine-matter derived from the naked crests cannot have been considerable. The erosion theory ascribes the formation of Alpine valleys to the agencies here briefly referred to. It invokes nothing but true causes. The artificers by which its work is performed are still there, though, it may be, in diminished strength; and if they are granted sufficient time, it is demonstrable that they are competent to produce the effects ascribed to them. And what does the fracture theory offer in comparison? From no possible application of this theory, pure and simple, can we obtain the slopes and forms of the mountains. Erosion must in the long run be invoked, and its power therefore conceded. The fracture theory infers from the disturbances of the Alps the existence of fissures; and this is a probable inference. But that they were of a magnitude sufficient to determine the conformation of the Alps, and that they followed, as the Alpine valleys do, the lines of natural drainage of the country, are assumptions which do not appear to me to be justified either by reason or by observation.

Royal Institution,
September 1864.

P.S.—The foregoing paper was in the printer's hands before it was my privilege to read the last Anniversary Address to the Geographical Society by its President, Sir Roderick Murchison. I have since considered the arguments, and given, I trust, due weight to the authorities urged and cited in that excellent Address against the theory of erosion, as applied to the valleys of the Alps. But the effect on my mind is not such as to induce me to alter the opinions, based on observed facts, which I have ventured to express in these pages.

XXXI. *On the Law of the Expansion of the Gases by increase of Temperature.* *By* Professor POTTER, *A.M.*[*]

IN the theory of heat, the law of the expansion of the gases by increase of temperature is a most important subject, not only on account of the air-thermometer having been taken as the

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[*] Communicated by the Author.

standard to which the liquid and solid thermometers were compared, but also with respect to the chemical hypothesis of equivalent combining-volumes of gases.

The laws of the expansion of the gases by equal increments, proposed by Gay-Lussac, and of uniform expansion, proposed by Dalton, give results which differ very little between the points of the freezing and boiling of water, but they diverge greatly at higher temperatures. Dalton's law, expressed in the formula $V=V_0 \cdot \epsilon^{\alpha t^\circ}$, where V is the volume of a gas at t° above the zero-point of the thermometric scale, and V_0 the volume at the zero of the scale, had a greater *primâ facie* claim to be considered a physical law than that of Gay-Lussac, of which the formula is $V=V_0(1+\alpha t^\circ)$, because, the expansion for one degree in the latter being $\frac{V_1-V_0}{V_0}=\alpha$, obtained by putting $t^\circ=1^\circ$, we do not see why any temperature for a gas may not be taken as reasonably as the freezing-point of water, and that generally $\frac{\delta V}{V}=\alpha=$ constant: then integrating we obtain for Dalton's law $V=V_0 \cdot \epsilon^{\alpha t^\circ}$; and expanding we have

$$V=V_0\epsilon^{\alpha t^\circ}=V_0\left(1+\alpha t^\circ+\frac{\alpha^2 t^{\circ 2}}{1\,.\,2}+\frac{\alpha^3 t^{\circ 3}}{1\,.\,2\,.\,3}+\&\text{c.}\right).$$

By stopping at the term with the first power of $\alpha=\frac{1}{480}$ on Fahrenheit's scale nearly, we have Gay-Lussac's law. Nevertheless I now think that Gay-Lussac's law is a nearer approximation than Dalton's in ordinary temperatures, and a very much nearer one for high temperatures.

The important experiments of M. Regnault show that the coefficient of expansion for air increases with the density, or as the molecules are nearer together*. Carbonic acid gas exhibits the same property still more strongly, and sulphurous acid gas shows it in a still higher degree. The expansion or value of α for the interval between the freezing- and boiling-points of water at ordinary atmospheric pressure for carbonic acid gas being ·37099, it becomes ·38455 at a pressure of rather more than three atmospheres; whilst hydrogen gas shows no reliable variation.

Now as the atoms of a gas approach each other when the temperature is diminished, we have a right to expect the same result at lower temperatures at the same pressure: and Gay-Lussac's law gives such a result; for the constant increment $\alpha=\frac{V_1-V_0}{V_0}$ bears a smaller ratio to the actual volume $V=V_0(1+\alpha t^\circ)$ as the degrees t° are increased, and a greater ratio as the degrees $-t^\circ$ are more below the freezing-point of water.

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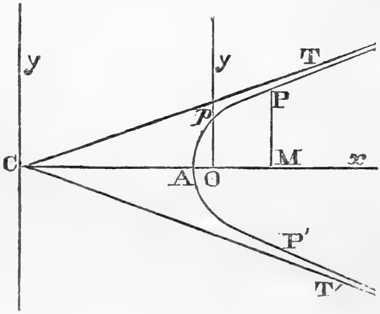
* *Relation des Expériences*, &c., vol. i. p. 110.

The air-thermometer being taken as the standard, we are not able to employ it to show its own errors, but must have recourse to the liquefiable gases, where the deviation from Gay-Lussac's law becomes very great compared with that of air, and will consequently be well ascertained by taking the air-thermometer as exact in the first instance, and afterwards attributing to air the same law, with different constants, as that found for the liquefiable gas. In this manner we shall find that the air-thermometers must give place to a thermometer formed with hydrogen gas, or the mercurial thermometer accurately graduated for the law of uniform expansion, or otherwise for low temperatures the spirit-thermometer accurately graduated according to the law of the hyperbolic expansion of alcohol, when critically exact temperatures are required to be known.

M. Regnault found* that a thermometer formed with sulphurous acid gas, on being compared with an air-thermometer, gave the following results for the mean value of α for Centigrade degrees :—

From $\overset{\circ}{0}$ Centigrade to	$\overset{\circ}{98}{\cdot}12$	C. the mean value of α	$=\cdot0038251$
,, ,,	$102{\cdot}45$	,, ,,	$=\cdot0038225$
,, ,,	$185{\cdot}42$	,, ,,	$=\cdot0037999$
,, ,,	$257{\cdot}17$	,, ,,	$=\cdot0037923$
,, ,,	$299{\cdot}90$	,, ,,	$=\cdot0037913$
,, ,,	$310{\cdot}31$	,, ,,	$=\cdot0037893$



In seeking for the law which will include these results, we shall find that they conform to a hyperbolic law of expansion; and Gay-Lussac's law arises from taking at great distances from the centre the asymptote of the hyperbola for the arc; and thus Gay-Lussac's law becomes an exceedingly near approximation for hydrogen gas, and a very near one for oxygen, nitrogen, and atmospheric air.

To apply the equation of the hyperbola to the results for sulphurous acid, in the figure let C x, C y be the axes of coordinates to the origin C as centre of the hyperbolic arc P A P′; let C T, C T′ be the asymptotes; let O be any origin from which the temperatures are represented along the axis of x, whilst the

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* *Relation des Expériences,* &c., vol. i. pp. 188, 189.

volumes of the gas are represented by the ordinates, as $PM = y$ when $CM = x$. Let $CA = a =$ semi-major axis of the hyperbola, $CO = m$, and $OM = x'$ the temperatures t° on the Centigrade scale, so that $x = m + x'$; then, the equation of the hyperbola being

$$y^2 = \frac{b^2}{a^2}(x^2 - a^2)$$

$$= \frac{b^2}{a^2}\left((m + x')^2 - a^2\right),$$

we have to find from three conditions the values of $\frac{b^2}{a^2}$, m, and a^2.

Now, taking the volume of the gas at the freezing-point of water, and (represented in the figure by $pO = y_0$) as 100 measures, and $OM = x'$ being the Centigrade degrees, we have, if $PM = y'$,

$$y_0{}^2 = \frac{b^2}{a^2}(m^2 - a^2),$$

$$y'^2 = \frac{b^2}{a^2}\left((m + x')^2 - a^2\right),$$

and similarly

$$y''^2 = \frac{b^2}{a^2}\left((m + x'')^2 - a^2\right),$$

which by eliminating give

$$m = \frac{a^2}{2b^2}\left(\frac{y'^2 - y_0{}^2}{x'}\right) - \frac{x'}{2} = \frac{a^2}{2b^2}\left(\frac{y''^2 - y_0{}^2}{x''}\right) - \frac{x''}{2},$$

$$\frac{b^2}{a^2} = \frac{x''(y'^2 - y_0{}^2) - x'(y''^2 - y_0{}^2)}{x'' \,.\, x'^2 - x' \,.\, x''^2},$$

$$a^2 = m^2 - \frac{a^2}{b^2} \cdot y_0{}^2.$$

Calculating the volumes $y_1, y_2, y_3 \ldots y_6$ from M. Regnault's values of α at the Centigrade temperatures $x'_1, x'_2, x'_3 \ldots x'_6$, we have as follows: at

$x'_1 = 98{\cdot}12$ C. then $y_1 = y_0 \times 1{\cdot}37532$,
$x'_2 = 102{\cdot}45$,, $y_2 = y_0 \times 1{\cdot}39162$,
$x'_3 = 185{\cdot}42$,, $y_3 = y_0 \times 1{\cdot}70458$,
$x'_4 = 257{\cdot}17$,, $y_4 = y_0 \times 1{\cdot}97527$,
$x'_5 = 299{\cdot}90$,, $y_5 = y_0 \times 2{\cdot}13701$,
$x'_6 = 310{\cdot}31$,, $y_6 = y_0 \times 2{\cdot}17586$.

Taking $x'_0\,y_0$, $x'_1\,y_1$, $x'_5\,y_5$ to determine the constants $\frac{b^2}{a^2}$, m, and a^2, we find

$$\frac{b^2}{a^2}=\cdot 13913,\text{ and }\frac{b}{a}=\cdot 3730,$$

$$m=277\cdot 464,$$

$$a^2=5111\cdot 2,\text{ and }a=71\cdot 492.$$

Applying these to find the remaining values of y by the equation of the hyperbola, we have

$$\begin{aligned}y_0&=100\\ y_1&=137\cdot 533,\\ y_2&=139\cdot 176,\\ y_3&=170\cdot 584,\\ y_4&=197\cdot 628,\\ y_5&=213\cdot 700,\\ y_6&=217\cdot 613,\end{aligned}$$

and we see that the ordinates of the hyperbola furnish volumes as near to M. Regnault's experimental results as can be expected.

The value of $\frac{b}{a}=\cdot 3730$ is trig. tangent of the angle which the asymptote makes with the axis of x, and is the extreme value of α, which we see is not the same as for other gases, which M. Regnault expected* might be the case in the limit for very high temperatures, or in the state of extreme dilatation.

By differentiating the equation of the hyperbola to the centre as origin, or

$$y^2=\frac{b^2}{a^2}(x^2-a^2),$$

we have

$$\frac{dy}{dx}=\frac{b}{a}\cdot\frac{1}{\sqrt{1-\frac{a^2}{x^2}}};$$

and when $x=\pm a$, then $\frac{dy}{dx}=$ infinity; and this must be at the point of liquefaction of the gas, which, when known under a given pressure, will give an important datum, and probably much more accurate than can be found from the discussion of experiments like the preceding. When $x=$ infinity, we have $\frac{dy}{dx}=\frac{b}{a}$, which equals α in Gay-Lussac's law. The law of Amontons being expressed in the form $p=\kappa\rho(1+\alpha t^\circ)$, must be received as the true law within the limits of accuracy which can be attributed to the laws of Boyle and Gay-Lussac, of which it is compounded, but it is not an absolutely exact law for any gas.

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* *Relation des Expériences*, &c, vol. i. p. 120.

XXXII. *On Molecular Physics.* *By* Prof. W. A. Norton.

[Continued from p. 204.]

Molecular Constitution and Mechanical Properties of Bodies.

EVERY body of matter consists of separate particles, or molecules in a state of equilibrium under the action of the forces proper to the particles, or of these in connexion with extraneous forces taking effect upon the particles. The interstices between the molecules we conceive to be pervaded by both the electric and the universal æther, having probably different densities in different substances. The state of equilibrium in which each particle of the mass subsists, implies that the effective forces acting upon it, from opposite sides, are equal and directly opposed, or else that the effective forces of each side are equal to zero. The different mechanical properties of different substances may be ascribed, primarily, to differences in the value of the ratio of the constants of electric attraction and repulsion $\left(\frac{n}{m}\right.$, in Table I.$\left.\right)$; and to a certain extent also to differences in the size of the molecular atmospheres, upon which the value of k in Table I. partly depends. In consequence of these supposed differences in the value of the ratio $\frac{n}{m}$, each substance should have its own special curve of molecular action. It is natural to suppose that the constant n of the force of attraction exerted by the atom upon its atmosphere would in general increase with the mass of the atom, and so that the force of cohesion would be greatest in those substances whose atomic weights are the greatest. But as we cannot affirm that the weight of an atom must necessarily be proportional to the force of attraction exerted by it upon its electric atmosphere, and as the constant m may also be subject to variations, substances of nearly equal atomic weights (*e. g.*, gold, platinum, bismuth, and lead) may have different properties.

The molecules of a substance in the solid state may be aggregated together as a homogeneous mass, or in groups more or less complex. The mechanical properties of the mass vary with the mode of aggregation. The form of aggregation assumed, in the process of solidification, depends upon the circumstances, with respect to cooling, pressure, &c., under which the solidification occurs. The effect of the same circumstances should vary with different substances, with their properties in relation to heat; but these properties are primarily dependent upon the general features in the constitution and condition of the molecules, upon which the laws of effective molecular action, as shown by the proper curve, depend.

Contemplating from our present point of view the varying mechanical states and conditions which the same substance may assume under different circumstances, we are led to recognize, as an essential physical feature, upon which such changes either wholly or partially depend, the fact that the mechanical condition of the individual molecules is not fixed and unchangeable, but liable to material variations. We perceive their atmospheres expanding under the influence of heat, and contracting from the effect of external pressure, and that certain phenomena and permanent changes of property result from these atmospheric changes (*e. g.* changes of property in passing from the solid to the liquid form, or *vice versâ*; permanent displacement of particles produced by the temporary action of forces of a certain intensity upon bodies).

States of Aggregation of Matter.—These are three essentially different states of equilibrium. In the *solid* form, the particles immediately contiguous to each other are in a condition of equilibrium under the action of their own molecular forces; if more distant particles exercise any effective action, it is attractive, and neutralized by a similar action on the other side of the particle. To be more definite, each molecule of the mass is surrounded by others at various orders of distance from it; and each pair of molecules at the first order of distance from each other are in a condition of equilibrium by themselves, which is equivalent to saying that their electric atmospheres are separated by the distance O*a*, fig. 1 (p. 203). For the second order of distance the action should then be attractive; but it may very well be that when a permanent equilibrium of the mass has been reached, the atmospheres of two particles at this order of distance will be so expanded by their attractive action, on the line of their centres, that, for the diminished value of $\frac{n}{m}$ thus resulting, the distance between the atmospheres on this line will be the increased distance O*a* for the curve corresponding to this diminished value of $\frac{n}{m}$. Upon this supposition, each particle would be separately in equilibrium with every particle contiguous to it, both at the first and second order of distance. We shall have occasion to note hereafter that this state of things is probably more or less perfectly realized under different circumstances of solidification. As to the action of more distant molecules, it is first to be observed that, if two molecules are in equilibrium under their mutual actions, the attractive and repulsive impulses exerted by each upon the central atom of the other must be equal, and therefore that no effective action, either attractive or repulsive, can be transmitted to other more distant particles on the same line. Under these circumstances, one molecule, in receiving the action of another, *intercepts* the action that would otherwise take effect upon other more distant molecules. This being admitted, it may be perceived, on examining Table I., that the attractive actions of particles which lie beyond the second order of distance from a given particle, will be in a great measure intercepted by intervening particles. In what has now been stated with respect to the solid condition, we have had in mind a homogeneous mass of molecules only. We cannot here enter upon the consideration of the case in which the molecules are aggregated into groups.

In the *liquid* state, the contiguous particles repel each other; and particles more distant exert no sensible action, or a feeble attractive one. Here, as in the case of a solid, the sensible action is confined chiefly to particles that lie at the first and second orders of distance. These remarks apply to the general mass of the liquid. The molecular atmospheres are in an expanded condition from the effect of the heat of fluidity; and it is from this fact that the peculiar properties of the liquid state result. As we draw near the surface of the liquid, the atmospheres are in a condition of greater and greater expansion as the necessary result of the process of liquefaction, and therefore their proper attractive actions are less and less. From this cause it happens that each particle near the surface is more effectively attracted by those below it, beyond the first order of distance, than by those above it, and thus each layer of particles is compressed upon that immediately below it; also to a certain depth more particles will exert their attraction from below than from above. As a consequence, the density must increase from the surface to a certain small depth below it, and a force of compression be exerted throughout the whole liquid mass. This force determines, and is in equilibrium with, a mutual repulsion between the particles of the liquid. From the essential nature of a liquid, as we shall soon see, this increasing molecular repulsion, from the surface downward, operates in all directions from each molecule, and so tends to neutralize the attractive actions between molecules separated by the second order of distance; as the final result, therefore, at the depth at which the density ceases to increase, and all greater depths, the action between two such moleules should be either feebly attractive, or altogether evanescent*.

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* The theory of the existence of a contractile force at the surface of a liquid, as the result of molecular action, was advocated by Young and Poisson, and employed by them in explanation of the phenomena of capillarity. It has also been ably sustained and illustrated by Professor Henry by many ingenious experiments.

The views which have now been presented enable us to form a definite conception of the probable arrangement of the molecules of a liquid. If the state of equilibrium be such as we have represented, we must conclude that a perfectly symmetrical arrangement of particles, similar to that which subsists in crystals, prevails throughout the whole mass.

We conceive the fundamental distinction between a solid and a liquid, from the mechanical point of view, to be that the external impulses which fall upon the molecule of a solid, are propagated, either wholly or chiefly, in their original line of direction; while those which fall upon the molecule of a liquid are radiated in every direction from it. The physical cause of this difference in the mode of propagation of a force appears to be the simple fact that in the process of liquefaction the molecular atmospheres are forced by the heat of fluidity to a decidedly greater distance from the atoms which they surround; thus leaving below them a much larger volume of universal æther, to receive the impulses propagated down to it. If this difference between the mode of propagation of impulses by the molecules of a solid and liquid be admitted, it is not difficult to see that we have a sufficient cause for the different mechanical properties attendant upon these two states of aggregation, without having recourse to the prevalent idea of a permanent polarity of simple atoms. So far as any polarization of molecules comes into operation, we shall have occasion to remark, in discussing briefly the topic of crystallization, that it is simply an induced, and for the most part a temporary condition of the molecular atmospheres, developed in the act of solidification.

In the *aëriform* state the particles are so widely separated that each is repelled by all those which surround it, within the limit of effective action, and the equilibrium is determined by external pressure. The properties of gases and vapours, and the laws of their expansion and contraction, are deducible from equation (3), p. 200. The value of x that obtains when a vapour formed at any temperature has its maximum tension, is the distance Od, fig. 1, answering to the maximum molecular repulsion dn; and this varies for different temperatures, because the ratio $\frac{n}{m}$ decreases as the temperature rises. (See different values of maximum repulsion answering to different values of the ratio $\frac{n}{m}$ given in Table I., p. 200.)

The process of transition from the solid to the liquid state occurs at the surface of the mass. As the heat is absorbed, the molecules near the surface recede from each other; and when this expansion has reached a certain point, the attractive forces of the particles at the different orders of distance come successively into action, being less intercepted by intervening particles. At the same time, the individual molecular atmospheres expand, or recede from their central atoms, under the action of the heat-pulses that penetrate to these atoms; and so the energy of the attractive force of each of these molecules declines. The surface particles will thus continue to recede at the same time that they are restrained by the attractions of those below them. This effect will extend from the surface downward; and as a final result, a certain number of layers are brought into the liquid condition, in which, as we have seen (p. 278), the particles mutually repel each other, in consequence of the exertion of a compressing force at the surface. In the case of a liquid that emits vapour at the temperature of liquefaction, we must conclude that the particles at the very surface become ultimately subject to an effective repulsion from the united action of those below it, which is in equilibrium with the tension of the vapour resting on the surface; and that this effective repulsion extends to all points above the surface.

The heat of fluidity is consumed in forcing up the molecular atmospheres. As a final result of the liquefaction, these atmospheres remain in an expanded condition. The effect of this expansion is to diminish the values of v given by equation (1) (see p. 200), and increase the distance Oa, fig. 1. The actual distance between two contiguous atmospheres is less than the increased distance Oa, by reason of the compressing force that takes effect throughout the liquid mass. But the ultimate compression imparted to the individual atmospheres will depend in a great degree upon the final value of the attractive action v between the molecules, and may therefore still be less than that which obtained in the solid state. In this diminished value of v we have, at the same time, the explanation of the diminished force of cohesion attendant upon the liquid state. The comparative densities of the liquid and solid also depend upon v. For we have just seen that the distance between the contiguous atmospheres of two particles of the liquid is less than the increased value of Oa, but this distance may, according to the intensity of the attractive force v, be either greater or less than the original value of Oa, which was the distance between the atmospheres of the same particles in the solid condition. Accordingly the liquid may be either more or less dense than the solid from which it is derived.

The passage from the liquid to the solid state is essentially the inverse of that which has just been under consideration, and in the general survey we are now taking need not be considered in detail. The mass of molecules and their individual atmospheres now contract instead of expanding; and in the final act of solidification the contiguous molecules assume the positions due to their own special forces. While all this is being accomplished, the molecular atmospheres contract, and heat is given out.

The explanation of the process of *evaporation* will be readily inferred from what has already been stated with regard to the condition of the surface of a liquid (p. 280). The nice equipoise of the surface particles may be disturbed either by a slight elevation of temperature, or a diminution of the tension of the vapour resting upon them. The cooling effect of the evaporation is to be attributed to the expansion which the electric atmospheres experience on being freed from the compressing forces previously existing*.

In the process of *ebullition,* the expansive action of the heat absorbed by the lower layers of the liquid increases until the superincumbent pressure, the cohesive attraction of the vessel for the liquid, and the effective attractions subsisting between the molecules of the liquid (represented by the ordinates between *a* and *b*, fig. 1), are overcome. When this point is reached at any part of the liquid stratum, the separated particles will expand rapidly into bubbles of vapour, in opposition to the pressure of the atmosphere, and the attractions denoted by the decreasing ordinates between *b* and *c*, fig. 1. The expansion should continue until the distance between the atmospheres of two particles increases to the limit O*d*, at which the repulsion attains to its maximum value; or rather to a limiting distance somewhat greater than O*d*, at which the repulsion due to the heat-pulses present in the molecules, *plus* the molecular repulsion at that distance, is equal to the external pressure.

It cannot proceed further than this without a direct expenditure of heat-force, which will raise the temperature of the vapour. The heat which becomes latent, as the phrase is, is expended in the act of expansion, and in forcing up the molecular atmospheres in opposition to the attractive action of the atoms and all compressing forces. The amount of work thus taken up by the atmospheres manifests itself also as work of expansion, since it is so much work of the atomic attraction and of the compressing forces neutralized. When the heat-pulses are not wholly ex-

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* It is apparently not necessary to suppose, as has been done on p. 280, that the tension of the vapour resting on the surface of a liquid, when at its maximum, is in equilibrium with the outward repulsion experienced by the outer layer of liquid particles. The equilibrium may be a dynamical one, the vapour may be continually rising at certain points of the surface and continually passing back into the liquid condition at other points, the condensation compensating exactly for the evaporation.

pended in this manner, a portion of them pass into the molecular atmospheres and elevate the temperature of the liquid. If the pressure upon the free surface of the liquid exceeds the pressure of the atmosphere, the molecular atmospheres are more compressed, the value of m becomes greater, and the ratio $\frac{n}{m}$ diminishes in consequence; from this cause the limit of the recess of the particles (Od, fig. 1) diminishes, and the maximum repulsion dn increases (see Table I.). The resulting vapour has, therefore, at the same time a higher tension and a greater density.

According to the theoretical views now advanced, the "interior work" which Tyndall maintains is expended in the act of liquefaction, and also in that of vaporization, in "moving the atoms into new positions," or in conferring "potential energy" upon them, is consumed in each instance in pressing up the electric atmospheres that surround the atoms of the substance; and heat disappears in the process in proportion to the effect thus produced.

[To be continued.]

XXXIII. *Summary of the Properties of certain Stream-Lines.*

By W. J. Macquorn Rankine, *C.E., LL.D., F.R.SS.L.&E.*[*]

1. THE investigation, of which the present paper is a summary, consists of three parts. It is a sequel to one of which an abstract was read at the Meeting of the British Association in 1863, and which has since been printed in full in the Philosophical Transactions[†]. It relates to the paths in which the particles of a liquid move past a solid body. In the previous paper (which was confined to motion in two dimensions) those paths were called "Water-Lines," and were treated of with a view mainly to their use as figures for the horizontal or nearly horizontal water-lines of ships. In the present paper they are called "Stream-Lines," as being a more general term, and one less liable to be misunderstood when motion in three dimensions is considered.

The term "Neoïd" (νηοειδὴς, ship-like) proposed in the previous paper as a general name for water-line curves in two dimensions, may be extended to all the stream-lines discussed in the present paper; for they are all applicable to certain lines on the surface of a ship.

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[*] Communicated by the Author, having been read at the British Association Meeting, Bath, September 19, 1864.

[†] An abstract of that previous investigation appeared in the Philosophical Magazine for October 1863.

PART I.—*On some Exponential Stream-Lines in two Dimensions.*

2. It is well known that amongst the functions which satisfy the conditions of liquid motion in two dimensions, are comprehended all those of the form

$$y+\Sigma . e^{ay} \cos ax.$$

Such functions as the above obviously represent curves consisting of an endless series of repetitions of the same figure; and many of those curves resemble the profiles of waves.

3. The first part of the investigation consists of a discussion of the properties of the curves represented by the simplest of those exponential stream-line functions, viz.

$$b=y-e^{-y} \cos x. \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(I.)}$$

By giving to b a set of values in arithmetical progression, this function is made to represent a set of stream-lines, dividing an indefinitely extended plane layer of liquid into a series of curved streams of equal flow. Each of those stream-lines consists of an endless series of repetitions of the same figure, the length parallel to x of each repetition being 2π; and each repetition consists of a pair of symmetrical halves.

4. The graphic construction of those stream-lines is very easy, by the aid of a general method of constructing curves first used by Professor Clerk Maxwell, and applied by the present author to stream-lines in the previous investigation already referred to.

Draw a series of straight lines parallel to x, and having for their equation

$$y=m,$$

—the values of m being in arithmetical progression, positive and negative, with a fraction for their common difference, which should be the smaller the more accurate the drawing is to be. Then draw a series of curves of hyperbolic-logarithmic cosines, having for their equation

$$e^{-y} \cos x=m',$$

—the values of m', positive and negative, forming an arithmetical progression, and having the same common difference with those of m. The curves with positive values of m' lie between $x=0$ and $x=\frac{\pi}{2}$; those with negative values between $x=\frac{\pi}{2}$ and $x=\pi$; and the straight line parallel to y, at $\frac{\pi}{2}$, is an asymptote to them all. One and the same mould serves to trace all those curves; for they differ only in the maximum value of y, which is

$\pm$hyp. log m'. Then trace a series of curves diagonally through the intersections of the network already drawn, in such a manner as to make $m-m'=b$ for each curve; these will be the required stream-lines.

5. The ordinates for which x is an odd multiple of $\pm\frac{\pi}{2}$ are asymptotes to all the stream-lines at the negative side of the axis of x, and are also intersected by each stream-line at the point where $y=b$.

6. Maximum values of y for all the stream-lines occur on the ordinates where x has the value 0, or any even multiple of $\pm\pi$.

7. Minimum values of $+y$ and $-y$ occur on each ordinate where x is an odd multiple of $\pm\pi$, but for those stream-lines only for which $b>1$. The stream-lines for which $b<1$ do not intersect those ordinates.

8. The stream-line for which $b=1$ consists of an endless series of equal and similar curves, each adjacent pair of which cut each other at right angles and the axis of x at angles of 45°, in the points where x is an odd multiple of $\pm\pi$.

9. Each stream-line for which $b<1$ consists of an endless series of equal and similar detached curves, having maximum and minimum values of x given by the equation

$$\cos x = -e^{-1+b}.$$

10. Each stream-line for which $b>1$ is made up as follows:—at the positive side of the axis of x, a continuous curve, presenting an endless series of equal and similar waves; at the negative side, an endless series of equal and similar detached curves.

11. The wave-line curves thus formed, as they become more remote from the axis of x (that is, as b increases), approximate more and more nearly to the trochoidal form, which is known to be that of free waves in deep water; and so rapid is that approximation, that though for $b=1$ the difference between the two kinds of wave-line is very great, it becomes almost undistinguishable for $b=1\frac{1}{4}$.

12. Quantities proportional to the component velocities of a particle and to the square of its resultant velocity, are derived from the stream-line function as follows:

$$\left.\begin{aligned} u &= \frac{db}{dy} = 1 + e^{-y}\cos x = 1 + y - b, \\ v &= -\frac{db}{dy} = -e^{-y}\sin x; \end{aligned}\right\} \quad . \quad . \quad \text{(II.)}$$

$$\left.\begin{aligned} u^2 + v^2 &= 1 + 2e^{-y}\cos x + e^{-2y} \\ &= 1 + 2(y-b) + e^{-2y}. \end{aligned}\right\} \quad . \quad . \quad \text{(III.)}$$

At the point where the curves $b=1$ cross the axis of x, both the component velocities are null. The unit of velocity in each of those expressions is the velocity of a particle at an infinite distance in the positive direction from the axis of x, for which particle we have $u=1$, $v=0$.

13. Suppose the plane of x and y to be vertical, and y to be positive downwards; let the absolute value of the unit of measure (that is, the radius of the circle whose circumference is a wave-length) be denoted by R; and let the heaviness (or weight of a unit of volume) of the liquid be W. Then the stream-lines for which b is not less than 1 may represent the profiles of a series of forced waves, capable of travelling with the absolute velocity

$$c=\sqrt{g\mathrm{R}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(IV.)}$$

being the same with that of free waves of the same length; and the absolute values of the velocities of any particle relatively to still water will be

$$\left.\begin{array}{l}\text{horizontal component, } c(u-1)=ce^{-y}\cos x;\\ \text{vertical component, } cv=-ce^{-y}\sin x;\\ \text{resultant velocity, } c\sqrt{\{(u-1)^2+v^2\}}=ce^{-y}.\end{array}\right\} \quad . \qquad \text{(V.)}$$

14. Those forced waves differ from free waves in the following respects. First, in free or trochoidal waves, each wave-surface is a surface of constant pressure, so that the upper surface of the liquid needs no pressure to be applied to it to compel the waves to travel; whereas in the waves now in question the pressure at each wave-surface is not constant, being expressed by the following formula,

$$p=\text{constant }+\mathrm{W}b-\frac{\mathrm{WR}e^{-2y}}{2}, \quad . \quad . \quad . \qquad \text{(VI.)}$$

of which the last term is variable; and the upper surface requires a pressure varying according to this law to be applied to it, in order to compel the waves to travel.

Secondly, free or trochoidal waves begin to break as they reach the cycloidal form, in which the surface near the crest is vertical, and the crest forms a cusp; whereas in the waves now in question the steepest possible form, which cannot be passed without breaking, is that of the stream-line $b=1$, whose crest is formed by two surfaces meeting each other at right angles, and sloping in opposite directions at 45°. Thirdly, the particles of water in free waves revolve in circles, and do not permanently advance; whereas the orbit of each particle in the waves now in question is an endless coiled or looped curve, in which each revolution is accompanied by an advance. The figure of that orbit is determined by the ratio which its radius of curvature bears to the unit of measure R, viz.

$$\frac{\sqrt{\{(u-1)^2+v^2\}}}{u}=\frac{e^{-y}}{1+y-b}. \quad . \quad . \quad \text{(VII.)}$$

The waves whose motion is investigated by Professor Stokes in the Cambridge Transactions are of a character intermediate between trochoidal waves and those here considered.

15. As waves are frequently observed whose figures present a general likeness to that now described, it is probable that a pressure approximating to the law expressed by equation (VI.) may be exerted upon them by the wind.

16. It is evident that a pressure varying according to that law, or nearly so, will be exerted by the bottom of a ship upon the water, when the figures of the buttock-lines, or vertical longitudinal sections of her after-body, are exponential stream-lines, or trochoidal waves approximating to them, as in Mr. Scott Russell's system of shipbuilding.

PART II.—*On Lissoneoïds in three Dimensions.*

17. The second part of the investigation relates to the mathematical properties of stream-lines of smoothest gliding in three dimensions. The properties of such lines in two dimensions were investigated, and the name "Lissoneoïds" proposed for them, in the previous paper already referred to. Their essential mechanical properties are, to have fewer and less abrupt maxima and minima of the speed of gliding of the particles on them than on other stream-lines belonging to the same mathematical class, and to be the fullest lines of their class consistently with not raising more waves than are unavoidable, when they are employed as the lines of a ship.

18. The mathematical condition which such a stream-line fulfils is, that at the midship-section or broadest part of the solid to which the line belongs, two points of maximum and one of minimum speed of gliding coalesce into one point.

19. The investigation shows that the before-mentioned condition is expressed mathematically as follows, for any stream-line which at its greatest breadth is parallel to the axis of x. Let u be the longitudinal component, and v and w the transverse components of the speed of gliding of a particle along the stream-line; then at the point where that line crosses the midship section, supposing that we have $v=0$, $w=0$, the following equation must be fulfilled:

$$u\,\frac{d^2u}{dx^2}+2\,\frac{du^2}{dy^2}+2\,\frac{du^2}{dz^2}=0: \quad . \quad . \quad \text{(VIII.)}$$

The corresponding equation in two dimensions is formed by omitting the term in $\frac{du}{dz}$.

Part III.—*On some Stream-Lines of Revolution.*

20. The third part of the investigation relates to the stream-lines in which particles flow past certain totally immersed oval solids of revolution, bearing the same relation to a sphere that the oval neoïds described in the previous paper bear to a circle. These lie upon a series of surfaces of revolution, and are the sections of those surfaces by planes passing through the axis.

21. Let the axis of figure be that of x, and let there be two points in it, called *foci*, situated at the distances $+a$ and $-a$ from the origin. The distance a may be called the *excentricity*. Let the perpendicular distance of any particle from the axis be denoted by y; let f be a constant length, called the *parameter*; and let b be the radius of a cylinder which is an asymptote to a given stream-line surface. Then the equation of that surface is as follows:

$$b^2=y^2-f^2\left\{\frac{x+a}{\sqrt{\{(x+a)^2+y^2\}}}-\frac{x-a}{\sqrt{\{(x-a)^2+y^2\}}}\right\}. \quad \text{(IX.)}$$

Or, in another form, let θ and θ' be the angles which two lines drawn from the given particle to the foci $(+a)$ and $(-a)$ respectively make with the axis of $+x$; then

$$b^2=y^2-f^2(\cos\theta'-\cos\theta). \quad . \quad . \quad . \quad . \quad \text{(X.)}$$

22. For the primitive oval solid, $b=0$; and by giving b^2 a series of values increasing in arithmetical progression, a series of stream-line surfaces are formed, of gradually increasing width, which divide the liquid mass into a series of concentric tubular streams of equal discharge.

23. The graphic construction of the stream-lines is as follows. From each of the foci draw a set of diverging straight lines, making angles with the axis whose cosines are in arithmetical progression, the common difference being a sufficiently small fraction. Through the network formed by these lines trace diagonally a series of curves traversing the two foci. (The equation of each of those curves is $\cos\theta'-\cos\theta=m$, and they are identical with the lines of force of a magnet having its poles at the foci.) Multiply the parameter (f) by the square roots of the terms of the arithmetical progression, and draw a series of straight lines parallel to the axis and at the distances from it so found; these will be the asymptotes expressed by the equation $b=f\sqrt{m}$. Then through the network formed by those parallel straight lines and the before-mentioned series of curves trace

diagonally a new series of curves, which will be the stream-lines required.

24. The stream-lines thus drawn closely resemble those in two dimensions, but are somewhat fuller. For those at a distance from the axis, the difference of form is scarcely perceptible; for those near the axis, and especially for the primitive oval, the greater fulness of form is conspicuous.

25. The ratios of the component velocities of a particle on a stream-line surface of revolution to the velocity of a particle at an infinite distance from the disturbing solid are given by the expressions

$$u=\frac{bdb}{ydy}; \quad v=-\frac{bdb}{ydx}. \quad . \quad . \quad . \quad . \quad \text{(XI.)}$$

26. By applying to those stream-lines of revolution the principles of the second part of the investigation, it is found that the radius (b) of the asymptotic cylinder of a lissoneoïd surface of revolution bears the following relation to the greatest radius (y_0) of the surface itself, and to the excentricity (a) of the set of surfaces to which it belongs:

$$b^2=y_0^{\,2}-2(a^2+y_0^{\,2})\cdot\frac{3y_0^{\,2}-2a^2}{3y_0^{\,2}+2a^2}. \quad . \quad . \quad \text{(XII.)}$$

In order that this equation may be real, $\frac{y_0}{a}$ must not be less than $\sqrt{\frac{2}{3}}$, nor greater than $\left(\frac{4}{3}\right)^{\frac{1}{4}}$. The corresponding parameter is found by the general formula

$$f^2=\frac{(y_0^{\,2}-b^2)\sqrt{a^2+y_0^{\,2}}}{2a}. \quad . \quad . \quad . \quad . \quad \text{(XIII.)}$$

In the oval lissoneoïd of revolution, $b^2=0$, $\frac{y_0^{\,2}}{a^2}=\frac{2}{\sqrt{3}}=1\cdot1547$; and $\frac{f^2}{a^2}=\frac{\sqrt{\sqrt{3}+2}}{3}=0\cdot644$.

September 1864.

XXXIV. *On the History of Thermo-dynamics.* *By* P. G. Tait, *M.A., &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I WISH to make a few additional remarks on this subject: especially as it appears to me, after a careful perusal of all that Dr. Tyndall has written upon it, that he does not yet quite understand the points which Prof. Thomson and I wished to establish when, in consequence of his lecture on "Force," we wrote our article in 'Good Words' nearly two years ago.

We have all along held that the questions as to the nature of heat, and its equivalence to mechanical energy, were settled by Davy; and that Rumford experimentally obtained a very fair approximation to the equivalent. Also that, as Newton had completely enunciated the Conservation of Energy in ordinary mechanics, Davy's experiments brought heat under the same law, and that, therefore, in the beginning of the present century the Dynamical Theory of Heat was completely *established,* although not *developed.* The development has been since furnished by the experiments and reasonings of Joule on Electric Thermo-dynamics*, his experimental determinations of the mechanical equivalent, his experiments and reasoning on the thermal effects of the condensation and rarefaction of air†; and by the theoretical writings of Helmholtz, Clausius, Rankine, and Thomson.

Hence, so far as regards the question of heat alone, Mayer has no title to the position Dr. Tyndall claims for him. He did no more than repeat what Davy and Rumford had done better; and he has never, so far as even Dr. Tyndall's partisanship can show, attempted anything of the nature of either the theoretical or experimental developments which have advanced thermo-dynamic science during the present century. What, in point of fact, did Mayer do in thermo-dynamics? In 1842 he published a paper, of which Prof. Thomson and I remarked as follows. "In this paper the results obtained by preceding naturalists are stated with precision—among them the fundamental one of Davy—new experiments are suggested, and a method for finding the dynamical equivalent of heat is pro-

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* Of these the following are perhaps the most important:—

"On the Production of Heat by Voltaic Electricity" (Proc. Roy. Soc. Dec. 17, 1840). Printed in Phil. Mag. 1841, with the title "On the Heat evolved by Metallic Conductors of Electricity, and in the Cells of a Battery during Electrolysis."

"On the Electric Origin of the Heat of Combustion" (Phil. Mag. 1841). Extension of the same (Report of British Association, 1842).

"On the Heat evolved during the Electrolysis of Water" (Lit. and Phil. Soc. of Manchester, Jan. 1843).

"On the Calorific Effects of Magneto-Electricity, and the Mechanical Value of Heat" (Phil. Mag. 1843).

"On the Heat disengaged in Chemical Combinations," sent to the French Academy in 1846 (Phil. Mag. 1852).

"On the Mechanical powers of Electro-magnetism, Steam, and Horses," by Scoresby and Joule (Phil. Mag. 1846).

"On the Economical Production of Mechanical Effect from Chemical Forces" (Manchester Memoirs, 1852, and Phil. Mag. 1853).

† "On the Changes of Temperature produced by Rarefaction and Condensation of Air," sent to the Royal Society, June 1844 (printed in Phil. Mag. 1845).

pounded." To this we added, in a note, as follows. "Mayer's method is founded on the supposition that diminution of the volume of a body implies an evolution or generation of heat; and it involves essentially a false analogy between the natural fall of a body to the earth, and the condensation produced in an elastic fluid by the application of external force. The hypothesis on which he thus grounds a definite numerical estimate of the relation between the agencies here involved, is that the heat evolved when an elastic fluid is compressed and kept cool, is simply the dynamical equivalent of the work employed in compressing it. The experimental investigations of subsequent naturalists have shown that this hypothesis is altogether false, for the generality of fluids, especially liquids, and is at best only *approximately* true for air; whereas Mayer's statements imply its indiscriminate application to all bodies in nature, whether gaseous, liquid, or solid, and show no reason for choosing air for the application of the supposed principle to calculation, but that at the time he wrote air was the only body for which the requisite numerical data were known with any approximation to accuracy." To every word of this, with the exception of the word "imply," which is not strong enough, I still adhere. Dr. Tyndall's mode of dealing with it is characteristic. He says—"not what Mayer's words 'imply,' but what they are"—and then quotes, *not* from the paper of 1842, to which alone we referred (as the only one which could have a chance of priority over either Joule or Colding), but from a pamphlet published in 1845.

As to the question which might have arisen between Séguin and Mayer, supposing nothing to have been done in the matter by Davy and Rumford; everything that was done by Mayer in 1842 (I still confine myself to the question of Heat alone) was done by Séguin in 1839. Dr. Tyndall is correct in his remark that Séguin did not, as was originally supposed by Joule, give 363 kilogrammetres as the dynamical equivalent. But to say, as Dr. Tyndall does, that "*there is no determination whatever of the mechanical equivalent of heat in the above* [*i. e.* Séguin's] *Table,*" is simply an error, for Séguin gives all the requisite data, though the thermal unit he employs is by no means convenient. Nothing can indeed be more distinct than his evaluation. An hypothesis explicitly stated by him as to the heat of condensation of vapour, now known to be wrong and to give much less than the true thermal effect, rendered the numbers in his Table largely in error.

In Séguin's work we find the following passages:—

"Ceci reviendrait à dire que la vapeur n'est que l'intermédiaire du calorique pour produire la force, et qu'il doit exister entre le mouvement et le calorique un rapport direct, indépendant de l'intermédiaire de la vapeur ou de tout autre agent que l'on pourrait y substituer."

Compare this with Dr. Tyndall's quotation (Phil. Mag. Sept. 1862). "The law," says Mayer, "'Heat = Mechanical Effect' is independent of the nature of an elastic fluid, which only serves as the apparatus, by means of which the one force is converted into the other."

Again, Séguin says, "La force mécanique qui apparaît pendant l'abaissement de température d'un gaz comme de tout autre corps qui se dilate, est la mesure et la représentation de cette diminution de chaleur;" and further, in speaking of steam escaping into the air, "L'effort qu'elle exerce en recul contre les appareils qui la laissent échapper, ou la vitesse qu'elle communique à l'air ambiant, forme un équivalent de la perte de chaleur qu'elle éprouve." Yet, according to Dr. Tyndall, it was Mayer "who first used the term 'equivalent' in the precise sense in which you" (Joule, to whom the letter is addressed) "have applied it."

But there is more than this in Séguin's very able work. He points out distinctly that steam which has done work in an engine ought not to heat the water in the condenser so much as if it had been led directly into it. He had made, he says, numerous experiments to test this, without however obtaining sufficiently decisive results. Again, he points out how very small a proportion of the heat of the steam is really employed in doing work. He says that the work obtained from a steam-engine, as ordinarily used, is represented by "un abaissement de température d'environ 20°, *qui équivaut au trentième environ du calorique employé pour réduire en vapeur l'eau nécessaire à sa formation.*" That is, only $\frac{1}{30}$th of the heat used, disappears as heat, and is given out as work. Thus we see that his 20° represent $\frac{1}{30}$th of the latent heat of steam, at 100° C. and at the atmospheric pressure: and his Table gives for the corresponding work done by a cubic metre of steam (in round numbers) about 7000 kilogrammetres. Taking 540 as the latent heat, and 0·6 kilogramme as the weight of a cubic metre, of steam, we have for the mechanical equivalent $\frac{30 \times 7000}{0{\cdot}6 \times 540} = 650$, about 50 per cent. too great.

As to the Conservation of Energy. Dr. Tyndall, in a note to his last paper, ascribes the term, or terms, to Rankine. I cannot ascertain precisely when the term "Conservation" was introduced, but it must have been suggested at once to an English writer by the old term "Conservation of Vis Viva," of which the Conservation of Energy is only an extension. At all events Helmholtz's "Erhaltung" is an identical word, and was employed before Rankine wrote on the subject. But the term Energy (as Dr. Tyndall surely is aware) is due to Young, who introduced it as a convenient English term synonymous with *vis viva.* Its extension to the two forms "static" and "dynamic" was made by Thomson. Rankine improved these to "potential" and "actual;" and in 'Good Words' Thomson and I have employed "kinetic" as less ambiguous and more suggestive than "actual."

As to the discovery of the Conservation of Energy, I hold that to lay down, without experimental bases, such a maxim as "*causa æquat effectum*" is entirely subversive of common sense and logic in an experimental science such as natural philosophy. The establishment of the Conservation of Energy was utterly out of the sphere of the "Thinker;" and it would be absurd to give him more credit than is due to the promulgator of a clever speculation. Thousands of equally clever, but less lucky, though not more baseless, speculations are every day mercilessly exterminated by experiment.

Celestial Dynamics forms no part of the Thermo-dynamic theory, though it affords exceedingly beautiful applications of it. The same must be said of Animal and Vegetable Physiology. Such applications, as is well illustrated by the famous little sentence of Joule's postscript of 1843, always attend careful work at a theory; they are not *discoveries,* but inevitable *consequences,* to the experimental or mathematical investigator. The word or two, required to complete the suggestions of Stephenson and Herschell, occurred to many minds, merely to be recorded in passing—as by Helmholtz in a popular lecture, and by Thomson in the proceedings of a society.

I have written again to the Philosophical Magazine because I imagine that Dr. Tyndall still misunderstands the views which Prof. Thomson and I maintain on the history of the subject; and that it is this which has led him to charge us with misrepresentations. His special charges against Prof. Thomson, which receive fresh development in every successive article, are so obviously unfounded that he can hardly be surprised that Prof. Thomson has not judged it necessary even to notice them.

I am, Gentlemen,
&c. &c.,
P. Guthrie Tait.

6 Greenhill Gardens, Edinburgh,
September 12, 1864.

XXXV. *On the Erosion of Valleys and Lakes; a Reply to* Sir Roderick Murchison's *Anniversary Address to the Geographical Society. By* A. C. Ramsay, *F.R.S.*[*]

AFTER the publication of my memoir "On the Glacial Origin of certain Lakes in the Ice-worn regions of Europe and North America," several eminent British and Continental geologists, and some other persons who have only a general literary acquaintance with physical geology, did me the honour, in special memoirs, or in letters in newspapers, to express opinions that my views were deserving of the strongest opposition. To none of these opponents have I heretofore made any reply, and some of them, I found, were dealt with by men who met their arguments more ably perhaps than I could have done myself. Besides, I considered that if my theory, as I believe, be true, it would be sure in the long run to make its way just in the slow and steady manner it seems to me to be now doing. We all profess to appeal to nature, and "in nature there is no opinion; there is truth in everything that is in nature; and in man alone is error." To those who are not geologists in any practical sense it would never occur to me to reply. Physical geology, in the true meaning of the term, does not exist without a thorough practical acquaintance with, and experience of, rocks of all kinds on a large scale. The man who merely wanders about a country and looks curiously at rocks, without a long course of severe training, has no more scientific right to form a definite opinion as to the causes that brought about the external configuration of the land than the father of a family would have to decide questions in comparative anatomy, because for half his life he had daily carved beef, mutton, pork, fowls, and fish.

Of late, however, an exceedingly authoritative protest against my theory has been entered by Sir Roderick Murchison, in his Anniversary Address to the Geographical Society,—an address issued indeed to the geologists of Europe; for the portion that bears upon icy phenomena has been printed separately for special distribution. It would almost be uncourteous on my part silently to pass over the remarks of one who in his own person has attained the highest honours in the Geological and the Geographical Societies, and who is besides my oldest living geological friend. "As a geologist, with wide experience, the President of the geographers clearly states his conviction"[†] that my theory of the origin of certain lakes and other theories of denudation connected therewith, are so opposed to obvious facts, that, if his conviction be well founded, the wonder seems to me that

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[*] Communicated by the Author.

[†] Geological Magazine, No. 3, p. 127.

any man of weight and knowledge could be found to follow me at all. I may therefore be pardoned if in this instance I depart from the course of leaving the value of my theory to be worked out solely by time.

I have said that Sir Roderick has entered an *authoritative protest*, because, as several persons have remarked to me, so much stress has been laid on the *argumentum ad hominem*, liberally as regards Continental geologists, and more sparingly with American and English names. Indeed, in reading the Address, I was more than once reminded of the observation of one of my opponents, who in the 'Reader' observed to this effect, "that Professor Desor entirely disagrees with Professor Ramsay—how can he do otherwise? for Desor has lived among glaciers all his life." In like manner Studer and Escher von der Linth, "by numerous appeals to nature," Gastaldi, De Mortillet, and many more are all arrayed in opposition to the theory, the presumption being that the chances are therefore infinitely against it, and I must needs be wrong because they are so eminent, and some of them have lived so long among the Alps. For, differing from them, how is it likely that a man can be right who has only explored the Alps five or six times with a special object, even though he may have spent five-and-twenty years on subjects allied to or identical with it? Such is the general impression produced, not on myself alone, by many of Sir Roderick's remarks. I have no objection to this kind of argument; it is so old in the history of science that its value is understood. To compare great things with our small matter, Copernicus and Galileo experienced it, Hutton and Playfair knew it well; the most eminent geologists were for long deaf to the voice of William Smith, let him charm ever so wisely; and Agassiz himself, in glacial geology, had among his chief opponents distinguished seniors, some of whom even now only hesitatingly follow him. It is easy to "appeal to nature," but the language of her reply is not always to be understood merely by long poring on her face; and it generally happens that many an abortive effort is made before some happy accident reveals the key.

In my original memoir, when discussing the origin of the lake-basins, I found it necessary in some degree to treat of disturbances of rocks in general. Accordingly, Sir Roderick very properly regards the question as one not merely of lakes, but as involving his belief "with the vast majority of practical geologists, that the irregularities of the surface of the Alps have been primarily caused by dislocations and denudations;" and again, that "until lately geologists seemed so be generally agreed that most of the numerous deep openings and depressions which exist in all lofty mountains *were primarily due to cracks which took place during* *the various movements which each chain has undergone at various periods,"* &c. The meaning of this, I conceive to be, that mountain valleys lie in lines of curvature, dislocation, and fracture, and that the mountains on each side of them are mountains, far less because of denudation than by reason of operations of fracture and dislocation. Therefore important lakes that fill true rock-basins lie only in lines of fracture, or else, as in the myriad lakes of North America, in hollows of wider dislocation somewhat aided by subsequent denudations.

Every reasoning mind respects authority when it bears on questions that have been reduced to demonstration; but this is precisely what has not been done with respect to the origin of *special* Alpine lakes and valleys by those whose main argument is disturbance of strata. Assertions and crude ideas in all kinds of books and papers are "as plenty as blackberries;" but for clear demonstrations—none are given. Nor does Sir Roderick either attempt or point to any when he says that in the Alps he "long ago came to the conclusion that the chief cavities, vertical precipices, and subtending deep, narrow gorges, have been *originally* determined by movements and openings of the crust, whether arranged in anticlinal or synclinal lines, or not less frequently modified by great transversal or lateral breaks, at right angles to the longitudinal or main folds of elevation or depression." Now in my paper I gave six stratigraphical reasons to show why the lakes do not lie in hollows of disturbance, and then pointed to ice as the only remaining agent by which they could be formed, thus attempting to reduce the matter as nearly as I could to a demonstration; and what I want is an attempt at demonstration in return. But where is the proof beyond the general assertion and impression that craggy-sided mountains and valleys prove dislocations which gape. If they were mere close or nearly close fractures and denudation did the rest, the argument is equally in favour of my view; for valleys which have been scooped out by denudation often necessarily coincide with lines of fracture, a proposition obvious to every geologist. But I want the proof that the Alpine valleys are dislocations. Let any one go into them and prove it in numerous cases, with his geological map in his hand, by the arrangement of the rocks on either side, and by the fracture or fault visible, or otherwise certainly demonstrable in the bottom. Where are these valley faults, whose name ought to be legion, marked in the best geological maps of Switzerland? If they exist, they remain yet to be indicated in definite lines; for indeed none know better than the many eminent geologists of Switzerland and the north of Italy, for whom and for whose work I have the highest respect, that the geological map of their country is as yet but an admirable sketch, and in all probability will remain so till their governments authorize more general and uniform painstaking surveys. When this is done, and when all the faults and curvatures possible *are actually laid down, and when geological sections on a true scale* have been run across the Alps, it will then be possible to reason with precision on the denudation of the mountains; and it will be found (what is well known now) that before the present surface of the valleys saw the light, vast piles of strata, as in Wales, have been removed by denudation, and the valleys were formed long after the latest important disturbances of the strata took place.

And now to prove that I also respect authority, let me quote from books of immortal repute; and surely those who reverence authority most, will not disdain that of Hutton and Playfair. What say the father of physical geology and his great disciple? "If," says Hutton, reasoning on this subject, "the valley was made for the rain by any other natural cause, either we should tell by what means this work had been performed, or all reasoning on the subject is at end, *and fancy substituted in its place.* If, again, the river be considered as the means employed by nature in making this valley, then all the solid parts between the bounding mountains must have been removed." Again, reasoning on the weathering and erosion that *originated* the pyramids on and around Mont Blanc, he observes, "It is true, indeed, that geologists have everywhere imagined to themselves great events, or powerful causes, by which these changes in the earth should be brought about in a short space of time; but they are under a double deception; *first,* with regard to time, which is unlimited*, whereas they want to explain appearances by a cause acting in a limited time; *secondly,* with regard to operation, their supposition of a great *débâcle* is altogether incompetent for the end required." Again, arguing on the approximately horizontal gneissic strata of the neighbourhood of Monte Rosa, he shows that the great isolated peaks have been separated by "the greatest degradation, in being wasted by the hand of time. . . . Here," he says, "is nothing but a truth that may almost everywhere be perceived" if we had only faculties to perceive it.

Again, reasoning on strata that correspond on opposite sides of valleys, Playfair, in the Huttonian Illustrations, says, "there is no man, however little addicted to geological speculations, who does not immediately acknowledge that the mountain was once continued quite across the place in which the river now flows; and, if he ventures to reason concerning the cause of so wonderful a change, he ascribes it [in the modern fashion] to some great convulsion of nature, which has torn the mountain asunder

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* In the original, "limited.". This is an evident misprint.

and opened a passage for the waters. It is only the philosopher, who has deeply meditated on the effects which action long continued is able to produce, and on the simplicity of the means which nature employs in all her operations, who sees in this nothing but the gradual working of a stream, that once flowed over the top of the ridge which it now so deeply intersects, and has cut its course through the rock, in the same way, and almost with the same instrument, by which the lapidary divides a block of marble or granite." And in the Alps (p. 122) he shows that "the sharp peaks of the granite mountains . . . . but mark so many epochs in the progress of decay," while the loftiness of the harder peaks is due not to mere upheaval but to the circumstance "that the waste and *detritus* to which all things are subject will not allow soft and weak substances to remain long in an exposed and elevated situation." "Thus, with Dr. Hutton (p. 126), we shall be disposed to consider those great chains of mountains, which traverse the surface of the globe, as cut out of masses vastly greater, and more lofty than anything that now remains." I could multiply sentences of this kind from the writings of these great philosophers; but enough has been said to recall to memory the fact that before the present race of "practical geologists" had written a line, men of rare knowledge, keen sagacity, and the highest intellectual powers, by appeals to nature already held those views which some of their degenerate descendants have so readily repudiated, but to which a younger school show strong symptoms of returning. I doubt also if some of the Swiss and Italian geologists will be quite content to stand godfathers to the opinion that the Alpine valleys generally are apt to lie in lines of mere curvature or fracture, whether close or gaping; but without further authority than that of personal conversation it would be improper to quote their names.

Unless I were to write a special elementary treatise on denudation, enough has now been said to show that the theory of formation of great systems of valleys by erosion in which water and ice are main agents, is not a mere absurdity, and I do not therefore care minutely to analyze the assertions that many of the Alpine rivers "flow in fissures or deep chasms, . . . which water alone never could have opened out;" or again, that the Rhine and the Danube "never could have eroded those deep abrupt gorges through which they here and there flow, and which are manifestly due to original ruptures of the rocks." To the neglected and even half-forgotten school of Hutton and Playfair, and to many expert geologists of the present day whose lives have been spent in practically analyzing the rocky structures of countries, the *manifest* nature of such "original ruptures" is anything but evident; and I for one believe that the "ruptures" are only manifest to those who accept such hypotheses "without inquiring into what has been the former state of things, or what will be the future"*. To this day there is no error so common, even among geologists, as that which vaguely attributes the form and nature of the present surface-outlines of the earth chiefly to the operation of violent disturbance in recent geological times, not clearly perceiving that the great and small outlines of mountain-chains, of valleys, of river-gorges and of plains are the combined results of an immense number of operations, many of these going back to exceedingly remote periods of geological antiquity, and a great proportion of their details being lost even to probable conjecture.

These operations, however, in the production of scenery mainly resolve themselves into the following series, the parts of which, ever since land and water first existed, may be arranged in any possible combination.

a. *Oscillation with respect to the sea-level of rocks that have or have not been contorted and metamorphosed, accompanied by pauses in oscillation of greater or less duration.*

b. *Great plains of marine denudation.*

c. *Subaërial denudations of all kinds; wearing away of sea-coasts; and in the interior of the country, chemical decompositions, frost, snow, ice, wind, rain, and rivers; modified by height of land, and the various positions, hardness, and other characters of rocks.*

Contortion and metamorphism seem to be essential accompaniments of all great mountain-chains. It may also possibly be proved that in intensely contorted regions mountain-chains are high or low according to the relative antiquity of disturbance, while sometimes the irregular protuberances, as in the Devonian and other rocks of the Rhine and Moselle, have been planed away altogether.

Plains of marine denudation are sure to be inclined at a very low angle if formed during slow depression of the land.

Further, while the sea helps to make bays, the other agents of waste enumerated above cut out all mountain-peaks not volcanic, all the *minor valleys*, in this term including such valleys as those of the Alps, the Highlands, Wales, &c., but not such a valley as the great one that lies between the Alps and the Jura.

Fractures and volcanos, in the production of the great scenic features of continental physical geography, are, as a rule, mere subordinate and subsidiary accidents, the first modifying the effects of denudation by juxtaposition of different kinds of rocks, and the second (which seem to be connected with general elevations) forming accidental mountains, hills, and hilly regions,

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[*] Hutton, vol. ii. p. 257.

which, as in the Andes, may form non-essential parts of mountain-chains.

I shall now make some remarks on what has been said in the Address respecting the action of ice in general, and its share in forming lakes that are true rock-basins in particular, taking these in connexion with other points at issue.

"Before entering on the consideration of the new theory of the power of moving ice," Sir Roderick gives a brief review of the recent progress of Alpine glacial geology, meaning by recent principally those twenty-five or thirty years that have elapsed since Agassiz began to insist not only on the enormous size of the old glaciers of the Alps, but on what is now generally recognized as the true glacial theory. "Granting to the land glacialists their full demand" for the great size of the old glacier of the Rhone, it is stated by Sir Roderick, backed by the authority of Sir Charles Lyell, that there is *nothing* in that fact "which supports the opinion that the deep cavity in which the lake [of Geneva] lies was excavated by ice;" for among other things it is "to be noticed in the case of the Lake of Geneva" that it "trends from E. to W., whilst the detritus and blocks sent forth by the old glacier of the Rhone have all proceeded to the N. and N.N.W., or in direct continuation of the line of march of the glacier which issued from the narrow gorge of the Rhone. By what momentum, then, was the glacier to be so deflected to the west that it could channel or scoop out, on flat ground, the great hollow now occupied by the Lake of Geneva? And, after effecting this wonderful operation, how was it to be propelled upwards from this cavity on the ascent, to great heights on the slopes of the Jura mountains?" The same argument it is stated holds good of the Rhine glacier, which I have attempted to show scooped out the shallow hollow of the Lake of Constance. One would suppose these questions to be so conclusive, that the mere asking is enough, and any opposite views must be absurdities which no man of any sound knowledge could entertain; and yet men are found who do entertain them in part or in whole, even authors of great authority on geological and physical subjects, not only in the three kingdoms, but on the continents of Europe and America. Now with regard to the great old glacier of the Rhine, the sentence bearing on it is so worded that I am unable to make out whether it is implied that in the belief of Sir Roderick Murchison no great glacier issuing from the Upper Rhine valley ever overspread the region around the Lake of Constance, or whether he and M. Escher von der Linth simply at one time could not find signs of a glacier that so "plunged into the flat region on the east and north" (of the Hohe Sentis) "as to have scooped out the cavity in which the lake lies." If the former, then Sir

Roderick's opinion seems to have been formed a long time ago; for, adopting M. Escher's authority, anyone who consults his map of the ancient extension of the Alpine glaciers, will see that he draws an enormous glacier, which issuing from the broad flat valley of the Rhine, tranquilly overspread the country on all sides of the lake, and without the necessity for any plunge, could only have been fed by smaller tributary streams of ice that, if such existed, descended on the northern slopes of the Hohe Sentis*.

In like manner, Sir Roderick is of opinion that the basin of the Lake of Geneva was not scooped out by ice, because "it trends from east to west," or at right angles to the main flow of the glacier—because ice, *per se,* neither has nor has had any excavating power"—because (p. 12) "in valleys with a very slight descent, ... no erosion whatever takes place, particularly as the bottom of the glacier is usually separated from the subjacent rock or vegetable soil by water arising from the melting of the ice," and because even in gorges "whence the largest glaciers have advanced for ages, we meet with islands of solid rock and little bosses still standing out, even in the midst of the valleys down which the icy stream has swept," and "there is no proof of wide erosion"—and, yet again, because (p. 15) "ice has so much plasticity that it has always moulded itself upon the inequalities of the hard rocks over which it passed," and "has never excavated the lateral valleys, nor even cleared out their old alluvia," and furthermore, in general terms, because ice could not have been propelled up an inclination from the bottom of a lake, let the angle, I presume, be ever so small.

Now the east and west course of the lake is here treated as if the glacier of the Rhone which overspread it were the only glacier which helped to cover the area; but if any one will take the trouble to refer to the map which accompanies my memoir, or, better still, to M. Escher's, he will see that the mass of ice must have been prodigiously swelled by the great tributary glacier of Chamouni, which, descending from Mont Blanc, filled a valley some fifty miles in length, and joined the Rhone glacier near the lower end of the Lake of Geneva. Neither does it require much reasoning to see that during the cold of the glacial epoch all the higher region south of the lake must have maintained its glaciers and filled the valleys that run north; for even now some of the

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* I have to apologize to my friend M. Escher von der Linth for not having used his map of the ancient glaciers as my chief authority when my Memoir on the Lakes was read. The first time I saw his map, which was sent me by Principal Forbes of St. Andrews, was after the publication of my memoir. Had I seen it in time, I would certainly have availed myself, in the construction of my sketch map, of the authority of a geologist so accurate and distinguished as Escher von der Linth.

peaks are tipped with perpetual snow. The Rhone glacier had therefore no lack of tributaries to maintain its mass over all the area of the Lake of Geneva, though towards the west, where the glacier thinned away, that mass would be less than over the eastern half of the lake, where weight and grinding-power must, I believe, on that account have necessarily been greater. But the main flow of the ice, after escaping from the Rhone valley, was necessarily of a mixed nature, partly to the N.W., and also to a great extent to the N.E. and S.W., simply because the N.W. face of the glacier abutted on the Jura. For it requires no profound knowledge of physics to perceive that any body, whether actually plastic like pitch, or of a modified plasticity that may be fractured and reunite like *jelly**, or that by "fracture and regelation" behaves like a plastic body,—I say it requires no profound knowledge of physics to understand that such a body, constantly renewed and pressed on from behind, when opposed by a high impassable barrier (like the Jura), will spread itself out in the direction of least resistance, that direction in the case of the Rhone glacier having been at right angles to the general pressure, or N.E. and S.W., whence I believe the general form and trend of the Lake of Neuchatel.

But, in the second place, is there indeed no proof that ice "neither has nor has had any excavating power," whether in valleys of large or of low inclination, narrow or broad? Then why is it that all the rivers that flow from glaciers, great and small, are so muddy? Surely no one will contend that all "the flour of rocks" that gives to the rivers a pipeclay colour has been washed in by streams from the surface. Alpine club men who drink (rarely) of the brooks that run on the surface of the ice will repudiate the idea; those who fancy they see in the Loess of the Rhine the old glacier-ground mud of the Alps will shrink from it; and many, if not all the Alpine geologists versed in ice whom I have conversed with in Italy and Switzerland, will, I venture to say, still hold that glaciers by erosion seriously affect their beds. What else is the meaning of the striation and deep grooving, the mammillation and the glassy polish, even of quartz, and of all the Alpine rocks, whether hard or soft? The mud of the rivers is chiefly derived from this incessant ice-waste; and that is why it is so *unearthy*, so clean, fresh, and impalpable. Were it merely or chiefly surface-wash, derived from the hills and washed underneath and carried forward below the glaciers, the sediment in great part would be dirty, torrential, and coarse enough, especially if, as is stated, glaciers do not seriously grind along their rocky floors. So far from a glacier exercising only a trifling grinding-power, "because it is usually separated from the subjacent

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* I have obtained this comparison from the Master of the Mint.

rock or vegetable soil by water arising from the melting of the ice," the grinding power is so immense, that in unweathered ground comparatively recently covered by a glacier, every foot of surface is often polished and striated. If, indeed, water usually separates ice from the rock so that it does not press upon it, a glacier, whether 30 or 3000 feet thick, would need to be treated in the main as a floating body; and it is well known that with floating ice there is some eight or ten times as much ice below as above the water.

As for bosses "still standing out in the midst of the valleys" proving that glaciers have no erosive power, the reader unlearned in theories of denudation will easily understand that the same kind of argument might be applied to the pillars of earth left for a time in the midst of a railway-cutting the actual excavation of which he had not seen; or because Goat Island still stands in the middle of the falls, the Niagara has not cut its gorge; or because other low islands lie higher up, the river has not worn out a channel on either side of them and will not destroy them; or in marine denudation, that the chalk between Old Harry and his Wife and the mainland of Swanage Bay, and that between the Needles and the Isle of Wight, has not been washed away by the sea, because the islets still stand in the midst. If, however, it be said that the glacier-islets are the result of old subaërial denudations before the glacier began to flow, I might perhaps doubt it, but, for evident reasons, for the purpose of this argument, I will not quarrel with it. If they have not been left prominent either by streams or ice, then, according to the hypothesis which accounts for these valleys by disturbance, the bosses in the midst of the glaciers are the result of a process of dislocation of which I should like to see the special proof.

The peculiarity and in part the amount of this wearing action of ice is indeed due to that very "plasticity" which enables ice to mould "itself upon the inequalities of the hard rock." And it is just therein that its excavating power differs from that of water. Still water cannot excavate a large basin-shaped hollow, and in the depths of a lake water is still; but glacier-ice, having "moulded itself upon the inequalities of the hard rocks over which it passed," can even move right over a barrier of rock and grind it into *roches moutonnées*. The very fact that a *roche moutonnée* has, as stated by Sir Roderick, a "*Stoss-Seite*," is indeed proof that with sufficient pressure behind, a glacier can to some extent pass uphill; and those who remember the great size and height of many of these barriers in Switzerland, as, for instance, the Kirchet and the hill behind the Grimsel, will be prepared to follow the arguments urged in my original paper—and, for different reasons, also held by De Mortillet—viz. that a glacier of sufficient thickness could not only fill a lake, but could flow up the low angle of the ascent towards the outflow and escape beyond its bounds*.

If a glacier can round, polish, and cover with striations the rocks over which it passes—if, flowing from its caverns, it can charge rivers thickly with the finest mud, then it can wear away its rocky floor and sides. Here indeed an appeal to nature may safely be made, and the answer will be easily obtained; for, standing on the surface of scores of glaciers, such as those of the Aar, and casting the eye upward, the whole mountain-sides are *moutonnés*, and parallel striations running along and down the valley are universal; and not there alone, but miles and miles below the end of the puny glaciers of today the signs of the same wearing actions of grander ice-streams are visible both in and thousands of feet above the present bottoms of the valleys. It needs no subtle argument to prove it. Nature proclaims it; we have but to open our eyes and look upon it to see that ice grinds, and has ground and planed away the surface of rocks, as surely as a planing machine cuts iron, and for much the same cause. "What more," says Hutton, writing of analogous waste, "what more is required? *Nothing but time.* It is not any part of the process that will be disputed†; but after allowing all the parts the whole will be denied; and for what? only because we are not disposed to allow that quantity of time which the ablution of so much wasted mountain might require." "TIME," says Playfair, "performs the office of *integrating* the infinitesimal parts of which this progression is made up;" and though I have in this Magazine formerly attempted to show, for purely geological reasons, that the greater valleys in the Alps existed before the so-called glacial period, yet I know perfectly well, not only that since that time glaciers have worn a vast quantity of matter out of them, but that, given sufficient time, a glacier of itself might scoop out a valley of any depth, just as running water may do the same, or as surely as that, given sufficient time, the sea will wear away any island, soft or hard, large or small, that rises amidst its waves.

In further proof of the assertion that glacier-ice can have no serious effect in wearing away its bottom, great stress is laid on the well-known fact that such short and steep glaciers as those

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* Unless I am much mistaken, geologists will some day be much surprised at the size and kind of hills that they will be obliged to allow that glaciers have travelled *over*.

† Things, however, that he considered almost self-evident are now disputed every day. The tendency of opinion begins to set in the opposite direction.

of the Brenva and Miage ride over their moraines. I know these glaciers well, and the statement that they do ride on their moraines is perfectly true; but few geologists, and probably no physical philosopher will rest his reputation on the assertion that, if those glaciers were to increase till they attained their ancient size, when as mere tributary sources they helped to swell the enormous mass that ploughed all down the Val d'Aosta to beyond Ivrea,—will anyone, I say, rest his reputation on the belief that these moraine heaps would lie where they now do, underneath a thousand or thousands of feet of ice, unmoved to all eternity, or at least till the complete decline of the glaciers permitted the loose material to be attacked by running water? If so, again, whence the muddy glacier rivers, and whence the scratched stones that come *from under* the glaciers? Tyndall will not believe in their immobility, nor De Mortillet, nor Gastaldi, nor Darwin, who was the first to show that the larger glaciers of Wales had ploughed the drift out of some of the greater valleys of the country; and many other geologists of weight will equally shrink from the idea. Has ice no weight? Do the huge glaciers of Victoria-land and of Greenland exert no pressure on the ground over which they flow? and are there no stones and no powder of rocks beneath to help the grinding-power? Rub iron with your finger often and long enough, and it will wear a channel in the metal; for the skin, like the passing glacier, will be renewed, while the iron has no means of restoration. If yielding water can wear out a channel, which few people will deny, far more, then, must the weight of a thick glacier exercise a prodigious abrading-power; for surely no one on reflection will be so bold as to assert that 50 feet, or one, two, or three thousand vertical feet of ice with a specific gravity of nearly 0·92 will everywhere, or nearly everywhere, be separated from its floor by a stratum of water so complete that the glacier rarely touches the bottom. If Agassiz, Forbes, and Tyndall, backed by Studer, Escher, and Gastaldi, were to tell me so (and they would not dream of it), my reverence for authority (and it is great) could not persuade me to believe them.

If, then, glaciers can waste rocks and deepen valleys, is it possible that the great old glaciers under favourable circumstances have excavated lake-basins, when rocks of unequal hardness came in their course, or when from special causes the pressure of ice was unusually great on certain areas? Or were they apt to do so by a combination of these causes, when, ceasing to flow through valleys of great or of moderate inclination, they descended into regions that are comparatively level?

I will not repeat what I have elsewhere printed about the effect of ice passing over rocks of unequal hardness, nor yet what

I have said of the confluence of immense glaciers like those that once united in the valley of the Lago Maggiore at what are now the Borromean Isles. But it seems to me that to any one who allows any excavating power to a glacier, it will be evident that when the general inclination of a valley was comparatively steep, a glacier could have had no opportunity of cutting for itself any special basin-shaped hollows. Its course, with a difference, is like that of a torrent. But in a flat-bottomed part of a valley, or in a comparative plain that lies at the base of a mountain-range, the case is not the same. For instance, to take an extreme case, if a glacier tumble over a slope of 45°, no one would dream of the ice-flow producing any special effect, except that in the long run, the upper edge of the rock that forms the cataract being worn away, its average angle would be lowered. And so of minor slopes; if the ice flowing fast (for a glacier) rendered the rocky surface underneath unequal, such inequalities could not become great and permanent; for the rapidly flowing ice would attack the projecting parts with greater power and effect than the minor hollows, and so preserve an approximate uniformity, or an average angle of moderate inclination. But when a monstrous glacier descended into a comparative plain, or into a low, flat valley, the case was different. There, to use homely phrases, the ice had time to select soft places for excavation, and there, if, from the confluence of large glaciers, or for other reasons, the downward pressure of the ice was of extra amount, the excavating effect, I contend, must have been unusually great in special areas, and have resulted in the formation of rock-bound hollows. And though the glacier of Ivrea has been constantly quoted as a case that completely proves the absurdity of my theory, this merely shows the unwariness of those who quote it; for not only are there a great many rock-basins full of water above Ivrea in among the vast *roches moutonnées* near the opening of the plain, but, where beyond this point the glacier spread out so wide on the Pliocene plain, it has scooped away so much material that parts of that plain are below the average level of the plains of Piedmont that lie outside the great moraine. Given sufficient time and extension of the glacier, and more matter still would have gone away. The same argument equally applies to the case on the Lake of Zurich, where glacier débris is said to lie on alluvial detritus. In reply to the question why in the actual valley of Aosta there are no lake-basins, I might with equal propriety say, Many contorted regions are much faulted, and there is often an evident connexion between contortion and faults; but in some contorted regions there are few or no faults, and the reason of their absence remains to be accounted for. I have attempted to explain why the rock-basins are present, and not why they are absent. It may be that some of the alluvial flats of the valley are lake-hollows filled up.

But another statement urged by Sir Roderick against my theory is, that the scooping-out of such hollows by ice is impossible, because ice cannot flow up an inclined plane. If so, I repeat, what is the meaning of the "*Stoss-Seite*" or upper side of a *roche moutonnée* that bars a wide glacier valley, through which barrier perhaps a mere narrow river gorge passes—as, for instance, in the case of the Kirchet so well known to Alpine men, or, on a smaller scale, of the *roches moutonnées* near the slate-quarries in Nant Francon? In both cases the barrier remained intact till the drainage of the glacier-formed lakes cut gorges through them—or, if Sir Roderick prefer it, till convulsions made gorges. Its *moutonnée* form will convince every accomplished glacialist that the ground was once covered by ice. The strike of the rocks will be enough for ordinary geologists; for no man can suppose who sees the corresponding forms of the *roches moutonnées* on either side of the narrow gorge of the Aar, that that gorge existed before the period of the great glacier, and that the glacier flowed entirely between the walls of the narrow passage. If I am right in this, then the great old glacier of the Aar flowed right over the hill, from bottom to top, and away into regions far beyond, in the manner I have imperfectly shown in my little book on the old glaciers of Switzerland and North Wales, and equally so whether the gorge was formed by sudden violence or by water.

In the existence, therefore, of "*Stoss-Seiten*," and in their upward striations, both in small and large *roches moutonnées*, there is proof that the belief that glaciers cannot flow over hillocks, and even hills of considerable size, is a mere assertion founded on prejudice: to me the wonder is, that any one can ever have believed it who has truly observed phenomena in the Alps, or who is familiar even with the ancient glaciation of our own country. And if this be so, I see no difficulty in accepting the hypothesis that the length and inclination of the slope which the bottom of a glacier may ascend depend simply on the thickness of the ice, and on the amount of the propelling power behind, that power being due to the weight and mass of the descending ice, and the average angles of the valley behind the point whence the upward ascent begins*.

Now, in dealing with this question, most of the geologists who have opposed me have treated the larger lake-hollows much as they do *Time*. Unconsciously they seem to me to be afraid both of it and of them. "Look," they seem to say, "at these mountains,

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* I think it might be possible to make a very good approximate calculation on this point, and I hope it may yet be done.

how awfully high and rugged they are; can any amount of time, aided by weather, torrents, rivers, and glaciers produce such effects? Old writers, like Hutton and Playfair, and a few modern observers (some of whom, both in America and Europe, have great familiarity with rocks), say they can; but we know that rending and fracture is the chief agent, and denudation is in comparison quite a trifling affair. Look, again, at the hollows of the lakes, how awfully deep they are! How is it possible for a glacier ever to have slid up a hill from a depth so profound?" In treating *the slopes as great*, consists the viciousness of this supposed argument. Unconsciously, some of the arguers are drawing exaggerated diagrams in their minds. They foreshorten the slope, increase in their mind's eye its steepness, and forget their trigonometry altogether. But let me beg of them to try to realize the real state of the case, and see how small by comparison the depth really is, and how gentle the slope. Were the bottom of the Lago Maggiore not undulated (for I believe the islands to be mere *roches moutonnées*), this slope from the deepest part of the lake (2600 feet) to its outflow would only be 2° 21′ in a distance of about 12 miles, a slope so gentle that, were a man standing on it, by the eye he would barely be able to tell whether he was on an inclined plane or not*. Again, take the Lake of Geneva from the place where it is nearly a thousand feet deep to Geneva, the average slope is only about 25′, an angle so small that any geologist looking at it would be apt to consider the surface as horizontal. The question, then, as regards the lakes resolves itself into this: Is it possible that the ice of the great old glaciers could ever have travelled up these exceedingly small inclinations for a distance, say of 12 miles in the one case and 20 to 25 miles in the other?

And now, in connexion with this point, I could wish that Sir Roderick had expressed an opinion whether or not he agrees with the old geologists, that (p. 7) "the Lakes of Geneva and Neufchatel were so filled up with snow and ice that the advancing glaciers travelled on them as bridges of ice, the foundations of which occupied the cavities." If this were so, then, in other words, the lower strata of ice in the hollow of what is now a lake remained in a condition of static equilibrium, and over this ice the advancing part of the glacier slipped or was propelled. Strictly speaking, it is evident that this state of static equilibrium is impossible; for all the ice of a glacier a little below the surface being, even in winter, in a melting state, the lower strata

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* In my original paper on the glacial excavation of certain lakes, I made an unfortunate error in calculation, stating that the angle is about 5°. In an able article in the 'Reader,' Professor Jukes corrected the error, and made the slope 2°.

above alluded to must have been destroyed and renewed over and over again; and as glacier-ice is practically anything but a rigid body, I think it would be easy to show that, just as in Arctic regions in winter the more rapid flow of the lower strata of ice, with a temperature of about 32°, shatters the more rigid and slowly-moving upper layers which have a temperature far below that point, so, for other reasons, the motion of some 2000 vertical feet of ice sliding over the basin, would be communicated to the lower strata; for pressure in ice produces adhesion of parts. I for one cannot conceive a horizontal fracture of 40 miles in length over the area of the Lake of Geneva, clearly dividing two bodies of ice, the lower of which was, where thickest, nearly 1000 feet, and the upper and sliding stratum must have been nearly 3000 feet thick. It is, in fact, a piece of mere elementary knowledge that any heavy body passing steadily across any other body, the parts of which are moveable, will communicate motion to the parts over which it passes, whether one or both of those bodies be viscid or plastic, or of some other compound character; and when I wrote my original paper it never occurred to me that there was any need of mentioning a point so obvious. But in a glacier that fills a lake-basin, this is by no means the only, and perhaps not the principal, cause of motion. A glacier does not throughout all its course move on simply by virtue of gravity. Pressure from behind has a great deal to do with it; as, for instance, in the case of the Rhone glacier, familiar to so many, and cited by Professor Merian and Dr. Tyndall. There, at the cataract, the ice fractures and slides down comparatively rapidly in masses, but at the base, where it moves slowly, pressure from behind causes the masses to touch and reunite, and the whole slides on, a re-formed mass, into the lower valley, the inclination of which is small. So, in the case of the lakes, the depths of which seem so appalling, but the real angles of the beds of which are so small, there seems to me nothing either impossible or remarkable in the idea that the long and enormous onflowing inclined mass of the glacier of the Rhone pushed before it in the plain (for such it is) its own more sluggish continuation up a slope of 25′ for a distance of 20 or 25 miles. I believe that the same argument is equally applicable to the Lago Maggiore, where the already vast glacier, swelled by the mighty tributary of the Val d'Ossola, was thus enabled to push along the low average slope of 2° 21′ for a distance about half as great. The very islands in many a lake once filled with ice help to prove this; for, as in the case of Loch Lomond, they are mere *roches moutonnées*, and I for one cannot conceive that the mammillation ceases immediately below the surface of the water.

Having got thus far, I will not repeat my arguments to show that (as I attempted to prove in my original memoir) the Alpine and other ice-worn lakes known to me do not lie in areas of *special subsidence,* nor in *gaping fractures,* nor in simple *synclinal basins,* nor in *hollows of watery erosion.* If any one who reads this is curious about them, he must refer to that memoir*; but this at least I may be permitted to say: I used at all events arguments, even somewhat elaborate, and not mere statements, and whether these arguments are fated to be successful time alone will show. That they were at all events of some value, the names of the distinguished geologists who have accepted my theory helps to show; and I could add to these other names as high as the very highest of those on whose authority Sir Roderick so much depends, did propriety permit me to quote from letters and commit men to opinions which they have not expressed in print.

But before leaving the subject, let me say a little more about the possibility of these lakes lying in fractures. For this purpose let us take some of those that lie on the north side of the Alps, partly in the region of the Miocene strata. If they lie in lines of gaping fracture, nearly as wide as the present lakes, then on the hills, say between the Lake of Lucerne and Thun, and between Thun and the Lake of Zurich, the Miocene strata *would be crumpled up in zigzag lines across the average line of strike, to an amount corresponding to the distance between the severed strata in the spaces now overlooking and occupied by the lakes.* This is not the case. Again, if the fractures were mere narrow cracks, then the amount of denudation that took place so as to form the wide valleys has been enormous, and within a mere fraction of what I require, especially when we consider that the great denudation necessary to widen the fractures would have filled up the lake-basins. The theory of the chief formation of Alpine valleys having been effected by weather, water, and ice, would therefore still hold good.

I might continue these arguments, and discuss in detail what Sir Roderick has said about Scandinavia, North America, and other regions, and among other things show how unprecise is the knowledge that we actually possess respecting the details of the boulder-beds that overspread some of them, and how unsafe it is to conclude, because a country is not actually mountainous, and does not now lie high above the sea-level, that it was never covered by glacier-ice in motion, and may not at one time have lain much higher. In spite of Agassiz's memoirs, it is not long since all the lower Till of Scotland was considered not to be ordinary moraine-matter at all, but to have been formed

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* They are also given in 'The Physical Geology and Geography of Great Britain.'

solely in the sea by the transporting agency of icebergs. Let those who still believe it refer for proof to the contrary to Mr. Geikie's admirable work 'On the Phenomena of the Glacial Drift of Scotland.' I know enough of the superficial strata in North America to foresee that the erratic deposits there will some day also be divided into terrestrial and marine series, and I am pretty sure that Sir William Logan will not deny the probability. For the vast size of the ancient glaciers of that continent, I would refer to Professor Dana's admirable Manual of American Geology. It is a mistake to suppose that the striations there merely run from north to south, for Sir William Logan, who has mapped them, proves that they often conform to the bends of the valleys.

As regards the great lakes of that continent, so far from being "cavities originally due to a combination of ruptures and denudations of the rocks," it is impossible intimately to know the country and believe it. There the Silurian strata, amid which the lakes lie, are arranged so tranquilly and at angles so low, that the flattest chalk of Great Britain may be almost said to be tumultuous in comparison; and the forthcoming sections of Sir William Logan conclusively prove that around the lakes there is no trace of dislocation to help to form the hollows, nor yet do they lie in hollows of special subsidence. Only Lake Superior covers a faint synclinal curve; and Lake Ontario, so far from occupying an area of special depression, actually lies on a very low anticlinal bend of soft strata, the top of which has been denuded away. That Sir William, who has been called the best stratigraphical geologist in America, believes that ice has something to do with the scooping out of rock-basins, any one may see who refers to his late masterly report on the geology of Canada; and Professor Newberry, whom Sir Roderick knows as a physical geologist and geographer, adheres strongly to that opinion.

As for the observation of my friend M. de Verneuil, that the orographic hollows in Spain are precisely those that "a theorist" might "attribute to excavation by ice," I decline to be judged by it, till I have seen them and declared that opinion. I object, both for myself and my supporters, that we should be judged in a manner so vague. And further, I think I appeal to Nature to some purpose when, neither for the first nor the second time, I ask philosophers to consider why it is that not only drift- and moraine-dammed lakes, but striated rock-basins of all sizes occur in such prodigious numbers in America, Scandinavia, the Highlands, and in all other rocky temperate regions, high or low, that have been glaciated, while in tropical and subtropical regions they are so rare as to be quite exceptional elsewhere than in mountain areas that now or once maintained their glaciers.

Several other points raised by Sir Roderick in that part of his Address that relates to physical geology, glaciers, and icebergs remain to be discussed. I have entered, however, on this argument with great reluctance, and, unless circumstances again constrain me, I shall leave the remaining questions untouched.

XXXVI. *Historic Notes on the Conservation of Energy.*

By Professor BOHN.

To Professor Tyndall, Esq., F.R.S.

I READ your "Notes on Scientific History"[*] with great pleasure and satisfaction. I agree perfectly with all you say respecting Mayer's researches as compared with those of others; in some respects, indeed, I am inclined to go further than you do. Seven years ago I studied the history of the principle of the Conservation of Energy, and arrived at the same conclusion, with respect to the *modern* development of this theorem, as the one for which you so ably and warmly contend. Your recent disinterested advocacy of Mayer's claims, and my own conviction that historic truth is the sole object of your research, inspire the hope that the following remarks will be found worthy of attentive perusal.

Descartes, so far as I know, was the first to give expression to the thought that whatever is not material must necessarily be indestructible. This non-material something he called "Force," a word which subsequently had, for a long period, divers and consequently vague meanings.

In Descartes's *Principia Philosophiæ* (Pars II., § xxxvi.) we find the following:—"Deum esse primariam motus causam et eandem semper motus quantitatem in universo conservare.

"Motûs naturâ sic animadversâ, considerare oportet ejus causam, eamque duplicem: Primò scilicet universalem et primariam, quæ est causa generalis omnium motuum qui sunt in mundo; ac deinde particularem, à quâ fit, ut singulæ materiæ partes motus, quos priùs non habuerunt, acquirant. Et generalem quod attinet, manifestum mihi videtur illam non aliam esse, quàm Deum ipsum, qui materiam simul cum motu et quiete in principio creavit[†], jamque per solum suum concursum ordinarium, tantundem motûs et quietis in eâ totâ quantum tunc posuit conservat. Nam quamvis ille motus nihil aliud sit in materiâ motâ quàm ejus modus; certam tamen et determinatam habet

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[*] Phil. Mag. S. 4. vol. xxviii. p. 25.

[†] Phil. Mag. S. 3. vol. xxiii. p. 442 (1843): Mr. Joule, "That the grand agents of nature are by the Creator's fiat indestructible."

quantitatem, quam facilè intelligimus eandem semper in totâ rerum universitate esse posse, quàmvis in singulis ejus partibus mutetur. Ita scilicet ut putemus, cùm una pars materia duplò celeriùs movetur, quàm altera, et hæc altera duplò major est quàm prior, tantundem motûs esse in minore quàm in majore, ac quantò motus unius partis lentior fit, tantò motum alicujus alterius ipsi æqualis fieri celeriorem. Intelligimus etiam perfectionem esse in Deo, non solùm quòd in se ipso sit immutabilis, sed etiam quòd modo quàm maximè constanti et immutabili operetur: Adeò ut iis mutationibus exceptis, quas evidens experientia, vel divina revelatio certas reddit, quasque sine ullâ in creatore mutatione fieri percipimus, aut credimus, nullas alias in ejus operibus supponere debeamus, ne qua inde inconstantia in ipso arguatur. Unde sequitur quàm maximè rationi esse consentaneum, ut putemus ex hoc solo, quòd Deus diversimodè moverit partes materiæ, cùm primùm illas creavit, jamque totam istam materiam conservet, eodem planè modo, eâdemque ratione quâ priùs creavit, eum etiam tantundem motûs in ipsâ semper conservare."

Descartes therefore, precisely like Colding, bases the proof of his theorem on a divine attribute. The unsatisfactory nature of such a proof is manifest. Are we not equally justified in asserting that the assumption of a constant quantity of motion involves a limitation of divine power? The almightiness of God must manifest itself by actual achievement, new motion must incessantly be created; therefore, assuming with Descartes the indestructibility of that which exists, the quantity of motion must increase.

Every attempt to deduce a natural law from an *à priori* conceived attribute of God must inevitably be utterly fruitless.

Leibnitz was the first to publish, in its proper form, the general theorem of the conservation of *vis viva,* and to demonstrate the same by empirically ascertained and rationally established theorems. He at once opposes Descartes's views, and introduces the important conception of *vis viva.* All this will be found in his article in the *Acta Eruditorum,* Lips. 1686, entitled: "Brevis demonstratio erroris memorabilis Cartesii et aliorum circa legem naturæ, secundum quam volunt a Deo eandem semper quantitatem motus conservari, qua et in re mechanica abutuntur."

In the warm discussion which arose, Leibnitz argued that the assumption of the incorrectness of his views involved the necessity, or at least the possibility of a perpetual motion; which latter he urged is manifestly absurd. Colding employs the same argument; and Helmholtz, in 1847, in his well-known work 'On the Conservation of Force,' attributes great importance to this theorem concerning the absurdity of perpetual motion.

John Bernoulli, however, gave the clearest exposition of the principle of the conservation of *vis viva*, as will be admitted after a perusal of his correspondence with Leibnitz (*Vivorum cel. G. C. Leibnitii et Joh. Bernoulli commercium phil. et math.*), of his "Discours sur les lois de la communication du mouvement" (*Opera omnia*, tom. iii. p. 1), and especially of his memoir entitled "De vera notione virium vivarum earumque usu in dynamicis" (*Opera omnia*, tom. iii. p. 239).

Allow me to draw your particular attention to the following two passages of the last-named memoir:—

"§ III. Hinc patet *vim vivam* [quæ aptius vocaretur *facultas agendi*, Gallice *le pouvoir*] esse aliquid reale et substantiale, quod per se subsistit, et quantum in se est, non dependet ab alio. Unde concludimus, quamlibet vim vivam habere suam determinatam quantitatem, de qua nihil perire potest, quod non in effectu edito reperiatur. Hinc sponte fluit, vim vivam semper conservari; adeo ut quæ ante actionem residebat in uno pluribusve corporibus, nunc post actionem reperiatur necessario in alio, vel aliis pluribus corporibus, nisi quid in prioribus remanserit. Atque hoc est, quod vocamus *conservationem virium vivarum*."

One fact of peculiar interest is John Bernoulli's assertion that the *vis viva* which apparently disappears—that is to say, the *vis viva* which is not employed in external work such as the raising of a weight—may be consumed in molecular work. The following extract from § 9 of the above memoir will establish this point.

"Si corpora non sunt perfecte elastica, aliqua pars virium vivarum, quæ periisse videtur, consumitur in compressione corporum, quando perfecte se non restituunt; a quo autem nunc abstrahimus, concipientes, compressionem illam esse similem compressioni elastri, quod post tensionem factam impediretur ab aliquo retinaculo, quo minus se rursus dilatare posset, et sic non redderet, sed in se retineret vim vivam, quam a corpore incurrente accepisset; unde nihil virium periret, etsi periisse videretur."

The conversion of *vis viva* into heat, or at least the possibility of such a conversion, was first asserted by Augustin Fresnel. The French translation of Thomson's 'Chemistry' contains an appendix "On Light" from Fresnel's pen, wherein he says:—

"C'est un principe général du mouvement des fluides élastiques, que, de quelque façon que l'ébranlement s'étende ou se subdivise, la somme totale des forces vives reste constante. Et voilà principalement pourquoi la force vive doit être considérée comme la mesure de la lumière, dont la quantité totale reste toujours à très peu près la même, tant qu'elle ne traverse du moins que des milieux très transparens. Les corps noirs et même les surfaces métalliques les plus brillantes ne réflechissent pas à beaucoup près la totalité de la lumière qui tombe sur leur surface; les corps imparfaitement transparens, et même les plus diaphanes, quand ils sont assez épais, absorbent aussi (pour me servir de l'expression usitée) une quantité notable de la lumière incidente; mais il n'en faut pas conclure que le principe de la conservation des forces vives, n'est plus applicable à ces phénomènes; il résulte au contraire de l'idée la plus probable qu'on puisse se faire sur la constitution mécanique des corps, que la somme des forces vives doit toujours rester la même (tant que les forces accélératrices qui tendent à ramener les molécules à leurs positions d'équilibre n'ont pas changé d'intensité), et que *la quantité de forces vives qui disparaît comme lumière est reproduite en chaleur*."

Should you consider the contents of this letter suitable for the pages of the Philosophical Magazine, I should feel honoured by its publication in that journal.

Giessen, August 14, 1864.

XXXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 240.]

June 16, 1864.—Major-General Sabine, President, in the Chair.

THE following communication was read:—

"On the Properties of Silicic Acid and other analogous Colloidal Substances." By Thomas Graham, F.R.S., Master of the Mint.

The prevalent notions respecting solubility have been derived chiefly from observations on crystalline salts, and are very imperfectly applicable to the class of colloidal substances. Hydrated silicic acid, for instance, when in the soluble condition, is properly speaking a liquid body, like alcohol, miscible with water in all proportions. We have no degrees of solubility to speak of with respect to silicic acid, like the degrees of solubility of a salt, unless it be with reference to silicic acid in the gelatinous condition, which is usually looked upon as destitute of solubility. The jelly of silicic acid may be more or less rich in combined water, as it is first prepared, and it appears to be soluble in proportion to the extent of its hydration. A jelly containing 1 per cent. of silicic acid, gives with cold water a solution containing about 1 of silicic acid in 5000 water; a jelly containing 5 per cent. of silicic acid, gives a solution containing about 1 part of acid in 10,000 water. A less hydrated jelly than the last mentioned is still less soluble; and finally, when the jelly is rendered anhydrous it gives gummy-looking white masses, which appear to be absolutely insoluble, like the light dusty silicic acid obtained by drying a jelly charged with salts, in the ordinary analysis of a silicate.

The liquidity of silicic acid is only effected by a change which is permanent (namely, coagulation or pectization), by which the acid is converted into the gelatinous or pectous form, and loses its miscibility with water. The liquidity is permanent in proportion to the degree of dilution of silicic acid, and appears to be favoured by a low temperature. It is opposed, on the contrary, by concentration, and by elevation of temperature. A liquid silicic acid of 10 or 12 per cent. pectizes spontaneously in a few hours at the ordinary temperature, and immediately when heated. A liquid of 5 per cent. may be preserved for five or six days; a liquid of 2 per cent. for two or three months; and a liquid of 1 per cent. has not pectized after two years. Dilute solutions of 0·1 per cent. or less are no doubt practically unalterable by time, and hence the possibility of soluble silicic acid existing in nature. I may add, however, that no solution, weak or strong, of silicic acid in water has shown any disposition to deposit *crystals,* but always appears on drying as a colloidal glassy hyalite. The formation of quartz crystals at a low temperature, of so frequent occurrence in nature, remains still a mystery. I can only imagine that such crystals are formed at an inconceivably slow rate, and from solutions of silicic acid which are extremely dilute. Dilution no doubt weakens the colloidal character of substances, and may therefore allow their crystallizing tendency to gain ground and develope itself, particularly where the crystal once formed is completely insoluble, as with quartz.

The pectization of liquid silicic acid is expedited by contact with solid matter in the form of powder. By contact with pounded graphite, which is chemically inactive, the pectization of a 5 per cent. silicic acid is brought about in an hour or two, and that of a 2 per cent. silicic acid in two days. A rise of temperature of 1°·1 C. was observed during the formation of the 5 per cent. jelly.

The ultimate pectization of silicic acid is preceded by a gradual thickening in the liquid itself. The flow of liquid colloids through a capillary tube is always slow compared with the flow of crystalloid solutions, so that a liquid-transpiration-tube may be employed as a colloidoscope. With a colloidal liquid alterable in viscosity, such as silicic acid, the increased resistance to passage through the colloidoscope is obvious from day to day. Just before gelatinizing, silicic acid flows like an oil.

A dominating quality of colloids is the tendency of their particles to adhere, aggregate, and contract. This idio-attraction is obvious in the gradual thickening of the liquid, and when it advances leads to pectization. In the jelly itself, the specific contraction in question, or *synæresis,* still proceeds, causing separation of water, with the division into a clot and serum; and ending in the production of a hard stony mass, of vitreous structure, which may be anhydrous, or nearly so, when the water is allowed to escape by evaporation. The intense synæresis of isinglass dried in a glass dish over sulphuric acid *in vacuo,* enables the contracting gelatine to tear up the surface of the glass. Glass itself is a colloid, and the adhesion of colloid to colloid appears to be more powerful than that of colloid to crystalloid.

The gelatine, when dried in the manner described upon plates of calc-spar and mica, did not adhere to the crystalline surface, but detached itself on drying. Polished plates of glass must not be left in contact, as is well known, owing to the risk of permanent adhesion between their surfaces. The adhesion of broken masses of glacial phosphoric acid to each other is an old illustration of colloidal synæresis.

Bearing in mind that the colloidal phasis of matter is the result of a peculiar attraction and aggregation of molecules, properties never entirely absent from matter but greatly more developed in some substances than in others, it is not surprising that colloidal characters spread on both sides into the liquid and solid conditions. These characters appear in the viscidity of liquids, and in the softness and adhesiveness of certain crystalline substances. Metaphosphate of soda, after fusion by heat, is a true glass or colloid; but when this glass is maintained for a few minutes at a temperature some degrees under its point of fusion, the glass assumes a crystalline structure without losing its transparency. Notwithstanding this change, the low diffusibility of the salt is preserved, with other characters of a colloid. Water in the form of ice has already been represented as a similar intermediate form, both colloid and crystalline, and in the first character adhesive and capable of reunion or "regelation."

It is unnecessary to return here to the fact of the ready pectization of liquid silicic acid by alkaline salts, including some of very sparing solubility, such as carbonate of lime, beyond stating that the presence of carbonate of lime in water was observed to be incompatible with the coexistence of soluble silicic acid, till the proportion of the latter was reduced to nearly 1 in 10,000 water.

Certain liquid substances differ from the salts in exercising little or no pectizing influence upon liquid silicic acid. But, on the other hand, none of the liquids now referred to appear to conduce to the preservation of the fluidity of the colloid, at least not more than the addition of water would do. Among these inactive diluents of silicic acid are found hydrochloric, nitric, acetic, and tartaric acids, syrup of sugar, glycerine, and alcohol. But all the liquid substances named, and many others, appear to possess an important relation to silicic acid, of a very different nature from the pectizing action of salts. They are capable of displacing the combined water of the silicic acid hydrate, whether that hydrate is in the liquid or gelatinous condition, and give new substitution-products.

A liquid compound of *alcohol* and silicic acid is obtained by adding alcohol to aqueous silicic acid, and then employing proper means to withdraw the water from the mixture. For that purpose the mixture contained in a cup may be placed over dry carbonate of potash or quicklime, within the receiver of an air-pump. Or a dialyzing bag of parchment-paper containing the mixed alcohol and silicic acid may be suspended in a jar of alcohol: the water diffuses away, leaving in the bag a liquid composed of alcohol and silicic acid only. A point to be attended to is, that the silicic acid should never be allowed to form more than 1 per cent. of the alcoholic solution, otherwise it may gelatinize during the experiment. If I may be allowed to distinguish the liquid and gelatinous hydrates of silicic acid by the irregularly formed terms of *hydrosol* and *hydrogel* of silicic acid, the two corresponding alcoholic bodies now introduced may be named the alcosol and alcogel of silicic acid.

The *alcosol* of silicic acid, containing 1 per cent. of the latter, is a colourless liquid, not precipitated by water or salts, nor by contact with insoluble powders, probably from the small proportion of silicic acid present in solution. It may be boiled and evaporated without change, but is gelatinized by a slight concentration. The alcohol is retained less strongly in the alcosol of silicic acid than water is in the hydrosol, but with the same varying force, a small portion of the alcohol being held so strongly as to char when the resulting jelly is rapidly distilled at a high temperature. Not a trace of silicic ether is found in any compound of this class. The jelly burns readily in the air, leaving the whole silicic acid in the form of a white ash.

The *alcogel*, or solid compound, is readily prepared by placing masses of gelatinous silicic acid, containing 8 or 10 per cent. of the dry acid, in absolute alcohol, and changing the latter repeatedly till the water of the hydrogel is fully replaced by alcohol. The alcogel is generally slightly opalescent, and is similar in aspect to the hydrogel, preserving very nearly its original bulk. The following is the composition of an alcogel carefully prepared from a hydrogel which contained 9·35 per cent. of silicic acid:—

Alcohol	88·13
Water	0·23
Silicic acid	11·64
	100·00

Placed in water, the alcogel is gradually decomposed—alcohol diffusing out and water entering instead, so that a hydrogel is reproduced.

Further, the alcogel may be made the starting-point in the formation of a great variety of other substitution jellies of analogous constitution, the only condition required appearing to be that the new liquid and alcohol should be intermiscible, that is, interdiffusible bodies. Compounds of ether, benzole, and bisulphide of carbon have thus been produced. Again, from *etherogel* another series of silicic acid jellies may be derived, containing fluids soluble in ether, such as the fixed oils.

The preparation of the *glycerine* compound of silicic acid is facilitated by the comparative fixity of that liquid. When hydrated silicic acid is first steeped in glycerine, and then boiled in the same liquid, water distils over, without any change in the appearance of the jelly, except that when formerly opalescent it becomes now entirely colourless, and ceases to be visible when covered by the liquid. But a portion of the silicic acid is dissolved, and a *glycerosol* is produced at the same time as the glycerine jelly. A glycerogel prepared from a hydrate containing 9·35 per cent. of silicic acid, was found by a combustion analysis to be composed of

Glycerine	87·44
Water	3·78
Silicic acid	8·95
	100·17

The glycerogel has somewhat less bulk than the original hydrogel. When a glycerine jelly is distilled by heat, it does not fuse, but the whole of the glycerine comes over, with a slight amount of decomposition towards the end of the process.

The compound of sulphuric acid, *sulphagel*, is also interesting from the facility of its formation, and the complete manner in which the water of the original hydrogel is removed. A mass of hydrated silicic acid may be preserved unbroken if it is first placed in sulphuric acid diluted with two or three volumes of water, and then transferred gradually to stronger acids, till at last it is placed in concentrated oil of vitriol. The sulphagel sinks in the latter fluid, and may be distilled with an excess of it for hours without losing its transparency or gelatinous character. It is always somewhat less in bulk than the primary hydrogel, but not more, to the eye, than one-fifth or one-sixth part of the original volume. This sulphagel is transparent and colourless. When a sulphagel is heated strongly in an open vessel, the last portions of the monohydrated sulphuric acid in combination are found to require a higher temperature for their expulsion than the boiling-point of the acid. The whole silicic acid remains behind, forming a white, opaque, porous mass, like pumice. A sulphagel placed in water is soon decomposed, and the original hydrogel reproduced. No permanent compound of sulphuric and silicic acids, of the nature of a salt, appears to be formed in any circumstances. A sulphagel placed in alcohol gives ultimately a pure alcogel. Similar jellies of silicic acid may readily be formed with the monohydrates of nitric, acetic, and formic acids, and are all perfectly transparent.

The production of the compounds of silicic acid now described indicates the possession of a wider range of affinity by a colloid than could well be anticipated. The organic colloids are no doubt invested with similar wide powers of combination, which may become of interest to the physiologist. The capacity of a mass of gelatinous silicic acid to assume alcohol, or even oleine, in the place of water of combination, without disintegration or alteration of form, may perhaps afford a clue to the penetration of the albuminous matter of membrane by fatty and other insoluble bodies, which seems to occur in the digestion of food. Still more remarkable and suggestive are the *fluid* compounds of silicic acid. The fluid alcohol-compound favours the possibility of the existence of a compound of the colloid albumen with oleine, soluble also and capable of circulating with the blood.

The feebleness of the force which holds together two substances belonging to different physical classes, one being a colloid and the other a crystalloid, is a subject deserving notice. When such a compound is placed in a fluid, the superior diffusive energy of the crystalloid may cause its separation from the colloid. Thus, of hydrated silicic acid, the combined water (a crystalloid) leaves the acid (a colloid) to diffuse into alcohol; and if the alcohol be repeatedly changed, the entire water is thus removed, alcohol (another crystalloid) at the same time taking the place of water in combination with the silicic acid. The liquid in excess (here the alcohol) gains entire possession of the silicic acid. The process is reversed if an alcogel be placed in a considerable volume of water. Then alcohol separates from combination, in consequence of the opportunity it possesses to diffuse into water; and water, which is now the liquid present in excess, recovers possession of the silicic acid. Such changes illustrate the predominating influence of mass.

Even the compounds of silicic acid with alkalies yield to the decomposing force of diffusion. The compound of silicic acid with 1 or 2 per cent. of soda is a colloidal solution, and, when placed in a dialyzer over water *in vacuo* to exclude carbonic acid, suffers gradual decomposition. The soda diffuses off slowly in the caustic state, and gives the usual brown oxide of silver when tested with the nitrate of that base.

The pectization of liquid silicic acid and many other liquid colloids is effected by contact with minute quantities of salts in a way which is not understood. On the other hand, the gelatinous acid may again be liquefied and have its energy restored by contact with a very moderate amount of alkali. The latter change is gradual, 1 part of caustic soda, dissolved in 10,000 water, liquefying 200 parts of silicic acid (estimated dry) in 60 minutes at 100° C. Gelatinous stannic acid also is easily liquefied by a small proportion of alkali, even at the ordinary temperature. The alkali, too, after liquefying the gelatinous colloid, may be separated again from it by diffusion into water upon a dialyzer. The solution of these colloids, in such circumstances, may be looked upon as analogous to the solution of insoluble organic colloids witnessed in animal digestion, with the difference that the solvent fluid here is not acid but alkaline. Liquid silicic acid may be represented as the "peptone" of gelatinous silicic acid; and the liquefaction of the latter by a trace of alkali may be spoken of as the peptization of the jelly. The pure jellies of alumina, peroxide of iron, and titanic acid, prepared by dialysis, are assimilated more closely to albumen, being peptized by minute quantities of hydrochloric acid.

Liquid Stannic and Metastannic Acids.—Liquid stannic acid is prepared by dialyzing the bichloride of tin with an addition of alkali, or by dialyzing the stannate of soda with an addition of hydrochloric acid. In both cases a jelly is first formed on the dialyzer; but, as the salts diffuse away, the jelly is again peptized by the small proportion of free alkali remaining: the alkali itself may be removed by continued diffusion, a drop or two of the tincture of iodine facilitating the separation. The liquid stannic acid is converted on heating it into liquid metastannic acid. Both liquid acids are remarkable for the facility with which they are pectized by a minute addition of hydrochloric acid, as well as by salts.

Liquid Titanic Acid is prepared by dissolving gelatinous titanic acid in a small quantity of hydrochloric acid, without heat, and placing the liquid upon a dialyzer for several days. The liquid must not contain more than 1 per cent. of titanic acid, otherwise it spontaneously gelatinizes, but it appears more stable when dilute. Both titanic and the two stannic acids afford the same classes of compounds with alcohol &c. as are obtained with silicic acid.

Liquid Tungstic Acid.—The obscurity which has so long hung over tungstic acid is removed by a dialytic examination. It is in fact a remarkable colloid, of which the pectous form alone has hitherto been known. Liquid tungstic acid is prepared by adding dilute hydrochloric acidcarefully to a 5 per cent. solution of tungstate of soda, in sufficient proportion to neutralize the alkali, and then placing the resulting liquid on a dialyser. In about three days the acid is found pure, with the loss of about 20 per cent., the salts having diffused entirely away. It is remarkable that the *purified* acid is not pectized by acids or salts even at the boiling temperature. Evaporated to dryness, it forms vitreous scales, like gum or gelatine, which sometimes adhere so strongly to the surface of the evaporating dish as to detach portions of it. It may be heated to 200° C. without losing its solubility or passing into the pectous state, but at a temperature near redness it undergoes a molecular change, losing at the same time 2·42 per cent. of water. When water is added to unchanged tungstic acid, it becomes pasty and adhesive like gum; and it forms a liquid with about one-fourth its weight of water, which is so dense as to float glass. The solution effervesces with carbonate of soda, and tungstic acid is evidently associated with silicic and molybdic acids. The taste of tungstic acid dissolved in water is not metallic or acid, but rather bitter and astringent. Solutions of tungstic acid containing 5, 20, 50, 66·5, and 79·8 per cent. of dry acid, possess the following densities at 19°, 1·0475, 1·2168, 1·8001, 2·396, and 3·243. Evaporated *in vacuo* liquid tungstic acid is colourless, but becomes green in air from the deoxidating action of organic matter. Liquid silicic acid is protected from pectizing when mixed with tungstic acid, a circumstance probably connected with the formation of the double compounds of these acids which M. Marignac has lately described.

Molybdic Acid has hitherto been known (like tungstic acid) only in the insoluble form. Crystallized molybdate of soda dissolved in water is decomposed by the gradual addition of hydrochloric acid in excess without any immediate precipitation. The acid liquid thrown upon a dialyzer may gelatinize after a few hours, but again liquefies spontaneously, when the salts diffuse away. After a diffusion of three days, about 60 per cent. of the molybdic acid remains behind in a pure condition. The solution of pure molybdic acid is yellow, astringent to the taste, acid to test-paper, and possesses much stability. The acid may be dried at 100°, and then heated to 200° without losing its solubility. Soluble molybdic acid has the same gummy aspect as soluble tungstic acid, and deliquesces slightly when exposed to damp air. Both acids lose their colloidality when digested with soda for a short time, and give a variety of crystallizable salts.

GEOLOGICAL SOCIETY.

[Continued from p. 243.]

June 22, 1864.—W. J. Hamilton, Esq., President, in the Chair.

1. "On the Fossiliferous Rocks of Forfarshire and their contents." By James Powrie, Esq., F.G.S.

Referring to his former paper for a detailed description of the lower members of the Forfarshire Old Red Sandstone, the author now gave a general sketch of the relations of the several beds, and then descriptions of the species of Crustacea and Fish occurring in them. The latter belong to five genera, two of which (*Ischnacanthus* and *Euthacanthus*) are new. After discussing the nature of *Parka decipiens*, and shortly noticing the genera of Crustacea that occur in the same rocks, Mr. Powrie concluded his paper with a short synopsis of the distribution of the members of the Old Red Sandstone in Forfarshire, and a discussion respecting the subdivision of that formation, in which he stated that *Pterygotus, Parka decipiens*, and *Cephalaspis* are always associated in the same beds, and extend through all the fossiliferous rocks of Forfarshire, instead of the latter characterizing a higher horizon than the others.

2. "On the Reptiliferous Rocks and Foot-print Strata of the North-east of Scotland." By Prof. R. Harkness, F.R.SS. L. & E., F.G.S.

The author showed that the foot-print sandstones of Ross-shire constitute the upper portion of the Old Red Sandstone formation, and that the strata embraced in a line of section from the Nigg to Cambus Shandwick, from above the Gneiss to the foot-print sandstones of Tarbet Ness inclusive, are conformable throughout, and are referable to each of the three divisions of the Old Red Sandstone,—namely, the conglomerates and yellow sandstones (of a thickness of 1500 feet) belonging to the Lower Old Red Sandstone; the grey flaggy sandstones and shales of Geanies—the equivalent of the Caithness flags—containing *Osteolepis, Coccosteus*, and *Acanthodes*, and thus referable to the Middle Old Red; thirdly, conformable strata, consisting of conglomerates and foot-bearing and other sandstones appertaining to the higher members of the system. The foot-bearing sandstones have a thickness of 400 feet, and represent the reptiliferous sandstones of the Elgin area, though not overlain by Cornstones as in that district.

The author, in conclusion, remarked that though *Stagonolepis* is decidedly *Teleosaurian* in its affinities, it does not consequently mark a Mesozoic group of rocks; for *Mastodontosauriæ*, which abound in the Trias, occur in the Coal-measures; and stratigraphical evidence shows us that Teleosaurian crocodiles have a wider geological range, since they are met with in the Old Red Sandstone.

3. "On some Bone- and Cave-deposits of the Reindeer-period in the South of France." By John Evans, Esq., F.R.S., F.G.S.

The deposits to which the author particularly called attention in this paper are those which have been, and are still being explored under the direction of MM. Lartet and Christy, and which were visited by him under the guidance of the latter gentleman and accompanied by Mr. Hamilton, Prof. Rupert Jones, Capt. Galton, Mr. Lubbock, and Mr. Franks.

Mr. Evans first gave a detailed description of the physical features of the valley of the Vézère, and of the contents of the caverns of Badegoule, Le Moustier, La Madelaine, Laugerie-Haute, Laugerie-Basse, the Gorge d'Enfer, and Les Eyzies, giving a list of the animal-remains discovered, which are for the most part of the same species from all the caverns.

The author then discussed the antiquity of the deposits according to four methods of inquiry,—namely, from geological considerations with regard to the character and position of the caves; from the palæontological evidence of the remains found in them; from the archæological character of the objects of human workmanship; and from a comparison with similar deposits in neighbouring districts in France; and he came to the conclusion that they belonged to a period subsequent to that of the *Elephas primigenius* and *Rhinoceros tichorhinus*, but characterized by the presence of the Reindeer and some other animals now extinct in that part of Europe.

4. "On the Carboniferous Rocks of the Donetz and the Granite-gravel of St. Petersburg." By Prof. J. Helmersen. (In a letter to Sir R. I. Murchison, K.C.B., F.R.S., F.G.S., &c.)

This letter relates (1) to the discovery in the Donetz Mountains of additional beds of coal and of iron-ore; (2) to the proposed use of this coal for steam-purposes on the Volga; (3) to two geological expeditions to be sent out in 1864 for the purpose of surveying the Permian basin of Russia; and lastly, to the successful completion of an Artesian boring at St. Petersburg. In this well the following beds were passed through:—Alluvium, 88 ft.; Silurian clay, 300 ft.; sandstone, 137 ft.; bed of gravel, the result of the degradation of granite.

5. "On a supposed Deposit of Boulder-clay in North Devon." By George Maw, Esq., F.G.S., F.L.S.

A deposit of brown clay which occurs near Fremington, in North Devon, and has been worked for several years, was described by the author in this paper, and referred by him to the Boulder-clay formation. The smallest amount of subsidence necessary for the deposition of this clay at its present highest level would place a large area of Devonshire under water.

Mr. Maw considered the raised beach at Croyd as being a much more recent deposit than the gravel just described; and in connexion with the question of the former submergence of Devonshire during the glacial period, he discussed the relation of the latter to a deposit of granite-drift gravel at Petrochstow, concluding that it could only have been transported thither during the submergence of the high ridges which intersect at right angles the country between the two deposits.

6. "On the former existence of Glaciers in the High Grounds of the South of Scotland." By J. Young, M.D., F.R.S.E.

The heights bordering the counties of Peebles and Dumfries are stated by the author to contain well-preserved remains of a group of Glaciers belonging to a later period than the Boulder-clay, and some of which have been already alluded to by Mr. Geikie and Mr. Chambers. Dr. Young then describes the physical geography of the region, grouping the several hills into three ranges—the Broad Law Range, the White Coomb Range, and Hartfell—from which certain glaciers formerly descended into the valleys; and he further divides the glaciers into two classes, which he terms respectively the "Social" and the "Solitary." The author then describes the form and extension of the masses of detritus which he considers to be glacial débris, contrasting their characters with those of the patches of Boulder-clay occurring in the neighbourhood.

Many indications of glaciers are shown to be much obscured by the prevalence of peat in the district; but, in addition to the moraine matter, smoothed surfaces and *roches moutonnées* are occasionally seen.

7. "On the Formation and Preservation of Lakes by Ice-action." By Thomas Belt, Esq.

During a residence of two years in the province of Nova Scotia, the author observed the remarkable number of lakes, great and small, occurring there, sometimes in connected chains and sometimes on the sides and tops of hills. The lake-basins are stated to be chiefly in extremely hard quartzites and metamorphosed schists, irregularly studded with masses of Boulder-clay, beneath which are seen scratches, grooves, &c., that have been produced by ice-action. The author then describes all the phenomena in detail, and gives a *résumé* of the theory of their glacial origin, as propounded by Professor Ramsay, coming to the conclusion that in this way only can the facts be consistently explained.

8. "A Sketch of the Principal Geological Features of Hobart, Tasmania." By S. H. Wintle, Esq.

The hills upon which Hobart is built, as well as those in the vicinity, are mostly composed of New (?) Red Sandstone, capped with Greenstone of variable composition and of great thickness in some places.

The Carboniferous Limestone(?) is stated to be very extensively developed throughout the island, and to be very fossiliferous; the author describes its lithological characters, as well as those of the Devonian rocks and the Silurian slates of Mount Wellington, which last have, as yet, proved unfossiliferous; but he states that Mr. Gould has found a *Calymene Blumenbachii* in similar rocks in the interior. He then, after describing the Coal-formation of the island, and remarking upon the anthracitic nature of the coal, passes on to the "Boulder Drift (?)," which consists of immense boulders, principally of felspathic trap and greenstone, imbedded in stiff clay in some parts, and in loam in others. The boulders are also associated with fragments of New Red Sandstone and nodular masses of Dolomite.

The author concludes by describing the mode of occurrence, in the valley of the Derwent, of a marine deposit which he considers of Postpliocene age, and which is found at an elevation of upwards of 100 ft. above the sea-level, and at a distance of from 50 to 100 yards from the water's edge—thus showing that the valley of the Derwent and the neighbouring country had been recently upheaved.

XXXVIII. *Intelligence and Miscellaneous Articles.*

ON THE EBULLITION OF WATER, AND ON THE EXPLOSION OF STEAM-BOILERS. BY M. L. DUFOUR.

GASES, it is known, tend eminently to promote the vaporization of liquids with which they are in contact. But the superficial gaseous layer which adheres to solids, acting at first like gases themselves, is gradually removed by prolonged and successive heating. When the solid surfaces are deprived of it, they no longer by their contact excite changes of condition, but become indifferent in the liquid.

What confirms this view is the circumstance that, by maintaining or producing on the surface of bodies a gaseous layer, ebullition of a liquid is immediately produced if the temperature is suitable, and any retardation of ebullition is avoided. The following experiment realizes these conditions. Two platinum wires, communicating with the outside, pass through a cork in which a thermometer fits, and dip in water. They are connected with the two poles of a galvanic element, and a slight disengagement of gas, due to electrolysis, takes place on their surface. Under these circumstances, and so long as the current passes, it is impossible to obtain the least retardation of boiling. If these wires cease to be connected with a battery, after some successive heatings and by diminishing the superficial pressure, retardations are produced similar to those mentioned above. If the current is then made to pass, ebullition is immediately produced. If the retardation is considerable (from 15 to 20 degrees), closing the circuit produces so abundant a production of vapour as to resemble a true explosion. The vapour appears to break away with an effort from the liquid mass, and the vessel experiences concussions almost strong enough to break it. This experiment, which has frequently succeeded in my hands with ordinary water, is more striking in the case of slightly acidulated water, for then the retardations are more pronounced.

It is therefore, I think, a property of water to tend in most cases to retain the liquid state, even when ebullition ought to take place, provided the boiling-point has been reached by a diminution of the superficial pressure after the liquid has been already heated, and after it has been in contact for some time with the solid substances of the vessel. This property is perhaps not without interest in its application to the explosions of steam-boilers. This formidable phenomenon is still enveloped in much obscurity. Various attempts have been made to explain it; among others, by saying that in a perfect calm, while the issue of vapour is suspended, everything being motionless in the apparatus, and all the dissolved air expelled, the water may accidentally become heated beyond the point corresponding to its pressure, and then if ebullition sets in, it suddenly furnishes a mass of vapour which breaks the envelopes. But the embarrassing circumstance, and the one found in most cases, is that the accident takes place without the heating having been continued, while the workmen and the machine were at rest, and when, from cooling, the pressure in the machine had diminished. These conditions, almost always mentioned with surprise in these accidents, exhibit an undoubted analogy with the experiments which I have described. Is it not possible that at a moment of repose, and while the heating has been discontinued, the cooling which sets in at first diminishes the pressure of vapour existing in the boiler? As water, in virtue of its great specific heat, cools very slowly, it retains for a longer time a temperature which ought to produce ebullition under this diminished pressure. This ebullition doubtless takes place most frequently in proportion as the diminution of pressure permits; but it may happen that, under exceptional circumstances, a retardation similar to that described above is produced, and then after a longer or shorter delay ebullition sets in, either spontaneously, or in consequence of some foreign disturbance. This ebullition ought to manifest the characters many times observed in my apparatus, where the concussions raised the heavy support to which the retort was fixed. From the large quantity of water contained in a boiler, these strokes might well cause a fracture of the sides, and the disastrous effects of this kind of accidents.

The explanation which I attempt to give accounts, it is seen, for a boiler-explosion, even when heating has ceased, when all the machine is in a state of cooling, and the pressure has been diminished. Comparing the details ordinarily noted in this kind of explosion with the conditions of the experiment above mentioned, it is impossible not to observe a striking analogy, if the hints above given are correct; and it would remain to find out the means of preventing these deplorable accidents. No solid body by its contact seemed to me to determine ebullition with certainty at the desired point; and all of them at length and by repeated heating become inactive. Contact of gases, on the contrary, invariably provokes ebullition as soon as the temperature makes it possible. Hence, as M. Donny has already said, it is desirable permanently to produce gases in the interior of the boiler. Wires or platinum plates which dip in the water, and by which enters the current of even a feeble battery, would very probably be sufficient to prevent retardations of ebullition.

P.S.—Since writing this Note, I have read (*Cosmos*, April 7, 1864) of a fact which agrees very well with this proposed theory of the explosion of boilers.

This is the explosion at Aberdare, where two boilers burst. The water supplied appeared to contain a little sulphuric acid. Some pieces of the sides presented by Mr. Fairbairn to the Manchester

Philosophical Society were deeply corroded from chemical action. The explosion has naturally been attributed to this attack of the sides by the acid, and doubtless an acidulated liquid ought to attack the sides.

Now we know that sulphuric acid, even in very small quantity, imparts to water the property of undergoing retardations of ebullition much more considerable and much more frequent than those of pure water. If, then, boiler explosions arise from a retardation in the ebullition of water when the pressure diminishes in the boiler, as I explain in my Note, it is seen that the two accidents in England are easily explained, inasmuch as the feeding water contained a little acid.—*Comptes Rendus*, June 6, 1864.

APPLICATION OF ZEIODELITE.

Zeiodelite is a mixture prepared by melting together 20 to 30 parts of roll sulphur with 24 parts of powdered glass or pumice, and which forms a mass as hard as stone, that resists the action of water and of the strongest acids. Prof. R. Böttger recommends it, therefore, for making water- and air-tight cells for galvanic batteries.—Poggendorff's *Annalen*, July 1864.

DETERMINATIONS OF TEMPERATURE IN THE DEPTH OF SOME BAVARIAN MOUNTAIN LAKES.

In the *Sitzungsberichte* of the Royal Bavarian Academy of 1862, Prof. Jolly describes a bathometer and aminimum thermometer of his invention, and gives some observations of the temperature at various depths in the Königssee, the Obersee, and the Walchensee, which may find a place here.

Depth in metres.	Temperature in degrees C.	Depth in metres.	Temperature in degrees C.
Königssee, 1862, Aug.		Obersee, 1862, Sept.	
0	14°·9–15°·2	0	15°·1
22·6	7·89	27·1	7·55
26·8	6·61	31·4	9·12
37·8	6·58	62·3	6·59
67·2	6·00		
95·5	5·83	Walchensee, 1862, Oct.	
104·3	5·81		
153·3	5·38	0	15·0
163·2	5·50	58·3	6·76
198·0	5·44	97·6	6·07
204·1	5·52	98·6	6·12
216·5	5·34	107·0	5·91
		248·8	5·17

Hence in these lakes (as in those of Switzerland) the temperature approaches, without actually attaining, that of the maximum density of water (from inadequate depth), and without following any regular progress in its decrease.—Poggendorff's *Annalen*, August 1864.

ON THE METEORITE OF ALBARETO IN THE MODENESE.

BY DR. W. HAIDINGER*.

This meteorite, and the pamphlet by the Jesuit Dominico Troili describing it, have been mentioned by Chladni (1798 and 1819), by Ende (1804), and by Sir D. Brewster (Edinburgh Journal of Science, 1819). Chladni, whose careful inquiries at Modena in 1819 could not make out any trace of this stone, thought it definitively lost. But lately a specimen of it was found to exist in the University Museum of Modena; and of this, Dr. Hörnes, kindly assisted by Messrs. Greg, Senoner, Bianconi, and Bombici, obtained for the Imperial Museum of Vienna a fragment of 13·31 grammes in weight. It is tufaceous in aspect, dark grey, with numerous globular concretions—some greenish grey (as the Piddingtonite of Skalka, or the Chladnite of Bishopville), others dark grey or black, one of them conspicuous for its less density, yellowish-grey tint, dark-brown crust, and atoms of native iron disseminated through it. The particles of native and protosulphuretted iron irregularly distributed through the whole mass are sometimes discernible to the unaided eye; in one place two brownish-black globules are united by metallic iron in such a way as to allow us to suppose the group to be a fragment of a larger piece of native iron including globules of silicates, like the Himalayan iron. The globules are easily detached from the surrounding mass. The outer surface, offering the impressions common to all meteorites, is covered, on a surface of about 25 square lines, with a blackish-brown, nearly opake crust. In general aspect the Albareto meteorite stands next to those of Benares, Trenzano, and Weston. Its density, at 15° R., is 3·344. The sulphuretted iron of the meteorites, generally passing under the denomination of magnetic iron-pyrites, is, according to Prof. Rammelsberg, a mechanical compound of protosulphuretted iron (75·37 per cent.), sulphuretted copper (0·71 per cent.), chromate of iron (2·83 per cent.), and nickeliferous iron (19·83 per cent.), of 4·787 density, yellowish brown, soluble in acids without residuous sulphur, and magnetic in consequence of the nickel contained in it. The sulphuretted iron, in its state of purity, as it occurs in the Garnallee meteorite, in grains of the size of a pea, is, according to Prof. Wöhler, a combination of 1 atom of iron with 1 atom of sulphur ($Fe\,S$ or iron 63·64, sulphur 36·36). For this sulphuret, hitherto not known to exist among the minerals composing the terrestrial crust, Dr. Haidinger proposes the denomination of Troïlite (commemorative of the first describer of the Albareto meteorite), and the following mineralogical characters:—amorphous, in minute particles, disseminated through the lithoid substance of meteorites, metallic brightness, bronze-brown, streak black, hardness 4, density 4·5–4·6; chemical formula, $Fe\,S$. According to Troili's pamphlet (Modena, 1766), the meteorite in question may have originally had a weight of about 25 lbs. It fell in the middle of July 1766, 5^{h} P.M., the sky being serene, but covered westward with heavy clouds, with frequent thunder and lightning. Witnesses assert its fall to have been preceded by a sound resem-

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* Communicated by Count Marschall.

bling a cannonade and the hissing of a cannon-ball through the air; some describe it as having been in a state of incandescence, while others saw it dark and smoking. It penetrated into the ground to the depth of less than a "braccio" (about $2\frac{1}{4}$ Vienna feet), and was dug out still hot, spreading a sulphurous smell, and covered with a crust. Troili, although quite uncertain as to the nature of the phenomenon, which he ascribes to a subterraneous commotion having thrown the stone into the air, whence it fell again to the ground, was evidently highly anxious to state its reality and every circumstance concerning it. This, at a time when the scepticism about these phenomena was such that anyone who asserted their reality could only expect incredulity and even ridicule, gave a most meritorious proof of moral courage. Supposing the Albareto meteorite to have fallen to the ground in a nearly vertical direction, and to have come from the west (as did the sounds preceding its fall), its point of departure may be traced to the constellation of Leo, well known to be the point from which the falling stars of the November epoch proceed. Its trajectory may have been a segment of the elliptical orbit of a whole swarm of bodies moving within the sphere of terrestrial attraction on the branch of a hyperbolic orbit through cosmical space. Dr. Haidinger on this occasion recalled to mind his hypothesis on the cause of the high temperature in meteoric masses—passing through the terrestrial atmosphere and generating heat by rapid compression, in the same way as in Prof. Mallet's experiment, successfully repeated before the Academy of Paris in 1803. Of late years (1840–57) the experimental researches of Messrs. Bianconi, Thomson, Joule, and Tyndall have shown that the temperature of a thin string of water rapidly forced through a narrow spiral tube rises from 1° F. to 4° F., that a solid body surrounded by a rapid air-current was more heated than the ambient air, and that, if the rapidity of the air-current be brought to 1780 feet in a second, this difference of temperature may be raised to 137°. Even in an atmosphere rarefied to the utmost limits, any solid progressing within it at the rate of meteorites (6–30 miles a second) would come to a far higher temperature, still increased by the *transformation of active forces* (light, electricity, magnetism, &c.) into heat, in consequence of the resistance opposed to the rapid career of such a body. Prof. Bunsen, in a note on the Meteoric Iron of Atacama (Leonhard and Bronn's *Jahrbuch*, 1857, p. 265), calculated the loss of active force during the fall of a solid coming into the terrestrial atmosphere with a planetary celerity to be sufficient to heat it to 1,000,000° C. Supposing $\frac{998}{1000}$ of this heat to be lost in the ambient medium, such a body would still touch the ground with a temperature of 2000° C.

What Dr. Haidinger has done for meteorites, Prof. Tyndall has ascertained for hailstones—the existence of a facial plane with incipient fusion in consequence of the condensation of the air, and of a dorsal one, on which the rarefaction of the air has caused the congelation of atmospheric water. Similar circumstances have been observed by Prof. Göth, on hailstones fallen at Gratz in the summer of 1846 (*Wiener Naturwissenschaftliche Abhandlungen*, published by Haidinger, vol. i. p. 91).—*Imp. Acad. Sc. Vienna*, March 27, 1864.

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XXXIX. *On Luminous and Obscure Radiation.*

By JOHN TYNDALL, *F.R.S., &c.*[*]

1. SIR WILLIAM HERSCHEL discovered the obscure rays of the sun, and proved that the position of maximum heat was beyond the red of the solar spectrum[†]. Forty years subsequently Sir John Herschel succeeded in obtaining a thermograph of the calorific spectrum, and in giving striking visible evidence of its extension beyond the red[‡]. Melloni proved that an exceedingly large proportion of the emission from a flame of oil, of alcohol, and from incandescent platinum heated by a flame of alcohol, is obscure[§]. Dr. Akin inferred from the paucity of luminous rays evident to the eye, and a like paucity of extra-violet rays, as proved by the experiments of Dr. Miller, that the radiation from a flame of hydrogen must be mainly extra-red; and he concluded from this that the glowing of a platinum wire in a hydrogen-flame, as also the brightness of the Drummond light in the oxyhydrogen-flame, was produced by a change in the period of vibration[||]. By a different mode of reasoning I arrived at the same conclusion myself, and published the conclusion subsequently[¶].

2. A direct experimental demonstration of the character of the radiation from a hydrogen-flame was, however, wanting, and this want I have sought to supply. I had constructed for me, by Mr. Becker, a complete rock-salt train of a size sufficient to

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[*] Communicated by the Author.

[†] Phil. Trans. 1800.

[‡] Phil. Trans. 1840. I hope very soon to be able to turn my attention to the remarkable results described in Note III. of Sir J. Herschel's paper.

[§] *La Thermochrose*, p. 304.

[¶] Phil. Trans. vol. cliv. p. 327.

[||] Reports of the British Association, 1863.

permit of its being substituted for the ordinary glass train of a Duboscq's electric lamp. A double rock-salt lens placed in the camera rendered the rays parallel; the parallel rays then passed through a slit, and a second rock-salt lens placed without the camera produced, at an appropriate distance, an image of this slit. Behind this lens was placed a rock-salt prism, while laterally stood a thermo-electric pile intended to examine the spectrum produced by the prism. Within the camera of the electric lamp was placed a burner with a single aperture, so that the flame issuing from it occupied the position usually taken up by the coal points. This burner was connected with a T-piece, from which two pieces of india-rubber tubing were carried, the one to a large hydrogen-holder, the other to the gas-pipe of the laboratory. It was thus in my power to have, at will, either the gas-flame or the hydrogen-flame. When the former was employed, I had a visible spectrum, which enabled me to fix the thermo-electric pile in its proper position. To obtain the hydrogen-flame, it was only necessary to turn on the hydrogen until it reached the gas-flame and was ignited; then to turn off the gas and leave the hydrogen-flame behind. In this way, indeed, the one flame could be substituted for the other without opening the door of the camera, or producing any change in the positions of the source, the lenses, the prism, and the pile.

3. The thermo-electric pile employed is a beautiful instrument constructed by Ruhmkorff. It belongs to my friend Mr. Gassiot, and consists of a single row of elements properly mounted and attached to a double brass screen. It has in front two silvered edges, which, by means of a screw, can be caused to close upon the pile so as to render its face as narrow as desirable, reducing it to the width of the finest hair, or, indeed, shutting it off altogether. By means of a small handle and long screw, the plate of brass and the pile attached to it can be moved gently to and fro, and thus the vertical slit of the pile can be caused to traverse the entire spectrum, or to pass beyond it in both directions. The width of the spectrum was in each case equal to the length of the face of the pile, which was connected with an extremely delicate galvanometer.

4. I began with a luminous gas-flame. The spectrum being cast upon the brass screen (which, to render the colours more visible, was covered with tinfoil), the pile was gradually moved in the direction from blue to red, until the deflection of the galvanometer became a maximum. To reach this it was necessary to pass entirely through the spectrum and a little way beyond the red; the deflection then observed was

30°.

When the pile was moved in either direction from this position, the deflection diminished.

5. The hydrogen-flame was now substituted for the gas-flame; the visible spectrum disappeared, and the deflection fell to

12°.

Hence, as regards rays of this particular refrangibility, the emission from the luminous gas-flame was two-and-a-half times that from the hydrogen-flame.

6. The pile was now moved to and fro, and the movement in both directions was accompanied by a diminished deflection. Twelve degrees, therefore, was the maximum deflection for the hydrogen-flame; and the position of the pile, determined previously by means of the luminous flame, proves that this deflection was produced by extra-red undulations. I moved the pile a little forwards, so as to reduce the deflection from 12° to 4°, and then, in order to ascertain the refrangibility of the rays which produced this small deflection, I relighted the gas. The rectilinear face of the pile was found invading the red. When the pile was caused to pass successively through positions corresponding to the various colours of the spectrum, and to its extra-violet rays, no measurable deflection was produced by the hydrogen-flame.

7. I next placed the pile at some distance from the invisible spectrum of the flame of hydrogen, and *felt* for the spectrum by moving the pile to and fro. Having found it, I without difficulty ascertained the place of maximum heating. Changing nothing else, I substituted the luminous flame for the non-luminous one; the position of the pile when thus revealed, was beyond the red.

8. It is thus proved that the radiation from a hydrogen-flame is sensibly extra-red. The other constituents of the radiation are so feeble as to be thermally insensible. Hence, when a body is raised to incandescence by a hydrogen-flame, the vibrating periods of its atoms must be shorter than those to which the radiation of the flame itself is due.

9. The falling of the deflection from 30° to 12° when the hydrogen-flame was substituted for the gas-flame is doubtless due to the absence of all solid matter in the former. We may, however, introduce such matter, and thus make the radiation originating in the hydrogen-flame much greater than that of the gas-flame. A spiral of platinum wire plunged in the former gave a maximum deflection of

52°

at a time when the maximum deflection of the gas-flame was only

33°.

10. It is mainly by convection that the hydrogen-flame disperses its heat: though its temperature is higher, its sparsely-scattered molecules are not able to cope, in radiant energy, with

the solid carbon of the luminous flame. The same is true for the flame of a Bunsen's burner; the moment the air (which destroys the solid carbon-particles) mingles with the gas-flame, the radiation falls considerably. Conversely, a gush of radiant heat accompanies the shutting out of the air which deprives the gas-flame of its luminosity. When, therefore, we introduce a platinum wire into a hydrogen-flame, or carbon-particles into a Bunsen's flame, we obtain not only waves of a new period, but also convert a large portion of the heat of convection into the heat of radiation.

11. The action was still very sensible when the distance of the pile from the red end of the spectrum on the one side was as great as that of the violet rays on the other, the heat-spectrum thus proving itself to be at least as long as the light-spectrum.

12. Bunsen and Kirchhoff have proved that, for incandescent metallic vapours, the period of vibration is, within wide limits, independent of temperature. My own experiments with flames of hydrogen and carbonic oxide as sources, and with cold aqueous vapour and cold carbonic acid as absorbing media, point to the same conclusion*. But in *solid* metals augmented temperature introduces waves of shorter periods into the radiation. It may be asked, "What becomes of the long obscure periods when we heighten the temperature? Are they broken up or changed into shorter ones, or do they maintain themselves side by side with the new vibrations?" The question is worth an experimental answer.

13. A spiral of platinum wire suitably supported was placed within the camera of the electric lamp at the place usually occupied by the carbon points. This spiral was connected with a voltaic battery; and by varying the resistance to the current, it was possible to raise the spiral gradually from a state of darkness to an intense white heat. Raising it to a white heat in the first instance, the rock-salt train was placed in the path of its rays, and a brilliant spectrum was obtained. The pile was then moved into the region of obscure rays beyond the red of the spectrum. Altering nothing but the strength of the current, the spiral was reduced to darkness, and lowered in temperature till the deflection of the galvanometer fell to 1°. Our question is, "What becomes of the waves which produce this deflection when new ones are introduced by augmenting the temperature of the spiral?"

14. Causing the spiral to pass from this state of darkness through various degrees of incandescence, the following deflections were obtained:—

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* Phil. Trans. vol. cliv. p. 327.

TABLE I.

Appearance of spiral.	Deflection by obscure rays.
Dark	1°
Dark	6
Faint red	10·4
Dull red	12·5
Red	18·0
Full red	27·0
Bright red	44·4
Nearly white	54·3
Full white	60·0

15. The deflection of 60° here obtained is equivalent to 122 of the first degrees of the galvanometer. Hence the intensity of the obscure rays in the case of the full white heat is 122 times that of the rays of the same refrangibility emitted by the dark spiral used at the commencement. Or, as the intensity is proportional to the square of the amplitude, the *height* of the æthereal waves which produced the last deflection was eleven times that of the waves which produced the first. The *wave-length,* of course, remained the same throughout.

16. The experimental answer, therefore, to the question above proposed is, that the amplitude of the old waves is augmented by the same accession of temperature that gives birth to the new ones. The case of the obscure rays is, in fact, that of the luminous ones (of the red of the spectrum, for example), which glow with augmented intensity as the temperature of the radiant source is heightened.

17. In my last memoir[*] I demonstrated the wonderful transparency of the element iodine to the extra-red undulations. A perfectly opake solution of this substance was obtained by dissolving it in bisulphide of carbon, and it was shown in the memoir referred to that a quantity of iodine sufficient to quench the light of our most brilliant flames transmitted 99 per cent. of the radiation from a flame of hydrogen.

18. Fifty experiments on the radiant heat of a hydrogen-flame, recently executed, make the transmission of its rays, through a quantity of iodine which is perfectly opake to light,

100 per cent.

To the radiation from a hydrogen-flame the dissolved iodine is therefore, according to these experiments, *perfectly transparent.*

19. It is also sensibly transparent to the radiation from solid bodies heated under incandescence.

20. It is also sensibly transparent to the obscure rays emitted by luminous bodies.

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[*] Phil. Trans. vol. cliv. p. 327. [This memoir will appear in the December Number of the Philosophical Magazine.]

21. To the mixed radiation which issues from solid bodies at a very high temperature, the pure bisulphide of carbon is also eminently transparent. Hence, as the bisulphide of carbon interferes but slightly with the obscure rays issuing from a highly luminous source, and as the dissolved iodine seems not at all to interfere with them, we have in a combination of both substances a means of almost entirely detaching the purely thermal rays from the luminous ones.

22. If vibrations of a long period, established when the radiating body is at a low temperature, maintain themselves, as indicated in paragraph 14, side by side with the new periods which augmented temperature introduces, it would follow that a body once pervious to the radiation from any source must always remain pervious to it. We cannot so alter the character of the radiation that a body once in any measure transparent to it shall become quite opake to it. We may, by augmenting the temperature, diminish the percentage of the total radiation transmitted by the body; but inasmuch as the old vibrations have their amplitudes enlarged by the very accession of temperature which produces the new ones, the total quantity of heat of any given refrangibility transmitted by the body must increase with increase of temperature.

23. This conclusion is thus experimentally illustrated. A cell with parallel sides of polished rock-salt was filled with the solution of iodine, and placed in front of the camera within which was the platinum spiral. Behind the rock-salt cell was placed an ordinary thermo-electric pile, to receive such rays as had passed through the solution. The rock-salt lens was in the camera in front, but a small sheaf only of the parallel beam emergent from the lamp was employed. Commencing at a very low dark heat, the temperature was gradually augmented to full incandescence with the following results:—

TABLE II.

Appearance of spiral.	Deflection.
Dark	1°
Dark but hotter	3
Dark but still hotter . . .	5
Dark but still hotter . . .	10
Feeble red	19
Dull red	25
Red	35
Full red	45
Bright red	53
Very bright red	63
Nearly white	69
White	75
Intense white	80

24. To the luminous rays from the intensely white spiral the solution was perfectly opake; but though by the introduction of such rays the transmission, as expressed in parts of the total radiation, was diminished, the quantity absolutely transmitted was enormously increased. The value of the last deflection is 440 times that of the first; by raising therefore the platinum spiral from darkness to whiteness, we augment the intensity of the obscure rays which it emits in the ratio of 1 : 440.

25. A rock-salt cell filled with the *transparent* bisulphide of carbon was placed in front of the camera which contained the platinum spiral raised to a dazzling white heat. The transparent liquid was then drawn off and its place supplied by the solution of iodine. The deflections observed in the respective cases are as follows:—

Radiation from White-hot Platinum.

Through transparent CS^2.	Through opake solution.
73°·9	73°·0
73·8	72·9

All the luminous rays passed through the transparent bisulphide, *none* of them passed through the solution of iodine. Still we see what a small difference is produced by their withdrawal. The actual proportion of luminous to obscure, as calculated from the above observations, may be thus expressed:—

26. *Dividing the radiation from a platinum wire raised to a dazzling whiteness by an electric current into twenty-four equal parts, one of these parts is luminous and twenty-three obscure.*

27. A bright gas-flame was substituted for the platinum spiral, the top and bottom of the flame were shut off, and its most brilliant portion chosen as the source of rays. The result of forty experiments with this source may be thus expressed:—

28. *Dividing the radiation from the most brilliant portion of a flame of coal-gas into twenty-five equal parts, one of those parts is luminous and twenty-four obscure.*

29. I next examined the ratio of obscure to luminous rays in the electric light. A battery of fifty cells was employed, and the rock-salt lens was used to render the rays from the coal points parallel. To prevent the deflection from reaching an inconvenient magnitude, the parallel rays were caused to issue from a circular aperture 0·1 of an inch in diameter, and were sent alternately through the transparent bisulphide and through the opake solution. It is not easy to obtain perfect steadiness on the part of the electric light; but three experiments carefully executed gave the following deflections:—

Radiation from Electric Light.—Experiment No. I.

Through transparent CS^2.	Through opake solution.
72°·0	70°·0
Experiment No. II.	
76°·5	75°·0
Experiment No. III.	
77°·5	76°·5

Calculating from these measurements the proportion of luminous to obscure heat, the result may be thus expressed:—

30. *Dividing the radiation from the electric light emitted by carbon points, and excited by a Grove's battery of forty cells, into ten equal parts, one of those parts is luminous and nine obscure.*

31. The results may be thus presented in a tabular form:—

Table III.—Radiation through dissolved Iodine.

Source.	Absorption.	Transmission.
Dark spiral	0	100
Lampblack at 212° Fahr. .	0	100
Red-hot spiral	0	100
Hydrogen-flame	0	100
Oil-flame	3	97
Gas-flame	4	96
White-hot spiral	4·6	95·4
Electric light	10	90

Repeated experiments may slightly alter these results, but they are extremely near the truth.

32. Having thus in the solution of iodine found a means of almost perfectly detaching the obscure from the luminous heat-rays of any source, we are able to operate at will upon the former. Here are some illustrations:—The rock-salt lens was so placed in the camera that the coal points themselves and their image beyond the lens were equally distant from the latter. A battery of forty cells being employed, the track of the cone of rays emergent from the lamp was plainly seen in the air, and their point of convergence therefore easily fixed. The cell containing the opake solution was now placed in front of the lamp. The luminous cone was thereby entirely cut off, but the intolerable temperature of the focus, when the hand was placed there, showed that the calorific rays were still transmitted. Thin plates of tin and zinc were placed successively in the dark focus and speedily fused; matches were ignited, gun-cotton exploded, and brown paper set on fire. Employing the iodine solution and a battery of sixty of Grove's cells, all these results were readily obtained

with the ordinary glass lenses attached to Duboscq's electric lamp. They cannot, I think, fail to give pleasure to those who repeat the experiments. It is extremely interesting to observe in the middle of the air of a perfectly dark room a piece of black paper suddenly pierced by the invisible rays, and the burning ring expanding on all sides from the centre of ignition.

33. On the 15th of this month I made a few experiments on solar light. The heavens were not free from clouds, nor the London atmosphere from smoke, and at best I obtained only a portion of the action which a clear day would have given me. I happened to possess a hollow lens, which I filled with the concentrated solution of iodine. Placed in the path of the solar rays, a faint red ring was imprinted on a sheet of white paper held behind the lens, the ring contracting to a faint red spot when the focus of the lens was reached. It was immediately found that this ring was produced by the light which had penetrated the thin rim of the liquid lens. Pasting a zone of black paper round the rim, the ring was entirely cut off and no visible trace of solar light crossed the lens. At the focus, whatever light passed would be intensified nine hundredfold; still even here no light was visible.

34. Not so, however with the sun's obscure rays; the focus was burning hot. A piece of black paper placed there was instantly pierced and set on fire; and by shifting the paper, aperture after aperture was formed in quick succession. Gunpowder was also exploded. In fact we had in the focus of the sun's dark rays a heat decidedly more powerful than that of the electric light similarly condensed, and all the effects obtained with the former could be obtained in an increased degree with the latter.

35. I introduced a plano-convex lens of glass, larger than the opake lens just referred to, into the path of the sun's rays. The focus on white paper was of dazzling brilliancy; and in this focus the results already described were obtained. I then introduced a cell containing a solution of alum in front of the focus. The intensity of the *light* at the focus was not sensibly changed; still these almost intolerable visual rays, aided as they were by a considerable quantity of invisible rays which had also passed through the alum, were incompetent to produce effects which were obtained with ease in the perfectly dark focus of the opake lens.

36. Thinking that this reduction of power might be due to the withdrawal of heat by reflexion from the sides of the glass cell, I put in its place a rock-salt cell filled with the opake solution. Behind this cell the rays manifested the power which they exhibited in the focus of the opake lens.

37. The rendering of metals incandescent by obscure rays has not yet been accomplished. This is a question on which Dr. Akin has been engaged for some years, and it is not my intention to publish anything relating to it until the very promising arrangements which he has devised have had a sufficient trial.

38. Melloni's experiments led him to conclude that rock-salt transmits obscure and luminous rays equally well, and that a solution of alum of moderate thickness entirely intercepts the invisible rays, while it allows all the luminous ones to pass. Hence the difference between the transmissions of rock-salt and alum ought to give the obscure radiation. In this way Melloni found that 10 per cent. only of the radiation from an oil-flame consists of luminous rays. The method above employed proves that the proportion of luminous heat to obscure, in the case of an oil-flame, is probably not more than one-third of what Melloni made it.

39. In fact this distinguished man clearly saw the possible inaccuracy of the conclusion that none but luminous rays are transmitted by alum; and the following experiments justify the clauses of limitation which he attached to his conclusion:—

The solution of iodine was placed in front of the electric lamp, the luminous rays being thereby intercepted. Behind the rock-salt cell containing the opake solution was placed a glass cell, empty in the first instance. The deflection produced by the obscure rays which passed through both produced a deflection of

80°.

The glass cell was now filled with a concentrated solution of alum; the deflection produced by the obscure rays passing through both solutions was

50°.

Calculating from the values of these deflections, it was found *that of the obscure heat emergent from the solution of iodine, and from the side of the glass cell,* 20 *per cent. was transmitted by the alum.*

40. A point of very considerable importance forces itself upon our attention here—namely the vast practical difference which may exist between the two phrases, "obscure rays," and "rays from an obscure source." Many writers seem to regard these phrases as equivalent to each other, and are thus led into grave errors. A stratum of alum solution $\frac{1}{25}$th of an inch in thickness is, according to Melloni, entirely opake to the radiation from all bodies heated under incandescence. In the foregoing experiments the layer of alum solution traversed by the obscure rays of our luminous source was thirty times the thickness of the layer which

Melloni found sufficient to quench all rays emanating from obscure sources.

41. There cannot be a doubt that the invisible rays which have shown themselves competent to traverse such a thickness of the most powerful adiathermic liquid yet discovered are also able to pass through the humours of the eye. The very careful and interesting experiments of M. Janssen*, prove that the humours of the eye absorb an amount of radiant heat exactly equal to that absorbed by a layer of water of the same thickness, and in our solution the power of alum is added to that of water. Direct experiments on the vitreous humour of an ox lead me to conclude that one-fifth of the obscure rays emitted by an intense electric light reaches the retina; and inasmuch as in every ten equal parts of the radiation from an electric lamp nine consist of obscure rays, it follows that, in the case of the electric light, nearly two-thirds of the whole radiant energy which actually reaches the retina is incompetent to excite vision. With a white-hot platinum spiral as source, the mean of four good experiments gave a transmission of 11·7 per cent. of the obscure heat of the spiral through a layer of distilled water 1·2 inch in thickness. A larger proportion no doubt reaches the retina†.

42. Converging the beam from the electric lamp by a glass lens, I placed the opake solution of iodine before my open eye, and brought the eye into the focus of obscure rays; the heat was immediately unbearable. But it seemed to me that the unpleasant effect was mainly due to the action of the obscure rays upon the eyelids and other opake parts round the eye. I therefore cut, in a card, an aperture somewhat larger than the pupil, and allowed the concentrated calorific beam to enter my eye through this aperture. The sense of heat entirely disappeared. Not only were the rays thus received upon the retina incompetent to excite vision, but the optic nerve seemed unconscious of their existence even as heat. What the consequences would have been had I permitted the luminous third of the condensed beam to enter my eye, I am not prepared to say, nor should I like to make the experiment.

43. On a tolerably clear night a candle-flame can be readily seen at the distance of a mile. The intensity of the electric light used by me is 650 times that of a good composite candle, and as the non-luminous radiation from the coal points which reaches the retina is equal to twice the luminous, it follows that at a common distance of a foot, the energy of the invisible rays

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* *Annales de Chimie et de Physique*, tom. lx. p. 71.

† M. Franz has shown that a portion of the sun's obscure rays reach the retina.

of the electric light which reach the optic nerve, but are incompetent to provoke vision, is 1300 times that of the light of a candle. But the intensity of the candle's light at the distance of a mile is less than one twenty-millionth of its intensity at the distance of a foot, hence the energy which renders the candle perfectly visible a mile off would have to be multiplied by 1300 × 20,000,000, or by twenty-six thousand millions, to bring it up to the intensity of that powerless radiation which the eye receives from the electric light at a foot distance. Nothing, I think, could more forcibly illustrate the special relationship which subsists between the optic nerve and the oscillating periods of luminous bodies. The nerve, like a musical string, responds to the periods with which it is in accordance, while it refuses to be excited by others of vastly greater energy which are not in unison with its own.

44. By means of the opake solution of iodine, I have already shown that the quantity of luminous heat emitted by a bright red platinum spiral is immeasurably small *. Here are some determinations since made with the same source of heat and a solution of iodine in iodide of ethyle, the strength and thickness of the solution being such as entirely to intercept the luminous rays.

Radiation from Red-hot Platinum Spiral.

Through transparent liquid.	Through opake solution.
43°·7	43°·7
43·7	43·7

These experiments were made with exceeding care, and all the conditions were favourable to the detection of the slightest difference in the amount of heat reaching the galvanometer; still the quantity of heat transmitted by the opake solution was found to be the same as that transmitted by the transparent one. In other words, the luminous radiation intercepted by the former, though competent to excite vividly the sense of vision, was, when expressed in terms of actual energy, absolutely immeasurable.

45. And here we have the solution of various difficulties which from time to time have perplexed experimenters. When we see a vivid light incompetent to affect our most delicate thermoscopic apparatus, the idea naturally presents itself that light and heat must be totally different things. The pure light emerging from a combination of water and green glass, even when rendered intense by concentration, has, according to Melloni, no sensible heating power †. The light of the moon is also a case in point. Concentrated by a polyzonal lens more than a yard in diameter

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* Phil. Trans. vol. cliv. p. 327.

† Taylor's Scientific Memoirs, vol. i. p. 392.

upon the face of his pile, it required all Melloni's acuteness to *nurse* the calorific action up to a measurable quantity. Such experiments, however, demonstrate, not that the two agents are dissimilar, but that the sense of vision can be excited by an amount of force almost infinitely small.

46. Here also we are able to offer a remark as to the applicability of radiant heat to fog-signalling. The proposition, in the abstract, is a philosophical one; for were our fogs of a physical character similar to that of the iodine held in solution by the bisulphide of carbon, or to that of iodine or bromine vapour, it would be possible to transmit through them powerful fluxes of radiant heat, even after the entire stoppage of the light from our signal lamps. But our fogs are not of this character. They are unfortunately so constituted as to act very destructively upon the purely calorific rays; and this fact, taken in conjunction with the marvellous sensitiveness of the eye, leads to the conclusion that long before the *light* of our signals ceases to be visible, their radiant heat has lost the power of affecting, in any sensible degree, the most delicate thermoscopic apparatus that we could apply to their detection.

Royal Institution, October 1864.

XL. *On Evansite, a new Mineral Species.*

By David Forbes, *F.R.S., &c.*[*]

THIS mineral was brought from Hungary in the year 1855 by the late Mr. Brooke Evans of Birmingham[†], and was then reported to be found in some abundance as an incrustation in drusic cavities which occurred in the brown iron ores. It was regarded as pertaining to the mineral species allophane[‡], with which it agrees in many of its physical properties, as hardness, colour, specific gravity, &c., as well as in the percentage of loss sustained upon heating the mineral to redness.

The specimen I received from Mr. Evans was labelled Allophane from Zsetcznik, Gomar Comitat, and was very beautiful in appearance, consisting of an agglomeration of small stalactites with reniform and globular excrescences on brown hæmatite, many of these excrescences much resembling artificial or natural pearls, having both the figure and characteristic pearly lustre of such.

I doubted the identity of the mineral with allophane; and a

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[*] Communicated by the Author.

[†] After whom the species is now named.

[‡] A considerable number of specimens had been given by Mr. Evans to private collections in England all labelled "allophane," and I understand that many more had likewise been distributed in Germany under the same name.

preliminary blowpipe examination immediately confirmed this opinion by proving the absence of silica in any quantity, and indicating the presence of phosphoric acid; and consequently I was more disposed to regard it as hydrargyllite or Gibbsite. I commenced, however, a systematic examination of the mineral, but my sudden departure and prolonged absence in South America has prevented my having had an opportunity of making the results public until my recent return.

The physical characters of Evansite are as follows:—Amorphous and without trace of crystallization; reniform or botryoidal; colourless or milk-white, and sometimes faintly tinged with yellow or blue, and occasionally presenting iridescent hues; streak white; translucent to semi-opake. Lustre, vitreous or resinous; splendid and waxy internally; very brittle. Fracture semiconchoidal and shining.

Hardness 3·5 to 4, scratching calc-spar with facility but not fluor-spar; one fragment, however, was found to leave a faint mark on fluor-spar.

Specific gravity. Several determinations were carefully made, and precautions were taken to expel all air from between the laminæ of the mineral by using boiling distilled water and allowing it to cool down to the temperature of 60° Fahr.; the results were as follows:—

1. Using 28·51 grains of the translucent colourless mineral in small fragments, the loss in water was found to be 15·59 grains, and the consequent specific gravity 1·822.

2. With 13·686 grains similar to last, the loss in water was 7·31 grains, and the calculated specific gravity consequently 1·872.

3. With 12·87 grains of faint-yellow-coloured mineral in fragments, the loss obtained was 6·13 grs., and the consequent specific gravity would be 2·099.

4. When 18·793 grains of semiopake mineral in one piece was immersed under water, it lost 9·55 grains, and consequently had a specific gravity of 1·965.

The mean of these four determinations will give 1·939 as the specific gravity of Evansite.

The behaviour of this mineral before the blowpipe was found to be as follows:—

In a closed tube it immediately evolved water, decrepitated, and, on continued application of heat, gave off more water and remained behind in the form of a milk-white powder. On testing, the water evolved did not show any reaction with Brazil wood, red or blue litmus, or turmeric test papers.

In an open tube the same reactions were observed. Heated between platinum points it very slightly swelled out, became of a milk-white colour, and presented, when viewed through the glass, an innumerable series of minute cracks; did not fuse in the strongest heat; appeared to colour the outer flame bluish green, but so feebly as to be all but indistinct. On moistening the mineral with sulphuric acid, this reaction was rendered rather more apparent. On charcoal it proved infusible and unaltered, in both oxidizing and deoxidizing flames; but when heated, after moistening it with a solution of nitrate of cobalt, an intense blue colour was communicated to the assay. It dissolved readily both in borax-glass and phosphate of soda* in the oxidating flame, forming colourless glasses, which remain colourless on cooling: some of the faint-yellow-coloured specimens give a very light-coloured yellow glass when hot, but, on cooling, become colourless, a reaction due to the presence of iron. In the reducing-flame both these fluxes give the same reactions. In a few cases the glass formed by phosphate of soda shows a trace apparently of silica floating in the clear glass bead.

A qualitative chemical examination showed the mineral to be completely soluble in sulphuric, nitric, and hydrochloric acids. The solution, when treated by a stream of sulphuretted hydrogen gas passing through it, gave no precipitate whatever. The acid solution gave a yellow precipitate, indicative of phosphoric acid, when treated with molybdate of ammonia; and further, alumina and a trace of oxide of iron were found, but no lime, glucina or zirconia, which were specially tested for.

Fluorine was examined for by treating 12·10 grains in a platinum crucible with sulphuric acid at a gentle heat, the crucible being at the same time covered with a glass plate waxed on the under side and kept cold on the upper side; some characters were traced through the wax with a fine point; no visible etching was remarked after the operation.

The mineral, therefore, consisted only of water, alumina, and phosphoric acid with an accidental trace of oxide of iron and silica. Its quantitative analysis was conducted as follows:—

Determination of the Water.

22·22 grs. of the transparent colourless mineral left, after heating to redness, 13·49 grs. residue; also evolved 8·73 grs. grs. water, equivalent to 39·285 per cent. water in the mineral.

15·38 grs., same quality, left under same treatment 8·93 residue; also 6·45 grs. water, equivalent to 41·18 per cent.

13·365 grs., translucent but of a faint yellow colour, left 8·105 grs. residue; also 5·26 grs. water, which would make 39·37 per cent.

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* Instead of, as commonly, using microcosmic salt (phosphate of soda and ammonia), I prefer employing the dried phosphate of soda prepared by heating strongly the above until all ammonia is driven off. It will be found much more convenient in practice, as it melts gently, and does not froth and spit as the microcosmic salt does.

24·877 grs., translucent and colourless, heated in a water-bath at 212° Fahr. for twenty hours, left 20·25 grs. residue, being 4·627 grs. water, or equal to 13·69 per cent. water given off at 212° Fahr.; on further heating to redness left 14·94 grs. residue, thus giving a total of 5·31 grs. water, or equivalent to 39·91 per cent.

The average of these four experiments affords 39·945 per cent. water.

Determination of the Insoluble Matter (Silica).

18·07 grs. of the mineral were dissolved in hydrochloric acid with addition of a little nitric acid; some flakes remained persistently insoluble, and were collected on a filter, washed, dried and incinerated, and weighed 0·250 gr., or equal to 1·39 per cent.

13·365 grs. of the translucent but yellow-coloured mineral, after having been previously ignited to determine the amount of water present, were now dissolved in nitrohydrochloric acid; the insoluble residue collected on a filter, washed, and determined after incineration, weighed 0·46 gr., or equivalent to 3·44 per cent. I satisfied myself, however, that this result is quite erroneous and much too high, owing to a part of the phosphate of alumina in the mineral becoming itself insoluble, through the previous heating it had been submitted to in determining the percentage of water in it.

Determination of the Phosphoric Acid.

22·22 grs. of the white translucent mineral were dissolved in nitrohydrochloric acid, and to the solution an excess of a solution of molybdate of ammonia, previously rendered strongly acid by addition of nitric acid in large excess, was added until all phosphoric acid present was precipitated in the form of the yellow phosphomolybdate of ammonia. After filtration this precipitate was dissolved in ammonia, and the solution then precipitated by adding a mixed solution of sulphate of magnesia, chloride of ammonium, and caustic ammonia. The precipitate of phosphate of ammonia and magnesia was allowed to stand for twelve hours, then filtered off, washed with ammonia-water, and determined on ignition, affording 6·40 grs. pyrophosphate of magnesia, equivalent to 4·09 grs. phosphoric acid, or 18·42 per cent. phosphoric acid in the mineral.

Another estimation of the phosphoric acid in the mineral was made by Girard's modification of Reynoso's process, as follows:—

15·38 grs. were dissolved in nitric acid, and 22 grs. of metallic tin then added to the solution and boiled until entirely oxidized; the solution was then filtered off, and the insoluble oxide and phosphate of tin dissolved in excess of sulphide of ammonium by digestion; the solution was filtered from some little insoluble residue, and then precipitated by the addition of a previously mixed solution of sulphate of magnesia, chloride of ammonium, and ammonia in excess, allowed to stand twelve hours, and the precipitated phosphate of ammonia and magnesia then filtered off and determined as in the last case; the pyrophosphate of magnesia amounted to 4·605 grs., equivalent to 2·944 grs. phosphoric acid, or 19·01 per cent.

A third determination of the phosphoric acid was now made upon 13·365 grs. dissolved in hydrochloric acid, some 50 grs. crystallized tartaric acid added, and then ammonia in excess; the solution remained clear, and was then precipitated by a mixed solution of sulphate of magnesia, chloride of ammonium, and liquid ammonia, and allowed to stand twelve hours. The supernatant solution was now carefully decanted, and the precipitate redissolved in hydrochloric acid, a little tartaric acid added, and then ammonia in excess: after standing twelve hours the precipitated phosphate of ammonia and magnesia was collected and determined as usual; the pyrophosphate of magnesia weighed 4·14 grs., equivalent to 2·63 grs. phosphoric acid, or 19·73 per cent. in the mineral. The mean of these three determinations of phosphoric acid will consequently amount to 19·05 per cent.

Determination of the Alumina.

22·22 grs. (the same as employed as before mentioned in determining the phosphoric acid by the molybdate-of-ammonia method) were here made use of, and the solution, after separating the precipitate of phosphomolybdate of ammonia, was now subjected to the action of a stream of sulphuretted hydrogen gas until no more precipitate of sulphide of molybdenum fell; it was then filtered from this precipitate, and the solution, after boiling to remove any excess of the gas, precipitated by ammonia, by which the alumina present was thrown down, which, being washed, dried, and incinerated, weighed 8·90 grains, or consequently 40·05 per cent. in the mineral.

Another determination of the alumina was made on the quantity of mineral (15·38 grs.) used in determining the phosphoric acid according to the tin method. The matter insoluble in sulphide of ammonium was, as far as possible, dissolved in nitrohydrochloric acid, this solution was added to the nitric-acid solution obtained in the first instance after filtering off the oxide and phosphate of tin, and the whole then precipitated by ammonia and the alumina collected. From its appearance, however, it was suspected that it might contain tin; it was redissolved in sulphuric acid and a stream of sulphuretted hydrogen passed through the solution, when a considerable precipitate of the sulphides of lead and tin[*] fell, which was filtered off, and the alumina determined as usual by precipitation by ammonia. After ignition it weighed 5·90 grs., or equivalent to 38·36 per cent. in the mineral.

The average of these two determinations of alumina will be 39·20 per cent.

From the results of the above determinations the analyses will now stand as follows:—

	a.	*b.*	*c.*
Water	8·73	6·45	5·260
Phosphoric acid .	4·09	2·94	2·630
Alumina	8·90	5·90	5·290[†]
Insoluble (silica) .	0·31	0·09[†]	0·185
Loss in analysis .	0·19	......	......
	22·22	15·38	13·365

And calculating the percentages derived from these results,—

	a.	*b.*	*c.*	Mean.
Water	39·29	41·18	39·37	39·95
Phosphoric acid .	18·42	19·01	19·73	19·05
Alumina	40·05	38·36	41·51[†]	39·31
Insoluble (silica) .	1·39	1·45[†]	1·39	1·41
Loss	0·85	......	......	0·28
	100·00	100·00	100·00	100·00

From the above analysis the formula $3Al^2O^3, PO^5+18HO$ may, I think, be safely deduced. This formula will, on calculation, represent the following percentage composition:—

$$\begin{array}{lcl}
3Al^2O^3 = 153{\cdot}78 & = & 39{\cdot}75 \text{ Alumina.}\\
6PO^5 = 71{\cdot}00 & = & 18{\cdot}36 \text{ Phosphoric acid.}\\
18HO = 162{\cdot}00 & = & 41{\cdot}89 \text{ Water.}\\
\overline{386{\cdot}78} & & \overline{100{\cdot}00}
\end{array}$$

For comparison I annex a Table showing the chemical composition of all the hydrated phosphates hitherto announced as having been found in the mineral kingdom.

	$3Al^2O^3, 2PO^5+12HO.$		$2Al^2O^3,PO^5$ $+5HO.$	$2Al^2O^3,PO^5$ $+6HO.$	$2Al^2O^3,PO^5$ $+8HO.$	$Al^2O^3,PO^5,$ $+8HO.$
	Wavellite.	Kapricite.	Kalaite.	Peganite.	Fischerite.	Gibbsite.
	Barnstaple.	Hungary.	Silesia.	Striegis.	Nischna Tugal.	Mass.U.S.
Phosphoric acid.	34·98	35·49	30·90	30·49	29·03	37·62
Alumina . . .	37·18	39·59	44·50	44·49	38·47	26·66
Oxide of iron .	....	....	1·80	2·20	1·20	....
Oxide of copper.	....	....	3·70	....	0·80	....
Gangue . . .	....	....	....	....	3·00	....
Water . . .	28·00	24·92	19·00	22·82	27·50	35·72
	100·16	100·00	99·90	100·00	100·00	100·00
	Fuchs.	Städeler.	Zellner.	Hermann.	Hermann.	Hermann.

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[*] Doubtless the lead had been in the tin as an impurity.

[†] Determined as loss.

XLI. *On Induction in a Rotating Conductor.*

By E. Jochmann[*].

THE equations (26) and (27) of my memoir "On the Electric Currents induced by a Magnet in a Rotating Conductor"[†] can be easily integrated, by means of a development in series according to spherical functions, in the special case of a conducting sphere rotating around one of its diameters. The results, on account of their remarkable simplicity, shall be here given.

Let

$$s=\sqrt{a^2+b^2+c^2}$$

be the distance of an inducing pole from the centre of the sphere, and λ denote the angle between the directions of s and r, where

$$r=\sqrt{x^2+y^2+z^2};$$

then if ρ represent, as before, the distance of the point (x, y, z), within the conductor, from the inducing pole, so that

$$\rho^2=r^2+s^2-2rs\cos\lambda,$$

we shall have

$$\mathrm{V}=2nk\Sigma\mu\frac{r(cr-sz\cos\lambda)}{s\rho(\rho+s-r\cos\lambda)},$$

where the summation is to be extended to all the existing magnetic poles. If, further, we put

$$\Psi=2nk\mathrm{K}\Sigma\mu\frac{bx-ay}{\rho s(\rho+s-r\cos\lambda)},$$

the components of the current-density at the point (x, y, z) will be

$$u=y\frac{\partial\Psi}{\partial z}-z\frac{\partial\Psi}{\partial y},$$

$$v=z\frac{\partial\Psi}{\partial x}-x\frac{\partial\Psi}{\partial z},$$

$$w=x\frac{\partial\Psi}{\partial y}-y\frac{\partial\Psi}{\partial x}.$$

From the form of these expressions it is manifest that the radial components of the current at each point within the sphere vanish,—in other words, that all the currents flow on concen-

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[*] From the *Journal für die reine und angewandte Mathematik,* vol. xxxvi. p. 329.

[†] Phil. Mag. S. 4. vol. xxvii. p. 522.

tric spherical surfaces. It likewise follows therefrom that the results are also immediately applicable to the case of a shell bounded by two concentric spherical surfaces. In this case, however, we must also assume a distribution of free electricity on the inner surface of the hollow sphere, of such a nature that, in virtue of its presence together with that of the electricity within the conductor and on its external surface, the potential V may acquire the above value. The form of the current-curves within the conductor is determined by the equations

$$r = \text{const.}, \quad \Psi = \text{const.}$$

The components of the action of the system of induced currents upon an external magnetic pole m are

$$-m\frac{\partial Q}{\partial \xi}, \quad -m\frac{\partial Q}{\partial \eta}, \quad -m\frac{\partial Q}{\partial \zeta},$$

where ξ, η, ζ are the coordinates of the pole, and

$$Q = \frac{\partial}{\partial \xi}\int \Psi' \frac{x'}{\mathrm{r}}\, dx'\, dy'\, dz' + \frac{\partial}{\partial \eta}\int \Psi' \frac{y'}{\mathrm{r}}\, dx'\, dy'\, dz' + \frac{\partial}{\partial \zeta}\int \Psi' \frac{z'}{\mathrm{r}}\, dx' dy' dz',$$

in which expression Ψ' denotes what Ψ becomes when x, y, z are changed into x', y', z', and, for brevity, we have put

$$\mathrm{r}^2 = (x'-\xi)^2 + (y'-\eta)^2 + (z'-\zeta)^2.$$

The integration can easily be effected when it is required to calculate the reaction of the system of induced currents upon a *single* inducing pole, or when, after differentiation, ξ, η, ζ are made equal to a, b, c respectively, and consequently r to ρ. For then, putting for simplicity $b=0$, we have in the case of a solid sphere of radius R,

$$Y = -\frac{nk\mathrm{K}\pi a\mu^2}{2s^2}\left[\frac{9\mathrm{R}s^2 - 7\mathrm{R}^3}{s^2-\mathrm{R}^2} + \frac{9s^2-\mathrm{R}^2}{2s}\log\frac{s-\mathrm{R}}{s+\mathrm{R}}\right];$$

and in the case of a hollow sphere with internal and external radii equal to R_1, R_2, respectively,

$$Y = -\frac{nk\mathrm{K}\pi a\mu^2}{2s^2}\left[\frac{9rs^2 - 7r^3}{s^2-r^2} + \frac{9s^2-r^2}{2s}\log\frac{s-r}{s+r}\right]_{r=\mathrm{R}}^{r=\mathrm{R}_2};$$

whilst in both cases

$$X=0, \quad Z=0.$$

The relation between these results and those obtained in the former memoir for the case of a plane disk is manifest.

The result takes a remarkably simple form under the hypothesis of a sphere rotating under the influence of a constant magnetic force, such as that of the earth. The coordinate plane $y=0$ may now be made to coincide with the plane of the axis of rotation and the direction of magnetic force, or, as we may call it, the plane of the magnetic meridian. This done, we must put $b=0$, and afterwards allow s and μ to increase indefinitely in such a manner that the ratios $\frac{a}{s}=\sin\gamma$ and $\frac{\mu}{s^2}=\mathrm{T}$ may preserve constant values. The quantity T will then represent the intensity of the constant magnetic force, and γ the angle between the direction of this force and the axis of rotation. In this case the current curves reduce themselves to the system of circles represented by the equations

$$r=\text{const.},\quad y=\text{const.},$$

all of which lie in planes parallel to that of the magnetic meridian. The constant current-density within each current-curve will be

$$\tfrac{1}{2}nk\mathrm{KT}\sin\gamma\,.\,\lambda,$$

if

$$\lambda=\sqrt{r^2-y^2}$$

be the radius of the current-circle. With respect to its external action, the current-system deports itself like a magnet whose axis coincides with that of y, or, in other words, is perpendicular to the plane of the magnetic meridian. Worthy of notice is the analogy which exists between this result and the one deduced by Poisson from his magnetic theory of rotation-magnetism in his memoir "Sur le Magnétisme en mouvement"*, as well as the one found by Green†, in the case where a sphere of imperfect electric conductibility is supposed to rotate under the influence of a constant electrostatic force, or where a sphere consisting of a magnetizable substance endued with coercive force rotates under the influence of a constant magnetizing force.

Berlin, March 1864.

XLII. *On the Influence of the Pitch of the Tuning-Fork on the Mechanism of the Human Voice.* By John Bishop, *F.R.S.*‡

All those who have paid attention to acoustics know that what is denominated *pitch* in musical science refers to a certain definite number of vibrations or undulations of the air, and also that, for musical purposes, a tuning-fork has been constructed to yield a note or sound termed C, which we may assume as the fundamental note in the diatonic scale, or gamut.

Since, then, the pitch of the tuning-fork determines that of all the other notes, both in music, musical instruments, and

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* *Mém. de l'Acad. des Sciences*, vol. vi. p. 497.

† *Journal für die reine und angewandte Mathematik*, vol. xlvii. p. 187.

‡ Communicated by the Author.

tones of the human voice in singing, it is of the greatest importance, not only that the pitch of the fork should be uniform, but that it should be conformable to the structure and functions of the human organs of voice, to which all other instruments of sound ought to be subordinate.

How widely this principle has been departed from, and how injurious these deviations have been to the vocal mechanism, it is the object of the following remarks to show.

The pitch having once been disturbed, the Philharmonic Society adopted a particular one, the Opera another, and then almost every maker of musical instruments chose his own pitch, until at last it became difficult to get together performers on two or three instruments which were of the same pitch. The pitch of tuning-forks with "C Philharmonic" marked on them may be very different, and there seems no guarantee for the correct pitch of many of these forks; and where the pitch is so various, singers do not know whether or not the music they have been accustomed to sing is within the compass of their voice.

In this state of uncertainty, the Society of Arts appointed a committee to investigate the subject, and to discover and report on the best means of remedying these difficulties. Upon this report being considered, 528 vibrations were recommended for adoption by the Society*.

When the nature of sound was first investigated, the number of vibrations in the air which were necessary to constitute a sound of a given pitch was accurately ascertained. It was determined that any elastic body, such as a stretched cord or a spring, whose vibrations to and fro recurred every second, should be denominated C, and that every power of the number two, expressing vibrations within the limits of the range of musical instruments, should constitute C in the diatonic scale of music. On this system was the tuning-fork constructed, being taken for convenience at the 10th power of 2, consisting of 1024 vibrations, which will be according to the German and the French system of notation, and 512 *double* vibrations on the system of the English method of computation—this pitch, or some other of a less number, having obtained the sanction of all the musicians and mathematicians who had studied acoustics with reference to musical science. Among the latter may be noticed the names of Euler, I. and D. Bernouilli, Riccati, Poisson, Savart, Dr. Young, Weber, and Sir John Herschel. Among those who composed music nearly on this pitch of the fork are Handel, Mozart, Beethoven, and nearly all the great composers up to the beginning of the present century; and, moreover, musical-instrument

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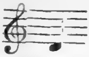
* Journal of the Society of Arts, June 8, 1864.

makers had, up to this period, observed nearly the same fundamental pitch for the tuning-fork.

The following Table, with which I have been supplied by the kindness of my friend Mr. A. J. Ellis, will show the truth of the statement that the pitch of C has been arbitrarily raised, in various degrees in different countries, since the time of Handel, and also that the Italian-Opera pitch is such as to render a great deal of vocal music impossible to be sung by any vocalists except those who possess an unusually extended compass; also that the scale of the Society of Arts is too high for general use, and has failed to produce that uniformity which was designed by its members; and the question resolves itself into whether the slight difference in the brilliancy of instrumental music is compensated for by the increase of difficulty and injury to the human organs of voice. We all know that the performance of the most favourite overture, executed by the most perfect of orchestras, faded into comparative insignificance when the tones of a Pasta, a Malibran, or a Jenny Lind reached the ear.

TABLE of the Varieties of Pitch.

When any note but C is the pitch note, the pitch of C is calculated from it according to the semitonic temperament of twelve equal semitones if not otherwise expressed, or according to the mesotonic temperament or system of perfect major thirds.

No.	Authority.	Pitch notes.	
		 A	 C
1.	Dr. Smith, organ of Trinity College, Cambridge, 1758;  *d* = 262 (mesotonic).........	....	233·42
2.	Usual organ pitch	....	240·00
3.	Handel's tuning-fork, 1740 (mesotonic)	416	247·35
5.	Theoretical pitch, Hullah and Tomlinson, 1842.	....	256
6.	Philharmonic (1812–42)	433	257·47
10.	French normal diapason (1859)......................	435	258·66
14.	Stuttgardt Congress of Musicians (1834).........	440	261·63
17.	Vienna Orchestra, 1834 (Schleibler)	440·87	262·14
18.	Berlin Orchestra, 1834 (Schleibler)	441·625	262·59
19.	Society of Arts, London (1860)	....	264
21.	Italian Opera, 1860 (Society of Arts' Report)...	455	270·55

N.B.—The pitch is here considered as the number of *double* vibrations in a second. In France and Germany the number of *single* vibrations is usually taken, and hence the preceding figures would be doubled. Thus the French normal diapason is called 870.

Shortly after the commencement of the present century there arrived in this country German performers on wind instruments whose pitch was much more acute than that of our standard. An opinion then prevailed that the tones of wind instruments were improved by this higher pitch, and under this impression the Philharmonic Society and the orchestral departments of the theatres acceded to this strange notion, or what might rather be termed delusion.

The effect of this alteration in the fundamental pitch of music has been very important. The pianoforte-makers have been obliged to shorten the strings of their instruments; the organ-builders to shorten their pipes; the flute-makers to cut off a portion of the length of their tubes; and what is termed the opera pitch has transformed the C of the olden time into D; and since Nature has made no corresponding change in the length of the cords of the vocal organs of the human race, it is manifest that some changes must be made, either to adapt musical compositions to the changes in musical pitch, or to reduce the standard pitch conformably to the structure of the human organs, in order to render their execution possible.

It has been already stated that the greater portion of our best music was composed at a period when the tuning-fork made about 512 vibrations for C. This is the case in the works of Handel, Mozart, Beethoven, and in the old madrigals and masses. Let us suppose a person has a tenor voice whose limit on the old scale of pitch is A. He can no longer sing the same music when A is transformed into B. Take, again, the *soprano* or *alto*, where, with the new pitch, the music is rendered impossible of execution. We find accordingly that, in order to diminish the evils which the present pitch has inflicted on the human voice, a large number of Handel's and Mozart's popular songs have been transposed by Callcott and others into lower keys, so as to bring them, for private use, into a pitch as near as possible to that in use in the time of the composers. This is, however, only a partial remedy for the much greater evil, since it leaves the entire works of these great masters untouched as far as relates to their performance in public. Now, although this may be easily effected in short pieces of music, no one would think of changing the key for such a work as the Messiah, or a whole Mass; and yet many singers can no longer join in the execution of these celebrated productions. It must not be forgotten that in this age of vocal harmony, music was intended for assemblages of choral performers, and not merely for the few who possess such an extended range of voice as would enable them to disregard the change of pitch.

We come now to the effects produced on the organs of voice by straining the vocal cords beyond their proper tension. A young lady, endowed with a fine soprano voice reaching to C in alt. or 1024 vibrations, by straining the vocal cords on that note raised to D, lost the power of exercising her voice for musical purposes during nearly three years. Even Madame Goldschmidt complains of the strain which the change of pitch has produced in her vocal organs; and it is well known what an extended range of flute-like sounds this charming and accomplished singer possessed. Further, it is well known that the tones at the extreme limits of phonation are never so pure in quality, or so agreeable to listen to, as the notes within those limits; and moreover, when in order to execute a given note the vocal cords are stretched beyond their normal elastic length, they do not always so readily regain their tone of elasticity; and if this be permanently impaired, the voice loses some of its range of notes, and will be unable to regain the power to execute the melody as before. The struggle to execute a pitch beyond the normal limits sometimes gives rise to spitting of blood, and has been known to produce apoplexy. These circumstances are surely sufficient to render the reduction of the pitch of C to its former limit of 512 vibrations imperatively necessary.

The principle we wish to impress is, that the pitch of musical instruments intended to accompany the human voice should be made subordinate to the anatomical structure and mechanism of the human organs, instead of the latter being rendered subordinate to the former. Consequently the pitch of C should again be 512 vibrations; and we advise all persons interested in the choice of pianos and other musical instruments intended for vocal accompaniments, to insist upon having them of that pitch.

There appears to have been a general complaint against the present pitch of our musical instruments by almost all the higher class of singers; and on appealing to one of the most scientific of our pianoforte-makers for his opinion, he stated that the pitch had ruined many a fine voice, but, as long as the public demand for the higher pitch remains, it is not in the power of the instrument-maker to remedy the evil. We know how strongly Sir John Herschel protested against the decision of the Committee of the Society of Arts, and all parties appear to have considered the decision at which they arrived as only a temporary measure; and its complete failure to produce uniformity is a confirmation of his views, and shows the necessity for further investigation.

These remarks have not been written as a mere theory, but in consequence of the numerous cases of injury to the human organs of voice from the above-mentioned causes which have been from time to time submitted to the author's opinion.

XLIII. *On the Cohesion-Figures of Liquids.*
By Charles Tomlinson, *F.C.S.**

[With Two Plates.]

AT the Meeting of the British Association at Manchester in 1861, I had the honour of submitting to the Chemical Section a subject then new to science, namely, the cohesion-figures of liquids. In the memoir that was read†, I endeavoured to show that when a drop of an independent liquid (that is, not a solution) is gently deposited from the end of a glass rod or from the point of a dropping-tube upon chemically clean water in a chemically clean glass, the drop flashes out into a definite figure as it enters into solution or diffuses over the surface. Each figure is characteristic of the liquid, and is a function of the cohesive force and diffusibility of the liquid, and the adhesion of the surface on which it is deposited. The figure may also probably be represented in other ways. It may be a function of the solubility and the diffusibility of the liquid in question, or of the solubility, the density, and the molecular attraction; while in the case of certain figures which are produced beneath the surface, and which I have named *submersion figures*, each figure seems to be a function of the solubility, the density, and the molecular attraction.

In the production of cohesion-figures, water is the most convenient adhesion surface. It must be contained in a glass that is kept chemically clean by occasional washing in sulphuric acid or in a solution of caustic potash, so that the water, which need not be distilled, may present a chemically clean surface. A shallow glass about 4 inches in diameter is adapted to these experiments. I have had a number of such glasses made for the purpose, and have placed a couple of them upon the table. The temperature best adapted for these experiments is that of an ordinary room, which in winter or summer may be taken at about 60°. I have not studied these figures by artificial light, but have been informed that they admit of being reflected in an enlarged form so as to be seen by an audience. I have published various precautions respecting these figures, both with respect to temperature and variations in the area of the adhesion surface. I have also shown how these figures may be applied to the detection of adulteration in liquids, and also how suggestive many of these figures might be to the pattern-designer, from the great beauty and novelty of form and the exquisite harmony of colours displayed in them‡.

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* Communicated by the Author, having been read before the British Association at Bath, September 15, 1864.

† Phil. Mag. for October 1861.

‡ Ibid. March 1862.

In order that the application of this subject to qualitative analysis might be brought more directly before the attention of persons who would value an easy and expeditious test, I read a paper before the Pharmaceutical Society in February last, "On the Verification of Castor Oil and Balsam of Copaiba by means of their Cohesion-figures." These two liquids were selected to introduce the subject of cohesion-figures in general, and its application as a rough and ready test. The members were so much interested in the subject that they requested me to furnish their engraver with some figures for insertion in their journal, and also to state what variations took place in the figure of castor oil from various markets and of various growths. Accordingly I examined twelve specimens of castor oil collected from different sources, together with three or four specimens of balsam copaibæ, and wrote a report on the same, which will be found in the Society's Journal*.

In order to invite attention to the commercial oils, I read a paper in March last before the Society of Arts, "On the Verification of Olive-oil by means of its Cohesion-figure." The subject excited some interest and discussion†.

In these communications I endeavoured to show that the cohesion-figure of the same substance is liable to certain variations in different specimens, especially in the case of the oils, which may be more or less viscid, more or less acidified or resinified. I do not here refer to the variations arising from adulteration and admixture; for this point is insisted on in all my papers. But I was not unmindful of the changes likely to be induced by age; for in my paper published in March 1862 (Philosophical Magazine), I state that the cohesion-figure of the oil of lavender, for example, may vary in different specimens, since it varies in density from 0·87 to 0·94. The cohesion-figure of the oil of lavender is so striking that I was induced to try a number of specimens in 1861, and in all of them I obtained the peculiar Carrageen-moss pattern—unless, as was often the case, the specimen had been adulterated with turpentine, in which case there was no difficulty in detecting the adulteration. I also found that an essential oil, entirely different from that of lavender in its properties and cohesion-figure, and also of less density, might be made to give a somewhat similar cohesion-figure by dissolving a small portion of camphor in it under a gentle heat, so as to bring it to about the same density and texture as oil of lavender. I stated in my first paper, that if two independent liquids could be found of the same density and physical molecular constitution (that is, equally fluid or viscid, &c.), they would form the same

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* Pharmaceutical Journal for March and April 1864.

† Society of Arts Journal for March 4, 1864.

cohesion-figure on water, although of different chemical constitution. Dr. Gladstone has has been so kind as to send me a specimen of salicylate of methyle, $ꞒH^3,.Ꞓ^7\,H^5\,Ɵ^3$, which gives a figure somewhat resembling that of oil of lavender—arising, I have no doubt, from a similar physical constitution. Cases of this kind must be rare, and are not likely to interfere with the application of cohesion-figures as a test. A much more serious objection is the alteration which oils undergo by long keeping. The specimens of oil of lavender, for example, which in 1861 gave the Carrigeen-moss-pattern figure, were so changed in 1864 as to give only a plain film without any distinctive character. On redistilling these oils, however, so as to get rid of oxidized products, the distillate produced the lavender-oil pattern as it did in 1861.

On the other hand, the method is occasionally so delicate as to excite the surprise of persons who have sought in vain for a method of detecting differences in oils &c. which they largely use in the course of their trade. For example, a manufacturer informed me that it would be a great thing for him to be able to detect the difference between beef-oleine and mutton-oleine. The respective cohesion-figures of these substances show the difference plainly. Again, balsam copaibæ is often adulterated with castor oil, for the detection of which the usual tests are either troublesome or inadequate. The method of cohesion-figures detects the adulteration immediately. Again, olive oil is frequently mixed with poppy oil, or sesame-seed oil; not only may these mixtures be detected by means of their cohesion-figures, but also the relative proportions of the respective oils.

Although for all practical purposes water would be used as the adhesion surface in the production of these figures, considerable interest arises from noting changes undergone by the figures when other surfaces are used. In my first paper, in 1861, it was stated that wood-spirit on the surface of mercury gave a very different figure from what it did on the surface of water; and in my second paper (Phil. Mag. March 1862) I described the cohesion-figures of water, ether, and alcohol on the surface of sulphuric acid, and also those of one or two essential oils on the surface of acetic acid. I have lately obtained a large variety of figures by experimenting on such adhesion surfaces as those of cocoa-nut oil, castor oil, paraffin, spermaceti, white wax, olive oil, lard, and sulphur. Of course the substances, such as paraffin, wax, &c., which are solid at ordinary temperatures, were melted for the purpose of these experiments. Too high a temperature was found to be disadvantageous, on account of the tendency of the drop to assume the spheroidal state. Lard is advantageous, on account of the length of time that it remains fluid after having been thoroughly melted. Castor oil was used at various temperatures, but some of the finest figures were obtained on its surface at the ordinary temperature of the air of the room. Fine figures were also obtained on the surface of cold olive oil. Some of these figures are represented in Plate VI.

Cocoa-nut Oil.—A drop of ether flattened into a very perfect disk about two-eighths of an inch in diameter: this was surrounded by a dentated ring, from which proceeded a multitude of rays, as shown in Pl. V. fig. 4. The best-defined figure was obtained when the temperature of the surface was about 80°.

Alcohol produced a disk about three-fourths of an inch in diameter, with a small boss in the centre surrounded by a number of concentric circles faintly tinted with iridescent colours, while the edge of the disk was delicately fringed with very short radial lines. The figure was first of the size shown in fig. 1, No. 1; it then expanded to No. 2, and disappeared by closing in upon its centre. The figure was quite sharp and distinct, giving the idea of a lid of a box turned in the lathe. The best result was obtained when the surface was at about 90°.

Benzole gave a figure about 2 inches in diameter, consisting of a central depressed disk about three-eighths of an inch in diameter, with a slight conical projection in the centre and surrounded by a broad smooth flat ring terminating in a sharply-cut edge. Oil of turpentine gave a somewhat similar figure, only the outer edge was wavy. Paraffin oil and Persian naphtha also give figures of the same type. Oil of lavender gave a central disk, from which issued wavy processes which were torn away by the adhesion of the surface.

Castor Oil.—When the oil is at about 94° F., a drop of ether forms a large figure bounded by a well-defined circular edge, in the centre of which figure is a plain disk surrounded by a narrow plain line; just outside the disk is the engine-turned pattern, and beyond this, as far as the boundary edge, the disk is quite smooth. The engine-turned pattern seems to be produced by the revolution, or rather oscillation, of the central disk on the heated surface. On cold castor oil the ether figure consists of a central boss surrounded by rippled waves, very much like the rose-pattern of the turner. Fig. 3 represents these two figures.

When the oil is at 94°, a drop of alcohol forms a central star in a large disk surrounded by iridescent rings. But a finer figure is produced on cold castor oil: the drop spreads out into a large disk with broad iridescent bands just within the sharp-cut edge; having attained a diameter of about 3 inches, it retreats towards the centre, leaving a beautiful network of minute globules. A drop of camphorated spirit produces a still finer figure, an idea of the beauty of which can scarcely be conveyed in words. A figure of a similar type is produced on the surface of olive oil (see Pl. V. fig. 6), and will be described further on.

The benzole figure at 83° consists of a small central disk surrounded by an engine-turned pattern, beyond which rippled waves extend to the circumference of a large figure. On the cold oil the engine-turned pattern is wanting (see fig. 11). Paraffin oil gives a somewhat similar figure. The turpentine figure consists of a central boss surrounded by a large flat ring. On the cold oil, a drop of oil of cajuput has a central depression surrounded by a large flat ring.

Paraffin.—Solid paraffin was melted in an evaporating-dish, and after the heat had been removed and the surface had become tranquil, a drop of ether, allowed to fall on the surface at 180°, was at first spheroidal; it then flattened down, and by its evaporation solidified a portion of the paraffin with which it was in contact; the solidified portion rushed wildly about under the retroactive force of a little remaining ether, until it disappeared by solution in the liquid paraffin.

Alcohol formed a disk which sailed about, shooting out flat disks resembling petals, so as to give the figure the appearance of a flower (see Pl. V. fig. 5).

Paraffin oil formed a disk which sailed about with much agitation, sending off waving lines. The oils of turpentine and cajuput, and some others, solidified a portion of the paraffin—in some cases permanently, while in others the solidified portions moved about over the surface.

Olive oil and pure tallow oil assumed the spheroidal state on the surface at 180°, and then sank.

Spermaceti.—On the surface of melted spermaceti a drop of ether becomes spheroidal, or, if the temperature be not too high, flattens down into a small raised disk which spins rapidly; or it may solidify a portion of the spermaceti, when the solid portion darts about in wide sweeps rapidly over the surface until it is again taken up by the liquid.

Alcohol, when the surface is at about 160°, forms a well-defined disk with a waving border and concentric rings, and a delicately fringed iridescent edge (see fig. 2); but at a lower temperature (about 116°) it solidifies a portion of the spermaceti into the form of a small coracle, which sails about carrying its small cargo of alcohol. Turpentine at 121° behaves in a similar manner, only the solidified portion breaks up and darts about. Oil of cajuput at 127° forms a large disk with a faint depressed centre. Benzole forms a large plain disk, in the centre of which is a small spinning disk with a raised conical projection in the centre. Camphorated spirit slightly chills the spermaceti, rotates in the form of a small lens or boss with an agitated kind of motion;

then settles down into a charming star-like figure surrounded by rings and iridescent colours, and a sharp boundary line. In one case the central boss slipped off the figure, leaving the disk with its iridescent rings, &c. complete.

White Wax.—With the surface at 142°, ether solidified a portion which rushed wildly about. Alcohol solidified a portion in the form of a ring, which immediately broke up and was dispersed in radial lines, while the alcohol settled down into a small sharply-defined disk. At a higher temperature, such as 170°, a drop of alcohol will solidify a cup-shaped cavity for its reception; but if the drop be held for a short time over the surface so as to become warmed, it at once subsides into a sharp well-turned disk.

Lard.—Good figures are formed on the surface of melted lard with ether, alcohol, and the oils of turpentine, savin, paraffin, lavender, and some others. A drop of camphorated spirit formed a very beautiful figure, some idea of which may be gathered from fig. 7. The rippled concentric circles display several of the orders of Newton's, or rather of Nobili's rings.

Olive Oil.—A fresh flask of the variety of this oil known as *extra sublime,* was opened for the purpose, and portions of it were poured into the ordinary 4-inch cohesion-figure glasses. It answers admirably as an adhesion surface at ordinary temperatures. A drop of ether produces a small beautiful figure, consisting of a disk surrounded by rays. Alcohol forms a disk about 3 inches in diameter; the drop diffuses well, and the disk is perfectly circular, with a central boss. Iridescent rings containing the colours of four or five orders fringe the edge of the disk in broad bands, and outside this large disk is a fainter and more shadowy disk. When the figure is fully developed, it rapidly opens, and closes in upon the centre like a curtain being drawn in, and so vanishes, leaving no trace behind. Camphorated spirit (fig. 6) is even more beautiful and persistent than alcohol; the iridescent rings are dentated, and this adds greatly to their beauty; the film, which is of a very large size, retreats slowly inwards to the centre, leaving the camphor in the form of minute dots disposed in radial lines; these lines in their turn retreat towards the centre, where the camphor collects in the form of a flat ring.

Benzole forms a large plain disk with a cavity in the centre. Turpentine, cajuput, and lavender also form disks; but the most curious figure is given by a drop of pure wood-spirit. It flashes out into a disk about 1½ inch in diameter, then retreats inwards with the elasticity of a spring, leaving a delicate fringe made up of innumerable small dots; the disk then becomes toothed at the edge so as to give it the appearance of a small circular saw; the disk retreats inwards, and the point of each tooth projects a number of globules, the ultimate figure being a small disk in the centre with an immense number of dots radiating towards it. In fig. 9 an attempt is made to represent this effect, first as the figure is expanding, and secondly as it is retreating. On cold castor oil a similar figure is produced, only the dots are more numerous and finer; and there is a very curious difference in the figure of a drop of the wood-spirit of commerce as compared with that of the pure spirit. On cold olive oil and on lard at 120°, or on cocoa-nut oil at a lower temperature, the drop of impure spirit forms a small lens with ten or twelve short blunt arms projecting from it (see fig. 10), and each arm shoots out a multitude of globules; and not doing so in equal times from each arm, there is a reactionary movement, which causes the disk to describe half a turn in one direction and then half a turn in an opposite direction, the effect of which is to dispose the dots not in radial, but in curved lines, the curves often bending in opposite directions. The formation of the figure is sufficiently slow to allow it to be studied, and the effect is very curious. The difference between the two figures distinguishes the pure from the impure spirit in a very marked manner. It should be noticed that the surface of the oil soon becomes saturated, so that not more than two or three figures can be produced in succession; but by wiping the surface with a piece of filtering paper, its adhesion is restored.

Sulphur.—When sulphur is melted so as to be sufficiently liquid to pour easily, some good figures may be formed on its surface. A drop of ether at first assumes the spheroidal state; it then forms a boss surrounded by two or three rings of the thinnest orders of colour, steel-blue prevailing, and the figure is bounded by an excentric ring some way off. Benzole forms a good figure, consisting of a boss, an irregular star from which small lenses are shot out, and these are circumscribed by a flat circle (Pl. V. fig. 15). Oil of lavender forms a boss surrounded by iridescent rings in waving lines, then a large silvery space, and a narrow boundary ring of iridescent colours (see fig. 13). The oils of rosemary, turpentine, and paraffin form each a boss surrounded by large wavy iridescent clouds of most brilliant metallic colours, paraffin oil being most brilliant of all. Creosote and carbolic acid form disks which flatten out into waving figures (see fig. 12). The figure formed by camphorated spirit is shown in fig. 8. Camphor moves about over the surface; water forms and occupies a cup-shaped cavity, solidifying the sulphur as it evaporates.

Submersion Figures.

In the Philosophical Magazine for June 1864, I have described a new variety of the cohesion-figures of liquids, in which the drop, instead of diffusing over the surface of the adhesion liquid, sinks into it and diffuses through it. For this purpose a column of liquid in a cylindrical glass is employed. When water is used, a few drops of a strong solution of ammonia, or of oxalate of ammonia, or of alum are added, for the purpose of throwing down the lime, and also of assisting in the development of the figure. A strong solution of cochineal in water forms a figure which is typical of a number of cases of this kind of diffusion. A single drop on the surface sinks down, opens into a ring, which becomes depressed at two opposite points, and lets down lines with other rings attached to them; while each ring, at 90° from the point of attachment of each line, lets fall two other lines with a ring attached, which ring in like manner, from two points 90° distant from the line, lets fall other lines; and in this way the figure is developed slowly and symmetrically. Oil of lavender in a column of spirits of wine behaves in a similar manner, only the figure is much more complicated and crowded. A drop of fousel oil in a column of paraffin oil passes through some complicated changes resulting in a kind of pointed dome, the lower edge of which is cut into four symmetrical arches; from the springing of each pair of arches a line is let down, and from the extremity of this proceeds four smaller domes similarly arched, and letting down four other lines and four other still smaller domes, forming a figure which lasts a considerable time, exciting surprise in all who have seen it by the kind of architectural symmetry produced. Figures of another type are formed in columns of benzole, of ether, &c. In some cases very perfect rolling rings are formed, for the details of which I must refer to my paper, my business today being to point out a variety of other forms of cohesion-figures by submersion; for which purpose cylindrical columns of cocoa-nut oil, castor oil, paraffin, spermaceti, white wax, lard, and olive oil were used as in the case of cohesion-figures on the surface. Heat must be employed when necessary; but the best results are obtained with the cold oils, or with only just a sufficient amount of heat to render the solid substances fluid. Indeed the figures vary considerably with considerable differences of temperature, not only with substances which require to be melted, but with oils which are fluid at ordinary temperatures.

Cocoa-nut Oil.—When a column was at about 160°, a drop of patchouli oil flashed out into rings and festoons. Oil of cloves formed a wide ring, from which proceeded numerous festoons and small rings; oil of cinnamon two or three large rings and festoons. Oil of cummin descended as a riband with a globule attached, from which proceeded upwards a dome cut into arches with lines terminated by knobs at the springing of each pair of arches. Balsam copaibæ (when the oil was at 165°) descended in the form of a single thick glassy ring of perfect structure. Some of the fixed oils formed pretty figures in a column at 110°. Colza descended in the form of a cup with the edges turned over and inwards, suspended by a line from the surface of the column to the centre of the cup. Linseed oil also forms a hemispherical cup with the edges turned over, but without the suspending line. Sesame a similar figure, only instead of the suspending line there was an arched projection in the centre of the cup. Castor oil sank rapidly in the form of a cup with the edges turned over and inwards. Some of these figures are represented in Plate VI. and are marked *a, b, c, d, e*.

Castor Oil, Olive Oil.—A column of each of these oils is well adapted for the exposition of sets of figures differing in many particulars, but all distinguished by a *bulb* and a *stem.* Each figure may be compared to a thermometer with a small bulb and a long delicate stem. For example, when a drop of oil of cloves is deposited on the surface of a column of cold castor oil, 6 or 7 inches in length, the greater portion sinks beneath the surface (see No. 1), while the remaining portion forms a disk on the surface, attached to the submerged globule by means of a short neck (see No. 2). The weight of the globule drags upon the disk and forms it into a conical cavity, containing a speck of air, which, as the disk collapses by the weight of the descending globule, becomes enclosed and is drawn out with a portion of the oil of cloves into the form of a long narrow tube (No. 3): the disk at the surface, now reduced to the diameter of the tube, remains attached to the surface, and, indeed, is so persistent in its character that, long after the bulb has spread over the bottom of the vessel, this tube or thread remains attached to its moorings at the surface, and even interferes with the proper development of a second figure in the same column if the latter be narrow. As the tube is drawn out by the descending globule, its material is supplied partly by the surface of the globule, and partly by the medium, namely the castor or olive oil. The latter, in passing over the surface of the oil-of-cloves spheroid, detaches a portion of its substance, and thus allows the tube to increase in length. In the meanwhile the original drop of oil of cloves, which near the surface was a sphere, flattens out into the form of a spheroid; and when it has descended about one-third of the length of the column it appears to open, and the apparent opening is ornamented on either side by the well-turned volutes of an Ionic capital (see No. 4, and the figure further developed in No. 5). This effect appears to be due to the penetration of the spheroid at its lower surface by a portion of the medium itself, which enters and diffuses within the spheroid in the form of a very perfect ring. The volutes are the effects of an optical illusion arising from seeing through a number of the segments which compose the ring on either side, while at the front and back of the ring the edges only of the segments are seen. Figures of this type, with variations in detail, which it would take too long to describe, are produced in cold castor oil by oil of cinnamon, creosote, carbolic acid, sulphuric acid, sulphate of indigo, and glycerine.

In a column of olive oil a drop of creosote, of carbolic acid, of oil of cinnamon, or of eugenic acid is similarly penetrated from below. Other oils, however, in olive oil are penetrated from above. For example, a drop of croton oil descends at first as a bulb and stem, the stem, as before, being moored to the surface*. The bulb flattens out into the form of an oblate spheroid, and in attempting to flatten still more it gets turned over at the upper part into the form of a ring, but presenting the appearance shown in A; the medium closes in upon the opening, which becomes deeper, as in B and C, until at length it penetrates to the bottom of the spheroid, forming a trumpet-shaped mouth as in D. In doing so it unites with the stem, which thus appears to have penetrated to the very bottom of the spheroid; while, to supply its length, the medium licks over the outer surface of the spheroid, as in the former case, and thus allows the stem to accompany the altered spheroid to the bottom of the vessel. This series of processes, which it takes so long to describe, may be understood at a glance by a reference to the figures A to D.

In some cases, instead of the voluted figure, the opening from below is pyriform, and as the spheroid descends the pear-shaped figure describes a circle within the spheroid, which gives it the appearance of rocking to and fro upon its stem (see figs. a^1, a^2, a^3). Thus while in a column of olive oil a drop of cinnamon or of eugenic acid opens from below and forms a figure with volutes, a drop of oil of cloves, or of creosote, or of carbolic acid is penetrated from below and a pear-shaped opening is formed within the spheroid. We use the word opening as we do the word volutes, to express the appearances presented. The opening, however, is a penetration of the spheroid by the medium in which it is subsiding.

In a column of lard at about 170° and upwards, a drop of oil of cloves (and of some other oils) forms festoons and rings; but at lower temperatures, as from 140° down to 82° (at which point the lard used by me solidified), figures of the bulb-and-stem type are formed. But in the case of castor oil there is some varia-

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* In the figures a portion only of the stem is shown.

tion: the drop of castor oil forms a bulb and stem: the bulb is penetrated from above, separates from the stem, and descends as a rolling ring (see figs. b^1, b^2, b^3, b^4). Croton oil also forms a beautiful large ring, from which festoons descend, and from the end of each festoon a ring separates and then commences rolling. A drop of balsam copaibæ forms a bulb and stem; the bulb enlarges, expands upwards into a dome-shaped figure, from the lower edge of which festoons and rings are let down, which rings multiply and produce other festoons and rings. A drop of creosote forms rings and festoons when the lard is at about 130° or 140°; but at 110° it forms a bulb and stem, the bulb being penetrated from above. Carbolic acid at the lower temperature forms a bulb and stem, the bulb being penetrated from above. A drop of oil of cloves forms rings and festoons in hot lard, but a bulb and stem with penetration from below at a lower temperature. The same remark applies to oil of cinnamon: at about 90° the spheroid is largely penetrated from below.

It would occupy too much space and require too much pictorial illustration to enter into further details respecting these submersion figures. They all admit of being grouped under some four or five types: not that the figures of any one type are identical; for whether they be rings and festoons, or bulb-and-stem figures, or dome-shaped, or cones or rolling rings, each liquid presents characters of its own, which are again subject to further variations in different media.

King's College, London,
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XLIV. *On a Defect in the Theory of Saturation.*
By Edmund J. Mills, *B.Sc.*[*]

THE theory of atomicity—or, as it should be more correctly termed, the theory of saturation—may be justly considered, according to Wurtz's suggestion[†], as a development of the doctrine of multiple proportions. It expresses the result of an extensive induction, that there is a definite limit to the combination of one substance with another, and that this limit may be approached by successive stages. The atomicity or saturability of a given body is expressed by the number of unit weights of hydrogen which can be made to combine with a certain standard weight of it. Thus the radicals represented by the following formulæ,

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[*] Communicated by the Author.

[†] *Leçons de Philosophie Chimique*, p. 221.

C^2, C^2H, C^2H^2, C^2H^3, C^2H^4, C^2H^5,

having the standard weights

24, 25, 26, 27, 28, 29,

are saturated respectively by

6, 5, 4, 3, 2, 1

unit weights of hydrogen. Complete saturation, with respect to either, is represented in the formula

$$C^2H^6.$$

Owing, however, to the difficulty of always obtaining hydrogen-compounds, or to the absence of them, the following point has been allowed in practice—that a constant weight of any other element, equivalent to a unit weight of hydrogen, shall be accepted in the place of the latter, and be considered, equally with it, a measure of the saturability of the given substance. This concession has been most frequently made in the case of chlorine-, bromine-, and iodine-compounds—an equivalent of either of the elements mentioned being supposed to function, with respect to saturation, in precisely the same manner as one part by weight of hydrogen. It is to this point that I wish briefly to direct attention.

The question as to interchangeability of saturating function between any elements must depend not only on their being capable of transposition in terms of equivalent value, but also on their *affinity* for the substance to be saturated. For it would be impossible to attribute to the vicarious element (as, for example, chlorine used in the place of hydrogen) the power of saturation at all, unless it had an affinity for the substance employed; nor could it conveniently be taken, if, as is sometimes the case, the affinity were variable in its nature. Furthermore this exchange of function cannot be considered an equal one unless the two elements are precisely alike in their affinity for the third body.

Let us suppose, for illustration's sake, a radical X' combined with chlorine, bromine, and iodine, the last two being successively weaker compounds. The measure of the full saturability of X' will be the largest possible quantity (say, of one of *these* three elements) which has the greatest affinity for it. Hence it is obvious that $X'Cl$ will only be fully saturated; $X'Br$ and $X'I$ will be deficient by some portion of a saturability, a positive number, which may be termed x in the former, and $(x+y)$ in the latter case. Each of these quantities might of course approach or exceed unity if X were poly-equivalentic; and it is hardly necessary to say that these remarks apply to any radicals combined with X, so long as they are interchangeable and of the same equivalency.

Such a doctrine of residual saturability appears of very considerable interest. Based as it is on the well-known phenomena of difference in affinity, it rests on a property of bodies still unsubmitted to numerical measurement, but always received as a fact. The idea of unequal affinity rests chiefly on results derived from the decomposition of bodies—being thus complementary to the current doctrine of saturation, which more expressly leans on the facts of their synthesis. At the present moment especially it is desirable that the importance of connecting these should be taken into consideration. For unless we are prepared to recognize this connexion, we are logically bound to fall back on hydrogen-compounds only, and to content ourselves, in the prospect of much inconvenience, with a dogmatic announcement of merely partial value. But, on the other hand, by accepting it, we are able to explain the formation of certain compounds whose existence otherwise is quite anomalous.

Thus chloride of silver is ordinarily considered a completely saturated compound. Silver, indeed, being mono-equivalentic, must, after combination with an equivalent of chlorine, be incapable of entering, according to the common view, into any further union whatsoever. This chloride contains no poly-equivalentic radical—that kind which is said to possess the power of "accumulating" as its exclusive privilege. Yet the compound

$$3\mathrm{AgCl}+2\mathrm{AgBr},$$

long since described, is perfectly definite; and, to judge from its solubility in solutions of hydrochloric acid and of the chlorides of potassium and sodium, chloride of silver also combines with these bodies. The formula of chlorobromide of silver may be expressed under the general one

$$m\mathrm{X}'\mathrm{R}'+n\mathrm{Z},$$

Z being a quite undetermined body. We see that the residual saturability of three molecules of chloride of silver is capable of causing them to unite with two of bromide of silver. This consideration is in favour of the chloride being the more saturated compound.

The following examples may also be adduced:—

$$\begin{array}{ll}\mathrm{AgBr} & +\mathrm{HBr},\\ \mathrm{AgI} & +2\mathrm{KI},\\ \mathrm{AgI} & +\mathrm{KI},\\ \mathrm{KI} & +\mathrm{I},\end{array}$$

and also the pair

$$\begin{array}{ll}\mathrm{AgCl} & +\mathrm{AgNO}^3,\\ \mathrm{AgI} & +\mathrm{AgNO}^3,\end{array}$$

the occurrence of which will hardly be attributed to the diequivalentic nature of oxygen.

Again, certain chlorides, which are usually supposed to be fully saturated, unite with water, a substance in which the diequivalentic oxygen is understood to be completely satisfied. Thus we have the hydrates

$$\mathrm{LiCl} + n\mathrm{Aq}\ (n=1 \text{ or } 2\,?),$$
$$\mathrm{NaCl} + 2\mathrm{Aq},$$

but not a hydrated chloride of potassium. The formation of many double salts, and of direct combinations with water, appear to me to be only explicable on the ground here proposed.

This question is not verbal. Affinities are, tacitly at least, taken into account in all our reasonings; it is impossible for the theory of saturation to be independent of them. The defect in the theory consists in this—omitting to notice the fact that interchangeable weights of the same equivalentic order do not usually represent precisely similar affinities. Hence we may arrive at the following general statement:—Any two radicals are not equal in saturating power for a third, unless they are equal in equivalency and affinity also; and in most cases of combination there is still residual saturability, due to affinity, enabling the new compound itself to enter into combination.

XLV. *On the Dynamical Theory of Heat.*

By Joseph Gill, *Esq.*[*]

NOTHING in the universe can be supposed to be in a state of isolation; a lone body is evidently a physical impossibility; and therefore it seems impossible to trace back to an ultimate source any physical phenomenon displaying the action of energy or force; for we can perceive no material origin of power, but only a circulation of existing force distributed thoughout all the matter of creation. Wherever an increase of energy appears in any portion of matter, we may assume that an equivalent of energy must have disappeared from some other portion of matter, as the phenomena are only indications of disturbed equilibrium. When a weight is raised above the surface of the earth, it acquires potential energy in the shape of an increased amount of the attraction of gravitation, which must have cost an expenditure of an equal amount of energy from some other body. When a spring is wound up, or an elastic fluid compressed, potential energy appears in the shape of repulsion

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[*] Communicated by the Author.

in quantity equivalent to the force which had been expended in producing the tension.

It seems impossible to form any satisfactory idea of the nature of the forces of attraction and repulsion; and after ages of research by intellects of the highest order the subject still remains enveloped in mystery. Chemical forces seem to be specific properties inherent in matter; and the general forces of attraction and repulsion are by some considered as essential properties of ponderable matter, while others incline to attribute them to the action of a more subtle fluid medium present in all space, so that matter cannot be conceived of apart from the universal influence of this all-pervading fluid. The force or *vis viva* of motion may be conceived of as resulting from an original impression of mechanical power on matter at its creation, or as a consequence of the action of potential energies of attraction and repulsion resulting from primitive positions of disturbed equilibrium, in which atoms and masses may have been placed at their formation.

The general sources from which we can derive ready-made elemental power are the natural movements of the atmosphere in the phenomena of the winds, and the fall of water under the influence of common terrestrial gravitation. But it is often more expedient to produce mechanical power by artificial means, or by the arbitrary employment of the natural atomic forces of inanimate matter; and for this purpose we preferably avail ourselves of the chemical forces, generally through the medium of heat. In the combustion of common carbon fuels the heat is supposed to arise from intensely active molecular motion caused by the clashing of the oxygen atoms against those of the carbon, in which process the rapid motion of translation of the excited particles falling violently together under the influence of chemical attraction, is changed by the shock into intense molecular motion of vibration, or orbital movements, or perhaps some mode of individual expansion of the molecules, supposed to constitute the phenomena of heat or temperature. This clashing of the sympathetic particles in the act of combustion suggests the idea of a forcible separation of the same elements at some former period when the fuel was formed. Thus it is supposed that the solar energy at some remote time in the history of creation, when some of the common processes of nature were probably more vigorous than we now perceive them, enabled the vegetative vital principle in the ripe primeval productions of the teeming soil to separate the carbon from the carbonic acid gas of the atmosphere, and to assimilate it in a solid state in the gigantic grottoes of the ancient forests of the carboniferous epoch.

In order that the oxygen may combine with the carbon and form carbonic oxide and carbonic acid in the act of combustion, the particles must be brought so close together as to come within the spheres of chemical attraction under the influence of which they unite. It would be difficult to effect this approximation of the particles by gradually squeezing together the two bodies in mass, as general molecular repulsion increases to a powerful degree under the compression of matter; but in electricity and heat we possess agencies which, operating directly on the individual particles of matter, can produce results of intensity of action incomparably more powerful than can be effected by any mechanical means which can be brought to bear on masses. Thus the friction of a chemical match, by concentrating the mechanical action, is sufficient at a few points of actual molecular contact to generate a very high local temperature, which throws the excited particles into such violent molecular motion as to impel them over the boundaries of repulsion fairly within the sphere of chemical attraction. Under the influence of this force they combine with intense atomic action, which, communicating from particle to particle, causes the rapid lighting of the whole mass; and by the same play of molecular movements the phenomena of fire or flame may be supposed to originate, from the burning of the humblest rushlight to the most destructive conflagration.

The intense vibratory or orbital motions of the compound particles formed in the act of combustion constitute the heat or temperature of the flame, while solid particles of carbon, rendered incandescent by the highly excited atmospheres in which they are enveloped, are supposed to increase its luminosity, giving consistence and body to the fainter light of the glowing gases. The attraction of chemical affinity no longer exists between the homogeneous compound particles resulting from the combustion, and they are now under the uncontrolled influence of common molecular repulsion, which tends to separate the particles, increasing in intensity in some geometrical ratio of the inverse distances.

In the case of gunpowder and other explosive compounds, in the solid form containing in themselves the elements whose combination gives rise to the phenomena of combustion, the action of the chemical forces can take place in a confined space, and the accompanying repulsion, resulting from both the temporary heat and the permanent expansion of the resulting gases, may be availed of as a direct dynamical agent not only in impelling projectiles, but also in giving motion to a piston acting in a cylinder like that of a steam-engine. The same result can be obtained from the close combustion of explosive gaseous mixtures; and a considerable amount of power can be made available from the direct pressure arising from the temporary heat of the explosive action, even when the bulk of the resulting gases is not, after cooling, greater than that of their constituents,—as experiment shows that in some such cases a momentary increase of bulk of 14 or 15 times the volume when cold may result, indicating a temperature of at least 7000° F. But in obtaining motive power from fire, the molecular motion of the flame is generally transmitted to water in a closed boiler, where, by increasing the intestine movements of the liquid particles, it raises the temperature until, at a point depending on the superincumbent pressure, the particles in more immediate contact with the source of heat undergo a sudden change of condition, and, absorbing instantaneously a large additional amount of heat (at the ordinary temperature of ebullition), assume the state of vapour and rise to the surface. Under a pressure of 1700 atmospheres water might be heated to about 1200° F. (or 1000° above the common boiling-point), in which condition it would contain heat enough to convert the whole mass into steam, the available amount of repulsion being in proportion; so that by expanding down to atmospheric pressure against a resistance gradually decreasing from 1700 atmospheres to 1, it might perform a quantity of work eight or ten times as great as is generally obtained from the best steam-engines. This enormous amount of potential energy stored up in the superheated water in the shape of highly excited repulsion, would seem to be due more to the concentration of the heat in possession than to any excess of quantitative heat which the hot fluid may be supposed to contain above the heat of conversion of the same weight of steam at atmospheric pressure.

Energy may be put into an elastic fluid, or rather the fluid can be put into a condition of increased energy, either by compression or by the actual addition of heat; and in both cases the quantity of sensible heat, or the temperature, is increased. I have before expressed my opinion that compression, apart from the idea of molecular friction, is not a source of quantitative heat, but only of concentration of heat or temperature. In the act of compression the particles are forced closer together; and supposing the total quantity of heat to remain unchanged, the increase of temperature may result from concentration of molecular action as to the number of vibrations made by each particle in a given time. By the actual addition of heat to an elastic fluid under constant volume, the same effect may be produced of increasing the number of molecular vibrations in a given time. In both cases the heat-tone may be supposed to become more acute from a corresponding change of the thermic rhythm of the molecular vibrations. In compression the increased repulsion is accounted for by the decrease of molecular distances, and *by the addition of heat under constant volume the molecular distances are also in effect decreased*; for by the addition of heat or molecular *vis viva,* the particles of a gas moving with increased energy would approach closer together at some points of their orbits, and recede further asunder at the opposite points; and the general result of the increased energy of the molecular movements would be an increase of tension if the repulsion increases, in some geometrical ratio of the inverse distance, because *the repulsion would increase by the closer approach of the particles at their points of greater vicinity more than it would diminish by the increased distance at the opposite points.* Thus the tension of elastic fluids (and the activity of thermo-dynamical phenomena generally) may be referred to the common principle of molecular repulsion, called into action by heat considered as molecular motion.

The motion of matter in masses may be stopped in various ways, and transformed into different shapes of energy; but it is difficult to conceive how molecular motion could be stopped by any arrangement or combination of common matter. Molecular motion is being continually produced in natural phenomena, and by artificial agencies, at the expense of energy in various shapes. It can be transferred from one body to another, and modified indefinitely as to its intensity; but its production being continual, it must be supposed to accumulate indefinitely, while the other forms of energy from which it has its origin must at the same time be decreasing in equal proportion—an assumption which is contrary to all rational probability. This difficulty does not exist in the dynamical theory of heat, as this theory supposes a direct conversion of heat, or molecular motion of common matter, into work in the same proportion as work is actually proved to be convertible into heat or molecular motion by friction; and thus a continual and invariable circulation of phenomena by interchange or transformation in terrestrial dynamics can be satisfactorily accounted for, *if the direct conversion of heat into work can be proved to be true.*

Any theory of phenomena which involves the circumstance of a continually accumulating amount of disturbance of the mobile equilibrium requisite for the general permanence of physical existence must be considered unsatisfactory. The meteoric theory of the sun's replenishment, independently imagined by Mayer and by Mr. Waterston, and admirably developed by Professor W. Thomson, is based on reasoning which might obtain for it universal favour, were it not for the idea it involves of a continual transfer of matter in one direction only, and its consequent accumulation in the sun, which is opposed to the ideas of constancy and permanence which naturally present themselves to the unprejudiced mind in contemplating these sublime phenomena. Now by an interesting coincidence it appears that, by supposing the meteoric theory so modified as to avoid the foregoing objection, it would result from this modification that terrestrial molecular motion may, in agreement with known physical laws, be converted into the force of gravitation by the agencies of the meteoric phenomena, thus removing a strong objection which would otherwise oppose the repulsion theory of thermo-dynamics which I am endeavouring to develope, while at the same time it supports the idea of a general cosmical stability and permanence. In speculations of this kind we cannot expect to arrive at conclusions beyond a mere preponderance of probability on one side or the other of any undecided point; and in such investigations the ardent study of the limited train of facts we possess inevitably leads into the regions of the unknown; for the vast field of research offers no boundaries; no one part of the subject can be fairly detached from the apparently endless circuit of connexions and relations with which it is bound; and in directing our experimental researches beyond the narrow limits of established fact in search of additional truth, it were unwise to reject the suggestions of analogical probability.

Radiant heat is supposed to be propagated in the same manner as light. All bodies radiate heat, and every point of the surface of each is a distinct centre from which undulations of the universal æther proceed in straight lines in all directions without interfering with each other, or with the innumerable waves with which space is filled. Heat in common terrestrial matter has been well designated a mode of motion; but it must be conceived of as molecular motion of a vibratory or orbital character, limited by the balance of forces affecting the particles of a mass under given circumstances. No uninterrupted independent rectilinear motion, either in an atom or a mass, or in the stright-line transmission of radiation or electricity, can be imagined of as heat, but rather as *vis viva*, which may become heat when the motion of a body is stopped and transformed into individual movements of its own particles, or of those of other matter.

It is allowed that the chief physical source of terrestrial energy is the sun. The wonderful balance of the perturbations of the solar system, as shown by Laplace and other astronomers, and the rational probability of the immense antiquity of the earth's existence indicated by geological evidence, suggest ideas of the measure of time in cosmical existence somewhat in agreement with the overwhelming measures of quantity and extension in the universe which even the limited powers of man can faintly appreciate. All things finite must have had a beginning, and *may have* an end; though the certainty of the prospective eternity of our spiritual existence is gladly acknowledged by the wisest and the best, and it is difficult to conceive of the annihilation of even common matter. It is probable that our planet has already undergone changes the nature of which may never be clearly revealed to human intelligence; and whenever the great catastrophe may occur which is to change the present order of terrestrial existence, is it not probable that the agencies employed, though not opposing or inverting natural law, may still differ essentially from the ordinary routine of physical phenomena which, with a continual rotation of periodic change, conveys the idea of general cosmical permanence?

Analogy would indicate the probability that the planets may be receiving from the sun something more than mere motion, or radiant heat and light, as generally described, and the idea is not perhaps without support from physical facts. The vital solar fluid which reaches the earth may become, as it were, analyzed and assimilated in the various active processes of nature by which organisms are built up; whilst, besides radiant heat, there is emanating from the earth into space a current of comparatively effete cosmical matter which joins the general effluent solar tide, carrying with it part of the *vis viva* of the terrestrial molecular motion which we call heat, and another portion of this heat is transferred by the universal æther, in the act of radiation, to the solar tide. The accumulation of molecular motion on the earth's surface is thus prevented by a continual change into the *vis viva* of the motion of translation of the effluent particles, which, with the general solar tide in their outward progress, go on changing this *vis viva* into potential energy of centripetal gravitation.

There is reason to believe that the idea of a universal ocean of interstellar æther has been entertained from the remotest times of physical speculation; and this idea still prevails, being supported by a strong preponderance of probability. The undulatory theory of light shows that the mighty pulse of this cosmical vital fluid is beating throughout the universe at the amazing rate of four hundred and seventy-four millions of millions of strokes per second! All these pulsations are caught by the delicate optical mechanism of the eye in a single second, and the resulting impression is that of light. From this single manifestation some faint idea may be conceived of the wonderful properties which should characterize this cosmical fluid. As compared with terrestrial matter, it is supposed to be imponderable and comparatively non-resisting, and to be universally present.

It is known that the sun has an atmosphere containing in a state of vapour or gas several substances which exist on our planet in a solid form. How far may the solar atmosphere be imagined probably to extend? The retardation of Encke's comet is generally attributed to the resistance of the interstellar æther; but it is not improbable that the extension of the solar atmosphere may play an important part in producing the effects observed. The meteoric theory of the sun's heat has much probability in its favour. The immense number of meteors which are periodically seen from some parts of the earth's surface would seem to prove the existence of an incalculable crowd of small asteroids circulating round the sun. Moving through a resisting medium, these small masses may be naturally supposed to be continually suffering a loss of projectile force or *vis viva* of orbital motion, and consequently to be gradually approaching nearer and nearer to the sun. The zodiacal light is supposed to owe its existence to these crowded meteoric masses. "However this may be, it is at least proved that this phenomenon arises from matter which circulates in obedience to planetary laws; the entire mass constituting the zodiacal light must be continually approaching, and incessantly raining its substance down upon the sun"*. The heat produced by the collision of an incombustible asteroid thus falling into the sun, has been calculated to be equal to 9000 times the heat generated by the combustion of an equal asteroid of solid coal. Thus in the force of gravitation we perceive an agency competent to maintain at the surface of the sun a temperature far surpassing anything we can conceive of terrestrial combustion. Helmholtz has shown that the force of gravitation now existing as potential energy in the principal bodies of the solar system might, if converted into heat, raise the temperature of a mass of water equal to the sun and planets in weight 28 millions of degrees Centigrade. From the incomparable penetrative power of the solar rays, it is inferred that the temperature of their source must be enormous; and is it not probable that the almost inconceivable temperature of the fiery ocean forming the sun's surface should volatilize the meteoric matter continually falling into it, reducing it to a state of gaseous tenuity far beyond anything we can perceive in terrestrial phenomena, and that from this vast alembic the subtle though still ponderable matter of the sun's atmosphere should be emitted in a continuous stream radiating into space? It has been supposed by eminent physicists that gaseous particles fly in straight lines through space; and if the atomic projectiles from the sun's surface be supposed to be uninterrupted in their outward progress, they may be imagined to move with a velocity which, in combination with the outward impulsion of the excited æther, should carry them with gradually diminishing motion to an immense distance into space (probably far beyond the orbit of the

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* Prof. Tyndall.

most distant planet of the system), till, arrived at the limits where the repulsive force and the power of gravitation are in equilibrium, the outflowing current should come quietly to a state of rest on the vague shores of the solar aërial ocean in circumambient space. Here the solar atoms would have no motion, except perhaps some slight vibratory movement from the almost exhausted undulations of the æther in these distant regions of space; but gravitation would soon again begin to predominate, and each atom, like a pendulum in vibration, would begin gradually to return towards its centre of gravitation in the sun. While thus on the verge between their outward and inward movements, the atoms would be in a state of almost perfect equilibrium or neutrality of general forces; and the meeting of the returning ripples with the spent waves still moving slowly on, under the expiring influence of outward repulsion, might induce an approach of the atoms sufficient to cause aggregation, from the effect of mutual attraction in the absence or abeyance of other forces.

We nowhere perceive homogeneity in nature; and even the distribution of highly rarefied gaseous matter in space cannot be supposed to be effected with strict geometrical symmetry. The slightest inequalities of distance between the atoms hovering on their boundaries of solar gravitation would form centres of aggregation under the influence of mutual atomic attraction, which would become the nuclei of meteoric formations. In the agglomerated masses some incipient heat would be generated in the act of aggregation, by converting the motion of translation with which the particles approached each other into reciprocating or orbital molecular motion; but the general temperature would probably be low, and the newly formed meteoric masses might be of considerable density.

The return of the meteoric matter, in the shape of these newly formed or still forming masses, to its central source in the sun presents an interesting subject of speculation. Whatever ideas may be entertained of the interstellar æther as a resisting medium, it seems certain that the meteorites falling towards the sun would meet with considerable resistance from the outflowing tide of the solar atmosphere. Many years ago a theory of the sun's dynamical action appeared which was perhaps entitled to more attention than it received. It supposed that the centrifugal action of the sun's rotation on his axis causes his equatorial rays to issue obliquely in the direction of the motion of the planets, and thus to influence the planetary motions by direct dynamical action. The meteorites in their approach towards the sun would be deflected from a direct radial course by the slanting direction of the issuing lines of the solar current, and would thus be gradually thrown into orbital courses which might continue for a long time under the influence of planetary laws. Finally, the effect of the resisting medium causes the sun's attraction to preponderate more and more, and the great stream of meteoric matter rolls on, gradually approaching the sun, in a vast spiral vortex. As graphically imagined by Prof. W. Thomson, "each meteor thus goes on moving faster and faster, and getting nearer and nearer the centre, until some time, very suddenly, it gets so much entangled in the solar atmosphere as to begin to lose velocity. In a few seconds more it is at rest on the sun's surface, and the energy given up is vibrated across the district where it was gathered during so many ages, ultimately to penetrate, as light, the remotest regions of space." It is supposed that the principal store of energy which is to furnish future sunlight is in the *vis viva* and the gravitating power of the meteoric bodies at present circulating inside the earth's orbit, and probably giving origin to the zodiacal light. However vast these clouds of meteoric matter may be, analogy would certainly indicate that their decrease from the fiery rain which they are continually pouring down upon the sun should be made up from some perennial source of supply, and also that some cosmical process should prevent the continual growth of the sun's mass which would result from the conditions of the meteoric theory in its present apparently incomplete form. If the idea of the volatilization of the meteoric matter from the sun's surface, and its diffusion into space, does not involve any contradiction to known physical laws, the system of solar meteorology which I have endeavoured faintly to sketch would tend to show, 1st, that a continuous supply of solid meteoric matter may be accounted for; 2ndly, that the sun's mass may remain constant; and 3rdly, that the molecular motion of common matter, supposed by the repulsion theory of thermo-dynamics to be directly inconvertible into work, may be converted into the force of gravitation by the supposed phenomena of the meteoric theory; and hence would result strong presumptive evidence of a general stability of equilibrium in all the physical phenomena of the universe. The quantity of heat emanating from the sun is so vast that no physical theory of its replenishment can be considered satisfactory which should not provide for an indefinite supply of heat-producing material; and no physical theory can meet this requirement except on the principle of circulation and transformation. And are not all natural analogies in favour of such a principle?

However attractive such speculations may be to the fancy, I should not choose to obtrude hypotheses of cosmical dynamics unconnected with practical objects. In my long observations on the working of heat-engines, I have not been able to verify the disappearance of heat supposed by the dynamical theory to be converted into work; and no experiments that I have been able to devise, or that have, to my knowledge, been made by others, have proved the fact. Hence I have been led to investigate the subject from a different point of view, in the hope of discovering some satisfactory clue towards the unravelling of this very intricate inquiry. My chief aim in these pursuits has been, and is, to see a satisfactory application of sound thermo-dynamical principles to the improvement of our heat-engines. An eminent scientific authority has called the practical application of thermodynamics "the grandest question of all;" and while our practical engineers and mechanicians should encourage the noble ambition of elevating their technical operations to the rank of true scientific performances, our eminent men of science should not disdain to study in its practical details the Titanic engine which forms one of Britain's noblest boasts; and thus with united energies we may reasonably hope to achieve improvements hitherto unlooked for in our thermic prime movers.

Palermo, May 23, 1864.

XLVI. *On Shooting-Stars.* *By* Father SECCHI[*].

THIS year, as in 1861, the electric telegraph between the observatories at the Collegio Romano and at Civita Vecchia was employed in order to make simultaneous observations, at these places, on the shooting-stars of August. The object of these observations was to determine the height at which these meteors become luminous, and to ascertain the limits of our atmosphere. The observations were commenced on the 5th, and concluded on the 10th of August. There were four observers at Rome, between whom the whole celestial vault was divided; and two at Civita Vecchia, who had instructions to observe the north-eastern half of the heavens from the Great Bear to Aquarius. M. Stabuti and Prof. F. Armellini had the kindness to assist me in these observations; the former, aided by MM. Devarno and Caravani at the chronometer, observed at Civita Vecchia; the latter, together with the other assistants of the observatory, observed with me at Rome. The object of these observations being to fix, precisely, the positions and trajectories, rather than the number of the meteors, many of the latter (contemporaneous ones included) were neglected when their number prevented the positions of all from being well determined.

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[*] From a letter to M. E. de Beaumont, published in the *Comptes Rendus de l'Académie des Sciences*, September 26, 1864.

The apparition of a star was indicated from one place to the other by a touch of the telegraph, given at each place for each star seen; and when the touches at both places were contemporaneous, the position as seen from Civita Vecchia was immediately telegraphed to Rome, so that we were able to see at once what parallax existed between the two stations.

The following Table shows the results, relative to the number of meteors, which were obtained:—

Date of observation in August.	Period of observation.			Number of stars observed.			Proportional number per hour.	Rectified number.
	Commencement.	End.	Duration.	Contemporaneous.	Non-contemporaneous.			
					At Rome.	At Civita Vecchia.		
	h m	h m	h m					
5	8 48	10 14	1 26	5	31	11	28·9	
6	8 45	10 23	1 38	12	38	8	30·6	
7	9 15	10 28	1 13	8	43	3	41·9	
8	9 39	10 37	0 58	20	34	21	55·8	
9	9 8	10 48	1 40	19	53	19	43·2	
10	9 2	12 5	3 13	29	118	63	45·7	63

The moon and mists were somewhat troublesome on the 9th and 10th; the rectified number expresses the result, in one hour, when moon and mist were both absent. The contemporaneous observations proved, as in 1861, that the parallaxes were, in general, enormous, but nevertheless such as to establish the considerable height of our atmosphere. Notwithstanding the indetermination caused by errors of observation, it was impossible, even for an inexperienced observer, to be deceived, since the parallaxes changed the places of entire constellations. Nevertheless we shall see that much uncertainty still remains with respect to the distances, and that the solution of the problem is not so simple as is generally believed.

The determination of the heights of these meteors would be very easy if, in reality, the same point of the trajectory had been observed at the same instant from both stations. For if the two visual rays to this point (say the commencement) were drawn, from their intersection a perpendicular let fall on the plane of the horizon, and the foot of the latter joined to the two stations, a pyramid would be obtained the length of whose vertical edge would be the height of the star. The magnitude of this perpendicular might be obtained, either from calculation, or by a graphic construction similar to that employed in gnomonics for vertical, declined dials. It is merely necessary to know the azimuths observed at the two stations, and the angular altitudes. From this construction it follows, too, that the magnitude of the perpendicular ought to remain the same when the elevations of the two stations are employed; the degree of accordance between the two results serves as a criterion of the trustworthiness of the observation. Repeating the same construction for the end of the trajectory, another point is ascertained, after which the direction and inclination of the actual trajectory are readily deduced.

In order to determine the coordinates of altitude and azimuth, the trajectories were first transferred to a celestial globe 0·53 metre in diameter, the apparent trajectories corresponding to the two stations being marked with different colours. Afterwards all the altitudes and azimuths, of the sixty-nine contemporaneous trajectories which were visible from beginning to end, were determined on the globe itself (placed according to the sidereal hour, which had previously been calculated from the mean times of observation) by means of a moveable vertical quarter of a circle.

These preliminaries completed, I proceeded to the graphical constructions, referring all to the horizon of Rome, considered as parallel to that of Civita Vecchia; for the difference scarcely exceeds half a degree, the distance between the stations being merely 65 kilometres.

The results of these constructions showed that in a great number of cases the values of the perpendicular heights agreed tolerably well, or at all events that they could be made to agree by supposing very probable errors of observation amounting to 1 or 2 degrees. There were many cases, however, in which this agreement was inadmissible. Nevertheless, as no doubt could be entertained of the contemporaneity of the observations, and as the general directions of the apparent trajectories accorded with those which parallax required, we were compelled to admit that the same point of the actual trajectory had not in reality been observed, but that the star had been seen from the two stations at different points of its course.

This conclusion involves no improbability; for, *first*, the light of many stars is very feeble at the beginning and end of its visible course, and a difference of distance amounting to 60 or 70 kilometres might easily render it invisible from one of the two stations; *secondly*, notwithstanding all possible care and attention, the eye perceives the star only after its course has commenced, and it is not at all uncommon to find disaccordance even between two observers at the same place; and *lastly*, the strength of the observer's vision must affect essentially the result. This is proved indirectly by the fact that, for the observed ignition, at the middle of its course, of a star which burst into a red-coloured flame and was seen from both stations, the construction gave a height nearly identical for both stations, amounting to 105 kilometres.

Here, then, is a new and unexpected difficulty in obtaining exact results. Its solution, nevertheless, is not difficult. It will suffice in fact to apply in such cases the principles of descriptive geometry, and to trace the direction of the real trajectory according to any two portions of the apparent trajectories seen from the two different stations. Prof. F. Armellini kindly undertook this determination in many of the cases which proved most difficult and rebellious to the simple geometrical construction. His results were very satisfactory; it was thus proved that these cases followed the rule applicable to the others, but it was necessary to admit that the star had become visible at different times from the two stations. Another consequence of these more accurate constructions is the approximate correctness of the mean of the two heights deduced from the simple construction—at all events when the latter do not differ too greatly.

This premised, I proceed to the results obtained for the heights of the meteors. In round numbers the mean of all these heights was from 101 to 100 kilometres. There were, in fact,

6	stars whose heights were between	40	and	60	kilometres.
7	,, ,, ,,	60	,,	80	,,
10	,, ,, ,,	80	,,	100	,,
17	,, ,, ,,	100	,,	120	,,
3	,, ,, ,,	120	,,	140	,,
5	,, ,, ,,	140	,,	160	,,
2	,, ,, ,,	160	,,	180	,,
1	,, ,, ,,	180	,,	200	,,
2	,, ,, ,,	200	,,	220	,,
3	,, ,, ,,	200 and upwards.			

Thus the heights of 27 out of the whole 56 stars (about 50 per cent.) fell between 80 and 120 kilometres. As in many cases the trajectory was perpendicular to the vertical plane passing through the two stations, the height of the trajectory was constructed directly by the principle of parallaxes, and amounted to 93 kilometres. The mean of the heights of the points of extinction was 75 kilometres. The greatest of the observed heights amounted to between 240 and 260 kilometres; but it must not be forgotten that these values may have been influenced by the errors of observation above discussed. Nevertheless in these cases the heights must have exceeded 200 kilometres.

Another remarkable circumstance is the small horizontal distance of these meteors: in no case did it exceed 222 kilometres, or 2 geographical degrees. A somewhat curious consequence of this is that no meteor seen from one of two stations, more than 444 kilometres asunder, can be identical with any of those seen from the other. If the space that can be examined by an observer from a given station were referred to a globe half a metre (in diameter), we should find that a franc-piece would cover just as much of its surface.

This explains why, in certain showers of shooting-stars, the latter have often been so concentrated in one place and wholly invisible in another, and also why the shooting-stars of the period of the 10th of August are not visible in the southern hemisphere. We may also obtain from the above considerations some conception of the prodigious number of these meteors; for if we were to take a circle whose radius is equal to the distance from Rome to Paris, and to suppose the density of these meteors to be 63 per hour (as actually found this year at Paris by M. Coulvier-Gravier, and at Rome by ourselves), the number of meteors falling thereon, daily, would be found to be 18,144. This density, however, is very small, and the surface in question is not even equal to the half that of the continent of Europe.

In conclusion, we must, I think, admit that the height of our atmosphere exceeds 200 kilometres ($124\frac{1}{4}$ English miles), and that at this elevation the density of the air is sufficient to excite light when violently compressed at the surface of these bodies. I say to *excite light*, and not always to produce combustion; for, according to the observations where a veritable combustion was seen to be determined in the middle of the star's course, it may be questioned whether in reality every train is an actual combustion, or whether it may not arise from the production of an electric light developed during the violent friction of the meteor against the air; the heat accompanying which, however, may sometimes elevate the temperature of the body to the point of fusion. Subsequent observations must decide this question.

In closing this letter I may observe that the point of departure of most of the shooting-stars always lay between Cepheus and Cassiopeia, but that the parallax must necessarily cause this point to vary for different stars as well as for the same star as seen from different stations. Thus a star which to us appeared to be altogether without tail, the eye being in its direction from Cassiopeia, was seen from Civita Vecchia with a pretty long tail and in another part of the heavens. Two, indeed, were observed to have *opposite* directions; but these manifestly *moved very slowly.*

XLVII. *On Molecular Physics.* *By* Prof. W. A. Norton.

[Continued from p. 282.]

IN considering the changes of state through which the same substance may pass, we have been led to recognize, as an important physical principle upon which the mechanical properties manifested in each new condition in a great degree depend, that the physical condition of the individual molecules is liable to permanent variations from the effect of heat, and that these variations consist in expansions of the electric atmospheres which surround the atoms of the molecules. If we take a more extended view, and consider the diverse permanent changes of condition which the same substance may experience while in the same state of aggregation, we may discern the operation of a still more comprehensive principle, viz., that the physical state of the atmosphere of a molecule, and therefore the curve which represents its action upon surrounding molecules, is liable to permanent alteration from the action of external forces generally. It is well known that if a mechanical force, of considerable intensity, be applied for a short interval of time to a body, the result will be a permanent change in its form. The experiments of Hodgkinson have indeed established that a certain set may be imparted to bars of cast iron, by a temporary load which is but a small fraction of its breaking load, and that "there is no weight, however small, that will not injure the elasticity" of such a bar. As we cannot suppose that a given mass of molecules, while retaining forces of mutual action of unvarying intensity, can take up an infinite number of positions of equilibrium, differing but slightly from each other, we must conclude that the individual molecules experience some change of condition, which occasions a change in the intensities of the forces they exert in a given direction.

From our present theoretical point of view, such possible changes of condition consist in compressions, or expansions, of the molecular atmospheres, either as a whole or unequally on different sides. In the former case there will be a variation in the size of the atmosphere, and in the intensities of the forces of attraction and repulsion exerted by the molecule at a given distance, but the forces exerted in different directions will have an equal intensity. In the latter case there will be a variation in the form of the atmosphere, and an inequality of action in different directions. The form assumed will be spheroidal, or approximately so, supposing it to have been originally spherical; and the mechanical result will be the exertion of an increased force of attraction from the sides of the molecule at which its atmosphere is compressed, and a diminished force from the sides at which the atmosphere is expanded. This follows, as a necessary consequence, from the fundamental conception of molecular forces, developed on pages 199 to 204, as may be distinctly seen by attending to the values of $\frac{n}{m}$ and r. It will thus be seen that the molecular atmospheres, in assuming the spheroidal form, under special circumstances, determine the existence of molecular axes of cohesive attraction, "whose force is inversely related to their length." In the direction of such axes, then, the limit of stable equilibrium (O*a*, fig. 1, p. 203) will be least for the shorter axis, and greater for the longer axis.

When a force of pressure applied to a body determines a permanent compression, the molecular atmospheres remain compressed, or in closer proximity to their central atoms, and the force of cohesive attraction is permanently increased, and the limit of stable equilibrium diminished. The heat developed is a necessary result of the compression of the atmospheres. If the elastic reaction to the pressure after it is withdrawn were perfect, the atmospheres would resume their original form and dimensions, the original molecular forces would be recovered, and the heat evolved would be absorbed again. In general when mechanical forces are applied to a body, the heat evolved, or absorbed, is a necessary accompaniment of the compressions, or expansions, superinduced in the atmospheres of the particles, and may be regarded as a sensible indication of the extent of such changes of molecular condition. The mechanical work, of which the heat evolved serves as the measure, is expended in urging the atmospheres nearer to their central atoms.

On the other hand, if heat be directly applied to a body, it has a tendency opposite to that of a mechanical pressure, or to expand the molecular atmospheres, and so to reduce the intensity of the cohesive attraction at a given distance. It is to be observed also that heat has a tendency to dissolve the groups in which the particles of a solid may be aggregated, and, when the point of fusion is reached, will effectually break up such groups and bring the mass to the condition of a homogeneous and symmetrical arrangement of molecules.

Solidification, or Crystallization.—It is a well recognized principle that solidification and crystallization are the same process. This great principle was first propounded by the learned and acute Dr. Young in 1807, in his lectures on Natural Philosophy. It has also been advocated by Biot and other physicists, and more recently has been reasserted and ably sustained by Professor Dana, in his admirable paper "On Cohesive Attraction," published in vol. iv. second series, of Silliman's Journal. If now it be admitted that solidification is in every instance but a more or less perfect crystallization, it will be perceived that the investigation of the mechanical process of crystallization must consist essentially in an inquiry into the conditions and results of the operation of the molecular forces under special circumstances. The general nature of these forces, and the laws of the variation of effective molecular action with the distance between two molecules, have already been under discussion. We have also seen (pp. 382, 383) that the mechanical condition of an individual molecule is subject to change under the operation of heat, and external forces generally, by reason of a change produced either in the dimensions or form of the atmosphere of the molecule, and that it may thus acquire permanent axes of attractive force. To establish a sufficient basis for a general explanation of crystallization, we have only to remark further that the molecule of every particular substance has primarily and inherently its own special physical condition, by virtue of which it exercises an effective action that would be represented by a special curve, and experiences under the operation of heat and other causes its own peculiar changes of mechanical condition. Upon this idea it may be seen that every substance may have its particular form of crystallization, although the molecules should be devoid of all natural polarity.

The different systems of crystallization may be regarded as so many different systems of equilibrium of masses of molecules, under the operation of molecular forces diversely modified by the circumstances that determine the crystallization. The general nature of the modifications consists in a spheroidal form imparted to the molecular atmospheres, and the consequent development of certain axes of attraction—that is, of diameters of least or greatest length, in the direction of which the attraction has at a given distance a maximum or minimum value, and the limit of stable equilibrium (O*a*, fig. 1, p. 203) a minimum or maximum value.

Crystallization begins at a certain point of a liquid, and is generally determined by the loss of heat, or the evaporation of the liquid solvent. We already have seen reason to believe that the molecules of the liquid have a symmetrical arrangemeut previous to the crystallization (p. 279). Whether this be admitted or not, such an arrangement obtains in the crystal formed from the liquid. The particles successively take positions in the corners or angular points of a series of polyhedral figures; as cubes, prisms, octahedrons, &c. Any two such figures lying contiguous to each other, have a common face, or, as in the case of the octahedron, a common angular point. The crystallization takes place either successively or simultaneously in the faces of these figures. We have then first to consider the process of crystallization as it may occur in a single plane.

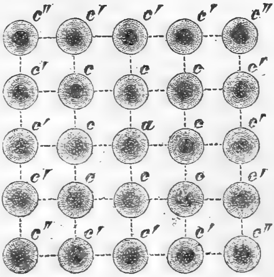
The result of every such process is the arrangement of the molecules in the angular points of a series of quadrilaterals; which may be squares, rectangles, rhombuses, or rhomboids. If we suppose, in the first instance, several molecules to unite along a single line, and molecules posited on either side of this line to unite with those already crystallized, three different general modes of arrangement may occur: the new particles may take up positions opposite those of the first line, or opposite the middle points of the intervals between these particles, or opposite other than the middle points of these intervals. In the first case, squares or rectangles will be formed; in the second, rhombuses, which may in special cases be squares; and in the third, rhomboids, which in special cases may be rectangles. The general tendency of the crystallization occurring along the first line should be, by reason of the compression of the molecular atmospheres along this line, and the consequent expansion of them in a direction perpendicular to it, to develope an axis of increased attraction in this primary line of crystallization, and an axis of diminished attraction in the perpendicular direction. When this result is reached, and successively along the lines of particles parallel to the first, the figure assumed will be either a square, a rectangle, or a rhombus. The two molecular axes will be coincident with the sides of the minute rectangular figures that make up the larger rectangle, and with the diagonals of each minute rhombus. The condition essential to the formation of a square is that the properties of the molecules in reference to cooling (or, in general, in reference to the propagation and absorption of impulses) should be such that each set of four contiguous molecules are, when in the incipient state of crystallization, in the same physical condition. That a rectangle may be formed, a group of four particles must unite; but the escape of the heat-pulses that occurs primarily in the direction of one of the sides of the rectangle must determine a greater compression of the molecular atmospheres in this direction than in that of the other side. That the figure of a rhombus may be assumed, two particles must first unite, and subsequently two other particles must take up, under the attractive action of these, positions opposite the middle of the interval between them. To understand how a rhomboid may result, we must observe that when a line of particles is crystallizing, each particle, m, as it becomes united, exerts a certain disturbing action upon a particle m' next in the line, and also upon a contiguous particle n at one side of the line. When the particle m' unites, it also modifies the condition of n; but as its action is subsequent to that of m, it is possible that in special cases the final result may be an inequality of disturbance, so that m and m' will attract n unequally, and the position of equilibrium assumed by it will in consequence not be opposite the middle of the interval between m and m'. In this case the electric atmosphere of n will have a form differently modified, and its second axis will be oblique to the line of primary crystallization in which the axis first developed lies.

The figure first assumed must then depend upon the fundamental properties, with respect to heat, &c., of the individual molecules of the substance; and the general tendency must be, for the first group of molecules to acquire increased dimensions by successive solidifications of a series of figures similar to the first.

In what precedes, we have had regard only to the play of the ordinary molecular forces, and the modifications which the ordinary molecular action may experience from the loss of heat, and in the act of solidification; but there must result in each instance of the union of two particles a modification of the condition of their atmospheres, which should develope a new force of attraction that may play an important part in the continuation of the process of crystallization. For when two particles unite by crystallization, their atmospheres on their nearer sides will become compressed, and consequently on their further sides expanded. Each molecule will thus virtually be brought into an electro-polar condition, with the positive pole turned outward. This positive pole, or excess of electric æther, tends to bring all the molecules lying in the prolongation of the line of the first two into the same electro-polar condition, and in this state of induced polarization a force of electric attraction will subsist between the particles. As one particle after another in the line comes to unite with those previously crystallized, its previous polarization will be enhanced, and it will exert an increased attractive force upon those not yet crystallized. At the same time, by the compressive and repulsive action of the contiguous atmospheres, the particle with which the new one unites will lose a considerable portion of its polarity. This reflex action, attendant upon every act of union, should eventually greatly diminish, if not wholly remove, the prior induced polarization.

Another effect of this reflex action to be noticed is, the increased expansion of the atmosphere of the molecule which experiences the reactive compression in the direction perpendicular to the line of crystallization. This molecule thus acquires an increased positive polarization on the outer side, lying in this perpendicular direction, and therefore exerts an increased force of electric attraction in this direction. In the varying operation of this induced electric polarization, and of the reflex action just noticed, we may discern the probable origin of those supposed variations of axial attraction which Professor Dana has shown will suffice for the explanation of secondary planes in crystallization.

Fig. 2.



To illustrate by a special case, let fig. 2 represent a process of crystallization in which the particles are arranged in successive squares. When *c, c, c* are the outer particles, their outer sides will be positively polarized, and will consequently exert an electric attraction, in addition to the increasing molecular attraction that results from the cooling, upon the molecules c', c', c' immediately exterior to them. The next step in the process should be the union of the molecules thus attracted; and all of these molecules should have the same tendency to unite, unless there should be a material inequality in the physical condition of the outer particles *c, c, c, c* on different sides of the square, *c c c c*. But the four corner particles, c'', c'', c'', c'', cannot thus be directly brought into union with the particles of the crystal. They must either unite with the nearest particles of those newly attached, or remain disunited, to become incorporated at a later stage of the process. In the normal or complete growth of the crystal, the first would be the result. If, on the other hand, the forces of the new lines of outer particles c', c' c', c' should fall off materially in intensity, so that the corner particles c'' are not taken up in the same step of the process as the others, secondary lines would arise at the angles of the square c', c'', c', c''. In the whole cubical crystal, of which fig. 2 represents a section, *secondary planes* would be formed at the edges. Every such line or plane may have different positions, according as the corner particles in question (c'') become incorporated in the next stage of the growth of the crystal, or in some of the subsequent stages. This Professor Dana has distinctly shown. What we have here to observe is simply that, by reason of the reflex action above noticed (p. 386), when new particles become united to c', c', these particles, c', c', will in fact exert a more energetic attraction laterally upon c'', c''; and hence the union of c'', c'' with the crystal may then be determined. If not, the next augmentation of the attractive force attendant upon the union of the next set of molecules may determine this result.

In order that a complete polyhedral crystal may be formed, it is necessary that molecules on one or both sides of a plane in which crystallization occurs should become united with those that take up their positions at the angles of the plane figure. Thus two sets of four molecules in parallel planes may take up positions of equilibrium at the eight angular points of a cube, or six may form an octahedron, &c. The conditions that will determine the figure of equilibrium assumed may be inferred from the general considerations already presented (pp. 385 and 386). The compression of the molecular atmospheres in the first plane of crystallization will tend to develope a third axis of attraction perpendicular to this plane. Various systems of crystallization are possible, since the particles on one side of the first plane may take up positions of equilibrium perpendicularly opposite to those that crystallize in this plane, or opposite the intervals between parallel pairs of these particles, or opposite the centre of the quadrilateral figure which they form*.

Professor Dana has shown that the various fundamental forms of crystals may be obtained by regarding the crystal as a mass of bipolar molecules, of a spherical or spheroidal form, in contact along certain lines, which are the conjugate axes or conjugate diameters of the spheroids. This conception of the constitution of a crystal is, in a geometrical point of view, equivalent to that which has now been given. For we have only to conceive spheroids to be inscribed in the polyhedral figures of the compound crystal molecules just supposed, to obtain the representative spheroidal molecules of Prof. Dana; which will also touch each other along similar conjugate diameters of the different spheroids. It is not difficult to make out the various positions of equilibrium of the particles that must obtain in the different fundamental forms, and the various physical conditions of the particles upon which these forms must depend†.

In his paper "On Cohesive Attraction," Professor Dana has apparently put the explanation of the *cleavage* of crystals on the true basis, by attributing it to alternations in the intensity of the attraction in a series of parallel planes. If such alternations really exist, we naturally seek for the explanation of them in alternations of the mechanical condition of the molecules upon

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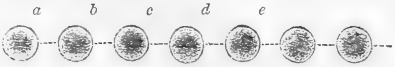
* This is only a partial view of the matter.

† The hypothesis of a permanent polarity of atoms or molecules has subserved a valuable purpose in linking together phenomena under one physical conception in several departments of physical science; but the progress of science has materially tended of late to shake the confidence reposed in it as a supposed truth of Nature. It will be conceded that it is the dictate of true philosophy to hold it in abeyance until it shall have become abundantly evident that the phenomena in question cannot be deduced from the fundamental conception of the constitution of a molecule, and of the primary forces of attraction and repulsion, to which all other molecular phenomena can be referred.

which the energy of the molecular forces depend. It is conceivable that such differences may result from the heat evolved in the process of crystallization. Let fig. 3, *a*, *b*, *c*, *d*, *e*, &c., be a line of particles crystallizing in regular succession. When *a* unites with *b*, the heat given out will expand the atmosphere of *c*; and it is possible that, after this effect has been produced, the expanded atmosphere will not become condensed, under the operation of the crystallizing forces, as much as it otherwise would have been, and hence that the molecular attractive force of *c* will be less than would have otherwise resulted. If the attraction between *b* and *c* should thus be materially lessened, there would be in consequence less heat evolved in the union of the two, and so the attractive force of *d* would be less weakened than that of *c* has been. Accordingly, in the union of *c* and *d* an excess of heat would again be given out. In this way a series of alternations in the intensity of the cohesive attraction might be brought about along the line of crystallized molecules.

Fig. 3.



It will be observed that the same fundamental idea pervades all the explanations that have now been given of changes of molecular aggregation, whether these are attended with a change of state, or only with a change of density and form. This is the idea that the physical and mechanical condition of a molecule may change with varying circumstances, and that it may undergo a permanent change, although the temperature should remain the same. The change of condition consists simply in the expansion or contraction of the electric atmosphere of the molecule (p. 382). We have recognized, also, that while the processes of transformation are going on, the normal distribution of the electric æther, that forms the atmosphere of a molecule, may become disturbed, and that thus a transient electric polarity of the molecule may be induced which may play an important part in the process.

Upon the theory of crystallization here offered, the phenomenon of *dimorphism*, and all changes of form, in the same crystal, produced by heat and external causes generally, are but simple results of the modifications superinduced by these causes, in the form or distance from the central atoms of the molecular atmospheres—that is, in the physical features of the molecules, upon which the system of crystallization in every instance depends.

[To be continued.]

XLVIII. *Notices respecting New Books.*

The Laboratory Guide for Students of Agricultural Chemistry. Arranged by Arthur Herbert Church, *M.A., Professor of Chemistry in the Royal Agricultural College, Cirencester.* Post 8vo. London: Van Voorst. 1864. Pp. viii and 94.

THIS little work is divided into two Parts. The first, intended as an outline of the general course of qualitative analysis, begins with an enumeration and brief description of the more commonly occurring elements; then follow concise directions for preparing and purifying the reagents required in the subsequent processes; and next about twenty-three pages devoted to "The Method of Analysis." The second Part consists of a series of examples for practice in quantitative analysis, selected from among such substances as are most likely to come in the way of an agricultural chemist.

It is this second part which gives to the book its distinctive character and its chief value. The examples here given have been judiciously selected so as to embrace a considerable variety of processes, while the working directions are almost always clear and sufficient for the object in view. A few of the best volumetric methods have been included, among which are two very good ones that we do not remember to have frequently met with in works of this class—namely, Mohr's process for the estimation of chlorine by nitrate of silver and chromate of potash, and the late Dr. Pugh's method of estimating nitric acid. This portion of the work appears to us so well planned and so likely to be useful, that we should be glad to see it extended so as to occupy the whole book instead of the last fifty-three pages merely. On the other hand, we do not think that the consequent exclusion of the part devoted to qualitative analysis would greatly diminish the value of the work. As it is, Professor Church leaves so much, of what it is necessary for the student to know respecting this branch of the subject, to be supplied from other sources, that he might, without much danger, have left the whole of what is given in this portion of his book to be supplied in the same way. Indeed, were this a suitable occasion for discussing the question, very much might be said against that system of laboratory teaching in which a "Scheme" or "Method" of qualitative analysis, such as that contained in the first part of this work, is put into the hands of the student at the very beginning of his course. We believe that the very opposite system to this would, more than anything else, tend to promote the rational study of chemistry. As far as possible, we would have students encouraged to rely upon their own experience and observations rather than upon books of any sort; and most of all we would endeavour to persuade them to use no *Analytical Schemes* or *Tables* until they can construct them from their own knowledge, or are independent of them altogether. Mr. Church's work, however, not being addressed to students of scientific chemistry, so much as to those whose object is merely to become acquainted with one of its most important practical applications, and who perhaps in most cases have neither time nor inclination to devote much attention to the study of chemistry for its own sake, it would be unfair to charge upon it as a fault that it follows the most commonly received system of instruction in the science, even although we may be of opinion that that system is not the best possible.

We have exceedingly little to say in the way of adverse criticism on the details of this work. Here and there, possibly, are signs of somewhat hasty composition or correction: to this cause, for instance, we attribute the occurrence of such an expression as "oil of vitriol plus its own bulk of water" on page 77, an expression which is much less pleasant than "oil of vitriol diluted with its own bulk of water," which we find on the following page. Again, on page 17 we are told that solutions of silver yield "a buff precipitate with hydrate of soda," where for *hydrate* we are doubtless intended to read *carbonate*. The method of preparing absolute alcohol, recommended on page 13, might likewise be improved; for alcohol can be more completely and easily dehydrated by using a sufficient quantity of good quicklime than by either carbonate of potash or sulphate of copper.

In conclusion, we have again to express our conviction that Mr. Church has produced a book well qualified to be of use to those to whom it is addressed; and if we have suggested a plan by which, without increasing the size of the book, its utility might, in our opinion, be made still greater, it is by no means because we consider its value small in its present shape.

XLIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 320.]

June 16, 1864.—Major-General Sabine, President, in the Chair.

THE following communication was read:—

"On the Reduction and Oxidation of the Colouring Matter of the Blood." By G. G. Stokes, M.A., Sec. R.S., &c.

1. Some time ago my attention was called to a paper by Professor Hoppe*, in which he has pointed out the remarkable spectrum produced by the absorption of light by a very dilute solution of blood, and applied the observation to elucidate the chemical nature of the colouring matter. I had no sooner looked at the spectrum, than the extreme sharpness and beauty of the absorption-bands of blood excited a lively interest in my mind, and I proceeded to try the effect of various reagents. The observation is perfectly simple, since nothing more is required than to place the solution to be tried, which may be contained in a test-tube, behind a slit, and view it through a prism applied to the eye. In this way it is easy to verify Hoppe's statement, that the colouring matter (as may be presumed at least from the retention of its peculiar spectrum) is unaffected by

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* Virchow's Archiv, vol. xxiii. p. 446 (1862).

alkaline carbonates and caustic ammonia, but is almost immediately decomposed by acids, and also, but more slowly, by caustic fixed alkalies, the coloured product of decomposition being the hæmatine of Lecanu, which is easily identified by its peculiar spectra. But it seemed to me to be a point of special interest to inquire whether we could imitate the change of colour of arterial into that of venous blood, on the supposition that it arises from reduction.

2. In my experiments I generally employed the blood of sheep or oxen obtained from a butcher; but Hoppe has shown that the blood of animals in general exhibits just the same bands. To obtain the colouring matter in true solution, and at the same time to get rid of a part of the associated matters, I generally allowed the blood to coagulate, cut the clot small, rinsed it well, and extracted it with water. This, however, is not essential, and blood merely diluted with a large quantity of water may be used; but in what follows it is to be understood that the watery extract is used unless the contrary be stated.

3. Since the colouring matter is changed by acids, we must employ reducing agents which are compatible with an alkaline solution. If to a solution of protosulphate of iron enough tartaric acid be added to prevent precipitation by alkalies, and a small quantity of the solution, previously rendered alkaline by either ammonia or carbonate of soda, be added to a solution of blood, the colour is almost instantly changed to a much more purple red as seen in small thicknesses, and a much darker red than before as seen in greater thickness. The change of colour, which recalls the difference between arterial and venous blood, is striking enough, but the change in the absorption spectrum is far more decisive. The two highly characteristic dark bands seen before are now replaced by a *single* band, somewhat broader and less sharply defined at its edges than either of the former, and occupying nearly the position of the bright band separating the dark bands of the original solution. The fluid is more transparent for the blue, and less so for the green than it was before. If the thickness be increased till the whole of the spectrum more refrangible than the red be on the point of disappearing, the last part to remain is *green*, a little beyond the fixed line *b*, in the case of the original solution, and *blue*, some way beyond F, in the case of the modified fluid. Figs. 1 and 2 in the accompanying woodcut represent the bands seen in these two solutions respectively.

4. If the purple solution be exposed to the air in a shallow vessel, it quickly returns to its original condition, showing the two characteristic bands the same as before; and this change takes place immediately, provided a small quantity only of the reducing agent were employed, when the solution is shaken up with air. If an additional quantity of the reagent be now added, the same effect is produced as at first, and the solution may thus be made to go through its changes any number of times.

5. The change produced by the action of the air (that is, of course, by the absorption of oxygen) may be seen in an instructive form on partly filling a test-tube with a solution of blood suitably

diluted, mixing with a little of the reducing agent, and leaving the tube at rest for some time in a vertical position. The upper or oxidized portion of the solution is readily distinguished by its colour; and if the tube be now placed behind a slit and viewed through a prism, a dark band is seen, having the general form of a tuning-fork, like figs. 1 and 2, regarded now as a single figure, the line of separation being supposed removed.

Fig. 1.

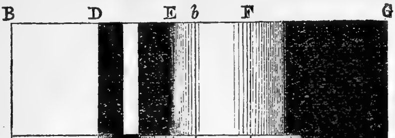


Fig. 2.

Fig. 3.

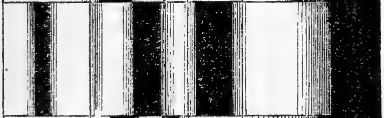
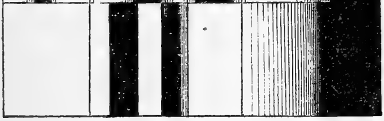


Fig. 4.



6. Of course it is necessary to assure oneself that the single band in the green is not due to absorption produced merely by the reagent, as is readily done by direct observation of its spectrum, not to mention that in the region of the previous dark bands, or at least the outer portions of it, the solution is actually more transparent than before, which could not be occasioned by an *additional* absorption. Indeed the absorption due to the reagent itself in its different stages of oxidation, unless it be employed in most unnecessary excess, may almost be regarded as evanescent in comparison with the absorption due to the colouring matter; though if the solution be repeatedly put through its changes, the accumulation of the persalt of iron will presently tell on the colour, making it sensibly yellower than at first for small thicknesses of the solution.

7. That the change which the iron salt produces in the spectrum is due to a simple reduction of the colouring matter, and not to the formation of some compound of the colouring matter with the reagent, is shown by the fact that a variety of reducing agents of very different nature produce just the same effect. If protochloride of tin be substituted for protosulphate of iron in the experiment above described, the same changes take place as with the iron salt. The tin solution has the advantage of being colourless, and leaving the visible spectrum quite unaffected, both before and after oxidation, and accordingly of not interfering in the slightest degree with the optical examination of the solutions, but permitting them to be seen with

exactly their true tints. The action of this reagent, however, takes some little time at ordinary temperatures, though it is very rapid if previously the solution be gently warmed. Hydrosulphate of ammonia again produces the same change, though a small fraction of the colouring matter is liable to undergo some different modification, as is shown by the occurrence of a slender band in the red, variable in its amount of development, which did not previously exist. In this case, as with the tin salt, the action is somewhat slow, requiring a few minutes unless it be assisted by gentle heat. Other reagents might be mentioned, but these will suffice.

8. We may infer from the facts above mentioned that *the colouring matter of blood,* like indigo, *is capable of existing in two states of oxidation, distinguishable by a difference of colour and a fundamental difference in the action on the spectrum. It may be made to pass from the more to the less oxidized state by the action of suitable reducing agents, and recovers its oxygen by absorption from the air.*

As the term *hæmatine* has been appropriated to a product of decomposition, some other name must be given to the original colouring matter. As it has not been named by Hoppe, I propose to call it *cruorine,* as suggested to me by Dr. Sharpey; and in its two states of oxidation it may conveniently be named *scarlet cruorine* and *purple cruorine* respectively, though the former is slightly purplish at a certain small thickness, and the latter is of a very red purple colour, becoming red at a moderate increase of thickness.

9. When the watery extract from blood-clots is left aside in a corked bottle, or even in a tall narrow vessel open at the top, it presently changes in colour from a bright to a dark red, decidedly purple in small thicknesses. This change is perceived even before the solution has begun to stink in the least perceptible degree. The tint agrees with that of the purple cruorine obtained immediately by reducing agents; and if a little of the solution be sucked up from the bottom into a quill-tube drawn to a capillary point, and the tube be then placed behind a slit, so as to admit of analyzing the transmitted light without exposing the fluid to the air, the spectrum will be found to agree with that of purple cruorine. On shaking the solution with air it immediately becomes bright red, and now presents the optical characters of scarlet cruorine. It thus appears that scarlet cruorine is capable of being reduced by certain substances, derived from the blood, present in the solution, which must themselves be oxidized at its expense.

10. When the alkaline tartaric solution of protoxide of tin is added in moderate quantity to a solution of scarlet cruorine, the latter is presently reduced. If the solution is now shaken with air, the cruorine is almost instantly oxidized, as is shown by the colour of the solution and its spectrum by transmitted light. On standing for a little time, a couple of minutes or so, the cruorine is again reduced, and the solution may be made to go through these changes a great number of times, though not of course indefinitely, as the tin must at last become completely oxidized. It thus appears that purple cruorine absorbs *free* oxygen with much greater avidity than the tin

solution, notwithstanding that the oxidized cruorine is itself reduced by the tin salt. I shall return to this experiment presently.

11. When a little acid, suppose acetic or tartaric acid, which does not produce a precipitate, is added to a solution of blood, the colour is quickly changed from red to brownish red, and in place of the original bands (fig. 1) we have a different system, nearly that of fig. 3. This system is highly characteristic; but in order to bring it out a larger quantity of substance is requisite than in the case of scarlet cruorine. The figure does not exactly correspond to any one thickness, for the bands in the blue are best seen while the band in the red is still rather narrow and ill-defined at its edges, while the narrow inconspicuous band in the yellow hardly comes out till the whole of the blue and violet, and a good part of the green, are absorbed, The difference in the spectra figs. 1 and 3 does not alone prove that the colouring matter is decomposed by the acid (though the fact that the change is not instantaneous favours that supposition), for the one solution is alkaline, though it may be only slightly so, while the other is acid, and the difference of spectra might be due merely to this circumstance. As the direct addition of either ammonia or carbonate of soda to the acid liquid causes a precipitate, it is requisite in the first instance to separate the colouring matter from the substance so precipitated.

This may be easily effected on a small scale by adding to the watery extract from blood-clots about an equal volume of ether, and then some glacial acetic acid, and gently mixing, but not violently shaking for fear of forming an emulsion. When enough acetic acid has been added, the acid ether rises charged with nearly the whole of the colouring matter, while the substance which caused the precipitate remains in the acid watery layer below*. The acid ether solution shows in perfection the characteristic spectrum fig. 3. When most of the acid is washed out the substance falls, remaining in the ether near the common surface. If after removing the wash-water a solution, even a weak one, of ammonia or carbonate of soda be added, the colouring matter readily dissolves in the alkali. The spectrum of the transmitted light is quite different from that of scarlet cruorine, and by no means so remarkable. It presents a single band of absorption, very obscurely divided into two, the centre of which nearly coincides with the fixed line D, so that the band is decidedly less refrangible than the pair of bands of scarlet cruorine. The relative proportion of the two parts of the band is liable to vary. The presence of alcohol, perhaps even of dissolved ether, seems to favour the first part, and an excess of caustic alkali the second, the fluid at the same time becoming more decidedly dichroitic. The blue end of the spectrum is at the same time absorbed. The band of absorption is by no means so definite at its edges as those of scarlet cruorine, and a far larger quantity of the substance is required to develope it.

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* If I may judge from the results obtained with the precipitate given by acetic acid and a neutral salt, a promising mode of separation of the proximate constituents of blood-crystals would be to dissolve the crystals in glacial acetic acid and add ether, which precipitates a white albuminous substance, leaving the hæmatine in solution.

This difference of spectra shows that the colouring matter (hæmatine) obtained by acids is a product of the decomposition, or metamorphosis of some kind, of the original colouring matter.

When hæmatine is dissolved in alcohol containing acid, the spectrum nearly agrees with that represented in fig. 3.

12. Hæmatine is capable of reduction and oxidation like cruorine. If it be dissolved in a solution of ammonia or of carbonate of soda, and a little of the iron salt already mentioned, or else of hydrosulphate of ammonia, be added, a pair of very intense bands of absorption is immediately developed (fig. 4). These bands are situated at about the same distance apart as those of scarlet cruorine, and are no less sharp and distinctive. They are a little more refrangible, a clear though narrow interval intervening between the first of them and the line D. They differ much from the bands of cruorine in the relative strength of the first and second band. With cruorine the second band appears almost as soon as the first, on increasing the strength or thickness of the solution from zero onwards, and when both bands are well developed, the second band is decidedly broader than the first. With reduced hæmatine, on the other hand, the first band is already black and intense by the time the second begins to appear; then both bands increase, the first retaining its superiority until the two are on the point of merging into one by the absorption of the intervening bright band, when the two appear about equal.

Like cruorine, reduced hæmatine is oxidized by shaking up its solution with air. I have not yet obtained hæmatine in an acid solution in more than one form, that which gives the spectrum fig. 3, and which I have little doubt contains hæmatine in its oxidized form; for when it is withdrawn from acid ether by an alkali, I have not seen any traces of reduced hæmatine, even on taking some precautions against the absorption of oxygen. As the alkaline solution of ordinary hæmatine passes, with increase of thickness, through yellow, green, and brown to red, while that of reduced hæmatine is red throughout, the two kinds may be conveniently distinguished as *brown hæmatine* and *red hæmatine* respectively, the former or oxidized substance being the hæmatine of chemists.

13. Although the spectrum of scarlet cruorine is not affected by the addition to the solution of either ammonia or carbonate of soda, yet if after such addition the solution be either heated or alcohol be added, although there is no precipitation decomposition takes place. The coloured product of decomposition is brown hæmatine, as may be inferred from its spectrum. Since, however, the spectrum of an alkaline solution of brown hæmatine is only moderately distinctive, and is somewhat variable according to the nature of the solvent, it is well to add hydrosulphate of ammonia, which immediately developes the remarkable bands of red hæmatine. This is the easiest way to obtain them; but the less refrangible edge of the first band as obtained in this way is liable to be not quite clean, in consequence of the presence of a small quantity of cruorine which escaped decomposition.

Some very curious reactions are produced in a solution of cruorine by gallic acid combined with other reagents, but these require further study.

14. Hoppe proposed to employ the highly characteristic absorption-bands of scarlet cruorine in forensic inquiries. Since, however, cruorine is very easily decomposed, as by hot water, alcohol, weak acids, &c., the method would often be inapplicable. But as in such cases the coloured product of decomposition is hæmatine, which is a very stable substance, the absorption-bands of red hæmatine in alkaline solution, which in sharpness, distinctive character and sensibility rival those of scarlet cruorine itself, may be employed instead of the latter. The absorption-bands of brown hæmatine dissolved in a mixture of ether and acetic acid, or in acetic acid alone, are hardly less characteristic, but are not quite so sensitive, requiring a somewhat larger quantity of the substance.

15. I have purposely abstained from physiological speculations until I should have finished the chemico-optical part of the subject; but as the facts which have been adduced seem calculated to throw considerable light on the function of cruorine in the animal economy, I may perhaps be permitted to make a few remarks on this subject.

It has been a disputed point whether the oxygen introduced into the blood in its passage through the lungs is simply dissolved or is chemically combined with some constituent of the blood. The latter and more natural view seems for a time to have given place to the former in consequence of the experiments of Magnus. But Liebig and others have since adduced arguments to show that the oxygen absorbed is, mainly at least, chemically combined, be it only in such a loose way, like a portion of the carbonic acid in bicarbonate of soda, that it is capable of being expelled by indifferent gases. It is known, too, that it is the red corpuscles in which the faculty of absorbing oxygen mainly resides.

Now it has been shown in this paper that we have in cruorine a substance capable of undergoing reduction and oxidation, more especially oxidation, so that if we may assume the presence of purple cruorine in venous blood, we have all that is necessary to account for the absorption and chemical combination of the inspired oxygen.

16. It is stated by Hoppe that venous as well as arterial blood shows the two bands which are characteristic of what has been called in this paper scarlet cruorine. As the precautions taken to prevent the absorption of oxygen are not mentioned, it seemed desirable to repeat the experiment, which Dr. Harley and Dr. Sharpey have kindly done. A pipette adapted to a syringe was filled with water which had been boiled and cooled without exposure to the air, and the point having been introduced into the jugular vein of a live dog, a little blood was drawn into the bulb. Without the water the blood would have been too dark for spectral analysis. The colour did not much differ from that of scarlet cruorine; certainly it was much nearer the scarlet than the purple substance. The spectrum showed the bands of scarlet cruorine.

This, however, does not by any means prove the absence of purple cruorine, but only shows that the colouring matter present was chiefly scarlet cruorine. Indeed the relative proportions of the two present in a mixture of them with one another and with colourless substances, can be better judged of by the tint than by the use of the prism. With the prism the extreme sharpness of the bands of scarlet cruorine is apt to mislead, and to induce the observer greatly to exaggerate the relative proportion of that substance.

Seeing then that the change of colour from arterial to venous blood *as far as it goes* is *in the direction* of the change from scarlet to purple cruorine, that scarlet cruorine is capable of reduction even in the cold by substances present in the blood (§ 9), and that the action of reducing agents upon it is greatly assisted by warmth (§ 7), we have every reason to believe that *a portion* of the cruorine present in venous blood exists in the state of purple cruorine, and is reoxidized in passing through the lungs.

17. That it is only a rather small proportion of the cruorine present in venous blood which exists in the state of purple cruorine under normal conditions of life and health, may be inferred, not only from the colour, but directly from the results of the most recent experiments[*]. Were it otherwise, any extensive hæmorrhage could hardly fail to be fatal, if, as there is reason to believe, cruorine be the substance on which the function of respiration mainly depends; nor could chlorotic persons exhale as much carbonic acid as healthy subjects, as is found to be the case.

But after death there is every reason to think that the process of reduction still goes on, especially in the case of warm-blooded animals, while the body is still warm. Hence the blood found in the veins of an animal some time after death can hardly be taken as a fair specimen as to colour of the venous blood in the living animal. Moreover the blood of an animal which has been subjected to abnormal conditions before death is of course liable to be altered thereby. The terms in which Lehmann has described the colour of the blood of frogs which had been slowly asphyxiated by being made to breathe a mixture of air and carbonic acid seem unmistakeably to point to purple cruorine[*].

18. The effect of various indifferent reagents in changing the colour of defibrinated blood has been much studied, but not always with due regard to optical principles. The brightening of the colour, as seen by reflexion, produced by the first action of neutral salts, and the darkening caused by the addition of a little water, are, I conceive, easily explained; but I have not seen stated what I feel satisfied is the true explanation. In the former case the corpuscles lose water by exosmose, and become thereby more highly refractive, in consequence of which a more copious reflexion takes place at the common surface of the corpuscles and the surrounding fluid. In the latter case they gain water by endosmose, which makes their refractive power more nearly equal to that of the fluid in which they are contained, and the reflexion is consequently diminished. There is nothing in these cases to indicate any change in the mode in which light is absorbed by the colouring matter, although a change of tint to a certain extent, and not merely a change of intensity, may accom-

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[*] Funk's Lehrbuch der Physiologie, 1863, vol. i. § 108.

[†] Physiological Chemistry, vol. ii. p. 178.

pany the change of conditions under which the turbid mixture is seen, as I have elsewhere more fully explained*.

No doubt the form of the corpuscles is changed by the action of the reagents introduced; but to attribute the change of colour to this is, I apprehend, to mistake a concomitant for a cause, and to attribute, moreover, the change of colour to a cause inadequate to produce it.

19. Very different is the effect of carbonic acid. In this case the existence of a fundamental change in the mode of absorption cannot be questioned, especially when the fluid is squeezed thin between two glasses and viewed by transmitted light. I took two portions of defibrinated blood; to one I added a little of the reducing iron solution, and passed carbonic acid into the other, and then compared them. They were as nearly as possible alike. We must not attribute these apparently identical changes to two totally different causes if one will suffice. Now in the case of the iron salt, the change of colour is plainly due to a deoxidation of the cruorine. On the other hand, Magnus removed as much as 10 or 12 per cent. by volume of oxygen from arterialized blood by shaking the blood with carbonic acid. If, as we have reason to believe, this oxygen was for the most part chemically combined, it follows that carbonic acid acts *as if it were* a reducing agent. We are led to regard the change of colour not as a *direct* effect of the *presence of carbonic acid*, but a consequence of the *removal of oxygen*. There is this difference between carbonic acid and the *real* reducing agents, that the former no longer acts on a dilute and comparatively pure solution of scarlet cruorine, while the latter act just as before.

If even in the case of blood exposed to an atmosphere of carbonic acid we are not to attribute the change of colour to the direct presence of the gas, much less should we attempt to account for the darker colour of venous than arterial blood by the small additional percentage of carbonic acid which the former contains. The ascertained properties of cruorine furnish us with a ready explanation, namely that it is due to a partial reduction of scarlet cruorine in supplying the wants of the system.

20. I am indebted to Dr. Akin for calling my attention to a very interesting pamphlet by A. Schmidt on the existence of ozone in the blood†. The author uses throughout the language of the ozone theory. If by ozone be meant the substance, be it allotropic oxygen or teroxide of hydrogen, which is formed by electric discharges in air, there is absolutely nothing to prove its existence in blood; for all attempts to obtain an oxidizing gas from blood failed. But if by ozone be merely meant oxygen in any such state, of combination or otherwise, as to be capable of producing certain oxidizing effects, such as turning guaiacum blue, the experiments of Schmidt have completely established its existence, and have connected it, too, with the colouring matter. Now in cruorine we have a substance admitting of easy oxidation and reduction; and connecting this with Schmidt's results, we may infer that scarlet cruorine is not merely a

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* Philosophical Transactions, 1852, p. 527.

† Ueber Ozon im Blute. Dorpat, 1862.

greedy absorber and a carrier of oxygen, but also an *oxidizing agent*, and that it is by its means that the substances which enter the blood from the food, setting aside those which are either assimilated or excreted by the kidneys, are reduced to the ultimate forms of carbonic acid and water, as if they had been burnt in oxygen.

21. In illustration of the functions of cruorine, I would refer, in conclusion, to the experiment mentioned in § 10. As the purple cruorine in the solution was oxidized almost instantaneously on being presented with free oxygen by shaking with air, while the tin-salt remained in an unoxidized state, so the purple cruorine of the veins is oxidized during the time, brief though it may be, during which it is exposed to air in the lungs, while the substances derived from the food may have little disposition to combine with free oxygen. As the scarlet cruorine is gradually reduced, oxidizing thereby a portion of the tin-salt, so part of the scarlet cruorine is gradually reduced in the course of the circulation, oxidizing a portion of the substances derived from the food or of the tissues. The purplish colour now assumed by the solution illustrates the tinge of venous blood, and a fresh shake represents a fresh passage through the lungs.

L. *Intelligence and Miscellaneous Articles.*

INFLUENCE OF HEAT-FORCE ON VEGETABLE LIFE.

BY GEORGE BENTHAM, PRESIDENT OF THE LINNÆAN SOCIETY*.

I CANNOT conclude my remarks upon the recent progress of biological science without alluding to the modern discovery of the dynamical theory of heat, or equivalence of heat and force—a wonderful theory, which the lectures of Tyndall have rendered practically clear even to the unscientific mind, but which, nevertheless, it is difficult to follow in all its details without feeling a certain bewilderment of the brain. I may refer to Dr. Carpenter's article "On the Application of the Principle of Conservative Force to Physiology," in the 'Quarterly Journal of Science' for January and April of the present year, for a general review of the influence of this force on vegetable and animal life, and, for a more popular summary, to the graphic sketch of the relation of the sun to life, contained in the closing portion of Prof. Tyndall's twelfth lecture. But in this summary occur the following passages, which, however correct in regard to the great principle they are intended to illustrate, might yet, I think, without explanation, lead the ordinary reader into considerable error with regard to some great biological facts, and upon which therefore, on account of the high standing of the distinguished author, and the general circulation which the work must command, I think it necessary to offer a few observations.

"The earth's atmosphere contains carbonic acid, and the earth's surface bears living plants; the former is the nutriment of the latter. The plant apparently seizes the combined carbon and oxygen, tears them asunder, storing up the carbon, and letting the

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* From the *Anniversary Address*, delivered May 24, 1864.

oxygen go free. By no special force, different in quality from other forces, do plants exercise this power: the real magician is here the sun." (p. 430.)

"But we cannot stop at vegetable life; for this is the source, mediate or immediate, of all animal life. In the animal body, vegetable substances are brought again into contact with their beloved oxygen, and they burn within us, as a fire burns in a grate. This is the source of all animal power, and the forces in play are the same in kind as those which operate in inorganic nature. In the plant the clock is wound up, in the animal it runs down. In the plant the atoms are separated, in the animal they recombine." (p. 431.)

The sun "rears, as I have said, the whole vegetable world, and through it the animal. The lilies of the field are his workmanship, the verdure of the meadows, and the cattle upon a thousand hills. He forms the muscle; he urges the blood; he builds the brain. His fleetness is in the lion's foot; he springs in the panther; he soars in the eagle; he glides in the snake. He builds the forest, and hews it down,—the power which raised the tree, and which wields the axe, being one and the same. The clover sprouts and blossoms, and the scythe of the mower swings, by the operation of the same force." (p. 432.)

Notwithstanding the assertion to the contrary, it must be admitted that there is here a little of poetry mixed with rigid mechanical truth; and we, as special investigators of the complex phenomena of animal and vegetable life and living beings, ought not to allow this quiet and summary dismissal of what is ordinarily, though perhaps erroneously, called "vital force" without remark. Life may not be a force in the sense which natural philosophers give to the term, but it is a power which so materially modifies the action of heat-force, that it comes within the general and more popular meaning of the word. Life cannot indeed be set in action without the operation of heat-force; but, on the other hand, the sun cannot build a tree without the assistance of life. What life really is we do not know: its origin is probably beyond our investigation; but its existence and continuity cannot be denied. Notwithstanding the objections of heterogenists, to which I had occasion last year to allude, I cannot but remind you that in the present state of science we have as yet no prospect of proofs that any new life is created, that any new living being is built, by the sun or any other force, out of matter organic or inorganic. Life is continuous, and has been so from a period beyond human cognizance. We witness its cessation, but it has never been known to commence. Every new being grows out of, and is a portion detached from, a preexisting one. We cannot even fix precisely the moment when its independent life commences. It is not when the detached bud first shoots out its own roots, not when the seed bursts, or the egg-shell is broken, or the young animal is born; for the bud, the embryo, or fœtus had a previous existence, more or less independent of, or connected with, the parent, according to species. It is not at the moment of fertilization or impregnation;

for the bud, and even the ovum in cases of parthenogenesis, may grow into independent beings without ever being impregnated. Nor can our most powerful instruments perceive the moment when the first embryo-cell receives that impress which has irrevocably determined the form which the perfect being is to assume, within those narrow limits which neither impregnation nor any other influences set in action by the sun can ever make it exceed. And if life is once stopped, if interrupted, be it for a moment, no force can set it in action again. It may lie dormant for a long (but not perhaps indefinite) succession of years; its action may be absolutely imperceptible or limited to the resistance of disorganization, until recalled into more active operation by the action of the sun on surrounding influences; but if during the dormant period (of the seed, egg, &c.) life has once ceased, nothing will restore it: the action of the same sun upon the same surrounding influences will produce decomposition, not growth. The word "force" may indeed be properly limited to mechanical force, and it may be incorrect to say that life is a force different in quality from other forces; but, as we must have some term equivalent to the popular sense, we may call life a *power* different in quality from force. Dr. Carpenter (p. 80) proposes to term it a *germinal capacity*; but it is surely much more than a capacity, to be paraphrased as the "power of utilizing, after its own particular fashion, the heat which it receives, and of applying it as a constructive power to the building up of its fabric after its characteristic type" (p. 87). There is here this difference between the term and its paraphrase, that the one expresses a passive, the other an active idea.

"Vegetable substances, brought into contact with their beloved oxygen in the animal body, will burn within it as a fire burns in the grate." True; but that burning will be fermentation and corruption, unless brought under the influence of the living parts of the body to be converted into growth. I say *growth*, not *building*; for building the brain and the forest is a metaphor which must lead the unscientific mind far astray from all that science has as yet taught us. Nothing in life is built, in the ordinary "sense" of the term; no portion, no single cell, has been externally added to a living being; everything has grown out of it, every new cell is gradually compounded within a living cell.

"The plant apparently seizes the combined carbon and oxygen, tears them asunder, storing up the carbon, and letting the oxygen free." True; but it does much more. Every living being, animal or vegetable, absorbs compound substances, decomposes them, liberates at once a portion (chiefly oxygen in the case of most plants), and stores up a portion. Of this portion some may be deposited unchanged in visible particles in various parts of the animal or plant, but some also undergoes a further decomposition and dilution into a state hitherto concealed from our observation, from which it emerges recombined, having already received a peculiar impress, definitely differing in every species, or even in every individual—differences then inappreciable, it is true, to our senses, but evidenced by the forms the animal or plant is compelled to assume as it grows. And the process is substantially the same in animals and plants; both absorb, decompose, select, reject, and recombine. An animal may select what a tree rejects; but so also may one plant select what another rejects. None feed upon carbon or oxygen alone. Some are not satisfied without drawing their nutriment direct from the living plant or animal; many feed upon organic substances, in which the decomposition after death has scarcely commenced; and most, if not all, appear to require for their support some small portion, at least, of matter in which life is or has been. In both animals and vegetables the clock is wound up, and it runs down; in both, the atoms are separated and recombined, and, in both, these operations take place in a totally different way from what they do in the same bodies under the same influences, the moment life is extinct, the moment the vital power ceases to act. It is this vital power, its continuity and infinite divisibility, its unity and infinite diversity, the concordances, discrepancies, and reciprocal action and influences of the infinity of forms it produces, that our Society is specially called upon to investigate. As systematists, we have so to discriminate, describe, and class these forms as to enable us readily to identify them, both individually and collectively, to comprehend one another and ourselves in treating of them, and to retain and store in our minds and books what is known of their resemblances, differences, and peculiarities, of their influences and relations to each other and to the lifeless world, as a starting-point for future observation. As biologists, we have to study life itself in all its phases, and the multifarious influences by which it is continued, preserved, multiplied, checked, injured, destroyed, or extinguished. But, in addition, we must not neglect to learn from natural philosophers what are those general forces which act on organic as well as on inorganic bodies, and whilst carefully watching every modification these forces undergo, when applied in combination with vital power, gratefully accept any proved identity of action in the living and inanimate world.

ANALYSIS OF LANGITE, A NEW MINERAL FROM CORNWALL.

BY M. PISANI.

Professor Maskelyne presented a short time ago to the Geological Society of London some specimens of a new mineral found in Cornwall, to which he has given the name *Langite*. It is a greenish-blue hydrated subsulphate of copper, forming crystalline crusts and small right rhomboidal prisms on a coarse argillaceous schist called *killas* in Cornwall.

The crystals of Langite are small and short; by their union they form macles analogous to those of Arragonite. Translucid; lustre vitreous. Its colour is a beautiful greenish blue, and that of its powder a pale blue. Hardness, 3·5; specific gravity about 3·05. Heated in a test-tube it gives water. Before the blowpipe, on charcoal, it gives with soda a bead of copper. It is insoluble in water, but soluble in weak acids and ammonia. Its hydrochloric acid solution, when diluted, gives an abundant precipitate with chloride of barium. It differs from brochantite in containing more water; therefore, as its external aspect is also quite different, it deserves to form a separate species. It is to be noticed that Berthier formerly analyzed an amorphous brochantite from Mexico, in which he had found as much water as in Langite, while its colour was green like that of other brochantites.

Langite gave on analysis—

		Oxygen.		Ratios.
Sulphuric acid	16·77	10·0		3
Oxide of copper . ..	65·92	13·3	13·6	4
Lime...............	0·83	0·2		
Magnesia	0·29	0·1		
Water	16·19	14·4		4
	100·00			

which corresponds to the formula

$$4CuO, SO^3+4HO.$$

This formula requires—

Sulphuric acid	17·06
Oxide of copper	67·59
Water	15·35
	100·00

It thus only differs from brochantite by containing one more equivalent of water.—*Comptes Rendus*, October 10, 1864.

ON THE HISTORY OF ENERGETICS.

To the Editors of the Philosophical Magazine and Journal.

University of Glasgow,
October 5, 1864.

Gentlemen,

So far as I know, the earliest introduction of a distinct term to denote the *mechanical* form of what is now called "potential energy" is due to Carnot, who, in a scarce and little-known essay on Machines in general, uses the phrase "*force vive virtuelle*" in that sense.

The step which I took in 1853, of applying the distinction between "Actual Energy" and "Potential Energy," not to motion and mechanical power alone, but to all kinds of physical phenomena, was suggested to me, I think, by Aristotle's use of the words *δύναμις* and *ἐνέργεια*.

I am, Gentlemen,
Your most obedient Servant,
W. J. Macquorn Rankine.

Erratum in October Number.

In Prof. Rankine's paper on Stream-lines, Equation II.,

$$\text{for } -\frac{db}{dy} \text{ read } -\frac{db}{dx}.$$

ON THE TEMPERATURE OF SEA-WATER.

BY M. CHARLES MARTINS*.

I have read with great interest M. Edlund's Note on the Formation of Ice in the Sea, published in the *Archives des Sciences Naturelles* of July 20. His observations render it difficult not to admit that ground-ice may be formed in salt water as well as in fresh. The fact that, in nature, the temperature of sea-water descends below zero without ice being formed can no longer be contested. In the two voyages made in 1838 and 1839 by the corvette 'La Recherche' to Spitzbergen, I determined the temperatures at the bottom of the sea in the neighbourhood of the glaciers, which, in that region, often descend to the sea, and even advance, overhanging the surface of the water, to some distance from the coast. In these experiments, published fifteen years ago in the *Voyages de la corvette 'la Recherche,'* vol. ii. p. 279, and in the *Annales de Chimie et de Physique*, 3e sér. vol. xxv. p. 172, I used, for temperatures above zero, Walferdin's overflow-thermometers with arbitrary scales. For temperatures below zero, I employed thermometrographs with indexes unprotected from pressure; but I took care to correct their indications by means of coefficients obtained from comparative experiments made with overflow-instruments, protected from pressure by tubes of crystal: besides this, I always took the precaution (indispensable in experiments of this kind) to employ *several* thermometrographs at the same time, in order that their indications might rectify each other.

Most of the experiments were made in August 1839, opposite the great glacier at the bottom of Magdalena Bay, on the east coast of Spitzbergen, in latitude 79° 34′, and longitude 8° 49′ east of Paris. The temperature of the surface was always a little above zero; it varied, in fact, from 0°·1 to 1°·2. Nevertheless twice a day, at low water, enormous masses of ice fell down into and cooled the sea. The temperature of the air at the surface of the sea varied from 0°·7 to 6°·0. From the surface of the water down to a depth of 70 metres I *never* found the temperature to be below zero; but beyond this depth, and down to the bottom of the sea, the temperature was *always* below zero, its mean value being about −1°·75. The lowest temperature was found at a depth of 110 metres, and at a distance of 1350 metres from the glacier at the bottom; it was −1°·91. The most elevated of these low temperatures was found at the less considerable depth of 73 metres, and amounted to −1°·29. It would be wrong, however, to conclude that the temperature sinks regularly as the depth increases, for at 136 metres I only found it depressed to −1°·78.

In the open sea I never observed a temperature lower than zero at any depth whatever. For instance, on the 20th of July 1839, in 73° 36′ north latitude, and longitude 18° 32′ east of Paris, I lowered, to a depth of 870 metres, four of Walferdin's thermometers protected from pressure by tubes of crystal soldered by the blowpipe. Their indications agreed wonderfully well with each other, and showed a

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* From a letter to M. E. Plantamour, published in the *Bibliothèque Universelle, Archives des Sciences Phys. et Nat.* vol. xxi. p. 37 (1864).

mean temperature of $0^{\circ}\cdot 10$ at the bottom of the sea. At less depths I always observed higher temperatures.

I place very little reliance on the two experiments of Professor Nordenskiöld. They were made in winter, on the coasts of the Island of Aland—at a depth of 21 feet, and at a distance of 100 feet from the coast—with a single alcohol thermometer provided with a mercury index. Neither the author nor M. Edlund describes the instrument; they do not state whether it was protected from, or exposed to pressure; and no proof is given that the *unique* indication derived from the bottom of the sea was exact. I may venture to advise readers desirous of becoming acquainted with the minute precautions which, in experiments of this kind, are necessary in order to gain for the results the confidence of physicists, to refer to the memoir I published in 1848 and 1849 in the *Voyages de 'la Recherche,'* and in the *Annales de Chimie et de Physique.* To avoid the incessant recommencement of the study of a question, it is necessary to consult the works of those by whom we have been preceded. I have carefully analyzed those of Scoresby and of Parry, who, in 1811 and 1827, had already observed, on many occasions, temperatures of the sea below zero, both at the surface and at depths varying from 90 to 1314 metres. M. Edlund will find the tabular statement of them in my memoir. Doubtless the procedure and the instruments of the English navigators are not beyond the reach of criticism; but the fact of the depression, below zero, of the temperature of sea-water was established by them at the above period, and has since been verified by other travellers.

ON THE ANCIENT AQUEDUCT OF ALATRI. BY FATHER SECCHI*.

The town of Alatri, of Pelasgic origin, famous for its encircling walls of Cyclopean construction, is very ancient. Placed on the summit of a calcareous mountain, it was altogether deprived of water, and was separated from the nearest mountains by a valley, about 125 metres in depth. According to a celebrated inscription, the Censor L. Betilienus Varus conducted water to the town by means of an aqueduct 340 feet high, and for this purpose he caused arcades and strong pipes to be constructed: "*fornices, fistulas solidas fecit.*" The recent researches, made by the order of His Holiness Pius IX., in order again to provide this important town with potable water, have led to the discovery of a large portion of the ancient aqueduct. The levels which I have made show that the lowest point of the aqueduct was 110 metres below the highest part of the town; this accords with the 340 feet mentioned in the inscription. We have here, therefore, a water-conduit in the form of an inverted siphon, under a pressure of 11 atmospheres, constructed 160 years before our present era. It is difficult to say what quantity of water was conducted; but the dimensions of the aqueduct (the buttresses of which measure 1·75 by 1·45 metre) show that it must have been sufficient to supply the baths of the town, several public fountains, and the whole of the town itself, now found to be traversed with pipes of lead and terra cotta. Near the Acropolis, pipes of bronze

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* From a letter to M. Elie de Beaumont, published in the *Comptes Rendus* of September 26, 1864.

have also been discovered, so that the *fistulas solidas* of the inscription may possibly refer to pipes of this metal.

It is remarkable that the pipes of terra cotta which have been found agree precisely with the description given by Vitruvius (*Architura*, lib. viii. cap. vii. no. 51). The aqueduct itself is constructed according to the principles handed down to us by this author; for it is carried along a horizontal line, at the level of the acropolis, to a distance of about 7 kilometres; thence it descends, skirting the mountain, until, after reaching its lowest point, it proceeds once more horizontally for about 500 or 600 metres, and finally reascends (see Vitruvius, *ibid.* no. 50). Thus the total length of the siphon amounts to 5 or 6 kilometres. All attempts to find the *specus* of the aqueduct have been unsuccessful; for the demolition of these works —caused in the first place by the barbarians, and afterwards by peasants—has been enormous; the foundations alone have escaped destruction. The metals discovered, however, appear in many cases to indicate that the conduit was of a mixed kind; it is possible that it was formed of different kinds of substances, corresponding to the different heights to which the water was raised; for a large portion of the water was arrested at half the above height. Constructions in mortar, of great beauty and solidity, have been found, and it appears probable that the Romans were in the habit of strengthening their pipes, externally, by imbedding them in this mortar (in Italian, *calce-struzzo*).

A field was also discovered under which still exists a magnificent and complete system of drainage, by means of long lines of terracotta pipes. These pipes have a mean diameter of 0·45 of a metre, their length being 1·10 metre, and their thickness 0·025 of a metre. At present they are filled with a water-sediment and with clay; their depth under the present soil is 2·50 metres, but it is evident that, originally, this depth was less and has since been increased by new soil. The ends of the pipes overlap each other about 4 centimetres only. No cement was used at their junctions; but spaces of about 1 centimetre were left, in order, probably, to facilitate filtration. This field was probably the one used for military exercises, to which reference is made in the same inscription as being one of the interesting works of Betilienus.

We have here, therefore, a complete system of drainage, resembling the modern one, but constructed twenty centuries ago, and still in a state of perfect conservation. The utility of these works and the merits of the man of genius who constructed them are proved by the esteem in which he was evidently held by his fellow citizens. He was twice elected censor, a statue was erected to his memory, and his son was exempted from military service.

As before observed, water is again about to be conducted to the town of Alatri. The success of the modern conduits employed at Anagni, where the water rises at one bound to a height of 221 metres under the action of force-pumps, leaves no doubt whatever of the ultimate success of the projected constructions, where the new conduit will have a length of from 14 to 15 kilometres, and where the half of this space will be under a pressure of from 6 to 12 atmospheres.

On a more recent visit I found several ancient flint weapons known to the peasants under the name of *thunder stones*. Generally speaking they lie at a very small depth below the soil.

PHENOMENA OBSERVED IN THE SPECTRA PRODUCED BY THE LIGHT OF INDUCTION-CURRENTS IN TRAVERSING RAREFIED GASES. BY M. J. CHAUTARD.

In examining the spectra produced by rarefied gases raised to incandescence under the influence of the current of a Ruhmkorff's coil, I have noticed various new phenomena.

The degree of incandescence of the gas may vary either in consequence of the greater or less density of the ponderable matter in the tube, or from the intensity of the inducing current. M. Plücker has described the phenomena arising from variations in the elasticity of the gas, but he has not examined the different conditions of the spectra when the resistance offered to the current of the pile is made to vary.

This variation of resistance in the inducing current may be produced by two distinct methods—either by elongating the wire traversed by the current, or by introducing a bar of soft iron into an auxiliary coil which is traversed by the circuit of the battery which works the Ruhmkorff's apparatus.

Working so as to vary gradually the intensity of the pile, I observed the following phenomena with the tubes at my disposal:—

1. The dazzling red light of the *hydrogen*-tube finishes by being changed into a livid whitish-green tint; the spectrum, instead of offering three magnificent rays, red, green, and violet, which characterize this gas, and which occupy almost the position of the three Fraunhofer's rays C, F, and G, only give a very pale green.

The glass does not become appreciably heated in the hand.

The stratification in the enlarged part of the tube has the same appearance and the same precision as in the case of the normal current.

In using only the external pole of the induction apparatus, the current still traverses the gas, but the light becomes so pale that no shading can be distinguished by the prism.

2. In *nitrogen* the disappearance of several shades can be distinguished; but here the vanishing of the colours seems to take place in inverse order. Thus the rays of red and of orange first become weaker; the violet only disappears finally; the yellow and green rays remain, spite of the diminution of the light.

3. The rays in *carbonic acid* are very numerous with the ordinary current of the coil; but if the tension of the inducing circuit is diminished, the same absorption of the extreme shades is noticed. Red commences to disappear, then the violet rays, as well as the green ray nearest the red.

The external circuit used alone does not allow the shades of the spectrum to be distinguished.

4. *Bromine* gives a magnificent spectrum furrowed by about nineteen of the most beautiful rays separated by almost dark intervals. The introduction of a resistance into the inducing circuit modifies neither the nature of the spectrum nor the number of rays; there is seen only a simple enfeeblement in the general aspect of the tints, which never completely disappear.—*Comptes Rendus*, Aug. 22, 1864.

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[FOURTH SERIES.]

DECEMBER 1864.

LI. *On the Theory of Light.*—Second Memoir.

By Professor L. LORENZ, *of Copenhagen*[*].

I HAVE communicated, in a previous memoir[†] "On the Theory of Light," certain theoretical investigations which I have since continued and further developed. But before entering more fully upon the details of the results obtained, I may be allowed to take this opportunity of making a few remarks upon the fundamental principles of the theory, and upon the particular position which it occupies relatively to other theories.

Researches into the region of mathematical physics bear witness almost universally to a firm belief in the power of inductive reasoning to penetrate to the forces concealed in the interior of bodies, in order, starting from thence as from a centre, to construct the explanation and laws of phenomena. This is the path entered upon by Laplace and those of his school: it was supposed that the problems of mathematical physics, like those of astronomy, were accessible, even as to their innermost nature, by the inductive method. Molecular forces have been in all cases assumed as the starting-point, which, like the forces of universal attraction, are supposed to be a function of the mutual distances of the molecules, proportional to their masses, and to act only in the direction of the lines joining their centres. The sum of hypotheses involved in this assumption has scarcely been clearly seen; but how wide the basis is which has been chosen for the erection of almost all manner of wonderful structures has been sufficiently shown by modern analysis. But have these conceptions respecting the nature of molecular forces led to con-

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[*] Translated from Poggendorff's *Annalen*, vol. cxxi. p. 579.

[†] Phil. Mag. S. 4. vol. xxvi. p. 81.

sequences which could have been arrived at in no other way? and have these consequences always been correct?

The *Theory of Capillarity* can be developed equally well *without* these hypotheses; and the only result to which they have led is the principle discovered by Laplace and Poisson, that the height of liquids in capillary tubes at different temperatures is proportional to the densities of the liquids. This principle, however, is false. The same assumption, applied to the doctrine of *Electricity*, led Poisson to the determination of the relation between the two electric constants; but this conclusion also has turned out to be incorrect. Poisson's calculation has been partially altered, and the final deductions veiled under an unsummed summation-formula; but does such a result give us any right to consider the accuracy of our previous hypotheses as established?

This assumption led, in Cauchy's hands, to wonderful results by its application to the theory of light. Originally it served to explain the dispersion of light; it was, however, at once found that a materially vacuous space would disperse light, provided that a definite hypothesis was made regarding the nature of force as a function of the distance. One would have supposed that so important a glimpse into the nature of molecular forces would necessarily lead to further consequences; but this was not the case. On the contrary, it was found in this case as in others—for example, in the cases of the hypotheses which it was necessary to add in order to explain double refraction—that the new hypotheses were serviceable for those purposes only for which they had been invented: ulterior consequences could not and might not be drawn from them.

I will not, however, dwell upon objections of this kind, since they have already been made, but still have not overthrown the theory. They have on their side only a certain degree of probability, while against them are ranged all the important results which have hitherto been deduced from the theory. On the contrary, I will lay stress upon a new objection, inasmuch as it in reality upsets the theory, while the great results of the theory which can be opposed to the objection, are not so great but that, as I shall show, they can be much more completely attained by a different method.

The phenomenon of circular polarization obliged Cauchy to assume a *periodicity* in the internal structure of bodies. Against this nothing can be objected; for such a heterogeneity is precisely the most *general* case, and homogeneity, on the contrary, a particular case. Cauchy, however, committed the fundamental mistake of assuming that the mean values of the periodic displacements of the particles of the æther were approximately dependent merely on the mean values of the periodic coefficients of his differential equations. This theorem, taken as one of general application, can be easily disproved by a simple example. The following differential equation may serve as such :—

$$\left(a+b\cos\frac{x}{\alpha}\right)\frac{d\phi}{dx}+\phi=0,$$

where we will assume a to be greater than b, and α very small. The mean value of the periodic coefficient $a+b\cos\frac{x}{\alpha}$ is a, and the mean value of the integral would therefore, according to the above theorem, be approximately

$$\phi=e^{c-\frac{x}{a}}.$$

But this result is incorrect; for integration gives exactly

$$\phi=e^{c-\int\frac{dx}{a+b\cos\frac{x}{\alpha}}},$$

and afterwards approximately, if α is very small,

$$\phi=e^{c-\frac{x}{\sqrt{a^2-b^2}}},$$

a value which differs essentially from the foregoing, inasmuch as the constant of periodicity b enters into it.

The theorem, therefore, cannot be maintained in all its generality; neither is it any more specially applicable to Cauchy's differential equations. In the *first* place, not only circular polarization, but also double refraction, if the equations are otherwise correct, must result from the periodicity of the coefficients. In order to prove this, we have no need to lose ourselves in endless calculations, for nature herself has carried out the calculation. Brewster and, more recently, M. Schultze (*Verhandl. d. rheinl. Gesellsch.* 1861) have in fact shown that *transparent substances in thin layers are doubly refracting.* This important fact therefore demonstrates that a periodicity in the interior of bodies must involve double refraction.

In the *second* place, it is at once obvious that the *thickness* of the layers of a periodically heterogeneous body must exert an influence upon the course of the ray which will depend upon its wave-*length,* and therefore that, whatever differential equations we take as our basis, chromatic dispersion at least is deducible from the periodicity of the coefficients. This explanation of chromatic dispersion shows moreover very simply why it is that chromatic dispersion is peculiar to material bodies and is not possessed by a vacuum.

If, however, we now find that theory requires us, on the one hand, to assume a periodicity in the internal structure of bodies, but that, on the other hand, this assumption brings with it not only the explanation of circular polarization, which had necessitated its being made, but also the explanation of double refraction and chromatic dispersion—phenomena which had given rise to the fundamental hypotheses of the molecular forces—these latter hypotheses become quite superfluous; and an hypothesis which is superfluous is false. The whole complicated apparatus of hypotheses becomes merely an arbitrary appendage to the theory as soon as all can be explained by periodicity.

Thus, as in other branches of mathematical physics, so also in optics, we are obliged to give up the common conceptions concerning the nature of molecular forces. It would be of very little use to try to construct the theory of light upon new physical hypotheses, or upon new conceptions of what goes on in the interior of bodies, concerning which we are probably quite unable to form *any* conception whatever. The science of our day takes a totally different direction, and seeks to free itself from all such conceptions, which are only *ignes fatui*, and perhaps no better guides than the conceptions of Bacon's time were in their day.

In the theory of light we do not require to know any magnitudes but such as we can directly or indirectly take cognizance of. These magnitudes are, the *Intensity*, the *Velocity*, the *Direction of emission*, the *Colour*, the *Phase*, and lastly the position of the *Plane of Polarization*. Light, as is well known, does not make a momentary impression upon our eye; and the intensity which we measure is therefore a *mean* intensity, or the sum of all the impressions made during a short interval of time; but if we suppose the eye capable of perceiving the intensity of every colour, at every single moment of time, and at every point in space, it would then be able to determine not merely the mean intensity, but also the velocity, the direction of emission, the colour, and the phase; for every alteration of any one of these would modify the impression. We may therefore comprehend all these magnitudes under one idea—that of *Intensity* in a wide sense. In addition to this function of the time and of the coordinates of space, we only require two more in order to be able to determine the position of the plane of polarization; light is thus at every point and at every moment fully characterized by *three* magnitudes dependent upon the time (t) and the coordinates of the point (x, y, z).

The problem of the theory now becomes to find three partial differential equations which hold good for all media, and express the dependence of these three magnitudes upon the independent variables (x, y, z, t). From these equations we must then be able to deduce all the phenomena of light, those only excepted which depend upon unknown forces, such as electrical and chemical forces. Hence, conversely, only the fundamental equations can be in turn deduced from these phenomena, but nothing like a physical theory: as well might we expect to deduce such a theory from the phenomena of reflexion in a concave mirror or from the refraction of a lens, as from diffraction or double refraction &c. On the contrary, the physical explanation is probably hidden under the unknown forces already referred to.

If we now try to find the three partial differential equations which are to be the foundation of the theory, we soon discover that the intensity and plane of polarization cannot be directly introduced into such equations, but that auxiliary magnitudes dependent upon these must be taken instead. Let these auxiliary magnitudes be ξ, η, ζ; they may be spoken of simply as light-components. I will not now dwell longer upon the manner in which the three differential equations are arrived at in my former memoir; for *after* they have been found it is immaterial *how* this has been done; their truth must be evidenced by their capability of accounting for all the phenomena of light, with the exception of those which depend upon unknown forces. These equations, in a somewhat altered form and without the dashes over the components, are as follows:—

$$\left.\begin{aligned}\frac{d}{dy}\left(\frac{d\xi}{dy}-\frac{d\eta}{dx}\right)-\frac{d}{dz}\left(\frac{d\zeta}{dx}-\frac{d\xi}{dz}\right)&=\frac{1}{\omega^2}\frac{d^2\xi}{dt^2},\\ \frac{d}{dz}\left(\frac{d\eta}{dz}-\frac{d\zeta}{dy}\right)-\frac{d}{dx}\left(\frac{d\xi}{dy}-\frac{d\eta}{dx}\right)&=\frac{1}{\omega^2}\frac{d^2\eta}{dt^2},\\ \frac{d}{dx}\left(\frac{d\zeta}{dx}-\frac{d\xi}{dz}\right)-\frac{d}{dy}\left(\frac{d\eta}{dz}-\frac{d\zeta}{dy}\right)&=\frac{1}{\omega^2}\frac{d^2\zeta}{dt^2}.\end{aligned}\right\}\quad .\ .\ .\ (\mathrm{A})$$

In addition to these equations, and complementary to them, we have two others, which express the dependence of the intensity and the position of the plane of polarization upon the components ξ, η, ζ, the intensity of the light being determined by the equation

$$\mathrm{I}=\frac{1}{\omega^2}(\xi^2+\eta^2+\zeta^2),$$

and the plane of polarization by the equation

$$\xi(x'-x)+\eta(y'-y)+\zeta(z'-z)=0,$$

where x', y', z' are the current coordinates of this plane. The components are therefore proportionate to the cosines of the angles which the perpendicular to the plane of polarization makes with the three axes of coordinates respectively.

The magnitude ω is a function of x, y, and z, which for *homogeneous* media becomes a constant. In these cases the equations assume a well-known simple form, whence the well-known laws of the propagation of light in homogeneous media can be deduced, as well as the laws of diffraction (when this is not complicated with simultaneous refraction and reflexion), those of interference, of the polarization of the diffracted ray, and of the decrease of the intensity of the light emitted by a luminous point with the square of the distance. I need not now stay to discuss the manner in which these laws are deduced from the familiar equations, and I will consider it as well known and established that the equations are true for homogeneous media. It results from the form of the integrals that light may be regarded as a wave-motion, without nevertheless our being able to form any kind of notion as to the nature of this wave-motion, as must be sufficiently evident from what has gone before. The integrals further show that ω is the velocity of propagation.

Passing on to *heterogeneous* bodies, we see at once that the determination of the notions of intensity and plane of polarization is to a certain extent arbitrary, inasmuch as these cannot be experimentally ascertained for heterogeneous bodies. The plane of polarization is arbitrarily fixed as for homogeneous media, while for the determination of the intensity we have the rule that the intensity of all the refracted and reflected light is equal to that of the incident light when the refracting body is perfectly transparent and homogeneous: whether such a medium is actually to be met with in nature is of no consequence. In order to arrive at this, we might have multiplied the components by some power of ω and then have fixed this power by the condition already made, as was done in the previous memoir; but we shall soon see that this is already accomplished in the above equation of intensity.

In the previous memoir I have deduced the laws of double refraction, of circular polarization, and of chromatic dispersion from the differential equations; moreover the principles which are to serve to calculate the reflexion and refraction of light result from the integration of the equations, inasmuch as I have found that the four magnitudes

$$\eta, \quad \zeta, \quad \frac{d\eta}{dx}-\frac{d\xi}{dy}, \quad \frac{d\zeta}{dx}-\frac{d\xi}{dz}$$

have the same value on both sides of the plane of coordinates $y\,z$, which we assume as the limiting surface of the two media. Before trying to develope further the consequences of the results already obtained, I will show how a theory of the reflexion and refraction of light which agrees perfectly with experiment, may be deduced from the last limiting conditions.

It is well known that Neumann, in his classical work on the reflexion and refraction of light, treated this problem in a very complete manner; and the results of his calculation have been universally confirmed by experiment. The suppositions from which he sets out seem, nevertheless, at the first glance quite opposed to ours: his luminous vibrations are situated in the plane of polarization and in the plane of the wave, the intensity of the light is measured by the square of these vibrations, and, finally, his four limiting conditions are quite different from ours. If, however, we disregard the arbitrary physical signification which he has assigned to his light-components, it appears at once that he has used different auxiliary magnitudes from ourselves. We will denote these new components by ξ', η', ζ', and we will in the first place investigate the relation between these and our own.

In the previous memoir it has been shown that, for a periodically heterogeneous body, the components ξ, η, ζ can be developed in series whose terms contain the variable factor

$$\cos(kt-lx-my-nz-\Lambda),$$

wherein k, l, m, n are constants, and Λ, on the other hand, in general a function of x, y, z, which becomes a constant in the first three terms only. The portion of these series which contains Λ as a variable, indicates a periodic motion changing with the periodicity of the body; while the other portion of the components, which we will denote by ξ_s, η_s, ζ_s, represents the proper visible motion propagating itself in plane waves.

In accordance with the differential equation (A), we have

$$\frac{k^2}{\omega^2}\xi=m(m\xi_s-l\eta_s)-n(l\zeta-n\xi_s)+\ldots.,$$

the subsequent terms containing merely the other portion of the components. By multiplication by ξ we get from this equation

$$\frac{k^2}{\omega^2}\xi^2=m\xi_s(m\xi_s-l\eta_s)-n\xi_s(l\zeta_s-n\xi_s)+\ldots.$$

The intensity of the *visible* light, which we will denote by I_s, is that portion of the expression $\frac{1}{\omega^2}(\xi^2+\eta^2+\zeta^2)$ which contains Λ only as a constant; and to determine this we easily get the equation

$$\mathrm{I}_s=\frac{1}{k^2}\left[(n\eta_s-m\zeta_s)^2+(l\zeta_s-n\xi_s)^2+(m\xi_s-l\eta_s)^2\right].$$

The same intensity, expressed by means of Neumann's components, gives

$$\mathrm{I}=\xi'^2+\eta'^2+\zeta'^2.$$

Besides, ξ_s, η_s, ζ_s are proportional to the cosines of the angles contained between the perpendicular to the plane of polarization of the visible light and the axes of coordinates, whereas Neumann's resultants lie in this plane; thus we have

$$\xi_s\xi'+\eta_s\eta'+\zeta_s\zeta'=0.$$

The last resultant, moreover, lies in the plane of the wave, whence we have

$$l\xi'+m\eta'+n\zeta'=0.$$

From these equations result the following relations between the two sets of components, namely,

$$k\xi'=n\eta_s-m\zeta_s,\quad k\eta'=l\zeta_s-n\xi_s,\quad k\zeta'=m\xi_s-l\eta_s,$$

equations to which we can also give the form

$$\frac{d\xi'}{dt}=\frac{d\eta_s}{dz}-\frac{d\zeta_s}{dy},\quad \frac{d\eta'}{dt}=\frac{d\zeta_s}{dx}-\frac{d\xi_s}{dz},\quad \frac{d\zeta'}{dt}=\frac{d\xi_s}{dy}-\frac{d\eta_s}{dx}.$$

Our limiting equations, which we may write as follows,

$$[\eta_s]_{x=0}^{x=\epsilon}=0,\quad [\zeta_s]_{x=0}^{x=\epsilon}=0,\quad \left[\frac{d\eta_s}{dx}-\frac{d\xi_s}{dy}\right]_{x=0}^{x=\epsilon}=0,$$

$$\left[\frac{d\zeta_s}{dx}-\frac{d\xi_s}{dz}\right]_{x=0}^{x=\epsilon}=0,$$

where ϵ is an infinitely small quantity, shall now be expressed with the new components. From the last two equations we get at once

$$[\eta']_{x=0}^{x=\epsilon}=0,\text{ and }[\zeta']_{x=0}^{x=\epsilon}=0.$$

By the first equations, we have

$$\left[\frac{d\eta_s}{dz}-\frac{d\zeta_s}{dy}\right]_{x=0}^{x=\epsilon}=0;$$

accordingly

$$[\xi']_{x=0}^{x=\epsilon}=0.$$

Thus all the components, ξ', η', ζ', take the same values at the limiting surface of the two media, which agrees perfectly with Neumann's hypotheses. Lastly, the fourth limiting equation, which with our components we may express by

$$[m\eta_s+n\zeta_s]_{x=0}^{x=\epsilon}=0,$$

may be arrived at as follows for the new components.

Let q be the angle which the perpendicular to the plane of the wave makes with the ray of light. Since our resultant, as was shown in the previous memoir, makes the same angle with the plane of the wave, we have

$$\sin q = \pm \frac{l\xi_s + m\eta_s + n\zeta_s}{\sqrt{l^2+m^2+n^2}\ \sqrt{\xi_s^2+\eta_s^2+\zeta_s^2}},$$

whence follows

$$\tan q = \pm \frac{l\xi_s + m\eta_s + n\zeta_s}{\sqrt{(n\eta_s - m\zeta_s)^2 + (l\zeta_s - n\xi_s)^2 + (m\xi_s - l\eta_s)^2}}.$$

We get, further, from the above values of η' and ζ',

$$k(n\eta' - m\zeta') = l(m\eta_s + n\zeta_s) - (m^2+n^2)\xi_s.$$

By eliminating ξ_s from these two equations, after having exchanged the denominator of the first for $k\sqrt{\xi'^2+\eta'^2+\zeta'^2}$, we have

$$m\eta + n\zeta = \frac{k}{l^2+m^2+n^2}\left[l(n\eta' - m\zeta') \pm (m^2+n^2)\tan q\sqrt{\xi'^2+\eta'^2+\zeta'^2}\right];$$

and this expression must therefore have the same value on both sides of the limiting surface. If, adopting Neumann's notation, we call the angle which the plane of the wave and the plane of refraction (yz) make with each other ϕ, and the angle which the new resultant makes with the line of intersection of the plane of the wave and the plane of refraction ψ, we have

$$\frac{l}{\sqrt{l^2+m^2+n^2}} = \cos\phi,$$

$$n\eta' - m\zeta' = \pm\cos\psi\sqrt{n^2+m^2}\ \sqrt{\xi'^2+\eta'^2+\zeta'^2}.$$

By introducing into it these magnitudes, the last limiting equation becomes

$$\left[\sqrt{\xi'^2+\eta'^2+\zeta'^2}(\cos\phi\sin\phi\cos\psi \pm \sin^2\phi\tan q)\right]_{x=0}^{x=\epsilon} = 0.$$

This condition agrees exactly with Neumann's hypothesis, if only we take the double sign arbitrarily, as Neumann did. Strictly speaking, Neumann started from the assumption that the intensity of the incident light is equal to the intensity of all the refracted and reflected light; he, however, refers this assumption back to the previous one. Conversely, therefore, we may conclude that our four limiting equations include the principle of the maintenance of the intensity, and hence that our intensity equation, by which this condition must be fulfilled, is rightly chosen.

Now that we have thus deduced Neumann's hypotheses from our own, the problem we had proposed to ourselves is fully solved; for it follows from the results obtained that the same theory of reflexion and refraction, complete and accordant with experiment, which Neumann has developed, can likewise be deduced from our fundamental equations. Here, as previously in the theory of double refraction, we have arrived at one of the stations from which the formal part of the theory takes its start, and, thanks to the great development which this part of the theory has attained, a whole section of the doctrine of light again lies before us (without our needing to take one step further) as a simple consequence of our theory. If the theory of reflexion and refraction had not already been developed as it has been, we should not endeavour to express the limiting equations by means of any other auxiliary magnitudes than our components; for both the limiting equations and the law of double refraction, when expressed by means of these, assume their simplest form, and hence also the calculation with them would be simpler and more elegant.

In the above calculation we have indeed taken into consideration the immediate effect of the periodic part of the components, which must accompany every wave of light in the periodically heterogeneous body and is dependent upon it; there arises, however, also a secondary effect of the periodic motions of the two media, a mutual reflex action, which cannot be without influence upon the visible light-motion. Here, as throughout all nature, we meet with a perturbation; and this small departure from the results obtained will probably be capable of confirmation by experiment. Were both media homogeneous, both the calculation and the principle of the maintenance of the intensity would be exactly true; not so, however, in the opposite case. This also may be made directly evident; for a part of the original quantity of light must necessarily be extinguished in the production of periodic wave-motions within the body. I will here pass over another perturbation, to which I have previously directed attention[*], and which depends upon the fact that the two media are not separated by a perfectly sharp mathematical plane.

The loss of visible light above mentioned must not be confounded with that which arises from simple absorption. The latter can be easily calculated. Modern investigations into the reflexion and refraction of imperfectly transparent and metallic bodies have, in fact, led to the remarkable result, that the same laws which apply in the case of transparent bodies apply here also, with the single difference, that the refractive ratio now assumes the complex form $a \pm b\sqrt{-1}$. From this fact we can draw the important conclusion that our differential equations hold good not only for transparent bodies, but for *all bodies without exception.* It is moreover apparent, if we endeavour to

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[*] Poggendorff's *Annalen*, vol. cxi. p. 111.

find the refractive ratio by means of the serial developments of the former memoir, that it can also assume this more general form; but it is difficult to indicate the conditions under which this will occur. It appears, however, that this case must arise when the interval between two similar points of a body is not small relatively to the wave-length. For this reason, for instance, a transparent body ceases to be transparent when pulverized.

If we now place the results of the former memoir side by side with our present ones, we perceive that our object—namely, to deduce all the phenomena of light, which do not depend upon unknown, electrical or chemical forces, from our fundamental equations—is now attained; for the explanation of *Double Refraction*, of *Circular Polarization*, of *Chromatic Dispersion*, of *Reflexion*, and of *Refraction* results from them as a simple consequence. The general theory of diffraction may here be passed by, for it can afford no control of our theory. For homogeneous bodies, it is easily deducible from our equations; but as soon as the phenomenon is complicated with simultaneous reflexion and refraction, the difficulty lies even more in finding the conditions which correspond to each particular experiment than in the calculation itself; and agreement between calculation and experiment would rather prove that the conditions had been rightly chosen, than be a control of the theory. I think therefore that I may regard the accuracy of our fundamental equations as fully established, and I will in the sequel direct attention only to a few further consequences of the results that have been obtained.

The periodic coefficient ω occurring in our equations may be expressed, as has already been done in the previous memoir, in a general manner by the equation

$$\frac{1}{\omega^2}=\frac{1}{\Omega^2}\left(1+\Sigma\epsilon_p\cos\frac{a_px+b_py+c_pz+d_p}{\alpha_p}\right),$$

where Ω, ϵ_p, &c. are constants. According to this formula, we consider the body as made up of several systems of parallel layers, whose thickness is α_p, and whose perpendicular makes with the axes of coordinates angles which are determined by their cosines a_p, b_p, c_p. Further, let a, b, c be the velocities of propagation which the components possess in the directions of the three axes, while $a_{1,1}$, $a_{2,2}$, $a_{3,3}$ are connected therewith by the equations

$$a_{1,1}=\frac{\Omega^2}{a^2},\quad a_{2,2}=\frac{\Omega^2}{b^2},\quad a_{3,3}=\frac{\Omega^2}{c^2}.$$

These last magnitudes, like $a_{1,2}$, &c., in the former memoir, can now be determined from the constants of the body; and for

$\alpha_p=0$ we easily find

$$a_{1,1}=1-\Sigma\frac{\epsilon_p{}^2}{2}a_p{}^2,\quad a_{2,2}=1-\Sigma\frac{\epsilon_p{}^2}{2}b_p{}^2,\quad a_{3,3}=1-\Sigma\frac{\epsilon_p{}^2}{2}c_p{}^2.$$

If a body is, for instance, stratified in one direction only, and we take the perpendicular to the layers as the axis of x, we have $a_p=1$, and $b_p=c_p=0$. Consequently the velocities b and c become equal to each other when a, or the velocity of propagation of the components in the direction of the perpendicular, has reached its greatest value. Such a substance therefore behaves like a doubly refracting, uniaxal, *negative* crystal, whose axes are perpendicular to the layers of the body. This result agrees with the experiments of M. Schultze (*Verh. der rheinl. Gesellsch.* 1861).

If we call the velocity of light *in vacuo* O, the refractive index corresponding to the velocity a is $\frac{\mathrm{O}}{a}$, and a *mean* refractive index will be expressed by

$$n=\sqrt{\frac{1}{3}\mathrm{O}^2\left(\frac{1}{a^2}+\frac{1}{b^2}+\frac{1}{c^2}\right)}.$$

This mean refraction coefficient is independent of the position of the axes chosen, even if the small magnitudes α_p are not quite neglected; for we find

$$a_{1,1}=a_{2,2}+a_{3,3}=3-\Sigma\frac{\epsilon_p{}^2}{2}+\Sigma\epsilon_p{}^2\alpha_p{}^2\frac{k^2}{\Omega^2},$$

whence we have

$$n=\frac{\mathrm{O}^2}{\Omega^2}\left(1-\frac{1}{6}\Sigma\epsilon_p{}^2+\frac{4}{3}\pi^2\frac{\mathrm{O}^2}{\Omega^2}\Sigma\epsilon_p{}^2\frac{\alpha_p{}^2}{\lambda^2}\right),$$

a value which is independent of the choice of the axes.

In the last equation k is put $=\frac{2\pi}{\lambda}$, where λ denotes the wave-length, in order that it may at once be evident that the mean refraction coefficient, or its square, assumes the form $a+b\,\frac{1}{\lambda^2}$, where b is always a positive quantity. The smaller therefore the wave-length is, so much the more refrangible does the light become—a law which has universally proved true; still, the above approximate formula might possibly not be sufficient for absorbent media, and in this way exceptions to the rule might arise.

The form which we have given to the function ω has been the most simple and convenient for the foregoing calculations; but if we wish in addition to form any conclusions respecting the internal constitution of bodies, it becomes necessary to determine the constants more precisely. The coefficient $\frac{1}{\omega^2}$ is in general any function $f(x, y, z)$ which satisfies the equation

$$f(x, y, z)=f(x+2p\alpha_1, y+2q\beta_1, z+2r\gamma_1),$$

if p, q, r are arbitrary whole numbers, and α_1, β_1, γ_1 are very small constants. This condition expresses that the body is heterogeneous in such sort that the heterogeneity eludes observation by a very quickly returning periodicity, and the body thus *appears* to be homogeneous.

Such a function can be expressed by the well-known formula

$$f(x,y,z)=\Sigma\int\frac{d\varpi}{\varpi_1}f(\alpha,\beta,\gamma)\cos\pi\left(\frac{i(x-\alpha)}{\alpha_1}+\frac{i_1(y-\beta)}{\beta_1}+\frac{i_2(z-\gamma)}{\gamma_1}\right),$$

where i, i_1, i_2 are whole numbers which run through the series of numbers from $-\infty$ to $+\infty$, and the integral is taken from $-\alpha_1$, $-\beta_1$, $-\gamma_1$ to α_1, β_1, γ_1. Further, $d\varpi$ is put in place of $d\alpha\, d\beta\, d\gamma$, and ϖ_1 is the value of $\int d\varpi=8\alpha_1\beta_1\gamma_1$.

If the term which contains $i=i_1=i_2=0$ is separately extracted from this sum, and if moreover the negative values of i are excluded by multiplying the sum by 2 and taking the terms which contain $i=0$ at half their value, we get, with the meaning of the sign of summation thus altered,

$$f(x,y,z)=\int\frac{d\varpi}{\varpi_1}f+2\Sigma\int\frac{d\varpi}{\varpi_1}f\cos\pi\left(\frac{i(x-\alpha)}{\alpha_1}+\frac{i_1(y-\beta)}{\beta_1}+\frac{i_2(z-\gamma)}{\gamma_1}\right),$$

where f is put for $f(\alpha, \beta, \gamma)$.

If we now compare this value of the coefficient

$$\frac{1}{\omega^2}=f(x, y, z)$$

with the previous one, we have

$$\frac{1}{\Omega^2}=\int\frac{d\varpi}{\varpi_1}f,$$

$$\epsilon_p\cos\frac{d_p}{\alpha_p}=2\Omega^2\int\frac{d\varpi}{\varpi_1}f\cos\pi\left(\frac{i\alpha}{\alpha_1}+\frac{i_1\beta}{\beta_1}+\frac{i_2\gamma}{\gamma_1}\right),$$

$$\epsilon_p\sin\frac{d_p}{\alpha_p}=-2\Omega^2\int\frac{d\varpi}{\varpi_1}f\sin\pi\left(\frac{i\alpha}{\alpha_1}+\frac{i_1\beta}{\beta_1}+\frac{i_2\gamma}{\gamma_1}\right).$$

From the last two equations we may deduce the value of $\Sigma\epsilon_p{}^2$, and simplify the resulting expression by introducing the product of two equal integrals instead of the square of the definite integral. If the variables of the one integral are then distinguished by accents, and f' put instead of $f(\alpha', \beta', \gamma')$, we obtain

$$\Sigma\epsilon_p{}^2 = 4\Omega^4 \Sigma \int \frac{d\varpi}{\varpi_1} f \int \frac{d\varpi'}{\varpi_1} f' \mathrm{C},$$

where

$$\mathrm{C} = \cos \pi \left(\frac{i(\alpha-\alpha')}{\alpha_1} + \frac{i_1(\beta-\beta')}{\beta_1} + \frac{i_2(\gamma-\gamma')}{\gamma_1} \right).$$

But in accordance with the above formula for $f(x, y, z)$, we have

$$2\Sigma \int \frac{d\varpi'}{\varpi_1} f' \mathrm{C} = f(\alpha, \beta, \gamma) - \int \frac{d\varpi'}{\varpi_1} f(\alpha', \beta', \gamma'),$$

and therefore

$$\Sigma\epsilon_p{}^2 = 2\Omega^4 \left[\int \frac{d\varpi}{\varpi_1} f^2 - \left(\int \frac{d\varpi}{\varpi_1} f \right)^2 \right].$$

The values of Ω^2 and $\Sigma\epsilon_p{}^2$ can now be introduced into the above expression for the mean refractive index. Since, however, we may suppose the perturbing effect of chromatic dispersion to be eliminated by calculation, we may entirely disregard the small quantity α_p, and only introduce the new magnitudes into the equation

$$r^2 = \frac{\mathrm{O}^2}{\Omega^2} - \frac{\mathrm{O}^2}{6\Omega^2} \Sigma\epsilon_p{}^2,$$

where r accordingly is the value of the reduced mean refractive index.

We will now try to apply the results we have obtained. It must not, however, be overlooked that the last equation gives only an approximation to the value of r, inasmuch as it was originally assumed that the magnitudes ϵ_p were small. I have indeed succeeded in developing the exact value of r in a series the first terms of which are the above, and I am ready to communicate this calculation in case it shall be called for; but the above approximate formula may nevertheless be considered sufficient for the present purpose.

It is well known how the idea of the absolute *refractive power* early arose in science, how it has been discarded by the prevailing theory as useless, and how it nevertheless, claiming a certain currency in spite of the theory, is perpetually reappearing. It is only needful to cast a glance at the general survey of refractive powers of various bodies given by Albr. Schrauf in Poggendorff's *Annalen*, vol. cxvi., to be convinced of a certain regularity. Especially it may be seen that the refractive power, freed from the influence of dispersion, as it is expressed by the formula

$$\mathrm{M} = \frac{r^2-1}{\rho},$$

where ρ is the density of the substance, remains nearly constant for the same body at different densities, and that the refractive power of mixtures can be calculated from those of their separate constituents by the formula

$$Mp = M_1 p_1 + M_2 p_2 + \dots$$

if p is the weight of the mixture, and the marked letters are taken as applying to the several constituents of the mixture. An additional law, which seems not to have been remarked, appears from the same table of refractive powers (*loc. cit.* p. 211), namely, that the refractive power always and without exception decreases somewhat with the refractive index. It was precisely the present theory which first drew my attention to this point.

The above-mentioned empirical laws can in fact all be deduced from our theory, but only on the supposition that bodies are made up of transparent particles or molecules separated by interstices in which the velocity of light is the same as in a vacuum. The molecules must likewise, so far as these laws remain applicable, be unalterable, in the sense that any alteration of the body affects merely the size of these interstices and the arrangement of the molecules themselves.

In the integral

$$\frac{1}{\Omega^2} = \int \frac{d\varpi}{\varpi_1} f,$$

we may therefore separate the elements which belong to the empty spaces surrounding the molecules. Since the velocity of light (ω) is here equal to O, and therefore

$$f = \frac{1}{O^2},$$

we obtain

$$\frac{1}{\Omega^2} = \frac{1}{O^2} + \int \frac{d\varpi}{\varpi_1}\left(f - \frac{1}{O^2}\right),$$

if we allow the integration now to extend only to the unalterable portion of the space ϖ_1, occupied by the molecules.

If ρ is put for the density of the substance, then $\rho\varpi_1$ remains a constant quantity, which may be denoted by c, for all alterations of density. According to this, we obtain

$$\frac{1}{\Omega^2} = \frac{1}{O^2}(1 + \rho P),$$

where

$$P = \int \frac{d\varpi}{c}(O^2 f - 1),$$

a quantity which is independent of the density of the substance.

The integral

$$\int \frac{d\varpi}{\varpi_1} f^2$$

can be transformed in the same way. If we put

$$S = \int \frac{d\varpi}{c} (O^4 f^2 - 1),$$

where the integration has reference only to the portion of the space ϖ_1 occupied by the molecules, we obtain

$$\Sigma \epsilon_p^2 = 2\rho \frac{S - 2P - \rho P^2}{(1 + \rho P)^2},$$

and hence

$$r^2 = 1 + \rho P - \frac{1}{3} \rho \frac{S - 2P - \rho P^2}{1 + \rho P}.$$

Since, according to our supposition, the last term is very small, the refractive power is nearly constant, namely

$$M = \frac{r^2 - 1}{\rho} = P.$$

If we take into consideration the last term also, the refractive power assumes nearly the form

$$M = Q - \frac{R}{r^2},$$

where Q and R are two positive constants. The refractive power therefore diminishes with the refractive index.

The above formula,

$$\frac{1}{\Omega^2} = \frac{1}{O^2}(1 + \rho P),$$

holds good for mixtures as for substances in general. If the densities and volumes of the several constituents were originally $\rho_1, \rho_2, \rho_3 \ldots$ and $v_1, v_2, v_3 \ldots$, while the volume of the mixture is v, the constituents possess in the mixture the densities $\rho_1 \frac{v_1}{v}$, $\rho_2 \frac{v_2}{v}, \ldots$ The constants $P_1, P_2, \ldots$ of the constituents, however, do not alter with the density; thus we also have

$$\frac{1}{\Omega^2} = \frac{1}{O^2}\left(1 + \rho_1 \frac{v_1}{v} P_1 + \rho_2 \frac{v_2}{v} P_2 + \ldots\right).$$

Now $v\rho, v_1\rho_1, v_2\rho_2, \ldots$ are proportional to the weights of the mixture and of its several constituents; we thus obtain, if $p, p_1, p_2, \ldots$ are these weights,

$$pP = p_1 P_1 + p_2 P_2 + \ldots$$

In a similar manner we also obtain

$$pQ=p_1Q_1+p_2Q_2+\dots$$
$$pR=p_1R_1+p_2R_2+\dots$$

The present theory therefore does not only afford an explanation of the constancy of the refractive power and of the formula for mixtures, but it defines these laws even more distinctly than before, and in particular shows that the refractive power must diminish with the refractive index: this is fully confiremd by experiment. And these results may suffice us on this occasion; for it is needless to enter further into the details of experiment, partly because it is self-evident that a closer agreement can be obtained with two constants than with one, and partly because our formulæ are yet only approximately true.

In conclusion, it will not be uninteresting to see how the various phenomena of light instruct us as to the internal constitution of material bodies. Double refraction indicates to us, in the first place, the regular stratification of crystals, a character which is also revealed by their external properties. The thickness of the layers cannot, however, be deduced from double refraction, for this would not cease even if the thickness were infinitely small; circular polarization, on the other hand, and especially the more general chromatic dispersion, prove that this is not the case, and that in all bodies, the elastic fluids scarcely excepted, there is a stratification of measurable dimensions. Circular polarization further indicates a want of symmetry in the interior of certain crystals, which is likewise evidenced by their external characters; and the rare occurrence of this kind of polarization teaches us that such a want of symmetry is not the common case. Finally, the refractive power indicates that the limit of the periodicity in the interior of bodies is on one side of a vacuum, and therefore that material bodies consist of separate transparent molecules.

LII. *On Molecular Physics.* *By* Prof. W. A. NORTON.

[Continued from p. 389.]

Heat.

THE general nature of heat, and the general cause of its evolution, have already been considered (p. 196). According to the fundamental ideas presented, heat must be developed whenever the electric atmospheres of the molecules of any substance are urged into closer proximity to the atoms which they surround. Any disturbance of the equilibrium of the particles of a given mass, or any change in the relations of the particles of any body to those of other bodies, which may have the effect of producing this compression of the molecular atmospheres, should, then, be a source of heat. Thus chemical combination of two particles in which they are drawn into close union, collision of bodies, external pressure, and friction are different sources of heat. The disturbance of the electric equilibrium of contiguous molecules may also give rise to the evolution of heat—by reason of the increased repulsion exerted by the excess of electric æther accumulated upon certain of the molecules, or upon one side of them, or of a discharge of the æther occurring from one molecule to another.

Propagation of Heat.—Primarily the heat-pulses are developed in the universal æther associated with the atoms of bodies. These pulses may be conveyed outward through the universal æther posited between the atoms, and in this way be freely *transmitted* through substances; or they may be more or less taken up or *absorbed* by the electric atmospheres of the molecules which they encounter. Such absorbed pulses may be given out again or *radiated* in their original form, or they may pass on to contiguous molecules through the *electric* æther that pervades the interval between them. It is probably in this latter mode chiefly that heat is *conducted* from particle to particle of a substance, though the pulses that are given out by any particle to the surrounding *universal* æther may be in part propagated onward, absorbed by the particles they encounter, and partially propagated onward again in the same manner to the next particles.

The flow of heat from one particle to another of a substance tends to disturb the electric condition of the particles; for the repulsive action of the heat-pulses in the atmosphere of one particle tends to urge away a portion of the electric æther from the contiguous side of the next particle in the line of propagation, and so to induce a negative electric state upon that side, and a positive state on the further side. This disturbance of the electric equilibrium of contiguous molecules may give rise to a discharge of the electric æther from the one to the other, and a consequent more ready propagation of the heat-pulses from the one to the other. Upon this idea there is a close analogy between the conduction of heat and the conduction of electricity in the galvanic current, both depending upon the facility with which an electric polarization of contiguous particles is determined. The origin of this propagated polarization in the one case is the addition of repulsive pulses to the atmosphere of a molecule, and in the other the accumulation of an excess of the repulsive electric æther around a molecule or upon one side of it. A confirmation of these views is afforded by certain phenomena of thermo-electric currents, from which it appears that the conduction of heat is in reality attended with the disturbance of the electric equilibrium*; and they are also sustained by the fact of the close agreement that subsists between the conducting-powers of the metals for heat and electricity. The analogy between the conduction of heat and the conduction of a galvanic current may be more fully stated thus:—A stream of heat consists of two sets of pulses, viz. those conveyed by the electric æther, and those conveyed by the universal æther; and the propagation is attended with, and partly by means of, an induced molecular polarization. The pulses propagated by the electric æther tend to develope this polarization, which determines a discharge of æther from one atmosphere to the next. But the other set of pulses, in proportion as they are taken up by the nearer side of the next atmosphere, tend to weaken the induced polarization, and so to check the flow of the heat. A galvanic current comprises two similar sets of pulses, is attended and promoted still more effectually by a similar molecular polarization; and the absorption of the æthereal pulses, and their subsequent emission as heat, tends to check the flow of the current.

The feeble conducting-power of many substances is probably due to an aggregation of their particles in groups, with large intervening spaces. The bad conductivity of gases and liquids, both for heat and galvanic electricity, agreeably to the views just offered, must be ascribed to a feeble polarizing action of one particle upon another. This appears to be a consequence of the peculiar state of the molecular atmospheres which determines the fluid condition (p. 279), combined with the effective mutual repulsion of the particles in this condition. In the case of liquids like water, whose ultimate molecules are compound, a portion of the heat propagated should be consumed in expanding the compound molecules.

Capacity of Bodies for Heat.—The fundamental law discovered by Dulong and Petit, that the specific heats of elementary substances are inversely proportional to their chemical equivalents or atomic weights—or, in other words, that the atoms of such substances have the same capacity for heat—if interpreted in the language of the present theory, amounts to this, viz., whatever may be the difference of condition of the molecular atmospheres of elementary substances, if the same amount of heat be imparted to these atmospheres, the same amount will be given off again and interchanged with surrounding bodies having the same temperature. This would seem to imply that the portion of the heat absorbed, which is consumed in expanding the atmosphere, is the same for different simple molecules, and that the remaining portion, which is taken up as new pulses by the atmosphere, is also the same for molecules of different elementary

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* See De la Rive's Treatise on Electricity, vol. ii. p. 536 &c.

substances. There is nothing in the conception we have formed of a molecule, and of the probable difference of physical condition in molecules of different substances, that is apparently opposed to this result.

The capacity for heat of compound is in general greater than that of simple molecules, and is greater in proportion as the molecule is more complex. This indicates that when the temperature is raised 1°, a certain portion of the heat is expended in urging asunder the constituents of the molecules, and that this portion is greater in proportion as the molecule is more complex.

Heat-rays of different rates of vibration.—The calorific spectrum shows that the heat emitted from a hot body is composed of rays of an infinite variety of rates of vibration between certain limits. The physical cause of this fact will appear if we reflect that the heat-rays have their origin in the vibrations of the atomettes of the molecular atmospheres, and that these are situated at every variety of distance from the central atoms, between certain limits. For the circumstances of equilibrium of these atomettes are different, and their rates of vibration when displaced should be different. The fact that the most intense heat-rays, in ordinary cases of combustion, are those of low refrangibility and the phenomena of the evolution of calorific and luminous rays when a body is heated to incandescence, indicate that the electric atomettes which are nearest the central atoms have the lowest rate of vibration.

We have seen that the expansive action of heat is a necessary consequence of the fundamental principle that the heat-pulses constitute a repulsive force, and that they are absorbed, more or less, by the molecular atmospheres which they encounter. It may be urged as an objection to the notion that radiant heat is a repulsive force, that bodies when heated do not exert any sensible repulsive action upon other contiguous bodies. We are not prepared to admit that experiment has furnished no evidence of such repulsive action under any circumstances; but the entire absence of such action upon bodies of sensible magnitude would in fact be no decisive proof that waves of radiant heat do not convey a series of preponderating repulsive impulses; for if these impulses penetrate to the atoms of the molecules, they should be ultimately taken up by their atmospheres, and expended as an expansive force upon these atmospheres, and in urging the molecules asunder; and if they do not reach the atoms, no motion should be imparted to them. Since it is improbable that the more intense impulses of heat will be wholly absorbed by the particles immediately at the surface of the body upon which they fall, a direct repulsive action of the heat may take effect to a certain depth below the surface. Have we not in the spheroidal state of liquids evidence of such action exerted by the radiant heat from the hot vessel upon the liquid resting upon its interior surface?

It is only when the heat-waves impinge upon isolated particles, or a small group of particles, that a progressive motion should be imparted. This supposition is apparently realized in the case of cometary matter repelled by the sun*.

Light.

The question of the precise relation which the two physical agents, light and heat, bear to each other, has not been definitively settled; but the weight of evidence preponderates very decidedly in favour of the doctrine of their essential identity. The only "formidable outstanding objection" to this view consists in the fact that a strong light may be obtained which has little, if any, heating power†. According to Melloni, the greenish light obtained by transmission of white light through a peculiar species of green glass coloured by oxide of copper, "exhibits no calorific action capable of being rendered perceptible by the most delicate thermoscopes, even when it is so concentrated by lenses as to rival the direct raysof the sun in brilliancy." May it not be that the explanation of the possible existence of light without heat, thus made out, is to be found in the presence, in the luminous beam, of a certain number of radiations which have individually too feeble an intensity to exercise any calorific action upon bodies, but are still capable of producing a sensible impression upon the organ of vision? Upon the theory of the constitution of molecules adopted in the present paper, we may suppose that vision results from some action of the luminous pulses upon the molecular atmospheres merely, while heat-expansion is not produced unless the individual pulses have sufficient intensity to penetrate to the surface of the central atoms of the molecules. According to this idea, as rays that penetrate to different depths in the molecular atmospheres of a medium should be unequally absorbed, the rays of feeble intensity or of pure light may be separated from those which, by reason of their

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* In the article by the author, "On the Theoretical Determination of the Dimensions of Donati's Comet" (see Silliman's Journal, vol. xxxii. No. 94. p. 54, &c.), it is established by rigorous calculation that the particles of matter disseminated over the breadth of the tail of the comet were exposed to a force of repulsion from the sun, of various degrees of intensity, between two ascertained limits. In the light of the theoretical views now offered, we must conclude that the matter thus unequally repelled consisted of particles of different sizes, or different absorptive powers for heat, or of different-sized groups of particles.

† See Report of Rev. Baden Powell, M.A., F.R.S., for 1854, on Radiant Heat, published in the Smithsonian Report for 1859, p. 368.

greater intensity, are capable of penetrating to the atoms of the medium and exercising a calorific action upon them.

It is generally admitted that vision is produced by the transverse vibrations of the rays of light. The fact that when two rays of heat, polarized in perpendicular planes, meet in opposite states of vibration they do not neutralize each other, has been generally regarded as an indication that the calorific action of a ray must also result from transverse vibrations; but this does not appear to be a legitimate conclusion, if we adopt Professor Challis's theory, that the luminiferous æther is a highly elastic fluid, having the same properties as elastic fluids in general, and that the æthereal undulations consist of simultaneous longitudinal and transverse vibrations, attended with variations in the density of the medium, as in the case of waves of sound. For if transverse vibrations, in perpendicular planes, meet in opposite states they cannot neutralize each other, and must develope direct vibrations, which will take the place of those which counteract each other and will exert a calorific action. In fact Prof. Challis conceives that "heat is the result of the mechanical action of the direct vibrations;" while "light is due to the transverse vibrations."

The intimate association of heat and light leads to the inference that they emanate from the same source, viz. the molecular atmospheres of bodies; and if, as has been intimated, the two emanations are essentially the same, we infer that rays of light as well as of heat originate in vibratory movements of the atomettes of these atmospheres. The atomettes lying at different distances from the central atom of a molecule will have different rates of vibration, increasing with the distance; and so the waves proceeding from them will have every variety of pulsation between the lowest limit, answering to the bottom, and the highest, answering to the top of a molecular atmosphere. Accordingly the red rays will proceed from the lower portions of the atmosphere, and the violet from more elevated portions.

If the electric atmospheres diminish in density by insensible degrees from bottom to top, there should be no break in the continuity of the rays between the two extremes. But we know, from the existence of bright bands in the spectra obtained from coloured flames and from the highly heated vapours of metals and other substances, that the rates of vibration of the luminous rays given off by incandescent vapours seldom, if ever, vary by insensible degrees from one extreme to the other. We must conclude therefore that the electric atmospheres of highly heated molecules are made up of distinct layers of unequal density.

Phenomena attending the propagation of light.—The *absorption* of light by a medium may be regarded as the taking up of the æthereal pulses by the electric atmospheres of the medium.

In order that a ray may be completely absorbed, it must encounter a layer of the electric atmosphere of a molecule which pulsates naturally in unison with the wave-pulsation of the ray. The absorbing action of a molecule should therefore depend upon the physical condition of its atmosphere as to rates of pulsation, density, &c., and also upon the comparative extent of its electric and æthereal atmospheres. For example, a medium would permit the free passage of any ray that did not penetrate to the surface of the electric atmospheres of its molecules. On the other hand, a medium would intercept rays that should penetrate to atmospheric layers that are in unison with the rays. Accordingly, if an incandescent vapour should emit rays of certain colours, as shown by bright bands in its spectrum, those colours, if transmitted through the vapours, should be absorbed, and the spectrum given by transmitted light should show dark lines answering to the bright lines of the other spectrum—which is the well-known discovery of Kirchhoff and Bunsen.

According to the theory of crystallization presented on p. 384 &c., in all the systems of crystallization in which the axes of molecular attraction are unequal, the electric atmospheres of the molecules are compressed unequally in the lines of direction of these axes. Now, if these atmospheres are compressed unequally, the same will be true of the æthereal atmospheres which pervade them. Thus in all forms of crystals which have unequal axes, the æthereal atmospheres of its separate particles will have unequal densities in the directions of the molecular axes. It is well known that all such crystals have the property of double refraction, and that this property is attributed to a supposed inequality of density or of elasticity of the æther in the direction of certain molecular axes. A mechanical pressure exerted along a certain line or plane also developes the property of double refraction in ordinary refracting media; and such compression should give rise to an increased density of the æthereal atmospheres along the line of pressure. Accordingly our general theory of the constitution of molecules and of molecular forces conducts to the physical basis assumed, in the undulatory theory of light, in explanation of double refraction.

The phenomenon of "atomic circular polarization" by liquids, discovered by Biot, who established that the effect depended solely upon the number of atoms encountered by the light, whatever may be the density of the medium—and the phenomenon of "magnetic circular polarization," in which the direction of rotation of the plane of polarization corresponds with that of the revolution of the circular magnetic currents—are decided indications that optical phenomena result mainly from the action of the *atmospheres* of molecules upon the rays of light. Numerous facts, which go to show that the absorptive action of media upon light and heat depends in a great degree upon the physical constitution of the separate molecules, confirm this conclusion.

The general phenomena and laws of reflexion, refraction, polarization, diffraction, &c., should obtain upon this supposition no less than upon the conception that the phenomena are to be referred to the interstitial æther. It remains to be seen whether the theorems and formulæ deduced by Fresnel and other physicists from the undulatory theory of light, can be shown to be substantially in accordance with this notion of molecular actions.

Note.—Objections to the theoretical views offered in the preceding pages will readily occur to the scientific reader; but it does not comport with the design of the present communication to anticipate objections, or to attempt to enforce the general conclusions deduced from the fundamental positions taken by appeals to special facts. We must be content for the present to rest our conclusions mainly upon general considerations. A connected view of the whole ground to be surveyed is almost a necessary preliminary to the many detailed investigations that must be undertaken before the theory can be established on a firm foundation.

It will be perceived that the most characteristic feature of the general theory under discussion is that it locates the source of physical phenomena in the atmospheres of molecules, instead of in the atoms or in the interstitial æther. In pursuing our deductions into other departments of physics, other general conceptions have been reached, some of which it may be advisable to state here, as circumstances may delay somewhat the publication of the remainder of the article.

1. An *electric current* (hydro-electric or thermo-electric) has its origin in the opposite polarization of the adjacent sides of contiguous molecules, developed by the play of the molecular forces, under special circumstances, or by an inequality in the action of the pulses of heat upon the atmospheres of the molecules. The current consists of an actual flow of electric æther from molecule to molecule, determined by a previous electric polarization propagated from that which is the source of the current. There are also conveyed, in the direction of the positive current, streams of impulses, both by the electric and universal æther, which, by a partial lateral dispersion, produce the magnetic effects of the current.

2. The *mutual attractions* or *repulsions* exerted between two electric currents may be ascribed to a change in the tension of the æther between the wires, produced by the lateral actions of the currents.

3. *Induction-currents* result from an electric polarization of the molecules, suddenly induced by the same lateral action of a primary current when first established, or by the increased action of a previous electric or magnetic current—or suddenly vanishing when the current is broken, or the force of action decreases. The polarization in the first instance is the opposite to that which prevails in the primary current, owing to the indirect nature of the inducing action.

4. The *circular currents of a magnet* consist of electric currents that follow continuous chains of particles lying in the surface of the compound molecules of the magnet. These currents have their origin in an electric polarization of the particles, developed by a direct action of the impulses propagated from the exciting current. In permanent magnets the polarization thus originated becomes permanently established, and a permanent current remains *as a necessary consequence of the play of the molecular forces in the chain.* A magnet therefore derives its power directly from the inexhaustible primary forces of attraction and repulsion, and must retain its virtue unimpaired until the chain of molecules is broken by heat, or the excited molecular conditions upon which the currents depend are removed by some external cause.

5. *Terrestrial magnetism* is due to electric currents in the mass of the earth running in the general direction from east to west, and developed by the collision of the molecules with the æther of space. Both the rotatory and orbital motion of the earth are concerned in producing these currents. A part of the force of such currents must be converted into heat, and the earth derive a portion of its heat from this source. If this be true, the remarkable formal relations that subsist between the magnetism and heat of the earth are probably the result in a great degree of this physical bond by which the two principles are partially united. (See the investigation, by the author, of these relations, in an article on Terrestrial Magnetism, published in vol. iv. second series, of Silliman's Journal.)

This theory of the origin of terrestrial magnetism, as a part of the general theory of molecular physics here presented, was brought by the author to the notice of the Connecticut Academy about two years since. It appears from a pamphlet recently received by the editors of Silliman's Journal, that a theory quite similar to this was propounded several years since by Gustav Hinrichs of Copenhagen. Hinrich's theory, or one having the same essential features, I find is advocated by Prof. Challis in the Number of the Philosophical Magazine published in February 1861.

LIII. *On some curious Effects of the Molecular Forces of Liquids.* *By* G. Vander Mensbrugghe*.

1. *Formation of liquid bubbles in a peculiar condition.*

In meteorology the question as to the state of the vapour in mists is far from being definitively solved; the hypothesis of vesicles, although strongly opposed, is not yet disproved. It is well known that one of the principal arguments against this hypothesis is, that the mode of formation of these vesicles is inconceivable. We are now acquainted, however, with an interesting fact which shows that a liquid film, unclosed and of any curvature, may take the form of a hollow sphere. This experiment has, in fact, been described by M. Félix Plateau†, son of the illustrious physicist. The author operated, however, on soap-water solely; so that it might be urged that his results do not, strictly considered, apply to pure water. Thanks to special circumstances, however, I have been able to prove that pure water comports itself in this respect almost like soap-water. I observed it in the following manner.

Having thrown from a window, 12 metres above the ground, some pure water contained in a cup, I noticed that the sheet of liquid became transformed into hollow spheres whose greatest diameters were about 4 centimetres in length; after a fall of from 8 to 10 metres the bubbles burst, and became scattered into innumerable droplets.

I reproduced the phenomenon a great many times, with distilled water, by taking vessels of different forms and changing the mode of projection; I succeeded almost invariably, though the bubbles varied greatly in number and diameter; the greatest diameter, however, did not exceed 5 or 6 centimetres. The resolution of the liquid sheet into hollow spheres was effected with greater rapidity the narrower the sheet and the greater its curvature. This peculiarity, it may be observed, is easily explained; for the molecular pressure, directed towards the concave part of the sheet, must necessarily increase with the curvature of the liquid surface.

The best results may be obtained thus:—A cup, about 10 centimetres broad and three-quarters full, being taken, the water is projected, with moderate velocity, by moving the hand from left to right (for example), in order that the bubbles which are formed may not mutually prevent themselves from being seen. The operator must place himself at least 6 metres above the

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* From the *Bulletins de l'Acad. Roy. de Belgique*, ser. 2. vol. xviii. No. 8.

† "Sur un mode particulier de production de bulles de savon," *Bull. de l'Acad. de Belgique*, ser. 2. vol. xiii. p. 286. [Phil. Mag. S. 4. vol. xxvi. p. 407.]

ground; for otherwise either the liquid sheet will not have time to resolve itself into hollow spheres, or, should these spheres be produced, it will be found impossible to observe them and to notice their rupture.

This experiment fully confirms the conclusion arrived at by M. Félix Plateau, according to whom the simultaneous agglomeration of liquid molecules into perfectly closed envelopes cannot be regarded as a necessary condition of the formation of vesicles; the admission of the generation of unclosed films of any curvature whatever will suffice—a generation which assuredly involves no impossibility. Without doubt the question of the duration of the bubbles remains to be resolved. The larger ones, it is true, burst in less than a second; but do the smaller spherules likewise do so? I have not been able to establish this point, in consequence of the difficulty of judging whether these small spheres are full or empty—a difficulty which is increased by the circumstance that the envelope is much thicker than that of the bubbles of soap-water.

Apart from the meteorological question, the application of the procedure above described to different kinds of liquid appeared to me to possess some interest. I first tried soap-water, and thus found that although bubbles form themselves very well, their diameters are not great; and moreover they burst as rapidly as when pure water is employed. If these results differ from those described by M. Félix Plateau, it is merely because, instead of turning rapidly round on projecting the liquid, I simply threw it with a relatively moderate velocity, so that the sheet was much less broad, and much thicker. I tried also a very great velocity, so as to render the sheet very thin, and I then obtained a very great number of small spheres accompanied by some very light bubbles more or less large and pretty durable; I even succeeded, by rapidly projecting a concentrated solution of soap (*qui s'était prise en gelée*), in producing three bubbles, the largest of which had a diameter of at least 25 centimetres, whilst that of the two others amounted to about 8 or 10; all three had a duration of about half a minute.

My method succeeds very well with alcohol, though the bubbles burst very quickly. Amongst the volatile oils, I operated with success upon oil of turpentine, and especially so upon petroleum oil. With heavy oils (of which I only tried olive oil) the experiment is a little less successful: the bubbles which are formed are very small, in consequence of the liquid being prevented by its own viscosity from expanding into a broad sheet. Lastly, I obtained good results with several saline solutions.

I have not tried mercury. It appeared to me, in fact, scarcely necessary to do so, since the pretty experiment of M. Melsens*, in which bubbles of mercury are produced by employing, it is true, a very different method, has long been known.

When a large quantity of liquid is operated upon, it may easily be made to take the form of a sheet with determinate curvature. To do so, it is merely necessary to propel it by a force-pump through tubes provided with suitable terminations. To apply this method I had two such terminations constructed; the form of one was a semicylindrical canal, and that of the other a semiconical one. The length of the canal was about 50 millimetres, and the section of efflux was the space enclosed between two concentric semicircles whose radii were 20 and 17 millimetres respectively. I used both well-water and soap-water. The semicylindrical termination gave, with well-water, a multitude of bubbles, from 3 to 4 centimetres in diameter, which burst after a trajectory of some metres; and with soap-water, a great number of hollow bubbles which floated in the air. The semiconical termination produced, with ordinary water, a sheet which gradually became broader and thinner until it resolved itself into a shower of hollow bubbles which burst a few instants after their formation; with soap-water innumerable spherules were thus formed, of which a great number had very thin envelopes.

I also employed terminations narrower than 3 millimetres, but they yielded results much less developed than were the preceding ones.

On the whole, these experiments appeared to me to prove that the majority of liquids, if not all, after being spread out in sheets of proper breadth and thickness, may assume the form of hollow spheres.

2. *Floating globules of mercury.—Attractions and repulsions exhibited by these globules.*

For some time past physicists have been frequently occupied with the examination of the globular form assumed by a liquid, even at ordinary temperatures, at the surface of the same or of a different liquid. The communication of an experiment which I believe to be new, and which, whilst showing in a remarkable manner the effects of the molecular actions of liquids, also furnishes the means of proving capillary attractions and repulsions, will therefore be here not out of place. I operated thus:

A broad capsule being filled with distilled water, a globule of mercury, of about 0·5 of a millimetre in diameter, was taken on the blade of a knife and brought into proximity with the liquid, the blade being inclined as little as possible. The latter was

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* See *L'Institut* for 1845, p. 207.

then turned very gently around its sharp edge so as to put the globule, placed very near this edge, into contact with the water. This contact established, the knife was carefully withdrawn, and the globule of mercury left floating on the surface of a liquid thirteen and a half times less dense than itself.

This phenomenon suggests several remarks. In the first place, why is the globule not made wet by the liquid? The fact is due, I believe, to the layer of air condensed at the surface of the globule: this explanation appears to be the more probable, since I was able to make the globule float immediately after it had remained immersed in the water for more than a quarter of an hour, though not after it had been submerged for an hour or upwards; the layer of air in the latter case had been displaced, or at least partially so. In the second place, it may be asked, will it suffice, in explaining the present phenomenon, to say that the weight of the mercurial globule is equal to that of the displaced water, the depression formed around the mercury being, of course, included? Must we not take into account the fact that, the water being concave immediately beneath the globule, the capillary pressure there must be less than at surrounding points? or does there come into play in this case a special effect of cohesion, for instance a resistance opposed by the surrounding liquid to the deformation of its surface? I have made a great number of experiments and calculations in order to elucidate these questions decisively, but up to the present time I have not been successful.

As already remarked, the experiment above described furnishes a very convenient method of showing, clearly, capillary attractions and repulsions. In fact at the moment of withdrawing the blade of the penknife, the globule of mercury is seen to suffer a quick repulsion. This is evidently a capillary effect due to the elevation of the water along the blade, and to the depression of this liquid around the mercury. The sides of the capsule also exert an energetic repulsion. To prove attraction, I endeavoured to make two globules float in such a manner that, when still, their distance asunder might be about 20 millimetres. In a few moments they moved towards each other with a velocity which, from being very small at first, increased rapidly as the distance between them diminished. Immediately after contact the two globules united and formed one, the layer of air adhering to each of them having been so far displaced by the shock as to allow cohesion to produce its effect. This union of the globules does not readily take place unless the mercury is sufficiently pure, and the surface of the water free from small filaments and corpuscles; for the latter interfere considerably with the capillary actions, and render the distance between the mercurial surfaces in question sufficiently great to prevent the manifestation of molecular attraction.

An interest is given to these experiments by the fact that capillary actions make themselves felt therein at far greater distances (20 millims. to 25 millims.) than with the bodies usually employed in order to exhibit these actions in physical lectures. I may add that, in spite of the smallness of the masses which act upon one another, all these movements may be followed with the greatest facility, in consequence of the large quantity of light reflected by the non-submerged zones.

The effect of the cohesion between two spherules in juxtaposition enabled me to increase gradually the volume of the initial globule. For this purpose, it was merely necessary to float successively several very small spherules, which ultimately all united themselves to the first globule. I was thus enabled to seek experimentally the maximum diameter of a sphere able to maintain itself at the surface of distilled water. I found it to be very nearly 0·87 of a millimetre. With well-water, I found the maximum diameter to be about 1 millimetre.

I also tried to make droplets of mercury float on olive oil, and succeeded perfectly; the globules, however, had at most a diameter of a third of a millimetre.

These experiments suggested the idea of floating small solid spheres of great density. To cite one case only, a spherule of platinum 0·3 to 0·4 of a millimetre in diameter, was easily made to float on the surface of water.

In the last place I submitted several saline solutions to experiment; amongst others, solutions of chloride of sodium, nitrate of barytes, and carbonate of soda. It appeared to me that the maximum diameter increased at first with the degree of concentration, but that this augmentation had a limit, beyond which the maximum diameter diminished. I propose, however, to examine this point more closely on some future occasion.

LIV. *The Bakerian Lecture.—Contributions to Molecular Physics. Being the Fifth Memoir of Researches on Radiant Heat.* By John Tyndall, *F.R.S., &c.*[*]

§ I. *Preliminary considerations.—Description of apparatus.*

THE natural philosophy of the future must, I imagine, mainly consist in the investigation of the relations which subsist between the ordinary matter of the universe and

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[*] From the Philosophical Transactions, Part II. 1864, having been read at the Royal Society March 17, 1864.

the æther in which this matter is immersed. Regarding the motions of the æther itself, as illustrated by the phenomena of reflexion, refraction, interference, and diffraction, the optical investigations of the last half century have left nothing to be desired; but as regards the atoms and molecules, whence issue the undulations of light and heat, and their relations to the medium which they move, and by which they are set in motion, these investigations teach us nothing. To come closer to the origin of the æthereal waves—to get, if possible, some experimental hold of the oscillating atoms themselves—has been the main object of the researches in which I have been engaged for the last five years. In these researches radiant heat has been used as an instrument for exploring molecular condition, and this is the object which I have kept constantly in view throughout the investigation which I have now the honour to submit to the Royal Society.

The first part of these researches is devoted to the more complete examination of a subject which was briefly touched upon at the conclusion of my fourth memoir—namely, the action of liquids, as compared with that of their vapours, upon radiant heat. The differences which exist between different gaseous molecules, as regards their power of emitting and absorbing radiant heat, have been already amply illustrated. When a gas is condensed to a liquid, the molecules approach and grapple with each other by forces which are insensible as long as the gaseous state is maintained. But though thus condensed and enthralled, the æther still surrounds the molecules. If, then, the powers of radiation and absorption depend upon them individually, we may expect that the deportment towards radiant heat which experiment establishes in the case of the free molecule, will maintain itself after the molecule has relinquished its freedom and formed part of a liquid. If, on the other hand, the state of aggregation be of paramount importance, we may expect to find on the part of liquids a deportment altogether different from that of their vapours.

Melloni, it is well known, examined the diathermancy of various liquids, but he employed for this purpose the flame of an oil-lamp, covered by a glass chimney. His liquids, moreover, were contained in glass cells; hence the radiation from the source was profoundly modified before it entered the liquid at all, for the glass was impervious to a considerable part of the radiation. It was my wish to interfere as little as possible with the primitive emission, and also to compare the action of liquids with that of their vapours, when examined in a tube stopped with plates of rock-salt. I therefore devised an apparatus in which a layer of liquid of any thickness could be enclosed between two polished plates of rock-salt. It was skilfully constructed for me by Mr. Becker, and the same two plates have already done service in more than six hundred experiments.

The apparatus consists of the following parts:—A B C (fig. 1) is a plate of brass, 3·4 inches long, 2·1 inches wide, and 0·3 of

Fig. 2.

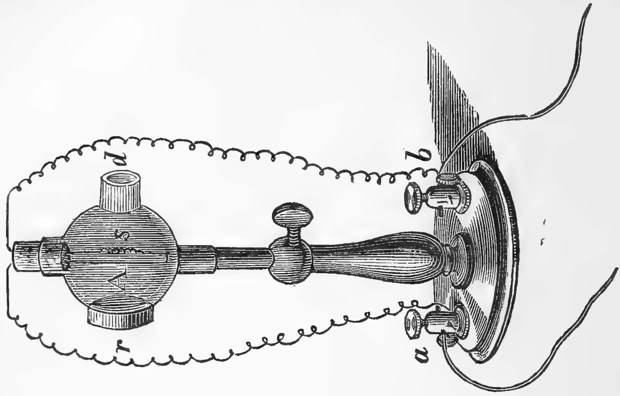
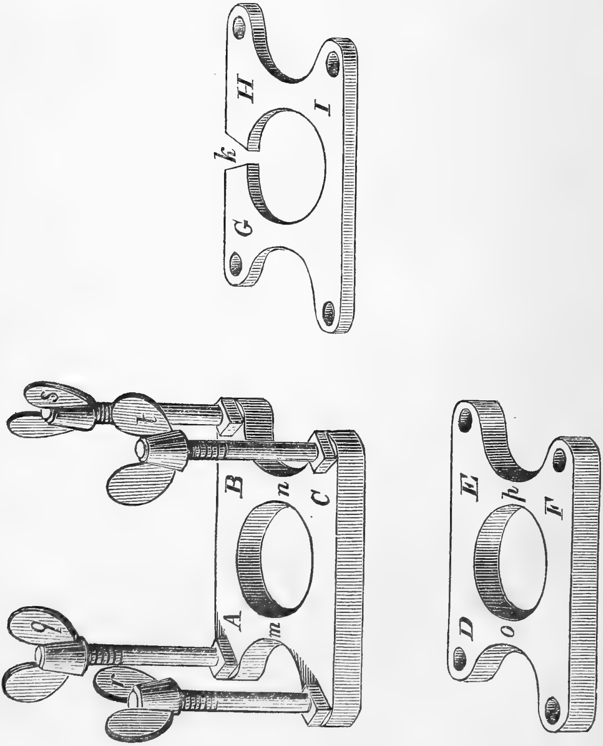


Fig. 1.



an inch thick. Into it, at its corners, are rigidly fixed four upright pillars, furnished at the top with screws, for the reception of the nuts *q r s t*. D E F is a second plate of brass of the same size as the former, and pierced with holes at its four corners, so as to enable it to slip over the four columns of the plate A B C. Both these plates are perforated by circular apertures, *m n* and *o p*, 1·35 inch in diameter. G H I is a third plate of brass of the same area as D E F, and, like it, having its centre and its corners perforated. The plate G H I is intended to separate the two plates of rock-salt, which are to form the walls of the cell, and its thickness determines that of the liquid layer. Thus when the plates A B C and D E F are in position, a space of the form of a shallow cylinder is enclosed between them, and this space can be filled with any liquid through the orifice *k*. The separating plate G H I was ground with the utmost accuracy, and the surfaces of the plates of salt were polished with extreme care, with a view to rendering the contact between the salt and the brass water-tight. In practice, however, it was found necessary to introduce washers of thin letter-paper between the plates of salt and the separating plate.

In arranging the cell for experiment, the nuts *q r s t* are unscrewed, and a washer of india-rubber is first placed on A B C. On this washer is placed one of the plates of rock-salt. On the plate of rock-salt is placed the washer of letter-paper, and on this again the separating plate G H I. A second washer of paper is placed on this plate; then comes the second plate of salt, on which another india-rubber washer is laid. The plate D E F is finally slipped over the columns, and the whole arrangement is tightly screwed together by the nuts *q r s t*. The use of the india-rubber washers is to relieve the crushing pressure which would be applied to the plates of salt if they were in actual contact with the brass plates; and the use of the paper washers is, as already explained, to render the cell liquid-tight. After each experiment, the apparatus is unscrewed, the plates of salt are removed and thoroughly cleansed; the cell is then remounted, and in two or three minutes all is ready for a new experiment.

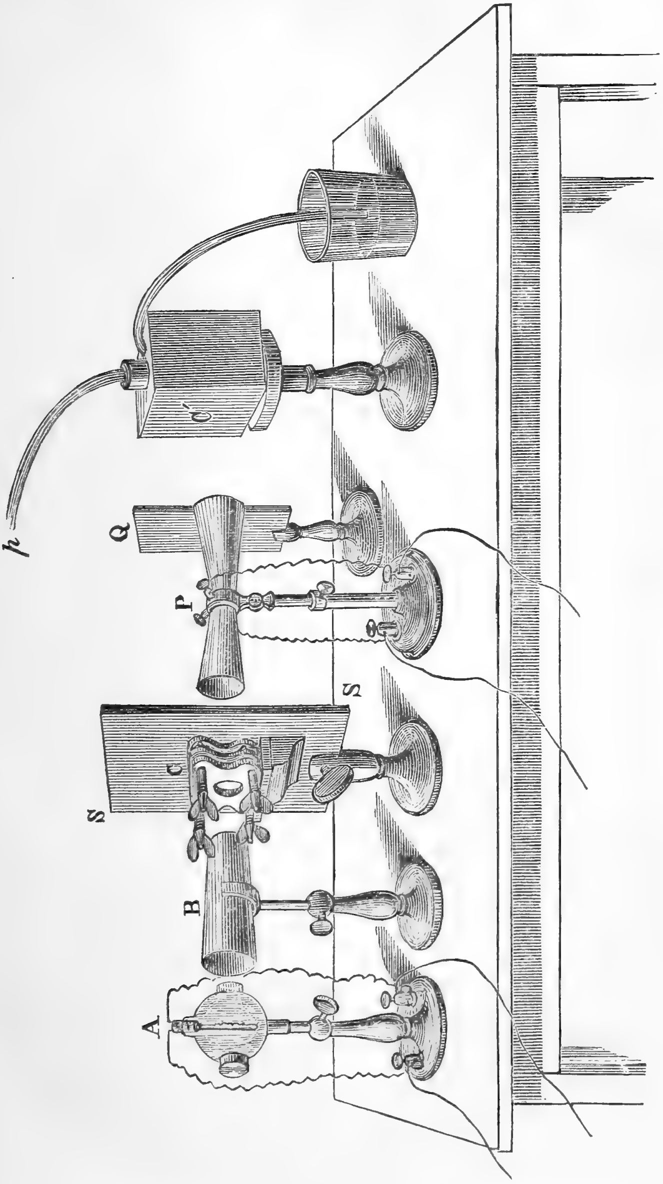
My next necessity was a perfectly steady source of heat, of sufficient intensity to penetrate the most absorbent of the liquids to be subjected to examination. This was found in a spiral of platinum wire, rendered incandescent by an electric current. The frequent use of this source of heat led me to construct the lamp shown in fig. 2. A is a globe of glass 3 inches in diameter, fixed upon a stand, which can be raised and lowered. At the top of the globe is a tubulure, into which a cork is fitted, and through the cork pass two wires whose ends are united by the platinum spiral *s*. The wires are carried down to the bindingscrews *a b*, which are fixed in the foot of the stand, so that when the instrument is attached to the battery no strain is ever exerted on the wires which carry the spiral. The ends of the thick wire to which the spiral is attached are also of stout platinum; for when it was attached to copper wires, unsteadiness was introduced through oxidation. The heat from the incandescent spiral issues by the opening *d*, which is an inch and a half in diameter. Behind the spiral, finally, is a metallic reflector, *r*, which augments the flux of heat without sensibly changing its quality. In the open air the red-hot spiral is a capricious source of heat; but surrounded by its glass globe its steadiness is admirable.

The whole experimental arrangement will be immediately understood from the sketch given in fig. 3. A is the platinum lamp just described, heated by a current from a Grove's battery of five cells. It is necessary that this lamp should remain perfectly constant throughout the day; and to keep it so, a tangent galvanometer and a rheocord are introduced into the circuit.

In front of the spiral, and surrounding the tubulure of its globe, is the tube B with an interior reflecting surface, through which the heat passes to the rock-salt cell C. This cell is placed on a little stage soldered to the back of the perforated screen S S, so that the heat, after having crossed the cell, passes through the hole in the screen, and afterwards impinges on the thermo-electric pile P. The pile is placed at some distance from the screen S S, so as to render the temperature of the cell C itself of no account. C′ is the compensating cube, containing water kept boiling by steam from the pipe *p*. Between the cube C′ and the pile P is the screen Q, which regulates the amount of heat falling on the posterior face of the pile. The whole arrangement is here exposed, but in practice the pile P and the cube C′ are carefully protected from the capricious action of the surrounding air.

The experiments are thus performed. The empty rock-salt cell C being placed on its stage, a double silvered screen (not shown in the figure) is first introduced between the end of the tube B and the cell C—the radiation from the spiral being thus totally cut off, and the pile subjected to the action of the cube C′ alone. By means of the screen Q, the total heat to be adopted throughout the series of experiments is obtained: say that it is sufficient to produce a galvanometric deflection of 50 degrees. The double screen used to intercept the radiation from the spiral is then gradually withdrawn until this radiation completely neutralizes that from the cube C′, and the needle of the galvanometer points steadily to zero. The position of the double screen, once fixed, remains subsequently unchanged, the slight and slow alteration of the source being neutralized by the rheocord. Thus the rays in the first instance pass from the spiral through the empty rock-salt cell. A small funnel, supported by a suitable stand, dips into the aperture which leads into the cell, and through this the liquid is poured. The introduction of the

Fig. 3.



liquid destroys the previous equilibrium, the galvanometer needle moves, and finally assumes a steady deflection; and from this deflection we can immediately calculate the quantity of heat absorbed by the liquid, and express it in hundredths of the entire radiation.

For example, the empty cell being placed upon its stand, and the needle being at 0°, the introduction of iodide of methyle into the cell produced a deflection of 30°·8. The total radiation on this occasion was 44°·2. Taking the force necessary to move the needle from 0° to 1° as our unit, the deflection 30°·8 corresponds to 32 such units, while the deflection 44°·2 corresponds to 58·3 such units. Hence the statement

$$58{\cdot}3 : 100 = 32 : 54{\cdot}9,$$

which gives an absorption of 54·9 per cent. for a layer of liquid iodide of methyle 0·07 of an inch in thickness.

§ II. *Absorption of radiant heat of a certain quality by eleven different liquids at five different thicknesses.*

The following Table contains the results obtained in this manner with the respective liquids there mentioned. It embraces both the deflection produced by the introduction of the liquid, and the quantity per cent. intercepted of the entire radiation. It has been intimated to me by some of my continental friends that the publication of such details as would enable a reader to judge of the precision attainable by my apparatus would be desirable. In this paper I shall, to some extent, endeavour to satisfy this desire, making use, however, of my ordinary experiments.

TABLE I.—Radiation of heat through Liquids. Source of heat, red-hot platinum spiral. Thickness of liquid layer 0·07 of an inch.

Name of liquid.	Deflection.	Absorption per 100.
Iodide of methyle . .	30·8°	54·9
Iodide of ethyle . . .	33·0	60·4
Benzole	35·3	67·0
Amylene	37·7	74·8
Sulphuric ether . . .	39·0	79·4
Acetic ether	39·6	81·6
Alcohol	41·0	86·6
Water*	43·3	91·4
Total heat	44·2	100

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* To prevent the water from attacking the cell, it was always first saturated with the substance of the cell itself, namely, transparent rock-salt.

In these experiments I employed a less delicate galvanometer than that used in my former researches. The experiments were made on the 29th of September, and on the following day I repeated them with the following results:—

Table II.—Radiation of heat through Liquids. Source of heat, red-hot platinum spiral. Thickness of liquid layer 0·07 of an inch.

Name of liquid.	Deflection.	Absorption per 100.
Iodide of methyle . .	33°·5	53·7
Iodide of ethyle . . .	35·5	58·7
Benzole	37·5	64·4
Amylene	39·5	70·7
Sulphuric ether	41·0	75·4
Acetic ether	41·5	76·9
Formic ether	42·4	80·0
Alcohol	43·5	84·2
Water	44·7	90·5
Total heat	46·7	100·0

On the 28th of October my most delicate galvanometer was at liberty, and with it I executed the experiments performed with the coarser one. The following are the results:—

Table III.—Radiation of heat through Liquids. Source, red-hot platinum spiral. Thickness of liquid layer 0·07 of an inch.

Name of liquid.	Deflection.	Absorption per 100.	
Bisulphide of carbon	9°·0	12·5	
Chloroform	25·2	35·0	
Iodide of methyle	36·0	53·2	54·3
Ditto, strongly coloured with iodine .	36·0	53·2	
Iodide of ethyle	38·2	59·0	59·6
Benzole	39·2	62·5	65·7
Amylene	42·0	73·6	72·3
Sulphuric ether	42·6	76·1	77·4
Acetic ether	43·4	78·0	79·3
Formic ether	43·3	79·0	80·0
Alcohol	44·4	83·6	85·4
Water	45·6	88·8	90·9
Total heat	48·0	100·0	

I have here placed beside the results obtained with the delicate galvanometer, the means of those obtained with the coarser one. It is not my object to push these measurements to the last degree of nicety; otherwise the satisfactory agreement here exhibited might be made still better.

To render the experiments on liquid transmission more complete, I operated with layers of various thicknesses, employing throughout my most delicate galvanometer. The results of these measurements are recorded in the following series of Tables:—

Table IV.—Radiation of heat through Liquids. Source, red-hot platinum spiral. Thickness of liquid layer 0·02 of an inch.

Name of liquid.	Deflection.	Absorption per 100.
Bisulphide of carbon	4°·0	5·5
Chloroform	12·0	16·6
Iodide of methyle	26·0	36·1
Iodide of ethyle	27·5	38·2
Benzole	31·3	43·4
Amylene	38·0	58·3
Boracic ether	39·0	61·8
Sulphuric ether	39·5	63·3
Formic ether	40·0	65·2
Alcohol	40·5	67·3
Water	43·7	80·7
Total heat	48·0	100·0

Table V.—Thickness of liquid layer 0·04 of an inch.

Name of liquid.	Deflection.	Absorption per 100.
Bisulphide of carbon	6°·1	8·4
Chloroform	18·0	25·0
Iodide of methyle	33·0	46·5
Iodide of ethyle	35·0	50·7
Benzole	37·0	55·7
Amylene	40·0	65·2
Boracic ether	41·0	69·4
Sulphuric ether	42·0	73·5
Acetic ether	42·1	74·0
Formic ether	42·5	76·3
Alcohol	43·2	78·6
Water	45·0	86·1
Total heat	48·0	100·0

Table VI.—Thickness of liquid layer 0·14 of an inch.

Name of liquid.	Deflection.	Absorption per 100.
Bisulphide of carbon	11°·0	15·2
Chloroform	28·6	40·0
Iodide of methyle	40·0	65·2
Iodide of ethyle	40·9	69·0
Benzole	41·5	71·5
Amylene	43·0	77·7
Sulphuric ether	43·2	78·6
Acetic ether	44·0	82·0
Formic ether	44·5	84·0
Alcohol	44·8	85·3
Water	46·3	91·8
Total heat	48·0	100·0

Table VII.—Thickness of liquid layer 0·27 of an inch.

Bisulphide of carbon	12°·5	17·3
Chloroform	32·3	44·8
Iodide of methyle	40·8	68·6
Iodide of ethyle	41·5	71·5
Benzole	42·0	73·6
Amylene	44·1	82·3
Sulphuric ether	44·8	85·2
Acetic ether	45·0	86·1
Formic ether	45·2	87·0
Alcohol	45·7	89·1
Water	46·1	91·0
Total heat	48·0	100·0

The foregoing results are collected together in the following Table:—

Table VIII. Absorption of heat by Liquids. Source, platinum spiral heated to a bright redness by a voltaic current.

Liquid.	Thickness of liquid in parts of an inch.				
	0·02.	0·04.	0·07.	0·14.	0·27.
Bisulphide of carbon	5·5	8·4	12·5	15·2	17·3
Chloroform	16·6	25·0	35·0	40·0	44·8
Iodide of methyle	36·1	46·5	53·2	65·2	68·6
Iodide of ethyle	38·2	50·7	59·0	69·0	71·5
Benzole	43·4	55·7	62·5	71·5	73·6
Amylene	58·3	65·2	73·6	77·7	82·3
Sulphuric ether	63·3	73·5	76·1	78·6	85·2
Acetic ether	......	74·0	78·0	82·0	86·1
Formic ether	65·2	76·3	79·0	84·0	87·0
Alcohol	67·3	78·6	83·6	85·3	89·1
Water	80·7	86·1	88·8	91·8	91·0

Had it been desirable to push these measurements to the utmost limit of accuracy, I should have repeated each experiment, and taken the mean of the determinations. But considering the way in which the different thicknesses check each other, an inspection of the Table must produce the conviction that the results express, within small limits of error, the action of the bodies mentioned.

§ III. *Absorption of radiant heat of the same quality by the vapours of those liquids at a common tension.*

As liquids, then, those bodies are shown to possess very different capacities of intercepting the heat emitted by our radiating source; and we have next to inquire whether these differences continue after the molecules have been released from the bond of cohesion. We must, of course, test the vapours by waves of the same period as those applied to the liquids; and this our mode of experiment renders easy of accomplishment. The heat generated in a wire by a current of a given strength being invariable, it was only necessary, by means of the tangent compass and rheocord, to keep the current constant from day to day in order to obtain, both as regards quantity and quality, an invariable source of heat.

The liquids from which the vapours were derived were placed in a small long flask, a separate flask being devoted to each. The air above the liquid and within it being first carefully removed by an air-pump, the flask was attached to the experimental tube in which the vapours were to be examined. This tube was of brass, 49·6 inches long, and 2·4 inches in diameter, its two ends being stopped by plates of rock-salt. Its interior surface was polished. At the commencement of each experiment, the tube having been thoroughly cleansed and exhausted, the needle stood at zero*. The cock of the flask containing the volatile liquid was then carefully turned on, and the vapour allowed slowly to enter the experimental tube. The barometer attached to the tube was finely graduated, and the descent of the mercurial column was observed through a magnifying lens. When a pressure of 0·5 of an inch was obtained, the vapour was cut off and the permanent deflection of the needle noted. Knowing the total heat, the absorption in 100ths of the entire radiation could be at once deduced from the deflection. The following Table contains the results of the first series of experiments made with the platinum spiral as source.

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* It is hardly necessary to remark that the principle of compensation described in my former memoirs was employed here also.

Table IX.—Radiation of heat through Vapours. Source, red-hot platinum spiral. Tension, 0·5 of an inch.

Name of vapour.	Deflection.	Absorption per 100.	Mean.
Bisulphide of carbon . .	17°·0	4·7	4·7
Bisulphide of carbon . .	16·8	4·6	
Chloroform	22·5	6·2	6·3
Chloroform	22·8	6·3	
Iodide of methyle . . .	34·0	9·7	9·7
Iodide of methyle . . .	34·0	9·7	
Iodide of ethyle	46·0	18·0	17·8
Iodide of ethyle	45·5	17·6	
Benzole	48·5	20·4	20·4
Benzole	48·5	20·4	
Amylene	56·3	27·3	27·3
Amylene	56·2	27·2	
Total heat	78·3	100·0	

The absence of all caprice or uncertainty in the measurements is, I think, demonstrated by the foregoing Table, which is simply an average sample of the degree of coincidence obtained in separate measurements. Two determinations were made in each case; and it will be seen that while, in some instances, the second experiment yielded the same result as the first, in one instance only does the difference amount to half a degree of the galvanometer.

The foregoing measurements were executed on the 5th of October. On the 7th they were in part repeated, with the following results:—

Table X.

Name of vapour.	Deflection.	Absorption.
Bisulphide of carbon . . .	16°·5	4·7
Chloroform	22·8	6·5
Iode of methyle	33·0	9·6
Iodide of ethyle	45·0	17·7
Benzole	48·0	20·6
Amylene	55·3	27·5
Alcohol	55·7	28·1
Formic ether	58·2	31·4
Sulphuric ether	58·5	31·9
Acetic ether	59·9	34·6
Total heat	78·0	100·0

Placing these results beside those recorded in Table IX., the manner in which they check each other will appear.

Comparison of Tables IX. and X.—Absorption.

	Table IX.	Table X.
Bisulphide of carbon . .	4·7	4·7
Chloroform	6·3	6·5
Iodide of methyle . . .	9·7	9·6
Iodide of ethyle	17·8	17·7
Benzole	20·4	20·6
Amylene	27·3	27·5

The agreement, it will be seen, is as perfect as could be desired.

Augmenting the opening through which the heat passed from the source into the experimental tube, the total heat was increased, and the experiments with a few of the vapours were repeated. The total heat in the last case produced a deflection of 78°, which is equal to 350 units; the total heat now employed produced a deflection of 83°, which is equal to 605 units. It is easy to see that the experiments now to be recorded furnish a direct check on the calibration of the galvanometer. As long as the quality of the heat remains unchanged, the absorption ought to be the same with a high total heat as with a low one. But if experiment show that this is the case, it proves also that the calibration on which the calculation of the absorption depends, cannot be in error. The following results were obtained on the 8th of October:—

TABLE X *a*.

	Deflection.	Absorption.
Amylene	66°·0	28·7
Alcohol	66·4	29·2
Formic ether	68·5	32·5
Formic ether	68·5	32·5
Sulphuric ether	69·2	33·6
Sulphuric ether	69·1	33·4
Acetic ether	69·7	34·5
Acetic ether	69·7	34·5
Total heat	83·0	100·0

Placing the results beside those recorded in Table X., we have the following comparison:—

Comparison of Tables X. and X *a*.

Amylene	27·5	28·7
Alcohol	28·1	29·2
Formic ether	31·4	32·5
Sulphuric ether	31·9	33·5
Acetic ether	34·6	34·5

The differences here are inconsiderable, and lean to neither side; within these limits, therefore, the calibration must be correct; it shall be tested more severely in another part of this paper.

§ IV. *Absorption of the same heat by the same vapours when the quantities of vapour are proportional to the quantities of liquid.—Comparative view of the action of liquids and their vapours on radiant heat.*

We are now in a condition to compare the action of a series of volatile liquids with that of the vapours of those liquids upon radiant heat.

Commencing with the substance of the lowest absorptive energy, and proceeding to the highest, we have the following order of absorption:—

Liquids.	Vapours.
Bisulphide of carbon.	Bisulphide of carbon.
Chloroform.	Chloroform.
Iodide of methyle.	Iodide of methyle.
Iodide of ethyle.	Iodide of ethyle.
Benzole.	Benzole.
Amylene.	Amylene.
Sulphuric ether.	Alcohol.
Acetic ether.	Formic ether.
Formic ether.	Sulphuric ether.
Alcohol.	Acetic ether.
Water.	

Here, as far as amylene, the order of absorption is the same for both liquids and vapours. But from amylene downwards, though strong liquid absorption is in a general way paralleled by strong vapour absorption, the order of both is not the same. There is not the slightest doubt that next to water alcohol is the most powerful absorber in the list of liquids; but there is just as little doubt that the position which it occupies in the list of vapours is the correct one. This has been established by reiterated experiments. Acetic ether, on the other hand, though certainly the most energetic absorber in the state of vapour, falls behind both formic ether and alcohol in the liquid state. Still, on the whole, I think it is impossible to contemplate these results without arriving at the general conclusion that the act of absorption is in the main molecular, and that the molecule maintains its power as an absorber and radiator when it changes its state of aggregation. Should, however, any doubt linger as to the correctness of this conclusion, it will speedily disappear.

A moment's reflection will show that the comparison here instituted is not a strict one. We have taken the liquids at a common thickness, and the vapours at a common volume and pressure. But if the layers of liquid employed were turned bodily into vapour, the volumes obtained would *not* be the same. Hence the quantities of matter traversed by the radiant heat are neither equal nor proportional to each other in the two cases; and to render the comparison strict they ought to be proportional. It is easy, of course, to make them so; for the liquids being examined at a constant volume, their specific gravities give us the relative quantities of matter traversed by the radiant heat, and from these and the vapour-densities we can immediately deduce the corresponding volumes of the vapour. Calling the quantity of matter q, the vapour-density d, and the volume V, we have

$$Vd=q,$$

or

$$V=\frac{q}{d}.$$

Dividing, therefore, the specific gravities of our liquids by the densities of their vapours, we obtain a series of volumes proportional to the masses of the liquids employed. The densities of both liquids and vapours are given in the following Table:—

Table of Densities.

	Vapour.	Liquid.
Bisulphide of carbon	2·63	1·27
Chloroform	4·13	1·48
Iodide of methyle	4·90	2·24
Iodide of ethyle	5·39	1·95
Benzole	2·69	0·85
Amylene	2·42	0·64
Alcohol	1·59	0·79
Sulphuric ether	2·56	0·71
Formic ether	2·56	0·91
Acetic ether	3·04	0·89
Water	0·63	1·00

Substituting for q the numbers of the second column, and for d those of the first, we obtain the following series of vapour volumes, whose weights are proportional to the masses of liquid employed.

Table of Proportional Volumes.

Bisulphide of carbon . .	0·48
Chloroform	0·36
Iodide of methyle . . .	0·46
Iodide of ethyle	0·36
Benzole	0·32
Amylene	0.26
Alcohol	0·50
Sulphuric ether	0·28
Formic ether	0·36
Acetic ether	0·29
Water	1·60

Employing the vapours in the volumes here indicated, the following results were obtained:—

Table XI.—Radiation of heat through Vapours. Mass of vapour proportional to mass of liquid.

Name of vapour.	Tension in parts of an inch.	Deflection. °	Absorption per 100.
Bisulphide of carbon .	0·48	8·4 8·5	4·3
Chloroform	0·36	13·0 13·0	6·6
Iodide of methyle . .	0·46	20·0 20·4	10·2
Iodide of ethyle . .	0·36	30·6 30·6	15·4
Benzole	0·32	33·4 33·1	16·8
Amylene	0·26	37·7	19·0
Sulphuric ether . . .	0·28	42·5 42·6	21·5
Acetic ether	0·29	44·0 44·0	22·2
Formic ether . . .	0·36	44·5 44·7	22·5
Alcohol	0·50	45·0 44·9	22·7

Here the discrepancies revealed by our former series of experiments entirely disappear, and it is proved that for heat of the same quality the order of absorption for liquids and their vapours is the same. We may therefore safely infer that the position of a vapour as an absorber or radiator is determined by that of the liquid from which it is derived. Granting the validity of this inference, the position of *water* fixes that of *aqueous vapour*. From the first seven Tables of this memoir, or from the *résumé* of results in Table VIII., it will be seen that for all thicknesses water exceeds the other liquids in the energy of its absorption. Hence, if no single experiment on the vapour of water existed, we should be compelled to conclude, from the deportment of its liquid, that, weight for weight, aqueous vapour transcends all others in absorptive power. Add to this the direct and multiplied experiments by which the action of this substance on radiant heat has been established, and we have before us a body of evidence sufficient, I trust, to set this question for ever at rest, and to induce the meteorologist to apply without misgiving the radiant and absorbent property of aqueous vapour to the phenomena of his science.

§ V. *Remarks on the chemical constitution of bodies with reference to their powers of absorption.*

The order and relative powers of absorption of our vapours, when equal volumes are compared, are given in Table X.: the chemical formulæ of the substances, and the number of atoms which their molecules embrace, are as follows:—

	Formula.	Number of atoms in molecules.
Bisulphide of carbon .	$C S^2$	3
Chloroform	$C H Cl^3$	5
Iodide of methyle . .	$C H^3 I$	5
Iodide of ethyle . . .	$C^2 H^5 I$	8
Benzole	$C^6 H^6$	12
Amylene	$C^5 H^{10}$	15
Alcohol	$C^2 H^6 O$	9
Formic ether	$C^3 H^6 O^2$	11
Sulphuric ether . . .	$C^4 H^{10} O$	15
Acetic ether	$C^4 H^8 O^2$	14
Boracic ether	$B C^6 H^{15} O^3$	25

Here, for the first six vapours, the radiant and absorbent powers augment with the number of atoms contained in the molecules. Alcohol and amylene vapours, however, are nearly alike in absorptive power, the molecule of amylene containing 15 atoms, while that of alcohol embraces only 9. But in alcohol we have a third element introduced, which is absent in the amylene; the oxygen of the alcohol gives its molecule such a character as enables it to transcend that of the amylene, though the latter contains the greater number of atoms. Here the idea of *quality* superadds itself to that of number. Acetic ether also has a less number of atoms in its molecule than sulphuric ether;

but whereas the latter has but one atom of oxygen, the former has two. Formic ether and sulphuric ether are almost identical in their absorptive powers for the heat here employed; still formic ether has but 11 atoms in its molecule, while sulphuric has 15. But formic ether possesses two atoms of oxygen, while sulphuric possesses only one. Two things here suggest themselves as influential on the absorbent and radiant power, which may be expressed by the terms *multitude* and *complexity*. As a molecule of multitude, amylene, for example, exceeds alcohol; as a molecule of complexity, alcohol exceeds amylene; and in this case, as regards radiant and absorbent power, the complexity is more than a match for the multitude. The same remarks may be made with reference to sulphuric and formic ether: the former excels in multitude, the latter in complexity, the excess in the one case almost exactly balancing that in the other. Adding two atoms of hydrogen and one of carbon to the formic ether, we obtain acetic ether, and by this addition the balance is turned; for though acetic ether falls short of sulphuric ether in multitude, it transcends it in absorbent and radiant power. Outstanding from all others, when equal volumes are compared, and signalizing itself by the enormous magnitude of its absorption, we have boracic ether, each molecule of which embraces no less than 25 atoms. The time now at my disposal enables me to do little more than glance at these singular facts; but I must direct the attention of chemists to the water molecule: its power as a radiant and an absorbent is perfectly unprecedented and anomalous, if the usually recognized formula be correct.

§ VI. *Transmission of radiant heat through bodies opake to light.—Remarks on the physical cause of transparency and opacity.*

In Table III. a fact is revealed which is worth a little further attention. The measurements there recorded show that the absorption of a layer of iodide of methyle, strongly coloured with iodine (which, doubtless, had been liberated by the action of light), was precisely the same as that of a perfectly transparent layer of the liquid. The iodine, which produced so marked an effect on light, did not sensibly affect the radiant heat emitted by the platinum spiral. Here are the numbers:—

	Absorption.
Iodide of methyle (transparent)	53·2
Iodide of methyle (strongly coloured with iodine) .	53·2

In this case, the incandescent spiral, or a flame, was visible when looked at through the liquid; I therefore intentionally deepened the colour (a rich brown), by adding iodine, until the layer was of sufficient opacity to cut off wholly the light of a brilliant jet of gas. The transparency of the liquid to the radiant heat was not sensibly affected by the addition of the iodine. The luminous heat was of course cut off; but this, as compared with the whole radiation, was so small as to be insensible in the experiments.

It is known that iodine dissolves freely in the bisulphide of carbon, the colour of the solution in thin layers being a splendid purple; but in layers of moderate thickness it may be rendered perfectly opake to light. I dissolved in the liquid a quantity of the iodine sufficient, when introduced into a cell 0·07 of an inch wide, to cut off wholly the light of the most brilliant gas-flame. Comparing the opake solution with the transparent bisulphide, the following results were obtained:—

	Deflection.	Absorption.
Bisulphide of carbon (opake) . . .	9·0	12·5
Bisulphide of carbon (transparent) .	9·0	12·5

Here the presence of a quantity of iodine, perfectly opake to a brilliant light, was without measurable effect upon the heat emanating from our platinum spiral. The liquid was sensibly thickened by the quantity of iodine dissolved in it.

The same liquid was placed in a cell 0·27 of an inch in width; that is to say, a solution which was perfectly opake to light, at a thickness of 0·07 was employed in a layer of nearly four times this thickness. Here are the results:—

	Deflection.	Absorption.
Bisulphide of carbon (transparent) .	13·6	18·8
Bisulphide of carbon (opake) . . .	13·7	19·0

The difference between the two measurements lies within the limits of possible error.

Bisulphide of carbon is commonly used to fill hollow prisms, when considerable dispersion is sought for in the decomposition of white light. Such prisms, filled with the opake solution, intercept entirely the luminous part of the spectrum, but allow the extra-red rays free passage. A heat-spectrum of the sun, or of the electric light, may be thus obtained entirely separated from the luminous one. By means of a prism of the transparent bisulphide, I determined the position of the spectrum of the electric light upon a screen, and behind the screen placed a thermo-electric pile so that when the screen was removed the extra-red rays fell upon the pile. I then substituted an opake prism for the transparent one: there was no visible spectrum on the screen; but the removal of the latter at once demonstrated the existence of an invisible spectrum by the thermo-electric current which it generated, and which was powerful enough to dash violently aside the needles of a large lecture-room galvanometer.

To what, then, are we to ascribe the deportment of iodine towards luminous and obscure heat? The difference between both qualities of heat is simply one of period: in the one case the waves which convey the energy are short and of rapid recurrence; in the other case they are long and of slow recurrence. The former are intercepted by the iodine, and the latter are allowed to pass. Why? There can, I think, be only one answer to this question—that the intercepted waves are those whose periods coincide with the periods of oscillation possible to the atoms of the dissolved iodine. Supposing waves of any period to impinge upon an assemblage of molecules of any other period, it is, I think, physically certain that a tremor of greater or less intensity will be set up among the molecules; but for the motion to *accumulate* so as to produce sensible absorption, coincidence of period is necessary. Briefly defined, therefore, transparency is synonymous with *discord*, while opacity is synonymous with *accord* between the periods of the waves of ether and those of the molecules of the body on which they impinge. The opacity, then, of our solution of iodine to light shows that its atoms are competent to vibrate in all periods which lie within the limits of the visible spectrum; while its transparency to the extra-red undulations demonstrates the incompetency of its atoms to vibrate in unison with the longer waves.

This simple conception will, I think, be found sufficient to conduct us with intellectual clearness through a multitude of otherwise perplexing phenomena. It may of course be applied immediately to that numerous class of bodies which are transparent to light, but opake in a greater or less degree to radiant heat. Water, for example, is an eminent example of this class of bodies: while it allows the luminous rays to pass with freedom, it is highly opake to all radiations emanating from obscure sources. A layer of this substance one-twentieth of an inch thick is competent, as Melloni has shown, to intercept all rays issuing from bodies heated under incandescence. Hence we may infer that, throughout the range of the visible spectrum, the periods of the water-molecules are in discord with those of the æthereal waves, while beyond the red we have coincidence between both.

What is true of water is, of course, true in a less degree of glass, alum, calcareous spar, and of all the substances named in the first section of this paper. They are all in discord with the visible spectrum; they are all more or less in accord with the extra-red undulations of the spectrum.

Thus also as regards lampblack: the blackness of the substance is due to the accord which reigns between the oscillating periods of its atoms and those of the waves embraced within the limits of the visible spectrum. The substance which is thus impervious to the luminous rays is moreover the very one from which the whitest light of our lamps is derived. It can absorb all the rays of the visible spectrum, it can also emit them. But though in a far less degree than iodine, lampblack is also to some extent transparent to the longer undulations. Melloni was the first to prove this; and in an experiment described in a former memoir, I myself found that 30 per cent. of the radiation from an obscure source found its way through a layer of lampblack which cut off totally the light of the most brilliant jet of gas. I shall have occasion to show that, for certain sources of heat of long period, between 40 and 50 per cent. of the entire radiation is transmitted by a layer of lampblack which is perfectly opake to our most brilliant artificial lights. Hence, in the case of lampblack, while accord exists between the periods of its atoms and those of the light-exciting waves, discord, to a considerable extent, exists between the periods of the same atoms and those of the extra-red undulations.

[To be continued.]

LV. *Note on certain statements in Elementary Works concerning the Specific Heat of Gases. By* Professor Donkin, *F.R.S.*[*]

A YOUNG student of natural science showed me a few days ago the following statement in Galloway's 'Second Step in Chemistry' (London, 1864). It had naturally surprised him, and he asked for an explanation, which I was quite unable to give.

"From the calculations of Laplace and Poisson, and the experiments of Clement and Desormes, of De la Roche and Berard, and of Gay-Lussac and Dulong, it has hitherto been assumed that the specific heat of a gas under a constant pressure is always greater than the specific heat under a constant volume; but M. Regnault has lately found, by an entirely new method, that the difference between the two kinds of specific heat is either null or extremely small." (P. 585, paragraph 1321.)

This paragraph is not accompanied by any note or reference, but it is enclosed in inverted commas, and I soon discovered that it is a translation of a passage in Ganot's *Traité élémentaire de Physique* (Paris, 1859). See p. 312, end of paragraph 334.

There is an English translation of Ganot, in which the same passage occurs, and is left, as it is in the original, without note or comment.

I applied for further information to some of my scientific

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[*] Communicated by the Author.

friends, and Professor Price pointed out to me a note in Jamin's *Cours de Physique,* which appeared at first sight to assert the equality of the two kinds of specific heat. The probable explanation of it (vide *infra*) was suggested to me by Professor H. Smith and Sir B. Brodie.

At p. 491, vol. ii. of Jamin's book, there is a note on the velocity of sound. If V be the velocity calculated on the supposition that pressure is proportional to density, $V\sqrt{1+\theta}$ the true velocity, c the specific heat (or "capacity" as it is here called) at constant pressure, and c' at constant volume, the ordinary theory accepts the equation

$$1+\theta=\frac{c}{c'}.$$

In the note referred to, Jamin objects to this equation on the ground that the old proof of it involved the assumption that the temperature of a gas is lowered by *free* expansion. After describing the process which he considers to imply this error, he says,—

"Mais ce raisonnement est inexact. Quand un gaz se dilate, il est vrai qu'*habituellement* il se refroidit, mais c'est parce qu'il produit du travail, et s'il arrive qu'on le dilate en le faisant pénétrer dans un vase vide, il ne change plus de température (page 435). Les deux capacités sont donc *nécessairement* égales entre elles. Par conséquent, l'équation (β) est vraie, mais elle devient fausse si l'on y remplace $1+\theta$ par $\frac{c}{c'}$."

(The equation (β) is velocity $=V\sqrt{1+\theta}$.)

This note is objectionable in several respects. In the first place, it is not a safe conclusion that a result is false because it has been obtained by fallacious reasoning. Secondly, although the reasoning objected to probably did contain, in the minds of its first authors, the fallacy attributed to it, it is capable of being so interpreted as not to contain it. Thirdly, the modern mechanical theory of heat supplies a demonstration* of the equation

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* Let p, V, ρ, t, h be the pressure, volume, density, temperature, and actual heat of a unit of mass of gas. Let $E=Ah$ be the energy which would have to be spent in communicating the actual heat h, and let $c'=\frac{dh}{dt}$ be the real specific heat (or specific heat at constant volume), and c the specific heat at constant pressure. Then assuming that $pV=a+\alpha t$, we shall have

$$p\,dV+V\,dp=\alpha\,dt. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Suppose the gas to be compressed to volume $V+dV$ (dV being negative), then dp, dt being the increments of pressure and temperature, the increment of actual heat is $c'dt$, and the corresponding energy is $Ac'dt$. Now

$1+\theta=\frac{c}{c'}$, which can only be objected to on two grounds—viz. that some (or all) of the quantities assumed as constant are not rigorously constant, and that the usual mechanical theory of sound is not rigorously correct; and both objections would be irrelevant to the present purpose.

Lastly, the assertion that "the two capacities are necessarily equal," appears at first sight to mean that c and c' are necessarily equal. But as the author cannot have intended to deduce this conclusion from a fact which proves that c and c' are necessarily *unequal*, it is probable that he only meant to say that when heat is spent upon a given quantity of gas, how much of it goes to make the gas *hot* depends only upon the change of temperature. If this be so, the language used is very inaccurate, and almost certain to mislead an ordinary student.

But, whatever Jamin may have meant, it is certain that Ganot really did mean to say that Regnault had proved experimentally the equality (or near equality) of c and c'. And neither he nor his translator, nor the author of the 'Second Step,' have noticed that to assert this equality is to deny the conservation of energy.

It may be conjectured that Ganot had somewhere met with the statement "les deux capacités sont égales entre elles," intended in the non-natural sense suggested above, and confirmed

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this must be equal to the whole energy spent in the operation, viz. $-pd\mathrm{V}$; for if the gas were now allowed to expand freely to its former volume, it would retain its new temperature, while nothing would have been spent except the work of compression. Hence $-pd\mathrm{V}=\mathrm{A}c'dt$; and combining this equation with (1), we obtain by eliminating dt,

$$\frac{dp}{p}=-\frac{\alpha+\mathrm{A}c'}{\mathrm{A}c'}\frac{d\mathrm{V}}{\mathrm{V}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Next, let the gas (in its original condition) be heated, and at the same time allowed to expand under constant pressure p, until its volume becomes $\mathrm{V}+d\mathrm{V}$ and its temperature $t+\delta t$. The increment of actual heat will be $c'\delta t$, and the whole energy required for the operation will therefore be $\mathrm{A}c'\delta t+pd\mathrm{V}$. But, by the definition of c, the whole energy required is $\mathrm{A}c\delta t$. Hence, since in this case (1) gives $pd\mathrm{V}=\alpha\delta t$ (for $dp=0$), we have $(\mathrm{A}c'+\alpha)\delta t=\mathrm{A}c\delta t$, or $\mathrm{A}c'+\alpha=\mathrm{A}c$; hence from (2),

$$\frac{dp}{p}=-\frac{c}{c'}\frac{d\mathrm{V}}{\mathrm{V}}=\frac{c}{c'}\frac{d\rho}{\rho},$$

of which, according to the ordinary theory of sound, the equation $1+\theta=\frac{c}{c'}$ is a consequence.

It is hardly necessary to add that this demonstration is only given for convenience of reference, and not as containing anything new.

in that sense by some experiment of Regnault's, that he took it in its natural sense, and transferred it to his book.

The character of elementary scientific books has become a matter of great importance since the recognition in our old Universities of physical studies as instruments of education. It is for this reason alone that I have thought it worth while to offer these remarks for publication in the Philosophical Magazine.

Oxford, October 27, 1864.

LVI. *On the Nomenclature of the Physical Sciences.*

By C. J. Monro*.

IN a paper lately published in the Philosophical Transactions, Professor Thomson has called the Astronomer Royal a naturalist. It would not be more inconsistent with English usage to call him a physician; but the most startling innovation seemed better than to call anybody a natural philosopher. Whatever is or ought to be the meaning of *philosophy*, it has nothing to do with special branches of science. To use the word in this manner is to do violence to its history, and is inconsistent with its modern application in other connexions.

But I venture to think that the reform leaves untouched the most serious vice of the misnomer. If *philosophy* is more grossly misused in the substantive, *nature* is more gravely misused in the adjective. Even as far as it goes, the advantage of the change is not unqualified: by rescuing the word *philosophy* from its misuse in the term *natural philosophy*, we shall sacrifice the distinction between natural philosophy and natural history. The distinction is ill expressed indeed, and even, if I rightly understand the usage on the subject, inaccurately drawn; but it is real at bottom. Natural history describes things, to speak roughly, as they come: it comprehends the typical examples of what Dr. Whewell calls the classificatory sciences. Natural philosophy analyzes its object; but perhaps because we have a good deal restricted our view to a particular kind of analysis, mathematical analysis, it is assumed that its object consists exclusively of things without life.

However, the whole system is in confusion. In the classification of the physical sciences we meet the word *nature*, or the corresponding Greek word in different forms, of which none suggests its own meaning much more than the meaning of any other.

In the first place we have the general term *physical science*, or, as Mr. Faraday apparently prefers to say, *natural knowledge*.

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* Communicated by the Author.

Under physical science we have the subdivisions *natural history*, *physiology*, and *natural philosophy*. I do not say these are exactly conterminous; probably they are not all invariably used in quite the same senses. Anatomy might sometimes be admitted into one of them, and sometimes not. But all would be, at first sight, terms as general as physical science itself; and *physiology*, which is in fact the most restricted, is in form the most general of the three. Again, *natural philosophy* has for its principal subdivision *physics*, a term apparently more general than any we have yet met with.

That is to say, the science of *nature* is divided into history of *nature*, discourse of *nature*, and philosophy of *nature*; and the philosophy of *nature* is divided, let us say, into chemistry and *naturals*. Considering that these sciences differ at least as much in their objects as in their methods, and that no one would ever think of dividing science into history, discourse, and philosophy, we may ask whether confusion could go further than this. Something like it might be found in Blackstone; elsewhere hardly.

I shall not examine the history of these phrases. One of them, however (*physics*), the most unmeaning as it stands, but the most defensible in its origin, it is worth while to observe, because it suggests a practical lesson in the art of nomenclature which may be useful if the present system is to be reformed. As the word seems to exclude chemistry, I suppose it is short for *mathematical physics*. Now this term, though clumsy, is perfectly correct if we accept the more general use of the word *physical*; for it suggests exactly what it denotes, that part namely of physical science which can be treated mathematically. The inconvenience of it is that it expresses its *differentia* in a word five syllables long; and now that this is dropped, we apply the name of the *summum genus* to an *infima species*. The moral is that an essential part of a compound term should not be expressed by an adjective five syllables long.

There is scarcely a chance of the general adoption of any reform deliberately suggested, especially if the suggestion comes with no sort of authority; but it is worth while at least to consider the materials we have to dispose of.

The words *physic*, *physical*, *physiology*, come from the Greek representatives of a root which expresses *growth*. *Natural* comes from a root which has been used for scientific purposes in Latin as the equivalent of the other. But the idea of growth has been applied in two ways.

First, to things which are the subjects of generation proper—plants and animals.

Secondly, to things which are the subjects of generation in a wide and metaphorical sense. In this application it was very early contrasted with artificial creation; but in later times, perhaps under the influence of Plato, the Greek derivatives have more emphatically suggested a contrast with things supposed to be above growth, not below it, with things eternal. Hence the antithesis of moral and physical, even mental and physical.

It is in the first application that we speak of physiology, and perhaps of natural history; all the other terms above mentioned belong to the second application.

Our materials would be most economically turned to account by using the word *natura* in the second and wider application, and *phý'sis* in the first and narrower one. But there would be something arbitrary in the selection, and it would be the formal abandonment of perhaps the oldest generalization in philosophy.

The only practicable consistent system would consist in using both *natura* and *phý'sis* in the wider application; to preserve, that is, the phrases *physical* and *natural science* or *knowledge*, and to find other terms for the subdivisions. But then what are we to do with physiology? It would probably be necessary to keep *biology*, although, as it ought properly to mean the science of lives, lifetimes, or livelihoods, it is not very good Greek for the science of life; *zoology* has been appropriated as the science of *zôa*, animals. But the main division of the province was expressed in old times by the words *émpsȳcha* and *ápsȳcha*, corresponding to our correct but less manageable adjectives *organic* and *inorganic*. I see no good reason against *empsychology* and *apsychology*: the words are long, but easily pronounced and easily contrasted by their accentuation. For a different reason, *apsychology* might not be currently used—namely, because it is a negative word; it would not, in fact, be necessary to use it so often.

Apsychology would approximately coincide with natural philosophy. It remains to consider its principal species, physics. This term is the easiest replaced of all. I propose a reform which would have the advantage of rescuing another word from a sense in which I contend it is used improperly at present. It is not quite true, as I shall show, that this section of physical science is distinguished by its mathematical treatment. But what does distinguish it is the idea of force conceived as fundamentally the same throughout it. I would therefore call it *dynamics*. That *dynamics* is used already in a more restricted sense in opposition to *statics*, is a reason in favour of the change; for it is wrongly so used. The idea of force is common to statics and "dynamics;" what distinguishes "dynamics" is the idea of motion. It should be called *cinetics*. This word may seem tainted with the heresy condemned by Sir John Herschel at the opening of the eighth chapter of his 'Astronomy.' But I am not questioning the reality of force; I merely object to its giving a name to a single subdivision of the science concerned with it. As the number of the convertible forms of force increases, dynamics will tend to swallow up apsychology and will extend far into empsychology. But by that time the nomenclature will be ready for reform again. At present, what I should call *dynamics* scarcely embraces chemistry, because it is not yet determined, I believe, what in the latter science is the analogue of the other forms of force.

It must be admitted that the adoption of such a term as *empsychology* would somewhat disturb the modern use of one of the most famous words in the philosophical vocabulary: I mean the word *psȳchḗ*. But in Greek, I believe, of all ages, Pagan, Jewish, and Christian, *psȳchḗ* comprehends animal life, and in Aristotle it comprehends vegetable life. And though (again I suppose under the influence of Plato) psychology is especially contrasted with physics, this is because we are more Platonic than Plato. *All* or *every life* may be, as he says, *immortal, unborn, and indestructible*; but in the classical passage where he says it is all this, he applies the word *phy'sis* to it, and the church accepted the application to more mysterious essences still. Whatever else the object of psychology may be, it is certainly something that grows, and its method, so far as it has one, is the method of the physical sciences. The science of language is part of psychology, and Mr. Max Müller claims a position for it among the physical sciences. If this is conceded, the theory of chances should take its place as the mathematical department of psychology, and as another of the physical sciences. It would be mathematical physics, but of course not dynamics. *Psychology* would sound awkward, as I have admitted, by the side of *empsychology*, or as a subdivision of it; I should prefer to speak of *mental science*, and contrast it with *material science*: but I think the awkwardness would not be intolerable; it would be nothing to the present anomalies.

This extension of the term *physics* would be the rectification of another frontier, that of *metaphysics*. Metaphysics will always be contrasted with physics, without reference to the history of the former word, or to the exact sense of the Greek preposition. Accordingly it is now often confounded with psychology. But as soon as psychology or mental science is understood to be a part of physical science, *metaphysics* may be kept without difficulty to its proper sense, the science of existence as such. Whether there is such a science or not, is a distinct question: those who think there is, whether right or wrong, will want the word; and so will those who wish to contradict them.

It would be too much to expect that even one of my suggestions will be adopted; but I hope that somebody will make better ones. Of one thing I am pretty sure, that no one will say that the present state of things is not disgraceful. I know it is easy to overrate the importance of such matters, but I think it is also easy to underrate it.

Hadley, Middlesex.

LVII. *On Tasmanite, a new Mineral of Organic Origin.* By Arthur H. Church, *M.A. Oxon., Professor of Chemistry, Royal Agricultural College, Cirencester*[*].

In the Tasmanian Court of the International Exhibition of 1862, a very remarkable kind of fuel was shown by the "Dysodile Company"; it was catalogued as "resiniferous shale." In the Jermyn Street Museum of the School of Mines a specimen of the same mineral is termed "Combustible Shale, River Mersey, north side of Tasmania." In the British Museum the specimen is unlabelled.

I have now completed the investigation, begun in 1862, of this mineral, and in the present communication give the chief results of my experiments.

The true dysodile from Glimbach near Giessen, analysed by Delesse, does not seem to be identical either in chemical or physical constitution with the Tasmanian mineral. I shall, however, investigate this point fully if I am successful in obtaining a sufficient supply of the true dysodile, which is said to occur at Mellili near Syracuse, and at Salzhaufen in Hessia.

The so-called resiniferous shale is distinctly laminated, the organic matter, which occurs in scales, being disposed in planes parallel to the lamination, and probably causing it. These scales are of a reddish-brown colour, and form from 30 to 40 per cent. of the rock. Their shape may be best judged of by the accompanying figures,—1, 2, and 3 representing the aspect of the scales as viewed from above, 4, 5, and 6 their appearance as seen edgewise.

The average diameter of the disks is about ·03 of an inch, while their thickness at the centre is sometimes as much as ·007.

Separation of the Organic Substance.—As none of the ordinary solvents of resinoids and similar bodies seemed capable of dissolving out the carbonaceous constituent of the mineral, the following plan of effecting the separation was adopted. A large quantity of the mineral was crushed to a coarse powder, placed

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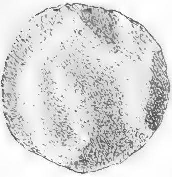
[*] Communicated by the Author.

in a Phillips's precipitating-glass, and strong hydrochloric acid poured upon it. A trace of carbonic anhydride was thus set free from the small quantity of carbonate of calcium present, while the alumina and ferric oxide of the mineral were partly dissolved. These chemical actions served to break up the mineral, and the organic 'scales' became for the most part disengaged, and floated, owing to the high gravity of the hydrochloric solution, which had been further increased by the addition of chloride of calcium. The scales were collected from the surface by a strainer, and washed repeatedly by decantation; by this method of purification the inorganic matter in them was reduced to a minimum.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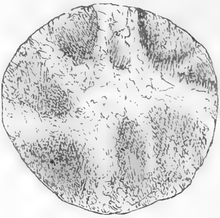
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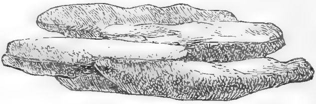
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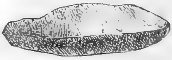
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The substance thus prepared presents such remarkably distinct chemical and physical characters, that I venture to assign to it a distinct name, *Tasmanite.*

I have already described the mode of occurrence and the physical appearance of the Tasmanite scales; the action of certain chemical reagents and of heat upon this substance may now be recorded: I should mention that the density of the substance is about 1·18; its hardness 2. It is translucent, of a reddish-brown colour. Lustre resinous, and fracture conchoidal.

Hydrochloric acid has no action on Tasmanite; nitric acid slowly oxidizes it, disengaging carbonic acid and pernitric oxide, while the sulphur appears as sulphuric acid. If the action of the nitric acid be stopped before all the substance has disappeared, the residue will be an orange-brown powder, which burns still more readily than Tasmanite, but it is not explosive.

Sulphuric acid readily carbonizes Tasmanite, torrents of sulphurous anhydride being disengaged at the same time.

Aqueous solutions of the alkalies appear to be without action on Tasmanite.

Alcohol, ether, bisulphide of carbon, benzole, turpentine, mineral turpentine, and paraffine oil do not appear to exert the least solvent action upon Tasmanite, even on the application of heat: the result might be different under an increased pressure.

When Tasmanite is heated in the air, it burns readily with a very smoky flame and offensive odour. Submitted to destructive distillation, it fuses partially and yields oily and solid products having a disagreeable smell, recalling that of some specimens of Canadian petroleum. One is tempted to suggest that the natural rock-oils may in some instances originate in the action of heat upon substances similar to Tasmanite shale.

Composition of Tasmanite.—Qualitative analysis of Tasmanite showed it to contain not only a large quantity of carbon and hydrogen, but also a very considerable proportion of sulphur; and it was found that the most careful mechanical treatment of the specimens failed to separate from them completely the mineral impurities. That the sulphur detected was an integral part of the carbonaceous matter itself, and was not owing to the presence of an inorganic sulphide or sulphate, was proved in several ways, and was further confirmed by the observation that the more completely the mineral matter had been removed, the more sulphur was found in the specimen of Tasmanite operated upon.

I am indebted to my friend Mr. W. H. Perkin for the first four of the following analyses of purified Tasmanite. In Nos. I., II., and III. the substance was burnt in a current of air, and finally of oxygen, the sulphurous acid being absorbed by binoxide of lead; in analysis IV. the combustion was performed with chromate of lead. In analysis V. to XI. the sulphur or the ash were alone determined. The sulphur was obtained by oxidizing the mineral with strong nitric acid and bichromate of potassium in a capacious flask, diluting largely with water when complete solution had been effected, filtering to separate the silica of the ash, and precipitating with nitrate of barium. After standing twelve hours, the precipitated sulphate of barium was collected, washed completely, and ignited with the usual precautions. In analysis V. the sulphur was oxidized by means of the gradual addition of chlorate of potassium instead of bichromate to the mixture of nitric acid and Tasmanite. The mineral must be completely destroyed and dissolved in order to extract the whole of the sulphur. The two preparations submitted to analysis are distinguished as *a* and *b*.

a.

I. ·4288 grm. of substance gave 1·1466 grm. carbonic acid and 0·0354 grm. ash.
II. ·3535 grm. of substance gave ·3049 grm. water and ·0284 grm. ash.
III. ·3087 grm. of substance gave ·2729 grm. water and ·0248 grm. ash.
IV. ·3668 grm. of substance gave ·9796 grm. carbonic acid and ·315 grm. water.
V. ·4785 grm. of substance gave ·175 grm. of sulphate of barium.
VI. ·5125 grm. of substance gave ·041 grm. of ash.

b.

VII. 6·16 grains of substance gave 2·12 grains of sulphate of barium.
VIII. ·337 grm. of substance gave ·121 grm. of sulphate of barium.
IX. ·654 grm. of substance gave ·2302 grm. of sulphate of barium.
X. ·421 grm. of substance gave ·0525 grm. of ash.
XI. ·482 grm. of substance gave ·0580 grm. of ash.

These results correspond to the following percentage numbers:—

	I.	II.	III.	IV.	V.	VI.	VII.	VIII	IX.	X.	XI.
Carbon	72·92	...	...	72·83							
Hydrogen ...	......	9·58	9·82	9·54							
Sulphur	......	...	...	...	4·97	...	4·72	4·93	4·83		
Ash...........	8·25	8·03	8·03	...	...	8·00	...	...	...	12·47	12·03

The following are the mean percentages, rejecting the hydrogen of analysis III. as evidently too high:—

Carbon . . .	72·88	
Hydrogen . .	9·56	
Sulphur . . .	4·90	
Ash (*a*) . . .	8·14	(*b*) 12·24

Before these numbers can be taken to represent the centesimal composition of pure Tasmanite, they require recalculation after deduction of the ash, which is, without doubt, an accidental impurity. This ash mainly consists of silica and alumina; it contains also a small quantity of ferric oxide and of some soluble sulphate, this latter compound being derived in all probability from the oxidation of the sulphur contained in the Tasmanite proper. Since this ash has almost exactly the same composition as the shaly rock in which the grains of Tasmanite occur, we may directly subtract it without introducing any appreciable error. The percentages deduced from analyses I. to IV. are given below after deduction of the mean percentage of ash, 8·14; analysis V. after deduction of 8 per cent. of ash; and analyses VII. to XI. after deduction of 12·24 per cent. of ash.

Carbon	79·34
Hydrogen . . .	10·41
Sulphur	5·32
Oxygen (by diff.) .	4·93

These numbers may fairly be taken as representing the centesimal composition of Tasmanite; the most noteworthy point regarding them is the high percentage of sulphur. Tasmanite is, I believe, the first carbonaceous mineral which has been found to contain a large amount of sulphur in combination, not with a metal as in pyritic coal, but in intimate union with the carbon and hydrogen of the substance. It would seem to be allied to retinite, although that mineral contains no sulphur; yet the chief constituent of many specimens of retinite yields, on analysis, percentages of carbon and hydrogen almost exactly the same as those just recorded. The formula $C^{10} H^{16} O$, or $C^{40} H^{64} O^{4}$, has been suggested for retinite. A similar formula, in which sulphur is introduced, requires nearly the same percentages as those yielded in the analysis of Tasmanite:—

	Experiment.	Theory, $C^{40} H^{62} O^{2}$, S.
Carbon . . .	79·34	79·21
Hydrogen . .	10·41	10·23
Sulphur . .	5·32	5·28
Oxygen . . .	4·93	5·28
	100·00	100·00

It will be seen that the experimental percentages of carbon and hydrogen are a little higher than the theoretical: as one can easily account for this excess in the case of a substance which is hygroscopic and contains sulphur, I have been led to prefer the suggested formula to one or two others which demand a higher percentage of carbon and hydrogen. The sulphur determinations are accordant; but there is an apparent deficiency of oxygen —4·93 per cent. instead of 5·28. If, however, we accept the theoretical percentages of carbon and hydrogen and the experimental percentage of sulphur, we arrive, by difference, at the following percentage of oxygen:—

	Experiment.	Theory, $C^{40} H^{62} O^{2}$, S.
Oxygen . . .	5·24	5·28
Sulphur . . .	5·32	5·28

If we accept the formulæ $C^{40} H^{64} O^{4}$ and $C^{40} H^{62} O^{2}$, S for pure retinite and for Tasmanite respectively, we may compare them by assuming the latter mineral to differ from the former only by containing H^2O less, and by the presence of 1 atom of S in lieu of 1 atom of O:—

$$C^{40}H^{62}O^3+H^2O \; . \; . \; . \; . \quad \text{Retinite.}$$
$$C^{40}H^{62}O^2S \; . \; . \; . \; . \; . \; . \quad \text{Tasmanite.}$$

To suggest a rational formula for the remarkably complex molecules of retinite and Tasmanite may seem premature; but it is possible that in these minerals we have the hydrated oxide (retinite gives off water when heated) and the anhydrous sulphide respectively of an oxygenated radical, $C^{20}H^{31}O$—

$$\left.\begin{matrix}C^{20}H^{31}O\\C^{20}H^{31}O\end{matrix}\right\}O+Aq \quad \text{Retinite.}$$

$$\left.\begin{matrix}C^{20}H^{31}O\\C^{20}H^{31}O\end{matrix}\right\}S \quad \text{Tasmanite.}$$

These minerals may be derivatives of a turpentine, $C^{20}H^{32}$; or the radical I have assumed them to contain may be a homologue of benzoyle,

$$C^7H^5O+13CH^2=C^{20}H^{31}O.$$

LVIII. *On the History of Force.* *By* Dr. C. K. Akin[*].

IN the Number of the Philosophical Magazine for October last (p. 289), Professor Guthrie Tait repeats an assertion already made by him upon two previous occasions, first in the Philosophical Magazine (see vol. xxv. p. 429, 1863), and next in the Proceedings of the Royal Society of Edinburgh (see No. 59, p. 122), that "Newton had completely enunciated the Conservation of Energy in ordinary mechanics;" and in another portion of the same Number of this Magazine (p. 311), Professor Bohn cites passages from the writings of Descartes and John Bernoulli bearing on the same question of the Conservation of Force also. For nearly two years, I have been engaged at intervals in collecting materials for a History of the Philosophy of Force, but which circumstances now oblige me to lay for a while aside. I am thus induced to publish meanwhile part, at least, of what little I have hitherto discovered that is new or interesting, in the Philosophical Magazine; for doing which the publications above referred to at present afford me an additional incentive.

I. *On the Conservation and Conversion of Force.*

1. Prof. Tait rests the claims which he advances in behalf of Newton, on the following passage from the *scholium* to the Third Law of Motion, in Newton's *Principia*:—

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[*] Communicated by the Author.

> "Si æstimetur agentis actio ex ejus vi et velocitate conjunctim, et similiter resistentis reactio æstimetur conjunctim ex ejus partium singularum velocitatibus et viribus resistendi ab earum attritione, cohæsione, pondere, et acceleratione oriundis; erunt actio et reactio . . . sibi invicem semper æquales."

The dots (. . .), indicating an elision, stand for the words *in omni instrumentorum usu.* Now, I cannot help thinking that the omission of these words completely alters the meaning of the above passage. Newton, in the passages preceding the above, is bent upon showing that the effective force of a moving system may be measured by its *momentum* (*mv*); and he states that this assumption is proved by the fact that, in machines, the moving force may be so estimated,—the velocity being always inversely proportional to the mass or inertia, after the resistance has been subtracted. "Cæterum mechanicam tractare," Newton adds, "non est hujus instituti. Hisce volui tantùm ostendere, quàm latè pateat quamque certa sit lex tertia motus," *i. e.*, that action is always opposed by an equal reaction. Yet this law, however important, cannot but be considered as a corollary from, rather than equivalent to, the principle of the conservation of force itself. Moreover, in the same *scholium* occurs a passage relative to the state of imperfectly elastic bodies after collision, which will scarcely be considered to favour the advocacy of Prof. Tait:—

> "A congressu et collisione corporum nunquam mutabatur quantitas motus, quæ ex summâ motuum conspirantium et differentiâ contrariorum colligebatur . . . . Porro ne quis objiciat regulam . . . . præsupponere corpora vel absolutè dura esse, vel saltem perfectè elastica . . . . addo quod . . . si regula illa in corporibus non perfectè duris tentanda est, debebit solummodo reflexio minui in certâ proportione pro quantitate vis elasticæ."

How does Newton account for, in this case, the velocities lost? But doubtful, if not impossible, as Newton's authorship of the principle in question appears from the above passages, Query 31 in his 'Optics' will be found still less compatible with it. Perhaps the remark of John Bernoulli concerning parts of this query—"ridiculum dicerem, si a tanto Viro non scripta essent" (*Opera*, vol. iii. p. 253)—may be rather too severe, yet its contents will be found difficult to reconcile with the assertion of Prof. Tait:—

> "Some other principle [than inertia] was necessary for putting bodies into motion; and now they are in motion, some other principle is necessary for conserving the motion. For . . . . it appears, that motion may be got or lost. But . . . . motion is more apt to be lost than got, and is always upon the decay. For . . . . if two equal bodies [*e. g.*] meet directly *in vacuo*, they will, by the laws of motion, stop where they meet, and lose all their motion . . . unless they be

elastick. . . . If it be said, that they can lose no motion, but what they communicate to other bodies, the consequence is, that *in vacuò* they must go on and penetrate one another's dimensions. . . . Seeing therefore the variety of motion, which we find in the world, is always decreasing, there is a necessity of conserving and recruiting it by active principles; such as are the cause of gravity . . . . . and the cause of fermentation."

I trust I shall be able to enter more fully upon some future occasion into a consideration of these statements of Newton, which are followed by others no less remarkable.

2. With regard to Prof. Bohn's extracts, I would observe that both the principles, of the conservation of energy or motion, and of force, owe their first enunciation in a scientific form—at least, as regards particular instances—to Huyghens. It was Huyghens who, in the *Journal des Savants* for March 1699 (vol. ii. p. 534), first stated the following laws regarding the collision of perfectly elastic bodies:—

(1) "La quantité du mouvement qu'ont deux corps, se peut augmenter ou diminuer par leur rencontre; mais il y reste toujours la même quantité vers le même côté, en soustrayant la quantité du mouvement contraire." (2) "La somme des produits faits de la grandeur de chaque corps dur, multiplié par le quarré de sa vitesse, est toujours la même devant et après la rencontre."

(See also Phil. Trans. vol. iv. p. 927.) In Huyghens's posthumous dissertation, *De Motu Corporum ex Percussione* (*Oper. rel.* vol. ii.), the second of the two propositions quoted is reproduced; but of the first proposition only one-half is given, in these words (p. 84):—

(3) "Corporibus duobus sibi mutuo occurrentibus non semper post impactum eadem motus quantitas in utroque simul sumpto conservatur quæ fuit ante, sed vel augeri potest vel minui."

It is to this and the subsequent propositions that Bernoulli, wishing to disprove the anti-Leibnitzian estimate of force, refers in saying (*Opera,* vol. iii. p. 254),

"Observatum est a multis, præsertim ab Hugenio . . . . . motus quantitatem, etiam in corporibus perfecte elasticis, in immensum posse augeri et minui."

And it is apparently with the very same object as that which Bernoulli had in view that Huyghens, who had adopted Leibnitz's measure of force, in reproducing his proposition in the treatise last quoted, omits that other portion which he had appended to it in the French journal. However, as is well known, the whole of proposition (1) is perfectly correct*; and proposition (3) appears

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* Cf. Cor. 3 to Newton's Third Law of Motion:—"Quantitas motus quæ colligitur capiendo summam motuum factorum ad eandem partem, et differentiam factorum ad contrarias, non mutatur ab actione corporum inter se."

incompatible with it only so long as it is not considered that what Huyghens calls *quantitas motus* in the latter means, with reference to any two bodies proceeding to mutual impact, their relative velocity (γ) multiplied into the mass (μ) of the quicker body,—which is an arbitrary if not incorrect estimate of, and certainly different from what is ordinarily (and also by Newton) called, quantity of motion or *momentum*.

To the passages quoted already in the Philosophical Magazine from John Bernoulli's *De Vera Notione Virium Vivarum*, the following extracts may be usefully added:—"La force vive, produite dans un corps . . . . . est équivalente à cette partie de la cause qui s'est *consumée* [italics by the transcriber] en la produisant; puisque toute cause efficiente doit être égale à son effet pleinement exécuté" (*Discours sur le Mouvement*, 1727, *Opera*, vol. iii. p. 36). And, "Tout le monde regarde comme un axiome incontestable, que toute cause efficiente ne saurait périr, ni tout ni en partie, qu'elle ne produise un effet égal à sa perte" (*ibid.* p. 56).

3. As for the principle of the conservation of force in its wider sense, it was first enunciated, by implication, in Huyghens's *Horologium Oscillatorium*, published in 1673, Prop. 4 of Part 4 of which is as follows:—"Si pendulum è pluribus ponderibus compositum, atque è quiete dimissum, partem quamcunque oscillationis integræ confecerit, atque inde porro intelligantur pondera ejus singula, relicto communi vinculo, celeritates acquisitas sursum convertere, ac quousque possunt ascendere; hoc facto, centrum gravitatis ex omnibus compositæ, ad eandem altitudinem reversum erit, quam ante inceptam oscillationem obtinebat" (*Oper. var.* vol. i. p. 126). James Bernoulli generalized this proposition, asserting "quod commune centrum gravitatis plurium ponderum non possit ascendere altius per gravitatis eorum effectum, quam unde descendit" (*ibid.* p. 247).

4. Intimately connected with the principle of the Conservation of Energy strictly so called (taking the word *energy*, according to the proposition of Young, as English for *vis viva*) is that of the Correlation of Forces, or, as it has been called by a logician of great reputation, of the Allotropy of Force. There has been of late a good deal of controversy regarding the priority of invention or discovery of this last-named principle; and it may consequently be interesting, in an historical point of view, to take cognizance of passages of much earlier date than any hitherto relied upon as establishing such priority, and upon which I have, in the majority of cases, rather accidentally lighted. The following is an extract from Placidus Heinrich's *Die Phosphorescenz*, &c., published in 1812:—

"Unterdessen wissen wir wenigstens so viel mit Zuverlässigkeit, dass in der Natur nichts verloren geht . . . . alles erhält sich durch einen steten Umtausch: das eine gewinnt durch den Verlust des andern: das eine entsteht durch das Verschwinden des andern. . . . . Also im Universum nie Verlust, nur Wechsel und Umtausch." (Vol. ii. p. 283.)

The next quotation is from a paper by Dr. Mohr, of Coblentz, on Heat, published in 1837:—

"Ausser den bekannten 54 chemischen Elementen gibt es in der Natur nur noch ein Agens, und dieses heisst Kraft; es kann unter den passenden Verhältnissen als Bewegung, chemische Affinität, Cohäsion, Elektricität, Licht, Wärme und Magnetismus hervortreten, und aus jeder dieser Erscheinungsarten können alle übrigen hervorgebracht werden . . . . Vermöge der Kraft des Armes reisst man die Induktionsrolle von einem Magneten los, es entsteht in dem darum geschlungenen Schraubendrathe ein elektrischer Strom, welcher bei Unterbrechung als Funke, oder bei verengerter Leitung als glühender Drath (Wärme und Licht) erscheint; derselbe erregt magnetische Polarität, wenn er als Schraubendrath um eine Stahlnadel geleitet wird; er zersetzt das Wasser wodurch er geleitet wird, und hebt zugleich seine Affinität und Cohäsion auf; und da nun der dünne Platindrath, die Ampère'sche Schraube und der Wasserzersetzungs-Apparat in derselben Kette eingeschlossen sein können, so leuchtet ein, wie die Kraft des Armes unter verschiedenen Verhältnissen, als Wärme, Licht, chemische Affinität, Magnetismus und Cohäsion zum Vorschein gekommen ist." (Baumgartner's *Zeitschrift für Physik*, vol. v. pp. 442–3.)

This passage is followed by two more pages, showing in greater detail the connexion and transmutation of the several known forces, and a transcript or translation of which I hope to give upon some future occasion. The author concludes his observations with the following judicious remarks:—

"Ohne Zweifel lassen sich alle physikalischen Erscheinungen der sogenannten Imponderabilien unter eine dieser Rubriken bringen. . . . . . Es bleibt aber von dieser flüchtigen Andeutung bis zur vollkommnen Einsicht in die Natur der Sache noch unendlich viel zu thun übrig." (*Ibid.* p. 445.)

With regard to heat, besides showing that its nature or form is motion, which is the principal object of the paper, the writer states (p. 422):—"Was . . . eine Kraft hervorbringt, muss selbst eine Kraft sein;" and again (p. 421):—"Was . . . eine Kraft aufhebt muss selbst eine Kraft sein"; whence he concludes, considering the effects of heat:—"Die Wärme erscheint in unzähligen Fällen als eine Kraft" (p. 421).

In the last place, it gives me great pleasure to quote, among the earliest statements concerning the transmutation of forces, the following passage from the 18th series of Mr. Faraday's electrical researches, published in January 1840:—"We have many processes by which the form of the power may be so changed that an apparent conversion of one into another takes place. So we can change chemical force into electric current, or the current into chemical force. The beautiful experiments of Seebeck and Peltier show the convertibility of heat and electricity; and others by Œrsted and myself show the convertibility of electricity and magnetism" (Res. in Electr., § 207, i.).

II. *On Gravitation.*

In his 'Life of Newton,' Sir D. Brewster states (vol. i. p. 268):—"Kepler could not fail to suspect that some power resided in [the sun] by which the motions of the planets were produced; and he went so far as to conjecture that this power diminishes as the square of the distance of the body on which it is exerted; but he immediately rejects this law in favour of that of the simple distances." Again, further on (p. 282):—"Bouillaud maintained that the force of attraction must vary reciprocally as the square, and not, as Kepler asserted, in the simple ratio of the distance." In a similar manner, Sir I. Newton stated (see Rigaud's Hist. Essay, App. p. 32):—"Bullialdus wrote that all force, respecting the sun as its centre . . . must be reciprocally in a duplicate ratio of the distance from the centre." A reference to the original writings of Kepler and Bouillaud has suggested to me the following remarks, which, as they may be of some general interest, I purpose herewith publishing.

5. In the Introduction to the *Astronomia Nova,* referred to also by Sir D. Brewster, Kepler indeed "distinctly recognizes the mutual gravitation of matter"; but he extends that notion only to terrestrial bodies and the moon (see *Astr. Nova,* p. ***4). As regards the action of the sun upon the planets, Kepler states (*l. c.* p. 185):—"Virtus ex Sole in mundum per speciem egressa rapidus quidam torrens est, qui Planetas omnes adeoque totam forsan auram ætheriam ab occasu in ortum rapit, se ipso non aptus corpora ad Solem adducere vel ab eo longius propellere; quod esset infinitæ sollicitudinis opus." Again, in enumerating the six "axioms" which account for the planetary motions (p. 186), he distinguishes between a "virtus, quæ ex Sole," and through which every planet "de loco in locum, secundum longitudinem zodiaci transponatur," and a "virtus quæ est propria Planetæ," and from which he assumes "accessus Planetæ ad Solem et ab eo recessus oriri." Hence it is evident that Kepler did not consider the idea of gravitation as applicable to the action of the sun on the planets; and from cap. 33, entitled "Virtutem quæ Planetas movet, residere in corpore Solis," it appears also that the "virtus" of which the "debilitas sequitur

proportionem distantiarum" (p. 168), according to Kepler, was a tangential, and not a centripetal force.

6. Again, as regards Bouillaud, he opens cap. 12 of his *Astronomia Philolaïca* by the following observation (p. 21):—" .... Constat quod veritati magis aptum et congruens videtur, et quod valde probabilius sit planetas, et cætera corpora cœlestia per propriam formam moveri, quam ab anima adsistente." He then goes on to animadvert upon the views of Kepler, enunciated in cap. 33 of the *Astron. Nova* and elsewhere. Now Kepler had stated (*l. c.* p. 178):—"Demonstratum est cap. 32 planetarum motus intensionem et remissionem sequi proportionem distantiarum simplicem. At videtur virtus ex Sole emanans intendi et remitti debere in proportione duplicata vel triplicata distantiarum seu linearum effluxus"; and the chapter (34) in which this is stated is headed, "Qua mensura virtus ex Sole motrix, per mundi amplitudinem attenuatur." With regard to this passage, Bouillaud observes (p. 23):—"Hoc non negavit Keplerus, attamen [etc.] .... Sed hæc Kepleri responsio levis admodum est. Nam si in superficiali quantitate considerat illam virtutem motricem, necessario imminuere eam debuit in ratione dupla intervallorum: si vero in solis lineis superiori propositioni contradicit," etc. Bouillaud closes this chapter with the following observations (p. 24):—"Dico Solem à propria sua forma circa proprium axem moveri, qua ignitus et lucidus est, cæteris vero planetis nullam motus speciem imprimere, quæ illos vehat, ipsos vero singulos à singulis formis, quibus præditi sunt, circumduci." From the passages quoted, it appears, in the first place, that Bouillaud denied altogether the existence of any reaction between the sun and the planets, and, in the next place, that the force regarding which Bouillaud contended that its magnitude must depend on the inverse ratio of the square of distance (for reasons which Kepler himself had previously fully developed) was the same tangential force before assumed by Kepler, and not a centripetal force. It may not be superfluous to state also, regarding this same force, that, so far as it appears, Kepler supposed the force to be exerted only on the part of the sun; the sun itself not being liable to any reciprocal action on the part of the planets.

7. A third name which figures prominently in the history of gravitation, after Kepler and Bouillaud, is that of Borelli. Sir I. Newton wrote (see Rig. Ess. App. p. 30), "Borelli did something;" and Sir D. Brewster adverts more fully to his speculations. Yet it remains to be stated more precisely what was the exact "something" that "Borelli did."

In his *Theoricæ Mediceorum Planetarum* (p. 76), Borelli states:

"Supponamus præditum planetam à vertigine solarium radiorum in orbem ferri circa solem per circulorum peripherias ab occasu ad ortum; et quoniam ut dictum est motus circularis naturaliter quemdam imprimit impetum ipsi mobili, quo mediante à centro removetur, atque expellitur, veluti in funda ac rota observare licet, ergo, dum prædictus planeta circulariter rotatur, removebitur à centro Solis. . . Proindeque his aderunt duo motus directi inter se contrarii, alter perpetuus, ac uniformis, quo planeta impulsus à propria magnetica virtute sibi connaturali vero successive admovet solari corpori, alter verò difformis, et continuè decrescens, quo planeta à puncto (E) expellitur." Again (p. 78): "Ut dictum est, virtus motiva planetæ . . . componitur ex circulari impulsu, et gradu virtutis prementis uniformis, et ex gradu virtutis repellentis;" and (p. 81):—"Ut supponamus prædictum motum . . . . pendere à magnetica impellente virtute . . . , et motum circularem planetæ circa Solem." Hence, it is certainly evident that Borelli extended the idea of gravitation also to the sun (as a one-sided action); but then it is equally evident also that he supposed the attraction of gravity to be uniform, *i. e.* independent of distance (as may be seen more at large in the passages omitted from the above quotation),—and further, that he considered the motions of the planets to be the result of three forces—one circular, the other centrifugal, and the third attractive or centripetal. According to our present notions, a force acting circularly is impossible; and a centripetal force, although the expression is still used, is no force at all, but only a tendency produced by the possible resolution of velocities; whilst the motions of the planets are supposed be the result of an attractive force acting conjointly with certain impressed tangential velocities.

London, November 1864.

LIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 400.]

November 17, 1864.—Major-General Sabine, President, in the Chair.

THE following communication was read:—

"Comparison of Mr. De la Rue's and Padre Secchi's Eclipse Photographs." By Warren De la Rue, F.R.S.

I have stated, in the Bakerian Lecture read at the Royal Society on April 10, 1862, that the boomerang (prominence E)* was not depicted on Señor Aguilar's photographs. This is true of the prints which came into my hands in England. A visit to Rome in November 1862, however, afforded an opportunity for the examination of the first prints which had been taken in Spain on the day of the

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* See Index Map, Plate XV. Phil. Trans. Part I. 1862.

eclipse, previous to those printed off for general distribution by Señor Aguilar. I was agreeably surprised to find that the photograph of the first phase of totality showed not only this prominence very distinctly, but also other details, presently to be described, which were quite invisible in Señor Aguilar's copies. I had in fact experienced some difficulty in comparing measurements of my photographs with those of Señor Aguilar's, on account of the indistinctness (woolliness) of the latter, which I have attributed to Padre Secchi's telescope not having followed the sun's motion perfectly. A careful examination of the prints in Padre Secchi's possession has, however, convinced me that this was not the case during the period of exposure of the first negative ; for I have been able to identify with a magnifier many minute forms which could only have been depicted by the most perfect following of the sun's apparent motion. For instance, my statement that the prominence H (the fallen tree) was not seen from having been mixed up with the prominence G, is not applicable to Padre Secchi's copy of the first phase of totality, for in it every detail of the fallen tree can be made out.

On expressing to Professor Secchi my surprise at the great discordance between the copy of the first phase of totality sent to me by Señor Aguilar and that of the same phase in his possession, I was informed that after a few positive prints had been taken from the then unvarnished negative, it was strengthened by the usual photographic process with nitrate of silver. This I look upon as an unfortunate mistake, as the images of the prominences were increased and their details hidden, and the beauty of the negative for ever lost.

It occurred to Padre Secchi and myself that although there was no hope of procuring more satisfactory prints from the original negative of the first phase of totality, yet some advantage would arise from taking an enlarged negative from the positive print in his possession, although it could not be expected to yield as perfect an impression as might have been obtained by enlarging from the original photograph. The enlargement has been successfully accomplished in my presence ; and although Padre Secchi will take such means as he may think proper to make known the results of comparisons he may make between my photographs and his own, it will not be out of place for me to add a few remarks by way of appendix to my paper.

Taking the prominences in the order in my index map, Plate XV. :—

Prominence A (the cauliflower or wheatsheaf) has the same form in Padre Secchi's photograph as in mine. It extends considerably less in height above the moon's edge in this copy than in that printed off from the strengthened negative (Señor Aguilar's copy) ; the bright points of the two branching streams which issue from the summit towards the North are well depicted in the Secchi photograph, but not the fainter parts.

There exists a faint indication of the minute prominence B in the S. photograph.

The convolutions of the prominence C (the floating cloud) are seen in the S. photograph, and its form coincides absolutely with that of mine; it is a little nearer the moon's edge at the point *c*, probably because the telescope was uncovered relatively a little later than at Rivabellosa.

The prominence D cannot be clearly traced in the S. photograph.

The boomerang E is distinctly visible in the S. photograph; the point *e* is apparently prolonged; but this I attribute to an accidental photographic stain, for the bright part *e'* can be well made out.

The long prominence F cannot be made out in the S. photograph, probably from the cause explained in reference to C.

The fallen tree (H in the S. photograph) corresponds in its minutest details with its picture in my own. The articulated extremity *h*, the round points *h' h''*, the point *h'''*, and the connecting branch joining it with the stem are clearly seen.

The prominence G from *g* to *g'* corresponds precisely in the S. photograph with its image in my own, and a dark marking near *g* also is seen; the narrow portion of this prominence, from *g* to the point immediately below *h*, is not seen in the S. photograph.

The prominence I (the mitre) agrees in form in the S. photograph with its image in my own, even the faint point *i* is there seen. This prominence in the S. photograph extends further from the edge of the moon than in mine; and whereas in my photograph the convex boundary next the moon is cut off by the moon's limb, in Padre Secchi's the convex boundary is complete, and hence in all probability the prominence I presented another case of a floating cloud.

About midway between G and I there is a small round prominence visible in the S. photograph not seen in mine, which may be accounted for from our different positions in respect to the central line of the eclipse.

Between I and K, at a distance from I equal to about two-thirds the angular interval, there is in the S. photograph a prominence consisting of two round dots, which extend beyond the moon's limb to precisely the same extent as the prominence K protrudes in Professor Secchi's photograph beyond the moon's limb in excess of what it does in my own.

The prominence K has precisely the same form in every respect in the S. photograph as in mine, so far as mine shows it; but on account of parallax, more of it is seen in the S. photograph than in mine.

Beyond K is another prominence, visible in the S. photograph about 17° distant from K, a small round prominence which could not have been visible from my station.

Of the remaining prominences, L, M, N, O, P, Q, R, none were visible at the epoch of the photograph.

In conclusion, the photographic images of the prominences, so far as they are common to the two photographs taken at Miranda and Desierto de las Palmas, accord in their most minute details. The photographs must, from the difference of position of the two stations, have been made at an absolute interval of about seven minutes; and this fact, while it strongly supports the conclusion that the protuberances belong to the sun, at the same time shows that there is no change in their form during an interval much greater than the whole duration of an eclipse.

LX. *Intelligence and Miscellaneous Articles.*

A LETTER FROM JOHN DAVY, M.D., F.R.S., TO THE EDITORS OF THE PHILOSOPHICAL MAGAZINE IN REPLY TO CERTAIN CHARGES MADE BY C. BABBAGE, ESQ., F.R.S, ETC., AGAINST THE LATE SIR HUMPHRY DAVY, WHEN PRESIDENT OF THE ROYAL SOCIETY.

GENTLEMEN,—

MR. BABBAGE, in his recently published work, 'Passages from the life of a Philosopher,' has brought two charges against the late Sir Humphry Davy, when President of the Royal Society, both of them injurious to his character, if substantiated:—one, of a breach of promise; the other, of "transferring between three and four hundred pounds from the funds of the Royal Society into his own pocket"*.

These charges are contained in the thirteenth Chapter, under the heading of "Recollections of Wollaston, Davy, and Rogers," and are incongruously associated with an account of "The Thaumatrope," and with anecdotes of the poet. Mr. Babbage, in his Preface to another work†, expresses the opinion "that the famous maxim *de mortuis nihil nisi bonum,* appears to savour more of female weakness than of manly reason." In these his "recollections," or rather his assertions, so far as they relate to Sir Humphry Davy, he strictly confines himself to *nihil nisi malum.*

As the brother of Sir Humphry Davy, will you allow me to reply to these charges in your pages; I shall endeavour to be as brief as the subject will permit.

First, of the breach of promise.—Mr. Babbage's statement is the following:—"In 1826 one of the Secretaryships of the Royal Society became vacant. Dr. Wollaston, and several other of the leading members of the Society and of the Council wished that I should be appointed. This would have been the more agreeable to me, because my early friend Herschel was at that time the senior Secretary.

"This arrangement was agreed to by Sir Humphry Davy, and I left town with the full assurance that I was to have the appointment. In the mean time Sir Humphry Davy summoned a Council at an unusual hour—eight o'clock in the evening—for a special purpose, some arrangements of the Treasurer's accounts.

"After the business relating to the Treasurer was got through, Sir Humphry Davy observed that there was a Secretaryship vacant, and he proposed to fill it up. Dr. Wollaston then asked Sir H. Davy if he claimed the nomination as a right of the President, to which Sir H. Davy replied that he did, and then nominated Mr. Children. The President, as President, had no such right, and even if he had possessed it, he had promised Mr. Herschel that I should be his colleague. There were upright and eminent men on that Council; yet not one of them had the moral courage to oppose the President's

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* *Op. cit.* pp. 187–189.

† Reflections on the Decline of Science in England.

dictation, or afterwards set it aside on the ground of its irregularity"*.

From whom, I would ask, and did ask Mr. Babbage, had he this information? In a letter which I have received from him of the 17th of September in reply to one from me, he writes, "With respect to what took place at that Council, the late Dr. Fitton, who was present, gave me the account I have stated." Mr. Babbage added, "Not long before his death, Dr. Fitton *gave* me several MSS. and other papers which he thought might be interesting to my family. He then again related the account of the Council which I have given in my work."

On this I remark, that Dr. Fitton was not then on the Council†, and that I have been assured by one of the three surviving members of that Council‡, that no such words as those atttibuted to Dr. Wollaston were spoken, and that there was no discussion of any kind on the occasion.

Mr. Herschel, now Sir John Herschel, was one of those present; and I would ask, why has not Mr. Babbage adduced him as his informant? From what I have learnt he would not support Mr. Babbage in the statement, nor indeed in the statement that Sir Humphry Davy had promised him that Mr. Babbage should be his colleague.

I would further remark that, inasmuch as Dr. Wollaston and Sir H. Davy had been so long Secretaries together, it seems highly improbable that the former would question the latter in the manner asserted. It is true that the President has not the *right* of naming for a Secretaryship; but it is equally true that it had become the usage: Mr. Babbage himself was fully aware of this; for at p. 140 of his 'Reflections on the Decline of Science in England,' published in 1830, he states, of the officers and Council of the Society, "the fact is that they are the private nominations by the President;" and, at p. 72, on the authority of the late Mr. Barrow, he affirms, "that it had been the custom for years for the President of the Royal Society to nominate the Council."

In my letter to Mr. Babbage, adverting in conclusion to the affair of the Secretaryship, I wrote, "that you felt aggrieved is certain from what you have stated," adding, "I need hardly remark that too often under that state of feeling, an *animus* is created which tends to misinterpretation. Had you called on the President for an explanation at the time, would it not have been more in accordance with what is just and honourable than to have brought such a charge against him after his death [after an interval of 35 years]? He might have stated reasons, if not reconciling you to your disappointment, yet amply justifying his conduct, or convincing you that you laboured under a mistake as to a promise."

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* *Op. cit.* p. 187.

† In the Council-book, I am informed by the Assistant-Secretary that there is no minute respecting either the nomination of Mr. Babbage or Mr. Children as Secretary at the Meeting in question, that held at 8 P.M., on the 16th November, 1826.

‡ These were Mr. Gompertz, Mr. Herschel, and Mr. South.

I shall now proceed to the more serious charge—the asserted "transfer of between three and four hundred pounds from the funds of the Royal Society into his [Sir H. Davy's] own pocket"*. This charge, according to Mr. Babbage's statement, is founded on the facts (and I do not question their accuracy) that the late Mr. Murray purchased for 500 guineas the copyright of the President's Discourses, which were published at the request of the Council †, and that the Council, at a meeting held on the 31st of December, 1826, "Resolved, that 500 copies of the President's Discourses about to be printed by Mr. Murray be purchased by the Society at the usual trade price." At that meeting the President was in the chair; the members present were Captain Beaufort, Messrs. Brown, Children, Gilbert, and Herschel, Sir E. Home, Captain Kater, Drs. Pearson, Prout, and Young. Mr. Babbage, in commenting on the transaction, exculpates Mr. Murray; the gravamen of the charge he lays on the President and the Council—he for selling the copyright of his Discourses (which he certainly had a right to do) and the Council for ordering the purchase of the copies at a cost of £381 5*s*.

Mr. Babbage states that in the following year, when he was on the Council, he inquired why the "Resolution of Council" above named was passed; and that Dr. Young's reply was, that it was "in order to *induce* Mr. Murray to print the President's speeches."

It would appear from the wording of the resolution, that, previous to its passing, Mr. Murray had undertaken to publish the Discourses; nor does it appear that there was any previous promise? Be that as it may, the Council was responsible for their act.

I have recently applied to Mr. John Murray, the son of the late Mr. Murray, for information on the subject: he has courteously replied, stating his regret that the only information he can give is contained in a schedule which he annexed, specifying the date of the publication and the results—he adding that all his search after letters and copies of letters which may have passed has proved fruitless. From this document, of which the subjoined ‡ is a copy, it would

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* *Op. cit.* p. 189.

† See advertisement to the 'Discourses' by their author.

‡ "Sir H. Davy's Discourses, published by Mr. Murray in January 1827. 500 guineas paid by Mr. Murray to Sir H. Davy for the copyright.

850 copies printed, the cost of which (including the sum paid for the copyright) was £742 4*s*. 5*d*.

		£	*s.*	*d.*
500	copies sold to the Royal Society at the trade price, 15*s*. 3*d*.	381	5	0
25	sent out as presents from Sir H. Davy, according to his list.			
11	to Stationers' Hall.			
152	copies sold at Mr. Murray's Annual sale	92	9	4
71	various, for	49	19	11
91	wasted, producing	2	0	0
	Eventual loss to Mr. Murray	216	10	2
850		£742	4	5"

appear that, if the whole of the impression had been sold at trade price, Mr. Murray would have been a loser, and to no inconsiderable amount, viz. £94 1*s.* 11*d.* May I be allowed to ask what is the inference? Is it not a logical conclusion that the publisher expected that the demand for the 'Discourses' would have been so great as to require more than one edition, so as to remunerate him for his outlay? Nor, it may be presumed, was such an expectation, though not realized, unreasonable, taking into account the demand for a preceding work of the author's, the 'Lectures on Agricultural Chemistry,' which passed through several editions, and for the copyright of which he received £1000, and £50 for every fresh edition, and the demand also for a later and for a posthumous work, 'Salmonia' and 'Consolations in Travel.' Nor, when we further consider how highly the 'Discourses' were approved, should the sum payed for them by a liberal and enterprising publisher, such as was the late Mr. Murray, excite surprise.

It may perhaps be said that the President should have presented the copyright of the 'Discourses' to the Royal Society. Had the Society been in want of funds, there would have been a just reason for making a present to it; but as the Society's funds were ample, such a present was no wise needed; at least, such we must infer was the opinion of the Council. That he was considered free from blame in the transaction, may be inferred from the circumstance that on his resigning the Chair of the Royal Society on account of failing health, a vote of thanks to him, proposed by the Council, of which Council Mr. Babbage was a member, was unanimously agreed to by the Society: the following is a copy of it; the original, now in my possession, is formally written on parchment:—"At a meeting of the Royal Society, held on Thursday the 15th of November, 1827, the President stated from the chair that he was directed by the Council to submit the following resolution to the Society, which was unanimously agreed to—That the regret of the Fellows of the Royal Society be expressed in the strongest terms to their late excellent President, Sir Humphry Davy, Baronet, for the state of health which has unhappily compelled him to relinquish the chair, together with their thanks for the unremitting diligence with which he has at all times endeavoured to promote the interests of science and the welfare of the Royal Society, and for the learned and eloquent discourses with which at each Anniversary during his Presidency, he concluded the business of the year."

I have repressed my feelings in writing thus calmly on such a subject. When I call to mind the little regard my brother had for wealth—that to enrich himself he would never take out a patent, though urged so to do, for the safety lamp, or for the protection of the copper sheathing of ships, at a time it promised to be of the greatest use *—I must confess at least astonishment that, when dead,

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* At that time, in a letter to me, expressing his sanguine expectations of success, after adverting to the fortune ("the immense fortune") he might make if he had chosen to take out a patent for the invention, and that he had determined to give it to his country, he added, "in everything con-

an accusation should have been brought against him equivalent to that of robbery, and that stated to have been committed in the last stage of a life devoted to science with so much honour to himself, and benefit to his country and to mankind.

It may be asked why I did not reply to this last charge at the time it was first made, viz. in 1830, in Mr. Babbage's 'Reflections on the Decline of Science in England and on some of its Causes.' I was then abroad on foreign service, and did not return until the lapse of two years. Then Mr. Babbage's book had almost passed into oblivion: moreover, as the Council of the Royal Society were included in the charge, its members (they so many and able) might be considered the proper persons to reply to it; indeed, even now, my giving it attention is by some friends I have consulted considered unnecessary; and so I might consider it, did I not know that whereever pitch is thrown it adheres, and that the renewal of the charge in a book such as Mr. Babbage's is, which may be referred to hereafter, if passed over in silence, might be supposed to be founded on truth.

I am, Gentlemen,
Your obedient Servant,
John Davy.

Lesketh How, near Ambleside,
October 20, 1864.

ON THE COMPARISON BETWEEN THE ENGLISH AND METRICAL READINGS IN DOUBLE-SCALE BAROMETERS.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the July Number of your Magazine is a paper by my friend Mr. Packe, in which he attributes the larger part of the discrepancy between the barometric pressures corresponding to the French and English boiling-points to the difference between the standard temperatures of the French and English units of length.

I believe this conclusion to be erroneous, and I propose to state, as briefly as possible, my reasons for dissenting from it.

"First," writes Mr. Packe, "as to the discrepance arising from the standard temperatures. That of the English barometer being 30° F. higher than that of the French scale, when the mercurial column is reduced to the freezing-point, the scale of the French barometer is also reduced to the freezing-point, but the scale of the English one is only reduced to the temperature of 62° F.

"*The consequence is that the French barometer, when reduced, will always read higher than the English barometer.*"

The unsoundness of this inference will appear from the following considerations.

By reducing the French barometer we obtain the length of a column of mercury at 0° C., estimated in millimetres, at the standard temperature of 0° C.

By reducing the English barometer we obtain the length of the

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nected with interests [meaning moneyed interests] I am resolved to live and die at least *sans tâche*."—See my Memoirs of his Life, vol. ii. p. 176.

same column of mercury, at the same temperature, estimated in English inches, at the standard temperature of 62° F.

From Guyot's Tables for the conversion of millimetres into English inches, and *vice versâ,* we express millimetres at the standard temperature of 0° C. in terms of English inches at the standard temperature of 62° F.

The consequence is that the reading of the French barometer, when reduced and converted into English inches by Guyot's Tables, ought always to coincide exactly with the reading of the English barometer.

From the following further quotation from Mr. Packe's paper, it is easy to see how he has arrived at a different conclusion:—

"For exact observation, therefore, it is useless to have a barometer marked with a double scale—the French and English: they cannot be made to coincide; *e. g.*

"Let the barometer read 29 inches = 736·59 millims. (temp. 62° F. = 16°·67 C.). In the English scale at 62° (the temperature of the standard) no correction is made for the brass scale. The only correction is for the expansion of the mercury, —·087,

$$\begin{array}{lr} & \text{in.} \\ & 29 \\ - & \cdot 087 \\ \hline \text{reduced} & 28\cdot 913 = 734\cdot 38 \text{ millims.} \end{array}$$

"But in the French scale, the temperature of the standard being 32° F., the correction to be made is for the expansion of the mercury — the expansion of the scale:

$$\begin{array}{lr} & \text{mm.} \\ \text{Expansion of mercury for } 16^{\circ}\cdot 67 \text{ C.} = & 2\cdot 212 \\ \text{Expansion of brass scale for} \quad ,, \quad = & -\ \cdot 231 \\ \hline & 1\cdot 981 \end{array}$$

$$\begin{array}{lr} & \text{mm.} \\ & 736\cdot 59 \\ - & 1\cdot 981 \\ \hline \text{reduced} & 734\cdot 61 = 28\cdot 9224 \text{ inches.''} \end{array}$$

Mr. Packe evidently supposes that in a barometer with a double scale, when the attached thermometer is at 62° F. = 16°·67 C., and the English reading is 29 inches, the metrical reading will be 736·59 millims.

Now, if the metrical scale is properly graduated, this will not be the case. 736·59 millims. is what the reading would be if the metrical scale were at its standard temperature of 0° C. But by hypothesis it is at 62° F.; it has therefore expanded through the space due to an increase of temperature of 30° F., that is, through ·23 millim.

If, then, the English reading be 29 inches, the corresponding metrical reading will be 736·36 millims.

$$\begin{array}{lr} & \text{mm.} \\ & 736\cdot 36 \\ \text{Reduction for } 16^{\circ}\cdot 67 \text{ C.} & 1\cdot 98 \\ \hline \text{Reduced reading} \quad . \quad . & 734\cdot 38 = 28\cdot 913 \text{ inches;} \end{array}$$

precisely the same result that is obtained by the direct reduction of the English reading.

I make the proviso, *if the metrical scale is properly graduated*, as there is reason to fear that this is not the case with many double-scale barometers made in this country.

I am, Gentlemen,
Your obedient Servant,
WILLIAM MATHEWS, Jun.

51 Carpenter Road, Edgbaston,
November 7, 1864.

OBSERVATIONS OF THE SPECTRUM OF JUPITER.
LETTER FROM FATHER SECCHI TO M. DE BEAUMONT.

In my last communication on the spectra of the celestial bodies I gave new observations, which confirmed for the planet Jupiter the existence of special bands due to its atmosphere. I said that I hoped to be able to measure these bands with the greatest precision, and to learn whether or not they coincided with the terrestrial atmospheric bands. I can now communicate the result of these measurements.

To obtain them I constructed specially an excellent astronomic micrometer, with a screw having a thread of four-tenths of a millimetre, the cross wires being replaced by a metal plate with a very fine slit. This single slit is used instead of the graduated scale of the spectroscope which I used last year; it is illuminated in the same manner with light graduated at will; by moving the head of the screw, which is divided into 100 parts, this luminous line may be made to coincide with an obscure band or any given ray; but as its light would efface the obscure ray if it extended throughout the spectrum, its length was limited so that it only encroached upon it to about one-third or one-fourth of the length of the latter. In this way the micrometric line could be placed in continuation of the spectral ray with an astonishing precision.

The solar rays and that of sodium served as the starting-point for the measurements: the latter was introduced at the moment of observation; the others were observed the same day, some time before sunset, with the same micrometer and spectrometer applied to Merz's large refractor. Great care was taken to fix each time the micrometric slit on one of the strongest atmospheric rays, to see if they coincided or not with those of Jupiter. In the interval between the observation of the terrestrial atmosphere and that of Jupiter the instrument was left untouched, although it was subsequently observed that this precaution was superfluous. The atmospheric rays were determined by observing the air near the horizon. I also sometimes made use of the moon, which was a little above the horizon; and then I had the advantage of making one observation on the planet and another on the moon, and then of returning to the planet, by which the control was made more exact.

The various bands are not equally easy to measure, for some are more diffused on one side than on the other; that of the dark red is a little difficult, especially if the air is not very pure. In this case the observations were somewhat multiplied. The following numbers are the result of at least three measurements. Those taken in succession usually agree better with each other than those taken on different evenings; perhaps we have here the same phenomenon as in the case of the double stars.

I shall first give the results obtained for the terrestrial atmosphere, and then those of Jupiter, and I shall refer the position of the bands to the darker line of the band D.

Relative position of the Terrestrial Atmospheric Rays.

	$D-C^6$.	$D-C$.	$D-B$.
	r.	r.	r.
July 16........	1·85	2·85	3·95
20........	..	2·75	3·95
20........	1·70	2·72	4·11
21........	1·76	2·73	3·99
20 (Moon) ..	..	2·03	3·93
Mean....	1·77	2·74	3·98

For Jupiter I shall call D′, γ, β the lines analogous to those of the terrestrial atmosphere D, C, B. The following are the intervals:—

Intervals of the Bands of Jupiter.

	$D'-\gamma$.	$D'-\beta$.
	r.	r.
July 17	1·90	3·70
17	1·90	4·00
19	1·98	3·49
20	1·83	3·79
20	2·02	3·85
21	1·93	3·78
Mean	1·92	3·77

It is seen by these measurements that the bands γ and β of Jupiter do not coincide in position with those of the terrestrial atmosphere, but that the two, C^6 and B, approach to some extent. Their relative distance is also different; for we have

$$C^6-B=2^{r}\cdot21, \text{ while } \gamma-\beta=1^{r}\cdot85.$$

Their difference exceeds all possible limit of error.

Hitherto we have only compared lines with each other as to their relative place; but it is interesting to see if the starting-point is the same—that is, if the line D′ in Jupiter coincides with D in the atmosphere. Several micrometric measurements gave a constant difference between D and D′, this being $0^{r}\cdot 34$ towards the red; but the observation was very difficult, and deserved a careful examination. For this purpose, during two successive evenings the slit was pointed on the line of maximum obscurity in the terrestrial atmospheric band, and it was found to be in the exact prolongation of that of sodium. The micrometer was left untouched until Jupiter appeared. It was observed that the darkest band of Jupiter was outside the slit and the ray D by its own size; so that even the maximum of this band does not coincide with that of the terrestrial atmosphere. If the bands γ and β are referred to those of sodium, we find

$$D-\gamma=2\cdot 26, \quad D-\beta=4\cdot 11.$$

These numbers agree with those of our atmosphere no better than the preceding. The band β is not far removed from B; and it might be said that the difficulty of the measurement would allow for the difference; but it exceeds the probable error of the measurements. The ray C and its terrestrial atmospheric band are quite wanting in Jupiter; and, on the other hand, γ, which is greatly developed, suggests C^6 more than C. I may say that C^6 is very developed in our atmosphere on foggy days.

These results were confirmed, as far as it was possible to rely upon them, by the observations and the figures published last year in the 'Memoirs' of the Observatory, where it might be seen that the system of Jupiter's bands on the red parts differ from the terrestrial band.

Besides these rays and several others, Jupiter at first sight shows another band δ outside D towards the blue, which is analogous to the δ of Brewster, and it would be necessary to proceed to further measurements even for this. But I would not delay the communication of these results to you before the planet is too near the horizon, in order that these results, which are the most striking, may be confirmed by those who possess powerful instruments.

Before finishing, I may reply to a possible objection: If the position of these lines is correct, the solar rays will not be visible upon Jupiter, which does not agree with received ideas. I answer that this does not necessarily follow from my measurements; the solar rays may in fact exist; but being very much spread out and very fine, they would be confounded with the penumbra of these bands themselves; for in fact the position of the solar rays falls so nearly in the neighbourhood of the bands, that for want of light in the planet it would be difficult to see them without a more powerful instrument. In fact, if the moon is viewed with a small diaphragm in front of the object-glass, so as to reduce the light of the spectrum almost as much as has been done for Jupiter, the red rays can only be distinguished with extreme difficulty. I think, therefore, that these observations do not disprove the existence of solar rays in the planet, but show that its atmosphere has a strong absorbing power, and a different one from ours.—*Comptes Rendus*, August 17, 1864.

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SUPPLEMENT TO VOL. XXVIII. FOURTH SERIES.

LXI. *Continuation of a Theory of the Dispersion of Light.* *By* Professor CHALLIS, *M.A., F.R.S., F.R.A.S.*[*]

HAVING been much occupied with preparing for publication a volume of astronomical observations, I have been hindered from proceeding with the researches relating to the Dispersion of Light, which were commenced in a communication to the Number of the Philosophical Magazine for last June. On account of this interruption, it will be expedient, before advancing beyond the point to which the investigation was brought in that communication, to mention briefly the principal steps of the antecedent reasoning. It was assumed that an æthereal wave within a transparent medium obeys the same laws as one without, but is propagated with less velocity in consequence of being acted upon by an extraneous force always opposite in direction, and proportional in magnitude, to the accelerative force due to the æther. The density of the æther was supposed to be the same within the medium as without, and the extraneous retarding force was considered to be the mean effect of the reflexion of the motion of the æther from the atoms of the medium, a vast number of atoms being supposed to be contained in a portion of space the linear dimensions of which are extremely small compared to the breadth (λ) of a wave. The amount of retardation depending not only on the number of atoms in a given space and their arrangement, but also on their *mobility*, it is necessary, in order to calculate the rate of propagation in the medium, to determine the motion of an individual atom as resulting both from the dynamical action of the waves and the molecular forces of the medium. The form of the atom being assumed to be spherical, the determination of the acceleration due to the waves was shown to depend on the solution of a hydrodynamical problem of which this is the enunciation:—A series of waves defined by the equations

$$\mathrm{T}=\kappa a\sigma=m\sin\frac{2\pi}{\lambda}(\kappa at-x+c)$$

[*] Communicated by the Author.

is incident in the direction of the axis of x on a fixed smooth sphere of given radius; it is required to find the condensation at any point of the surface of the sphere at any instant. Accordingly the Article in the June Number consists almost exclusively of investigations preparatory to the solution of this problem. In the first place the solution was attempted by employing only the two usual fundamental equations, and results were arrived at which were subsequently shown to be incompatible with the given conditions of the problem. The third general equation was then taken into account, and another process of solution, involving the principle of the new equation, was then entered upon, but not brought to a conclusion. I propose now to resume the argument, taking it up from the point (in p. 458) at which the second process was commenced.

It is unnecessary to repeat that part of the reasoning (ending in p. 461) by which the equation

$$\kappa^2 a^2 \cdot \frac{d\sigma}{ds} + \frac{d\mathrm{V}}{dt} = 0, \quad . \quad . \quad . \quad . \quad . \quad (\alpha)$$

was obtained. It will suffice to explain here that in this equation σ is the condensation, and V the *total* velocity, at any point whose coordinates are x, y, z at the time t; $d\sigma$ is the increment of condensation, at the given time, along the line of motion passing through that point, corresponding to the increment ds of space along the same line; and $\frac{d\mathrm{V}}{dt}$ is the partial differential coefficient of V with respect to t. The factor $\kappa^2 a^2$, which holds the place of a^2 in the usual mode of investigation, takes account of the *composite* character of the motion. The equation only embraces quantities of the first order, and is exclusive of the action of any extraneous force; in other respects it is perfectly general. For our present purpose we have to apply it to cases in which the motion is symmetrical with respect to an *axis*.

In such cases, if r be the distance of any point from the origin of coordinates, and θ the angle which this line makes with the axis of x, σ and V are functions of r and θ. Also, if U and W be the resolved parts of the velocity respectively along and perpendicular to the same line, $\mathrm{V}^2 = \mathrm{U}^2 + \mathrm{W}^2$. Consequently we have the equations

$$\frac{d\sigma}{ds} = \frac{d\sigma}{dr} \cdot \frac{dr}{ds} + \frac{d\sigma}{rd\theta} \cdot \frac{rd\theta}{ds},$$

$$= \frac{d\sigma}{dr} \cdot \frac{\mathrm{U}}{\mathrm{V}} + \frac{d\sigma}{rd\theta} \cdot \frac{\mathrm{W}}{\mathrm{V}},$$

and

$$\frac{d\mathrm{V}}{dt} = \frac{d\mathrm{U}}{dt} \cdot \frac{\mathrm{U}}{\mathrm{V}} + \frac{d\mathrm{W}}{dt} \cdot \frac{\mathrm{W}}{\mathrm{V}}.$$

Hence, by substituting in the equation (α),

$$\left(\kappa^2 a^2 \cdot \frac{d\sigma}{dr} + \frac{d\mathrm{U}}{dt}\right)\mathrm{U} + \left(\kappa^2 a^2 \cdot \frac{d\sigma}{rd\theta} + \frac{d\mathrm{W}}{dt}\right)\mathrm{W} = 0. \quad . \quad (\beta)$$

At the same time the equation of constancy of mass to the same approximation is

$$\frac{d\sigma}{dt} + \frac{d\mathrm{U}}{dr} + \frac{2\mathrm{U}}{r} + \frac{d\mathrm{W}}{rd\theta} + \frac{\mathrm{W}}{r}\cot\theta = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (\gamma)$$

Now, if it be required to apply the equations (β) and (γ) to cases of motion in which there is no other relation between U and W than that which results from the mutual action of the parts of the fluid, since the analysis is required to determine that relation, we must equate separately to zero the quantities in brackets in the equation (β). In fact, if that equation be multiplied by δt, it will seem to be formed by a combination of D'Alembert's Principle with the Principle of Virtual Velocities; and as by hypothesis there is no given relation between the virtual motions $\mathrm{U}\delta t$ and $\mathrm{W}\delta t$, the factors by which they are multiplied must separately vanish. If U and W be eliminated from the two equations thus obtained and the equation (γ), the result is

$$\frac{1}{\kappa^2 a^2} \cdot \frac{d^2 . \sigma r}{dt^2} = \frac{d^2 . \sigma r}{dr^2} + \frac{1}{r^2}\left(\frac{d^2 . \sigma r}{d\theta^2} + \frac{d . \sigma r}{d\theta}\cot\theta\right). \quad . \quad . \quad (\delta)$$

This equation has been employed to determine the resistance of the air to a vibrating sphere, the centre of the sphere being the origin of coordinates. It appears, however, to be only applicable to the case in which the fluid is confined within fixed boundaries. For it is evident that in that case just as much incompressible fluid must flow in the direction contrary to the motion of the sphere as the volume of the sphere displaces; which is precisely the result to which the analysis conducts when the vibrations are not very rapid, and the movement of the fluid is consequently very nearly the same as if it were incompressible. The analysis also shows that the same equation applies to the case in which the sphere is fixed and the mass of fluid is caused to vibrate bodily. In fact we may pass from the one case to the other by conceiving motion equal and opposite to that of the sphere to be impressed both on the sphere and on the fluid, such impressed velocity not altering the value of σ, and therefore not altering the function that σ is of r, θ, and t, as given by the above equation. In both these cases the velocities U and W are related to each other in a manner depending only on the mutual action of the parts of the fluids, the fixed boundaries being supposed to be so far distant from the sphere as not to affect the law of the motion.

But the conditions of the problem are wholly different if, as I assume to be the case, the sphere vibrates in fluid of unlimited extent, and impresses disturbances upon it which are propagated indefinitely into space. By applying to this case the hydrodynamical principles on which our reasoning is now proceeding, U and W are found to have a known relation to each other, by introducing which into the equations (β) and (γ) and integrating, the motion of the fluid may be completely determined. I do not produce the details of this reasoning here, because they are contained in the former paper (pages 462 and 463), and have been given in several other communications. It results from the solution of this problem that the *relative* velocity of the fluid in contact with the sphere is $\mathrm{T}\sin\theta$, $-\mathrm{T}$ being the velocity of the centre of the sphere, and the angle θ being reckoned from the axis of x on the negative side. This value, however, implies that terms involving the small ratio of the radius of the sphere to λ have been neglected, or that the fluid comports itself as if it were incompressible.

Suppose now the velocity T to be impressed at each instant on the sphere and the fluid, so that the sphere will be reduced to rest. The *actual* velocity of the fluid along its surface will then be $\mathrm{T}\sin\theta$. But on this supposition the fluid through its whole extent moves with the velocity T, and, excepting so far as condensation is produced by impact on the sphere, all its parts have the same density as in the state of rest. To pass from this case to that of *waves* of variable density impinging on the sphere, it is sufficient for a first approximation, considering the small ratio of the radius of the sphere to the breadth of the waves, to substitute $\sigma-\sigma_1$ for σ in the equation which gives the condensation in the first case, σ_1 being the condensation at any instant of the incident wave at the points of incidence. Through the small extent of the hemispherical surface on which the waves impinge, σ_1 may be regarded as uniform, without omitting quantities more significant than those already neglected.

But for the determination of the motion beyond the plane yz passing through the centre of the sphere other considerations are necessary, because, on account of the varying density of the incident wave, the impressed velocity is there altered by the mutual action of the parts of the fluid, and the effect of such action can be ascertained only by the solution of a partial differential equation. The course of the reasoning now requires an investigation of the amount of this modification of the impressed velocity.

Since the equation (β) is perfectly general for motion symmetrical with respect to an axis, and applies to all points of the fluid at all times, it will be true if we pass from one point to another contiguous point by substituting $\theta+\delta\theta$ for θ, while r and t are constant. Hence the equation obtained by differentiating (β) with respect to θ only will be true. We shall thus get

$$\left(\kappa^2a^2\cdot\frac{d^2\sigma}{d\theta dr}+\frac{d^2\mathrm{U}}{d\theta dt}\right)\mathrm{U}+\left(\kappa^2a^2\cdot\frac{d\sigma}{dr}+\frac{d\mathrm{U}}{dt}\right)\frac{d\mathrm{U}}{d\theta}$$
$$+\left(\kappa^2a^2\cdot\frac{d^2\sigma}{rd\theta^2}+\frac{d^2\mathrm{W}}{d\theta dt}\right)\mathrm{W}+\left(\kappa^2a^2\cdot\frac{d\sigma}{rd\theta}+\frac{d\mathrm{W}}{dt}\right)\frac{d\mathrm{W}}{d\theta}=0.$$

For the disturbance of the fluid produced by the first hemispherical surface, the relation between U and W and also that between $\frac{d\mathrm{U}}{d\theta}$ and $\frac{d\mathrm{W}}{d\theta}$ are known, and this equation is consequently not required for determining that part of the motion. But for the remainder of the motion, U and W, as we have argued above, and by consequence $\frac{d\mathrm{U}}{d\theta}$ and $\frac{d\mathrm{W}}{d\theta}$, are related to each other *in part* by modes of fluid action ascertainable only by integration. To find such relation between the latter two quantities, we must assume, according to a principle already applied, that

$$\kappa^2a^2\cdot\frac{d\sigma}{dr}+\frac{d\mathrm{U}}{dt}=0,\ \text{and}\ \kappa^2a^2\cdot\frac{d\sigma}{rd\theta}+\frac{d\mathrm{W}}{dt}=0.$$

Also, as consequences of these equations, we shall have

$$\kappa^2a^2\cdot\frac{d^2\sigma}{d\theta dr}+\frac{d^2\mathrm{U}}{d\theta dt}=0,\ \text{and}\ \kappa^2a^2\cdot\frac{d^2\sigma}{rd\theta^2}+\frac{d^2\mathrm{W}}{d\theta dt}=0.$$

The foregoing equation is therefore satisfied on this assumption. But it is particularly to be remarked that this process implies that the relation between U and W does not depend *wholly* on the mutual action of the parts of the fluid. For if that were so, the two latter equations would not be required, and we should have the kind of motion which was previously shown to be inconsistent with the conditions of the present problem. Consequently the four equations will serve to determine only those parts of U and W of which the relation is not otherwise assignable; and it is clear that, since the original equations are linear with constant coefficients, this portion of the motion may be determined independently of the rest.

After eliminating $\frac{d\mathrm{U}}{dt}$, $\frac{d\mathrm{W}}{dt}$, $\frac{d^2\mathrm{U}}{d\theta dt}$ and $\frac{d^2\mathrm{W}}{d\theta dt}$ by means of the four equations above and the equation (γ), and substituting q for $\frac{d\sigma}{d\theta}$, the result is

$$\frac{1}{\kappa^2a^2}\cdot\frac{d^2\,.\,qr}{dt^2}=\frac{d^2\,.\,qr}{dr^2}+\frac{1}{r^2}\left(\frac{d^2\,.\,qr}{d\theta^2}+\frac{d\,.\,qr}{d\theta}\cot\theta-\frac{qr}{\sin^2\theta}\right),\quad .\quad .\quad (\epsilon)$$

which, as might have been anticipated, is the equation (δ) differentiated with respect to θ. By a like process we might have obtained the equation resulting from the differentiation of (δ) with respect to r; but the above equation suffices for the present purpose. I proceed now to obtain a particular and exact solution of it.

Let it be assumed that $qr=\phi \sin\theta \cos\theta$, and that ϕ is a function of r and t only. Then, by substituting this value of qr in the equation, the result is

$$\left(\frac{1}{\kappa^2 a^2}\cdot\frac{d^2\phi}{dt^2}-\frac{d^2\phi}{dr^2}+\frac{6\phi}{r^2}\right)\sin\theta\cos\theta=0,$$

which accords with the assumption that ϕ is a function of r and t. This differential equation, which admits of exact integration, gives for ϕ an expression containing arbitrary functions of $r-\kappa at$ and $r+\kappa at$. Excluding the function of $r+\kappa at$, which is inapplicable to the question, it will be found that

$$\frac{d\sigma}{rd\theta}=\left(\frac{f(r-\kappa at)}{r^4}-\frac{f'(r-\kappa at)}{r^3}+\frac{f''(r-\kappa at)}{3r^2}\right)\sin\theta\cos\theta.$$

But if W_1 be the part of W which depends only on the mutual action of the fluid elements, from what is said above the left-hand side of this equation is equal to $-\frac{1}{\kappa^2 a^2}\cdot\frac{dW_1}{dt}$. Hence, by integration,

$$W_1=\kappa a.\left(\frac{f_1(r-\kappa at)}{r^4}-\frac{f(r-\kappa at)}{r^3}+\frac{f'(r-\kappa at)}{3r^2}\right)\sin\theta\cos\theta,$$

no arbitrary function of r and θ being added, because by the conditions of the problem W_1 is a periodic function of the time. The preceding investigation would suffice for finding the corresponding part of U, and the condensation at any point due to the mutual action of the parts of the fluid. But as the sequel of the reasoning requires only the knowledge of the velocity and condensation *at the surface of the sphere*, this inquiry may be omitted. It may also be here remarked that as the form of the function f is determined by the given expression for the velocity T of the incident waves, and as the conditions that $W_1=0$ where $\theta=\frac{\pi}{2}$ and $\theta=\pi$ are satisfied, *all* the conditions of the problem are definitely satisfied by the foregoing value of W_1, which may therefore be considered as the only solution of the equation (ϵ) appropriate to the question.

By supposing r in the general expression for W_1 to be equal to c the radius of the sphere, we obtain the velocity along the surface of the sphere, which I shall henceforth designate by the same symbol W_1. Since f is a periodic function having the same value of λ as T, after the substitution of c for r the second term in the brackets will be of the order $\frac{c}{\lambda}\times$ the first, and the third term of the order $\frac{c^2}{\lambda^2}\times$ the first. Hence the second and third terms are insignificant in comparison with the first, and may be omitted. But whether they are omitted or not, we may put the expression for W_1 under the general form

$$W_1=\left(\alpha T+\beta\frac{dT}{dt}\right)\sin\theta\cos\theta,$$

α and β being unknown constants. We have thus arrived at a formula for the alteration which the impressed velocity along the second hemispherical surface undergoes by fluid action; and as the impressed velocity is T sin θ, the entire expression for the velocity along that surface is

$$T\sin\theta\,(1+\alpha\cos\theta)+\beta\frac{dT}{dt}\sin\theta\cos\theta.$$

I have made use of this expression in the Philosophical Magazine for February 1860 (p. 90), but I had not then obtained it by so complete an investigation as that here given. We have now to determine by means of the velocities along the two hemispherical surfaces the total pressure on the sphere produced by the dynamical action of the waves, the sphere being at first supposed to be fixed.

For the first hemispherical surface

$$-\frac{\kappa^2a^2d\sigma}{cd\theta}=\frac{dW}{dt}=\frac{dT}{dt}\sin\theta.$$

Hence integrating, and determining the arbitrary constant so that $\sigma=\sigma_1=\frac{T}{\kappa\alpha}$ where $\theta=\frac{\pi}{2}$, it will be found that

$$a^2\sigma=a^2\sigma_1+\frac{c}{\kappa^2}\cdot\frac{dT}{dt}\cos\theta.$$

Consequently the whole pressure on the hemisphere, resolved in the direction of the incidence of the waves, is

$$2\pi a^2c^2\int\sigma d\theta\sin\theta\cos\theta\qquad\text{from }\theta=0\text{ to }\theta=\frac{\pi}{2},$$

that is,

$$2\pi c^2.\left\{\frac{a^2\sigma_1}{2}+\frac{c}{3\kappa^2}\cdot\frac{dT}{dt}\right\}.$$

Similarly for the second half of the surface,

$$-\frac{\kappa^2 a^2 d\sigma}{cd\theta}=\frac{d\mathrm{W}}{dt}=\frac{d\mathrm{T}}{dt}\sin\theta\,(1+\alpha\cos\theta)+\beta\frac{d^2\mathrm{T}}{dt^2}\sin\theta\cos\theta.$$

Hence, by integrating from $\theta=\frac{\pi}{2}$ to $\theta=\pi$,

$$a^2\sigma=a^2\sigma_1+\frac{c}{\kappa^2}\cdot\frac{d\mathrm{T}}{dt}\left(\cos\theta+\frac{\alpha}{2}\cos^2\theta\right)+\frac{c\beta}{\kappa^2}\cdot\frac{d^2\mathrm{T}}{dt^2}\cdot\frac{\cos^2\theta}{2};$$

and the whole pressure, resolved as before in the direction of incidence, is

$$2\pi c^2.\left\{-\frac{a^2\sigma_1}{2}+\frac{c}{\kappa^2}\cdot\frac{d\mathrm{T}}{dt}\left(\frac{1}{3}-\frac{\alpha}{8}\right)-\frac{\beta c}{8\kappa^2}\cdot\frac{d^2\mathrm{T}}{dt^2}\right\}.$$

Hence, if the mass of the sphere be Δ × its volume, Δ being an unknown constant, and if the sum of the two pressures above obtained be divided by this mass, the acceleration which the waves tend to produce in the direction of their propagation is

$$\frac{1}{\kappa^2\Delta}\cdot\left\{\frac{d\mathrm{T}}{dt}\left(1-\frac{3\alpha}{16}\right)-\frac{3\beta}{16}\cdot\frac{d^2\mathrm{T}}{dt^2}\right\}.$$

Passing now to the case of a moveable sphere, it is allowable, on the principle of the coexistence of small vibrations, to determine the dynamical effect of a given series of waves by considering, apart from any other motion the sphere may have, that which the waves would produce by themselves. But it is evident that the waves are effective only in proportion to the *relative* velocity of the fluid and the sphere; that is, x being the coordinate of the centre of the sphere, and regarded positive in the direction of propagation, the effect is proportional to

$$\mathrm{T}-\frac{dx}{dt},\ \text{or}\ m\sin\frac{2\pi}{\lambda}(bt+c)-\frac{dx}{dt},$$

the term $-x$ in the expression for T being omitted, because the excursions of the sphere are supposed to be extremely small about a mean position, which may be taken for the origin of x. Now $\frac{dx}{dt}$, since it depends only on T, will be a periodic function having the same period as T, and consequently $\mathrm{T}-\frac{dx}{dt}$ will be similarly periodic. Hence, calling this quantity T′, if we substitute T′ for T in the foregoing expression for the accelerative force the sphere being fixed, we shall have the acceleration of the sphere in motion, so far as it is due to the action of the waves.

Again, as I have argued in the article "On Double Refraction" in the Philosophical Magazine for December 1863 (p. 472), the displacement of any atom of the medium in which the light is propagated will necessarily bring into play the molecular forces of the medium. Having found that the reasoning on this point in the article just cited requires correction, I propose now to determine again the acceleration of the atom due to the elasticity of the medium. Suppose that by the action of the æthereal waves the mean interval between consecutive atoms, estimated in the direction of propagation, is *diminished* by a quantity (ϵ) extremely small compared to that interval. Then since the resulting molecular action is proportional to the *relative* displacement of the atoms, the acceleration of an atom due to this cause is $-e^2 \cdot \frac{d\epsilon}{dx}$, ϵ being considered to be a function of x, and e^2 being an unknown constant. But since the movements of the atoms are determined by the action of the waves, it follows that these movements and the values of ϵ are propagated through the medium with the velocity (b) of the propagation of the waves. Hence, just as in any case of uniform propagation,

$$v = b\epsilon = f(x - bt),$$

v being the velocity of any atom. Consequently

$$-e^2 \cdot \frac{d\epsilon}{dx} = \frac{e^2}{b^2} \cdot \frac{dv}{dt} = \frac{e^2}{b^2} \cdot \frac{d^2x}{dt^2}, \text{ very nearly.}$$

It should be remarked here that e^2 may be regarded as a measure of the force by which an atom displaced relatively to surrounding atoms tends to return to a position of *relative* equilibrium. On account of the small movements with which we are concerned, which do not sensibly affect the density of the medium, this force must be very nearly the same as that by which a *single* atom displaced tends to return to its position of absolute equilibrium; so that e^2 may be taken as the measure of molecular elasticity in the given direction of propagation. In the former investigation this quantity was incorrectly stated to be a measure of elasticity as inferred for a *continuous* body from a given relation between its pressure and density, which is dependent on the other measure, but not identical with it. It is possible that e^2, as resulting from the immediate action of molecular forces, may be comparable in magnitude with b^2.

The investigation has now conducted to the following equation for determining the motion of the atom:

$$\frac{d^2x}{dt^2} = \frac{1}{\kappa^2\Delta} \cdot \left\{\frac{d\mathrm{T}'}{dt}\left(1 - \frac{3\alpha}{16}\right) - \frac{3\beta}{16} \cdot \frac{d^2\mathrm{T}'}{dt^2}\right\} + \frac{e^2}{b^2} \cdot \frac{d^2x}{dt^2}.$$

A first integration of this equation gives

$$\frac{dx}{dt} = \frac{b^2}{\kappa^2\Delta(b^2 - e^2)} \cdot \left\{\mathrm{T}'\left(1 - \frac{3\alpha}{16}\right) - \frac{3\beta}{16} \cdot \frac{d\mathrm{T}'}{dt}\right\},$$

no arbitrary constant being added, because we have to consider no other motion than that which the waves originate, which, by hypothesis, is wholly periodic. At the same time we have

$$\frac{dx}{dt}=m\sin\frac{2\pi}{\lambda}(bt+c)-\mathrm{T}'.$$

Hence the last equation may be put under this form,

$$\frac{d\mathrm{T}'}{dt}+\mathrm{QT}'=\mathrm{MQ}\sin\frac{2\pi}{\lambda}(bt+c),$$

in which

$$\mathrm{Q}=-\frac{16}{3\beta}\left(\kappa^2\Delta\left(1-\frac{e^2}{b^2}\right)+1-\frac{3\alpha}{16}\right)\text{ and }\mathrm{M}=m\left(1-\frac{16-3\alpha}{3\mathrm{Q}\beta}\right).$$

Consequently by a second integration, after substituting cot ψ for $\frac{2\pi b}{\lambda\mathrm{Q}}$,

$$\mathrm{T}'=-\mathrm{M}\sin\psi\cos\left(\frac{2\pi}{\lambda}(bt+c)+\psi\right).$$

Now the condition of transparency of the medium requires that T′, the relative velocity of the æther and the atom, should be proportional, or very nearly so, to the velocity of the æther; for in that case the mean retardation of the waves is produced by the atoms in motion nearly in the same manner as by the atoms fixed. This condition is fulfilled if ψ be very nearly equal to $\frac{\pi}{2}$, or if Q be a very large quantity, which is the case if β be very small. We are thus led to conclude that the term containing β as a factor in the general value of W is insignificant when the problem is to determine the dynamical effect of the æthereal waves in producing *vibrations* of the atoms. I have elsewhere shown (Phil. Mag. for November 1859, p. 332) that, in inquiring whether the waves have the effect of producing permanent motions of translation of the atoms, that term has to be taken into account.

Supposing, therefore, that $\psi=\frac{\pi}{2}$, the results obtained, after substituting for Q in the above value of M, are

$$\mathrm{T}'=\mathrm{M}\sin\frac{2\pi}{\lambda}(bt+c),\quad \mathrm{M}=m\cdot\frac{b^2-e^2}{b^2-e^2+\frac{b^2}{\kappa^2\Delta}\left(1-\frac{3\alpha}{16}\right)}.$$

Let μ represent the ratio of the rate of propagation (κa) out of the medium to the rate (b) within the medium. Then by the reasoning I have given in the article "On Double Refraction" (pp. 475, 476), if δ be the density of the medium and $\mu^2-1=\mathrm{H}\delta$, the atoms being supposed fixed, the mobility of the atoms is taken into account by multiplying the right-hand side of this equation by the factor that multiplies *m* in the above expression for M. Thus we have

$$\mu^2-1=\frac{\mathrm{H}\delta(\kappa^2a^2-\mu^2e^2)}{\kappa^2a^2-\mu^2e^2+\frac{a^2}{\Delta}\left(1-\frac{3\alpha}{16}\right)} \quad . \quad . \quad . \quad . \quad (\eta)$$

This equation to the first power of e^2 may be put under the form

$$\mu^2-1=\mathrm{A}-\mathrm{B}e^2,$$

A and B being positive quantities. Hence the equation I have employed in my theory of Double Refraction (Phil. Mag. for December 1863, p. 479) is verified by this new investigation.

In all the foregoing reasoning λ has been supposed to have a given value. To apply the equation (η) in accounting for the phenomenon of Dispersion, it is necessary to discuss the character of the factor α, this being the only quantity in it which can contain λ explicitly. Now it was found that the velocity (W) of the æther along the second half of the spherical atom is given generally by the equation

$$\mathrm{W}=\mathrm{T}\sin\theta+\alpha\mathrm{T}\sin\theta\cos\theta.$$

If the same kind of reasoning were applied to the case of a uniform *stream* encountering the atom, which may be regarded as a case of vibratory motion for which λ is indefinitely great, the second term of the value of W would be found to disappear. (See Phil. Mag. for November 1859, p. 324.) Hence α is a function of λ which decreases as λ increases. In general that term expresses the difference between the effect of a stream and that of a series of vibrations, which difference is owing to the *prevention* of lateral spreading by the state of vibration. I have remarked in the preceding paper on Dispersion (p. 460), that in consequence of waves being compounded of separate vibrations parallel and perpendicular to rectilinear axes, lateral spreading may be counteracted in such manner that the whole motion, direct and transverse, may be included in a cylindrical space of small transverse section. The same cause must operate in a degree to check the lateral spreading by which the portion of the incident waves that passes the atom tends to supply the place of the portion which the atom intercepts. In the paper "On Double Refraction" (p. 468), an expression is given for the function *f* which defines the law of the diminution transversely of the vibrations parallel to a single axis. From this expression it may be inferred that at a given position the diminution for any number of component vibrations, whether or not their axes be parallel, may be denoted by such an expression as

$$\Sigma \cdot \left\{ \frac{A'r^2}{\lambda^2} - \frac{B'r^4}{\lambda^4} + \frac{C'r^6}{\lambda^6} - \&c. \right\},$$

r, the distance from an axis, being different for every different axis. As, on account of the small ratio of the radius of the atom to λ, we are concerned here only with values of r extremely small compared to λ, it may be presumed that the first term of the series is much more considerable than the remainder. Guided by these considerations, I shall now assume that the factor α is equal to $\frac{h}{\lambda^2}$, h being an unknown constant, but necessarily positive, because $\cos\theta$ is negative. This quantity being substituted for α in the equation (η), the relation between μ and λ may be put under the form

$$(\mu^2-1)^2-(\mu^2-1)A+\frac{\mu^2-1}{\lambda^2}B+C=0, \quad . \quad . \quad . \quad . \quad (\theta)$$

in which

$$A=\frac{\kappa^2a^2}{e^2}-1+\frac{a^2}{\Delta e^2}+H\delta, \quad B=\frac{3ha^2}{16\Delta e^2}, \quad C=H\delta\left(\frac{\kappa^2a^2}{e^2}-1\right).$$

These expressions show that A, B, and C are *positive* quantities; but theory alone is incapable of determining their numerical values. I proceed now to test the equation (θ) by experimental data.

For this purpose I have adopted Fraunhofer's values of λ, and, for a first instance, have selected his values of μ for Flint Glass, No. 13. (See arts. 437 and 751 of the "Treatise on Light" in the *Encyclopædia Metropolitana.*) To determine the constants of (θ), three equations were formed by means of the values of μ and λ for the rays B, E, and H, the solution of which gave the results

$$A=9{\cdot}12778, \quad B=1{\cdot}39857, \quad C=11{\cdot}97853.$$

The values of λ for the other rays were then calculated by the formula (θ) from the corresponding values of μ, and compared as follows with observation:—

Ray.	Value of μ.	λ by observation.	λ by calculation.	Excess of calculation.
B	1·62775	2·541	(2·541)	0·000
C	1·62968	2·422	2·425	+0·003
D	1·63504	2·175	2·174	−0·001
E	1·64202	1·945	(1·945)	0·000
F	1·64826	1·794	1·796	+0·002
G	1·66029	1·587	1·592	+0·005
H	1·67106	1·464	(1·464)	0·000

The values of λ in brackets were those used in determining the constants. The adopted unit of λ, which is arbitrary, was chosen for convenience in calculating.

The following results were obtained by a like comparison for oil of cassia, which was selected on account of its great dispersive power. The numbers are taken from Professor Baden Powell's paper in the Transactions of the Royal Society for 1837, Part I. p. 22.

Ray.	Value of μ.	λ by observation.	λ by calculation.	Excess of calculation.
B	1·5885	2·541	(2·541)	0·000
C	1·5918	2·422	2·428	+0·006
D	1·6017	2·175	2·174	−0·001
E	1·6155	1·945	(1·945)	0·000
F	1·6295	1·794	1·791	−0·003
G	1·6607	1·587	1·583	−0·004
H	1·7002	1·464	(1·464)	0·000

The equations for determining the constants gave

$$A=4{\cdot}55574,\quad B=0{\cdot}64905,\quad C=4{\cdot}46624.$$

In the first example, the difference between the first and last values of μ is 0·0433, and that between the first and last values of λ is 1·077. Hence a difference of 0·001 in λ corresponds on the average to a difference of 0·00004 in μ. In the same manner it will be found that in the second example a difference of 0·001 in λ corresponds to 0·00010 in μ. Taking these proportions into account, it will be seen that the accordance between theory and observation is much more exact in the above comparisons than in the corresponding ones made by Professor Powell in the Transactions of the Royal Society for 1835, Part I. p. 252, and in the paper above cited. The accordance might be made still closer by assuming α to be equal to $\frac{h}{\lambda^2}+\frac{k}{\lambda^4}$; but as another constant would then have to be determined, the resulting equation would amount to little more than a formula of interpolation. The above comparisons, however, by showing that the equation (θ), *derived entirely from antecedent physical principles,* is capable of giving results in accordance with experiment, afford evidence both of the truth of the theoretical explanation of dispersion, and also of the principles on which it rests. I had previously obtained other equations, none of which were found on trial to admit of satisfactory comparison with experiment; but at the same time, as the present more complete investigation has shown, they were not strictly deduced from the *à priori* principles.

The applications of my hydrodynamical researches which I have made from time to time in the undulatory theory of light, have now embraced most of the leading phenomena, excepting that hitherto I have not indicated their application to the phenomena of *Diffraction.* The theoretical explanations that have been given of this class of facts rest mainly on an *assumed law of limited lateral divergence,* and on this assumption have been successful. Now, from reasoning contained in this and previous communications, I am justified in inferring that this law is a consequence of the composite character of waves, and of the circumstance that the component vibrations are partly direct and partly transversal. Also the supposition usually made in the mathematical treatment of problems of diffraction, that the waves are compounded of indefinitely small parts each possessing the property of limited lateral divergence, is in accordance with this hydrodynamical theory of the composition of waves. I have never been able to perceive how such a supposition is consistent with a theory of light which regards the æther as composed of discrete atoms. On the whole the results of my researches give reasons for the conclusion that the undulatory theory of light rests legitimately on no other than a hydrodynamical basis.

Cambridge, November 21, 1864.

LXII. *Mineralogical Notes.* *By* Professor N. S. MASKELYNE *and* Dr. VIKTOR VON LANG, *of the British Museum.*

[Continued from p. 150.]

[With a Plate.]

On the Crystalline Form of Malachite. *By* Viktor von Lang.

AFTER I had published my first Note on this subject I came, in the mineral collection of the British Museum, upon several more tolerably well crystallized specimens of malachite, of which the following is a description. These new observations not only confirmed my previous ones, but also led me, as I believe, to a somewhat more accurate determination of the crystallographic elements of malachite, viz.

$$a : b : c = 0{\cdot}7823 : 1 : 0{\cdot}4036.$$

$$ac = 90° \ 3'.$$

They were deduced from the angles

$$110.1\bar{1}0 = 75° \ 56'$$

$$101.100 = 61 \ \ 53$$

$$\bar{1}03.100 = 98 \ \ 43,$$

which are the means of numerous measurements—the first angle being observed on crystals from Siegen*, the two others on crystals from Nijni Tagilsk.

Including the new planes on the crystals I am going to describe, we have for malachite the planes

$$100,\ 010,\ 001,\ 110,\ 101,\ \bar{1}02,\ \bar{1}03,\ ^{1}04,$$
$$\bar{1}24,\ \bar{1}34,\ 123,\ \bar{1}12,\ \bar{3}23,\ \bar{2}21,$$

for which we find the following angles calculated with the above elements:—

	100.	010.	001.	110.	1$\bar{1}$0.
001	88° 57′	90° 0′	0° 0′	89° 10′	89° 10′
110	37 58	52 2	89 10	0 0	75 56
101	61 53	90 0	27 4	68 11	68 11
$\bar{1}$04	96 19	90 0	7 22	94 59	94 59
$\bar{1}$03	98 43	90 0	9 46	96 53	96 53
$\bar{1}$02	103 29	90 0	14 32	100 36	100 36
$\bar{1}$24	96 11	78 39	13 30	87 56	101 54
$\bar{1}$34	96 3	73 15	18 15	84 36	106 32
$\bar{1}$23	98 25	75 8	17 44	87 34	105 52
$\bar{1}$12	103 14	78 54	18 13	93 33	107 23
$\bar{3}$23	114 38	76 11	30 32	101 11	119 11
$\bar{2}$21	128 16	60 27	53 10	100 39	142 20

The following newly observed combinations are, like all crystals of malachite, twinned on the axis (100).

Plate VII. fig. 1.—100, 101, $\bar{2}21$.

The specimen on which this by no means small crystal occurred consists of larger crystals penetrating each other. But this crystal was the only one of which at least one end, formed by the enumerated planes, was not in contact with other crystals. The planes 101 on the top seem to have been produced by cleavage. The planes $\bar{2}21$ are not good, and are striated parallel with their intersection with the prism 110.

The observed angles are

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* These crystals were already described in the previous Note.

$\bar{1}10.\bar{2}21 = 39^\circ$ appr. $37^\circ\ 40'$ calc.
$\bar{2}21.\bar{2}\bar{2}1 = 57$,, 59 6 ,,
$[221.\bar{2}\bar{2}1][\bar{1}10.\bar{1}\bar{1}0] = 44$,, 44 37 ,,
$[\bar{2}21.\bar{1}\bar{2}2][\bar{1}10.0\bar{1}1] = 50$,, 51 6 ,,

The last two angles are angles between edges, and could be measured pretty accurately with the microscope.

Fig. 2.—100, 110, 101, $\bar{1}03$, $\bar{1}23$.

Fig. 3.—100, 010, 110, $\bar{1}23$, $\bar{3}23$.

These forms were observed on small crystals from Medno Roudiansky mine, near Nijni Tagilsk, Ural. They are, with regard to the distinctness of the smaller planes, the best crystals I had for measurement. I found on them the angles

$101.100 = 62^\circ\ 53'$ $61^\circ\ 53'$ calc.
$\bar{1}03.100 = 98\ 48$ 98 43 ,,
$\bar{1}03.101 = 36\ 58$ 36 51 ,,
$\bar{1}03.110 = 96\ 52$ 96 53 ,,
$\bar{1}03(100)\bar{1}03 = 17\ 31$ 17 26 ,,
$\bar{1}23.010 = 74\ 54$ 75 8 ,,
$\bar{1}23.\bar{1}\bar{2}3 = 29\ 33$ 29 44 ,,
$\bar{1}23.110 = 87\ 47$ 87 34 ,,
$\bar{1}23.1\bar{1}0 = 105\ 48$ 105 68 ,,
$32\bar{3}.100 = 66$ appr. 65 22 ,,
$32\bar{3}.3\bar{2}\bar{3} = 27\frac{1}{2}$,, 28 44 ,,
$32\bar{3}.110 = 62$,, 60 49 ,,

Crystals from a specimen from an unknown locality were similar to fig. 2, only the planes 101 were wanting, and the planes $\bar{1}23$ were more developed. The planes reflected the light very well. I observed

$100\ \bar{1}03 = 98^\circ\ 49'$ $98^\circ\ 43'$ calc.
$\bar{1}03\ \bar{1}\bar{2}3 = 30$ appr. 29 44 ,,

The collection of the British Museum contains also specimens from the now exhausted locality at Rheinbreitenbach. But the crystals from this locality, although the largest of all, are not good for measurement, as they penetrate each other very much, and are moreover coated with foreign matter. They seem to be combinations of 110 and $\bar{1}23$, the prism 110 being not much developed, as in fig. 3.

But, induced by the greater size of these crystals, I tried to determine directly the mean coefficient of refraction for this substance. I obtained without great difficulty, out of a part of a crystal, a prism formed by the cleavage-planes* of the two twinned individuals and by an artificial plane in one zone with the two other planes. This artificial plane formed in consequence two angles, the better one of them being 23° 26′. But for the minimum deviation of the ordinary ray through this angle I could only get the approximate value 20° 34′, having neither direct sunlight nor artificial light at my disposal—an essential condition when one has to work with such small prisms. As the prism is parallel to the mean axis of elasticity, one finds from the given data for the mean coefficient of refraction the value

$$\beta=1{\cdot}887.$$

On the Crystalline Form of Gismondine. *By* Viktor von Lang.

Under the name of Gismondine a zeolithic mineral is designated, which in the form of small square pyramids occurs on a basaltic lava in the neighbourhood of Rome. The British Museum contains two specimens of it, one from Valerano, the other from Capo di Bove, both localities near Rome. Some mineralogists were of the opinion that these square pyramids are only twin crystals of Phillipsite, which indeed is found in forms nearly approaching an octahedron; but then the lines of twinning may always be seen on the planes of the octahedron, of which not the least trace could be found on the crystals of the two specimens in the British Museum. Nor by polarized light could I detect in these crystals any twin structure, but I found by the aid of it that the crystals are single individuals which belong to the prismatic system.

A closer inspection indeed of the twinned crystals shows at once that the four planes forming the top of the supposed pyramid never meet in one point; they are, in fact, combinations of a vertical prism (1 1 0) with a horizontal one (1 0 1), fig. 1, Pl. VII. A section perpendicular to the axis *a* became, when turned between two crossed Nicol prisms, dark and bright, showing by this fact that the axis *a* cannot be the axis of a square pyramid; the planes of polarization were for this section parallel with its edges, rendering also impossible the supposition that

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* These two cleavage-planes formed an angle of 55° 28′ (56° 14′ calc.), which could be measured with great accuracy. From this it follows that the twin plane bisects the obtuse angle of the two cleavage-planes. These facts seem to prove that the crystals from Rheinbreitenbach are not twinned on the plane (0 0 1)—an opinion expressed by M. Hessenberg in the last Number of his valuable Mineralogical Notes, with which he favoured me while the above was going to press.

the crystals are prismatic pyramids. By means of a second section perpendicular to the axis c, it was found that the plane of the optic axes is indeed perpendicular to the axis a, and that the character of the mean line (probably the first) coinciding with the axis c is negative. The points of the axes themselves could not be seen, as the angle of the optic axes is too great for this; but another section, a little inclined to the former one, showed one axis, by which one could see that the negative angle of the optic axis is smaller for red than for blue light.

The planes of the crystals are not very good for measuring, but the crystals are cleavable parallel to planes (101), and by means of such cleavage-planes I determined the angles

$$\begin{aligned} 101\cdot\bar{1}01 &= 93^\circ\ 41' \\ 110.101 &= 65\ \ 18 \\ 110.1\bar{1}0 &= \text{——}\quad 90^\circ\ 50' \text{ calc.,} \end{aligned}$$

whence we find

$$a:b:c=1:0{\cdot}9856:0{\cdot}9377.$$

As for the axes of optic elasticity, we found

$$\mathfrak{a} \parallel c \qquad \mathfrak{b} \parallel a \qquad \mathfrak{c} \parallel b;$$

the symbol of the optical orientation becomes

$$\mathfrak{b}\,\mathfrak{c}\,\underline{\mathfrak{a}}.$$

Last summer I had the opportunity of seeing two quite similar specimens in the mineralogical collection of the Berlin University. The accompanying label gave, "Gismondine [variété décrite par Gismondi, qui est la plus rare]: Capo di Bove près de Rome. Monsignore Lavinio de Medice, Speelo, Roma."

On the Crystalline Form of Herschelite. *By* Viktor von Lang.

It has hitherto been supposed that Herschelite crystallizes in the hexagonal system; but the optical investigation of the numerous specimens in the mineralogical collection of the British Museum showed that Herschelite belongs to the prismatic system, and that its hexagonal forms are only produced by the twin association of six individuals. One has only to grind down the crystals a little, as the top plane is always rounded, to see with the polarizing apparatus the internal structure of the crystals as represented in fig. 4, Pl. VII. Each triangular section is doubly refracting, and gives moreover two optic axes, the planes of which bisect the central angle of each section. The angle of the optic axes is very small, the character of the first mean line negative.

In making these observations, one has only to take care that the crystal is not too thin, as, the double refraction being not very considerable, it is then difficult to see the differences of the intensity of the light in turning round the crystal. In consequence of these remarks, the prism-planes of the crystals of Herschelite obtain the symbol 100; and the planes in the zone [100, 001], 001 being the top plane, become planes of horizontal prisms parallel to the shorter diagonal of a prism 110, on the planes of which the crystals are twinned. As none of the planes are very good for measurement, the following crystallographic data of Herschelite can only be considered as approximations.

The observed planes seem to correspond to the symbols

100, 301, 502, 201, 101, 001.

Taking for the elements

$$a : b : c = 1 : 0{\cdot}5774 : 0{\cdot}8576,$$

we calculate for these planes the angles

301.100 = 21° 14′
502.100 = 25 0
201.100 = 30 15
101.100 = 49 23
110.100 = 60 0

The observed combinations are:—

Pl. VII. fig. 1.—001, 502.

On a crystal from Aci Reale, Sicily. One of these crystals gave

$502 . 50\bar{2}$ = 49° 51′ (50° 0′ calc.)

Fig. 2.—100, 301, 101, 001.

From Aci Castello near Aci Reale, Sicily. I found on a crystal,

100.301 = 21° 56′ 21° 14′ calc.
100.101 = 48 30 49 23 „
$101.10\bar{1}$ = 98 46 98 46 „

These crystals, like the foregoing, occur on a sort of lava; they are often associated together in spherules, but are also found single, imbedded in the vesicular mass, from which they can easily be got out. Such crystals, very small but with tolerably good planes, were combinations only of 301 and 001, with an angle

$301 . 30\bar{1}$ = 39½°–42° (42° 28′ calc.).

The top face of these crystals was also tolerably plane, but rough.

Fig. 3.—100, 201, 001.

These crystals, from Cyclops, Catanea, are also interesting on account of their association, coating with Phillipsite a mass of basalt. I observed

$$100.201 = 31\tfrac{1}{2}^\circ \quad (30^\circ\ 15'\ \text{calc.})$$
$$110(100)110 = 59\text{–}61^\circ (60 \quad 0 \quad ,, \quad)$$

The plane 101 seems also to occur on some of these crystals. The plane 001 on one of them showed re-entering angles, which, although not measurable, are also a proof of the twin structure.

A very interesting specimen of Herschelite from Victoria, Australia, was lately given to the British Museum by Mr. Selwyn, the colonial geologist, who found it himself. The crystals, occurring, like those just described, on basalt, are aggregated together in a greater quantity; and although much larger than those from Italy, are still less fit for good measurements, the planes being broken in every direction. They have the form of fig. 1; and I observed on one of the crystals

$$502.50\bar{2} = 50\tfrac{1}{2}^\circ\ (50^\circ\ 0'\ \text{calc.}).$$

The angles of the top plane are very much rounded, so that the edges 502.502 are quite obliterated.

The optical properties of these crystals are exactly the same as those of the Italian specimens.

LXIII. *The Bakerian Lecture.—Contributions to Molecular Physics. Being the Fifth Memoir of Researches on Radiant Heat.* By JOHN TYNDALL, *F.R.S., &c.*

[Continued from p. 458.]

§ VII. *Influence of temperature on the transmission of radiant heat.*

THE power of varying at will the temperature of the platinum spiral renders it peculiarly suitable for the examination of the influence of temperature on the transmission of radiant heat. To obtain sources of different temperatures, Melloni resorted to lamps, to spirals heated to incandescence by the flame of alcohol, to copper laminæ heated by flame, and to the surfaces of vessels containing boiling water. No conclusions regarding temperature can, as will afterwards be shown, be drawn from such experiments; but by means of the platinum spiral we can go through all those changes of temperature, *retaining throughout the same vibrating atoms*, and we can therefore investigate how the alteration of the rate of vibration affects the rate of absorption. The following series of experiments were executed on the 9th of October, with a platinum spiral raised to barely visible redness, and vapours at a tension of 0·5 of an inch.

Table XII.—Radiation of heat through Vapours. Source of heat, platinum spiral barely visible in the dark.

Name of vapour.	Deflection.	Absorption per 100.
Bisulphide of carbon	7°·5	6·5
Bisulphide of carbon	7·45	6·4
Chloroform	10·5	9·1
Chloroform	10·5	9·1
Iodide of methyle	14·5	12·5
Iodide of methyle	14·5	12·5
Iodide of ethyle	24·2	20·9
Iodide of ethyle	24·5	21·1
Benzole	31·0	26·7
Benzole	30·0	25·9
Amylene	37·6	35·6
Amylene	37·8	35·9
Sulphuric ether	41·1	43·4
Sulphuric ether	41·0	43·4
Formic ether	41·7	45·0
Formic ether	41·8	45·3
Acetic ether	43·6	49·8
Acetic ether	43·4	49·3

On the 10th of October the following results were obtained with the same platinum spiral, raised to a white heat:—

Table XIII.—Radiation of heat through Vapours. Source, white-hot platinum spiral.

Bisulphide of carbon	3°·5	2·9
Bisulphide of carbon	3·4	2·8
Chloroform	6·7	5·6
Chloroform	6·7	5·6
Iodide of methyle	9·2	7·7
Iodide of methyle	9·4	7·9
Iodide of ethyle	15·4	13·0
Iodide of ethyle	15·0	12·6
Benzole	19·3	16·6
Benzole	19·0	16·4
Total heat	59·2	100·0
Amylene	27·6	22·6
Amylene	27·7	22·7
Formic ether	30·5	25·0
Formic ether	30·7	25·2
Sulphuric ether	31·4	25·7
Sulphuric ether	31·7	26·0
Acetic ether	33·0	27·0
Acetic ether	33·2	27·3
Total heat	60·0	100·0

With the same spiral, brought still nearer to its point of fusion, the following results were obtained with four of the vapours:—

Table XIV.—Radiation through Vapours. Source, platinum spiral at an intense white heat.

Name of vapour.	Deflection. °	Absorption.
Bisulphide of carbon . .	14·5	2·5
Bisulphide of carbon . .	14·5	2·5
Chloroform	23·0	3·9
Chloroform	23·0	3·9
Formic ether	60·4	21·3
Formic ether	60·5	21·3
Sulphuric ether	62·3	23·6
Sulphuric ether	62·5	23·8
Total heat	82·7	100·0

In the experiments recorded in the foregoing Table, a total heat of 82°·7, or 588 units, was employed; and to test whether the absorption calculated from this high total agreed with the absorptions calculated from a low total, a portion of the current was diverted, the branch passing through the galvanometer producing a deflection of 49°·4. This corresponds to 77 units. The source, it will be observed, is here quite unchanged; the rays are of the same quality, and pass through the tube in the same quantity as before; but in the one case the absorption is calculated from the deflection among the high degrees, and in the other case it is calculated from deflections among the low degrees of the galvanometer.

The experiments were limited to formic and sulphuric ether, with the following results:—

	Deflection. °	Absorption.	Absorption from Table XIV.
Formic ether . .	17·7	23	21·3
Sulphuric ether . .	19·1	24·8	23·7

The agreement is such as to prove that no material error can have crept into the calibration.

Placing the results obtained with the respective sources side by side, the influence of temperature on the transmission comes out in a very decided manner.

TABLE XV.—Absorption of heat by Vapours. Tension 0·5 of an inch.

Name of vapour.	Source, platinum spiral.			
	Barely visible.	Bright red.	White hot.	Near fusion.
Bisulphide of carbon	6·5	4·7	2·9	2·5
Chloroform	9·1	6·3	5·6	3·9
Iodide of methyle	12·5	9·6	7·8	
Iodide of ethyle	21·0	17·7	12·8	
Benzole	26·3	20·6	16·5	
Amylene	35·8	27·5	22·7	
Sulphuric ether	43·4	31·4	25·9	23·7
Formic ether	45·2	31·9	25·1	21·3
Acetic ether	49·6	34·6	27·2	

The gradual augmentation of penetrative power as the temperature is augmented is here very manifest. By raising the spiral from a barely visible heat to an intense white heat, we reduce the absorption, in the cases of bisulphide of carbon and chloroform, to less than one-half. At barely visible redness, moreover, 56·6 and 54·8 per 100 get through sulphuric and formic ether respectively; while, of the intensely white-hot spiral, 76·3 and 78·7 per 100 pass through the same vapours. By augmenting the temperature of solid platinum, we introduce into the radiation waves of shorter period, which, being in discord with the periods of the vapours, get more easily through them.

What becomes of the more slowly recurrent vibrations as the more rapid ones are introduced? Do the latter take the place of the former? This question is answered by experiments made with an opake solution of iodine, and with lampblack. As the temperature of the platinum spiral increases from a dark heat to the most intense white heat, the absolute quantity transmitted through both these bodies steadily augments. But this heat is wholly obscure, for both the solution and the lampblack intercept all the luminous heat. Hence the conclusion that the augmentation of temperature which introduces the shorter waves augments at the same time the amplitude of the longer ones, and hence also the inference that a body like the sun must of necessity include in its radiation waves of the same period as those emitted by obscure bodies.

§ VIII. *Changes of the position of diathermic bodies through changes of temperature of the same source.—Radiation from lampblack compared with that from platinum at the same temperature.*

Running the eye along the numbers which express the absorptions of sulphuric and formic ether in Table XV., we find that, for the lowest heat, the absorption of the latter exceeds that of the former; for a bright red heat they are nearly equal, but the formic still retains a slight predominance; at a white heat, however, the sulphuric slips in advance, and at the heat near fusion its predominance is decided. I have tested this result in various ways, and by multiplied experiments, and placed it beyond doubt. We may at once infer from it that the capacity of the molecule of formic ether to enter into rapid vibration is less than that of sulphuric. By augmenting the temperature of the spiral we produce vibrations of quicker periods, and the more of these that are introduced, the more transparent, in comparison with sulphuric ether, does formic ether become. Thus what I have called its complexity tells upon the vibrating periods of the formic ether; the atom of oxygen which it possesses in excess of sulphuric ether renders it more sluggish as a vibrator. Experiments made with a source of 212° Fahr. establish more decidedly the preponderance of the formic ether for vibrations of slow period.

TABLE XVI.—Radiation through Vapours. Source, Leslie's cube, coated with lampblack. Temperature, 212° Fahr.

Name of vapour.	Absorption.
Bisulphide of carbon	6·4
Iodide of methyle	18·4
Chloroform	19·5
Sulphuric ether	54·8
Formic ether	60·9

For heat issuing from this source, the absorption by formic ether is 6·1 per cent. in excess of that by sulphuric.

“Deeming the result worthy of rigid confirmation, I repeated the experiments, and obtained the following deflections:—

TABLE XVII.

Name of vapour.	Deflections.
Bisulphide of carbon	9°·3
Iodide of methyle	25·0
Chloroform	26·5
Iodide of ethyle	34·0
Benzole	35·5
Amylene	46·8
Sulphuric ether	47·3
Sulphuric ether	47·7
Formic ether	49·7
Formic ether	49·9
Acetic ether	51·4

When the absorptions were calculated from these deflections, the absorption of formic ether was found to be 6·3 per cent. in advance of that of sulphuric.

But in both Tables XVI. and XVII. we notice another case of reversal. In all the experiments with the platinum spiral recorded in Table XV., chloroform showed itself less energetic as an absorber than iodide of methyle; but in Tables XVI. and XVII. chloroform shows itself to be decidedly the more powerful of the two. Cases of this kind have, in my estimation, a peculiar significance, and I therefore take care to verify them. The experiments with all the vapours were therefore repeated, with the following results:—

TABLE XVIII.—Radiation through Vapours. Source, Leslie's cube at 212° Fahr.

Name of vapour.	Deflection.	Absorption.
Bisulphide of carbon .	15°·0	6·6
Iodide of methyle . .	38·3	18·8
Chloroform	40·7	21·6
Iodide of ethyle . .	46·2	29·0
Benzole	50·0	34·5
Amylene	57·8	47·1
Sulphuric ether . .	60·3	54·1
Formic ether . . .	62·1	60·4
Acetic ether . . .	64·3	69·9
Total heat	71·4	100·0

The absorption by formic ether is here also 6·3 per cent. in excess of that effected by sulphuric ether; while, as in the last two Tables, chloroform excels iodide of methyle.

Preserving the quality of the heat unchanged, but reducing its quantity from 71°·4=227 units to 52°·3=86·5 units, the following results were obtained:—

TABLE XIX.

Name of vapour.	Deflection.	Absorption.
Iodide of methyle . .	16°·5	18·3
Chloroform	18·5	20·6
Iodide of ethyle . .	24·4	27·1
Benzole	30·0	33·3
Amylene	38·6	48·6
Sulphuric ether . .	40·3	53·2
Formic ether . . .	42·8	60·0

Placing the figures of Tables XVI., XVIII., and XIX. side by side, we have an opportunity of seeing how results obtained on different days check each other.

Table XX.—Source, blackened cube of boiling water.

Name of vapour.	Absorptions from Table XVI.	Table XVIII.	Table XIX.
Bisulphide of carbon	6·4	6·6	
Iodide of methyle	18·4	18·8	18·3
Chloroform	19·5	21·6	20·6
Iodide of ethyle	——	29·0	27·1
Benzole	——	34·5	33·3
Amylene	——	47·1	48·6
Sulphuric ether	54·8	54·1	53·2
Formic ether	60·9	60·4	60·0
Acetic ether	——	69·9	

Were it essential to my purpose, I should certainly be able to make even the small differences which here show themselves to disappear. But the agreement is such as to place the reliability of the experiments beyond doubt. *It will be seen that, contrary to the results obtained with a white-hot spiral, in all three cases, where a blackened cube of boiling water was the source, chloroform exceeds iodide of methyle, and formic ether exceeds sulphuric in absorbent power.* To confirm the demonstration, I once more resorted to the white-hot spiral, and obtained the following results:—

Table XXI.—Radiation through Vapours. Source, white-hot platinum spiral.

Name of vapour.	Deflection.	Absorption.
Chloroform	9°·8	4·5
Chloroform	9·5	4·5
Iodide of methyle	16·0	7·3
Iodide of methyle	15·8	7·3
Formic ether	42·1	24·2
Formic ether	42·3	24·5
Sulphuric ether	43·6	26·3
Sulphuric ether	43·5	26·2
Total heat	70·9	100·0

Here chloroform retreats once more behind iodide of methyle, and formic ether behind sulphuric.

The positions of sulphuric and formic ether are reversed within the range of the experiments made with the platinum spiral, but this is not the case with the chloroform and the iodide of methyle.

Even when the spiral was at a barely visible heat, the iodide was decidedly the most opake of the two; the same result was obtained with a spiral heated below redness, as proved by the following figures:—

Name of vapour.	Deflection.	Absorption.
Chloroform	8°·5	12·14
Chloroform	8·5	12·14
Iodide of methyle .	10·0	14·28
Iodide of methyle .	10·0	14·28
Total heat	47·3	100·0

Here the iodide is still predominant. Is it, then, a question of *temperature* merely? or is there a special flux emitted by the lampblack, to which chloroform is particularly opake? In other words, is there a special accord between the rates of vibration of lampblack and chloroform? To answer this question I operated thus:—The platinum spiral was heated by only two cells, and the strength of this current was lowered by the introduction of resistance. When decidedly below a red heat, the spiral was plunged into boiling water. Bubbles of steam issued from it, proving that its temperature was above 212° Fahr. By augmenting the resistance its heat was lowered, until it was no longer competent to produce the least ebullition. It was then withdrawn from the water, and employed as a source: the following are the results:—

Table XXII.—Source, platinum spiral at 100° C.

Name of vapour.	Deflection.	Absorption.
Bisulphide of carbon . . .	5°·7	7·03
Chloroform	14·0	16·8
Iodide of methyle . . .	15·3	18·0

No reversal was here obtained. The temperature was then reduced so that the total heat fell from 81 units to 59 units; but not even in this case (when the temperature was considerably below that of boiling water) could the reversal be obtained. The absorptions approach each other, but the iodide has still the advantage of the chloroform.

Table XXIII.—Source, platinum spiral, heated under 100° C.

Name of vapour.	Deflection.	Absorption.
Bisulphide of carbon . .	5°·2	9·2
Chloroform	10·0	17·3
Iodide of methyle . . .	10·8	18·2

It is not, therefore, temperature alone which determines the inversion: the experiments prove that there is a greater synchronism between the vibrating periods of chloroform and lampblack than between those of chloroform and platinum raised to the temperature of the lampblack. It will be seen, however, that as the temperature of the platinum falls, the opacity of the chloroform increases more quickly than that of the iodide: with an intensely white-hot spiral, as shown in Table XXI., the absorption of chloroform is to that of the iodide as 100 : 162, while, with the spiral heated to a temperature of 212° Fahr., the ratio of the absorptions is as 100 : 105.

§ IX. *Radiation from gas-flames through vapours.—Reversals of position.*

We have hitherto occupied ourselves with the radiation from heated solids: I will now pass on to the examination of the radiation from flames. The first experiments were made with a steady jet of gas issuing from a small circular burner, the flame being long and tapering. The top and bottom of the flame were excluded, and its most brilliant portion was chosen as the source. The following results were obtained:—

Table XXIV.—Radiation of heat through Vapours. Source, a highly luminous jet of gas.

Name of vapour.	Deflection.	Absorption.	White-hot spiral.
Bisulphide of carbon . .	8°·9	9·8	2·9
Chloroform	10·9	12·0	5·6
Iodide of methyle . . .	15·4	16·5	7·8
Iodide of ethyle	17·7	19·5	12·8
Benzole	20·0	22·0	16·5
Amylene	27·5	30·2	22·7
Formic ether	31·5	34·6	25·1
Sulphuric ether	32·5	35·7	25·9
Acetic ether	34·2	38·7	27·2
Total heat	53·8	100·0	

It is interesting to compare the heat emitted by the white-hot carbon with that emitted by the white-hot platinum; and to facilitate the comparison, I have placed beside the results in the last Table those recorded in Table XIII. The emission from the flame is thus proved to be far more powerfully absorbed than the emission from the spiral. Doubtless, however, the carbon, in reaching incandescence, passes through lower stages of temperature, and in those stages emits heat more in accord with the vapours. It is also mixed with the vapour of water and carbonic acid, both of which contribute their quota to the total radiation. It is therefore probable that the greater accord between the periods of the flame and those of the vapours is due to the slower periods of the substances which are unavoidably mixed with the body to which the flame mainly owes its light.

The next source of heat employed was the flame of a Bunsen's burner, the temperature of which is known to be very high. The flame was of a pale-blue colour, and emitted a very feeble light. The following results were obtained:—

Table XXV.—Radiation of heat through Vapours. Source, pale-blue flame of Bunsen's burner.

Name of vapour.	Deflection.	Absorption.	From Table XXIV. Luminous jet of gas.
Chloroform . . .	5·0°	6·2	12·0
Bisulphide of carbon	9·0	11·1	9·8
Iodide of ethyle .	11·3	14·0	19·5
Benzole	14·5	17·9	22·0
Amylene	19·6	24·2	30·2
Sulphuric ether . .	25·8	31·9	35·7
Formic ether . .	27·0	33·3	34·6
Acetic ether . .	29·4	36·3	38·7
Total heat . . .	50·6	100·0	100·0

The total heat radiated from the flame of Bunsen's burner is greatly less than that radiated when the incandescent carbon is present in the flame. The moment the air is permitted to mix with the luminous flame, the radiation falls so considerably that the diminution is at once detected, even by the hand or face brought near the flame. Comparing Tables XXIV. and XXV., we see that the radiation from the Bunsen's burner is, on the whole, less powerfully absorbed than that from the luminous gas jet. In some cases, as in that of formic ether, they come very close to each other; in the case of amylene and a few other substances they differ more markedly. But an extremely interesting case of reversal here shows itself. Bisulphide of carbon, instead of being first, stands decidedly below chloroform. With the luminous jet, the absorption of bisulphide of carbon is to that of chloroform as 100 : 122, while with the flame of Bunsen's burner the ratio is 100 : 56; the removal of the carbon from the flame more than doubles the relative transparency of the chloroform. The case is of too much interest to be passed over without verification: here is the result obtained with a different total heat:—

	Deflection.	Absorption.
Chloroform	16°·5	8·4
Chloroform	16·0	8·2
Bisulphide of carbon .	19·0	9·7
Bisulphide of carbon .	19·4	9·9
Total heat	68·4	100·0

And again, with an intermediate total heat,—

	Deflection.	Absorption.
Chloroform	10°·2	8·4
Chloroform	10·0	8·4
Bisulphide of carbon .	12·0	9·8
Bisulphide of carbon .	11·8	9·7
Total heat	60·0	100·0

There is therefore no doubt that, while in the case of a platinum spiral at all temperatures, of a luminous gas-flame, and, more especially, in the case of lampblack heated to 212° Fahr. the absorption of chloroform exceeds that of bisulphide of carbon, for the flame of Bunsen's burner the bisulphide is the more powerful absorber of the two. The absorptive energy of the chloroform, as shown in Table XX., is more than three times that of the bisulphide, while in Table XXV. the action of the bisulphide is nearly twice that of the chloroform. We have here, moreover, another instance of the reversal of formic and sulphuric ether. For the luminous jet the sulphuric ether is decidedly the more opake; for the flame of Bunsen's burner it is excelled in opacity by the formic.

§ X. *Radiation from the flames of hydrogen and carbonic oxide through air and other media.—Influence of period with reference to absorption.*

The main radiating bodies in the flame of a Bunsen's burner are, no doubt, aqueous vapour and carbonic acid. Highly heated nitrogen is also present, which may produce a sensible effect: the unburnt gas, moreover, in proximity with the flame, and warmed by it, may contribute to the radiation, even before it unites with the atmospheric oxygen. But the main source of the radiation is, no doubt, the aqueous vapour and the carbonic acid. I wished to separate these two constituents, and to study them separately. The radiation of aqueous vapour could be obtained from a flame of pure hydrogen, while that of carbonic acid could be obtained from an ignited jet of carbonic oxide. To me the radiation from the hydrogen-flame possessed a peculiar interest; for, notwithstanding the high temperature of such a flame, I thought it likely that the accord between its periods of vibration and those of the cool aqueous vapour of the atmosphere would still be such as to cause the atmospheric vapour to exert a special absorbent power upon the radiation. The following experiments test this surmise:—

Table XXVI.—Radiation through Atmospheric Air. Source, a hydrogen-flame.

	Deflection.	Absorption.
Dry air	0°	0
Undried air	21·5	17·20
Total heat	60·4	100·0

Thus, in a polished tube 4 feet long, the aqueous vapour of our laboratory air absorbed 17 per cent. of the radiation from the hydrogen-flame. A platinum spiral, raised by electricity to a degree of incandescence not greater than that obtainable by plunging a wire into the hydrogen-flame, was used as a source of heat; of its radiation, the undried air of the laboratory absorbed

5·8 per cent.,

or one-third of the quantity absorbed when the flame of hydrogen was employed.

The plunging of a spiral of platinum wire into the flame reduces its temperature; but it at the same time introduces vibrations which are not in accord with those of aqueous vapour: the absorption by ordinary undried air of heat emitted by this composite source amounted to

8·6 per cent.

On humid days the absorption of the rays emitted by a hydrogen-flame exceeds even the above large figure. Employing the same experimental tube and a new burner, the experiments were repeated some days subsequently, with the following result:—

Table XXVII.—Radiation through Air. Source, hydrogen-flame.

	Absorption.
Dry air	0
Undried air	20·3

The undried air here made use of embraced the carbonic acid of the atmosphere; after the foregoing experiments, the air was conducted through a tube containing a solution of caustic potash, in which the carbonic acid was intercepted, while the air charged itself with a little additional moisture. The absorption then observed amounted to

20·3 per cent.

of the entire radiation. The exact agreement of this with the last result is, of course, an accident; the additional humidity of the air derived from the solution of potash happened to compensate for the action of the carbonic acid withdrawn.

The other component of the flame of Bunsen's burner is carbonic acid; and the radiation of this substance is immediately obtained from a flame of carbonic oxide. With the air of the laboratory the following results were obtained:—

TABLE XXVIII.—Radiation through Atmospheric Air. Source, carbonic-oxide flame (very small).

	Deflection.	Absorption.
Dry air	0°	0
Undried air	10·0	16·1

Of the heat emitted by carbonic acid, 16 per cent. was absorbed by the common air of the laboratory. After the air had been passed through sulphuric acid, the aqueous vapour being thus removed while the carbonic acid remained, the absorption was 13·8 per cent.

An india-rubber bag was filled from the lungs; it contained therefore both the aqueous vapour and the carbonic acid of the breath. The air from the bag was conducted through a drying apparatus, the mixed air and carbonic acid being permitted to enter the experimental tube. The following results were obtained:—

TABLE XXIX.—Air from the lungs containing CO^2. Source, carbonic-oxide flame.

Tension in inches.	Deflection.	Absorption.
1	7·2°	12·0
3	15·0	25·0
5	20·0	33·3
30	30·8	50·0

Thus the tube filled with dry air from the lungs intercepted 50 per cent. of the entire radiation from a carbonic-oxide flame. It is quite manifest that we have here a means of testing with surpassing delicacy the amount of carbonic acid emitted under various circumstances by the act of expiration *.

That pure carbonic acid is highly opake to the radiation from the carbonic-oxide flame, is forcibly evidenced by the results recorded in the following Table.

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* [See article by W. F. Barrett "On a Physical Analysis of the Human Breath," at p. 108 of the present volume.—ED.]

Table XXX.—Radiation through dry Carbonic Acid. Source, carbonic-oxide flame.

Tension in inches.	Deflection.	Absorption.
1·0	33°·7	53·0
2·0	37·0	61·7
3·0	38·6	66·9
4·0	39·4	70·0
5·0	40·0	72·3
10·0	41·4	78·7

About four months subsequent to the performance of these experiments they were repeated, using as a source a much smaller flame of carbonic oxide. The absorptions were found somewhat less, but still very high. They follow in the next Table.

Table XXXI.—Radiation through dry Carbonic Acid. Source, small carbonic-oxide flame.

Tension in inches.	Deflection.	Absorption.
1·0	17°·3	48·0
2·0	20·0	55·5
3·0	21·7	60·3
4·0	22·8	65·1
5·0	24·0	68·6
10·0	26·0	74·3

For the rays emanating from the heated solids employed in all my former researches, carbonic acid proved to be one of the most feeble absorbers; but here, when the waves sent into it emanate from molecules of its own substance, its absorbent energy is enormous. The thirtieth of an atmosphere of the gas cuts off half the entire radiation; while at a tension of 4 inches, nearly 70 per cent. of the whole radiation is intercepted.

The energy of olefiant gas, both as an absorbent and a radiant, is well known; for the solid sources of heat just referred to, its power is incomparably greater than that of carbonic acid; but, for the radiation from the carbonic-oxide flame, the power of olefiant gas is feeble when compared with that of carbonic acid. This is proved by the experiments recorded in the following Table.

Table XXXII.—Radiation through dry Olefiant Gas. Source, carbonic-oxide flame.

Tension in inches.	Deflection.	Absorption.
1	17°·0	24·2
2	26·0	37·1
4	33·0	49·1
Total heat .	47·3	100·0

Four months subsequent to the performance of the above experiments, a second series were made with olefiant gas, and the following results obtained :—

Table XXXIII.—Radiation through dry Olefiant Gas. Source, small carbonic-oxide flame.

Tension in inches.	Deflection.	Absorption.	From Table XXXI.
1·0	11°·4	23·2	48·0
2·0	17·0	34·7	55·5
3·0	21·6	44·0	60·3
4·0	24·8	50·6	65·1
5·0	27·0	55·1	68·6
10·0	32·1	65·5	74·3

Beside the absorption by olefiant gas, I have placed that by carbonic acid derived from Table XXXI. The superior power of the acid is most decided in the smaller tensions; at a tension of an inch it is twice that of the olefiant gas. The substances approach each other more closely as the quantity of gas augments. Here, in fact, both of them approach perfect opacity; and as they draw near to this common limit, their absorptions, as a matter of course, approximate.

The temperature of a hydrogen-flame, as calculated by Bunsen, is 3259° C., while that of a carbonic-oxide flame is 3042° C. The foregoing experiments demonstrate that accord subsists between the oscillating periods of these sources and the periods of aqueous vapour and carbonic acid at a temperature of 15° C. The heat of the flame goes to augment the amplitude, and not to quicken the vibration.

Sent through carbonic oxide, the radiation from the carbonic-oxide flame gave the following absorptions :—

Table XXXIV.—Radiation through Carbonic Oxide. Source, carbonic-oxide flame.

Tension in inches.	Deflection.	Absorption.
1	18°·0	29·0
2	27·0	43·5
4	34·0	56·4
10	37·3	65·5

The absorptive energy is here high—greater, indeed, than that of olefiant gas; it falls considerably short, however, of that exhibited by carbonic acid. This result shows us that the main radiant in the flame is its *product* of combustion, and not the carbonic oxide heated prior to combustion.

Wishing to examine the radiation from a flame whose product of combustion is sulphurous acid, through sulphurous acid, I resorted to the flame of bisulphide of carbon. Here, however, we had carbonic acid mixed with the sulphurous acid of the flame. Of the heat radiated by this composite source, the absorption by an atmosphere of sulphurous acid amounted to

60 per cent.

The gas was sent from its generating retort through drying-tubes of sulphuric acid into a glass experimental tube 2·8 feet long. The comparative shortness of the tube, and the mixed character of the radiation, rendered the absorption less than it would have been had a source of pure sulphurous acid and a tube as long as that used in the other experiments been employed.

I subsequently caused the radiation from the carbonic-oxide flame to pass through a few of our vapours, with the following results:—

Table XXXV.—Radiation through Vapours (tension 0·5 inch).
Source, carbonic-oxide flame.

Name of vapour.	Deflection.	Absorption.
Bisulphide of carbon .	5°·5	9·8
Chloroform	6·0	10·7
Formic ether	14·5	25·8
Sulphuric ether	18·0	32·1
Total heat	43·0	100·0

The same vapours were employed to test the radiation from the hydrogen-flame, with the following results:—

Table XXXVI.—Radiation through Vapours (tension 0·5 inch).
Source, hydrogen-flame.

Name of vapour.	Deflection.	Absorption.
Bisulphide of carbon .	8°·8	11·9
Chloroform	9·9	13·4
Sulphuric ether	32·0	42·2
Formic ether	35·0	49·3
Total heat	48·5	100·0

We here find that, in the case of every one of the four vapours, the synchronism with hot aqueous vapour is greater than with hot carbonic acid. The temperature of the hydrogen-flame is higher than that of the carbonic oxide; but the radiation from the more intense source is most copiously absorbed. It has been already proved that, for waves of slow period, formic ether is more absorbent than sulphuric ether; while for waves of rapid period, the sulphuric ether is the more powerful absorber. For the radiation from hot carbonic acid, the absorption of sulphuric ether, as shown in Table XXXV., is between 6 and 7 per cent. in excess of that of formic ether; while for the radiation from hot aqueous vapour, the absorption by formic ether, as shown in Table XXXVI., is 7 per cent. in excess of that by sulphuric. That the periods of aqueous vapour, as compared with those of carbonic acid, are slow, may therefore be inferred from these experiments.

The two following Tables illustrate the action of carbonic acid gas and olefiant gas respectively, on the radiation from a flame of hydrogen:—

Table XXXVII.—Radiation through Carbonic Acid Gas. Source, hydrogen-flame.

Tension in inches.	Deflection.	Absorption.
1	5°·5	7·4
2	9·5	12·8
4	11·0	14·9
30	19·0	25·7
Total heat .	48·5	100·0

Table XXXVIII.—Radiation through Olefiant Gas. Source, hydrogen-flame.

Tension in inches.	Deflection.	Absorption.	From Table XXXVII.
1	12°·0	16·2	7·4
2	18·0	24·3	12·8
4	24·0	32·4	14·9
30	38·5	58·8	25·7
Total heat .	48·5	100·0	100·0

A comparison of the last two columns, one of which is transferred from Table XXXVII., proves the absorption of the rays from a hydrogen-flame by olefiant gas to be about twice that of carbonic acid; while, when the source was a carbonic-oxide flame, the absorption by carbonic acid at small tensions was more than twice that effected by olefiant gas.

§ XI. *Radiation through liquids.—Influence of period.—Conversion of long periods into short ones.*

Water at moderate thickness is a very transparent substance; that is to say, the periods of its molecules are in discord with those of the visible spectrum. It is also highly transparent to the extra-violet rays; so that we may safely infer from the deportment of this substance its incompetence to enter into rapid molecular vibration. When, however, we once quit the visible spectrum for the rays beyond the red, the opacity of the substance begins to show itself: for such rays, indeed, its absorbent power is unequalled. The synchronism of the periods of the water-molecules with those of the extra-red waves is thus demonstrated. I have already proved that undried atmospheric air manifests an extraordinary opacity for the radiation from a hydrogen-flame, and from this deportment I inferred the synchronism of the cold vapour of the air and the hot vapour of the flame. The vibrating-period of a molecule is, no doubt, determined by the elastic forces which separate it from other molecules, and it is worth inquiring how these forces are affected when a change so great as that of the passage of a vapour to a liquid occurs. The fact established in the earlier sections of this paper, that the order of absorption for liquids and their vapours is the same, renders it extremely probable that the period of vibration is not materially affected by the change from vapour to liquid; for, if changed, it would probably be changed in different degrees for the different liquids, and the order of absorption would be thereby disturbed*. The following Table, in which the deportment of our series of liquids towards the radiation from a hydrogen-flame is recorded, will throw additional light upon this question:—

TABLE XXXIX.—Radiation through Liquids. Source, hydrogen-flame. Thickness of liquid layer 0·07 of an inch.

Name of liquid.	Absorption.	Transmission.
Bisulphide of carbon . .	27·7	72·3
Chloroform	49·3	50·7
Iodide of ethyle . . .	75·6	24·4
Benzole	82·3	17·7
Amylene	87·9	12·1
Sulphuric ether	92·6	7·4
Formic ether	93·5	6·5
Acetic ether	93·9	6·1
Water	100·0	0·0

Through a layer of water 9·21 millimetres thick, Melloni found a transmission of 11 per cent. of the heat of a Locatelli lamp. Here we employ a source of higher temperature, and a layer of water only one-fifth of the thickness used by Melloni,

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* The general agreement in point of colour between a liquid and its vapour favours the idea that the period, at all events in the great majority of cases, remains constant when the state of aggregation is changed.

and still we find the whole of the heat intercepted*. A layer of water, 0·07 of an inch in thickness, is sensibly opake to the radiation from a hydrogen-flame. Hence we may infer the coincidence in period between cold water and aqueous vapour heated to a temperature of 3259° C.; and inasmuch as the period of the water-molecules has been proved to be extra-red, the period of the vapour-molecules in the hydrogen-flame must be extra-red also.

Another point of considerable interest may here be adverted to. Professor Stokes has demonstrated that a change of period is possible to those rays which belong to the violet and extra-violet end of the spectrum, the change showing itself by a degradation of the refrangibility. That is to say, vibrations of a rapid period are absorbed, and the absorbing substance has become the source of vibrations of a longer period. Efforts, I believe, have been made to obtain an analogous result at the red end of the spectrum, but hitherto without result; and it has been considered improbable that a change of period can occur which should *raise* the refrangibility of the light or heat. Such a change, I believe, occurs when we plunge a platinum wire into a hydrogen-flame. The platinum is rendered white by the collision of molecules whose periods of oscillation are incompetent to excite vision. There is in this common experiment an actual breaking up of the long periods into short ones—a true rendering of unvisual periods visual. The change of refrangibility differs from that of Professor Stokes, firstly, in its being in the opposite direction—that is, from low to high; and secondly, in the circumstance that the platinum is heated by the collision of the molecules of aqueous vapour, and before their heat has assumed the *radiant form*. But it cannot be doubted that the same effect would be produced by radiant heat of the same period, provided the motion of the ether could be raised to a sufficient intensity. The effect in principle is the same, whether we consider the platinum wire to be struck by a particle of aqueous vapour oscillating at a certain rate, or by a particle of ether oscillating at the same rate. And thus, I imagine, by a chain of rigid reasoning, we arrive at the conclusion that a degree of incandescence, equal to that of the sun itself, might be produced by the impact of waves, of themselves incompetent to excite vision†.

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* From the opacity of water to the radiation from aqueous vapour, we may infer the opacity of aqueous vapour to the radiation from water, and hence conclude that the very act of nocturnal refrigeration which causes the condensation of water on the earth's surface gives to terrestrial radiation that particular character which renders it most liable to be intercepted by the aqueous vapour of the air.

† Some time after this was written I learned that Dr. Akin had previ-

The change of quality produced in the radiation by the introduction of a platinum spiral into a hydrogen-flame is illustrated by a series of experiments, executed for me by my assistant, Mr. Barrett, and inserted subsequently to the presentation of this memoir.

TABLE XXXIX. *a.*—Radiation through Liquids. Sources: 1. Hydrogen-flame; 2. Hydrogen-flame and platinum spiral.

	Transmission.			
	Thickness of liquid 0·04 inch.		Thickness of liquid 0·07 inch.	
Name of liquid.	Flame only.	Flame and spiral.	Flame only.	Flame and spiral.
Bisulphide of carbon	77·7	87·2	70·4	86·0
Chloroform	54·0	72·8	50·7	69·0
Iodide of methyle	31·6	42·4	26·2	36·2
Iodide of ethyle	30·3	36·8	24·2	32·6
Benzole	24·1	32·6	17·9	28·8
Amylene	14·9	25·8	12·4	24·3
Sulphuric ether	13·1	22·6	8·1	22·0
Acetic ether	10·1	18·3	6·6	18·5
Alcohol	9·4	14·7	5·8	12·3
Water	3·2	7·5	2·0	6·4

Here the introduction of the platinum spiral changed the periods of the flame into others more in discord with the periods of the liquid-molecules, and hence the more copious transmission when the spiral was employed. It will be seen that a transmission of 2 per cent. is here obtained through a layer of water 0·07 of an inch in thickness.

Another series of experiments, also executed by my assistant, gave the following results of the radiation of a hydrogen-flame through layers of water of five different thicknesses:—

Radiation through Water. Source, hydrogen-flame.

	Thickness of liquid.				
	0·02 inch.	0·04 inch.	0·07 inch.	0·14 inch.	0·27 inch.
Transmission per 100	5·8	2·8	1·1	0·5	0·0

Wishing to compare the radiation from a flame of ordinary

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ously inferred, from the paucity of luminous and extra-violet rays in the hydrogen-flame, that its periods must be extra-red. And he deduced from this that the heating of a platinum wire in a hydrogen-flame must consist of a change of period. A very interesting communication from Dr. Akin on this and kindred subjects will be found in the 'Reader' for the 26th of September 1863.—April 5th, 1864.

coal-gas with that of our hydrogen-flame, I reduced the former to the dimensions of the latter. The flame thus diminished had a blue base and bright top, and the whole of it was permitted to radiate through our series of liquids. The following results were obtained:—

TABLE XL.—Radiation through Liquids. Source, small gas-flame. Thickness of liquid layer 0·07 of an inch.

Name of liquid.	Deflection.	Absorption.	From Table XXXIX.
Chloroform	28°·7	39·8	49·3
Bisulphide of carbon .	36·0	53·2	27·7
Iodide of ethyle . .	41·7	72·3	75·6
Benzole	43·4	79·4	82·3
Amylene	45·0	86·1	87·9
Sulphuric ether . .	46·6	93·3	92·6
Formic ether . . .	46·6	93·3	93·5
Alcohol	46·8	94·1	
Acetic ether	46·9	94·4	93·9
Water	47·4	97·1	100·0
Total heat	48·0	100·0	

I have placed the results obtained with the hydrogen-flame in the third column of figures. For some of the liquids it will be observed that the absorption of the heat issuing from the small gas-flame is nearly the same as that of the heat issuing from the flame of hydrogen. A very remarkable difference, however, shows itself in the deportment of bisulphide of carbon as compared with that of chloroform. For the small gas-flame chloroform is the most transparent body in the list; it is markedly more transparent than the bisulphide of carbon, while for the hydrogen-flame the bisulphide greatly excels the chloroform in transparency. The large luminous gas-flame previously experimented with differs also from the small one here employed. With the large flame, the absorption by the bisulphide is to that by the chloroform as

$$100 : 121,$$

while with the small flame the absorptions of the same two substances stand to each other in the ratio of

$$100 : 76.$$

Numerous experiments were subsequently made, with a view of testing this result, but in all cases the bisulphide was found more opake than the chloroform to the radiation of the small gas-flame. The same result was obtained when a very small oil-flame was employed; and it came out in a very decided manner when the source of heat was a flame of bisulphide of carbon. *It was found moreover that, whenever two liquids underwent a change of position of this kind, the vapours of the liquids underwent a similar change*; in its finest gradations, the deportment of the liquid was imitated by that of its vapour.

§ XII. *Explanation of certain results of* Melloni *and* M. Knoblauch.

And here we find ourselves in a position to offer solutions of various facts which have hitherto stood as enigmas in researches upon radiant heat. It was for a time generally supposed that the power of heat to penetrate diathermic substances augmented as the temperature of the source of heat became more elevated. Knoblauch contended against this notion, showing that the heat emitted by a platinum wire plunged into an alcohol flame was less absorbed by certain diathermic screens than the heat of the flame itself, and justly arguing that the temperature of the spiral could not be higher than that of the body from which it derived its heat. A plate of glass being introduced between his source and his thermo-electric pile, the deflection of his needle fell from 35° to 19° when the source was the platinum spiral; while, when the source was the flame of alcohol, when the glass was introduced the deflection fell from 35° to 16°, proving that the radiation from the flame was intercepted more powerfully than that from the spiral—showing, in other words, that the heat emanating from the body of highest temperature possessed the least penetrative power. Melloni afterwards corroborated this experiment.

Transparent glass allows the rays of the visible spectrum to pass freely through it; but it is well known to be highly opake to the radiation from obscure sources—in other words, to waves of long period. A plate 2·6 millimetres thick intercepts all the rays from a source of 100° C., and allows only 6 per cent. of the heat emitted by copper raised to 400° C. to pass through it*. Now the products of the combustion of alcohol are aqueous vapour and carbonic acid, whose waves have been proved to be of slow period, and hence of that particular character which are most powerfully intercepted by glass; but by plunging a platinum wire into such a flame, we virtually convert its heat into heat of higher refrangibility; we break up the long periods into shorter ones, and thus establish the discord between the periods of the source and the periods of the diathermic glass, which, as before defined, is the physical cause of the transparency. On purely *à priori* grounds, therefore, we might infer that the introduction of the platinum spiral would augment the penetra-

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* Melloni.

tive power of the heat through the glass. Melloni, with two plates of glass of different thicknesses, found the following transmissions for the flame and the spiral:—

For the flame.	For the platinum.
41·2	52·8
5·7	26·2

The same remarks apply to the transparent selenite examined by Melloni. This substance is highly opake to the extra-red undulations; but the radiation from an alcohol flame is almost wholly extra-red, and hence the opacity of the selenite to this radiation. The introduction of the platinum spiral shortens the periods and augments the transmission. Thus, with two specimens of selenite, of different thicknesses, Melloni found the transmission to be as follows:—

Flame.	Platinum.
4·4	19·5
1·7	3·5

So far the results of Melloni correspond with those of M. Knoblauch; but the Italian philosopher pursues the matter further, and shows that M. Knoblauch's results, though true for the particular substances examined by him, are far from being applicable to diathermic media generally. Melloni shows that in the case of *black* glass and *black* mica, a striking inversion of the effect is observed; that is to say, that through these substances the radiation from the flame is more copiously transmitted than the radiation from the platinum spiral. For two pieces of black glass of different thicknesses, he found the following transmissions:—

From the flame.	From the platinum.
52·6	42·8
29·9	27·1

And for two plates of black mica the following transmissions were found:—

From the flame.	From the platinum.
62·8	52·5
43·3	28·9

These results were left unexplained by Melloni; but the solution is now easy. The black glass and the black mica owe their blackness to the carbon incorporated in them, and the blackness of this substance, as already remarked, proves the accord of its vibrating-periods with those of the visible spectrum. But it has been proved that carbon is in a considerable degree pervious to the waves of long period—that is to say, to such waves as are emitted by a flame of alcohol. The case of the carbon is therefore precisely antithetical to that of the transparent glass—the former transmitting the heat of long period most freely, and the latter transmitting the heat of short period most freely. Hence it follows that the introduction of the platinum wire, by converting the long periods of the flame into short ones, augments the transmission through the transparent glass and selenite, and diminishes it through the black glass and the black mica.

§ XIII. *Radiation of hydrogen-flame through lampblack, iodine, and rock-salt.—Diathermancy of rock-salt examined.*

Lampblack, as already stated, is in accord with the undulations of the visible spectrum; it absorbs them all; but it is partially transparent to the waves of slow period. As, therefore, the waves issuing from a flame of hydrogen have been proved to be of slow period, we may with probability infer that its radiation will penetrate the lampblack. A plate of rock-salt was placed over an oil-lamp until the layer of soot deposited on it was sufficient to intercept the light of the brightest gas-flame. The smoked plate was introduced in the path of the rays from the hydrogen-flame, and its absorption was measured; the plate was then cleansed, and its absorption again determined. The difference of both gave the absorption of the layer of lampblack. The results were as follows:—

Table XLI.

	Deflection.	Absorption.
Smoked rock-salt	44°·2	82·7
Unsmoked plate	15·8	24·0

The difference between these gives us the absorption of the lampblack; it is 58·7 per cent.; and this corresponds to a transmission of

41·3 per cent.

of the radiation from the hydrogen-flame.

Iodine, in a solution sufficiently opake to cut off the light of our most brilliant lamps, transmitted of the heat of the hydrogen-flame

99 per cent.

In experimenting on liquids with heat of slow period, I noticed that the introduction of the empty rock-salt cell caused the needle to move through a much larger arc than when the source was a luminous one. This suggested to me that a greater proportion of the heat of slow period was absorbed by the rock-salt. I made a few experiments to test the diathermancy of the salt, with the following results:—

For the heat of a hydrogen-flame, the transmission through a perfectly transparent plate of rock-salt was

82·3 per cent.

For a spiral of platinum wire heated to whiteness by an electric current, the transmission was

87 per cent.

For the same spiral lowered to bright redness, the transmission was

84·4 per cent.

For the same spiral lowered to moderate redness, the transmission was

83·6 per cent.

Nothing was changed in these experiments but the heat of the spiral; the direction of the rays, and the size of the radiating body, remained throughout the same; still we find a gradually augmenting opacity on the part of the rock-salt as the temperature of the source is lowered. There cannot, I think, be a doubt that MM. De la Provostaye and Desains are right in their conclusion that rock-salt acts differently on different calorific rays, and is not, as Melloni supposed, equally transparent to all. For the heat of the hydrogen-flame it is more opake than for that of the moderately red spiral.

§ XIV. *Connexion between radiation and conduction.*

This memoir ought perhaps to end here; I would, however, ask permission to make a few additional remarks on a subject which was briefly touched upon towards the conclusion of the first of this series of memoirs. I make these remarks with diffidence, for I have reason to know that authorities for whom I entertain the highest respect do not share my views regarding the connexion which subsists between the radiation and conduction of heat.

Let us suppose heat to be communicated to the superficial stratum of the molecules of any body; say, the molecules at the extremity of a metal bar. They vibrate, and the motion communicated by them to the external æther is dispatched in waves through space. The vibrating superficial molecules must also set in motion the æther *within* the body, and a portion of this motion will be transferred to the stratum of molecules next adjacent to the superficial ones, heat as a consequence appearing to penetrate the mass. But irrespective of the æther, the molecules of the body occupy positions which are determined by their attractive and repulsive forces; so that if any one molecule be forcibly moved from its position of equilibrium, it will of necessity disturb its neighbours. In a system of molecules so related, it is manifest that motion could be transmitted independently of the æther which surrounds them. If we could imagine the æther entirely away, the motion that we call heat would still be propagated from molecule to molecule through such a body. *Conduction* would manifest itself, while radiation would be absent through want of a medium.

In matter, however, as we have it, molecular motion is only in part transmitted *immediately* from molecule to molecule, being in part transmitted mediately by the æther. Now the quantity of motion transmitted by the æther to our second stratum of molecules cannot be the *whole* of that which the first or superficial stratum imparted to the æther. The æther must retard and indeed squander the internal molecular motion; and were the medium absent—were the cushion removed which interferes with the direct propagation of motion from molecule to molecule—conduction would be freer than at present; the heat would penetrate further into the mass than when the æther intervenes.

The reasoning just employed leads to the inference that those molecules which experience most resistance from the æther must be the least competent to transfer the motion of heat from one to the other. The *direct* power of communication is enfeebled by the æther, and the motion obtained *indirectly* cannot make good the loss. We are thus led to the conclusion that the best radiators ought to prove themselves the worst conductors.

A broad consideration of the subject shows that the conclusion is in general harmony with observed facts. Organic substances are all exceedingly imperfect conductors of heat, and they are all excellent radiators. The moment we pass from the metals to their compounds we pass from a series of good conductors to bad ones, and from a series of bad radiators to good ones *.

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* And we also pass, as a general rule, from a series of bodies which vibrate in accord with the visible spectrum to a series which vibrate in discord with the spectrum. The lowering of the rate of vibration is a consequence of chemical union. The comparative incompetence of *compound* bodies to oscillate in visual periods has incessantly declared itself in these researches. I would here refer to a most interesting illustration of the same kind, derived from the experiments of MM. De la Provostaye and Desains. These distinguished experimenters were the first to record the important fact that the qualities of heat emitted by bodies at the same temperature may be very unlike. Two experiments illustrate this fact. The first is recorded in the *Comptes Rendus*, vol. xxxiv. p. 951. One half

In the earlier memoirs of MM. De la Provostaye and Desains*, and in that of MM. Wiedemann and Franz, I find the following facts:—The radiative power of platinum is five times that of silver; its conductive power is one-tenth that of silver. Platinum has more than twice the radiative power of gold; it has only one-seventh of the conducting power. Zinc and tin are almost equal as conductors, and they are also nearly equal as radiators. Silver has about six times the conductive power of zinc and tin; it has only one-fourth of their radiative powers. Brass possesses but one-half the radiative energy of platinum; it possesses more than twice its conductivity. Other experiments of MM. De la Provostaye and Desains† confirm those hitherto referred to. Taking the absorbent power, as determined by these excellent experimenters, to express the radiating power, and multiplying their results by a common factor to facilitate comparison with those of MM. Wiedemann and Franz on conduction, we obtain the following Table:—

TABLE XLII.—Comparison of Conduction and Radiation.

Name of metal.	Conduction.	Radiation.
Silver	100	11
Gold	53	27
Brass	24	42
Tin	15	90
Platinum . . .	8	100

We here find that, as the power of conduction diminishes, the power of radiation augments—a result, I think, completely in harmony with that to which a consideration of the molecular

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of a cube was coated with lampblack, and the other half with cinnabar. The cube being filled with oil at a temperature of 173° C., it was found that the emission from the cinnabar was more copiously absorbed by a plate of glass than that from the lampblack. In the second experiment, they found that, while 39 per cent. of the radiation from a bright surface of platinum was transmitted by a plate of glass, only 29 per cent. of the radiation from the opposite surface of the same plate, which was coated with borate of lead, was transmitted. These results are quite in harmony with the views which I have ventured to enunciate. We may infer from them that the heat emitted by the respective *compounds*—the cinnabar and the borate of lead—is of slower period than that emitted by the *elements*; for experiment proves that as the periods are quickened the glass becomes more transparent. At a temperature of 100° C., moreover, the emission from borate of lead was found equal to that from lampblack (*Comptes Rendus*, vol. xxxviii. p. 442), while at a temperature of 550° C. it had only three-fourths of the emissive power of the lampblack. With reference to the theoretic views which these researches are intended to foreshadow, the results of MM. De la Provostaye and Desains are of the highest interest.

* *Comptes Rendus*, 1846, vol. xxii. p. 1139.

† *Annales de Chimie*, 1850, vol. xxx. p. 442.

mechanism leads us. There is but one serious exception known to me to the law here indicated; this is copper, which MM. De la Provostaye and Desains place higher than gold as a radiator, though it is also higher as a conductor. When, however, the immense change in radiative power which the slightest film of oxide can produce, and the liability of heated copper to contract such a film, are taken into account, the apparent exception will not have too much weight ascribed to it. I have had a cube of brass coated electrolytically with copper, silver, and gold; and, of all its faces, that coated with copper has the least emissive power. This is probably due to some slight impurity contracted by the silver. What we know of the deportment of minerals also illustrates the law. Rock-salt I find to be a far better conductor than glass, while MM. De la Provostaye and Desains find the relative emissive powers of the two substances to be as 17 to 6: the radiant power of the salt is little more than one-third that of the glass. So also with regard to alum: as a conductor it is immensely behind rock-salt; as a radiator it is immensely in advance of it.

Royal Institution, March 1864.

LXIV. *On Gun-cotton, with reference to the New Methods of* General Baron von Lenk *for preparing and employing this Substance. By* M. Pelouze, *Member of the Institute, and* M. Maurey, *Commissioner for Gunpowder*[*].

I. *Preparation of Gun-cotton in France and in Austria.*

A SHORT time after M. Schönbein's discovery of this new explosive substance, its manufacture on a large scale was commenced at the powder-mills of Bouchet. This establishment, from 1847 to 1848, yielded about 5000 kilogs. for the numerous experiments made in France with a view to substitute this substance for gunpowder in mines and in fire-arms.

The experiments made in Austria with the same object appear to reach no further back than the year 1851; but they have been pursued for a longer time than in other countries, thanks to the perseverance of General Baron von Lenk, who was on the first German Commission at Mainz, and who has continued to occupy himself with the question. Up to 1862 the manufacture of Austrian gun-cotton remained a mystery. "It is," says Commandant d'Andlau, writing from Vienna on the 15th November 1861, "a secret which time alone will reveal." No stranger

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[*] Translated from the *Annales de Chimie et de Physique* for October 1864, by Dr. E. Atkinson, Royal Military College, Sandhurst.

was admitted into the factory at Hirtenberg, where General von Lenk's method was carried out. The Commandant d'Andlau added that, after most satisfactory trials, the Emperor of Austria had decided on the adoption of a new material for the employment of gun-cotton in all the field artillery.

Two main objections have in France hindered the replacement of gunpowder by gun-cotton: one is based on its bursting action on the fire-arms, the other on the accidental decompositions and spontaneous explosions which have been noticed, first in France and then abroad. We meet with these objections in the documents communicated by General von Lenk, which, although edited favourably towards his views, show, in combating them, contrary opinions in the body of the Committee of the Austrian Artillery. These opinions appear latterly to have acquired such an ascendency, that the idea of having a form of artillery specially created for the use of gun-cotton has been given up. If the manufacture of this substance in Austria is not yet abandoned, it is considerably reduced, especially since an explosion in July 1862.

The cause of this accident, like the explosions at Vincennes and at Bouchet fourteen years before, could only be a spontaneous explosion. It would be superfluous to revert to facts confirmed in France; but we may quote the following passage from an official report relative to the Austrian explosion:—"The deposition which was made on the 31st July 1862 relative to the explosion which took place the night before in the magazine No. 9 near Simmering, supposes, as the cause could not be ascertained, that this explosion was due to the spontaneous decomposition of gun-cotton."

We have not been able to obtain from General von Lenk a copy of this deposition. In answer to our inquiries, he stated that as the inquest did not ascertain the cause of the accident, it might as well have been due to gunpowder as to gun-cotton. We cannot admit this, since for centuries no case of spontaneous combustion has been met with either in magazines for powder for ammunition, or in those for sporting or mining powder, while gun-cotton, which is still on its trial, has furnished numerous cases in laboratories under the very eyes of chemists, and in magazines where the explosion could not be otherwise explained. Regarding this, we may observe that one of the documents from Austria compares with these spontaneous explosions the explosions which may be produced while powder is being made. This comparison does not hold good. Explosions simply due to accidents in manufacture, such as a blow, a piece of grit, imprudence of the workmen, or a disturbance of the mechanism, cannot be compared with those produced subsequently to the manufacture by reactions between the elements of the compound.

General von Lenk does not deny the reactions which may cause the inflammation of the gun-cotton when they disengage a sufficient heat; but he thinks they may be avoided by taking the precautions which he has recently made known.

His method depends on the same reactions as those used in the powder works at Bouchet seventeen years ago, and which are described in a memoir of the 17th of February 1849. The Austrian, like the French gun-cotton, is a compound produced by the immersion of cotton in a mixture of nitric and sulphuric acids. Their proportions may be varied within tolerably wide limits without modifying the quality of the product. Yet as the most successful the author of the memoir of 1849 mentions a mixture of 3 volumes of nitric acid with 7 of sulphuric acid, equal in weight to 1 of nitric to 2·86 of sulphuric acid. This is almost the ratio of 1 to 3 adopted by General von Lenk. At Hirtenberg each acid is admitted into the mixing-vessel by a small orifice, in order to moderate the increase of temperature. At Bouchet, where this precaution was not taken, an increase of about 20° was observed; but the mixture was prepared sufficiently long beforehand to allow it to sink to the temperature of the surrounding air before the immersion of the cotton.

Moreover differences of this kind could have no influence on the qualities of gun-cotton. We shall describe those to which General von Lenk attaches most importance. According to him, the Bouchet method, in which 200 grammes of cotton are immersed in a litre of the mixture, would not give the same product as that obtained by working with a much larger proportion of the mixture and by means of a special apparatus. This apparatus is a rectangular trough, divided longitudinally into three compartments, and kept at the temperature of a current of water which circulates between double sides. The first is a reservoir which supplies the second with the mixture, so as to have constantly a bath of 30 kilogs. for 100 grammes of cotton. The cotton is thus immersed in 300 times its weight of acids. It is stirred, and as soon as it appears completely soaked with acid, which only requires about a minute, it is placed on a small strainer over the bath and subjected to a regulated pressure, so as always to leave the same weight of acids. With a little skill 1·150 kilog. is obtained as the weight of the pressed mass; hence 100 grammes of cotton take 1·050 kilog. from the bath. These operations are constantly repeated on the same bath, replacing each time by 1·050 kilog. of the new mixture that which the cotton has removed.

The cotton is transferred from the strainer to the third compartment; then, when it is in sufficient quantity to fill this almost entirely, it is withdrawn and placed in vessels, where it remains forty-eight hours. These vessels are surrounded by water, so that the temperature never experiences such an elevation as to produce decomposition.

The contents of these vessels are placed in a machine, the rotation of which expels in a few minutes three-fourths of the non-combined acid. These weakened acids are not further used in the manufactory; they are supplied to a contractor, who gives in return concentrated sulphuric acid.

To remove the remainder of the acids, the cotton is washed by agitating it in a current of water, and leaving it immersed for six weeks. At the expiration of this time the cotton is again dried by rotation, then soaked in a boiling solution of carbonate of potass of 2° Baumé, and treated with pure water until there is no alkaline reaction.

Finally, after a third and last drying by rotation, the cotton is dried in the air when the weather is suitable, otherwise in an oven whose temperature does not exceed 20°.

This is General von Lenk's method.

At Bouchet the same operations were effected in the same order, but with the following differences:—(1) in the proportion of the cotton to the acids, as explained above; (2) in the duration of the impregnation, which was one hour instead of forty-eight; (3) in that of the washing in the current of water (an hour or an hour and a half instead of six weeks); (4) in the machine for removing the acids and the water, which were presses instead of rotating machines; (5) in the mode of neutralizing the last traces of acids; with this view a cold ley of ashes was used, in which the cotton was immersed for twenty-four hours, while General von Lenk boils it for a few minutes in a solution of carbonate of potass.

We may here reply to an assertion in a German Report, with an English translation of which General Lenk has furnished us. It is there pretended that the *French cotton* retained, after washing, so much free acid, that a layer of wood ashes was neutralized by contact with it, and became acid after long usage. A cotton which, like that manufactured at Bouchet, had remained for twenty-hours in an alkaline ley could not produce such a result.

The use of rotators has the advantage over presses of not spoiling the fibres of cotton. A better-looking preparation may thus be obtained, but this modification has no influence on the chemical composition.

We shall not discuss the other differences between the method of Bouchet and that of Hirtenberg. The most certain means of ascertaining the influence which they may exert is to compare their respective products. This we have done within the last three months, with the cooperation of M. Faucher, Assistant Commissioner of Gunpowder, and of M. Chapoteaut, assistant to one of us.

Before describing our comparative experiments, we must mention a last modification which General von Lenk has introduced into the manufacture of Austrian gun-cotton.

It is regarding the use of soluble glass, with a view to *close the fibres of cotton by the precipitation of silicate to retard the disengagement of gas, and then to eliminate the traces of acid which might be there.* (The words in italics are extracted from a note of General von Lenk.)

This preparation is applied by arranging the gun-cotton in a rotatory apparatus, into which leads a pipe pouring upon it solution of glass of 12° Baumé. The gun-cotton thus treated is dried and left for a sufficiently long time to allow the carbonic acid of the atmosphere to unite with the soda of the glass, which causes the precipitation of silica. The carbonate of soda being removed by washing, the insoluble silicate remains as a sort of sheath, adhering to the fibres of the cotton, which thus increases about 2 per cent. in weight.

Here is a difference between the two methods; soluble glass has never been used in France; but we shall show in describing our experiments that this modification has not the importance ascribed to it by General von Lenk.

Our experiments have been made upon three series of pyroxyles.

In the first we class the large number of specimens which have been prepared and examined in the laboratory, principally with a view to determine the quantity of pyroxyle produced by a given weight of cotton, by varying the quantity of the mixture, its proportion, and the duration of the impregnation.

The second series comprises three specimens of the Lenk manufacture: one which General Lenk has presented as a specimen of the Hirtenberg manufacture; another which came from London, the product of a private manufactory in England on the model of that of Hirtenberg; the third, which has been prepared under our own eyes, by the method of General von Lenk.

We may observe that this third specimen was immersed in a current of water for four days instead of for six weeks, and silicate of soda was not used. In our view an immersion of four days was sufficient to remove all trace of acids. The washing with water was followed by a washing with potash; we even think that it might have been abridged without the least inconvenience. As regards the silicate of soda, it would have introduced an uncertainty into the determination of the atomic formula of gun-cotton.

The third series comprises three specimens made by the Bouchet method: one kept from the old manufacture of 1847, and two others made recently. For one of these latter the Lenk mixture was used (1 of nitric to 3 of sulphuric acid), and for the other a mixture of 1 volume of nitric and 2 volumes of sulphuric acid; which is equal to 1 part by weight of the former to 2·46 of the second acid. This proportion was designated at Bouchet by the name of *unequal volumes*.

II. *Quantity of Gun-cotton yielded by a given weight of cellulose.*

Disregarding the few thousandths of foreign matters, purified cotton is cellulose, $C^{12}H^{10}O^{10}$, or $C^{24}H^{20}O^{20}$. In a German Report signed by MM. Redtenbacher, Schrötter, and Schneider, the following formula is assigned to gun-cotton,

$$C^{12}H^{7}O^{7}, 3NO^{5}, \text{ or } C^{12}H^{7}(NO^{4})^{3}O^{10},$$

which corresponds to the following composition:—

Carbon	24·24
Hydrogen	2·36
Oxygen	59·26
Nitrogen	14·14
	100·00

The equation of the reaction may be stated in two ways:—

1. By assuming that, in contact with the mixture of nitric and sulphuric acids, the cotton loses water, which is replaced by the first of these acids,

$$C^{12}H^{10}O^{10}+3NO^{5}=C^{12}H^{7}O^{7}3NO^{5}+3HO.$$

2. By supposing that the hydrogen of the cellulose is replaced by an equal number of equivalents of hyponitrous acid,

$$C^{12}H^{10}O^{10}+3NO^{5}=C^{12}H^{7}(NO^{4})^{3}O^{10}+3HO.$$

According to this, 100 parts of cotton ought to give 183 of gun-cotton. But by varying in more than 100 experiments the proportions of the bodies concerned, we could never obtain more than 178 parts.

The German Report is silent regarding the yield, which, in our opinion, is the most solid basis for the composition of gun-cotton. We do not mean to say thereby that the exact determination of the quantity of gun-cotton produced renders useless the elementary analysis of the latter; but it is necessary that the analysis agree with the quantity of the product.

Our experiments on the yield were made with cotton of good quality, which had been previously washed in a boiling solution of carbonate of soda or of soap, and freed as far as possible from all foreign substances, and particularly from fragments of cotton-grains. Before using, it was carefully dried in a Gay-Lussac's stove at a temperature between 100° and 115°.

The sulphuric acid marked 66° on Baumé's areometer. The nitric acid had the density 1·500 at 9°; it was slightly nitrous, and of a yellow colour. The relative proportions of sulphuric and nitric acids were varied so as to present (1) the composition of the Lenk mixture; (2) that of the Bouchet unequal volumes; (3) various proportions intermediate between 2 and 3 of sulphuric for 1 of nitric acid.

The proportions of the mixture to the cotton were also varied, so as to give that formerly used at Bouchet, that indicated by General Lenk, and various proportions increasing to a limit where the weight of the acid was 500 times that of the cotton.

Lastly, the time during which the cotton was immersed in the acids varied from one hour to sixty-six. In all these experiments the yields varied within small limits without exceeding 178 for 100 of cotton.

If weak acids are taken, or concentrated ones in which sulphuric acid is present in considerable quantities (8 or 10 parts, for instance, to 1 of nitric acid), the yields are less. The same is the case if the time of immersion is too much diminished and is reduced to two or three minutes. Moreover, in these different cases the product is not the gun-cotton as obtained by the methods pursued at Bouchet and at Hirtenberg; it has smaller ballistic effects, and it is generally soluble in a mixture of alcohol and ether.

The yields of the manufactory, whether at Hirtenberg or at the Bouchet powder-mills, are far from those obtained in the laboratory in small quantities. In fact, according to General von Lenk, 64·50 kilogs. of undried cotton are necessary for 100 kilogs. of gun-cotton, which corresponds to a yield of 155. Supposing that the cotton contains 6 to 7 per cent. of moisture, the yield of dry cotton would have been 165 to 167 per cent. The constant yield at Bouchet when the manufacture had acquired a certain regularity was 166·25 per cent.

Without drawing from these numbers any conclusion respecting the theory of the formation of gun-cotton, we cannot pass over a circumstance so important as that of the virtually identical yield obtained in the large establishments in question.

III. *Analysis of Gun-cotton.*

The composition given above, according to the German chemists, $C^{12}H^7O^7\,3NO^5$, may be considered as that of a cellulose in which 3 equivalents of water are replaced by 3 equivalents of nitric acid, so that General von Lenk's gun-cotton would be a trinitrocellulose. This is the name by which they have described it.

They have further expressed the opinion that the pyroxyle prepared with less proportions of acids, as at Bouchet, might present a different composition.

One of us had determined in 1847 the composition of gun-cotton, and had represented it by the formula

$$C^{24} H^{17} O^{17} 5NO^5.$$

We had therefore first to determine whether the product then investigated was different from that of Lenk's gun-cotton, and in case the Bouchet cotton proved chemically identical with the Lenk cotton, what ought to be its true formula.

We believe we have bestowed on these researches the greatest pains, and we think we have overcome all the difficulties which the combustion of gun-cotton presents. We may at once say that we have established the identity, chemically speaking, of the gun-cotton of General Lenk and of that from Bouchet, and we have arrived at a formula only differing by one equivalent of water from that adopted in 1847.

The new formula,

$$C^{24} H^{18} O^{18} 5NO^5,$$

corresponds to the following numbers:—

Carbon	25·00
Hydrogen	3·13
Oxygen	59·72
Nitrogen	12·15
	100·00

It is so close to the old formula, $C^{24} H^{17} O^{17} 5NO^5$, that analysis alone would not have justified the change. Our choice is based on the proportion of the yield. In fact the new formula supposes a yield of 177·78 of gun-cotton for 100 of cotton, while the old one merely corresponds to a yield of 175. Our direct experiments, related previously, have led to the number 178.

All the gun-cottons we analyzed were previously washed in a mixture of ether and alcohol (which removed from them some thousandths of fatty matters and of soluble substances), then dried for several hours in an oven at a temperature between 40° and 50°. All had the same composition.

Our formula,

$$C^{24} H^{18} O^{18} 5NO^5,$$

ought to give, as gaseous products of the decomposition of gun-cotton by oxygen, carbonic acid and nitrogen in the proportion of

Carbonic acid corresponding to C^{24}	48 volumes
Nitrogen corresponding to N^5	10 ,,

or, for 100 volumes of the total gas,

Carbonic acid	82·8 volumes
Nitrogen	17·2 ,,

The German formula should give

Carbonic acid	48 volumes
Nitrogen	12 ,,

or, for 100 volumes of the total gas,

Carbonic acid	80 volumes
Nitrogen	20 ,,

The difference in the two proportions of nitrogen, 17 per cent. in the case of our formula, 20 per cent. in the case of the German formula, is sufficient to permit a choice between the two.

The following are the conditions under which we have worked. Gun-cotton mixed with about twenty times its weight of pure oxide of copper was placed in a green glass tube, and tapped so that it occupied a length of about 20 centimetres. Above it was placed, first, about an equal length of coarse oxide of copper, and lastly of pure reduced copper. The gaseous products of combustions were collected over mercury in graduated tubes, when a sufficient volume of gas had first been rejected. The total volume of gas suitably cooled was first noted, then the carbonic acid was absorbed by potash, and the residue calculated as nitrogen.

This combustion requires great care. There are in fact two dangers—that of not completely reducing by the copper the nitrous vapours arising from the decomposition of the gun-cotton, and that of not completely changing into carbonic acid the carbonic oxide produced in the same decomposition. To avoid them the two columns of coarse and of fine oxide must be kept at redness throughout the duration of the experiment, and the progress of the decomposition must be so regulated that the gases produced traverse these two columns with extreme slowness.

In a great number of analyses, made with our different specimens, we found the numbers between 17·2 and 17·5.

We have moreover verified our formula by weighing the nitrogen as gas. The weight of gas is 14·14 per cent. in the German formula, and 12·15 per cent. in ours. This difference, again, is sufficient to permit a choice between the two formulas.

The analysis was made in the following manner. At the bottom of a green glass tube perfectly pure carbonate of lead was placed, then the mixture of a known weight of gun-cotton with about fifty times its weight of free oxide of copper, then coarse oxide, and lastly reduced copper. The carbonate of lead was heated so as to expel the air in the tube until the disengaged gas was entirely absorbed by potash. The combustion of the gun-cotton was then made with all the precautions mentioned above. On the completion of the combustion, the tube was swept out with carbonic acid. All the nitrogen was thus obtained in the gaseous state after the absorption by potash of the carbonic acid produced.

In this way we have always obtained from 12 to 12·4 per cent. by weight for the nitrogen; and these two determinations are in perfect agreement with the formula which we adopt.

The German Report does not mention the conditions under which the estimation of nitrogen was made—a regrettable omission. We have felt it necessary to allude to this, because nitrogen is that one of the elements which shows the greatest divergence between the two formulæ, and is therefore that which ought most certainly to exhibit the true one.

In our formula the hydrogen corresponds to 3·13, and in the German one to 2·36. We have found numbers between 3·10 and 3·30. The German chemists have many times mentioned 2·8,—a number which, although inexact, is nearer our formula than theirs.

As to the carbon, we found 24·75 and 25 per cent.; but as it only differs in the two formulæ by about 0·008, it is less fitted than the other elements of gun-cotton for the control of its composition. We may, however, remark that in this point again the analyses in the Austrian Note give numbers as near one as the other formula; there is even an analysis in which the carbon agrees perfectly with ours.

In fine, both the gun-cottons manufactured under the conditions we have mentioned—those of General Lenk, like the Bouchet ones—have the same composition, corresponding to the formula

$$C^{24}H^{18}O^{18}\,5NO^5,$$

and not to the formula

$$C^{12}H^7O^7\,3NO^5.$$

IV. *Action of Heat on Gun-cotton.*

General von Lenk attributes the bad results obtained in France by the Commission of 1846, to the circumstance that sufficient attention had not been bestowed on the preparation of the gun-cotton, and that a definite and adequately nitrated compound had not been taken. He therefore works under conditions which he considers favourable to nitration, and thinks he has obtained a gun-cotton which offers great resistance to decomposition.

We shall not discuss the theoretical value of this assertion, but it seems inadmissible. It is, on the contrary, probable that a gun-cotton would decompose the more easily the further it varied from the type cellulose, and therefore the more nitrated. In any case General von Lenk asserts that the gun-cotton made by his method explodes at a temperature of 136° C., and resists any lower temperature. This is an important point in the discussion, and one which we have made the object of numerous experiments.

These experiments were first made with assay flasks, closed or not, which were immersed in a bath of boiling water.

All the specimens thus heated to 100° were decomposed in a longer or shorter time, and a few minutes were sufficient in all cases to exhibit a disengagement of nitrous vapour.

The decomposition takes place in various different ways, which cannot be reproduced at will.

Four different modes of decomposition at a temperature of 100° may be noted, each characterized by the disengagement of nitrous fumes.

1. The gun-cotton detonates violently.
2. It decomposes without detonation, leaving a white pulverulent residue, which is acid, quite soluble in water, and contains no nitrogen; it forms about half the weight of the gun-cotton.
3. It leaves a yellow amorphous inexplosive residue, partially soluble in water, and reducing tartrate of copper and potass like grape-sugar.
4. It gives a slight residue, about 8 to 10 per cent. of its weight, of a black substance, like carbon. In this case the flask is entirely lined with a yellow powder, which dissolves completely in the alkalies with a considerable disengagement of ammonia. (This seemed to be ulmate of ammonia.) Acids precipitate from this solution a dirty yellow substance which is also soluble in alkalies. The carbonaceous residue itself, though it is little altered, disengages ammonia when treated with potass. This production of ammonia by the mere action of heat on a substance formed of nitric acid and ammonia is a remarkable fact.

Other experiments, made on various gun-cottons at temperatures of 90° and then at 80°, gave exactly the same results, except that the phenomena of decomposition, instead of appearing after a few minutes, only showed themselves after some hours.

At 60° and even at 55° gun-cotton is decomposed. After the lapse of some days the flask was seen to fill with thick red vapours; and the same pulverulent, non-nitrogenized residue of which we have spoken was obtained. No ignition was observed in these latter experiments.

We may also mention a decomposition which one of us observed when about a gramme of gun-cotton was placed in a Gay-Lussac's hot-air bath containing oil, the temperature of which was only 47°. The gun-cotton thus decomposed came from a specimen prepared by an immersion of forty-eight hours, and soaked by the Lenk method. The detonation, which was very brisk, was exhibited immediately after the gun-cotton had been placed in contact with the metal.

This fact recalls another, quoted in an Austrian report—the explosion of a specimen at a temperature of 69°. General von Lenk attributed this explosion to sulphuric and perhaps nitric acid remaining in the gun-cotton. Yet our explosion, observed at a lower temperature, was not caused by the presence of these acids.

We dwell upon this detonation at 47°, because this heat may be attained and even exceeded by the action of the solar rays.

In fact at the Bouchet powder-mills, that is, in a country with a mean climate, a temperature of 69° was observed in masses of cotton placed in the sun on drying-cloths.

The preceding experiments show incontestably that, contrary to General von Lenk's assertion, his gun-cotton resists the action of heat no better than the Bouchet. In all these cases the silicated Austrian gun-cotton behaved in the same manner as ours.

With these facts, of decomposition produced at temperatures near 50°, it may be asked if gun-cotton does not decompose even at ordinary temperatures. Is it susceptible therefore of detonating spontaneously when kept in considerable masses in magazines?

Many chemists have quoted examples of the decomposition of gun-cotton at the ordinary temperature. They have generally noted as products of this decomposition nitrous vapours, highly oxidized substances such as formic, oxalic, and acetic acids, and as residue gummy or saccharine substances. These instances of the alteration of gun-cotton at the ordinary temperature, it has been attempted to ascribe to acids left in the gun-cotton by imperfect washing.

It is first to be observed that washings are easy with small quantities of substance. Then, as was known from the first, sulphuric acid exerts a destructive action on gun-cotton, and it is evident that great care must have been taken to remove the least traces, and therefore the washings must have been made with the greatest care.

Without detailing the known cases of decomposition of gun-cotton at the temperature of the places in which it was preserved, we shall limit ourselves to speaking of decompositions which we have observed in specimens of the manufacture of 1847, which had been washed with special care, either with pure or with alkaline water.

Of twenty-eight specimens less than a few grammes in weight placed in a small stoppered bottle, sixteen had undergone various alterations.

We took at random one of the altered specimens for examination. This specimen consisted originally of 6 grammes of gun-cotton which had been washed with potash and left from the 17th of March 1850 (14 years) in a stoppered bottle imperfectly closed. It had left a residue of 79 per cent., of a deep yellow colour, strongly acid, but *without sulphuric acid.* This residue dissolved *completely* in water, and, like grape-sugar, reduced potassio-tartrate of copper. Its boiling solution emitted a fresh acetous odour, and, remarkably enough, it disengaged ammonia under the action of potass.

Hence, under ordinary atmospheric conditions, there are indubitable instances of spontaneous alterations of gun-cotton, and what is more, of a gun-cotton washed with alkaline water.

But we have seen that, when warm, gun-cotton decomposes in four different ways, that in certain cases it detonates, and that in others, apparently identical, it is destroyed without inflammation. Why should it not be so with gun-cotton kept at low temperatures? Why in the case of simple decomposition at ordinary temperatures may not cases of detonation also occur? The analogy is too evident to oblige us to resort to the supposition of bad washings to explain the explosions of gun-cotton.

We admit that a badly washed gun-cotton is more exposed to decomposition than a well-prepared gun-cotton. But seeing the facility with which all specimens of gun-cotton, of whatever origin, decompose at 60°, and especially seeing that half the specimens kept by one of us under exceptionally favourable circumstances have decomposed, it is right to conclude that the storing of large quantities of gun-cotton offers terrible chances of explosion.

Can we conclude, with General von Lenk, that explosions are impossible, or at any rate very improbable, because he kept for a dozen years without alteration large quantities of gun-cotton? To do so we must neglect the explosion in Austria, near Simmering, which, as we have already said, could only be explained by a spontaneous inflammation of gun-cotton.

The best-washed gun-cotton, that of General von Lenk, becomes acid by lengthened exposure to the sun. A gun-cotton originally alkaline, exposed thus to the action of the light for a few weeks in a glass vessel (that is, in contact with sides which have alkaline tendencies), gives an acid reaction.

In darkness even this acidification is finally infallibly produced; and we have seen specimens of gun-cotton which, after being passed through alkaline solutions, and then kept in closed boxes for several years, finished by corroding the paper which enclosed them.

Hence in gun-cotton submitted to the action of air and light, or even kept in the dark, there is a commencement of alteration. This alteration, very feeble at the beginning, may last several years without increasing or presenting any inconvenience; but suddenly, and without perceptible cause, it may develope and cause an increase of temperature which produces a detonation. When gun-cotton is seen to alter and become quite soluble by a long exposure to diffused light, we cannot refuse to admit the possibility of a decomposition accompanied by detonation; for detonation in the case of so unstable a substance must be very near decomposition.

It has been desired to attribute the cases of spontaneous decomposition, especially in France, to the fact of the cotton not having had a sufficiently long nor sufficiently energetic impregnation with the sulphuric and nitric acids.

We cannot admit this opinion, after having observed similar effects of the action of temperatures of 100°, 80°, 60°, and 55° on the Bouchet gun-cottons steeped for an hour, and on General von Lenk's steeped for forty-eight hours. We are inclined, on the contrary, to think that the gun-cotton prepared with large quantities of very concentrated acids, and by prolonging the duration of immersion, is more subject to spontaneous inflammation.

V. *Comparison of the Lenk and Bouchet Gun-cottons as regards their ballistic and bursting properties.*

We have now to make known the results of trials made with the ballistic pendulum, to compare the two kinds of gun-cotton as regards their ballistic power.

Twenty-five shots were fired with the Lenk cotton, fifteen with the Bouchet cottons, with a charge of 3 grammes, and with round balls weighing 25·50 grms.

Taking for each set the mean velocity of the balls, then the strongest, and then the weakest shot, we found—

	Gun-cottons.	
	Lenk. m.	Bouchet. m.
Mean velocity . .	385·36	394·32
Strongest shot . .	441·53	445·94
Weakest shot . .	357·63	357·63

In the discharge of the same specimen of powder greater differences than the above are met with. For instance, the gun-cotton brought by General von Lenk from Austria was fired twice. It gave—

	m.
The 17th February	374·40
The 8th March	408·40

From these results we think we may conclude that both the Lenk and the Bouchet gun-cottons have the same ballistic force.

For these experiments the charge of gun-cotton occupied a length of 5 centimetres in the barrel. It was proposed to try it again by ramming it more strongly, and thus reducing the length to 3 centimetres; but on the first round with this form of charge, and with 3 grammes of gun-cotton made at Paris by General von Lenk's method, the cannon burst.

This fact is analogous to that which has several times been observed in firing the Bouchet gun-cotton. We find in it a proof of the resemblance of the Austrian and the French gun-cottons, as regards their bursting effects.

We shall not revert to all the attempts of the Commission of 1846 to remedy this inconvenience of the too rapid combustion of gun-cotton, but we may speak of those made with the same view by General von Lenk.

He at first used pressed cartridges, which did not succeed; in one of the Notes which he has communicated to us, we read that a bronze gun charged with such a cartridge was rendered useless by the second round.

The cartridges which appear most to diminish the bursting effect of gun-cutton on the sides of the arms are those made of paper cylinders covered with woven gun-cotton, and which he calls elongated cartridges (*cartouches allongées*).

According to the same Note, by means of these latter cartridges, a thousand rounds of about 481 grammes each were fired from a twelve-pounder (*pièce à douze*), giving a velocity of 427 metres, and without injuring the barrel.

But this velocity, at which the experiments in question stopped, is less than the velocity of 480 metres obtained in France with the same description of gun and a charge of 2 kilogrammes of ordinary powder. This latter velocity the Commission of 1846 desired when it used 667 grammes of gun-cotton; but it has not been shown that the cartridges of the Lenk system would not be injurious to fire-arms if the quantity of gun-cotton were increased so as to obtain the same velocity as in France.

Further, the author of one of the Austrian Reports admits that the desired result has not yet been attained, and that the mechanical means employed to prevent the gun-cotton from exercising its destructive effects neutralize part of its propulsive force. He arrives at the conclusion that the problem will only be solved when cannon have been made so strong that the destructive effect may be neglected. This is our opinion also; but it is impossible to enter on this path when we are stopped by the objection of spontaneous explosion, which, we think, rules the entire question.

VI. *Summary.*

In spite of the differences existing between the method of General von Lenk and that followed for seven years at the Bouchet powder-works, the same gun-cotton is obtained in both cases, except as regards 2 per cent. of a silicate, which is found in the gun-cotton taken as a type of the Hirtenberg manufacture.

This silicate was not met with in the gun-cotton proceeding from an English manufacture established on the pattern of the Hirtenberg one. In none of our experiments did it exert any appreciable influence on the properties of gun-cotton; its addition appeared therefore useless.

Neglecting this inert substance, all the gun-cottons we analyzed, Austrian, English and French, presented the same percentage composition.

German chemists having adopted a formula for General von Lenk's cotton which does not agree with our analyses, we repeated them a great number of times, and compared them, so as to retain no doubt about our new formula.

Another passage of their Report gives 136° as the lowest temperature at which the Hirtenberg gun-cotton explodes. This is a point in which our experiments compel us to differ from them. In fact, this gun-cotton, like the Bouchet, produced several explosions at a temperature of 100°. Once, even, gun-cotton made at Paris by the Lenk method, and perfectly washed, exploded at 47°.

By sufficiently prolonging the action of a temperature of 80°, 60°, and 55°, we have observed decompositions of the same kind in the Austrian and in the French gun-cotton.

From this we are convinced that with time the first must undergo the same decompositions as the second.

Of twenty-eight specimens of the Bouchet manufacture which we have examined after a lapse of seventeen years, sixteen had decomposed at the ordinary temperature. This explains the spontaneous combustions in consequence of the elevation of temperature which must take place in large masses.

We have observed the same ballistic force in the two kinds of gun-cotton.

The bursting property which led to the rejection of gun-cotton in the French artillery, appears to be as energetic in the case of the Austrian gun-cotton.

This property may in any case be diminished by using special cartridges, consisting of paper cylinders covered with woven gun-cotton; but in experiments made in Austria with twelve-pounders, a less initial velocity was reached than was desired in France.

Moreover an opinion opposed to the use of gun-cotton prevails now in Austria; for the special form of artillery which had been created for its employment has been there given up.

LXV. *Researches into the Thermal Phenomena of Voltameters, and Measurement of the quantities of Heat absorbed in Electro-chemical Decompositions.* *By* F. M. Raoult*.

The method employed in these researches is founded upon theorems relative to electromotive forces which have been recently published in the *Annales de Chimie et de Physique* (4th series, vol. ii. p. 317, July 1864).

The voltameter is placed inside a mercurial calorimeter, and is traversed by the current i of a Daniell's battery, P. In the course of this current there is a sine-compass A, which has two turns of a thick wire. Another sine-compass, B, with a very long and very thin wire (length 3600 metres, diameter 0·1 millimetre), is connected with the electrodes of the voltameter, and is traversed by an exceedingly feeble derived current e, negligible in comparison with the principal current i.

Let 23,900 be the heat evolved by the current of a Daniell's cell, of unit strength, during the reduction of 1 equivalent of copper †;
31·6 grammes the equivalent of copper (H=1 gramme);
T, the heat evolved in the voltameter;
p, the weight of copper reduced, in one element of the battery P, during the electrolysis;
e, the intensity of the derived current in the sine-compass with long wire B;
d, the intensity of the current which a *unit* Daniell's element produces in the same compass B;
f, the intensity of the current produced in the same compass B, by an element whose electromotive force is equal to that of the voltameter.

e, d, and f measure respectively the electromotive forces of the compound battery, of a Daniell's element, and of the voltameter.

e varies usually by nearly $\frac{1}{100}$ in the course of an experiment: as the variation takes place uniformly, the value of e introduced

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* *Comptes Rendus*, vol. lix. p. 521 (September 19, 1864).

† *Comptes Rendus*, Sept. 14, 1863. [Phil. Mag. S. 4. vol. xxvi. p. 522.]

into the formulæ is that taken at the middle of the time. d is measured by the method of *opposition*; f by the method of *alternate circuits**, after a sufficient number of elements have been added to the battery to make the intermittent current transmitted by the commutator equal to the primitive continuous current i.

An element x, *without resistance*, capable of producing in the voltameter the same current i as that of the battery P, would give, in the sine-compass with long wire B, a current of the intensity $e-f$ (for the demonstration see the complete memoir). The electromotive force of the element x, compared with that of a Daniell's element, is therefore $\frac{e-f}{d}$. Now since the quantity of voltaic heat evolved in the entire circuit, for the same fraction $\frac{p}{31 \cdot 6}$ of electricity transmitted, is proportional to the electromotive force, it follows that the heat evolved in the voltameter, by reason of its resistance, is

$$t = \frac{e-f}{d} 23900 \times \frac{p}{31 \cdot 6}. \quad . \quad . \quad . \quad . \quad (\alpha)$$

Such is the quantity of heat which would be evolved during the experiment in a metallic conductor of equal resistance with the voltameter.

This quantity t, contrary to what has been hitherto asserted, is always less than T. The difference $\mathrm{T}-t$ represents, for the fraction $\frac{p}{31 \cdot 6}$ of an equivalent of electricity transmitted, the heat furnished to the calorimeter by a local action. The *local heat* K, evolved in the voltameter for 1 equivalent of electricity transmitted, or of substance decomposed, is therefore

$$(\mathrm{T}-t)\,\frac{31 \cdot 6}{p},$$

or

$$\mathrm{K} = \mathrm{T} \times \frac{31 \cdot 6}{p} - \frac{e-f}{d} . 239000. \quad . \quad . \quad . \quad (\beta)$$

The inverse electromotive force f of the voltameter diminishes, by $f \times 239000$ thermal units, the heat which the current i produces in the circuit for 1 equivalent of electricity transmitted. (*Comptes Rendus*, 14th September, 1863 [Phil. Mag. *loc. cit.*])

The sum X of the calorific effects, positive or negative, pro-

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* The method of *opposition* and that of *alternate circuits*, which were devised by myself, were communicated to the Académie des Sciences on the 21st of February, 1859, and are described in the *Annales de Chimie et de Physique*, 4th series, vol. ii. pp. 321 and 326.

duced by the voltameter during the decomposition of 1 equivalent of substance is therefore

$$-X=-f\times 239000+K,$$

or

$$X=\frac{e}{d}\times 239000-T\times\frac{31{\cdot}6}{p}. \quad . \quad . \quad . \quad . \quad . \quad (\gamma)$$

My research is divided into two parts.

In the first part I give an experimental demonstration of the formulæ (α), (β), and (γ), and I establish the laws, hitherto misconceived, which regulate the development of heat in voltameters. The experiments related to sulphate of copper and acidulated water; they led to the following conclusion:—

"A voltameter introduced into the circuit of a battery weakens the electromotive force, and thus destroys in the complete circuit a quantity of heat which is always greater than what is required for the decomposition effected. The excess varies according to circumstances; but in every case a secondary action takes place at the electrodes, whereby a quantity of heat is imparted to the voltameter equal to the excess of heat destroyed. And, finally, the sum of the various calorific effects of the voltameter is equal to the heat absorbed by the decomposition which goes on within it."

This effect can be explained if we suppose that the substances first formed at the electrodes, and which cause the polarization, are unstable compounds which, like binoxide of hydrogen, evolve heat by their decomposition.

In the second part I have determined, according to equation (γ), the quantities of heat, X, absorbed by the decomposition of 1 equivalent of sulphate of copper, of water, of cupric chloride, and of hydrochloric acid.

In these experiments it is essential to avoid the local recombination of the gases in the voltameter. To effect this, the positive electrode is placed inside a small closed pipe-bowl, luted with wax to the end of a thin glass tube of equal diameter, whereby the oxygen or chlorine liberated is conducted out of the apparatus.

For the electrolysis of chlorides, a rod of retort-carbon, dipping into hydrochloric acid saturated with chlorine, is used as the positive pole.

The following Table gives the mean of the results obtained, compared with the calorific equivalents of the same substances in a state of solution, determined by Favre and Silbermann and by myself (*Comptes Rendus,* July 4, 1864).

	Heat X absorbed by decomposition.	Heat evolved by combination.	
		Raoult.	Favre and Silbermann.
Water	−33803	......	34462
Sulphate of copper	−29895	......	29605
Hydrochloric acid (dilute)	−33859	35200	40192
Cupric chloride	−28371	29500	34500

Observations.

Water.—The number −33803 is the mean of the results, agreeing within $\frac{1}{30}$, obtained with water containing sulphuric acid or soda.

Sulphate of Copper.—The sulphate of copper employed was acidulated beforehand with $\frac{1}{10}$th of sulphuric acid, in order that its conductivity might not alter during the experiment.

Hydrochloric Acid.—The acid employed was of such a strength that it decomposed exactly into hydrogen and chlorine, but nevertheless evolved no heat when mixed with water. The concentrated acid gives a lower number; but if the heat which this acid evolves by combination with excess of water is added to the result, it makes up very nearly the mean value 33859.

Cupric Chloride.—As this compound is converted into subchloride of copper in contact with the negative pole, it could not be employed. The difficulty was got over by putting acidulated sulphate of copper on the side of the negative electrode, and hydrochloric acid saturated with chlorine on the side of the positive electrode. According to the law of *moduli*, the quantity of heat (28371) destroyed by this voltameter ought to be that which corresponds to the decomposition of chloride of copper, CuCl.

LXVI. *Note on Ray-Transmutation.* By Dr. C. K. Akin.*

In a communication made by Prof. Tyndall to the (last) November Number of the Philosophical Magazine, entitled "On Luminous and Obscure Radiation," the following sentences occur:—"Dr. Akin inferred from the paucity of luminous rays evident to the eye, and a like paucity of extra-violet rays, as proved by the experiments of Dr. Miller, that the radiation from a flame of hydrogen must be mainly extra-red; and he concluded from this that the glowing of a platinum wire in a hydrogen-flame, as also the brightness of the Drummond light in

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* Communicated by the Author.

the oxyhydrogen-flame, was produced by a change in the period of vibration. By a different mode of reasoning I arrived at the same conclusion myself, and published the conclusion subsequently." The reference appended to the last sentence is to "Phil. Trans. vol. cliv. p. 327." The following passage is taken from a paper read before the Royal Society by Prof. Tyndall on March 17, 1864, and will be found in the place indicated, at p. 360:—

"Professor Stokes has demonstrated that a change of period is possible to those rays which belong to the violet and extra-violet end of the spectrum, the change showing itself by a degradation of the refrangibility. That is to say, vibrations of a rapid period are absorbed, and the absorbing substance has become the source of vibrations of a longer period. Efforts, I believe, have been made to obtain an analogous result at the red end of the spectrum, but hitherto without result; and it has been considered improbable that a change of period can occur which should *raise* the refrangibility of the light or heat. Such a change, I believe, occurs when we plunge a platinum wire into a hydrogen-flame. The platinum is rendered white by the collision of molecules whose periods of oscillation are incompetent to excite vision. There is in this common experiment an actual breaking up of the long periods into short ones—a true rendering of unvisual periods visual. The change of refrangibility differs from that of Professor Stokes, firstly, in its being in the opposite direction—that is, from low to high; and secondly, in the circumstance that the platinum is heated by the collision of the molecules of aqueous vapour, and before their heat has assumed the radiant form. But it cannot be doubted that the same effect would be produced by radiant heat of the same period, provided the motion of the æther could be raised to a sufficient intensity. The effect in principle is the same, whether we consider the platinum wire to be struck by a particle of aqueous vapour oscillating at a certain rate, or by a particle of æther oscillating at the same rate. And thus, I imagine, by a chain of rigid reasoning, we arrive at the conclusion that a degree of incandescence, equal to that of the sun itself, might be produced by the impact of waves, of themselves incompetent to excite vision" [*].

1. The important parts of the above passage are the two couples of sentences, the one beginning with the words "The platinum is rendered white," and concluding with "unvisual periods visual;" and the other beginning with the words "But it cannot be doubted," and concluding with "at the same rate." The first two sentences in this "chain of reasoning" are "different"

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[*] To this passage the following footnote is appended:—"Some time after this was written I learned that Dr. Akin had previously inferred, from the paucity of luminous and extra-violet rays in the hydrogen-flame, that its periods must be extra-red. And he deduced from this that the heating of a platinum wire in a hydrogen-flame must consist of a change of period. A . . . communication from Dr. Akin on this and kindred subjects will be found in the 'Reader' for the 26th of September, 1863.—April 5, 1864.

from the corresponding parts of my own reasonings, in the same sense as two separate links of a chain are different from the same links when held together by a third, connecting link. Prof. Tyndall states that the platinum is rendered "white" by oscillations which are invisible; and he hence concludes at once that there is a "breaking up of long periods into short ones." This, however, is simply a *non-sequitur*. I founded my own conclusions concerning the origin of lime-light, &c., upon the evident deficiency of the oxyhydrogen-flame in Newtonic or luminous rays, *and* upon its probable poverty in Ritteric or so-called chemical rays, as compared with lime-light. And what I had thus at first but conjectured, regarding the Ritteric rays, was soon after corroborated by Prof. W. A. Miller's experiments on the comparative photographic effects of the oxyhydrogen-flame in its pure state, and of lime-light produced by the oxyhydrogen jet*.

2. Before adverting to the second, principal part of Professor Tyndall's argument, a word or two may be apposite regarding his intermediate statements. The change of refrangibility which takes place in the oxyhydrogen-flame by the introduction of lime, or in the simple hydrogen-flame by the introduction of platinum, according to Prof. Tyndall, differs from that taking place in fluorescence "in its being in the opposite direction." This, as shown, Prof. Tyndall's reasoning does not prove. He states also that the platinum is "heated by the collision of the molecules of aqueous vapour, and before their heat has assumed the *radiant form*." I expressed the same idea by saying that the act of transmutation, in the case of lime-light, took place "*in statu nascenti*, as it were," of the rays†.

3. The concluding part of Prof. Tyndall's induction affirms that "the effect in principle is the same, whether we consider the platinum wire to be struck by a particle of aqueous vapour oscillating at a certain rate, or by a particle of æther oscillating at the same rate." In our present ignorance concerning the mode in which material particles act upon each other, or of the real constitution of æther, an assertion like the above will scarcely be considered as contributing to render "a chain of reasoning" "rigid." In my own case, I reasoned concerning this matter as follows‡ :—"Every kind of radiation possesses, with respect to any given substance, a certain heating power, which depends (1) on the amplitude of the given ray; (2) on the absorptive power of the given substance for the given ray; and (3) in some unknown manner on the length of undulation of the given radiation. Any kind of radiation may, hence, be competent to raise

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* Report of the British Association, 1863, p. 95.

† Ibid. p. 101.

‡ See 'Reader,' September 26, 1863, p. 349. col. 2.

any substance whatever to any required temperature, by a suitable adjustment of the element of amplitude alone—provided the substance considered be not absolutely pervious to, or an absolute reflector of, the given radiation." Now, since metals do absorb Herschellic rays, after some preliminary considerations it was concluded "that incandescence, or an emission of Newtonic rays, which, as proved, might be engendered even by Ritteric rays, will be still more easy to produce by means of Herschellic rays, to which, for some reason or other, a greater heating power is universally acknowledged to belong." In this manner, it was shown as at least highly probable (and no induction can be absolutely certain) that the effect which in the case of lime-light is produced *in statu nascenti* of the rays, might eventually be produced also by rays incident upon lime or platinum after emanation from a source placed at a distance.

4. It will be seen from the above quotations, to what extent, and in what sense, Prof. Tyndall's "reasoning" was "different" from my own. But I will venture to make yet one further remark, of more general application. The British Association is one of the foremost scientific bodies in the United Kingdom; and it is well known that, soon after its annual meetings are over, an account of its proceedings, authentic for the most part, is published in the 'Athenæum' newspaper. Clearly it is the duty of scientific persons to consult the 'Athenæum' during such portion of the year, in the same way as it is their duty to consult the Philosophical Magazine all the year round. Now in No. 1872 of the 'Athenæum,' p. 337, column 3 (Sept. 12, 1863), the following was published:—"The author conceived that the action of carbon and lime rendering the strongest heat of burning hydrogen luminous, were instances of the Herschelian rays being raised to the Newtonian; and as Prof. Stokes had termed the other influence 'fluorescence,' Dr. Akin proposed to term this 'calcescence,' from the power of lime to turn heat into powerful illumination"*.

These sentences are taken from an abstract of the first of the two papers read by me before the Mathematical and Physical Section of the British Association at Newcastle, but for which I am not responsible. It would be superfluous to point out here its inaccuracies; but the explanation of lime-light proposed by me is but too transparent, and in fact twice repeated, in the passage I have quoted. Now I will allow, for argument's sake, that the abstract referred to had escaped Prof. Tyndall's notice. In January 1864 an article appeared in the 'Saturday Review,'

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* The term "calcescence" was actually suggested to me by one of the Secretaries of the Section of the British Association before which the paper referred to was read at Newcastle.—C. K. A.

giving the outlines of a discourse delivered by me at Cambridge on the subject of "Calcescence." In that article only one of the three experiments suggested by me was mentioned; and the explanation of lime-light, also, was not explicitly referred to; but, on the other hand, it was stated that the matter had been more fully treated in papers read at the Meeting of the British Association at Newcastle. Surely any one, like Prof. Tyndall, feeling an interest in the subject, after reading the above-mentioned article (and which is actually adverted to, though not distinctly, in the passage quoted before from the Philosophical Transactions), ought to have referred, under such circumstances, to the 'Athenæum' for 1863, to see whether there were to be found in it further particulars concerning this subject. Perhaps, the 'Reader' having become more extensively known by that time, the abstract published in its columns on Sept. 26, 1863, would have likewise fallen then into Professor Tyndall's hands. In my own case, and regarding the matter now in hand, I searched the back volumes of every sort of accessible periodicals, and other works which might possibly bear on it, in order to discover whether I had been anticipated, perhaps by some author whose writings are now forgotten. Such a proceeding (in which I have but followed the example of Sir H. Davy and other philosophers too conscientious and rich in original ideas of their own to wish to appropriate those of others) I believe all the more necessary, as it frequently occurs that persons read about things which they afterwards forget having read of, and then fancy to be the original products of their own minds. Thus I should not be at all astonished if Prof. Tyndall, in September 1863, had read the abstract quoted from the 'Athenæum,' but, from shortness of memory, by January or March 1864 had forgotten all about it, so far at least as the mere fact of reading goes*. A search for precedents, if it will not always revive recollection, would at least prevent repetitions in such cases. But sufficient stress, I venture to think, cannot be laid on the necessity that scientific persons should not neglect any means which they have at command, in order to become acquainted with the current progress, not less than with the past advances, in science. It may be allowable for poets to ignore their contemporaries or predecessors, in order to preserve their own originality of thought, whether in form or in substance. In the case of discoverers, the public is interested to learn, not what they would be capable of doing if nobody else were or had been in the

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* Upon the whole, I cannot help thinking that the passage quoted above from the Philosophical Transactions reads more like an imperfect reminiscence, supplemented by conversation and discussion (see the 'Reader,' no. 67. p. 461), than anything else.

field, but what they are capable of doing that is new. It may be difficult to decide which habit is of more importance—to study the literature of the past, or to attend to the publications of the present day. Much waste of thought would be prevented by either; but the latter proceeding has the additional advantage of preventing the necessity of such explanations as those preceding, extorted by the natural obligations of self-defence and the defence of right*.

5. Prof. Tyndall states (Phil. Mag. S. 4. vol. xxviii. p. 338):—"The rendering of metals incandescent by obscure rays has not yet been accomplished. This is a question on which Dr. Akin has been engaged for some years, and it is not my intention to publish anything relating to it until the very promising arrangements which he has devised have had a sufficient trial"†. Regarding this statement, I think it useful to publish the substance of a letter lately addressed by me to the President of the British

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* Merely as an illustration of what has been stated above regarding forgetfulness, I shall mention the following instance.

In the paper commented on in this Note, Prof. Tyndall says, amongst other things (p. 332):—"In solid metals augmented temperature introduces waves of shorter periods into the radiation. It may be asked, 'What becomes of the long obscure periods when we heighten the temperature? Are they broken up or changed into shorter ones, or do they maintain themselves side by side with the new vibrations?' The question is worth an experimental answer." Now that question had been practically answered by Prof. Draper, as long ago as 1847. He said (Phil. Mag. S. 3. vol. xxx. p. 350:—"It is to be remarked that, while the more refrangible end [of the spectrum] undergoes a great expansion, the other extremity exhibits a corresponding though a less change . . . . arising from the increased brilliancy of the light . . . [as the temperature rises]." No doubt, Prof. Tyndall has read the paper from which these latter passages are taken, and which has generally been considered (and more especially so by Melloni) as of great importance. But Prof. Tyndall has evidently forgotten the fact, or else he would certainly have mentioned it; and yet it would be impossible for him to assert that his own query regarding the invisible rays would have been suggested to him had he not previously read the above statement concerning the visible rays. In a similar manner, it would not be difficult to trace back what Prof. Tyndall says concerning the light and heat of a candle (*l. c.* p. 339) to what, it is at least likely, was its original source.

† I must be allowed to express here a doubt whether Sir H. Davy, for instance, or any other predecessor of Prof. Tyndall at the Royal Institution, having read in a public print that two persons were engaged in making researches upon a certain subject with the aid and sanction of the British Association, would have chosen "that very subject" "for attack" some little time after. Nor do I believe that Davy or Young, having publicly pledged themselves not to "publish" anything relating to the subject till a certain contingency, would have meanwhile proceeded with it privately; at all events they would not, as soon as they had obtained what might turn out to be the desired result, have rushed off to repeat (and thus, to all intents, publish) the experiment before their "colleague"—the sheets in which that pledge was published being then scarcely dry, and the contingency referred to as yet unaccomplished.

Association, and which was as follows:—"Some time ago, I received a letter from the Assistant General Secretary of the British Association, in which my attention was requested to a Resolution, which was adopted by the General Committee at the last Meeting of the Association, and to the effect, 'That Prof. Griffith and Dr. Akin be requested to continue their Report on the Transmutation of Spectral Rays.' In answer to this communication, I now beg leave to state that, after the experience of the last two years, and more especially of last summer, I feel it would be a hopeless undertaking for me to continue, at Oxford, the experiments begun there. Moreover, I am not sure whether, after the end of the present year, I shall be able to give my attention any longer to scientific researches." Thus, as far as I am concerned, the trial of the "arrangements devised by" me, and of the apparatus constructed for the experiments begun at Oxford, is at an end. Such being the case, I have no doubt that, with the means at his command and his experimental proficiency, Prof. Tyndall will now realize and "publish" a discovery which I have assigned the methods for accomplishing, and which I should have probably effected myself, I may say, years ago, had I been seconded as I had hoped, either by persons or by circumstances. Astronomers have placed the merit of the mathematicians who first conjectured the existence of Neptune above that of the practical observers who actually discovered that planet. In my own case, I have made considerable sacrifices of time, and even of feeling, in order to prove by experiment what I had deduced originally from logical reasonings. Being now practically shut out from pursuing the subject any further, I shall leave it for physicists to decide whatever merit may belong to the originator of the research, as compared with the merit of him, whoever it may be, who, more fortunate, shall bring the research to a satisfactory close*.

London, November 1864.

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* Prof. Tyndall's experiments on rays transmitted by iodine dissolved in bisulphide of carbon, and reported in the November Number of the Philosophical Magazine, although they may ultimately prove to be correct, are evidently inconclusive when made in the manner reported—that is, apparently, in daylight. I have also observed that, in looking through a prism of such iodine solution, at certain thicknesses, a double image of objects appears—the one violet, the other red. Now, since bisulphide of carbon is about equally powerful as an absorbent both of Ritteric or chemical and of Herschellic or caloric rays, it remains to be proved whether the Ritteric rays do not cling, as it were, in transmission to the violet rays, in a similar manner to that in which the Herschellic rays adhere to the red rays—the penetrative power of the invisible rays exceeding in both cases that of the corresponding visible rays.

LXVII. *Notices respecting New Books.*

The Astronomical Observer. A Handbook to the Observatory and the common Telescope. By W. A. Darby, *M.A., F.R.A.S.* London: R. Hardwicke. 1864.

THIS book contains in alphabetical order the constellations visible to an observer in lat. 50° N. Under each head the constellations are first briefly described, the nebulæ within its limits are next given, then the star-clusters, and lastly the double stars. The right ascension and declination of each object is given and the magnitudes of its components; directions are added for finding it with a common telescope. In many cases a brief description of the object is included: *e. g.*, in the constellation Cygnus, under the head of "Double Stars," "β 6.—19h 25m 17s., N. 27° 40′ 42″. *a* 3, topaz-yellow; *b* 7, sapphire-blue; dist. 34″·4. One of the finest of the double stars, the colours in brilliant contrast; on the Swan's bill, in the base of the cross of Cygnus. Pointed at by a line from Vega carried 1° *f.* γ Lyræ, and rather less than as far again." The work will probably be useful to the persons for whom it is designed—amateurs who do not possess an equatorially-mounted telescope.

Prefixed to the book is an Introduction not easy to describe. We should guess that Mr. Darby first wrote the catalogue, and then by way of introduction jotted down without order or method anything bearing on the subject just as it occurred to him. Accordingly some of the points mentioned are very pertinent, *e. g.* the description of Sir J. Herschel's mode of observing and registering double stars, the tables of test objects, &c. Other parts are quite the reverse. The sketch of the history of astronomy is meagre and inaccurate. Mr. Darby appears to believe the assertion of Josephus, that "Abraham was a most intense observer of the stars, and the first to bring astronomy from Chaldea into Egypt." Shortly after, he informs his readers that "to Egypt, the oldest of nations, belongs the honour of producing the most eminent astronomers of ancient times. Pythagoras, Euclid, Archimedes, Eratosthenes, Ptolemy, and Hipparchus were all of the Alexandrian school." Now this is really too bad. Mr. Darby had no occasion to go into the history of his subject; but as he chose to do so he was bound to be accurate in his statements. Surely it is well enough known that the Alexandrian Greeks were not much more Egyptians than Englishmen living in Calcutta are Hindoos. Many of the most conspicuous of them were not so much as born in Egypt; *e. g.*, Hipparchus was of Nice in Bithynia. Then what are we to say to Archimedes, whose name and fame are so indissolubly bound up with Syracuse? or to Pythagoras, of whom at all events we know this, that he lived some few hundred years before the Alexandrian school was founded?

LXVIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 324.]

November 9, 1864.—W. J. Hamilton, Esq., President, in the Chair.

1. "Notes on the Geology of Jamaica; with Descriptions of new Species of Cretaceous, Eocene, and Miocene Corals." By P. Martin Duncan, M.B., Sec. G.S., and G. P. Wall, Esq., F.G.S.

The authors first referred to the Miocene age of the Corals that have hitherto been described from the West Indies, and then stated that in this paper conclusive evidence was brought forward, for the first time, of the existence of an Eocene formation in Jamaica. They next noticed successively the lithological characters of the different members of the Jamaican fossiliferous rocks, and then described two new species of Corals from the Lower Cretaceous beds, and six from the Miocene, besides giving notices of additional known forms from all the strata; and the conclusion was drawn, that the facies of these Cretaceous Corals was suggestive of a close alliance having existed between this fauna and that of Gosau in the Eastern Alps. The question of the existence of Lower Cretaceous strata in other West Indian islands having been discussed, attention was drawn to the character of the Eocene Corals, as being confirmatory of Mr. Barrett's views on the existence of that formation in the island, and the paper was concluded by some additional remarks on the Miocene beds, and their probable correlation with those of Trinidad, Antigua, &c.

2. "On the Correlation of the Irish Cretaceous Strata." By Ralph Tate, Esq., F.G.S.

The non-existence in Ireland of the formations between the Lower Lias and the Upper Greensand having been stated, Mr. Tate first showed that the Cretaceous formations occurring near Belfast are referable to the so-called Upper Greensand (Hibernian Greensand of the author) and to the Upper Chalk, the latter consisting chiefly of a "White Limestone" with flints, and containing species known to occur in the Upper Chalk of Norwich and Meudon, with others allied to Maestricht forms. The basement-beds, forming lithologically a passage to the Hibernian Greensand, are (1) chloritic limestone with Sponge-remains belonging to about thirty species, and (2) a calcareo-chloritic sandstone with three species of Echinoderms, the dominant form being *Ananchytes gibba.* These passage-beds are only locally developed, and when they are absent the junction of the Greensand and the White Limestone is very abrupt. The Hibernian Greensand was considered by Mr. Tate to represent the Upper Greensand, the Chalk-marl, and the lower part of the Lower Chalk of England, and to be the miniature counterpart of D'Orbigny's Étage Cenomanien. It nowhere exceeds 55 feet in thickness; but it nevertheless contains the following beds:—(1) Chloritic sands and sandstones of Colin Glen, or the Zone of *Exogyra columba*; (2) Chloritic sandstones of Woodburn, or the Zone of *Inoceramus Crispi*?; (3) Yellow-sandstones and Marls with Chert, or the Zone of *Ostrea carinata*; and (4) Glauconitic sands, or the Zone of *Exogyra conica.*

LXIX. *Intelligence and Miscellaneous Articles.*

VERIFICATION OF THE LAW OF ELECTROLYSIS WHEN EXTERNAL WORK IS PERFORMED BY THE GALVANIC CURRENT. BY M. J. L. SORET.

IN order to explain, conformably with the mechanical theory of heat, the production by an electric current of effects external to the circuit which it traverses, such, for instance, as the performance of mechanical work or the generation of induced currents, recourse has been had to an hypothesis proposed by Helmholtz, Scoresby and Joule, Clausius, and other physicists. According to this hypothesis, the law of electrolytic action is regarded as still holding good for this special case. In order to supplement previous investigations, I undertook the verification of this point by comparing the amount of chemical action with the mean intensities of currents, usually discontinuous, by which external work was performed.

In order to produce powerful external effects, I have employed for the most part a Ruhmkorff's coil, but I found it needful in most cases to substitute a contact-breaker formed of a toothed wheel and platinum spring for the ordinary contact-breaker of this apparatus. By this means I obtained a much more rapid succession of currents, and at the same time greater stability in the needle of the sine-compass by which the mean intensity of the current was measured.

After a great number of experiments performed for the purpose of assuring myself of the exactitude of the method I was employing, I arrived at the following results, which confirm the law of electrolysis for these conditions.

In a circuit of great resistance, containing a Daniell's battery, the sine-compass, a voltameter charged with sulphate of copper, and the inducing coil of the Ruhmkorff's apparatus, the weight of copper deposited in the voltameter was found to be always proportional to the mean intensity of the current, whether the current was continuous (that is to say, when the contact-breaker of the Ruhmkorff's apparatus was excluded from the circuit, in which case no external action took place) or whether it was discontinuous (that is to say, when the contact-breaker was put in action, in which case an external action was set up, though this was certainly of small importance relatively to the total work performed by the current).

A similar result was obtained with a circuit of small resistance, consisting only of the Ruhmkorff's apparatus, the compass, and a single Daniell's cell in which a plate of platinum was substituted for the copper. The chemical action was measured by the quantity of copper deposited upon the platinum plate. With this disposition of the apparatus, the proportion of external work when the contact-breaker was put in action was considerable.

Under the same conditions, the quantity of electro-positive metal (cadmium) which is dissolved in the battery is also proportional to the intensity of the current. This determination cannot be made with as much accuracy as that of the weight of copper deposited, but the mean result of the experiments agrees with the law of electrolysis.—*Comptes Rendus*, vol. lix. p. 485 (September 12, 1864).

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END OF THE TWENTY-EIGHTH VOLUME.

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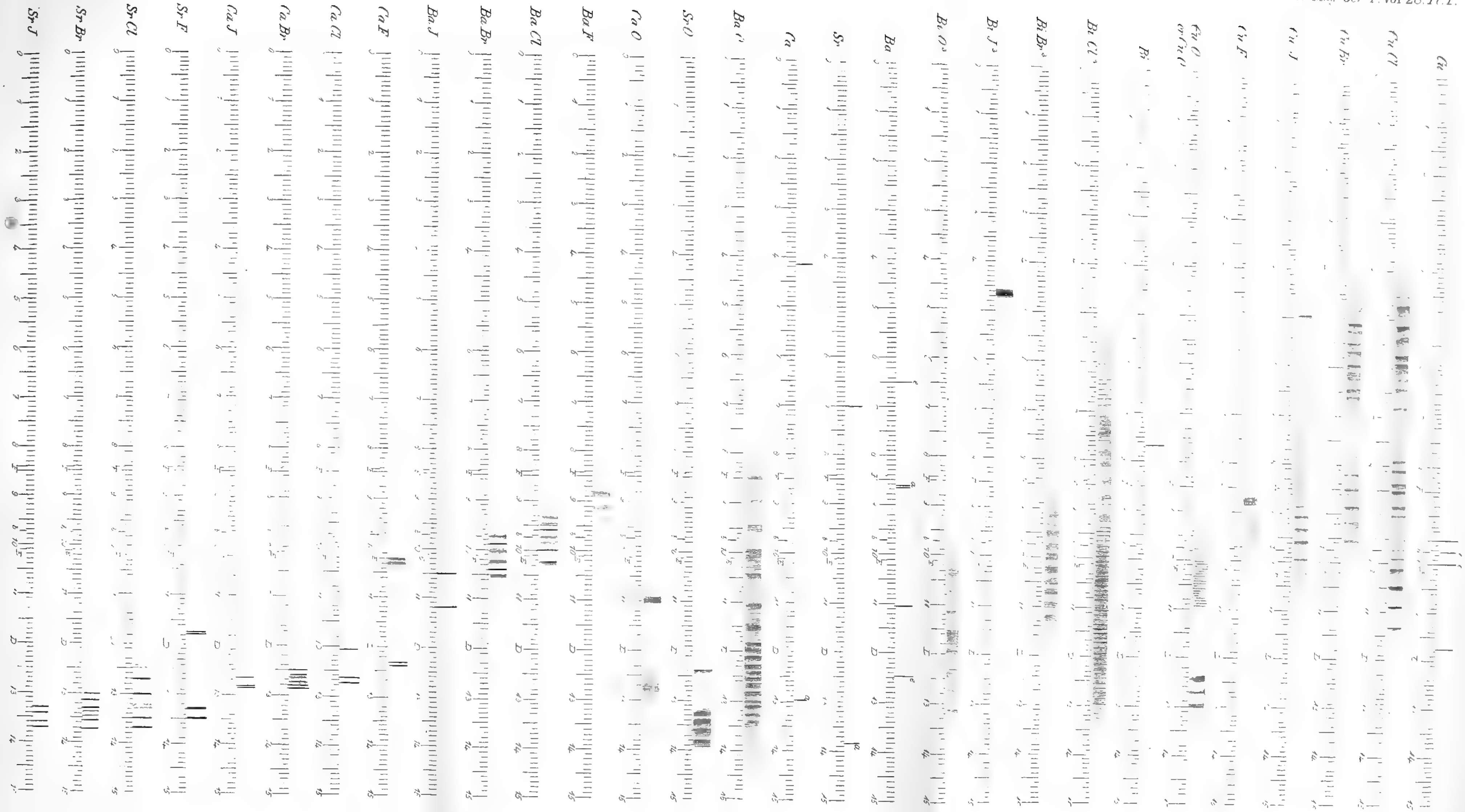
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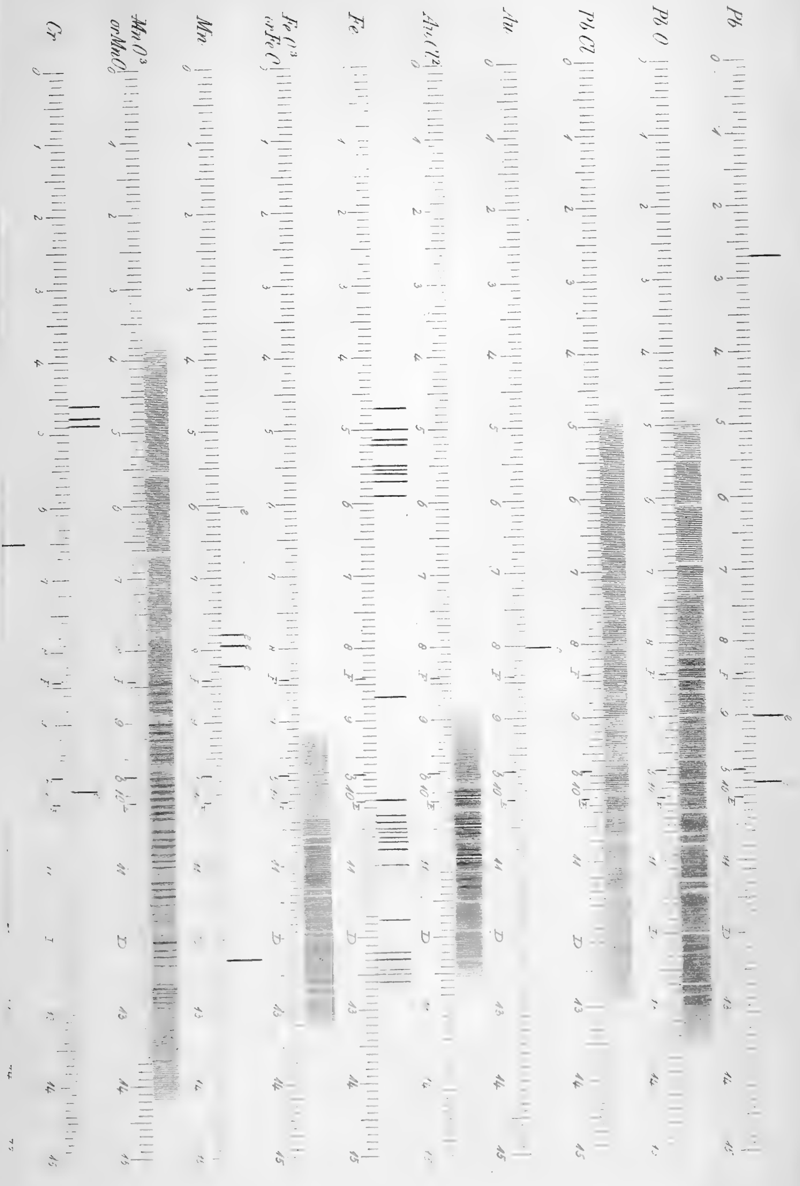

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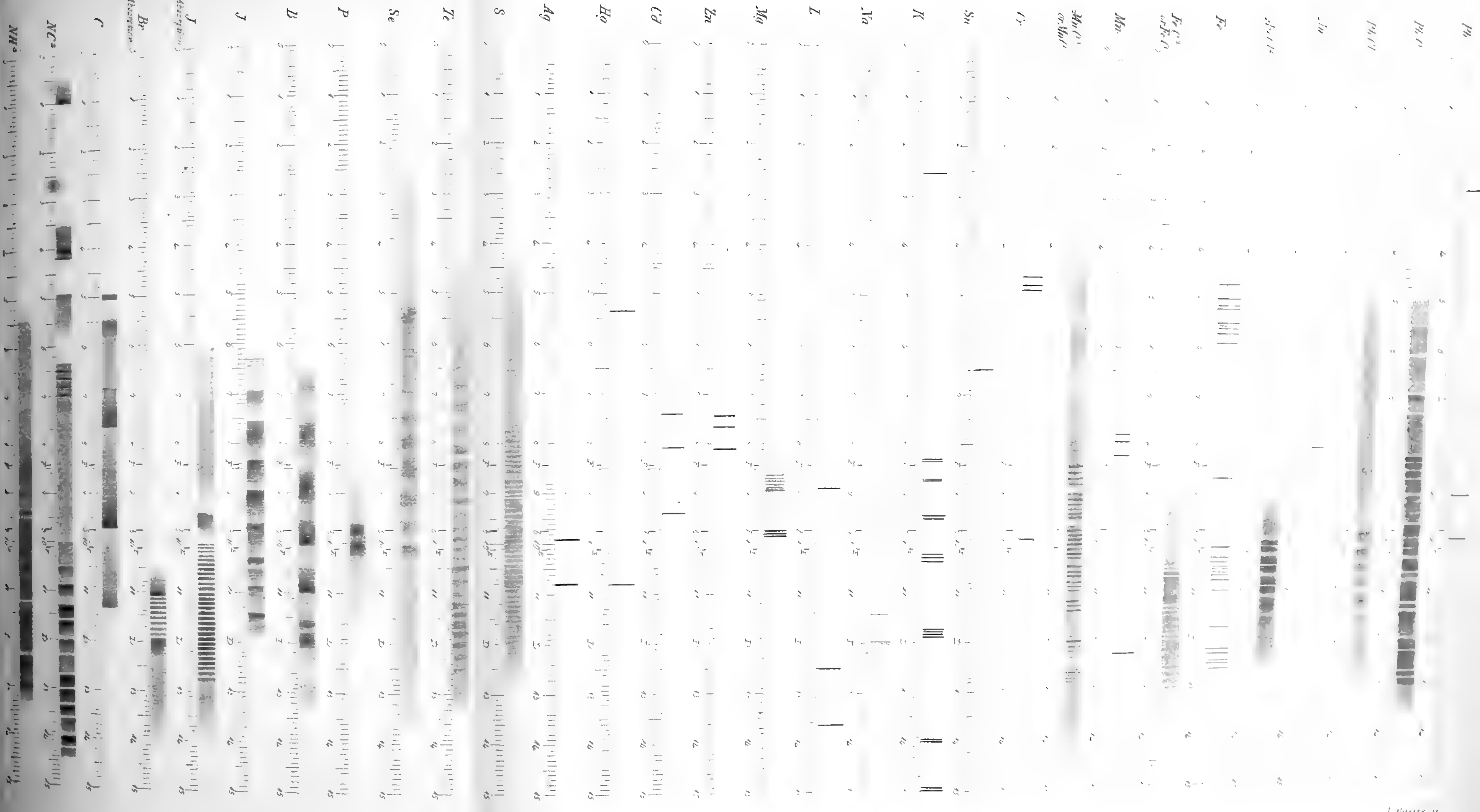
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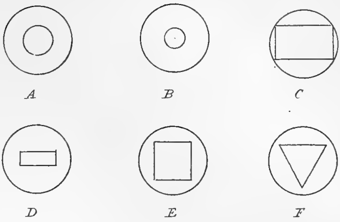

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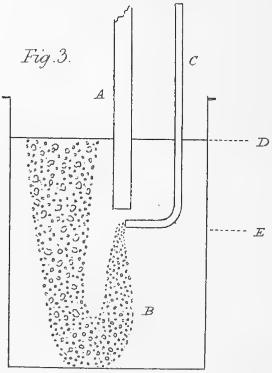

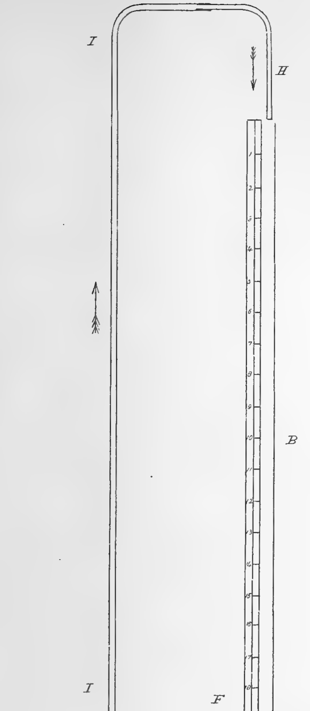
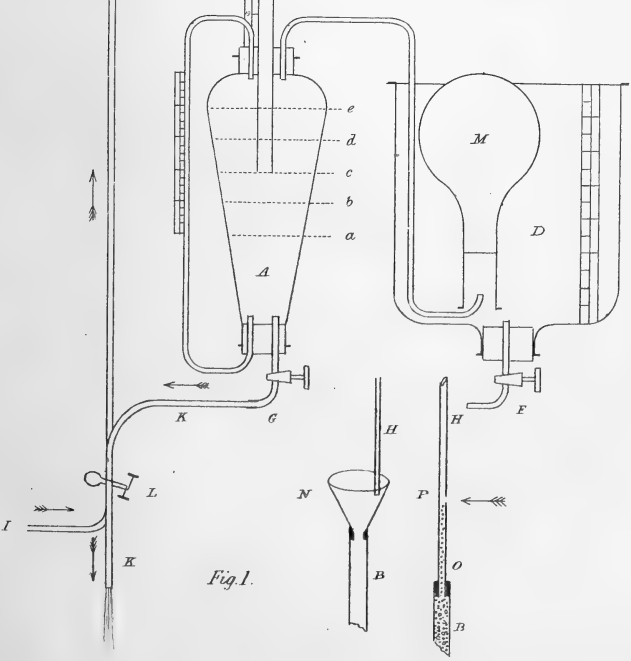

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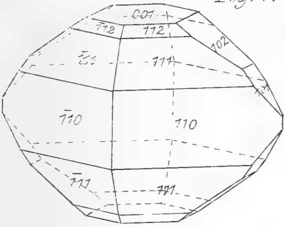

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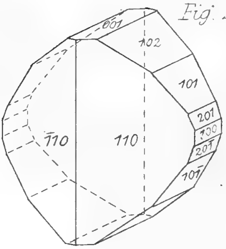

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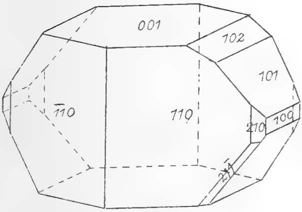

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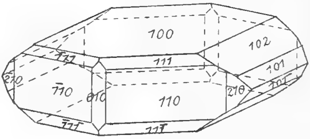

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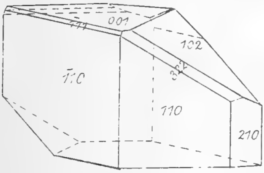

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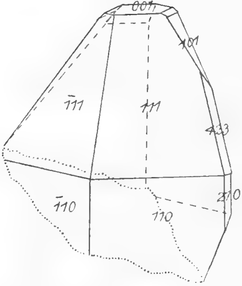

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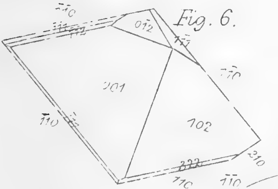


Fig. 8.

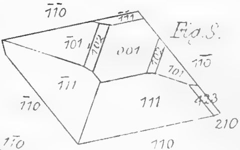
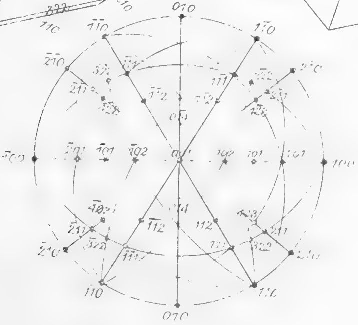
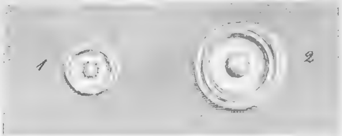


Fig. 9.

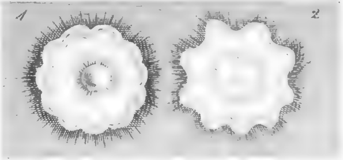


W. Lang. lith.

1 Alcohol on Cocoa-nut Oil

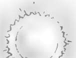


2 Alcohol on Spermaceti



3 Ether on

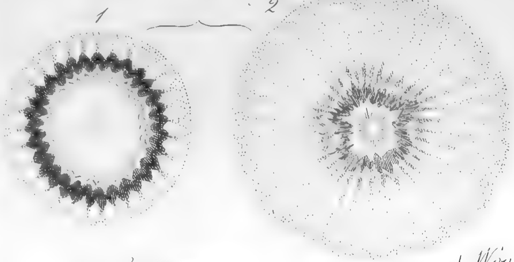
Cold } hot
Castor Oil



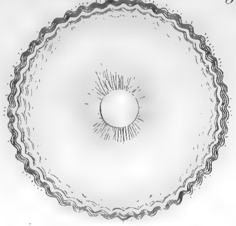
4 Ether on Cocoa nut oil at 80°

5 Alcohol on Paraffin at 100°

6 Camphorated Spt. on Olive Oil

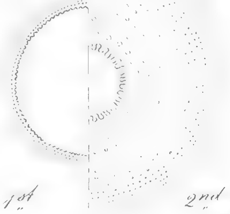


7 Camphorated Spirit on Lard or Spermaceti



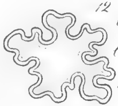
8 Camphorated Spt. on Sulphur

9 Pure Wood Spirit on Castor Oil

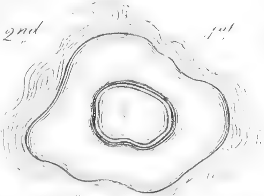


10 Commercial Wood Spirit on Lard

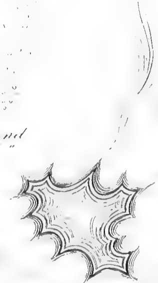
11 Benzole on Castor Oil



12 Creosote or Carbolic Acid on Sulphur



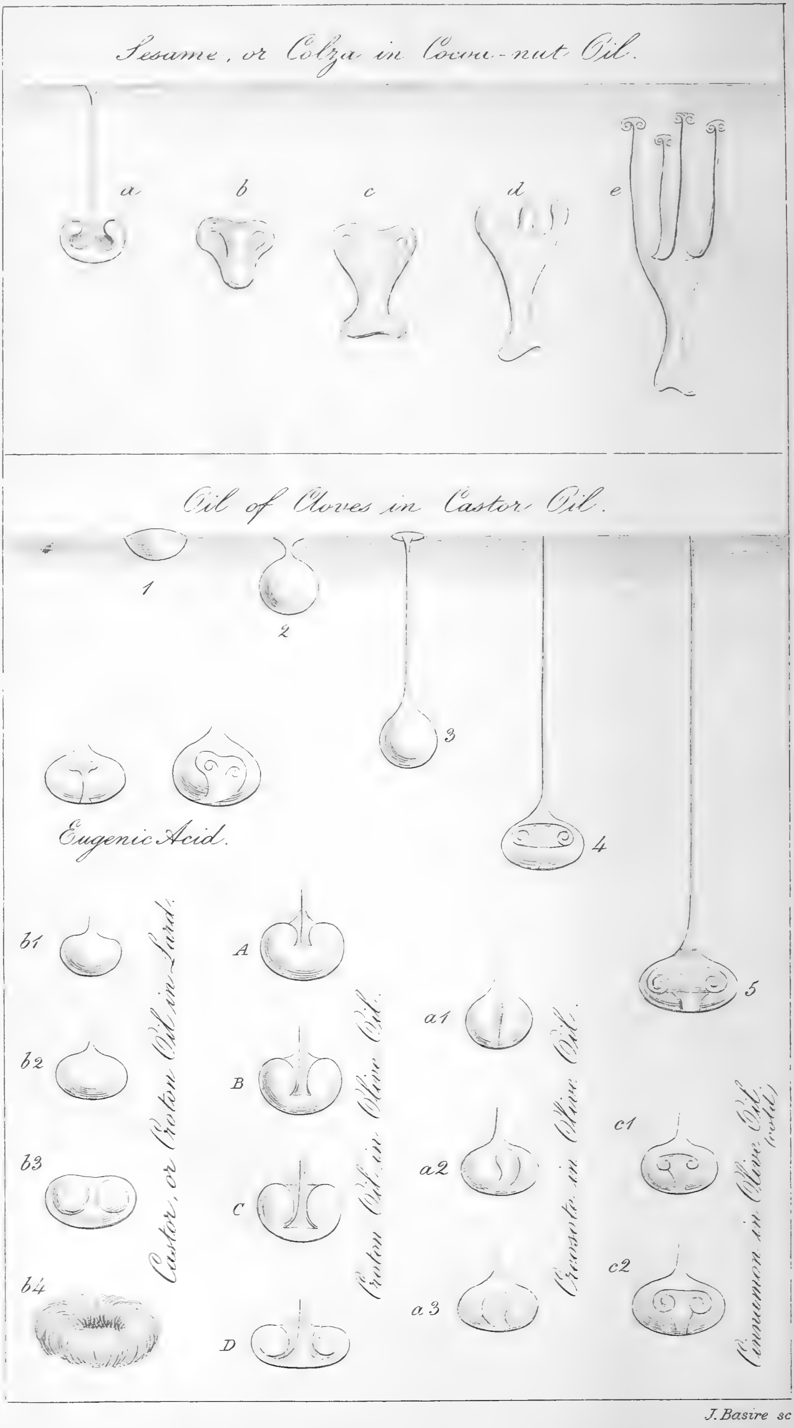
13 Oil of Lavender on Sulphur



Oil of Rosemary or Turpentine on Sulphur

15 Benzole on Sulphur

J. Basire sc


Sesame, or Colza in Cocoa-nut Oil.
a
b
c
d
e
Oil of Cloves in Castor Oil.
1
2
3
4
5
Eugenic Acid.
b1
b2
b3
b4
Castor, or Croton Oil in Lard.
A
B
C
D
Croton Oil in Olive Oil.
a1
a2
a3
Creosote in Olive Oil.
c1
c2
Cinnamon in Olive Oil. (cold)

J. Basire sc.

Malachite

Fig. 1.

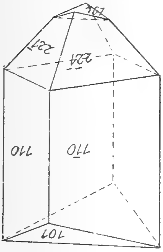
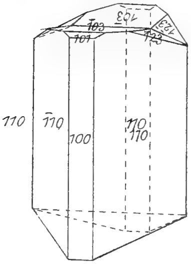


Fig. 2.



Gismondine.

Fig. 1.

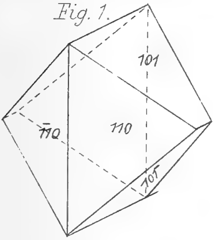
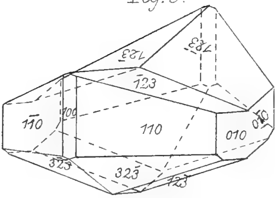


Fig. 3.



Herschelite.

Fig. 1.

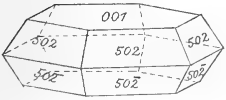


Fig. 2.

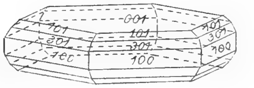


Fig. 3.

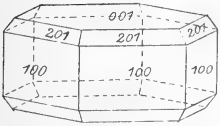
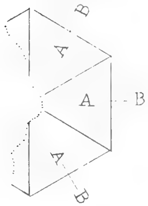


Fig. 4.



V. Lang fec.