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"Nec araneorum 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 condât,
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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[FOURTH SERIES.]

JULY 1868.

I. *On the Dynamical Theory of Heat.*

By JOSEPH GILL, Esq.*

THE first law of thermodynamics enounces that heat and mechanical energy are mutually convertible. The direct conversion of mechanical energy into heat seems sufficiently obvious from various known phenomena of friction and percussion; and the admirable experiments of Joule have shown, with an accuracy sufficient for all practical purposes, that in the process of friction mechanical energy is directly converted or transformed into heat in the proportion of 772 foot-pounds for each British unit of heat. The direct conversion of heat into work is not so obvious, and satisfactory experimental proofs of the fact are still wanting.

Physically considered, the universe has been defined as matter and motion; and, as far as human means are concerned, it is allowed that matter and motion can neither be created nor destroyed. In all the wonderful transformations of matter revealed by chemistry, it is well known that the quantities of the substances acted on are absolutely constant; not an atom is destroyed or created; and physical science now demonstrates in a manner almost equally convincing that motion, or the free unbalanced action of force on matter, as well as force itself in a potential state, where it is counterbalanced by some equivalent resistance, is also absolutely constant in all the various transformations through which it may be caused to pass. The genius of Lavoisier established the truth of the conservation of matter; and many living authorities of the highest scientific standing have concurred in establishing the certainty of the conservation of

* Communicated by the Author.

Phil. Mag. S. 4. Vol. 36. No. 240. July 1868.

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force,—a theory which appears to shed a fresh light upon almost every branch of physical inquiry, and to impart a new interest to subjects the study of which was already beginning to settle down into a quiet current of fixed opinions.

The grand simplicity of the idea of the conservation of force, together with the tendency to generalize which influences most inquirers in scientific matters, would naturally lead to the belief that if mechanical energy is directly convertible into heat, heat should be also directly convertible into mechanical energy, and, of course, in the same proportion. Hence it is assumed that when a hot perfect gas performs work by expansion in a cylinder supposed to be neutral to the thermometric effects of heat, and having a piston moving without friction, it must naturally lose as much heat in raising a weight of 772 pounds one foot as would raise the temperature of one pound of liquid water 1 degree of Fahrenheit's scale. By the same reasoning, steam, in performing work in vessels supposed to be equally neutral to heat, should lose exactly the same quantity of heat in doing the same quantity of work; and as perfect steam (neither superheated nor containing water in suspension) cannot lose heat without undergoing a corresponding amount of condensation, it is assumed that in the working of the steam-engine as much steam is condensed (theoretically) as corresponds to the amount of work performed in the proportion of Joule's equivalent, and consequently that the quantitative heat of the steam passing into the condenser is by so much less than the total heat of the steam furnished by the boiler to the engine.

More than thirty years ago Seguin, in his interesting work 'On the Influence of Railways,' endeavoured to trace the dynamical relation which he saw must exist between the heat applied to the boiler and the energy developed by the engine; and it is remarkable that, in many experiments made by him on the actual working of steam-engines, with the express purpose of proving the disappearance of quantitative heat which he thought must be converted into the work done, he could not detect the expected disappearance of heat.

Soon afterwards Mayer published some very interesting speculations on the mechanical equivalent of heat; and, from theoretical considerations on the effect of heat in increasing the elastic pressure of air, he deduced a numerical value of the dynamical equivalent, which agrees very closely with the result of Dr. Joule's independent and highly philosophical experiments. These experiments proved indisputably that mechanical energy is directly convertible into heat; but so far no experiments seem to have proved with equal clearness that heat was directly convertible into mechanical work. More recently this part of the inquiry

was taken up by the distinguished engineer M. Hirn of Colmar, who, like M. Seguin at an earlier period, instituted a series of experiments on the actual working of large steam-engines, to ascertain, if possible, the disappearance of heat equivalent to the work done as assumed by the dynamical theory; but he could not obtain satisfactory proof of the direct disappearance of quantitative heat from the working steam as an equivalent of the work done. These negative results led to a controversy, in which the correctness of the dynamical theory was ably sustained by Professor Clausius; and at his suggestion M. Hirn made a fresh series of experiments on the large scale as before, from which he still could detect no disappearance of the heat supposed to be changed into the work done. His apparent facts were again ably combated by the supporters of the dynamical theory; and finally, in 1862, M. Hirn published a work in which he admits that his former views were erroneous, and shows himself a convert to the dynamical theory, giving a fresh set of experiments on large engines, in which he shows that heat does disappear from steam in the act of performing mechanical work. He argues that it must be so, as work or heat would be produced from nothing unless we allow that an equivalent of heat has been actually transformed into the work done, or reproduced in the heat developed by friction—and consequently that, in all cases where work has been performed by a hot elastic fluid, the fluid must contain less heat after the work is done than it contained before, the heat which has disappeared being the equivalent of the work done, and in fact transformed into this work. Moreover, when a gas is compressed it becomes hotter; when it expands against a moderated resistance its temperature falls. In a simply dynamical point of view, this case is like the winding up and unwinding of a spring: force from some exterior source is transferred to the compressed fluid, which in expanding back against a moderated resistance to its initial tension, gives out the same amount of force. But how explain the heating and cooling of the gas? Formerly the explanation was easy, on the supposition that the specific heat of gases varied with the density, the capacity for heat increasing as the density decreased. Regnault's experiments showed that the capacity of gases for heat is constant, or nearly so; consequently the facts of the heating and cooling of a gas by compression and expansion are inexplicable, and really without a cause, unless we allow a direct relation between the work expended or produced and the heating and cooling of the gas.

It has been supposed that M. Hirn's more recent experiments, described in his *Exposition Analytique et Expérimentale*, prove an exact proportionality between the work performed by the engine

and the excess of heat supposed to exist in the steam as it leaves the boiler above the quantity of heat which it contains as it leaves the cylinder, which latter quantity is found in the condenser—in other words, that the work performed is proportional to the heat which disappears from the steam in its passage through the engine. After careful examination of the phenomena, I think it will be found that this statement, though on the whole undoubtedly true, is not yet clearly established by direct experimental proof.

The quantity of work performed was ascertained by a friction-brake; but in these experiments it was of comparatively small importance to ascertain the exact quantity of work performed in each case, the object being rather to effect a constant amount of work with a variable consumption of steam. The tension of the steam in the boiler, its degree of superheat, and the quantity and temperature of the water injected into the condenser being all maintained constant, the external work performed by the engine may be modified in two distinct ways.

(1) This work may be diminished or increased by opening more or less the steam admission-cock.

(2) This cock being kept full open, and consequently a free passage of steam through it, the amount of work may be increased or diminished by cutting off the steam earlier or later during the stroke. Therefore, reciprocally, the work may be maintained constant if, while the amount of cut-off is diminished, the steam-admission is also diminished by "wire-drawing."

By carefully clothing the cylinder, the loss of heat from exterior cooling may be reduced to a very small quantity. The proportion of work lost in friction and other resistances cannot be avoided to the same extent, nor is it easy to calculate it with even approximate exactness. But if the amount of work done by the engine is maintained constant while the consumption of steam is varied, as above mentioned, it may be supposed that the amount of work lost in friction &c. of the moving parts of the machine is also constant (or nearly so); so that the total quantity of external work performed by the steam may be supposed to be equal in each case. And as the consumption of steam to produce this constant amount of work may be caused to vary considerably (more than one-third in these experiments) by varying the mode of its admission to the cylinder, if still we find in each case that the *disappearance* of heat is the same in quantity, notwithstanding the variation in the quantity of steam consumed, it may be fairly deduced that the loss of heat in the steam is in all cases proportional to the amount of work performed.

To ascertain the amount of heat which disappears (or is changed into work) in each case, it is requisite, first, to know the quantity

of heat given to the steam before its admission to the cylinder, and, secondly, the quantity of heat given out by the steam in the condenser; the difference should obviously be the heat consumed in performing the work.

The quantity of heat required to evaporate a given weight of water under given conditions is known by Regnault's formulæ. Its value is complex; but this consideration does not greatly affect the present inquiry, as we are certain that the steam would give out in condensing all the heat it had previously absorbed for its formation, provided none of it be lost by any intermediate process—such, for instance, as the performance of external work at its expense.

In these recent experimental researches of M. Hirn, the steam on leaving the boiler was always more or less superheated, without change of pressure. This condition is indispensable for obtaining correct results, because ordinary steam generally carries into the cylinder a certain quantity of water, necessarily variable in amount, and very difficult to measure or estimate; consequently it is impossible in these circumstances to value correctly the quantity of heat furnished by the boiler.

The additional amount of heat required to superheat the steam a given number of degrees is also ascertained by a corresponding formula. We can thus ascertain the number of thermal units in the steam furnished to the cylinder in a given time; and, theoretically, this same quantity of heat should be found in the condenser, provided that none of it were lost during its passage through the engine by being changed into the work done.

The quantity of heat passing out from the engine in a given time can be easily ascertained by noting the quantity of water heated a certain number of degrees in the condenser during the same time. The difference between this quantity of heat and the total heat of the steam as it leaves the boiler in the same time should be the equivalent of the work done in that time.

In all these experiments the pressure in the boiler was maintained nearly constant, not varying more than one-tenth of an atmosphere; the temperature of the saturated steam was therefore known from the tables of tension. The temperature of the superheated steam was ascertained by a thermometer in the steam-pipe close to the cylinder, where the pressure was nearly the same as that in the boiler. The temperature of the condenser-water was taken every minute while the engine was in regular action, and was represented in each case by the mean of thirty observations.

To ascertain with accuracy the respective quantities of the steam and the injection-water employed in given times under the varying circumstances of the experiments was a task of great

difficulty. The quantity of steam furnished to the engine per second with the throttle-valve full open, and consequently the maximum amount of cut-off to produce the given constant amount of work, was ascertained by keeping the engine in constant action during a whole day; and the number of strokes made by the engine during this period was taken by a counter. By dividing the whole quantity of water by the whole number of strokes of the engine, the weight of steam for each stroke was ascertained.

It was much more difficult to ascertain the quantity of steam furnished to the engine in a given time while working without cut-off, and regulated to the same constant rate of work by throttling or "wire-drawing" the steam. In this case the consumption of steam, and consequently the consumption of fuel, increased; the furnace adapted for the more limited rate of combustion which sufficed for the more economical mode of working was not sufficient for the larger consumption; and under these circumstances the difficulty of maintaining the conditions of the supply of steam constant during a whole day's work was so great as to render it almost impossible to ascertain accurately by the former method the quantity of steam supplied; consequently another method was adopted for the purpose. The quantity of cold water supplied to the condenser, and the tepid water discharged from the air-pump in a given time, were each accurately gauged, the respective temperatures being taken into account; and the excess of the latter quantity over the former, representing the steam passing through the engine, being divided by the number of strokes in the given time, showed the weight of steam used per stroke.

The first series of experiments described by M. Hirn were made with a large engine making twenty-seven strokes or revolutions per minute, and producing the constant work of 150 horse-power, as shown by a friction-brake. Under the most economical conditions of the action of the engine this work was produced from k. 0.34554 of steam per second, representing 228.16 French thermal units or calories; and under the least economical conditions the steam used was k. 0.46927 per second, equal to 286.38 calories. In the former case a disappearance of 40.34 calories was observed; and in the latter case heat equal to 38.08 calories disappeared.

A second set of experiments with the same engine, producing work equal to 116 horse-power, showed a consumption of k. 0.23548 of steam per second in one case, and k. 0.32307 in another—the observed loss of heat being 30.51 calories in the first case, and 32.13 calories in the second case.

Another set of experiments with a smaller engine, making

ninety-three revolutions per minute, and producing the constant work of 90 horse-power, showed the steam used to be k. 0.1902 per second in one case, with an observed loss of heat equal to 21.5 calories; and in another case the consumption of steam was k. 0.229 per second, with a loss of heat equal to 22.58 calories.

In the most economical of the above experiments a constant amount of work equal to 116 horse-power was obtained from k. 0.23548 of steam per second, the steam, of five atmospheres' pressure, being superheated 93° C. (from 152° to 245°) and cut off early, so that the expansion was as 6 : 1. In another experiment, the steam being equally superheated and cut off near the end of the stroke, so that the expansion was only as 1.15 : 1, the rate of work being kept constant by throttling or wire-drawing the steam, the same 116 horse-power required the consumption of k. 0.32307 of steam per second, being 36 per cent. more than in the former case. Now, everything else being equal, the furnace must furnish in the second case one-third more heat than in the first case. If, therefore, reasons M. Hirn, we find in the condenser-water also one-third more heat, we may fairly deduce that the amount of heat consumed in its passage through the engine depends entirely on the quantity of external work produced; for by this mode of experimenting, the quantity of work obtained being constantly 116 horse-power, the proportion of loss from friction &c. should be the same in all cases, and therefore need not be calculated. Now the heat which disappeared was nearly equal in both experiments, being 30.51 calories in the first case, and 32.13 in the second; and the mean of all the experiments showed a still nearer approach to equality. It was therefore deduced that in all cases an exact proportionality exists between the work produced by the engine and the difference between the quantity of heat furnished before the steam enters the cylinder and the quantity of heat remaining in the steam as it leaves the cylinder.

As a result of my own experiments, and from what I have understood of the experimental investigations of Seguin and of Hirn on the working of steam-engines, I have for many years entertained the opinion that the *direct* change of heat into work has not been satisfactorily shown by experiment; and it still appears to me that when the heat employed in working the engine is supposed to be merely the heat of the steam as it enters the cylinder (whether saturated or superheated), and *when the steam works at full pressure throughout the whole stroke*, none of this heat should disappear theoretically in the production of work, as in this case all the heat of the steam which passes through the cylinder should be found in the condenser. Con-

sequently, as regards tangible proof from direct experiment, M. Hirn's earlier opinion that heat did not disappear from the steam in the production of work may have been really in accordance with facts as far as they were perceived, though at variance with the whole truth; and I would submit that the conclusions arrived at from his more recent experiments, as above quoted, though in accordance with truth, may be questioned as to their satisfactory proof by experiment. In short, I now imagine that our investigations on the subject by experiments on the actual working of steam-engines have not been conducted on really correct grounds, and that the results have therefore been anomalous. It is no doubt true that the heat furnished by the fire is in all cases more than the heat which passes into the condenser; and if no heat were otherwise lost, this excess would correspond to Joule's equivalent; but when the engine works with the full boiler-pressure throughout the whole stroke, it seems evident that the whole work is in effect done in the boiler by the expansion of the water into steam, and is merely transmitted by the steam to the piston; so that the heat equivalent to the work done disappears from the boiler, and is locally made up by the fire directly without any indication of these phenomena being perceptible in the subsequent parts of the process of working the engine. If this view of the subject is correct, it is difficult to perceive how M. Hirn's deduction of an exact proportionality between heat lost and work done could be fairly proved from the experiments (though doubtless the law is true); for with a cut-off at one-sixth of the stroke, about two-thirds of the work would be done by the expansion of the isolated steam in the cylinder, with a corresponding disappearance of heat in the condenser, which should be very perceptible; while in proportion as the engine worked with steam approaching to full pressure, the proportion of work actually done by the steam in the cylinder would become less and less, until the disappearance of heat from the working steam might become inappreciable.

In order to maintain the work of the engine constant with a variable consumption of steam, throttling or wire-drawing was used more as the expansion from cut-off became less; and as the process of wire-drawing increases the heat of the working steam *at the expense of the heat in the boiler*, the quantity of heat passing into the condenser would be affected also by this cause.

It is certain that the performance of work by the expansion of isolated steam drawing solely on its own self-contained sources of energy must cost to the steam a full equivalent of force in some shape; and as in this case no other form of force can be directly detected except heat (or molecular motion, as it is now

defined), and it is mathematically demonstrated that heat should disappear in such cases and might be transformed into the work done, it may be freely admitted that the work performed by the expansion of isolated steam is accompanied by a corresponding disappearance of heat. This disappearance of heat, if the steam is saturated, should cause a corresponding condensation of part of the mass, or an equivalent amount of cooling if the steam is sufficiently superheated to bear abstraction of heat without condensing. If a reservoir of saturated steam be put in free communication with a cylinder and loaded piston, the piston's outward motion would be accompanied by an expansion of the whole mass of steam, and a condensation of steam-particles throughout the mass equivalent to the amount of work done by the piston. If, now (the communication with the working cylinder being closed), fresh steam from some other source be forced into the reservoir until its original pressure is recovered, the compression thus effected should reevaporate the liquid particles of condensed steam, and the general mass of steam should become again dry, or merely saturated. If yet a further quantity of steam equal to a stroke of the working piston be injected into the reservoir, the mass of steam should become superheated; and the fall of temperature and pressure consequent on the succeeding stroke would now bring back the steam to its initial state of saturation. Thus it would seem that heat should equally disappear from an isolated mass of steam doing work by expansion, whether the steam be saturated or superheated; but in the case here imagined the disappearance of heat would be from the whole mass in the reservoir and cylinder together.

In the circumstances above imagined, the source of a continuance of work would be the injection of a continued supply of steam into the reservoir; and the operations of injecting and withdrawing steam being intermittent and occurring at alternating intervals, a corresponding fluctuation of density and temperature would occur in the whole mass, with periodic superheating or condensation, as the case might be, but without permanent disappearance of heat except in the cylinder, the contents of which formed part of the general mass during the working stroke, and participated in the general loss of heat assumed to be transformed into the work done. Thus, if the cylinder be $\frac{1}{10}$ of the volume of the reservoir, the disappearance of heat from the cylinder itself would be $\frac{1}{10}$ of the whole heat supposed to disappear; the remaining $\frac{9}{10}$ would be replaced in the reservoir by the succeeding stroke of the injecting pump, while the contents of the cylinder would be discharged into the condenser in the state in which the fluid remained at the end of the working stroke.

If the steam supplied to the reservoir were injected at the same time and at the same rate as the steam is withdrawn in giving motion to the working piston, so that the density and pressure in the reservoir should remain constant, it would appear that the whole work would be done directly by the injecting piston, and merely transferred to the working piston by means of a moveable plug of steam at constant pressure and temperature. In this case no heat should disappear either from the reservoir or from the cylinder; and the contents of the cylinder at the end of the working stroke should be identical with an equal mass of steam of corresponding pressure and temperature from any common source.

The formation of steam from the water in a boiler may be considered equivalent to the injection of steam ready formed from some other source; and during the whole stroke, if at full pressure, or the full-pressure part of the stroke if working expansively, only a small fraction of the heat assumed to be converted into work would disappear from the cylinder itself, being in the proportion of the contents of the cylinder to the whole contents of the steam-space in the boiler, and smaller as the fluctuations of pressure in the boiler due to the intermittence of supply to the cylinder are bounded by narrower limits. On the other hand, all the loss of heat which corresponds to the work done by expansion in the cylinder after the cut-off should take place in the cylinder itself. Hence, in experimental researches on the assumed disappearance of heat from the steam in the act of doing external work, we should not expect to find tangible proof of this phenomenon by comparing the quantity of heat in the steam working at full pressure in the cylinder during the whole stroke with the amount of heat found in the condenser; for theoretically these quantities may be nearly equal, if, as supposed above, the work given out by the piston is virtually done in the boiler; and the steam which fills the cylinder at the end of a working stroke may differ but slightly from an equal weight of common saturated steam of the same pressure taken directly from a boiler. If such be the case, it were useless to search *in this part of the process* for a loss of heat equivalent to the work done; for the exhaust-steam dashing tumultuously into the vacuous condenser should produce there as much heat as there is cold produced in the cylinder by the expansion which drives out the steam—as in Joule's experiment of causing compressed air to expand into a vacuous receiver, by which process the total quantity of heat remains unaltered. This view of the case may help to account for the apparently anomalous results obtained by Seguin and by Hirn in their researches on this subject; and as in my own experiments sufficient account was not taken of the influence of

expansion from cut-off, I think I can now detect sufficient cause why I could not discover the disappearance of heat which might be fairly attributable to a transformation into the work done by the engine.

On the other hand, if an isolated mass of steam, for example the steam which fills the full-pressure portion of the stroke at the moment of cut-off, be caused to produce work by its own expansion during the remainder of the stroke, it must lose something equivalent to this work—apparently a proportional quantity of its own heat; and as saturated steam cannot part with any of its heat without suffering a condensation of some of its particles, it should follow that the moderated expansion of steam in the act of doing work should be accompanied by the condensation of steam-particles throughout the mass, representing an aggregate amount of heat equivalent to the work done. Thus the steam working expansively in the cylinder isolated from exterior sources of heat or cold, should assume the condition of a mist from the formation of numerous minute water-particles throughout its mass; and as the heat which had previously maintained these particles in the state of vapour no longer exists in the cylinder, having, in fact, been transferred or transformed into the work done by the expansion, the remaining mass of expanded steam must contain so much less heat, and should accordingly show less in the condenser.

The grand principle of the Conservation of Energy indicated with indisputable certainty that the work done by a steam-engine must cost its full equivalent to the fire; but in the absence of any direct proof of the disappearance of this equivalent of heat from the steam in its passage through the engine, I was fain to suppose that the heat in passing into the boiler might undergo a transformation into some form of repulsion in the vapour-particles corresponding in intensity to the temperature and pressure of the steam—and that the work done might be attributed to an equivalent disappearance of this repulsion with a fall of temperature and tension in the steam, but without loss of its quantitative heat, considered simply as molecular motion. It will perhaps be allowed that this hypothesis was not altogether unreasonable so long as there was no satisfactory experimental proof of the direct disappearance of heat from the steam in its passage through the engine; and even if I had proved this disappearance of heat, I might still have remained in doubt as to the mode of the change of heat into work so long as I was uncertain whether this process might take place simply on mechanical principles applied to molecular action, as shown in Professor Rankine's hypothesis of molecular vortices. This uncertainty has been removed by the complete confirmation of the results of Regnault's

experiments, showing a constant specific heat of air at different densities which I have recently obtained from a repetition of some of his experiments in a different shape; and I hope to be able soon to complete some experiments which should furnish additional proof of the actual disappearance of heat in the production of work in the steam-engine. The want of such direct proof is acknowledged; and its importance is evident when we consider how intimate is the connexion between a more extensive knowledge of this subject in a practical point of view and the improvements to be made in the wide field which still remains open for effecting economy of fuel in the working of our thermic prime movers.

Palermo, April 10, 1868.

II. *On Vortex-rings in Air*.

By Professor ROBERT BALL.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE production of vortex-rings in air is experimentally shown by the interesting phenomena produced by Professor Tait. These are described by Sir William Thomson in the *Philosophical Magazine* for July 1867. I repeated these experiments with the kind aid of Dr. Emerson Reynolds; in doing so an interesting variation of them presented itself which renders the air-rings actually visible.

While engaged on this subject, Mr. Yeates suggested to me to try the effect of discharging an air-ring at a column of smoke; this was done, and a most curious and unexpected appearance was the result. The box employed was a 2-foot cube, and had a hole 8 inches in diameter, the side opposite having a piece of stout sacking strained over it. A blow on the sacking causes an air-ring to dart from the hole. Two large flasks containing respectively hydrochloric acid and ammonia were arranged with their mouths in close proximity and their contents vigorously boiled; from the union of their vapours a dense column of the fumes of chloride of ammonium ascended. This apparatus was placed about eight or ten feet in front of the box. An air-ring on first leaving the box was of course invisible; when it reached the column of smoke it could be seen to force its way through it; but when it left the column and during its subsequent existence its appearance was very remarkable. At first the idea suggested itself that the single air-ring had formed two concentric smoke-rings; but closer examination and an improved column of smoke showed what had really occurred. The air-ring had penetrated

the smoke quite uninjured ; it had not apparently left any of its particles behind, nor had it admitted an atom of smoke into it ; but it had drawn with it sufficient of the smoke to form a complete shell, which enclosed it, and thus rendered the air visible. The phenomenon is quite in accordance with that conservative tendency which theory shows must belong to a vortex-filament. The appearance is one of great beauty, and suggested the name "negative smoke-ring."

It was considered desirable to make these experiments before an evening scientific meeting of the Royal Dublin Society. Smoke-rings are seen to greatest perfection when directed along a sunbeam admitted into an otherwise darkened room ; and though they can be seen beautifully in ordinary diffused sunlight, by gaslight they make a very poor show ; special means of illumination are therefore necessary. The following arrangement is found to succeed admirably. By a simple optical contrivance the radiation from a brilliant lime-light is concentrated into a slightly conical beam, all other light in the room being extinguished. The box is placed in the beam, the orifice facing the lamp, and from twenty to thirty feet distant from it ; at the box the diameter of the circular section of the luminous cone is from 3 to 4 feet. A smoke-ring driven from the box will, if suitably directed, traverse the beam through its entire length until it reaches the lamp ; and as it is brilliantly illuminated throughout its path, the appearance presented is of great beauty. A second box can be placed at the lamp end of the beam, so as to show the effect of the collision of rings. The fumes produced by burning a small piece of phosphorus developes an ample supply of smoke in the interior of the box. This mode of producing smoke was suggested to me by Dr. William Barker ; it is simpler to manage inside the box than the apparatus necessary for forming chloride of ammonium. By the introduction of coloured glasses various pleasing effects can be produced. If a sheet of tissue-paper be very lightly attached to a frame and interposed in the beam, a vigorously sent smoke-ring will sweep it away in a striking manner.

To exhibit the air-rings, the column of smoke already described is placed so as to ascend through the beam, and rather nearer the box than the lamp. An air-ring from the box is, of course, invisible till it reaches the column, while in its passage from the column to the lamp the curious phenomenon already mentioned is most beautifully shown. The negative smoke-rings are much better seen when thus illuminated than by ordinary daylight. A box of the dimensions previously given, viz. a 2-foot cube with a hole of 8 inches diameter, was found to answer better for this purpose than a somewhat smaller box with a hole of 6 inches aperture.

The column of smoke may perhaps serve as a test for the existence of vortex-rings in other cases, when, produced in air, they are invisible. This was tried in one instance. It is a common trick to blow out a candle by the puff of air from the muzzle of a gun when a percussion-cap is exploded on the nipple. This puff is probably a vortex-ring. Owing to the high velocity with which it moves, it is difficult to trace the effect produced by the column of smoke. One person, however, firing from an elevated position and at a distance of fifteen or twenty feet from the flasks down upon the column, and another watching against a dark background, a distinct ring was occasionally seen to dart from the column after the percussion of a cap; but whether these were "negative rings," as was expected, or ordinary "smoke-rings," was not easy to determine.

I remain, Gentlemen,

Yours &c.,

Royal College of Science for Ireland,
Dublin, May 28, 1868.

ROBERT BALL.

P.S.—Since writing the foregoing, it occurred to me to fill the box with ammoniacal gas and to discharge rings from this at a column of the vapour of hydrochloric acid. In this case the ring, before reaching the column, is perfectly invisible, and the existence of the column is only seen by slight traces of partially condensed vapour. As was expected, a beautiful ring appeared, from the combination of the two gases, when the ammoniacal ring reached the column.

June 16, 1868.

III. *On C. S. Lyman's new form of Wave-apparatus*.*

THE theory of waves that has been generally taught since the days of Newton is that which represents wave-motion as consisting in the alternate rising and falling of the parts of a liquid in vertical lines, as in the two branches of a U-shaped tube; this is usually cited as Newton's theory of waves. There is to be found, indeed, in the *Principia* the hypothesis of vertical oscillations, and also the cut of the bent tube, so persistently copied by subsequent writers; yet it is evident that Newton adopted the hypothesis rather as an expedient for a special purpose (that of finding approximately the relation of a wave's length to its period) than as strictly true to nature; for he concludes his investigation with the remark, "These things are true, upon the supposition that the parts of water ascend or descend in a right line; but, in truth, that ascent and descent is rather performed in a circle [*verius fit per circulum*]; and there-

* From Silliman's American Journal for January 1867.

fore I propose the time defined by this proposition as only near the truth." This important qualifying clause seems to have been quite overlooked by those who have professed to give Newton's theory of waves.

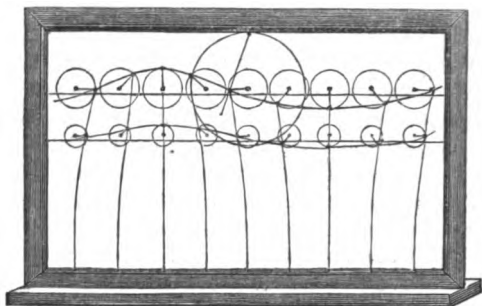
The modern theory, which teaches that in wave-motion all the particles of a liquid are revolving synchronously in vertical circles, though so broadly hinted at by Newton himself as the true one, in the words italicized above, has nevertheless been developed for the most part only within the present century. It was first clearly stated and ably advocated by Gerstner in 1804. More recently it has been satisfactorily established as accordant with observed facts and the laws of mechanics, by the experimental researches of Weber and Scott Russell, and the mathematical investigations of Stokes, Rankine, and others. A concise exposition and demonstration of the theory may be found in a paper by Professor Rankine, in the *Philosophical Transactions* for 1863. Though but just beginning to find its way sparingly into the text-books, it has become the generally accepted theory among men of science; and having in reality supplanted the old hypothesis as an expression of scientific truth, there is no good reason why it should not also supplant it in the lecture-room.

In teaching this theory, however, it is often found difficult to make pupils understand how the infinitude of simultaneous revolutions, which it supposes, can take place without mutual interference, and in such a way as to produce the observed phenomena. It was to obviate this difficulty, and illustrate, as far as practicable, the *modus operandi* in question, that the piece of mechanism was devised which is the subject of this article. It presents to the eye not merely the surface contour of a wave with its undulatory motion (which to some extent other forms of wave-apparatus also do), but, besides doing this in a more exact and satisfactory manner, it exhibits the motions also that are at the same time taking place below the surface in the whole mass of liquid affected. This completeness of illustration is due to the circumstance that in the apparatus the elementary motions are the same essentially as in actual waves; hence the leading points, geometrical and dynamical, of the theory of waves are presented naturally and in their true relations.

The construction of the apparatus will be readily understood from a brief description and the accompanying woodcut.

In front of a plane surface are two series of revolving arms or cranks, the length of the lower ones being half that of the upper. Two elastic wires connect the crank-pins of each series; upright wires also connect each pair of cranks, and pass down through a plate into the base. The cranks all revolve synchronously; they thus keep their relative position, and come into any given position

successively, each in its turn. The relative position of the cranks of each horizontal series is such that the directions of any two, in



regular order, differ by the same fraction of a whole revolution that the distance between their axes is of a whole wave-length. Thus in the apparatus the wave-length is supposed to be divided into eight equal parts; and hence the common difference between the directions of adjacent crank-arms is one-eighth of a circle, as shown in the figure. The cranks in each vertical set have their positions always alike. The number of cranks, whether taken horizontally or vertically, is arbitrary—a matter of convenience in construction. The synchronous revolution of the cranks is effected by means of any suitable mechanism—such as equal toothed wheels on the several axes, with alternate idle wheels connecting them; or equal rag-wheels, with endless chain or metallic ribbon; or equal cranks, with a rigid connecting frame or plate. The first method is used in the original machine, the third in the model for the Patent-Office, the second and third in the larger and smaller sizes respectively for the market.

The crank-pins represent as many liquid particles, the circles on the background their orbits. The transverse wires represent continuous lines of particles, which at rest would be horizontal, and be represented by the lines on the background drawn just below the centres of the orbits—the upper one of these being the surface-line, the lower a line of particles one-ninth of a wave's length down. The upright wires represent lines of particles which at rest would be vertical. Every point in these moving lines describes its own distinct orbit.

The apparatus is constructed to a scale, and so represents a wave of given length, height, and period; but equally represents also a wave of any other length and proportionate height, though of period proper to its length, according to the law of that relation, as stated further on. In the original instrument, for example, the wave-length is 36 inches, height from trough to crest 4 inches, and period for that length 0.76 second; but it

equally represents a wave whose length is 36 feet and height 4 feet, with period 2.63; and similarly for other proportional dimensions.

Among the particular points in wave-phenomena which are elucidated by this apparatus may be enumerated the following:—

1. *The undulating surface-profile*.—This is shown in the motion of the upper flexible wire, which presents a continuous contour-line, of the exact curvature throughout of a true normal wave—instead of a broken contour of arbitrary form, by means of rising and falling balls, as in the ordinary wave-apparatus.

2. *The undulatory motion of all subprofiles, or lines of equal pressure down to still water*.—The representative of such lines is the lower transverse wire, which moves similarly to the upper one, but with a less curvature. Every such line of equal pressure is a continuous one, composed of particles in a state of dynamical equilibrium, and constituting an ideal moving wave exactly as if at the surface, the corresponding phases of all such waves being on vertical lines.

3. *The genesis of the undulatory motion from the circular motion of revolution*.—This is seen in the mode in which the crank-pins, in each transverse series, or the particles which they represent, come in regular succession into a given position as they revolve synchronously in their orbits.

4. *The equality of the height of a wave from trough to crest, with the diameter of the orbits of the surface particles*.—This is obvious in the apparatus, and follows directly from the mode in which the wave-surface is generated.

5. *The direction of motion of particles of water in the different phases of a wave*.—A glance at the motion of the crank-pins shows that a particle at the wave's crest is moving forward, or in the direction in which the wave is propagated, and a particle at the trough in the reverse direction, or backward, that a particle on the forward slope is rising, and one on the back slope descending. The same is true of particles in all the subwaves, or surfaces of equal pressure, down to still water.

6. *The length of a pendulum keeping time with the wave*.—This is equal to the radius of a circle whose circumference is the wave's length. Such a circle is the large one drawn on the background, as shown in the figure. Its radius is to that of a particle's orbit (or length of a crank-arm) as the particle's weight is to its centrifugal force. Or putting R and r for these radii respectively, and t for the time of revolution, we make

$$R : r :: g : \frac{4\pi^2 r}{t^2},$$

whence

$$t = 2\pi \sqrt{\frac{R}{g}};$$

which is the period of a revolving pendulum, or the time of a double oscillation of a simple pendulum whose height is R . Compare (10).

7. *The dependence of a wave's period on its length alone, not on its height.*—This is a corollary from the preceding. The period varies as the square root of the length, and is the same for all subwaves as for the surface-wave, the length being the same for all. The height, within certain limits, is independent of the length, as appears in the apparatus, and as may be inferred from the formulæ given further on. It depends on the centrifugal force of the particle, and this ultimately on the external forces generating it.

8. *The varying direction and intensity of the resultant force acting at each instant on a given particle in a wave.*—The component forces are two—the particle's gravity and its centrifugal force. The former is represented by the vertical radius of the large circle, the latter by the radius vector of the revolving particle; their resultant, then, is represented by the third side of the triangle of forces, or the side formed by the wire pendulum. This resultant must be always normal to the wave-surface, as the wire pendulum is seen to be always at right angles to the elastic wire representing that surface.

9. *The condition of a wave's rupture at the crest.*—When the centrifugal force becomes equal to gravity (or the radius of the orbit to that of the large circle), the resultant force for a particle at the highest point of its orbit or crest of the wave must be zero, and the particle consequently fly from its orbit, or the crest break in foam.

10. *The trochoidal form of the wave-curve.*—The point of suspension of the pendulum, that is, the upper extremity of the vertical radius of the large circle, may be regarded as the instantaneous centre about which an element of the wave-curve at the point of normality of the pendulum is described. Consequently, if this circle be rolled under a horizontal straight line, a point within it distant half the height of a wave from the centre will trace the wave-profile, which therefore is a trochoid. The rolling circle is the same for all wave-profiles down to still water, the lengths of the tracing-arm only differing. The circumference of this circle equals, of course, the wave's length.

11. *The greater sharpness of the crests than of the troughs of waves.*—This follows from the preceding, and is shown in the relative positions of the crank-pins—nearer together at the crests, further apart in the troughs. The trochoids become necessarily sharper at the upper bend, and less so at the lower, as the tracing-arm approaches to an equality with the radius of the rolling circle; until, when that equality occurs, the trochoid

passes into the cycloid, which has sharp cusps. The cusp of the inverted cycloid, then, is the limit of sharpness of a wave's crest. The equality above named is equivalent to that of the centrifugal force of a particle with its gravity (9). When the latter condition occurs, the wave-curve is cycloidal, and only then.

12. *The limits of possible curvature of waves.*—That curvature must always lie between the cycloid at the one extreme, and the straight line at the other, embracing trochoids of every possible variety.

13. *The greater elevation of the crests above the level of still water, than depression of the troughs below it.*—The difference between this elevation and depression is equal to twice the height due to the orbital velocity of the particles—that is, to twice the height from which a body must fall to acquire that velocity—or is a third proportional to the radius of the rolling circle and that of the particle's orbit; that is, putting R and r for these radii respectively, v for the orbital velocity ($= \frac{2\pi r}{t}$), and D for the difference in question,

$$D = \frac{r^2}{R} = \frac{v^2}{g}.$$

When r equals R , then $D = r$, or half the height of the wave.

14. *The elevation of the centres of the orbits of particles above the positions of the same particles at rest.*—This is shown in the distance of the axes above the corresponding lines on the background. These lines show the positions of lines of particles at rest, which in motion form the wave-profiles represented by the transverse wires. The elevation in question is equal to the height due to the particle's orbital velocity, or is a third proportional to the diameter of the rolling circle and the radius of the orbit, or is equal to the area of the orbit divided by the length of the wave; that is, putting H for this elevation, l for the wave's length, and the other symbols as before,

$$H = \frac{r^2}{2R} = \frac{v^2}{2g} = \frac{\pi r^2}{l}.$$

When r equals R , $H = \frac{r}{2}$, or one-fourth the height of the wave.

To this elevation is due one-half the mechanical energy of a wave, the other half to the motion of its particles. That energy is, in other words, half potential, half actual.

15. *The decreasing diameter of the orbits with depth.*—This is seen in the shorter crank-arms below, and the decreasing amplitude of sway of the upright elastic wires down to their points of rest, which mark the depth of still water. The decrease of the orbits in diameter takes place in a geometrical ratio, and is ap-

proximately one-half for each increase of depth equal to one-ninth of a wave-length; or more exactly, putting r and r' for the radii respectively of a surface-orbit and of one whose middle depth is k , it is

$$r' = re^{-\frac{k}{R}},$$

R being, as before, the radius of the rolling circle, and e the base of the Napierian logarithms.

16. *The peculiar swaying motion of continuous lines of particles of equal pressure which at rest are vertical.*—These lines are alternately lengthened and shortened, and bent to right and left, as represented by the upright elastic wires.

17. *The varying distortions undergone by blocks or sections of water originally rectangular, or rectangular when at rest.*—Such sections are represented by the spaces between the wires, and their distortions by the distortions of these spaces.

18. *The fact of sensibly still water at half a wave's length below the surface.*—This is exhibited in the absence of lateral motion at the lower extremities of the upright wires, and is a necessary result of the law of diminution of orbits with depth, as given above (15).

19. *The varying strain in wave action on floating bodies.*—This is seen in the varying angle made by the upright wires with the upper transverse wire: the latter shows the position of a raft, for example, lying on the wave-surface; the former, that of a long thin body, as a board floating end down; hence the varying relative direction of the wires shows the strain to which a body is subjected, having both breadth and depth, as the hull of a vessel.

Many other points besides the above may be studied to advantage in connexion with this apparatus, but it is not important to specify them here. Enough has been stated to illustrate its utility, and indicate in what respects it differs from every other form of wave-apparatus.

For convenience of reference and for the sake of completeness, a few formulæ are added, expressing other relations among wave-phenomena not so directly exhibited by the instrument, but important to be presented in connexion with it. Putting V for the velocity of propagation of a wave, and the other symbols as before, the length of the wave is

$$l = 2\pi R = \frac{gt^2}{2\pi} = \frac{2\pi V^2}{g} = tV;$$

its period

$$t = \sqrt{\frac{2\pi l}{g}} = 2\pi \sqrt{\frac{R}{g}} = \frac{l}{V};$$

the velocity of a particle in its orbit, or at the crest of the wave,

$$v = \frac{2\pi r}{t} = r \sqrt{\frac{g}{R}} = r \sqrt{\frac{2\pi g}{l}} = \frac{Vr}{R} = \frac{2\pi r V}{l} = \frac{gr}{V} = \frac{tgr}{l};$$

the velocity of propagation of the wave

$$V = \frac{l}{t} = \frac{gt}{2\pi} = \sqrt{\frac{gl}{2\pi}} = \sqrt{gR} = \frac{gr}{v};$$

the sine of the angle of steepest slope of surface is

$$\sin \theta = \frac{2\pi r}{l} = \frac{r}{R}.$$

It will be understood that the normal wave, to which the theory applies, and which the apparatus illustrates, is the wave on deep water, or water a wave's length at least in depth. In shallow water the orbits are no longer circles, but ovals, or approximate ellipses of less height than length, according to the degree of shallowness.

When waves pass from deep into shallow water, as toward a beach, they become gradually shorter, their total energy is imparted to a less and less mass of liquid, and the extent of the motion of the particles is proportionately increased. The crests also travel faster than the troughs; so that the front of each wave becomes by degrees steeper than the back, and at length curls forward and falls over, exhibiting the well-known roll of surf. The formulæ for waves in deep water, therefore, require modification to adapt them to waves in shallow water, where depth of liquid and ellipticity of orbit enter as elements.

It has been necessary, in order properly to explain the apparatus and its uses, to give more fully the leading points of the theory of waves than would be required, were the works containing it more generally accessible. For these points the papers of Professor Rankine have been chiefly consulted. It is hoped that this outline of the theory, thus incidentally given, will prove not unacceptable to such instructors as may not have at hand the original works, and that this new piece of apparatus may contribute somewhat toward imparting a clearer understanding of the phenomena of waves.

The apparatus has been patented, and is manufactured by Messrs. E. S. Ritchie and Son, the well-known philosophical-instrument makers, of Boston, Massachusetts.

IV. *On the Velocity of Sound in Tubes.*

By A. KUNDT*.

TWO years ago the author laid before the Academy a method of determining the velocity of sound in elastic fluids, which consists in setting in vibration the column of air enclosed in a longitudinally vibrating tube. These vibrations may be made visible by light powder strewed in the tube; and by measuring the resultant dust-figures, the wave-lengths of the same exciting tone in different gases, and therewith the corresponding velocities of sound, may be readily found†.

Since it is not always certain in these experiments that the exciting tone of the longitudinally sounding glass tube remains in successive experiments exactly the same, the apparatus was first of all so altered that not only did one end of the longitudinally vibrating tube produce dust-figures in an enveloping tube, but also the other end in a second tube. If then the tube on one side was filled with the gas to be investigated, and that on the other with air, and the dust-figures were produced, the wave-lengths measured in the gas and in air, belonging to precisely the same tone, gave the exact ratio of the velocities of sound in the gas and in air.

When, however, the velocities of sound in the simple gases were examined by this method, it was observed that, in spite of the utmost care in purifying and drying the gases, and in spite of frequent endeavours, discrepancies were met with in the determinations, which seemed to indicate that sources of error as yet unknown influenced the result.

It was, moreover, ultimately found that these sources of error are not specially incident to the method used, but under certain circumstances always occur when sound is transmitted through tubes. The author accordingly subjected to a minute examination all the circumstances which might influence the velocities of sound in elastic fluids in tubes. At first, for the sake of simplicity, only air was used.

The mode in which the experiments were made was as follows:—The tube on one side of the apparatus was always unchanged, so that the dust-figures were always produced in it under the same conditions, and their length could therefore be taken as the normal length; on the other side were placed the tubes to be investigated, in which the velocity of sound was to be determined under varied conditions; and the dust-figures were simultaneously produced in the two. The ratio of the wave-

* Translated from the Berlin *Monatsbericht* for December 1867.

† Phil. Mag. January 1868, p. 41.

lengths of the two sides gives directly the ratio of the velocities of sound in the air in the two tubes.

The results of the investigation are as follows:—

(1) The velocity of sound in air is diminished when the diameter of the tube is decreased. The diminution, however, only becomes perceptible below a certain diameter; in all wide tubes the velocity of sound is equal. A decrease in the velocity is sometimes perceptible when the diameter of the tube is equal to a quarter wave-length of the tone used. The amount of the diminution of velocity with a decreasing diameter may be seen from the Table under (2).

(2) The diminution of the velocity of sound in air in tubes increases with the wave-length of the tone used. The following Table gives the velocities of sound of the air in five tubes of different diameters with three different notes, whose semiwave-lengths are as 90, 45, and 30 millims. The velocity of sound in the widest tube is equal to that in the open air = 332·8 metres.

Velocity of Sound for various Notes in glass tubes of varying diameter.

Semiwave-length of the note . . }	$\frac{L}{2} = 90$ millims.	$\frac{L}{2} = 45$ millims.	$\frac{L}{2} = 30$ millims.
Diameter of the tube.	Velocity of sound, that in the widest tube being = 332·8 metres.		
millims.	metres.	metres.	metres.
55·0	332·80	332·80	332·80
26·0	332·73	332·66	333·45
13·0	329·47	329·88	330·87
6·5	323·00	327·14	328·14
3·5	305·42	318·88	

(3) Powder strewed in a tube leaves the velocity of sound unaltered in wide tubes; in narrow ones it diminishes it the more the greater its quantity. This diminution may amount to 10 metres*.

(4) The influence of the powder is increased when it is very finely divided and is violently moved by the motion of the sound—as, for instance, when silicic acid is used.

(5) If the wall of the tube is roughened on the inside, or if a larger or a rougher surface is exposed to the air by means of paper or metal sheets inserted and fitting as close as possible the wall of the tube, the velocity of sound in narrow tubes is thereby materially diminished; and this diminution is considerably more than could be produced by the mere diminution in the section

* In the above experiments the quantity of powder used was so small that it could not appreciably influence the results.

of the tube by the interposed septum. In very wide tubes an alteration in the velocity of sound when the wall of the tube is altered is everywhere not perceptible.

(6) The velocity of sound in air is not appreciably dependent on the intensity of the note.

It follows from these results, that in all those cases in which it cannot be certainly established that the tubes are so wide as not at all to influence the velocity of sound, the value obtained is too small. In all the ordinary musical instruments, the diameter of the sounding columns of air, as compared with the wave-length, is small; and there must therefore be a diminution of the velocity of sound in all. The attempts to make the observed tones of stopped and of open pipes accord with theory are fruitless, if the velocity of sound in air is taken as a basis and not that in the pipe, which must always be first specially ascertained.

It has been already observed by Dulong that, apart from all corrections for the ends of tubes, the velocity was found to be too small, though this was not ascribed to the influence of the wall of the tube.

Two modes of explaining the changes observed in the velocity present themselves:—

Friction of the air in the tube might first of all diminish the velocity of sound. But it has already been proved elsewhere that the internal friction of air cannot diminish the velocity of sound in the open air; and from several reasons it is probable that in a tube (in which, certainly, the density of air increases towards the sides) the amplitude, but not the velocity, may be altered by friction. It is true that the friction in a tube has not been demonstrated to be without influence on the velocity.

Yet the origin of the discrepancies observed may probably be sought in an exchange of heat which takes place between the air which propagates the sound and the wall of the enveloping tube. Since part of the heat produced in the motion of sound is given to the wall, and an equal part of that consumed at another instant is replaced by the wall, the produced and consumed heat are not entirely used, as Laplace assumes, for the acceleration of sound. Hence the velocity of sound must be smaller. The factor which Laplace introduces into the formula no longer corresponds to $\frac{c}{c_1} = 1.41$, but, inasmuch as heat is exchanged, is smaller. If all the produced and consumed heat were imparted and restored, we should have $k=l$; that is, the velocity would sink from 332.8 to 280 metres, Newton's value. A diminution of one-half is in fact evident from the Table.

Since all the circumstances which increase the surface of the sounding column of air, as compared with its volume, favour an

exchange of heat between air and wall, it is clear that the velocity of sound, if an exchange of heat everywhere takes place, must be the smaller the narrower the tube, the rougher its wall, or the larger and rougher the surface of a second tube inserted. In like manner powder must have greater influence the finer it is and the narrower the tube; for it is then better distributed in the sounding air*.

The greater, finally, the duration of the oscillation of a tone, or its wave-length, the more considerable is the exchange in the longer time; hence the diminution of velocity must, under the same circumstances, be greater.

Since, as has been said, all the influences of the wall, of the powder, and so forth are infinitely small in their action on the velocities when the diameter of the tube reaches a certain magnitude, the method, notwithstanding the discrepancies observed, may be used for exact determinations of wave-lengths, and therefore of velocities of sound, if the tubes for the dust-waves are wide enough. The author had proposed, in the next place, to test more extensively than was hitherto possible two deductions from the theory—that is, that the velocity of sound under different pressures is the same, and that its variation by the temperature is expressed by the factor $\sqrt{1 + \alpha t}$.

The velocity of sound in air was determined in wide tubes between pressures of 400 and 1760 millims. Within these limits of pressure, which correspond to a difference in pressure of nearly two atmospheres, the velocity of sound remained quite constant.

To ascertain whether the influence of the diameter of the tubes, when narrower ones are used, is the same under different pressures, the velocity was determined in some narrower tubes under a pressure of from $\frac{1}{2}$ to $2\frac{1}{2}$ atmospheres. Using a note whose semiwave-length was 45 millims. in a tube of 3.5 millims. diameter, the velocity of sound under a pressure of $2\frac{1}{2}$ atmospheres was about 4 metres greater than under a pressure of half an atmosphere.

The author refers this to the circumstance that, with air at greater density, the greater quantity of heat produced and consumed is not exchanged with the wall in the same ratio as the smaller quantity of heat with a smaller density.

The velocity of sound at 0° and 100° was then determined, and in tubes which were so wide that their walls had no influence. One side of the apparatus with one dust-wave tube being immersed in melting ice, the other with its dust-wave tube was

* Powder, by its increasing the mass to be moved, might somewhat impede the velocity. I have convinced myself, however, by experiments which will be discussed in the complete paper, that this circumstance is inadequate for the explanation of the considerable variations.

heated in steam, and the wave-lengths of the same tone determined at 0° and 100° , which thus gave the ratio of the velocities of sound.

The following Table gives, as the result of seven experiments, the velocity of sound for 100° , taking the velocity at 0° as 332·8 metres :—

	Metres.
1.	388·20
2.	388·84
3.	389·15
4.	388·47
5.	389·02
6.	389·64
7.	388·60
Mean . . .	388·99

Calculating the value from the formula $332·8\sqrt{1+100\alpha}$ ($\alpha=0·003665$), we get 389·03. The difference of the observed and of the calculated value amounts therefore to 0·04 metre.

It is therefore proved that up to 100° C. the change in the velocity of sound is accurately expressed by the factor $\sqrt{1+\alpha t}$, and therefore that Laplace's factor $\frac{c}{c_1}$, the ratio of the specific heats of air, is constant within very narrow limits.

As this constancy is thus experimentally proved, observations of the velocity of sound at 100° may conversely be used to determine α , the coefficient of expansion of air.

Although α is not obtained with excessive accuracy from a single determination, the deviations of α in various observations are not too far apart; and from the mean of the seven observations we get

$$\alpha=0·003662.$$

We see from this that, in spite of the considerable changes in the velocity of sound in tubes, the method in question with a suitable choice of apparatus yields very accurate determinations; and, in the future, accurate values for the velocities of sound of different gases, and therewith of the ratios of their specific heats, may be expected from it.

V. *On the Theory of Pressure in Fluids.* By R. MOON, M.A.,
Honorary Fellow of Queen's College, Cambridge*.

I DESIRE to commend the following points in the theory of pressure in fluids to the attention of mathematicians.

I. Suppose that we have a closed cylinder filled with air, and, for the sake of precision of statement, suppose the axis of the cylinder to be horizontal. Suppose also that at the time t every particle of the air is destitute of velocity, while the density of the air varies on this wise; viz., the air to the left hand of a vertical plane which bisects the axis of the cylinder has the uniform density D , while the air to the right of the same plane has the density $2D$.

Under these circumstances, according to the received theory with regard to the law of pressure in fluids, the air in the right-hand half of the cylinder will exert on the air in the left-hand half exactly double the pressure which the air in the left-hand half exerts on the air in the right-hand half; that is, the law of pressure of the received theory contradicts, in this case, the universal principle that action and reaction are equal and opposite.

II. Suppose that we have a vertical cylinder closed at its lower end, and having an air-tight piston capable of moving freely in the upper part of it. Below the piston the cylinder is filled with air, which is kept in equilibrium by means of a weight, W , resting on the piston, above which there is a vacuum.

If a second weight, W , be placed upon the piston, we know that the equilibrium will be destroyed. Let us see, however, how, according to the received theory of pressure, a thin stratum of air immediately beneath the piston will comport itself under these circumstances.

The pressure of the piston upon this lamina and the pressure of the lamina upon the piston must under *all* circumstances be equal and opposite.

Consequently, if the principle of the received theory be true (viz. that the pressure varies only with the density), the pressure of the air upon the piston (and therefore the pressure of the piston upon the air) must remain unaltered until a change has taken place in the density of the air in the lamina.

Now to a change in the density of the air in the lamina one thing is essential, viz. relative motion of the particles of which the lamina is composed; and to such motion of the particles two things are requisite, viz. :—

(1) A preexisting variation of pressure in the lamina, through the operation of which relative motion of its particles may be generated.

* Communicated by the Author.

(2) An interval of time during which such variation of pressure may act.

But while it has thus been seen that variation of density in the lamina must be preceded by variation of pressure in and upon the lamina, the received theory asserts that these two things, viz. variation of density and variation of pressure, are necessarily coexistent.

Hence in the case just stated, since at the time t , when the second weight is introduced, there is no variation in the density of the air in the lamina immediately beneath the piston, it follows, according to the received theory, that when the second weight is introduced there will be no variation of pressure in the lamina, and therefore nothing to generate that which, according to the received theory, is the essential mark and test of a change of pressure, viz. variation of density. According to the received theory, therefore, the equilibrium will be maintained after the second weight has been introduced, which is absurd.

It is clear that in the case just considered, the effect of the introduction of the second weight will be to propagate instantaneously into the air beneath the piston a pressure of finite amount, extending to a finite distance below the piston—such additional pressure having its maximum immediately below the piston, and thence diminishing gradually till, if the portion of the cylinder below the piston be long enough, it will finally vanish. If the portion of the cylinder below the piston be small, as compared with the magnitude of the weight resting upon the piston and with the density of the air, such additional pressure will extend throughout the entire mass of air beneath the piston; and we shall have, besides, an additional pressure of finite amount exerted simultaneously upon the base of the cylinder.

III. Suppose that we have a vertical cylinder having its lower end closed and resting upon a solid pier, and having, as before, an air-tight piston capable of moving freely in the upper part of it. Above the piston is a vacuum, while below it the cylinder is filled with air which at the time t is destitute of velocity, but of which the density varies according to the following law, viz.

$$\rho = D e^{ax},$$

where x denotes the vertical distance from the base of the tube. Gravity is supposed not to act; but a constant downward pressure is exerted upon the piston equal in amount to the pressure which, according to the received theory, would be due to the air immediately beneath the piston at the time t .

Under these circumstances the effective force which, according to the received theory as to the pressure in fluids, will be exerted upon each particle of the air at the time t will be uniform. As-

sume this force to be f , then at the end of the time $t + dt$ each particle of the air will have acquired a velocity $f dt$, and will have described a space $\frac{f dt^2}{2}$.

Moreover, since the motion during the time dt (being such as has been described) can in no way have altered the density, it is clear that at the end of the second interval dt each particle will have acquired a velocity $2f dt$, and will have described a space $2f dt^2$, the density remaining unaltered.

Reasoning from step to step in the same manner, at the end of the finite time t_1 , each particle will have acquired a velocity ft_1 , and will have described the space $\frac{ft_1^2}{2}$.

It is obvious, however, that no such effects as those above described, and which, if the received theory of pressure be universally true, must necessarily take place, can occur in the case we are considering. At the end of the time $t + t_1$, equally as at the end of the time $t + dt$, the particles in contact with the base of the cylinder will have described *no* space, they can have acquired *no* velocity.

It is clear, in fact, that any theory of pressure which, under the above circumstances, would render the value of $\frac{dp}{dx}$ other than zero, as the received theory on the subject does render it, must be erroneous.

IV. Suppose that we have a vertical cylinder, closed at both ends and resting on a solid pier, filled with air which is in equilibrium, gravity being supposed to act.

Suppose that at the time t a second force begins to act, which is equal to the force of gravity and acts in the same direction.

Under these circumstances, either the law of pressure in the air in the cylinder will be instantaneously altered or it will not.

If it be instantaneously altered, the received theory as to the law of pressure is contradicted; for it is impossible that the density of the air in the cylinder can be altered instantaneously, change of density implying motion, for which lapse of time is indispensable.

But if the pressure throughout the air be not instantaneously altered, the following consequence will occur; viz. each particle of the air will be acted upon (1) by the forces which acted upon it before the time t , which forces destroy each other, since before t the air was in equilibrium; (2) by the constant force g . Hence at the end of the time $t + dt$ each particle will have acquired the velocity $g dt$, and will have described the space $\frac{g dt^2}{2}$, no change taking place in the density of the air.

And, reasoning as in the last case, at the end of the time $t + t_1$ each particle will have acquired the velocity gt_1 , and will have described the space $\frac{gt_1^2}{2}$, results which, it is obvious, are not true as regards the particles at the base of the cylinder.

In this case also, therefore, it is evident that the adoption of the received theory of pressure has led us to untrue results*.

6 New Square, Lincoln's Inn,
June 10, 1868.

VI. *Description of a new Thermostat.* By FREDERICK GUTHRIE, Ph.D., F.R.S.E., Professor of Physics and Chemistry, Royal College, Mauritius†.

IN physical and chemical research, and also in the arts, the problem of maintaining a constant temperature frequently presents itself. To this end Bunsen constructed his ingenious thermostat, whose action depends upon the expansion of mercury. For many purposes Bunsen's thermostat answers admirably; but in cases where a perfectly constant temperature is required, I have not found it sufficiently nimble in its action of adjustment.

Requiring, for some experiments on heat, to maintain water for many hours at a temperature approaching to absolute constancy, I constructed the thermostat here figured, and have found it to answer very satisfactorily.

As others may have experienced the same want, and as I am not able to find any description of a similar instrument, I beg to submit the following description to the readers of the *Philosophical Magazine*.

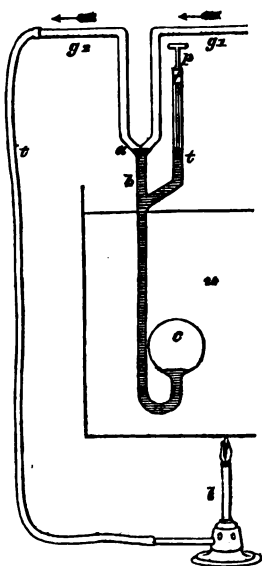
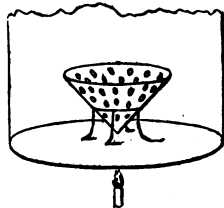
I may state at once that, with ordinary care and by using the "distributor" afterwards described, I have no difficulty in keeping a temperature constant to $0^{\circ} \cdot 2$ Fahrenheit for six hours, including the time when, towards evening, an additional pressure is applied through the service-pipe. The gas from the service-pipe enters at g_1 , passes through g_2 , and is conducted by the flexible tube t to the lamp which heats the reservoir of water, u . The tubes g_1 and g_2 join to form the tube b . The lower extre-

* In the above case we must have $\frac{dp}{dx} = 0$ at the base of the cylinder at the time t when the second force begins to act, the value of $\frac{dp}{dx}$ taken without regard to sign increasing as we ascend. The action of the second force will also cause an instantaneous increase of finite amount in the pressure on the base of the cylinder.

† Communicated by the Author.

mity of *b* passes into the water and carries a bulb, *c*. The tube *b* also carries a branch, *t*, the upper end of which carries a cork through which works very stiffly the solid plug *p*. The cork in *t* is provided with a small hole to allow air to pass. Mercury is poured into *t* until it rises to a convenient height in *b*. The plug *p* is adjusted and the whole held very firmly in a clamp. The action is obvious. At a certain temperature the air in *c* will occupy such a volume that the mercury will be forced up *b* until its surface, *a*, begins to shut off the gas. The flame of *l* will thereupon diminish in size. By pushing the plug *p* further down mercury will be forced up *b*, and a consequent lowering of the temperature will follow. By this means the instrument can be set for any temperature without disturbing the mercury.

To avoid the necessity of constant stirring, and to disperse the currents of heated water rising from below, a perforate cone of sheet iron, as shown in the accompanying figure, is supported upon the bottom of the vessel containing the water*.



VII. On the Expansion of Solids by Heat.

By M. H. FIZEAU†.

IN the new research which I have the honour of laying before the Academy, I propose to present the continuation of my researches on the expansion of solids by heat, and especially of crystallized bodies.

Having observed a great number of new bodies, and having investigated them in various directions with a view to make evident

* Since the above was in type, I have been informed that Bunsen also has employed the expansion of air in his thermostat. There remains, therefore, so little originality in my form of the apparatus, that the inconvenience which would ensue to the printers of the Magazine alone prevents the suppression of the above.—F. G.

† Translated from the *Comptes Rendus*, May 25, 1868.

a general law which appears to rule these phenomena, having, moreover, lately succeeded, thanks to the able advice of M. Des Cloiseaux, in observing with certainty some more complex crystals belonging to the oblique system, I may now attempt to develop with more detail, and with the sanction of experiment, the theoretical views laid down in my first memoir*.

Some remarks may first be made and some principles laid down with the view of removing objections which might be raised relative to the regularity and constancy of the phenomena in question.

All observations agree in showing that the phenomenon of the change of volume of a body by heat, whether it be an expansion or a contraction, is produced in a continuous manner and with a regularity perfectly similar to that of the changes in temperature—the same volumes corresponding constantly and identically to the same temperatures.

Any idea of sudden and accidental, and, so to say, capricious variations may then be rejected in this class of phenomena, as well as the existence of slow variations which in the course of time might occur in the numerical value of the coefficients of expansion.

I may cite, in support of the invariability of these coefficients, two determinations made with great care, at an interval of a year, on the same quartz crystal supported on the same platinum tripod,—this latter having during this interval undergone several hundred alternations of temperature between 7° and 80° . These two coefficients correspond to the direction of the axis of the crystal.

First determination . $\alpha = 0.0000078118$,

Second determination $\alpha = 0.0000078117$.

When, in my former memoir, I endeavoured to coordinate the phenomena then known regarding the expansion of crystals belonging to the various crystalline systems, the limited number of the observations might indeed indicate a simple and general law, but did not permit its being established with sufficient precision; yet, guided by the analogies which were already manifest between the principal characters of these phenomena and those which the phenomena of luminous and calorific propagation present in their relations to the general symmetry of crystals, I have entered upon the path which for some time has been opened to us by Fresnel's celebrated researches on the propagation of light in crystals—a path which has been successfully followed by Senarmont in his discoveries of the unequal propagation of heat in the same bodies. And just as these two classes

* May 21 and 28, 1866.

of facts have been connected by very general geometrical considerations, in like manner for the phenomena of expansion we may, by analogous considerations, express the law of the variations met with in the numerical values of the expansions when they are considered according to different directions. There are then three distinct physical phenomena which may be connected with analogous theoretical views; these are the propagation of light and of heat through crystals, and the expansion by heat of the crystal itself; and these theoretical views are precisely of the nature of those which geometers use when they investigate ellipsoidal surfaces. In fact, one and the same principle serves as a common starting-point in the theoretical explanation of these three orders of phenomena—that is, the consideration of the three principal directions or rectangular axes endowed with well-defined physical and geometrical properties, and around which are attached as rigorous consequences the totality of these phenomena in their most varied manifestations.

I must here simply attempt to define the action of these axes relatively to the phenomena of expansion with which we are occupied; and I shall adduce a certain number of experiments which clearly prove that these axes correspond to real and distinct physical properties, which prevent us from regarding them as a mere geometric fiction sufficient to group empirically the data of observation. For the future they will be designated as *axes of expansion*. The expression *axes of elasticity* used in my first paper having become inadequate owing to the results observed in oblique crystals, we shall see in the sequel that in these crystals the three kinds of axes are no longer superposed as in other crystalline systems, but are really separated from each other by angular distances which are frequently considerable.

It has been shown in the first memoir that, if it be attempted to express in a general manner the value of the expansion of a crystal in any direction referred to three rectangular axes, a very simple formula is obtained, merely containing the squares of the cosines of the angles made with the three axes, as well as the three principal coefficients of expansion corresponding to these axes.

But it is important to remark that the reasoning which has led to this result depends really on the following principle:—

However complex be the crystalline form, however varied the expansions observed in the various directions, be they even in one case expansions, in another contractions, there are only three primitive expansions distinct and independent of each other, and solely manifested in three fixed directions at right angles to each

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other; these are the three axes of expansion. In any other direction than those, there will be observed only the simultaneous effects of these three primitive expansions, which will always manifest themselves individually, according to their intensities and their own directions, and constant for the unit of length.

It may be added that, all the elementary parts of the crystal being identical among themselves, these axes are not represented by three lines having a determinate position in the interior of the crystal, but really by three rectangular systems of parallel lines taken at each of the internal points.

To complete the enunciation of what appears essential in these singular properties, which in some sense seem to indicate a tri-nary arrangement in the elements of the crystallized substance, it remains to be stated in what each of the principal expansions must differ from any other resultant expansion, or, in other words, what is the distinctive character of an axis of expansion.

Suppose a sphere isolated in the body of the crystal at a certain temperature; if it be heated, the sphere will expand unequally in the direction of its different radii, and in the most general case its form will be ellipsoidal; but there will always be three diameters rectangular to each other, at the ends of which the displacement of a point on the surface of the sphere will be in a radial direction—that is to say, in the prolongation of the radius itself and without any lateral deviation.

That is the true character of axes of expansion, and the principle of the geometrical construction from which, in the first memoir, we have deduced the formula which we here examine.

I shall now give the results of experiments made with a view of controlling the accuracy of the general formula in many of its most important consequences, by trying to find in the various crystalline systems the most decisive phenomena and those most accessible to observation.

Denoting by D the coefficient of expansion in any direction whatever, determined by the angles δ , δ' , and δ'' which this direction makes with the three axes of expansion, and calling α , α' , and α'' the three coefficients of expansion along the three axes, we have the following relation,

$$D = \alpha \cos^2 \delta + \alpha' \cos^2 \delta' + \alpha'' \cos^2 \delta''; \quad \dots \quad (1)$$

but we have at the same time the known relation which expresses that the three angles δ , δ' , and δ'' are referred to three rectangular axes,

$$\cos^2 \delta + \cos^2 \delta' + \cos^2 \delta'' = 1. \quad \dots \quad (2)$$

Cubical System.

The general characters and the properties of this crystalline system lead us to consider the three principal expansions equal

to each other ; that is to say,

$$\alpha = \alpha' = \alpha'' ;$$

equation (1) then becomes

$$D = \alpha (\cos^2 \delta + \cos^2 \delta' + \cos^2 \delta''),$$

and, by equation (2),

$$D = \alpha ;$$

that is to say, the expansion is constant, independent of the direction in question, and always equal to that which takes place along the axes, the position of which cannot be revealed by any difference in the expansions, and which must be regarded as indeterminate. The following are some observations which refer to crystals of this system. These numbers and the following represent the linear expansion for the unit of length for 1 degree at the point $\theta = 40^\circ$ of the thermometric scale.

Fluor-spär.—Normal to a face of octahedral cleavage,

$$\alpha = 0.00001911,$$

$$\alpha = 0.00001910 ;$$

on a face of the cube (another crystal),

$$\alpha = 0.00001910 ;$$

on a face cut at an angle of 5° to a face of the cube (another crystal),

$$\alpha = 0.00001915.$$

Galena.—At right angles to a face of cubic cleavage,

$$\alpha = 0.00002014 ;$$

on a cut octahedral face,

$$\alpha = 0.00002014.$$

Cubic pyrites.—At right angles to a natural face of the cube,

$$\alpha = 0.00000907 ;$$

on a face cut in a group of Peruvian crystals without any common direction,

$$\alpha = 0.00000908.$$

Suboxide of Copper.—At right angles to a face of the rhomboidal dodecahedron,

$$\alpha = 0.00000093 ;$$

on a face situated at 90° from the foregoing,

$$\alpha = 0.00000093 ;$$

on a face cut in a group of crystals without any common direction,

$$\alpha = 0.00000093.$$

Dimetric and Hexagonal or Rhombohedral Systems.

These two systems, distinct in crystallographic relations, merge into each other in their optical phenomena, their conductivity for

heat, and their mode of expansion, which appears to depend on their symmetrical structure about a principal crystallographical axis. The expansions are here different in different directions; and reasons deduced from the symmetry of the structure show that one of the axes of expansion must coincide with the principal crystallographical axis, the two others being perpendicular to it. If we assume, by the same reasons of symmetry, equality in the coefficients corresponding to these two latter axes of expansion, where $\alpha' = \alpha''$, equation (1) will become

$$D = \alpha \cos^2 \delta + \alpha' (\cos^2 \delta' + \cos^2 \delta'') ;$$

but equation (2) gives

$$\cos^2 \delta' + \cos^2 \delta'' = 1 - \cos^2 \delta = \sin^2 \delta ;$$

we shall then have (α being the expansion for the former axis)

$$D = \alpha \cos^2 \delta + \alpha' \sin^2 \delta. \quad . \quad . \quad . \quad (3)$$

Taking any direction at right angles to the first axis, which gives

$$\delta = 90^\circ, \quad \cos^2 \delta = 0, \quad \sin^2 \delta = 1,$$

the equation is reduced to

$$D = \alpha' ;$$

that is to say that, in any direction at right angles to the first axis, which coincides with the axis of symmetry, the expansion is constant, and that it is impossible to distinguish the axes of expansion.

In any direction making with the first axis the same angle $\delta = 54^\circ 44'$, we have

$$\cos^2 \delta = \frac{1}{3} \quad \text{and} \quad \sin^2 \delta = \frac{2}{3},$$

and equation (3) becomes

$$D = \frac{\alpha + 2\alpha'}{3},$$

which is precisely the expression for the mean linear expansion of the crystal.

Varied experiments have been made on various kinds of crystals with a view of verifying the accuracy of the following remarkable property, which is deduced from theory:—All crystals which have the form of right prisms, of rhombohedra, of regular hexagons, or forms derived from these, present a certain angular direction (the same for all crystals) which makes with the principal crystallographical axis an angle of $54^\circ 44'$ (an angle for which $\cos^2 \delta = \frac{1}{3}$); and along this direction we ought to find exactly the third of the cubical expansion, or the mean linear expansion for each crystal.

It has been seen before that in the most general case this condition must be satisfied by the normals to the faces of a regular

octahedron suitably arranged; but in the present case this octahedron of mean expansion must be conceived as having one axis coinciding with the principal axis of the crystal, while the two others may be arranged in any manner whatever.

To make this decisive comparison between experiment and the result deduced from theory, three expansions must evidently be measured, α , α' , α^m , in the three following directions: the first parallel, and the second perpendicular to the principal crystallographical axis, the third making an angle of $54^\circ 44'$ with this axis; while the two latter directions may be in any azimuth whatever around the same axis.

Under these circumstances the cubical expansion will be

$$\alpha^{\text{cub}} = \alpha + 2\alpha';$$

the mean linear expansion,

$$\alpha^{\text{lin}} = \frac{\alpha + 2\alpha'}{3};$$

and the theory will only be satisfied provided this latter value coincide with the value found directly for α^m —that is, if we have

$$\alpha^m = \alpha^{\text{lin}}.$$

The following are the results of these observations:—

α is the expansion along the principal crystallographic axis.

α' along the normal to this same axis.

α^{lin} the mean linear expansion deduced from α and from α' .

α^m the expansion in the direction of the angle $54^\circ 44'$, with a possible error of $5'$ to $10'$ in the cutting of the crystal*.

Siberian Zircon. (Prism with square base.)

α	.	.	.	.	=	0.00000443
α'	.	.	.	.	=	0.00000233
α^{lin}	.	.	.	.	=	0.00000303
α^m	.	.	.	.	=	0.00000304

Emerald (Beryl). (Hexagonal.)

α	.	.	.	.	=	-0.00000106
α'	.	.	.	.	=	0.00000137
α^{lin}	.	.	.	.	=	0.00000056
α^m	.	.	.	.	=	0.00000057

Iceland Spar. (Rhombohedral.)

α	.	.	.	.	=	0.00002621
α'	.	.	.	.	=	-0.00000540
α^{lin}	.	.	.	.	=	0.00000514
α^m	.	.	.	.	=	0.00000507

* I must here render fresh testimony to the remarkable skill and the intelligent zeal with which M. Henri Soleil has cut the crystals used in these researches.

Quartz. (Rhombohedral.)

α	.	.	.	.	=	0.00000781
α'	.	.	.	.	=	0.00001419
α^{lin}	.	.	.	.	=	0.00001206
α^{m}	.	.	.	.	=	0.00001206

Bismuth. (Rhombohedral.)

α	.	.	.	.	=	0.00001621
α'	.	.	.	.	=	0.00001208

The third observation was made at right angles to a natural face of the rhombohedron—that is, at an angle of $56^\circ 24'$ with the principal axis. The corresponding calculation was made by means of formula (3) :

Calculation	.	.	.	α	=	0.00001334
Observation	.	.	.	α	=	0.00001338

Rhombic System.

As the situation of the axes of expansion is always subordinate to the crystalline structure, it must be admitted that to identical directions, as regards symmetry of faces, there correspond identical expansions—a principle which leads directly to fixing the situation of the three axes of expansion in this system parallel to the three crystallographical axes.

Suppose, in fact, a rectangular parallelepipedon constructed on the three crystallographic axes as edges; it will be at once seen that during the expansion of this solid the displacement of any point upon one of its faces can only take place parallel to the edge at right angles to this face and without lateral deviation, which is the character of the axial expansion; for if it were otherwise, and if the displacement took place along a certain inclined direction, it would be sufficient to turn the crystal through 180° in the plane of the face in question to find another direction in which the crystallographical relations would be the same, and along which, consequently, the same inclination would be produced.

The same kind of considerations may be employed to determine the position of the first axis in the dimetric prism previously considered, as well as in the hexagonal prism. For the case of the rhombohedron an analogous conclusion is reached by attributing to the crystal two successive rotations of 120° .

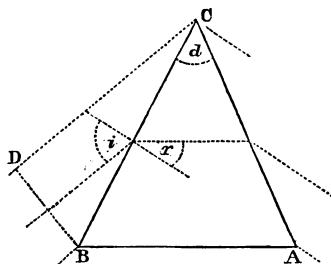
A subsequent communication will be devoted to completing the subject of the right rhomboidal system, and to considering the oblique rhomboidal system.

VIII. *On the comparative Efficiency of different Forms of the Spectroscope.* By EDWARD C. PICKERING*.

IT is the object of the present paper to furnish a means of comparing with accuracy spectroscope prisms of different forms, and to determine what must be their refracting angle to produce the greatest dispersion with the least loss of light. We have then to consider the dispersion, the loss by reflection, and that by absorption.

1. *Dispersion.*—The dispersion of any part of the spectrum is proportional to the angular interval between two rays of nearly equal refrangibility as the two parts of a double line.

Let α be the refracting angle of the prism ABC, n the index of refraction of the less refrangible ray. For minimum deviation,



$$r = \frac{\alpha}{2}, \sin i = n \sin \frac{\alpha}{2} \dots \dots \dots (1)$$

If dn be the difference of the indices of refraction of the two rays, di will be their angular divergence. Differentiating (1),

$$di = \frac{\sin \frac{\alpha}{2}}{\cos i} dn = \frac{1}{n} \tan i dn, \dots \dots \dots (2)$$

in which $\frac{1}{n} \tan i$ serves as a measure of the dispersion under different angles of incidence. It differs essentially (when the angle of incidence is large), from the deviation, which is commonly, but incorrectly, assumed as the measure.

Comparative Dispersion and Deviation of a Ray entering a medium in which $n=1.5$.

Angle of incidence $i \dots$	0°	15°	30°	45°	56° 19'	60°	75°	80°	85°	90°
Dispersion $\frac{1}{n} \tan i \dots$	0	.179	.385	.667	1.000	1.1565	2.488	3.781	7.620	∞
Deviation $i - r \dots$	0° 5' 4"	10° 32'	16° 53'	22° 38'	24° 44'	34° 55'	38° 58'	43° 23'	48° 11'	
Deviation reduced to same unit as dispersion.....	0	.231	.465	.746	1.000	1.093	1.543	1.720	1.917	2.121

* From the American Journal of Science and Arts, vol. xlv. (May 1868). Communicated by the Author.

The dispersion then increases much more rapidly than the deviation; hence in spectroscopes whose deviation is the same, that one will disperse most, in which i and therefore α is the greatest.

The above discussion applies strictly only to the emergent ray; but in the position of minimum deviation, the dispersion of a prism is just double this, as may be seen from the general formula for dispersion (Radicke's 'Optics,' vol. i. p. 179). I propose hereafter to discuss the question whether greater dispersion with the same loss of light could not be obtained by some other position of the prism.

2. *Reflection.*—In estimating the loss by reflection, it is usual to assume that the same proportion of the incident light is lost at each successive refraction. But in reality the light so refracted is partially polarized, and in this state another law determines the amount reflected. Fresnel showed that of a ray polarized in the plane of incidence, the proportion reflected

$$B' = \frac{\tan^2(i-r)}{\tan^2(i+r)},$$

while a ray polarized in a plane perpendicular to the first, would lose by reflection

$$A' = \frac{\sin^2(i-r)}{\sin^2(i+r)}.$$

Regarding common light as composed of two beams of equal intensity polarized at right angles, the amount reflected would be $\frac{1}{2}A' + \frac{1}{2}B'$, and that transmitted $\frac{1}{2}[(1-A') + (1-B')]$. On meeting a second surface inclined at the same angle of incidence, the amount transmitted would be $\frac{1}{2}[(1-A')^2 + (1-B')^2]$, and after passing m surfaces $\frac{1}{2}[(1-A')^m + (1-B')^m]$.

This formula can be applied directly to the m surfaces of the prisms of a spectroscope, since in the position of minimum deviation i and r are the same for all, and therefore the amount transmitted is the same, whether the passage is from glass to air or from air to glass.

The formulæ of Fresnel are used in preference to those of Cauchy, although the latter have been proved by Jamin and others to be more correct. But the coefficient of ellipticity which they involve is neither so commonly nor so easily found as the index of refraction. Furthermore, for glass the difference would probably be so small that it could be neglected.

3. *Absorption.*—The average length of glass traversed by the light is one-half the base AB, multiplied by N the number of prisms; and the amount escaping absorption is proportional to

the logarithm of this distance, or to $\log N \times BC \sin \frac{1}{2} \alpha$, or in prisms admitting the same amount of light (that is, in which BD is the same) to $\log BD \times N \frac{\sin \frac{1}{2} \alpha}{\cos i}$, since $BC = \frac{BD}{\cos i}$; but the dispersion is proportional to $N \frac{\sin \frac{1}{2} \alpha}{\cos i}$; hence in spectroscopes dispersing equally, and composed of prisms of the same material, *the loss by absorption will be the same in all*; so that, as far as the absorption is concerned, it makes no difference whether a spectroscope is composed of a large number of very acute-angled prisms, or of a less number in which the angle is more obtuse.

Thus we avoid the difficulty which seemed at first sight insurmountable, since the actual amount of light absorbed varies not only with the material, but with the refrangibility of the rays, and according to laws not yet discovered.

The following Tables give the deviation, dispersion, and amount of light escaping reflection, of spectroscopes composed of from one to ten prisms of indices of refraction 1.5, 1.6, and 1.7.

Table I. applies to prisms of 45° , Table II. to those of 60° (these being the forms in general use), and Table III. where the angle is such that the reflected light would be totally polarized (α being $67^\circ 22'$, 64° , and $60^\circ 56'$ in the three cases respectively). This form of prism appears to present great advantages for large spectroscopes, since at most but one-half of the light can be reflected, while one prism disperses as much as two of 45° .

TABLE I.— 45° Prisms.

	n .	1 sur- face.	1 prism.	2 prisms.	3 prisms.	4 prisms.	5 prisms.	10 prisms.
Deviation	1.5	$12^\circ 32'$	$25^\circ 4'$	$50^\circ 8'$	$75^\circ 12'$	$100^\circ 16'$	$125^\circ 20'$	$250^\circ 40'$
$i - \frac{\alpha}{2}$...	1.6	$15^\circ 15'$	$30^\circ 30'$	$61^\circ 0'$	$91^\circ 30'$	$122^\circ 0'$	$152^\circ 30'$	$305^\circ 0'$
	1.7	$18^\circ 5'$	$36^\circ 10'$	$72^\circ 20'$	$108^\circ 30'$	$144^\circ 40'$	$180^\circ 50'$	$361^\circ 40'$
Dispersion	1.5	.467	.935	1.870	2.804	3.739	4.674	9.348
$\sin \frac{\alpha}{2}$	1.6	.484	.968	1.936	2.904	3.872	4.840	9.680
$\frac{\sin \frac{\alpha}{2}}{\cos i}$...	1.7	.504	1.008	2.016	3.023	4.031	5.039	10.078
Transmitted	1.5	.957	.916	.841	.774	.724	.661	.461
$\frac{1}{2}[(1-A')^m$	1.6	.943	.892	.799	.719	.651	.592	.391
$+ (1-B')^m]$	1.7	.926	.859	.745	.653	.578	.516	.324

42 Comparative Efficiency of different Forms of the Spectroscope.

TABLE II.—60° Prisms.

	<i>n</i> .	1 sur- face.	1 prism.	2 prisms.	3 prisms.	4 prisms.	5 prisms.	10 prisms.
Deviation ...	1.5	18° 35'	37° 10'	74° 20'	111° 30'	148° 40'	185° 50'	371° 40'
	1.6	23° 8'	46° 16'	92° 32'	138° 48'	185° 4'	231° 20'	462° 40'
	1.7	28° 13'	56° 26'	112° 52'	169° 24'	225° 44'	282° 10'	564° 20'
Dispersion ...	1.5	.756	1.512	3.023	4.535	6.046	7.558	15.116
	1.6	.833	1.667	3.334	5.000	6.667	8.334	16.668
	1.7	.949	1.899	3.797	5.696	7.594	9.493	18.986
Transmitted .	1.5	.945	.895	.811	.742	.686	.641	.509
	1.6	.920	.853	.748	.672	.618	.578	.491
	1.7	.888	.801	.681	.608	.565	.538	.505

TABLE III.—Angles of Prisms 67° 22', 64°, and 60° 56'.

	<i>n</i> .	1 sur- face.	1 prism.	2 prisms.	3 prisms.	4 prisms.	5 prisms.	10 prisms.
Deviation ...	1.5	22° 38'	45° 16'	90° 32'	135° 48'	181° 4'	226° 20'	452° 40'
	1.6	26° 52'	52° 104'	156° 208°	203° 260°	250° 500°	300° 600°	600° 1200°
	1.7	29° 4'	58° 8'	116° 16'	174° 24'	232° 32'	290° 40'	580° 20'
Dispersion ...	1.5							
	1.6	1	2	4	6	8	10	20
	1.7							
Transmitted .	1.5	.923	.863	.763	.691	.639	.600	.520
	1.6	.899	.818	.702	.629	.582	.552	.505
	1.7	.874	.780	.657	.588	.549	.523	.501

To apply these Tables to an example, let us compare three spectroscopes of ten prisms each, of angles 45°, 60°, and 64°, the index of refraction being 1.6.

10 prisms of	Deviation.	Dispersion.	Transmitted.	Trans. $\times$ cos <i>i</i> .
45°	305° 0'	9.680	.3911	.308
60	462 40	16.668	.4912	.294
64	520	20.000	.505	.268

Again, comparing spectroscopes producing equal deviation,

	Deviation.	Dispersion.	Transmitted.	Trans. $\times$ cos <i>i</i> .
12 prisms of 45°	366° 0'	11.616	.339	.268
8 " 60	370 8	13.334	.532	.319
7 " 64	364	14	.521	.276

From the first example we see that by using 64° prisms instead of 45° , we obtain more than double the dispersion, with even less loss of light, while in the second case seven 64° prisms prove much more efficient than twelve of 45° .

All these calculations seem to point to the superiority of 60° prisms over those of 45° . A much greater angle is objectionable from the increased distortion produced by the slightest imperfection in the refracting faces.

In prisms admitting the same amount of light, the more acute the angle the less is the quantity of glass and the less the area of each face. The ground might be taken that a 45° prism could be made larger than one of 60° at the same expense, and thus the difference in light remedied. In this case, however, it would be necessary to enlarge the telescope, number of prisms, and in fact the whole instrument. Even supposing this change made, the prisms of larger angle preserve their superiority, though not in so marked a degree. The calculation is readily made by multiplying the transmitted light by $\cos i$, as is done in the above examples.

The index of refraction varying with the refrangibility of the rays, the dispersion, loss of light, &c. would vary in different parts of the spectrum. The change would, however, be small, and could be determined, if necessary, by merely altering n .

Institute of Technology, Boston,
February 29, 1868.

IX. *An Optical Experiment.* By JOHN C. DOUGLAS, *East-India Government Telegraph Department*.*

THE following experiment (or rather experiments) furnishes a new means of demonstrating experimentally several laws of vision, particularly such as relate to visual direction; and, suitably modified, may possibly be of some practical value. Prepare a rather thick looking-glass by sprinkling its surface with prepared chalk or other fine powder, shaking off the superfluous powder so that the glass be covered with a thin coating of powder, through which the reflected image of the face may be seen with tolerable distinctness. If the glass so prepared be held 6 to 8 inches from the face of the observer, and so that he can see a reflection of his own features, on shutting one eye the powder will no longer appear equally distributed, but will appear arranged in lines radiating from a point on the glass in front of the reflected image of the pupil of the eye used in observing the phenomenon.

* Communicated by the Author.

If (the appearance described above being visible) the observer move his head with relation to the glass, the centre from which the lines appear to radiate and the whole system of lines move. If, however, the observer move his eyeball in its orbit, the glass and his head being stationary, the centre and system of radiating lines do not appear affected. On regarding the prepared glass with both eyes, he will see two centres, from which radiate two systems of lines, one centre being opposite to the image of the pupil of each eye. The appearance is not so distinctly seen, however, with two eyes as with one; and if one eye be stronger than the other, *i. e.* more effective for purposes of vision when both are open, the system of lines appertaining to the weaker eye is less distinct than that appertaining to the stronger. The superior distinctness of the appearance when one eye only is used is due to the fact that the eyes mutually interfere when used together; for it is not necessary to shut one eye, but merely to interpose a sheet of card or other opaque body between it and the mirror. I have never failed to distinguish the radiating appearance in my own case, nor in the many cases in which I have endeavoured to show it to unscientific persons; and I believe, if only one eye be used at first, and the instructions given above be strictly adhered to, the appearance may be readily seen by any person; but as I have heard of an instance in which the phenomenon could not be distinguished, I have thought it necessary to give rather minute instructions*.

The phenomenon is so easy of explanation that it is not necessary I should explain it, I will therefore confine myself to pointing out that the lines seen represent the apparent directions of the lines joining the particles of powder and their images, and it is evident that the centres of the systems of lines are in the perpendiculars to the surface of the mirror drawn through the centres of visual direction. The apparatus may be modified in accordance with the wants of each observer, the principle upon which its construction is based remaining the same; the glass may be of any attainable thickness; a glass plate may be placed in front of the mirror at various distances from it to carry the powder; engraved lines, dots, &c. may be employed in lieu of powder; figures or other characters may be written on the mirror or glass plate if required; and the whole instrument may be suitably mounted.

The applications of the instrument described appear numerous; a few are given below:—Experimental proof is afforded of the prin-

* The effect was most striking when the powder was very thinly and uniformly spread over a piece of plane glass placed before the mirror, at such a distance that the powder was rather less than half an inch from the reflecting surface.

cial laws of linear perspective—as the existence of vanishing points, that they are in a line drawn through the centre of visual direction parallel with the vanishing lines, that vision with one eye only can be considered in perspective, &c.; the coincidence of the centre of visual direction and the centre of motion of the eyeball is proved by the immobility of the radiating lines when the eyeball only is moved, and their movement when the head is moved with relation to the mirror; a means is afforded of finding a point on a looking-glass, a perpendicular to the surface drawn through which would pass through the centre of visual direction; a principle is afforded on which an instrument may be constructed for measuring the distance between the centres of motion of the eyes. The instrument I used for this purpose consisted of a card divided along one edge by lines drawn on both sides of it, and as nearly as possible coincident; this card being held in front of a mirror and viewed obliquely, so that the divisions on the card and on its image might be seen at the same time (the card being held with the divided edge horizontal), two lines on the card were seen to coincide with the images of two lines corresponding to them on the other side of the card; and the distance between the two lines so coinciding with reflected lines being read off on the card, gave the distance between the centres of motion of the eyeballs. This instrument admits of great improvement; and it will be evident that its delicacy may be increased by increasing the proportion the distance between the image and object bears to the distance of the mirror from the eyes. It has been stated that, if no apparent motion could be produced in a distant object by closing one eye after having covered that object by another nearer the eye, the eye so closed must be inoperative when both eyes are used, the greater perfection of the one eye obscuring altogether the effect of the weaker organ. And so far as apparent direction is concerned the experiment seems conclusive. But this view appears to need modification; for in a case in which both systems of lines were distinctly visible simultaneously, proving both eyes were employed, no apparent motion could be produced by closing the left eye; and although the left system of lines was fainter than the right, and the left eye was undoubtedly weaker than the right, the fact that the system appertaining to the left eye was distinctly visible when both eyes were used simultaneously proves that, in regarding the mirror, both eyes were effective.

It appears, therefore, that the powdered mirror affords a delicate means of detecting the share each eye has in vision, of exposing their inequalities of strength, and of detecting if both are actually effective when used together—in the last, being more delicate than the apparent-motion test.

Sir David Brewster (*Optics*, p. 294) has stated that the centre of visual direction coincides with the geometrical centre of the eyeball; but it has been objected to this, that Volkman's experiment on visual direction proves that the lines of visual direction cross each other in a point anterior to the geometrical centre, and that calculation will show this is inconsistent with the curvatures and refractive powers of the humours composing the refractive media of the eye; but as, in the powdered-mirror experiment, the grain of powder seen singly and forming the centre of the system of lines marks the point through which a perpendicular to the surface of the mirror represents the line of visual direction, and as this line is immoveable when the eyeball is moved, it follows that this line passes through a point in the eyeball stationary with regard to the eyeball, notwithstanding the motion of the eyeball; *i. e.* wherever the centre of visual direction may be, it is the centre of motion, if not the centre of figure, of the eyeball.

Madras, May 20, 1868.

X. *On Purification of Sewage.*

By G. W. WIGNER, *F.R.A.S.**

A NEW mode of purification of sewage by the addition of chemical agents has been introduced by Mr. Sillar and myself, and some experiments have been tried on a large scale at Tottenham. The process is a cheap one, and, as far as can be judged from the Tottenham experiments, is very successful. The time occupied in precipitation is very short, and the proportion of impurities removed is large.

A tank holding 5000 gallons was filled with sewage and clarified eight times in succession, the average time occupied in filling and settling being less than twenty minutes. The supernatant water was clear, and almost free from either taste or smell. The analyses below show that more than 80 per cent. of the ammonia originally present in the sewage was carried into the sediment. The produce of the 40,000 gallons was about 8 cwt. of manure, closely resembling guano in appearance. A large tank, holding 40,000 gallons, was afterwards filled and precipitated with equally satisfactory results.

Some more experiments are to take place shortly; and if these are successful, the details of the process will be published.

* Communicated by the Author.

Abstract of Analyses of Sewage &c.

Tottenham Experiments.

	Average sewage.	Average water from sewage.	Tottenham water.
Total solid matter per gallon . . . }	203·89	81·96	48·70
Organic matter . . .	109·20	14·62	11·31
Ammonia	3·970	·584	
Phosphoric acid . . .	7·23		
Common salt . . .	57·10	57·51	9·21
Silica, alumina, and various salts . . }	30·36	9·82	28·18

Sewage Residue or Manure.

Water	4·45
Organic matter containing ammonia	2·37.
Phosphoric acid	5·33
Sulphate of lime	1·67
Silica, alumina, &c.	68·50
	<hr/> 100·00

XI. Notices respecting New Books.

A Dictionary of Chemistry and the Allied Sciences. By HENRY WATTS, B.A., F.R.S. 5 vols. 8vo. London: Longmans (1863-68).

THE special literary form in which a science shall find appropriate expression is not by any means a matter of indifference. A body of truth which is replete with laws, the meet reward of meritorious centuries, may indeed, like a wealthy individual of high birth and long descent, assume the most varied exterior without discredit; but should it be scarcely yet provided with definitions, it has, like some struggler of recent lineage and narrow means, merely a single guise, which it would be impossible to improve without an awkward betrayal of the peculiar manners and partial education of the wearer. This single guise is known to literature as the *dictionary*; and in it are most fitly adduced, as belonging to a primitive epoch, the facts and circumstances on which the inductive method is to proceed. In the course of time, as hypothesis and verification mingle with their predecessors and result in law, it becomes allowable to discard in form many of the original details (whose accurate ascertainment had been nevertheless important), and to state deductively the substance of much that has been achieved. When such a period has been fairly reached, the *manual* is its legitimate exponent. Eventually, as collateral branches spring from the parent stock, a science divides itself into large (but peculiar) subjects of inquiry, and in each case the *treatise* exhibits the final results or total issue of such a proceeding.

These being, as far as we have been able to gather, the functions

which have been generally assigned to the literary forms in question, there are probably but few instances in which they have been appropriately exercised. Among very early writers the treatise is in reality a fragment of a dictionary; the manual appears too soon; and all three are confounded in a spurious "system." In modern times, the treatise partly maintains its true position and has partly degenerated into the "paper" (which is a small contribution to knowledge); the dictionary comprehends everything; and the manual is an abstract of the dictionary. It is indeed not an uncommon circumstance to find the deductive method now excluded, as far as possible, from a chemical manual; while the author introduces in its place small portions of physics, physiology, geometry, geology, or theology, which much resemble, in their mutual disconnexion and worthlessness, the scraps of information which precede a diary.

Considerations such as these cannot have escaped the learned and distinguished editor, who is in great part author, of the chemical dictionary now before us; and if he has bestowed upon his labours a collective name which we are disposed to think unsuitable, this must be attributed rather to the faults and mistaken demands of the times than to any misconception of a term.

It will be evident to anyone who will take the trouble to make the inquiry, that our English chemical dictionaries (to say nothing of those published in foreign languages) have successively exceeded their office to a continually increasing extent. Thus, in the preface to Nicholson's dictionary (to which we more especially refer, inasmuch as it was the grandparent of the present work), it is laid down, with much truth, that "when a subject does not, in itself, demand much arrangement, or if the natural arrangement be such as not to be apprehended but by those who have made considerable advances in that department of knowledge, it is evident that the utility of a dictionary to the Learner will more than compensate for the offence given to the Masters of the Science. And accordingly it is found that no systematical arrangement of mere words or terms, with their explanations, can possess so much utility as that which follows the order of the alphabet. When Macquer's Dictionary first appeared, the author remarked that Chemistry was little more than a collection of facts, scarcely entitled to the name of Science, or capable of either synthetic or analytic explanation. Whence he drew a just conclusion in favour of the Dictionary form." Nicholson proceeds to show that, even at the time he writes (1795), the argument for this form is as cogent as ever. Ure's dictionary (1821) was the immediate successor of Nicholson's. But it may be seen from the titlepage, and still more from the contents of that work, that it was the intention of the writer to diverge much from the plan adopted by his predecessor; and we are led, in somewhat fulsome phraseology, to expect "the principles of the science investigated anew; and its applications to the phenomena of nature, medicine, mineralogy, agriculture, and manufactures detailed." We have also an ingenious novelty in this book, in the shape of "an introductory dissertation containing instructions for converting the alphabetical arrangement into a syste-

matic order of study." It was attempted to carry out all these ambitious intentions in the compass of a single volume; but the professions of the author were very incompletely realized in the product of his pen. The fourth and last edition of Ure's dictionary appeared during the years 1831 and 1835, and exhibits most of the faults and merits of the first. No further edition was published; and during the last thirty years English chemical students have been accustomed to consult ill-advised "systems" of chemistry, small (and for the most part spurious) manuals, and Mr. Watts's excellent translation of Gmelin's 'Handbook' (which, its first volume excepted, is really a dictionary). There was undoubtedly a general desire that a new dictionary should be written, having the same general scope as that of Ure; and the long and arduous task has been most zealously undertaken and ably accomplished by Mr. Watts. His aim has been, we believe, to give to a reader already somewhat versed in the subject a succinct account of modern scientific chemistry and those other sciences which are situated in its immediate vicinity. Applied science constitutes no part of his design, save in its theoretical bearings; general theory and laboratory operations receive very full attention; and the editor consistently avails himself of every mode of exposition.

While we have deemed it our duty to recall the attention of authors to the formal functions of the three typical modes of scientific publication, and especially with reference to so important a work as the present, we have to regret that we are unable to give so much relative space to the discussion of its substance as has been the case with its form.

Mr. Watts's dictionary suffers little from the circumstance that its publication has extended over a series of five years, and that (as the preface to the fifth volume informs us) its preparation has been the labour of nine. Thus, for example, Dr. Odling's article "Atomic Weights," which occurs among the first, and would probably have failed, in less able hands, to indicate the transition period of chemical notation, is an excellent judicial statement of the arguments in favour of the modern principles of chemistry; and while these are illustrated with great firmness and perspicuity, the reader is very properly left to decide for himself on several points which had been raised. Neither, again, is it of very material importance that the symbolic values proposed by Gerhardt and Cannizzaro have been successively employed in these pages; and care is taken to prevent any mistake on the part of the student by making at the outset a distinction in the symbol itself. If we add to this that the nomenclature keeps pace with the notation, and is nowhere allowed to be obscure or doubtful, we have said enough to convince those for whom these volumes are intended of the editor's caution with respect to minor but significant particulars.

One of the most striking features in this dictionary is the attention which has been paid to chemical theory. Not only is this evident in the article to which we have but recently alluded, but in Professor G. C. Foster's treatment of Classification, Acid, Alkali, and

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under other titles the historical development and present position of our modern hypotheses are portrayed with equal fidelity. The wisdom of such a course is sufficiently obvious, and is perfectly in accordance with the rest of the editor's plan. Good theories are now more required than the class of facts with which we are accustomed to deal. The law of multiple proportions has, fortunately for numerous investigators, enabled us to correct and employ in logic experimental results of comparatively inferior value; and it is beginning to be felt that with the accumulated labours of the last half century, some of us ought to examine anew the fundamental conceptions of chemistry. It is quite time to dispute afresh the manifold assumptions of the atomic theory, to discuss the minuter phenomena of chemical reactions, to test with rigour the philosophical stability of our definitions and notation. Few among those who, amidst the turmoil of often too hasty experiments, really care to reflect upon, and to stand face to face with, the full logical consequence of their results, will be disposed to doubt that the whole aspect of chemical theory is destined to at least as great a revolution as it has yet witnessed—a revolution in which the gross prevailing materialism will probably have to succumb, when we may expect to enjoy the freedom and purity of exact science. It is, then, most gratifying to find not only that a work like the present, whose authoritative influence will not be disputed, contains several special theoretical treatises, but that a great portion of its contents discloses quite as much theoretical as practical discussion.

Although it formed no part of Mr. Watts's plan (nor indeed would it have been desirable) to record in detail the operations of applied chemistry, there are cases in which he has very properly relaxed a rule whose rigid enforcement might have disappointed some of his readers. Gun-cotton, iron, copper, bleaching, for example, are subjects which could not be well treated at all without some reference to the manufactory; and while the student will probably learn from such articles quite as much of processes on the large scale as he desires to know, the manufacturer will find abstracts of and references to all the important memoirs which bear upon the principles he carries out.

Another point which we must not omit to notice is the fulness with which laboratory operations are described. This is particularly the case with the various branches of qualitative and quantitative analysis, the description of whose methods has been entrusted to chemists such as Mr. Conington, Drs. Dittmar and Russell, who have made this department their peculiar study. Here and there we have observed an article somewhat too compressed—that on Specific Gravity, for example; and we have noticed some omissions. We can, however, have no hesitation in attributing such deficiencies to the absolute necessity, beginning to be generally felt, that the whole should be brought to a conclusion as soon as possible; for it had been found unavoidable to depart from the original plan, which only included three volumes, and also to extend the time of publication. The mature judgment of the editor is nowhere better displayed

than in the care with which he has condensed the concluding matter and rejected the less important topics or laborious trivialities which we have noticed in other dictionaries.

In this brief review of the most extensive and valuable production of its kind which has yet appeared in the English language, the nature of our task has precluded the detailed observations which we might have felt it our duty to make in reference to a smaller book. Anything which Mr. Watts undertakes is invariably executed in so thorough, clear, and direct a manner, that his editorship and principal authorship are sufficient recommendations to any one who is acquainted with chemical literature. He has, moreover, been fortunate in obtaining the services of several distinguished chemists and physicists, who have contributed articles on subjects to which they have paid special attention. The whole work has thus the completeness and authority which are due to a well-organized literary plan well executed in all its parts; and it will probably be for many years the chief standard of reference in this country.

We venture to make, in conclusion, a recommendation which we are sure will meet with the approval of the majority of our readers. Mr. Watts's dictionary contains several distinct treatises, which have already assumed a high and distinct rank. When business requirements may permit of such a course, we shall be glad to see such articles as Heat, Electricity, and Analysis published in a separate form. Many persons who, for various reasons, do not intend to become possessors of the entire dictionary, would desire to possess one or more of those treatises, which, from their size, portability, and special character, are so very commodious for laboratory use. We are indeed persuaded (and the opinion is perhaps gaining ground) that chemistry ought earnestly to commence making the treatise its more customary form; but if this work should prove to be the last, it will certainly have been the best of its race.

Celestial Objects for Common Telescopes. By the Rev. T. W. WEBB, M.A., F.R.A.S. Second Edition, Revised and Enlarged. London; Longman and Co. 1868 (pp. 318).

The title of this work sufficiently indicates that the author's subject is not the aspects of the heavenly bodies collectively, nor their relative motions, but their aspects individually as seen through a "common telescope." A "common telescope" is defined to be one not exceeding about 5 feet in length and 4 inches in aperture. The chief contents of the work are a descriptive account of the solar system, and a list of double stars and nebulae. In both of these divisions, the author, while freely mentioning what others have made out by the use of higher powers, has kept very fairly within his limit of acting as a guide to those possessed of "common telescopes." Thus, in drawing up the list of double stars, he examined in succession with a 3·7-inch aperture all the objects in Admiral Smyth's Bedford Catalogue that there was any prospect of reaching, and retained those only "which seemed to possess sufficient general as well as scientific interest" (p. 170). To the list thus selected were added

several objects which, though unexamined, appeared to be certainly worthy of notice; and these are very properly distinguished by being placed between brackets. The article on the moon deserves particular notice. It comprises just a fifth part of the whole work, and is an excellent sketch of "selenography." It is accompanied by an elaborate map, slightly exceeding 12 inches in diameter, exhibiting the chief objects on the moon's surface that common telescopes can be expected to reach. The writer has given, by way of introduction, a few remarks on the telescope, and some rules for using it, which cannot fail of being serviceable to beginners.

XII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxxv. p. 465.]

May 7, 1868.—Dr. W. B. Carpenter, Vice-President, in the Chair.

THE following communication was read:—

"On Waves in Liquids." By W. J. Macquorn Rankine, C.E., LL.D., F.R.S.

(1) *Object of this Paper.*—It has long been known that in a uniform canal filled with liquid, the speed of advance of a wave in which the horizontal component of the disturbance is uniform from surface to bottom is equal to the velocity acquired by a heavy body in falling through half the depth of the canal. But, so far as I know, it has not hitherto been pointed out that a similar law exists for waves transmitting a disturbance of any possible kind in a liquid of limited or unlimited depth, provided only that the upper surface of the liquid is a surface of uniform pressure. The object of this paper is to demonstrate that law, and to show some of its applications.

(2) *Velocity of Advance defined.*—Throughout this investigation the velocity of advance of a wave will be defined to be the mean between the velocities with which the shape of the wave advances, relatively to a surface-particle at the crest and to a surface-particle in the trough respectively. In ordinary rolling waves the velocities of particles in those two positions are equal and contrary, so that the speed of advance as above defined is equal to the speed of advance of the wave relatively to the earth. A wave of translation in which the velocities of particles at the crest and hollow are not equal and contrary, may be regarded as produced by compounding the motion of a rolling wave with that of a current whose velocity is half the difference of the velocities of those particles.

(3) *Relation between height of wave and horizontal disturbance at the surface.*—The following relation between the height of a wave and the horizontal disturbance of the surface-particles has already been proved and made use of by various authors; and it is demonstrated here for convenience only. Let $+u_1$ and $-u_1$ be the velocities of a surface-particle at the crest and trough of a wave respectively. Let a be the velocity of advance of the wave as defined in article 2. Con-

ceive a horizontal current with the uniform velocity $-a$ to be combined with the actual wave-motion; the resultant motion is that of an undulating current, presenting stationary waves in its course; and the forces which act on the particles are not altered. The resultant velocity of a particle at the crest becomes $-a+u_1$; and the resultant velocity of a particle in the trough becomes $-a-u_1$. Let the height from trough to crest be denoted by Δz ; then, since the upper surface of the liquid is supposed to be a surface of uniform pressure, the principle of the conservation of energy gives the following equation:

$$g\Delta z = \frac{1}{2}\{(a+u_1)^2 - (a-u_1)^2\} = 2au_1. \quad (1)$$

(4) *Virtual Depth of Uniform Horizontal Disturbance*.—By the phrase “virtual depth of uniform horizontal disturbance,” or, for brevity’s sake, *virtual depth*, I propose to denote *the depth in the liquid to which a uniform horizontal disturbance would have to extend, in order to make the amount of horizontal disturbance equal to the actual amount*. That is to say, conceive that a pair of vertical planes normal to the direction of advance, and each of the breadth unity, coincide at a given instant, one with the trough-line or furrow, and the other with the crest-line or ridge, which bound one of the slopes of a wave. We will suppose this to be the front slope, merely to fix the ideas; for similar reasoning applied to the back slope leads to the same results. At a given depth z below the surface, let $-u''$ be the horizontal velocity with which particles are in the act of passing backwards through the plane at the trough, and $+u'$ the velocity with which particles are passing forwards through the plane at the crest; then the rate by volume at which liquid is passing into the space between those two planes is

$$\int u' dz + \int u'' dz,$$

the integrations extending from the surface to the bottom. Let k denote the virtual depth; then

$$k = \frac{\int u' dz + \int u'' dz}{2u_1}. \quad (2)$$

(5) *Relation between Virtual Depth and Speed of Advance*.—In an indefinitely short interval of time dt , the volume of liquid which passes into the space between the two vertical planes mentioned in article 4 is

$$2ku_1 dt;$$

and in order to make room for that volume of liquid, the front slope of the wave must sweep in the same interval of time through an equal volume. But the volume swept through by the front of the wave is

$$adt\Delta z;$$

so that, cancelling the common factor dt , we have the following equation:

$$a\Delta z = 2ku_1;$$

but, according to equation (1), $\Delta z = \frac{2au_1}{g}$; which value being substituted in the above equation, gives

$$\frac{2a^2u_1}{g} = 2ku_1,$$

and therefore

$$\frac{a^2}{g} = k, \text{ and } a = \sqrt{gk}; \quad \dots \dots (3)$$

so that the *velocity of advance of a wave* (defined as in article 2) *is equal to that acquired by a body in falling through half the virtual depth*; and this is true for all possible waves in which the upper surface is a surface of uniform pressure.

(In article 6 of the paper, the speed of advance of a wave of translation is expressed by combining the speed of a rolling wave, $\sqrt{gk}$, with that of a supposed current, as stated in article 2.

In articles 7, 8, and 9 the law which connects the speed of advance of a wave with the virtual depth is compared with the already known laws of the transmission of rolling waves in water of limited or unlimited depth. The principal results may be summed up as follows.

Let T be the periodic time of a wave, in seconds; $h = \frac{gT^2}{4\pi^2}$ the *equivalent pendulum*—that is, the height of the pendulum whose period is the same; $c = \frac{\text{length}}{2\pi}$ the *rolling radius*, being the radius of a circle whose circumference is equal to a wave-length; u_1 the greatest horizontal velocity, and w_1 the greatest vertical velocity of a surface-particle; a the velocity of advance; then

$$a = \sqrt{gk} = \sqrt{\frac{w_1gc}{u_1}} = \frac{w_1}{u_1} \cdot \sqrt{gh} = \frac{w_1}{u_1} \cdot \frac{gT}{2\pi},$$

and

$$k = \frac{w_1}{u_1} \cdot c = \frac{w_1^2}{u_1^2} \cdot h.)$$

(10) *Oblique Advance of Forced Waves*.—Let s be the velocity with which a floating solid body is driven horizontally; the wave which that solid body pushes or drags along with it is forced to advance at the velocity s also; while the virtual depth of disturbance, k , bears some relation to the depth of immersion and figure of the solid body. If the speed of advance corresponding to that depth, $a = \sqrt{gk}$, is less than s , a pair of wave-ridges diverge obliquely from the path of the floating body towards opposite sides; and the sine of the angle which each of those ridges makes with that path is $\frac{a}{s}$. Such is the mode of formation of the obliquely spreading waves which travel along with ships*.

* See Watts, Rankine, Napier, and Barnes, 'On Ship-building,' Division I. Article 156, p. 79.

When the velocity of the floating body is less than the speed of advance corresponding to the depth to which it disturbs the liquid in its immediate neighbourhood, it is probable that the virtual depth of disturbance of parts of the liquid beyond the immediate action of the floating body adjusts itself to the velocity, and assumes the value $\frac{v^2}{g}$.

11. *Possibility of Obliquely Advancing Tidal Waves.*—It is possible that instead of a depth less than the virtual depth corresponding to the speed of advance of a tidal wave, the ridge of that wave may place itself in a position oblique to the parallels of latitude, according to the principle stated in article 10. It still remains to be ascertained, by the study of tidal observations, whether such phenomena take place in the tides of the ocean.

12. *Terminal Velocity of Waves.*—It is known that in deep water all waves left free from the action of disturbing forces tend ultimately to assume the condition of free rolling waves, whose velocity of advance depends on their periodic time, and is expressed by the equation

$a = \frac{gT}{2\pi}$. This, then, may be called the *terminal velocity* of a wave of a given period. It follows that if a wave is raised through the disturbance produced by a solid body, that wave will at first travel with a speed depending on the virtual depth of the original disturbance; but as it advances to a greater and greater distance from the disturbing body, the velocity of advance will gradually approximate to the terminal velocity corresponding to the periodic time, and the virtual depth will continually adjust itself to the changing velocity, and approximate gradually to the equivalent pendulum corresponding to the periodic time. Such is the cause of the forward curvature of the ridges of the obliquely diverging waves which follow a ship*.

May 14.—Lieut.-General Sabine, President, in the Chair.

The following communications were read:—

“Further Observations on the Spectra of the Sun, and of some of the Stars and Nebulæ, with an attempt to determine therefrom whether these Bodies are moving towards or from the Earth.” By William Huggins, F.R.S.

§ I. Introduction.

The author states that at the time of the publication of the “Observations on the Spectra of the Fixed Stars,” made jointly by himself and Dr. W. A. Miller, Treas. R.S., they were fully aware that the direct comparisons of the bright lines of terrestrial substances with the dark lines in the spectra of the stars, which they had accomplished, were not only of value for the more immediate purpose for which they had been undertaken, namely, to obtain information of the chemical constitution of the investing atmospheres of the stars, but might also possibly serve to reveal something of the motions

* This is explained in greater detail in a paper read to the Institution of Naval Architects on the 4th of April 1868.

of the stars relatively to our system. If the stars were moving towards or from the earth, their motion, compounded with the earth's motion, would alter to an observer on the earth the refrangibility of the light emitted by them, and consequently the lines of terrestrial substances would no longer coincide in position in the spectrum with the dark lines produced by the absorption of the vapours of the same substances existing in the stars.

The method employed by them would certainly have revealed an alteration of refrangibility as great as that which separates the lines D. They had, therefore, proof that the stars which they had examined, among others Aldebaran, α Orionis, β Pegasi, Sirius, α Lyre, Capella, Arcturus, Castor, Pollux, were not moving with a velocity which would be indicated by such an amount of alteration of position in a line.

Since, however, a change of refrangibility corresponding to that which separates the components of D would require a velocity of about 196 miles per second, it seemed to them premature to refer to this bearing of their observations. The earth's motion, and that of the few stars of which the parallax has been ascertained, would make it probable that any alteration in position would not exceed a fraction of the change which would have been observed by them.

The author has since, for several years, devoted much time and labour to this investigation, and believes that he has obtained a satisfactory result.

He refers to Doppler, who first suggested that the relative motion of the luminous object and the observer would cause an alteration of the wave-length of the light; and to Ballot, Klinkerfues, Sonncbe, Fizeau, and Secchi, who have written on the subject.

The author is permitted to enrich his paper with a statement of the influence of the motions of the heavenly bodies on light, and of some experiments made in an analogous direction, which he received in June 1867 from Mr. J. C. Maxwell, F.R.S.

It is shown that if the light of the star is due to the luminous vapour of sodium or any other element which gives rise to vibrations of definite period, or if the light of the star is absorbed by sodium-vapour, so as to be deficient in vibrations of a definite period, then the light, when it reaches the earth, will have an altered period of vibration, which is to the period of sodium as $V+v$ is to V , when V is the velocity of light and v is the velocity of approach of the star to the earth. Equal velocities of separation or approach give equal changes of wave-length.

§ II. Description of Apparatus.

A new spectroscope is described, consisting in part of compound prisms, which gives a dispersive power equal to nearly seven prisms of 60° of dense flint glass. Various methods were employed for the purpose of ensuring perfect accuracy of relative position in the instrument between the star spectrum and the terrestrial spectrum to be compared with it; a new form of apparatus, which appears to be trustworthy in this respect, was contrived. Many of the ob-

servations were made with vacuum-tubes or electrodes of metal, placed before the object-glass of the telescope.

§ III. *Observations of Nebulæ.*

The author states that he has examined satisfactorily the general characters of the spectra of about seventy nebulæ. About one-third of these give a spectrum of bright lines; all these spectra may be regarded as modifications of the typical form, consisting of three bright lines, described in his former papers.

Some of these nebulæ have been reexamined with the large spectroscopé described in this paper, for the purpose of determining whether any of them were possessed of a motion that could be detected by a change of refrangibility, and whether the coincidence which had been observed of the first and the third line with a line of hydrogen and a line of nitrogen would be found to hold good when subjected to the test of a spreading out of the spectrum three or four times as great as that under which the former observations were made. The spectrum of the Great Nebula in Orion was very carefully examined by several different methods of comparison of its spectrum with the spectra of terrestrial substances.

The coincidence of the lines with those of hydrogen and nitrogen remained apparently perfect with an apparatus in which a difference in wave-length of 0.0460 millionth of a millimetre would have been detected. These results increase greatly the probability that these lines are emitted by nitrogen and hydrogen.

It was found that when the intensity of the spectrum of nitrogen was diminished by removing the induction-spark in nitrogen to a greater distance from the slit, the whole spectrum disappeared with the exception of the double line, which agrees in position with the line in the nebulæ; so that, under these circumstances, the spectrum of nitrogen resembled the monochromatic spectra of some nebulæ. It is obvious that if the spectrum of hydrogen were greatly reduced in intensity, the strong line in the blue, which corresponds to one of the lines of the nebular spectrum, would remain visible after the line in the red and the lines more refrangible than F had become too feeble to affect the eye.

It is a question of much interest whether the few lines of the spectra of these nebulæ represent the whole of the light emitted by these bodies, or whether these lines are the strongest lines only of their spectra which have succeeded in reaching the earth. Since these nebulæ are bodies which have a sensible diameter, and in all probability present a continuous luminous surface, we cannot suppose that any lines have been extinguished by the effect of the distance of the objects from us. If we had reason to believe that the other lines which present themselves in the spectra of nitrogen and hydrogen were quenched on their way to us, we should have to regard their disappearance as an indication of a power of extinction residing in cosmical space, similar to that which was suggested from theoretical considerations by Chéseaux, and was afterwards supported on other grounds by Olbers and the elder Struve.

It is also shown that at the time of the observations this nebula was not receding from us with a velocity greater than 10 miles per second; for this motion, added to the earth's orbital velocity, would have caused a want of coincidence of the lines that could have been observed. If the nebula were approaching our system, its velocity might be as much as 20 or 25 miles per second; for part of its motion of approach would be masked by the effect of the motion of the earth in the contrary direction.

§ IV. *Observations of Stars.*

A detailed description is given of the comparisons of the line in Sirius corresponding to F, with a line of the hydrogen spectrum, and of the various precautions which were taken against error in this difficult and very delicate inquiry. The conclusions arrived at are:—that the substance in Sirius which produces the strong lines in the spectrum of that star is really hydrogen; further, that the aggregate result of the motions of the star and the earth in space, at the time the observations were made, was to degrade the refrangibility of the dark line in Sirius by an amount of wave-length equal to 0·109 millionth of a millimetre.

If the velocity of light be taken at 185,000 miles per second, and the wave-length of F at 486·50 millionths of a millimetre, the observed alteration in period of the line in Sirius will indicate a motion of recession between the earth and the star of 41·4 miles per second.

At the time of observation, that part of the earth's motion which was in the direction of the visual ray was equal to a velocity of about 12 miles per second from the star.

There remains unaccounted for a motion of recession from the earth amounting to 29·4 miles per second, which we appear to be entitled to attribute to Sirius.

Reference is made to the inequalities in the proper motion of Sirius; and it is stated that at the present time the proper motion of Sirius in declination is less than its average amount by nearly the whole of that part of it which is variable, which circumstance may show that a part of the motion of the star is now in the direction of the visual ray.

Independently of the variable part of its proper motion, the whole of the motion which can be directly observed by us is only that portion of its real motion which is at right angles to the visual ray. Now it is precisely the other portion of it, which we could scarcely hope to learn from ordinary observations, which is revealed to us by prismatic observations. By combining both methods of research, it may be possible to obtain some knowledge of the real motions of the brighter stars and nebulae.

Observations and comparisons, similar to those on Sirius, have been made on α Canis Minoris, Castor, Betelgeux, Aldebaran, and some other stars. The author reserves the results until these objects have been reexamined. It is but seldom that the atmosphere

is favourable for the successful prosecution of this very delicate research.

§ V. *Observations of the Sun.*

The author has observed the sun with three distinct objects in view :—

1. He has sought to discover if the spectrum of the light from the less luminous part of the sun near the limb, differs in any respect from that of the light from the central parts of his disk.

2. He hoped to obtain a view of the red prominences visible during a solar eclipse by reducing the light from our atmosphere by dispersion ; for, under these circumstances, if the red prominences give a spectrum of bright lines, these lines would remain but little diminished in brightness, and might become visible.

His observations in these two directions have been hitherto unsuccessful.

3. He proposed to seek to gain, from an examination of the spectra of the umbræ and penumbæ of solar spots, some information as to the nature of these phenomena. He has successfully applied the large spectroscope, already described, to the light from the umbra of a spot.

His observations are in accordance generally with those communicated by Mr. Lockyer to the Royal Society.

The author describes the examination of a spot on April 15th, 1868. He shows that about three-fourths of the apparent light of the umbra came from that region of the sun, and the remaining fourth from the intervening illuminated atmosphere of the earth. He observed an increase of width in most of the dark lines of the solar spectrum. The lines C and F, due to hydrogen, did not appear stronger in the spectrum of the umbra. No new lines were detected, nor were any of those of the normal solar spectrum observed to be wanting, in the spectrum of the light from the umbra. No bright lines were seen.

Some of the conditions of the solar surface are considered which the phenomena observed may be supposed to indicate.

A cooler state of the heated vapours by which the lines of absorption are produced would diminish the radiation from the gas itself, and so leave more completely uncompensated the absorption by the gas of the light from behind it. Though in this way an apparent increased intensity of the dark lines would result, the observations seem to suggest a state of the vapours, connected with tension and temperature, in which their power of absorption for each line embraces an increased range of wave-length. Some of the conditions under which this state of things may be brought about are discussed.

The absence of bright lines is not considered conclusive of the complete absence of light in the umbra from luminous gas ; for if there existed in the spot or above it the same vapours in a cooler state, the light would be almost wholly absorbed, and the feebler emanations of the cooler vapour might not do more than render less

intense the dark gaps produced by the vapours in the stronger light of all refrangibilities which is evidently present.

What is the source of the light in the umbra which gives the continuous spectrum? May the dense and intensely heated gases, which probably form the inner substance of the sun, emit, in some cases, lines so greatly expanded as to form, when numerous spectra are superposed, a sensibly continuous spectrum? Dr. Balfour Stewart has suggested that, as gases possess a power of *general* absorption of light, a heated mass of gas, if sufficiently dense to be opaque or nearly so, would give a continuous spectrum as well as the spectrum of bright lines peculiar to it.

“On the Spectrum of Brorsen’s Comet, 1868.” By William Huggins, F.R.S.

In January 1866 I communicated to the Royal Society the result of an examination of a small comet visible in the beginning of that year*. I examined the spectrum of another small and faint comet in May 1867. The spectra of these objects, as far as their very feeble light permitted them to be observed, appeared to be very similar. In the case of each of these comets, the spectrum of the minute nucleus appeared to consist of a bright line between *b* and *F*, about the position of the double line of the spectrum of nitrogen, while the nebulosity surrounding the nucleus and forming the coma gave a spectrum which was apparently continuous.

Unfavourable weather prevented me from obtaining an observation of Brorsen’s comet, at its present reappearance, before April 29. Since that evening I have examined it on May 2, 4, 6, 7, 12, 13. As I have not noticed any change in its spectrum during this time, I will put together the results of my observations on different nights, in order to avoid the repetition which would occur if the observations were arranged in the order in which they were made.

I tried various spectroscopes upon this object. The best views of its spectrum were obtained with a spectroscope of the form already described in my former papers†, and furnished with one prism of very dispersive flint glass, with a refracting angle of 60° . Some measures were taken with a similar spectroscope with two prisms of 60° .

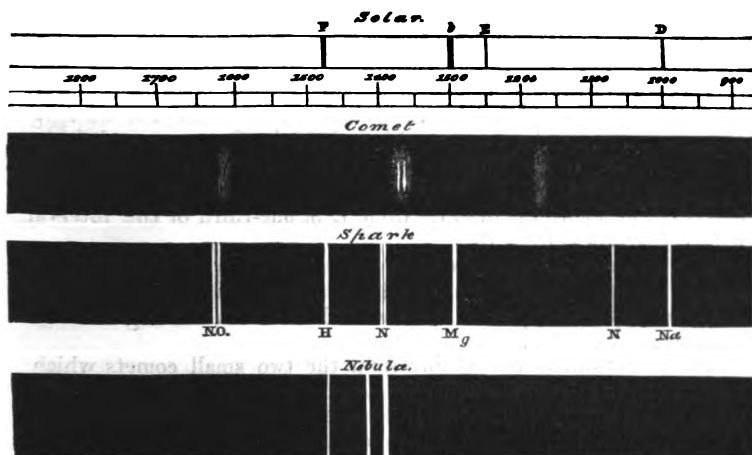
The comet appears in the telescope as a nearly round nebulosity, in which the light increases rapidly towards the centre, where on some occasions I detected, I believe, a small stellar nucleus. Generally this minute nucleus was not to be distinguished from the bright central part of the comet. I suspected two or three bright points in the coma. May 7, I perceived a small extension of the faint surrounding nebulosity in a direction opposite to the sun, so as to form a short tail.

The spectrum of this comet consists for the most part of three bright bands. The length of the bands in the instrument shows that they are not due alone to the stellar nucleus, but are produced by the light of the brighter portions of the coma.

* Proceedings of the Royal Society, vol. xv. p. 5.

† Philosophical Transactions, 1864, p. 415, &c.

I took some pains to learn the precise character of these luminous bands. When the slit was wide they resembled the expanded lines



seen in some gases—for example, the line F in the spectrum of hydrogen at the atmospheric pressure. As the slit was made narrow the two fainter bands, namely the one in the yellow and the one in the blue, appeared to fade out without becoming more defined. I was unable to resolve these bands into lines. In this respect they are very different from the bright lines of the nebulae, which become narrow as the slit is made narrow.

The middle band, which is so much brighter than the others that it may be considered to represent probably three-fourths, or nearly so, of the whole of the light which we receive from the comet, appears to possess similar characters. In this nebulous band, however, I detected occasionally two bright lines, which appeared to be shorter than the band, and may be due to the nucleus itself. This suspicion seems to be strengthened by the circumstance that when by moving the telescope the image of the comet was made to pass before the slit, these brighter lines were only observed when the middle of the comet was upon the slit; while the nebulous band continued as long as any part of the comet, except its extreme margin, was upon the slit.

Besides these three bright bands there was a very faint continuous spectrum. This spectrum is omitted in the diagram, as it could scarcely be represented without making it appear too strong relatively to the bright bands.

The position in the spectrum of the bands was determined by micrometrical measures, and also by simultaneous comparison of the bands with the bright lines of magnesium, sodium, hydrogen, and nitrogen. The brightest band, which is in the green part of the spectrum, is nearly in the position of the brightest line of the nebulae,

which coincides with the double line of the spectrum of nitrogen ; but, as the diagram shows, the band in the comet is in a small degree less refrangible than the line of nitrogen. This difference of refrangibility cannot be attributed to the comet's motion, since at the time the observations were made the comet was approaching the earth.

The band in the blue is considerably more refrangible than F, and is nearly as refrangible as the group of bright lines in the air-spectrum which have the numbers 2642, 2669 in the map and tables of my paper "On the Spectra of the Chemical Elements" *.

The least refrangible of the bands occurs in the yellow part of the spectrum, at about the distance from E of one-third of the interval which separates E from D.

The spectrum of this comet resembles the diagram given by Donati of the spectrum of Comet I., 1864†. The positions of the three bands seen by him appear to agree with those which the bright bands of this comet occupy.

This comet differs remarkably from the two small comets which I examined, in the much smaller relative proportion of the light which forms a continuous spectrum. In Brorsen's comet, as it now appears, the bright middle part of the nebulosity seems to have a constitution analogous at least to that of the nucleus, and to be self-luminous ; in the other comets the coma, which surrounded a distinctly marked nucleus, gave a continuous spectrum. The three comets resemble each other in the circumstance that the light of the bright central part was emitted by the cometary matter, while the surrounding nebulosity reflected solar light.

The telescopic observations of the heads of Donati's comet and of other large comets have shown that the luminous material is not at once driven off into the outer portions of the coma and the tail, but usually forms in front of the nucleus a dense luminous cloud, which for a time seems to be identical in the character of its light with that of the nucleus. It is, I believe, the outer portions only of the coma (which are frequently separated by dark spaces from the nucleus) and the tail which the polariscope has shown to shine by reflected light.

The positions of the bands in this comet would seem to indicate a chemical constitution different from that of the nebulae which give a spectrum of bright lines. It will be seen in the diagram that, though the brightest of the bands in the spectrum of the comet differs but little in position from the brightest line of the nebulae, the other bands are found in parts of the spectrum widely removed from those in which the other lines of the nebulae occur. The suggestion presents itself whether the broad, nebulous bands may not indicate conditions of temperature and molecular state different from those which occur in the gaseous nebulae. Plücker has shown that nitrogen and some other substances give totally different spectra under different conditions of temperature and tension. The spectrum of this comet, how-

* Philosophical Transactions, 1864, p. 158.

† Astronomische Nachrichten, No. 1488.

ever, does not resemble the other spectrum of nitrogen, which Plücker distinguishes as the spectrum of the first order*.

June 11.—Lieut.-General Sabine, President, in the Chair.

The following communication was read:—

“On the Occlusion of Hydrogen Gas by Metals.” By Thomas Graham, F.R.S., Master of the Mint.

In my experiments, already published, on the occlusion of hydrogen by the metals palladium, platinum, and iron, the absorption of the gas was observed to be of uncertain occurrence at low temperatures, but was ensured by heating the metal, whether in the form of sponge or aggregated by hammering, and allowing it to cool slowly and completely in a hydrogen atmosphere. This fact was referred to the condition of absolute purity of the metallic surface being essential to the first absorbing action, as it is to the action of platinum-foil or wire in determining the combustion of the gaseous mixture of oxygen and hydrogen, as observed by Faraday. A new method of charging the metals with hydrogen at low temperatures has lately presented itself, which is not without interest.

When a plate of zinc is placed in dilute sulphuric acid, hydrogen is freely evolved from the surface of the metal, but no hydrogen is occluded and retained at the same time. A negative result was indeed to be expected from the crystalline structure of zinc. But a thin plate of palladium immersed in the same acid, and brought into metallic contact with the zinc, soon becomes largely charged with the hydrogen, which is then transferred to its surface. The charge taken up in an hour by a palladium plate, rather thick, at 12° , amounted to 173 times its volume.

The absorption of hydrogen was still more obvious when the palladium plate was constituted the negative electrode in acidulated water to a Bunsen battery of six cells. The evolution of oxygen gas at the positive electrode continuing copious, the effervescence at the negative electrode was entirely suspended for the first twenty seconds, in consequence of the hydrogen being occluded by the palladium. The final absorption amounted to 200.4 volumes, and was greater in amount than the volume of hydrogen occluded by the same plate heated and cooled in an atmosphere of the gas, which did not exceed 90 volumes.

It is worthy of remark that, although the hydrogen enters the palladium and no doubt pervades the whole mass of the metal in such circumstances, the gas exhibits no disposition to leave the metal and escape into a vacuum, at the temperature of its absorption. Thus a thin plate of palladium, charged with hydrogen in the manner described, was washed, dried by a cloth, and then sealed up in an exhausted glass tube. On breaking the tube under mercury after two months, the vacuum was found perfect. No hydrogen had vaporized in the cold (about 12°); but on the application afterwards of a heat of 100° and upwards, 333 volumes of gas were evolved from the metal.

* Philosophical Transactions, 1865, p. 9.

A similar result was obtained on making a hollow palladium cylinder, of which the length was 115 millimetres, diameter 12 millimetres, and thickness 1 millimetre, the negative electrode in an acid fluid, while the closed cavity of the cylinder was kept exhausted by means of a Sprengel aspirator. No hydrogen whatever passed through into the vacuous cavity in several hours, although the gas was no doubt abundantly absorbed by the outer surface of the cylinder and pervaded the metal throughout.

It appears, then, that when hydrogen is absorbed by palladium the volatility of the gas may be entirely suppressed; and hydrogen may be largely present in metals without exhibiting any sensible tension at low temperatures. Occluded hydrogen is certainly no longer a gas, whatever may be thought of its physical condition. The same conclusion was indicated by another series of experiments, in which it was found that, to be occluded by palladium, and even by iron, hydrogen does not require to be applied under much pressure, but, on the contrary, when highly rarefied is still freely absorbed by these metals.

The occluded hydrogen is readily extracted from palladium by reversing the position of the latter in the decomposing cell of the battery, so as to cause oxygen to be evolved on the surface of the metal. The hydrogen is then drawn out as rapidly as it had previously entered the palladium, and the metal is exhausted in a complete manner by such treatment. When palladium charged with hydrogen is left exposed to the atmosphere, the metal is apt to become suddenly hot, and to lose its gas entirely by spontaneous oxidation.

Platinum may be charged with hydrogen by voltaic action, as well as palladium, but with the usual inferior proportion of gas. The charge of hydrogen taken up in a decomposing voltaic cell by old platinum in the form of a tube, of the thickness of a small crucible, was 2.19 volumes. This absorbed gas was also readily withdrawn from the platinum and oxidized on reversing the place of the metal in the decomposing cell. The platinum acquired its well-known polarizing-power in virtue of the occluded hydrogen. This power was retained by the metal after being washed with pure water and wiped with a cloth, and was brought into action on placing the metal in dilute acid. The temperature required to expel the hydrogen so absorbed by platinum was found to be little short of a red heat, although the gas had entered the metal at a low temperature.

Soft iron, left some time in a dilute acid, occluded 0.57 volume of hydrogen. This charge of gas was also retained at low temperatures, and did not escape into a vacuum till the temperature was raised nearly to redness. This proves that, like platinum, iron is not penetrated through in the cold by hydrogen, the temperature of emission being elevated considerably*.

* In M. Cailletet's experiment of exposing a thin sheet of iron to an acid, the metal is no doubt penetrated through by hydrogen in the cold, but apparently from the penetrating agency of the acid which is insinuating itself into the metal at the same time.—*Comptes Rendus*, 4 Mai 1868

While hydrogen was absorbed freely by palladium and platinum as negative plates, no oxygen whatever was absorbed by plates of the same metals in the position of positive electrodes. Oxygen gas was disengaged freely on the surface of the latter without being condensed. A platinum plate which had acted for several hours as a positive electrode, gave afterwards, when submitted to heat with exhaustion, a small trace of carbonic acid but no oxygen.

The familiar igniting-power of platinum sponge (or clean plate) upon a jet of hydrogen in the air seems to depend solely upon the influence of the metal upon its occluded hydrogen. The hydrogen appears to be polarized, and to have its attraction for oxygen greatly heightened. I beg to offer the following representation of this phenomenon, with an apology for the purely speculative character of the explanation. The gaseous molecule of hydrogen being assumed to be an association of two atoms, a hydride of hydrogen, it would follow that it is the attraction of platinum for the negative or "chlorylous" atom of the hydrogen molecule which attaches the latter to the metal. The tendency, imperfectly satisfied, is to the formation of a hydride of platinum. The hydrogen molecule is accordingly polarized, *orienté*, with its positive or "basylous" side turned outwards, and having its affinity for oxygen greatly enlivened. It is true that the two atoms of a molecule of hydrogen are considered to be inseparable; but this may not be inconsistent with the replacement of such hydrogen atoms as are withdrawn, on combining with oxygen, by other hydrogen atoms from the adjoining molecules. It is only necessary to suppose that a pair of contiguous hydrogen molecules act together upon a single molecule of the external oxygen. They would form water, and still leave a pair of atoms, or a single molecule of hydrogen, attached to the platinum.

The oxidation of alcohol, ether, and similar hydrocarbons, through the agency of platinum, likewise appears to be always an immediate consequence of a similar polarization of the hydrogen of those substances, or of some other oxidable constituent.

As has already been remarked, it does not follow that, because a gas is occluded by a metal, under the pressure of the atmosphere, at a low temperature, the gas will also escape from the metal into a vacuum at the same temperature, a much higher temperature being often required for the expulsion of the gas than for its first absorption. This is particularly true of carbonic oxide occluded by iron. Cast iron is much too porous for such experiments, and allows carbonic oxide, equally with other gases, to pass through abundantly by the agency of gaseous diffusion. Even with malleable iron there is a difficulty in observing, owing to the long time during which that metal continues to discharge carbonic oxide from its own store of that gas. But a malleable iron tube, first thoroughly deprived of its natural gas, was found to allow carbonic oxide to pass through it into a vacuum very slowly compared with hydrogen, although the volume of carbonic oxide which the metal is capable of absorbing is very sensible, amounting to 4 volumes, and more considerable than the volume of hydrogen which the same metal can occlude. Car-

bonic oxide did not sensibly pass through iron of 1·7 millimetre in thickness till the temperature was greatly elevated; and then the passage of gas was, in a minute—

Of carbonic oxide, at a full red heat, 0·284 cub. centim. per square metre of surface.

Of hydrogen oxide, at a full red heat, 76·5 cub. centims. per square metre of surface.

The condition of hydrogen as occluded by a colloidal metal may be studied with most advantage in its union with palladium, where the proportion of gas held is considerable. In the pulverulent spongy state, palladium took up 655 volumes of hydrogen; and so charged it gave off no gas *in vacuo* at the ordinary temperature, nor till its temperature was raised to nearly 100°. Hammered palladium foil has been observed to take up quite as much gas. But the condition in which palladium appears to be most absorptive is when precipitated from a solution of about 1·6 per cent. of the chloride, by the action of a voltaic battery, in form of a compact metal. Palladium is not one of the metals readily thus precipitated; but it may be thrown down upon a thin platinum wire, in brilliant laminæ, by the action of a large single cell. The palladium after a time detaches itself from the wire, exhibiting a bright white metallic surface where it had been in contact with the platinum, and a dull surface, suggesting metallic arsenic, on the side exposed to the acid. As so prepared, it does not contain any occluded hydrogen. But the metallic films, when heated to 100° in hydrogen, and allowed to cool slowly for an hour in the same gas, were found to occlude 982·14 volumes of gas, measured with thermometer at 11°, and barometer at 756 millimetres. This is the largest absorption of hydrogen observed. From palladium so charged there was a slight indication of the escape of hydrogen into a vacuum, with extreme slowness in the cold. This charged palladium is represented by weight as

Palladium 1·0020 grm.	99·277
Hydrogen 0·0073 grm.	·723
	<hr/>
	100·000

It is in the proportion of one equivalent of palladium to 0·772 equivalent of hydrogen*, or there is an approximation to single equivalents Pd H. But the idea of definite chemical combination is opposed by various considerations. No visible change is occasioned to the metallic palladium by its association with the hydrogen. Hydrides of certain metals are known, as the hydride of copper (Wurtz) and the hydride of iron (Wanklyn); but they are brown pulverulent substances with no metallic characters. Indeed a hydride of palladium itself can be formed, but not preserved, on account of its great instability. Following the process of M. Wurtz for the hydride of copper, nitrate of palladium was boiled with sulphuric acid, and the sulphate of palladium (a red crystalline salt)

* H=1, Pd=106·5.

prepared. A solution of this salt, with an excess of sulphuric acid, was precipitated by the hypophosphite of soda; a black powder fell, which speedily underwent decomposition at 0° , evolving copious volumes of hydrogen gas. The final residue appeared to be pure palladium, of its usual black amorphous appearance, and with no trace of crystallization. It is singular that this palladium precipitate contained no occluded hydrogen; and even when heated, and afterwards exposed to an atmosphere of hydrogen in the usual manner, the palladium black so prepared condensed no sensible quantity of that gas.

I am inclined to conclude that the passage of hydrogen through a plate of metal is always preceded by the condensation or occlusion of the gas. But it must be admitted that the rapidity of penetration is not in proportion to the volume of gas occluded; otherwise palladium would be much more permeable at a low than at a high temperature. A plate of that metal was sensibly exhausted of hydrogen gas at 267° , but continued permeable, and in fact increased greatly in permeability at still higher temperatures, and without becoming permeable to other gases at the same time. In a striking experiment, a mixture of equal volumes of hydrogen and carbonic acid was carried through a small palladium tube, of which the internal diameter was 3 millimetres, and the thickness of the wall 0.3 millimetre. From the outer surface of this tube gas escaped into a vacuum, at a red heat, with the enormous velocity of 1017.54 cub. centims. per minute for a square metre of surface. This gas did not disturb baryta-water. It was pure hydrogen.

A still more rapid passage of hydrogen was observed through the substance of a hollow cylinder of palladium 1 millimetre in thickness, at a higher temperature, approaching the melting-point of gold. The palladium cylinder being enclosed in a porcelain tube charged with pure hydrogen, was exhausted as usual, and gave 105.8 cub. centims of gas in five minutes; measured with bar. 753 millims., therm. 10° . As the external surface of the palladium tube amounted to 0.0053 square metre, the passage of gas was

3992.22 cub. centims. from a square metre of surface, per minute.

The rate of penetration of hydrogen through the same palladium tube, at the lower temperature of 265° C., was previously observed to be

327 cub. centims. from a square metre of surface, per minute.

The velocity of penetration thus appears to increase in a rapid ratio with the temperature.

When carbonic acid was substituted for hydrogen, at the same high temperature, a very minute penetration was perceived, amounting to

1.86 cub. centim. from a square metre of surface, per minute.

This gives for carbonic acid one twenty-thousandth part of the rate of hydrogen. Whether it is a penetration of the same sort, although greatly less in degree, or rather the consequence of a sensible porosity in the palladium (of which it would become the measure), remains uncertain.

The quantity of hydrogen held by the metal at these high temperatures may become too small to be appreciated; but I presume it is still present, and travels through the metal by a kind of rapid cementation. This extreme mobility is a singular property of hydrogen, which was involved in the fundamental discovery, by MM. H. Sainte-Claire Deville and Troost, of the passage of that gas through plates of iron and platinum at high temperatures.

The marked rapidity of the passage of the same gas through a thin sheet of caoutchouc appears to be more capable of explanation on known principles. Caoutchouc of less than 0.1 millimetre in thickness, if impregnated with hydrogen, loses its gas entirely by the most momentary exposure to the air. A tube of 2 millimetres in thickness, through which hydrogen and carbonic acid were singly passed, each for an hour, was found to retain—

Of hydrogen	0.0113 volume.
Of carbonic acid	0.2200 „

The absorption, then, is in the proportion of 1 hydrogen to 20 carbonic acid, but the comparative rate of penetration of the two gases through a sheet of caoutchouc is as 1 hydrogen to $2\frac{1}{2}$ carbonic acid; or the hydrogen moves eight times as rapidly as the density of its solution would indicate. But these gases differ in diffusibility as carbonic acid 1 to hydrogen 4.7. The rapid passage of hydrogen through caoutchouc is thus partly explained by the rapid manner in which that gas is brought to one surface of the sheet and conveyed away from the other by gaseous diffusion. Again, both substances travel through the substance of the caoutchouc by their diffusibility *as liquids*. Suppose hydrogen in that form to be nearly as much more diffusive than the other substance as it is when both are gaseous, then the observed rapid passage of hydrogen through caoutchouc would appear to be fully accounted for.

Liquid diffusion has also a bearing upon the rapid dissemination of hydrogen through a soft colloid metal, like palladium or platinum, at a high temperature. The liquid diffusion of salts in water is known to be six times as rapid at 100° as at 0° . If the diffusion of liquid hydrogen increases with temperature in an equal ratio, it must become a very rapid movement at a red heat. Although the quantity absorbed may be reduced (or the channel narrowed), the flow of liquid may thus be increased in velocity. The whole phenomena appear to be consistent with the solution of liquid hydrogen in the colloid metal. The “solution affinity” of metals appears to be nearly confined to hydrogen and carbonic oxide, so that metals are not sensibly penetrated by other gases than these.

ROYAL INSTITUTION OF GREAT BRITAIN.

May 22, 1868.—“On some Effects of the Heat of the Oxyhydrogen Flame.” By William Odling, M.B., F.R.S.

I.

Chemical changes, whether of combination or decomposition, result in the production of new bodies which, under the conditions

of the change, have for the most part a greater stability than the original bodies.

One evidence of this greater stability is afforded by the development of a quantity of heat (the heat of chemical action) from the produced bodies having a smaller potential heat than the original ones.

It results, both from reason and experiment, that in order to undo or reverse any definite chemical action, just so much heat must be directly or indirectly expended as was evolved by the original action.

For the same quantity of heat evolved, the resulting temperature varies with the mass and kind of matter heated, and with the rapid or gradual evolution of the heat.

When the evolution of heat is instantaneous, the resulting temperature may be calculated from the quantity of heat evolved and the mass and specific heat &c. of the matter heated.

By a unit of heat is meant the quantity of heat necessary to raise the temperature of one kilogramme of water one degree Centigrade, or, more accurately, from 0° to 1° .

II.

Every 18 grammes of water is a combination of two 1-gramme proportions of hydrogen H, with one 16-gramme proportion of oxygen O; and by the combination of 2 grammes of hydrogen with 16 grammes of oxygen, there are developed 68 units of heat.

Of these 68 units of heat, however, little more than 57 units are really due to the chemical action, nearly 11 units of heat being evolved by the contraction of the original mixed gas into two-thirds its volume of steam, and by the further condensation of the resulting steam into 18 cubic centimetres of water.

While the quantity of heat evolved by the combination of a given quantity of oxygen and hydrogen is invariable, the intensity of the heat may vary from a scarcely recognizable rise of temperature up to the highest temperature of the oxyhydrogen blowpipe flame, capable of fusing platinum and silica.

A most remarkable effect of the intense temperature resulting from the combination of oxygen and hydrogen into water, is the partial decomposition of water into oxygen and hydrogen, discovered by Mr. Grove in 1846.

At this high temperature hydrochloric acid and carbonic anhydride gases also undergo partial decomposition, into hydrogen and chlorine and into carbonous oxide and oxygen respectively.

Upon what do these singular decompositions by heat, of bodies formed with great evolution of heat, depend? or with what class of chemical phenomena may they be associated?

III.

Under certain familiar conditions, chemical action seemingly takes place to its utmost possible extent in a single direction only, with production of a maximum amount of the substance that is formed with maximum evolution of heat.

For example, taking atomic proportions in grammes, the heat of formation of chloride of zinc, ZnCl_2 , is 101 units; and the heat of

formation of chloride of copper, CuCl_2 , is 60.5 units. Hence, with chlorine in solution and excess of both copper and zinc, there is finally produced the maximum possible amount of chloride of zinc and no chloride of copper.

Again, on addition of sufficient zinc to solution of chloride of copper, there is complete combination of chlorine with zinc and complete separation of chlorine from copper, *i. e.* complete burning of the one metal and complete unburning of the other.

IV.

But under simpler though less familiar conditions, chemical action habitually takes place in more than one direction simultaneously, with production of correlative products in varying proportions.

Thus, with hydrogen and excess of both chlorine and oxygen, although the heat of formation of oxide of hydrogen, H^2O , is 57 units, and the heat of formation of chloride of hydrogen, 2HCl , is only 47.5 units, yet in this case the hydrogen does not combine with the oxygen to the exclusion of the chlorine, but divides itself between the oxygen and the chlorine in proportions which vary with the conditions of the experiment.

In accordance with this result it is found that, at the same red heat, excess of chlorine will effect the partial decomposition of water with extrusion of oxygen, and, conversely, that excess of oxygen will effect the partial decomposition of hydrochloric acid with extrusion of chlorine.

So that, beginning with the two chemical substances water and chlorine, or beginning with the two chemical substances hydrochloric acid and oxygen, or beginning with the three chemical substances hydrogen, chlorine, and oxygen, there exist at a full red heat the four chemical substances, water, hydrochloric acid, chlorine, and oxygen—the proportions of the four substances depending certainly upon the relative quantities present of the elements concerned, and most probably also upon the temperature of the experiment.

Similarly, beginning with the one chemical substance water (Grove), or beginning with the two chemical substances oxygen and hydrogen (Bunsen), there always exist at a sufficiently high temperature the three chemical substances water, oxygen, and hydrogen.

Although, by exposure to a red heat, the electrolytic mixture of oxygen and hydrogen gases becomes completely combined or transformed into water, yet, as recently shown by Bunsen, at the high temperature of 2024° only one-half, and at the still higher temperature of 2844° only one-third of the mixture undergoes combination, the other one-half or two-thirds remaining in the state of mixed gas.

V.

Chemists are acquainted with many reciprocal actions comparable with those of chlorine upon water, and of oxygen upon hydrochloric acid, the most familiar instance being probably the decomposition of ignited oxide of iron by hydrogen with extrusion of iron, and the converse decomposition of oxide of hydrogen by ignited iron with extrusion of hydrogen.

Similarly, sodium will decompose the oxides of carbon, while carbon will decompose oxide of sodium; and just as a sufficient excess of chlorine may be made to effect the almost complete decomposition of a given quantity of water, so may a sufficient excess of carbon (or carbonous oxide) be made to effect the almost complete decomposition of a given quantity of sodium-oxide or zinc-oxide, as in the ordinary processes for obtaining the two metals, notwithstanding that, for an equal consumption of oxygen, the respective combination heats of sodium and zinc exceed by far the combination heat of carbon or carbonous oxide.

Again, although the combination heat of oxygen and carbonous oxide is 68 units, while that of oxygen and hydrogen is only 57 units, yet, as was shown by Bunsen many years ago, upon exploding a mixture of oxygen with a joint excess of carbonous oxide and hydrogen, the oxygen does not attach itself exclusively to the carbonous oxide, but divides itself between the carbonous oxide and hydrogen in a ratio determined by their relative proportions.

GEOLOGICAL SOCIETY.

[Continued from vol. xxxv. p. 399.]

March 25th, 1868.—Prof. T. H. Huxley, LL.D., F.R.S., President, in the Chair.

The following communications were read:—

1. "On some new species of Palæozoic Crustacea from the Upper Silurian rocks of Lanarkshire, &c., and further Observations on the Structure of *Pterygotus*." By Henry Woodward, Esq., F.G.S., F.Z.S.

The nature of the remains which have been referred by Mr. Salter to *Eurypterus* (*Pterygotus*) *punctatus* was first discussed by the author, who came to the conclusion that the Lanarkshire specimens belong to a new species—*Eurypterus scorpioides*,—while the chelate antennæ and the detached lip-plate must have belonged to other species.

Eurypterus scorpioides is the first of the new forms now described by Mr. Woodward, and is represented by a specimen exhibiting an almost entire individual, and certain other fragments. The punctate ornamentation of this species may be readily distinguished from the scale-like markings of *Pterygotus* and *Slimonia*. The second new form, *Eurypterus obesus*, is remarkable for the great obesity of the thoracic somites; it is represented by the impression and counterpart of an entire specimen. Its small size suggested to the author the possibility of its being the young of some larger species. The third new species, *Pterygotus raniceps*, is at present known only by a single example; its head is remarkable on account of its obtusely pointed triangular form and prominent marginal eyes.

In conclusion the author made some observations on the structure of *Pterygotus*, showing that it possessed a series of branchial plates—leaf-like bodies presenting a highly vascular and delicate struc-

ture, arranged in a linear series of from 6 to 8 in each row, and appearing to have occupied a position beneath the thoracic plate on the ventral surface of the body, as seen in *Limulus* at the present day. He also suggested that *Pterygotus perornatus* and *P. crassus* are both varieties of *P. bilobus*.

2. "On the Coniston Group." By Prof. R. Harkness, F.R.S., F.G.S., and Dr. H. A. Nicholson, F.G.S.

The object of this communication was to record the occurrence of a new and unique horizon, containing a rich Graptolite-fauna, in that portion of the Silurian series of the Lake-district termed the Coniston-flags by Professor Sedgwick. The authors also gave a detailed description of these flags, and pointed out their physical and palæontological relations with the Coniston Limestone below and the Coniston Grits above them.

The palæontological relations of the Coniston Limestone and of the underlying green slates and porphyries have been previously shown to be those of the Bala and Caradoc group. The mudstones succeeding to the Coniston Limestone yield an entirely new fauna, including six species of *Diplograpsus*, all of which, with one exception, are in Britain characteristic Upper-Llandeilo forms; and the evidence of the other species is in the same direction. In Ireland, however, many of these species have been obtained from strata of Caradoc age. The fossils of the Coniston Grits have very little affinity with those of the Kendal Flags, nor do they exhibit such a facies as would connect them with the lower members of the Upper Silurian series. Palæontologically, therefore, this Coniston series must be looked upon as a continuous group of rocks; and the physical evidence leads to the same conclusion. There is thus, in the Lake-district, a greater development of Caradoc and Bala rocks than is to be found elsewhere in the British Islands, as we are now required to add a great thickness of strata, possessing, on the whole, a decidedly Lower-Silurian fauna, but containing some new forms of life in its higher portions.

3. "Death of Fishes on the coast of the Bay of Fundy." By Dr. A. Leith Adams, F.G.S., 22nd Regiment.

On the 24th of September, during a heavy gale from the west, impinging almost straight on to the entrance of the Lagoon, known as Anderson's cove, enormous numbers of fish were observed floating dead upon the surface of the water, and thrown up in quantities by the waves. On the gale subsiding, the whole surface of the lagoon and its banks were covered with dead fish, to the depth of a foot in some places. It was evident that the shoal had been literally ground to pieces against the rocks by the force of the waves. In conclusion the author referred to the vast quantities of fossil fish found in the Devonian and other strata, which suggested catastrophes allied to the above incident.

4. "On Volcanoes in the New Hebrides and Banks Islands." By R. Atkins, Esq., of the 'Southern Cross.'

The author described the islands of Tanna, Lopevi, and Ambrym,

in the New Hebrides, and Santa Maria and Great Banks Islands, among the Banks-Island group, as being now active volcanoes, and gave an account of a visit to the Hot Springs of Great Banks Island. These springs deposit quantities of almost pure sulphur.

April 8th, 1868.—Prof. T. H. Huxley, LL.D., F.R.S.,
President, in the Chair.

The following communications were read :—

1. "On the Affinities and probable Habits of the extinct Australian Marsupial, *Thylacoleo carnifex*, Owen." By W. H. Flower, Esq., F.R.S., F.G.S., &c.

Thylacoleo was first described by Prof. Owen in the Philosophical Transactions for 1859, from an imperfect skull, the characters of which led to the conclusion that it was "one of the fellest and most destructive of predatory beasts," having its nearest affinities among existing marsupials with *Dasyurus ursinus*, although the interval be still very great between them. In a subsequent description of a more perfect skull, Prof. Owen's views of the affinities, though not of the habits and food of the animal, were modified. It was stated to be more nearly related to the Diprotodonts, Nototheres, Koalas, Phalangers, and Kangaroos, but at the same time to exemplify "the simplest and most effective dental machinery for predatory life and carnivorous diet known in the Mammalian class."

The author of the present paper, while entirely concurring with Prof. Owen in his later views of the affinities of *Thylacoleo*, and pointing out in detail its relations, especially with the Rat Kangaroos (*Hypsiprymnus*) and the Phalangers (*Phalangista*), demurred to the soundness of the conclusion as to its predaceous habits. He remarked that as the greater number, if not all, of the known animals of the group to which *Thylacoleo* undoubtedly belongs are either vegetable or mixed feeders, the probabilities would be that this creature conformed with its congeners in this respect, unless it possessed any such striking adaptive modification of the normal typical dentition of the group as to lead to a directly opposite conclusion.

He then proceeded to discuss this question, showing that in its rodent-like incisors, rudimentary canines, and hypsiprymnoid premolars it presents no sufficient approximation to any of the true predaceous carnivores, either placental or marsupial, as in his opinion to justify the inference as to its habits which is expressed in the name bestowed upon it.

2. "On the Thickness of the Carboniferous Rocks of the Pendle Range of Hills, Lancashire." By E. Hull, Esq., B.A., F.R.S., F.G.S., of the Geological Survey of Scotland.

This paper was supplementary to a former communication by the author, in which he endeavoured to prove the south-easterly attenuation of the Carboniferous *sedimentary* strata of the North of England, while the *calcareous* member (the Mountain-limestone) attained its

greatest vertical development in Derbyshire, and thence thinned away northward and westward. The author now gave the results of his subsequent investigations while engaged in the survey of the Pendle Range and the neighbourhood of Burnley and Blackburn, which have shown that the increase in the thickness of the *sedimentary* deposits is continued into that district, the aggregate thickness of the Coal-measures, the Millstone-grit, and the Yoredale series being in the Burnley district 18,635 feet, while in Leicestershire it has dwindled down to 3100 feet. In discussing the question of the source of these sediments, the author came to the conclusion that they were derived from a primæval Atlantis,—a view which he considered to be strengthened by the fact that the Carboniferous sedimentary strata of North America also swell out towards the north-east, and become attenuated towards the south and west.

3. "Observations on the relative Ages of the leading physical Features and Lines of Elevation of the Carboniferous district of Lancashire and Yorkshire." By E. Hull, Esq., B.A., F.R.S., of the Geological Survey of Scotland.

The author first described the Pendle Range as a great arch of Carboniferous rocks, bordered on the north and south by a succession of parallel (W.S.W. to E.N.E.) arches and troughs, to all of which he assigned a Præpermian age. He regarded them as belonging to the earliest of three consecutive periods of disturbance, to which all the principal flexures and faults of the district may be referred. The Pennine Chain, which runs nearly north and south, he believed to have been upheaved during a later period, namely, the close of the Permian, while the numerous north-west faults of the district under consideration he referred to the close of the Jurassic period. Mr. Hull described in detail the evidence upon which these conclusions rested, observing that immediately upon the close of the Carboniferous period the northern limits of the Lancashire and Yorkshire coal-fields were determined by the upheaval and denudation of the beds along east and west lines, the coal-fields themselves retaining their original continuity across the region now formed of the Pennine Hills, from Skipton southwards. At the close of the Permian period these coal-fields were dissevered by the uprising of the area now formed of the Pennine range, by lines of upheaval ranging from north to south, nearly at right angles to the former, this fact being of itself an evidence of difference of age. In conclusion the author pointed out that the denudation of the rocks of the district may be referred to seven periods, beginning with the commencement of the Permian and ending with the Postglacial; he defined the duration and effect of each of these periods, and stated the evidence on which his conclusions rested.

XIII. *Intelligence and Miscellaneous Articles.*

ON THE SPECTRUM OF BRORSSEN'S COMET.

BY FATHER SECCHI.

TAKING advantage of a few tolerably bright evenings, I have examined the prismatic spectrum of Brorsen's comet, which is now tolerably visible.

The spectrum is not visible enough with compound spectroscopes with a slit, the light being too feeble; I therefore used a simple spectroscope with direct vision. To fix the position of the lines, I first used the direct image of the comet seen with its spectrum; but as the light was still too faint, I determined the relative positions of the lines by comparing them with those of Venus, placing the comet and the planet successively in the same position on the finder. Both methods gave identical results, but the latter gave most light.

The spectrum of the comet is discontinuous; it consists at first of feeble light, filling the field of view, in which three zones stand out with sufficient distinctness to appear more expanded than the rest. The brightest is the middle zone, which occupies the green colour, and corresponds to the region comprised between the magnesium (*b*) and the hydrogen (F), but much nearer the former; the breadth of this zone is very restricted, and does not exceed one-fifth of the distance of the two lines. When the atmosphere is particularly favourable, it is almost reduced to a single bright line, about as broad as the nucleus of the comet. Another brilliant zone, though far less intense, is met with in the greenish yellow, in the middle of the distance between the sodium (D) and the magnesium (*b*). Another band in the red is sometimes met with, but it is fixed with difficulty. The third zone of luminous intensity, about halfway between the two former, is on the side of the blue, at about one-third of the distance from F to G, starting from F. This band is brilliant enough to be well measured, and produces by scintillation the appearance of a line.

The following is the relative position of these bands referred to the spectrum of Venus in terms of the micrometer:—

Sodium (D)	5·13
Comet, first luminous band.....	5·92
Magnesium (<i>b</i>)	6·83
Comet, second, very brilliant band..	7·07
Hydrogen (F)	7·94
Comet, last, rosy line	8·52
Line G	10·87

These measurements are as exact as the feebleness of the comet's light permits, which is scarcely that of a star of the seventh magnitude.

These observations lead us to interesting results. It seems at

first that we are justified in concluding the light to be formed solely of reflected solar light ; now that which comes from the sun would perhaps only constitute the diffused basis of the field of vision. The comet would then have a light of its own, the tint of which very much resembles that of the nebulae, but its position greatly differs from that of the nebular lines, one of which coincides with *f*. The other line of the comet is also different in position, and is on the further side, nearer magnesium than the nebular ray. It is thus proved that the displacement does not result from motion, but is due to the nature of the cometary matter. As these bands are more luminous than those of a star of equal magnitude, we are led to the assumption of a special light emanating from the comet. The measurements are not close enough to justify our comparing these spectra with those of other known spectra ; and we now know, moreover, that this comparison would be illusory, for the visibility of the spectra in gases depends on numerous circumstances which we cannot determine.

In the second place, this spectrum strongly resembles that which other astronomers besides myself have observed in other comets. We may thus generalize these results by induction ; and that would furnish an argument for the extraplanetary origin of these bodies.

Taking Venus near its greatest digression, and analyzing it with a spectroscope with angular vision with two prisms, I observed that the light indicated traces of atmospheric action at a height above the horizon too great for the influence of the terrestrial atmosphere to be perceptible. This must then be referred to the atmosphere of Venus itself. It is well seen in the region of Brewster, named δ , and in the other region in front of the sodium. These bands are due to aqueous vapour. The lines of nitrogen of the air near F are also greatly strengthened, which proves the presence of an atmosphere about this planet analogous to our own.

These observations have been confirmed on the simple spectroscope made with Merz's and with Hofmann's prisms, using eyepieces formed with cylindrical lenses, which were sent me by M. Merz, and which possess a high degree of perfection.

Now that with such powerful means we can penetrate into the constitution of the stars, I have put the question to myself whether the diversity of the spectra in certain types of stars does not rather arise from differences in the proportions of substances which constitute them, than from an absolute diversity in the substances themselves. I have commenced this investigation in detail by comparing stars of the second type (yellow stars like our sun) and red stars of the third type.

Comparing the two stars α Orionis and Arcturus, a surprising identity is found between the principal lines and those of our sun. More than thirty lines were found to be identical with those of Fraunhofer's map, which by its size is better fitted for these comparisons than Kirchhoff's large map. The fundamental difference appeared to consist in the strongest lines being more intense in

α Orionis than in Arcturus; and all are less so than in the sun. It might, moreover, be asked whether even in our sun the intensity of these lines is constant; and this we are led to doubt when we compare Fraunhofer's figure with that of Van der Willigen, in which fainter lines are met with than those marked by the first observer. The group which stands out the best is that of iron; α Orionis has, moreover, systems of gaseous absorption analogous to those of our atmosphere. But this kind of investigation is only just sketched out, and much time is required to arrive at definite results.

Unfortunately this investigation cannot be accelerated, owing to the small number of nights without any moon, and suitable for it in other respects, and to the danger of fatiguing the sight too much. A phenomenon is in fact frequently met with which shows how an observer must be somewhat on his guard. When, after having kept the eye fixed on a star for some time, we look into the finder of the large telescope for several seconds, a perfectly dark band of the same size as the spectrum is seen across the field itself. The eye is thus paralyzed by the decomposed light far more than by a white light of equal intensity. Inversely, on putting the eye to the telescope, for the first few moments scarcely any of the numerous details are observed, which begin to come out little by little until complete definition is attained. These phenomena show that the eye must be spared, and that without care it might be injured in these researches.

But at the same time we see what an intensity the system of cylindrical ocular lenses can retain. Some time ago it could scarcely have been suspected that the eye could be fatigued by the light of the spectrum of a star.—*Comptes Rendus*, May 11, 1868.

ON GILDING GLASS FOR THE PREPARATION OF OPTICAL MIRRORS. BY W. WERNICKE.

In the last ten years various methods of silvering glass for the preparation of optical mirrors have been described, which have been successfully applied in practice. In order also to enfeeble the action of the sun's rays in solar observations, it is usual now to coat the object-glasses of telescopes with thin layers of silver—a method first proposed by Foucault, and now introduced into several observatories. The nature of silver is opposed to a continued use of the metallic mirror; for when exposed to the air it loses its lustre after no great length of time, owing to the foreign gases present in the air.

Hence it has been frequently proposed to replace silver by gold for the above purposes; but the methods published have turned out to be of little use when tried. In vol. cxi. of Liebig's *Annalen*, Liebig has described a method based on the reduction of a specially prepared alkaline solution of gold by a mixture of alcohol and ether. The success of this method depends, however, on points not fully understood; and it is therefore inapplicable on a large scale.

Engaged on optical experiments, I have recently found a method which never fails and can always be easily and conveniently executed. For the preparation of a brilliant and firmly adhering layer of gold three solutions are prepared, which are kept separate, and when used need only be mixed in definite proportions :—

1. A solution of chloride of gold in water containing 1 grm. gold in 120 cubic centims. The gold is dissolved in the smallest quantity of aqua regia, the excess of acid evaporated on the sand-bath, and the solution diluted 120 cubic centims. It is not necessary to heat the hydrochloric acid solution until protochloride is formed; for the presence of a small trace of free acid is unimportant in the formation of a good mirror. The solution of gold must, however, be absolutely free from such metals as are deposited in the metallic form by the reducing-liquid, and more especially from silver. If the chloride of gold contains traces of chloride of silver, most of the gold is deposited in the metallic form, and the thin badly coloured mirror is soon detached from the glass.

2. Soda-lye of the specific gravity 1.06. This need not be pure; in my experiments I have used commercial soda (containing chlorine and sulphuric acid) made caustic by ordinary lime, with the same success as chemically pure caustic soda.

3. The reducing-liquid. 50 grms. of sulphuric acid are mixed with 40 grms. of alcohol and 35 grms. of water; and after the addition of 50 grms. of finely powdered manganese, the mixture is distilled at a gentle heat on the sand-bath, and the vapours passed into a flask containing 50 grms. of cold water. The distillation is continued until the volume of the water is doubled; the liquid obtained, which contains aldehyde along with some formic and acetic ether, is mixed with 100 cubic centims. of alcohol and 10 grms. of sugar converted by means of nitric acid. The change of the sugar is effected by dissolving 10 grms. of ordinary cane-sugar in 70 cubic centims. of water, adding 0.5 grm. of nitric acid of the specific gravity 1.34, and boiling for a quarter of an hour. This reducing-liquid, preserved in well-corked bottles, may be used for several months with the same success.

In order to prepare a plane or a concave mirror, one part of the soda-lye is mixed in a suitable vessel with four times its volume of the solution of gold, and a quantity of reducing-liquid amounting to $\frac{3}{5}$ or at most $\frac{1}{3}$ of the whole is added. The mixture is rapidly coloured green, owing to the liquid separated; it is immediately brought in contact with the surface to be gilded, and in such a manner that the gold can deposit from the top downwards. The rapidity of the gilding depends on the temperature. With a mean temperature of 18° C. the mirror begins to form in 30 minutes, in 1½ hour it is transparent with a splendid green colour, and in 2½ to 3 hours it is so thick that it is only translucent with a dark-green light. At a temperature of 50° to 60° C. the same effect takes place in 20 to 25 minutes, and at a temperature of 75° even more rapidly; it is not desirable to use a higher temperature, for the gold appears then to adhere less firmly to the glass. Within the limits mentioned,

the excellence of the mirror is the same as regards lustre and durability; but it may sometimes happen that at a high temperature the air contained in the water ascends in small bubbles and thus causes fine holes in the mirror, which are not visible in reflected, but are so in transmitted light; for this reason, when a high temperature is used, it is convenient to raise nearly to boiling the alkaline solution of gold before the addition of the reducing-liquid, and before bringing it in connexion with the glass to be gilded.

The mirror, when ready, is carefully washed with water, leaned against the wall with the bottom resting on bibulous paper and the coated side underneath, and dried in the air at the ordinary temperature; it exhibits then a perfect polish.

In preparing the glasses for gilding, it is in most cases sufficient to clean the surface with soda-lye and alcohol. Care must be taken not to use an acid for cleaning; the layer of gold in this case subsequently becomes detached from the glass.—Poggendorff's *Annalen*, January 1868.

ON SOME SPECIAL ELECTROMAGNETIC PHENOMENA, AND ON WEBER'S HYPOTHESIS ON ELECTROMAGNETISM. BY E. VILLARI.

The author has investigated the influence which the initial magnetic state of a bar of iron or of steel exerts on induced magneto-electrical currents excited in an adjacent conductor by any modification whatever, be it increase or diminution, in the magnetic condition of the bar. His object was to verify Weber's theory, which was also proposed by De la Rive, and which is only a complement of that of Ampère, according to which, molecular currents preexist in magnetic bodies, and are merely directed by the action of magnetization, carrying along with them in this motion the particles of the body. He thinks that if there is molecular motion, the rapidity with which it is effected must depend on the initial magnetic condition, like the magneto-electrical currents which result from it, and which themselves depend on the rapidity of the magnetic modification.

The apparatus he used consisted of a bar of steel or iron, round which were coiled two conducting wires; through the outside one a first current passed, which magnetized the bar, and then other weaker ones, direct or inverse, which more or less modified the original magnetism; in the other, the inside, which was connected with a galvanometer, the breaking of the outside coil and the alteration in the magnetism produced induced currents, which were to be measured.

The author himself gives the following summary of his researches, which, in his view, fully confirm Weber's theory:—

- (1) Magnetization and demagnetization are accompanied by molecular movements.
- (2) The rapidity with which these molecular motions are effected depends on the initial molecular condition of the iron or the steel.

(3) The magneto-electrical current is not always proportional to the increase or diminution of the magnetic moment of the magnet; it depends not only on this quantity, but also on the rapidity with which the magnetic modification is effected.

(4) This modification is not always proportional to the intensity of the current which produces it, even taking into account the magnetic capacity of the steel or iron bar in which it is produced.—Poggendorff's *Annalen*, vol. cxxxiii. p. 233; *Bibliothèque Universelle*, May 15, 1868.

APPLICATION OF THE VIBRATIONS OF COMPOUND BARS TO DETERMINING THE VELOCITY OF SOUND. BY J. STEFAN.

The method introduced by Chladni of determining, from the longitudinal tones of bars, the velocity of sound in these bars, is not applicable to those bodies which have not the form of long bars, or cannot by friction be made to sound. For such cases the new process serves.

The body to be investigated is constructed in the shape of a small bar, and fastened on a longer bar of wood or glass, which can itself be readily made to sound. This compound bar may now be made to sound by friction, and thus the number of vibrations of the fundamental note or of a higher tone be determined. Knowing, then, the velocity for the longer bar, that for the smaller one may be deduced by a formula which, it is true, is somewhat complicated.

By this method the following observations, among others, were made:—

The velocity of sound in wax = 730 metres, and is thus a little more than twice as great as in air. This number refers to the temperature 20° C.; with increase in temperature the velocity of sound decreases so much that a decrease of 40 metres corresponds to an increase of 1°, and at 30° the velocity of sound in wax and in air are the same.

The velocity of sound in grease is at 20° C. only half as great as in wax, and with an increase in temperature diminishes somewhat more quickly than in wax.

For the velocity of sound in caoutchouc, values were found which fall between 30 and 60 metres; the softer the caoutchouc, the less the velocity of sound.

These results recall those of Helmholtz on the velocity of the propagation of nervous excitation, which are within the same limits as the velocity of sound in soft caoutchouc, and suggest the thought that the velocity of nervous excitation coincides with that of sound, and that nervous excitation can be propagated in longitudinal waves.—*Berichte der Kaiserlichen Akademie in Wien*, April 30, 1868.

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April 1863.

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[FOURTH SERIES.]

AUGUST 1868.

XIV. *Experimental Researches in Magnetism and Electricity.—*
*Second Series. By H. WILDE, Esq.**

[With a Plate.]

§ 3. *On the Electric Condition of the Terrestrial Globe, and on the Absolute Character of the Law of Definite Electrolysis.*

86. **I**N a former paper, which I had the honour of communicating to the Royal Society†, reference was made to a certain class of phenomena in which, when presented to the mind for the first time with the view of predicting the magnitude of any effect which would be produced in a body by the addition of proportionate increments of a given force, the results exhibited by experiment are altogether different in magnitude from those which ordinary analogical reasoning would have led to, and consequently produce in the mind the idea of paradox.

87. In that paper several prominent instances of this relation between the quantitative properties of things and the mind of the observer, in new and untried conditions, are also given; and among them perhaps the most interesting are those shown in Tables I. II. III. IV., wherein equal increments of magnetism induce quantities of this same force much greater than those manifested in the original magnet. And, further, a quantity of magnetism or of electricity indefinitely small is demonstrated to be capable of inducing, or making manifest, quantities of these same forces indefinitely great.

88. Another class of phenomena, no less interesting than the one above mentioned, is that in which, by the continued addition of

* Communicated by the Author.

† Philosophical Transactions, 1867, p. 89. Philosophical Magazine, August 1867.

Phil. Mag. S. 4. Vol. 36. No. 241. Aug. 1868.

equal increments of a given force to a body, the effects produced are entirely the reverse of those which, without experiment, would have been predicted; and it is only by the employment of a peculiar order of analogies, drawn from a wide range of physical phenomena, that the appearance of these effects in any new condition of things can even be suspected. A remarkable instance of the principle of inverted action here alluded to is that in which a thin plate or disk of metal, when presented to a jet of air escaping under strong pressure from an orifice in a plane surface, is attracted to, instead of being repelled from, the flat surface from which the air issues*. The continued addition of equal increments of heat to a body likewise furnishes many examples of these inversions; but it will be sufficient to mention the spheroidal condition of water in relation to evaporation to indicate the bearing which these remarkable facts have upon the phenomena to be hereafter described.

89. A consideration of many such examples of inverted action as those above referred to led me to suspect that similar actions might exhibit themselves in those departments of electrical science which have hitherto only been explored with apparatus of a limited range of power. In this expectation I have not been disappointed; for, with the powerful electromotors which I have had the good fortune to command, I have not only succeeded in clearing up several points of doubtful knowledge respecting the electric condition of the earth, and of electrolytic action generally, but I have also discovered some new electrochemical facts in connexion with the electrolyzation of water which must ultimately lead to an important modification of our present ideas on the nature of that substance.

90. The experiments which I now purpose to give an account of were made in the course of an inquiry into the possibility of transmitting electric signals through metallic cables submerged without an insulating envelope. This inquiry involved, as a preliminary investigation, a consideration of the function which the earth performs as a conductor of electricity when employed for telegraphic purposes; but before giving an account of this investigation, I will briefly state the opinions respecting the electric condition of the earth as already put forth.

91. At the time when voltaic electricity was first employed for transmitting intelligence to distant places, it was generally believed that a complete metallic circuit, involving the use of two line wires, was absolutely necessary for that purpose, until Steinheil, in 1837, made the discovery that one of these wires might be dispensed with, provided each extremity of the other

* Manchester Memoirs, new series, vol. v. p. 208. Philosophical Magazine, S. 2. vol. v. p. 250.

wire was terminated with a metal plate inserted in the moist ground. This physicist also pointed out that the resistance offered by the earth to the propagation of the current along the wire might be reduced to such an extent as to be quite insensible when compared with that of the double metallic circuit; so that not only was one-half of the wire economized, but even the resistance that such a mixed circuit of earth and wire presented was diminished by one-half.

92. The explanation of this singular and important fact, as given by Steinheil, was that the conducting-power of the earth, though very inferior when compared with that of the metals, yet its bad conductivity was more than compensated for by the immensity of its section. This interpretation of the earth's action naturally led to the opinion that the electric current, after traversing the whole length of the telegraph-wire, returned through the earth back to its origin at the battery, in the same manner as if the chain of earthy particles extending between the distant electrodes had been confined in an insulated tube of large dimensions. The difficulties, however, which encumbered this hypothesis when the distance between the electrodes became considerable, have led many physicists to abandon it as being inconsistent with certain well-ascertained facts.

93. The present view of the earth's action in relation to telegraphic circuits, as supported by Gauss*, De la Rive†, Walker‡, and many others, is, not that the earth conducts the electricity from the distant end of the wire back to its origin at the battery, but that the earth acts as a vast reservoir of neutral electricity, into which the positive and negative currents from each of the insulated wires are absorbed, in the same manner as a discharge of static electricity is received into the ground.

94. In the course of an elaborate series of experiments on the propagation of electric currents in the earth, M. Matteucci made the curious observation that, when the distance between the electrodes at each extremity of the insulated wire was very small, the resistance of the terrestrial bed increased, up to a certain point, in proportion to its length, according to the recognized law for ordinary conductors; but when the distance between the electrodes exceeded thirty yards, the resistance of the earth gradually diminished until at a distance of a few miles it finally became null§.

95. The only explanation which I have as yet met with of this singular limitation of the earth's resistance is that of De la Rive,

* *Traité de Télégraphie Électrique*, by M. l'Abbé Moigno: Paris, 1852, chap. 3.

† Treatise on Electricity, by A. De la Rive, vol. iii. p. 446.

‡ C. V. Walker's 'Electric Telegraph Manipulation,' p. 37.

§ *Annales de Chimie et de Physique*, 1851, vol. xxxii. p. 248.

who thinks that the anomalous results obtained by Matteucci may be accounted for by supposing that the molecules interposed between the two electrodes, not being so numerous when the latter are near together, may constitute the electric chain by the mutual neutralization of their opposite electricities, which is preceded by their polarization; but when the electrodes are very distant from each other this communication can no longer take place, and they are then discharged, by means of the bed with which they are in contact, into the entire mass of the terrestrial globe*. From this explanation it will be seen that De la Rive, in common with Matteucci, holds the opinion that a diminution of the distance between the electrodes in contact with the earth is attended with an increase of resistance to the passage of the current from the electromotor.

96. This brief historical sketch of the present state of knowledge on the function which the earth performs as a conductor of electricity, as made known by those physicists who have preceded me in this research, will, I think, lead to a full comprehension of my own experiments in the same direction, and of the new phenomena which form the subject of this paper.

97. The principal electromotors which I employed were the machines described in the First Series of these researches, and consisted (1) of a $2\frac{1}{2}$ -inch magneto-electric machine (58) worked by hand. This machine would melt four inches of iron wire $\cdot 025$ of an inch in diameter in short circuit, *i. e.* when the iron wire was directly interposed between the polar terminals of the machine.

98. (2) A 5-inch electromagnetic machine excited by a $2\frac{1}{2}$ -inch magneto-electric machine (63).—This machine was furnished with two armatures, one for the production of "quantity," and the other for the production of "intensity" effects. The quantity-armature would melt fifteen inches of iron wire $\cdot 075$ of an inch in diameter, and the intensity-armature thirty-six inches of iron wire $\cdot 035$ of an inch in diameter, in short circuit.

99. (3) A 10-inch electromagnetic machine (66) excited by a $3\frac{1}{2}$ -inch magneto-electric machine.—This machine was provided with the quantity- and intensity-armatures before described (70), (71), (72). The quantity-armature of this machine, when excited by the $3\frac{1}{2}$ -inch machine, would melt about fourteen inches of iron rod $\frac{1}{4}$ of an inch in diameter, and the intensity-armature seven feet of iron wire $\frac{1}{16}$ of an inch in diameter.

100. (4) A well-excited Grove's cell, the platinum of which was $6\frac{1}{2} \times 3$ inches.—This electromotor would melt (making allowance for the heat abstracted by the polar conductors) seven-eighths of an inch of iron wire $\cdot 035$ of an inch in diameter. Two, three,

* Treatise on Electricity, by Aug. De la Rive, vol. iii. note to p. 448.

and four of the same cells, in series, would melt two, three, and four times the length of wire of the same diameter that one cell would melt. Two, three, and four of the same cells, arranged for quantity, would melt a 2-, 3-, and 4-fold strand of the same wire and of the same length that one cell would melt.

101. I have chosen to adopt the melting of a given quantity of wire as the measure of the magnitude of the current from the electromotors above mentioned, because I have not yet found any other method which can at all compare with it in expressing easily and without ambiguity the associated properties of quantity and intensity, especially when dealing with currents of such magnitude as those evolved from the 5- and 10-inch machines. By comparing the absolute quantity of wire melted by a standard Grove's cell with that melted by the machines, under the different conditions to which attention will be directed, a tolerably good idea of the magnitude of the forces brought into operation may be obtained.

102. The other electromotors employed in these experiments were:—(5) a weakly charged battery of thirty-three Daniell's cells, such as are commonly used by the Electric Telegraph Company for signalling-purposes. This battery was arranged in three troughs of eleven cells each. (6) A large well-excited Daniell's cell; and, lastly, a single pair of zinc and platinum plates excited by dilute sulphuric acid, spec. grav. 1.110.

103. The building which contained the various electromotors is situated about 100 feet from the edge of the basin of a navigable canal, in which most of the following experiments on the electric condition of the earth have been made. Connexion between the various electromotors and the electrodes and other conductors submerged in the canal was made by means of two lengths of copper-wire rope, each 140 feet in length and nearly half an inch in diameter. These wire-rope connexions consisted of seven thick copper wires twisted together, and were supported through the air and insulated from one another by means of a pole fixed upon the bank of the canal.

104. The conductors to be submerged, which also served the purpose of electrodes, consisted of two lengths of naked copper-wire rope, each 169 feet long, and of exactly the same diameter and construction as those used for the aerial connexions (103).

105. Two other lengths of copper rope, of the same length and diameter as the preceding (104), were each carefully wrapped with a double covering of linen tape, in the manner adopted for insulating the coils of the large electromagnet of the 10-inch machine (68). These two lengths of tape-covered copper rope were laid parallel with each other and then bound tightly together with string wound round them in a spiral direction, so

that though the conductors were in close proximity to each other, yet no metallic contact between them was established. The integrity of the insulation of these double or twin conductors (as I propose to call them) was examined in air with the powerful current from the 10-inch intensity-armature, and was found to be perfect. These double or twin conductors are represented in fig. 11, Plate I., with the ends A, B and C, D carefully separated.

106. Owing to the great size and good conducting-capacity of the aerial connexions, and of the naked and the tape-covered conductors to be immersed in the canal, the whole of the 618 ($2 \times 140 + 2 \times 169 = 618$) feet of copper rope, when formed into a complete metallic circuit in air, did not greatly diminish the power of the current from any of the electromotors, except that from the 10-inch quantity-armature, which current, however, after passing through the 618 feet of conductor spread out on a dry boarded floor, was still sufficiently powerful to melt twelve inches of iron wire .095 of an inch in diameter when introduced into the circuit; while the currents from the 5- and 10-inch intensity-armatures, after passing through the same length of conductor, would melt nearly the same quantity of wire as they would in short circuit; and the currents from the $2\frac{1}{2}$ -inch machine and the single Grove's cell, after passing through the 618 feet of wire rope, would still melt, the former 4 inches, and the latter $\frac{3}{4}$ of an inch of iron wire .016 of an inch in diameter.

107. As this size of iron wire (.016 of an inch in diameter) will be frequently referred to hereafter, it will be more convenient to speak of it as the thin wire.

108. The two naked copper ropes (104) were each coiled into a ring about 2 feet in diameter, with their several convolutions in close metallic contact. The rings were then placed in metallic connexion with the respective ends of the aerial conductors and immersed in the canal, with the planes of the rings opposite to each other, about three feet apart, and perpendicular to the surface of the water.

109. The disposition of the aerial conductors and electrodes in relation to each other and to the electromotors will be better understood on reference to Plate I. fig. 12, in which E represents the electromotor, F, G its polar terminals, H, I the aerial conductors for establishing connexion between the electromotor and the ring-electrodes K, L immersed in the canal M. The respective ends of the naked copper ropes will be indicated, when necessary, by the same letters (A, B and C, D) as those of the twin conductors, fig. 11.

110. A tangent galvanometer, or pieces of iron wire of different lengths and diameters, could be introduced at pleasure at the point N, between the pole G and the conductor H, for the purpose of indicating, by galvanometric deflection, or by the heat

produced in the iron wires, the passage of a current through the conductors into the earth.

111. The current from the battery of thirty-three Daniell's cells in series was first transmitted through the tangent galvanometer in short circuit, when the needle stood permanently at 10° .

112. When the same battery-current was transmitted into the canal, through the conductors and electrodes arranged as in fig. 12, the galvanometer introduced at N still indicated 10° of current, thereby showing a *null resistance* on the part of the water in the canal to the passage of the whole amount of current which the battery was capable of producing.

113. But on attempting to transmit the powerful alternating currents from either the quantity- or the intensity-armatures of the 5- and 10-inch machines into the canal through the same arrangement of conductors and electrodes as in the preceding experiment, the amount of current actually transmitted was not sufficient to make three inches of the thin iron wire (107) introduced at N sensibly warm to the fingers, the result now indicating an almost *total resistance* to the passage of the current which the electromagnetic machines were capable of producing.

114. On uncoiling the two rings of copper rope and extending them in straight lines, about twenty feet apart, along the bottom of the canal towards the points O, P, with the distant ends C, D of the conductors entirely disconnected, the current from the 5-inch intensity-armature made two feet of iron wire .035 of an inch in diameter bright red-hot at N, and the current from the 10-inch intensity-armature melted four feet of wire .050 of an inch in diameter, the result now indicating a great diminution of the apparent resistance of the earth to the passage of a powerful electric current.

115. But on attempting to transmit the current from the 5- and 10-inch quantity-armatures through the conductors extended in the canal, the amount of current now transmitted was only sufficient to make fifteen inches of the thinner wire used in the preceding experiment (114) sensibly warm to the fingers, the result again exhibiting an increase of the earth's resistance similar to that observed when the conductors were coiled up into rings.

116. Similar effects of resistance to the passage of the current into the earth, as indicated by the non-heating of the thin wire at N, were also obtained when the $2\frac{1}{2}$ -inch machine, or the single Grove's cell, was connected with the submerged conductors in their extended form.

117. That this resistance to the passage of the current from the 5- and 10-inch quantity-armatures and that from the single Grove's cell was not due to the resistance of the copper conduc-

tors themselves (106), or to any fault in the connexions, was at once made evident by joining together the distant ends C, D of the extended conductors, when the iron wires introduced at N were immediately melted.

118. In order that a discharge of electricity through the extended conductors K, L might be obtained, it was essential that the connexion between the ends A, B and the aerial connexions H, I, although immersed in the water, should be entirely metallic; for when one of these ends, A, was separated from the other end, I, by only the thickness of a piece of tape, no heating effects sensible to the touch were produced in the thin iron wire at N by the powerful current of the 10-inch intensity-armature, even when the distant extremities C, D of the conductors were joined together.

119. Though the current from a single Grove's or Daniell's cell was not powerful enough to heat sensibly any thin wire at N when either of these cells was placed in connexion with the extended conductors with their ends C, D disconnected, yet an amount of current was always found passing through the conductors sufficient to produce a deflection of the galvanometer at N. Moreover, when contact between the Grove's or Daniell's cell (or one formed of zinc and platinum, excited by dilute sulphuric acid) and the extended conductors was maintained for a few moments and then broken, the galvanometer always indicated a counter current from the extended conductors about half the strength of that previously passing from the electromotor.

120. That none of the effects of conduction and resistance of the terrestrial bed, as manifested by the heating or non-heating of the wires at N, were brought about by the exercise of any mutual influence of the conductors upon each other through the intervening body of water, or, again, by any change in the electric condition of the earth taking place by reason of a change in the linear distance of the electrodes from each other, was proved by the following experiments (122, 123) (137-139).

121. One of the aerial copper-rope conductors, H, was extended upon insulated supports along the side of the canal to a point 300 feet beyond where the other aerial conductor, I, entered the canal, so that the electrodes or conductors in the canal might now be 300 feet instead of a few feet from each other. In consequence of the great thickness and good conducting-capacity of the aerial connexions, they offered, practically, no resistance to the passage of the currents from any of the electromotors used in the experiments.

122. On repeating, in the same order, all the preceding experiments with the submerged conductors, both in their coiled and extended form, results were obtained from the electromotors

identical in every respect with those observed when the electrodes were only a few feet apart. Thus a series of thirty-three Daniell's cells indicated 10° of current on the galvanometer at N, whether the distance between the ring-electrodes was 3 feet or 300 feet; while with the quantity- and intensity-currents from the 5- and 10-inch electromagnetic machines the opposite effects of resistance, as manifested by the non-heating of thin wire at N, were obtained, whether the ring-electrodes were at the greater or at the less distance (113).

123. Again, when the conductors were uncoiled and extended in the canal 300 feet from each other, the currents from the 5- and 10-inch intensity-armatures melted and made red-hot the same lengths and thicknesses of wires at N as when the conductors were extended in the canal 9 feet apart; while, on the other hand, the currents from the 5- and 10-inch quantity-armatures encountered the same resistance, as manifested by the non-heating of the wires at N, at the greater as at the less distance (115).

124. The following Table contains the results of a carefully made series of experiments with different numbers of similar Daniell's cells, when the ring-electrodes were immersed in the canal at distances of 3 and 300 feet from each other.

TABLE V.

No. of Daniell's cells in series.	Deviation of galvanometer with battery in short circuit.	Deviation of galvanometer when distance between electrodes = 3 feet.	Deviation of galvanometer when distance between electrode = 300 feet.
1	9.5	3.75	3.75
2	10.0	5.75	5.75
5	10.0	7.50	7.50
11	10.0	8.75	8.75
22	10.0	9.75	9.75
33	10.0	10.0	10.0

125. The results contained in columns 3 and 4 of the above Table prove conclusively that no change whatever occurs in the electric condition of the earth in consequence of a mere change in the distance of the electrodes from each other. It will also be evident that variations in the intensity of the battery-current are sufficient to produce corresponding changes in the resistance of the earth similar to those produced by the quantity- and intensity-currents from the electromagnetic machines when the conductors were extended in the canal (114, 115).

126. Returning to a consideration of the effects produced by the alternating current from the 5-inch intensity-armature when the ring-electrodes were connected with the aerial conductors of

equal length (113):—Although, when the electrodes were three feet apart, a sufficient amount of current was always passing into the canal to produce a spark and shock on breaking contact at N, yet it was insufficient, as we have seen, to heat sensibly wire so thin as $\cdot 016$ of an inch in diameter. When, however, the electrodes were made to approach so as to be within one inch of touching each other, a length of three inches of the above-mentioned wire was made red-hot.

127. In order that this mutual influence which the electrodes exercised on each other, to promote an increase of discharge from the electromotors, might be more closely investigated when the distance between the electrodes was still further reduced, the tape-covered twin conductors were submerged in the canal in their extended form, as shown in fig. 11, with the ends A B connected with the respective ends of the aerial conductors, and the distant ends C, D entirely disconnected. From the porous nature of the tape-covering with which the conductors were enveloped, it will be obvious that this covering, though a good insulator in air, would, when saturated with water, offer little or no obstruction to the passage of an electric current either into the earth or between the conductors.

128. When the current from the 5-inch intensity-armature was transmitted through the submerged twin conductors, 2 feet of the same-sized wire which was made red-hot at N when the naked conductors were extended in the canal (114) was now melted. And the current from the 5-inch quantity-armature, which previously only made 15 inches of wire $\cdot 021$ of an inch in diameter warm to the touch, now melted this same wire, and also made red-hot 15 inches of thicker wire, $\cdot 050$ of an inch in diameter.

129. On coiling the twin conductors into a double ring about 2 feet in diameter and immersing it in the canal in the place previously occupied by the separate ring-electrodes of naked copper rope, the 15 inches of thicker wire ($\cdot 050$ of an inch in diameter), which was only made red-hot by the 5-inch quantity-armature in the last experiment, was now melted, thereby indicating the passage of a more powerful current from the electromotor when the twin conductors were coiled into a ring than when they were extended along the bed of the canal. With this arrangement of the double coil, the current from the 10-inch intensity-armature melted 6 feet of wire $\cdot 065$ of an inch in diameter.

130. That the increased quantity of current passing through the twin conductors, above that transmitted through the naked conductors, whether in their coiled or extended form, was due to the direct influence which the twin conductors exercised on

each other, and is an effect entirely distinct from that which the earth produces when receiving into itself a discharge of electricity, was at once made evident by lifting the saturated double coil out of the canal and suspending it in the air, when the current from the 5-inch quantity-armature was still sufficiently powerful to make bright red-hot, at N, 15 inches of the same wire which was only made warm to the touch when the naked conductors were extended separately in the canal (115).

131. That the resistance which the naked ring-electrodes presented to the passage of the current from the 5-inch and 10-inch intensity-armatures did not arise from the circumstance of their convolutions being in close metallic contact with each other, was proved by the fact that the same resistance to the passage of the current from these armatures (as shown by the non-heating of thin wire at N) was experienced when the tape-covered conductors were coiled up into two separate rings and immersed in the canal at the same distance from each other as were the naked ring-electrodes.

132. Suspecting that the resistance which the two tape-covered coils when separated offered to the transmission of the current into the earth was due to an inductive action set up among the several convolutions of each coil and operating to produce a repulsion of the current on itself, the following experiments were made for the purpose of testing the accuracy of this view.

133. Each length of the tape-covered conductors was, in the first place, uncoiled, and extended in a straight line along the bottom of the canal, with a lateral distance between them of about 20 feet. On transmitting the current from the 5-inch intensity-armature through the conductors, a length of 2 feet of iron wire .035 of an inch in diameter was made bright red-hot at N, showing the passage of the same amount of current as when the naked conductors were extended in the canal under like circumstances.

134. Each of the conductors was then bent backwards and forwards along the bottom of the canal in three lengths of 56 feet, so that the conductors were each of them in the form of an elongated letter N. On transmitting the current from the same armature through this arrangement of conductors, the 2 feet of wire at N was made somewhat less bright red than in the previous experiment.

135. Again, on binding the three lengths of each of the conductors together with string, and immersing each of the conductors thus folded in the canal, as shown in fig. 13, the 2 feet of wire at N, though hotter than could be endured by the touch, was not made visibly red-hot.

136. From a consideration of the opposite effects of resistance and conduction exhibited when the tape-covered conductors were arranged separately in the canal in the several ways described (131, 133, 134, 135), it is evident that the transmission of the current from the electromotor is preceded by an inductive action upon the liquid particles in contact with the conductors, which action, according as it is adverse or favourable, retards or accelerates the discharge of the current into the earth. The increased quantity of electricity discharged into the earth from the three-fold conductors, above that from the same conductors when coiled into separate rings, is not surprising, as we have already seen that in the case of the coiled twin conductors the inductive action of a number of convolutions in contact with each other on every side was much more perfect (as shown by the melting of thick wires at N) than when these same twin conductors were extended in the canal (128, 129).

137. To recapitulate what has already been advanced in proof of the action of the electrodes being independent of any mutual influence which they might be supposed to exercise on each other through the intervening body of water when transmitting a current of electricity into the earth:—We have seen that the same effects of conduction and resistance were produced, and equal amounts of current were transmitted into the earth from the same electrodes, whether the distance between them was 3 feet or 300 feet—a result altogether incompatible with the well-ascertained laws which have been determined in connexion with the transmission of electric currents through liquids contained in insulated vessels.

138. That the distance between the electrodes does, under certain circumstances, exercise an influence on the quantity of electricity transmitted from the electromotors was evident from the heating and fusing of wires at N when the conductors were brought into close proximity to each other by the contrivance of the twin conductors. But that this influence operates within limits extremely small was also evident from the fact that, when the ring-electrodes were only 3 feet apart, none of the electromotors would heat the thin wire at N. Moreover, that the mutual influence of the electrodes upon each other when in close proximity is an action entirely distinct and separable from the influence which these electrodes exercise upon the earth is shown by the heating of wires at N when the saturated coil of twin conductors was lifted out of the canal and suspended in the air (130).

139. This distinction between the two inductions becomes still more obvious from the fact that, when the twin conductors were extended in the canal, the inductive influence of the con-

ductors upon each other acted in *conjunction* with the normal inductive action of the earth to promote *an increase* of discharge from the electromotors above that observed when the naked or the tape-covered conductors were extended separately in the canal (114, 115, 128). On the other hand, when the tape-covered threefold conductors were extended separately in the canal (fig. 13), the inductive influence of each conductor upon itself acted *in opposition* to the normal action of the earth, so as *to diminish* the amount of discharge from the electromotor below what was observed when the same conductors were extended in straight lines, or arranged in the form of the letter N.

140. Reverting to the fact that when contact was made between the 5- and 10-inch intensity-armatures and the conductors extended separately in the canal, the heating and melting of wires at N indicated the passage of a powerful current into the earth (114, 123):—A question of great importance now arose as to whether this current was transmitted in consequence of a physical conductivity of the terrestrial bed, or by the electrolyzation of the liquid in contact with the conductors. Hitherto electricians have been content to affirm that the earth absorbs or drinks up the electricity transmitted from the electromotors. Matteucci had indeed found that when the electrodes were plunged into the soil impregnated with sulphate of copper or sea-water, the quantity of electricity discharged from the electromotor was increased; but, beyond this fact, no experiments have yet been recorded with the view of establishing the absolute dependence of the transmission of the current into the earth upon the electrochemical properties of the liquids incorporated with it.

141. One important step towards the establishment of this dependence was the diminution of the earth's resistance observed when the tension of the current transmitted along the conductors was increased (114, 115)—a result entirely in accordance with what is observed in the electrolyzation of water in insulated vessels. Moreover we have also seen that though the discharge of the current into the earth from the extended naked conductors was distinct from, as well as less in amount than, the discharge between the twin conductors extended in the same liquid, yet it was of the same kind and in the same direction in the one case as in the other. The difference, then, between the discharge of the conductors into the earth and into each other being one of degree only, it seemed to me that some advantage might be derived from the study of the action of the naked and tape-covered conductors upon each other when coiled and immersed

in insulated vessels, for the purpose of comparing the results with those obtained when the same conductors were immersed in liquids in contact with the earth.

142. A large wooden tub, partially filled with the same canal-water as that in which the previous experiments were made, was insulated from the ground by means of three fireclay bricks (fig. 14); and in this tub the two ring-electrodes of naked copper rope were immersed with the planes of the rings about 3 inches apart.

143. On making contact between the 5-inch intensity-armature and the coils immersed in the tub, a resistance to the passage of the current (as manifested by the non-heating of thin wire at N) was experienced, similar to that which was observed when the same electrodes were immersed in the canal (113). On diminishing the distance between the electrodes from 3 inches to 1 inch, the 3 inches of thin wire .016 of an inch in diameter was now made red-hot, as was the case when these coils were immersed at the same distance from each other in the canal (126).

144. Similar effects of resistance to the passage of the current from the 5-inch intensity-armature to those obtained with the naked ring-electrodes were also experienced when the tape-covered ring-electrodes were immersed separately in the tub at the same distances from each other as in the preceding experiment.

145. Again, no transmission of the current sufficient to heat thin wire at N occurred when contact was made between the coiled electrodes, when 3 inches apart, and the $2\frac{1}{2}$ -inch machine, or a single Grove's cell—the results agreeing in this respect also with those obtained in the canal.

146. The tape-covered conductors were now rearranged in the tub as a double coil of twin conductors, bound together with string, with the ends C, D disconnected.

147. On transmitting the current from the 10-inch intensity-armature through the immersed coil, 5 feet of wire .065 of an inch in diameter was melted at N, while the current from the 5-inch quantity-armature made 15 inches of wire .050 of an inch in diameter bright red-hot, whereas the current from the same armatures melted respectively 6 feet and 15 inches of the same-sized wires when the double coil was immersed in the canal (129)—a result which again shows, but in a converse manner, that the mutual influence of the conductors upon one another was supplemented by the inductive influence of the earth, as in the case where the conductors were extended jointly and separately in the canal (114, 128).

148. Contact was now made between the double coil and a single Grove's cell; but although the galvanometer indicated

the passage of a tolerably strong current through the coil, yet it was not sufficiently powerful to make the thin wire red-hot at N.

149. When contact was made between the double coil and the single pair of zinc and platinum plates contained in a glass vessel (102), the passage of a current through the water in the tub was indicated by the liberation of bubbles of hydrogen from the platinum plate, as well as by the deflection of the galvanometer at N. This result was also obtained when the current from the same pair of plates was transmitted through a small double coil of twin conductors (165) immersed in a glass of distilled water.

150. When the ends A, B of the double coil in the tub, or of the coil in the glass vessel, were connected with the respective terminals of the galvanometer, after all contact with the voltaic couples had been broken, the galvanometer always indicated a strong secondary current from the coils, similar to that observed when the conductors were extended separately in the canal (119).

151. A quantity of common salt was now dissolved in the tub of fresh water, sufficient to make the solution as nearly as possible of the same specific gravity as sea-water. On repeating the experiments with the naked and tape-covered coils immersed separately in the tub with the planes of the coils 3 inches apart (142), the currents from the 5- and 10-inch intensity-armatures which, when the coils were at the same distance apart in fresh water, would not melt any thin wire at N, now melted 30 inches of iron wire .035 of an inch in diameter, and 6 feet of wire .065 of an inch in diameter respectively; and the current from the 2½-inch machine now melted 4 inches of the thin wire (107).

152. When the tape-covered conductors were again arranged as a double coil of twin conductors, so little resistance was offered to the transmission of the alternating current through the liquid, that the 5- and 10-inch intensity-armatures now melted the maximum quantity of wire at N that they were capable of melting in short circuit; and the current from a single Grove's cell would now make $\frac{3}{4}$ of an inch of the thin wire red-hot.

153. Instead of the bright flash and combustion of the copper, which accompanied the interruption of the current from the 10-inch intensity-armature on making and breaking contact between the ends C, D of the double coil while dry and suspended in air, a small spark only was visible when it was immersed in salt water, and the thin wire, when introduced between C and D, was not made red-hot.

154. On substituting dilute sulphuric acid, spec. grav. 1.110, for the salt water in the tub, and transmitting the alternating

current from the 10-inch intensity-armature through the coil, so perfect was now the communication between the conductors through the electrolyte, that not the smallest spark could be obtained on making and breaking contact between the ends C and D.

155. A phenomenon already observed, on a small scale, by De la Rive* when engaged in transmitting the alternating currents from a magneto-electric machine through dilute sulphuric acid by means of platinum electrodes, was here reproduced in a most remarkable manner. When the coils were immersed in the tub of fresh or salt water, or in dilute sulphuric acid, it was observed that not a single bubble of gas arose to the surface of the liquids, no matter for how long a time the powerful alternating currents from the 5- and 10-inch intensity-armatures were transmitted.

156. That these currents were capable of producing energetic electrochemical effects, simultaneously with the non-appearance of gas in the tub, was proved by introducing into the circuit at N a pair of large platinum plates plunged in a glass vessel containing dilute sulphuric acid, when an abundant liberation of the mixed gases from each of the plates occurred, while at the same time no gas was liberated from the coils in the tub. Hence it seemed as though the alternating current could be transmitted through the same kind of liquid, either with or without electrolyzation, by simply varying the arrangement of the electrodes.

157. When, however, the direct current from any of the armatures was transmitted through the double coil, the passage of the current was in all cases accompanied by the liberation of hydrogen from the liquid in which the coil was immersed.

158. That the absence of any appearance of electrolyzation of water in the tub, when the alternating currents of the 5- and 10-inch intensity-armatures were transmitted through the coils, was due to the recombination of the electrolytic products formed by each alternation of the current, was made evident in the following manner.

159. The double coil of twin conductors was immersed in the tub of salt water, and the ends C, D (fig. 14) were connected by means of a short piece of the thin iron wire. One of the other ends, A, was connected directly with the zinc of the single Grove's cell, while contact with the other end, B, and the platinum was established at N by means of another short piece of iron wire somewhat thicker than that which joined the ends C, D.

160. When the current from the Grove's cell was transmitted through the coil, the thin wire at C D, after a few moments, gra-

* *Comptes Rendus*, 1837, vol. iv. p. 837.

dually became red-hot, while the thicker wire at N, though hotter than could be endured by the finger, was not made visibly red during the maintenance of the battery-contact.

161. On reversing the battery-connexions, by joining the end A to the platinum and the end B to the zinc by means of the thicker wire at N, this wire immediately became bright red-hot, while the thinner wire at C D remained cold; but on continuing the battery-contact for about a minute, the thicker wire at N gradually became less bright, until it finally ceased to be visibly red, when the thin wire at C D again became red-hot; and as often as the battery-connexions were reversed, so often did the wires at the battery and distant ends of the coils become alternately, but never simultaneously red-hot.

162. Another phenomenon attending the alternate heating of the iron wires was the liberation of hydrogen from the double coil only during such time as the thin wire at C D was maintained at a red heat, but never while the thicker wire at N was heated; for as often as the battery-connexions were reversed, the liberation of hydrogen from the coil immediately ceased, and did not reappear until the thin wire at C D was reheated.

163. Moreover, on making and breaking contact between the ends A, B of the double coil after they had been maintained in connexion with the Grove's cell for a short time with the ends C, D disconnected, sparks could be obtained from the coil for more than five minutes after all connexion with the electromotor had been broken; the galvanometer, as might have been expected, always indicated a strong secondary current from the coils, in the reverse direction to that transmitted by the electromotor.

164. From a consideration of the foregoing experiment, made with the view of explaining the absence of any appearance of electrolyzation of the water in the tub by the alternating currents from the electromagnetic machines, it will be evident, from the heating of the thin wire at C D when contact was first made between the battery and the double coil, that the hydrogen evolved by the electrolyzation of the water formed an insulating envelope surrounding one of the conductors, which thereby enabled a considerable portion of the current from the electromotor to traverse the conductors instead of being transmitted through the water. But when the battery-contacts were reversed, the cessation of the stream of hydrogen from the coil, and the non-heating of the thin wire at C D, indicated the reunion of the hydrogen envelope with the liquid in contact with the conductor; while the hydrogen evolved by the new current on the other conductor did not become visible until its affinity for the oxide produced by the previous current had been satisfied. But by continuing the battery-contact, a new hydrogen

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envelope was formed, which enabled the comparatively feeble current from a single Grove's cell to exhibit, by its prolonged action, electrodynamic effects at C D which the powerful, but momentary, alternating currents from the 10-inch intensity-armature were unable to produce.

165. Proceeding from the liquids in the tub which offered the least to those which presented the greatest amount of resistance to the passage of an electric current, a small double coil of twin conductors was immersed in a large glass jar of Gallipoli oil with the ends C, D disconnected. This coil was formed of two separate lengths of cotton-covered copper wire, each 150 feet long and .050 of an inch in diameter.

166. On making and breaking contact between the ends A, B of the double coil and the polar terminals of the 10-inch intensity-armature while the coil was immersed in the jar, not the slightest spark, nor any other indication of the passage of a current through the oil, was obtained—the result being, in this respect, the same as if the ring of twin conductors had been suspended in the air.

167. That the peculiar arrangement of the double coil was eminently favourable for exhibiting the transmission of a current through a conducting liquid was evident from the fact that when the same coil was immersed in the jar filled with dilute sulphuric acid instead of oil, so powerful was the current transmitted through the liquid, that a length of 2 feet of each of the ends A, B, extending from the surface of the liquid to the terminals of the electromotor, was made red-hot. This experiment, besides exhibiting the greatest possible difference between the conducting-power of liquids under conditions eminently favourable for obtaining an electrolytic discharge, also shows with what enormous rapidity the liberation and recombination of the electrolytic products takes place; for during the whole time that the ends A, B of the copper wires were maintained at a red heat, not a single bubble of gas made its appearance in the dilute acid in which the double coil was immersed.

168. I have said that when an electromotor consisting of a single pair of zinc and platinum plates, excited by dilute sulphuric acid, was placed in connexion with a double coil of twin conductors immersed in a vessel of distilled water, the transmission of the current through the water was attended by the liberation of hydrogen from the platinum in a visible manner (149).

169. As this result has never, to my knowledge, been obtained before by a current of so low an intensity as that from a single pair of zinc and platinum plates, it seemed to me that the anomalous exception which Faraday thought he had discovered to his

own law of definite electrolysis, when experimenting with a similar pair of plates, had no real existence. Nevertheless, as exception might probably be taken to the fact that the coil was made of an oxidizable metal (though I have no doubt that a coil of platinum wires would behave in precisely the same manner), and in order that a question that has excited so much discussion amongst physicists, and is still regarded by some as an undetermined point in electrical science, might be definitely settled, I was induced to repeat Faraday's experiment as nearly as possible in the manner in which he himself conducted it*, but with certain variations which suggested themselves to me while observing the effects produced by the alternating current from the single Grove's cell upon the double coil when immersed in the tub of salt water.

170. Two glass vessels, Q and R, fig. 15, were filled with the same dilute sulphuric acid, spec. grav. 1.25. The vessel Q contained a single pair of amalgamated zinc and platinum plates, which were connected respectively, by means of thick copper wires, with two platinum plates immersed in the vessel R. On introducing a very sensitive galvanometer, or a bit of bibulous paper saturated with a solution of iodide of potassium, into the circuit of one of the connecting wires, the deflection of the needle or the evolution of iodine indicated the passage of an electric current through the liquids in both vessels; but no matter for what length of time connexion was maintained between the electromotor Q and the electrodes in R (by means of the thick wires), no liberation of gas appeared on the platinum plates in either of the vessels. Hence it was that Faraday inferred that a feeble current might be transmitted from an electromotor without the electrolyzation of the liquids in contact with the electrodes. So convinced, however, was I that the absence of any appearance of electrolyzation in this case was due to the resistance and reactions of the nascent gases enveloping the platinum plates in Q and R, that it only appeared necessary for me to alternately invert the direct current from the electromotor Q, before it arrived at the electrodes in R, in order that the same recombination of gas on the electrodes and increased discharge from the electromotor might be brought about as when the alternating current from the Grove's cell was transmitted through the coil immersed in the tub of salt water.

171. This alternation of the direction of the current was effected by means of a commutator, S, fig. 15, placed between the vessels Q and R. Care was taken that the alternating metals of the commutator were separated from each other by a sufficient thickness of insulating material to prevent the metals which re-

* Philosophical Transactions, vol. cxxiv. p. 448.

ceived the inverted current from bridging across or making a short metallic circuit between them and the plates in the vessel Q, as, otherwise, deceptive results would have been obtained.

172. When the commutator was at rest, but arranged so that the zinc and platinum plates in Q should be in metallic communication with the electrodes in R, no liberation of gas appeared on the platinum plates in either of the vessels. But on making the commutator revolve at such a velocity that the current, after leaving Q and before arriving at R, should be alternately inverted about 1000 times per minute, the whole surface of the platinum plate in Q gradually became covered with bubbles of hydrogen, which finally detached themselves from the plate and escaped through the liquid in the same manner as when the same pair of zinc and platinum plates were connected (without the intervention of a commutator) with the double coil of wire immersed in distilled water (149).

173. The proof that the passage of an electric current through the vessels Q and R was accompanied by the electrolyzation of the liquid in Q was therefore full and complete. Now Faraday has shown (*Experimental Researches*, 809, 991) that when several electrolytic cells, containing the same dilute sulphuric acid, are arranged in the same circuit, the amount of electrolytic action is absolutely equal in each cell; consequently whatever be the amount of electrolytic action in the vessel Q, an equal amount of the same kind of action, whether visible or invisible, must inevitably take place in the vessel R. It is therefore demonstrated, both by experiment and by the law of simultaneous definite action, that the transmission of an electric current through an electrolyte is invariably attended by the electrolyzation of those sections of it which are in contact with the electrodes; and, consequently, without electrolyzation the passage of an electric current from an electrode into an electrolyte does not occur (166).

174. Before dismissing this part of my researches on the transmission of electric currents through liquids contained in insulated vessels, which have been made with the view of comparing the results with those obtained with similar liquids when forming part of the terrestrial bed, I must not omit to make reference to the important discoveries of Ritter, Faraday, Grove, De la Rive, and others, on the influence which metallic surfaces exercise in producing gaseous combination, and of the secondary currents attending such combination, all of which are connected more or less intimately with the experiments which I have described. To De la Rive especially belongs the merit of effecting the transmission of a current through a liquid without any appearance of electrolyzation, by employing the current from a voltaic battery of considerable intensity, alternated by means of

a commutator, instead of that from a magneto-electric machine. He also found that an alternating current meets with less resistance when transmitted through an electrolyte than does the direct current from the same voltaic battery, and rightly conjectured that the non-appearance of the gases liberated by the alternating current was due to their recombination on the surface of the electrodes. Moreover De la Rive, in common with many other physicists, has maintained that the transmission of the current, and the electrolyzation of the liquid in contact with the electrodes, are inseparably connected. I do not, however, find that any one has yet given a direct proof, or one free from ambiguity, that the transmission of a current of so low an intensity as that used in Faraday's experiment is dependent upon the electrolyzation of the liquid through which such current is transmitted. Hence it is that up to the present time the absolute generality of the law of definite electrolysis has been doubted by some philosophers, and has never acquired more than a high degree of probability with others.

175. Returning to the phenomena exhibited by the conductors when in contact with the terrestrial bed:—It has been shown that all the effects of resistance and conduction produced by the naked and tape-covered coils immersed in the tub of fresh water were identical with those exhibited when these same coils were immersed in the canal (113, 143), (131, 144), (129, 147), (119, 150). We have also seen that the current from the $2\frac{1}{2}$ -inch machine, which would not heat thin wire when the conductors were coiled up separately in the canal or in the tub of fresh water, would nevertheless melt four inches of this wire at N when the coils were immersed in the tub of salt water at a distance of 3 inches apart (151). Hence it seemed highly probable that similar differences of resistance to the passage of the current would also be found between the fresh and salt water in contact with the terrestrial bed.

176. Two of the copper-rope conductors used in the canal experiments were therefore extended separately in the sea, on the coast of Lancashire, in about one fathom of water. On transmitting the current from the $2\frac{1}{2}$ -inch machine through the extended conductors, the same quantity of thin wire (four inches) was melted at N as when the conductors were coiled up separately in the tub of salt water (151).

177. But when the submerged conductors were placed in connexion with the single Grove's cell, the current which made three-quarters of an inch of thin wire red-hot at N when the double coil was immersed in the tub of salt water (152) was

now insufficient to make the same length of thin wire hotter than could be endured by the finger, the result thereby indicating a greater amount of resistance to the passage of the current into the earth than that which the salt water in the tub presented to the passage of the current between the same conductors when in the form of a double coil. This difference of resistance to the passage of the current, according as the conductors were extended in the sea or coiled up together in the tub of salt water, coincides with the difference which we have already observed when the same conductors were coiled up in the tub of fresh water and extended separately in the canal (115, 147).

178. To sum up the results of these experiments, as far as we have gone, on the transmission of electric currents through liquids contained in insulated vessels, and those forming part of the terrestrial globe:—By operating in both cases with the same electromotors and arrangement of conductors, all the various phenomena of conduction and resistance obtained in the tub of fresh and salt water were identical in kind with those obtained in the canal and in the sea, the difference in degree being due to the close proximity of the twin conductors when in the form of a double coil in the tub, which, as we have seen, augmented the amount of discharge from the electromotors above that which the normal action of the earth induced when these same conductors were extended separately in contact with it. From the increased amount of discharge obtained from the 10-inch intensity-armature when the double coil was immersed in dilute sulphuric acid (154), and the absolute stoppage of the current when a similar double coil was immersed in a vessel of oil (166), we may justly infer that similar extremes of conduction and resistance would manifest themselves if these liquids formed part of the terrestrial globe, so that the earth would consequently become either a conductor or an insulator, according to the nature of the liquids incorporated with its substance. Again, the resistance which the earth, whether forming the bed of the sea or of the canal, presents to the passage of a current of low intensity, and the diminution of this resistance by an increase of the intensity of the current, coincide with the same order of resistances and intensities of the current observed in the electrolyzation of liquids in insulated vessels. We have also seen that the transmission of the current from even the most feeble of the electromotors used in these experiments, either through dilute sulphuric acid or distilled water, was in every case attended by the electrolyzation of the liquids into which the electrodes were plunged. Hence we may justly conclude, even without the exhibition of the electrolytic products themselves (which will, however, be shown in experiments to be hereafter adduced (198–205)), that the transmis-

sion of an electric current from a conductor into the earth is invariably accompanied by the electrolyzation of the liquids in contact with this conductor, and that without such electrolyzation transmission of an electric current from an electrode into the earth does not occur.

179. For this reason I have not thought it necessary to make any experiments upon the electric condition of the earth by inserting electrodes in the ground at different distances from each other, as, owing to the varying degrees of moisture on different parts of the earth's surface, arising from differences of elevation, of climate, and of geological structure, it would be impossible by such means to arrive at any general law on the subject. Moreover, as the normal inductive action of the earth at any given point is, as we have seen, dependent upon the facility with which the liquids in contact with it can be electrolyzed, it follows that the electrolyzation of any liquid artificially introduced into the earth at that point cannot truly represent the amount of discharge which the earth is capable of inducing from a given electromotor (116, 176). We can easily understand, therefore, how Matteucci, by establishing communication between his electrodes and the earth by means of a solution of sulphate of copper or other liquids with which he impregnated the ground, and also by a variable degree of saturation of the soil, might obtain those anomalous variations of the earth's resistance which he attributed to differences in the length of the terrestrial matter extending between the electrodes, but which anomalous variations I have not met with in the course of my investigations.

180. Hitherto all the phenomena which we have observed to attend the passage of an electric current into the earth have been manifested by the heating of wires and by other effects produced at the electromotor ends of the submerged conductors, while the distant ends of the latter were disconnected. I will now proceed to describe some of the effects produced at the distant ends C, D of the conductors when extended separately in the canal and in the sea.

181. The two naked copper conductors (104) were extended along the bottom of the canal at a sufficient distance from each other to prevent any lateral discharge from taking place between them. A length of about 3 feet of each of the distant ends C, D of the conductors was allowed to project out of the water for the purpose of introducing iron wires or a tangent galvanometer into the circuit. The other ends, A, B, were joined on to the respective polar terminals of the electromotors.

182. When the current from a single Daniell's cell, or that

from a pair of zinc and platinum plates excited by dilute sulphuric acid, was transmitted through the submerged conductors, the galvanometer at C, D indicated the same degree of current as when these conductors were extended in the air, thereby showing that the current had sustained no appreciable loss in consequence of the non-insulation of the conductors.

183. On transmitting the current from the single Grove's cell through the submerged conductors, $\frac{5}{8}$ of an inch of the thin wire was melted at the distant point C, D, being nearly the same quantity as was melted by the same cell when the conductors were extended in the air (106).

184. On transmitting the current from the $2\frac{1}{2}$ -inch machine through the submerged conductors, the quantity of thin wire melted at C, D was 3 inches, whereas 4 inches of the same wire was melted when the conductors were extended in the air,—the result indicating, as in the experiment with the single Grove's cell, that an increase in the electric intensity was attended by a diminution of the amount of current which arrived at the distant ends of the conductors.

185. This diminution of the amount of an intensity-current arriving at the point C, D, reached its maximum when the current from the 10-inch intensity-armature was transmitted through the submerged conductors, as in this case the current arriving at C, D, though very considerable, was only sufficient to melt 4 feet of wire .050 of an inch in diameter instead of 6 feet of wire .065 of an inch in diameter, which the machine would melt when the conductors were in air. In this experiment it will be seen that the quantity of wire melted at C, D is the same as that melted at N by the current from the same armature when the ends C, D were disconnected (114). From other experiments made with the 5-inch quantity- and intensity-armatures, it appeared that just as the amount of electrodynamic effect at N was augmented by increasing the intensity of the current when the ends C, D were disconnected, so also was this augmentation of the electrodynamic effect at N attended by a reciprocal diminution of the amount of current arriving at C, D. On the other hand, just as the electrodynamic effects at N were diminished by lowering the tension of the current when the ends C, D were disconnected, so also was this diminution of the electrodynamic effect at N attended by a reciprocal increase of the amount of current arriving at C, D.

186. I have already said that when the two copper conductors were extended in the sea with the ends C, D disconnected, the current from the $2\frac{1}{2}$ -inch machine was sufficient to melt 4 inches of thin wire at N; whereas the current from a single Grove's cell, connected with the same arrangement of conduc-

tors, would not make the shortest length of the same wire red-hot (176, 177).

187. That the increase of discharge from the $2\frac{1}{2}$ -inch machine into the sea above that which was transmitted into the canal from the same electromotor, under like conditions, should be attended by a corresponding diminution of electrodynamic effect at the distant point C, D, was only what might have been expected from the results already obtained in the canal: thus, when the current from this machine was transmitted through the conductors extended in the sea, the amount of current arriving at C, D would only make $\frac{3}{4}$ of an inch of the thin wire red-hot; whereas when the conductors were extended in the canal, the current from the same machine would melt, at C, D, 3 inches of this same wire. When, however, a single Grove's cell was connected with the conductors extended in the sea, the current arriving at the distant point was, owing to its comparatively low intensity, so little diminished that it would make $\frac{1}{2}$ an inch of the thin wire red-hot.

188. The similarity of the results exhibited at the distant ends of the conductors when extended in the sea and in the canal with those obtained with the double coil immersed in the tub of salt water, affords another illustration of the dependence of the phenomenon of the earth's conductivity upon the electrochemical properties of the liquids in contact with the conductors, and leads me to think that currents of electricity of great quantity, but of an intensity below that which is required to effect the electrolyzation of the liquid in which the conductors are submerged, might be transmitted to considerable distances without the necessity of surrounding the conductors with an insulating envelope. How far it may be practicable, under the most favourable circumstances, to transmit to distant places electric currents sufficiently powerful to be made available for any useful purpose under conditions so anomalous as those above mentioned, it is impossible to say without further experiments with conductors of much greater length than those which I have employed. I will, however, just mention that in a series of experiments made with two lengths of the same copper rope as that used in the preceding experiments, each 750 feet long, extended in the canal, while a marked diminution of electrodynamic effects was observed at C, D when currents of high tension were transmitted through these conductors, yet currents of low tension arrived at C, D with but little diminution of their primitive intensity. Even with a current of so great an intensity as that from the 10-inch intensity-armature it was very interesting to witness the powerful electrodynamic effects produced at the ends C, D of the naked conductors; for after the current had passed

through a body of water 750 feet in length, a sufficient quantity still remained to produce a brilliant electric light, and to melt 22 inches of iron wire $\cdot 050$ of an inch in diameter. With the current of lower intensity from the 5-inch quantity-armature, which melted at C, D 12 inches of iron wire $\cdot 035$ of an inch in diameter when the two 750-feet lengths of conductor were extended in air, a sufficient quantity of the current still remained to melt 7 inches of the same wire ($\cdot 035$ of an inch in diameter) at C, D after it had traversed a body of water 750 feet in length.

§ 4. *On the Transmutable Nature of Water.*

189. In everything but the exhibition of the electrolytic products themselves, we have seen that all the phenomena attending the discharge of the electromotors through liquids contained in insulated vessels were identical in kind with those exhibited in similar liquids in contact with the earth, but with the remarkable difference before treated upon, viz. that while in the former case the electrolyzation of the liquids is dependent upon the mutual action of the electrodes on each other through the intervening electrolyte, in the latter case no mutual action of the electrodes is necessary for the electrolyzation of these same liquids when in contact with the terrestrial bed. Now the establishment of this distinction between the two actions completely overturns those electrochemical theories which make the transmission of an electric current through an electrolyte dependent upon a series of decompositions and recompositions of the liquid extending all the way between the two electrodes.

190. Nor is the phenomenon of the independent action of the electrodes, when in contact with the terrestrial bed, less fatal to the generally received doctrine of the atomic composition of water than it is to the electrochemical theories above referred to. For if the oxygen evolved at one electrode be independent of the hydrogen evolved at the other, then would it appear that water is the ponderable base of both oxygen and hydrogen, and might therefore be transformed either into oxygen or into hydrogen alone.

191. Consequences of such vital importance to chemical science as those which I have just indicated rendered it imperatively necessary that the fact of the evolution of the electrolytic products at the electrodes in contact with the earth should not rest on an inference only, so long as the proof was within the reach of experiment. I therefore made an arrangement by which the electricity in its passage into the earth should electrolyze the water in contact with it in a visible manner, and at the same time enable me to collect the electrolytic products for the purpose of examination.

192. The principal electromotor used in these experiments was the 10-inch intensity-armature, the currents from which were turned all in the same direction by means of a commutator. The current from this armature, as I have already said, would melt in short circuit 7 feet of iron wire .065 of an inch in diameter.

193. About 300 feet of naked copper rope, T, fig. 16, such as was used in the previous experiments (103), were extended in the canal in a single length, and connected with the aerial conductor I, for the purpose of obtaining a good discharge from the pole F of the machine.

194. The other aerial conductor, H, connected with the pole G, instead of terminating in the canal in the same manner as the conductor I, terminated in a pool of fresh water, U, formed in the ground about 200 feet from the canal. This pool was about 30 inches in diameter and 10 inches deep.

195. A thin deal box, V, 1 foot square and 8 inches deep (into the construction of which no metal was allowed to enter), was fitted up as a pneumatic trough for the purpose of collecting the electrolytic products in tubes or jars. This trough was floated in the pool and then filled with fresh water.

196. The free extremity of the aerial conductor H was terminated with a piece of thick copper wire well insulated with gutta percha, and the end of this copper wire was again terminated with a strip of platinum $3\frac{1}{2}$ inches long and $\frac{1}{2}$ an inch wide. The strip of platinum was introduced into the open end of a eudiometer-tube 18 inches long and $\frac{1}{2}$ an inch internal diameter, graduated on its outer surface into hundredths of a cubic inch.

197. When contact was made between the poles of the electromotor and the arrangement of conductors above described, so long as the platinum termination of the aerial conductor H was not allowed to touch the earth or the water in the pool, no indication of the passage of a current appeared on the galvanometer at N, nor in any other manner. But directly the platinum electrode touched the water, either in the pool or in the pneumatic trough, the galvanometer indicated the passage of a current into the earth, while a stream of small bubbles of gas rose from the electrode to the upper end of the inverted eudiometer-tube, which had been previously filled with water. These bubbles of gas, when collected in sufficient quantity, were examined and found to be hydrogen.

198. On reversing the polar connexions at the machine, so that the platinum termination of the conductor H formed a positive instead of a negative electrode, the gas which collected at the top of the eudiometer-tube was now found to be oxygen.

199. A few measures of sulphuric acid were now mixed with

the water in the pool and in the trough for the purpose of increasing the conductivity of the water, and thereby facilitating the discharge of the electromotor into the earth.

200. When contact with the electromotor was made in such a manner that the platinum termination of the conductor H formed a positive electrode, 0.36 of a cubic inch of oxygen was generated in the eudiometer in one minute.

201. On reversing the polar connexions at the electromotor, so that the platinum should now form a negative electrode, 0.73 of a cubic inch of hydrogen was generated in the eudiometer in one minute.

202. A large platinum plate was now substituted for the small strip of platinum at the extremity of the conductor H, and the eudiometer-tube was replaced by a glass jar, into which the platinum plate was introduced.

203. On transmitting the current from the machine into the earth so that the platinum plate formed a positive electrode, the magnitude of the current was sufficient to melt 3 feet of iron wire .035 of an inch in diameter at N, and to generate, in six minutes, 20 cubic inches of oxygen, which represent the transmutation of 6.92 grains of water into the same number of grains of gaseous oxygen at the ordinary atmospheric pressure and temperature.

204. On reversing the polar connexions, so that the platinum now formed a negative electrode, 20 cubic inches of hydrogen were generated in three minutes, which volume of gas represents the transmutation of 0.43 of a grain of water into the same weight of gaseous hydrogen.

205. These experiments, besides exhibiting in the most striking manner the electrolyzation of water when in contact with the terrestrial bed, also show, by the volumes of oxygen and hydrogen generated in equal times, and by the times occupied in the evolution of constant volumes, that these gases were evolved at the electrodes in the proportions in which they enter into combination to form water.

206. In order that no reasonable doubt may be entertained that the oxygen or hydrogen evolved from the electrode in the pool had no relation whatever to the oxygen or hydrogen liberated on the surface of the conductor extended in the canal, I will just add another observation to those already made in proof of the independent action of the electrodes when plunged into masses of liquid of planetary dimensions. In the summer of 1861 I had the good fortune to witness, near Manchester, the effects of a discharge of lightning on a piece of copper wire which had been used by my direction for the purpose of establishing a connexion between a line of telegraph and the signal-

ling-instruments. One part of the copper wire happened to be placed in close proximity to the copper lightning-conductor of a factory chimney, while another part of the same wire passed within an inch or so of touching a thick iron gas-pipe connected, of course, with the underground mains. The lightning, it would seem, in its passage between the clouds and the earth, finding a much easier path by way of the copper wire and gas mains than by way of the lower extremity of the lightning-rod terminating in the moist earth, melted and volatilized a length of about 15 feet of that portion of the copper wire which extended between the lightning-conductor and the gas-pipe.

207. Now the power which atmospheric electricity, or electricity of tension, possesses of effecting the electrolyzation of water is so well established* that no reasonable doubt can be entertained that the fusing of the copper wire in the instance above mentioned was attended by the electrolyzation of the water in contact with the metal gas mains, in the same manner as the current from the 10-inch intensity-armature, which fused 3 feet of iron wire at N also, simultaneously electrolyzed the water in the pool. But in the case of the electrolyzation of water at the lower extremity of a lightning-rod, the very foundation of any mutual action which might be supposed to exist between the positive and negative electrodes of an electromotor discharging itself into the earth is entirely removed.

208. Moreover, as the electrolyzation of the water in contact with the inferior extremity of a lightning-conductor is independent of the length of conductor extending upwards above the surface of the earth, it would follow that if it were possible to prolong this conductor through space to a planetary body (Mars, for example) containing the same electrolyte as that forming part of the terraqueous globe, or if such a line of metal were extended between two aqueous spheres of planetary dimensions (fig. 17), then would a current of electricity traversing this line of metal (according to the polarity of the electrodes) transmute the water of each of these spheres either into oxygen or into hydrogen alone.

209. Though I have been at considerable pains to prove absolutely that the passage of an electric current from a conductor

* M. Van Troostwyk, "Sur la résolution de l'eau en gaz oxygène et hydrogène par l'étincelle électrique," *Annales de Chimie*, tome v. p. 276, 1790. Barry, "On the Chemical Action of Atmospheric Electricity," *Phil. Trans.* 1831, p. 165. Faraday, "Electrochemical Decompositions by Common Electricity," *Phil. Trans.* 1833, pp. 34-676. Andrews, "On the Polar Decomposition of Water by Common and Atmospheric Electricity," *British Association Report*, 1855.

into water is in every case accompanied by electrolyzation, yet I have nowhere ventured to affirm that it is impossible for water to conduct an electric current without electrolyzation when no electrode is present; since it is only at the surfaces where the electrode and the electrolyte touch each other that the electrolytic phenomena are really manifested, and, so far, the presence of an electrode composed of a substance heterogeneous to that which is in course of electrolyzation is essential to the absoluteness of the law of definite electrolysis. Now there is no evidence whatever to show that water, whether in insulated vessels or in contact with the earth, conducts an electric current after it leaves the electrode in any way different from that in which the current is propagated in solid bodies such as metals; and consequently no hypothesis founded upon a contrary assumption has any claim to be admitted. When, therefore, the positive or negative electrode of an electromotor is plunged into a body of water in contact with the earth, it seems to me that the corpuscular motion of the electrode, by the very act of communicating itself to the molecules of water in immediate contact with it, transforms them into molecules of oxygen or hydrogen, the electric impulse (after leaving the electrode) being then received into the entire mass of the terraqueous globe by a physical conductivity similar to that by which the current is propagated in metallic bodies.

210. It is evident that the conclusions which I have arrived at respecting the transmutation of water, either into oxygen or into hydrogen, when this liquid is in contact with the earth, and also with regard to the mode in which the current is propagated after leaving the electrode, must extend to the mode of propagation of the current and to the electrolyzation of water contained in insulated vessels.

211. The possibility of generating and conducting an electric current in a closed ring or circuit of water without the intervention of electrodes of any kind by a physical conductivity similar to that by which a current is generated and propagated in a circuit entirely metallic has already been proved by Faraday, who, by coiling an india-rubber tube filled with acidulated water round the armature of an electromagnet, and then magnetizing and demagnetizing the armature, succeeded in generating alternate induction-currents in the aqueous circuit, just as if the armature and helix of water formed part of a magneto-electric machine*.

212. MM. Van Breda and Logeman have pointed out that, even in this case, when the generating aqueous circuit was completed by the wire of a galvanometer as described by Faraday, electrolyzation occurs at the extremities of the wire in contact

* Philosophical Magazine, S. 4. vol. vii. p. 265.

with the liquid coil*; and since the arrival at the galvanometer of the induction-current must necessarily have been preceded by its generation in the convolutions of the aqueous conductor surrounding the armature, this experiment affords another proof that the presence of an electrode, whether employed in transmitting or in receiving an electric current, is essential to the production of electrolytic phenomena. But in a modification of this experiment which I have made with a water helix formed in an india-rubber tube 37 feet in length, 20 feet of which were coiled round the electromagnet, while the other 17 feet were used simply as a conductor, after removing the galvanometer-wires which had first indicated the generation of the induction-current, and plunging the ends of the india-rubber tube into a glass vessel containing the same acidulated water as that which filled the tube, in order that the induced current should now be generated and propagated in a closed homogeneous circuit of water returning into itself, it is manifest that though the induced current would be generated and conducted as certainly as when the galvanometer formed part of the circuit, yet, owing to the absence of electrodes, there could be no electrolyzation of the water in any part of the circuit. Hence, if Faraday failed to prove that a feeble electric current could be transmitted from an electrode into an electrolyte without electrolyzation, this great philosopher has at any rate the rare merit of having indicated the only method by which a current can be generated and propagated in an electrolyte without the intervention of electrodes, and consequently without electrolyzation.

213. From a further comparison of the phenomena attending the propagation of an electric current in an aqueous circuit with those observed in a metallic one (both closed and interrupted), it would appear that the electrolytic products which make their appearance at the point of interruption of an aqueous circuit such as we have just considered (212), are the result of the disruptive discharge of the electrolyte against the electrode, just as the spark or the electric light is the result of the disruptive discharge from metallic or graphite conductors through air,—the principal difference between the two discharges (and one which exhibits in a remarkable manner the transmutations of matter and force) being this, that whereas the ponderable particles of matter which are projected from solid conductors (such as metals or graphite) by the disruptive discharge give out the whole of their specific energies in the form of heat and light at the moment of discharge, the specific energies of the ponderable particles of matter which are projected from the water by the electrolytic discharge, on the contrary, remain suspended until they are

* Philosophical Magazine, S. 4, vol. viii. p. 465.

made to develop themselves at the will of the experimenter, either suddenly, by explosion, or gradually, in the form of the oxyhydrogen-flame.

214. As the electrolytic products, as we have seen, are only manifested and obtained at the point where the generating or conducting aqueous circuit is interrupted by an electrode, just as the spark only occurs at the point of interruption of the metallic circuit in air, so a theory framed upon the supposition that the transmission of an electric current through a closed metallic circuit depends upon a succession of invisible sparks following each other through the solid portions of the conductor, might be supported with the same amount of plausibility as an hypothesis based upon the assumption that the propagation of an electric current through a closed aqueous circuit (212), or through the homogeneous portions of a liquid, after the current leaves the electrode, is dependent upon a series of imperceptible decompositions and recompositions of the whole of the electrolyte traversed by the current.

215. That the evolution of electrolytic oxygen and hydrogen from water is an operation of transmutation, and not one of decomposition, was further shown by the converse experiment of the reconversion of oxygen or hydrogen alone into water. The strip of platinum terminating the conductor H (197) was introduced into an inverted test-tube $4\frac{1}{2}$ inches long and $\frac{1}{2}$ an inch internal diameter, the upper half of which was filled with hydrogen and the lower half with acidulated water. The lower end of the tube was then immersed in the pool of acidulated water (199); and the platinum electrode, exposed to the hydrogen and water in the tube, was made positive, by placing the conductor H in permanent connexion with the platinum terminal of a single cell of the nitric-acid battery; while the zinc terminal of the same cell was connected with the conductor extended in the canal.

216. After connexion between the battery-cell and the electrode in contact with the hydrogen and water in the pool had been maintained for about five hours, the water had risen in the tube one inch and a half, thereby indicating the transmutation of a corresponding volume of hydrogen into water.

217. On substituting an equal volume of oxygen for the hydrogen in the tube, and making the platinum electrode negative by a much more feeble current than that used in the preceding experiment, the water completely filled the tube in about thirty hours, thereby indicating the transmutation of the whole of the oxygen into water.

218. From these experiments we may justly infer that if a

feeble electric current were generated in any manner in a metallic conductor extending between two aqueous spheres of planetary dimensions, and if one of these spheres were surrounded by an atmosphere of oxygen and the other sphere by an atmosphere of hydrogen, then would the negative and positive electrodes terminating the extended conductor transmute the oxygen envelope of one sphere and the hydrogen envelope of the other sphere into water.

219. That the views which I have enunciated respecting the transmutation of water into oxygen or into hydrogen, and the new facts which I have adduced in support of them, should not have been previously touched upon in some way, would indeed have been surprising, considering the number of powerful intellects which have been engaged in the field of electrochemical science during the present century. Thus we find Davy, in the course of some experiments on the electrolyzation of water in contact with globules of mercury on which oxide was formed without the apparent liberation of free hydrogen, suggesting the same idea which I have been engaged in demonstrating, viz. that water might be the ponderable basis of both oxygen and hydrogen*; but he hesitates to adopt so formidable a conclusion, as in no other case could he obtain one gas without the corresponding quantity of the other, or without some product into which the other gas might be supposed to enter. Subsequent researches on the power which hydrogen in the nascent state has to combine with the oxides, have fully justified this suspense of judgment. But in following up this speculation in the case of the electrolyzation of water when the positive and negative electrodes of the powerful battery of 2000 double plates of the Royal Institution were plunged into separate vessels of water, connexion between them being established through the human body by inserting a finger in each vessel, this philosopher would hardly seem to have been just to his original conception; for had he not attempted to account for the phenomena by the theory of transfer, or been hampered with the hypothesis that the liberation of the oxygen and hydrogen in the separate vessels depended upon a series of decompositions and recompositions of the liquids extending all the way between the two electrodes, no other conclusion could have been arrived at but that which he hesitated to adopt.

220. Another natural philosopher, whose researches would seem to have led to results intimately connected with some of those which I have obtained, is Mr. Grove, who has pointed out in the most forcible manner an insuperable objection which the

* Elements of Chemical Philosophy, by Sir Humphry Davy, LL.D., 1812; vol. i. pp. 172, 485-488.

action of his gas battery presents to the electrochemical theory of Grothuss, which makes the propagation of the current between two electrodes dependent upon a series of decompositions and recompositions of the water through which the current is transmitted*. Grove's objection to the theory is this:—In a single pair of the gas battery we must, according to the theory of Grothuss, suppose that oxygen and hydrogen in their separate states tear asunder oxygen and hydrogen already united—the force which produces the composition of water being in fact regarded by the theory as sufficiently strong to overcome the force by which its constituents are already held together. Grove also points out that the gas battery presents cases in which, according to this theory, a more feeble affinity overcomes a more powerful one, as when water is the electrolyte, and binoxide of nitrogen and oxygen are the gases.

221. Now the action of the gas battery, while altogether inexplicable by the theory of Grothuss, is very simple when viewed in connexion with the new results which I have obtained, and with the dynamical theory of gases which has recently made such advances through the labours of Joule, Clausius, and others. For if free oxygen and hydrogen may now be looked upon merely as the same ponderable matter, separated into molecules of greater and less weight in a state of rapid motion, the lighter molecules (hydrogen) moving at a much greater velocity than the heavier molecules (oxygen), in order that equal volumes of these gases may produce equal pressures, it is no longer surprising, now that the influence which platinum exercises in inducing gaseous combination is known, that the molecules of gas should be able to communicate their motion to the comparatively inert molecules of water which are in contact with the surface of the platinum, in a similar manner to that in which the translatory motion of inelastic bodies is by collision transformed into the motion of heat, that portion of the water extending between the electrodes but not in immediate contact with them being not electrolyzed, but simply completing the circuit in the same manner as an ordinary metallic conductor.

222. That the conclusions which I have arrived at respecting the nature of water will escape the penalties usually visited upon opinions which run counter to established ideas, whether true or false, is hardly to be expected on a subject on which such strong views are held as on the composition of water. But for the benefit of those who, like myself, refuse to accept the explanation of any phenomenon as being absolutely true, without first considering it in relation to the mind of the individual who essays to interpret it, I will just observe that, in a critical examination of

* Philosophical Magazine, S. 3. vol. xxvii. p. 348.

the various notions which have up to the present time been held on the nature of water, it is highly important that a proper estimate should be taken of the powerful influence which the chronological order in which some discoveries in physical science are made has in establishing in the minds of philosophers of one generation ideas of causation and composition which are looked upon as absurd by philosophers of another. Thus we find that water, up to the time of Cavendish and Watt*, was considered to be an element, simply on account of the inability of chemists to convert it into anything else; while the present notion of the compound nature of water rests entirely on the inability of modern chemists to obtain water from oxygen or hydrogen alone, or to obtain either of these gases alone from water.

223. Now, had the chronological order in which the experiments described in this paper and those of Cavendish and Watt were made been reversed, the doctrine of the transmutable nature of water would at once have been accepted, as agreeing strictly with observation and experiment. But the theory of the atomic composition of water, if broached for the first time at the present day, would be summarily rejected as not accounting either for the evolution of oxygen or hydrogen from water forming part of the terraqueous globe (200, 201), or for the recombination of either of these gases alone into water (216, 217).

224. Nor would the theory of Grothuss under these altered circumstances meet with any better reception at the present day than would be accorded to the theory of the composition of water, because the former theory, as we have seen, breaks down completely when called upon to account for the electrolyzation of planetary masses of water by means of a single electrode, or when the distance between the two electrodes is considerable; and yet, although the theory of Grothuss was invented solely for the purpose of reconciling the phenomenon of the electrolyzation of water with the theory of its composition †, and therefore ought not to be used in any manner to support the latter theory, there will not be found wanting some minds in which these two theories are so confusedly mixed together that Grothuss's theory will be brought forward to explain and support the theory of the composition of water, while the latter theory will, in its turn, be adduced as proof sufficient of the truth of the theory of Grothuss.

225. It is to philosophers of a breadth and liberality of mind sufficient to realize the full force of the accidental influences which I have just indicated that I look for an impartial consideration of this great question—to philosophers who are so far

* Philosophical Transactions, 1784, pp. 130, 329.

† "Théorie de la décomposition des liquides par l'électricité galvanique," par C. J. D. de Grothuss, *Annales de Chimie*, vol. lviii. p. 64 (1806).

faithful to the evidence of their own senses and to their own understandings as not to be unduly influenced by the truth as it appears to this man or to that, but who, with a singleness of purpose which is regardless of all consequences, seek to behold the truth only as it is in nature.

[Our correspondent's concluding paragraphs (222, 225) have induced us to publish his statements regarding the transmutable nature of water, although we do not agree with him, and believe that the results of his experiments will bear a very different interpretation. We are of opinion that the supposed conversion of water into oxygen or hydrogen (197 *et seq.*) is simply a decomposition of water, the oxygen being evolved in the one pool, the hydrogen in the other, *the earth acting as the conductor between the two pools*. The large surface of the conducting wire in the one pool would naturally make the amount of gas evolved at that pole appear very small. We also think that the conversion of hydrogen or oxygen into water admits of a different explanation; it seems to us that the author has simply formed a gas battery.—W. F.]

XV. *On the Theory of Pressure in Fluids.* By R. MOON, M.A.,
Honorary Fellow of Queen's College, Cambridge*.

IN a former paper† I treated of four cases of pressure, in the first of which the received theory as to the law of pressure in elastic fluids was shown to contradict a universally received principle, viz. the principle that action and reaction are equal and opposite. In the second case considered, the assumption of the truth of the same law was shown to lead to an absurd result; while in the two remaining cases the same assumption was shown to lead to results which are palpably erroneous.

If we inquire into the evidence upon which the received law of pressure in elastic fluids rests, it will, I think, be found that the law having been proved experimentally to hold in the case of equilibrium, it was assumed, in the absence of any obvious reason to the contrary, to hold generally; and certain illustrious analysts having shown that its adoption led to some striking and beautiful results, suspicion was lulled, and acquiescence in the correctness of the theory became general.

The cases considered in my former paper establish conclusively that the law which has thus been assumed is not universally true. A further inquiry into the subject therefore becomes necessary; and such an inquiry, so far as the case of motion in one direction is concerned, I propose to institute in the present paper. It will be found, if I mistake not, to lead to conclusions of the greatest interest, simplicity, and value.

* Communicated by the Author.

† See Philosophical Magazine, vol. xxxvi. p. 27.

Suppose that we have a cylindrical tube of indefinite length filled with air; and let x be the ordinate of the position of rest of a lamina made by planes perpendicular to the axis of the tube; y the same ordinate at the time t ,—the whole motion being supposed parallel to the axis of the tube, to which the ordinates are measured parallel.

Let ρ , v , p denote respectively the density, velocity, and pressure at the time t at the point whose ordinate at that time is y .

If D be the density of equilibrium, we shall have the following equation of motion, viz.

$$0 = \frac{d^2y}{dt^2} + \frac{1}{D} \frac{dp}{dx}, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

an equation which holds without reference to any hypothesis as to the nature of the law of pressure, resting on the simple assumption that the fluid is a continuous substance.

It is obvious that in any particular case of motion each of the quantities p , ρ , v must be capable of being expressed in terms of x and t , i. e. that we may assume

$$p = f_1(xt),$$

$$\rho = f_2(xt),$$

$$v = f_3(xt).$$

From the last two, eliminating x , we may obtain

$$t = f_a(\rho, v);$$

and eliminating t , we may obtain

$$x = f_b(\rho, v).$$

Substituting these values in the equivalent of p , we shall obtain

$$p = \text{funct.}(\rho, v).$$

We may hence perceive the gigantic character of the assumption made in the received theory by taking, under all circumstances, $p = a^2\rho$.

All that we know of the pressure *à priori* is that p is a function of ρ and v , or, as we may put it, that

$$p = F_a(\rho) + F_b(\rho, v);$$

where F_b is some function of ρ and v which vanishes when $v=0$. Now, even if Mariotte's law established (which it does not*)

* Mariotte's law has only been proved to hold when the air is in equilibrium. In the last three cases of my former paper it is clear that the air is not in equilibrium at the time when we have $v=0$ throughout the entire mass, at which time it is shown in each case that Mariotte's law does not hold.

that under all circumstances when $v=0$ we have $p=a^2\rho$, that would only entitle us to assume that

$$p=a^2\rho+F_b(\rho, v),$$

where F_b is some unknown function of ρ and v which vanishes when $v=0$, the making $F_b=0$ when v does not $=0$ being to be regarded as a pure unqualified assumption.

Returning from this digression—since p may be regarded as a function of ρ and v , we shall have

$$\frac{dp}{dx} = \frac{dp}{d\rho} \frac{d\rho}{dx} + \frac{dp}{dv} \frac{dv}{dx} = R \frac{d\rho}{dx} + V \frac{dv}{dx} \text{ suppose,}$$

where R and V are functions of ρ and v only; and substituting this value in (1), observing that

$$v = \frac{dy}{dt}, \quad \rho = D \cdot \left[\frac{dy}{dx} \right]^{-1},$$

we get

$$0 = \frac{d^2y}{dt^2} + \frac{V}{D} \frac{d^2y}{dx dt} - R \left[\frac{dy}{dx} \right]^{-2} \frac{d^2y}{dx^2}, \quad \dots \quad (2)$$

in which, by virtue of the above equivalents for ρ and v , V and R may be considered as functions of $\frac{dy}{dt}$ and $\frac{dy}{dx}$ only.

In treating of the integration of this equation, I shall assume that V and R are *definite* functions, *i. e.* that they contain neither arbitrary constant nor arbitrary function. If, by reason of the expression for p in terms of ρ and v (and therefore in terms of $\frac{dy}{dt}$, $\frac{dy}{dx}$) involving arbitrary constants or arbitrary functions, such or the like constants or functions enter into the composition of (2) when exhibited in its most general form, I shall assume that definite values have been assigned to such constants and functions; so that it is only to the equation (2) when modified in the manner just indicated that the remarks immediately following must be conceived to apply.

Thus much being premised, I shall simply assume that (2) is integrable by Monge's method, and shall follow out some of the consequences of that assumption.

One consequence of that assumption will be that (2) is capable of being derived from a single partial differential equation of the first order, of the form

$$F\left(xyt \frac{dy}{dx} \frac{dy}{dt}\right) = \phi\left\{F_1\left(xyt \frac{dy}{dx} \frac{dy}{dt}\right)\right\}, \quad \dots \quad (2a)$$

where ϕ is arbitrary; whence it follows, putting $\phi = \alpha$, where α is any constant, that (2) is capable of being derived from an equation of the form

$$F\left(xyt \frac{dy}{dx} \frac{dy}{dt}\right) = \alpha, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where α is an arbitrary constant independent of the value of any constant appearing in F .

Differentiating (3), we get

$$\left. \begin{aligned} 0 &= F'(x) + F'(y) \frac{dy}{dx} + F'\left(\frac{dy}{dx}\right) \frac{d^2y}{dx^2} + F'\left(\frac{dy}{dt}\right) \frac{d^2y}{dx dt}, \\ 0 &= F'(t) + F'(y) \frac{dy}{dt} + F'\left(\frac{dy}{dx}\right) \frac{d^2y}{dx dt} + F'\left(\frac{dy}{dt}\right) \frac{d^2y}{dt^2}, \end{aligned} \right\} \quad (4)$$

By hypothesis, (2) is derivable from these last two equations and from (3); and since α appears in (3) and does not appear in (2) or in either of the equations (4), it is clear that (2) must be derivable from equations (4) alone, without taking account of (3).

Hence the right side of (2) must be identical with the right side of the following equation, viz.

$$\begin{aligned} 0 &= A \left\{ F'(x) + F'(y) \frac{dy}{dx} + F'\left(\frac{dy}{dx}\right) \frac{d^2y}{dx^2} + F'\left(\frac{dy}{dt}\right) \frac{d^2y}{dx dt} \right\} \\ &\quad + B \left\{ F'(t) + F'(y) \frac{dy}{dt} + F'\left(\frac{dy}{dx}\right) \frac{d^2y}{dx dt} + F'\left(\frac{dy}{dt}\right) \frac{d^2y}{dt^2} \right\}, \end{aligned}$$

where A, B are functions of $txy \frac{dy}{dx} \frac{dy}{dt}$. Therefore, comparing the coefficients of corresponding terms, we must have

$$\begin{aligned} 1 &= BF'\left(\frac{dy}{dt}\right), \\ \frac{V}{D} &= BF'\left(\frac{dy}{dx}\right) + AF'\left(\frac{dy}{dt}\right), \\ -R \left[\frac{dy}{dx} \right]^{-2} &= AF'\left(\frac{dy}{dx}\right), \\ 0 &= B \left\{ F'(t) + F'(y) \frac{dy}{dt} \right\} + A \left\{ F'(x) + F'(y) \frac{dy}{dx} \right\}; \end{aligned}$$

whence we have, eliminating A and B ,

$$\left. \begin{aligned} 0 &= \frac{\overline{F' \left(\frac{dy}{dx} \right)}^{-2}}{F' \left(\frac{dy}{dt} \right)} - \frac{V}{D} \cdot \frac{F' \left(\frac{dy}{dx} \right)}{F' \left(\frac{dy}{dt} \right)} - R \overline{\frac{dy}{dx}}^{-2}, \\ 0 &= \frac{F' \left(\frac{dy}{dx} \right)}{F' \left(\frac{dy}{dt} \right)} - R \overline{\frac{dy}{dx}}^{-2} \cdot \frac{F'(x) + F'(y) \frac{dy}{dx}}{F'(t) + F'(y) \frac{dy}{dt}}, \end{aligned} \right\} \dots (5)$$

the first of which gives us

$$\frac{F' \left(\frac{dy}{dx} \right)}{F' \left(\frac{dy}{dt} \right)} = \frac{V \pm \sqrt{V^2 + 4RD^2 \cdot \frac{dy}{dx}}^{-2}}{2D} \dots (6)$$

Let

$$F \left(xyt \frac{dy}{dx} \frac{dy}{dt} \right) = F \left(xyt \frac{D}{\rho} v \right) = f(xyt\rho v),$$

then

$$F' \left(\frac{dy}{dx} \right) = F' \left(\frac{D}{\rho} \right) = -\frac{\rho^2}{D} f'(\rho),$$

$$F' \left(\frac{dy}{dt} \right) = f'(v), \quad F'(x) = f'(x), \quad F'(y) = f'(y), \quad F'(t) = f'(t);$$

therefore (6) gives us

$$0 = f'(\rho) + \frac{V \pm \sqrt{V^2 + 4R\rho^2}}{2\rho^2} \cdot f'(v); \dots (7)$$

while from (5) we get, reducing,

$$\begin{aligned} \frac{f'(x) + f'(y) \frac{D}{\rho}}{f'(t) + f'(y)v} &= \frac{D}{2R\rho^2} (V \pm \sqrt{V^2 + 4R\rho^2}) \\ &= -\frac{2D}{V \mp \sqrt{V^2 + 4R\rho^2}}, \end{aligned}$$

or

$$\left. \begin{aligned} 0 &= \frac{(V \mp \sqrt{V^2 + 4R\rho^2})}{2D} f'(x) + f'(t) \\ &+ \left(\frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2\rho} + v \right) f'(y); \end{aligned} \right\} \dots (8)$$

f must satisfy (7) and (8) simultancously.

If $\omega_1 = C_1$ and $\omega_2 = C_2$ be the respective integrals of the equations

$$\left. \begin{aligned} 0 &= dv - \frac{V + \sqrt{V^2 + 4R\rho^2}}{2\rho^2} d\rho, \\ 0 &= dv - \frac{V - \sqrt{V^2 + 4R\rho^2}}{2\rho^2} d\rho, \end{aligned} \right\} \dots \dots (8 a)$$

we shall have in ω_1, ω_2 two values for f , since they obviously satisfy (7) and also, being free from x, y , and t , satisfy (8).

Again, we have for the integration of (8) the following auxiliary equations, viz.

$$\begin{aligned} 0 &= dx - \frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2D} dt, \\ 0 &= dy - \left(\frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2\rho} + v \right) dt, \\ 0 &= d\rho, \\ 0 &= dv. \end{aligned}$$

Hence we shall have the two following additional values for f , viz.

$$f = \phi \left\{ \begin{aligned} &\rho, v, \left[x - \frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2D} \cdot t \right], \\ &\left[y - \left(\frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2\rho} + v \right) \cdot t \right], \end{aligned} \right\} \dots \dots (9)$$

where ϕ is arbitrary, provided these values satisfy also equation (7).

But if (9) satisfies (7), the latter will be satisfied by

$$f = \phi \left\{ x - \frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2D} \cdot t \right\},$$

in order to which we must have

$$\left. \begin{aligned} 0 &= \frac{d}{d\rho} (V \mp \sqrt{V^2 + 4R\rho^2}) \\ &+ \frac{V \pm \sqrt{V^2 + 4R\rho^2}}{2\rho^2} \cdot \frac{d}{dv} (V \mp \sqrt{V^2 + 4R\rho^2}); \end{aligned} \right\} (10)$$

or, which is the same thing,

$$\left. \begin{aligned} 0 &= (V \mp \sqrt{V^2 + 4R\rho^2}) \frac{d}{d\rho} (V \mp \sqrt{V^2 + 4R\rho^2}) \\ &- 2R \frac{d}{dv} (V \mp \sqrt{V^2 + 4R\rho^2}), \end{aligned} \right\} \dots (10 a)$$

which, taking alternately the upper and lower signs, constitutes a pair of equations of condition which must be satisfied when f is susceptible of both the values (9).

Moreover, since if (9) satisfies (7) the latter will be satisfied by

$$f = \phi \left\{ y - \left(\frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2\rho} + v \right) t \right\},$$

we have the further equations of condition,

$$0 = \frac{d}{d\rho} \left(\frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2\rho} + v \right) + \frac{V \pm \sqrt{V^2 + 4R\rho^2}}{2\rho^2} \cdot \frac{d}{dv} \left(\frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2\rho} + v \right) \Big\},$$

which reduce to the following, viz.

$$0 = \pm \frac{2\sqrt{V^2 + 4R\rho^2}}{\rho} + \frac{d}{d\rho} (V \mp \sqrt{V^2 + 4R\rho^2}) + \frac{V \pm \sqrt{V^2 + 4R\rho^2}}{2\rho^2} \cdot \frac{d}{dv} (V \mp \sqrt{V^2 + 4R\rho^2}).$$

Hence if (10) hold, we must have

$$V^2 + 4R\rho^2 = 0,$$

a conclusion which may be rejected on account of defect in generality*. Therefore when equations (10) hold, (9) must reduce itself to

$$f = \phi \left\{ \left(x - \frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2D} \cdot t \right), \rho, v \right\}, \quad \dots \quad (11)$$

and we shall thus have for f four values in all, viz. those given by the last equation together with those previously found, viz. ω_1, ω_2 .

Now it will be remembered that the original integral which we assumed for (2) was (2a), or, which is the same thing,

$$f(xyt\rho v) = \chi \{ f_a(xyt\rho v) \}. \quad \dots \quad (12)$$

It is clear, however, that this last equation may be put under the form

$$f_a(xyt\rho v) = \chi_{-1} \{ f(xyt\rho v) \},$$

where χ_{-1} denotes an arbitrary function; and if we treat this equation in the same way in which we have treated (12), we shall arrive at precisely the same formulæ for the determination

* The grounds upon which I rest this conclusion will hereafter be more distinctly pointed out.

of f_a as those at which we have already arrived for the determination of f .

Thus the occurrence of two of the four values of which f has been shown to be susceptible is at once accounted for; and if we reflect that almost universally when an equation of the second order is integrable by Monge's method, there are two equations of the first order of the form (2a) from each of which it is separately derivable, there cannot, I think, be a doubt that the four values above derived for f must be attributable to the values of f and f_a in equation (12), and to the corresponding functions in the other first integral of (2).

Acting from this clue, we shall be justified in assuming, and we shall find it to be the fact, that when the equations of condition (10) are satisfied, (2) will be susceptible of the two following integrals, from each of which separately it is capable of being derived, viz.

$$\left. \begin{aligned} \omega_1 &= \phi \left\{ x - \frac{V - \sqrt{V^2 + 4R\rho^2}}{2D} \cdot t \right\}, \\ \omega_2 &= \psi \left\{ x - \frac{V + \sqrt{V^2 + 4R\rho^2}}{2D} \cdot t \right\}. \end{aligned} \right\} \quad \dots \quad (13)$$

If we now recur to the equations of condition (10) or (10a), or, as they may be written,

$$\left. \begin{aligned} 0 &= P_2 \frac{dP}{d\rho} - 2R \frac{dP_2}{dv}, \\ 0 &= P_1 \frac{dP_1}{d\rho} - 2R \frac{dP_1}{dv}, \end{aligned} \right\} \quad \dots \quad (14)$$

where

$$P_1 = V + \sqrt{V^2 + 4R\rho^2}, \quad P_2 = V - \sqrt{V^2 + 4R\rho^2},$$

the form of the equations leads us to conclude that, if neither of the quantities P_1, P_2 vanishes (as for instance P_1) so as to render $\frac{dP_1}{d\rho} = \frac{dP_1}{dv} = 0$, we shall have

$$P_1 = P_2,$$

which implies that we have $\sqrt{V^2 + 4R\rho^2} = 0$,—a conclusion which I have already pointed out as one to be rejected as deficient in point of generality*.

* This may be seen more distinctly as follows. In the case supposed, (13) become

$$\omega_1 = \phi \left\{ x - \frac{V}{2D} t \right\}, \quad \omega_2 = \psi \left\{ x - \frac{V}{2D} t \right\},$$

whence we get $\omega_1 = \text{funct. } \omega_2$; and therefore we must have $\rho = \text{funct. of } v$, since ω_1, ω_2 involve ρ and v only. Now not only is this conclusion, viz. that $\rho = \text{funct. } v$, clearly defective in point of generality, but, if it were true, we

But if one of the quantities P_1, P_2 is constant—for instance, if we have

$$V + \sqrt{V^2 + 4R\rho^2} = 2\alpha,$$

where α is constant, we shall have

$$R\rho^2 = \alpha^2 - \alpha V,$$

or

$$0 = \rho^2 \frac{dp}{d\rho} + \alpha \frac{dp}{dv} - \alpha^2,$$

which gives us

$$p = -\frac{\alpha^2}{\rho} + \chi\left(v + \frac{\alpha}{\rho}\right),$$

a value which, it will be found, satisfies *both* the equations of condition (14).

In this case equations (8 a), from which ω_1, ω_2 are to be derived, become

$$\begin{aligned} 0 &= dv - \frac{\alpha}{\rho^2}, \\ 0 &= dv - \left\{ \chi'\left(v + \frac{\alpha}{\rho}\right) - \alpha \right\} \frac{d\rho}{\rho^2}; \quad . \quad . \quad (15) \end{aligned}$$

$\therefore \omega_1 = v + \frac{\alpha}{\rho}$, and equations (13) become

$$\begin{aligned} v + \frac{\alpha}{\rho} &= \phi \left\{ x - \frac{\chi'\left(v + \frac{\alpha}{\rho}\right) - \alpha}{D} t \right\}, \\ \omega &= \psi \left\{ x - \frac{\alpha}{D} t \right\}, \end{aligned}$$

where $\omega = \text{const.}$ is the integral of (15).

If we put $u = v + \frac{\alpha}{\rho}$, the three equations which determine the circumstances of the motion, *i. e.* the pressure, density, and velocity, become

$$\left. \begin{aligned} p &= -\frac{\alpha^2}{\rho} + \chi(u), \\ u &= \phi \left\{ x - \frac{\chi'(u) - \alpha}{D} t \right\}, \\ \frac{1}{\rho} + \int \frac{du}{\chi'(u) - 2\alpha} &= \psi \left\{ x - \frac{\alpha}{D} t \right\}, \end{aligned} \right\} \quad . \quad . \quad (16)$$

might assume $p = \text{a function of } \rho \text{ only}$ —an assumption which is open to all the objections which in my former paper have been shown to apply to the law of the received theory, *viz.* $p = a^2\rho$.

results which have been evolved from the general equation (2) without the aid of any subsidiary hypothesis with regard to the nature of the law of pressure, by the simple application to (2) of the ordinary process for the integration of partial differential equations of the second order involving two independent variables*.

If the fact of the expression for the pressure containing an arbitrary function, thus causing *three* arbitrary functions to enter into the complete solution of the problem, occasions surprise, I may observe that it may be shown *à priori* that such must be the case.

6 New Square, Lincoln's Inn,
July 7, 1868.

XVI. *On the Diammonic Carbonate, or Normal Carbonate of Ammonium.* By EDWARD DIVERS, M.D., F.C.S., Lecturer on Natural Philosophy, Charing Cross Hospital†.

IT is now nearly thirty years ago since H. Rose wrote, in his classical memoir on the compounds of ammonia with carbonic acid‡, "the neutral anhydrous carbonate of ammonia cannot be combined with the quantity of water sufficient to convert the ammonia into oxide of ammonium." The carbonates of ammonia have since been reinvestigated by Deville§; and he admits the existence of only two crystalline combinations—the tetrammonic dihydric tricarbonat (or true sesquicarbonat), and the ammonic hydric carbonat (or bicarbonat).

Resting on the authority of these distinguished chemists, the text-books tell us that the carbonat of ammonium *cannot be isolated*. I have now to announce that the normal or diammonic carbonat can be prepared in the simplest manner imaginable. I must say that, with the above assertions before me, I was much pleased to succeed very easily in my efforts to form this body; but I certainly was not astonished, because comparatively recently it has been shown by Dr. Hofmann that two bodies whose place is little beyond the threshold of chemistry (formic aldehyde and formamide), and which it was said could not be produced, are actually to be prepared in the most ordinary manner.

Diammonic carbonat is formed by treating the commercial

* The truth of equations (16) may be readily verified, observing that, in virtue of the value there given for p , (2) can be put under the form

$$0 = \frac{du}{dt} + \frac{X'(u)}{D} \frac{du}{dx} + \frac{\alpha}{\rho^2} \frac{d\rho}{dt} + \frac{\alpha^2}{D\rho^2} \frac{d\rho}{dx}.$$

† Communicated by the Author.

‡ Pogg. *Ann.* vol. xlv. p. 373.

§ *Ann. de Chim.* S. 3. vol. xl. p. 87.

carbonate with a solution of ammonia. Some of this substance dissolves in the ammonia-water; the rest remains, as a skeleton of the original solid, in the condition of a soft mealy semicrystalline mass. This is the normal carbonate. If it be digested for some days in a closed vessel with the ammoniacal liquor after this has had its free ammonia renewed in it by the passage of some ammonia gas through it while kept cool by external means, a remarkable phenomenon is observed. Even when the solid is at first in such quantity as to leave the mixture only semifluid, the whole will gradually, particularly when occasionally agitated, become a solution. Warming the vessel in the early period of the digestion seems to have little effect in hastening this peculiar solution; while cooling it in ice has little effect (if any) in increasing the solidification of the mixture. This interesting occurrence of the slow disappearance of the solid carbonate I shall not in the present paper attempt to explain (though I am quite satisfied that I am able to do so); for my experiments in this direction are, as yet, incomplete. If now to the clear liquid more of the commercial carbonate be added, further solution of it occurs; and now a gentle heat seems favourable to this process. Either on cooling after warming, or by the action of applied cold, fine spicula fill the liquid, almost solidifying it from the commencement of their formation, though, somewhat like the potassic silicofluoride, they do not much diminish the transparency of the whole. These minute crystals, which are identical in composition with the mass left when the commercial carbonate is treated with ammonia, rapidly increase in quantity, but not materially in individual size. They are not grouped in stars or bundles, but diffused uniformly through the liquid. By jolting the vessel, the crystals, if not too numerous, may be shaken together so as to form a shrunken mould, as it were, of the inside of the vessel. Removed from their mother-liquor, or, more correctly, drained from and squeezed free of their mother-liquor, they begin to decompose. But with proper precautions to prevent this decomposition, they can be exhibited in soft masses of minute crystals of brilliant silky lustre. They smell most intensely of ammonia. They dissolve very freely in water, but require about seventy measured parts of ordinary rectified spirit to dissolve them. In strong ammonia solution they dissolve at first very sparingly; but solution continues to go on slowly, as I have above described it doing in the case of the carbonate in the mealy condition. Exposed freely to the air, the salt entirely loses its lustre in a few moments, evolving torrents of ammonia, and becoming at the same time damp from the liberation of water. After a while (very rapidly if disturbed and repeatedly pressed between fresh bibu-

lous paper) it becomes a white powder of the acid carbonate, weighing little more than half the original substance—the matters volatilized, therefore, being not only ammonia and water, but also a good deal of the entire salt or its constituents. Not only do the crystals become damp in the open air, but also, and rapidly, when they are in closed vessels.

The diammonic carbonate in the same crystalline condition may also be obtained by dissolving the commercial salt to saturation in water and then passing ammonia gas through the solution kept cool. A moderately strong aqueous solution of the new salt, cooled in ice, does not crystallize; but when treated with ammonia gas, the salt crystallizes out from it. By digestion afterwards in the ammoniacal liquor the crystals gradually disappear, and do not return when the liquid is cooled again to 0° C. Gently heated in a closed tube, the crystals melt to a transparent liquid, which volatilizes and forms a semicrystalline moist sublimate. If the fused mass be cooled, it gradually solidifies to a mass of prisms; this mass, and most of the sublimate, spontaneously volatilize when the tube is kept open; at least they did so on the occasion in which they were thus tried.

To prepare the salt for analysis, it was rapidly deprived of the bulk of the mother-liquor by draining on bibulous paper, and then submitted to moderate pressure between fresh layers of paper. In the case of sample I., after draining the crystals on paper, they were enclosed in a small close glass chamber (extemporized for the purpose) and packed tightly between bibulous paper, and the whole was then left for some time surrounded with ice. The vessel had to be opened from time to time to renew the paper and break down the flakes of crystals; but these operations were performed with all possible expedition. When the drying appeared to be complete, the soft crystalline cake of still lustrous crystals was removed, pressed for a moment firmly between a fresh fold of paper (no distinct damping of which was observed), and then rapidly transferred to weighing-tubes and forthwith analyzed. By the time the substance was in the tubes it had lost much of its crystalline lustre, and began to be slightly adherent from acquired dampness, so rapidly does the salt change.

Sample II. was not enclosed in the glass chamber and kept cool by external cold; but the active volatilization of its highly ammoniacal mother-liquor maintained for a time a sensibly low temperature in the mass. Some difference in behaviour during the drying was observed, in consequence of this different arrangement. After a few changes of paper little progress seemed to be made in the drying; and besides, the paper began to adhere somewhat closely to the cake, which also lost most of its lustre. The cause of these phenomena was undoubtedly the decomposi-

tion of some of the salt, attended with the liberation of water, and a deposition of the little-soluble acid carbonate on the surface of the cake and in the pores of the paper. Not to lose too much ammonia, the salt was collected in a slightly damp condition. Sample III. was the same preparation as II., but was subjected to a much longer process of drying, with, however, little apparent advantage.

The substance IV. was the semicrystalline solid which is left when the commercial carbonate is first treated with ammonia solution. It was dried as was II., and behaved similarly, except that it had no lustre to lose. Lastly, V. was the crystalline form of the carbonate obtained by dissolving the commercial carbonate (freed from adhering acid carbonate) to saturation in water mixed with some strong solution of ammonia, and then keeping the solution cool for a couple of hours in ice. It was not prepared with much dispatch for analysis; and the numbers obtained show evidence of marked decomposition having occurred. In sample I., which appears to have contained over 2 per cent. moisture, the ammonia will be seen to have been almost exactly in quantity equivalent to that of the carbonic anhydride. The portion of II. destined for the determination of the ammonia was spoiled.

If either of the forms of the carbonate be leisurely pressed with free exposure, so long as it damps fresh paper it becomes the dry hydric ammonic (or acid) carbonate. One specimen thus obtained was analyzed.

The ammonia was determined by standard solutions of sulphuric acid and soda, the carbonate being dropped from the weighing-tube into a flask containing a small excess of the acid diluted, the solution boiled and shaken to get rid of all carbonic anhydride, and then the soda added to neutralization. The carbonic anhydride was estimated in the direct way, after its expulsion by hydrochloric acid and passage over fused calcic chloride and blue-vitriolized pumice, by combining it with soda-lime in a U-tube. The apparatus was arranged and the operation performed with the precautions prescribed in Fresenius's 'Quantitative Analysis.'

Sample I.—7877 grm. gave 2948 grm. carbonic anhydride; 74565 grm. gave 21318 grm. ammonia.

Sample II.—7063 grm. gave 2715 grm. carbonic anhydride.

Sample III.—7172 grm. gave 2857 grm. carbonic anhydride; 5483 grm. gave 1547 grm. ammonia.

Sample IV.—105965 grm. gave 4042 grm. carbonic anhydride; 4627 grm. gave 12886 grm. ammonia.

Sample V.—·85645 grm. gave ·3353 grm. carbonic anhydride ;
·53245 grm. gave ·1411 grm. ammonia.

These data give the following percentage composition for the five several preparations :—

	I.	II.	III.	IV.	V.
Carbonic anhydride.	37·43	38·44	39·84	38·15	39·15
Ammonia . . .	28·59	..	28·21	27·85	26·50

The formula of the diammonic carbonate with one atom of water of crystallization,



gives the following numbers :—

Carbonic anhydride . .	38·60
Ammonia	29·82
Water	31·58

The powder into which the diammonic carbonate passes on exposure gave 21·66 per cent. ammonia, determined in ·671 grm. Theory requires for $\text{CO}^3 \text{H} (\text{NH}^4)$ the number 21·52.

XVII. On a modified Spectroscope for use in the examination of Minerals. By J. EMERSON REYNOLDS, Member of the Royal College of Physicians, Edinburgh, Keeper of the Minerals and Analyst to the Royal Dublin Society*.

THE modified spectroscope presently to be described was arranged some time ago by me, for the purpose of supplying a deficiency long felt in using the ordinary form of spectrum-apparatus.

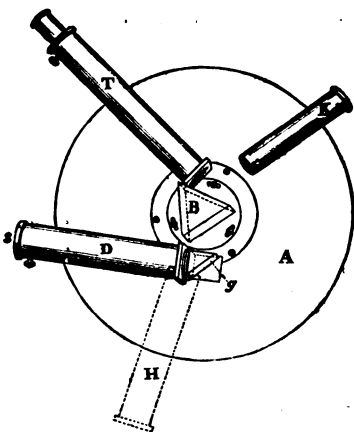
I believe all who have much employed the delicate and beautiful mode of investigation afforded by the method of spectrum-analysis, will agree with me that the ordinary single-prism arrangement is by no means well suited for use in the routine work of a chemical laboratory. When the eye of the operator is applied to the telescope of the instrument, the slit and the flame in which the test-specimen is being ignited are out of view and beyond easy control. Practice will, of course, overcome much of the difficulty attendant upon this circumstance; but for rapid and satisfactory work it is always necessary to obtain the aid of an assistant. The same objection applies with equal force to the direct-vision spectroscope of M. Hofmann, excellent though this instrument is in other respects. Every chemist knows how advantageous it is to have his apparatus entirely under his own

* Communicated by the Author, having been read before the Royal Dublin Society, December 16, 1867.

control; hence I set to work to remedy the defect above referred to, and have succeeded in arranging the spectroscope so as to admit of its use in ordinary analysis with great facility and comfort to the operator.

The accompanying woodcut represents the spectroscope as seen from above.

A is a circular table of brass surmounting a pillar and heavy foot, not shown in the drawing. In the centre of the table and on a raised metal disk the prism B is fixed securely, but the small carrier-plate admits of slight rotation round its centre in order that the adjustment of the prism may be easily effected; the latter is a glass cell filled with bisulphide of carbon, the refracting angle being 59° . The tube D is fitted to the table, and carries at one end the slit S and adjusting-screw, and at the other the collimating lens. On a little stage is fixed the reflecting prism *g*, the use of which will presently be shown. T is the observing-telescope, which has a magnifying-power of about 6; this tube is carried by a stout arm, which latter permits considerable angular motion of the telescope. K is a short tube, also moveable round the centre of the table; at one end of the tube is placed a photographed scale, and at the other a lens, with the scale in its principal focus.



In using this instrument, light from any source, after passing through the slit S and the plano-convex lens, instead of being allowed to fall directly on the first face of B, as in the ordinary instrument, is reflected at a right angle to its first path by means of the little prism *g*, and then passes through the prism B, suffering refraction and dispersion; the spectrum so produced is then observed with the telescope T. In accurate experiments, of course, it is necessary to measure the distances which intervene between the lines produced by any flame under examination: this is easily effected by adjusting the tube K; and when a candle is placed behind the photographed scale, a bright image of the latter is thrown into the telescope T by reflection from the second face of B. When in use, the instrument is covered with a japanned tin cap blackened inside, with slits cut in the sides to admit of

the passage of the tubes D, T, and K; by this means all extraneous light is prevented from reaching the instrument.

The point of novelty in this spectroscope is, in addition to some minor mechanical details, the reflection, by means of the prism *g*, of the bundle of rays transmitted by the slit at right angles to their former path, with a minimum loss of light and without the annoyance of secondary spectrum. The advantage gained is an important practical one. While using but one prism for dispersion, the eyepiece of the observing-telescope and the slit S are brought close together, so that the analyst, having his eye at the former, still has the slit, the gas-flame, and the platinum wire carrying the substance to be examined perfectly under his own control. The approximate position of the slit in the ordinary instruments is shown by the dotted line H, in which case it is scarcely within easy reach if the apparatus be of large size.

By thus increasing the facility with which the spectroscope may be used in the examination of minerals, I hope it may induce many mineral-chemists to employ it in their investigations, and thus render their observations on the constituents of minerals more accurate and in every way satisfactory.

I may conclude with a few hints as to the method which experience has shown me is most satisfactory in testing minerals with the spectroscope when the colourless flame of the Bunsen burner is used as the source of heat:—

1. Moisten the finely powdered mineral with pure water, take up some of the paste on a loop of platinum wire and slowly introduce into the flame, observing the spectrum produced, if any.

2. Moisten the same portion, or a fresh quantity if the first be lost, with a drop of pure hydrochloric acid; again subject the test-specimen to the heat of the flame, and observe. Most silicates yield up their bases easily by this treatment.

3. To the same test add a drop of a mixture of hydrofluoric and hydrochloric acids, allow the liquid to act for a few minutes, and then heat as before.

By adopting this systematic treatment, no substance which gives a characteristic spectrum under these circumstances can escape detection; and if the three tests be applied to the same specimen, the first one, by causing the volatilization of the greater part of the most volatile constituents, renders the succeeding stages more clear and precise.

Finally, in examining sulphides, it is necessary to heat a portion of the substance in a small closed tube; in the sulphur sublimate thallium is easily detected if it be present in the ore; and the residue needs but to be moistened with hydrochloric acid, and heated, for the detection of the presence of such metals as admit of easy recognition by the method of spectrum-analysis.

XVIII. *The Internal Motions of Gases compared with the Motions of Waves of Light.* By G. JOHNSTONE STONEY, M.A., F.R.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Dublin, July 14, 1868.

THE following pages are a collateral inquiry in a memoir on the Constitution of the Atmospheres of the Sun and Stars, which was sent to the Royal Society in May of last year. They are now separated, as it has appeared desirable to shorten that memoir. Could you spare space for them in the *Philosophical Magazine*? Their aim is to make quantitative estimates in regard to the internal motions of gases, which, though rough, may suffice for a preliminary comparison of these motions with the motions of the waves of light.

For the sake of perspicuity and to avoid wearisome periphrases, free use is made of the hypothesis that the lines of motion within gaseous molecules are or resemble orbits. All that we really know about them is that they are periodic, and that their periods are the same as those of the waves of light to which they give rise. But there is the same convenience in thus speaking in the language of a definite hypothesis, which has been provisionally adopted for the sake of this convenience, as there is when we speak of waves of light, its transverse vibrations, and the luminiferous æther, although we know from Lorenz's and Riemann's investigations that light may be an electrical phenomenon, to which the language of the wave-theory is very imperfectly applicable. And it should be distinctly kept in view that the numerical estimates at which we arrive, and which are our principal aim, are independent of this hypothesis.

The second part of the inquiry, in which the conditions which limit the extent of an atmosphere are sought out, will be found in the memoir spoken of above.

I am, Gentlemen,

Your obedient Servant,

G. JOHNSTONE STONEY.

1. The only class of bodies about the molecular constitution of which we have any satisfactory* information is gases. These

* The dynamical theory of the molecular constitution of gases, which, if I mistake not, may be ranked in point both of importance and probability along with the wave-theory of light, does not appear to have yet met with that general attention and acceptance which it seems to deserve. It may not be out of place, therefore, to add to the numberless proofs which have been drawn from its interpreting the phenomena of gases by many writers, but especially by Clausius, the following negative proof, which demon-

appear to consist of molecules moving about actively and irregularly in all directions, the path of any one being for the most part rectilinear, or, in other words, most of its motion being executed at a sufficient distance from the neighbouring molecules to be beyond the reach of sensible influence from them. Every now and then, however, each molecule comes sufficiently near some other molecule to have its course bent, on which occasions it darts off in a new direction. Moreover many facts in physics and chemistry lead irresistibly to the conclusion that the molecules are resolvable into simpler elements; and the probability distinctly is that each in most gases is a highly complex system. When a body so constituted is enclosed, the molecules by flinging themselves against the walls of the containing vessel produce the pressure of the gas. If the enclosure be at the same temperature as the gas, they do so without gain or loss of *vis viva*. But if the wall be at a higher temperature, the activity of those molecules which strike it is increased, and *vice versa*. The altered activity is shared with the rest of the gas by conduction and convection—or, more slowly, by conduction only, if the circumstances do not admit of convection; and so the temperature of the whole becomes changed.

2. If the temperature of the gas be raised while the gas is kept within a confined space, the vivacity of all the motions will be thus increased; but this will not affect d , the mean distance at which the molecules are spaced, and scarcely affect l , the average length to which a molecule can travel before it becomes engaged with another molecule. If, on the other hand, the gas

strates that no statical theory, whether on the hypothesis of a continuous substance or of distinct particles, is *possible*.

A gas is susceptible of enormous dilatation and compression without an abrupt change in the laws upon which its pressure depends; hence, if it consist of particles at rest, the force which acts in any direction on any one must be the result of forces emanating from many others, no one contributing more than a share, which may be regarded as infinitesimal. Hence it is easy to see that if the density be changed, the pressure will vary as the square of the density; for the force in any direction on any one particle will increase as the number of the particles on that or the opposite side (according as the elementary forces are attractive or repulsive) near enough to act on it, *i. e.* will increase as the density; and the number of particles subjected to this augmented force which are found within each element of volume will also have increased in the same proportion. Hence the pressure per square millimetre across any surface within the gas will increase as the square of the density; and as this is a law which does not exist in gases, it follows that no gas consists of distinct particles at rest. The same proof applies, by the principles of the Differential Calculus, to the hypothesis of a continuous and homogeneous substance. For this proof, given more at-large, see *Proceedings of the Royal Irish Academy*, vol. vii. (1858) p. 37.

be allowed to expand without a change of temperature, d and l become larger, while the velocities are but little changed from what they were before*. If the gas be changed for another, for instance if hydrogen be substituted for oxygen at the same temperature and pressure, the number of molecules remains the same, d also remains the same, l is probably a little altered. But the mass of each molecule is now but one-sixteenth of the mass of each molecule of oxygen; and to compensate for this, the velocities with which the molecules move are 16^2 or upwards of 250 times as swift.

3. The molecular motions consist of two very distinct parts—the motions of the molecules among one another, and the motions in the interior of each molecule. In an eminently gaseous body the former of these classes of motion engrosses a large part of the *vis viva* †; but the motions are irregular, and on this account, as well as probably because they are too coarse, they are unfitted to absorb or develope vibrations in the luminous æther, except possibly in a very small degree, just as the waving of the hand may produce a slight sound. So far, therefore, as the rectilinear motions of the molecules are concerned the gas will be eminently transparent. But each molecule is also a little separate system, the constituent parts of which are in energetic motion among themselves. These minuter motions are interfered with by the neighbouring molecules only during the instants when the path of a molecule is being deflected upon its close approach to another molecule; and as the intervening times when the path is rectilinear are long in comparison with these brief instants, the internal motions of each little system will be for the most part undisturbed and therefore regular. And accordingly we must presume that it is they which influence the æther, producing those bright lines which constitute nearly the whole spectrum of an incandescent gas, and are in a corresponding degree influenced by the æther, absorbing these same rays. How wonderfully regular the internal motions of the molecules are, and at the same time how complex, appear to be

* That the velocities will be somewhat altered seems evident when we bear in mind the great change which, when a gas expands, has been effected in the ratio of the periods of perturbation to the whole time (see next section). But it seems equally plain that the alteration of the velocities will be but small except at high temperatures, and in such gases as have lines which are much dilated by the perturbations at high temperatures. This is in conformity with the comparative fixity of the two specific heats of a gas, within the limits to which experiments have been pushed.

† Nearly two-thirds in those simple gases in which the two specific heats bear to one another their usual ratio. Considerably less than two-thirds in most compound gases, in all which suffer condensation in the act of combining. See a memoir by Clausius in the *Philosophical Magazine* for 1857, vol. xiv. p. 125.

revealed to us by the fixity of the rays in the spectrum of each gas, and by their number within the limits of the visible spectrum and the doubtless greater number that exist beyond. That the motions are, however, not altogether free from perturbation is made clear by such cases as that of the lines corresponding to D in the sodium spectrum, which are not linear except at low temperatures, but of a breadth increasing with the temperature, and ill defined at the edges. This suggests a perturbation of motions that would otherwise be regular by the disturbing influence exerted by other molecules when they are near enough. If during some of the appulses the period of a vibration be somewhat accelerated, by others somewhat retarded, and if in all cases the vibration tend to settle down from the constrained towards its normal period during the undisturbed intervals between contacts, then will the effect be such as we see. If the irregularity fade away so readily as to last but a trifling part of the interval between the collisions, the line in the spectrum will be sharply defined—as in the sodium-line in a spirit-lamp, or the thallium-line in the flame of a Bunsen's burner. If it continue, though with a marked decrease of intensity, through a considerable part of the time during which the molecule is pursuing a straight course, the spectral line will have a sensible breadth and hazy edges, as in the sodium-line of a Bunsen's burner. And if the commotion be so violent that but little restoration can be effected before a fresh contact and a fresh perturbation, the spectrum of a gas may even become continuous. All the stages up to that of a continuous and nearly uniform spectrum can, for example, be produced in hydrogen. It has been observed, too, that rays of high refrangibility are more easily dilated into spectral bands of the class with which we are here dealing than rays of low refrangibility*. Perhaps this may arise from the circumstance that a ray of high refrangibility is due to an atom of the molecule the periodic time of whose motion is shorter than those of other motions which give rise to rays of low refrangibility; this atom, therefore, goes through many more of its little evolutions in the instant while a passing molecule is near enough to affect it, and is likely to suffer a proportionately more serious disturbance. If we have been right in assigning the cause of this class of dilated spectral lines, it is impossible to resist the impression that cases will sometimes arise in which the periodic time of an atom of a molecule will yield to a force tending to disturb it more freely in one direction than in the other, and will thus produce a ray expanding on one side

* This, however, is far from being uniformly the case; and there are even instances of gases, as, for example, oxygen, in which the dilatation first takes place at the red end of the spectrum.

only, or more on one side than on the other—such as is the wing on one side only of the narrowest of the magnesium lines which form the group *b* in the solar spectrum.

4. Another and quite different case which probably exists is where, in virtue of forces acting within the molecule, the periodic time of a motion within it is subject to fluctuation, going through a regular cycle of changes over and over again. In such a case, instead of a line in the spectrum we should find a band of definite breadth, and usually with well-defined edges, as in the spectra of silicon and zinc. It may be observed that this kind of fluctuation, unlike that which is attributable to a disturbing agent external to the molecule, seems quite as likely to affect motions of long as motions of short period; and accordingly the species of bands of which it is probably the cause are met with indifferently in all parts of the spectrum. Such bands are not unfrequent; and, in fact, the motions within a molecule are usually so multifarious that we often find evidence in the spectrum of a single gas of the coexistence of all the various classes of motions.

5. It is a consequence of the laws that regulate the exchange of heat, that the undulations of the æther excited by the motions of any one particular range of periods within a body can only at most appropriate to themselves a definite amount of *vis viva*, which amount is a function of θ and τ , where θ is the temperature of the body, and τ the period of vibration. Its symbolical expression would be of the form $\iint \phi(\theta\tau) d\tau dt$, where dt is the element of time, θ constant, and τ to be taken between the limits corresponding to the breadth of the spectral line or band under consideration. This puts a major limit beyond which the brightness of any particular ray of a gas at a given temperature cannot pass. But the intensities of the vibrations corresponding to the bright rays may, and generally do, in various degrees fall short of the maximum values thus fixed; or, in other words, the quantity of gas experimented on is usually too small to be absolutely opaque in reference to any of its rays, and is commonly very unequally opaque in reference to the several rays. This opacity, and consequently the energy of the orbital motions upon which it depends, is often very disproportionately affected by changes in the general *vis viva* of the system—so much so that rays which are inconspicuous as compared with others in their neighbourhood at one temperature, may become the most prominent rays at a more elevated temperature, as in iron. It sometimes happens, too, that under circumstances which have not yet been sufficiently studied, and which are but little understood, but of which temperature is one, the whole character of the motions changes—so that below a particular temperature they give rise to

one system of rays, and above that temperature to a wholly different system, as, for example, in nitrogen*.

6. When we compare different gases, we find that their molecules differ both in mass and in the motions that prevail within them. That the internal motions differ is abundantly testified by the amazing variety in the grouping of the spectral lines to which the various gases give rise. And as the number of molecules per cubic millimetre is known to be the same in all perfect gases when taken at the same temperature and pressure, it follows that the masses of the molecules are in most simple gases proportional to what chemists call their atomic weights, and, in those instances in which this is not the case, that they stand in the same simple relation to these atomic weights as the densities of the gases nearly do.

7. Very accurate measures have been effected of several of the minute magnitudes concerned in the phenomena of light; but these are the only determinations yet made in molecular physics upon which we can fully rely. Nevertheless we are not left to mere conjecture in regard to the other quantities with which we have to deal; for by many indirect methods it is possible to estimate their dimensions, not indeed with precision, but yet so as to indicate unmistakably whereabouts they lie, and, as it were, to introduce us into their neighbourhood. Such loose determinations are of great use, from their enabling us to form just conceptions of the kind of magnitudes of which we talk, and so guarding us against many errors.

8. The molecules of a gas are darting about with very different velocities; but at each temperature there is a certain mean velocity towards which the collisions that prevail within the gas tend to bring velocities which deviate from it†, and round which accordingly the innumerable velocities existing at any moment group themselves. This mean velocity Clausius succeeded in estimating at about

$$485 \sqrt{\frac{T}{273 \cdot \rho}} \text{ metres per second } \ddagger,$$

where T is the absolute temperature estimated in Centigrade degrees, and ρ the relative specific gravity of the gas compared

* The phenomena of nitrogen very explicitly declare that temperature is but one condition, and that it may be overruled by other circumstances. The lines withdrawn from the solar spectrum by the cold nitrogen of our atmosphere all (so far as they are known) belong to the nitrogen spectrum "of the second order," as it is called by Plücker—that spectrum which, in his experiments, was only obtained at the higher temperatures.

† The mean velocity so defined is not the arithmetic mean of the values of v , but the square root of the arithmetic mean of the values of v^2 .

‡ Phil. Mag. 1857, vol. xiv. p. 124,

with air. Again, Professor Maxwell has deduced from observations on the viscosity of gases, and also from the rate of diffusion of olefiant gas into air, the mean length of the path which a molecule of air describes between successive collisions. It is, at the temperature of 15°C . and the pressure of an atmosphere, about

7 eighth-metres*,

using the term eighth-metre as a convenient abbreviation of the $\frac{1}{10^8}$ th part of a metre†. This is about the ninth part of the

* The length of the mean excursions of the molecules deduced by Maxwell from a determination of the coefficient of viscosity of air which Stokes obtained from Baily's pendulum-experiments is 5·7 VIIIth-metres (Phil. Mag. 1860, vol. xix. p. 32).

From an experiment of Graham's on the rate of the diffusion of olefiant gas (a gas of nearly the same specific gravity as air) into air, he deduces the value 6·5 VIIIth-metres (Phil. Mag. 1860, vol. xx. p. 31).

Recent experiments by Maxwell himself on the viscosity of air (Phil. Trans. 1866, p. 258) assign the value 10·6 VIIIth-metres.

These determinations are very different; but even greater discrepancies would be of little moment where our object is merely to gain some insight into the order of magnitudes with which we are dealing.

The mean of the values obtained by the two methods is 7·3 VIIIth-metres.

† I here venture to employ a nomenclature for large multiples and sub-multiples which I have found so convenient that I think I may safely recommend it.

In this nomenclature the successive decimal subdivisions of the metre are:—

the decimetre,
the centimetre,
the millimetre,
the fourth-metre, IVth-metre, or IVth m.
the fifth-metre, Vth-metre, or Vth m.
and so on;

a fourth-metre meaning a metre divided by 10^4 , and so of the others. Thus microscopical quantities can be conveniently measured in fifth-metres, the Vth-metre or metre divided by 10^5 being rather more than the diameter of a corpuscle of human blood; the wave-lengths of light in VIIIth-metres, as they all lie between 39 and 77 VIIIth-metres; and so on. A fourth-metre is about the thickness of a sheet of medium writing-paper.

Again, a sixteenth-second of time is to be understood to mean a second divided by 10^{16} . This is the period in which the visible vibrations of light are to be measured—the shortest violet double vibration occupying rather more than 13 XVIth-seconds, and the longest red about 25·6 XVIth-seconds.

So, again, decimals may be conveniently spoken of as fifth-units, ninth-units, &c., or, as I would suggest, for euphony, and since there is so frequently occasion to speak of decimals, as fifth-eins, ninth-eins, &c., using the affix *-ein* as equivalent to *unit*. Thus the coefficient of the dilatation of platinum is 8·56 VIth-eins, that of zinc 29·4 VIth-eins, and those of most other metals between these limits. These measures are equivalent to

wave-length of the ray D. From this value and from the mean velocity* of the molecule, it follows that its path is deflected about

7 unit-nines of times in a second,

and accordingly that the mean duration of each of its little rectilinear excursions is about

14 eleventh-seconds,—

meaning by a unit-nine a unit multiplied by 10^9 , and by an eleventh-second a second of time divided by 10^{11} .

This fragment of time, tiny as it is, is nevertheless more than 50,000 times the vastly shorter period that suffices for a double vibration of red light, and than 100,000 times the duration of a double vibration of the extreme violet ray. And as the periods of all the motions within a molecule which give rise to visible

0·000,008,56 and 0·000,029,4. They may be further abbreviated into 8·56 VIth and 29·4 VIth.

A similar and equally convenient nomenclature for multiples is found in the use of affixes. Thus the decimal multiples of the metre are—

the decametre,
the hectometre,
the kilometre,
the metre-four, metre IV, or m. IV,
the metre-five, metre V, or m. V,
and so on ;

a metre-five meaning, of course, a metre multiplied by 10^5 .

Thus the mean distance of the sun is 14·74 metre-tens, which is equivalent to 147,400,000,000 metres. The distances of the fixed stars are to be measured in metre-sixteens, the distance corresponding to a parallax of 1" being 3·04 m. XVI, which means the same as 30,400,000,000,000 kilometres. Light takes about 1·06 year to travel a metre-sixteen, using Foucault's determination of the velocity of light, viz. 29·8 m. VII. (metre-sevens) per second.

So, again, using the term ohmad to designate the absolute electromagnetic unit of electrical resistance referred to the metre and second, the B.A. unit is the ohmad-seven, or ohmad multiplied by 10^7 . Resistances will be conveniently measured in ohmad-sevens or ohmad-nines, insulation in ohmad-XIII or ohmad-XV. If this nomenclature were adopted, it would keep constantly in view how the measures we may in practice find it most convenient to use are related to one another and to the absolute unit.

So, again, high numbers may be as conveniently dealt with as ordinary numbers. Thus the eye receives per second 5 unit-fourteens, or 5·XIV, of waves of yellow light. This number is the same as 5×10^{14} .

It is an advantage of this nomenclature that it is equally adapted to any language, and is equally intelligible in all.

* The mean velocity for this purpose ought to be the arithmetic mean of the values of v , which would be somewhat less than the mean given above ; but the ratio of the two means is such (nearly 11 to 12 according to Maxwell, see Phil. Mag. 1860, vol. xix. p. 23) that to use one for the other cannot prejudice an inquiry like the present, which does not admit of or attempt exactness.

spectral rays must lie between these limits, we are forced to conclude that, on the average, something like from fifty to one hundred thousand of these little* orbital revolutions are executed between two consecutive collisions. It is now no longer surprising that the temporary perturbation caused while two molecules are whirling past each other, has, in many instances, had abundant time to pass away early in the interval between two collisions, so as to leave the greater part of the minute internal motions to be executed in the undisturbed manner which the definiteness of the spectral lines attests to us. We come also to see how the motions with which the molecules dart about amongst one another cannot produce or intercept light; in fact they are far too sluggish—just as the motions of our fingers, or a very gentle waving of the hand, do not produce sound.

9. Again, Clausius has shown† that it is consistent with what we know of the most perfect gases to suppose d , the mean distance between the molecules, to be about $\frac{1}{60}$ of l , the length of

* It is very difficult, and at the same time it is of importance, because it guards us from error, to appreciate in some degree such amazingly minute intervals of time as those with which we are here concerned.

The double vibrations of visible light are executed in periods of time which range from 1.3 to 2.6 XVth-seconds. These swift little motions are accordingly related to a fifteenth-second in somewhat the same way as the motions of our limbs are to a second of time. Now the XVth-second is the same portion of a second that a second is of upwards of thirty millions of years. Hence the motions of light bear the same relation to one second of time which the motions of our limbs bear to that almost inconceivable cosmical period—a vast succession of geological ages, during which several races of animals, culminating in man, have appeared, have lasted long, and have finally perished upon our globe.

When the study of nature has guided our thoughts into such a region as this, our minds can scarce resist the impulse to wander outside the domain of science into the adjoining precincts of imagination, and to speculate that if there were sentient beings with bodies that move as deftly as this æther, and with thoughts and perceptions as quick as their bodies are active, there would be sufficient time for them within a small fragment of one second to live the lives of all the generations of men that have dwelt upon this earth, thinking all their thoughts and doing all their acts. The mind is almost carried beyond itself in the contemplation of such periods.

It is from the vastness of a second in reference to light, that with its little waves light can travel such an immense distance as 298 millions of metres in that period of time. It puts a strain upon the mind to form a conception of the mere moment of time that suffices for the light of a candle, at this unheard-of pace, to travel one hand-breadth from the flame; but brief as this instant is, it corresponds to what several weeks would be in reference to our movements: the light has had time to execute hundreds of thousands of its tiny vibrations; and it is no wonder if it has already lost all trace of everything that is fleeting or irregular in the disturbance originally created by the incandescent matter of the flame. Accordingly man has never detected any peculiarity in the vibration when examined in a situation which he calls close to its origin.

† Pogg. *Ann.* 1858, vol. iii. p. 251; or *Phil. Mag.* 1859, vol. xvii. p. 89.

the mean excursion of a molecule between its collisions; and it is, at all events, not likely that it is *much* more than $\frac{1}{60}$ in the gases experimented on by Professor Maxwell. Hence the mean distance at ordinary temperatures and pressures is probably of the same order as a ninth-metre, which is the millionth part of a millimetre. It is therefore probable that there are not fewer than something like a unit-eighteen of molecules in each cubic millimetre of a gas at ordinary temperatures and pressures*.

XIX. *On Geological Time, and the probable Date of the Glacial and the Upper Miocene Period.* By JAMES CROLL, of the Geological Survey of Scotland.

[Continued from vol. xxxv. p. 384.]

IT will not do, however, to measure marine denudation by the rate at which the sea is advancing on the land. There is no relation whatever between the rate at which the sea is *advancing* on the land and the rate at which the sea is *denuding* the land. For it is evident that as the subaërial agents bring the coast down to the sea-level, all that the sea has got to do is simply to advance, or at most to remove the loose materials which may lie in its path. The amount of denudation which has been effected by the sea during past geological ages, compared with what has been effected by subaërial agency, is evidently but trifling. Denudation is not the proper function of the sea. The great denuding agents are land-ice, frost, rain, running water, chemical agency, &c. The proper work which belongs to the sea is the transporting of the loose materials carried down by the rivers, and the spreading of these out so as to form the stratified beds of future ages.

We have thus seen that geology, alike with physics, is opposed to the idea of an unlimited age to our globe. And it is perfectly plain that if there be physical reasons, as there certainly are, for limiting the age of the earth to something less than 100 millions of years, geological phenomena, when properly interpreted, do not offer any opposition.

Perhaps one of the things which has tended to mislead on this point is a false analogy which is supposed to subsist between

* Hence we may see how entirely remote from a state of emptiness that which usually passes under the name of a vacuum chamber really is. If there be a unit-eighteen of molecules in every cubic millimetre of the air about us, there will remain a unit-XV in every cubic millimetre of the best vacuums of our ordinary air-pumps. The molecules are still closely packed, within about an eighth-metre of one another; i. e. there are about sixty of them in a wave-length of orange light.

astronomy and geology, viz. that geology deals with unlimited *time*, as astronomy deals with unlimited *space*. A little consideration, however, will show that there is not much analogy between the two cases.

Astronomy deals with the countless worlds which lie spread out in the boundless infinity of space ; but geology deals with but one world. No doubt both reason and analogy are favourable to the idea that the age of the material universe, like its magnitude, is immeasurable—although, however, we have no reason to conclude that it is eternal, any more than we have to conclude that it is infinite. But when we compare the age of the material universe with its magnitude, we must not take the age of one of its members (say, our globe) and compare it with the size of the universe. More than this, we must not take the age of all the presently existing systems of worlds and compare their age with the magnitude of the universe ; but we must take the past history of the universe as it stretches back into the infinity of bygone *time*, and compare it with the presently existing universe as it stretches out on all sides into the infinity of *space*. For worlds precede worlds in time as worlds lie beyond worlds in space. Each world, each individual, each atom is evidently working out a final purpose, according to a plan prearranged and predetermined by the Divine Mind from all eternity. And each world, like each individual, when it serves the end for which it was called into existence, disappears to make room for others. This is the grand conception of the universe which naturally impresses itself on every thoughtful mind that has not got into confusion about those things called in science the *Laws of Nature*.

But the geologist does not pass back from world to world as they stand related to each other in the order of *succession in time*, as the astronomer passes from world to world as they stand related to each other in the order of *coexistence in space*. Besides, the researches of the geologist are not only confined to one world, but it is only a portion of the history of that one world that can come under his observation. The oldest of existing formations, so far as is yet known, the Laurentian Gneiss, are made up of the waste of previously existing rocks, and these, again, probably of the waste of rocks still prior. Regarding what succeeds these old Laurentians, geology tells us much ; but regarding what preceded, we know nothing whatever. For anything that geology shows to the contrary, the time which may have elapsed from the solidifying of the earth's crust to the deposition of the Laurentians—an absolute blank—may have been as great as the time that has elapsed since then till the present day. Physical science limits the age of the globe to a

period not exceeding something like 100 millions of years. Taking the age of the earth's crust at 100 millions of years, which probably is a high estimate, how long is it since the materials which now form these Laurentian rocks were laid down in the ancient sea-bottom in the form of sand and mud? This is a question which no one can answer; for we have no means of knowing how much of the 100 millions of years were exhausted before the Laurentian age. At all events, it must be very considerably less than 100 millions of years since the commencement of the Laurentian period.

Sir William Logan thinks that the time which separated the limestones containing the *Eozoon Canadense* from the Upper-Cambrian period may be as great as the time which elapsed between the Upper Cambrian and the nummulitic limestones of the Tertiary period. If this conjecture be anything like correct, then it is hardly possible that 50 millions of years can have elapsed since the Cambrian period. Assuming that the glacial epoch began about a quarter of a million years ago, and that the rate at which species change is uniform, we found that, adopting Sir Charles Lyell's mode of calculation, 60 millions of years have probably elapsed since the beginning of the Cambrian period. But I presume little or no weight can be placed on this mode of calculation; for it is based upon an assumption for which there is, I fear, very little warrant, viz. that the rate at which species change has been anything like uniform during geological ages. If any very great amount of time elapsed between the solidifying of the earth's crust and the Laurentian period, the probability is that the commencement of the Cambrian period does not date so far back as 60 millions of years.

Table I. shows the excentricity of the earth's orbit and longitude of the perihelion for 3 millions of years back, and 1 million of years to come, at periods fifty thousand years apart. From what we have already seen, it is very evident that 3 millions of years must stretch a considerable distance back into the geological history of our globe. And if geological climate depends upon, or is much affected by, the condition of the earth's orbit as regards excentricity, we have in this Table, combined with the other three, the means by which a rough idea of the character of the climate during at least a considerable portion of the Tertiary period may be arrived at.

TABLE I.

Number of years before A.D. 1800.	Excentricity of orbit.	Longitude of perihelion.	Number of years before and after A.D. 1800.	Excentrici t of orbit.	Longitude of perihelion.
-3,000,000	0-0365	39 30	-1,000,000	0-0151	248 22
-2,950,000	0-0170	210 39	- 950,000	0-0517	97 51
-2,900,000	0-0442	200 52	- 900,000	0-0102	135 2
-2,850,000	0-0416	0 18	- 850,000	0-0747	239 28
-2,800,000	0-0352	339 14	- 800,000	0-0132	343 49
-2,750,000	0-0326	161 22	- 750,000	0-0575	27 18
-2,700,000	0-0330	65 37	- 700,000	0-0220	208 13
-2,650,000	0-0053	318 40	- 650,000	0-0226	141 29
-2,600,000	0-0660	190 4	- 600,000	0-0417	32 34
-2,550,000	0-0167	298 34	- 550,000	0-0166	251 50
-2,500,000	0-0721	338 36	- 500,000	0-0388	193 56
-2,450,000	0-0252	109 33	- 450,000	0-0308	356 52
-2,400,000	0-0415	116 40	- 400,000	0-0170	290 7
-2,350,000	0-0281	308 23	- 350,000	0-0195	182 50
-2,300,000	0-0238	195 25	- 300,000	0-0424	23 29
-2,250,000	0-0328	141 18	- 250,000	0-0258	59 39
-2,200,000	0-0352	307 6	- 200,000	0-0569	168 18
-2,150,000	0-0183	307 5	- 150,000	0-0332	242 56
-2,100,000	0-0304	98 40	- 100,000	0-0473	316 18
-2,050,000	0-0170	334 46	- 50,000	0-0131	50 14
-2,000,000	0-0138	324 4	A.D. 1800. 0	0-0168	99 30
-1,950,000	0-0427	120 32	+ 50,000	0-0173	38 12
-1,900,000	0-0336	188 31	+ 100,000	0-0191	114 50
-1,850,000	0-0503	272 14	+ 150,000	0-0353	201 57
-1,800,000	0-0334	354 52	+ 200,000	0-0246	279 41
-1,750,000	0-0350	65 25	+ 250,000	0-0286	350 54
-1,700,000	0-0085	95 13	+ 300,000	0-0158	172 29
-1,650,000	0-0035	168 23	+ 350,000	0-0098	201 40
-1,600,000	0-0305	158 42	+ 400,000	0 0429	6 9
-1,550,000	0-0239	225 57	+ 450,000	0-0231	98 37
-1,500,000	0-0430	303 29	+ 500,000	0-0534	157 26
-1,450,000	0-0195	57 11	+ 550,000	0-0259	287 31
-1,400,000	0-0315	97 35	+ 600,000	0-0395	285 43
-1,350,000	0-0322	293 38	+ 650,000	0-0169	144 3
-1,300,000	0-0022	0 48	+ 700,000	0-0357	17 12
-1,250,000	0-0475	105 50	+ 750,000	0-0195	0 53
-1,200,000	0-0289	239 34	+ 800,000	0-0639	140 38
-1,150,000	0-0473	250 27	+ 850,000	0-0144	176 41
-1,100,000	0-0311	55 24	+ 900,000	0-0659	291 16
-1,000,000	0-0326	4 8	+ 950,000	0-0086	115 13
			+1,000,000	0-0528	57 31

On looking over Table I., it will be seen that there are three principal periods when the excentricity rose to a very high value, with a few subordinate maxima between. It will be perceived also that during each of those periods the excentricity does not remain at the same uniform value, but rises and falls, in one case twice, and in the other two cases three times. About 2,650,000 years back we have the excentricity almost at its inferior limit. It then begins to increase, and fifty thousand years afterwards, namely at 2,600,000 years ago, it reaches $\cdot 0660$; fifty thousand years after this period it has diminished to $\cdot 0167$, which is about its present value. It then begins to increase, and in another fifty thousand years, viz. at 2,500,000 years ago, it approaches to almost the superior limit, its value being then $\cdot 0721$. It then begins to diminish, and at 2,450,000 years ago it has diminished to $\cdot 0252$. These two maxima, separated by a minimum and extending over a period of 200,000 years, constitute the first great period of high excentricity. We then pass onwards for upwards of a million and a half years, and we come to the second great period. It consists of three maxima separated by two minima. The first maximum occurred at 950,000 years ago, the second or middle one at 850,000 years ago, and the third and last at 750,000 years ago—the whole extending over a period of nearly 300,000 years. Passing onwards for another million and half years, or to about 800,000 years in the future, we come to the third great period. It also consists of three maxima one hundred thousand years apart. These occur at the periods 800,000, 900,000, and 1,000,000 years to come, respectively, separated also by two minima. Those three great periods, two of them in the past and one of them in the future, included in the Table, are therefore separated from each other by an interval of upwards of 1,700,000 years.

In this Table there are three periods when the excentricity approaches to about its inferior limit, and the orbit becomes almost circular. The first is 2,650,000 years ago, when the excentricity was $\cdot 0053$; the next is at 1,300,000 years ago, when it was only $\cdot 0022$; and the next will occur in about 24,000 years hence, when its value will be $\cdot 0033$. There is, therefore, an interval of 1,350,000 years between the minima in the one case, and an interval of 1,324,000 years in the other case.

The Table shows also four subordinate periods of high excentricity, of which the one that I have supposed corresponds to the glacial epoch is the chief. Three are in the past, and one in the future.

If the glacial epoch resulted from a high state of excentricity, there must have been at least five ice-periods, including the glacial. *Phil. Mag.* S. 4. Vol. 36. No. 241. Aug. 1868. L

cial epoch, during the past three millions of years—two as severe, and two much less severe than the glacial epoch.

It may be thought that so many as four or five ice-periods in the course of 3 millions of years past is inconsistent with the evidence of geology on that point. This, however, is at least very doubtful. It is quite possible that these three millions of years may embrace the greater part of the Tertiary period. Now we have evidence of at least three ice-periods since the beginning of the Tertiary period—one about the middle of the Eocene period, another during the Upper Miocene period, and the third and last the well-known glacial epoch*; and it is quite possible that the evidence of more may yet be found.

But before discussing the nature of the evidence which geology affords of the existence of former glacial periods, we shall turn our attention briefly to Tables II., III., and IV. These three Tables embrace the three periods of greatest excentricity during the past 3 millions of years. The excentricity, longitude of the perihelion, &c. are given at periods of ten thousand years apart.

There are still eminent astronomers and physicists who are of opinion that the climate of the globe never could have been seriously affected by changes in the excentricity of its orbit. This opinion results, no doubt, from viewing the question as a purely astronomical one. Viewed from an astronomical standpoint, as has been already remarked, there is actually nothing from which any one could reasonably conclude with certainty whether a change of excentricity would seriously affect climate or not. By means of astronomy we ascertain the extent of the excentricity at any given period, how much the winter may exceed the summer in length (or the reverse), how much the sun's heat is increased or decreased by a decrease or an increase of distance, and so forth; but we obtain no information whatever regarding how these will actually affect climate. This must be determined wholly from physical considerations, and it is an exceedingly complicated problem. An astronomer, unless he has given special attention to the physics of the question, is just as apt to come to a wrong conclusion as any one else. The question involves certain astronomical elements; but when these are determined, everything then connected with the matter is purely physical. Nearly all the astronomical elements of the question are comprehended in the accompanying Tables.

In Tables II., III. and IV., column I. represents the dates of the periods, column II. the excentricity, column III. the

* See Lyell's 'Principles,' vol. i. chap. x. (tenth edition).

longitude of the perihelion. In Table IV. the excentricity and the longitude of the perihelion of the six periods marked with an S are copied from a letter of Mr. Stone to Sir Charles Lyell, published in the Supplement of the Phil. Mag. for June 1865; the eight periods marked L are copied from M. Leverrier's Memoir in the *Connaissance des Temps* for 1843. For the correctness of everything else, both in this Table and in the other three, I alone am responsible.

Column IV. gives the number of degrees passed over by the perihelion during each 10,000 years. From this column it will be seen how irregular is the motion of the perihelion. At four different periods it had a retrograde motion for 20,000 years. Column V. shows the number of days by which the winter exceeds the summer when the winter occurs in aphelion. Column VI. shows the intensity of the sun's heat during midwinter, when the winter occurs in aphelion, the present midwinter intensity being taken at 1000. These six columns comprehend all the astronomical part of the Tables. Regarding the correctness of the principles upon which these columns are constructed, there is no diversity of opinion. But these columns afford no direct information as to the character of the climate, or how much the temperature is increased or diminished. To find this we pass on to columns VII., VIII. and IX., calculated on physical principles. Now, unless the physical principles upon which these three columns are calculated be wholly erroneous, undoubtedly change of excentricity must very seriously affect climate. Column VII. shows how many degrees Fahrenheit the temperature is lowered by a decrease in the intensity of the sun's heat corresponding to column VI. For example, 850,000 years ago, if the winters occurred then in aphelion, the direct heat of the sun during midwinter would be only $\frac{837}{1000}$ of what it is at present at the same season of the year. Column VII. shows that this decrease in the intensity of the sun's heat would lower the temperature $45^{\circ}.3$ F.

The principle upon which this result is arrived at is this:—The temperature of space, as determined by Sir John Herschel, is -239° F. M. Pouillet, by a different method, arrived at almost the same result. Taking the midwinter temperature of Great Britain at 39° , consequently, $239^{\circ} + 39^{\circ} = 278^{\circ}$ represent the number of degrees of rise due to the sun's heat at midwinter; in other words, it takes a quantity of sun-heat which we have represented by 1000 to maintain the temperature of the earth's surface at Great Britain 278° above the temperature of space. Were the sun extinguished, the temperature of our island would sink 278° below its present midwinter temperature, or to the temperature of space. But 850,000 years ago, if the

TABLE II.

I.	II.	III.	IV.	Winter occurring in aphelion.					IX.
Number of years before a.n. 1800.	Excentricity of orbit.	Longitude of perihelion.	Number of degrees passed over by the perihelion. Motion retrograde at periods marked R.	V.	VI.	VII.	VIII.	Midwinter temperature, the Gulf-stream being diminished in proportion to the excentricity.	
				Excess of winter over summer, in days.	Midwinter intensity of the sun's heat. Present intensity = 1000.	Number of degrees by which the midwinter temperature is lowered.	Midwinter temperature.		
						F.	F.	F.	
2,650,000	0.0053	318 40	95 45	15.4	906	26.2	12.8	6.6	
2,640,000	0.0173	54 25	39 12			33.3	5.7	— 3.3	
2,630,000	0.0331	93 37	33 35	22.2	884	38.3	0.7	— 10.5	
2,620,000	0.0479	127 12	31 24	27.4	862	41.5	— 2.5	— 15.0	
2,610,000	0.0591	158 36	31 28	30.6	851	41.8	— 2.8	— 15.4	
2,600,000	0.0660	190 4	30 24	30.9	850	39.2	— 0.2	— 11.7	
2,590,000	0.0666	220 28	29 28	28.3	859	33.9	5.1	— 4.2	
2,580,000	0.0609	249 56	27 28	22.9	878	27.1	11.9	5.3	
2,570,000	0.0492	277 24	27 38	16.2	902				
2,560,000	0.0350	305 2	R 6 28						
2,550,000	0.0167	298 34	R 44 36						
2,540,000	0.0192	253 58	5 21						
2,530,000	0.0369	259 19	23 48	17.1	899	28.0	11.0	4.0	
2,520,000	0.0537	283 7	26 57	25.0	871	35.9	3.1	— 7.0	
2,510,000	0.0660	310 4	28 32	30.6	851	41.5	— 2.5	— 15.0	
2,500,000	0.0721	338 36	28 32	33.5	841	44.2	— 5.2	— 18.8	
2,490,000	0.0722	7 36	29 0	33.6	841	44.3	— 5.3	— 19.0	
2,480,000	0.0662	35 46	28 10	30.8	850	41.7	— 2.7	— 15.2	
2,470,000	0.0553	63 26	27 40	25.7	868	36.6	2.4	— 8.1	
2,460,000	0.0410	89 13	25 47	19.1	892	30.0	9.0	1.3	
2,450,000	0.0252	109 33	20 20	11.7					

TABLE III.

1,000,000	0-0151	248 32	65 28	153	906	26.1	13.9	6.7
990,000	0-0224	313 50	44 13	20.5	887	31.5	7.5	-0.4
980,000	0-0329	358 2	34 38	22.8	878	33.8	5.2	-4.1
970,000	0-0441	33 40	34 9	24.0	874	35.0	4.0	-5.8
960,000	0-0491	66 49	31 2	23.0	878	34.0	5.0	-4.3
950,000	0-0517	97 51	29 51	19.7	890	30.6	8.4	0.4
940,000	0-0195	127 42	28 39	14.2	910	25.0	14.0	8.2
930,000	0-0423	156 11	25 39					
920,000	0-0305	181 40	12 35					
910,000	0-0156	194 15	R 59 13					
900,000	0-0102	135 2	R 8 1					
890,000	0-0285	127 1	25 32	21.2	884	32.2	6.8	-2.0
880,000	0-0456	152 33	27 50	28.2	859	39.0	0.0	-11.5
870,000	0-0607	180 23	29 18	32.9	843	43.6	-4.6	-18.0
860,000	0-0708	209 41	29 47	34.7	837	45.3	-6.3	-20.3
850,000	0-0747	239 28	29 46	32.4	845	43.2	-4.2	-17.4
840,000	0-0698	269 14	29 14	29.0	857	40.0	-1.0	-12.8
830,000	0-0623	298 28	27 36	23.1	881	33.1	5.9	-3.1
820,000	0-0476	326 4	22 26					
810,000	0-0296	348 30	R 4 41					
800,000	0-0132	343 49	R 50 30					
790,000	0-0171	293 19	10 18	15.2	907	26.0	13.0	6.7
780,000	0-0325	303 37	25 1	21.2	884	32.2	6.8	-2.0
770,000	0-0455	328 38	28 34	25.1	870	36.0	3.0	-7.2
760,000	0-0540	357 12	30 6	26.7	864	37.7	1.3	-9.5
750,000	0-0575	27 18	31 12	26.1	867	37.0	2.0	-8.6
740,000	0-0561	58 30	32 25	23.6	878	34.6	4.4	-5.2
730,000	0-0507	90 55	34 19	19.6	890	30.6	8.4	0.4
720,000	0-0422	125 14	52 12	14.3	910	25.0	14.0	8.2
710,000	0-0307	177 26	30 47					
700,000	0-0220	208 13						

TABLE IV.

I. Number of years before A.D. 1800.	II. Excentricity of orbit.	III. Longitude of perihelion.	IV. Number of de- grees passed over by the perihelion. Motion retro- grade at periods marked R.	Winter occurring in aphelion.				IX. Midwinter tempe- rature, the Gulf- stream being dimi- nished in propor- tion to the exen- tricity.
				V. Excess of winter over summer, in days.	VI. Midwinter inten- sity of the sun's heat. Present in- tensity = 1000.	VII. Number of degrees by which the mid- winter temperature is lowered.	VIII. Midwinter tempe- rature.	
250,000	0.0258	59 39'	15 19'	17.4	898	F. 28.3	F 10.7	F. 3.7
240,000	0.0374	74 58	27 51	22.2	885	33.2	5.8	-3.2
S 230,000	0.0477	102 49	21 44	23.2	877	34.1	4.9	-4.6
S 220,000	0.0497	124 33	20 22	26.7	864	37.7	1.3	-9.5
S 210,000	0.0575	144 55	23 23	26.5	865	37.4	1.6	-9.1
200,000	0.0569	168 18	21 46	24.7	871	35.7	3.3	-6.7
S 190,000	0.0532	190 4	19 18	22.1	881	33.1	5.9	-3.1
S 180,000	0.0476	209 22	18 45	20.3	887	31.3	7.7	-0.6
S 170,000	0.0437	228 7	8 31	16.9	900	27.8	11.2	4.3
160,000	0.0364	236 38	6 18	15.4	905	26.2	12.8	6.5
150,000	0.0332	242 56	3 33	16.1	903	26.9	12.1	5.6
140,000	0.0346	246 29	13 5	17.8	896	28.8	10.2	3.0
130,000	0.0384	259 34	15 13	20.1	888	31.0	8.0	-0.1
120,000	0.0431	274 47	19 1	21.4	883	32.4	6.6	-2.1
110,000	0.0460	293 48	22 30	22.0	881	33.0	6.0	-2.9
L 90,000	0.0473	316 18	23 44	21.0	885	32.0	7.0	-1.5
L 80,000	0.0398	340 2	24 11	18.5	894	29.4	9.6	2.1
L 70,000	0.0316	4 13	23 9	14.7	908	25.5	13.5	7.5
L 60,000	0.0218	27 22	18 46					
L 50,000	0.0131	46 8	4 6					
L 40,000	0.0109	50 14	R 21 38					
L 30,000	0.0151	28 36	R 22 46					
L 20,000	0.0188	5 50	38 10					
L 10,000	0.0187	44 0	34 28					
0	0.0168	78 28	21 2					
		99 30						

winters occurred in aphelion, the heat of the sun at midwinter would only equal 837 instead of 1000 as at present. Consequently, if it takes 1000 parts of heat to maintain the temperature 278° above the temperature of space, 837 parts of heat will only be able to maintain the temperature $232^{\circ}\cdot7$ above the temperature of space; for $232^{\circ}\cdot7$ is to 278 as 837 is to 1000. Therefore, if the temperature was then only $232^{\circ}\cdot7$ above that of space, it would be $45^{\circ}\cdot3$ below what it is at present.

This method of calculating how much the temperature is lowered by a given reduction of the sun's heat is that given by Sir John Herschel in his 'Outlines of Astronomy,' § 369 *a*. About three years ago, in an article in 'The Reader,' I endeavoured to show that this method is not rigidly correct. It results from the principles of the dynamical theory of heat, and is also supported by experiments made by Professor Draper, of New York, and others, that the rate at which a body radiates its heat off into space is not directly proportionate to its absolute temperature. The rate at which a body loses its heat as its temperature rises, increases more rapidly than the temperature. As a body rises in temperature, the rate at which it radiates off its heat increases; but the *rate* of this increase is not uniform, but increases with the temperature; consequently the temperature is not lowered in proportion to the decrease of the sun's heat. But this error is probably neutralized by one of an opposite nature, to which I shall now refer.

We know that absolute zero is at least 493° below the melting-point of ice; consequently, if the heat derived from the stars is able to maintain a temperature of -239° or 222° of absolute temperature, then nearly as much heat is derived from the stars as from the sun. But if so, why do the stars give so much heat and so very little light? If the radiation from the stars could maintain a thermometer 222° above absolute zero, then space must be far more transparent to heat-rays than to light-rays, or else the stars give out a great amount of heat but very little light, neither of which suppositions is probably true. The probability is, I venture to presume, that the temperature of space is not much above absolute zero. In this case, by adopting -239° as the temperature of space, we make the values given in column VII. too small. But as the two errors tend to neutralize each other, these values may in the meantime be accepted as not very far from the truth, or at least as near as can be arrived at in the present state of science on this point. But whether these values be too high or too low, one thing is certain, that a very slight increase or a very slight decrease in the quantity of heat received from the sun must affect temperature to

a considerable extent. The direct heat of the moon, for example, cannot be detected by the finest instruments which we possess; yet from 238,000 observations made at Prague during 1840-66, it would seem that the temperature is sensibly affected by the mere change in the lunar perigee and inclination of the moon's orbit*.

Column VIII. gives the midwinter temperature. It is found by subtracting the numbers in column VII. from 39° , the midwinter temperature. Column IX. shows the midwinter temperature of the centre of Scotland, on the supposition that the Gulf-stream was diminished in volume in proportion to the excentricity. In former papers it was explained how a change of excentricity must affect ocean-currents†.

I have not given a Table showing the temperature of the summers at the corresponding periods. This could not well be done; for there is no relation at the periods in question between the intensity of the sun's heat and the temperature of the summers. One is apt to suppose, without due consideration, that the summers ought to be then as much warmer than they are at present, as the winters were then colder than now. Sir Charles Lyell, in his 'Principles,' has given a column of summer temperatures calculated upon this principle. Astronomically the principle is correct, but physically it is totally erroneous, and calculated to convey a wrong impression regarding the whole subject of geological climate. The summers at those periods, instead of being much warmer than they are at present, would in reality be far colder than they are now, notwithstanding the great increase in the intensity of the sun's heat resulting from the diminished distance of the sun. If a country is free from snow and ice, then no doubt the temperature will rise during summer as the intensity of the sun's heat increases; but when such a country is enveloped in perpetual snow and ice, the temperature of the summers will never rise much above the freezing-point, no matter what the intensity of the sun's heat may be. The physical reason of this was explained on a former occasion‡. In a country covered with ice, the direct heat of the sun is often very intense, in fact scorching. It will raise the temperature of the mercury in the thermometer exposed to the direct rays of the sun, but it fails to heat the air. Captain Scoresby, for example, when in lat. $80^{\circ} 19' N.$, found the side of his ship heated by the direct rays of the sun to about 100° ,

* See Professor C. V. Zenger's paper "On the Periodic Change of Climate caused by the Moon," *Phil. Mag.* for June 1868.

† *Phil. Mag.* for August 1864 and February 1867.

‡ *Phil. Mag.* for February 1867.

while the air surrounding the ship was actually 18° below the freezing-point. On another occasion he found the pitch melting on the one side of the ship by the heat of the sun, while water was freezing on the other side from the intense coldness of the air.

The mean temperature of Van Rensselaer Harbour, in lat. $78^{\circ} 37'$ N., long. $70^{\circ} 53'$ W., was accurately determined from hourly observations made day and night over a period of two years by Dr. Kane. It was found to be as follows:—

Winter	. .	$-28^{\circ}59$
Spring	. .	$-10^{\circ}59$
Summer	. .	$+33^{\circ}38$
Autumn	. .	$-4^{\circ}03$

But although the quantity of heat received from the sun at that latitude ought to have been greater during the summer than in England*, yet, nevertheless, the temperature is only $1^{\circ}38$ above the freezing-point.

The temperature of Port Bowen, lat. $73^{\circ} 14'$ N., was found to be as follows:—

Winter	. .	$-25^{\circ}09$
Spring	. .	$-5^{\circ}77$
Summer	. .	$+34^{\circ}40$
Autumn	. .	$+10^{\circ}58$

Here the summer is only $2^{\circ}4$ above the freezing-point.

If we go to the Antarctic regions, where the influence of ice is still more felt, we find the summers even still colder. Capt. Sir James Ross found, when between lat. 60° and 77° S., that the mean temperature never rose even to the freezing-point during the entire southern summer; and when near the ice-barrier on the 8th of February, 1841, a season of the year equivalent to August in England, he had the thermometer at 12° at noon, and so rapidly was the young ice forming around the ships that it was with difficulty that he escaped being frozen in for the winter. And on the February of the following year, when he again visited that place, he had the thermometer standing at 19° at noon, and the sea covered with an unbroken sheet of young ice as far as the eye could reach from the mast-head. This extraordinary low temperature at that season of the year was wholly owing to the presence of the ice. Had there been no ice on the Antarctic continent, Sir James would have had a summer

* See Mr. Meech's memoir "On the Intensity of the Sun's Heat and Light," Smithsonian Contributions, vol. ix.

hotter than that of England, instead of one actually below the freezing-point.

Now, during the glacial epoch, when Europe was almost covered with snow and ice, the summers could not possibly have been much warmer than they are at present in Arctic and Antarctic regions. In other words, during the glacial epoch the mean summer temperature would be very little above the freezing-point.

[To be continued.]

XX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 68.]

May 28, 1868.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read :—

“On the Impact of Compressible Bodies, considered with reference to the Theory of Pressure.” By R. Moon, M.A., Honorary Fellow of Queen’s College, Cambridge.

Suppose that we have two *rigid* cylinders of equal dimensions, which have their axes in the same straight line; suppose, also, that one of the cylinders is at rest while the other moves towards the first with the velocity V in a direction parallel to both the axes; the consequence of the collision which under such circumstances must take place, will manifestly be that half the momentum of the moving cylinder will be withdrawn from it, and will be transferred to the cylinder which originally was at rest.

The mode in which velocity or momentum will thus be collected from the different parts of the one cylinder, and distributed amongst those of the other, is obvious. Exactly the same amount will be withdrawn from the velocity of each particle of the impinging cylinder, and exactly the same amount of velocity will be impressed on each particle of the cylinder struck.

And the reason of this is equally obvious, since, if such were not the case, the particles of each cylinder would *contract*—a supposition which is forbidden by the very definition of rigidity.

But if, instead of being perfectly rigid, each cylinder is in the slightest degree compressible, a variation in the effect will occur.

As before, momentum of finite amount will be transferred from the one cylinder to the other, but the mode of collection of the velocity withdrawn from the one, and the mode of distribution of that injected into the other, will no longer be the same as before.

In order that the moving cylinder may not be reduced to absolute rest by the collision, it is obvious that the cylinder originally at rest, or a portion of it, must be moved out of the way, so as to allow of the continuance, even in a modified degree, of the other’s

motion; and this can only be effected on the terms of a transference of velocity or momentum taking place from the one cylinder, or part of it, to the other cylinder, or part of it.

But when the cylinders are compressible, we are freed from two conditions which obtain when the cylinders are rigid.

In the first place, it is no longer necessary to suppose, neither should we be justified in assuming, that the velocity abstracted from each particle of the impinging cylinder, or transferred to each particle of the cylinder struck, is the same; on the contrary, all experience tells us that, in bodies susceptible of compression, compression is always produced by collision—in other words, that variation of velocity, in the parts about which the collision takes place, is the immediate and invariable concomitant of collision.

In the second place, when the cylinders are compressible, it is no longer essential to suppose that the effect of the collision will be to withdraw velocity from every particle of the impinging cylinder, and to impart velocity to every particle of the cylinder struck. Undoubtedly such may be the case if the cylinders are short, if they are possessed of only a moderate degree of rigidity, and if the velocity before impact of the impinging cylinder is considerable. But if the cylinders be long, while the velocity of the impinging cylinder is of moderate amount, the contrary may occur. The condition that the cylinder originally at rest shall not oppose an immediate insurmountable barrier even to the modified motion of the other may, obviously, be sufficiently satisfied if a motion of contraction is imparted by the collision to a definite portion of the second cylinder.

But when the cylinders are compressible, equally as when they are rigid, the collision must cause the instantaneous abstraction of velocity or momentum, either from the whole of the impinging cylinder, or from a definite part of it, and the instantaneous communication of the velocity so withdrawn, either to the whole of the cylinder struck, or to a definite part of it.

We have hitherto assumed the velocity of each particle of the impinging cylinder to have been originally uniform. Let us now suppose, however, that immediately before impact a counter velocity of variable amount is impressed on the different parts of the impinging body, so that, at the instant of impact, *before taking account of the effect of collision*, the velocity at any point of the impinging body may be expressed by $V - V_1$,—where V is constant, but V_1 has the value zero at the surface of collision, and thence gradually increases as we recede towards the other extremity of the cylinder, so that $V - V_1$, which expresses the velocity of the impinging cylinder before impact, has its greatest value at the surface of collision, and diminishes as we recede therefrom.

It is clear that, in the case we are now considering, the collective momentum abstracted from the impinging cylinder by the collision will be less, and finitely less, than that which was abstracted by the collision in the former case, in which the velocity of each particle of the impinging cylinder was supposed uniform and equal to V .

For, if M be the momentum lost by collision when the velocity before impact is uniform and equal to V , it is clear that when the velocity before impact is represented by $V - V_1$, the quantity V_1 may be such that the momentum *before impact* may be finitely less than M ; from which it follows inevitably that the amount of momentum lost by collision in this latter case must be less than M .

Let us now vary the *data* by supposing that the velocity before impact *increases* instead of diminishes as we recede from the surface of collision, so that at the moment of impact, before taking account of the effects of collision, the velocity at any point of the impinging cylinder is represented by $V + V_1$ instead of $V - V_1$.

It is clear that the momentum abstracted by the collision in this latter case will be *greater*, and finitely greater, than in the case where the velocity before impact is uniform and equal to V . Let the additional momentum abstracted in this case be M_1 , the whole momentum so abstracted being represented by $M + M_1$.

Let us now make a final variation in the conditions of the problem, by supposing that at the moment of impact, and irrespective of the impact, a velocity equal and opposite to V is communicated to each particle of the impinging cylinder, so that at that instant, without taking account of any action of the one cylinder upon the other, the velocities of the two cylinders along their surfaces of contact will be equal, or, rather, will be alike zero, at the same time that at every other point of the impinging cylinder there will be a variable velocity V_1 increasing in amount as we recede from the surface of contact.

In estimating the effect of the cylinders being in contact under the circumstances last described, it is clear that the abstraction from each particle of the impinging body of the velocity V can only be regarded as preventing the transference to the second cylinder of so much of the momentum $M + M_1$ as that velocity, if it had constituted the entire velocity before impact of the impinging body, would have given rise to, viz. M_1 —and that the momentum M_1 , whose appearance in the expression $M + M_1$ is due to the fact of the first cylinder having been originally endowed with the variable velocity V_1 in addition to the constant velocity V , will still continue to be transmitted to the second cylinder from the first.

We are thus led to this singular and, doubtless, pregnant conclusion, that in a continuous material system in which there is neither discontinuity of velocity nor discontinuity of density, all the consequences of collision may occur, viz. the instantaneous transmission of a finite amount of momentum from one part of the system to another, provided we have discontinuity in the *tendency to compression* in the different parts of the system.

The author has endeavoured, in former communications to the Royal Society, to show that when the velocity in a fluid diminishes in the direction to which the motion tends, the slower particles will offer a resistance to the motion of the faster particles, which the received theory fails to take into account. The foregoing speculation

goes to prove that the circumstance of the surfaces of contact of contiguous elements of the fluid having the same velocity constitutes no objection to the reality of such resistance.

GEOLOGICAL SOCIETY.

[Continued from p. 74.]

April 15th, 1868.—Sir R. I. Murchison, Bart., K.C.B., F.R.S., &c.,
Vice-President, in the Chair.

The following communications were read:—

1. "On the Disposition of Iron in variegated Strata." By George Maw, Esq., F.G.S.

In this paper the author gave detailed descriptions of the phenomena of variegation in rocks of various ages and composition, with numerous analyses of the differently coloured portions. The conclusions at which he had arrived were the following:—(1) That the assumed production of the colouring-matter of red beds from the decomposition of Iron Pyrites appears untenable. (2) That the bleaching of red beds by the reducing action of fossil carbonaceous matter will not account for the facts of variegation. (3) That this bleaching and nearly every form of variegation of red beds are caused by the passage of iron from the discoloured areas, unaccompanied by any change of combination, excepting the invariable conversion of the anhydrous into the hydrated peroxide. (4) That the arrangement of the iron has not been produced by solution. (5) That it has in some cases been produced by a variety of evident conditions independent of mere chemical reaction, while in other cases it seems to have occurred arbitrarily. (6) That it is unaccompanied by any other change in the combination or position of the constituents of the stratum, and appears to be wholly independent of its chemical constitution, any simple chemical theory being, in short, insufficient to account for the movement of the iron. (7) That the motion has sometimes taken place centripetally and sometimes centrifugally. (8) That the partial conversion of the carbonate of protoxide of iron into hydrated peroxide is one of the most frequent purely chemical causes of variegation. Other causes are the conversion of the red anhydrous into the yellow hydrated peroxide, the secondary formation of the bisulphide and the several stages of its decomposition in mechanical association with the peroxide, and, lastly, the infiltration of lime and magnesia into red beds. (9) That some of the more complicated forms are due partly to segregation, and partly to changes of combination. (10) That the ferruginous banding of yellow sandstones is the result of the segregation of the hydrous peroxide of iron into lines, which are invariably adjacent to a bleached part of the stratum, over which they appear to have advanced, gathering up the peroxide of iron in their course, and leaving behind them a depleted area.

2. "On the older Rocks of South Devon and East Cornwall."
By Harvey B. Holl, M.D., F.G.S.

The author divided the rocks of the district to which the communication referred into a Lower, Middle, and Upper South-Devon Group, and stated that the lowest beds were brought up along a line of country extending from Dartmoor by Kingston Down to the Brown Willey granite, where they formed a broad anticlinal axis. These rocks are unfossiliferous, and may not be lower in the series than the base of the Ilfracombe group of North Devon, or the highest part of the group immediately below it, the latter being more probably represented by some still lower beds of red and greenish grits brought up to the surface in the anticlinal axis of St Broeck's Down further to the west.

The Middle South-Devon Group comprises at its base the discontinuous calcareous range of the Looe River, St. Germans, Brickfortleigh, Ashburton, and Bickerton, above which is a mass of blue and claret-coloured slates, which separate it from the Plymouth or Torbay Range.

This calcareous and fossiliferous group is succeeded by higher beds of blue and claret-coloured argillaceous slates, followed by hard, red, dark, micaceous schists, and purple and greenish grits, which constitute the author's Upper South-Devon Group. These rocks are very sparingly fossiliferous, and probably correspond to the upper and Morthoe portions of the Ilfracombe series of North Devon.

The unconformable position of the Culm-measures is seen in the circumstance that they rest upon different parts of the underlying Devonian rocks—sometimes on the limestones of the Torbay range, sometimes on the slates, at others on the volcanic rocks. This entirely separates the South-Devon rocks from the Carboniferous system.

The occurrence of the genus *Pteraspis*, and probably *Cephalaspis* with *Phyllolepis concentricus* and (?) *Holoptychius*, and other fish-remains, appeared to the author to go a good way towards identifying these Cornish and South-Devon beds with the Old Red Sandstone of Scotland. These fossils range up to the very base of the Torbay limestones.

The author referred the whole of the rocks treated of, with the exception of the purple and greenish grits of St. Broeck's Down, to the Middle Devonian system, and considered that if the lower or Linton rocks were to be met with at all on the south side of the Culm-trough, it would be in the high ground which forms the watershed of West Cornwall.

In the concluding portion of the paper Dr. Holl entered upon the palæontological relations of the different South-Devon groups, and especially those of the Petherwyn beds.

XXI. *Intelligence and Miscellaneous Articles.*

ON SECONDARY CURRENTS AND THEIR APPLICATIONS.

BY M. G. PLANTÉ.

OBSERVATIONS of the intensity of the secondary current developed in a voltameter with lead electrodes induced me, in 1860, to construct a *pile* or *secondary battery* by which, within certain limits, the work of the voltaic battery could be accumulated*. I have now to describe a new mode of arranging the apparatus, as well as the effects of *quantity* and of *tension* which can be obtained by its means.

The instrument, which I designate *secondary quantity battery*, consists of a rectangular gutta-percha vessel provided with a number of lateral grooves very close to each other, and containing a number of lead plates immersed in water acidulated by sulphuric acid. The odd-numbered plates are all joined, and may be connected with one of the poles of a battery; the even plates are also joined, and may be connected with the other pole, so that all the plates form one single secondary element of large surface.

When, on such an apparatus constructed with six lead plates 20 centims. square, the current of two small nitric-acid couples is allowed to act, and when the circuit is broken by means of the commutator so as to close the secondary one, temporary incandescence is obtained of a platinum wire 1 millim. in thickness and 8 centims. in length—a result which would otherwise be impossible with the feeble source which furnishes the principal current.

To obtain more powerful effects, I have constructed batteries of from twenty to forty lead plates united together; these batteries produce very intense calorific effects, such as the fusion of iron or steel rods, when they have been charged by two or three Bunsen's elements from 15 to 20 centims. in height.

In the production of many of the effects of voltaic electricity, the *quantity* of electricity furnished by the current would be insufficient if it were not associated with a certain *tension*. M. Thomsen of Copenhagen described, in 1865, a *polarization battery* based on the current produced by platinized platinum electrodes, and by means of which he obtained a series of secondary currents sufficiently close to form a continuous series having higher tension than the principal one.

I thought that the intense current furnished by lead electrodes might be used for producing temporary effects of great tension, and I have attained this object by arranging the secondary couples so that they could be charged simultaneously in *quantity* and then discharged in *tension*.

The *secondary tension battery* which I have constructed consists of forty secondary couples, each formed of a very narrow gutta-percha vessel containing two lead plates, 20 by 20, immersed in dilute acid. The poles of all the couples contained in the same box terminate in a commutator so constructed that it can join them either in surface or in tension.

If this battery be charged by the aid of three Bunsen's elements

* Phil. Mag. S. 4. vol. xix. p. 468.

of mean size, and the commutator be so turned as to close the secondary circuit, a platinum wire more than 2 metres long and $\frac{1}{4}$ millim. thick can be made incandescent for a few instants. Chemical decompositions requiring great tension may also be shown, as well as physiological effects and a voltaic arc of short duration. In fact, we obtain for a brief space the same effects as are produced by a battery of fifty-five to sixty elements arranged in a series.

The calorific effects produced by this apparatus might be used for firing mines, and the physiological effects for medical purposes.

In short, the secondary battery with lead electrodes enables us to obtain either powerful temporary effects of quantity from a continuous source of electricity of small quantity, or powerful temporary effects of tension from a continuous source of small tension.

This concentration of the work of the battery is doubtless only obtained through the intervention of a chemical action, and is effected with the losses inevitable in every organ of change. There is not, as in the case of induction for instance, the direct production of one physical effect by another physical action; but the final result is none the less an accumulation or a modification of electrical force which can be utilized in certain circumstances.

These facts show, moreover, the importance of the part which secondary currents must play in electrochemistry, and the applications which may result therefrom.—*Comptes Rendus*, June 22, 1868.

ON THE FORMATION OF PEROXIDE OF SILVER BY OZONE.

BY F. WÖHLER.

If, through water feebly acidulated by sulphuric acid, the current from a few Bunsen's elements be passed, and if a silver plate be used as the positive electrode, it immediately begins to be covered with a black substance. This substance is peroxide of silver, easily recognizable by its property of dissolving in ammonia with a brisk evolution of nitrogen. Thus prepared it forms black amorphous crusts, unlike the beautifully crystallizing peroxide which separates from a dissolved silver-salt at the positive pole.

This mode of formation is interesting, inasmuch as it probably takes place directly from the ozone at the positive pole; for it was long ago observed by Schönbein, and noted as one of the characteristic properties of ozone, that bright silver is superficially changed into peroxide by ozonized oxygen. In the experiments the current was strong enough to form ozone when platinum was used as an electrode; but when silver was used no trace of any odour of ozone could be perceived; and it must therefore be assumed that all the ozone is used for the oxidation of the silver.

After a certain quantity of peroxide has been deposited on the silver, it begins slowly to disengage oxygen; and simultaneously there is deposited on the negative pole a small quantity of grey amorphous metallic silver. At the same time there is always a small quantity of silver in solution. These are probably secondary phenomena, arising from the action of the sulphuric acid collected at the positive pole, which decomposes the peroxide.—Liebig's *Annalen*, May 1868.

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[FOURTH SERIES.]

SEPTEMBER 1868.

XXII. *On the Polarization of Heat at 100° C., and on the Motion accompanying Heat-conduction.* By PROFESSOR MAGNUS*.

IN a former essay on the polarization of heat, and on its passage through parallel plates†, it has been already shown that the heat radiated from a red-hot plate of platinum at an oblique angle only comes in part from the surface, another portion being radiated from the interior. This fact was deduced as a consequence of the polarization of the heat radiated from such a surface. For since the plane of polarization has the same position as that of light refracted at a certain angle, we are compelled to assume that, at all events, one portion of the emergent rays suffers a refraction at the surface; and in order that this may be the case, the heat must come from the interior of the plate. Since, however, this polarization takes place according to the same laws as that of light, we are further compelled to conclude that the propagation of the heat in the interior takes place in the same manner as that of light, namely in transverse oscillations. In the above-mentioned publication it was asserted that the conduction of heat depends upon this species of motion. This assertion, however, was only based upon the fact that the motion which is called heat cannot be of two kinds, and that, if its propagation through air, vacuum, or any other diathermanous substance takes place by means of transverse oscillations, its propagation in the interior of such bodies as are not diathermanous, which propagation we call conduction, must be of the same nature.

This conclusion could not be drawn with certainty, because it

* *Monatsbericht der königlich preussischen Akademie der Wissenschaften zu Berlin.* März 1868.

† *Monatsbericht*, 1866, p. 62.

was still possible that it was only the luminous portion of the heat which was polarizable. If, on the contrary, we could prove that the heat which is radiated at an oblique angle from bodies of any temperature, and therefore at a very low one, is also partly polarized, it would be proved that in the case of dark bodies the heat which they radiate comes in part from their interiors, and is propagated in them by transverse undulations. I think, then, we should be justified in asserting that the conduction of heat, or its propagation in athermanous bodies, also depends upon transverse oscillations.

MM. La Provostaye and Desains state that they obtained polarized heat from a platinum plate whose temperature was below incandescence. Their experiments were made with platinized plates at a temperature of between 330° and 360° C.; and with these they say they have obtained very distinct polarization, though to a less degree than with smooth plates. It was shown in the above-mentioned experiments with incandescent plates that these do not show any polarization if they are perfectly platinized. The plates used by MM. Provostaye and Desains, therefore, could not have been fully platinized. These experimenters do not enter into any explanation of the polarization of the heat radiated from these plates. It is also clear that by the employment of rough plates they were not in a position to arrive at the conclusion that the polarization depended upon the refraction of the heat issuing from the interior. For the same reason, their results, although very interesting, determine nothing in regard to the point at issue. It was therefore necessary to undertake new experiments, in order to decide whether the heat which is radiated from bodies of a lower temperature (say, 100° C.) is polarized.

The means hitherto employed for the investigation of the polarization of heat depend upon its passage through double-refracting plates or through columns of mica plates. Neither of these could be here employed, because they do not allow the passage of the dark heat. There only remained, therefore, to make use of reflection for this purpose. Since, however, as is well known, only a small portion of the heat falling upon a mirror is reflected, especial precautions and arrangements had to be made in order to measure the reflected heat.

The essential portion of the apparatus was a reflecting mirror, 65 millims. wide and 130 millims. long, of black polished glass. This was placed at the end of a horizontal tube of 60 millims. diameter and 120 millims. length. This tube was made of stout pasteboard, in order to prevent heat reaching the mirror by conduction. Both ends of the tube were closed by plates having circular openings of 35 millims. diameter. The axis of the tube, or the line joining the centres of the openings, passed, when produced, through the centre of the mirror, which could receive any

inclination required towards the axis. In the following experiments the two always made an angle of 35° with one another. The thermo-electric pile with its cone was so fastened to the mirror by means of an arm, that the rays falling upon the mirror in the direction of the axis of the tube were reflected into the cone of the pile. The entire arrangement could be turned with the tube and its axis so that the mirror and thermo-pile could be brought into all azimuths. To protect them from external influences, they were surrounded by a large pasteboard box, which was further protected by thick hanging covers. Only a small part of the tube extended beyond the box, and by this means the mirror and thermo-pile could be turned from the outside without opening the box. An index fastened to the tube showed the position in the interior. Between the box and the source of heat another large screen was erected, in which there was an opening of the same size as those at the two ends of the tube. As the rays passed through these three openings, they reached the mirror in almost parallel directions. For the source of heat a tin-plate vessel was employed, which could be maintained at 100° C. by means of a current of steam. This steam-box was so fastened upon a stout wooden slab that it could be turned upon a horizontal axis whose continuation passed through the centre of the mirror. The face of this vessel, which radiated upon the mirror, made an angle of 35° with the horizontal. Other plates could be fastened upon the steam-box, so as to serve as radiating surfaces. The steam entered the vessel out of a large glass bolt-head, and left it by means of a tube which led into a large flask containing many litres of cold water. In order to remove the water condensed in the vessel which served as the source of heat, the tube which led off the steam was so bent in the inside of the box, that its opening always reached down to the lowest part. When, therefore, the water had reached this point, it was forced out of the box by the freshly arriving steam. A moveable screen of double metal plates stood between the radiating vessel and the fixed screen, in which the first diaphragm was situated. It was removed whenever an observation had to be made, and replaced as soon as the needle had completed its deviation.

The observation was made by observing the first displacement of the needle. It may, it is true, appear that it would be preferable to wait until the needle had assumed a constant position. But the results are then less sharp and exact. During the long continuance of the radiation the pile becomes, in fact, warmed, and consequently the position of the needle never remains quite constant. At the same time the space in the inside of the box becomes warmed, and the same value is not always obtained on repeating the same experiment. Accordingly the method of

observing the first deviations was retained, since, as appears from the numbers given below, the values obtained on repeating the same experiment agree as closely with one another as can be expected in such experiments. The vessel serving as source of heat was, in the first place, so turned that the radiating surface was vertical and turned to one side (say, to the left). It was then turned horizontally towards the ceiling, then vertically towards the other side, and finally horizontally towards the floor. For each of these positions the magnitude of the deviation was determined by turning the reflecting mirror in succession vertical towards one side, then horizontal towards the ceiling, and vertical towards the other side. The construction of the apparatus did not allow of its being turned horizontally downwards.

For each of these positions the observation was repeated three times, and the mean of these observations, taken immediately one after the other, was adopted. When the radiating surface is directed upwards or downwards, we should have expected equal values for similar positions of the mirror. Nevertheless, in spite of all care to avoid the introduction of errors through variations in the adjustment, the values obtained were invariably higher when the radiating surface was turned upwards than when it was directed downwards. The cause of this is evidently the superior warmth of the roof of the room in these experiments above the floor. The former was, in fact, warmed by the air ascending from the radiating vessel and from the neighbouring bolt-head of boiling water. The roof accordingly radiated more heat than the floor or the walls of the room, which was not artificially heated; and a portion of this heat, reflected by the radiating surface itself, reached the pile along with that directly radiated. Hence also the fact is accounted for, that the difference found on turning the radiating surface upwards and downwards was greatest when the radiating surface was of polished metal; for in such surfaces the radiating-power is very small, while the reflection is greater than from any others. Since through the wintry cold during which these experiments were conducted the window-side and the back of the room had sometimes different temperatures, large screens were set up to diminish this injurious influence, at half a metre on both sides of the radiating vessel. In observations such as these, indeed, the greatest care is requisite. The galvanometer used was the very sensitive one constructed for former experiments, and since improved.

If the heat radiated at 100° C. had not been polarized, the values must have been the same at every position of the radiant surface for each of the positions mentioned of the reflecting surface. The numbers given below, however, prove that the deflections of the galvanometer were smaller at every position of the radiant surface when its plane of reflection coincided with that of the

mirror, than when the two formed a right angle with one another. Hence it follows that the radiant heat is partly polarized, and that, as with incandescent platinum plates, its plane of polarization is perpendicular to the plane of reflection. Starting from the hypothesis that the deflections of the galvanometer are proportional to the quantities of heat, the values found give the relation between the polarized and the entire amount of radiated heat. For, assuming that the mirror in both positions reflects only polarized heat, the entire radiated heat is proportional to the sum of the reflected quantities in the two positions of the mirror, and the difference of these quantities is proportional to the polarized portion.

In the first place, experiments were made in which the polished face of the radiating vessel made of tinned iron-plate sent its heat against the reflecting mirror. In the following Table are contained three series of experimental numbers obtained in this manner. In the first the original observations are given, in order to show how far these agree with the mean values; in the remainder the mean values alone are given. A polished copper plate was then fastened upon the tin plate, and this again was afterwards replaced by a polished plate of aluminium. The latter was used to decide whether metals of very different specific gravity might show differences in the polarization, and consequently in their refractive powers.

TABLE I.—Radiating surface :—polished metal.

Radiating surface.	Analyzing mirror.	Tinned-iron plate.						Copper.	Aluminium.	
		Series of experiments I.			II.	III.			I.	II.
					mean.	mean.	mean.	mean.	mean.	mean.
Horizontal upwards.	Left	9	8.5	9	8.75	7.4	9	9.2	7.3	7.6
	Horizontal ...	4.5	5	5	4.8	3.7	6.5	6.8	4	4.1
	Right	8	8.5	9	8.5	7.25	9.2	8.7	7.2	6.6
	Left	3	3.75	4	3.5		3	2.8	1.6	2.25
To the left.	Horizontal ...	5	5	5	5	3.4	5.5	3.1	3.3	4.5
	Right	4.5	5	4.25	4.5	2.1	3.6	2.6	2.1	2.9
Horizontal downwards.	Left	6.25	6	6.5	6.25		5.2	3.1	4.5	4.3
	Horizontal ...	4	4	4	4	2.75	3	2.1	2.8	2.7
	Right	5.5	6	5.75	5.75	5.2	6	3.75	4.9	4.6
	Left	3.25	3	3.5	3.25	4.5	2.6	2.6	2.8	3.2
To the right.	Horizontal ...	5	4.5	5	4.8	4.6	5	3.1	4	3.9
	Right	4.25	3.25	3.25	3.6	2.3	2.8	2.75	3.1	2.8
Mean of all values with perpendicular planes of reflection					6.46	5.57	6.65	5.16	5.2	5.3
Mean of all values with parallel planes of reflection					3.96	3.07	3.58	3.27	2.7	3
Total heat or sum					10.42	8.64	10.23	8.43	7.9	8.3
Polarized portion or difference					2.50	2.50	3.07	1.89	2.3	2.3
Percentage of polarized portion of the entire heat					24	29	30	22.4	29	28

Upon the radiating surface of the tinned-iron vessel a transparent glass plate of 1 millim. thickness was now fastened. It is clear that the temperature assumed by the outer surface of this glass plate was lower than that of the metallic face upon which it was fastened, and from which it derived its heat. Nevertheless the quantities of heat which it delivered are much greater than those radiated from the metal plate. This superior action of the glass cannot be due to its sending out heat from its entire mass; for it is known from Melloni's researches that glass does not allow heat of 100° C. to pass through. Consequently the superiority of the glass must be entirely owing to its greater radiating-power.

TABLE II.—Radiating surface :—transparent glass.

Radiating surface.		Analyzing mirror.		Series of experiments I.				II.	III.
							mean.	mean.	mean.
Horizontal upwards.	{	Left.....	44	43·5	43·25	43·6	39·3	47·25	
		Horizontal ...	35	35	34·75	34·9	33	36·25	
		Right	42	42·5	42	42·2	42·7	45·25	
To the left.	{	Left.....	35	35·25	35	35·1	30·2	36·3	
		Horizontal ...	42	42·5	42	42·2	36·5	45·3	
		Right	34·5	34·25	34·5	34·4	32·7	35·2	
Horizontal downwards.	{	Left.....	44	43·75	44	43·9	39·5	45·41	
		Horizontal ...	35·5	35·5	35	35·3	33·5	36·8	
		Right	43	43	43	43	42·2	43·5	
To the right.	{	Left.....	35·25	36	34·5	35·6	31·3	35·7	
		Horizontal ...	42·75	42	42	42·2	37·1	43	
		Right	39·5	35	35·5	35·5	31·8	35·7	
Mean of all values with perpendicular surfaces of reflection							42·8	39·5	44·95
Mean of all values with parallel surfaces of reflection.							35·1	32·1	35·99
Total heat or sum.....							77·9	71·6	80·94
Polarized portion or difference							7·7	7·4	8·96
Percentage of polarized portion							9·9	10·3	11·1

The numbers show that the heat radiated from the glass at an angle of 35° is also polarized, and indeed perpendicularly to the plane of reflection, exactly as with metals. In order, however, to get rid entirely of the notion that radiation had taken place through the substance, the transparent plate was replaced by one made of black perfectly opaque glass. With this the following values were obtained :—

TABLE III.—Radiating surface:—black glass.

Radiating surface.	Analyzing mirror.	Series of experiments I.				II.	III.
					mean.	mean.	mean.
Horizontal upwards.	Left	44·5	46·25	45	45·2	36	42·1
	Horizontal ...	34·7	35	35	35	27·7	34·7
	Right	44	44·5	44·5	44·3	33·8	45
To the left.	Left	34	34	33	33·7	28	34·5
	Horizontal ...	44	43·5	44·25	44	34·5	42
	Right	34·75	35	34	34·6	26	35·7
Horizontal downwards.	Left	45	45	45	45	35·5	42
	Horizontal ...	34	35	34	34·3	27·5	35·5
	Right	44·5	45·5	44	44·7	33·6	45·2
To the right.	Left	35	35·5	36·5	35·6	27·5	33·7
	Horizontal ...	44	43·5	44·25	44	34·2	42·3
	Right	34·75	34·5	35	34·75	26·8	36·3
Mean of all values with perpendicular planes of reflection					44·8	34·6	43·1
Mean of all values with parallel planes of reflection					34·6	27·2	35·1
Total heat or sum					79·4	61·8	78·2
Polarized portion or difference					10·2	7·4	8·0
Percentage of polarized portion					12·9	12·0	10·3

The agreement of these values with those given by the transparent plate is a new proof that the transparent glass is no more diathermanous for heat of 100° C. than the black opaque glass, and that both kinds of glass possess radiating-powers for heat which are very nearly equal.

If the polarization of the radiated heat depended upon the partial emergence of the heat from the interior and its refraction on leaving the surface, then with a perfectly rough surface the refraction must take place in all directions, and consequently no polarization at all should be observable*. It appeared, therefore, of interest to try how the polarization was affected by using a dull, ground glass plate in place of the polished one. The rough glass plate was of the same kind of glass and of the same thickness as the smooth one before used. As is seen from the following numbers, the polarization was much less than with the smooth plate; but still polarization did occur. Hence the suggestion immediately offered itself that the glass was not sufficiently rough. Accordingly a perfectly rough substance, namely

* "Wenn die Polarisation der ausgestrahlten Wärme davon herrührt, dass die Strahlen zum Theil aus dem Innern kommen und beim Austreten an der Oberfläche gebrochen werden, so müsste für den Fall, dass die Oberfläche vollkommen rauh wäre, die Brechung nach allen Richtungen stattfinden und daher gar keine Polarisation zu beobachten sein." As the above appears ambiguous, I give the text literally.—F. G.

black cloth, was chosen, and stuck for this purpose upon a tin plate and fastened upon the radiating vessel.

The cloth, as is shown by the following numbers, exhibited no polarization at all; and herein lies a further proof that the radiant heat comes in part out of the interior of the body, and is polarized at its surface by refraction.

TABLE IV.—Radiating surfaces :—rough glass and black cloth.

Radiating surface.	Analyzing mirror.	Rough glass.		Black cloth.	
		I.	II.	I.	II.
Horizontal upwards...	Left.....	mean. 40·2	mean. 44·8	mean. 41·5	mean. 42·5
	Horizontal	35·3	38·7	41·7	41·5
	Right	38·3	42·3	39·8	41·5
	Left.....	34·8	38·1	39·7	42·9
To the left.	Horizontal	37·5	42·6	39·7	41·8
	Right	34·5	37·1	39·2	41·4
Horizontal downwards	Left.....	38·8	43	40	43·4
	Horizontal	34·5	37·4	39·2	41·8
	Right	38·7	41·6	40	42·3
	Left.....	34·3	37·3	40·5	42·25
To the right.	Horizontal	38	42·2	38·8	41
	Right	34·2	38	40	41·4
Mean of all values with perpen- dicular planes of reflection...		38·6	42·7	39·97	42·1
Mean of all values with parallel planes of reflection		34·6	37·7	40·05	41·9
Total heat		73·2	80·4	80·02	84·0
Polarized portion		4·0	5·0	1·92	0·2
Percentage of polarized portion...		5·5	6·2		

The propagation in the interior of a body to the surface may be regarded as radiation; but then we must admit that such a radiation takes place in all parts of the inside of athermanous bodies. It is in fact known that the metals, such as gold and silver, are diathermanous in very thin sheets. This has been shown by M. Knoblauch for solar heat. For heat of 100° C. and lower temperatures, such as here employed, no observations had been previously made.

It follows from this investigation, that when heat is propagated in the interior of bodies, transverse oscillations take place at every temperature. Or if such oscillations be not linear, they are yet such motions as to have components perpendicular to the direction of propagation, which produce the same effect as rays of heat.

XXIII. *On the General Solution of Algebraic Equations.*

By THOMAS P. KIRKMAN, M.A., F.R.S.*

ALL algebraic equations can be solved as clearly as the cubic and the quartic, that of the $(n+1)$ th degree after that of the n th, by the use of the following theorem, and of familiar algebraic operations.

Let any of the various ways of writing the group Ω of Πn substitutions made with the n elements $1, 2, 3, \dots, (n-1), n$, as a product of $n-1$ groups, be this,

$$\Omega = G_2 \cdot G_3 \cdot G_4 \dots G_{n-2} \cdot G_{n-1} \cdot G_n,$$

where G_r is a group of the r th order, in which the $n-r$ final elements are undisturbed.

Let H_1 be any given asymmetric function of the n variables $x_1, x_2, x_3, \dots, x_n$ which has Πn values by their permutations.

Let $G_n \cdot H^i$ be the sum of the n values of H_1^i made by the substitutions of G_n , and let

$$G_n^a \cdot H^i = J_i^a$$

be the a th power of that sum. It is here convenient to write the exponent a in an algebraical sense over G_n ; and it can obscure nothing, since G_n^2 , considered as the group product $G_n \cdot G_n$, is simply G_n .

Also let

$$G_{n-1}^b \cdot G_n^a \cdot H^i = G_{n-1}^b \cdot J_i^a$$

be the b th power of the sum of $n-1$ values of J_i^a which are made on the group G_{n-1} ; and so on.

And let Γ_m be the group of the m cyclical permutations of the first m elements, the remaining $n-m$ being undisturbed.

We can construct without any difficulty, except tedious algebraic routine, the function following, for any given system of positive exponents that we desire,

$$\Sigma_\beta = \Gamma_n Z_\gamma^\beta = \Gamma_n \Gamma_{n-1}^\beta \Gamma_{n-2}^\gamma \dots \Gamma_5^\lambda \Gamma_4^\mu \Gamma_3^p \Gamma_2^m \Gamma_1^c \dots G_{n-2}^c G_{n-1}^b G_n^a \cdot H^i,$$

which is the sum of n given values of the β th power of

$$Z_\gamma = \Gamma_{n-1} Y_\delta^\gamma,$$

which is the sum of $n-1$ given values of the γ th power of

$$Y_\delta = \Gamma_{n-2} X_\epsilon^\delta, \text{ \&c.,}$$

till we come to read the function

$$\Gamma_4 S_m^p,$$

* Communicated by the Author.

the sum of four given values of the p th power of

$$S_m = G_2 R_l^m,$$

the sum of two given values of the m th power of

$$R_l = G_3 Q_k^l,$$

the sum of three given values of the l th power of Q_k , and thus finally to read the function

$$J_i = G H^i,$$

the sum of the n values of the asymmetric H_i^i which we choose to construct on G_n .

Our theorem is:

(1) The first sum of values so read, but the last in construction, viz.

$$\Sigma_\beta = \Gamma_n Z_\gamma^\beta,$$

is a given rational and symmetric function of the n variables $x_1, x_2, \dots, x_n$, whatever be the positive exponents $\beta, \gamma, \dots, b, a, i$.

(2) Every one of the succeeding sums of values, viz.

$$\Gamma_{n-1} Y_i^\gamma, \Gamma_{n-2} X_i^\delta, \dots, \Gamma_4 S_m^p, G_2 R_l^m \dots G_n H^i,$$

is a given irrational and symmetric function of those variables, if the solution of equations of the n th and inferior degrees is known.

The demonstration is very simple.

(1) It is evident that

$$S_m = R_l^m + {}_1R_l^m = (Q_k^l + {}_1Q_k^l + {}_2Q_k^l)^m + ({}_3Q_k^l + {}_4Q_k^l + {}_5Q_k^l)^m$$

is invariable by $\Gamma_2 \cdot \Gamma_3$, which represents all the six permutations of x_1, x_2, x_3 . It follows that

$$\Gamma_4 S_m, \text{ or } \Gamma_4^\mu S_m^p,$$

whatever be the integers μ and p , is a function invariable by the Π_4 permutations of x_1, x_2, x_3, x_4 ; whence of necessity

$$\Gamma_5^\lambda \Gamma_4^\mu S_m^p$$

is invariable by the Π_5 permutations of x_1, x_2, x_3, x_4, x_5 ; and so on, till we see that

$$\Sigma_\beta = \Gamma_n Z_\gamma^\beta$$

is invariable by the permutations of $x_1, x_2, x_3, \dots, x_n$; i. e. the function Σ_β is rational and symmetrical in these n variables, whatever be the positive exponents.

It will be here useful to make a distinction between the roots $a_1, a_2, \dots, a_n$ of the equation

$$(x-a_1)(x-a_2)(x-a_3) \dots (x-a_{n-1})(x-a_n) = 0 = U,$$

or the *entering roots* of $U=0$ (as I call them), all asymmetric

functions of the n elements $a_1, a_2, \dots, a_n$, and the roots

$$\{a_1\}, \{a_2\}, \{a_3\}, \dots \{a_n\},$$

which we obtain by solution of $U=0$: these I call the *issuing roots* of $U=0$. They are all irrational functions symmetric in the n elements $a_1, a_2, \dots, a_n$, or in any set of n elements of which Σa^r is a symmetric function.

I denote by a bracketed function $\{R\}$ always a symmetric irrational function of our variables $x_1, x_2, \dots, x_n$, the issuing root of a given equation which we are supposed to have solved.

(2) As our rational symmetric function Σ_β can be formed, and is supposed to be formed, for every system of exponents that we require, we can write for any system in which β only is variable the n equations

$$\begin{aligned} Z_\gamma + {}_1Z_\gamma + {}_2Z_\gamma + \dots + {}_{n-1}Z_\gamma &= \Sigma_1, \\ Z_\gamma^2 + {}_1Z_\gamma^2 + {}_2Z_\gamma^2 + \dots + {}_{n-1}Z_\gamma^2 &= \Sigma_2, \\ \vdots & \\ Z_\gamma^n + {}_1Z_\gamma^n + {}_2Z_\gamma^n + \dots + {}_{n-1}Z_\gamma^n &= \Sigma_n, \end{aligned}$$

where $Z^r, {}_1Z^r, {}_2Z^r, \dots, {}_{n-1}Z^r$ are n values of Z^r made on Γ_n . From these n conditions we obtain

$$\{Z_\gamma\} = \Gamma_{n-1}Y_\delta,$$

the issuing root of a given equation of the n th degree, having for coefficients rational and symmetric functions of $x_1, x_2, \dots, x_n$. This symmetric and irrational function $\{Z_\gamma\}$ can be written for every system of exponents $\gamma, \delta, \dots, c, b, a, i$, in which γ only is variable; that is, we have

$$\begin{aligned} Y_\delta + {}_1Y_\delta + {}_2Y_\delta + \dots + {}_{n-2}Y_\delta &= \{Z_1\}, \\ Y_\delta^2 + {}_1Y_\delta^2 + {}_2Y_\delta^2 + \dots + {}_{n-2}Y_\delta^2 &= \{Z_2\}, \\ \vdots & \\ Y_\delta^{n-1} + {}_1Y_\delta^{n-1} + {}_2Y_\delta^{n-1} + \dots + {}_{n-2}Y_\delta^{n-1} &= \{Z_{n-1}\}. \end{aligned}$$

Hence

$$\{Y_\delta\} = \Gamma_{n-2}X_i^\delta$$

is the issuing root of a given equation of the $(n-1)$ th degree, whose coefficients are irrational and symmetric functions of our n variables. And by the same certain process which has transformed the asymmetric sums $\Gamma_{n-1}Y_\delta$ and $\Gamma_{n-2}X_i^\delta$ into the symmetric irrational functions $\{Z_\gamma\}$ and $\{Y_\delta\}$, we can go on through the entire list of asymmetric sums of values for every system of exponents, transforming each sum into the issuing root of a given equation, till finally we obtain the system

$$\begin{aligned} H_1 + H_2 + H_3 + \dots + H_n &= \{J_1\} \\ H_1^2 + H_2^2 + H_3^2 + \dots + H_n^2 &= \{J_2\}, \\ \vdots & \\ H_1^n + H_2^n + H_3^n + \dots + H_n^n &= \{J_n\}; \end{aligned}$$

and the n asymmetric values of H_1 made on G_n are thus finally transformed, by the solution of a given equation of the n th degree, into the irrational shapes

$$\{H_1\}, \{H_2\}, \{H_3\}, \dots \{H_n\},$$

which are all symmetric functions of $x_1, x_2, \dots, x_n$.

From this position to the solution of the general equation $\Theta=0$ of the $(n+1)$ th degree, there is but one, little, easy step.

If α be any imaginary root of $y^{n+1}=1$, and if $x_0, x_1, x_2, \dots, x_n$ be the roots of $\Theta=0$, we have

$$x_0 = \Sigma x + (\alpha + \alpha^2 + \alpha^3 + \dots + \alpha^{1+n})(x_1 + x_2 + x_3 + \dots + x_n);$$

or, which is the same thing,

$$x_0 = \frac{1}{n+1} \Sigma x + \frac{1}{n+1} P_1^{\frac{n+1}{n+1}} + \frac{1}{n+1} P_2^{\frac{n+1}{n+1}} + \dots + \frac{1}{n+1} P_n^{\frac{n+1}{n+1}},$$

where

$$\begin{aligned} P_1 &= x_0 + \alpha x_1 + \alpha^2 x_2 + \alpha^3 x_3 + \dots + \alpha^n x_n, \\ P_2 &= x_0 + \alpha x_2 + \alpha^2 x_3 + \alpha^3 x_4 + \dots + \alpha^n x_1, \\ P_3 &= x_0 + \alpha x_3 + \alpha^2 x_4 + \alpha^3 x_5 + \dots + \alpha^n x_2, \\ &\vdots \\ P_n &= x_0 + \alpha x_n + \alpha^2 x_1 + \alpha^3 x_2 + \dots + \alpha^n x_{n-1}, \end{aligned}$$

$P_2, P_3, P_4, \dots, P_n$ being here formed with P_1 on $G_n = \Gamma_n$ by the cyclical permutations of $x_1, x_2, \dots, x_n$, while x_0 and $\alpha, \alpha^2, \dots$ are undisturbed.

This expression of x_0 becomes, after raising $P_1, P_2, \dots, P_n$ each to the $(n+1)$ th power by ordinary involution,

$$\begin{aligned} x_0 &= \frac{1}{n+1} \Sigma x + \frac{1}{n+1} (S + H_1)^{\frac{1}{n+1}} + \frac{1}{n+1} (S + H_2)^{\frac{1}{n+1}} \\ &\quad + \dots + \frac{1}{n+1} (S + H_n)^{\frac{1}{n+1}}, \end{aligned}$$

where S is a symmetric function of $x_0, x_1, x_2, \dots, x_n$, and therefore a rational function of the coefficients of $\Theta=0$, and where H_1 is a function of the same $n+1$ variables which has Πn values by their permutations. And since H_1 is invariable by the cyclical permutations of $x_0, x_1, x_2, \dots, x_n$, because $P^{n+1} = (\alpha^n P)^{n+1}$, what-

ever r may be, it receives all its values by the permutations of the n variables $x_1, x_2, x_3, \dots, x_n$.

Wherefore, by what precedes, the n values of H_1 ,

$$H_1, H_2, H_3, \dots, H_n,$$

can be transformed into the issuing roots

$$\{H_1\}, \{H_2\}, \{H_3\}, \dots, \{H_n\},$$

which are symmetric irrational functions of $x_1, x_2, x_3, \dots, x_n$, and also of necessity symmetrical irrational functions of the $n+1$ variables $x_0, x_1, \dots, x_n$. These functions $\{H\}$ are consequently transformable into given irrational functions of the coefficients of $\Theta=0$, and the algebraic solution of $\Theta=0$ is infallibly completed by the substitution of these issuing roots so transformed, for the n asymmetric functions $H_1, H_2, \dots, H_n$ in our value of x_0 . All this can be verified for the simple cases of $n+1=3$, $n+1=4$, and $n+1=5$.

It may be supposed that these processes will be encumbered with hopeless ambiguity by the multiplicity of values of the issuing roots

$$\{Z_r\}, \{Y_r\}, \{X_r\}, \dots, \{J_r\},$$

which appear in the coefficients of the equations of the n th and lower degrees which have to be solved.

But the truth is that there is no ambiguity at all, if we take care that the issuing roots

$$\{Z_1\}, \{Z_2\}, \dots, \{Z_{n-1}\}$$

are all corresponding roots (that is, if we read them all with the same radicals affected by the same signs and imaginaries), and if we take care that the issuing roots

$$\{Y_1\}, \{Y_2\}, \dots, \{Y_{n-2}\},$$

⋮

as well as

$$\{J_1\}, \{J_2\}, \dots, \{J_n\},$$

are in each set of solutions a set of corresponding roots. For the rest, it matters not which of the n issuing roots we take for $\{Z_r\}$, nor, after that, which of $n-1$ issuing roots we take for $\{Y_s\}$ &c. By whatever path we choose to march, we shall assuredly arrive at the symmetrical expression of $\{J_i\}$ the sum of n values of H_i , formed either on G_n , or on one of its $\Pi(n-1)-1$ derivatives; and x_0 is correct if expressed in n values of the H_i above found, which are formed on any one of those derivatives. In other words, we may write for $P_1, P_2, \dots, P_n$ in our expression of x_0 the n results of operation on them by any substitution, not in G_n , made with the n elements $1, 2, 3, \dots, n$.

We have, in our expression of the values of H , taken G_n identical with Γ_n , which is a course by no means necessary. It simplifies our view of the group Ω of Πn substitutions to suppose G_m everywhere identical with Γ_m ; and if Ω be constructed even in this unskilful way, the theorem will support us and bring us safely to our goal.

No selection of a form for Ω can save us from the necessity of constructing the symmetric function Σ_β for every combination of the positive exponents $a, b, c, \dots, l, m, p$ that satisfies

$$a < n+1, b < n, c < n-1, \dots l < 5, m < 4, p < 3;$$

but the Greek exponents need not always all be positive, as for certain systems Σ_β is symmetrical when one or more or all of them are zero, which reduces the Γ carrying the zero to a factor unity. The best choice of form for Ω will introduce in Σ_β the greatest number of zeros among the Greek exponents, and thus cause an enormous reduction of the algebraic labour. This labour will be abridged by the computer in proportion to his command of the theory of groups.

I have thus the pleasure of accomplishing a reasonable number of demonstrated impossibilities, mingled with regret at the ruin of one of the most charming of ancient mysteries. It is a comfort to know that no true Briton for the next twenty years will so far sin against all that is dignified and venerable in mathematics as to suffer himself either to understand or to believe one word of this. To the scientific reader who thinks of taking some day a second peep at this paper, I would say—don't; you will only step out of that enchanted fog of analytical sublimity into a boundless and commonplace bog of algebraic inutility. There is nothing here which adds to my right or to my wish to detain the philosopher. I am quietly satisfied, but far from being elated, by these results. In the opinion of myself and of my respected friend Posterity (the only section of the scientific public in these practical islands whose mathematical reading will ever qualify them to form anything like a judgment on the precise points of comparison), this finishing contribution to the rich theory of equations is a trifle compared with my extensive additions to the skeleton theory of groups, which are again a trifle compared with my creation of the entire theory of the polyedra. How sagaciously amused our profundities will find themselves, if ever they hear of these burrowings of a country mouse!

Croft, near Warrington.

XXIV. *On certain Objections to the Theory of the Equivalent of Refraction.* By Dr. ALBRECHT SCHRAUF, Custodian of the Imperial Mineralogical Cabinet of Vienna*.

ON a cursory examination of the optical literature of the past, we find numerous essays, of various dates, which, being based upon the theory of emission, have denied the correctness of the hypothesis of vibration. The idea of refractive power introduced into optics by Newton appears to have the same purport (*Schicksal*). Besides these older investigations, the most recent work of Rühlmann† are intended to show the inapplicability and incorrectness of the refractive power. Since in the above-named investigation several of my previously published conclusions on this subject are declared to be erroneous, I take this opportunity of placing these objections in their proper light.

I imagine, moreover, that there are many assertions in M. Rühlmann's criticism which he would not have published if he had not ignored, for reasons unknown to me, my later investigations—both those on "Equivalents of Refraction"‡ (1865), and my "Studies"§ published in December 1866. These two investigations, compared with my former publications (1862), contain many new points of view, and precisely in that very field which M. Rühlmann has chosen as the starting-point of the discussion. It appears, indeed, that the doubts now published by M. Rühlmann concerning the theory of refractive power were considered and explained by me in the course of the preceding year.

If we examine the results of M. Rühlmann's important work on the Thermo-optical Constants of Water, the first point which demands some attention is the sentence on page 202||, "That the magnitude M given by Schrauf is by no means a constant, but continually varies with the temperature."

The variation of the refractive power with the temperature, however, is not unknown to me, and received my mature consideration long ago. In my "Studies," p. 69, I stated a year previously, "A uniform result of the calculation, however, is that the values of M (refractive power) diminish with increasing temperature. The obvious objection here arises, that, in consequence of this observation, the function

* Translated by Frederick Guthrie, F.R.S.E., from a separate impression, communicated by the author, from Poggendorff's *Annalen*, vol. cxxxiii. p. 479.

† Poggendorff's *Annalen*, vol. cxxxii. p. 193.

‡ Schrauf, "Die Refraktionsäquivalente und optischen Atomzahlen der Grundstoffe," *Sitzungsb. der Wien. Akad.* 1865, vol. lii. (2) p. 176.

§ Schrauf, *Physikalische Studien über die gesetzmässigen Beziehungen von Materie und Licht*. Wien, Gerold, 1867, p. 249.

|| Pogg. *Ann.* vol. cxxxii.

Phil. Mag. S. 4. vol. xxxv. p. 395.

of the dependence of the refractive index upon the density of the body is not absolutely exact. To this objection it must, however, be replied that this chapter ('the Variation of the Density with increasing Temperature') is not by itself sufficient to determine the correctness or incorrectness of a function: the variations of the density of the body and its index of refraction are nearly of the same order; consequently the most diverse formulæ may be proposed, which may give almost the same results: but reference must be made to the results of the following chapter."

My expression at this place, that, in spite of the recognized diminution of M , the refractive index remained constant, is explained in the sentence (Studies, p. 63), "that the forces X_r retarding the light are functions of the temperature, and diminish with its increase."

In fact, I have (1865, 1866) derived the following successive formulæ for the indices of refraction.

$$\mu^2 = 1 + VDX_r \frac{v^2 s^2}{\lambda^2}, \quad (I.)$$

$$\frac{\mu^2 - 1}{D} = ZGX_r \frac{v^2 s^2}{\lambda^2} = M = \frac{\mathfrak{M}}{P} : (II.)$$

$$\mu^2 = 1 + ZGD \left[M + C \left(\frac{N}{\lambda} \right)^2 \right]; (III.)$$

in which—

μ is the index of refraction,

V is the volume,

D is the density,

Z, G are the number and magnitude of the optical atoms,

M is the refractive power,

N is the dispersive power,

P is the chemical equivalent,

$\mathfrak{M}$ is the Newtonian equivalent of refraction,

X_r is a symmetrical function of the forces retarding the light in the body,

v^2, s^2, λ^2 are the constants of the phase of the incident light.

The constancy of the refractive power rests accordingly upon the atomic arrangement of the molecules and upon the constancy of the retarding forces X_r . That the latter diminish with increase of temperature, I have, as above stated, already mentioned in my "Studies" (1866), and I examined the matter more closely as soon as an opportunity presented itself for treating conducted heat.

In my text-book (finished in the autumn of 1867) the *Ange-*

wandte *Physik der Krystalle**, I have in Chap. XIX. § 131, discussed the action of conducted heat upon light.

The developments carried out on pp. 352-354 are based essentially on the following fundamental propositions.

On the accession of free external heat, an alteration takes place in the length of the molecular paths and the rate at which they are traversed; hence an alteration in the volume and temperature. The changes in these two (length and rate), however, do not take place symmetrically, and they may have the same or opposite signs. In the latter case a contraction may take place on increasing the temperature of a body, or an expansion on cooling it.

We are taught by Fizeau's observations that in the case of certain solid bodies the indices of refraction increase even when the temperature increases. My calculations of the refractive power of liquid bodies show that the refractive indices diminish more rapidly than is due to the alteration in volume indicated by the temperature.

For the explanation of this fact we must assume that the amount of heat entering the body has a twofold effect. The change of volume corresponds to a change in the index of refraction, which corresponds to the alteration in density (formula 3). An increase of the temperature (that is, an alteration in the mean molecular velocity), will, by increasing the molecular velocity†, cause a diminution in the usual retardation of the luminous impulse in those cases where (as in gases and liquids) the molecules are in unstable equilibrium. In support of this is the fact that, according to my calculations in liquid bodies, the refractive power diminishes with an increase of temperature. This action of the temperature on the retarding force appears to be proportional to the variations of temperature, and the light-retarding forces X_r of the substance. (Compare my *Lehrbuch der Physik der Krystalle*.)

In contradistinction to gases and liquids, the molecules of solid bodies have no free motion, but are bound together in constant equilibrium. An elevation of temperature and increase of molecular velocity cannot, therefore, in all cases be regarded as assisting the work and motion performed by the rays of light.

Although, on the one hand, in part an increase and in part a decrease of the light-retarding forces X_r of the solid body may correspond to an increase of temperature, still, on the other hand, from the fact that in Iceland spar the one index of refraction

* Schrauf, *Lehrbuch der physikalischen Mineralogie*, vol. ii. *Lehrbuch der angewandten Physik der Krystalle*. Wien, Braumüller, 1868, p. 426.

† Compare Schrauf, *Physik der Krystalle*, p. 145. The propagation of the wave impulse.

tion diminishes, while the other increases with elevation of temperature, it follows that the influence of heat is considerably influenced by the internal grouping of the atoms around the axis.

Although, however, this secondary influence of temperature has long been known to me, I do not feel on this account in the least compelled to regard the theory of refractive power as incorrect. The following are, amongst others, my reasons :—

In my "Studies" (p. 115) I have said, "Many hypotheses are possible with regard to the variations of the magnitudes $ZG X_v$. The first is that the forces X_v are dependent upon the chemical properties. More attentive consideration, however, shows that there can be no physical reason why a little mass of O should act differently upon the light to an equal mass of H. We are hence led to the hypothesis that the retarding forces X_v are equal for different elements—an assumption which is rendered more than probable by the results obtained by it."

This hypothesis may be retained for the first approximation, which suffices in exactness for all hitherto performed investigations, even when we allow that X_v is a function of the temperature. The abnormal variation of the refractive power M remains within very narrow limits if all possible errors are eliminated. It is further to be remarked that we are always at liberty, by dint of comparing the refractive equivalents of various substances, to limit the constancy of X_v to the ordinary temperatures of observation. This I did in my "Studies."

Such comparison of the retarding forces, X_v , is possible under all circumstances, and must take place under exactly the same points of view under which the temperatures of the different bodies are themselves measured. Even for the latter we are accustomed to consider as equal the temperatures, and therefore, implicitly, the molecular-path velocities of various bodies in different thermal states, although we know little about the law of increase of temperature in bodies and the expenditure of free heat necessary to effect it.

In comparing the optical relations of various bodies, we can therefore still use the theory of the refractive equivalent, inasmuch as for this theory the proportion

$$\frac{X(a)}{X_v(b)}$$

is a constant for the same temperature.

If, on the other hand, we compare the refractive indices of a body for different temperatures, the twofold action of the influx of conducted heat is clearly distinguishable. The overpowering

action of the change of volume declares itself in the law of the refractive power; the change in the molecular path has, on the other hand, a secondary, very insignificant positive or negative influence upon the function X_v of the luminous propagation.

The proof of these statements is given by the numerous data in my "Studies." However, I may here collect the most important facts to prove that, by comparing different bodies for equal temperatures, even of the most diverse volumes, we may look upon the retarding forces X_v as constant, or even as equal. According to formula (II.), the constancy of M proves the constancy of X_v .

		D.	μ .	M.	N ² .
a. Carbon *.....	Anthracite	1.4	1.720	0.001809	
	Diamond	3.5	2.434	0.001820	
b. Water	Ice	0.9181	1.3089	0.0010046	
	Water	1.0	1.3330	0.0010048	
c. Titanic acid...	Anatase	3.95	mean 2.499	mean 0.0017167	
	Rutile	4.20	2.560	0.0017098	
d. Carbonate of calcium ...	Iceland spar	2.73	mean 1.5801	0.0007185	
	Arragonite	2.94	1.6178	0.0007115	
e. C ³ H ⁶ O ²	Propionic acid.....	0.996	1.376	0.001160	0.01266
	Formate of ethyle ...	0.940	1.357	0.001157	0.01271
f. C ⁷ H ¹⁴ O ²	Enanthylic acid.....	0.917	1.409	0.001391	0.01584
	Acetate of amyle.....	0.863	1.390	0.001398	0.01559
g. C ² H ⁴ O, 2(C ² H ⁴ O)	Aldehyde.....	0.781	1.322	0.001239	0.01416
	Butyric acid	0.961	1.387	0.001243	0.01376
h. C ⁵ H ¹⁰ O, 2(C ⁵ H ¹⁰ O)	Valeral.....	0.799	1.377	0.001451	0.01672
	Valerianate of amyle.	0.858	1.400	0.001450	0.01630

These examples will certainly amply suffice to prove that we may consider the function X_v as constant as soon as the bodies are compared at the same temperature. Examples *g* and *h* even show that, for the polymers of optically identical elements, X_v is not merely a constant but has the same value.

In regard to the polymers of optical *allomeric* elements, I must refer to my "Studies"†.

In view of the examples given, it may fairly be doubted whether new analytical formulæ are necessary to establish the relation between density and the indices of refraction.

Moreover I did not satisfy myself with the above-mentioned facts alone before I decided upon the assertion that the retarding forces X_v always bear the character of constants for equal temperatures. A further proof, for instance, is possible, based upon formula II. If we multiply both sides of this equation (II.) with the chemical equivalent P , we obtain on the left the Newtonian

* For the calculation of M , the density of air is taken = 1.

† Compare Schrauf, 'Studies,' p. 215; Schrauf, *Physik der Krystalle*, p. 310.

refractive index $\mathfrak{M}$, and on the right the product PVX_v . Since V represents the atomic volume of the unit of mass, PV corresponds to the so-called specific volume ($Sp\ V$). Accordingly

$$P\left(\frac{\mu^2-1}{D}\right)=PVX_v; \text{ or } \mathfrak{M}=X_v(SpV). \quad (IV.)$$

If the derivation of this equation be correct, and if, further, as above assumed, the function X_v has the character of a constant, then for substances of analogous construction the following equation will be true:—

$$\frac{\mathfrak{M}_a}{\mathfrak{M}_b} = \frac{SpV_a}{SpV_b}. \quad (V.)$$

The agreement of the observed values with this equation is complete and indeed almost surprising. In the examples given below, I may remark that the refractive equivalents $\mathfrak{M}$ refer to the density of air as unit, while the specific volumes are derived by taking the density of water as 1. The proportionality of the two functions is not hereby disturbed; on the contrary, it becomes more manifest, since for the following series of analogous substances the proportionality becomes numerical identity*, as is shown in (IV.). Of course for the verification of (V.) only substances of analogous formation can be employed.

		$\mathfrak{M}$.	SpV .
a.	Propionic acid . . . $C^3H^6O^2$.	0·0855	85·4
	Valerianic acid . . . $C^5H^{10}O^2$.	0·1333	131·2
	Önanthylic acid. . . $C^7H^{12}O^2$.	0·1810	173·6
b.	Aldehyde . . . C^2H^4O .	0·0545	56·0
	Butyric acid . . . $C^4H^8O^2$.	0·1093	107·8
c.	Acetone . . . C^3H^6O .	0·0774	77·3
	Caproic acid . . . $C^6H^{12}O$.	0·1568	148·7
d.	Valeral . . . $C^5H^{10}O$.	0·1248	120·3
	Valerianate of amyle . $C^{10}H^{20}O^2$.	0·2494	244·1
e.	Acetate of ethyle . . $C^4H^8O^2$.	0·1077	107·5
	Carbonate of ethyle . $C^5H^{10}O^3$.	0·1382	138·8
f.	Anhydrous acetic acid . $C^4H^6O^3$.	0·1101	110·1
g.	Methylic alcohol . . C^4H^4O .	0·0387	40·8
	Ethylic alcohol . . . C^2H^6O .	0·0617	61·8
	Amylic alcohol . . . $C^5H^{12}O$.	0·1307	128·8

These examples abundantly prove that for equal temperatures we are justified in considering X_v as constant, and that, further, the formulæ which I have deduced are sufficient.

* Compare Schrauf, "Ueber die gesetzmässigen Beziehungen der Atom-Volumina und Refraktionsäquivalente," *Sitzungsbericht der Wiener Akademie*, 1866; also *Physik. Studien*, p. 119.

To conclude my proof, I must consider yet another possible objection—namely, it is possible that we might wish to make use of a second hypothesis for the explanation of the secondary abnormal variations of the refractive power, and to derive these, not from the variation of X_0 , but from an alteration in the number and magnitude of the atoms, making use of the equation $M = ZGX_0$. I myself should assent to such an explanation if it were possible to reconcile an alteration of the atoms with the hitherto conceived notion of conducted heat. The nature of heat compels us rather, as before, to consider length and rate in relation to the paths of the molecules.

The above developments are essentially based upon the Studies published by me at the end of the year 1866. As the latter contain so many improvements and additions to my former publications, I cannot discuss the latter. They fulfilled their purpose of directing the attention of physicists to a hitherto neglected field, and they started the conviction that we cannot, as was formerly the case, regard light and matter as being without direct mutual relations.

The relations and fundamental propositions first advanced by me (1862) I have entirely reconsidered, as just mentioned, in my work 'Equivalents of Refraction,' 1865, and 'Studies,' 1866. I have even retracted in the latter publications many of my former conclusions. This is notably the case with my former formula, $\frac{B}{D^2}$ (compare formula II.). Accordingly I consider it superfluous to reply to M. Rühlmann's remarks on this subject; for I can only explain M. Rühlmann's remarks by supposing that he neglected my later publications, or was ignorant of their existence.

The dispersive power adopted* by me in my "Studies," formula (II.), shows no well-marked relation to temperature; and as far as the observations hitherto extend, the variations of the dispersive power are, in liquid bodies, partly positive, partly negative, in relation to the normal value.

Having in the above paragraphs endeavoured to place the objections against my theory in their true light, I must in this place call attention to certain further facts in confirmation of Newton's equivalent of refraction.

Of late a second formula has been advanced to represent the relation between density and index of refraction. This formula,

$$\frac{\mu - 1}{D} = m = \text{constant}, \quad \text{(VI.)}$$

* $PN = \mathfrak{N}$ is called the equivalent of dispersion.

is based upon the data of Biot and Arago, which are to the effect that the variation of the indices of refraction is proportional to the increase of density. They say, "*La réfraction d'un même gaz quelconque est toujours rigoureusement proportionnelle à sa densité*"*. Biot and Arago nevertheless decide to retain the Newtonian refractive power, since for gases, as is well known,

$$\mu^2 - 1 = 2(\mu - 1),$$

and hence the investigations on gases do not make manifest any distinction between formulæ (II.) and (VI.).

Beer, in his 'Introduction to Higher Optics' (p. 36), has again brought forward this formula (VI.). In a similar manner it has been recently employed by Dale and Gladstone, Landolt and Haagen.

In my 'Equivalents of Refraction' (1865) I have submitted this formula to an examination. To distinguish it from Newton's refractive power, I called formula (VI.) Biot's refractive power, and denoted it by the letter m . In the same manner I determined for a great number of substances, not only the Newtonian refraction-equivalents $\mathfrak{N}$, but also those values corresponding to the formula

$$P\left(\frac{\mu-1}{D}\right) = Pm = m. \quad \dots \quad (VII.)$$

These values may also claim to be considered refraction-equivalents. I distinguish them by the name "Biot's refraction-equivalents" (formula VII.). These latter values, however, in accordance with chemical usage, may be called "little refraction-equivalents," since invariably $\mathfrak{N} > m$.

The values of $\mathfrak{N}$ relate to a unit such that $\mathfrak{N}(H) = 0.004$. In order to facilitate a comparison of the numerical values, I introduced another unit which is so chosen that $[\mathfrak{N}](H) = 1.0$, and I call the value of $[\mathfrak{N}]$ the reduced refraction-equivalent. In these last-mentioned values, which are simply relative, we get rid of every assumption concerning the unit of density used. The other values of $\mathfrak{N}$ and $\mathfrak{N}$ are calculated on the base of the density of the air being unity. To convert these into numbers corresponding to the density of water considered as unity, we have merely to multiply by 773.45.

From what has been above said, it is very clear that I have attached proper importance to Biot's formula (VI.). But yet I am still compelled to profess my adherence to Newton's refractive power.

The reasons for this are in part theoretical and in part practical. I shall mention several in the sequel.

* *Mém. de l'Inst. Paris*, 1806, vol. i. p. 322.

The conceptions and propositions of the undulatory theory are fully in accord with the Newtonian refractive power. Thus Lorenz finds*

$$\mu^2 = 1 + Dx - D \frac{y}{1 + Dx},$$

a formula which also involves the idea of the refractive power.

Secondly, the previous formula (I.) agrees with the deductions in the chapter on dispersion. According to Cauchy, to whom we owe our present theory of dispersion,

$$v^2 = a_1 + a_2 k^2 + \dots$$

For empty space, in which we must assume the dispersion to be a minimum,

$$v^2 = a_1.$$

For every other medium in which there is dispersion the velocity diminishes; the velocity a_1 , for so-called empty space, is diminished. Accordingly

$$v_1^2 = a_1 - a_2 k^2.$$

By means of

$$\mu^2 = 1 + \text{const.} \left(\frac{1}{\lambda^2} \right),$$

we obtain a formula agreeing with the formula (I.) used by me.

Apart from these theoretical propositions, the relations demonstrable numerically appear to speak in favour of Newton's refractive power.

In consequence of the equations

$$\mu^2 - 1 = 2(\mu - 1) + (\mu - 1)^2$$

and

$$M = 2m + Dm^2,$$

$$m = \frac{1}{2}M - \frac{1}{4}DM^2 + \dots$$

The two formulæ (I.) and (V.) are inseparably connected, inasmuch as the values of Newton and Biot's refractive powers form mutually sums and differences which are influenced only in a secondary degree by the variable element, the density. Hence it is easy to perceive that the constancy of the one formula partly involves the constancy of the other. In all cases in which, as in gases, the density is small, m and $\frac{1}{2}M$ coincide. A similar agreement is shown in those cases where the variation of density is very trifling throughout the members of long series of bodies. For the latter reason, which must by no means be neglected, the homologous series in organic chemistry will not serve to determine between the above formulæ.

In order, however, to determine whether the one or the other

* Poggendorff's *Annalen*.

of these formulæ (I.) and (VI.) is better in accord with the facts, I sought to compare widely different modifications of the elements, since we may presume that these are subject to much less variations than complicated combinations. Calculation gives:—

		$\frac{\mu^2-1}{D}=M.$	$\frac{\mu-1}{D}=m.$
Carbon . .	{ Diamond . .	0·001820	0·000529
	{ Anthracite . .	0·001829	0·000664
Sulphur . .	{ Vapour . . .	0·000492	0·000246
	{ Solid	0·002017	0·000659
Phosphorus .	{ Vapour . . .	0·000626	0·000313
	{ Solid	0·002437	0·000784
Arsenic . .	{ Vapour . . .	0·000217	0·000108
	{ Solid	0·000650	0·000247

The comparison of these numbers leads us to maintain the Newtonian refractive power M , since the latter are either equal or simple multiples. In the Biot's formula m no such unconstrained agreement can be seen. Even the law of *simple* factors is here of no avail, although it is evident that undetermined factors are also necessary for m .

The law of simple factors, however, is a universally valid one for all physical properties of the elements; for the consideration of the atomic volumes, specific heats, &c. always leads us to the distinct proposition, "The functional values of analogous elements are either equal or simple multiples of one another."

Apart from the above examples, the values of the Newtonian refractive powers $[(\mu^2-1)V]$ for the elements show in the clearest manner the law of the homologous series.

In the following Table these values are given side by side with their factors:—

$\frac{\mu^2-1}{D}=M.$		$\frac{\mu^2-1}{D}=M.$	
O . . =0·000489 . (1)		As _g . . =0·000216 . (1)	
S _y . . =0·000492 . (1)		N . . =0·000596 . (3)	
Se . . =0·001589 . (3)		P _g . . =0·000602 . (3)	
S _f . . =0·002016 . (4)		As _f . . =0·000641 . (3)	
Al . . =0·000846 . (1)		Sn . . =0·000652 . (1)	
Zr . . =0·000854 . (1)		Si . . =0·001247 . (2)	
Be . . =0·001640 . (2)		Ti . . =0·002520 . (4)	
Tl . . =0·000324 . (2)		Ba . . =0·000332 . (2)	
Ka . . =0·000483 . (3)		Pb . . =0·000479 . (3)	
Na . . =0·000670 . (4)		Ca . . =0·000664 . (4)	
Li . . =0·001646 . (10)		Mg . . =0·001290 . (8)	

In contradistinction to this series of M, the Biot's refractive powers $\left(\frac{\mu-1}{D}\right)$ show, on the other hand, no such regular sequence of values. I shall here disregard the numbers which I have reckoned for m , and give below the numbers $[D(H^2O)=1]$ given by Haagen* :—

	$m = \frac{\mu-1}{D}$		$m = \frac{\mu-1}{D}$
Oxygen .	0.1813	Phosphorus .	0.4710
Sulphur .	0.4606	Arsenic .	0.2510

No agreement can be perceived from these numbers between the optical values of these substances (which are so nearly allied chemically), although such agreement was clearly manifested in the above comparison of the values of M.

There are some who certainly will not fail to point to the small deviations of M from the absolute values of the multiples. But I think that every one who considers the matter impartially will recognize the law of multiples as that which determines, in the first approximation, the numerical values of the elements. And hence I consider that the above comparison has sufficiently proved that the optical function of the elements shown by the Newtonian refractive power M follows the law of serial factors.

I consider that such a regularity, when we fully consider the analogous chemical series, furnishes a further proof of the correctness of the views which I have hitherto held.

Finally, I must mention the important fact that I have succeeded, by means of the optical atomic numbers (also a consequence of my equation I.), in deriving the crystalline form of compounds from the volumetric values of the elementary atoms occurring in the compound†.

Thus one relation follows another in an unconstrained manner, and no one is in contradiction with the facts. The hypotheses which have been hitherto forced simplify themselves in many respects, and unexpected new relations appear at every step.

In the preceding I have declared my adhesion to the hypothesis of simple factors, and I even consider such an hypothesis, similarly to the method of volumetric calculation, necessary to establish the truth and possibility of an optical function.

I must here add that the defenders of the formula $(\mu-1)V$

* Pogg. Ann. vol. cxxxi. p. 127.

† Schrauf, *Physikalische Studien*, pp. 240–248. Pogg. Ann. vol cxxx. p. 433. *Physik der Krystalle*, chap. xi. pp. 160–172.

cannot avoid a similar assumption. To prove this I will adduce the following examples.

From the latest treatise by Descloizeaux* (kindly sent to me) I select the observations for octahedral arsenious acid, As^2O^3 . In this,

$$\mu_p = 1.748, \quad d = 3.698, \quad P = 198, \quad D(\text{H}^2\text{O}) = 1,$$

$$\frac{\mu_p - 1}{D} = 0.2022, \quad P \frac{\mu_p - 1}{D} = m = 40.03.$$

Now according to Haagen (*loc. cit.*), referred to the density of water $m_p(\text{As}) = 20.02$ and $m_p(\text{O}) = 3.00$. Hence, by calculation from the elements†, we get for arsenious acid,

$$2m(\text{As}) + 3m(\text{O}) = m'(\text{As}^2\text{O}^3) = 49.44,$$

a number which differs from the observed one to the amount of 20 per cent.

If, going further, we reckon the indices of refraction from the elements, it follows from $P = 198$, $d = 3.698$, $m' = 49.4$, that the coefficient of refraction $\mu'(\text{As}^2\text{O}^3) = 1.916$, while direct observation gives $\mu = 1.748$.

The hypothesis of factors, or, as I call it, condensation, is therefore not superfluous.

Another example, which is at the same time of importance for the derivation of the elements, may be found in the terchloride of phosphorus, PCl^3 .

I have (1865) given $D(\text{H}^2\text{O}) = 773$, $m(\text{PCl}^3) = 0.0576$, $m(\text{P}) = 0.0243$, $m(\text{Cl}) = 0.0110$; these numbers being derived from direct observation. If now we reckon the value of the compound from those of the elements, we find at once

$$m(\text{P}) + 3m(\text{Cl}) = m'(\text{PCl}^3) = 0.0573.$$

The difference between observation and calculation scarcely amounts to $\frac{1}{2}$ per cent. If we put $D(\text{H}^2\text{O}) = 1$, my data change to $m(\text{P}) = 18.81$, $m(\text{Cl}) = 8.58$, and for chloride of phosphorus we should get from observation $m(\text{PCl}^3) = 44.52$, and from calculation $m' = 44.55$.

It is seen that no better agreement can be imagined between direct observation and calculation by means of the elements. And yet this very body (PCl^3) induced M. Haagen (*loc. cit.*) to derive for phosphorus in combinations the value $m'(\text{P}) = 14.6$, a number which departs considerably from that obtained by the direct observation of free phosphorus, $m = 18.8$ [if $D(\text{H}^2\text{O}) = 1$]. The reason of this deviation is to be sought for in the fact that

* "Nouvelles recherches sur les propriétés optiques des Cristaux," *Mémoires de l'Académie*, vol. xviii. Paris, 1867.

† The functions derived from direct observation are denoted in the sequel by m , those obtained by calculation by m' .

M. Haagen takes for chlorine a value 1.2 higher, $m'(\text{Cl})=9.79$, than corresponds to the observation of the isolated element.

Haagen, it is true, by means of this higher value for chlorine, gains the power of deriving the refractive equivalents of the chlorhydrocarbons under the assumption of a small mean error. But the above example of terchloride of phosphorus shows that the employment of mean values may easily lead to mistakes. I myself in my "Studies," in considering the organic series, could have adopted many more simple derivatives for $\mathfrak{M}$, if I had neglected the consequent great differences in the equivalent of dispersion $\mathfrak{N}$. If the dispersion be neglected, the mean differences for the refraction between observation and calculation from the elements are certainly small, and in many cases a very simple derivation from the elements is possible. But such a procedure is still one-sided.

Indeed the numbers for the compounds of chlorine give, as it appears, a very easy proof of the necessity of factors, namely:—

The difference between the observed values for chlorine and hydrogen (here, and afterwards, $D(\text{H}^2\text{O})=773$) is

$$m(\text{Cl}) - m(\text{H}) = 0.011 - 0.002 = 0.009;$$

and this difference ($\text{Cl}-\text{H}$) appears on deriving many of the chlorhydrocarbons.

If we compare such substitution-products (compare my "Studies"),

- (a) Chloride of ethylene, $\text{C}^2\text{H}^4\text{Cl}^2$, $m=0.0410$ $\Delta=0.0147$.
 Bichloride of ethylene, $\text{C}^2\text{H}^3\text{Cl}^3$, 0.0557
 (b) Chlorobenzol . . . $\text{C}^6\text{H}^5\text{Cl}$, $m=0.0646$
 Trichlorobenzol . . . $\text{C}^6\text{H}^3\text{Cl}^3$, 0.0869 $\Delta=2(0.0112)$,

the differences Δ derived from the observations for ($\text{Cl}-\text{H}$) differ considerably from the former number, 0.09. In the first case $\nabla=0.0057$, in the second $\nabla'=0.0021$. If in this calculation $D(\text{H}^2\text{O})=1$, then $\nabla=4.40$, a deviation which attains almost 10 per cent. of the value of the observation itself.

Similar facts and the necessary consideration of the dispersion have caused me in my "Studies" to retain the theory of simple factors, and to fit this as closely as possible to the molecular processes of chemical substitution. If in this investigation it has appeared that all the atoms of an element which occur in a compound are not of equal value, and if, further, the theory of Newton's equivalents of refraction leads to the assumption of several modifications of the elements, both of these are in accordance with modern chemistry; for chemistry already recognizes four modifications of carbon, distinguished by their atomicity.

XXV. *On the Experiment of Mahomet's Coffin.*

By G. JOHNSTONE STONEY, M.A., F.R.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

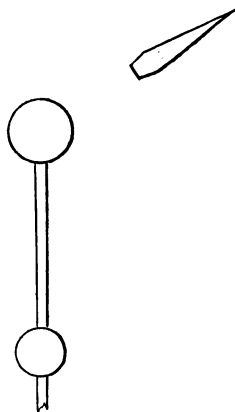
Dublin, August 12, 1868.

MR. W. F. BARRETT, during a visit to Dublin last spring, showed an uncommonly beautiful electrical experiment, which I understood him to say was described in the older treatises on electricity, but appeared to have since dropped out of recollection. I do not find any reference to it in such books as I have access to. You may perhaps, then, not deem it a waste of your space to recall attention to it, and insert an explanation of a phenomenon which at first sight seems very marvellous.

The experiment is easily made. Raise the knob of a Leyden jar to a considerably greater height than is usual. This may be done by boring a hole in the top of the ordinary knob of the jar and inserting into it a stem 3 or 4 decimetres long, carrying another knob. Charge the jar, and then drop down a little strip of gold-leaf towards the knob from a height of a couple of decimetres. The strip may be from 3 to 5 centims. long, and an attempt should be made to cut it into a form somewhat sharper at one end than the other. This is best done by heating a sharp penknife so as thoroughly to dry it, and then cutting the gold-leaf with tolerable pressure, and by a motion resembling minute strokes of a saw. All that need be aimed at is that the fragment have either more or sharper jags at one end than the other. If, when it is dropped towards the knob, the sharper end happens to be directed downward, the strip of gold will touch the knob and immediately dart away again; but if it be dropped with the blunter end downwards, it will approach without touching the knob and take up a position in its vicinity apparently without any support. There it will continue in mid air so long as the jar remains sufficiently charged.

The explanation may be conveniently divided into two parts—one respecting the distance at which the fragment of gold-leaf will establish itself, and the other explaining how its weight is supported.

At all distances the strip is charged by induction, electricity unlike that of the jar (which we shall suppose positively charged) being driven to the further and sharper end, while like electricity



is drawn to the nearer end, precisely equal quantities of both kinds being simultaneously evolved. Now, if the strip be far off, in which case the positive electricity of the knob will act with very nearly equal effect upon the two ends of the strip, positive electricity will escape from the sharp end more freely than negative from the blunter end. The strip will therefore become negatively charged and be attracted. On the other hand, if the strip be very near the knob, the difference between the forces with which the electricity of the knob acts upon the nearer and further ends will become so great that the negative electricity will be made to flow inwards, in spite of the bluntness of the inner end, more copiously than positive electricity escapes outwards. Here, then, the strip would become positively charged and be repelled. There is therefore an intermediate distance at which neither the repulsion nor the attraction preponderates; and at this distance the strip in the first instance places itself, or at least would place itself if it were devoid of weight.

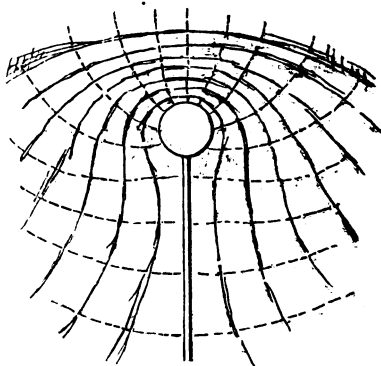
The gold-leaf may be regarded as acted on by three forces:—the relief of atmospheric pressure due to the attraction of the electricity of the knob upon the negative electricity of its nearer end (this is a force directed towards the knob); the similar relief of atmospheric pressure due to the repulsion of its positive electricity, which is a force acting outwards from the knob; and the weight of the gold acting downwards. If the two former forces lay in a right line, the weight of the gold-leaf could not be supported. We must therefore inquire into the disposition of the lines of force in the neighbourhood of the knob of a charged jar.

This may be done by first considering the form of the equipotential surfaces*, to which, as is known, the lines of force are

* If an electrified body were brought near the knob of a charged jar, it would by induction alter the distribution of electricity in the jar. But we may conceive a Leyden jar to be first charged, and that then the coatings, stem, and knob become perfect non-conductors. In this hypothetical case the approach of other electrified bodies would not alter the charge in the jar. Let us suppose that the charge in the jar is positive, and that a particle charged with one unit of positive electricity is taken from a position in which it is not acted on by any electrical forces, and brought up to a point P in the neighbourhood of the knob. The repulsion of the charge in the jar of course opposes this motion; and it will therefore require the expenditure of a certain amount of mechanical energy, or, as I should prefer to call it, *labour*, to overcome the electrical repulsion. This labour may be expressed in foot-pounds, kilogrammetres, or other more convenient units. The number which expresses the amount of labour that we must apply in order to carry the particle up to the point P is what is called the *potential* at that point of the electricity in the jar.

If all the points be found which require the same expenditure of labour as P does to carry the unit particle of electricity from a neutral position up to them, they will all lie on a surface which is called the *equipotential surface* passing through P.

everywhere perpendicular. The charge on the ball of a Leyden jar is evidently most intense on the top of it, and gradually feebler down its sides. Hence the force exerted on a molecule of free electricity close to the knob is greater if the molecule of electricity be above the ball than if it be at the same distance beside it. And accordingly, if the equipotential surfaces corresponding to the potentials V , $V - \alpha$, $V - 2\alpha$, &c. be drawn, V being the potential of the inside coating of the Leyden jar, and α being a small quantity, the first of these surfaces will be the surface of the ball and stem, and the others of the form represented by the unbroken lines in the diagram, since it is a property of these surfaces that the interval between them at any point of space is inversely as the intensity of the force at that point, so that they lie closest to one another above the ball, where, as we have seen, electrical attraction and repulsion are most intense, and further asunder at the sides, where the electrical force is feebler. Having now ascertained the general form



of the equipotential surfaces, we have only to draw perpendiculars to them everywhere in order to have the directions of the lines of force. It thus appears that these latter are curved upwards, as represented by the dotted lines in the figure, and that the repulsive force acting on the further end of the gold-leaf is accordingly more steeply inclined to the horizontal than the attractive force acting on its nearer end. Hence they have a resultant acting upwards, which may balance the weight of the gold-leaf.

If this resultant be too strong, it will draw the gold-leaf up into a higher position, in which the lines are less curved. If, on the other hand, the weight preponderate, it brings the gold-leaf into a lower position, in which the electrical forces acting on it become more inclined to one another. Now the effect of the increased inclination of these forces in the lower position, which tends to increase their upward component, may or may not preponderate over the reverse tendency of their diminished strength. This will depend on the size of the knob, and on its elevation above the outer coating of the Leyden jar; for the smaller the knob, the more intense will the accumulation of electricity on its top be as compared with that on its sides, and, accordingly, the more will its lines of force be bent upwards. And, again, since

all the lines of force must ultimately find their way to the outer coating or some other neutral position, those which begin by curving upwards must after a time incline in the opposite direction; and this will take place nearer to the knob the shorter the stem is which raises it. But if the knob be not exorbitantly large, and if it be sufficiently stilted up above the jar, the vertical component of the electrical forces acting on the strip will go on increasing from the top of the ball and for some distance down its sides; so that if the gold-leaf be not too heavy, it will find a position of stable equilibrium.

Again, if the resultant of the electrical forces be not directed through the centre of gravity of the gold-leaf, a couple will be formed which will incline the strip of gold-leaf so that, instead of lying along one of the lines of force, it will lie in some degree athwart them. But the new directions of the electrical forces will manifestly produce an opposing couple which will bring about equilibrium.

And, again, if the electrical forces produce a horizontal as well as a vertical component, this will cause the slip of gold-leaf somewhat to approach or withdraw from the knob. In either case its position may, and probably will, be different from that in which it discharges positive and negative electricity from its two ends in equal quantities, and in which alone its own electrical condition can remain constant. If, then, when the strip is in this position of constant charge, it happen that all the forces acting on it produce *e. g.* an *inward* resultant, it must approach the ball and thus come into a position in which it quickly becomes positively charged, from which it is therefore again driven outwards beyond the position of constant charge into a situation in which it becomes negative, from which it is therefore drawn in again. It thus flutters between two positions on either side of the position of constant charge. I have not seen this motion in the main body of the strip, although it certainly exists; but I have seen a fragment which happened to be slenderly attached to the side of a strip quivering with a motion which was no doubt due to it.

It now only remains to notice some minor details. I have seen the strip spin on its longer axis. This was doubtless because the jags, which it is difficult to avoid in cutting gold-leaf, happened on that occasion to turn upwards along one side of the strip and downwards along the other, so that it was moved like the electrical fly-wheel. Similarly, if the jags along one side allow a freer escape of electricity than those along the other, as often happens, the little strip traverses horizontally in a cone round the vertical line through the knob; for the little strip of gold commonly assumes a position in which its transverse dia-

meter is horizontal. If one side of the strip were lower than the other, it would be in a situation in which the electrical forces are both feebler and more horizontal. The attractive forces therefore tend to restore the transverse diameter to its horizontal position, the repulsive forces tend to draw it from it; and which of them it will obey depends on whether the attractive or repulsive forces have the greater moment round the longer axis of the strip. But if the moment of the repulsive forces preponderate, the plane of the strip becomes vertical, and any inequality of the jags along its sides will carry it up or down from its position of equilibrium, so that it probably soon flies off. It appears, therefore, to be only in those cases in which the moment of the attractive forces preponderates that the experiment is likely to succeed. Hence it would seem that a good shape for the strip would be that of an arrow-head with a sharp point and two blunt barbs. Finally, I have seen the strip take up a position several centimetres below the knob, showing that even lines of force which started from the stem curved upwards.

I am, Gentlemen,

Your obedient Servant,

G. JOHNSTONE STONEY.

XXVI. *A New Theory of Vision*.

By SAMUEL ROWLEY, M.A.*

MY purpose in the following communication is to offer a theory of vision which I believe has not been heretofore advanced, and which I venture also to believe to be the true one. But before entering upon its consideration it may be proper briefly to state, with reasons for rejecting them, the theories hitherto advanced.

The theory of Aguilonius supposes that all the images impressed on the retinæ at any given instant are seen in a plane perpendicular to the plane of the optic axes at their point of convergence, and parallel to the line joining the centres of the two pupils—and consequently that the two images of any object situated in this plane, occupying, as they necessarily do, one and the same place, are seen single with two eyes.

If any person, whom a little practice has fitted to make such experiments, will place at some short distance (say, 3 inches), in a line parallel to the eyes two small objects, one directly before the central point of the interval between the eyes, and

* [We are indebted to the author for proof-sheets of this paper in advance of the issue of the September Number of Silliman's Journal, to which it was first communicated. Eds.]

the other at a distance from this, say, $\frac{3}{4}$ of an inch, and direct his optic axis to the former, he will not see the two images of the latter as one, though their object is situated in the plane mentioned, but as two distinct images, the one beyond the other.

Experiments to the same effect might easily be multiplied; but the one given abundantly demonstrates the inaccuracy of the theory of Aguilonius.

The theory of *corresponding points*, so called (as held on physiological grounds by Galen, Alhazen, and Newton), cannot be denominated a theory of vision. It is directed to the accounting for but one of the phenomena of vision, that of single sight with two eyes. It does not deal with visible direction and distance, whereas these conditions, governing, as they do, external visible position, include the result of single sight with two eyes as a necessary consequence.

The theory of vision propounded by Dr. Smith in his 'Optics,' and that by Dr. Wells, from which he derives his explanation of the phenomenon of single vision with two eyes in his 'Essay on Single Vision with Two Eyes,' are practically (as I trust will satisfactorily appear hereafter) merely attempts to formulate the misapprehensions by consciousness of true visible distance which take place under the influence of habit. A simple experiment subversive of both these theories may be made thus. Let two small objects, as pins in a board, be placed directly in front of the middle point between the eyes, the one at some short distance, say, 6 inches, and the other at some more considerable distance, say, 18 inches, and let other two small objects be placed on opposite sides of the near one in lines between the eyes and the far object, and the optic axes be directed to the near object; then the two images of the remote object will be found at no greater distance from each other than the images of the objects at the sides of the near one, or all will be apprehended as lying in the figure of a rectangle.

But by the theory of Dr. Wells* the two images of the remote object (since this object lies in the direction which he has named the common axis) are seen in the optic axes, the right eye's image in the left eye's axis, and *vice versa*, and at a distance from each other (if located at the same distance from the eyes as the object)† of $4\frac{8}{10}$ inches, whereas the images of the two lateral near objects, since these occupy the line named *horopter*, will be seen, in the places of the objects, at a distance from each other of $1\frac{6}{10}$ inch‡.

As to the two images of any object more or less distant than

* Essay upon Single Vision with Two Eyes, p. 38. London, 1818.

† Ibid. page 27.

‡ Ibid. pages 5 and 46.

the convergence of the axes, the theory of Smith places them in lines drawn from the eyes to the object somewhere between the place of the object and the point of convergence, "but not very far from the real place," *i. e.* the place of the object*. Hence, while the two images of the object on either side of the point of convergence will be seen coincident with their object at a distance of $1\frac{5}{10}$ inch from the two images (seen in like manner) of the object on the other side, the two images of the remote object will be seen at a much less distance from each other, or, following the proportions of the construction given us by that author, at a distance of $\frac{5}{10}$ or $\frac{6}{10}$ inch.

Aguilonius is the only author who, meeting the question in its full extent, undertakes to assign for each image both the line of visible direction and the absolute visible distance from the retina on the line of visible direction.

Sir David Brewster indeed gives both conditions of external visible position for the images falling *on the vertices of the retinae*. But for every other image he merely assigns Kepler's line of visible direction, being silent regarding the visible distance—the essential complement of the former in determining visible position.

I will now propose and proceed to put to the test of experiment what I believe to be the true theory of vision—premising the remark that in establishing its existence it will not render the proof offered less exact if I do not undertake to determine the precise point where a given ray† strikes the retina, nor the precise point at which the line of visible direction crosses the optic axis, provided we admit (what I think will not be disputed) that where the angles with the optic axes of two rays falling one on each cornea are equal, the distances of the images on the retinae from the extremities of the axes are equal, and provided we admit (what I also think will not be disputed) that where the angle with the axis of one of the rays is but *very slightly* increased, the angle formed after decussation is not appreciably *unequally* increased—and further premising that I will neglect any effect of refraction, and also suppose a coincidence of the line of the ray with the line of visible direction.

All the component points of the total impression on the retina of either eye, before becoming cognizable mental phenomena, are simultaneously, and in lines drawn from them through a point situated, say, a little behind the centre of the crystalline lens, referred outward to a surface parallel to the impressed surface of the retina conceived to be simply expanded coextensively with the impression

* Smith's 'Complete System of Optics,' art. 137.

† Here, and wherever in this paper the word ray is used, it is intended to signify the central or axial ray of the pencil.

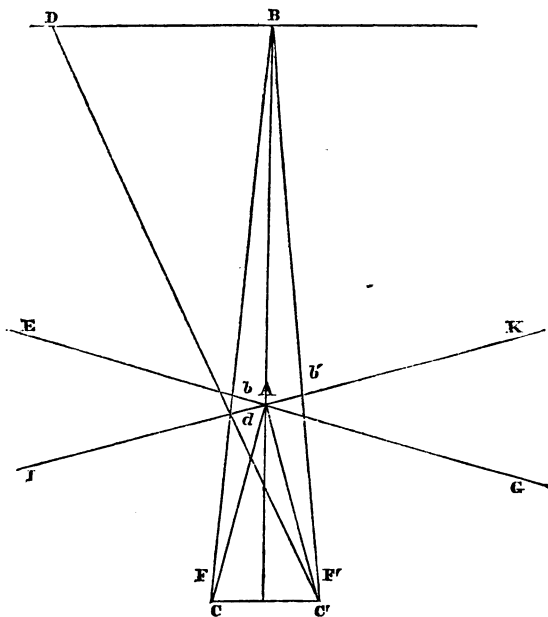
as expanded by outward reference, and cutting at right angles the optic axis of that eye at the point where it intersects the optic axis of the other eye.

Or, to change the form of statement, *the entire impressions on the retinae, before becoming objects of consciousness, are projected in space upon surfaces bisecting each other (perpendicularly to the plane of the axes) at an angle greater or less according to the distance—the component points of each impression being simultaneously referred outward in lines passing from them through a point a little behind the centre of the crystalline lens, but, excepting the expansion and the inversion resulting from the crossing in the eye of the directions of outward reference, undergoing no change of relative position—the distance between the planes passing at right angles to the optic axis through any two of the successive concentric zones of points which make up the retinal impression continuing the same.*

I do not mean here to assert that an outward projection actually takes place, but that the *effect* of such a projection produced by an intermediate process of mind is presented to the consciousness.

On a suitable plane surface take two points, C, C' (fig. 1),

Fig. 1.



2 $\frac{4}{10}$ inches apart, and in a line bisecting at right angles one

joining these points take other two points, A, B, distant respectively from the point of bisection 6 and 18 inches. Draw the straight lines CA, C'A, and perpendicular to them at A the two lines EG, IK. Through the points F, F', taken in the lines CA, C'A, $\frac{1}{10}$ inch from C, C' draw the lines FB, F'B, intersecting EG, IK, at b, b' . Through the points F' and d (a point in IK where a line passing through b perpendicular to CC' intersects it) draw a line. In this line, where a line passing through B parallel to CC' intersects it, take the point D. The distance of this point from B will be $4\frac{9}{10}$ inches.

Now suppose the points C, C' to represent the centres of the eyes; points F, F' the focal centres; A, B, D the situations of luminous points; the lines CA, C'A the optic axes produced; the lines EG, IK lines of intersection where two surfaces slightly convex toward the eyes, perpendicular respectively to the optic axes at their point of meeting, cut the plane of the axes; BF, BF' lines of luminous rays passing from the point B through the points F, F'; DF' the line of a ray passing from D through F' to the retina; b the point where the line BF intersects the line EG; and d the point of intersection of DF' with IK. Then, if the above-enunciated proposition be true, to wit, that an impression made by a luminous point on the retina of the right or left eye becomes visible and is seen where a line drawn from it through a point a little behind the centre of the crystalline lens pierces a slightly convex surface perpendicular to the optic axis of that eye at the place of its meeting with the axis of the other eye, the image of the point at B will be seen from the left eye at b , and the image of the point at D from the right eye at d , both in the same perpendicular of no variation*, or the image of the point at B will be seen by the left eye a little behind the place in which the image of the point at D will be seen by the right eye.

The experiment may be made thus:—Take a thin rule (as a plane scale) 12 inches in length with the right hand, and another rule with the left hand. Hold the rule in the right hand with its wide sides perpendicular to the plane of the horizon, in a line running away from the middle point between the eyes so that the near end shall be distant 6 inches, and the remote end rest upon a horizontal bar of a well-lighted window at right angles to the rule, and parallel to the eyes.

To obtain the former distance with accuracy, it is necessary to have two small objects, as two pins, placed equidistantly on opposite sides of the point of intersection of the bar by the rule, distant from each other $4\frac{8}{10}$ inches; and when these shall be

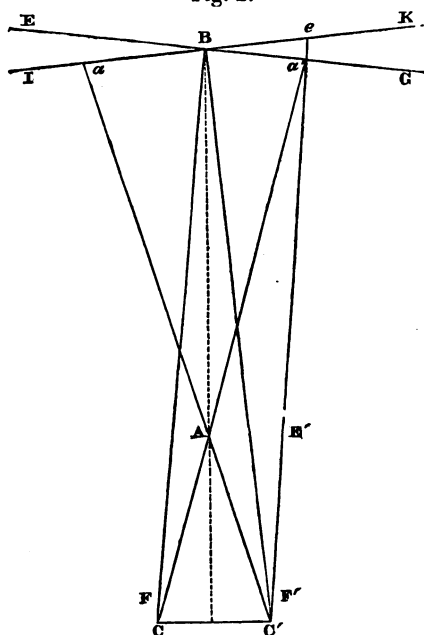
* By *perpendicular of no variation* is meant a line perpendicular to the base of vision, for the definition of which see note, p. 203.

seen across (or lie in the axes exactly fixed upon) the near end of the rule, that distance for eyes $2\frac{4}{10}$ inches apart will be accurately found. Now fix the axes of the eyes on the near end. The images of the remote end will appear as two at the sides, that of the left eye at some unknown distance on the left, and that of the right eye in like manner on the right. Holding the rule in the left hand in the same position with respect to the plane of the horizon as the other, apply its remote end on the window-bar to the left of the remote end of the rule in the right hand, so that the right eye's image of the former will cover the left eye's image of the latter. Now measure the distance between the far ends on the bar. If all the conditions of the experiment shall have been exactly complied with, the distance found will be $4\frac{9}{10}$ inches, as predetermined in the diagram.

The experiment, then, shows that the geometrical consequence of the principles of the proposition, as illustrated in the diagram, holds good in actual practice, and that when the axes are fixed upon the images of an object placed as a luminous point at A, the left eye's visible image of an object placed as a point at B in the line FB, is seen a little beyond where the right eye's visible image of an object placed as a point at D in the line drawn through F and *d* is seen, both in the same line of no variation. It is obvious by the proposition that it ought not to affect the "conjunction" of the images, wheresoever in the line passing through F' and *d* the object might be placed and this will be found to be the fact. It was placed opposite B at D merely for convenience of applying the diagram in actual experiment, and to procure a correct appreciation of the interval between the images.

Suppose, in the second place, the axes represented by CB,

Fig. 2.



C' B, fig. 2. Then by my proposition the left eye's visible image of the luminous point at A should be seen at a' in a surface cutting the axis F B at right angles in B (and the plane of the axes in the line E G), and the right eye's image at a .

But if the left eye's image of the point at A is seen at a' , then by the same proposition the right eye's image of the point at E' opposite A in a line between F' and e , a position in I K directly beyond a' should be seen in the same line of no variation a little beyond a' at e . If the experiment be made, it will be found that the right eye's visible image of an object placed as a point at E', nearly $1\frac{8}{10}$ inch from A, will be seen a little behind the place where the left eye's image of an object placed as a point at A is seen.

So the two middle images of the pins placed as a test of distance in the first of these experiments are seen united at the intersection of the surfaces of vision.

If the surfaces of vision were planes, since their line of intersection would then coincide with the line of intersection of the vertical planes of the axes, it is evident that the images of these pins longitudinally bisected by the latter planes would exactly coincide. But, owing to the slight curvature of the surfaces of vision, the line of intersection of these surfaces immediately begins very gradually to recede from that of the vertical planes of the axes, and the longitudinal axes of the images of the pins to cross over and fall on opposite sides, thereby preventing a mathematical coincidence of the images.

This effect of the departure of the intersection of the surfaces of vision (supposing the optic axes parallel to the horizon, to which the pins are perpendicular, to be directed to the bottoms of the images) may be counteracted and a mathematical coincidence of the images nearly produced by inclining the pin whose image is seen by the left eye a very little to the left, and the other duly to the right. Should this small inclination be given with such a curve that a line drawn from any point of the axis of the pin to the focal centre of the eye would intersect the line of intersection of the surfaces of vision, then an exact mathematical coincidence of the images would be secured.

The approximate result may be obtained if two rulers*, or the legs of a pair of compasses†, be so placed that their longitudinal axes shall lie in the vertical planes of the optical axes.

It is evident that for the two objects in the vertical planes of the axes, we may put a single object at the intersection of these planes‡.

But to make the accurate performance of these experiments

* See Wells, p. 36.

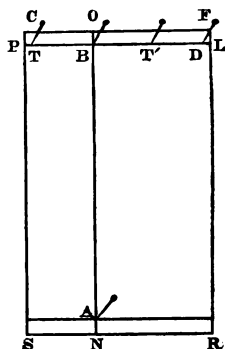
† Smith's 'Optics,' art. 977.

‡ Wells, p. 49.

easy, some special instrument must be provided. A piece of pine board whose opposite great surfaces are rectangles parallel to one another, 13 inches long and 8 inches wide, with five pins, furnishes the means for obtaining accurate results where the distance between the near and far points does not exceed 12 inches*.

To use this contrivance (supposing the distance from the middle of the line between the centres of the eyes to the near objective point to be 6 inches, and from the near to the far objective point 12 inches), in the surface $S R F C$ (fig. 3), $\frac{1}{2}$ an inch from the short side $S R$, and 5 inches from the long side $R F$, take the point A . Through this point and at right angles to the side $S R$ draw a line $N O$. In this line, 12 inches from the point A , take the point B . Through this point, and at right angles to this line, draw another line $P L$. In this latter line take the two points T, T' , each distant $2\frac{4}{10}$ inches from the point B , and $4\frac{8}{10}$ inches from the other; and finally $4\frac{2}{10}$ inches from the point B take the point D . At each of the points so taken set a pin. Now place the board in a horizontal position a little below and parallel to the plane of the axes, with the pin at A 6 inches from the middle of the line joining the centres of the eyes, and, together with the pin at B , in the vertical plane which bisects this line at right angles; which disposition of the pins is had when, looking at the images of the pin at A , the left eye's image of the pin at T' and the right eye's image of that at T are found lying in the axes.

Fig. 3.



When now the axes are fixed upon the images of the pin at A , the right eye's image of the pin at B will be seen in the same line of no variation as the left eye's of the one at D , and at a point a little beyond where the latter is seen; and when they are fixed upon the images of the pin at B , the left eye's image of that at A will be seen in a position a little before where the right eye's image of a pin, set nearly $1\frac{6}{10}$ inch directly to the right of A , will be seen, the distance between these being sensibly less than that between the images received from B and D when the axes were converged toward A .

* Another board of the same width and 18 inches long, serving as a support to this, to be held with one end against the nose by the hands pressing both pieces at the sides, will be found serviceable for maintaining steadiness of position.

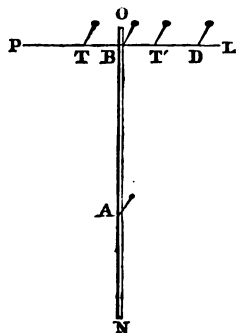
A still better instrument is a small wooden cross consisting of a slender bar and sliding transverse piece, which may be any slip of wood (as a piece of common lath) notched at the centre so as to receive the longitudinal bar in its full thickness at right angles, and also capable of being shifted and fixed by wedging at any point.

For all experiments in which the distance of the remoter of the two stations designated A and B, figs. 1 and 2, is not greater than 18 inches, the length of the bar should be 18 inches, and that of the transverse piece 12 inches.

To apply this instrument, instead of the window-bar and rulers in the first of the examples taken, fix the transverse piece at the distance of 17 inches from one end of the shaft, and laying the cross on a table with its level side upward, draw on the upper side of the shaft lengthwise the bisecting line NO , and lengthwise on the upper side of the transverse piece the bisecting line PL . At B , the point of intersection of these lines, set a pin, and at the point A , 12 inches from the point B , in the line NO , also one. At the points T, T' in the line PL , each distant $2\frac{4}{10}$ inches from B , set two other pins, and at D , $4\frac{9}{10}$ inches from B , another. Then placing the cross so as to lie with the extremity N of the shaft against the upper part of the nose, and its upper surface a little below and parallel to the plane of the axes, and keeping the head fixed, look with each eye in rapid succession at its image of the pin at A . If the right eye's image of the pin at T , and the left eye's of that at T' , be seen in these directions of the axes, the pins at A and B are 6 and 18 inches distant from the middle point of the line joining the centres of the eyes. But if they should fall within or without the axes thus directed, then the transverse piece, together with the pins at A and B , 12 inches apart, must be shifted slightly forward or backward until the requisite position is found.

Holding this instrument in the same position with respect to the eyes as that just above given to it for ascertaining distance, turn it toward a strongly illuminated white wall or sheet of white paper, or (which is better) the clear sky. If now the axes be fixed upon the images of the pin at A , the right eye's image of the pin at B and the left eye's of that at D will be seen lying in the same line of no variation, the former a little beyond the latter. But if, having transferred the transverse piece to the

Fig. 4.



distance of A, the axes be fixed upon the images of the pin at B, then the image of the pin at A seen by the left eye, and that of a pin placed nearly $1\frac{6}{10}$ inch directly to the right of A seen by the right eye, will be found lying in the same perpendicular of no variation, the latter beyond the former, the distance between the two images being less than in the first case.

In experimenting with this instrument, the position of the image of the pin at B seen by the left eye in a surface at right angles to the optic axis of that eye is visibly realized. For when the axes are fixed toward A (as in the first instance), the image of the transverse piece seen from the left eye visibly has a position at right angles to the axis of that eye, and intersects the corresponding image of the right eye, in a line of common intersection crossing each image where that image, whether referred back to the retina or to the distance of the transverse piece itself, would be pierced by the optic axis.

In making these experiments, care should be taken not to turn the axes into the oblique directions, to do which there is always a strong inclination; for then the images which were seen in those directions as described, the one before or behind the other, would be seen coincident.

I will now proceed to answer an apparent objection. In the first demonstration under my proposition, I undertake to show that the impression on the left eye from B becomes visible and is seen at *b*, by showing that it is seen a little behind where the impression on the right eye from an object placed at D is seen by the right eye, arguing that nothing else than a projection in the manner described can account for the local relationship in which these impressions are seen. The evidence of consciousness, then, that these impressions are seen in a certain local relationship, is made the groundwork of my argument. Now it may be urged that the evidence of consciousness is equally strong that the impression from the object at B on the left eye, when seen, lies in the axis of the right eye at the actual distance of the object itself at B*.

In the first place, let us determine whether consciousness, when carefully examined, does really continue to find this impression elsewhere than at *b*. To reveal in consciousness the true distance in a lateral direction of the visible image at *b* of an object at B (fig. 1), employing the pins and board, place a pin at *b*. The left eye's image of the pin at B will not be recognized to the left of the left eye's image of it.

Again, take three equal small rings of pasteboard. Attach to each, in the direction of a radius produced, a pin, by cementing with a drop of sealing-wax the head of the pin to the edge of the ring.

* See Wells, pp. 38 & 27.

At B, fig. 3 (the board duly widened), at a point $4\frac{8}{10}$ inches to the right of B, and a point of $4\frac{8}{10}$ inches to the left, station these pins, so that the rings shall be parallel to the eyes of the observer. At the points A, T, T', set simple pins. Now on converging the axes toward A, till the right eye's image of the pin at T and the left eye's of the pin at T' are hidden in the combined images of the pin at A, the images of the rings will be apprehended in their true visible places.

To make another experiment of the kind to which the last belongs, on a glass of a window situated at a convenient height stick a number of wafers, placing their centres in a horizontal line and separating the adjacent centres by a distance less than that between the centres of the eyes, but being extremely careful to make all these distances exactly equal. Direct the axes through the centres of two adjoining wafers, each axis through that centre lying on its own side, and presently all the visible images (except usually the left terminal image of the right eye's series and the right terminal one of the left eye's) will be recognized by consciousness in their true visible positions.

If the adjoining centres of the wafers are very close to each other (as a little over $\frac{1}{2}$ inch, supposing the wafers to be $\frac{1}{2}$ inch in diameter) and the eyes at a short distance, say, 4 inches, *the curvature of the surface of vision will be plainly appreciated*, and if the plane of the axes be turned on the line joining the intersection of the axes to the centre of the interval between the eyes from a coincidence to an angle with the horizontal plane passing through the centres of the wafers, *the curvature of each eye's series will be separately appreciated*.

An experiment of the same kind may also be made by means of the similar figures lying at equal distances in the surface of a papered wall.

If the experimenter will station himself facing such a wall and converge his optic axes so that the axis of his left eye shall pierce the centre of some figure in front of him, and that of his right eye the centre of the next one to the left of the other, then shortly each eye's image of the papered wall will, to his consciousness, take its true visible position at the distance of the convergence of the axes—the images of the figures appearing smaller or larger, according as the distance of the point of convergence is less or greater. A variation (from the standard) of the distance between the centres of the figures of the wall will produce a seeming elevation or depression of the visible images. The cause of this last, as well as that of the principal phenomenon, will be explained when it shall have been explained why the lateral images of an object which is at a greater or less distance

than that of the convergence of the axes are not apprehended in their true visible positions in the surfaces of vision.

Why, then, does consciousness mistake, precisely as we find it doing, the true place of the visible image of any object, lying not at the point of intersection of the axes, nor at any point equally distant with this from the base of vision*?

My answer is, that under tendencies resulting from experience, consciousness apprehends erroneously the distances really presented for its action according to the laws of vision, and with respect to each lateral image apprehends the distance of the surface of vision from the base of vision, and the distance in the surface from the image at the centre, for the distances that would exist between these surfaces and these images if the given image now laterally and obscurely seen were, with its corresponding image of the other eye, directly and distinctly examined at the centre.

Hence the place in which such an image seems to be seen.

For example, let the axes be turned to the images of an object at B (fig. 2). They will be seen at B, the left eye's $2\frac{4}{10}$ inches to the left from its image at a' of the object at A, and the right eye's $2\frac{4}{10}$ inches to the right from its image at a of the object at A, each in a surface of vision measuring, from the apex, 18 inches from the base of vision.

Let, now, the axes be turned to the images of the object at A (fig. 1). These will now be seen at A, that of the left eye's $\frac{8}{10}$ inch to the right from its image at b of the object at B, and the right eye's $\frac{8}{10}$ inch to the left from its image at b' of the object at B, each in a surface measuring, from the apex, 6 inches from the base of vision.

At first, then, the left eye's image of an object at B, when particularly examined, will always be apprehended at the distance of $2\frac{4}{10}$ inches to the left from the image at a' of an object at A in the surface of vision 18 inches from the base. Hence, after due experience, will arise a tendency of mind to apprehend this image at a distance of $2\frac{4}{10}$ inches to the left from the image of the object at A in the surface of vision 18 inches from the base. Therefore, when the axes are turned toward the images of the object at A, the mind will be influenced by this tendency, with respect to the left eye's image at b of the object at B, to apprehend 6 inches, the distance of the surface of vision from the base, as 18 inches, and $\frac{8}{10}$ inch, the distance in the surface from the image at A of the object at A, as $2\frac{4}{10}$ inches.

Again, the effect of experience in regarding directly the images

* By *base of vision* I mean a plane cutting perpendicularly, at the centre of the interval between the eyes, a line drawn through this point and the point of convergence of the optic axes.

of the object at A will be a habit whereby, when the axes are directed to the images of the object at B, the mind will be led to mistake $2\frac{4}{10}$ inches (the distance in the surface of the left eye's image at a' of the object at A to the right of its image at B of the object at B) for $\frac{8}{10}$ inch, and 18 inches (the distance of the surface of vision from the base) for 6 inches; and $2\frac{4}{10}$ inches (the distance of the surface of the right eye's image at a of the object at A to the left of its image at B of the object at B) for $\frac{8}{10}$ inch, and 18 inches (the distance of the surface of vision from the base) for 6 inches.

Thus, since the more remote parts of a solid object will seem to be seen at equally more remote distances in the image, and the entire image at a distance equal to that of the object, the image will seem to be seen with the same form and magnitude as really pertain to the object*.

We are now also prepared to explain the cases in which the visible image, though the object be situated at a greater or less distance from the base of vision than the point of convergence of the axes, is accepted by consciousness in its true place, as in the experiments with the equidistant pins, with the wafers upon glass, and the figures on the papered wall.

In these cases it will be perceived that any two images seen as one, though proceeding from two different objects, have the same positions in the surfaces of vision as they would if they proceeded from a single object placed at the intersection of the lines drawn from the two different objects to the eyes. Further, so great is the propensity to appreciate singly, that any slight discrepancy between these images and those produced by one and the same object will not prevent an acceptance of a correspondence; and since all the equivalent single objects would lie at the same distance, no habit can exist to influence the mind with respect to any images to locate the surfaces of vision at a greater or less distance from the base of vision than the true. Hence all the images will be apprehended in their true visible places. But if the interval between any two objects of such a series should differ from the other intervals, then for the two (of the four images presented) which are accepted as corresponding, or capable of being seen singly in direct vision, if these should be lateral (or if central, then for all the rest), from the necessary principle of habit above laid down, the surfaces of vision, measured from the common centre, will seem to exist at such a dis-

* In ordinary lateral vision (*i. e.* vision had at the sides of the line of intersection of the surfaces of vision) two corresponding images, or the corresponding parts of two complex images, at the concurrence of the axes are appreciated singly by reason of inattention to, or neglect of, one of them.

tance from the base of vision as that at which they would if these images were directly seen in the axes.

In this experiment with the papered wall, with every movement of the head a movement of all the images in the same direction takes place; because with the eyes fixed toward any two figures, a movement of the head in any direction will carry in the same direction all the points of intersection of the lines of direction, and consequently all the images in the two surfaces. So when the axes are made to pass through any two of a series of equal and equidistant objects to a point beyond, as in the case of the experiment with the wafers upon glass, a movement of the head in one direction will produce a movement of the images in a contrary direction.

There are other circumstances incident to a lateral image in the surface of vision, besides distance from its twin, which will produce habits operating to defeat a correct appreciation of the two distances.

Such a circumstance is the visible magnitude.

Thus, whatever the visible magnitude of an aggregate lateral image, which has been often seen in direct vision, this magnitude will be taken for that found on particular examination, and the visible distance for that corresponding to that magnitude. But since lateral images in the surface of vision not distinguishable from each other by consciousness may result from objects of very different magnitudes placed at suitably different distances, a given magnitude of image will not always excite the same tendency of mind.

To illustrate this, take the board with pins placed at A and B (fig. 3), depress the far end till the surface is hidden by the near end, and stopping the light flowing from the near pin to one of the eyes by interposing near the eye an adequate object, fix the axes of both toward the remote pin. As soon as the eye has ceased its alternations of adjustment (which have been performed so rapidly as to keep up a sensibly sustained distinctness of both images), and no longer experiences the sensation of focal adjustment to the near pin (which has excited a tendency of mind to mistake the image at the distance of the object), the now shadowy image of this pin will take its position beside that of the far pin. This it will generally do slowly, because the image in the surface might have resulted from a pin of due dimensions situated at every intermediate point, but in continuous succession giving up every intermediate position, it will finally come to rest in its true visible place. Occasionally, owing to the dominancy of some habit, it will seem to take a place at a greater or less distance than the true.

But habits created by magnitude may cause mistakes of visible distance involving also *the image in the axis*.

Thus, for example, if the mind has been accustomed to appreciate the images of mountains of moderate magnitudes, thereby tendencies will be impressed upon it to appreciate moderate dimensions with images of this class. Hence, when the image of a mountain of extraordinary magnitude is presented (in an ordinary state of the atmosphere), consciousness will be influenced to appreciate the dimensions as less than they really are, and, in order to this, the distance from the base of vision as a duly less one. And the effect on any concomitant images, as of trees or animals, would be a like seeming diminution of their visible magnitudes. For it is plain that if consciousness makes a mistake of distance, then, in obedience to the law of habit, it will assign that size which the image would really have at that distance, or that effect of lateral distance, or resulting expansion, which the divergence of the lines of direction would give at that distance.

Dr. Abercrombie relates the following instance as having occurred in his own experience. "I remember," he says, "once having occasion to pass along Ludgate Hill when the great door of St. Paul's was open and several persons were standing in it. They appeared to be very little children; but, on coming up to them, were found to be full-grown persons."

The theory which I have thus advanced, taken in connexion with the mistaking practice of consciousness, will, I believe, furnish a satisfactory explanation of all the phenomena of vision, some of the more interesting of which phenomena, as well as the subject of monocular vision, I propose in a future paper to consider.

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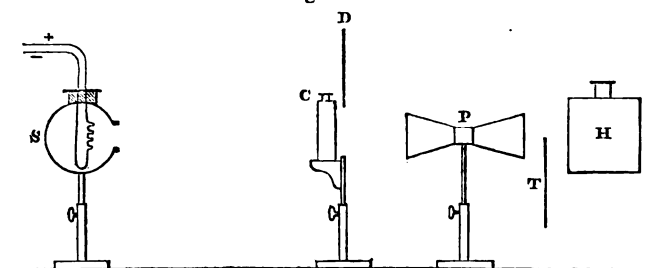
XXVII. *On Sources of Error in Determinations of the Absorption of Heat by Liquids.* By W. FLETCHER BARRETT, Lecturer on Physical Science at the International College, &c.*

DURING the autumn of 1865, whilst engaged in determining for Professor Tyndall the absorption of heat by various liquids, I observed that under certain conditions the more diathermic liquids exhibited a remarkable and anomalous deportment towards radiant heat. This observation led me to make a subsequent investigation, the results of which are given in the following paper.

* Communicated by the Author, having been read at the British Association, August, 1868.

The general outline of the apparatus employed is shown in fig. 1. The instruments were Professor Tyndall's, who kindly

Fig. 1.



allowed me their use, and also opportunity for making the experiments. The source of heat, S, was a platinum spiral raised to bright incandescence by an electric current, and surrounded by a glass globe having an aperture in front. Before this was a cell, C, with moveable rock-salt sides to contain the liquid under examination. Before this, again, and precisely opposite, was a thermo-electric pile, P, fitted with its conical reflector and attached to a very delicate galvanometer. The rock-salt cell was supported on a little shelf behind a perforated metal screen, D, from which the pile was distant some 18 inches. The rock-salt walls of the cell, in the experiments first to be described, were separated by a partition formed of an annular plate of mica 0.02 inch ($\frac{1}{2}$ millimetre) thick: this thickness determined that of the liquid layer under examination. The heat from the spiral was sometimes concentrated by the use of a rock-salt lens (not shown in the figure) which was placed between the source and the cell. To obtain greater sensitiveness, in most of the experiments the "compensating" arrangement was adopted. A blackened canister, H, filled with boiling water, was placed before the opposite face of the pile. By adjusting a screen, T, between the canister and the pile, the deflection of the galvanometer, caused by the radiation from S, could be exactly neutralized. The needle was thus brought to zero and maintained in its most sensitive position.

The empty cell having been mounted on its support in the path of the rays, the screen was moved until the needle stood precisely at zero. A liquid (*bisulphide of carbon*) was now poured into the cell through a fine funnel, and the needle of the galvanometer immediately observed through a telescope. Instead of moving in the direction of absorption, a marked deflection took place on the opposite side of zero, the needle finally coming to rest at 15° on the side of increased heat from the

source; that is, the introduction into the cell of the bisulphide of carbon *augmented* the heat falling on the thermo-pile. When the source of heat was cut off by a metal screen, the needle moved swiftly back to zero and then rapidly up to 67° on the opposite side. From this deflection the absolute increment in the transmission caused by the liquid could at once be calculated. And the mean of eight closely concordant experiments showed that when bisulphide of carbon was poured into a rock-salt cell, so as to form a layer $\frac{2}{100}$ of an inch thick, 9 per cent. more heat reached the pile through such a layer than through the empty cell.

Before passing on to further experiments, I will reply at once to two inquiries which may suggest themselves in explanation of this phenomenon. The one is that the temperature of the bisulphide of carbon might be higher than that of the cell, and that hence its introduction would produce the effect noticed. The other is that the exterior surfaces of the plates of rock-salt might be partially rounded by the frequent polishing which they receive, so that when the bisulphide of carbon was introduced the combination might have acted as a liquid lens. But direct experiments instituted for the purpose of testing these and other possible experimental errors show that they play no part in the phenomenon observed.

Other liquids beside bisulphide of carbon were now tried in layers of equal tenuity; and I was surprised to find a liquid which even surpassed the bisulphide of carbon in this apparently anomalous action on radiant heat. This was the case with *bichloride of carbon*. When this liquid was poured into the rock-salt cell so as to fill the space $\cdot 02$ inch thick, the needle moved from zero to 16° on the side of heat—equal to an increased transmission of $12\cdot 4$ per cent. At the same thickness bisulphide of carbon increased the transmission 9 per cent. But the fact has now, I believe, been well established that the bichloride of carbon is the most diathermic liquid as yet known. Of *chloroform*, a liquid considerably less diathermic than bisulphide of carbon, the same film of half a millimetre gave an increased transmission of $4\cdot 5$ per cent. With this liquid (among those examined at this thinness) the effect ceased; for *benzole*, a more powerful absorbent than chloroform, gave an *absorption* of 20 per cent. Of *Sulphuric ether*, a still more energetic absorbent, the same thin layer of half a millimetre absorbed 30 per cent. of the total emission from a red-hot spiral.

These experiments show that, in passing from feebler to more powerful absorbents, the increased transmission, most strikingly observed in the case of bichloride of carbon, becomes more and more disguised until finally it disappears and absorption predominates. A similar effect, as might be anticipated, takes place

by increasing the thickness of a diathermic liquid. With chloroform the addition of $\frac{1}{100}$ of an inch ($\frac{1}{4}$ of a millimetre) to the thickness of the liquid film changed the increased transmission of $4\frac{1}{2}$ per cent., that it previously gave, to an absorption of 8 per cent. In order to follow this change more closely, and to observe where the reversal takes place, I made a series of experiments with bichloride of carbon, gradually augmenting the thickness of the liquid layer until absorption became marked. These results I state at once in a tabular form. For the sake of clearness and brevity I have omitted the deflections, which gradually became less till zero was crossed, when they then increased on the side indicating absorption. Taking the transmission through the empty cell as equal to 100, the figures in the second column show the transmission by the layers of liquid of the thickness given in the first.

Radiation through Bichloride of Carbon enclosed between Rock-salt plates.

Source :—platinum spiral at a low white heat.

Thickness of liquid layer in parts of an inch.	Transmission per 100.
0·02	112·4
0·03	107·0
0·04	105·3
0·11	100·8
0·14	100·0
0·27	94·0

The singular fact is here brought out, that at a thickness of ·14 of an inch the transmission through the bichloride was the same as through the empty cell. Eighteen months after this some of these determinations were repeated, and no appreciable difference from the above Table was discovered.

In connexion with past and future determinations of the absorption of heat by liquids, I need hardly point out the influence of the fact here established, and the conclusions to which I shall allude in the sequel.

The foregoing experiments were made with a cell having *rock-salt* ends, and they show that *absorption* becomes manifest after a certain thickness in a diathermic liquid is obtained. This is not the case, however, when glass instead of rock-salt cells are employed to contain the same liquids: here the anomaly is still more striking.

The first experiment in the next series was made with a cell having plane and parallel glass ends 1·2 inch diameter. Representing by 100 the transmission through the empty glass cell,

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filling the cell with bisulphide of carbon increased this number to 106.

The experiment was twice repeated with the same result; and the effect was not uncertain or transitory, but a steady permanent augmentation.

I next tried a series of cells with parallel glass ends of varying distances asunder, ranging from 0.1 of an inch to 3 inches, bisulphide of carbon being used in each case. The source and the disposition of the apparatus were the same throughout, and the result of all the experiments showed that increasing the depth of the cell did not, as was the case with rock-salt cells, reverse the effect, but rather augmented the increased transmission—the transmission through an empty glass cell 3 inches deep being raised 9 per cent. by filling it with bisulphide of carbon.

Returning to rock-salt plates, for the sake of comparison a cell 2 inches deep was tried. As anticipated, instead of increased transmission, considerable absorption was now observed when the bisulphide of carbon was poured into the cell. Another similar rock-salt cell, 0.8 of an inch deep, was also tried, with the same result. In fine, with cells having rock-salt ends, after a thickness of 0.2 of an inch had been attained, absorption invariably was found on the introduction of bisulphide and bichloride of carbon; but *with cells having glass ends, augmented transmission was always observed with both liquids*, the bichloride of carbon having always a slight advantage.

There can therefore be no doubt about the fact that the absorption of the more diathermic liquids is entirely disguised when glass cells and a source of rays of the character here used are employed in their examination. Moreover the effect was found to remain the same when the nature of the source was altered, or its temperature raised or lowered, or when the cell was moved nearer to or further from the source of heat: nor was the result changed by causing the diaphragm in the screen (D, fig. 1) to be larger or smaller than the diameter of the aperture in the cell. The same result was also obtained whenever the experiments were made. For example, I here collate a number of determinations, some made at an interval of twelve months apart. The radiation through an empty glass cell 1.2 inch deep being taken as 100, the numbers

106.2, 106.9, 104.6, 106, 106,

or a mean of 6 per cent., expressed the increased temperature noticed when the cell was filled with bisulphide of carbon. Nevertheless the result is just as certain that when the walls of the cell are of rock-salt this increase is not shown; numerous experi-

ments, which it would be tedious to record, have established this fact.

Although the comparison between the rock-salt and glass cells was attended with so uniform a result, yet it is open to the objection that the walls of the two kinds of cells were of different thicknesses; for, from its friability, it is necessary to use rather thick plates of rock-salt. A stricter comparison was therefore instituted. Two plates of glass of exactly the same thickness as the plates of rock-salt, 0·3 of an inch, were cut from a slab of the so-called patent plate—a glass perfectly colourless when looked at edgewise. Two thinner plates, 0·1 of an inch, of the same glass were also procured to compare with the thick plates. The same moveable cell was employed for both the rock-salt and the glass, so that in both cases the diameter of the cell and the position which determined the depth of the liquid layer were the same. The source of heat was the same as in the former experiments, and nothing intervened between it and the cell. The conditions of the experiment being thus exact, I sought at the same time to determine more precisely the difference between the deportment of bisulphide and bichloride of carbon. The results of these experiments are given in the following Table:—

Source:—bright-red platinum spiral.

Layer of liquid $\frac{1}{10}$ of an inch thick.

	Free radiation through	Transmission.
I.	Empty cell, thin glass plates . . .	100·0
	Filled with bisulphide of carbon . . .	103·5
	Filled with bichloride of carbon . . .	104·0
II.	Empty cell, thick glass plates . . .	100·0
	Filled with bisulphide of carbon . . .	109·5
	Filled with bichloride of carbon . . .	111·2
III.	Empty cell, rock-salt plates . . .	100·0
	Filled with bisulphide of carbon . . .	97·5
	Filled with bichloride of carbon . . .	101·5

Here, the liquid layer remaining the same, the *thick* glass plates, it will be seen, instead of reversing the action, actually increase the transmission 6 per cent. over and above that which occurs when *thin* glass plates are used. The objection alluded to is thus not only removed, but the above result afforded a clue by which a solution of the enigma was finally obtained. The bichloride of carbon, it will also be observed, under all circumstances surpasses the bisulphide in augmenting the transmission.

An interesting result occurred in the course of the experiment with the glass cells. In place of the transparent bisulphide and bichloride of carbon, I tried the same liquids saturated with

iodine and thus rendered opaque. Notwithstanding the augmented absorption by the iodine, I still found a larger amount of heat fall on the pile in both cases on pouring the opaque liquid into the cell. When a glass cell $\frac{1}{2}$ an inch deep was used, the heat falling on the pile was raised 3 per cent. by the opaque liquid. When a glass cell 1.2 inch deep was employed, a similar increase of fully 5 per cent. was produced, and this by the introduction of a liquid so opaque that at this thickness it intercepted all the light of the brightest sun.

What, now, is the explanation of this singular action? Reviewing the experiments detailed in the foregoing paper, they are seen to divide themselves into two groups:—

First. Those where an increased transmission attended the introduction of moderately diathermic liquid *films* between plates either of rock-salt or of glass; and

Second. Those where this increase occurred with considerable thicknesses of the same liquids as before, but only when the liquids were enclosed between *glass* plates.

The most probable explanation of the first group of facts at once suggests itself—namely, that the introduction of the liquids diminishes the reflection taking place from the inner surfaces of the walls of the cell. In the empty cell a double reflection takes place when a pencil of rays passes from one plate of the cell to the other through the intervening space of air. If the cell be filled with a liquid whose refractive index is the same as that of the walls of the cell, this loss by reflection from the inner surfaces will be abolished; hence such a liquid, if its absorbent power were *nil*, or extremely low, would increase the transmission when replacing air in the interior of the cell. In the following Table are given the refractive indices of the substances employed; as these were not determined from the specimens used, they can only be regarded as approximately correct; that of glass is especially doubtful. The indices for rock-salt and bichloride of carbon were kindly furnished me by Dr. Gladstone, and are from his own recent determinations:—

Index of Refraction for the line A.

Plate-glass (mean)	1.525
Rock-salt	1.537
Bisulphide of carbon (mean) .	1.646
Bichloride of carbon	1.456
Chloroform	1.441

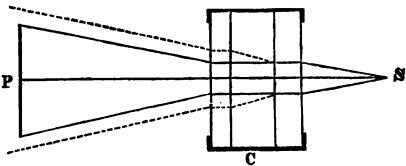
From this Table it will be seen that the index of refraction of bichloride and bisulphide of carbon is sufficiently near to rock-salt to greatly diminish the reflection from the interior of the

cell, whilst, further, bichloride and bisulphide of carbon are two of the most diathermic liquids known. The index of the bichloride is nearer that of glass and rock-salt than the index of the bisulphide, just as might be expected. Melloni has stated that 7·7 per cent. of the incident rays are reflected from a single polished plate of rock-salt*. Although this figure is probably rather too high, still there can be no doubt that any moderately transcendent liquid, when poured into a cell whose walls *were sufficiently near together*, would, by its optical density, increase the transmission more than it would intercept it. And this is exactly what has been found in the experiments described in the early part of this paper. As, moreover, this abolition of reflection of the inner surfaces of the cell will take place at all thicknesses of the liquid layer, the absorption hitherto attributed to the majority of liquids must be too low on account of this opposing effect—an effect as yet disregarded, but here shown to be very sensible.

The foregoing explanation, however, is quite incompetent to explain the second group of facts—the augmented heat observed when certain liquids were enclosed in vessels with glass ends. For here the augmentation continued until the thickness of the liquid was increased to the limits of experiment; and in this case the loss from absorption by the liquid must have been greater than the gain, even if the interior reflection had been entirely abolished.

Here the cause is, I believe, mainly due to a well-known effect of plane surfaces on the refraction of divergent rays. This will be clear if we consider the case represented in fig. 2. Let S be a source of rays diverging till they meet the cell C, and let P be the surface of the thermo-pile. If, now, the vessel C be empty, the rays, after passing through its sides, will move in the direction of

Fig. 2.



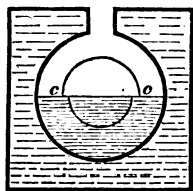
the dotted lines. Now let this vessel be filled with a liquid whose density is approximately the same as the walls of the cell, the rays will then take a course indicated by the continuous lines. In the latter case the angle of the rays emerging from the cell will be more acute. The distance of the divergent point S from the pile P has thus virtually been diminished, and, in consequence, many rays which, when the cell was empty, escaped the pile, now fall on its surface. To obtain this action it is not, of course, necessary that the refractive power of the liquid should be the same

* *La Thermochrose*, p. 195.

as the walls of the cell; for a similar effect will take place, in a greater or less degree, if the density of the body introduced exceed that of the medium it replaces.

It was easy to see the truth of this reasoning by tracking the course of a pencil of luminous rays through the empty and full cell. The light of the red-hot spiral that formed the source of heat was received on a semitransparent screen placed behind a glass cell half an inch deep. The beam was seen on the screen as a luminous circle 1.7 inch in diameter. Filling the cell with bisulphide of carbon, the diameter of this circle contracted to 1.6 inch. When the cell was half full, the effect of the comparison between the upper and lower half of the circle was very striking; this is shown in fig. 3. With the cell 3 inches deep the luminous circle *cc* was diminished from 2.4 inches, when the cell was empty, to 1.8 inch, when it was filled with bisulphide of carbon.

Fig. 3.



This observation explains the effect noticed when bisulphide or bichloride of carbon was poured into deep glass cells; for though sometimes concentrated by a rock-salt lens, the rays from the source were always more or less divergent, and overlapped the area of the pile. Lessening the divergency of the incident rays ought to diminish the effect; and this, by experiment, was proved to be the case. Hence also with *parallel* rays no concentration of the beam could take place on the introduction of the liquid, and with *convergent* rays a decreased amount of heat should reach the pile when a dense medium occupies the cell.

But why should not an increase occur with rock-salt as well as with glass cells? This anomaly puzzled me a good deal at first; for, on consulting the Table of refractive powers, there does not appear to be any reason why such a difference should exist. I think, however, the answer to this question is mainly to be found in the difference between the diathermancy of rock-salt and glass. Rays of heat pass through the former substance with but little alteration in their quality or quantity; but in passing through the latter, their character is changed as well as their amount lessened; after passing through one plate of glass they traverse a second plate with less absorption. It may be that the bisulphide of carbon absorbs somewhat the same class of thermal rays as glass; the heat-tint or thermochrose of the two bodies is possibly alike. Assuming this to be the case, when a cell with glass sides is used, the absorption of the liquid would be so far reduced that the action of the causes tending to increase the transmission would become evident. On the other hand, with a

rock-salt cell the heat-rays would not be sifted in any sensible degree, and hence the absorption by the deep layers of liquid which were employed would be sufficient entirely to mask the opposite effect observed with a glass cell. I regret that when making these experiments it did not occur to me to try how far the foregoing assumption is true, by causing the beam to pass through a plate of glass before it entered the rock-salt cell; and now I have not the opportunity of making this crucial test. But the following experiment, which strikingly shows the influence of sifting, may be taken in support of the view here stated. The divergent rays direct from the source of heat used throughout these experiments were caused to pass through an empty glass cell $\frac{1}{2}$ an inch deep. Filling on the pile, they gave a deflection represented by 100. Filling the cell with bisulphide of carbon augmented this number to 110, or an increased heat of 10 per cent. The cell was emptied, replaced, and a second cell 3 inches deep, already filled with bisulphide of carbon, was interposed in the path of the rays from the source. The beam had thus to traverse 3 inches of bisulphide of carbon before it reached the empty cell, so that on passing through an additional $\frac{1}{2}$ inch of the same liquid, but little or no further absorption would take place; and the action of the liquid in concentrating the rays should thus be more pronounced. Representing by 100 the radiation through the 3-inch stratum of liquid and the empty $\frac{1}{2}$ -inch cell, filling the latter with bisulphide of carbon raised this figure to 120, or an increased heat of 20 per cent. Compared with the former observation, this experiment shows that even in a glass cell at least 10 per cent. of the total radiation is absorbed by a layer $\frac{1}{2}$ an inch thick of bisulphide of carbon; much more likely, therefore, is it that in a rock-salt cell the absorption by deep layers of liquid should disguise the causes tending to augment the transmission.

Finally, it may be asked if pouring the bisulphide of carbon into a glass cell increases the heat reaching the pile, how is it that Melloni, who, as is well known, used glass cells in his experiments on the diathermancy of liquids, does not mention the fact? This is accounted for, I believe, by Melloni's method of experiment: he did not determine the transmission through his empty glass cell first, and then through the liquid added to the cell; but noting only the free radiation to his pile, he then introduced the cell and liquid at once. Consequently he assigned an absorption to some liquids which probably was wholly due to the glass walls of his cell. Hence it is that whilst Melloni states a layer of bisulphide of carbon .36 inch deep absorbs 37 per cent. of the heat from an argand lamp, Dr. Tyndall, who used a rock-salt cell and

a better method of experiment, finds the absorption by a corresponding thickness of liquid under 20 per cent.*

Summing up the results of this inquiry, the main points are as follows. :—When the more diathermic liquids are introduced between two plane parallel plates of rock-salt separated by a very small interval, the rays from an artificial source are found to be more freely transmitted than when air intervenes between the plates. For a space $\cdot 02$ inch wide the increased transmission amounts with bichloride of carbon to about 12 per cent., with bisulphide of carbon to 9 per cent., and with chloroform to 4·5 per cent. This effect disappears and less heat is transmitted (1) when the transclency of the liquid diminishes; *e. g.* the same thickness of sulphuric ether intercepts 30 per cent. of the heat previously passing through the empty cell; (2) when the distance between the plates is increased beyond, say, $\frac{1}{10}$ of an inch in the case of bisulphide, and $\frac{2}{10}$ of an inch in the case of bichloride of carbon. The increased transmission by these liquids reappears, however, in thicker layers when plane parallel *glass* plates are substituted for rock-salt, and continues, apparently indeed augmenting, as the depth of the cell increases, so far as the experiments were carried. Bisulphide of carbon, poured into a cell with glass sides 1·2 inch apart, increases the heat falling on the pile 6 per cent., and bichloride of carbon a still larger amount. Altering the temperature or nature of the source, the size of the aperture in a screen behind the cell, or the position of the cell, makes no material change in these results. But altering the character, or augmenting the thickness, of the walls of the cell has considerable influence. For example, if the cell-walls be of glass, increasing their thickness from one- to three-tenths of an inch raises the heat falling on the thermoscope 6 per cent. when equal depths of the selfsame liquid are poured into the cell. Again, by merely changing the parallel sides of the same cell from rock-salt to precisely similar plates of glass, the very same liquid can be shown to intercept a certain quantity of the heat falling on the thermo-pile in the one case, and to augment that quantity in the other—the difference amounting to upwards of 10 per cent. of the total radiation through the empty cell.

* Dr. Tyndall has pointed out (Phil. Trans. 1864, p. 328; Phil. Mag. vol. xxviii. p. 439) how seriously the glass cells used by Melloni must have influenced the results of his experiments. Melloni, indeed, saw this himself, but endeavoured to prove, by trying two of his liquids between rock-salt plates, that the error vanished in the thickness of liquid he employed; (*La Thermochrose*, pp. 163 and 199). The liquids chosen were colza oil and water—the worst he could have selected; for had Melloni made the comparison with more diathermic liquids, he would have discovered his error.

The explanation of the foregoing facts may be traced to two main causes. The increased transmission noticed with *films* of the more diathermic liquids chiefly arises from the reduction or abolition of the reflection taking place from the interior surfaces of the walls of the cell, owing to the optical density of the liquid introduced being nearer to the cell-walls' than that of the medium it replaces. But in glass cells of considerable depth, retaining the former explanation, the augmented heat there observed is probably *mainly* due to an effect of the refraction of divergent rays by plane surfaces; this gives rise to a concentration of the beam, which becomes sensible when accompanied by a great transalency of the liquid in the cell. In similar cells with rock-salt ends the effect is not observed, probably on account of such cells sifting the beam far less than glass, and thus permitting a higher absorption of the liquid. Nevertheless even with rock-salt cells the causes alluded to must necessarily render, to a certain extent, incorrect the precise absorption hitherto attributed to liquids. These sources of error in determining the true absorption by a liquid or solid can, however, be avoided by employing truly parallel rays; and these are best obtained from the sun.

In conclusion I would add that I do not pretend the observations contained in the foregoing paper can in any material way affect any portion of the celebrated researches on radiant heat which Professor Tyndall has so long conducted. On the contrary, to my mind, they add lustre to that research, by showing in some degree the difficult and delicate nature of the inquiry. In fact this little investigation of mine is not of such importance as to merit even this reference, were it not that I speak from the sincere and jealous attachment of a disciple to his master.

XXVIII. *On a Simple Method of Exhibiting the Combination of Rectangular Vibrations.* By W. FLETCHER BARRETT, Lecturer on Physical Science at the London International College, &c.*

PHYSICISTS are well acquainted with the elegant experiments of M. Lissajous, in which the vibrations of two tuning-forks, placed at rectangles, are optically combined by viewing a ray of light successively reflected from a mirror attached to each fork. A regular series of curves is thus obtained which give a perfect optical expression to each of the musical intervals, the curves augmenting in complexity as the dissonance between the forks increases.

* Communicated by the author, having been read at the British Association, August, 1868.

Instructive and beautiful as are these experiments, the extreme costliness of the apparatus necessary for their proper exhibition has hitherto debarred many from repeating them.

A more simple method of combining rectangular vibrations was long ago devised by Mr. Wheatstone, who employed for this purpose a slender prismatic rod of steel, fixed at one end, and free to vibrate at the other. One of the sides of the rod was filed away, until the vibrations were quicker in one direction than the other by a certain definite ratio. When pulled aside the rod compounded these vibrations, and the resultant motion became apparent by the movement of a silvered bead attached to the free extremity of the rod. The labour of filing away the steel, and the necessity of having a separate rod for each combination, has practically prevented this instrument coming into use. Beyond a most ingenious mechanical contrivance for representing the combination of vibrations, I am not aware that Mr. Wheatstone has designed any simple instrument for effecting this purpose*.

Upwards of two years ago I found a method of obtaining any desired combination by an extremely simple arrangement. A piece of straightened steel wire, about No. 16 gauge and some 12 or 18 inches long, is first well softened in a flame at a point 6 or 8 inches from the end, which length is then bent downwards. The extremity of the longer portion is fixed in a vice, a silvered bead is cemented by marine glue on to the summit of the bend, and the instrument is complete. The whole system is thrown into vibration by smartly tapping the wire near the point held in the vice, and *in a direction oblique to the plane of the two wires*. The vibration travels up the wire, rounds the bend, and throws the inclined arm into motion. The latter, being free, vibrates more easily than the portion which is fixed at one extremity; a compound motion is thus the result, and the spot of light, reflected from the bead, describes a curve expressing the resultant action.

The ratio between the vibrations of the two parts of the wire can evidently be adjusted, or altered, by raising or lowering the point clamped in the vice. The same end may also be obtained by loading the free portion of the wire by a little sliding weight. Both these means of adjustment I have tried with some success. I was not, however, prepared to find that an alteration in the angle of the bent wire would yield a more satisfactory result; but so it is. When the wires are parallel† and even in length, a

* May I here be allowed to express a wish that Mr. Wheatstone would collect and republish his numerous papers? So scattered are they, that I have searched in vain for the original description of the *Kaleidophone*.

† That is, nearly parallel, which is obtained by making a round bend.

combination of 1 to 1 is obtained, and the bead describes a circle passing into an oblique line; but on opening the free limb to an angle of about 30° , the figure changes into the complex curve given by the ratio of 4 to 5. Opening the angle still further, the curve expressing the ratio of 3 to 4 is obtained; then at 45° 2 to 3; and at an angle of 75° the figure of 8 comes out, expressing the ratio of 1 to 2. In fact, by varying the angle an entire series of combinations, more or less perfect, can be produced at will.

In order to avoid the probable breaking of the wire by repeated bendings, I had a light hinge joint made at the bend; but it was less satisfactory than the simple bent wire, which, if well annealed, will bear innumerable bendings, and can be renewed in a moment if broken. To obtain a steady figure, it is better to make the final adjustment by slightly raising or depressing the fixed wire.

Fig. 1.

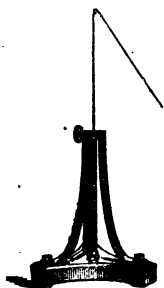


Fig. 2.

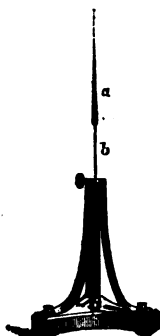


Figure 1 shows the instrument. The wire is capable of being *firmly* fixed at any height in a support which is attached to a heavy stand, more convenient in use than a vice.

Not only may this arrangement be used for exhibiting the combination of vibrations, but it also shows very prettily the formation of nodes and ventral segments. Placing a piece of white paper behind the instrument, or casting its shadow on a screen, the vibration of the wire may be distinctly seen by a great number of people. The fixed arm will then be noticed always to vibrate as a whole, and the bend always to be a ventral segment.

But on the free arm an instructive change is seen to take place in the position of a node which is there formed. When the arms are equal and parallel, and a ratio of 1 to 1 obtained, the

node is near the free extremity of the bent wire; as the wire is raised and the angle increases, the node rises nearer to the bend. It is also worth observing that, in any combination, the distance of the node from the free extremity of the wire, compared with its distance from the bend, is approximately the same as the ratio of the interval depicted by the figure.

I have now, in conclusion, to refer to another arrangement for effecting the combination of rectangular vibrations, which though rather less simple than the one just described, has the merit of being more easily adjusted and more permanent in character. This arrangement, shown in figure 2, has been adapted by Mr. Ladd from an instrument devised by Professor Helmholtz.

Two flat pieces of steel are here welded at right angles to each other into a single rod. The upper part (*a*) is tapering, and on its summit is fixed a polished silver bead. The lower part (*b*) is capable of being firmly fixed in a suitable support. According to the height at which *b* is clamped, so a corresponding portion is allowed to enter into vibration. A combination of the vibration of *a* with that of *b* can thus be obtained in any given ratio. Complete command of any figure can be had by marking its position on the lower strip of steel; and so nice an adjustment is possible, that an almost absolutely steady figure can be secured with a little care.

If I may presume to suggest a name, I propose to call the instrument described in this paper a *Tonophant*.

XXIX. Notices respecting New Books.

A Manual of Inorganic Chemistry, arranged to facilitate the Experimental Demonstration of the Facts and Principles of the Science.
By CHARLES W. ELIOT and FRANK H. STORER. Second Edition.
London: Van Voorst. (Pp. 605 and lix.)

AT a time when the attempt has become general in this country to elevate natural science to the rank of a branch of education, even the minutest incident of the transition may be considered important. If report be correct, we are about to be rescued from the hands of the popular lecturer and placed under the regimen of the schoolmaster; and even those by whom the advantages and functions of science are but little understood manifest an earnest interest in every symptom of the change.

The number of chemical manuals which have been published during the last three years, for the most part somewhat hastily, are a sufficient proof that chemists expect a new epoch in national education. We have now in common circulation thrice the average amount, perhaps, of such works; and new ones, as well as new editions of old ones, are announced as preparing for the press. Most of these books are literary experiments, and it is very unlikely that more than one or two of them will achieve a lasting reputation. Not

unfrequently an author, unable to guess what will be the forthcoming educational plan, writes without a definite idea of any kind to guide him, unless it be that of addressing some particular class of students at a given school; and if one insists wholly on the inductive method, another asserts the necessity of much deductive preface, while a third is lost in transcendental speculation. A great deal of this confusion might have been avoided by assuming the independent attitude and occupying the lofty position of the older writers. At the end of the last and beginning of the present century, for example, it was no doubt the intention of such chemists as Macquer, Lavoisier, Davy, and Thénard to place within the student's reach a manual which should exhibit chemistry as *a whole*; it is, in the main, works of such a nature that were then most extensively read. Even where detail was avoided rather than given, the reader could never have felt the impression from such a book to be fragmentary. He resembled an observer with the microscope viewing an object with a low power. He might see chemical facts brilliantly illuminated by inductive light, deduction proffered by a master mind, and, naming (according to custom) every chemist a philosopher, might perhaps imagine himself a witness of the golden age. The subsequent progress of science has, indeed, pointed out deficiencies in that epoch; but it has never made a general improvement on the foremost manuals which then appeared. They still remain as models of completeness; and a strong deductive tendency (sometimes latent, sometimes in bold relief) is one of the noblest characteristics they display.

The manual of Professors Eliot and Storer is evidently intended chiefly for laboratory use, and is designedly much more practical than theoretical in its tone. With its aid, a teacher might possibly dispense with lectures to a great extent—a course which the book itself may almost be said to suggest. The authors seem to use it as an aid to "classes in the laboratory;" and, from the appearance of two editions in the course of one year, it may be inferred that their plan has been favourably received in American schools. It is highly probable that some such scheme will be adopted among ourselves if chemistry really becomes an element of our national education. It would indeed be a misfortune if the lecture system should be wholly abolished; for we should thereby lose one of the principal means of retaining pure science among us. But good lecturers will never be numerous, certainly never in proportion to the general demand in the case supposed. Most of the students will be committed to the care of comparatively young instructors, whose slight theoretical attainments will inspire respect much more slowly than their practical gifts. Under these circumstances chemistry, if it is to be taught at all, will have to be taught partially—that is, for the most part in a practical way; and some work like the present will doubtless be in general use.

While decidedly of opinion that the partial teaching of a subject is rather a mitigated evil than an undoubted blessing, we have noticed with pleasure or interest that, of the very few chemical manuals which make strong inductive professions, not one can escape

the charge of being somewhat deductive or speculative. A well-marked instance appears in the present work. The following passage occurs at p. 605 :—"The existence of atoms is itself an hypothesis, and not a probable one; all speculations based on this hypothesis, all names which have grown up with it, all ideas which would be dead without it, should be accepted by the student provisionally and cautiously, as being matter for belief but not for knowledge." Yet the entire volume is provided with the dicta and terminology of the atomic theory, and even contains discussions of "true molecular formulæ." Such an inconsistency (or perhaps inadvertence) is the more to be regretted, since any allusion to the atomic theory in the systematic teaching of chemistry is quite unnecessary. The authors themselves evidently regard Daltonism with disfavour; and it is to be hoped that, for ordinary school-purposes, it may ere long cease to be regarded at all.

The matter of this work gives us great satisfaction. It is well arranged, drawn from good sources, and very correct. Many of the experimental illustrations are new, and upon all of them that we have examined a large amount of judgment and thoughtfulness have been expended: they are, moreover, sufficiently numerous for a lengthy course of instruction. Professors Eliot and Storer have therefore achieved, and very well achieved, the object they had in view. Their manual would no doubt have read more agreeably if the Berzelian nomenclature had been employed and multiplied formulæ (such as $\text{H}^2\text{N}^2\text{O}^6$ for hydric nitrate) had been omitted; but these may be matters of local and temporary expediency, which time, we trust, will remove. We should also recommend the enlargement of the appendix (on Chemical Manipulation), its present dimensions being somewhat disproportionately small.

The publication of an American manual of chemistry in England is a rare circumstance, and would in itself be of interest to our readers; but the intrinsic merits of the book constitute a more legitimate claim on their attention. Its unique character will also commend it to the perusal of every one whom the results of the present educational crisis may nearly affect.

XXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 157.]

May 28, 1868.—Lieut.-General Sabine, President, in the Chair.

THE following communications were read :—
 "On Supersaturated Saline Solutions." By Charles Tomlinson, F.R.S.

This memoir is divided into six parts. The *first* part contains a definition of the subject; the *second* an historical sketch; the *third* is on the action of *nuclei* in inducing crystallization, and the effect of low temperatures on a number of supersaturated solutions contained in chemically clean vessels; the *fourth* is on the formation of

a modified salt, as in the case of zinco-sulphate and sodic sulphate; the *fifth* contains an inquiry as to whether anhydrous salts form supersaturated solutions; and the *sixth* and last part is a summary, with a classified list of the salts examined.

1. *Definition*.—When water at a high temperature is saturated with a salt, and, on being left to cool in a closed vessel, retains in solution a larger quantity of the salt than it could take up at the reduced temperature, the solution is said to be supersaturated.

2. *History*.—During many years the phenomena of supersaturation were studied with reference to solutions of Glauber's salt. In 1809, Ziz of Mayence* showed that the sudden crystallization of these solutions is not due to agitation; that the vessels containing the solutions do not require to be hermetically sealed, but if put under a bell-glass, or loosely covered (as with a capsule), they can be preserved during a long time; that solids brought into contact with the solutions act as *nuclei* and produce instant crystallization, but that such solids act best as nuclei when dry; if wet or boiled up with the solution, they become *inactive*. The most efficient nucleus is a crystal of the salt itself. Air, if artificially dried, ceases to be a nucleus. Three varieties of the sodic sulphate are noticed, *i. e.* the *anhydrous* and the ordinary *10-atom hydrate*, and also a peculiar salt formed when supersaturated solutions in closed vessels are left to cool down. This x salt, as it is termed, contains less water of crystallization than the ordinary salt, and is more soluble. If the vessel in which it is formed be suddenly opened, or the mother-liquor touched with a nucleus, the mother-liquor instantly solidifies into the 10-atom hydrate, and the x crystals become opaque, like the boiled white of an egg.

In 1819 Gay-Lussac† referred the state of supersaturation to the inertia of the saline molecules, the molecular condition of the sides of the vessel, and other causes. He also showed that solutions of some other salts exhibit the phenomena of supersaturation. In 1832 the number of such salts was shown by Dr. Ogden‡ to be not less than twenty-one.

In 1825 Faraday§ published some experiments on the supersaturated solutions of Glauber's salt. Graham||, Turner¶, Ure, and others also contributed new facts; but the most elaborate inquiry was by M. Löwel between the years 1850 and 1857, the results of which are contained in six memoirs**. According to this writer, the ordinary 10-atom sodic sulphate increases in solubility from 32° to 93°·2 F., at which latter temperature it begins to fuse in its water of crystallization, and to deposit the anhydrous salt. This salt follows an inverse order of solubility as compared with the 10-atom hydrate, its solubility diminishing as the temperature rises; or, what is the

* Schweigger's Journal, 1815, vol. xv.

† Annales de Chimie et de Physique, 2nd ser. vol. xi.

‡ Edinb. New Phil. Journ.

§ Quarterly Journal of Science, vol. xix.

|| Trans. Roy. Soc. Edinb.

¶ Elements of Chemistry.

** Annales de Chimie et de Physique, 3rd ser. vols. xxix., xxxiii., xxxvii., xliii., xlv., xlix.

same thing, from 218° , the boiling-point of a saturated solution, down to 64° the solubility increases; but at 64° the solution undergoes a new molecular modification, and begins to form crystals of the 7-atom hydrate (the x salt of Ziz). This salt is much more soluble at ordinary temperatures than the 10-atom hydrate, its maximum solubility being at $80^{\circ}\cdot6$. Thus the sodic sulphate has three maxima of solubility, viz. $93^{\circ}\cdot2$ when it is under the molecular constitution of the 10-atom hydrate, $78^{\circ}\cdot8$ to $80^{\circ}\cdot6$ when it is under the molecular constitution of the 7-atom hydrate, and $62^{\circ}\cdot6$ to $64^{\circ}\cdot4$ when it is under the molecular constitution of the anhydrous salt. At these three maxima the saturated solutions are about equally rich in salt. The 7-atom hydrate and the anhydrous salt can only maintain their molecular constitution when in contact with the mother-liquor in closed vessels, in which they are sheltered from the air and from other bodies that act upon them as nuclei. No sooner are they exposed to the air than they become opaque and warm, and assume the molecular constitution of the 10-atom hydrate, as well as its solubility. Hence the conclusion is that supersaturated solutions of the sodic sulphate are not really so, since they hold a salt of much greater solubility at ordinary temperatures than the normal 10-atom salt. Löwel extends his inquiry to sodic carbonate and magnesia sulphate, and endeavours to show that in their supersaturated solutions salts of a lower degree of hydration and of greater solubility than the normal salts are formed; and his general conclusion is that all cases of supersaturation are in appearance only, and not in fact. As to the function of nuclei and the inner sides of the flasks in determining crystallization, he regards it as the effect of one of those mysterious contact actions known as *catalytic*, of which science has not yet been able to give a satisfactory explanation. Bodies that appear to be active in inducing crystallization are designated as *catalytic* or *dynamic*, while bodies that are apparently inactive are termed *non-catalytic* or *adynamic*. "It appears certain," he says, "that but for the mysterious action which the air and other bodies exert on supersaturated solutions, we should obtain sulphate of soda only in the modified state—that is, crystallized with seven equivalents of water, and possessing at ordinary temperatures of the air a much greater solubility than that of the normal 10-atom salt."

Later inquirers have endeavoured to explain the nature of the force exerted by nuclei in inducing crystallization under certain conditions, and their passivity under others. Gernez* tried no less than 220 solids, and of these he selected 39 that were active in inducing crystallization: 18 were insoluble; these were carefully washed in distilled water, and dried out of contact with air. When dry they were found to be without action on the solutions they had previously caused to crystallize. The 21 soluble substances were purified by recrystallization, and they all became inactive. Hence it is concluded that sulphate of soda is the only nucleus for solutions of the same salt. That is to say, whenever a glass rod or other body acts as a nucleus,

* Comptes Rendus, vol. lx. p. 833. A similar method was adopted by Schiff, Ann. der Chem. und Pharm. vol. cxi. p. 68.

it is contaminated with minute portions of the salt itself, which M. Gernez believes to exist in the air, not only of towns, but in the country. According to this view, the supersaturated solution of any other salt can only be crystallized by a saline nucleus of its own kind. But, as M. Jeannel* has pointed out, if this theory be true, we must have floating in the air specimens of all kinds of salts that form supersaturated solutions, and crystallize by the introduction of a solid nucleus; whereas there are some such salts which cannot exist in the presence of the oxygen or of the ammonia of the air. M. Jeannel shows that a few drops of an ordinary solution of a salt will induce crystallization in a supersaturated solution of the same salt without contact of air.

3. *On the Action of Nuclei.*—With respect to the action of nuclei on saline supersaturated solutions generally, the author refers to a theory of his† which seems to account for the liberation of gases from their supersaturated solutions (soda-water, seltzer-water, champagne, &c) when a solid nucleus that had been exposed to the air is immersed in them; while such nucleus becomes inactive if kept long in water, or passed through flame &c., and dried or cooled out of contact with air. The action of nuclei is referred to adhesion. Nuclei are active in inducing crystallization, or they are inactive, according to the state of chemical purity of their surfaces. In the case of a supersaturated saline solution, the sides of the vessel may act as nuclei, or any solid, and some liquid, bodies brought into contact with it. Now suppose the inner surface of the vessel to be made chemically clean, either by well washing it with strong sulphuric acid, or caustic alkali, or spirits of wine, or, as often happens, by boiling the saline solution in the vessel in which it is intended to be kept. In such cases there is perfect adhesion between the sides and the solution, and no salt will be liberated; the sides may in fact be regarded as merely a continuation of the liquid itself, and no salt can be formed there, any more than in the central parts of the liquid. But suppose the sides to be not chemically clean—to be more or less dirty, in fact; in such cases adhesion is diminished or destroyed, and the surface of the liquid next to such sides is virtually as free as its upper surface. Salt will be deposited there, other circumstances being favourable, really from want of adhesion between the side and the liquid that holds the salt in solution. Now apply this to the case of a so-called “adynamic,” “non-catalytic,” or “inactive” glass rod, or coin, or fragment of glass or of flint, &c. A glass rod placed in the solution does nothing more than form new sides, as it were, to the vessel; and its effect is merely that of the sides. If chemically clean, the rod will form no crystals about it, and hence it is “inactive” because its adhesion is perfect. If dirty, the surface of the solution in contact with it will be as free, or almost so, as the upper surface. It requires special means to produce a chemically clean surface; and when produced, it is not easy to maintain it. A short exposure to the air, or a mere touch, will suffice to cover it with an organic film, or with

* Ann. de Chim. et de Phys. 4th ser. vol. vi. p. 166; and Comptes Rendus, Jan. 2, 1866.

† Phil. Mag. July and August 1867.

Phil. Mag. S. 4. Vol. 36. No. 242. Sept. 1868.

motes or dust that prevent or lessen adhesion between it and the aqueous part of the solution, and apparently render an inactive solid active. When a glass rod &c. has been kept in water or passed through flame and dried, or cooled out of contact with the air, it is more or less chemically clean, and remains so while sheltered. When Ziz found a knitting-needle active on one solution, and by passing it through the cork which confined a similar solution it became inactive, he simply made the wire chemically clean by the friction. Air is not a nucleus, and when it appears to act as such, it is simply as a carrier of some solid particle not chemically clean. Hence narrow-necked flasks when opened retain their solutions liquid longer than wide-necked ones, as the former are less likely to catch motes &c. from the air than the latter. Supersaturated solutions are best preserved by plugging the necks of the flasks &c. with cotton-wool, since in cooling down the air is filtered in passing through the plug, and motes and dust are thus kept back.

Tubes made chemically clean by the action of strong sulphuric acid may be filled with a strong solution of sodic sulphate, and when cold the tubes may be placed in a freezing-mixture at 10° F. without any separation of the salt. Hence the author differs from M. Löwel's theory, which supposes a molecular change to take place when strong solutions of the salt are cooled down below 60° . Supersaturated solutions of various salts were cooled down to various temperatures from 32° to 0° F. without crystallizing. Sodic acetate, for example, was kept for some hours at 14° , when on touching it with a wire it became solid, and the temperature rose to 104° . Sodic arseniate, sodic succinate, sodic borate, sodio-potassic tartrate, potash alum, and other saline solutions were treated in this way. Some of these solutions become viscid at a low temperature, and do not immediately crystallize on removing the cotton-wool plug. If they be touched, or the side of the flask scratched with a chemically clean wire, there is no action; but if the wire be not chemically clean, the scratches immediately become chalky white by being covered with minute crystals of the salt, and the action then spreads until the solution becomes solid.

Some salts that are not very soluble in water, such as the plumbic acetate, form highly charged supersaturated solutions, and retain their liquid state below ordinary atmospheric temperatures. When at a certain point they suddenly solidify. Other solutions merely deposit the excess of salt above the condition of supersaturation, leaving the mother-liquor saturated; the cupric sulphate is an example of this.

The memoir contains a number of details respecting the action of nuclei, whether derived from the air, from the flask, from the salt itself, from the filter, or the cotton-wool used in closing the vessels. If the solution touch the wool, crystallization immediately sets in; or if the upper part of a chemically clean tube be touched with a finger slightly greasy before filtering into it the hot solution, the latter will cool down to the temperature of the air without crystallizing, nor will there be any effect if the tube be inclined so as to touch the clean portions of the inner surface; but the moment the solution comes

upon the edge of the finger-mark, crystallization sets in, and the solution becomes solid. Solutions not filtered that begin to crystallize at above 100° in open vessels, or even in closed flasks, may by filtration be freed from nuclei, and so cooled down in the latter to low temperatures without any separation of the salt.

4. *On the formation of a modified salt.*—The readiness with which sodic sulphate parts with its water of crystallization, and two or three other considerations, make it more than probable that a solution of sodic sulphate at high temperatures is really a solution of the anhydrous salt. But M. Löwel supposes that a supersaturated solution in cooling down below 60° assumes a new molecular constitution, viz. that of the more soluble 7-atom hydrate which it then holds in solution. The author gives an experiment to show that such cannot be the case, but that the solution continues to hold the anhydrous salt until a portion of it actually separates. If a boiling solution of two parts salt to one part water be filtered into vessels made chemically clean by being washed out with spirits of wine instead of sulphuric acid, and if these vessels, when cold, be placed in water at 32° , or from that to 40° , a few octahedral crystals of the anhydrous salt will be thrown down. The temperature will slightly rise; and if the tube be now set aside in a moderately warm air, the anhydrous salt will enter into solution, forming a dense lower substratum, from which the 7-atom hydrate will be produced in small quantity, there not being sufficient water present to form the ordinary 10-atom salt. The rest of the solution is still supersaturated; and if the plug be removed from the vessel, crystallization will set in from the surface and proceed rapidly downwards, carrying down enough water to convert the whole solution, as well as the 7-atom, into the 10-atom hydrate.

This process may be conveniently watched in the case of the zinc sulphate. When a saturated solution of this salt cools down from the boiling-point to about 70° , the monohydrated salt is thrown down in quantity, and, as the solution cools, a portion of this dissolves and a crop of acicular crystals is produced which readily melt down at about 100° . On removing the cotton-wool from the tube, crystallization sets in from the surface, and the ordinary 6-atom hydrate is produced.

The author examines M. Löwel's experiments on solutions of the sodic carbonate in which two modified hydrates are pointed out, viz. the $7\text{H O } a$ and the $7\text{H O } b$, which differ in solubility from each other and from the 10-atom salt; but as M. Löwel attaches great importance to the peculiar catalytic properties of the sides of his vessels in determining the formation of these salts, the author cannot help thinking that M. Löwel's results were due to portions of the sides of his vessel, not chemically clean, acting as nuclei. In chemically clean vessels M. Löwel's results have not been reproduced; for on reducing the temperature to a certain point depending on the strength of the solution, the whole became suddenly solid, with a rise in temperature of 35° or 40° . M. Löwel also points out two modifications produced from supersaturated solutions of the magnesia sulphate. The author has placed boiling saturated solutions, when cold, in

freezing-mixtures at 10° without producing any separation of the salt.

The ammonia phosphate throws down from its supersaturated solution an anhydrous powder, which, again entering into solution, forms a dense lower stratum in which a modified transparent crystallized salt is formed in small quantity.

The strontic nitrate also deposits an anhydrous salt in cooling down to about 62° ; but as this salt is not soluble in the solution, the modified salt is not formed.

Some solutions on being cooled down in freezing-mixtures suddenly become solid; others freeze and sometimes thaw again without any separation of the salt, as in the case of the cupric sulphate; but if a boiling saturated solution of this salt be prepared with strict attention to chemical purity, it may be cooled down to near 0° F. without any separation of the salt.

5. *Anhydrous Salts*.—The method adopted to ascertain whether an anhydrous salt forms a supersaturated solution was to make a solution of known strength, as indicated by some good Table of solubilities, raise it to the boiling-point, and then note whether salt began to be thrown down when the solution cooled down to the temperature indicated by the Table. For example, according to Poggiale's Table, 100 parts of water at 158° will dissolve 129.6 of sodic nitrate. This is the same thing as 622.22 grains of the salt in 1 ounce water. Such a solution on cooling down from the boiling-point began to deposit salt at 160° .

In like manner, according to Gay-Lussac's Table, 100 parts of water at 150° F. contain 125 of potassic nitrate. A solution of 125 parts salt to 100 of water began to deposit salt at about 149° . The deposit first began to be made on the side nearest the window, or the coldest side, when the flask was suspended in air; but if the flask were placed on metal, or any other good conductor, a ring of salt was first formed at the bottom, some 6° or 8° earlier than if the flask stood on a block of wood.

It has been frequently stated that the potassic bichromate forms a supersaturated solution. According to Kremer, 200 of water at 140° F. dissolve 100 parts of the salt. Such a solution, on cooling from the boiling-point, began to throw down crystalline flakes at 138° . The remarkable deepening in colour of this solution under the influence of heat is pointed out.

Sal-ammoniac, potassic chlorate, and some other salts were also examined, the conclusion being that anhydrous salts do not form supersaturated solutions.

6. *Conclusion and Summary*.—The author refers to the prevailing theory that supersaturation exists in appearance only and not in fact, since it is supposed to be the modified and more soluble salt that is in solution. If this were true, it ought to apply to all cases of supersaturation; and it has only been claimed in the case of a very few salts, and in them much importance has been attached to the active or the inactive condition of the sides of the vessels containing the solutions.

The author, while admitting, in the case of a very few solutions,

that a modified salt may be deposited, denies that it is due to any molecular change that takes place in the solution, either from reduction of temperature or any catalytic property of the sides of the vessel. His theory is that, when these modified salts are formed, it is the anhydrous salt that is held in solution, a portion of which is thrown down as the temperature falls; and this anhydrous deposit, entering again into solution, forms a dense substratum containing less water than the upper portions, so that when the modified salt forms in it, it is out of the reach of sufficient water to form the normal salt. When, on the contrary, under the influence of a nucleus, crystallization sets in from the surface, the normal salt is formed, and the crystals carry down sufficient water to convert the whole into the ordinary hydrated salt.

As to the action of nuclei or the sides of the vessel, when chemically clean the solution adheres to them as a whole, and there is no separation of the salt; when not chemically clean there is a stronger adhesion between the salt and the nucleus than between the salt and the solvent, and there is a separation of salt; and the action of separation once begun, may be rapidly propagated throughout the whole solution. Boiling saturated solutions may be cooled down in chemically clean vessels and kept for any length of time, not because they undergo any molecular change or hold a salt of greater solubility than the normal salt in solution, but they retain their fluid form simply from the absence of a nucleus.

The salts examined in this memoir are arranged into five groups according to their behaviour.

I. Salts of which the supersaturated solutions remain liquid at low temperatures.

Examples:—

Sodic sulphate.	Potash alum.
Sodic acetate.	Magnesia sulphate.
Sodic arseniate.	Baric acetate.
Sodic succinate.	Calcic chloride.
Sodic borate.	Cupric sulphate.
Sodio-potassic tartrate.	

II. Salts of which the supersaturated solutions suddenly solidify at low temperatures.

Examples:—

Sodic carbonate.	Sodic hyposulphite.
Sodic phosphate.	Strontic chloride.
Plumbic acetate.	

III. Salts of which the supersaturated solutions deposit their excess of salt at low temperatures or under the action of a nucleus, leaving the mother-liquor saturated.

Examples:—

Zinco-acetate.	Potassic arseniate.
Cupric sulphate.	Antimonio-potassic tartrate.
Baric chloride.	Citric acid.

IV. Salts of which the supersaturated solutions form modified salts of a lower degree of hydration.

Examples :—

Zinco-sulphate.
Sodic sulphate.

Magnesia sulphate.
Ammonia phosphate.

It will be seen that the sodic sulphate and the magnesia sulphate also occupy a place in Class I.

V. Anhydrous salts examined in this memoir that do not form supersaturated solutions :—

Potassic nitrate.
Potassic bichromate.
Sal-ammoniac.
Sodic nitrate.
Potassic chlorate.

Potassic ferrocyanide.
Baric nitrate.
Plumbic nitrate.
Ammonium nitrate.

“On the Tides of Bombay and Kurrachee.” By William Parkes, M. Inst. C.E.

The object of this paper is to exhibit the phenomena of diurnal inequality in the tides on the coasts of India, and describe the mode adopted by the author for obtaining formulæ based on astronomical elements for predicting them. It is accompanied by the following records of observations, given in a diagram form :—

Kurrachee, 1857–8,	December to March.
„ 1865,	March to August.
„ 1867,	The whole year.
Bombay 1867,	February to May.

The height and times predicted by the author for 1867, and published by the India Office, are given on the diagrams for that year, so that they may be compared with actual observation.

The continuous curves of the height of the water taken at Bombay, at every ten minutes for the four months above named, are also given.

By the rotation of the earth every meridian-line is brought twice a day under the influences which ultimately result in the well-known semidiurnal tidal movements—once when in the position nearest to the attracting body, and once when in that furthest from it. But the actual point in that meridian which is in the centre of those influences will be alternately north and south of the equator, to the extent of the declination of the attracting body. This alternation of the position of the centre of attraction from the northern to the southern hemisphere produces a diurnal tide, and that diurnal tide produces a diurnal inequality in the semidiurnal tide.

The character of the diurnal tide and the highly complex conditions under which its constantly varying solar and lunar component parts are combined are then traced. Being entirely dependent on the declinations of the sun and moon, the solar element vanishes twice a year, and the lunar element twice a month, each reappearing after the solar or lunar equinox, with its times of high and low water reversed.

The diurnal tide produces a diurnal inequality in height and time of high and low water, affecting simultaneously respectively high-water time and low-water height, and high-water height and low-

water time. In particular cases, the actual values of height and time of diurnal tide may be directly deduced from the values of diurnal inequality. From these it was found that diurnal tide follows the moon's movements at a much shorter interval than semidiurnal, the retardation of the former being from two to three hours only, while that of the latter is from thirty-four to thirty-six hours.

The mode adopted for identifying the varying values of diurnal inequality with their physical causes was then explained. A hypothetical series of diurnal tides, based on the varying values of the declination of the sun and moon, was calculated, the necessary local constants being deduced from the particular cases in which their values could be directly obtained. These hypothetical diurnal tides being combined with a series of semidiurnal tides deduced from the diagram of observations, the diurnal inequalities so obtained were compared with the actual diurnal inequalities. It was then found that a further element was wanting, which was approximately and provisionally obtained by the introduction of a second empirical diurnal lunar tide of twelve inches maximum half-range at Bombay, and six inches at Kurrachee. This tide was assumed, like the first and principal diurnal tide, to be dependent on the moon's declination, but to vanish at intervals of two or three days, before the moon crossed the equator. The author expresses an opinion that this empirical correction might probably be superseded by one more consistent with physical causes, if more extended and more correct observations were subjected to investigation.

Lastly, the comparison of calculated heights and times with the records of observations for four months at Bombay and eight months at Kurrachee were given. This showed that three calculated tides out of four were correct within three inches in height and fifteen minutes in time, the errors of the remainder ranging up to nine inches in height, and thirty minutes in time.

Since receiving the observations made at Bombay and Kurrachee in the year 1867, the author has subjected them to another process for obtaining the actual times and heights of diurnal tide, which has been more successful than that described in the paper.

The only data made use of were the diurnal inequalities in *height* at high and low water—the range of semidiurnal tide and the diurnal inequality in time, which were necessary to the previous process, being now altogether disregarded.

The diurnal inequalities in height were obtained by measuring the widths of the brown spaces where they were crossed by the vertical lines representing noon on successive days. The two daily values thus obtained are respectively the sine and cosine of an angle which represents the difference in time between semidiurnal and diurnal tide. Dividing the low-water by the high-water value gives the cotangent of that angle, and thence the angle itself. Thus the time of actual diurnal tide (first in relation to the time of semidiurnal low water, and then in relation to solar time) was obtained.

The actual range of diurnal tide was obtained by adding together the squares of the high-water and low-water values (sine and cosine), and taking the square root of the sum.

With these two series of results as ordinates, curves were drawn representing times and ranges of actual diurnal tide, which were thus presented in a convenient form for comparison with the diurnal tide which had been previously calculated.

The comparison confirmed the previous conclusion that the tide based on the simple declination theory was insufficient; and the empirical correction which had been adopted seemed to provide an approximation to the required addition to it, both in time and height. But it appeared that a better coincidence in time would have been obtained by assuming the diurnal tide at Kurrachee to be forty minutes earlier. This supposition was tested by treating the observations of 1865 in a similar manner, and also by recalculating a portion of the tides of 1867 with the earlier diurnal tide. In both cases the supposition was confirmed, a better agreement being obtained.

On treating the Bombay observations in the same manner, a fair general coincidence with the calculated diurnal tides was found to exist; but it was further found, on comparing together the Kurrachee and Bombay curves of actual diurnal tide (thus for the first time recorded for the same period), that the times were nearly identical at the two ports, and the range at Bombay about one-tenth greater than that at Kurrachee.

The tables for the four months over which the Bombay observations extend were recalculated with the diurnal tides which had been calculated for Kurrachee (but made forty minutes earlier, and increased in range by one-tenth); and the result was quite as good as that shown by the original tables. This fact would seem to point to the possibility that the diurnal tide is a vertical undulation, acting simultaneously, or nearly so, over a large area.

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[Continued from p. 158.]

May 6th, 1868.—Prof. A. C. Ramsay, LL.D., F.R.S., Vice-President, in the Chair.

The following communication was read:—

“On the Quaternary Gravels of England.” By Alfred Tylor, Esq., F.L.S., F.G.S.

Mr. Tylor first compared, by means of sections and models, the gravels of the Aire Valley at Bingley, of the Taff Vale between Quakers' Yard Junction and Aberdeen Junction, and of the Valley of the Rhonda near its junction with the Taff. He then described the cave-section of Bacon Hole, Gower, and the sections exposed at Crayford, Erith, and Salisbury, comparing the angles of deposition of gravel-beds concealing the escarpment of the chalk in these last three localities with the same conditions at Brighton and Sangatte.

By comparing the gravel-beds at different levels, and upon strata of different age and configuration, he showed in what respect they differ from each other. The bulk and height of the Quaternary deposits had strengthened the conviction which he expressed in his previous paper (on the Amiens gravel), that there was a long period,

reaching nearly to the Historical epoch, in which the rainfall was excessive, and which he termed the "Pluvial period."

These sections also led the author to the following conclusions:—

(1) That the *débris* was deposited by land-floods, and that the mode of deposition was quite distinct from that of moraines produced by the melting of ice. (2) That the character of the deposits in the valleys of the Aire, Taff, and Rhonda proves that they were formed under similar conditions. (3) That these gravel-beds point to a Pluvial period of great intensity and duration. (4) That the ice-action of which there is evidence was subordinate to the aqueous action. (5) That the fossiliferous Quaternary deposits have been best preserved where they have been formed in cavities lying between the edge of the bank of a river, estuary, or sea, and an escarpment running parallel with it at no great distance. (6) That the immediate source of the gravels was the high land adjoining the rivers, whence they had been washed down by rain, with the assistance of lateral streams, to the lower ground, where they had come into contact with larger quantities of running water, had been mixed with rolled materials, and spread in thick beds over the bottoms and slopes of valleys or the sides of escarpments. (7) That the surface of such a deposit rarely slopes at more than 2° or 4° , while the slope of the beds lower in the series near the escarpment averages 12° . The escarpment is usually concealed under a coating of gravel or loess.

Mr. P;RESTWICH dissented from the view of the author, that the valleys had been excavated to their present depth before the gravels were deposited; and, with reference to a former paper, explained that Mr. Tylor and himself had taken different points of observation near Montiers, and that his own views as to the separation which in some cases may be shown to exist between the high- and low-level gravels were correct.

Mr. EVANS also combated Mr. Tylor's views, and pointed out the difficulty of accounting for deposits of gravel such as are at present found in valleys already excavated to their present depth.

Mr. W. BOYD DAWKINS objected to calling in hypothetical causes to account for effects when existing causes are sufficient, and cited the sudden melting of snow as a sufficient cause, as had already been suggested by Mr. Prestwich.

Sir CHARLES LYELL supported the same view, and mentioned a case which had occurred at Salisbury some few years ago as an instance of the effects of such floods. He also cited the existence of flint implements in the gravels on either side of Southampton Water as evidence of the existence of man during a long period of excavation of valleys. He also mentioned the discovery by Dr. Harris of flint gravel identical with that of the present valleys beneath the basalt of Miocene date in Antrim.

Mr. SEARLES V. WOOD, Jun., insisted on the impossibility of even an enormously increased rainfall filling the valleys as suggested by Mr. Tylor, and pointed out the influence which such an accession of fresh water must have had on the animal life in the estuaries. He also mentioned tidal action as an excavating agent in valleys.

Prof. ANSTED showed, by calculation, that even a vast increase in the rainfall would not suffice to fill the valleys so as to deposit the gravels as at present found.

Mr. WHITAKER quoted the existence of distinct terraces of gravel one above the other in the Thames Valley as proving the gradual excavation of the valley.

Prof. MORRIS doubted as to the precise character and age of the deposits in the valleys in South Wales having been accurately ascertained.

Mr. TYLOR briefly replied.

Prof. RAMSAY made some concluding remarks, expressing his disagreement with the views of the author as to the enormous magnitude of the ancient rivers.

XXXI. *Intelligence and Miscellaneous Articles.*

ON THE PREPARATION OF CARBON TUBES, CRUCIBLES, ETC.

BY G. GORE*.

HAVING had occasion to require small rods and vessels of carbon, free (or nearly so) from silica, for experiments with hydrofluoric acid and with fused fluorides, I have been induced to devise the following method of obtaining them. Articles and vessels of the desired shape (but of sufficient dimensions to allow for shrinkage) were formed of different kinds of wood &c.; they were then allowed to dry in a warm place with occasional turning, then enclosed in a copper tube retort provided with two exit-tubes for the escape of gas, the tube placed horizontally between fire-bricks and heated with extreme slowness at first, and finally to bright redness, by means of a row of Bunsen's burners. It was necessary to continually turn the retort, and so to distribute the heat during the burning-process that none of the evolved tarry matter condensed upon the articles; otherwise it altered their form and dimensions in a remarkably curious and fantastic manner. The red heat was continued until gas ceased to be evolved. If the burning was too rapid, the articles fell to pieces or cracked very much.

The articles formed by this process consisted chiefly of rods, crucibles, boats, and tubes (the longest tubes made being $6\frac{1}{4}$ inches long, after burning); and the materials experimentally employed for their construction consisted of lignum vitæ, boxwood, beech, kingwood, ebony, ironwood, mahogany, zebra-wood, Memel oak, English oak, rosewood, "bastard rosewood," maple, lancewood, walnut wood, Norwegian pine, partridge-wood, "Braziletta," cocoa wood, vegetable ivory (or taqua-nut), coquilla-nut, and the hard shell of the cocoa-nut. The articles usually shrunk about one-fourth of their original dimensions during the process; but the amount of shrinkage varied with the different kinds of wood &c.; even with the greatest care many of them became twisted and shapeless during the process.

The best kinds of these various materials were found to be lignum

* Communicated by the Author.

vitæ, kingwood, ebony, and beech : rods made from most of these (if free from cracks) had a remarkably clear metallic ringing sound when struck ; those made from lignum vitæ were very hard and of exceedingly close texture ; those from beechwood best retained their form, but differed from all others in contracting unequally in the two directions across the grain of the wood, and, in consequence of this, round tubes and crucibles became oval during the burning-process. Rods made from lignum vitæ and the other denser kinds of material conducted electricity most admirably, and would probably serve well for carbon pencils for the electric arc ; but if they had been burned too rapidly their electric conducting-power was much less. Of the four materials above mentioned, lignum vitæ was the best.

The following were found to be the chief conditions of success in the process :—(1) vegetable materials of the hardest kind and closest texture ; (2) wood of the straightest grain, free from knots and splits ; (3) very slowly dried and heated ; (4) the heat very uniformly distributed ; (5) prolonged high temperature at the last ; and (6) gradual cooling.

Lignum vitæ evolved a very large amount of oily and tarry matter during the burning-process. The charcoal from it had a remarkably hard and shining surface, and two pieces of it rubbed together seemed like too smooth stones ; it was also very heavy, and its texture was so extremely close as to make it apparently quite impervious to liquids : even after immersion in the strongest hydrofluoric acid its surface had no acid taste ; it was feebly electro-positive to platinum in strong nitric acid, and therefore would not equal platinum, if substituted for it in a Grove's battery.

By the appearance of each kind of charcoal, the kind of wood from which it was formed could be distinctly determined, each wood retaining its usual superficial markings. Some kinds cracked transversely (for instance, boxwood and maple), others longitudinally (thus ironwood and partridge-wood) ; others twisted ; some kinds of charcoal were very hard (coquilla-nut, lignum vitæ, kingwood, and ebony) ; those of ebony and kingwood acquired a purple colour in places ; charcoal of beech and lancewood assumed a gentle curvature ; those of walnut and maple were easily broken across ; those of mahogany, pinewood, and walnut were soft ; those of English and Memel oaks were porous, &c.

ON MAGNETO-ELECTRICAL MACHINES.

BY MM. JAMIN AND ROGER.

In this research we have investigated, and believe we have succeeded in establishing, the laws of the production of electricity in magneto-electrical machines. These machines may be characterized by saying that they borrow from a motor, in the form of force, a given quantity of heat, and that they reproduce it, through the intervention of an electric current, in internal and external resistances. The question to be solved was to find the laws which regulate the quantity of heat borrowed on the one hand, and reproduced on the other.

Our machine consisted of six rotating plates, each provided with sixteen bobbins joined in tension and forming a total resistance R of twelve turns of the rheostat. These plates are joined in quantity, so as to form an electromotor of six independent machines sending their electricity into a common external circuit. The resistance of the whole is then equal to R divided by 6, or to two turns of the rheostat. In each series of experiments the velocity remained constant; it varied in the different series from 350 to 550 turns in a minute. The machines were driven by a Hugon's gas motor, the regularity of which has been proved to be perfect. By means of a brake on the principal shaft, the force, and therewith the heat imparted to the machine, could be regulated and measured. The heats regenerated in the external resistances were measured under the usual conditions.

All the experiments showed that the number C of thermal units thus regenerated in an external resistance increases as this resistance increases, diminishing then to zero when it becomes infinite. It attains a maximum for a value of x equal to R , or to twelve turns of the rheostat; it is exactly represented by the formula

$$C = \frac{x A^2}{(R + x)^2}.$$

But we know that in an external circuit x of a battery whose electromotive force is A and the internal resistance R , the heat regenerated is expressed by Joule's formula,

$$C = x i^2,$$

or by

$$C = \frac{x A^2}{(R + x)^2}.$$

The magneto-electrical machine behaves, then, as this battery would—with, however, an essential difference, which is that R does not represent its real internal resistance, which is $\frac{R}{6}$, but that

of each of the plates or of each of the electromotors which concur in producing the total current. We may say, then, that Ohm's law applies to the magneto-electrical machine with an essential modification, by supposing that each of the various plates is independent, and that its currents accumulate on the external circuit.

There is an important difference between the battery and the machine. The quantity of heat C_1 furnished by the battery in a given time is proportional to the electromotive force and the weight of zinc dissolved—that is, to the intensity of the current; so that we have

$$C_1 = \frac{A}{R + x},$$

which shows that this heat varies as the ordinates of an equilateral hyperbola. But the magneto-electrical machine seems to be only a battery which borrows its heat from a motor instead of taking it from a chemical action, and we might be led to suppose that the quantity of heat furnished should vary according to the same law. This is not the case. The quantity is represented by the empirical relation

$C' = \beta + \frac{(x-\alpha)A^2}{(R+x)^2}$, in which α and β are constants. It is a minimum for $x=0$; that is, when the external circuit is zero; it gradually increases till it equals $\frac{A^2}{2(R+\alpha)}$ for $x=R+2\alpha$; it decreases to β when x tends towards infinity, which is the case when the circuit is open. It follows thence that, if the brake be not touched, the working of the machine is progressively retarded as the resistance increases up to a value of x equal to $R+2\alpha$, resuming then increasing velocities when the external circuit continues to increase.

These laws may be demonstrated in another way. Experiment first measures the quantity C or $\frac{x A^2}{(R+x)^2}$, then the increase of work $T-T'$ which the motor furnishes to the machine when, the circuit being first open, the resistance x is introduced. $T-T_1$, divided by the mechanical equivalent of heat E , represents the heat imparted by the motor or $y-\beta$; we have therefore

$$\frac{T-T_1}{E} = C - \frac{\alpha A^2}{(R+x)^2}.$$

By this formula E may be calculated.

Our experiments have given us more than fifty values for E , which agree virtually with those found by other methods.

The heat taken from the motor $\beta + \frac{(x-\alpha)A^2}{(R+x)^2}$ reproduces in the circuit a quantity $C = \frac{x A^2}{(R+x)^2}$. According to Joule's law, it should reproduce in the internal circuit a number of thermal units equal to

$\frac{A^2 R}{6(R+x)^2}$, whence it follows that the difference between these quantities, that is,

$$C'' = \beta - \frac{\left(\alpha + \frac{R}{6}\right)A^2}{(R-x)^2},$$

represents the quantity of heat uselessly expended. Our experiments have shown that it is equal to two-thirds of that borrowed from the motor.

The discrepancies we have indicated between the laws of the magneto-electrical machine and those of the pile may be explained by an hypothesis which appears very probable. One portion of the heat C'' which is not utilized will be employed in overcoming passive resistances; it is constant; let us call it M . A second part will be employed in producing a reaction in the fixed magnets which cannot be experimentally valued, but which necessitates an absorption of heat; it is variable; let us call it C''' ; we have

$$C''' = \beta - M - \frac{\left(\alpha + \frac{R}{6}\right)A^2}{(R+x)^2}.$$

This reaction must then increase with the value of x , and attain its maximum when the circuit is open.

These laws being recognized and verified, we investigated the only application yet made of these machines, the electric light. Whenever a regulator is interposed in the path of the current, the velocity of the machine is diminished, as it would be by the interposition of a metal wire. The arc exerts a resistance x which can be determined by ascertaining how many turns of a rheostat must be introduced into the circuit to produce an equal retardation in the velocity of the machine. That being done, we have compared the heat disengaged by this arc with that reproduced in the resistance, and we have found that both are exactly equal. We have then been led to the conclusion that the arc only acts as a metal wire, either by the heat it regenerates, or by the diminution it produces in the intensity of the current.

This heat of the arc is very weak, scarcely equal to that produced by a gas jet burning a litre in a minute. To obtain this result, 100 litres of gas were consumed in Hugon's motor; and the heat reproduced does not exceed the hundredth of that consumed. But though small, as it is concentrated on a very narrow space on charcoal-points, it develops there an enormous temperature, and an intensity of light almost twice as much as would be obtained by directly burning the 100 litres of gas expended in its production, and four times as much, even, if the carbons are prepared by Carré's process.

There is thus an enormous loss of heat on the one hand, a remarkable gain of light on the other.

There is nothing paradoxical in this result. The magneto-electrical machine only utilizes a small portion of the heat absorbed; but it collects it disseminated over a large space, and concentrates it in a small volume; it takes it at a low temperature to produce an enormous heating of the carbons; it finds it as obscure heat to change it into light; it diminishes its quantity, and transforms its nature; it expends calorific radiations which are cheap, and makes them into luminous radiations which are dear; and it finally gives them at a cheaper rate than any other source of light.—*Comptes Rendus*, June 1, 1868.

ON THE EXPANSION AND COMPRESSION OF SATURATED VAPOURS.

BY M. CAZIN.

I beg to communicate the result of researches on saturated vapours on which I have been engaged for the last two years. The question was this—to observe under what circumstances a vapour is partially condensed when its volume is changed without either addition or loss of heat.

At the sitting of the Academy, January 25, 1866, I communicated a first series of experiments, on expansion alone. Since then I have constructed an apparatus by which the expansion or compression of vapours at high temperatures may be effected at pleasure.

A horizontal copper cylinder, closed by parallel glass plates and heated by the aid of an oil-bath, contains vapour in a state of satu-

ration ; its capacity is 6 litres, and its length 60 centims. A vertical cylinder of a litre capacity, communicating at its lower end with the preceding, is immersed in the same oil-bath ; it contains a piston of the same metal, which by being moved augments or diminishes the volume of the vapour. The rod of this piston passes through a stuffing-box at the top of the cylinder ; and the vapour can act upon the two faces of the piston. This vapour is supplied from a boiler containing the liquid, and which on the one hand communicates with the horizontal cylinder, and on the other with the upper part of the piston-cylinder.

When the oil-bath is at the desired temperature, the boiler is raised to the same point ; then the stopcocks are opened, so that the vapour is simultaneously admitted into the two cylinders. The two faces of the piston are then under the same pressure, and it can be moved without any great effort.

To effect a compression, for instance, communication is cut off between the boiler and the glass-plate cylinder ; a certain quantity of vapour is thus isolated in the apparatus. But the vapour is allowed to act on the upper face of the piston. The piston is then rapidly lowered, and the volume of isolated vapour is thus *momentarily* reduced, which is sufficient for observing the phenomena which take place in this vapour when it is compressed without either addition or subtraction of heat. The construction of the piston offers no difficulty, for it is not necessary that it perfectly retain the vapour.

The pressure of the vapour is given by a manometer fitted to the boiler and containing a column of liquid from the mercury to the boiler. By heating the tubes which conduct the vapour into the two cylinders, condensation is prevented and the vapour reaches the glass-plate cylinder dry and saturated.

The difficulties of construction have been very skilfully surmounted by M. Golaz ; and I have been able to observe the vapours of water, ether, chloroform, and benzine as high as ten atmospheres. The partial condensation of vapour produced by a sudden change of volume is manifested by a mist of very brief duration ; the action of the sides makes it at once disappear. When the mist is very dense, it is seen without difficulty when a mirror reflecting the light of the clouds is looked at through the glass plates ; but when very feeble, it has the appearance of a slight fume which undulates in the cylinder : the glass plates must in this case be very bright. There is one case in which this is impossible ; and that is, when the vapour is compressed. The action of the sides quickly brings the vapour to its initial temperature, and then a portion of this vapour is necessarily condensed. This condensation produces no mist ; but it dims the glasses, and thus observation is rendered difficult.

This inconvenience has been remedied by looking at the vapour through a telescope applied against the glass, which gives a sharp image of the central parts of the cylinder ; the glass plates are not seen, but the slightest mists which sudden change of volume in the vapour may produce are readily discriminated.

The following are some of the results of my experiments :—

Aqueous vapour is always rendered dull by expansion ; the mist

presents beautiful shades, according to its intensity. It is never rendered turbid by compression.

The vapour of ether has presented the reverse effect; it becomes turbid by compression and remains transparent by expansion.

These two results confirm those of M. Hirn, to whom the first observations on this subject are due.

The following are the new results which were the object of this research:—

The vapour of chloroform gave a mist when expanded up to 130° . At this temperature it was very feeble and was not easily obtained; the vapour remained transparent by compression. Above 136° , compression was accompanied by a very perceptible mist. The higher the temperature the more intense the mist, which was never observed with expansion. Experiments were made from 80° to 150° , and therefore up to nine atmospheres.

The vapour of benzole behaved like chloroform. There was mist by expansion alone up to 115° , and by compression alone above 130° . Observations between these temperatures were interrupted by an accident.

It has thus been experimentally proved that for these two liquids there is a temperature of *inversion*, below which the expansion of dry and saturated vapour, without either addition or subtraction of heat, is accompanied by a partial condensation, while above this temperature compression determines condensation.

There is thus verified a consequence of the formulæ of thermodynamics at which several authors have arrived by different methods.

The temperatures of inversion which I have calculated from these formulæ are 520° for water, 113° for ether, 123° for chloroform, 100° for benzole. The first of these numbers was found by Mr. Rankine in 1854; MM. Dupré and Combes have made analogous calculations for other liquids; and the differences of results are explicable by the choice of data, which are not accurately determined.

To give an idea of the degree of accuracy which my experiments present, I have calculated from thermodynamic formulæ the weights of chloroform vapour precipitated under the circumstances in which I worked:—

Original temperature.	Original volume.	Final volume.	Condensed vapour.	Thermometric effect.
	lit.	lit.		
100	5.844	6.862	0.174	-6.8
110			0.123	-7.3
120			0.070	-7.6
130	6.862	5.844	0.518	+8.5
140			0.644	+9.0
150			0.815	+9.6

The agreement between theory and experiment is as close as could be expected when we consider the want of purity in the liquids, and the uncertainty in regard to some of the data on which the calculations are based.—*Comptes Rendus*, June 8, 1868.