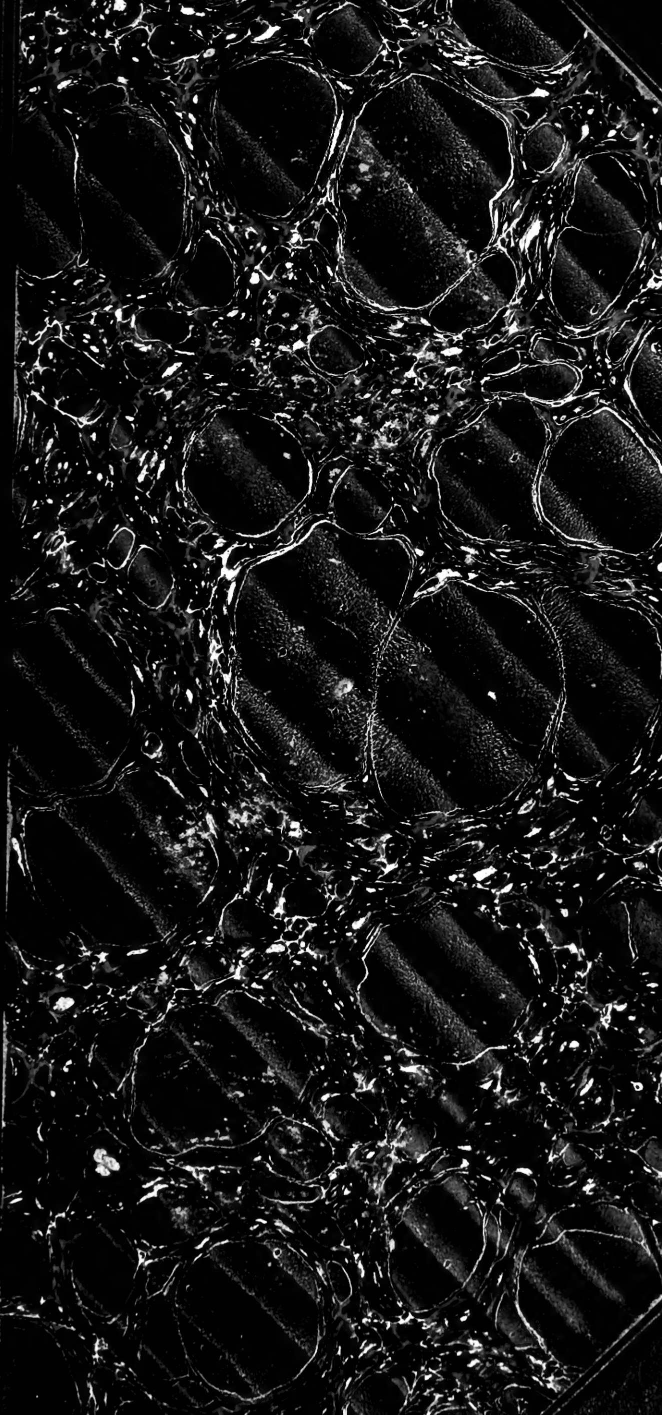

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

CONDUCTED BY

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

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

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

J. B. Pinelli ad Mazonium.

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NUMBER XCIII.—OCTOBER.

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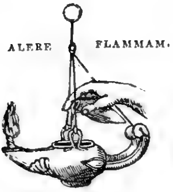
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THE

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PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

JULY 1857.

I. *Experimental and Theoretical Researches on the Figures of Equilibrium of a Liquid Mass withdrawn from the Action of Gravity.*—Third Series. *By* J. PLATEAU[*].

Theory of the modifications experienced by jets of Liquid issuing from circular orifices when exposed to the influence of vibratory motions.

§ 1. IN the preceding series[†], we deduced from the properties of our liquid figures the theoretical explanation of the constitution of jets of liquid issuing from circular orifices, and protected from every disturbing influence; we have now to study, again from a theoretical point of view, the curious phænomena which present themselves when vibratory movements are communicated to the liquid. Setting out, according to our announcement, from an idea of Savart's, we shall show how the effects of these vibratory motions combine with those of the forces of figure (*forces figuratrices*) which determine the transformation into detached masses, after which all the phænomena in question will explain themselves in a natural manner.

After having endeavoured to prove, by means of an ingenious hypothesis, that the agitation occasioned in the mass of liquid in the vessel by the efflux itself is capable of exciting vibrations in this mass directed normally to the plane of the orifice, Savart has shown that the result of such vibrations would be the

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[*] From the thirtieth volume of the Memoirs of the Royal Academy of Belgium.

[†] *Mémoires de l'Académie Royale de Belgique,* vol. xxiii. Taylor's Scientific Memoirs, vol. v. p. 584.

alternate expansion and contraction of the diameter of the jet; because the portion of the latter which issued during the continuance of a vibration directed from within outwards, would suffer a compression which would augment its thickness, whilst the portion which issued during the continuance of a vibration directed from without inwards would, on the contrary, suffer a traction which would attenuate it. According to our former researches, however, the formation of these expansions and contractions is due, not to vibratory motions, but to the instability of the equilibrium of the figure; nevertheless, when vibrations are communicated from without to the liquid in the vessel, and consequently really exist in this liquid, when, for instance, a sonorous instrument in a state of vibration is put in communication with the sides of the vessel, then the vibrations in question must necessarily tend to exert upon the vein an action of the kind conceived by Savart; and if these vibrations have a suitable period, there will necessarily be a concurrence between their actions and those of the forces of figure.

Before examining the subject more closely, we must refer to a point in our theory concerning jets not submitted to the above influence.

§ 2. In the second series, §§ 72, 74 and 82, we have seen that if, when a jet of liquid issues vertically downwards, we imagine the motion of translation to be perfectly uniform, the laws of the transformation of cylinders apply exactly to the jet; and from them may be deduced the laws of Savart governing the length of the continuous portion, and the tone produced by the shock of the discontinuous portion against a stretched membrane. But this uniformity in the motion of translation cannot be realized; it can only be approached by augmenting the charge*, and throughout the length of the continuous part the translatory motion is always more or less accelerated; whence it results, that in the absence of the forces of figure the jet would necessarily become more and more attenuated at greater distances from the orifice. Hence the liquid figure being no longer exactly cylindrical, the laws of the transformation of cylinders cannot, without some modifications, be applicable; and we were of opinion†, that, since the volume of the divisions‡ of a cylinder is less the less the diameter of the latter, the divisions of a jet ought during their descent to suffer a gradual diminution of volume bearing a certain relation to the above-mentioned attenuation. But, not-

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[*] Second Series, §§ 72 and 75.

[†] Ibid. § 76.

[‡] It will be remembered that the term *divisions* was given to those portions of a liquid cylinder each of which became converted into a detached sphere; and that during the transformation, the limits of all divisions are the circular sections at the most contracted parts of the jet.

withstanding its apparent legitimacy, this was but a hypothesis, and we were wrong in presenting it as the expression of a fact. For, in the first place, we were thus led to a conclusion difficult to admit, that is to say*, that the liquid descends more rapidly than the divisions, and that, moving thus in a kind of canal whose dimensions are alternately broader and narrower, its velocity suffers a series of periodic variations: again, if the divisions lost volume in the course of the continuous part, it would follow that the volume of each detached mass would be less than that of a primitive division; and as at all distances from the orifice the same quantity of liquid necessarily passes during the same time, the number of masses which would strike a stretched membrane during a second would be greater than the number of divisions generated in the same time at the contracted section,—a result evidently irreconcileable with our theory of the influence of vibrations upon the jet.

But there is another hypothesis, equally probable *à priori*, which does not involve the difficulties just alluded to, and which moreover is, as we shall see, supported by experiment. Instead of regarding each division as independent of its adjoining ones, and thus as diminishing freely and gradually in volume on account of the attenuation of the jet, so that the volumes of all those which at a given moment are situated on the continuous part of the jet diminish from the highest to the lowest, it is just as permissible to assume that these divisions are dependent one upon the other, and that, in virtue of this dependence, they all have the same volume, but that in consequence of the attenuation of the jet this uniform volume is intermediate between those which would correspond, individually, to the two extreme divisions. This intermediate volume will consequently be less the more the jet tends to become attenuated, in other words, the more feeble the charge. All complication thus disappears: the divisions descend with the same velocity as the liquid without altering their initial volume; the liquid does not pass from division to division, and hence the velocity of its translation suffers no periodic variations; lastly, each division which leaves the contracted section only furnishes matter sufficient for one detached mass, and consequently the number of masses which in a given time strike a stretched membrane, is always equal to the number of divisions which pass the contracted section during the same time. It is only when the charge is diminished or increased that the divisions will assume, from the moment of their generation, a less or a greater volume, which they will afterwards retain throughout the course of the continuous part.

It is essential to remark here, that these variations in the

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* Second Series, §§ 76 and 77.

volume of the divisions necessitate corresponding variations in their length, so that they are longer or shorter according as the charge is more or less strong.

§ 2*a*. Since the new hypothesis just presented is simpler, and since it accords theory with experiment, we shall in future adopt it, so that the 76th paragraph of the second series will require rectifying accordingly.

This hypothesis too, like the former one, leads us to the recognition of two kinds of influences acting oppositely upon the law which governs the length of the continuous part of the jet when the charge is made to vary; here again, however, greater simplicity is attainable.

In the first place, we must remember that if the translatory movement were uniform, the proportionality to the square root of the charge would always be satisfied, even down to very feeble charges*. But if the divisions descend with the accelerated velocity of the liquid, and if we assume that no change results therefrom in the duration of their transformation, they will describe a greater space during this transformation, so that the continuous part will be longer than if the acceleration did not exist; and the excess, compared with the length of the continuous part in the case of uniform motion, will be considerable under a feeble charge, whilst it will be inconsiderable under very strong ones, for the latter render the translatory motion in the continuous part sensibly uniform. On passing, therefore, from the first to the second of these charges, the ratio of the lengths of the continuous parts which respectively correspond to them will be nearer unity than if the acceleration were zero, that is to say, nearer unity than the ratio of the square roots of the charges.

But the divisions cannot descend with accelerated velocity without becoming simultaneously elongated†, and hence arise two causes of diminution in the period of transformation. We know, in fact‡, that the more the length of the divisions of a cylinder surpasses the limit of stability, the greater the rapidity of transformation, and, on the other hand, that the elongation which the jet thus experiences must diminish the thickness of its contractions more than that of its expansions; for on the former the effect of elongation is assisted, whilst on the latter it is opposed by the forces of figure. This second influence, that is to say, the diminution of the period of transformation—which diminution is greater the more the velocity of translation diverges from uniformity, or the feebler the charge—evidently tends to render the law more rapid than the proportionality to

* Second Series, §§ 72 and 75.

† Ibid. § 76.

‡ Ibid. § 66.

the square root of the charge, and consequently its tendency is opposed to that of the first influence.

Lastly, there is a third influence opposed to the preceding one, and hence of the same kind as the first. As we have already remarked on concluding the second paragraph, the nascent divisions must be shorter the more feeble the charge; but according to what has been above remarked, this shortening, by diminishing the excess in the length of each division beyond the limit of stability, ought to tend to augment the period of transformation.

Consequently the 78th paragraph of the second series relative to the neutralization of the two opposite kinds of influences, and hence to the manifestation of Savart's laws from moderate charges upwards, still holds true; it must be borne in mind, merely, that the influences to which it refers are not precisely those indicated in the 77th paragraph of the same series, but, as may be seen, are somewhat simpler.

The second part of the 82nd paragraph (2nd series), however, in which we sought to establish *à priori* the conditions under which Savart's laws respecting the tones produced by jets are realized, cannot now be maintained; for the considerations there presented are founded upon the first hypothesis. Reasoning according to the new hypothesis, we will remark, that as the charge increases, the jet issuing from a given orifice approximates more and more to what it would be were there no acceleration, and consequently the length of the nascent divisions converges towards what it would be in such a case; whence by virtue of the first part of the same paragraph 82, the laws of Savart will necessarily be satisfied from smaller, sufficiently strong charges upwards. This constitutes all that the new hypothesis can furnish respecting the conditions in question; it does not permit us to determine the smallest charge under which the same commence to be fulfilled, for it provides us with no accurate data for calculating the length of the nascent divisions.

Lastly, it will also be necessary to rectify the commencement of paragraph 83 (second series), which establishes, by the first hypothesis, the approximate uniformity in the velocity with which the contractions are carried over the space corresponding to a nascent division. According to the new hypothesis, the translatory motion of these contractions coincides with that of the liquid itself, and consequently we may calculate with exactitude —for the charge and orifice employed by Savart—by how much the velocity has augmented at a distance from the contracted section equal to six times the diameter of the latter, *i. e.* at a distance evidently greater than the length of a nascent division: the augmentation thus obtained scarcely exceeds a hundredth.

The new hypothesis, as well as the old one, therefore establishes the close approximation to uniformity in the motion of the contractions through the small space in question, and consequently the remaining part of the paragraph may be accepted as correct.

§ 3. These rectifications being made, we proceed to the subject in hand by recalling briefly the modifications which, according to the researches of Savart, every jet receives under the influence of vibrations. The first fourteen of the following numbers refer to jets descending vertically:—

No. 1. The continuous part becomes shortened.

No. 2. The thickness of the limpid portion appears augmented.

No. 3. Each of the masses which detach themselves at the lower extremity of the continuous part becomes flattened first in a vertical direction, that is to say, its horizontal diameter becomes greater than that of the sphere it tends to constitute.

No. 4. These flattened masses, being abandoned to themselves, tend to assume the spherical form, about which they only oscillate, however, in consequence of inertia; thus they become alternately flattened and elongated in a vertical direction, so that their horizontal diameter, which was at first greater than that of a sphere of the same volume, becomes afterwards less, then greater again, and so on.

These periodic variations in the horizontal diameter of the masses taking place simultaneously with their translatory motion, the impression left upon the eye by the rapid passage of any one of these masses would be that of a figure presenting a regularly disposed series of maxima and minima of thickness; the first corresponding to the places passed by the mass in its phases of greatest horizontal development, and the second to those by which it has passed during its phases of greatest horizontal contraction. Now as the successive masses pass (either exactly or nearly so) the same places in the same phases of their oscillations of form, the impressions which they would individually produce become superposed more or less completely, and the agitated part of the jet presents in a permanent manner the differences in thickness in question; in other words, this agitated part appears composed of a regular series of ventral segments whose nodes occupy fixed positions.

When the above superposition is imperfect, each ventral segment has the appearance of an assemblage of waves, each of which constitutes a kind of cone having for axis that of the jet itself. About the half of the first ventral segment is formed by the passage of the expansions of the bottom of the continuous part, so that the latter terminates near the middle of this ventral segment.

No. 5. The length and diameter of the ventral segments are greater the stronger the charge, and the greater the diameter of the orifice. The diameter of the nodes varies in the same manner.

No. 6. All these phænomena manifest themselves even under ordinary circumstances, when no vibrations are intentionally excited in the liquid of the vessel. The reason of this is, that, on the one hand, the shock of the discontinuous part of the jet against the liquid into which it falls gives rise to vibrations which are transmitted to the vessel by means of the air and the supports; and, on the other hand, the vessel also receives through its supports the small vibrations due to external noises which are propagated by the ground. It is only when, by suitable precautions, the vessel is protected from these two influences, that the jet assumes its natural aspect.

No. 7. All the phænomena enumerated in the first five of the above numbers, however, become much more developed and regular when a note, in unison with that which would be produced by the shock of the discontinuous part of the jet against a stretched membrane, is produced by the aid of some instrument in the neighbourhood of the apparatus. The continuous part then becomes considerably shortened; the diameter of the limpid portion increases still more; the ventral segments become enlarged, gathering themselves more together, so that the nodes which separate them become more elongated; lastly, the diameter of these nodes appears diminished.

No. 8. Other tones, not in unison, as before, though likewise produced by an instrument in the neighbourhood of the vessel, act upon the jet in an analogous manner, but with much less energy.

Lastly, there are tones which produce no effect.

No. 9. In the particular case where the note of the instrument is almost in unison, the continuous part of the jet is alternately lengthened and shortened, and the beats which coincide with these variations in length can be recognized by the ear.

No. 10. When the discontinuous part of the jet is received upon a body which can only render a determinate note, it often happens that the vibrations of this body modify the note peculiar to the jet. This does not appear possible, however, unless the difference in pitch between the latter note and that of the body which receives the shock does not exceed a minor third.

When the note of the jet is thus modified by a foreign one, a slight shock imparted to the apparatus, or a change in the position of the body impinged upon, often suffices to cause the jet to return to its own note, and this return is always effected suddenly.

When the difference between the two notes is very small, they may both be heard periodically or even simultaneously.

No. 11. The modifications experienced by the jet under the influence of vibrations are still greater, and acquire a perfect regularity when the sonorous instrument (No. 7), instead of being held at a certain distance from the apparatus, is put in contact with the sides of the vessel, and when its note is very intense and exactly in unison with that of the jet. The continuous part is then so much shortened, that the upper extremity of the first ventral segment almost touches the orifice; and, on the other hand, the superposition of the ventral segments formed by the individual masses (No. 4) is so exact, that no appearance of waves is any longer distinguishable.

No. 12. This extreme regularity permits us to distinguish clearly the apparent figure produced by the passage of the spherules interposed between the masses. This figure, which occupies the axis of the jet below the extremity of the continuous part, also possesses ventral segments and nodes, but both are shorter than those due to the passage of the masses.

No. 13. By means of an instrument placed thus in contact with the sides of the vessel, almost all notes are capable of producing effects analogous to those of a note in unison with that of the jet; but these effects are less considerable the greater the divergence between the note of the instrument and the unison in question.

No. 14. Moreover, under these conditions, even when the note natural to the jet is not in unison with that of the instrument, it may be brought into unison even if the difference between the number of vibrations were large enough to constitute an interval of a fifth above, or more than an octave below the note of the jet.

No. 15. If the jet, instead of descending vertically, issues horizontally, then under ordinary circumstances,—that is to say, when it is not exposed to the influence of a sonorous instrument, but is allowed to strike the liquid in the vessel which receives it, —its discontinuous part presents ventral segments and nodes like those observed, under the same circumstances, in jets descending vertically (No. 6), and the vibrations of an instrument also modify it in the same manner.

If the jet is made to ascend obliquely, the same phænomena are still observable as long as the angle formed with the horizon does not exceed from 20° to 25°.

No. 16. Beyond this limit, however, and up to 45° or 50°, the discontinuous part assumes other aspects. When the jet is not under the influence of a sonorous instrument, this discontinuous part appears scattered into a kind of sheaf in one and the same vertical plane. Under the influence of vibrations of a determinate period, it may happen that this sheaf resolves itself into two distinct jets, each possessing regularly formed ventral segments and nodes; sometimes, indeed, with a different determinate note the sheaf becomes replaced by three jets; lastly, there is always one note which reduces the whole to a single jet, presenting a perfectly regular system of ventral segments and nodes, and this is the note which also produces the greatest shortening of the continuous part.

No. 17. With one and the same charge and orifice, the number of vibrations corresponding to the note which produces the greatest effect upon the length of the continuous part, and upon the dimensions of the ventral segments of the jet, is less the greater the angle between a descending vertical line through the orifice and the direction in which the jet issues. The difference between the numbers of these vibrations in the two cases where the jet falls vertically and where it issues horizontally is inconsiderable, but the same becomes very great when the latter case is compared with that where the jet ascends vertically.

§ 4. In proceeding to the explication of these curious phænomena, we shall devote all paragraphs between this and the 24th to the consideration of jets descending vertically.

Experiment has shown us*, that, in the transformation of a cylinder of liquid, the length of a contraction is exactly, or at least very nearly, equal to that of an expansion; and, as we then asserted, we shall demonstrate in the sequel that this equality is rigorous at the commencement of the phænomenon. Now this result is evidently applicable to the nascent contractions and expansions of a jet, whence it follows that the respective durations of the passages of one of these contractions and of one of these expansions at the contracted section are equal. On the other hand, a division of a cylinder or of a jet being comprised between the middles of two successive contractions, and being thus composed of an expansion and two semi-contractions, the duration of the passage of a division of the jet at the contracted section is necessarily equivalent to the sum of those of the passages of an expansion and a contraction; and inasmuch as these two latter are equal, we arrive at this first conclusion: the duration of the passage either of a contraction or of an expansion at the contracted section is equal to half the time of passage of a division.

But we know† that the number of vibrations per second corresponding to the note produced by the shock of the discontinuous part of the jet against a stretched membrane, is equal to double the number of isolated masses which impinge upon this membrane in the same interval of time; and, in virtue of our new hypothesis (§ 2), this latter number coincides with that of the divisions which pass the contracted section during the same time;

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* Second Series, § 46.

† Ibid. § 82.

hence the duration of each of the vibrations in question, like that of the passage of a contraction or an expansion, is equal to half the time of passage of a division, and we may thus deduce the following fundamental conclusion:—

The duration of each of the vibrations corresponding to the note produced by a jet is equal to that of the passage of a contraction or an expansion at the contracted section.

§ 5. Let us now suppose, that, by means of the expedients indicated by Savart, the jet is first protected from the influence of vibrations proceeding from the fall of the liquid into the receiving vessel and from external noises, and then—the jet being thus abandoned to the sole action of the forces of figure—that a note exactly in unison with that which would be produced by the shock of the discontinuous part against a stretched membrane, is transmitted to the vessel whence the jet issues, and thus to the liquid it contains. The liquid is in a state of vibration as it flows from the interior of the vessel towards the orifice; consequently, if these vibrations have a vertical direction, each portion of the jet which passes the contracted section during the execution of a descending vibration will be animated with the velocity $\sqrt{2gh}$, together with that of this vibration, and hence such a portion will contain more liquid than that which would have passed during the same time in the absence of these vibrations. The excess of velocity will, in fact, tend to communicate itself to the part of the jet situated below that which we are now considering; but, disregarding for a moment the action of the forces of figure, we must at least admit that this inferior part will, by its inertia, offer a certain resistance; and that, as a consequence, the excess of liquid brought by the excess of velocity will tend to spread itself out horizontally, or, in other words, to produce a lateral expansion of the portion of the jet to which it belongs.

This granted, if the figure (almost cylindrical) which the jet would assume by the sole effects of the translatory motion of the liquid and of the circular form of the orifice were a figure of stable equilibrium, the portion which, under the action of the descending vibration, expands laterally as it passes the contracted section, would at the same time strive to regain its former shape. It follows from this, under the hypothesis in question, that as the expansion developed itself, it would also propagate itself to the underlying strata, and would constitute upon the surface of the jet an elevated wave of a certain length, which would descend with a velocity equal to the sum of that of its propagation and that of the liquid. Again, the portion of the jet which would immediately afterwards pass the contracted section in an ascending phase of vibration, and which on that account would traverse the same with the velocity $\sqrt{2gh}$ diminished by that of the vibration, would, for contrary reasons, give rise to a depressed wave on the surface of the jet, of the same length as the elevated one, and the former would follow the latter with the same velocity. Afterwards would come a new elevated wave followed by a new depressed one, and so on as long as the communication of vibrations continued.

But, in consequence of the instability of the cylindrical figure, and of the tendency of the jet to become transformed into detached masses, quite different results take place. Let us imagine that the lower extremity of one of the expansions which would be formed under the sole action of the forces of figure, due to instability, passes the contracted section precisely at the moment when a descending vibration commences in the liquid. Then inasmuch as the forces of figure push continuously an excess of liquid into this portion of the jet, which expands the latter without its having any tendency to return to its former shape, it is evident that the quantity of liquid brought at the same time by the additional velocity, due to the descending vibration, may spread itself out laterally and contribute to the formation of the expansion without having to surmount a contrary tendency. Besides, since the duration of the vibration is equal to the time employed by the portion of the jet,—converted into a nascent expansion by the forces of figure alone,—in passing the contracted section, the upper extremity of this portion will pass the contracted section precisely at the expiration of the vibration, so that the immediate action of the latter will have been exercised upon the whole portion in question and upon this portion only. Lastly, since the expansion produced by the combined actions above alluded to has no tendency to efface itself, neither will it be propagated to the subjacent strata, and hence it will not give rise to a wave. Thus the portion of the jet under consideration will, from the beginning, be more expanded than it would have been in the absence of vibrations; but it will have the same length, and will descend with the same velocity as in the latter case.

The descending vibration will be followed by an ascending one; and as the latter diminishes the velocity of passage at the contracted section, there will result, as we have already intimated, a diminution of volume in the portion of the jet under its influence, so that the same will tend to become thinner; but as the forces of figure tend to make a contraction of this portion, the attenuating action of the vibration will again be unopposed, and consequently no wave will be formed. We see, therefore, that, like the expansion which preceded it, the contraction thus formed by the double action of the forces of figure and of the vibration will be more developed, though it will have the same length, and will descend with the same velocity, as if the jet were abandoned to the sole action of these forces of figure.

The same thing will take place with all the other expansions and contractions. In virtue of the equality between the time employed by each of these portions in passing the contracted section and the duration of each vibration, all the expansions will coincide with the descending, and all the contractions with the ascending vibrations; the one as well as the other, therefore, will preserve their length and their velocity of translation; but they will all leave the contracted section in a more developed condition, in other words, in a more advanced phase of transformation than if no vibrations had been produced.

§ 6. But the action of these vibrations does not cease here. In fact, the velocities of the descending and ascending vibrations, which, as we have already intimated, change their direction in the expansions and contractions in order to produce a greater transversal development in the former, and a greater attenuation in the latter, cannot destroy themselves in each of these portions the moment they have passed the contracted section; these velocities, therefore, thus changed into transversal velocities, will continue, like acquired velocities, to augment those which result from the forces of figure.

§ 7. In order that the transmitted vibrations may with all their intensity exert upon the nascent divisions of the jet the action described in the two preceding paragraphs, it is necessary that they have a vertical direction at the orifice, as we have above imagined. Without doubt it would be difficult to show, *à priori*, that in propagating themselves to the orifice, the vibrations there actually assume this direction; but Savart, who occupied himself so much with the communication of vibrations, implicitly admits the fact. Indeed, on the one hand he supposes that these vibrations merely reinforce those which, according to him, are generated by the efflux itself, and which are necessarily vertical; and, on the other hand, he nowhere says that, in order to obtain a maximum action, it is necessary to give the sonorous instrument any particular position. At all events, in case of difficulty it would suffice to remark, that whatever may be the actual direction according to which the liquid molecules execute their vibrations in passing the orifice, we may always—except in the exceptional case where this direction is exactly horizontal—conceive each vibration divided into two others, of which the one, being horizontal, will have no influence on the transformation of the divisions of the jet, whilst the other, being vertical, will exert its whole action.

We have assumed, too, that each descending vibration commences at the moment when the lower extremity of each expansion passes the contracted section; but if this coincidence does not exist when the vibrations first exercise their influence, a struggle will take place between the actions of the forces of figure and those of the vibrations; and it is manifest that from that moment the transformation of the jet, which, being a phænomenon of instability, may be displaced by slight causes, will cause all the expansions and contractions to advance or recede, so as soon to establish the above coincidence, and thus to permit the concurrence and full liberty of the two systems of actions.

§ 8. These principles being admitted, we proceed to show how, one by one, all the modifications suffered by a jet under the influence of vibrations follow as consequences.

We must, in the first place, remember that when the jet is abandoned to the sole action of the forces of figure, the velocity with which its transformation is effected remains very small up to a considerable distance from the contracted section, so that the corresponding portion of the jet has a calm and limpid aspect; in the second place, that at a greater distance the expansions assuming a more rapid and perceptible development, the jet appears to widen up to the point where the masses detach themselves; and lastly, that beyond this point the diameter of the jet, which coincides with that of these masses themselves, is sensibly uniform*.

Such a jet being conceived, let the note we have heretofore been considering be produced in its proximity. Inasmuch as under the influence of this tone each division leaves the contracted section in a more advanced phase of transformation (§ 5), and as the transformation also departs from this phase with a greater velocity than it would have had under the sole action of the forces of figure (§ 6), it necessarily follows that the transformation in question will become completed in less time; consequently each division will attain the condition of a detached mass at a less distance from the orifice, and thus the continuous part will become shortened.

And since the expansions are more developed from their origin, we see, in the second place, that the apparent thickness of the limpid portion of the jet—which thickness, in each point of the length of this limpid portion, is evidently that acquired by the expansions when passing the same—will appear augmented.

Thirdly, the excess of transversal velocity received by the transformation from the vibrations, and which continues as acquired velocity, must necessarily cause the horizontal diameter of the successive masses to exceed that of the spheres which these masses tend to constitute, so that the said masses will be flattened in a vertical direction. But, manifestly, this horizontal

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* Second Series, § 70.

extension and this vertical flattening must render the capillary pressure at the contour of the masses greater than that at points near the axis, whence there will arise an increasing resistance which will ultimately destroy the transversal velocity.

The differences of pressure will then act freely, and the mass will return towards its spherical figure of equilibrium; but the phænomenon taking place with an accelerated velocity, this latter figure will not be a permanent one; on the contrary, the mass will become contracted in a horizontal, and elongated in a vertical direction, until the increasing resistance which results from the new inequalities between the pressures shall have destroyed the velocity acquired; afterwards the mass, being solicited by the differences of pressure which produced this resistance, will again return towards the spherical figure, and again go past it in order to become extended a second time horizontally and flattened vertically, after which it will recommence the same series of modifications, and will continue its oscillations of form as long as it continues to descend.

Thus, in the case of unison with the note which the shock of the discontinuous part would produce, the phænomena recorded in the first four numbers of paragraph 3 are very simply explained.

Since, however, the continuous part of the jet reaches almost to the middle of the first ventral segment, and consequently almost to the point of the agitated part corresponding to the first of its maxima of thickness, we must admit that each mass attains its first phase of greatest horizontal development a little before it becomes completely detached, and at the moment, no doubt, when it is connected with the mass which follows it only by a mere thread.

As to the appearance of systems of waves presented by the ventral segments when the phænomena are not altogether regular, it is evidently, as recognized by Savart, the result of the inexact superposition of many ventral segments produced individually by the successive masses: these ventral segments are seen simultaneously, and appear as it were through one another, in consequence of the persistence of their impressions upon the retina.

§ 9. It is evident that the interval of time between two phases of greatest horizontal contraction, or, in other words, the time employed by each mass in executing a complete oscillation of form, is independent of the velocity of translation; consequently the space described by a mass during the time in question increases with the velocity of translation; but this space is evidently the distance between the central points of two nodes or the length of a ventral segment*; this length, therefore, ought to increase with the charge.

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* It is thus that Savart appears to regard ventral segments whenever he

The volume of the nascent divisions also increases with the charge (§ 2), and as each division furnishes a detached mass, the volume of these masses must likewise increase with the charge; now the greater the volumes of these masses, the greater must be their horizontal diameters when the same attain their maxima and minima; but these greatest and least diameters are respectively the diameters of the ventral segments and nodes; hence the diameters of both ventral segments and nodes ought also to increase with the charge. The limit, however, to which this augmentation tends is not a wide one; for the greatest volume which the isolated masses can acquire is evidently that which they would possess if the translatory motion of the liquid were uniform, that is to say, the volume of the spheres into which an indefinite cylinder, formed of the same liquid and having a diameter equal to that of the contracted section, would resolve itself*.

Again, if the charge does not vary, but a greater orifice is employed, the volume of the divisions of the jet, and hence that of the detached masses, will also be more considerable; now the greater the masses the less rapid should be the oscillations of form, and consequently the greater the space through which they descend during one of these oscillations; thus the length of the ventral segments ought to increase with the diameter of the orifice. As to the respective diameters of the ventral segments and the nodes, it is evident, from what has been already advanced, that they will increase at the same time.

From the present paragraph it will be seen, therefore, that the experimental results in No. 5 of paragraph 3 are once more the necessary consequences of theory; in the case, of course, of vibrations having the same period as those of the note natural to the jet. Let us proceed to the Nos. 6 and 7 of paragraph 3.

§ 10. When the jet is not exposed to the influence of a sonorous instrument, but is received in a vessel placed simply upon the ground, the principal cause of the vibrations transmitted by the air and the supports of the vessel from which the jet escapes, is the shock of the detached masses against the liquid into which they fall; it will easily be understood, therefore, that the majority of such vibrations will have a period equal to that which would result from the shock of the masses in question against a stretched membrane, and consequently that their action upon the jet is explained by what has already been advanced in the preceding paragraphs. The intensity of the vibrations thus pro-

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occupies himself with their length, and in the paragraph which follows we have adopted his expressions; in reality, however, it is manifest that the space in question consists of an expansion and two semi-contractions.

* Second Series, § 74.

duced, however, not being great, the modifications of the jet cannot acquire all that development of which they are susceptible; besides, as the vibrations under consideration are not very regular, and are accompanied by other smaller and still more irregular ones proceeding from external noises, the phænomena must be affected by such irregularities; in fact, it was under these circumstances that Savart described the appearance of the waves within the ventral segments.

With respect to jets issuing under these circumstances from two different orifices, and under different charges, Savart measured, approximatively, the lengths and the diameters of the ventral segments as well as the diameters of the nodes. It may be of use to reproduce here the results of these measurements, in which the centimetre is assumed as unit.

Diameter of the orifice.	Charge.	Length of the continuous part.	Length of the ventral segments.	Diameter of the ventral segments.	Diameter of the nodes.
0·6	4·5	40	25	0·90	0·70
...	12	59	30	1·00	0·75
...	27	82	39	1·10	0·80
...	47	112	60	1·20	0·90
0·3	4·5	16	7·8	0·50	0·28
...	12	25	9	0·52	0·32
...	27	41	13	0·55	0·36
...	47	55	16	0·60	0·40

We may here remark, that as the length of a ventral segment coincides with the space described by a mass during one of its oscillations of form, and as the period of such an oscillation is constant for one and the same jet, the ventral segments of the latter ought to increase in length from the first downwards on account of the acceleration in the descent. It is somewhat singular, therefore, that Savart, who in another part of his memoir mentions this augmentation in reference to a particular experiment, has nevertheless in the above Table given the lengths in question as if they were absolute; it is to be presumed that they refer to the first ventral segment of each jet. In fact, the experiment in which Savart observed the augmentation of the lengths of the ventral segments must have rendered the effect more than ordinarily apparent, since the first ventral segment was generated very near the orifice.

§ 11. If, whilst the jet is falling freely into the vessel placed to receive it, a note in unison with its own is produced on an instrument in its neighbourhood, as we have hitherto supposed, then under the action of these more intense and perfectly regular vibrations the modifications of the jet will necessarily be more developed; that is to say, the limpid portion will appear still a little thicker, the continuous part will become still shorter, the ventral segments wider, and the nodes narrower. Moreover, the superposition of the ventral segments formed by each of the masses will be more exact, and thus they will overreach one another less toward their extremities, so that the ventral segments which result from their superposition will be more gathered together, and the nodes which separate the same will appear to be elongated. Now such is, in reality, as may be seen in No. 7 of paragraph 3, the state of the jet under the influence in question.

The phænomena would be still much more regular if the jet were in the first place protected from all foreign influences; and, in fact, Savart speaks of the great regularity of the ventral segments which manifest themselves when such a jet is received upon a stretched membrane which serves as a sonorous instrument for giving the unison.

§ 12. When the instrument employed gives a note not in unison with that of the jet, then as the vibrations no longer succeed each other at the same intervals as the passages of the expansions and contractions due to the forces of figure, there can no longer be an incessant concurrence between the two kinds of actions, and it will at once be perceived that very complicated effects must result from these alternations of accord and opposition. Nevertheless we will attempt to unravel to some extent the phænomena then presented by the jet.

To simplify our task as much as possible, we will assume all foreign actions to have been previously annulled. During the succession of phænomena, let us mentally seize the moment when the central point of a contraction due to the forces of figure passes the contracted section precisely at the same time that an ascending vibration is half completed; this vibration will then evidently concur with the forces of figure to increase the contraction. If the note of the instrument, however, has a higher pitch than that of the jet, in other words, if the duration of a vibration is less than that of the passage of the contraction, a greater or less portion of the bottom of the latter must have been in conflict with the end of the preceding descending vibration, and an equivalent part of the top of the contraction will also be in conflict with the commencement of the succeeding descending vibration, because these descending vibrations tend to expand the portions of the jet upon which they act. If, on the contrary, the note produced by the instrument has a lower pitch than that of the jet, it is evident that a concurrence will exist throughout the whole of the contraction, but that the commencement of the vibration must have been in conflict with the upper part of the preceding expansion, and that the end of this same vibration will be in conflict with the lower part of the following expansion.

It is easy to see that after a certain number of vibrations a similar effect will be reproduced, in other words, that the middle of an ascending vibration will again coincide with the middle of the passage of a contraction, and that this coincidence will occur periodically and at equal intervals. For example, if the duration of a vibration were five-sixths of that of the passage of a contraction or an expansion, then six double vibrations—each of which is composed of an ascending and a descending vibration—would be accomplished in the same time as the passage of five contractions and five expansions; and if we commence counting from one of the above coincidences, another coincidence will occur at the end of this interval of time; in our example, coincidences will occur at intervals equal to the duration of six double vibrations. Let us next attempt to discover what takes place during each of these intervals, or, in other words, between any such coincidence and the next following one.

For this purpose let us examine what occurs at the termination of the first half of one of these intervals. In the above example we shall evidently have again arrived at the middle of an ascending vibration; but if we consider that the interval commences with the passage of the beginning of a division (§ 4), and exactly embraces the passage of five whole divisions, we shall recognize that the middle of a division, that is to say, the middle of an expansion, is just passing the contracted section at the termination of the first half of the interval under consideration; this vibration, therefore, will be altogether opposed by the forces of figure; the conflict which is now at its maximum will evidently have been on the increase up to this moment, that is to say, it will have been occupying greater portions of the successive vibrations, in order afterwards to diminish by the same degrees.

These principles being granted, let us notice the consequences.

Each of the contractions corresponding to a coincidence will leave the contracted section in a more advanced phase of transformation, and thus each will become ruptured at a less distance from the orifice than if no vibrations had been produced; but the following contraction, being already under less favourable conditions, will not be so soon ruptured; and the subsequent ruptures, up to that of the contraction for which the conflict between the two actions has been greatest, will, in a similar manner, be effected at increasing distances from the orifice; after this the phænomena will retrograde, that is to say, the places of successive rupture will remount until a contraction corresponding to a coincidence again returns, when everything will recommence in the same order. It appears, then, that in such a jet the continuous part has different lengths which succeed each other periodically; the shortest of these lengths, however, ought to be considered as that of the veritable continuous part of the jet, because in it the continuity is never interrupted, and it must necessarily be shorter than it would be if the same jet were not submitted to the influence of a sonorous instrument.

At the same time, however, the shortening of the continuous part ought to be less than in the case of unison. In fact, if the note of the instrument has a higher pitch than that of the jet, the most complete coincidence between the two actions can, as above remarked, only establish itself about the middle parts of the contractions where it exists, and at the extreme portions there is conflict. If the note of the instrument has a lower pitch, the concurrence, it is true, extends throughout the whole contraction, but then conflict exists in the adjacent portions of the two expansions, between which the contraction is situated; and as these portions admit with less facility the liquid chased thither by the contraction, the latter cannot freely obey the two actions which simultaneously tend to attenuate it.

In the second place, the shortening, according to the above, ought to be less the more the note of the instrument deviates from being in unison with that of the jet; for the higher its pitch above the latter, the less the portion of the contraction which corresponds to the above-described coincidence; and the lower its pitch below the note of the jet, the further the conflict extends upon the adjacent expansions.

Lastly, since upon the contractions corresponding to coincidences, and up to a certain distance above and below each of the same, the action of the vibrations favours more or less that of the forces of figure, the other modifications determined by unison ought also to be manifested by the jet in an analogous, though in a less decided manner; thus the limpid portion will again appear a little thickened, and the agitated part will have ventral segments and nodes; these modifications, however, will be again less developed the greater the interval between the note of the instrument and that natural to the jet.

The complication of the subject permits us, therefore, merely to enunciate, as results, the following four conclusions:—When a note of a higher or lower pitch than that natural to the jet is produced at a certain distance from the apparatus,—1st, the continuous part ought to assume different lengths periodically; 2nd, the shortest of these lengths, which is that of the veritable continuous part, ought to be shorter than the unchanging continuous part was before the action of the instrument, but longer than it would be in the case of unison; 3rd, the jet ought to present, in a manner analogous to, though less decided than the case of unison, a small increase in the thickness of the limpid portion, and a system of ventral segments and nodes in the agitated portion; 4th, the development of all these phænomena ought to be less perfect the more the note of the instrument deviates from unison, so that notes having too high or too low a pitch ought to appear inactive.

We have assumed that all foreign actions were previously neutralized; but since these actions tend to produce similar effects (§ 10), it is evident that if they are left unneutralized they can scarcely do more than add to the intensity of the phænomena.

We may here intimate, that notes not in unison with that of the jet at the same time give rise to other effects, which, in general, though not very apparent in jets descending vertically, manifest themselves, as will be seen, when the efflux takes place under certain obliquities. These effects depend upon the conflict between the vibrations and the forces of figure, and consequently vanish in the case of unison; instead of diminishing, therefore, like the effects just studied, the ones now alluded to increase with the divergence from unison.

§ 13. The first of the four conclusions above enunciated is precisely verified, in a particular case, by the fact recorded in No. 9 of paragraph 3. In fact, when the note of the instrument is very near that of unison, the period of vibration differs very little from that of the passage of a contraction or an expansion; and consequently, when a coincidence takes place, it will be nearly complete, that is to say, the conflict will extend only over extremely small portions of the contraction or of the two adjacent expansions; for such a contraction, therefore, almost the same things will take place as in the case of perfect unison; hence at the moment when this contraction is ruptured, the continuous part of the jet will have nearly the same length as in the case of unison; afterwards its length will increase until that corresponding to the greatest conflict is attained; but, in consequence of the near approach to equality between the respective periods of a vibration and of the passage of a contraction or expansion, a considerable time will elapse before this maximum occurs, so that the gradual elongation of the continuous part will take place slowly enough to become perceptible; lastly, the same things will occur with the subsequent shortening of the continuous part, and so forth. As to the beats, it is clear that they result from the mutual reaction of the note of the instrument and that of the jet; for although Savart does not expressly say so, we may conclude from the manner in which he adduces the fact in question, that the jet ought to fall upon a stretched membrane.

Except in the particular case of a very small interval between the note of the instrument and that of the jet, Savart says nothing of the periodic changes of the continuous part; neither should he have done so, as we shall presently see. For intervals which do not fulfil the above condition, these changes are too rapid for the succession to be distinguished, so that all the lengths, as well as all the systems of ventral segments respectively corresponding to these lengths, ought to appear simultaneously; hence, under these circumstances, each of the ventral segments of the jet ought to appear formed of individual ventral segments not exactly superposed, and consequently (§ 8) to present the aspect of an assemblage of waves. But this aspect was not new to Savart, for he had observed it (§ 10) in the ventral segments of jets not submitted to the influence of a sonorous instrument.

§ 14. The three remaining conclusions in paragraph 12 appear to be confirmed by No. 8 of paragraph 3. Nevertheless, the manner in which Savart mentions the facts might throw some doubt upon the perfect exactitude of this accord. The following passages of Savart's memoir are the only ones which refer to the facts in question:—

"The octave and fifth below the note produced by the shock of the discontinuous part against a body employed for strengthening the tone, as well as its minor third, augmented fourth, and the octave above, produce upon the jet modifications analogous to those just described*, but always with much less energy; and there are notes which have no influence whatever upon the dimensions of the jet and the aspect it presents."

Subsequently, when speaking of a jet received at a small distance from the orifice upon a thick solid body, he says,—

"Just as when the jet is entire, we find that the octaves above and below, as well as the fifth and minor third above the note in question†, also influence the state of the jet, though to a less degree."

Lastly, with reference to the modifications experienced by a jet under the influence of a note in unison with that due to the shock of the discontinuous part against a stretched membrane, but protected from every other foreign influence, he remarks that—

"Analogous results are obtained when divers notes are produced upon a stringed instrument in the neighbourhood of the reservoir, but one of these notes always exercises a greater influence upon the jet than any of the others."

Do these passages imply, that, besides the unison, only the octave and fifth below, the minor third, the augmented fourth and

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* That is to say, to those produced by unison.

† That natural to the jet.

the octave above, modify the state of the jet? This is not probable; for if so, Savart, instead of saying "and there are notes which have no influence whatever, &c.," would have said, *and all other notes except the preceding ones are without influence*, &c. Ought these passages to be interpreted as admitting that the notes there signalized are, after the unison, the most active; and that of the remaining notes of the scale, some are simply less efficacious, whilst the rest exert no action whatever? But in this case can we believe that Savart would have expressed himself thus? We may further remark, that the augmented fourth indicated in the first passage is omitted in the second.

These vague expressions show that Savart studied but little the influence of notes differing from unison; at least, under the present circumstances, it appears to us that neither a partial disagreement nor an absolute agreement between our theoretical conclusions and experimental facts can be deduced. Happily, Savart afterwards sought to augment the energy of the action of the vibrations of the instrument; and the effects, as he then describes them, ought, as we shall soon see, to be regarded as altogether confirmatory of our conclusions.

[To be continued.]

II. *On the* Cirrous *form of Cloud.*

By W. S. JEVONS, *Assayer, Sydney Branch of the Royal Mint**.

CIRRUS is a name denoting those light fibrous tufts, branches, or scrolls of cloud often seen at great elevations in the atmosphere; and the general adjective-term *cirrous* may be conveniently applied to any appearance in clouds of this fibrous or feathery character.

The term was originally proposed by the meteorologist Howard; and in defining the *cirrus* as "Parallel, flexuous, or diverging fibres, extensible by increase in any or in all directions," he left little to be desired either as to accuracy or conciseness of description.

Though so happy in his descriptions and remarks on the appearance of the cirrus, he merely attempts to explain its formation by comparing it with the well-known experiment of the electrified lock of hair. In this latter phænomenon, the hairs or other fibres employed, being all charged with the same kind of electricity, become mutually repellent and spread out to the greatest possible distances from each other. The cloud-fibres, on the contrary, are generally seen to run parallel to each other, often, indeed, to great distances and throughout the most various

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* Communicated by Thomas Graham, Esq., F.R.S.

flexures, in a manner altogether forbidding the idea of any repulsive force between them. Howard also supposes the cirrous fibres to act as conductors between masses of dry and moist air of opposite electric conditions; but though fibres, once formed, might be capable of discharging the electricities with greater ease than the surrounding clear air, it does not appear to me how the watery particles, which he has moreover to suppose precipitated by some other means, come to be gathered up in this particular form,—a sort of action with which I know of no parallel (a regular *polar* arrangement being of course a very improbable supposition).

I have not been able to find that any more satisfactory theory has been yet proposed; and, indeed, it would seem that, since the establishment of Howard's arrangement and nomenclature of clouds, but little attention has been paid to their study at all, particularly as regards the *cirrus* cloud.

A simple and natural explanation of these cirrous fibres is, I believe, to regard them as minute streamlets of air forcing their way through a stratum of air of different temperature and moisture. We have only to suppose two neighbouring masses of air, completely or very nearly saturated with aqueous vapour and of different temperatures, to *filter* into each other in minute streamlets; and the watery particles which must most certainly be precipitated, according to the well-known theory of Dr. Hutton, will be arranged so as to present exactly the forms of these streamlets, and in fact produce a *cirrous cloud.*

The extremely lofty position in the atmosphere at which these clouds nearly always occur, renders it impossible for us as yet to ascertain even the conditions of the air surrounding them; and the untangible nature of a cloud, when reached, would render direct experiments upon the mode of its formation entirely out of the question. In the case of cirrous clouds especially, we can only observe with exactness their external character and other apparent conditions, and then employ a sort of *circumstantial evidence* to demonstrate their nature.

This I have attempted to do by producing miniature representations of clouds, under conditions in which the cause of formation could be certainly known.

The extreme mobility and invisible nature of gases would render any experiments upon them, on a small scale, extremely difficult and unsatisfactory; but for our present purposes we may, I believe, substitute *liquids*, for instance water, which being so much more sluggish and dense, will be so much the slower and more observable in their motions, and will not require the same care to prevent accidental disturbances by temperature, &c.

Now a thread-like or stream-like appearance is nearly always to be seen, more or less, when two liquids of different densities are being mixed together, as in making ordinary chemical solutions, mixing alcohol and water, &c.; and if a drop of ink be added to a glass of clear water, it will be observed to sink downwards into it in tape-like streams, and eventually to spread about in threads of a more or less *cirrous* character.

This same appearance may be shown more or less distinctly in a multitude of different ways, but the following experiment has been devised and selected as presenting the most complete and striking representation of a *cirrous* cloud; and it was the accidental observation of appearances nearly the same as those produced in this experiment which led me to form the present theory.

Exp. 1. To about 800 grms. of pure water add 2 or 3 drops of hydrochloric acid, and 1 grm. measure of a strong solution of white sugar (spec. grav. of solution 1·15). Warm this to rather above 100° F., and pour the greater portion into an ordinary glass beaker about 5 inches in diameter and 9 or 10 in height (fig. 1).

Fig. 1.

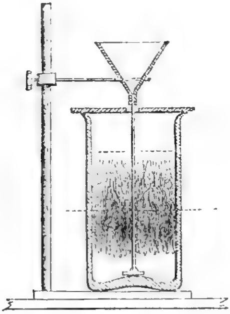
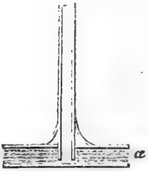


Fig. 2.



a. Discs of flannel or other stuffing.

This beaker should be surrounded by a second larger one to prevent disturbance of temperature; and a tube-funnel, allowing only a very slow stream to pass, must be placed in it reaching to the bottom, and with a termination like fig. 2; or with such similar contrivance as shall prevent all violent currents, and allow us to introduce further quantities of liquid without the least disturbance of the strata above.

The remainder of the hot solution of sugar must be added by this funnel; and before this is quite run out, a little pure cold water is to be added so as to cleanse the apparatus. Then, again, without allowing the stream to break, a second different stratum must be added, previously prepared, and consisting of 800 grms. of distilled water at the ordinary temperature of the air, with ·2 ($\frac{2}{10}$) of a gramme of crystallized nitrate of silver dissolved in it. The more gradually this stratum is inserted beneath the other, especially at the first, the more distinct will be the result of the experiment. The nicest management, indeed, and the most careful and patient manipulation are necessary in order to prevent any accidental and irregular mixture from taking place, which would confuse the shape of the cloud; but even in his case sufficient *cirrous* action will subsequently go on, to answer the purposes of our experiment.

A white precipitate of chloride of silver, of the usual cloud-like appearance, will immediately begin to form, and from the first will present an entirely cirrous character. Small streams in the form of threads or curiously shaped bands will be seen passing from one stratum into another, and often curving about in the most complicated and beautiful manner. After a time the middle of the glass will be filled by a dense and confused but still fibrous mass of cloud, which will probably soon extend itself to the bottom; but there will now also be seen with the greatest distinctness, numbers of these small parallel threads ascending and reaching nearly to the surface of the top stratum, of considerable length, and ending in evanescent points.

The slightest circular motion or disturbance communicated to the strata will cause these fibres to assume all sorts of curved and flexuous forms, which, however, in general still maintain their parallelism. And it is upon the exact resemblance which this miniature cloud bears to the common appearances of the cirrus, that the probability of the truth of this theory must be allowed principally to rest.

It is evident that the cloud of chloride of silver is produced by the gradual mixing of the upmost and lowest strata containing respectively hydrochloric acid and nitrate of silver, thus representing closely the precipitation of watery particles by the mixture of portions of moist air of different temperatures. It remains then only to consider the manner and cause of mixture.

The addition of one-eight per cent. of sugar solution was found to raise the specific gravity of water by about 4-10,000dths (that is, from 1·0000 to 1·0004 at 60° F.); but when heated to about 100°, its density is not more than about ·994 or ·995, so that at this temperature it will lie in a separate stratum above pure water at 60°.

The parts of these strata, however, which are immediately in contact, soon communicate their heat and tend to assume a mean temperature; and it is evident that whenever this is the case, the portions of liquid containing sugar must always be slightly denser than those that are pure, and must consequently sink below and displace the latter.

We shall thus have portions of the upper stratum continually sinking into the lower, and corresponding portions of the lower rising through the upper; and this movement, as the experiment demonstrates, takes place by an *interfiltration of minute, thread-like streams.*

[It is evident that the difference of temperature of the strata in this experiment is not a material point, being simply a means employed to enable us to lay one stratum upon another of a slightly greater density when of the same temperature, so that we may afterwards observe the mixing process and change of place in the most gradual manner possible.]

Exp. 2. Let the first experiment be now repeated in exactly the same manner, with the exception of adding the sugar to the lowest stratum instead of to the highest, as before.

The appearances will now be totally different: but little cloud at all will be seen to form, even after a considerable length of time; and whatever may happen to be caused by accidental disturbance will lie in a uniform or streaked flat sheet at the surface where it is produced, until it finally subsides to the bottom by its own density.

These two experiments exhibit a most striking contrast; and the only difference of conditions being in the inversion of the light and dense fluids, we are at once led to the conclusion, that different portions of liquids may, from the effects of very slight differences of specific gravity alone, be caused to mix and pass into each other in the form of minute streamlets, which, if rendered visible, as by the formation of a precipitate along their sides, present exact resemblances in form to the fibres of cirrous cloud.

Now as gases are subject to the same laws of equilibrium and pressure as liquids, excepting only as far as they are modified by the property of elasticity, it is probable that strata of the atmosphere, which, being at perfect freedom to assume the density due to the superincumbent pressure, will be further unaffected by their compressibility, must act otherwise; just like strata of very rare liquids, for instance, will, under the same circumstances, lie in quiet horizontal strata, or will displace each other violently or gradually, as the case may be. On this account it appears to me certain, that masses of air, in gradually displacing each other and mixing by reason of slight differences of density, will exhibit the same phænomena as we have seen to take place in liquids.

It may perhaps be objected, that liquid *cohesion* has some hand in producing the thread-like appearance produced in the first experiment, and that my argument fails, since cohesion is non-existent in gaseous bodies. Now though liquid cohesion might tend to keep the particles of each stratum together, and apart from those of another stratum for which they have less cohesion, and though we may conceive the two moving strata thus segregated into distinct threads, I do not think that this is at all the cause of the phænomenon. It is rather the much greater facility with which any two fluid bodies can move among each other when distinct channels are preserved, than when each particle of one is opposed to a particle of the other moving in the opposite direction,—just as in a street or a large crowd of people, the passage of all is much impeded unless those moving in opposite directions proceed along different sides or along distinct channels. It is probably a simple mechanical effect of the motions of small bodies of each fluid, produced immediately that the perfect equilibrium of the horizontal strata is in any way disturbed.

And again, I do not know that it is proved that cohesion is entirely non-existent in gases; by definition, a gas *is matter in which the repulsive forces entirely overcome the cohesive or attractive forces between the particles*; but supposing the gas so restrained by superincumbent pressure that no further expansion can take place, it is not impossible, as far as I see, that some difference of attractive *or* repulsive forces between particles of the same and particles of different gases, or the same gas in different conditions, may come into play, which, if not to be called *cohesion*, will at least produce the same apparent effects as that force. The undoubted attraction which many solids exercise on gases, as seen, for instance, in the cohesion of air to the glass tube of the barometer, and the absorption and condensation of gases by charcoal, platinum black, &c., show that gases do not possess repulsive forces only.

Another distinct property or force existing equally in liquids and gases is the *diffusive*, which would certainly tend to cause mixture of two different strata; but this is evidently not the least concerned in this phænomenon, since diffusion acts quite independently of, or contrary to gravity, and would therefore produce nearly as much effect in the second experiment, where no cirrous appearance (or rather only *flat cirrus* or cirrostratus) at all was seen, as in the first experiment.

I think it is pretty evident, then, that when two horizontal and tranquil strata of gases are in contact, the upper one being very slightly the denser, they will tend to change places, or to mix by filtering into each other in distinct portions, which in moving will assume the form of small channels or *threads*. If this do not take place, the strata could only remain the denser supported upon the lighter, until the half-chemical process of diffusion would cause their complete mixture; but no one can suppose the strata so equally *poised* that the difference of specific gravity would not cause mechanical movement.

We must carefully distinguish this gradual filtration from the violent motions of large masses of fluid, also produced by differences of density and pressure, but which are on so much larger a scale as to produce entirely different appearances. If the whole upper stratum in our first experiment were suddenly to become of greater density than the rest of the liquid beneath, it would undoubtedly sink down through the latter in large rapid streams; but instead of this it is only small quantities of liquid at the junction of the strata, mere pins' heads in size, which, from the equalization of temperature, become successively of sufficient density to sink, and they then do move with all the violence of which they are capable.

It is the violent upward motion of currents of moist air which produces the *cumulus*, the rarefaction and reduction of temperature, however, occasioning the precipitation of watery particles; but just as cumuli rise in detached rounded masses separated by portions of clear air, which must have more or less of a descending motion, so very small portions of air will filter upwards in distinct fine threads, with intervening threads moving in the opposite direction.

But we are not wanting in some analogous and easily observed appearances in the air itself, as in the long wavy threads or bands of smoke rising from the wick of a candle, or the light-streaks so distinctly seen moving across a sunbeam entering a room of which the air is smoky or thick. The chemist, too, cannot help remembering the thread-like clouds which always appear when hydrochloric acid and ammonia are exposed within the reach of each other's vapour. By carefully employing these chemicals, a true and distinct cirrous cloud might no doubt be produced; but gases are so easily disturbed and move so rapidly, as soon to cause complete mixture and confusion in any experiment on a small scale.

On this theory, then, cirrous fibres are considered to be thread-like streamlets of moist air, forcing their way by the effect of gravity alone through masses of air likewise moist, but of different temperature and density; watery particles being of course precipitated, according to Dr. Hutton's theory, and becoming visible over the surfaces of the streamlets.

It will now be well to mention a few points in which the analogy between the miniature experiment and the real atmospheric *cirrous* cloud evidently fails.

1st. The white precipitate of chloride of silver used to represent the watery particles of a cloud is not resoluble in fresh quantities of the fluid; but any portions of watery cloud-matter subsiding into or otherwise coming into contact with undersaturated air will immediately re-evaporate and vanish, so that appearances will be considerably modified.

2nd. The precipitate of chloride of silver has a tendency to subside, its gravity being much superior to that of water; but during the precipitation of aqueous vapour into cloud-matter a great amount of latent heat is given out, which, by expanding the air, diminishes its density, and gives it a tendency to rise above surrounding portions instead of subsiding, on account of the weight of the watery particles. Hence it is that cirrous fibres will be observed ascending much more frequently than descending; and that the ends of fibres have a general tendency to curve upwards, as we may observe in nature.

3rd. Atmospheric strata being usually in horizontal motion in different directions, cloud-fibres produced by the interfiltration of portions of these cannot have a perpendicular direction, but will generally lie in a nearly horizontal position, except perhaps towards the extremities. They will also more generally partake of the motion of the upper than the under current.

It is easily conceivable, on this theory, that by different combinations of currents and other circumstances, the most variable appearances and shapes will be assumed by the cirrous fibres, even such a variety as we observe in their beautiful natural forms.

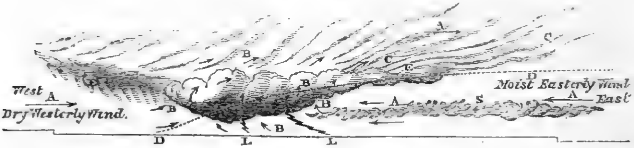
To render this paper complete and convincing, it would be necessary, in addition to showing that such cirrous filtration may take place among gases, to prove by facts that the conditions of our theory, viz. a moist and dense stratum of air lying upon another portion of air, however small in quantity, also saturated with aqueous vapour, but specifically lighter, may or does actually occur in the atmosphere. But our present knowledge of the motions of the currents of the atmosphere, and more especially of the distribution of aqueous vapour throughout them, is so imperfect as to render this extremely difficult. I must leave this task to more competent persons, content if they shall first of all pronounce that the essential point of the theory, a *filtration* action, is a reasonable explanation of the form and appearances of the *cirrus*.

I think, however, I may mention several instances in which such circumstances may very probably occur, and where this theory seems to me to afford a very probable explanation of remarkable phænomena, or else to lead directly to such an explanation.

Thunder-clouds in Sydney are nearly always preceded by a sort of lofty spreading crest of cirrus or cirrostratus, moving rapidly in a direction contrary to the wind at the surface of the earth. Beneath this there at last appear large masses of *cumulous* storm-cloud, either in huge towering columns, or in irregular torn portions exhibiting great agitation, from which the rain and lightning proceed, the whole moving in the same direction as the cirrus above. Squalls or a decided change of wind and weather after the storm show that the latter arises from the conflict of two currents, the lower one always moist and *sultry*. At the point of conflict, this lower current, as I suppose, is turned upwards, and then probably swept backwards by the other current which is replacing it. The upward motion produces a vast precipitation of vapour in the cumulous form, leading to rain and electrical excitation; while at a higher level, the air proceeding from the summits of these cumuli still preserves sufficient warmth and moistness to cause it to filter upwards into the other portions of the colder upper current. Would not an advancing cirrous crest be exactly the form of cloud produced on this theory?

Imaginary Section of a Thunder-cloud near Sydney.

Fig. 3.



D (dotted line) shows junction of two currents of air, the directions of these being indicated by arrows (A A).
B B, arrows showing upward and backward current of moist air.
L, lightning striking from thunder-cloud to earth.
C C, cirrous crest moving with upper current.
S, scud moving in lower moist current.
E, the appearance of *dropping portions of cloud* at foot or back of storm.

To proceed a little further in this explanation of the production of a thunder-storm, to which our cirrous theory has partly led us, may not the electrical excitation of the thunder-cloud be easily accounted for by supposing it to collect, like a *conductor*, the whole electrical charge of the lower moist and electrified current as successive portions of this reach the point of conflict and rise upwards? Electricity probably exists in the air precisely as on the surface of the excited glass plate of an electrical machine. There are large quantities of the electrical fluid or force distributed among individual particles; but these are so far separated by masses of non-conducting air, that no large quantity can be discharged at any one moment. When the whole mass of air, however, passes upward and *through* the cloud, the watery particles which are precipitated assume all the electricity of the particles of air, just as the metallic points of the conductor collect the whole electrical charge of the surface of the glass cylinder passing before it; and being a good conducting body, the cloud soon discharges itself to the earth in a succession of immense sparks or flashes, which will be kept up as long as a current of moist, warm and excited air continues to arrive at the storm-point, or this latter itself travels over new and excited regions of air. The continual supply of electricity, often amazingly great, at which some authors have much wondered (see Miller's 'Elements,' vol. i. p. 313), would at all events be easy to understand; and this explanation can be extended to the phænomenon of lightning in any *cumulous* cloud, since the cirrous crest or the stratous masses which also usually form, are in no way essential to the collection of electricity.

The section of a thunder-cloud, on the preceding page (fig. 3), is only intended to represent a certain class of storms occurring in Sydney, since other storms of different character occur even here, but it seems to agree exactly with the descriptions of storm-clouds in other localities (see Arago's 'Essay on Thunder and Lightning,' Chap. II.; or Howard's 'Climate of London,' Introduction, p. xlviii). Arago proves that lightning may issue from a single small cloud; but in this case I presume it will always be a cumulous cloud, both from the descriptions of the clouds in the cases cited, and from the remark of Beccaria, which he quotes, viz. that "thunder and lightning never issue from *smoky* clouds; that is to say, from those strata of clouds which are characterized by their apparent uniformity of composition and regularity of form," meaning, I suppose, stratous, cirrostratous or cirrous clouds.

It will perhaps have been observed in our experiment No. 1, that the streams descending from the upper stratum into the lower often end in little knobs, or drops, or *scrolls* of a peculiar and interesting shape. I do not understand why the descending streams should differ in shape from the ascending ones, which generally, but not always, end in evanescent points, though it might arise from the tendency of chloride of silver to subside, as mentioned before; but it is remarkable that similar appearances are often to be seen on the under surface of dense cirrostratous clouds, especially at the front or the tail of a thunder-cloud (as shown in figure at E). Sometimes these dropping portions of cloud, or *droplets,* seem to come in contact with dry air, when their well-defined form is destroyed, and a fibrous or fur-like appearance only remains. They appear to be truly portions of *subsiding* cloud.

To return to our theory, Howard says ('Climate of London,' Introduction, xliii), "Steady high winds are also preceded and attended by streaks (of cirrus) running quite across the sky in the direction in which they blow." This would be the precise effect produced by a rapid upper current into which, at some point or points, moist streamlets of warm air were filtering upwards from another current. Watery particles would be precipitated, and then rapidly swept away in long, flat, and nearly horizontal streaks.

An indirect electrical origin for the cirrus is not incompatible with this theory, and we may thus perhaps have a cue to the supposed connexion of cirrus and cirrostratus with *auroral* displays. It is not inconceivable that electrical or magnetical currents passing among moist and varying currents of air in the higher parts of the atmosphere, might occasion some rise of temperature in particular portions. If this amount only to $\frac{1}{10}$th of a degree Fahrenheit, or say only $\frac{1}{100}$th or $\frac{1}{500}$th of a degree, this would be quite sufficient to originate a very gradual and slow filtering action in these tranquil regions, and produce those very rare but exceedingly lofty scrolls of cirrus which are said to last sometimes for days together unchanged.

To follow out these speculations into their full results would require volumes; the object of the present paper is fulfilled in merely suggesting a *filtering action* as the explanation of many important phænomena of the atmosphere.

I will add a few remarks concerning the other well-known forms of clouds.

In our second experiment, the meteorologist cannot avoid recognizing a striking resemblance between the sheet-like white precipitate, only produced when the strata are disturbed and mixed by internal causes, and the cloud known as stratus, which has a flat or lenticular shape, and invariably a nearly horizontal position. Cirrostratus is often, indeed, the more proper name for the cloud artificially produced in our experiment, since accidental cirrous *mixture* of the strata nearly always takes place from imperfect manipulation; but the fibres thus produced always quickly settle into the horizontal position, and form a *streaked* sheet which exactly represents the cirrostratus.

The stratus, it is already well known, is produced by the mixture of portions of air saturated with moisture but differing in temperature; and we may now pronounce more precisely from the conditions of our experiment, that it is formed when strata, moving in contact with each other, are caused to mix at their common surface by friction or other similar mechanical causes. This is shown, too, by the form which it often assumes of parallel transverse bars or waves; for just as a breeze *ripples* the surface of water over which it blows, or as the latter again occasions *ripple-marks* on the sandy bottom over which it moves, one current in the atmosphere may produce a *ripple* in flowing over another; and as the two portions of air would mix more at one part of each undulation than any other, long waves of cloud would be the result. *Ripple* would, I think, be a convenient term for this appearance in descriptions of clouds.

The only other fundamental form of cloud besides the cirrus and the stratus is the cumulus, the nature of which is too well known to need any remarks here. The fundamental cloud-actions of the atmosphere may then be laid down as follows:—

1st. Cumulus, produced by the *elevation* and rarefaction of large masses of air.

2nd. Stratus, produced by the *mechanical mixture* of different portions of moist air at their common surface.

3rd. Cirrus, produced by the *interfiltration* of different portions of moist air.

Howard felt and marked the fundamental differences of these by giving them these distinct and admirably chosen names. They were not laid down as fundamental in obedience to any theory or deduced system of classification whatever, but because their appearance and conditions of occurrence in the atmosphere are so entirely distinct as at once to point to essential differences of nature, whatever these may be. Howard, indeed, had very little conception of how these clouds were formed; and carried away, perhaps, by the success of his first attempt at classification, he completed his nomenclature by applying to a number of other subordinate species of cloud, systematic terms obtained by the combination of these three principles, viz. cumulostratus, cirrocumulus, cirrostratus, and cumulo-cirrostratus or nimbus.

Now these compound terms will not be at all philosophical or advantageous unless the species of cloud they represent are actually formed by the *amalgamation* of the cloud-actions denoted by the component parts of the term. In adopting a theory of these actions, we should therefore have to re-examine and perhaps remodel these terms with a view to render them true and consistent.

Now it seems to me that the cirrous and stratous actions are those only which can be truly amalgamated, and take place in the same portion of air; and that if the other actions do ever unite, the result will not be the clouds universally denoted by the terms used. Cirrostratus is, indeed, a true intermediate species of cloud, and the only commonly occurring species of the kind. Not only in the second experiment did we produce a distinct *streaked sheet of cloud,* which is the exact appearance of cirrostratus, but it is evident, that, supposing the above theory of cirrus to be correct, the *cirrous* mixture of two portions of air in motion upon each other will seldom go on without a certain amount of mechanical mixture taking place at the same time (and *vice versâ*), which will produce cirrostratus, and explain the fact that this compound form of cloud is more common than either the simple cirrus or stratus alone.

If it be said that the compound term denotes two kinds of actions going on at different parts of the same cloud, and not necessarily in the same portion of air, that, for instance, cumulostratus (sometimes called, I think, the *anvil-shaped* cloud) is a cumulus extending at top into a stratous projection, I answer that the term becomes too indefinite, since a cumulus and stratus may be combined together in several other ways, so that *anvil-shaped cloud* is a more exact and desirable description.

But if even cumulostratus and nimbus should be retained on account of their well-established use, the term *cirrocumulus* is still outlying as entirely unmeaning and improper. Howard himself thus describes this modification :—"The cirrocumulus is formed from a cirrus, or from a number of small separate cirri, by the fibres collapsing, as it were, and passing into small roundish masses, in which the texture of the cirrus is no longer discernible, although they still retain somewhat of the same relative arrangement." The cirrocumulus is produced from the cirrus when the filtering action is from some cause or other stopped; watery particles once precipitated do not always evaporate again immediately the action stops, since they may be surrounded by air perfectly saturated with moisture; but the forms of the streamlets or cirrous fibres are soon broken up, and the cloud-matter aggregates into small rounded bodies. There is nothing in the formation of this sort of cloud which in the least resembles a cumulous action, and it occurs indeed in a totally different region of the atmosphere from true cumulus; the term cirrocumulus is therefore *improper*, as well as but imperfectly descriptive of the mere appearance of the cloud. I should propose in its place the short term *cirroidus*, or *cirroid cloud*, which sufficiently expresses its undoubted cirrous origin, and half-cirrous appearance.

Howard's nomenclature by no means exhausts the variety of common species of cloud; as there is cirroid cloud derived from a former cirrus, so there may be masses of cloud-matter remaining from former cumuli and strati, equally important and frequently occurring in the atmosphere, though not so distinctive in appearance. For those species I would propose the corresponding terms *cumuloid* and *stratoid* cloud; or if substantives be necessary, *cumuloidus* and *stratoidus*.

Every meteorologist must have felt the insufficiency of Howard's terms alone; and until an additional and numerous set of terms be devised to denote definite atmospheric phænomena, it will always be impossible, as at present, to record the state of the sky or the appearance of any remarkable cloud without a long wordy description. Considering that clouds are the only indications of those lofty and otherwise invisible currents and other atmospheric changes, producing ultimately those results which we take so much trouble to record on the surface of the earth, it is impossible to understand why their study has been so much neglected.

Sydney, New South Wales,
January 22, 1857.

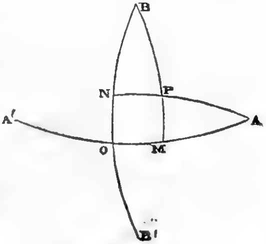
III. *On Spherical Geometry.*

By William Lupton, *Esq., M.A.*[*]

THE coordinate principle, which has been so successfully employed in investigating the properties of surfaces and plane curves, may be used with advantage in discussing the properties of curves traced on the surface of a sphere.

The system of coordinates which I propose to employ, has already been suggested by Professor Graves of Trinity College, Dublin, who, in the appendix to his translation of Chasles 'On Cones,' has shown how some of the fundamental relations may be deduced from principles of projection, as well as from the ordinary rules of trigonometry. There is, however, a peculiarity of the system of coordinates which he has adopted that seems to have escaped his notice, but which appears to be of considerable importance, as it enables us to deduce the spherical equations of curves from their common tri-coordinate definitions, and conversely from the properties of spherical curves, to derive the corresponding properties of the surfaces by whose intersection with the sphere they are formed.

1. Let A A′ and B B′ be two great circles cutting each other in point O. These great circles are the axes of coordinates, and their point of intersection is the origin. Now if we set off on the axes of coordinates $OA = OA' = 90°$ and $OB = OB' = 90°$, the position of any point P may be determined by drawing through that point the axes A P N and B P N, and taking the trigonometric tangents of the axes O M and O N as the coordinates of the point P. These coordinates may be expressed by the letters x and y.



2. If a great circle be drawn through the points A and B, it is clear that the coordinates of any point on it will be

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[*] Communicated by the Author.

$x=y=\tan 90°=\infty$. Hence the great circle AB may be said to correspond with what in plane coordinate geometry is designated the *line at infinity*. It is also evident that this circle divides the whole spherical surface into two hemispheres, in each of which there is a distinct origin. We shall, however, at present confine our attention to the consideration of one hemisphere.

3. We shall denote by X, Y, Z the ordinary rectilinear coordinates of any point on the surface of a sphere whose spherical coordinates are x, y. Now if the axes of spherical coordinates are inclined at an angle ω, it is evident that the tri-coordinate planes are such that the axes of X and Z are inclined at an angle ω, and are each perpendicular to the axis of Y. Hence the equation of the sphere, whose centre is at the origin of tri-coordinates, is

$$X^2+Y^2+Z^2+2XZ\cos\omega=R^2.$$

And if

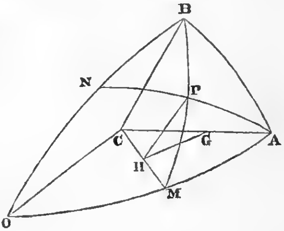
$$Z=PH,\quad Y=GH,\quad X=CG,$$

we have

$$Z=R\,.\,\frac{\sin PM}{\sin BM},$$

$$Y=R\,.\,\cos OM\,.\,\frac{\cos PM}{\sin BM},$$

$$\frac{X}{Y}=\tan OM=x.$$



Hence we have

$$\frac{Z}{Y}=\frac{\tan PM}{\cos OM}.$$

But

$$\frac{\sin PM}{\cos OM}=\frac{\sin PM}{\sin AM}=\frac{\sin NAO}{\sin MPA},$$

and

$$\frac{\sin MPA}{\sin NAO}=\frac{\cos NO}{\cos PM}$$

therefore

$$\tan PM\,.\,\frac{\cos NO}{\cos MO}=\frac{\sin NAO}{\sin ONA},$$

or

$$\frac{\tan PM}{\cos MO}=\frac{\sin NAO}{\cos ON}\cdot\frac{1}{\sin ONA}=\tan ON=y.$$

We have, therefore, when the axes of spherical coordinates are oblique and inclined at an angle ω, the following equations:—

$$\left.\begin{aligned}
&\frac{X}{Y}=x,\\
&\frac{Z}{Y}=y,\\
&X=\pm\frac{R\,.\,x}{(1+x^2+y^2+2xy\cos\omega)^{\frac{1}{2}}},\\
&Y=\pm\frac{R}{(1+x^2+y^2+2xy\cos\omega)^{\frac{1}{2}}},\\
&Z=\pm\frac{Ry}{(1+x^2+y^2+2xy\cos\omega)^{\frac{1}{2}}},
\end{aligned}\right\}\quad .\ .\ .\ .\quad (A)$$

4. When the axes of spherical coordinates are rectangular, the equations (A) become

$$\left.\begin{aligned}
&\frac{X}{Y}=x,\\
&\frac{Z}{Y}=y,\\
&X=\pm\frac{Rx}{(1+x^2+y^2)^{\frac{1}{2}}},\\
&Y=\pm\frac{R}{(1+x^2+y^2)^{\frac{1}{2}}},\\
&Z=\pm\frac{Ry}{(1+x^2+y^2)^{\frac{1}{2}}}.
\end{aligned}\right\}\quad .\ .\ .\ .\ .\ .\quad (B)$$

5. Whenever, therefore, it is required to find the spherical equation of the curve formed by the intersection of any given surface with the sphere, it is sufficient to substitute for X, Y, and Z in the equation of the surface their values as given above, and the resulting equation in x and y will be the spherical equation of the curve.

Hence "a spherical equation of the nth degree represents a curve formed by the intersection of the sphere with a cone of the nth degree whose vertex is at the centre of the sphere."

Similarly, it may be seen that if n be an even number, "a spherical equation of the nth degree represents the curve formed by the intersection of the sphere with a central surface of the nth degree."

As a particular case of the above theorems, we learn that "a spherical curve of the second degree is formed by the intersection of the sphere with a cone or central surface of the second degree, the vertex of the cone or centre of the surface being at the centre of the sphere."

6. We shall now apply the principle contained in arts. 3 and 4 to deducing the spherical equation of the great circle, and to the establishment of certain formulæ which will be found useful in investigating the properties of spherical curves. In what follows, the axes of spherical coordinates are supposed to be rectangular, and the radius of the sphere equal to unity.

A great circle is formed by the intersection of a sphere with a plane passing through its centre. Its equation is therefore found from

$$\mathrm{L}X+\mathrm{M}Y+\mathrm{N}Z=0\ldots 1,$$

by substituting for X, Y, Z, their values as given in (B), to be

$$\mathrm{L}x+\mathrm{N}y+\mathrm{M}=0,$$

where L, M and N are the cosines of the angles between the normal to the plane whose equation is (1), and the axes of X, Y, Z respectively. Hence, if P be the pole of the great circle, and CA, CO and CB be the axes of rectilinear coordinates, we have

$$\mathrm{L}=\cos \mathrm{PB}, \quad \mathrm{M}=\cos \mathrm{PO}, \quad \mathrm{N}=\cos \mathrm{PA}.$$

But

$$\cos \mathrm{PO}=\cos \mathrm{PM}\,.\cos \mathrm{OM}$$
$$=\cos \mathrm{PN}\,.\cos \mathrm{ON},$$

and

$$\tan \mathrm{PM}=\tan \mathrm{NO}\,.\cos \mathrm{MO}$$
$$\tan \mathrm{PN}=\tan \mathrm{MO}\,.\cos \mathrm{NO};$$

therefore

$$\mathrm{M}=\cos \mathrm{PO}=\cos \mathrm{PM}\,.\cos \mathrm{OM}$$
$$=\cos \mathrm{PM}\,.\frac{\tan \mathrm{PM}}{\tan \mathrm{NO}}=\frac{\sin \mathrm{PM}}{\tan \mathrm{MO}}$$
$$=\frac{\cos \mathrm{PB}}{\tan \mathrm{MO}}=\frac{\mathrm{L}}{\tan \mathrm{MO}}.$$

Similarly,

$$\mathrm{M}=\frac{\mathrm{N}}{\tan \mathrm{NO}}.$$

We also have

$$\mathrm{L}^2+\mathrm{M}^2+\mathrm{N}^2=1.$$

Hence if *a* and *b* be the coordinates of P, the pole of the great circle, we have

$$\left.\begin{aligned} &\frac{\mathrm{L}}{\mathrm{M}}=a,\\ &\frac{\mathrm{N}}{\mathrm{M}}=b,\\ &\mathrm{L}=\pm\frac{a}{(1+a^2+b^2)^{\frac{1}{2}}},\\ &\mathrm{M}=\pm\frac{1}{(1+a^2+b^2)^{\frac{1}{2}}},\\ &\mathrm{N}=\pm\frac{b}{(1+a^2+b^2)^{\frac{1}{2}}} \end{aligned}\right\} \quad \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot \quad (\mathrm{C})$$

and the equation of a great circle is

$$ax+by+1=0.$$

7. The angle between two great circles is the angle between the planes by whose intersection with the sphere they are formed. Hence if the equations of the planes be

$$\begin{aligned} \mathrm{LX}+\mathrm{MY}+\mathrm{NZ}&=0,\\ \mathrm{L'X}+\mathrm{M'Y}+\mathrm{N'Z}&=0, \end{aligned}$$

and the equations of the great circle be

$$\begin{aligned} ax+by+1&=0,\\ a'x+b'y+1&=0, \end{aligned}$$

we have

$$\cos\theta=\mathrm{LL'}+\mathrm{MM'}+\mathrm{NN'}=\frac{1+aa+bb'}{(1+a^2+b^2)^{\frac{1}{2}}(1+aa'+bb')^{\frac{1}{2}}}.$$

8. The angle between any two great circles is the distance between their poles. Hence if d be the distance between two points $x'y'$ and $x''y''$

$$\cos d=\frac{1+x'x''+y'y''}{(1+x'^2+y'^2)^{\frac{1}{2}}(1+x''^2+y''^2)^{\frac{1}{2}}}.$$

9. The length of the perpendicular from a given point on a given great circle is the complement of the distance between the given point and the pole of the great circle. Hence if p be the perpendicular from the point x', y', on the great circle whose equation is $ax+by+1=0$, we have

$$\sin p=\frac{1+ax'+by'}{(1+a^2+b^2)^{\frac{1}{2}}(1+a'^2+b'^2)^{\frac{1}{2}}}.$$

In a future paper I shall investigate general expressions for the transformation of spherical coordinates, and I shall proceed to the discussion of properties of spherical curves of the second degree.

ueen's College, Galway,
May 16, 1857.

IV. *Account of Experiments on the Perception of Colour.* By J. C. Maxwell, *B.A., Professor of Natural Philosophy, Marischal College, Aberdeen.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE experiments which I intend to describe were undertaken in order to render more perfect the quantitative proof of the theory of three primary colours. According to that theory, every sensation of colour in a perfect human eye is distinguished by three, and only three, elementary qualities, so that in mathematical language the quality of a colour may be expressed as a function of three independent variables. There is very little evidence at present for deciding the precise tints of the true primaries. I have ascertained that a certain red is the sensation wanting in colour-blind eyes, but the mathematical theory relates to the number, not to the nature of the primaries. If, with Sir David Brewster, we assume red, blue, and yellow to be the primary colours, this amounts to saying that every conceivable tint may be produced by adding together so much red, so much yellow, and so much blue. This is perhaps the best method of forming a provisional notion of the theory. It is evident that if any colour could be found which could not be accurately defined as so much of each of the three primaries, the theory would fall to the ground. Besides this, the truth of the theory requires that every mathematical consequence of assuming every colour to be the result of mixture of three primaries should also be true.

I have made experiments on upwards of 100 different artificial colours, consisting of the pigments used in the arts, and their mechanical mixtures. These experiments were made primarily to trace the effects of mechanical mixture on various coloured powders; but they also afford evidence of the truth of the theory, that all these various colours can be referred to three primaries. The following experiments relate to the combinations of six well-defined colours only, and I shall describe them the more minutely, as I hope to induce those who have good eyes to subject them to the same trial of skill in distinguishing tints.

The method of performing the experiments is described in the Transactions of the Royal Society of Edinburgh, vol. xxi. part 2. The colour-top or teetotum which I used may be had of Mr. J. M. Bryson, Edinburgh, or it may be easily extemporized. Any rotatory apparatus which will keep a disc revolving steadily and rapidly in a good light, without noise or disturbance, and can be easily stopped and shifted, will do as well as the contrivance of the spinning-top.

The essential part of the experiment consists in placing several discs of coloured paper of the same size, and slit along a radius, over one another, so that a portion of each is seen, the rest being covered by the other discs. By sliding the discs over each other the proportion of each colour may be varied, and by means of divisions on a circle on which the discs lie, the proportion of each colour may be read off. My circle was divided into 100 parts.

On the top of this set of discs is placed a smaller set of concentric discs, so that when the whole is in motion round the centre, the colour resulting from the mixture of colours of the small discs is seen in the middle of that arising from the larger discs. It is the object of the experimenter to shift the colours till the outer and inner tints appear exactly the same, and then to read off the proportions.

It is easy to deduce from the theory of three primary colours what must be the number of discs exposed at one time, and how much of each colour must appear.

Every colour placed on either circle consists of a certain proportion of each of the primaries, and in order that the outer and inner circles may have precisely the same resultant colour in every respect, there must be the same amount of each of the primary colours in the outer and inner circles. Thus we have as many conditions to fulfil as there are primary colours; and besides these we have two more, because the whole number of divisions in either the outer or the inner circle is 100, so that if there are three primary colours there will be five conditions to fulfil, and this will require five discs to be disposable, and these must be arranged so that three are matched against two, or four against one.

If we take six different colours, we may leave out any one of the six, and so form six different combinations of five colours. It is plain that these six combinations must be equivalent to two equations only, if the theory of three primaries be true.

The method which I have found most convenient for registering the result of an experiment, after an identity of tint has been obtained in the inner and outer circles, is the following:—

Write down the names or symbols of the coloured discs each at the top of a column, and underneath write the number of degrees of that colour observed, calling it + when the colour is in the outer circle, and — when it is in the inner circle; then equate the whole to zero. In this way the account of each colour is kept in a separate column, and the equations obtained are easily combined and reduced, without danger of confounding the colours of which the quantities have been measured. The following experiments were made between the 3rd and 11th of September, 1856, about noon of each day, in a room fronting the north, without curtains or any bright-coloured object near the window. The same combination was never made twice in one day, and no thought was bestowed upon the experiments except at the time of observation. Of course the graduation was never consulted, nor former experiments referred to, till each combination of colours had been fixed by the eye alone; and no reduction was attempted till all the experiments were concluded.

The coloured discs were cut from paper painted of the following colours:—Vermilion, Ultramarine, Emerald-green, Snow-white, Ivory-black, and Pale Chrome-yellow. They are denoted by the letters V, U, G, W, B, Y respectively. These colours were chosen, because each is well distinguished from the rest, so that a small change of its intensity in any combination can be observed. Two discs of each colour were prepared, so that in each combination the colours might occasionally be transposed from the outer circle to the inner.

The first equation was formed by leaving out vermilion. The remaining colours are Ultramarine-blue, Emerald-green, White, Black, and Yellow. We might suppose, that by mixing the blue and yellow in proper proportions, we should get a green of the same hue as the emerald-green, but not so intense, so that in order to match it we should have to mix the green with white to dilute it, and with black to make it darker. But it is not in this way that we have to arrange the colours, for our blue and yellow produce a pinkish tint, and never a green, so that we must add *green* to the combination of blue and yellow, to produce a neutral tint, identical with a mixture of white and black.

Blue, green, and yellow must therefore be combined on the large discs, and stand on one side of the equation, and black and white, on the small discs, must stand on the other side. In order to facilitate calculations, the colours are always put down in the same order; but those belonging to the small discs are marked negative. Thus, instead of writing

$$54\mathrm{U}+14\mathrm{G}+32\mathrm{Y}=32\mathrm{W}+68\mathrm{B},$$

we write

$$+54\mathrm{U}+14\mathrm{G}-32\mathrm{W}-68\mathrm{B}+32\mathrm{Y}=0.$$

The sum of all the positive terms of such an equation is 100, being the whole number of divisions in the circle. The sum of the negative terms is also 100.

The second equation consists of all the colours except blue; and in this way we obtain six different combinations of five colours.

Each of these combinations was formed by the unassisted judgment of my eye, on six different occasions, so that there are thirty-six independent observations of equations between five colours.

Table I. gives the actual observations, with their dates.

Table II. gives the result of summing together each group of six equations.

Each equation in Table II. has the sums of its positive and negative coefficients each equal to 600.

Having obtained a number of observations of each combination of colours, we have next to test the consistency of these results, since theoretically two equations are sufficient to determine all the relations among six colours. We must therefore, in the first place, determine the comparative accuracy of the different sets of observations. Table III. gives the averages of the errors of each of the six groups of observations. It appears that the combination IV. is the least accurately observed, and that VI. is the best.

Table IV. gives the averages of the errors in the observation of each colour in the whole series of experiments. This Table was computed in order to detect any tendency to colour-blindness in my own eyes, which might be less accurate in discriminating red and green, than in detecting variations of other colours. It appears, however, that my observations of red and green were more accurate than those of blue or yellow. White is the most easily observed, from the brilliancy of the colour, and black is liable to the greatest mistakes. I would recommend this method of examining a series of experiments as a means of detecting partial colour-blindness, by the different accuracy in observing different colours. The next operation is to combine all the equations according to their values. Each was first multiplied by a coefficient proportional to its accuracy, and to the coefficient of white in that equation. The result of adding all the equations so found is given in equation (W).

Equation (Y) is the result of similar operations with reference to the yellow on each equation.

We have now two equations from which to deduce six new equations, by eliminating each of the six colours in succession. We must first combine the equations, so as to get rid of one of the colours, and then we must divide by the sum of the positive or negative coefficients, so as to reduce the equations to the form of the observed equations. The results of these operations are given in Table V., along with the means of each group of six observations. It will be seen that the differences between the results of calculation from two equations and the six independent observed equations are very small. The errors in red and green are here again somewhat less than in blue and yellow, so that there is certainly no tendency to mistake red and green more than other colours. The average difference between the observed mean value of a colour and the calculated value is ·77 of a degree. The average error of an observation in any group from the mean of that group was ·92. No observation was attempted to be registered nearer than one degree of the top, or $\frac{1}{100}$ of a circle; so that this set of observations agrees with the theory of three primary colours quite as far as the observations can warrant us in our calculations; and I think that the human eye has seldom been subjected to so severe a test of its power of distinguishing colours. My eyes are by no means so accurate in this respect as many eyes I have examined, but a little practice produces great improvement even in inaccurate observers.

I have laid down, according to Newton's method, the relative positions of the five positive colours with which I worked. It will be seen that W lies within the triangle V U G, and Y outside that triangle.

The first combination, equation I., consisted of blue, yellow and green, taken in such proportions that their centre of gravity falls at W.

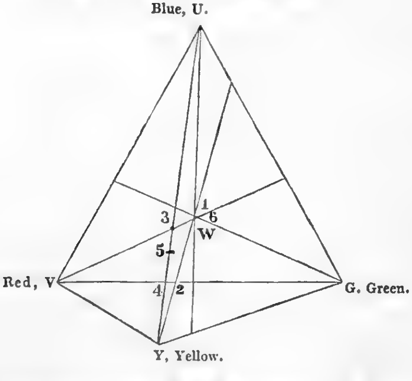
In equation II. a mixture of red and green, represented in the diagram by the point 2, is seen to be equivalent to a mixture of white and yellow, also represented by 2, which is a pale yellow tint.

Equation III. is between a mixture of blue and yellow and another of white and red. The resulting tint is at the intersection of Y U and W V; that is, at the point 3, which represents a pale pink grey.

Equation IV. is between V G and U Y, that is, at 4, a dirty yellow.

Equation V. is between a mixture of white, red, and green, and a mixture of blue and yellow at the point 5, a pale dirty yellow.

Equation VI. has W for its resulting tint.



Of all the resulting tints, that of equation IV. is the furthest from white; and we find that the observations of this equation are affected with the greatest errors. Hence the importance of reducing the resultant tint to as nearly a neutral colour as possible.

It is hardly necessary for me to observe, that the whole of the numerical results which I have given apply only to the coloured papers which I used, and to them only when illuminated by daylight from the north at mid-day in September, latitude 55°. In the evening, or in winter, or by candlelight, the results are very different. I believe, however, that the results would differ far less if observed by different persons, than if observed under different lights; for the apparatus of vision is wonderfully similar in different eyes, and even in colour-blind eyes the system of perception is not different, but defective.

TABLE I.—The observations arranged in groups.

Equation I.	V=0.	+U.	+G.	−W.	−B.	+Y.
1856, Sept. 3.	0	54	12	34	66	34
4.	0	58	14	31	69	28
5.	0	55	12	32	68	33
6.	0	54	14	32	68	32
8.	0	54	14	32	68	32
9.	0	53	15	32	68	32

Equation II.	−V.	U=0.	−G.	+W.	+B.	+Y.
Sept. 3.	59	0	41	9	71	20
4.	61	0	39	9	68	23
5.	61	0	39	9	67	24
6.	59	0	41	10	66	24
8.	60	0	40	9	69	22
9.	61	0	39	9	68	23

Equation III.	+V.	−U.	G=0.	+W.	+B.	−Y.
Sept. 3.	20	56	0	28	52	44
4.	23	58	0	30	47	42
5.	24	56	0	29	47	44
6.	20	56	0	31	49	44
8.	21	57	0	29	50	43
9.	21	58	0	29	50	42

Equation IV.	−V.	+U.	−G.	W=0.	+B.	+Y.
Sept. 3.	62	15	38	0	53	32
4.	63	17	37	0	46	37
5.	64	16	36	0	50	34
6.	62	19	38	0	46	35
8.	62	19	38	0	47	34
9.	63	17	37	0	49	34

Table I. (*continued*).

Equation V.	+V.	−U.	+G.	+W.	B=0.	−Y.
1856, Sept. 3.	56	47	28	16	0	53
4.	57	50	25	18	0	50
5.	56	49	24	20	0	51
6.	55	47	27	18	0	53
8.	54	49	26	20	0	51
11.	56	50	27	17	0	50

Equation VI.	+V.	+U.	+G.	−W.	−B.	Y=0.
Sept. 3.	38	27	35	24	76	0
4.	39	27	34	24	76	0
5.	40	26	34	24	76	0
6.	38	28	34	24	76	0
8.	39	28	33	24	76	0
11.	39	27	34	23	77	0

TABLE II.—The sums of the observed equations.

	V.	U.	G.	W.	B.	Y.
Equation I.	0	+328	+ 81	−193	−407	+191
... II.	−361	0	−239	+ 55	+409	+136
... III.	+129	−341	0	+176	+295	−259
... IV.	−376	+103	−224	0	+291	+206
... V.	+334	−292	+157	+109	0	−308
... VI.	+233	+163	+204	−143	−457	0

TABLE III.—The averages of the errors of the several equations from the means expressed in $\frac{1}{100}$ parts of a circle.

Equations.	I.	II.	III.	IV.	V.	VI.
Errors.	·94	·85	1·05	1·17	1·08	·40

TABLE IV.—The averages of the errors of the several colours from the means in $\frac{1}{100}$ parts of a circle.

Colours.	V.	U.	G.	W.	B.	Y.
Errors.	·83	·99	·80	·61	1·15	1·09

Average error on the whole ·92.

The equations from which the reduced results were obtained were calculated as follow :—

Equation for $(W)=(II)+2(III)+(V)-2(I)-4(VI)$.

Equation for $(Y)=2(I)+2(II)-3(III)+2(IV)-3(V)$.

These operations being performed, gave

	V.	U.	G.	W.	B.	Y.	
(W)	+ 701	+2282	+1060	−1474	−3641	+1072	=0.
(Y)	+2863	−2761	+1235	+1131	+ 299	−2767	=0.

From these were obtained the following results by elimination:—

Table V.

Equation		V.	U.	G.	W.	B.	Y.
I.	From (W) and (Y)	0	−54·1	−13·9	+32·0	+68·0	−32·0
	From observation	0	−54·7	−13·5	+32·1	+67·9	−31·8
II.	From (W) and (Y)	−59·6	0	−40·4	+10·4	+66·0	+23·6
	From observation	−60·2	0	−39·8	+ 9·2	+68·2	+22·6
III.	From (W) and (Y)	−21·7	+57·4	0	−30·2	−48·1	+42·6
	From observation	−21·5	+56·8	0	−29·3	−49·2	+43·2
IV.	From (W) and (Y)	−62·4	+18·6	−37·6	0	+45·7	+35·7
	From observation	−62·7	+17·2	−37·3	0	+48·5	+34·3
V.	From (W) and (Y)	+55·6	−49·0	+25·2	+19·2	0	−51·0
	From observation	+55·7	−48·7	+26·1	+18·2	0	−51·3
VI.	From (W) and (Y)	−39·7	−26·6	−33·7	+22·7	+77·3	0
	From observation	−38·8	−27·2	−34·0	+28·3	+76·2	0

James Clerk Maxwell.

Glenlair, June 13, 1857.

V. *Notes on Mineralogy.*—No. VI. *On the Siliceo-Felspathic Rocks of the South of Ireland. By the* Rev. Samuel Haughton, *M.A., Fellow of Trinity College, and Professor of Geology in the University of Dublin.*

It is well known that siliceo-felspathic trap rocks of a peculiar kind are found in the mining district of the Ovoca, Co. Wicklow, and in the mining district of Bonmahon, in the south of the county of Waterford; and the recent investigations of the Geological Survey in the west of Kerry and Cork have brought to light the existence of great quantities of similar rocks in the neighbourhood of Killarney, and in the mountains to the westward stretching to the south of Kenmare Bay; and it is not at all improbable but that these remarkable felspathic rocks may in this district be associated with the copper lodes, which have proved so productive in the Berehaven or Allihies Mine. These rocks have a general resemblance to each other in all these districts, and when once seen and recognized cannot be easily

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* Communicated by the Author.

mistaken for any other description of rock; they are of a pale bluish, or greenish-grey colour, weathering white to the depth of several inches, slightly translucent on the edges, of conchoidal fracture, and sharp metallic ring under the hammer.

The Cornish miners who are acquainted with the mining districts of Wicklow and Waterford, consider these rocks as the equivalents of their own Elvans, to which they bear no external resemblance, though it cannot be denied that they appear to exert an equally favourable influence on the productiveness of the metallic lodes with which they are associated; and the results of my analyses prove that they have an intimate relation to the granitic rocks in their chemical and mineralogical composition. The resemblance in composition to some varieties of granite is, indeed, so striking, that it requires but a slight effort of the imagination to conceive them as granites cooled under peculiar circumstances which prevented the development of the usual crystalline structure.

In some cases, however, these siliceo-felspathic rocks appear to be deposited in stratified beds, conformable to the slates and felspathic ash-beds with which they are found associated. This is particularly the case in the Ovoca district, where the mass of felspathic rock is found to lie between dark soft slates of the Silurian age, and has never been observed to penetrate these slates in dykes.

I shall now proceed to the discussion of the analyses of these rocks from the Wicklow, Waterford, and Killarney districts respectively.

1. *Siliceo-felspathic Rocks of the Vale of Ovoca, Co. Wicklow.*

The cupriferous and pyritous lodes of this district have a N.E. and S.W. bearing, and an underlay to the S.E. They appear to be nearly conformable to the planes of bedding of the slate in which they occur; and they are overlaid to the S.E. by a thick mass of siliceo-felspathic rock, which rises into the remarkable hill called the Bell Rock, on the west side of the Ovoca. The lodes are all dislocated by a left-handed heave coinciding apparently with the direction of the Ovoca Valley, and the felspathic rock partakes of this movement of the lodes. It has a stratified character throughout, and in places, as near the Tigroney Mine, it assumes completely the character of an ashey-slate, weathering perfectly white.

I obtained specimens of the Bell Rock from Mr. Edward Barnes, Resident Director of the Wicklow Copper-Mine Company, which were procured by blasting two or three feet into the rock, so as to obtain a portion quite free from the action of the weather. The specimens are of a pale greenish colour, exceedingly hard, striking fire freely under the hammer, but when subjected to long-continued action of the reducing flame of the blowpipe, melting slightly on the edges, particularly in the neighbourhood of the minute specks of silicate of iron which appear here and there through the body of the rock.

The following analysis will serve to give an exact idea of the composition of this rock, which is more siliceous than the felstones of Waterford and Killarney:—

	Per cent.	Atoms.	
Silica	81·36	...	1·808
Alumina	7·86	0·151	0·192
Peroxide of iron .	3·32	0·041	
Lime	0·99	0·035	0·206
Magnesia	0·45	0·022	
Potash	3·09	0·065	
Soda	2·63	0·084	
	99·70		

It is evident from this analysis, that the atoms of protoxides and peroxides are about equal in quantity, and that the rock may be represented by a mixture of felspar and quartz.

$$Q+F=1{\cdot}808,$$
$$F=0{\cdot}199.$$

From these equations, we find that its mineralogical composition is as follows:—

	Per cent.
Quartz	45·54
Orthoclase felspar	54·16
	99·70

2. *Siliceo-felspathic Rocks of Knockmahon, Co. Waterford.*

The felspathic rocks of Knockmahon are intimately associated with the copper lodes which have rendered that locality famous, and occur abundantly on the shore below the village of Bonmahon in prismatic masses of a columnar structure, which have received the name of the Bishop's Library. These rocks occur also, and are well shown, in a cutting for a tramroad connecting Tankardstown with Knockmahon Mine. In this latter locality they occur stratified conformably with the brown fossiliferous Silurian slates which are found at the Tankardstown Mine. The following analysis is of a specimen taken from the stratified siliceo-felspathic rocks of the cutting of the tramroad:—

	Per cent.		Atoms.
Silica	77·20	...	1·715
Alumina . . .	6·54	0·126	0·199
Peroxide of iron .	5·82	0·073	
Lime (carbonate).	1·81		
Magnesia . . .	0·60	0·030	0·206
Potash	3·69	0·078	
Soda	3·03	0·098	
Water	1·12		
	99·81		

This rock, like that at Ballymurtagh, Co. Wicklow, is a compound of quartz and felspar; if it be regarded as a sedimentary rock, it must be considered as a trappean ash, composed of felspar and fine quartzose mud, with a slight admixture of carbonate of lime. Its mineralogical composition is as follows:—

	Per cent.
Quartz	40·81
Orthoclase felspar . . .	57·19
Carbonate of lime . . .	1·81
	99·81

3. *Siliceo-felspathic Traps of Benaunmore, Co. Kerry.*

The hornstones or siliceo-felspathic rocks of Benaunmore occur in splendid columns, many of which, as described by Mr. Foot, of the Geological Survey, are 200 feet in length. The rock is more translucent than the felspathic traps of Wicklow or Waterford already described, and presents more of the character of a truly igneous product. Its analysis gave the following results:—

	Per cent.		Atoms.
Silica	71·52	...	1·554
Alumina . . .	12·24	0·238	0·277
Peroxide of iron .	3·16	0·039	
Lime	0·84	0·030	0·278
Magnesia . . .	0·39	0·019	
Potash	5·65	0·120	
Soda	3·36	0·109	
Loss by ignition.	1·20		
	98·36		

This rock, like the others already discussed, is a compound of quartz and felspar; and it is easy to calculate the proportions of the two minerals as follows:—

	Per cent.
Quartz	20·51
Orthoclase felspar . . .	77·85
	98·36

From the fact, that the felspathic trap of Benaunmore occurs in columnar masses, it may be inferred to be probably of igneous origin; it is massive, fine-grained, with rounded specks of quartz (globuliferous), and small occasional facets of felspar; brittle, of conchoidal fracture, somewhat lamellar, and translucent on the edges; with a ringing clink and striking fire freely when struck with the hammer.

I have lately had an opportunity, in conjunction with Dr. Wilde of Dublin, of examining a very large number of stone implements found in various parts of Ireland; and I find that the different varieties of siliceo-felspathic rocks were carefully sought out by the makers of these implements. Among the most common varieties so used, are the pure pale-green felstone, and a mottled porphyritic variety of the same kind of rock, streaked with pink felspar and dark-coloured metallic hornblende.

In the collection of stone implements preserved in the Collection of the Royal Irish Academy, there are also a number of stone implements from Jamaica, formed of the same kind of felstone, which would appear to have been particularly well suited to the purposes to which such implements are supposed to have been applied.

The felstones and siliceo-felspathic rocks of Ireland are only locally abundant, and as the weapons made from this kind of rock are found in all parts of Ireland, it is conjectured by antiquarians that an extensive trade in, and manufacture of, these felstone celts and weapons must have existed in former times in Ireland. This trade, if such existed, must have been confined to Ireland itself, as there is scarcely a single stone implement in the Collection of the Irish Academy which cannot be readily identified as made of an Irish rock; and in many instances the locality from which it was obtained can be assigned with tolerable accuracy.

VI. *Chemical Notices from Foreign Journals.*

By E. Atkinson, *Ph.D.*

[Continued from vol. xiii. p. 506.]

BY the action of hydrate of potash on oxide of cobalt at high temperatures in contact with the air, Schwarzenberg* obtained a brown fused mass, which on treatment with water leaves a quantity of black, lustrous, soft crystals of hexagonal and other rhombic forms. These crystals contain cobalt, oxygen, potash and water, and Schwarzenberg held that they were a compound of a new oxide of cobalt, which he named cobaltic acid, with potash and water in definite quantities; that the potash and

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* Liebig's *Annalen*, vol. xcvii. p. 211.

water mutually replace each other, so that with increase of the potash there is decrease of the water, and *vice versâ*. Schwarzenberg's analyses led to the formula $KO, 3Co^3O^5 + 3HO$. This relation of cobalt to oxygen was so unusual that the cobaltate of potash became the subject of separate investigation by Pebal* and Mayer †, who have, however, arrived at results which confirm in the main Schwarzenberg's statements.

Pebal's experiments were made chiefly with a view of determining the relation of cobalt to oxygen in the acid, and he accomplished this by the application of Bunsen's method of volumetric analysis. Pebal's analyses, which agreed well with each other, gave numbers corresponding to the formula Co^6O^{10}, KO, or $2(Co^3O^5), KO$. The relation of the cobalt to the oxygen is therefore the same as that found by Schwarzenberg; but Pebal does not consider that water is contained as an essential constituent in the substance.

Mayer's object was not solely to determine the relation of cobalt to oxygen, but also to examine whether cobaltic acid might not be a mixture of two oxides of cobalt, the more so as Fremy had stated that an oxide of cobalt, CoO^2, was formed under certain circumstances. Mayer also investigated with great completeness the various methods of forming cobaltate of potash. His experiments confirm essentially Schwarzenberg's statements, and his analyses agree with the formula $3(Co^3O^5)KO, 3HO$. But he does not consider that it can be called cobaltate of potash; it is rather a peroxide, which has the property of combining with bases; on treating it with water, the greater part of the potash is removed, although the relation of cobalt to oxygen remains constant; many instances of this class of compounds are known, which suffer a progressive decomposition with water. Mayer considers that the water is an essential constituent, and he explains the difference in this respect from Pebal's views as arising partly from the ready decomposibility of the potash compound with water, and partly from the production of different compounds, according as circumstances are modified.

Wöhler ‡ describes a new mode of forming suboxide of silver. When yellow arsenite of silver is digested at a moderate temperature with caustic soda, it rapidly becomes black. When this mixture is made to boil, allowed to settle, and the clear liquid poured off, it is found to contain *arseniate* of soda. The black precipitate is again boiled with fresh soda, then well washed out with water and dried. It then forms a heavy black powder, with a shade of grey, and under the burnisher has a metallic appearance. It consists of suboxide of silver containing a little metallic silver.

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* Liebig's *Annalen*, December 1856. † Ibid. March 1857.

‡ Ibid.

The reducing action exercised by arsenious acid on silver and copper in the presence of alkalies, extends also to some nitro-compounds[*]. When nitrobenzole is digested with solution of arsenious acid in excess of strong caustic soda, it is converted into aniline, which can be distilled off and obtained pure. Wöhler intends trying this reducing action on other nitro-compounds.

St.-Clair Deville and Caron[†] have investigated the preparation and properties of magnesium. A mixture is made of chlorides of magnesium and sodium with fluoride of calcium, to this mixture sodium in pieces is added, and the whole mixed well together. This mixture is then introduced into a red-hot earthen crucible, which is closed down. The action commences immediately, and when it is over, the lid is removed, and the mass stirred well with an iron rod until the mixture is quite uniform. It is then allowed to cool, and when about to solidify is poured out on an iron plate; this mass, when cold, is broken up and the globules of magnesium picked out. The rest of the mass can be remelted, and more magnesium obtained. Wöhler[‡] recommends that the mixture be allowed to become cold in the crucible, which is then broken and the globules separated from the adhering scoria by solution in water. They may be obtained much brighter by immersing them in a solution of sal-ammoniac, or of carbonate of soda, but must not be allowed to remain too long, as they dissolve with evolution of hydrogen. They are then washed and carefully dried at a gentle heat. Wöhler succeeded in preparing magnesium by the reduction of the double salt of chloride of magnesium and sodium, obtained by mixing the solutions of the two chlorides in equivalent proportions, evaporating them to dryness, and fusing the mass.

To purify crude magnesium, it is heated in a tube of dense gas coke to an almost white heat, and a slow stream of hydrogen passed through; on cooling, the metal is taken out, and melted under a mixture of chloride of magnesium, chloride of sodium, and fluoride of calcium. By increasing the proportion of the fluoride, the mixture is rendered less fusible than magnesium, so that the latter can be poured off at the moment at which the former solidifies.

Magnesium is volatile like zinc, and at almost the same temperature as that metal. When pure it volatilizes without leaving any residue, and the sublimed metal is white and coated with a small quantity of magnesia. Magnesium has about the same melting-point as zinc; heated somewhat higher it ignites and

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[*] Liebig's *Annalen*, April 1857.

[†] *Comptes Rendus*, February 23, 1857.

[‡] Liebig's *Annalen*, March 1857.

burns with a clear flame, in which blue rays are often perceived. Its specific gravity is 1·75; it can be filed, and takes an excellent polish; it resists the action of the air tolerably well, though not so well as zinc. When distilled in a rapid current of hydrogen, some metallic dust is carried away, which remains suspended in the hydrogen; and if this be set fire to, it burns with one of the most beautiful flames which can be conceived.

Deville and Caron are still engaged in an investigation of the physical properties of magnesium, and experiments are in progress on the reduction of the alkaline earths by means of carbon.

In a note on ozonometry, M. Bineau* seemed to attribute to nitric acid the property of liberating iodine from iodide of potassium in the cold, and in the presence of water. Béchamp† adduces a variety of facts which show that this is not the case, if the nitric acid be pure.

If to a given volume of dilute solution of iodide of potassium, which has been mixed with starch-paste, a drop of nitric acid such as is usually found in laboratories be added, the mixture becomes immediately blue; if to another volume of the iodide solution there be added even a great excess of the same nitric acid diluted, but which has had carbonic acid passed though it in the dark to expel nitrous acid, the mixture does not become blue; the same is the case on adding to the iodide solution pure nitrate of potash and sulphuric acid. But this becomes blue also if the nitrate of potash has been melted, or has been in contact with organic substances, or if traces of nitrite of potash be added. Ozonometric paper, therefore, does not become blue under the influence of pure dilute nitric acid; the coloration must be attributed to the nitrous acid always contained in concentrated nitric acid which has been prepared some time. Millon showed long ago that pure nitric acid, free from nitrous acid, did not liberate iodine from iodides, or sulphur from sulphides; but it has not been stated what is the action of nitrous acid on iodides. Béchamp examined this point, and found that the action of nitrous acid gave rise to the formation of nitric oxide, and was a convenient method of preparing this gas. He mixed an equivalent of nitrite of potash, and an equivalent of iodide of potassium dissolved in water, in an appropriate apparatus, and having expelled all the air by means of carbonic acid, added a few drops of sulphuric acid; a rapid evolution of gas ensued, which reddened in the air, and was completely absorbed by protosulphate of iron. This was binoxide of nitrogen, which could only have arisen from the action of nitrous acid on the

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* Phil. Mag., October 1856. *Comptes Rendus*, July 21, 1856.

† *Comptes Rendus*, Aug. 18, 1856.

iodide; for had it proceeded from the decomposition of the nitrous acid according to the equation

$$2NO^3 = NO^4 + NO^2,$$

there ought to have been red fumes in the apparatus, which was not the case. The decomposition must have taken place according to the equation

$$NO^3, KO + KI + 2SO^4\,H = 2SO^4\,K + I + NO^2 + 2HO,$$

and the volume of binoxide obtained was almost equal to that required by theory: 1300 cubic centimeters of gas were obtained from 10 grammes of iodide and 5 grammes of nitrite of potash. Binoxide of nitrogen is without action upon iodide of potassium if air be excluded.

Strecker has investigated methionic acid, which was obtained by Liebig in small quantities as a product of the action of anhydrous sulphuric acid on æther. He formed it in somewhat larger quantities by the following process. A flask containing anhydrous sulphuric acid was placed over a cylinder containing some æther, so that the vapours of the two substances came in contact with each other. When all the sulphuric acid disappeared, the liquid in the cylinder was shaken with water, whereupon a layer of etherial solution of sulphate of æthyle formed upon the surface, which was removed by means of a tap funnel. The aqueous solution, which contains the methionic acid, was boiled for a long time, then saturated with carbonate of baryta, and filtered off; the filtrate, on cooling, deposited the methionate of baryta in thin rhombic laminæ, which show the colours of thin plates in a high degree. The crystallized salt has the formula

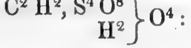
$$2BaO, C^2\,H^2\,S^4\,O^{10} + 4HO,$$

which agrees essentially with the formula of Redtenbacher and Liebig. *Methionate of lead* is obtained from methionate of baryta by precipitating the baryta from that salt, boiling the filtered solution with carbonate of lead, filtering and evaporating to crystallization. It crystallizes from a concentrated solution in large transparent rhombic prisms, which are probably isomorphous with the baryta salt. The lead salt has the formula

$$2PbO, C^2\,H^2\,S^4\,O^{10} + 4HO.$$

Methionate of copper crystallizes in blue rhombic columns, which effloresce in the air, and become whitish.

Methionic acid obtained from the lead salt by treatment with sulphuretted hydrogen, crystallizes from a concentrated solution over sulphuric acid in long crystalline needles. The acid is very stable, and can be boiled with dilute nitric acid without decomposition. The composition of the acid might be expressed by the formula,



$$\left.\begin{matrix} C^2\,H^2,\,S^4\,O^8 \\ H^2 \end{matrix}\right\}O^4 :$$

it would thus contain $2(S^2 O^4)$ in copulated combination with the radical $C^2 H^2$, which can replace two equivalents of hydrogen. The formation of methionic acid might be explained as arising from the oxidation of a part of the carbon. In fact, Strecker observed the occurrence of carbonic acid as well as an abundant evolution of sulphuric acid.

Some time after the preceding investigation was completed, Strecker recognized methionic in disulphometholic acid, the acid obtained by Buckton and Hofmann from the action of sulphuric acid on acetonitrile. And by preparing some of the acid according to the methods described by these chemists, he was able to establish its identity with methionic acid, not only by ocular demonstration, but by analysis.

Reinhold Hoffmann* has examined the monochloracetic acid, the existence of which, although pointed out by Dumas, and afterwards by Leblanc, has never been satisfactorily established. It is obtained by bringing chlorine into contact with monohydrated acetic acid; its formation begins probably even in the dark, but takes place best in sunlight, if the chlorine be not in too great excess. It is always accompanied by the formation of a small quantity of a higher chlorinated acetic acid, and it is probable that the formation of monochloracetic acid is the first step, and that from this the higher chlorinated acid is formed.

Monochloracetic acid, $C^4 H^3 Cl O^2$, is a well-characterized acid, and forms crystalline salts. It is, when pure, a crystalline solid at a temperature of 62°, and boils at 183°. In its physical properties it is much nearer trichloracetic acid than acetic acid. Its relation to alkalies exhibits considerable differences from its type; just as acetic acid when treated with alkalies yields marsh-gas,

$$\underset{\text{Acetic acid.}}{C^4 H^4 O^4} + \underset{\text{Potash.}}{2KO, HO} = \underset{\text{Marsh-gas.}}{C^2 H^4} + \underset{\substack{\text{Carbonate}\\\text{of potash.}}}{2KO\,CO^2} + 2HO;$$

and trichloracetic acid gives chloroform,

$$\underset{\substack{\text{Trichloracetic}\\\text{acid.}}}{C^4 H Cl^3 O^4} + 2KO\,HO = \underset{\text{Chloroform.}}{C^2 H Cl^3} + 2KO\,CO^2 + 2HO;$$

monochloracetic acid ought to give chloride of methyle,

$$\underset{\substack{\text{Monochloracetic}\\\text{acid.}}}{C^4 H^3 Cl O^4} + 2KO\,HO = \underset{\substack{\text{Chloride}\\\text{of methyle.}}}{C^2 H^3 Cl} + 2KO\,CO^2 + 2HO;$$

but chloride of methyle is not formed at all in this reaction; indeed the decomposition seems to be quite different, and is reserved by Hoffmann for further investigation.

Monochloracetic acid is reduced by the action of potassium

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* Liebig's *Annalen*, April 1857.

amalgam, just as Kolbe and Melsens found to be the case with trichloracetic acid. With a view of obtaining a corresponding bromine compound, Hoffmann sealed up bromine with acetic acid in a glass tube; but although they were exposed at a temperature of 100° C. to the strongest summer sunlight, no action was observed.

Chloral, C^4HCl^3, O^2, is generally considered to be a substitution product of aldehyde, C^4H^4, O^2, in which three equivalents of hydrogen are replaced by three of chlorine. Wurtz* wished to ascertain if chloral were really formed by the direct action of chlorine upon aldehyde, and exposed anhydrous aldehyde in excess to the action of chlorine. The only substitution product he obtained was chloride of acetyle, $C^4H^3ClO^2$, while at the same time a quantity of aldehyde was changed into metaldehyde, $C^8H^8O^4$, the modification into which it passes on being long kept, and some of this had been changed to a substance, $C^8H^7ClO^4$, by the action of chlorine.

He further tried whether chloride of acetyle gives chloral by the continued action of chlorine, and with this view placed chloride of acetyle, $C^4H^3O^2Cl$, in large globes filled with chlorine, and exposed them to the action of sunlight. A liquid was obtained, which gave on fractional distillation a substance boiling between 100° and 105° C., which was monochlorinated chloride of acetyle, $C^4H^2Cl^2O^2$. It is a colourless liquid, having an extremely irritating odour. The action of water on this substance gives rise to the formation of monochloracetic acid; thus

$$\underset{\text{Monochlorinated chloride of acetyle.}}{C^4H^2Cl^2O^2} + 2HO = HCl + \underset{\text{Monochloracetic acid.}}{C^4H^3ClO^4}$$

The reactions of ammonia and of alcohol upon monochlorinated chloride of acetyle, have been examined by Willm. On mixing alcohol with this substance a very violent action ensues, and the vessel in which it takes place requires to be cooled down. On washing the product with water, and rectification, a colourless liquid of etherial odour, and boiling at 143°·5, is obtained. This is monochloracetate of æthyle, and its formation is thus expressed:—

$$\underset{\text{Alcohol.}}{C^4H^6O^2} + \underset{\text{Monochlorinated chloride of acetyle.}}{C^4H^2Cl^2O^2, Cl} = HCl + \underset{\text{Monochloracetate of æthyle.}}{C^4H^5O, C^4H^2ClO^3}.$$

It is therefore acetic acid in which one equivalent of hydrogen in the acid is replaced by chlorine, for by acting upon it with caustic potash it is changed into monochloracetate of potash and alcohol.

Monochloracetamide, $C^4H^2ClO^2, NH^2$, is obtained by the action

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* *Annales de Chimie et de Physique*, January 1857. Liebig's *Annalen*, April 1857.

of ammonia on monochloracetic æther, and also by the action of dry ammonia on monochlorinated chloride of acetyle.

Limpricht has recently * given a preliminary notice of a reaction which supplies a link in a remarkable series of transformations, and shows that it is possible to convert certain acids into the corresponding alcohols.

By heating leucine, $C^{12} H^{13} NO^4$, in a retort over free flame, it melts at about 170°, and between 184° and 200° an oily liquid of an empyreumatic odour distils over, which solidifies on cooling into a mass of soft foliaceous crystals. These are soluble in water, and treated with hydrochloric acid liberate carbonic acid. The hydrochloric acid solution was evaporated to dryness, extracted with absolute alcohol, the alcoholic solution shaken with caustic potash, on which an oily liquid separated out, which on rectification over solid potash proved to be amylamine. The preparation of leucine from horn is not difficult, and in this way upwards of an ounce of pure amylamine was obtained. The decomposition may be thus expressed:—

$$\underset{\text{Leucine.}}{C^{12} H^{13} NO^4} = \underset{\text{Amylamine.}}{C^{10} H^{13} N} + \underset{\text{Carbonic acid.}}{2CO^2}$$

In the same way alanine, $C^6 H^7 NO^4$, the homologue of leucine, was found to give ethylamine, and there is little doubt that glycocoll, $C^4 H^5 NO^4$, would give methylamine.

This opens out a way of transforming certain acids into their alcohols, for Piria has shown that by distilling the salts of certain acids with formiates, aldehydes are obtained; these aldehydes give, as Strecker has found, when treated with hydrocyanic and hydrochloric acids, compounds such as leucine and alanine, which are homologous with glycocoll. Limpricht has now shown that ethylamine and amylamine can be prepared from alanine and leucine, and it has long been known that these bases give, when treated with nitrous acid, the nitrous æthers of alcohol and fusel oil, from which of course these latter substances may be formed. The various steps in the reaction in the case of alcohol will make this transformation clearer:—

$$\underset{\substack{\text{Acetate}\\\text{of soda.}}}{C^4 H^3 NaO^4} + \underset{\substack{\text{Formiate}\\\text{of soda.}}}{C^2 H NaO^4} = \underset{\text{Aldehyde.}}{C^4 H^4 O^2} + \underset{\substack{\text{Carbonate}\\\text{of soda.}}}{2(NaO CO^2)}$$

$$\underset{\text{Aldehyde.}}{C^4 H^4 O^2} + \underset{\substack{\text{Hydrocyanic}\\\text{acid.}}}{C^2 HN} + 2HO = \underset{\text{Alanine.}}{C^6 H^7 NO^4}$$

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* Liebig's *Annalen*, March 1857.

$$\underset{\text{Alanine.}}{C^6H^7NO^4} = 2CO^2 + \underset{\text{Ethylamine.}}{C^4H^7N}$$

$$\underset{\text{Ethylamine.}}{C^4H^7N} + \underset{\substack{\text{Nitrous}\\\text{acid.}}}{2NO^3} = \underset{\substack{\text{Nitrite of}\\\text{æthyle.}}}{(C^4H^5O\,NO^3)} + N^2 + 2HO$$

$$\underset{\substack{\text{Nitrite of}\\\text{æthyle.}}}{C^4H^5ONO^3} + \underset{\substack{\text{Sulphydrate of}\\\text{sulphide of}\\\text{potassium.}}}{6(KSHS)} = \underset{\text{Alcohol.}}{C^4H^5O, HO} + NH^3 + 2HO + 6KS^2.$$

VII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xiii. p. 528.]

December 11, 1856.—General Sabine, R.A., V.P. and Treasurer, in the Chair

THE following communications were read:—

"On Practical Methods for rapid Signalling by the Electric Telegraph." By Prof. W. Thomson, F.R.S.

I am at present engaged in working out various practical applications of the formulæ communicated some time ago in a short article on the "Theory of the Electric Telegraph" (Proceedings, May 17, 1855), and I hope to be able very soon to lay the results in full before the Royal Society. In the mean time, as the project of an Atlantic Telegraph is at this moment exciting much interest, I shall explain shortly a telegraphic system to which, in the course of this investigation, I have been led, as likely to give nearly the same rapidity of utterance by a submarine one-wire cable of ordinary lateral dimensions between Ireland and Newfoundland, as is attained on short air or submarine lines by telegraphic systems in actual use.

Every system of working the electric telegraph must comprehend (1) a plan of operating at one extremity, (2) a plan of observing at the other, and (3) a code of letter-signals. These three parts of the system which I propose will be explained in order,—I. for long submarine lines, and II. for air or short submarine lines.

I. *Proposed telegraphic system for long submarine lines.*

1. *Plan of operating.*—This consists in applying a regulated galvanic battery to give, during a limited time, a definite variation of electric potential determined by theory, so as to fulfil the condition of producing an electric effect at the other extremity, which, after first becoming sensible, rises very rapidly to a maximum, then sinks as rapidly till it becomes again, and continues, insensible.

The principle followed is that pointed out by Fourier, by which we see, that, when the wire is left with both ends uninsulated after any electrical operations whatever have been performed upon it, the distribution of electric potential through it will very quickly be reduced to a harmonic law, with an amplitude falling in equal proportions during equal intervals of time. Unless the electric operations fulfil a certain condition, this ulterior distribution is according to the simple harmonic law (that is, is proportional to the sine of the distance from either extremity, the whole length being reckoned as 180°). The condition which I propose to fulfil is, that the coefficient of the simple harmonic term in the expression for the electrical potential shall vanish. Then, according to Fourier, the distribution will very much more quickly wear into one following a double harmonic law (that is, the sine of the distance from one extremity, the whole length being reckoned as 360°). In this state of electrification the two halves of the wire on each side of its middle point, being symmetrically and oppositely electrified, will discharge into one another, as well as into the earth at their remote extremities; each will be like a single wire of half-length, with the simple harmonic distribution; and the wire will, on the whole, be discharged as fast as a wire of half the length, or four times as fast as a wire of the whole length, after an ordinary electrification. There is considerable latitude as to the mode of operating so as to fulfil this condition, but the theoretical investigation is readily available for finding the best way of fulfilling it in practice. The result, as I have tested by actual calculation of the electric pulse at the remote end, is most satisfactory. The calculations, and curves exhibiting the electric pulse in a variety of cases, will, I trust, very soon be laid before the Royal Society.

The time and law of operations being once fixed upon, a mechanical contrivance of the simplest kind will give the means of directing a regulated galvanic battery to perform it with exactness, and to any stated degree (positive or negative) of strength. Complete plans of all details I have ready to describe when wanted, and shall very soon be able to state exactly the battery power required for a cable of stated dimensions.

2. *Plan of observation for receiving a message.*—The instrument which I propose is Helmholtz's galvanometer, with or without modification. The time of vibration of the suspended magnet, and the efficiency of the copper damper, will be so arranged, that during the electric pulse the suspended magnet will turn from its position of equilibrium into a position of maximum deflection, and will fall back to rest in its position of equilibrium. The possibility of fulfilling these conditions is obvious from the form of the curve I have found to represent the electric pulse. The observer will watch through a telescope the image of a scale reflected from the polished side of the magnet, or from a small mirror carried by the magnet, and he will note the letter or number which each maximum deflection brings into the middle of his field of view.

3. *Code of letter-signals.*—The most obvious way of completing a telegraphic system on the plans which have been described, is to have the twenty-six letters of the alphabet written on the scale of which the image in the suspended mirror is observed, and to arrange thirteen positive and thirteen negative strengths of electric operation,

which will give deflections, positive or negative, bringing one or other of these letters on the reflected scale into the centre of the field of view. But it would be bad economy to give the simple signals to rare letters, and to require double or triple signals for double and triple combinations of frequent occurrence. Besides, by the plans which I have formed, it will, I believe, be easy to make much more than thirteen different positive and thirteen different negative strengths of electric operation, giving unmistakeably different degrees of deflection; and if so, then many of the most frequent double and triple combinations, as well as all the twenty-six letters of the alphabet singly, might be made by simple signals. But it is also possible (although I believe highly improbable), that in practice only three or four, or some number less than thirteen, of unmistakeably different deflections could be produced in the galvanometer at one end by electric operations performed on the other extremity. If so, the whole twenty-six letters could not each have a simple signal, and double signals would have to be chosen for the less frequent letters. Experience must show what number of perfectly distinct simple signals can be made, and I have scarcely a doubt but that it will be much more than twenty-six. Then it will be easy to invent a letter code which will use these signals with the best economy for the language in which the message is to be delivered. Towards this object I have commenced collecting statistics showing the relative frequency with which the different simple letters, and various combinations of simple letters, occur in the English language, and I must soon have information enough to guide in choosing the best code for a given number of simple signals.

The investigation leading to a measurement of the electro-magnetic unit of electricity in terms of the electro-static unit, published since the commencement of the present year by Kohlrausch and Weber, has given all that is required to deduce from Weber's own previous experiments the measurement of the electric conductivity of copper wire in terms of the proper kind of unit for the telegraph problem. The data required for estimating the rapidity of action in a submarine wire of stated dimensions would be completed by a determination of the specific inductive capacity of gutta percha, or better still, a direct experiment on the electro-static capacity of a yard or two cut from the cable itself. I have estimated the retardations of various electric pulses, and the practicable rate of transmitting messages by cables 2400 miles long, and of certain ordinary lateral dimensions, on the assumption that the specific inductive capacity of gutta-percha, measured as Faraday did that of sulphur, shell-lac, &c., is 2, from which it probably does not differ much. These estimates have been published elsewhere (Athenæum, Oct. 1856), and I shall not repeat them until I can along with them give a table of estimates for cables of various dimensions, with the uncertainty as to the physical property of gutta-percha either done away with by experiment, or taken strictly into account.

II. *Plan for rapid self-recording signals by air wires and short submarine cables.*

The consideration of the preceding plans has led me to think of a system of working air lines, and short submarine lines, by which great rapidity of utterance, considerably greater I believe than any hitherto practised, may be attained. I have no doubt but that on this system five or six distinct letters per second, or sixty words per minute, may be readily delivered through air lines and submarine lines up to 100 miles, or perhaps even considerably more, of length, and recorded by a self-acting apparatus, which I shall describe in a communication I hope to make to the Royal Society before its next meeting.

"On Practical Methods for Rapid Signalling by the Electric Telegraph." (Second communication.) By Prof. W. Thomson, F.R.S.

I. *Further remarks on proposed method for great distances.*

Since my former communication on this subject I have worked out the determination of operations performed at one extremity of a submarine wire, so adjusted, that when the other extremity is kept constantly uninsulated, the subsidence of the electricity in the wire shall follow the *triple harmonic law* (that is to say, the electrical potential shall ultimately vary along the wire in proportion to the sine of the distance from either end, one-third of the length of the wire being taken as 180°). The condensation of the electrical pulse at the receiving extremity, due to such operations, is of course considerably greater than that which is obtained from operations leading only to *the double harmonic* as described in my last communication; but experience will be necessary to test whether or not the precision of adjustment in the operations required to obtain the advantages which the theory indicates, can be attained in practice when so high a degree of condensation is aimed at. The theory shows exactly what amount and duration of residual charge in the wire would result from stated deviations from perfect accuracy in the adjustments of the operations; but it cannot be known for certain, without actual trial, within what limit such deviations can be kept in practice. From Weber's experiments on the electric conductivity of copper, and from measurements which I have made on specimens of the cable now in process of manufacture for the Atlantic telegraph, I think it highly probable that, with an alphabet of twenty letters, one letter could be delivered every two seconds between Newfoundland and Ireland (which would give, without any condensed code, six words per minute) on the general plan which I explained in my last communication; and that no higher battery power than from 150 to 200 small cells of Daniell's (perhaps even considerably less) would be required. Whether or not this system may ultimately be found preferable to the very simple and undoubtedly practicable method of telegraphing invented by Mr. Wildman Whitehouse, can scarcely be decided until one or both methods shall have been tested on a cable of the dimensions of the Atlantic cable, either actually submerged or placed in perfectly similar inductive circumstances.

II. *Method for telegraphing through submarine or subterranean lines of not more than* 500 *miles length.*

The plan which I have proposed to describe for rapid signalling through shorter wires, has one characteristic in common with the plan I have already suggested for the Atlantic telegraph; namely, that of using different strengths of current for different signals.

But in lines of less than 500 miles, condensed pulses, such as have been described, may be made to follow one another more rapidly than to admit of being read off by an observer watching the image of a scale in a suspended mirror; and a new plan of receiving and recording the indications becomes necessary.

Of various plans which I have considered, the following seems most likely to prove convenient in practice.

Several small steel magnets (perhaps each about half an inch long) are suspended horizontally by fine threads or wires at different positions in the neighbourhood of a coil of which one end is connected with the line wire and the other with the earth. Each of these magnets is held in a position deflected from the magnetic meridian by two stops on which its ends press; and two other small stops of platinum wire are arranged to prevent it from turning through more than a very small angle when actuated by any deflecting force making it leave the first position. When a current passing through the coil produces this effect on any one of these magnets, it immediately strikes the last-mentioned stops, and so completes a circuit through a local battery and makes a mark on prepared electro-chemical paper. For each suspended magnet there is a separate style, but of course one battery is sufficient for the whole printing process. One set of the different suspended magnets are so adjusted, that a current in one direction of any strength falling short of a certain limit makes only one of them move; that a current in the same direction, of strength exceeding this limit but falling short of another limit, moves another also of the suspended magnets; and so on for a succession of different limits of strength of current in one direction. The remaining set of suspended magnets are adjusted to move with different strengths of current in the other direction through the coil. Without experience it is impossible to say how many gradations of strength could be conveniently arranged to be thus distinguished unmistakeably. I have no doubt, however, that very moderate applications of electric resources would give at least three different strengths of current in each direction, which could with ease and certainty be distinguished from one another by the test which the suspended magnets afford. Thus, a signal of six varieties —one letter of an alphabet of six—could be recorded by almost instantaneous movements of six suspended magnets, making one, two or three marks by one set of three styles, or one, two or three marks by another set of three styles, placed all six beside one another, pressing on a slip of electro-chemical paper drawn by clockwork, as in the Morse instrument.

In subterranean or submarine lines of less than 100 miles length, it would be easy, by means of simple battery applications, followed by connexions with the earth, or by means of simple electro-magnetic impulses at one end of the wire, to give ten or twelve of such signals per second without any confusion of utterance at the other end. The confusion of utterance which would be experienced in working thus through longer lines would be easily done away with, in any length up to 500 miles, by following up each battery application with a reverse application for a shorter time, or by following up each electro-magnetic impulse by a weaker reverse impulse, so as approximately to fulfil the condition (described in my former communication), of reducing the subsidence of the electrification in the wire to the double harmonic form. It would, I believe, be readily practicable to send distinctly five or six such signals per second (each a distinct letter of an alphabet of six) through a wire of 500 miles length in a submarine cable of ordinary dimensions. To perform the electrical operations required for sending a message on this system, mechanism might be had recourse to, and, by the use of perforated slips, as in Bain's and other systems, it would be easy to work from twelve to twenty of the six-fold varied signals per second through lines of less than 100 miles length. Operating by the hand is, however, I believe, generally preferred for ordinary telegraphing; and no such speed as the last-mentioned could be attained even by a skilful operator working with both hands. Six distinct letters or signs of an alphabet of thirty, could, however, I believe, be delivered per second by the two hands working on a key-board with twelve keys (perhaps like those of a pianoforte), provided the keys are so arranged as to fulfil the following conditions:—

(1) That by simply striking once any one of a first set of six of the keys, an electric operation of one or other of the six varieties shall be made twice, the second time commencing at a definite interval (perhaps $\frac{1}{12}$th of a second) later than the first.

(2) That by striking one or other of the remaining six keys at the same time, or very nearly at the same time, as one of the first set, the second operation of the double electric signal will be that corresponding to the key of the second set which is struck, instead of being a mere repetition of the operation corresponding to the key of the first set.

It would certainly be easy to make a key-board to fulfil these conditions with the aid of some clockwork power. Then by arranging the thirty-six permutations and doubles of the six simple signals to represent an alphabet of thirty-six letters and signs, an experienced operator would have to direct his mind to only six different letters per second, while executing them by six double operations with his fingers. That it would be possible to work by hand at this rate there can be no doubt, when we consider the marvels of rapid execution so commonly attained by practice on the pianoforte; and it appears not improbable that in regular telegraphic work, practised operators of ordinary skill could perform from four to six letters with ease per second, or from forty to sixty words per minute, on lines of not more than 100 miles length. The six signals per second, which, according to the preceding estimate, could be distinctly conveyed by a submerged wire of 500 miles in length, could of course be easily performed by the hand, with the aid of a key-board and clockwork power adapted to make the double operations for giving rapid subsidence of electricity in the wire when any one key is touched, and to let the different strengths of current, in one direction or the other, be produced by the different keys. Thus without a condensed code, thirty words per minute could be telegraphed through subterranean or submarine lines of 500 miles; and from thirty to fifty or sixty words per minute through such lines, of lengths of from 500 miles to 100 miles.

The rate of from fifty to sixty words per minute could be attained through almost any length of air line, were it not for the defects of insulation to which such lines are exposed. If the imperfection of the insulation remained constant, or only varied slowly from day to day with the humidity of the atmosphere, the method I have indicated might probably, with suitable adjustments, be made successful; and I think it possible that it may be found to answer for air lines of hundreds of miles' length. But in a short air line, the strengths of the currents received, at one extremity, from graduated operations performed at the other, might suddenly, in the middle of a message, become so much changed as to throw all the indications into confusion, in consequence of a shower of rain, or a trickling of water along a spider's web.

"On the Equation of Laplace's Functions," &c. By W. F. Donkin, M.A., F.R.S., F.R.A.S., Savilian Professor of Astronomy, Oxford.

The equation $\frac{d^2u}{dx^2}+\frac{d^2u}{dy^2}+\frac{d^2u}{dz^2}=0$, when transformed by putting $x=r\sin\theta\cos\phi$, $y=r\sin\theta\sin\phi$, $z=r\cos\theta$, may be written in the form

$$\left\{\left(\sin\theta\frac{d}{d\theta}\right)^2+\left(\frac{d}{d\phi}\right)^2+(\sin\theta)^2 r\frac{d}{dr}\left(r\frac{d}{dr}+1\right)\right\}u=0\,; \qquad (1)$$

and if $u=u_0+u_1r+u_2r^2+\ldots+u_nr^n+\ldots$, we find on substituting this value in (1), and equating to zero the coefficient of r^n, that u_n satisfies the equation

$$\left\{\left(\sin\theta\frac{d}{d\theta}\right)^2+\left(\frac{d}{d\phi}\right)^2+n(n+1)(\sin\theta)^2\right\}u_n=0, \quad . \quad . \quad (2)$$

commonly called the equation of Laplace's functions. If we put $\sin\theta\frac{d}{d\theta}+n\cos\theta=\varpi_n$, then the equation (2) may be written

$$\left(\varpi_n\varpi_{-n}+n^2+\left(\frac{d}{d\phi}\right)^2\right)u_n=0\,;$$

and the operation ϖ_n possesses the following property, namely

$$\varpi_{-n}\varpi_n+n^2=\varpi_{n-1}\varpi_{-(n-1)}+(n-1)^2\,;$$

hence it is easily shown, that in general the complete solution of (2) is

$$u_n=\varpi_n\varpi_{n-1}\ldots\varpi_2\varpi_1\,.\,u_0,$$

where u_0 is the solution of

$$\left(\left(\sin\theta\frac{d}{d\theta}\right)^2+\left(\frac{d}{d\phi}\right)^2\right)u_0=0,$$

namely

$$u_0=f\left(e^{\phi\sqrt{-1}}\tan\frac{\theta}{2}\right)+\mathrm{F}\left(e^{-\phi\sqrt{-1}}\tan\frac{\theta}{2}\right);$$

and the operation $\varpi_n\varpi_{n-1}\ldots\varpi_2\varpi_1$ is easily seen to be equivalent to

$$(\sin\theta)^{-n}\left(\sin\theta\frac{d}{d\theta}\sin\theta\right)^n.$$

(This result is compared with that obtained in a different way by Professor Boole (Cambridge and Dublin Journal, vol. i. p. 18), to which it bears a general resemblance, but the author has not succeeded at present in reducing the one form to the other.)

In the case in which u_n does not contain ϕ, we have

$$u_0=\mathrm{C}_1+\mathrm{C}_2\log\tan\frac{\theta}{2}.$$

The general expression for a "Laplace's coefficient" of the nth order, not containing ϕ, is therefore $(\sin\theta)^{-n}\left(\sin\theta\frac{d}{d\theta}\sin\theta\right)^n.\mathrm{C}$; and if this be called v_n when $\mathrm{C}=1$, the development of $(1-2r\cos\theta+r^2)^{-\frac{1}{2}}$ is

$$v_0+v_1r+v_2\frac{r^2}{1.2}+\ldots+v_n\frac{r^n}{1.2\ldots n}+\ldots;$$

and it is shown that the coefficient of $\frac{r^n}{1.2\ldots n}$ in the development of $(1-2r\cos\theta+r^2)^{-\frac{i+1}{2}}$ is

$$(\sin\theta)^{-n-i}\left(\sin\theta\frac{d}{d\theta}\sin\theta\right)^n(\sin\theta)^i.$$

With respect to the development of

$$\left(1-2r(\cos\theta\cos\theta'+\sin\theta\sin\theta'\cos\phi)+r^2\right)^{-\frac{1}{2}},$$

it is shown that the coefficient of $r^n\cos i\phi$ may be put in either of the two forms,

$$\frac{2}{1.2\ldots(n-i).1.2\ldots(n+i)}(\sin\theta)^{-n}(\sin\theta')^{-n}\Theta^n\Theta'^n\left(\tan\frac{\theta}{2}\right)^i\left(\tan\frac{\theta'}{2}\right)^i,$$

or

$$\frac{2.1^2.3^2\ldots(2i-1)^2}{1.2\ldots(n-i).1.2\ldots(n+i)}(\sin\theta)^{-n}(\sin\theta')^{-n}\Theta^{n-i}\Theta'^{n-i}(\sin\theta)^{2i}(\sin\theta')^{2i},$$

where Θ represents the operation $\sin\theta\frac{d}{d\theta}\sin\theta$, and the factor 2 is in each case to be omitted when $i=0$. (This coefficient is a solution of the equation

$$\left\{\left(\sin\theta\frac{d}{d\theta}\right)^2+n(n+1)(\sin\theta)^2-i^2\right\}u=0,$$

of which the complete integral may be expressed in the form

$$(\sin\theta)^{-n}\left(\sin\theta\frac{d}{d\theta}\sin\theta\right)^{n-i}(\sin\theta)^{2i}\left(\mathrm{C}_1+\mathrm{C}_2\int d\theta(\sin\theta)^{-2i-1}\right),$$

at least in the case in which i is an integer not greater than n, for which case this form is here demonstrated.)

If it be assumed that the solution of (2), obtained on the supposition that n is an integer, may be extended to the case in which n is a general symbol, it follows that the solution of (1) will be obtained from it by changing n into $r\frac{d}{dr}$. This would give

$$u=(\sin\theta)^{-r\frac{d}{dr}}\left(\sin\theta\frac{d}{d\theta}\sin\theta\right)^{r\frac{d}{dr}}\left\{f\left(r,\ e^{\phi\sqrt{-1}}\tan\frac{\theta}{2}\right)\right.$$
$$\left.+\mathrm{F}\left(r,\ e^{-\phi\sqrt{-1}}\tan\frac{\theta}{2}\right)\right\},$$

which is easily shown to be equivalent to

$$u=f\left(\rho\sin\theta\frac{d}{d\theta}\sin\theta,\ e^{\phi\sqrt{-1}}\tan\frac{\theta}{2}\right)+\mathrm{F}\left(\rho\sin\theta\frac{d}{d\theta}\sin\theta,\ e^{-\phi\sqrt{-1}}\tan\frac{\theta}{2}\right),$$

where $\rho=r(\sin\theta)^{-1}$, but ρ is to be treated as a constant till after all operations.

This expression is shown to give known particular integrals, such as $(1-2r\cos\theta+r^2)^{-\frac{1}{2}}$, and

$$r^n(\sin\theta)^{-n}\left(\sin\theta\frac{d}{d\theta}\sin\theta\right)^n\left(\tan\frac{\theta}{2}\right)^i\cos i\phi.$$

It appears probable, therefore, that the generalization of the result obtained for the limited value of n is legitimate; but the author does not profess to demonstrate this conclusion, believing that the principle of the "permanence of equivalent forms" is not at present established in such a sense as to amount to a demonstration.

"A Memoir on Curves of the Third Order." By Arthur Cayley, Esq., F.R.S.

A curve of the third order, or cubic curve, is the locus represented by an equation such as $\mathrm{U}=(*\between x, y, z)^3=0$; and it appears by my "Third Memoir on Quantics," that it is proper to consider, in connexion with the curve of the third order, $\mathrm{U}=0$, and its Hessian $\mathrm{HU}=0$ (which is also a curve of the third order), two curves of the third class, viz. the curves represented by the equations $\mathrm{PU}=0$ and $\mathrm{QU}=0$. These equations, I say, represent curves of the third class; in fact, PU and QU are contravariants of U, and therefore, when the variables x, y, z of U are considered as point coordinates, the variables ξ, η, ζ of PU, QU must be considered as line coordinates, and the curves will be curves of the third class. I propose (in analogy with the form of the word Hessian) to call the two curves in question the Pippian and Quippian respectively. A geometrical definition of the Pippian was readily found; the curve is in fact Steiner's curve R_0 mentioned in the memoir "Allgemeine

Eigenschaften der algebraischen Curven," Crelle, t. xlvii. pp. 1–6, in the particular case of a basis-curve of the third order; and I also found that the Pippian might be considered as occurring implicitly in my "Mémoire sur les Courbes du Troisième Ordre," Liouville, t. ix. p. 285, and "Nouvelles Remarques sur les Courbes du Troisième Ordre," Liouv. t. x. p. 102. As regards the Quippian, I have not succeeded in obtaining a satisfactory geometrical definition; but the search after it led to a variety of theorems, relating chiefly to the first-mentioned curve, and the results of the investigation are contained in the present memoir. Some of these results are due to Mr. Salmon, with whom I was in correspondence on the subject. The character of the results makes it difficult to develope them in a systematic order; but the results are given in such connexion one with another, as I have been able to present them in. Considering the object of the memoir to be the establishment of a distinct geometrical theory of the Pippian, the leading results will be found summed up in the nine definitions or modes of generation of the Pippian, given in the concluding number. In the course of the memoir I give some further developments relating to the theory in the memoirs in Liouville above referred to, showing its relation to the Pippian, and the analogy with theorems of Hesse in relation to the Hessian.

"On the k-partitions of a Polygon and Polyace." By the Rev. T. P. Kirkman, M.A.

The problem relating to the polyace is the reciprocal of that relating to the polygon, and is not separately discussed. By the k-partitions of a polygon, the author means the number of ways in which the polygon can be divided by $(k-1)$ diagonals, no one of which crosses another; two ways being different only when no cyclical permutation or reversion of the numbers at the angles of the polygon can make them alike: it is assumed that the polygon is of the ordinary convex form, so that all the diagonals lie within its area. The author remarks, that the enumeration of the partitions of the polygon and polyace is indispensable in the theory of polyedra, and that in his former memoir "On the Enumeration of x-edra having Triedral Summits and an $(x-1)$-gonal Base," Phil. Trans. 1856, p. 399, he has, in fact, investigated the $(r-2)$-partitions of the r-ace or r-gon: so that the present memoir may be considered as a completion, or rather an extension and completion, of the investigations in his former memoir. The number of distinctions to be made in the problem of the present memoir is very great; thus, a partition of the polygon may be either reversible or irreversible; and if reversible, then the axis of reversion may be either agonal, monogonal, or diagonal, that is, it may pass through no angle, one angle only, or two angles of the polygon; and in the last case it may be either drawn or undrawn. Again, there may be a single axis or a greater number of axes of reversion: in the case of m such axes, the partition is said to be m-ly reversible; and in like manner an irreversible partition may consist of a single irreversible sequence of configurations, or it may contain such sequence m times repeated, it is then said to be m-ly irreversible. In consequence of this multiplicity of distinctions, the author's final results are necessarily very complicated, and cannot be exhibited in an abstract; they appear, however, to contain a complete solution of the problem, *i. e.* to afford the means of finding, without anything tentative, the number of the k-partitions of an r-gon when k and r are given numbers.

December 18.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—

"On the Scelidothere (*Scelidotherium leptocephalum,* Owen), a large extinct Terrestrial Sloth." By Professor R. Owen, F.R.S.

"On the Evidence of the existence of the Decennial Inequality in the Solar-diurnal Variations, and its non-existence in the Lunar-diurnal Variation of the Magnetic Declination at Hobarton." By Major-General Sabine, R.A., D.C.L., Treas. and V.P.R.S.

In a communication made to the Royal Society in the last Session, "On the Lunar-diurnal Magnetic Variation at Toronto," the author had stated that he could discover no trace of the lunar influence of the decennial inequality which constitutes so marked a feature in the solar magnetic variations. He has since read, in a memoir communicated to the Imperial Academy of Sciences at Vienna, entitled "On the influence of the Moon on the horizontal component of the Magnetic Force," that M. Kreil is of opinion that the observations of different years at Milan and Prague, when combined, would rather favour the contrary inference, viz. that the decennial inequality exists in the lunar as well as in the solar variations. The author was led therefore to re-examine this question by the aid of the observations of the Declination at the Hobarton Observatory, which he considers to be remarkably well suited for the purpose, as they comprise eight years of consecutive hourly observation with unchanged instruments and a uniform system of observation, and number, exclusive of Sundays, Christmas-days, and Good Fridays, and occasional but very rare omissions, no less than 51,998 observations.

These observations have been examined by the processes already described in the author's communication of last Session, and the results form the subject of the present paper, showing, in the author's belief, decided and systematic evidence of the existence of the diurnal inequality, having its minimum epoch in 1843–1844, and its maximum epoch five years later, in the mean diurnal variation due to the disturbances and in the more regular and ordinary solar-diurnal variation, and the absence of any trace of a similar inequality in the lunar-diurnal variation.

January 8, 1857.—William Robert Grove, Esq., V.P., in the Chair.

The following communications was read:—

"On the Function of the Thyroid Body." By Patrick Martyn, Esq., M.D. Lond., Surgeon R.N.

After referring to the form, situation, connexions and internal structure of the thyroid body, its large supply of blood and its capability of sudden alterations of bulk; the author briefly adverts to the unsatisfactory explanations which have been offered as to its function, and then proceeds to state his own views, as follows:—

"The upper part of the trachea, the larynx, and the passage of the fauces and mouth constitute the organ of voice; the two former are the essential or voicing part as mechanicians call it, that which produces the tone. The larynx and trachea—taking a share in other functions and being associated by juxtaposition and attachment with contiguous organs—are always pervious and open for respiration; lengthen and shorten, fall and rise with the œsophagus in deglutition, and bend and turn with the universal motions of the head and neck.

"To admit of this great mobility and flexibility, a certain structure is necessary. The larynx is a triangular box enclosing the apparatus of the chordæ vocales; its two cartilaginous sides or alæ, diverging from the front, are not fixed but free at the back, being completed by soft parts: the trachea is composed of a succession of incomplete cartilaginous hoops or rings lying apart, the back and intervals being made up and the tube completed by soft membrane.

"Now the structure of a wind instrument, such as that of the human voice is, requires the very opposite properties. It must be rigid, tense and inflexible. The qualities of the tone will be in exact proportion to these properties. How then is the soft, slack and flexible vocal tube rendered thus rigid, tense and inflexible, and fit to produce pure tone? The muscles of the larynx, the thyro-hyoid and sterno-thyroid, merely raise or lower, or fix it in any position: not lying on, or being parallel to, but diverging from the vocal tube, they cannot effect the object referred to. It appears to me that the thyroid body is provided for this purpose. The act of uttering a tone or of speaking stops the return of the blood from that organ, distends and renders it tense, and from the nature of its attachment round the top of the trachea and on the free sides of the alæ of the larynx, renders them fixed, firm, and tense also. This effect is aided by the aforesaid muscles, the thyroid body being interposed and giving them more advantageous mechanical action. This tension may be in any degree, and on energetic speaking or singing, the increased size of the part and the fulness of the collateral veins may be seen. This is the reason of its large supply and free distribution of blood. An instance of the want of this tension in an instrument may be seen in the bagpipe, where the porte-vent is attached to the chanter or voicing part by a flexible joint or by leather, and the tone is in consequence squeaking and uncertain.

"Besides thus giving rigidity, firmness and tension to the organ of voice, the thyroid body also acts in another capacity—as a loader. In most musical instruments, loaders are used to render the vibrations slower and longer, and the tone in consequence fuller, louder and deeper. They compensate for want of size and space, and give to a small instrument, or to a small vibrating or voicing part of an instrument, the power and quality of a large one. The human organ of voice is 8 inches long, and has the same power and better quality of tone than the instrument that most nearly approaches it,—the French horn, which is 9 feet, or the "vox humana" pipe of a moderate-sized organ, which is from 4 to 8 feet long. This economy of size in the human organ has always been wondered at, but never, that I know, explained. Besides the thyroid body, another part, the structure of which I shall describe on another occasion, aids in this admirable economy. The nearer mechanism of human design approaches to perfection, the more it resembles similar structures in animal mechanics. The base of all stringed instruments and musical boxes is loaded: in most wind instruments the voicing part is thus loaded and strengthened, as in the organ pipe, horn, flute, clarionet, &c. The bassoon, which in its lower notes approaches the human voice, is uncertain and wheezy in tone for want of this provision.

"When the thyroid body is small and thin, the voice will be found to be small and shrill; when large, the tone will be full and sonorous; when it is morbidly enlarged, the voice will be deeper and more base; and when very large, as in bronchocele, the voice will be smothered.

"The compass of the voice is in great part produced by the raising and lowering of the larynx, the shortening and lengthening of the vocal tube. The thyroid body partakes of this motion, at the same time firmly fixing and rendering tense the parts in each position. By its change of shape, bulk and density—flattening and thinning when the larynx is raised, enlarging and bulging when it is lowered—it aids in giving the particular tone or pitch, high and acute in the first case, full and deep in the second; and, in like manner, by its varying shape, bulk, density, and pressure, it takes a great part in producing the wonderful qualities of modulation and expression peculiar to the human voice. In animated conversation, declamation and singing, this may be seen.

"Its function then appears to be threefold—rendering the slack, mobile and flexible vocal organ or tube rigid, tense and inflexible, and fit to produce pure tone; by its bulk and density acting as a loader and strengthener, making the tone more sonorous, full and deep, and thus compensating for want of length and size in the organ; and finally, by its varying shape, bulk, density and pressure, furnishing an important aid in producing the inimitable qualities of modulation and expression enjoyed by the human voice.

"That it is a part of the organ of voice and an important accessory in giving it perfection, may be inferred also from its situation on the larynx and trachea, and its being supplied by the same nerves—its being largest in man, where the voice and speech are perfect—its being proportionally larger in women and children than in men, their smaller and more mobile organs requiring its peculiar aid. Among the lower animals, it is present (at least in a fully developed condition) only in the Mammalia, but among them there is a remarkable exception in the Cetacea—they have it not, and they have no voice. In Birds, which have such great power and modulation of voice, the structure of the vocal organ and tube is different from that in man, and sufficient in itself to produce these qualities.

"The importance of the thyroid body must be admitted when it is shown to be necessary for the perfection of the voice, and hence of speech—that great and indispensable agent in the cultivating and advancing the highest faculties of man."

GEOLOGICAL SOCIETY.

[Continued from vol. xiii. p. 533.]

June 3, 1857.—Col. Portlock, R.E., President, in the Chair.

The following communication was read:—

"On the species of Mastodon and Elephant occurring in the Fossil state in England.—Part II. Elephants." By H. Falconer, M.D., F.R.S., F.G.S.

In the introductory portion of Part I. of this Memoir, the author alluded to the ambiguity that has existed relative to the mammalian faunæ of the Miocene and Pliocene periods in consequence of palæontologists confounding several distinct forms of *Mastodon*, of different geological ages, under one name (*M. angustidens*); and on this occasion Dr. Falconer stated, that, in the application of the name *Elephas primigenius* (Mammoth) to a multitude of elephantine remains from various superficial and deep deposits, over a vast extent of territory, and of different ages, a similar, if not a greater, amount of error and confusion had arisen. In fact, at least half the habitable globe has been assigned to the Mammoth as his pasture-ground, if we were to accept the determinations of all those who have written on the remains of *Elephas primigenius*. The duration, too, of this nominal species in time is equally remarkable, so considered; since, as it has been quoted from the lower and the upper pliocene beds, as well as from the post-pliocene glacial gravels, it ought to have existed before the European area received its present geographical form, and indeed before the Alps, Apennines, and Pyrenees reached their present elevation. After noticing the difficulty met with by the geologist in the classification of the newer Tertiaries, on account of this ubiquitous presence of the Mammoth, the author proceeded to show that several species, belonging to two distinct subgenera, have been generally confounded under the name of *Elephas primigenius*; and that each had its limited range in geographical area and geological time. The present condition of the nomenclature of the subject, and the history of the established species of European fossil Elephants, namely *Loxodon meridionalis*, *Loxodon priscus*, *Euelephas antiquus*, and *Euelephas primigenius*, preceded an explanation of the principles on which the species are determined, and a description of the dental characters by which the Elephants are divisible into subgenera,—a succinct account of which was given in the former part of the Memoir (vol. xiii. p. 462). The "intermediate molars" in Elephants have never less than six divisions of the crown, and sometimes as many as eighteen. These molars do not all have an equal number of ridges: some Elephants have an augmentation of only one ridge to the crown of the penultimate of these molars; these are "hypisomerous," namely *Stegodon* and *Loxodon*; others, in which the number of the ridges progressively increases, are "anisomerous," and form a third natural group, namely the *Euelephas* or *Elephas* proper. The *Stegodon* has four species, fossil in India; and approaches the *Mastodon* in the form of the molars. The *Loxodon* includes the existing African Elephant and three fossil species, and is characterized by its distinct rhomboidal discs of wear on the grinders. *Euelephas* has thin-plated molars; but in some species there are intermediate stages, as regards the angular mesial expansion of the plates, between it and *Loxodon*.

Dr. Falconer next proceeded to review some well-ascertained mammalian faunæ localized in certain parts of Europe, where the conditions of deposit are most simple, and to apply the results to the more complex instances, where the remains of more than one distinct fauna are intermingled, or so closely deposited as to be too readily confused by collectors. With this view, the author instanced the Subapennine or pliocene deposits of the Astesan, and elsewhere in Piedmont and Lombardy, where *Trilophodon Borsoni*, *Tetralophodon arvernensis*, *Loxodon meridionalis*, *Lox. priscus*, and *Euelephas antiquus*, with *Rhinoceros leptorhinus*, *Hippopotamus major*, &c., are found associated together. In the Subapennine beds of the Val d'Arno, in Tuscany, *Tetralophodon arvernensis* and *Loxodon meridionalis* occur with the same *Hippopotamus* and *Rhinoceros*. Near Chartres, in France, *Loxodon meridionalis* accompanies *H. major* and *Rhinoceros leptorhinus*. The above-mentioned are necessarily the leading mammalian forms of the older Pliocene period. North of the Alps pliocene deposits similar to those of Italy occur in some parts of Switzerland, but they are soon overlaid towards the north by a distinct mass of erratic drift of a different age and with different mammalian remains. In the fluviatile "Loess" or "Lehm" of the valley of the Rhine, and in the Glacial Drift of the plains of Northern Germany, these post-pliocene deposits contain remains of the true Mammoth, with the tichorhine Rhinoceros, the Musk-buffalo, &c., which thus constitute the leading types of the post-pliocene mammalian fauna.

On the eastern coast of England, the Crag-deposits (the Red and Norwich Crags) yield the pliocene *Tetralophodon arvernensis*, *Loxodon meridionalis*, and *Euelephas antiquus*; and the so-called Elephant-beds at Cromer, Mundesley, and Hasborough furnish *Lox. meridionalis* and *Euel. antiquus*, with *Rhin. leptorhinus* and *Hip. major*. These characteristically pliocene fossils, however, are occasionally intermingled with the remains of the post-pliocene *Euelephas primigenius*, the latter fossils having been derived from the overlying and later drift-beds, which have thus proved a fertile source of the confusion and ambiguity already referred to. To some extent, similar conditions exist at Bracklesham Bay and Pagham Harbour, where molars of *E. primigenius* are found in the upper gravels, whilst remains of *E. antiquus* abound in the older mud-deposit, lately described in the Society's Journal by Mr. Godwin-Austen.

Dr. Falconer then considered the fluviatile deposits of the Valley of the Thames, in relation to their Elephantine remains; especially at Grays Thurrock and Brentford. At the former place the author recognizes the true pliocene assemblage of *Loxodon priscus*, *Euelephas antiquus*, *Hippopotamus major*, and *Rhinoceros leptorhinus*; but the group of mammals found at Brentford, according to the published determinations, indicate the close proximity of both the pliocene and post-pliocene faunæ at different levels of the same section. The Grays Thurrock deposits, and the lower beds at Brentford were inferred to be of an earlier age than any part of the Boulder-Clay or Sill.

The grouping of the *E. primigenius*, *Rhinoceros tichorhinus*, *Bubalus moschatus*, &c., in the newer gravels of England and elsewhere was next dwelt upon, as affording an additional clue to the tracing of the several characteristic mammalian faunas over the European area.

To the possible objection of there being too many large Proboscideans grouped in one fauna, the author replied that the bones of animals of three distinct species actually occur together in one stratum in Italy, and that six species are found in deposits of one age in the Sivalik hills.

Dr. Falconer concludes that the same mammalian fauna existed throughout the period during which both the Crag and the fluviatile beds of the Thames Valley were being deposited; and that a chronological division of the newer Tertiaries into older Pliocene, newer Pliocene or Pleistocene, and Post-pliocene is untenable; too much stress having been laid by authors upon the shell-evidence on this point. At the same time, it is not meant to be implied that all the species of the fauna ranged everywhere throughout the area: some in all probability were peculiar to the south, and others to the north.

The presence of the *Hippopotamus* in the pliocene deposits was pointed out as being of great importance in indicating the character of the pliocene land, which, extending between England and the Continent, must have afforded a great system of rivers and lakes, and probably had a comparatively warm temperature, as late as the deposition of the Grays beds, where also (as is well known) occur some southern freshwater shells, now extinct in England.

After some remarks on the negative evidence afforded by this mammalian fauna with regard to the supposed refrigeration of the land during the Pliocene period, Dr. Falconer reviewed the opinions of some English geologists on the physical conditions and faunæ of this region during the newer Tertiary epoch, especially the views of Mr. S. Wood, Mr. Prestwich, and Mr. Trimmer; and concluded with a few remarks on the occurrence of *E. antiquus* in the Cefn and Kirkdale Caves, and of *E. primigenius* in Kent's Hole, and on the non-existence of *E. primigenius* south of the Alps, and its restriction in the United States of America to the Northern and Central States. In the Southern States and in Mexico a distinct fossil species, *Euelephas Columbi*, hitherto undescribed, occurs along with remains of *Mastodon*, *Mylodon*, *Megatherium*, Horse, &c.

VIII. *Intelligence and Miscellaneous Articles.*

NOTE ON THE QUESTION—IS THERE ANY ADVANTAGE, IN CHEMICAL DECOMPOSITIONS, IN INTRODUCING AN INDUCTION APPARATUS OF ONE WIRE IN THE CIRCUIT OF A VOLTAIC PILE? BY C. DESPRETZ.

IN a battery with one or two liquids in which the amalgamated zinc is scarcely, if at all, attacked by dilute sulphuric acid, when the current is not established, the loss of weight of the zinc corresponds with the oxygen of the water decomposed in the voltameter. This result shows that the pile is the most perfect of machines, when its force is measured by the decomposition of water; for in this case the effective work is equal to the motive work within a hundredth, or sometimes a two-hundredth part. The pile is supposed to be completely isolated, and the amalgamated zinc not sensibly attacked by the dilute sulphuric acid.

An experiment of M. De la Rive has shown that a pile which decomposes water very weakly, acquires the power of decomposing it in a very marked manner, if an induction apparatus be introduced into the circuit. M. De la Rive has given the name of *voltaic condenser* to the induction apparatus of one wire which he employed.

It has lately been asserted, that by an arrangement analogous to that of M. De la Rive, there is an actual economy in the consumption of zinc. Such an opinion can only be founded upon exact experiments. If the quantity of zinc consumed were less than that corresponding with the oxygen of the water decomposed, the union of a pile and an induction apparatus would form a *more than perfect* machine. *Its useful effect would exceed the motive work,* which would be a singular result. To settle this question, the only course is to weigh the amalgamated zinc before and after the experiment, and to compare the loss of the metal with the weight of oxygen and hydrogen of the water decomposed in the voltameter.

To make the necessary comparison, eight Bunsen's elements were arranged in two sets of four elements. This arrangement is equivalent to two elements in tension, of four times the dimension. In the circuit was placed a small induction apparatus with a single wire. When half a litre of gas (oxygen and hydrogen) was evolved in the voltameter, the loss of the zinc was 1·578 grm., on the average of five nearly concordant experiments. The average duration was an hour and two minutes.

1·578 grm. of zinc corresponds with a quantity of water represented by a volume equal to 0·8131 litre of dry oxygen and hydrogen at 32° F. and a pressure of 0·76 metre. The experiment only furnished 0·500 litre of moist oxygen and hydrogen gas at a temperature of 60°·75 F., and a pressure of 0·7435 metre. In a dry state, at 32° F. and the pressure of 0·76 metre, the 0·500 litre is reduced to 0·4531 litre. Thus the induction apparatus causes a loss of 0·36 litre, or four-ninths of the internal work, or four-fifths of the actual work obtained; in other words, nearly half the zinc is not represented by the water decomposed in the voltameter. If time were the only object, and the zinc and acids used in the pile were regarded as negligeable matters, there would be an advantage in the use of the induction apparatus. The same pile, but without the induction apparatus, only produced half a litre of moist gas in about double the time.

The great loss of zinc here ascertained must not be attributed to the reversal of the current. The direction of the current remains the same. If each wire of the voltameter be covered with a graduated tube, the volume of one of the gases is double that of the other; the former is hydrogen, the latter oxygen. In passing the current through a solution of sulphate of copper, one electrode is covered with a coat of red copper, the other retains the colour of platinum. It is to the diversion of a part of the current by the conductor of the hammer that the greater part of the loss must be attributed.

At the commencement of the experiment the current divides into two very unequal parts,—one passes by the conductor of the hammer, and the other traverses the voltameter. This is very weak in comparison with the former; so much so that it is almost incapable of decomposing water. Nearly all the current passes by the conductor of the hammer, the resistance of the two plates of copper, of a few centimetres in length, of which this is composed, being extremely small compared with that of the voltameter. If the hammer be removed, the entire current passes into the voltameter, but the current which then traverses the pile has less intensity than when the conductor of the hammer is closed.—*Comptes Rendus,* May 18, 1857, p. 1009.

ON THE SLOW ACTIONS PRODUCED UNDER THE COMBINED INFLUENCE OF HEAT AND PRESSURE. BY M. BECQUEREL.

M. Becquerel has long been occupied with the slow actions produced at the surface or in the superficial strata of the globe at the ordinary temperature and pressure of the atmosphere. These actions have a chemical, electrical or mechanical origin, but the chemical cause is that which acts with the greatest efficacy. The following examples will give an idea of the influence of each of these three causes.

When a plate of iron is exposed to moist air, it soon becomes oxidized here and there, in points where there is heterogeneity, or where foreign bodies exist. These points constitute so many voltaic couples, which assist the original chemical action. In the electrochemical reaction which takes place, water is decomposed; the hydrogen which comes in contact with the oxide combines with the nitrogen of the air or that of adherent organic matter, forming ammonia or carbonate of ammonia, which is usually found in rust. The effect is still more manifest when a fragment of charcoal, or of any other body which is a good conductor, but less oxidizable than iron, is applied to the iron plate. Copper, lead, and silver, in contact with certain solutions, produce analogous effects.

Rocks with a felspar base, or which contain alkali, when crushed and when their fragments are carried away by water and rolled in the torrents, become decomposed by the mutual friction of their particles. M. Becquerel has proved this decomposition twenty years ago, by crushing basalt, felspar, &c. in an agate mortar in presence of water; the damp paste which is formed soon furnishes an alkaline reaction. The decomposition of the bodies pulverized is sometimes facilitated by mixing them with other bodies of which the elements are capable of acting by double decomposition upon those of the former.

Thus on triturating together in equal atomic proportions nitrate of lead and iodide of potassium, iodide of lead and nitrate of potash are obtained in a few moments. In operating with sulphate of soda and carbonate of lime, a double decomposition is also produced.

In a recent communication to the Academy, M. Daubrée has brought forward some new evidence in support of the preceding observations. He operates as follows:—Into a cask, to which he gives a rapid rotatory movement, he puts water with fragments of felspar and quartz; at the expiration of a certain time a mud produced by the trituration of the particles is deposited, and the water becomes alkaline. The felspar is consequently decomposed, at least partially.

In these investigations, M. Becquerel had hitherto operated only at the ordinary pressure and temperature of the atmosphere; in the present case he has experimented at temperatures and pressures more or less elevated, with the view of obtaining an idea of what must have taken place in the sedimentary rocks when they were covered by ejected rocks, such as granite, porphyries, basalt, &c. In the present memoir the combined influence of heat and pressure in chemical and electro-chemical actions are alone referred to. The effects resulting from mechanical actions will be treated of in a particular memoir.

The author operates as follows:—He takes a tube of 5–6 millims. in diameter and 2 decimetres in length, closed at one extremity; into this he puts the solid substance, and pours over it the solution which is to react. On the top of this some sulphuret of carbon or æther is poured; the tube is sealed up and placed in a stove heated to 212°—302° F. Another tube is sometimes introduced into the principal one, containing another volatile liquid, the elementsof which are to react upon the solid body or the solution; lastly, when electro-chemical effects are in question, the apparatus destined to produce these is placed in the tube. By means of these different modes of experimentation, M. Becquerel has obtained the following products:—

1. Arragonite in right rectangular prisms with two bevels at each apex, and the angles of which are measurable by the goniometer.
2. Protoxide of copper in pretty octahedral crystals.
3. Sulphurets of copper in prisms, having the aspect of the native sulphurets.
4. Sulphurets of silver and lead in lamellæ, having a metallic aspect.
5. Green carbonate of copper (malachite) and blue carbonate of copper in small nodules.
6. Metallic iodides, bromides and cyanides, insoluble and crystallized, &c.

From the researches of M. Becquerel it appears, that under the combined influence of heat and pressure, slow actions acquire new activity and produce interesting effects in a physico-chemical and geological point of view.—*Comptes Rendus*, May 11, 1857, p. 938.

NOTE ON THE OPTICAL PROPERTIES OF MAGNETIC BODIES.

BY M. VERDET.

In a note read some time ago before the Academy of Sciences*, the author made known some experiments showing that under the influence of magnetism the salts of iron exert an action upon polarized light opposite to that of water, glass, sulphuret of carbon and some other transparent substances. On studying the compounds of the other magnetic metals, he has found that a certain number of them act upon light in the same way as the compounds of iron.

He gave the name of *magnetic rotatory power* to the property of rotating the plane of polarization temporarily developed by magnetism in transparent substances. The magnetic rotatory power of most transparent substances he called *direct*, and that of the salts of iron, *inverse*. He now proposes to substitute for these terms the expressions *positive* and *negative*, which indicate the direction of the rotation. Water, sulphuret of carbon, glass and the other transparent substances of which the rotatory power is *positive*, rotate the planes of polarization in the direction in which positive electricity traverses the conducting wire of the electro-magnet; the salts of iron rotate it in the direction of movement of negative electricity.

The magnetic metals of which the author has investigated the transparent compounds, are iron, nickel, cobalt, manganese, chromium, titanium, and cerium. All these metals are attracted by electro-magnets, and form compounds endowed with the same property. There are other metals, such as platinum and its analogues, which appear to be magnetic, but all the compounds of which are diamagnetic; the magnetic character of these metals is therefore not absolutely certain†.

Iron.—Protosalts of iron are endowed with a negative magnetic rotatory power, evidenced by the weakness of the action which the aqueous solutions of these salts exert upon polarized light. This action is always weaker than would be that of the water contained in the solution, but in the same direction, and the author has not met with any protosalt of iron possessing a negative power sufficient to destroy entirely the positive power of the water. To make quite sure of this phænomenon, the author prepared solutions of sulphate of iron of different degrees of concentration, and found that the rotations produced agreed exactly with the hypothesis, that these solutions are mixtures of two bodies endowed with opposite rotatory powers‡.

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* See Phil. Mag. S. 4. vol. xii. p. 483.

† On the distinction between the magnetic and diamagnetic metals, see the 21st Series of 'Experimental Researches in Electricity,' by Professor Faraday.

‡ The experiments relating to these variously concentrated solutions presented a remarkable peculiarity. Considering them as formed of water

The negative magnetic rotatory power of the salts of peroxide of iron is much greater than that of the protosalts. An aqueous solution of perchloride of iron containing 40 per cent. of salt, exerts a negative action upon polarized light six or seven times greater than the action of water, and nearly equal to that of Faraday's heavy glass. Ætherial and alcoholic solutions give the same results. But wood-spirit appears to be the most suitable solvent; it can take up a considerable quantity of iron-salt, remaining much more transparent than water, æther or alcohol with a similar quantity of salt. By dissolving 55 parts of perchloride of iron in 45 parts of wood-spirit, a liquid is obtained, which, from its transparency, is adapted for exact observations, and the action of which upon polarized light is nearly double that of heavy glass, but in the opposite direction. The author made use of this liquid to see whether the magnetic rotatory power of the salts of iron varied according to the same laws as that of ordinary transparent substances. He compared the rotation produced by 1 centim. of the solution, with the opposite rotation of the same thickness of sulphuret of carbon, and caused the amount of the rotation to vary, by altering the intensity of the electro-magnet, the size and form of the armatures, or their distance from the transparent substance. The proportion of the two rotations was always the same, so that the negative rotation produced by the salts of iron varies in accordance with the same laws as the positive rotation caused by transparent substances in general.

From the experiments of Plücker and Faraday, it is known that the ferrocyanide of potassium is diamagnetic, and the ferridcyanide slightly magnetic. The author found that the rotatory power of the ferrocyanide is positive and pretty considerable; that of the ferridcyanide negative and very great. 15 parts of ferridcyanide, dissolved in 85 parts of water, furnish a liquid the rotatory power of which is twice as great as that of water.

Nickel.—All the salts of nickel have a positive rotatory power, so that their solutions exert a greater action upon polarized light than that of the water which they contain. This positive rotatory power is well marked, and comparable to that of the salts of zinc and tin.

Cobalt.—The magnetic rotatory power of the salts of cobalt is positive, but weaker than that of the salts of nickel. It is rather difficult to be shown, as no salt of cobalt can be dissolved in considerable quantity in water without diminishing the transparency of the liquid.

Manganese.—The protosalts of manganese possess a positive

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and anhydrous sulphate, the phænomena observed could be represented numerically, by attributing to the water and the anhydrous sulphate contrary actions proportionate to the density which these two bodies possess in the solution. On the other hand, this was impossible, supposing the solutions to contain water and crystallized sulphate of iron with 7 atoms of water. Hence it appears probable that the dissolved salt is not the crystallized, but the anhydrous sulphate; and the same kind of experiment may perhaps be applied to the solution of some analogous chemical questions.

rotatory power, comparable to that of the salts of cobalt; the salts of the sesquioxide possess too much colouring power to be adapted for these experiments. Nothing, however, is easier than to prove the magnetism of the three preceding metals and their salts.

Chromium.—The protosalts of chromium are difficult to prepare; those of the sesquioxide have so much colouring power that they cannot be dissolved in very small quantity in water without destroying all its transparency; but chromic acid and some chromates are very convenient for experiments. Neutral chromate of potash has a weak *negative* rotatory power, which, however, it is impossible to mistake; the rotatory power of the bichromate is also negative, and greater than that of the neutral salt; chromic acid has a negative rotatory power comparable to that of the protosalts of iron. Chromic acid and bichromate of potash are magnetic, whilst the neutral chromate is diamagnetic. On comparing this circumstance with the observations relative to the ferrocyanide of potassium, we shall be led to conclude that the positive rotatory power of the latter is not due to its being diamagnetic, but to the physical properties of the iron being as completely masked as its chemical properties in this compound*.

Titanium.—Bichloride of titanium, which is liquid at ordinary temperatures, has a *negative* magnetic rotatory power, a little superior in absolute value to the magnetic rotatory power of water. The author could not ascertain with certainty whether it is magnetic or diamagnetic; pure titanium is magnetic. Titanium is generally regarded as allied to tin, and the bichloride in particular as analogous to bichloride of tin. It is remarkable, that, under the influence of magnetism these two bodies exert contrary actions upon polarized light.

Cerium.—A concentrated solution of sulphate of cerium and a solution of chloride of cerium, appeared to possess a magnetic rotatory power a little less than that of water. It is therefore probable that the rotatory power of the salts of cerium is negative. The magnetism of these salts is as evident as that of the salts of chromium or manganese.

Thus, by the properties which they communicate to their transparent compounds, the magnetic metals are divided into two series, —one containing iron, chromium, titanium, and probably cerium, the other including nickel, cobalt and manganese. It is worthy of remark, that the most strongly magnetic metals, iron and nickel, are the types of these two series, and that the less magnetic metals, as it were, establish the transitions.—*Comptes Rendus*, June 8, 1857, p. 1209.

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* In studying the optical properties of the strongly-coloured solutions furnished by the persalts of iron, the salts of nickel, cobalt and chromium, and the chromates, it is essential to take into account the influence of the coloration upon the position of the tint of passage. Serious errors will be committed if the necessary correction be neglected.

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[FOURTH SERIES.]

AUGUST 1857.

IX. *Researches in Statical Electricity.* *By* Sir W. Snow Harris, *F.R.S.*[*]

[With a Plate.]

No. I.

1. Phænomena of an electrified hollow globe, and the general nature of electrical charge.
2. On the indications of the proof-plane.
3. On Coulomb's experiment with a hollow globe and circular plate of twice the diameter of the globe.
4. Empirical expression representing the electrical charge of insulated conductors.

1. IN my paper on some elementary laws of electricity, honoured by a place in the Transactions of the Royal Society for 1836, I ventured to call attention to certain phænomena of the proof-plane commonly employed to determine the electrical distribution in different points of a charged conductor. This question has recently again engaged my attention; I have been hence led to further investigate the general nature and operation of statical electrical force. In the course of my several inquiries certain facts have presented themselves, calculated, as it appears to me, to materially affect our views of electrical action. In order, however, to make these inquiries, as submitted in this present communication, clear and intelligible, it is desirable for me to briefly treat the question, as it were, *ab initio* in all its general elementary detail, so as to complete that continuous chain of reasoning requisite to the full development of every sound philosophical inquiry,—a privilege which will, I trust, be freely granted me.

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[*] Communicated by the Author.

2. I omit for the present all especial account of the several processes and electrical instruments of research employed in these inquiries; they are for the most part such as I have described in former papers in the Royal Society's Transactions, viz. the Hydrostatic Electrometer, the Scale-beam Electrometer, Unit-jar, Bifilar or Torsion Balance, Quantity-jar, &c.* I will merely observe, that I have, since my first announcement of these instruments, greatly improved them, and carried processes of quantitative measurement to a remarkable degree of precision, so that given and measured quantities of electricity may be deposited with perfect certainty on insulated conducting surfaces, the intensity or reactive force accurately deduced under a great variety of circumstances, of form, extent of surface, variation of distance, or any other element essential to the inquiry. I have further given most especial attention to the perfection of the several insulations upon which the accuracy of important deductions mainly depends. Wherever it is admissible, the conducting bodies are insulated by suspension filaments of strong silk-gut, carefully varnished with a solution of shell-lac or naphtha, and sustained by varnished glass supports, so that an extremely small surface is exposed in such kind of insulation. The glass rods and other solid insulators employed were also carefully varnished, and as slender as the nature of the experiment would admit, and were made perfectly dry when employed, by the dry heat of a curved iron heated to redness. By these means, and with attention to the air of the room, I have been enabled to retain the quantity-jar and needle of the balance in a charged state for two and sometimes three days together, and that, too, without much deterioration of charge. Suspended insulated conductors of some considerable extent have been observed to perfectly retain the quantity of electricity communicated to them, and far exceeding the time requisite for the experiment under examination; so that all calculation of loss of charge by atmospheric influence, and which is commonly a very precarious matter, became altogether eliminated in the inquiry.

3. *Preliminary views.*—If an insulated neutral conductor, N, Plate I. fig. 1, be immediately opposed to an insulated charged conductor P, then, as is well known, a peculiar and very extraordinary species of action ensues,—an action apparently of a sympathetic kind, and which at first seems to be an action exerted between the two bodies at a distance. The result of this action, whatever it be, is to change the actually existing electrical condition of the two bodies; *e. g.* the neutral body N, without any direct communication of electricity, exhibits a state of electrical excitation and becomes attractive of surrounding matter,

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* Transactions of the Royal Society for 1834, 1836, 1839.

whilst the existing attractive force of the charged body P on surrounding matter is sensibly diminished. The electrical state thus induced in the neutral body N, is a peculiar state: its near face *n*, immediately presented to the charged body P, assumes a condition of electrical force directly opposed to that of P, whilst its remote face, *p*, exhibits the same kind of force. On the other hand, the near face of the charged body P immediately opposed to N, although its electrical state is still the same, exhibits a very considerable increase of charge; and this is attended by such a diminution of charge in its remote face *r*, that this face *r* will, under some circumstances, not only approach neutrality, but it may actually pass that limit. The subsequent or actually existing states of these two bodies, therefore, instead of being as represented in fig. 1, may be better conceived by the diagram, fig. 2. This peculiar electrical disturbance, both in charged or neutral bodies, has been designated not unaptly by the term electrical induction. Conceiving that both these disturbances were present in every case of electrical attractive force, and that it was from these disturbances the subsequent force of attraction resulted, I was led, so long since as the year 1839, to consider the action of the charged on the neutral body, fig. 1, as a primary or direct inductive force, and the action of the now changed neutral body, fig. 2, upon the charged body, as a sort of secondary or reflected inductive force*, two terms which have since been fully recognized and employed by many cultivators of this branch of science.

4. Both the single- and double-fluid hypotheses of electricity have been employed in explanation of this induced electrical disturbance in bodies, and not altogether without success. The theory of two electrical fluids supposes a disunion of the combined electrical elements, and a greater or less accumulation of them in opposite parts of the given conductors. The single-fluid theory supposes an actual displacement of electricity in each of the opposed bodies, and a disturbed or new distribution of it. Although it is far from probable that either of these hypotheses is a true explanation, yet either may be employed in the way of elucidation, and to facilitate our comprehension of the kind of action we have to deal with. I adopt here the latter hypothesis, as being of a simple character, well calculated to represent the phænomena.

5. Assuming, then, the presence of a peculiar agency everywhere existing in the matter of our globe, and termed electricity, and supposing it to be of such a nature as to tend always to an equilibrium of distribution, then when a charged body, P, fig. 1, which we will assume to be charged positively, is directly opposed to a neutral body N, the tendency of the superabundant elec-

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* Transactions of the Royal Society for 1839.

tricity of P being to an equilibrium of distribution, a portion of the electricity proper to N, and which is adjacent to the charged body P, begins, as it were, to recede toward its remote parts, so as to provide for the reception of as much of the charge accumulated on P, as the near extremity of P is prepared to throw off upon it; a state of things already illustrated in fig. 2, in which there is an increased accumulation, *q*, in charged body, P, and a displacement, *n*, in neutral body, N. Supposing no impediment to motion, the bodies P and N are apparently attracted; they move toward each other up to contact, the charge becomes shared between them, and N is said to have taken up electricity from P. I must again repeat, that I do not employ this hypothesis further than as a means of elucidation and perspicuity; other modes of explanation may be equally admissible; but whatever hypothesis we resort to, we have virtually the same elements to deal with: it is the form only of representation to the mind which we change.

6. This being understood, it is very important to observe, that a charged body P, fig. 1, cannot possibly throw off any of its charge upon a second neutral body N, unless the neutral body N can assume this new and preparatory induced state represented in fig. 2: if from any disturbing cause that is not possible, then no attractive force ensues, and no electricity is communicated. Hence it is we find a much greater force of attraction between a charged and a neutral body susceptible of free inductive change, than between a charged and neutral body less open to such change. If the neutral body be connected with the earth, then the attractive force is, under the existing conditions, the greatest possible, since the electricity of the neutral body N, fig. 2, has then unlimited room to recede, and the induced change in the face *n* nearest the charged body as complete as it can be. The few following experiments are instructive and important.

Exp. 1. Let an extremely thin circular disc of gilded wood, *n*, fig. 3, be suspended by an insulating thread from one arm of a delicate balance B, and duly counterpoised by weights placed in the opposite scale-pan. Place an insulated and similar gilded disc *p* immediately under *n*, and proceed to communicate to *p* a given electrical charge: little or no attractive force will ensue, even although the distance between the two discs be very considerably diminished; indeed, under any circumstances the force will be small; there is, in fact, little or no capacity in the opposed faces *p n* of the discs for inductive change (5). If the disc *n* be a thin lamina of varnished talc little susceptible of induction, the force by this test is inappreciable.

Exp. 2. Increase the thickness of the two discs. Let the fixed disc *p*, fig. 4, for example, be supported on a sphere *sp* of some magnitude, and a few light cylindrical rings, or a projecting gilded reed *n r*, be added to the remote face *n* of the suspended disc *n*, so as to allow the electricity of the near face of *n* to recede, and a determination of the charge toward the face *p* of the charged disc *p* to ensue, then considerable attractive force follows; if, instead of suspending *n* by an insulating thread, we suspend it by a metallic thread, and connect *p* with a charged surface of great extension, then the attractive force is a maximum.

Exp. 3. Touch a charged surface with a small disc consisting of a thin lamina of varnished talc attached to a slender insulating rod of glass; it takes up no charge, or at least so little that it often fails to affect a delicate electroscope.

Exp. 4. Substitute an extremely thin conducting disc for the talc: similar results will ensue; the quantity of electricity brought away will be often inappreciable, as the state of the atmosphere and other insulations become more or less perfect. Increase the thickness of the conducting disc: the quantity of electricity brought away will also increase, and up to a limit at which we give the touching face of the disc all the capacity for inductive change of which it is susceptible.

These facts have an important application to the phænomena of statical electricity, and cannot be too forcibly insisted on: they may be easily and substantially verified experimentally.

7. It is apparent from the experiments of Cavendish, Saussure, Volta, Coulomb, and other celebrated philosophers, that if a hollow metal globe be insulated and charged (as it is termed) with electricity, all the charge will be found on its exterior surface; so that a small carrier-ball perfectly insulated on a slender support, takes up no charge after being introduced into the interior of the globe and placed in contact with its inner surface. There is no doubt that the charge is determined toward the exterior surface of the globe. A great variety of experimental evidence leads to this conclusion. Still the failure of an insulated carrier-ball to take up electricity from the interior surface of the globe is, taken abstractedly, by no means conclusive of the fact: it is quite possible for the whole interior of the globe to bristle, as it were, with electricity, and yet the carrier-ball fail to become charged in the slightest degree. This fact is of such great elementary importance, that I feel myself justified in calling attention to former experiments on this point, and which I have greatly extended and perfected.

Exp. 5. Let *a b*, fig. 5, be a hollow globe of glass of about 5 inches diameter, having a short neck at *a*, carefully varnished, and exposing an opening about an inch in diameter. Let this globe be filled with dry mercury up to the neck *a*, and be placed in an outer glass vessel A D, also filled with mercury; we have then all the conditions requisite for imparting a charge to the interior of the glass. For this purpose let a light insulated charging rod, *c b*, surmounted by an electrometer of repulsion, *m*, be introduced within the globe, and the mercury in the exterior vessel be connected with the ground by a metallic communication, *a t*. Let this system be charged in the usual way: when charged to any degree of intensity, as shown by the electrometer, remove the communication with the earth *a t*, and also the charging rod *c b*, by means of its insulating support *k*; run off the mercury first from the outer vessel A D, and then from the interior of the globe by means of a glass siphon, and place the now empty globe on an insulator, as represented, fig. 6. We may be now assured, on the faith of Franklin's celebrated experiment of the electrical jar with moveable coatings, that all the interior surface of the globe is covered with electricity. Introduce now a small insulated carrier-ball *b* into this charged globe so as to touch the interior electrified glass, and again withdraw it. The carrier-ball comes away quite neutral, as in the case of the hollow metal globe, notwithstanding that it has been actually brought into contact with a dense stratum of electricity, the presence of which may be made evident by simply attaching the carrier-ball to the lower point *b* of the insulated charging rod *b c*, fig. 5, and introducing it as before. The electrometer *m* will, if delicately hung, be immediately affected; or otherwise the charge may be shown by the medium of an ordinary gold leaf.

8. That the failure of the carrier-ball to take up electricity is in no degree dependent on the circumstance of what may be considered in the light of electrical accumulation on an insulating surface as distinguishable from the case of the hollow metal sphere, may be clearly shown by charging a plane glass surface having moveable coatings and treating the plane charged glass in a similar way. In this case electricity is freely taken up by the carrier-ball from the charged side: it is hence evident that the globular form of the surface is the immediate cause of the failure of the carrier-ball to take up electricity from the glass. The carrier-ball, in fact, cannot take on that induced electrical state (3) requisite to its reception of free electricity; the forces operating on it being in contrary directions, its natural electricity cannot recede from any point of its surface, hence all induced change in the distribution of its own proper electricity, *n p*, fig. 2, is impossible.

This experiment with charged glass may be effectively managed by employing water instead of mercury; or we may envelope the outer surface of the globe in tinfoil and electrify the glass internally by means of a point connected with the electrical machine and projecting within the globe.

9. That induction would go on within the globe were it free to do so, may be exemplified by one or two striking illustrations.

Exp. 6. Let a charged hollow globe of metal or glass, fig. 7, be placed on an insulating support as before, fig. 6; introduce within it the small insulated carrier-ball *b*, and whilst within the globe touch the carrier-ball with a light insulated wire, *c b*, projecting freely into the air; remove this wire and then withdraw the carrier-ball; the carrier-ball will be found charged with electricity opposite to that of the globe; if the globe be plus, the carrier-ball will be minus, as might be expected. In this state introduce the carrier-ball again within the globe, and so as to touch the interior surface; it comes away now quite neutral, that is to say, it has taken up positive electricity from the interior surface, either by immediate contact with the electrical particles in the case of a charged globe of glass, or through the medium of the metal surface in the case of a charged metal globe.

The same result ensues if we touch the carrier-ball with the free wire when in contact with the interior surface, provided both be raised together off the surface previously to withdrawal of the wire, otherwise the carrier-ball immediately takes up positive electricity and comes away neutral. The insulated free wire, however, comes away positively charged with the electricity which had retired from the carrier-ball.

If both the touching wire *c b*, fig. 7, and carrier-ball *b*, be raised together and removed without the globe, then the whole evinces positive electricity; for the carrier-ball, whilst in contact with the interior of the sphere, having first become negative, immediately takes up positive electricity from the charged sphere and becomes neutral, and probably remains so whilst in contact with the sphere; whilst the exterior ball *c* of the touching wire evinces positive electricity, being necessarily charged with the electricity superinduced upon it by the first induction (3), fig. 2. On raising the whole system out of the sphere, however, this superinduced electricity expands over the whole; for the original conditions are restored, whilst the new electricity taken up remains; hence the carrier-ball will now evince positive electricity. And this is really what happens when an insulated wire and ball are introduced within the globe of sufficient length to project into the air.

10. Although the two cases of charged globes to which I have thus called attention may at first appear different, the one being a case of a hollow globe of metal, the other of glass, yet a very little reflection will show that both cases are virtually the same

thing; the difference is a difference of degree, not of kind; they are, in fact, both reducible to the elements of the electrical or Leyden jar. Indeed, every case of what is commonly called a charged conductor, resolves itself into the form of a coated electric, and is the result of a peculiar disposition and combination of electrics and conductors; it is, in fact, the accumulation of electricity upon the terminating strata of a dialectric medium bounded by, and in direct contact with, conducting matter, either near or distant. Thus, in the case of what has been termed a charged hollow metallic globe, in which all the charge is conceived to be impelled, as it were, by the repulsive force of its particles from a centre of force, and so find its way to the surface of the metal, we find on an attentive examination the following arrangement of conductors and electrics, fig. 8, and into which every case of electrical charge may be finally resolved. We have, first, a metallic surface P; secondly, exterior to this an insulating medium *a* A, viz. atmospheric air, in a stratum of which, *a*, immediately surrounding the globe, there is a dense electrical accumulation; thirdly, beyond this stratum we have in continuation other air, A, not so immediately electrified, and susceptible of further inductive change: the external air is in its turn bounded by other conducting matter, N. When, therefore, we impart free electricity to the hollow sphere P, we do nothing more than cause an electrical accumulation to ensue upon the stratum *a*, according to the well-established principles of the Leyden experiment. We do not, in fact, charge the sphere at all, any more than we charge the coating of an electrical jar: indeed it is doubtful if in any case we could charge a metallic conductor taken apart from, or in the absence of, a dialectric boundary. The globe itself can be regarded in no other light than that of the inner coating of a given dialectric bounded by distant conducting matter, and which we may consider as the opposed coating; the metal of the globe is merely the conductor to the charge. The inductive action upon which the charge depends, may be shown to extend to great distances. Cavendish traced it from the centre to the walls of a room 16 feet in diameter*. Faraday traced it from a ball suspended in the middle of a room to the walls, 26 feet distant†.

If we examine the experiment of the charged glass globe (7), fig. 5, we find the elementary conditions precisely the same. Here we have (fig. 8) an interior coating of mercury P, then an external dialectric *a* A, which in this case is glass; finally, an outer coating of mercury N. Here, as in the preceding case, we do not charge the interior mercury, as is well known, although it may possibly, on being removed, be slightly electrified: the

* Cavendish, MS.

† Experimental Researches, 1303.

result of the operation is to cause an electrical accumulation on the interior surface of the glass. The great difference in the two cases simply consists in the more or less perfect application of the coatings to the dialectric medium upon which the charge depends. If we could suppose our hollow metal globe surrounded by a second external globe of metal, the two globes being near each other, but not anywhere touching, then we should have the two cases identical, as seen in the very ingenious apparatus employed by Faraday (Experimental Researches, 1195). If, further, we imagine that, subsequently to the charging of this system, both the globes were removed, as in the case of the mercury coatings, Exp. 5, and the intermediate air to remain, as it were, fixed and immoveable, then would be developed upon the boundaries of this stratum all the phænomena of the hollow glass globe above described; the charge would remain with the air. If, in the case of the charged glass globe, we allowed the interior metallic coating to remain under the form of thin metallic leaf attached to the glass, then the final experimental conditions would be identical. As it is, we operate upon the charge in the case of the hollow metal sphere, through the medium of the coating; in the case of the glass globe, we come into contact more immediately with the charge itself.

11. The theoretical view, therefore, of the celebrated experiment of what has been termed a charged hollow sphere, and which appears the best adapted to explain the phænomena, is that of charged electrics generally. The free electricity first communicated to the inner coating, viz. the metallic sphere, operates by induction upon the nearest matter susceptible of electrical change (3), and thereby developes or calls into operation the opposite electricity. The opposite forces thus brought into life tend to combine and exhibit attractive force, and consequently come as near together as want of conducting power in the intervening restraining dialectric will permit: the imparted electricity must therefore necessarily find its way upon the exterior surface of the hollow metal globe without the aid of any kind of repulsive force to which the phænomenon has been hypothetically attributed. It is in virtue of this kind of action that we are said to charge simple insulated conductors generally. The amount of charge, however, or quantity of electricity which can be sustained by them under a given electrometer indication, can never be so great as in the case of systematically coated electrics of comparatively small thickness. The case of simple conductors is much the same thing as the case of extremely thick glass, or the limiting of the free action of one of the coatings of an electrical jar; in either case the quantity of electricity which can be accumulated under a given degree of the electrometer is greatly dimi-

nished. The experiment with an electrified hollow globe therefore appears to have been expressed in very inexact terms, and the phænomenon of the exclusive appearance of the charge upon the exterior surface somewhat misapprehended.

12. It follows from these demonstrable conditions of electrical charge, that a stratum of what may be termed electrical particles must always necessarily exist upon the surfaces of a charged conductor, as is clearly demonstrable by experiment; and the electrical agency, whatever it be, penetrates to a greater or less degree the substance of the air itself, or other dialectric medium in contact with the conducting surface, as is well shown in Faraday's most comprehensive researches (1245). This is really the acceptation of the term electrical atmosphere; a term correctly applied by the celebrated Volta, who most thoroughly comprehended the practical nature of electrical force, notwithstanding that his power of rigorous thought has been questioned, and his theoretical views of electricity rather severely remarked on by an eminent writer not altogether unbiassed by theoretical opinion, and evidently not a little impatient of dissent*. That Volta was most perfectly correct in attributing the phænomena of charged electrical conductors to the presence of electrical atmospheres surrounding them, taken in the sense in which I have just explained the term, is absolutely demonstrable by the most conclusive experiments: we remove the metal, and there remains the charged stratum; in other words, the atmosphere of electrical particles, as it may be termed, entirely without and independent of it.

13. The electrical stratum thus found to exist on electrified conductors appears firmly held to the surface by attractive force, and is inseparable from it by movement of the body. Franklin whirled a charged ball attached to a silk cord many times round in the air and with great velocity, still the ball retained its charge†. An electrified conductor, therefore, when transferred from one place to another, may be supposed to carry the electrical stratum along with it, just as the metallic coating of charged glass would do. It is true that the term electrical atmosphere has been occasionally used in a vague and unsatisfactory sense, and has hence been justly discountenanced by many eminent physicists and mathematicians. When taken, however, in the sense in which I have applied it as expressive of a demonstrable fact, the question assumes quite another form. Volta, therefore, in referring the phænomena of electrical attraction and repulsion, and the operation of electrified bodies generally on each other, to the immediate action of the electrical particles themselves held firmly on

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* *Bibliothèque Universelle,* article 'Volta.'

† If we charge a plate of glass through plane moveable coatings of gilded wood, the coatings will adhere to the glass.

the surfaces of bodies, did in no way violate sound deduction from rigorous thought, or evince in any degree a vague and imperfect apprehension of the probable nature of electrical force. When we remove the coatings of a charged electric (7), Exp. 5, something is evidently left behind—some agency or source of power. What is that something? In what does it consist? It is evidently external to the metallic surface with which the dialectric surface was previously in contact, although inseparable from it so long as the two remain combined; and it is really from this something, which we express by the term electrical stratum or atmosphere, that the phænomena we observe arise, and not from an hypothetically charged conductor*.

14. On attentively reviewing these facts, it will be quite apparent, that in any endeavour to take away or abstract electricity from a charged surface through the medium of a conducting substance applied to it, it is absolutely essential to maintain in this substance a sufficient power of inductive change. This we cannot possibly do if the body be of extremely small dimensions and thickness, and be applied to the charged conductor within the envelope of the electrical stratum immediately upon its surface; because in such a position it will approach the conditions of the carrier-ball introduced into the hollow sphere (7), Exp. 5: its inductive susceptibility, and consequently its power of abstracting electricity, must become more or less damaged; indeed it is next to certain, that a small and very completely insulated disc of metal of indefinitely small thickness may under some circumstances come away from contact with an electrified surface very nearly neutral. These are important facts as affecting the practical operation of what is termed the proof-plane, and which consists of an extremely thin and small insulated disc of metal applied to the surface of a charged conductor, often with a view of determining by the quantity of electricity abstracted, the quantity of charge disposed in different points of the surface. The received theoretical view of the proof-plane supposes, that when the disc is placed upon any point of the charged conducting surface, it actually becomes part and parcel of the surface itself, so that on removal we may be supposed to have actually abstracted an element of the surface with all the charge belonging to it. Now it seems to me extremely difficult to satisfy the mind of the truth of this view, or to be assured that a small insulated disc, how-

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* The experiments of Beccaria and Franklin with the smoke of resin and colophonia, and which they observed to collect about electrified conductors, so as to envelope them, although perhaps no very satisfactory evidence of the existence of a similar atmosphere of electricity, are still not without very considerable interest. It is not easy to explain the adherence of these atmospheres of smoke to the charged surface, admitting the theory of electrical repulsive force.

ever thin we take it, can ever be anything more than a foreign body brought to share in the electrical charge of another body, on the principles laid down (3), fig. 2; and this will, I imagine, be satisfactorily conceived by reference to the following experiments.

Exp. 7. Take a small insulated conducting cylinder, *a*, fig. 9; suppose a light cylinder of gilded wood, about ·4 of an inch in diameter and ·6 of an inch high; apply it to the central portion of a charged rectangular conducting plate P, as indicated in the figure, and then find by the torsion or bifilar balance its reactive force. Let this cylinder be now divided into two portions, or what comes to the same thing, take a cylinder of half the altitude. Find the force after being applied to the charged plate P, as before; the force will be much less. Continue to diminish the thickness until a mere disc results; the force will continually decrease with the thickness. At last, when the thickness is taken indefinitely small, then with a certain charge little or no resulting force is apparent. We may here fairly ask, What is the limit of thickness at which we may arrest our division so as to repose with confidence on the reactive force when transferred to the balance, as a measure of the quantity of electricity actually existing in the point of the electrified surface to which the disc has been applied? Theoretically, we should, in continuing the division, arrive at last at a zero of charge. Now it is notorious that if we take a series of proof-planes of variable thickness and apply them to different points of a charged surface, we get all sorts of proportions of quantity of electricity in the respective points touched, as determined by the electrical balance after the method of Coulomb; the correctness of which in determining the relative quantity of electricity operating between the repellent balls of the balance is quite unquestionable, sufficient attention being given to the manipulation.

I endeavoured to ascertain by very direct experimental processes (Exps. 8 and 9), the altitude above the charged surface at which the reactive force imparted to a proof-disc of small thickness would be everywhere the same, or nearly so, it being evident, that, applied to certain points at a distance from the centre, the disc takes up a greater charge.

Exp. 8. A light cylinder of gilded wood, *a p*, fig. 10, about ·4 of an inch in diameter and 3 inches high, being placed on a charged rectangular plate P, as indicated in the figure, the reactive force of a proof-plane, *p*, applied to the remote face of this cylinder was observed for different points of the charged surface, and found to be everywhere the same, the plate P being charged with a given quantity of electricity. At the extremity *q* the reactive force was the same, whether the proof-plane *p* were applied on the surface at the foot *a* of the cylinder *ap*, or to its distant face *p*; at the centre (fig. 9) the force was found to vary considerably between the base and altitude of the cylinder itself. The following are the numerical results:—

At centre c.

Force on surface, 7°. Force at remote face of cylinder, 33°.

At extremity q.

Force on surface, 32°. Force at remote face of cylinder, 33°.

15. *Exp.* 9. Having satisfactorily investigated this fact, I proceeded to observe the rate of increase of force at the centre as the altitude of the intervening cylinder above the surface was increased, and to find the altitude of the point at which it would become equal to the force at the extremity, viz. 32 to 33 degrees. With this view I employed a series of small cylinders of gilded wood, varying from ·1 to ·75 of an inch in altitude, and about ·4 of an inch in diameter; also other similar cylinders varying from 1 to 6 inches in altitude, so as to obtain, by superposition or otherwise, any given height above the surface required. The following are some of the resulting forces as compared with the altitude, in which it is apparent that the force up to a certain limit increases with the square root of the altitude, or is nearly in that proportion, after which it ceases to increase, and is for further altitudes nearly the same.

Results of Exp. 9.

Height above surface in inches.	·1.	·25.	·5.	·75.	1.	1·5	2.	2·5.	3.
Reactive force in degrees.	7	11	15	20	22	25	28	31	32

On employing a cylinder from 5 to 6 inches high, the force from the plane with a given charge was observed to be everywhere the same.

16. It appears, therefore, from these results, that the effect of the intervening cylinder is to raise up the point of contact, and place the tangent or proof-plane *p*, so far without the influence of the electrical stratum upon the surface as to admit of a free inductive change; the cause, therefore, of a tangent disc of small thickness becoming more highly charged from the extremities of an electrified surface than from the centre, may mainly depend upon its position in respect of the electrical stratum (12) when placed in these two relative points. I have on a former occasion called attention to this circumstance. If, for example, we place a small disc of little thickness in the centre of a charged

surface, as at *c*, fig. 10, it would suffer more obstruction to inductive change from surrounding electrical particles than when placed at the extremity *q*, where it would be less involved, as it were, in counteracting forces. If placed directly at and outside the extremity *q*, then one of its faces would be still more exposed and free; it would therefore take up more electricity in these points than at the centre *c*, in which it was less free; a result perfectly in accordance with the phænomena of electric force already exemplified (6), Exps. 1, 2, &c. By giving the touching disc, therefore, a free action of induction, either by such means as resorted to in Exp. 9, just described, or by attaching to its remote surface a light gilded reed and ball of about 6 inches in length, as represented in fig. 11, we obtain an equal reactive force when transferred to the balance, from all points of the surface to which we apply it. I have little doubt but that, employed in this way, the method of finding the relative quantity of electricity on charged surfaces employed by Coulomb is very exact. I found, for example, that in charging a rectangular plate P, fig. 10, with one, two, three, &c. measured quantities of electricity, the reactive forces from an elevated cylinder *a b*, or from a tangent plate prepared as in fig. 11, were exactly in the same ratio, at whatever point of the surface it was applied. It is clear that in the preceding Exp. 9 the several altitudes admit of being considered as proof-planes of increasing thickness: were we to trust the indications of such planes, we might be led into very unsound deductions as to the distribution of electricity upon the charged plate. Suppose the thickness of the plane had been about the ·1 of an inch, the reactive force at the centre, as compared with the force at the extremity, would have given quantities in the proportion of 7 : 33, being nearly as 1 : 5. If we take the thickness ·25, then the proportion would have come out in the ratio of 11 : 33, or as 1 : 3; if ·5, it would have been as 15 : 33, or as 1 : 2 nearly; so that, as before observed (13), Exp. 7, it would be difficult to say at what limit we may arrest our measure in respect of a decrease of the thickness of the proof-plane, although by no means difficult to find the limit for its free inductive susceptibility in the opposite direction, as just shown. There are many important phænomena of electrical charge involved in these considerations, and to which the proof-plane, as usually employed, would certainly ill apply. If, for example, we impart the same quantity of electricity to a square, a circular plate, and a rectangular plane of equal area greatly extended as to length, then, as shown by Volta, the intensity of charge is greatly diminished in the case of the long plane, and we could dispose upon it a much greater quantity of electricity under the same degree of the electrometer. Yet a proof-plane applied to these surfaces in the usual way might actually evince a greater reactive force when applied to the rectangle, although the surface and quantity of electricity were actually the same as in the square or circular plate; the electrical stratum on the rectangle being less condensed, as it were, upon any given point of its surface. Now a touching body, prepared as in fig. 11, so as to be susceptible of a free induction, will, when transferred to the balance, at once show the difference of intensity in the two surfaces, and although not in this case affording a correct measure of the relative quantity of electricity, may under another form of the experiment be made available for that purpose.

17. It is to be here considered how far the presence of a cylindrical conductor *a p*, fig. 10, elevated above the charged plane, is calculated to disturb the original distribution, and change its quantitative condition upon different points of the surface. That a portion of the electrical accumulation becomes transferred to the cylinder is certain; but the quantity is not in proportion to its extent of surface, or such as to interfere in any appreciable degree with the original distribution, which will be found to remain precisely the same whether the cylinder *a p*, fig. 10, be present or not. The cylinder, in fact, may be considered to become charged in great degree by a new distribution of its own electricity, and to become affected in pretty much the same way as the carrier-ball and wire introduced into the hollow sphere (9), Exp. 6, fig. 7, that is to say, a portion of the electricity of the cylinder itself, in the face next the charged surface, first becomes displaced by induction (3), fig. 2, and retires towards its remote or distant face *p*, fig. 10. This preparatory change complete, the near or lower face of the cylinder then receives or shares in the total charge in proportion to the amount of this induction; so that a new quantity of electricity proper to the cylinder itself is thus called into action and becomes sensible at its remote portions, and the quantity of charge taken by the cylinder is limited in extent to the element of the surface it actually covers. Now the influence of this upon the general and original state of the distribution is quite inappreciable, as may be easily shown by taking the reactive force at any given point with and without the presence of the cylinder, when the force will be found precisely the same.

The method, then, of determining the reactive force generally of the charged plate P, or of any point of it, through the medium of an intervening elevated conductor of a given form and dimensions, appears to be the best and most accurate process which can be possibly employed, inasmuch as the electrical development at its distant face *p*, fig. 10, will always be in the direct proportion of the electrical force of the element of the surface which it actually covers.

18. These facts being duly considered, it appears by no means improbable that the distribution of an electrical charge through the medium of metallic surfaces, considered as conductors merely to the charge (10), may, upon the whole, be uniform, or nearly so, and that the deduction upon the evidence of the proof-plane, that it is far otherwise, is at all events questionable. According to the action of the proof-plane, the electrical charge accumulated upon a long rectangular plane R, fig. 12, would be represented by a curved line, such as *a* R *b*, a large portion of the charge being accumulated at the extremities. It is, however, just possible, on the other hand, from the phænomena observed, (14), (15), that the charge may be at least thicker at the centre, and admit of being represented by another kind of curve, such as *d c e**. If, however, we take the evidence of a simple and direct experiment, we should be led to conclude that the stratum of electrical charge upon the surface of electrified conducting bodies was uniformly distributed throughout. The following are a few striking and new experiments which greatly favour this conclusion, or are at least in accordance with it.

Exp. 10. R, fig. 13, is a rectangular plate of copper, about 4 feet in length and 3 inches in width, carefully insulated in the way already described (2). The suspended or moveable disc *n*, of the hydrostatic electrometer E, is brought immediately over any point of the surface. The base of the vertical column D, sustaining the wheelwork, &c., is set on a travelling carriage or rail so as to admit of the whole being easily moved backward or forward, and the suspended disc *n* of the electrometer easily transferred immediately over any required point. Let R be now charged with any given measured quantity of electricity, so as to bring the index *t* by the attraction of the disc *n* to a given point, say 10 degrees; the disc *n* being at a given distance above the plane, say an inch. Let the attracted disc be now gradually moved along over the surface of the plane from one extremity to the other; not any appreciable change will ensue in the position of the index; the force on the disc will be everywhere the same,—a result perfectly consistent with the hypothesis of uniform distribution. Any simple apparatus may be employed for this experiment, such as a common balance so poised as to admit of a small inclination without oversetting; or a vertical arm of light reed with pith-balls set on a delicate central axis, as shown in fig. 14, in which *c* is an insulated metallic cylinder,

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* It is worthy of remark, that in Beccaria's experiments of the collection of the smoke of colophonia upon an electrified cube, the smoke "lay higher on the flat parts of the cube than on the edges and corners."

4 feet in length and 1 inch in diameter, and E a delicate electrometer of the form just adverted to. The cylinder being charged with a given measured quantity of electricity, the arm of the electrometer inclines toward it a given number of degrees. Let this instrument, affected by a given inclination, be now carefully moved along against a guide-rail upon a base B: no change whatever will ensue in the degree of inclination of the arm. The charged cylinder exerts upon it the same force throughout.

Exp. 11. The cylinder *c*, fig. 14, being suspended as above described, I endeavoured by means of an electroscope of repulsion, *m q*, to discover whether any change would ensue in the divergence of the balls in changing their position along the charged surface. This electroscope was constructed and applied in the following way:—A slender varnished rod of glass, *c m*, fig. 15, attached to a small hook *m*, is cemented into a light cross arm *a b* of gilded wood, about an inch long, being equal to the diameter of the cylinder *c*, fig. 14; *b c* and *a d* are two light reeds terminating in balls of pith hung at each extremity of the crosspiece *a b*. The jointed hinges are made of very fine platinum wire turned into rings, and hang one within the other after the method of Cavallo. This construction gives to the reeds and balls an excessive freedom of motion, and is incomparably the best method of suspension for such electroscopes I have as yet met with. This electroscope is hung by means of the hooked ring *m* upon a small line of silk *a b*, fig. 14, stretched between the wooden balls terminating the glass arms supporting the insulated cylinder *c*. When the cylinder is electrified, the balls necessarily diverge and stand out freely from it on each side, as shown in the figure. Let this electroscope thus divergent be now gently and carefully moved along upon the line *a b* by means of a long slender rod of glass applied against the ring *m*: no appreciable change is observed in the divergence of the reeds; it is everywhere the same. If the divergent reeds be placed beyond either of the extremities of the cylinder, then they are apparently thrown outwards as well as sideways, as would necessarily be the case, but up to the extremity of the cylinder on either side I have never yet been enabled to detect any change such as represented in fig. 12; and that, whether by the movement of the electroscope, or by testing the divergence from different points under the same charge.

19. An interesting question here arises as to how far the divergence of the balls is really the result of repulsive force, properly so called. Whether, as maintained by Lord Stanhope and many other eminent electricians, the reeds and balls are attracted, as it were, into the air rather than absolutely repulsed, much in the same way that we observe an excited silk ribbon

attracted toward the wall of a room; the reeds evidently seek to diverge beyond the limit of the dense electrical stratum on the surface of the cylinder; and hence in charging the cylinder with one, two, three, &c. measured quantities of electricity, by which the thickness of the stratum may be supposed to be continually increased, we observe the distance of divergence become also increased. Beyond the extremity of the cylinder the balls would be attracted into the air with increased force in two directions, viz. from the extremity and from the side of the cylinder; hence they would appear as if repulsed sideways and outwards.

These experimental facts, as it appears to me, are quite consistent with a uniform thickness of stratum or distribution of electricity upon the surface of a charged conductor.

20. The distance to which the attractive force of a charged surface extends will be directly as the quantity of electricity accumulated, as may be thus experimentally demonstrated.

Exp. 12. Let the disc *n*, fig. 13, of the electrometer E be placed at a given distance above any point *p* of the rectangular plate R, suppose ·5 of an inch. Charge the plate R with a given quantity of electricity, and observe the force at the given distance, ·5 of an inch; let it be, for example, 16 degrees of the index at this distance. Let the distance be now increased; let it, for example, be made twice as great, or equal 1 inch; then the index will only show 4 degrees of force: the force will, in fact, be as the squares of the distances inversely. Under these circumstances, double the charge of the plate R; the force indicated will be again 16 degrees, that is to say, it will be as the square of the charge. Hence the distance to which an equal attractive force has extended from an area on the surface of the plate equal to that of the disc *n*, is as the quantity of electricity accumulated, that is, as the charge directly.

Exp. 13. Place the electrometer E, fig. 14, at a given distance opposite any point of the cylinder *c*; charge the cylinder with a given measure of electricity, and note the degree of inclination of the arm of the instrument. Let the distance of the ball of the electrometer from the cylinder be now made twice as great, and the charge on the cylinder doubled; the inclination of the arm will remain the same.

I endeavoured to find in this way the limits of distance at which the force was just sensible to very delicate electroscopes, and found them to be as the quantity of charge in the cylinder. Thus a given quantity being accumulated, the electroscope E (the sensibility of which could be very greatly increased by changing the position of a small piece of reed moveable on the lower arm, and by which it is poised) was placed at such a measured distance from the cylinder as admitted only of an extremely small movement, just as much as would indicate force. This distance being now made twice as great, the arm was observed to be similarly affected when the quantity of charge was doubled. These results accurately correspond with the law of striking distance in Lane's discharging electrometer, as I have before shown in my paper in the Royal Society's Transactions for 1834, p. 227. Since double, treble, &c. quantities of electricity exert attractive forces as the squares of these respective quantities, whilst, on the other hand, the forces decrease inversely as the squares of the distances between the striking points, we have necessarily the same striking force at all distances if we make the quantity of charge increase with the distance. Hence a charge which could strike over a distance of ·5 of an inch would, on being doubled, strike over twice that distance, or 1 inch; that is to say, the force at that distance would be the same as seen in the experiments 11, 12, 13, &c. just described*.

21. In the case of a conducting surface terminating in an acute angle or point, we may still have an equal distribution of the charge, the only difference being a more free action of the charge upon the angular portion. Thus in fig. 16, let P be an insulated conductor charged with electricity, and suppose it to be transformed into a pointed conductor, P*p*, by removing the triangular portions *a c*; then, as is evident, all the electrical stratum which formerly occupied those portions *a c* would become removed, and there would be less pressure or impediment, as it were, to the free inductive action of external matter on a given point *p* than there would be if the point were enveloped in electrical particles. The force, therefore, upon the principles already explained (15), would extend to a greater distance, and the striking distance of a point so circumstanced in respect of the electrical stratum greatly extended. The charge therefore would run off more freely, or be more freely received by a pointed conductor than by a point enveloped in surrounding electrical particles.

22. The result of experiment 9, and the deductions (15) and (16), enable us to operate upon metallic conductors in communication with the fixed disc of the electrical balance, or hydrostatic or other electrometers, without any care for the position

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* M. De la Rive, in his late comprehensive work on electricity, appears to consider this law of striking distance, as compared with the law of intensity, somewhat extraordinary. He says, "Ce qu'il y a d'assez remarquable, c'est que la distance à laquelle une décharge entre deux balles chargées d'électricités contraires peut avoir lieu, est simplement proportionelle aux quantités d'électricité, tandis que les forces attractives sont proportionelles aux carrés de ces forces." (Vol. i. p. 66.) It will be seen, however, that the above considerations fully explain the fact, and show it to be a necessary result of the laws of electrical force.

of such conductors or the point of communication. Thus if P, fig. 16, be a rectangular plate connected with the fixed disc *p* of the balance by means of a slight wire, P*p*, passing through a hole in the glass of the cage, the divergence of the needle *n* will be precisely the same when charged with the same quantity of electricity, in whatever position the plate P be placed, or with whatever point of it the wire P*p* communicate. It will be, for example, just the same whether we place P transversely and centrally as at P, or in the direction of its length as P′, the communicating wire being connected with either of its extremities.

[To be continued.]

X. *Chemical Notices from Foreign Journals.*
By E. Atkinson, *Ph.D.*

[Continued from p. 59.]

ON the older view of the constitution of fulminic acid it was considered to be bibasic, and polymeric with cyanic acid. But this view did not sufficiently well account for some of its properties; and Gerhardt, in the first edition of his 'Organic Chemistry,' held, that from the explosive properties of the fulminates their nitrogen was contained, not as cyanogen, but partially as NO^4, and he gave to fulminic acid the formula $C^4 N (NO^4) H^2$. The constitution of this acid has lately been the subject of separate investigation at the hands of Schischkoff* and Kekulé*, who have arrived at results which are very similar, and include Gerhardt's supposition.

From the highly explosive nature of the fulminates, and from the fact that cyanogen compounds are constantly formed in their various decompositions, Kekulé held that one half of their nitrogen was in the form of NO^4, and the other half as cyanogen. It follows from this, that the other half must be present in another form, and would constitute with the remaining constituents a compound belonging to the methyle group. The formula of fulminating mercury would then be $C^2 (NO^4) (C^2 N) Hg^2$, and would exhibit in its composition the greatest analogy with a large class of compounds; for instance, C^2 H, Cl, Cl, Cl, chloroform; $C^2 (NO^4)$ Cl, Cl, Cl, chloropicrine; C^2 H H H C^2 N, acetonitrile (cyanide of methyle). Fulminating mercury would stand nearest to chloropicrine and acetonitrile. Like the former, it would be a nitro-compound, and like the latter, a cyanogen compound. It might be a nitroacetonitrile, whose two hydrogen atoms are replaced by mercury, and hypothetical fulminic acid would be a nitroacetonitrile, C^2 HH (NO^4) C^2 N.

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* Liebig's *Annalen*, February 1857.

In accordance with this interpretation, it was to be expected that fulminating mercury, when treated with chlorine, would yield chloride of cyanogen and chloropicrine; and experiment showed this to be the case. Kekulé obtained as products of this reaction gaseous chloride of cyanogen, and an oil which had all the properties of chloropicrine, but was probably contaminated with chloride of carbon. In this reaction no carbonic acid was formed, and the equation would thus be,—

$$\underbrace{C^2(NO^4)(C^2N)Hg^2}_{\text{Fulminating mercury.}}+6Cl=\underbrace{C^2(NO^4)Cl^3}_{\text{Chloropicrine.}}+\underbrace{CyCl}_{\text{Chloride of cyanogen.}}+2HgCl.$$

By distilling fulminating mercury with hypochlorite of lime, pure chloropicrine is obtained. By treating fulminating mercury with sulphuretted hydrogen, Kekulé obtained sulphocyanide of ammonium and carbonic acid, the occurrence of which in this reaction had hitherto been unnoticed. The reaction did not, however, give concordant results on analysis. The previous explanations given of this reaction are not quite correct: it is rather to be assumed that the free fulminic acid decomposes with sulphuretted hydrogen at the moment of its formation into sulphocyanide of ammonium and free carbonic acid,

$$\underbrace{C^2(NO^4)CyH^2}_{\text{Fulminic acid.}}+2HS=2CO^2+\underbrace{CyNH^4S^2}_{\text{Sulphocyanide of ammonium.}}.$$

Schischkoff derives fulminic acid from the type biuret, to which he gives the rational formula $2(C^2O^2NH)NH^3$. If the hydrogen of the ammonia be replaced by the bibasic radical $C^4H^2(NO^4)$, we get the formula of fulminic acid, $2(C^2O^2NH).N(C^4H^2NO^4)$. In an analogous manner fulminuric* acid is derivable from the type urea $(C^2O^2NH)NH^3$, in which the hydrogen of the ammonia is replaced by the same tribasic radical, $C^4H^2(NO^4)$. If this view were correct, we ought to get from these compounds evidence of the presence of NO^4, and of the group $NC^4H^2(NO^4)$, which would be mononitroacetonitrile. Schischkoff made the experiment on fulminuric acid, which gave chloropicrine on treatment with hypochlorite of lime, as Kekulé had found was the case with fulminating mercury under the same treatment. Schischkoff found also that fulminuric acid, when treated with nitrosulphuric acid, gave a new body, which is *trinitroacetonitrile*, $NC^4(NO^4)^3$. This is a compound similar to camphor, possessing a penetrating unpleasant smell, which melts at 41°·5 and explodes at 220° C. Its formation may be thus expressed:—

$$\underbrace{(C^2O^2NH)(NC^4H^2NO^4)}_{\text{Fulminuric acid.}}+\underbrace{2NO^6H}_{\text{Nitric acid.}}=\underbrace{NC^4(NO^4)^3}_{\text{Trinitro-acetonitrile.}}+2HO+2CO^2+NH^3.$$

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* Phil. Mag. March 1856.

By the action of water and of alkalies, this substance furnishes a body $NC^4(NO^4)^2(NH^4)$, which is acetonitrile, NC^4H^3, in which two equivalents of hydrogen are replaced by $2(NO^4)$, and one of hydrogen by NH^4. By boiling this body with potash, and subsequently treating the salt thus obtained with oxide of silver, a beautiful salt is obtained, to which the author gives the formula $\left.\begin{array}{r}C^4(NO^4)^2(NH^4)\\ NH^3Ag\end{array}\right\}O^4$. This remarkable salt would thus belong to the type acetic acid, $\left.\begin{array}{r}C^4H^3\\ H\end{array}\right\}O^4$.

A new determination of the equivalent weight of antimony has been made by Dexter[*], who has obtained numbers which differ slightly from those obtained by Schneider and by Rose[†]. His determination deserves, however, great consideration, for it was made with all possible care and precision by the method originally used by Berzelius. The starting-point was the preparation of pure metallic antimony; and for the purpose of obtaining this, advantage was taken of the insolubility of metantimoniate of soda. Crystallized tartarized antimony was fused in a Hessian crucible with nitre and potash, and the fused mass poured out, allowed to cool, dissolved and filtered, and a solution of pure chloride of sodium added to it. The metantimoniate of soda precipitated was well washed out, and then converted into hydrated antimonic acid by treatment with nitric acid. The hydrated antimonic acid, well washed out and dried, was placed in a porcelain crucible lined with pure charcoal and strongly ignited. The reduced metal was not quite pure; it contained traces of sodium, for it was impossible to extract all soda from the metantimoniate by nitric acid. The metal was therefore finely powdered, mixed with some of the same antimonic acid, and again strongly ignited. The metal collected at the bottom in a regulus, covered with a slag of melted oxide of antimony. It was quite pure, as was shown by special experiments. Its specific gravity was 6·705 at a temperature of 3°·75. Scheerer for the same temperature found the specific gravity of antimony to be 6·708.

For the purpose of the determination, the metal was converted into antimonic acid by treatment with pure nitric acid, the antimonic acid ignited and weighed as antimonious acid, SbO^4. The number obtained as the mean of eleven very concordant experiments was 1529·4, or 122·3 on the hydrogen scale. This method of determination, although simple, has the disadvantage that a small error of observation has great influence on the result, and to avoid this source of inaccuracy Dexter made a great many experiments with another method. This consisted in determi-

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[*] Poggendorff's *Annalen*, April 1857.

[†] Phil. Mag. February 1857.

ning the quantity of gold which is reduced from a solution of aurochloride of gold by a known weight of pure metallic antimony. But this method gave very unsatisfactory results.

Wöhler* describes an improved method of preparing the crystallized silicon discovered by him†. It consists in fusing aluminium with five times its weight of soluble glass, and ten times its weight of cryolite. The soluble glass he uses is that made by Kuhlmann of Lille. This is powdered and mixed with the powdered cryolite; a Hessian crucible is half-filled with the mixture, and the piece of aluminium laid on it, and then filled up with the rest of the mixture. The crucible is then heated, and the mixture kept melted for about half an hour. On cooling, black reguli are obtained, quite saturated with silicon, and the surface is generally covered with 3- and 6-sided plates of silicon.

It is remarkable that these reguli cannot be remelted at the temperature at which they are formed. They can be heated to redness in any open crucible without melting and without being oxidized. If a red-hot globule be thrown into cold water, half the volume of aluminium flows out, and the silicon remains in the form of the globule as a crystalline mass filled with hollow spaces. In preparing silicon, it is better therefore to treat the regulus in this manner before treating it with hydrochloric acid; some aluminium is obtained in this way, which can be used for a fresh preparation of silicon.

The same chemist has, in conjunction with Buff‡, been engaged on some investigations of the relations of aluminium to the galvanic current. In the course of these they have discovered a gas containing silicon which is spontaneously inflammable in the air, and a new chloride of silicon with its corresponding oxide. When the galvanic current is passed through a solution of chloride of sodium, aluminium containing silicon being used as the electrodes, a gas is evolved at the positive pole which is spontaneously inflammable, and explodes when mixed with oxygen. It burns with a brilliant white light, producing white fumes of silica. If a piece of porcelain be placed in the flame, brown stains of amorphous silicon are deposited on it; and if the gas be passed through a glass tube heated to redness, a brown mirror of amorphous silicon is deposited. The gas also takes fire when mixed with chlorine. The formation of this body is very peculiar, occurring as it does at the positive pole, while there is a disengagement of hydrogen at the negative pole.

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* Liebig's *Annalen*, June 1857.

† Phil. Mag. April 1857.

‡ Liebig's *Annalen*, April 1857; and *Comptes Rendus*, June 29, 1857.

Wöhler and Buff have not yet been able to establish its quantitative composition, for it is always mixed with variable quantities of free hydrogen. It appears to contain an equal volume of hydrogen. This gas, siliciuretted hydrogen, is also obtained when aluminium containing silicon is dissolved in hydrochloric acid; but in this case it is mixed with so much free hydrogen that it is not spontaneously inflammable in the air.

On heating silicon to a dull red heat in a current of dry hydrochloric acid gas, the latter is decomposed, yielding hydrogen and a new chloride of silicon. This is a mobile fuming liquid, more volatile than ordinary chloride of silicon, and is decomposed by water into hydrochloric acid and into a white substance, which is a new oxide of silicon. The latter is slightly soluble in water, very soluble in alkalies, hydrogen being evolved, and silicic acid being formed. It is a hydrate which does not lose its water at 300° C., but heated to redness in the air, it takes fire and burns with a very white light, evolving hydrogen which inflames.

The attempts made to determine the composition of the new oxide and the new chloride have not given satisfactory results. Most of the numbers agree with the formulæ $Si^2 O^5$ and $Si^2 Cl^5$; but these appear improbable, and it is not certain that pure substances have been obtained. The corresponding bromine and iodine compounds have been obtained by the same methods. The bromide is a fuming liquid, very volatile; the iodide is a dark red solid, very fusible and volatile, which decomposes in moist air into hydriodic acid and the white oxide.

Baudrimont* has discovered a new sulphide of carbon, which stands in the same relation to carbonic oxide, CO, that bisulphide of carbon, CS^2, does to carbonic acid, CO^2. It may be formed by a variety of reactions, but the best method consists in passing the vapour of bisulphide of carbon over spongy platinum, or over pumice heated to redness. In this case CS^2 decomposes into sulphur, and into a gaseous body which is the new subsulphide. This reaction shows its formation in the most precise manner.

It is also obtained in the process of making bisulphide of carbon,—by the action of hydrogen on bisulphide of carbon at a red heat; by calcining sulphide of antimony with excess of charcoal; by the action of oxide of carbon on sulphuretted hydrogen at a high temperature, $CO + HS = HO + CS$; and by the action of sulphurous acid, or of chloride of sulphur on olefiant gas at a red heat; it is also produced in the destructive distillation of sulphocyanogen.

The first method yields it pretty pure, in the others it is mixed

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* *Comptes Rendus,* May 19 1857.

with sulphuretted hydrogen and oxide of carbon. It is purified by being rapidly passed through solution of acetate of lead, of subchloride of copper dissolved in HCl, and after being dried it is preserved over mercury.

It is gaseous, colourless, with an odour resembling ordinary sulphide of carbon, but strongly ætherial, and not at all disagreeable. It burns with a blue flame, producing carbonic acid, sulphurous acid, and a little sulphur. It is rather heavier than carbonic acid. Water dissolves its bulk of the gas, but decomposes it into sulphuretted hydrogen and carbonic oxide. With alkalies it is rapidly decomposed. With lime-water it gives sulphide of calcium, and its own volume of carbonic oxide,

$$\mathrm{CaO}+\mathrm{CS}=\mathrm{CaS}+\mathrm{CO}.$$

At a red heat it is slowly decomposed (1) by spongy platinum; (2) by the vapour of water into HS and CO; (3) by hydrogen into HS, and a carburetted hydrogen; (4) by copper into graphitoidal carbon, and sulphide of copper. Exposed to the sunlight with chlorine, a reaction takes place which gives rise to the formation of new bodies.

Exploded with oxygen in the eudiometer, it furnishes equal volumes of carbonic acid and sulphurous acid, from which, and from its decomposition with alkalies and with copper, its formula CS has been deduced.

It has been known that the cyanides of the alcohol radicals, when treated with potash, give off ammonia, and there is formed a member of the fatty acid series containing two equivalents more carbon than the alcohol radical from which it was derived. Thus cyanide of æthyle, $C^4H^5C^2N$, gives propionic acid, $C^6H^6O^4$; cyanide of amyle, $C^{10}H^{11}C^2N$, gives caproic acid, $C^{12}H^{12}O^4$. Becker* has made an interesting application of this reaction, and succeeded in producing margaric acid, $C^{34}H^{34}O^4$, by treating cyanide of cetyle, $C^{32}H^{31}C^2N$, with potash.

Cetylic alcohol was first prepared from spermaceti by known methods, and converted into iodide of cetyle by treatment with iodine and phosphorus. The conversion of iodide into cyanide of cetyle was effected by boiling the iodide of cetyle with an alcoholic solution of cyanide of potassium for several days, and the cyanide was converted into margaric acid by boiling it for some time with caustic potash. The acid thus obtained, and its baryta-salt, gave numbers corresponding to margaric acid; it had also the appearance of that acid, but its melting-point was 52° to 53° C., or 12 degrees lower than the melting-point assigned to margaric acid.

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* Liebig's *Annalen*, May 1857.

Becker has also prepared some of the double æthers, and compound æthers of cetylic alcohol. They are easily obtained by the same methods by which the corresponding compounds of the lower homologues of cetylic alcohol are procured.

Heinz* has investigated the composition of the solid part of olive-oil. He found that olive-oil contained palmatine as well as oleine, and very probably stearine and arachine. But the difficulties attendant on the separation, in the pure form, of the higher acids were too great to allow this point to be stated with certainty.

According to some older experiments by Sacc, linseed-oil was held to be the glycerine compound of two acids, of which one was liquid and oily, and the other solid and crystalline. The composition of the drying oils has been lately studied by Schüler †, who communicates the results of his experiments on linseed-oil. The mode of investigation and separation of the constituents was that usually employed in this kind of investigation. He finds in it a new fatty acid, which he names linoleic acid. When pure it is a pale yellowish liquid oil, which strongly refracts light. Treated with nitrous acid it becomes red, and after a time viscid; but there is formed no elaidic acid, as is the case with oleic acid under these circumstances. The analyses of the salts of this acid did not give accordant results; it seems to have a tendency to form acid salts. Numerous analyses of the pure acid gave, however, numbers corresponding to the formula

$$C^{32} H^{28} O^{4} = C^{32} H^{27} O^{3}, HO.$$

The number of equivalents of carbon exceeds that of hydrogen by four, while in ordinary oleic acid, $C^{36} H^{34} O^{4}$, the excess is two. From its composition and relation to nitrous acid it opens a new series of fatty acids.

Schüler thinks that the solid acid in linseed-oil is palmitic, and not margaric acid, as Sacc had held. Linoleic and palmitic acids have the same number of carbon and oxygen atoms, but differ in the number of their hydrogen atoms,

$C^{32} H^{32} O^{4}$	$C^{32} H^{28} O^{4}$.
Palmitic acid.	Linoleic acid.

Mayer ‡ has made a very extensive and complete series of analyses of the ashes of various seeds. Hitherto the quantitative relations existing between the inorganic and organic compounds contained in plants had not been investigated; and yet it is indubitable that certain mineral constituents of a plant have

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* *Journal für Praktische Chemie*, May 1857.

† Liebig's *Annalen*, February 1857.

‡ Ibid.

greater importance for certain organic substances than others; and it is impossible, for instance, to establish any relation between the mere total quantity of ash and the quantity of albuminous substances in a plant. Now albuminous substances never occur in a vegetable structure unaccompanied by phosphoric acid; it must be admitted, therefore, that the existence of the former is dependent on the presence of the latter, and the question arises what is the relation between them. In vegetable structures, which principally contain albumen, the relation between phosphoric acid, nitrogen and bases, is different to that existing in structures which principally contain vegetable caseine or gluten, and hence albumen requires a quantity of phosphoric acid and bases different to that which is necessary for caseine and gluten: experiment must decide under what limits these variations may take place.

The seeds which Mayer investigated were rye, wheat, barley, oats; and besides these he analysed several leguminous seeds, and several sorts of flour, groats and bran. These substances were from the most various parts of Germany, and grown on the most varied soils. Mayer describes at some length the analytical methods used, and states that some of the older analyses of these substances are not trustworthy on account of the imperfect methods by which they were made.

The general results he arrived at from his experiments and from known facts may be summed up as follows:—

1. The seeds of the cereals contain a nearly constant quantity of water, and differences of climate exert no influence in this respect.
2. The proportions of phosphoric acid and nitrogen vary, but these variations are within very narrow limits.
3. The proportion of ash in the seeds of the cereals varies also within very narrow limits.
4. The finer the flour is ground, the less is the quantity of nitrogen contained in it.
5. Bran is rich in phosphates and in nitrogen. Its ash consists principally of phosphates, and does not contain much silica.
6. Leguminous seeds are richer in nitrogen and phosphoric acid than cereal seeds. The latter contain pyrophosphates; the former, tribasic phosphates.
7. A remarkable relation exists between albuminous substances and phosphoric acid. An increase in the quantity of the former is accompanied by a proportionate increase in the quantity of the latter, and phosphoric acid is a necessary condition of the existence of albuminous substances.
8. This relation varies, however, for each kind of albumen—for vegetable albumen, legumine, and for gluten. Leguminous

seeds, which consist chiefly of legumine and soluble albumen, contain for the same quantity of phosphoric acid one and a half to twice as much nitrogen as the seeds of cereals, which consist principally of gluten.

9. The relation of phosphoric acid to nitrogen is modified when one of these albuminous substances is replaced by another in seeds of the same kind and variety, as has been shown by Millon.

10. The ash of most kinds of corn contains large quantities of magnesia, which is partially present as ammonio-phosphate of magnesia. And hence the proportion of nitrogen in a seed does not exactly give the quantity of albumen.

XI. *On the Nature of the Motion which we call Heat.*

By R. Clausius*.

1. BEFORE writing my first memoir on heat, which was published in 1850, and in which heat is assumed to be a motion, I had already formed for myself a distinct conception of the nature of this motion, and had even employed the same in several investigations and calculations. In my former memoirs I intentionally avoided mentioning this conception, because I wished to separate the conclusions which are deducible from certain general principles from those which presuppose a particular kind of motion, and because I hoped to be able at some future time to devote a separate memoir to my notion of this motion and to the special conclusions which flow therefrom. The execution of this project, however, has been retarded longer than I at first expected, inasmuch as the difficulties of the subject, as well as other occupations, have hitherto prevented me from giving to its development that degree of completeness which I deemed necessary for publication.

A memoir has lately been published by Krönig, under the title *Grundzüge einer Theorie der Gase*†, in which I have recognized some of my own views. Seeing that Krönig has arrived at these views just as independently as I have, and has published them before me, all claim to priority on my part is of course out of the question; nevertheless, the subject having once been mooted in this memoir, I feel myself induced to publish those parts of my own views which I have not yet found in it. For the present, I shall confine myself to a brief indication of a

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* From Poggendorff's *Annalen*, vol. c.

† This was first printed separately by A. W. Hayn in Berlin, and afterwards appeared in Poggendorff's *Annalen*, vol. xcix. p. 315.

few principal points, and reserve a more complete analysis for another time*.

2. Krönig assumes that the molecules of gas do not oscillate about definite positions of equilibrium, but that they move with constant velocity in right lines until they strike against other molecules, or against some surface which is to them impermeable. I share this view completely, and I also believe that the expansive force of the gas arises from this motion. On the other hand, I am of opinion that this is not the only motion present.

In the first place, the hypothesis of a rotatory as well as a progressive motion of the molecules at once suggests itself; for at every impact of two bodies, unless the same happens to be central and rectilineal, a rotatory as well as a translatory motion ensues.

I am also of opinion that vibrations take place within the several masses in a state of progressive motion. Such vibrations are conceivable in several ways. Even if we limit ourselves to the consideration of the atomic masses solely, and regard these as absolutely rigid, it is still possible that a molecule, which consists of several atoms, may not also constitute an absolutely rigid mass, but that within it the several atoms are to a certain extent moveable, and thus capable of oscillating with respect to each other.

I may also remark, that by thus ascribing a movement to the atomic masses themselves, we do not exclude the hypothesis that each atomic mass may be provided with a quantity of finer matter, which, without separating from the atom, may still be moveable in its vicinity.

By means of a mathematical investigation given at the end of the present memoir, it may be proved that the *vis viva* of the translatory motion alone is too small to represent the whole heat present in the gas; so that without entering into the probability of the same, we are thus compelled to assume one or more motions of another kind. According to this calculation, the excess of the whole *vis viva* over that of the translatory motion alone is particularly important in gases of a complicated chemical constitution, in which each molecule consists of a great number of atoms.

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* I must not omit to mention here, that some time ago Mr. William Siemens of London, when on a visit in Berlin, informed me that Joule had also expressed similar ideas in the Memoirs of the Literary and Philosophical Society of Manchester. My views being consequently no longer completely new, this was an additional reason why I should hasten their publication less than I otherwise probably should have done. Hitherto I have not been able to procure the memoir of Joule in question, and therefore I am ignorant how far he has pursued the subject, and whether his views coincide with mine in all points. It is to be regretted that Joule did not publish his memoir in a more widely circulated periodical.

3. In one and the same gas the translatory motion of the whole molecules will always have a constant relation to the several motions which, in addition to the above, the constituents of the molecules likewise possess. For brevity I will call the latter the *motions of the constituents.*

Conceive a number of molecules whose constituents are in active motion, but which have no translatory motion. It is evident the latter will commence as soon as two molecules in contact strike against each other in consequence of the motion of their constituents. The translatory motion thus originated will of course occasion a corresponding loss of *vis viva* in the motion of the constituents. On the other hand, if the constituents of a number of molecules in a state of translatory motion were motionless, they could not long remain so, in consequence of the collisions between the molecules themselves, and between them and fixed sides or walls. It is only when all possible motions have reached a certain relation towards one another, which relation will depend upon the constitution of the molecules, that they will cease mutually to increase or diminish each other.

When two molecules whose constituents are in motion come into collision they will not rebound, like two elastic balls, according to the ordinary laws of elasticity; for their velocities and directions after collision will depend, not only upon the motion which the whole molecules had before impact, but also upon the motion of those constituents which are nearest each other at the moment of collision. After the equalization of the several motions, however, when the translatory motion is, on the whole, neither increased nor diminished by the motions of the constituents, we may, in our investigation of the total action of a great number of molecules, neglect the irregularities occurring at the several collisions, and assume that, in reference to the translatory motion, the molecules follow the common laws of elasticity.

4. The explanation of the expansive force of gases and its dependence upon volume and temperature, as given by Krönig, suffers no essential modification through the introduction of other motions. The pressure of the gas against a fixed surface is caused by the molecules in great number continually striking against and rebounding from the same. The force which must thence arise is, in the first place, by equal velocity of motion inversely proportional to the volume of the given quantity of gas; and secondly, by equal volume proportional to the *vis viva* of the translatory motion: the other motions do not here immediately come into consideration.

On the other hand, from Gay-Lussac's law we know that, under constant volume, the pressure of a perfect gas increases in the same ratio as the temperature calculated from −273° C., which we call the absolute temperature. Hence, according to the above, it follows that the absolute temperature is proportional to the *vis viva* of the translatory motion of the molecules. But as, according to a former remark, the several motions in one and the same gas bear a constant relation to each other, it is evident that the *vis viva* of the translatory motion forms an aliquot part of the total *vis viva*, so that the absolute temperature is also proportional to the whole *vis viva* in the gas.

These considerations, together with others connected therewith to be given hereafter, induced me, in my memoir "On the Moving Force of Heat," to express the opinion that the specific heat of gases was constant; which opinion was in opposition to the experiments then known*. The quantity of heat which must be imparted to a gas, under constant volume, in order to raise its temperature is to be considered as the increase of the *vis viva* in the gas, inasmuch as in this case no work is done whereby heat could be consumed. The specific heat *under constant volume*, therefore, is in a perfect gas the magnitude which Rankine calls the *true* specific heat. Now the assertion that the true specific heat of a gas is constant, is simply equivalent to the assertion that *the total* vis viva *in the gas has a constant ratio to the* vis viva *of the translatory motion which serves us as a measure of the temperature.* With respect to the specific heat under constant pressure, I have proved in the memoir before cited, and by means of a hypothesis proceeding from the same considerations, that it differs only by a constant magnitude from the true specific heat.

5. The foregoing is true for permanent gases only, and even for these only approximatively. In general, the small deviations which present themselves can be easily accounted for.

In order that Mariotte's and Gay-Lussac's laws, as well as others in connexion with the same, may be strictly fulfilled, the gas must satisfy the following conditions with respect to its molecular condition:—

(1) The space actually filled by the molecules of the gas must be infinitesimal in comparison to the whole space occupied by the gas itself.

(2) The duration of an impact, that is to say, the time required to produce the actually occurring change in the motion of a molecule when it strikes another molecule or a fixed surface, must be infinitesimal in comparison to the interval of time between two successive collisions.

(3) The influence of the molecular forces must be infinitesimal. Two conditions are herein involved. In the first place, it is requisite that the force with which all the molecules at their

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* Poggendorff's *Annalen*, vol. lxxix. p. 393. Phil. Mag. vol. ii. pp. 1, 102.

mean distances attract each other, vanish when compared with the expansive force due to the motion. But the molecules are not always at their mean distances asunder; on the contrary, during their motion a molecule is often brought into close proximity to another, or to a fixed surface consisting of active molecules, and in such moments the molecular forces will of course commence their activity. The second condition requires, therefore, that those parts of the path described by a molecule under the influence of the molecular forces, when the latter are capable of altering appreciably the direction or velocity of the molecule's motion, should vanish when compared with those parts of its path with respect to which the influence of these forces may be regarded as zero.

If these conditions are not fulfilled, deviations in several ways from the simple laws of gases necessarily arise; and these deviations become more important the less the molecular condition of the gas fulfils the conditions in question.

On becoming acquainted with the celebrated investigations of Regnault on the deviations of gases from Mariotte's and Gay-Lussac's laws, I attempted, by means of the principles above intimated, to deduce some conclusions with respect to the molecular condition of several gases from the nature of the deviations which Regnault detected in the same. A description of this method, however, would be too prolix; and even the results, in consequence of the many difficulties encountered in actual calculation, are too uncertain to merit being here adduced.

Whenever, therefore, in the sequel a gas is spoken of, we shall, as before, conceive it to be one which *perfectly* fulfils the above conditions, and which Regnault calls an *ideal* gas, inasmuch as all known gases present but an approximation to this condition.

6. After these considerations on the *gaseous* condition, the question at once arises in what manner the *solid* and *liquid* conditions differ from the gaseous. Although a definition of these states of aggregation, in order to be satisfactory in all its details, would require a more complete knowledge than we at present possess of the condition of the individual molecules, yet it appears to me that several fundamental distinctions may be advanced with tolerable probability.

A motion of the molecules takes place in all three states of aggregation.

In the *solid* state, the motion is such that the molecules move about certain positions of equilibrium without ever forsaking the same, unless acted upon by foreign forces. In solid bodies, therefore, the motion may be characterized as a vibrating one, which may, however, be of a very complicated kind. In the first place, the constituents of a molecule may vibrate among themselves; and secondly, the molecule may vibrate as a whole: again, the latter vibrations may consist in oscillations to and fro of the centre of gravity, as well as in rotatory oscillations around this centre of gravity. In cases where external forces act on the body, as in concussions, the molecules may also be permanently displaced.

In the *liquid* state the molecules have no longer any definite position of equilibrium. They can turn completely around their centres of gravity; and the latter, too, may be moved completely out of its place. The separating action of the motion is not, however, sufficiently strong, in comparison to the mutual attraction between the molecules, to be able to separate the latter entirely. Although a molecule no longer adheres to definite neighbouring molecules, still it does not spontaneously forsake the latter, but only under the united actions of forces proceeding from other molecules, with respect to which it then occupies the same position as it formerly did with respect to its neighbouring molecules. In liquids, therefore, an oscillatory, a rotatory, and a translatory motion of the molecules takes place, but in such a manner that these molecules are not thereby separated from each other, but, even in the absence of external forces, remain within a certain volume.

Lastly, in the *gaseous* state the motion of the molecules entirely transports them beyond the spheres of their mutual attraction, causing them to recede in right lines according to the ordinary laws of motion. If two such molecules come into collision during their motion, they will in general fly asunder again with the same vehemence with which they moved towards each other; and this will the more readily occur, since a molecule will be attracted with much less force by another single molecule than by all the molecules which in the liquid or solid state surround it.

7. The phænomenon of *evaporation* appearing peculiarly interesting to me, I have attempted to account for the same in the following manner.

It has been stated above, that in liquids a molecule, during its motion, either remains within the sphere of attraction of its neighbouring molecules, or only leaves the same in order to take up a corresponding position with respect to other neighbouring molecules. This applies only to the mean value of the motions, however; and as the latter are quite irregular, we must assume that the velocities of the several molecules deviate within wide limits on both sides of this mean value.

Taking next the surface of a liquid into consideration, I assume that, amongst the varied motions to and fro, it happens that under the influence of a favourable cooperation of the translatory, oscillatory, and rotatory motions, a molecule separates itself with such violence from its neighbouring molecules that it has already receded from the sphere of their action before losing all its velocity under the influence of their attracting forces, and thus that it continues its flight into the space above the liquid.

Conceive this space to be enclosed, and at the commencement empty; it will gradually become more and more filled with these expelled molecules, which will now deport themselves in the space exactly as a gas, and consequently in their motion strike against the enclosing surfaces. The liquid itself, however, will form one of these surfaces; and when a molecule strikes against the same, it will not in general be driven back, but rather retained, and, as it were, absorbed in consequence of the renewed attraction of the other molecules into whose vicinity it has been driven. A state of equilibrium will ensue when the number of molecules in the superincumbent space is such, that on the average as many molecules strike against, and are retained by the surface of the liquid in a given time, as there are molecules expelled from it in the same time. The resulting state of equilibrium, therefore, is not a state of rest or a cessation of evaporation, but a state in which evaporation and condensation continually take place and compensate each other in consequence of their equal intensity.

The density of the vapour necessary for this compensation, depends upon the number of molecules expelled from the surface of the liquid in the unit of time; and this number is again evidently dependent upon the activity of the motion within the liquid, that is to say, upon its temperature. I have not yet succeeded in deducing from these considerations the law according to which the pressure of vapour must increase with the temperature.

The preceding remarks on the deportment of the surface of the liquid towards the superincumbent vapour, apply in a similar manner to the other surfaces which enclose the space filled with vapour. The vapour is in the first place condensed on these surfaces, and the liquid thus produced then suffers evaporation, so that here also a state must be attained in which condensation and evaporation become equal. The requisite quantity of condensed vapour on these surfaces depends upon the density of the vapour in the enclosed space, upon the temperature of the vapour and of the enclosing surfaces, and upon the force with which the molecules of vapour are attracted towards these surfaces. In this respect a maximum will occur when the enclosing surfaces are completely moistened with the condensed liquid; and as soon as this takes place, these surfaces deport themselves exactly like a single surface of the same liquid.

8. The reason why the presence of another gas above the liquid cannot impede the evaporation of the same may now be immediately explained.

The pressure of the gas on the liquid arises solely from the fact, that here and there single molecules of gas strike against the surface of the liquid. In other respects, however, inasmuch as the molecules of gas themselves actually fill but a very small part of the superincumbent space, the latter must be considered as empty, and as offering a free passage to the molecules of the liquid. In general these molecules will only come into collision with those of the gas at comparatively great distances from the surface, and the former will then deport themselves towards the latter as would the molecules of any other admixed gas. We must conclude, therefore, that the liquid also expels its molecules into the space filled with gas; and that in this case also the quantity of vapour thus mixed with the gas continues to increase until, on the whole, as many molecules of vapour strike against and are absorbed by the surface of the liquid as the latter itself expels; and the number of molecules of vapour to the unit of volume requisite hereto, is the same whether the space does or does not contain additional molecules of gas.

The pressure of the gas, however, exercises a different influence on the interior of the liquid. Here also, or at places where the mass of liquid is bounded by a side of the vessel, it may happen that the molecules separate from each other with such force that for a moment the continuity of the mass is broken. The small vacuum thus produced, however, is surrounded on all sides by masses which do not admit of the passage of the moved molecules; and hence this vacuum will only then become magnified into a bubble of vapour, and be able to continue as such, when the number of molecules expelled from its enclosing liquid walls is sufficient to produce an internal vapour-pressure capable of holding in equilibrium the pressure which acts externally and tends to compress the bubble again. Hence the expansive force of the enclosed vapour must be greater, the greater the pressure to which the liquid is exposed, and thus is explained the relation which exists between the pressure and the temperature of the boiling-point.

The relations will be more complicated when the gas above the liquid is itself condensable, and forms a liquid which mixes with the given one, for then of course the tendency of the two kinds of matter to mix enters as a new force. I shall not here enter into these phænomena.

As in liquids, so also in solids the possibility of an evaporation may be comprehended; nevertheless it does not follow from this that, on the contrary, an evaporation *must* take place on the surface of all bodies. It is, in fact, readily conceivable that the mutual cohesion of the molecules of a body may be so great, that, so long as the temperature does not exceed a certain limit, even the most favourable combination of the several molecular motions is not able to overcome this cohesion.

9. The explanation of the *generation* and *consumption* of heat accompanying changes of the state of aggregation and of the volume of a body, follows immediately, according to the above principles, from the consideration of the positive and negative work done by the active forces.

In the first place, let us consider the *interior* work.

When the molecules of a body change their relative positions, the change may be either in the same direction as that according to which the forces resident in the molecules tend to move the latter, or in a direction opposite to this. In the first case, a certain velocity is imparted by the forces to the molecules during their passage from one position to the other, and the *vis viva* of this velocity is immediately transformed into heat; in the second case, and disregarding for the present the action of extraordinary foreign forces, it is in virtue of heat that the molecules move in directions opposed to the interior forces, and the retardation which these molecules thereby suffer through the action of the opposing forces appears as a diminution of the motion of heat.

In the passage from the solid to the liquid state the molecules do not, indeed, recede beyond the spheres of their mutual action; but, according to the above hypothesis, they pass from a definite and, with respect to the molecular forces, suitable position, to other irregular positions, in doing which the forces which tend to retain the molecules in the former position have to be overcome.

In evaporation, the complete separation which takes place between the several molecules and the remaining mass evidently again necessitates the overcoming of opposing forces.

With respect to gaseous bodies, it is evident from what has been above remarked concerning the requisites of a perfect gaseous state, that as soon as a gas has once attained this state, molecular attractions have no longer to be overcome during its further expansion, so that during the changes of volume of an ideal gas no interior work has to be performed.

10. Besides the interior, we have in the next place to consider the *exterior* work, and the corresponding change in the quantity of heat.

In the first place, with respect to the changes in the heat of a permanent gas subjected to pressure or expanding by overcoming a counter-pressure, Krönig has already remarked at the conclusion of his memoir, that when one of the sides against which the molecules of gas strike is itself in motion, these molecules will not rebound with velocities equal to those they possessed when moving towards the side. If the side approaches the molecules, the velocity of the molecules on leaving the same is in general greater than before, if it recedes less. By means of special mathematical considerations, it may without difficulty be proved that the increase or decrease of the *vis viva* thereby produced must exactly correspond to the external work done by the expansive force of the gas; nevertheless it is not even necessary to give a special proof of this theorem, since the same is an immediate consequence of the general law of equivalence between *vis viva* and work.

If the side moves so slowly that the pressure of the gas against the moving side is just as great as against a stationary one, then, in determining the work, the velocity of the side no longer enters into consideration, but merely the whole path described by the same. On the contrary, if the velocity of the side is so great that in the vicinity of the same a sensible compression or rarefaction of the gas thereby ensues, then the pressure actually exercised by the gas during the motion must always be brought into calculation.

When an overflow takes place between two vessels filled with gases of different densities, or between a full and an empty vessel, on the whole no work will be performed, and therefore no change in the total quantity of heat can occur. It is not here asserted that no change in the quantity of heat takes place in either of the two vessels considered separately, for a mass of gas whose molecules move principally in a definite direction deports itself towards adjoining gaseous masses in the same manner as a moved side; and when the moved gaseous mass strikes against stationary walls, just as much motion of heat makes its appearance as *vis viva* is lost by the common translatory motion of the whole mass.

Just as in the changes of volume of gaseous bodies, so also in other cases the external work must be taken into consideration; as, for instance, the work which during the evaporation is employed to overcome external resistance, and thus to make room for the vapour as it is generated. In solid and liquid bodies which only suffer small changes of volume, the external work is also for the most part only small; nevertheless here also cases occur in which its influence becomes considerable.

11. Lastly, I must mention a phænomenon the explanation of which appears to me to be of great importance, viz. *when two gases combine with each other, or when a gas combines with another body, and the combination is also gaseous, the volume of the compound gas bears a simple ratio to the volumes of the single constituents, at least when the latter are gaseous.*

Krönig has already proved that the pressure exerted by a gas on the unit of its enclosing surface must be proportional to the number of molecules contained in the unit of volume, and to the *vis viva* of the several molecules arising from their translatory motion, the only one which Krönig considers.

If we apply this to simple gases, and assume that, when pressure and temperature are the same, equal volumes contain the same number of atoms—a hypothesis which for other reasons is very probable,—it follows that, in reference to their translatory motion, the atoms of different gases must have the same *vis viva.*

We will next examine in what manner this theorem remains true when applied to the molecules of compound gases.

12. In the first place, let us compare compound gases amongst themselves, *e. g.* two gases to form which the constituents have combined in ratios of volume respectively equal to 1 : 1 and 1:2. Nitric acid and nitrous acid may serve as examples.

With respect to these two gases, we know that quantities containing the same amount of oxygen occupy the same volume. Hence here, too, equal volumes contain the same number of molecules, although in the one gas each molecule consists of two, and in the other of three atoms; and we must further conclude, that even these differently constituted molecules have the same *vis viva* with respect to their translatory motion.

In most other compound gases we are led to the same conclusion; and in cases which do not submit themselves to this rule, it does not appear to me impossible that the discrepancy may be accounted for in one or both of two ways: either the gas was not sufficiently removed from its points of condensation when its volume was determined, or the chemical formula hitherto employed does not properly represent the manner in which the atoms are combined to form molecules.

On comparing compound and simple gases, however, an unmistakeable deviation from the foregoing rule shows itself, inasmuch as the space corresponding to an atom of the simple gas does not correspond to a molecule of the compound one. When two simple gases combine in equal volumes, it is well known that no change of volume takes place, whilst according to the above rule the volume ought to be diminished in the ratio of 2 : 1. Again, when a volume of one gas combines with two or three volumes of another, the combination is found to occupy two volumes, whereas according to rule it ought only to occupy one volume, and so on.

13. On seeking to explain these curious anomalies, and especially to find a common law governing the relations of volume in gases, I was led to adopt the following view as being most plausible. I beg to offer the same to the scientific public as a hypothesis which is at least worthy of further examination.

I assume that the force which determines chemical combination, and which probably consists in a kind of polarity of the atoms, is already active in simple substances, and that *in these likewise two or more atoms are combined to form one molecule.*

For instance, let equal volumes of oxygen and nitrogen be given. A mixture of these gases contains a certain number of molecules, which consist either of two atoms of oxygen or of two atoms of nitrogen. Conceive the mixture to pass into a chemical compound, and the latter then contains just as many molecules, which are merely constituted in a different manner, inasmuch as each consists of an atom of oxygen and an atom of nitrogen. Hence there is no reason why a change of volume should take place. If, on the other hand, one volume of oxygen and two of nitrogen are given, then in the mixture each molecule consists of two, and in the compound of three atoms. The chemical combination, therefore, has caused the number of molecules to diminish in the ratio of 3 : 2, and consequently the volume ought to diminish in the same ratio.

It is well known that some simple substances do not, in the gaseous form, occupy the volume which their atomic weights and the volumes of their combinations would lead us to anticipate, but another, and in most cases a smaller volume, which bears to the former a simple ratio. A special investigation of these substances would here be out of place, more especially as two of them, sulphur and phosphorus, deport themselves in other respects in so remarkable a manner, in consequence of the variety of conditions they are capable of assuming, that we may reasonably expect further discoveries from chemistry with respect to these bodies; and then, perhaps, besides other irregularities, those of the volumes of their vapours will be explained. Nevertheless I may here recall one circumstance which in some cases may possibly facilitate this explanation. I refer to the fact, that the above hypothesis, according to which the molecules of simple substances each consist of *two* atoms, may not be the only possible one.

On comparing with each other all cases of simple and compound gases, we must not expect to find immediately a perfect agreement throughout. I am of opinion, however, that, under the present uncertainty with respect to the inner constitution of several bodies, and particularly of those which possess a complicated chemical composition, too great weight ought not to be laid upon individual anomalies; and I deem it probable, that, by means of the above hypothesis respecting the molecules of simple substances, all relations of volume in gases may be referred back to the theorem, *that the several molecules of all gases possess equal* vis viva *in reference to their translatory motion.*

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14. Proceeding to treat the subject mathematically, we will

first deduce the expression which shows in what manner the pressure of the gas on the sides of the vessel depends upon the motion of its molecules.

As the shape of the vessel is indifferent, we will select that which is most convenient for our purpose. We will assume the vessel to be very flat, and that two of its sides consist of parallel planes so close to one another that their distance asunder is infinitesimal when compared with the other dimensions of the vessel. Hence we need not consider the cases where the molecules strike against one of the narrow strips of sides, and we may assume that each moves in a right line until it either strikes against another molecule or against one of the large parallel sides. In fact, to take the small sides into consideration would change nothing in the final result, and would only make the development more prolix.

Let us consider one only of the two large sides; during the unit of time it is struck a certain number of times by molecules moving in all possible directions compatible with an approach towards the surface. We must first determine the number of such shocks, and how many correspond on the average to each direction.

15. Hereafter we shall always assume the gas to be an *ideal* one; in other words, we shall disregard the irregularities proceeding from an imperfect gaseous state, so that in determining the pressure we may, with Krönig, introduce certain simplifications in place of considering the motion exactly as it takes place.

The whole number of shocks received by the side remains unchanged when we assume that the molecules do not disturb each other in their motion, but that each pursues its rectilineal path until it arrives at the side.

Further, although it is not actually necessary that a molecule should obey the ordinary laws of elasticity with respect to elastic spheres and a perfectly plane side, in other words, that when striking the side, the angle and velocity of incidence should equal those of reflexion, yet, according to the laws of probability, we may assume that there are as many molecules whose angles of reflexion fall within a certain interval, *e. g.* between 60° and 61°, as there are molecules whose angles of incidence have the same limits, and that, on the whole, the velocities of the molecules are not changed by the side. No difference will be produced in the final result, therefore, if we assume that for each molecule the angle and velocity of reflexion are equal to those of incidence. According to this, each molecule would move to and fro between the large parallel sides, in the same directions as those chosen by a ray of light between two plane mirrors, until at length it would come in contact with one of the small sides; from this it would be reflected, and then commence a similar series of journeys to and fro, and so forth.

Lastly, there is no doubt that actually the greatest possible variety exists amongst the velocities of the several molecules. In our considerations, however, we may ascribe a certain mean velocity to all molecules. It will be evident from the following formulæ, that, in order to maintain an equal pressure, this mean velocity must be so chosen that with it the total *vis viva* of all the molecules may be the same as that corresponding to their actual velocities.

16. According to these assumptions, it is evident, that, during the unit of time, each molecule will strike the side under consideration just as often as during that time it can, by following its peculiar direction, travel from the side in question to the other and back again. Let h be the distance between the large parallel sides, and ϑ the acute angle between the normal and the direction of motion; then $\frac{h}{\cos\vartheta}$ is the length of the path from one side to the other, and

$$\frac{u.\cos\vartheta}{2h} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

the number of impulses given to the side, u being the velocity of the molecule.

With respect to the directions of the several molecules, we must assume that on the average each direction is equally represented. From this it follows, that the number of molecules moving in directions which form with the normal angles included between ϑ and $\vartheta + d\vartheta$, has to the whole number of molecules the same ratio that the surface of the spherical zone, whose limiting circles correspond to the angles ϑ and $d\vartheta$, has to the surface of the hemisphere, that is, the ratio

$$2\pi \sin\vartheta d\vartheta : 2\pi.$$

Hence if n represents the whole number of molecules, the number which corresponds to the angular interval between ϑ and $\vartheta + d\vartheta$ will be

$$n \sin\vartheta d\vartheta,$$

and the number of shocks imparted by them will be

$$\frac{nu}{2h}\cos\vartheta \sin\vartheta d\vartheta. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

17. In order to determine the intensity of a shock, the whole velocity must be resolved into two components, one parallel and the other perpendicular to the side. Of these components, the first will not be affected by the shock, and will not enter into

consideration in determining its intensity; the second, however, whose magnitude is represented by $u \cos \vartheta$, will be changed by the shock into an equal velocity in the opposite direction. The action of the side upon the molecule, therefore, consists in depriving it in one direction of the velocity $u \cos \vartheta$, calculated according to the normal, and of imparting to it an equal velocity in an opposite direction; in other words, of imparting to it a velocity of $2u \,.\, \cos \vartheta$ in the latter direction. Hence the quantity of motion imparted to the molecule will be

$$2mu \,.\, \cos \vartheta, \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (3)$$

where m is the mass of the molecule.

Applying this to all molecules which correspond to the interval between ϑ and $\vartheta + d\vartheta$, we obtain during the unit of time,

$$\frac{nu}{2n} \cos \vartheta \sin \vartheta d\vartheta$$

times the same action, hence the quantity of motion imparted to these molecules during the unit of time is

$$\frac{nmu^2}{h} \cos^2 \vartheta \,.\, \sin \vartheta \,.\, d\vartheta. \quad . \; . \; . \; . \; . \; . \quad (4)$$

Integrating this expression between the limits $\vartheta = 0$ and $\vartheta = \frac{\pi}{2}$, we find the motion imparted by the side to all the molecules which strike against it during the unit of time to be

$$\frac{nmu^2}{3h}. \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (5)$$

Let us now conceive the side to be capable of moving freely; then in order that it may not recede before the shocks of the molecules, it must be acted upon on the other side by a counter force, which latter may in fact be regarded as continuous, in consequence of the great number of shocks and the feebleness of each. The intensity of this force must be such as to enable it, during the unit of time, to generate the quantity of motion represented by the above expression. Since all forces, however, are measured by the quantity of motion they can produce in the unit of time, the above expression at once represents this force as well as the pressure exerted by the gas, the latter being equilibrated by the former.

If α be the superficial area of the side, and p the pressure on the unit of surface, then

$$p = \frac{mnu^2}{3\alpha h}.$$

The product αh here involved gives the volume of the vessel or gas; hence representing the same by v, we have

$$p=\frac{mnu^2}{3v}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The same formula would have been obtained if, with Krönig, we had, for the sake of simplification, assumed that one third of the whole molecules move perpendicularly to the side under consideration, and the two remaining thirds in two other directions parallel to the side. Nevertheless I preferred deducing the formula for the pressure without using this simplifying hypothesis.

If we write the last equation in the form

$$\frac{3}{2}pv=\frac{nmu^2}{2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6a)$$

the right-hand side then denotes *the* vis viva *of the translatory motion of the molecules**. But, according to Mariotte's and Gay-Lussac's laws,

$$pv=\mathrm{T}\ .\ \mathrm{const.},$$

where T is the absolute temperature; hence

$$\frac{nmu^2}{2}=\mathrm{T}\ .\ \mathrm{const.};$$

and, as before stated, the *vis viva* of the translatory motion is proportional to the absolute temperature.

18. We may now make an interesting application of the above equations by determining the velocity u with which the several molecules of gas move.

The product nm represents the mass of the whole given quantity of gas, whose weight we will call q. Then g being the force of gravity,

$$nm=\frac{q}{g};$$

and from equation (6) we deduce

$$u^2=\frac{3gpv}{q}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Adopting the metre as unit of length, and the kilogramme as unit of weight, let us suppose a kilogramme of gas under the pressure of 1 atmosphere—10333 kilogrammes on the square

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* In accordance with a practice lately become general, and with what I have myself done in former memoirs, I call the *semi*-product of the mass into the square of the velocity the *vis viva*, because it is only with this definition of the notion that we can, without the addition of a coefficient, equate the expressions representing a quantity of work and the increase or decrease of *vis viva* which corresponds to the same.

metre—to be given. Then

$$g=9^{\mathrm{m}}\cdot 80896,$$
$$p=10333,$$
$$q=1.$$

To determine v, we know that, according to Regnault, a kilogramme of atmospheric air under the pressure of 1 atmosphere, and at the temperature of melting ice, occupies 0·7733 cubic metre. Hence ρ being the specific gravity of the gas under consideration, its volume at the temperature of melting ice will be

$$\frac{0\cdot 7733}{\rho};$$

and at the absolute temperature T, assuming 273° to be the absolute temperature of melting ice, it will be

$$\frac{0\cdot 7733}{\rho}\cdot\frac{T}{273}.$$

Hence we deduce

$$u^2=3\,.\,9\cdot 80896\,.\,10333\,.\,0\cdot 7733\,.\,\frac{T}{273\,.\,\rho}$$
$$=235130\frac{T}{273\,.\,\rho},$$

and consequently

$$u=485^{\mathrm{m}}\sqrt{\frac{T}{273\,.\,\rho}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

As particular cases, we obtain the following numbers corresponding to the temperature of melting ice:—

for oxygen	461^{m},
for nitrogen	492^{m},
for hydrogen . .	1844^{m}.

These numbers are the mean velocities which, for the totality of molecules, give the same *vis viva* as would their actual velocities. At the same time, however, it is possible that the actual velocities of the several molecules differ materially from their mean value.

19. By means of the equations above established, we will lastly examine *what relation exists between the* vis viva *of the translatory motion and the whole* vis viva, *or heat, in the gas.*

In doing so we will conceive the quantity of heat to be measured, not by the ordinary unit of heat, but by the mechanical unit of *vis viva*, or what is equivalent, by the unit of work. To this end we have only to divide the quantity of heat measured in the ordinary manner by the thermal equivalent of the unit of work, which as before I will denote by A. Let H be the quantity of heat thus determined.

Further, let c be the specific heat of the gas under constant volume, in other words, the true specific heat; then the increase of the quantity of heat in the quantity q of gas corresponding to an elevation of temperature $d\mathrm{T}$ is

$$d\mathrm{H}=\frac{qc}{\mathrm{A}}d\mathrm{T}.$$

Integrating this equation, we have

$$\mathrm{H}=\frac{qc}{\mathrm{A}}\mathrm{T}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

No constant need be added, since, as before remarked, the heat in the gas is proportional to the *vis viva* of the translatory motion, and hence also to the absolute temperature.

The expression on the right of this equation may be replaced by another which is very convenient for our present investigation.

The quantity of heat which must be imparted to the quantity of gas q in order to elevate its temperature by $d\mathrm{T}$ and its volume by dv is expressed thus,

$$\frac{qc}{\mathrm{A}}d\mathrm{T}+pdv,$$

wherein the first term represents the increase of the heat contained in the gas, and the second the quantity of heat consumed by work. If we assume the gas to be heated under constant pressure, the relation between $d\mathrm{T}$ and dv is thereby defined. For we have generally

$$pv=\mathrm{T}\,.\,\mathrm{const.};$$

and differentiating, under the supposition that p is constant, we obtain

$$pdv=d\mathrm{T}\,.\,\mathrm{const.};$$

whence the undetermined constant may be eliminated by means of the foregoing equation, and we have

$$dv=\frac{v}{\mathrm{T}}d\mathrm{T}.$$

Let us substitute this value of dv in the above equation, and at the same time note that, c' being the specific heat under constant pressure, the whole quantity of heat imparted to the gas in the case under consideration may be represented by $\frac{qc'}{\mathrm{A}}d\mathrm{T}$. In this manner we arrive at the equation

$$\frac{qc'}{\mathrm{A}}d\mathrm{T}=\frac{qc}{\mathrm{A}}d\mathrm{T}+\frac{pv}{\mathrm{T}}d\mathrm{T},$$

whence we conclude that

$$\frac{q(c'-c)}{A}\cdot T=pv. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

By means of this equation (9) becomes

$$H=\frac{c}{c'-c}\cdot pv. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

20. Let us now return to the equation (6*a*) before established, and for brevity let us denote the *vis viva* of the translatory motion by K, then

$$K=\frac{3}{2}pv.$$

By combining this with the foregoing equation we obtain

$$\frac{K}{H}=\frac{3}{2}\left(\frac{c'}{c}-1\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

The ratio of the *vis viva* of the translatory motion to the whole *vis viva* is thus reduced to the ratio between the two specific heats.

In order to compare with each other the values of the ratio $\frac{K}{H}$ corresponding to different gases, it will be found convenient to introduce in the above formula, in place of the specific heats calculated with reference to the unit of weight, those calculated according to the unit of volume, which for distinction may be represented by γ and γ'. The equation then becomes

$$\frac{K}{H}=\frac{3}{2}\cdot\frac{\gamma'-\gamma}{\gamma}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

If we neglect deviations which arise from an imperfect gaseous condition, and conceive all gases to be in the ideal state, then, as I have shown in my memoir "On the Moving Force of Heat*," the difference $\gamma'-\gamma$ is the same for all gases. Hence the ratio $\frac{K}{H}$ *is inversely proportional to the true specific heat of the gas calculated according to the unit of volume.*

For those simple gases which manifest no irregularities with respect to their volume, and for those compound ones which suffered no diminution of volume during the act of combination, γ, and therefore $\frac{K}{H}$ also, has the same value. For these gases we

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* Poggendorff's *Annalen*, vol. lxxix. p. 394. Phil. Mag. vol. ii. p. 1.

have approximately

$$\frac{\gamma'}{\gamma}=1{\cdot}421,$$

hence

$$\frac{K}{H}=0{\cdot}6315.$$

On the contrary, for those compound gases whose volumes during combination have been diminished, γ is greater, and consequently $\frac{K}{H}$ less; and, in fact, it is less the smaller the volume of the combination compared with the sum of the volumes of its constituents, the latter being all considered gaseous.

Thus is corroborated what was before stated, that the *vis viva* of the translatory motion does not alone represent the whole quantity of heat in the gas, and that the difference is greater the greater the number of atoms of which the several molecules of the combination consist. We must conclude, therefore, that besides the translatory motion of the molecules as such, the constituents of these molecules perform other motions, whose *vis viva* also forms a part of the contained quantity of heat.

Zurich, January 5, 1857.

XII. *On the Formation of Sulphide of Aluminium.*

By Charles W. Vincent, *Assist. Lib. Royal Institution*[*].

THE sulphide of aluminium has hitherto been prepared by the action which its components exert on each other when brought into contact at a high temperature. In the process described below, I have endeavoured to effect the same object by the deoxidation of sesquioxide of alumina by the monosulphide of sodium.

Monosulphide of sodium[†] was gently heated in a porcelain crucible to the fusing-point (when it acquires a flesh-coloured hue), powdered alumina was then gradually added, the whole being constantly stirred until the mass began to acquire a pasty consistence; the crucible was then closed and the temperature gradually raised to a dull red heat, at which temperature it was kept for about half an hour. On allowing to cool and then extracting with water, a yellow solution was formed containing soda, and the excess of sulphide of sodium together with a small quantity of alumina. The greater part of the last, however,

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[*] Communicated by the Author.

[†] Prepared by passing sulphuretted hydrogen through a solution of caustic soda and allowing the sulphide of sodium to crystallize out.

remained in the form of a somewhat bulky black precipitate of sulphide of aluminium at the bottom of the crucible. This precipitate was washed by decantation until tolerably free from alkali, the presence of a small quantity of which seemed to prevent its decomposition by water. In proportion, however, as the alkali is removed, the addition of water causes an evolution of sulphuretted hydrogen, a grayish-white precipitate of the hydrated sesquioxide of alumina being formed.

The original black precipitate, when exposed to the air on a filter, is converted in a very short time almost entirely into the oxide of aluminium. The sulphide, when heated in the air, ignites, sometimes burning quietly, at others giving off bright scintillations much resembling those of the metal, in both cases sulphurous acid being formed.

The alumina used in the above experiments was prepared by igniting alum, previously deprived of its water of crystallization; but alumina in almost every state appears to be attacked by the sulphide of sodium, even emery-powder and pipe-clay being, the first wholly, the second partially deoxidized by it.

XIII. *On the Formation of Craters, and the Nature of the Liquidity of Lavas.* *By* G. POULETT SCROPE, *Esq., M.P., F.R.S., F.G.S.*[*]

INTRODUCTION.

IT is now some thirty years since I published two works[†] upon the Phænomena of Volcanoes, Active and Extinct. I described in them, as accurately as I could, by pen and pencil, what I had observed during a residence of some duration among the volcanic districts of France and Italy; and explained, in considerable detail, the laws which, from those observations, I believed to regulate the remarkable developments of subterranean energies usually called volcanic, which have played so important a part in the construction of the superficial crust of our planet.

The general principle on which I proceeded in the theoretical portion of these works was the same which had been previously employed by Hutton and Playfair, and was subsequently adopted, with signal success, by Sir Charles Lyell,—namely, to refer, so far as is possible, appearances the origin of which has not been witnessed, to such causes as are seen or known to produce analogous appearances in the present day,—instead of resorting for the purpose to imaginary hypotheses.

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[*] From the Journal of the Geological Society, vol. xii. p. 326.

[†] "Considerations on Volcanoes," &c., 1825–6. "On the Geology of Central France," &c., 1826–7.

In the earlier volume of the two (the 'Considerations on Volcanoes'), however, I certainly overstepped this wholesome rule, by entering towards the conclusion of the work upon some rather crude speculations on a general theory of the globe; and this, together with defects of style and arrangement, and likewise of illustration, of which I became sensible only when it was too late to amend them, sufficiently accounts for the different reception these two works met with from geologists at the time. Neither, however, I presume to hope, were wholly without some beneficial result. At the period of their publication, the Wernerian theory of the precipitation from some aqueous menstruum, not merely of granite, and what were then called the primitive formations, but even of all the trap-rocks, still prevailed, and had the support of a large school of geologists in this country. I venture to think that the facts reported in my two volumes (especially those represented to the eye in the atlas illustrative of the volcanic remains of Central France) had some share in the final extinction of that German romance,—which some geologists as old as myself may remember to have been regarded almost in the light of a gospel-truth, and defended with all the acrimony of polemical controversy.

Some of the opinions, however, expressed in these works with respect to the laws that govern volcanic action, were severely criticised at the time. Others have been since opposed by rival theories. And, as these disputed questions have an important bearing on some of the most interesting problems of geology, I trust it may not be unprofitable to call attention to the more prominent among them.

I will advert on this occasion to two subjects especially, viz.

I. The origin, or mode of formation, of volcanic cones and craters.

II. The nature of the liquidity of lava at the time of its protrusion from a volcanic aperture.

I. *Formation of Cones and Craters.*—In both of the works to which I have alluded, I referred the formation of those remarkable circular hollows, usually called craters, which are of such frequent occurrence in volcanic districts, to explosive aëriform eruptions, breaking their way through the superficial rocks; and that of the external more or less conical hill or mountain which generally, but not always, environs a crater,—and which, indeed, often occurs without a crater, but always characterized by the quâ-quâ-versal dip of its constituent beds of lava and conglomerates,—to the accumulation, round and above an eruptive vent, of its fragmentary ejections and the lava-streams poured out from it.

I considered this law to be without exception; attributing the differences in figure and structure apparent among volcanic cones to the greater or less number and violence of the eruptions to which they were owing,—some being the product of a single eruption, others of a vast number, often repeated through a series of ages,—to differences in the position of the orifices of discharge, whether from the summit of the cone, or its base, or any intermediate points,—and whether from under water, or in the air,—to the varying mineral character of the products,—and to the influences of subsequent degradation.

At the same time I remarked that the earthquakes which always more or less accompany volcanic eruptions render probable a certain amount of elevation in mass of the pre-existing superficial rocks; and moreover that the rents they cause in the solid substance of the cone of a volcano in repeated eruption, into many of which rents liquid lava will be injected from the column rising in the central chimney, and cool down afterwards into more or less vertical dykes of solid rock, must have added considerably to the bulk and elevation of such a mountain, by a sort of inward distension.

This was no closet-theory,—because, as respects the cone and crater of Vesuvius at least, I had the advantage, in the years 1818, 1819, and 1820, of watching with my own eyes the outward growth of that cone, through a series of almost continual eruptions of a comparatively tranquil character, which during those years added considerably to its height and bulk by external accretions of ejected scoria and lava-currents. These last, the lava-streams, issued from small cones and craters formed upon the solid platform which then composed the summit of the great cone, and dribbled slowly down its slopes, consolidating so rapidly there as in few instances to reach the base of the cone at all; although night after night they were to be seen flowing from the summit in streams of considerable breadth and bulk, and glowing with a bright light on its steep sides.

Afterwards, in the latter part of the year 1822, I had seen the upper portion of this solid cone blown into the air (by which it lost a full third of its height), and a crater of vast dimensions drilled through its axis by continuous eruptive explosions of twenty days' duration.

I had previously made a close examination of the cones and craters of Etna, the Phlegræan Fields, the Lipari Isles, Central France, and the Rhine district; and their appearances accorded so completely with the supposition of an analogous mode of formation in their instances, that, upon the principle of explaining the unknown by the known, it seemed impossible, or at least unnecessary, to imagine any other origin for them.

"Elevation," "Denudation," and "Engulfment" Theories of Crater-formation.—It was, therefore, with no small surprise that I have since found this simple and natural mode of production denied to all cones and craters—including those of Vesuvius itself; and an hypothesis substituted of their originating in some sudden elevation of previously horizontal beds around a centre,—not (it would seem) of eruption, but of maximum elevation. I allude, of course, to the "Elevation-crater theory" of MM. Von Buch and Elie de Beaumont.

Sir Charles Lyell, M. Constant Prevost, and others, have amply refuted this unphilosophical theory; which, however, still appears to hold its ground to some extent on the Continent, through the prestige of the great names attached to it. It may, therefore, not be wholly useless to adduce some additional proofs of its unwarrantable character. But I must first be permitted to remark, that even Sir Charles Lyell, while supporting the view indicated above, of the generally eruptive origin of volcanic cones, has had recourse, in the case of some craters, to another agency, the influence of which I am induced to think he overrates;—I mean the excavating power of the sea in forming what he calls "craters of denudation." This phrase, I think, he first employed in a paper on the subject read before this Society in December 1849. It is not repeated in the latest edition of his "Principles," and I imagine, therefore, that he is no longer desirous of maintaining its propriety.

I by no means doubt, that in the case of craters formed beneath the sea, or in such close vicinity to it as to allow its waves and currents to enter and sweep round their interiors, these circumstances must have considerably modified the result. In the former case, that of subaqueous eruption, the resistance of the water above the vent would probably tend to throw off the ejected materials over a wider area. And thus, perhaps, we may account for the vast horizontal dimensions of the great crateriform basins of Italy,—Bolsena, Bracciano, Albano, and others, evidently of submarine origin. In the latter case, that of subaërial craters to which the sea has had access through some lateral opening, no doubt great degradation of their internal slopes and cliffs, as well as of the outside, will have often taken place. Many, indeed, will have had their enclosure reduced to a mere skeleton, like Santorini. Some, like Graham's Isle, have been entirely swept away. But the question being as to the *origin* of these crateriform hollows, not as to the cause of any subsequent alteration of figure, this, I believe, may in every instance, without exception, be most reasonably referred to volcanic explosive eruptions. And, therefore, the employment of such a phrase as "craters of denudation," in contradistinction to "craters of eruption," can only lead to a wrong conception of the originating forces.

Where, indeed, is to be found a crater, the formation of which cannot be accounted for (making allowance for the subsequent modifications already referred to) by eruptive phænomena of the same character as those which have before the eyes of trustworthy observers repeatedly drilled enormous craters through the axis of the cone of Vesuvius?

Is it the vast size of some craters which should render such an origin incredible in their instances? For example,—of the Val di Bué on the flank of Etna, the Caldera of Teneriffe, that of Palma, Santorini, or the external crater of Barren Island; which measure some three, five, or even six miles in diameter? But the crater of Vesuvius, formed in 1822, before my eyes, by explosions lasting twenty days, measured a mile in diameter, and was more than a thousand feet deep. The old crater of Somma, which half encircles the cone of Vesuvius, is about three times as wide as the crater of 1822. Are we, then, on that account alone, to believe that it could not have been produced by an eruption of proportionately greater violence,—when, too, such an eruption is known to have occurred about the time this crater must have been formed, namely, in the year 79, and to have overwhelmed three cities at the base of the mountain beneath its enormous fragmentary ejections? Is it not, on the contrary, much more in accordance with sound philosophy to ascribe the excavation of the old concentric crater of Somma to the same cause which but the other day was seen to excavate the new crater of Vesuvius, through the heart of the same mountain, than to invent for the former a different and fanciful process? But if Somma be admitted, notwithstanding its extent, to be a true crater of eruption, the same origin cannot be denied to that of Palma, Santorini, or others, on the ground of their size, which scarcely, if at all, exceeds that of Somma.

Sir Charles Lyell seems to doubt the Val di Bué being a true crater of eruption upon two grounds. First, because the beds composing the surrounding cliffs do not show the usual quâquâ-versal dip, but generally slope towards the sea. This, however, is merely the result of the eruption having broken out on one side of the central axis of the mountain,—a circumstance of frequent occurrence; and naturally so, because the old central vent is apt to be sealed up by the consolidated products of former eruptions, and the point of least resistance to the subterranean eruptive force will often, therefore, be a little on one side,—probably on a fresh point of a fissure broken through the flank of the mountain.

In fact, there must be a contest between the resisting powers of the sides of the mountain and of its upper part; and the weakest part, whichever it is, will give way, and be blown up.

Sir Charles's second reason is, that a sufficient amount of conglomerates is not to be seen on the mountain slopes around the Val di Bué, to account for the vacuity. But, besides that he himself speaks of "enormous masses of scoriæ on the flanks of Etna," it should be remembered that the aëriform explosions, when long continued, triturate the ejected matters, owing to their repeated fall into and rejection from the crater, to such a degree as to reduce the greater part at length to an impalpable powder, which is carried by the winds to a distance, sometimes of hundreds of miles, and spread in a thin layer over an enormous area of sea or land. And, moreover, the larger the dimensions of any crater, the more powerful and enduring will have been, in all probability, the explosions, and the more thoroughly triturated, during the process of its gradual enlargement, would be the fragments thrown up by them.

I remember being exceedingly surprised, after the termination of the Vesuvian eruption of 1822, forming a continual fountain of stones and ashes some miles in height, lasting through twenty days, and in the end completely gutting the mountain, to find that the prodigious amount of fragmentary matter thrown out from the crater had coated the outer slopes of the mountain only to an average thickness of a foot or two at most. But then the ashes which day by day were reduced to a finer and at length to an impalpable powder, so fine as to penetrate the closest rooms in the houses of Naples, were borne to vast distances by the winds. Much, too, was carried down into the plain, or the sea below the mountain, by the torrents of rain (producing *lave di fango*, or mud-lavas), such as overwhelmed Herculaneum, and which accompanied, as usual, the paroxysmal eruption of 1822.

Indeed, if we consider the statements adduced on good authority, of the prodigious distances to which ashes, and even large fragments of lapillo and of pumice, have been occasionally borne away from some of the volcanoes of South America and the Pacific (as, for example, in the eruption of Coseguina in 1835, and of Galongoon in 1822),—distances of more than a thousand miles (a large segment of the circumference of the globe), the whole of which intermediate space must have been strewn with them (and, in the first of these instances, it is said, to the depth of ten feet at the distance of twenty-four miles from the volcano), we may well conceive that eruptions productive of such an enormous amount of ejected matters may (nay, must) have blown into the air entire mountains of a magnitude far exceeding that of Vesuvius and Somma itself, or the bulk of matter wanting in the Val di Bué, and left in their place craters of corresponding dimensions.

Sir Charles Lyell suggests (as others have done before him), in regard to some of the largest known craters, another possible origin, which he calls *Engulfment*—that is, the subsidence of the upper part, or a large area, of a volcanic mountain into some abyss suddenly opened beneath. With respect to this supposition, without attempting to dispute its possibility, I must say that I am not aware of any such process having been ever witnessed by any credible observer so placed as to be able to distinguish between engulfment and ejection; and consequently that it were well to be cautious in admitting the occurrence of such a phænomenon, if the ordinary mode of action be sufficient to explain the facts really observed. We possess reports, it is true, of eruptions and earthquakes in Java, Sumatra, the Andes, and elsewhere, having caused the disappearance of the entire summit of a mountain, leaving a vast cavity in its place. But this is precisely the result that was observable after the eruption of Vesuvius in 1822. And in that instance we know there was no subsidence. The leading example usually adduced of such immense (supposed) engulfments is the truncation of the lofty cone of Papandayang, in Java, by an eruption in the year 1772. There, it is always said, a great area of the volcano "fell in and disappeared," swallowed up in the bowels of the earth, together with forty villages and their inhabitants. Such are the phrases usually made use of on these occasions, and very naturally so, by alarmed and unscientific observers. But recent explorers, especially Professor Junghuhn, have stated that these towns and villages of Papandayang were not swallowed up at all, but buried, like Pompeii, under the ejectamenta of the volcano; and Dr. Junghuhn, therefore, very properly refers the truncation of the mountain to eruptive explosions, rather than to subsidence.

It is, no doubt, quite conceivable, that within a volcanic mountain some internal reservoir, or subterranean lake of liquified lava, coated over by a coat of hardened rock or the accumulation of fragmentary matter, may be *tapped,* as it were, by an earthquake, and empty itself out of an aperture in the side of the mountain at a low level, leaving a cavity, which another earthquake, or the explosion of vapour and gases accumulated within it and increasing in temperature, may cause to burst like a vast bubble,—the overlying crust of rocks falling inwards. But such a supposition is, in the present state of our knowledge, purely conjectural, and unwarranted, if, as I have endeavoured to show, the ordinary phænomena of eruption suffice to account for the formation of the largest known craters. If it is to be resorted to in any case, it would be perhaps in that of the very small pit-craters occasionally met with in volcanic districts, such as the Gour de Tazana, and the lakes Pavin, Du Bouchet, and Servières in Central France. But even these show marks of explosive eruption in the scoriæ sprinkled around their banks. And the occurrence of even a single bed of scoria is certain proof of some explosions having taken place from a body of liquid lava beneath; though, as I have said, this *may* have been accompanied or followed by engulfment. Perhaps the singular character of the crater of Kilauea, in Owyhee, may be thought to claim for it an origin in subsidence rather than eruption. It is described as a vast sudden depression in what would otherwise be almost a level plain, on the side of the gently sloping volcanic mountain of Mauna Loa. It has an irregularly oval form, from three to five miles in diameter, and is usually encircled by vertical cliffs some hundred feet high. Its bottom consists of a lake of lava, on some points (which occasionally change their situation) in continual ebullition, and at a white heat; but coated over for the most part by an indurated crust upon which it is often possible to walk. Sometimes, however, the incrusted portion is in the centre of the lake, forming a rough platform, surrounded by a circle of incandescent and seemingly fused lava, —sometimes the outer circle forms a solid shelf, within which an inner basin of lava boils at a greater or less depth below its edge. It is evident, from the account of this crater given by Professor Dana, in the American Journal of Science, as gathered from the relations of various observers during nearly a century past, that the surface of a vast boiling lake of subterranean lava existing here, rises and sinks at irregular intervals of several years in duration; sometimes filling the entire cavity, and even pouring over its outer margin sheets of a very liquid lava,—sometimes sinking to a depth of a thousand feet or more,—especially when some outburst from a lower vent, or chain of vents, has *tapped* the internal reservoir. But, however interesting the characteristic features of this crater, both from the facilities it affords for observation, and the great scale on which they are developed, they do not seem to me to prove the origin of the cavity other than that of ordinary craters. The phænomena of Kilauea are not so exceptional as at first view might be supposed. Visitors who looked down into the great Vesuvian crater for a few years after its formation in 1822, saw pools of liquid and incandescent lava at its bottom, and small cones of scoria thrown up by an almost constant ebullition. The difference in the violence of the explosions, and in the amount of ejected scoria, arises, no doubt, as Professor Dana very justly observes, from the difference in the relative liquidity of the lavas,—those of Kilauea being very liquid, those of Vesuvius much more viscid and unyielding*. So also during the Vesuvian eruption of 1753, persons who ventured to the summit of the cone observed jets of liquid lava thrown up from the surface of a mass which occupied the bottom of the crater, and conducted itself exactly in the manner of a liquid in ebullition. Spallanzani remarked a similar appearance within the great crater of Etna in 1788. In the volcano of the Isle of Bourbon, Bory de St. Vincent describes a source of very liquid and glassy lava ceaselessly and somewhat tranquilly boiling over in concentric waves from the summit of a dome-shaped hillock composed of its overflowings.

Circular form of Craters.—A consideration which has not, perhaps, been sufficiently adverted to by geologists speculating on the origin of volcanic craters, is the cause of their invariably circular or nearly circular figure. If I am right in attributing their formation exclusively to aëriform explosions, it follows that each is, in fact, simply the external orifice of a more or less cylindrical bore drilled through the pre-existent rocks by repeated discharges of highly expansive aëriform fluids (probably for the most part steam) forcing their way upwards at some weak point; and that it is to the equal pressure in all directions of the expanding fluid that the circular form of the section of this orifice is due,—the same cause, in fact, which gives a spherical form to a bubble of air or gas rising through water. Indeed the eruptive explosions must be considered as occasioned by the rise of a succession of enormous bubbles from a great depth in the fluid lava below. Each single explosion attests the bursting of

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* Dana, American Journal, 1850, vol. ix. p. 383.

[Note by J. D. Dana.—I do not regard the origin of the crater of Kilauea essentially different from that of other craters. But there is this peculiarity, that the lavas have not in modern times, at least, overflowed the pit; and moreover the country around, neither in its height or slopes or scoria bears evidence of long-continued overflows. There is no cone about the crater. It is a pit, which certainly overflowed at first, but for a long period has discharged itself by lateral fissures. There are several other large pit-craters in the vicinity of Kilauea which are without any cones or slopes around them, being literally pits; they once contained boiling lavas to their top like the small active pools in the bottom of Kilauea, but a subterranean opening discharged them, and left a deep pit with vertical walls like Kilauea. The sides of the walls in such a case are as clear from scoria as a cliff of stratified limestone, because the undermining, owing to the drawing-off of the lavas, caused the sides to a certain distance around to fall from want of support, and so leave fresh fractures. I have attributed the origin of the Val di Bué (Bové) of Etna to the same cause that has produced Kilauea, and I still believe the view right. There is, in a certain sense, an "engulfment;" and so there is in the eruptions of Vesuvius. Mr. Scrope writes as if I had described from the observations of others alone, and does not appear to have seen my Report on the Volcanoes of the Pacific in my Geological volume connected with the Exploring Expedition.]

such a bubble from the surface of the liquid mass of lava in the vent. In moderately tranquil eruptions these succeed each other at considerable intervals. In the case of Stromboli, I noted that about five minutes usually occurred between every two explosions. When the eruption assumes a violent character, as in the Vesuvian one of 1822, the eructations, for such they are, succeed each other so rapidly as to produce an almost continuous roar, like the blowing-off of a thousand steam-boilers. And each explosion gives birth to one of those great globular volumes of white vapour, which, rolling over and over each other as they rise in the air in a vast column, occasion one of the most remarkable and magnificent appearances of a paroxysmal volcanic eruption. In the midst of these clouds of snowy vapour, a black column of stones, scoria, and ashes may be seen to shoot up to a vast height, generally attended with copious discharges of electricity generated by the friction of the ejected fragments, and forming a singular contrast to the jet of aëriform matters.

In some rare cases it is possible to witness the actual rise and bursting of these great bubbles of vapour. Spallanzani on his visit to Stromboli in 1780 saw the liquid surface of lava at a white heat within the orifice of the volcano surge alternately upwards, and after bursting like a great bubble, fall back again out of sight. In 1819 I was myself able to witness the same interesting phænomenon probably from the same position, a high point of the external crater-rim which overlooks the vent. At each belch, a shower of tattered fragments of lava, torn from the surface of the bubble as it broke, rose into the air with a cloud of vapour and a fierce roar, while steam seemed to be at intervals blowing off from another neighbouring vent. Hoffman, who visited the same volcano a few years later, describes in minute detail precisely the same phænomena.

The vast size of some craters, already noticed, may afford a notion of the enormous volumes of gaseiform matter that must have been discharged through them at the time of their formation by continuous explosions lasting for weeks and even months; since each individual bubble of vapour must have been of a magnitude to fill the entire horizontal section of the crater; and even for some time to aid in enlarging the area of this aperture by violent pressure against its rocky sides. The prodigious force with which they ascend, and therefore the great depth at which they are generated, may be judged from the vast vertical height, measured in miles, to which they have been seen to shoot up a continuous columnar fountain of ejections, consisting not merely of scoria and ashes, but often of rocky fragments of great size.

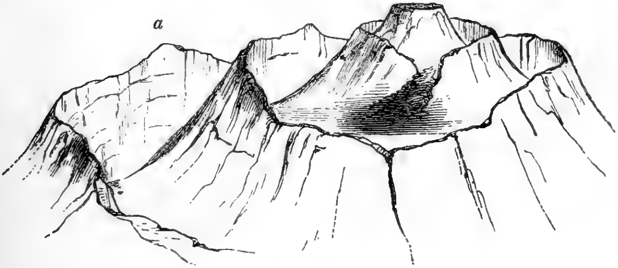
These, by their mutual friction, as they alternately fall back and are thrown up again, become, as has already been said, greatly comminuted; and the source of the explosive vapours having sooner or later exhausted its energies, the accumulation of these ashes in the vent at length appears to stifle their further development, and quiescence for a time ensues. [I am speaking here, of course, of the case of such a paroxysmal eruption as I had the advantage of witnessing in 1822.]

I have said that every crater is more or less circular in figure; but, since the orifice of discharge will almost necessarily be opened on the least resisting point of some fissure broken through the solid pre-existing rocks, we might expect its section to be often lengthened in the direction of this fissure, and consequently to be rather oval than strictly circular. And this expectation is justified by observation. Sometimes two orifices have been opened upon the same fissure so near together that their craters or cones intersect each other. In the range of Puys of Auvergne and the Velay such examples are frequent. And in the eruption of 1850 of Vesuvius two craters were formed on the summit of the cone divided only by a narrow ridge; their common horizontal axis coinciding with the line of the great fissure, which in the preceding year had been visibly broken through the side of the cone towards the north-east. Sometimes aëriform explosions take place from openings upon lateral fissures, and produce those minor, or (as they are often called) parasitic cones, of which several examples occur on the flanks both of Vesuvius and Etna. At other times the explosions are confined to the central vent of the volcano, the lava alone welling out, perhaps, at some lateral orifice. This, indeed, is the normal character of these phænomena. And it is this habitual predilection (as it may be called) of volcanic eruptions for the same identical vent, that occasions in so many instances the heaping-up of some vast mountain mass above and around it, subject to the occasional blowing-up of the central portion, to be re-formed again and again by subsequent eruptions. The result of the irregular alternation of these paroxysmal explosions and subsequent gradual expulsions of new matter is the appearance, so common in volcanic mountains, of a minor and central cone with its crater, rising within the circumference of some larger crater of earlier date, or in its immediate vicinity. The walls of the latter crater are of course often broken down on one or more sides (generally on the line of the original fissure);—perhaps reduced to a mere segment of its original circuit, by the combined operation of volcanic convulsions and aqueous erosions. Whoever will take the trouble to examine carefully an accurate map, on a sufficiently large scale, of almost any volcanic district (such, for example, as Vesuvius and the Phlegræan Fields, Etna and the Lipari Isles, the

Roman territory, the Grecian Archipelago, Madeira, Teneriffe, the Azores, Bourbon, St. Helena, Barren Island, the Leeward Isles, &c.), will see numerous unquestionable examples of this law by which crater is formed within crater, and new cones upon the ruins of old ones.

History of Vesuvius.—At the risk of repetition, I must be permitted to illustrate this law by the trite, but instructive, example of Vesuvius,—which only comes so often before us because from its proximity to Naples it has been open to more constant and accurate observations than any other known volcanic mountain. What, in brief, is the history of this volcano during the last century? Precisely one hundred years ago, in the year 1756, Vesuvius possessed no less than three cones and craters, one within the other, like a nest of boxes, besides the great encircling crater and cone of Somma (fig. 1). Sir W. Hamilton gives us a drawing of its appearance in this state.

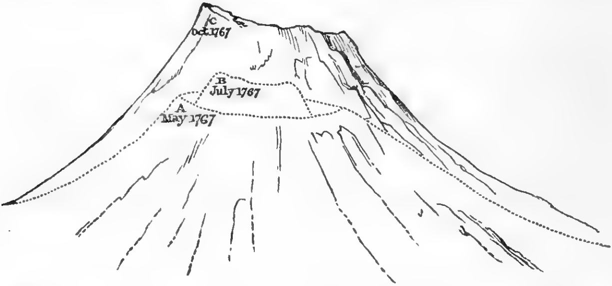
Fig. 1.—*Outline-sketch of Vesuvius as it existed in* 1756. (After Sir W. Hamilton.)



a. Somma.

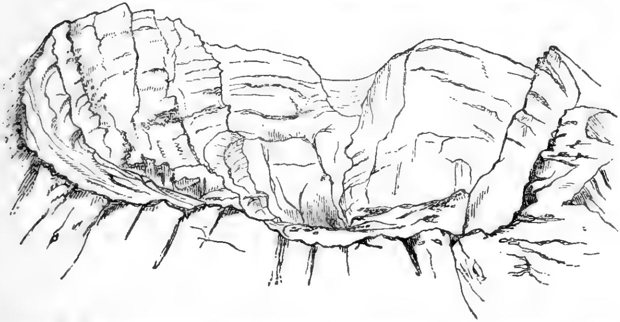
By the beginning of the year 1767, the continuance of moderate eruptions had obliterated the inmost cone and increased the intermediate one, until it very nearly filled the principal crater (fig. 2, A, B). An eruption in October of that year, 1767, completed the process, and re-formed the single cone into one continuous slope all round from the apex downwards (fig. 2, C). The dotted lines in fig. 2 (after Hamilton) represent the shape of the outer and inner cones before this eruption, and the space between them and the firm outline represents the amount by which the cone was in the intervening ten years augmented in bulk and height by the ejectamenta of that eruption. An interval of comparative tranquillity followed, until, in 1794, the paroxysmal eruption occurred, described by Breislak, which completely gutted this cone, then solid, lowered its height, and left a crater of great size bored through its axis. Later eruptions, especially that of 1813, not merely filled up this vast cavity with their products, but once more raised the height of the cone by some hundred feet. When I first saw it in 1819 the top formed a rudely convex platform, rising towards the south, where was its highest point. Several small cones and craters of eruption were in quiet activity upon this plain, and streams of lava trickled from them down the outer slopes of the cone. So things went on until October 1822, when the entire heart of the cone was again thrown out by the formidable explosions I have so often referred to, and a vast crater was opened through it; while the cone itself was found to have lost several hundred feet from its top. In fact nothing but an outer shell of it was left (fig. 3). Eruptions, however, soon recommenced. In 1826–7 a small cone was formed at the bottom of the crater, and, continuing in activity, had reached a height which rendered it visible from Naples in 1829, when of course it must have nearly filled up the crater. In 1830 it was 200 feet higher than the crater's rim; and in 1831 this cavity was completely filled, and the lava-streams began to flow over it down the outer cone. In the winter of that year a violent eruption once more emptied the bowels of the mountain, and left a new crater, which soon began to fill again from ejections upon its floor; and by the month of August 1834 this crater had been in its turn obliterated, and lava overflowed its edge towards Ottaiano. In 1839 the cone was again cleared out, and a new crater appeared in the shape of a vast funnel, accessible to its bottom, which for a few years then remained in a tranquil state. In 1841, however, a small cone began to form within it, and increased so rapidly, that in 1845 it was visible from Naples above the brim of the crater, which

Fig. 2.—*Outline-sketch of Vesuvius as it appeared in October 1767; with dotted outlines of its form in July and in May of the same year.*



soon after was completely filled. And the cone from that time went on increasing in bulk and height from the effect of minor eruptions, until in 1850 one of a violently explosive character opened the two deep craters on its summit, of which I have already spoken. The more recent eruption of May last, being confined chiefly to a prodigious efflux of lava from the outer side of the cone, unaccompanied by any extraordinary explosive bursts from the summit, has not altered materially the form impressed upon it in 1850.

Fig. 3.—*Crater of Vesuvius after the Eruption of October* 1822.



It is thus seen that within the last 100 years the cone of Vesuvius has been five several times gutted by explosive eruptions of a paroxysmal character, viz. in 1794, 1822, 1831, 1839, and 1850; and its central craters formed in this manner as often gradually refilled with matter, to be again in due time blown into the air. Meanwhile the old external crater of Somma is itself becoming choked up by the accumulation of all the lava-streams and fragmentary matter that are expelled towards the northern and outer side of the cone. It would be, therefore, in exact accordance with the habit of this volcano (as of volcanic mountains in general), if, after some further period either of quiescence or of moderate activity, the entire cone of Vesuvius should be blown up by a more than ordinarily violent paroxysm, and the crater of Somma itself reformed.

With this well-authenticated history of the mountain within our knowledge, would it not be wholly unphilosophical to deny (except upon such grounds of impossibility as have never been adduced) that the larger containing crater in the case of Vesuvius (and the argument applies to other similar volcanic mountains) had the same origin as the smaller contained ones; and that the external cones were produced in the same manner as the internal and similarly constituted ones? And therefore those who refuse to believe the former to be of eruptive origin must be prepared to extend their incredulity to the latter. Indeed the elevation-crater theorists usually do not shrink from this consequence. With them the cone of Vesuvius, and that of Monte Nuovo itself, were not the products of eruption, but of elevatory expansion by a single shock. Obviously, it ought to follow, that no volcanic mountain was ever in eruption at all, that the whole is an ocular illusion; at least, that the lava-streams we see pouring for weeks and months from the summit of a cone and hardening there, and the enormous showers of fragmentary matter which, during equally long periods, we see thrown up from the crater and falling on the surface of the cone, do not, even in the lapse of ages, add to its bulk, or tend by their frequent repetition to compose the substance of a volcanic mountain, but by some unaccountable process disappear without leaving a trace behind. I own that, to my mind, such an hypothesis is wholly unintelligible. I see in the ordinary phænomena of a volcanic mountain, such as I have described them in the brief record of the principal eruptions of Vesuvius during the last century, a simple and natural process by which such a mountain is gradually built up; and having observed this mode of formation going on in some instances before my eyes, I think it reasonable to apply it to explain the mode of formation of other mountains of the same class with their cones and craters, old and new, central and lateral, or parasitic; and making allowance, as I said at first, for a certain amount of internal accretion and elevation, by means of intrusive dykes and earthquake shocks, I know nothing in the appearance, figure, or structure of any volcanic mountain yet discovered, which such an ordinary and observed mode of formation will not account for.

[To be continued.]

XIV. *Note on the Equipotential Curve* $\frac{m}{r}+\frac{m'}{r'}=\mathrm{C}$.

By Arthur Cayley, *Esq.*[*]

The equation $\frac{m}{r}+\frac{m'}{r'}=\mathrm{C}$, where m, m', C are constants, and r, r' are the distances of a point P of the locus from two given points M, M′ respectively, expresses that the potential of the attracting or repelling masses m, m' has a constant value at all points of the locus. The locus is obviously a surface of revolution, having the line through the points M, M′ for its axis; and instead of the surface, we may consider the section by a plane through the axis, or what is the same thing, we may con-

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[*] Communicated by the Author.

sider r, r' as the distances *in plano* of a point P of the curve from the given points M, M′: such curve may be termed the equipotential curve. I propose in the present Note to investigate in a general manner, and without entering into any analytical detail, the general form of the curve corresponding to different values of the quantity C.

It is proper to remark, that the curve is not altered by changing the signs of each or any of quantities m, m', C (in fact, analytically the distances r, r' are essentially ambiguous in sign), so that we may without loss of generality consider m, m', C as all of them positive. The different branches of the complete analytical or geometrical curve have distinct mechanical significancies; thus r, r' being positive, $\frac{m}{r}+\frac{m'}{r}=C$ is the curve for which the potential of the attracting masses m, m' is equal to C; but $\frac{m}{r}-\frac{m'}{r'}=C$ is the curve for which the attracting mass m, and the repulsive mass m', have the potential C; but this is a distinction to which I do not attend. I write for homogeneity $\frac{k}{a}$ instead of C, where a is the distance between the points M, M′; the equation thus becomes

$$\frac{m}{r}+\frac{m'}{r'}=\frac{k}{a}.$$

Where a is a positive distance, m, m', k may be considered as positive abstract numbers. The curve is obviously a curve of the eighth order. When k is large in comparison with m, m', then since r, r' cannot be both of them small in comparison of a (for if one be small, the other will be nearly equal to a), it is clear that one of these distances, for instance r, will be small, and the other r' nearly equal to a. We in fact have (neglecting in the first instance $\frac{m'}{r'}$ in comparison with $\frac{m}{r}$) $\frac{m}{r}=\frac{k}{a}$, or more accurately, $\frac{m}{r}=\frac{k\pm m'}{a}$, i. e. $r=\frac{m}{k\pm m'}$, which shows that a part of the curve consists of two ovals, which are approximately concentric circles, radii $\frac{m}{k\pm m'}a$, about the point M as centre. In like manner a part of the curve consists of two ovals, which are approximately concentric circles, radii $\frac{m'}{k\pm m}a$, about the point M′ as centre. I denote by A, B, the two ovals about M, viz. A is the exterior, and B the interior oval; and in like manner by A′, B′ the two ovals about M′, viz. A′ is the exterior, and B′ the interior oval. The distances *inter se* of the ovals A and B, or of the ovals A′ and B′, are small in comparison with the radii of these ovals respectively; and if, to fix the ideas, m' be greater than M, then the ovals A′, B′ are greater than the ovals A and B.

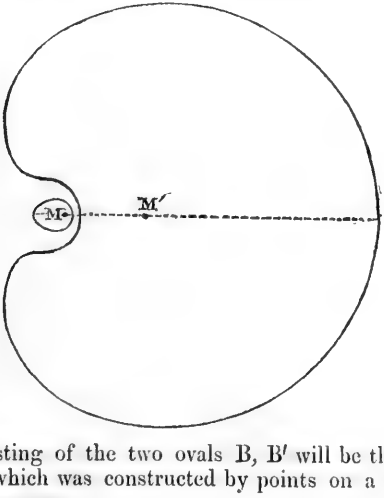
It is easy to see that the curve will have a node or double point on the axis if $k=(\sqrt{m'}\pm\sqrt{m})^2$; and we must first consider the case $k=(\sqrt{m'}+\sqrt{m})^2$. The node lies between the points M, M′, and its distances from these points are respectively as $\sqrt{m}:\sqrt{m'}$, that is, it is nearest to M. The transition from the original form is very obvious; the exterior ovals A, A′ have gradually expanded until they come in contact, and at the instant of doing so the two ovals change themselves into a figure of eight, AA′. The ovals B, B′ also expand and change their form, but they preserve the general character of ovals enclosing the points M, M′ respectively. The curve consists of a figure of eight AA′, and (inside of the two divisions thereof respectively) of the ovals B, B′ enclosing the points M, M′. The half of the curve nearest to M′ is, as before, preponderant in magnitude.

The next change when k continues to diminish is an obvious one: the figure of eight opens out into an hourglass-shaped oval AA′, while the ovals B, B′ continue increasing in magnitude and altering their form.

There will be again a node or double point when $k=(\sqrt{m'}-\sqrt{m})^2$; but to explain the transition to this special form, it is necessary to attend more particularly to the change of form in the oval B′ as k approaches to the value in question, viz. this oval lengthens out and begins to twist itself round the oval B; and when k' becomes $=(\sqrt{m'}-\sqrt{m})^2$, then the oval B′ has completely encircled B, the two extremities of B′ meeting together at the double point, which is a point beyond M (*i. e.* on the other side to M′), such that its distances from M, M′ are in the ratio of $\sqrt{m}:\sqrt{m'}$. And at the instant of contact there is, as in the former case, a modification of the form of the portions which come into contact, so that the node is an ordinary double point. The oval B′ has, in fact, become what may be termed a re-entrant figure of eight, , the small part of which encloses the oval B which encloses the point M, while the large part encloses the point M′. The curve consists of the exterior oval AA′ (which has probably lost wholly or partially its hourglass form, and is more nearly an ordinary oval), of the re-entrant figure of eight, B′, and of the enclosed oval B.

As k continues to diminish, the re-entrant figure of eight, B′, breaks up into two detached ovals lB′, mB′, the larger of which, lB′, encloses the other one and also the point M′; while the smaller one, $\frac{1}{2}B'$, does not enclose M′, but encloses the oval B which encloses M; the curve consists of the exterior oval AA′, the ovals lB′ and mB′ which have arisen out of the oval B′, and the oval B. As k further decreases, the ovals AA′ and lB′ continually increase in magnitude, and the ovals mB′ and B approximate more and more nearly together; and at length, when k becomes $=0$, the ovals AA′ and lB′ disappear at infinity, while the ovals mB′ and B unite themselves into a circle enclosing M, but not enclosing M′: the equation of this circle is, in fact, $\frac{m}{r}+\frac{m'}{r'}=0$; or what is the same thing, $r^2=\frac{m^2}{m'^2}r'^2$, and the points M, M′ have, in relation to this circle, the well-known relation that each is the image of the other.

The preceding description is, I think, intelligible without the assistance of a series of figures illustrating the different forms of the curve, but there is no difficulty in actually tracing the curve for any particular values of the constant parameters. Thus (taking the distance MM′ for unity) suppose that the equation of the curve is $\frac{1}{r}+\frac{4}{r'}=1.2$. (The value 1.2 was selected as a value not far from that for which the oval B′ becomes a re-entrant figure of eight, though the change of form is so rapid that this value shows only the incipient tendency of the oval B′ to take the form in question.) The form of the portion of the curve consisting of the two ovals B, B′ will be that shown by the figure, which was constructed by points on a double scale with some accuracy.



The case $m=m'$ is an exception, and must be considered separately: the curve is here in all its changes symmetrical about a perpendicular to the axis midway between the two centres M, M′. The curve in the first instance, *i. e.* when k is greater than $(\sqrt{m}+\sqrt{m})^2=4m$, consists of the two ovals B, A about M, and the two ovals B′, A′ about M′. As k decreases to $4m$, the two ovals A, A′ gradually increase in magnitude, and at length come together, as before, into a figure of eight, AA′; and as k continues to diminish, the figure of eight opens out into an hour-glass form AA′, which continues increasing in magnitude, and degenerating into the form of an oval. The interior ovals B, B′ approach more and more nearly together, lengthen out in the direction perpendicular to the axis, and present to each other a more and more flattened portion. The second value,

$$k=(\sqrt{m'}-\sqrt{m})^2,$$

which in the general case gives a node, in the present case only arises when $k=0$; and there is not then any node, but the curve degenerates in a similar manner to what happens for $k=0$ in the general case; viz. the oval AA′ disappears at infinity, while the ovals B, B′ coalesce together (their outer parts disappearing at infinity) into a pair of lines coincident with the perpendicular to the axis midway between the two centres.

2 Stone Buildings,
May 31, 1857.

XV. *Notices respecting New Books.*

Gasometry, comprising the leading Physical and Chemical Properties of Gases. By Robert Bunsen, *Professor of Chemistry in the University of Heidelberg. Translated by* Henry E. Roscoe, *B.A., Ph.D. With Sixty Illustrations.* London: Walton and Maberly.

FOR many years the reputation of Bunsen as a gas analyst has been so high, that working chemists generally have felt the necessity for a complete collection of the processes, the methods of manipulation, and the formulæ employed by him in his highly important and valuable researches. It is for such persons that the volume before us is intended.

One of the reasons, perhaps, which makes the methods of Bunsen more popular than those of Regnault and Reiset is, that they do not require the operator to start with so expensive an apparatus. But the method of Regnault, especially as regards the convenience of the apparatus, has great advantages, the chief fault being that by its use large variations in bulk are expressed by small numbers. One of the most successful of Bunsen's pupils[*] has so far combined the two me-

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[*] Professor Frankland.

thods, that while he takes the beautiful apparatus of Regnault as his model, he avoids his method of determining volumes by ascertaining the pressure of a constant volume.

The operations of gaseous analysis, more perhaps than any of the numerous branches of chemical research, involve the necessity for skill and tact in the construction and management of delicate apparatus. For some years past we have been accustomed, in perusing the researches of the more distinguished of his pupils, to notice how greatly the production of those researches was facilitated by the use of appliances having a character peculiar to the contrivances of Bunsen. They possess a neatness, a perfect adaptation to the purpose in view, which renders it as easy to distinguish the style of Bunsen in his apparatus, as of some artists in their pictures. The same remark applies with equal, if not greater force, to instruments owing their origin to Regnault.

We find in the work before us a complete treatise, not merely on the methods of examining and analysing gases, but on the construction of all the apparatus described, and the manipulation necessary therewith.

The work commences with a description of the methods of collecting gases from geisers and springs, with certain parts of the apparatus for which we are familiar, modifications of them having been used in some of the researches of Bunsen's pupils. The same remark applies to much of the apparatus used in the transference of gases, we having already become acquainted with the peculiar glass gasometer (fig. 16), described in a paper of Dr. Maxwell Simpson's (worked out in Bunsen's laboratory) on some new methods of determining nitrogen.

The construction and graduation of eudiometers, according to the author's own method, is very fully entered into; and now that chemists are pretty generally agreed on the superiority of his plan over any other, will be found very useful.

One great advantage of the work is, that none of the more minute details are omitted on the score of their being unimportant; such, for example, as the best and safest mode of cleaning eudiometers, &c. The book becomes, therefore, a truly practical guide for the working chemist.

Those who have been accustomed to eudiometrical analyses, have all found how much time is often wasted owing to the electrical apparatus being more or less out of order, or the atmosphere of the laboratory being so damp as to render it difficult at a short notice to obtain a spark strong enough to fire the mixture. This difficulty is entirely avoided by the contrivance described in the book, which, on account of its extreme usefulness, we transfer to our pages.

"The firing of the gaseous mixture is always effected by the electric spark. A small cylinder about 3 inches high and 1 broad serves as a Leyden jar. This cylinder is lined inside with tinfoil; but in order to avoid amalgamation, the outer metallic coating consists of platinum-foil. Electrophori, or common electrical machines, are very apt to become useless by remaining in the damp and cold rooms which are most suitable for gas laboratories. The jar is therefore best charged by means of the simple and effectual arrangement represented in fig. 32.

"It consists merely of a large porcelain tube, which, when held before the iron wire of the cylinder and rubbed with silk and amalgam*, evolves so much electricity that the jar is charged in a few seconds."

One great advantage of Bunsen's mode of treating the subjects is, that he illustrates by numerous examples, and enters fully into the best and shortest modes of calculating the results.

A considerable portion of the volume is devoted to the description of the methods required to meet special cases of gaseous analysis, the whole being copiously illustrated by examples, and even the most minute precautions being detailed.

Much new and interesting matter of the highest value is contained in the portion treating of the "manipulation in the absorption of gases;" and having described the order of the processes, the author proceeds to develope a general formula, enabling the operator to calculate the relative volumes of the constituents of a mixture of known gases.

The chapter on the specific gravities of gases strongly shows the neatness and ingenuity of the author as an operator. This is well seen in his illustration of the determination of the density of gaseous bromide of methyle, made with a flask of only 44 cubic centimetres capacity. Notwithstanding the small volume, the experiment gave 3·253, theory requiring 3·224.

In addition to methods involving the necessity for weighings, he uses effusion as a mode of determining the densities of gases where only small quantities are at the operator's disposal. The gas effuses through a minute aperture in a thin platinum plate attached to an apparatus resembling an inverted Mohr's burette with a glass stopcock. The observations of time are made with a pendulum vibrating half-seconds. The contrivance by means of which the times of effusion of the gases are determined, and warning is given of the approach of the termination of the experiment, is both simple and ingenious. In fact, when we couple the simplicity of the mode of operating with the fact that an experiment can be made on so small a quantity of gas as two cubic inches, it is scarcely too much to say that in many researches it will entirely do away with the necessity of weighings.

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* "The amalgam, by the aid of which a porcelain tube 3 feet long and 1½ inch thick may be made to supply the place of a tolerably powerful electrical machine, is made as follows:—Two parts of mercury are heated in a common test-tube, and one part of thin zinc-foil, and one part of zinc added whilst the metal is well stirred. In order to make the amalgam more plastic, it is melted and stirred several times, and then placed in a piece of the thickest and best silk, which serves as a rubber. In rubbing the tube, the silk is so arranged that only half the surface in contact with the porcelain is covered with amalgam, the remainder being left free. The powerful action of the amalgam begins generally after it has been some time in use, and it preserves its activity often for months."

A large portion of the work is devoted to the consideration of the laws of the absorption of gases in liquids, the methods of determining the coefficients of absorption, and numerous examples showing the coefficients of a considerable number of gases. A most valuable result of the perfection to which the determination of the coefficients of absorption can be carried, and one that cannot be too highly appreciated by chemists, is, that it enables us to ascertain whether a gas under examination is a mixture in atomic proportions or a truly chemical compound. It is well known to all who have paid any attention to this branch of chemistry, that Frankland and Kolbe have proved that eudiometric analysis (as might be expected) was incapable of showing the difference between two volumes of marsh-gas and equal volumes of hydrogen and methyle. Any process capable of throwing light on questions so difficult of solution will doubtless be thankfully received by chemists. In the work before us, Bunsen shows how perfectly the question may be solved by the method alluded to, even in cases like the present, where the greatest difficulty might have been anticipated, from the fact that the differences in the coefficients of absorption are very little. The result obtained by the absorptiometric method clearly indicates that marsh-gas, prepared by heating together the acetate with hydrate of potash, and purified by means of fuming sulphuric acid and potash, is not a mixture of methyle with hydrogen, nor an isomer of natural marsh-gas, but the same substance which is evolved by the mud volcanoes of Bulganak in the Crimea.

The author has incorporated in his work the results of the researches which he made some years ago (in conjunction with Prof. Stegmann) on the laws of the diffusion of gases. As usual, the apparatus employed and the manipulation generally are both novel and ingenious.

He deduces several facts from his experiments; among others, that the pores of the gypsum diaphragms do not act towards gases passing through them as a system of fine openings in thin plates, but as a system of capillary tubes. He has also made elaborate experiments to solve several other important questions, among which the following stands prominently forward:—"Do the volumes of two gases which have diffused into each other, stand to each other, as is universally admitted, inversely as the square roots of their densities?" To our surprise we find that a negative answer is returned to this question. We do not feel it necessary to enter into a detailed account of the author's experiments and deductions, as he admits this portion of the investigation to be still in an unfinished state.

A very important portion of the volume, and one which will be read with interest by all who are engaged in eudiometry, is on the phænomena of the combustion of gases. The author considers in this, the last portion of the work,—the heat of combustion—the temperature of the combustion—the explosive force of gases—and the temperature of ignition of gases. Several other instructive and interesting properties of gases are studied incidentally, such as their diathermanous properties, and the influence of diluents; moreover, the author shows the bearings of the facts educed on the action of affinity.

The volume concludes with copious and most valuable tables for the calculation of analyses.

To the chemist who is engaged in investigations of this nature, such a volume as that before us is invaluable; unhappily their number in this country is so limited, that we fear the work is likely to have a less-extended sale than it deserves.

The translation has been entrusted to one of the author's pupils, Dr. H. E. Roscoe, and it appears to be most carefully made.

XVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 72.]

January 8, 1857.—William Robert Grove, Esq., V.P., in the Chair.

THE following communications were read:—

"Experimental Researches on the Strength of Pillars of Cast Iron." By Eaton Hodgkinson, Esq., F.R.S., Professor of the Mechanical Principles of Engineering, University College, London.

In a previous paper on this subject (Philosophical Transactions, 1840), I had shown,—1st, that a long circular pillar, with its ends flat, was about three times as strong as a pillar of the same length and diameter with its ends rounded in such a manner that the pressure would pass through the axis, the ends being made to turn easily, but not so small as to be crushed by the weight; 2ndly, that if a pillar of the same length and diameter as the preceding had one end rounded and one flat, the strength would be twice as great as that of one with both ends rounded; 3rdly, if, therefore, three pillars be taken, differing only in the form of their ends, the first having both ends rounded, the second one end rounded and one flat, and the third both ends flat, the strength of these pillars will be as 1—2—3 nearly.

The preceding properties having been arrived at experimentally, are here attempted to be demonstrated, at least approximately.

The pillars referred to in my former paper were cast from Low Moor iron No. 3; they were very numerous, but usually much smaller than those used in the present trials. I felt desirous too of using the Low Moor iron in the *hollow* pillars employed on this occasion, not on account of its superior strength, but its other good qualities. The pillars from this iron were cast 10 feet long, and from $2\frac{1}{2}$ to 4 inches diameter, approaching in some degree, as to size, to the smaller ones used in practice. The results from the breaking weights of these were moderately consistent with the formulæ in the former paper, with a slight alteration of the constants, rendered necessary by the castings being of a larger size, and therefore softer than before, a matter which will be adverted to further on.

The formulæ for the strength of a hollow pillar of Low Moor iron No. 2,—where w is the breaking weight, in tons, of a pillar whose length is l in feet, and the external and internal diameters D and d in inches, the ends being flat and well bedded—are as below:

$$w = 46{\cdot}65 \times \frac{D^{3\cdot55} - d^{3\cdot55}}{l^{1\cdot7}},$$

from formula in Phil. Trans. 1840;

$$w = 42{\cdot}347 \times \frac{D^{3\cdot5} - d^{3\cdot5}}{l^{1\cdot63}},$$

from formula in present paper.

To obtain some idea of the relative strengths of pillars of different British irons, I applied, at Mr. Stephenson's suggestion, to Messrs. Easton and Amos, who procured for me twenty-two solid pillars, each 10 feet long and 2½ inches diameter, cast out of eleven kinds of iron (nine simple irons and two mixtures). The pillars were all from the same model, and were cast vertically in dry sand, and turned flat at the ends, as the hollow ones had been; two being cast from the same kind of iron in each case. The simple unmixed irons tried were as below, and all of No. 1.

		Mean breaking weight.
Old Park iron	Stourbridge	29·50 tons.
Derwent iron	Durham	28·03 ,,
Portland iron	Tovine, Scotland	27·30 ,,
Calder iron	Lanarkshire	27·09 ,,
Level iron	Staffordshire	24·67 ,,
Coltness iron	Edinburgh	23·52 ,,
Carron iron	Stirlingshire	23·52 ,,
Blaenavon iron	South Wales	22·05 ,,
Old Hill iron	Staffordshire	20·05 ,,

The mean strength of the pillars from the irons above varies from 20·05 to 29·50 tons; or as 2 to 3 nearly.

The pillars formed of mixed irons were found to be weaker than the three strongest of the unmixed series.

From many experiments, it was shown that the weight which would crush the pillars, if they were very short, would vary as 5 to 9 nearly.

The pillars in general were broken of four different lengths, 10 feet, 7 feet 6 inches, 6 feet 3 inches, and 5 feet, the ends of all being turned flat, and perpendicular to the axis. It was found that when the length was the same, the strength varied as the 3·5 power of the diameter; and when the diameter was the same and the length varied, the strength was inversely as the 1·63 power of the length. Both of these were obtained from the mean results of many experiments.

The formula for the strength of a solid pillar would therefore be

$$w = m \times \frac{d^{3\cdot5}}{l^{1\cdot63}},$$

where w is the breaking weight, d the diameter in inches, l the length in feet, and m a weight which varied from 49·94 tons in the strongest iron we tried, to 33·60 tons in the weakest.

The ultimate decrement of length, in pillars of various lengths but of the same diameter, varies inversely as the length nearly. Thus the ultimate decrements of pillars 10 feet, 7 feet 6 inches, 6 feet

3 inches, and 5 feet, vary as 2, 3, $3\frac{1}{2}$ and 4 nearly, according to the experiments, from which it appeared that the mean decrement of a 10-feet pillar was ·176 inch.

Irregularity in Cast Iron.

The formulæ arrived at in this paper are on the supposition that the iron of which the pillars are composed is uniform throughout the whole section in every part; but this was not strictly the case in any of the solid pillars experimented upon. They were always found to be softer in the centre than in other parts. To ascertain the difference of strength in the sections of the pillars used, small cylinders $\frac{3}{4}$ inch in diameter and $1\frac{1}{2}$ inch high, were cut from the centre, and from the part between the centre and the circumference, and there was always found to be a difference in the crushing strength of the metal from the two parts, amounting perhaps to about one-sixth. The thin rings of hollow cylinders resisted in a much higher degree than the iron from solid cylinders. As an example, the central part of a solid cylinder of Low Moor iron No. 2, was crushed with 29·65 tons per square inch, and the part nearer to the circumference required 34·59 tons per square inch; cylinders out of a thin shell half an inch thick, of the same iron, required 39·06 tons per square inch; and other cylinders from still thinner shells of the same metal, required 50 tons per square inch, or upwards, to crush them.

As these variations in cast iron have been little inquired into, except by myself, and have never, so far as I know, been subjected to computation, I have bestowed considerable trouble upon the matter, in an experimental point of view, and endeavoured to introduce into the formulæ previously given, changes which will in some degree include the irregularities observed.

"Memoir on the Symmetric Functions of the Roots of certain Systems of two Equations." By Arthur Cayley, Esq., F.R.S.

The author defines the term *roots* as applied to a system of $n-1$ equations $\phi=0$, $\psi=0$, &c., where ϕ, ψ, &c., are quantics (*i. e.* rational and integral homogeneous functions) of the n variables $(x, y, z, ..)$ and the terms *symmetric functions* and *fundamental symmetric functions* of the roots of such a system; and he explains the process, given in Professor Schläfle's memoir, "Ueber die Resultante eines Systemes mehrerer algebraischer Gleichungen," Vienna Transactions, t. iv. (1852), whereby the determination of the symmetric functions of any system of $(n-1)$ equations, and of the resultant of any system of n equations is made to depend upon the very simple question of the determination of the resultant of a system of n equations, all of them, except one, being linear. The object of the memoir is then stated to be the application of the process to two particular cases, viz. to obtaining the expressions for the simplest symmetric functions, after the fundamental ones of the following systems of two ternary equations, viz. first, a linear equation and a quadratic equation; and secondly, a linear equation and a cubic equation; and the author accordingly obtains expressions, as regards the first system, for the fundamental symmetric functions or symmetric functions of the first degree in respect to each set of roots, and for the symmetric functions of the second and third degrees respectively, and as regards the second system, for the fundamental symmetric functions or symmetric functions of the first degree, and for the symmetric functions of the second degree in respect to each set of roots.

"Memoir on the Resultant of a System of two Equations." By Arthur Cayley, Esq., F.R.S.

The resultant of two equations such as

$$(a, b, \ldots \between x, y)^m = 0$$
$$(p, q, \ldots \between x, y)^n = 0$$

is, it is well known, a function homogeneous in regard to the coefficients of each equation separately, viz. of the degree n in regard to the coefficients $(a, b, \ldots)$ of the first equation, and of the degree m in regard to the coefficients $(p, q, \ldots)$ of the second equation; and it is natural to develope the resultant in the form $k\mathrm{AP} + k'\mathrm{A}'\mathrm{P}' +$ &c., where A, A', &c. are the combinations (powers and products) of the degree n in the coefficients $(a, b, \ldots)$, P, P', &c. are the combinations of the degree m in the coefficients $(p, q, \ldots)$, and k, k', &c. are mere numerical coefficients. The object of the present memoir is to show how this may be conveniently effected, either by the method of symmetric functions, or from the known expression of the resultant in the form of a determinant, and to exhibit the developed expressions for the resultant of two equations, the degrees of which do not exceed 4. With respect to the first method, the formula in its best form, or nearly so, is given in the 'Algebra' of Meyer Hirsch, and the application of it is very easy when the necessary tables are calculated: as to this, see my "Memoir on the Symmetric Functions of the Roots of an Equation." But when the expression for the resultant of two equations is to be calculated without the assistance of such tables, it is, I think, by far the most simple process to develope the determinant according to the second of the two methods.

GEOLOGICAL SOCIETY.

[Continued from p. 74.]

June 17, 1857.—Col. Portlock, R.E., President, in the Chair.

The following communications were read:—

1. "On some Comparative Sections in the Oolite and Ironstone Series of Yorkshire." By John Phillips, M.A., F.R.S., F.G.S., Reader in Geology in the University of Oxford.

The author first called attention to the problem presented to geologists for solution, which is suggested by the extremely unlike mineral aspect, and only partial agreement in fossil contents, of the two separated oolitic tracts of North Yorkshire and the south of England. He showed on what principles W. Smith proceeded in 1817 and subsequent years to investigate the contemporaneous lines in these two tracts, and explained his own researches in the same direction in 1824 and following years. Though in regard to the main determinations arrived at and published by himself in 1829 there is no material change, he thought it desirable to place on record some facts ascertained with precision, which may help to fix our ideas of the affinity of certain calcareous beds in the Yorkshire series with some of the well-known members of the Oolite in the south of England; and at the same time give a basis for inquiry as to the geographical range of the ironstone, coal, and limestone of the district, and, by consequence, the physical conditions of the sea or estuary in which, or on the shores of which, the mingled deposits of the north-eastern part of Yorkshire were deposited.

For this purpose he presented, in the first place, the details of two great general sections, one on the actual sea-coast, the other on the old glacial sea-coast of the Hambleton Hills.

By comparing these sections, and adding to them other lesser sections, he showed the existence of five special plant-producing bands of sandstones and shales (occasionally yielding coal in three zones), four calcareous bands, and several layers of ironstone, in the lower oolitic series; and described in a general sense their geographical distribution, and the relation of certain lines of equal deposition ("isocthonal lines," as he terms them) to the general strikes and dips of the district.

In passing from the sea-coast, near Scarborough, westward toward Thirsk, the uppermost of the four calcareous bands of the coast is obliterated; the others undergo great modifications to the northward and westward. The sandstones, shales, and coal, with ironstone, diminish in thickness from the northern parts of the coast, where they amount to 700 feet; so that on proceeding southward a great part of this whole thickness is lost before reaching the Derwent. The inferior oolite is richly ferruginous north of a line passing from Thirsk to Robin Hood's Bay, having its maximum value in Rosedale; the ironstone-bands at the base of the upper lias are very productive in the same district, and acquire their maximum of value in the district farthest to the north-west, namely about Eston Nab, Lofthouse, and Skinner's-grave.

The discussion of the geographical range of organic remains and physical conditions of the sea-bed was glanced at; but the details were reserved for a further communication.

2. "On the Oolite Rocks of Gloucestershire and North Wilts." By Prof. Buckman, F.G.S.

The object of this communication was to point out the general characteristics of the different members of the Oolitic group of rocks as they occur in the Cotteswold Hills and in the extension of the higher beds of the series into North Wilts. In illustration of this subject the author supplied two carefully prepared sections, the result of surveys made by the professors and students of the Royal Agricultural College of Cirencester. One of the sections extends from the Vale of Gloucester through the bold Cotteswold scarp at Birdlip to the Chalk-hills in Wiltshire; the other from Cirencester, through the Vale of Moreton, to Shipston on Stour; each following one of the great Roman roads.

After noticing the labours of previous workers in the Oolites of this district, especially those of Mr. W. Smith, Mr. Lonsdale, Sir R. Murchison, and Mr. E. Hull, Prof. Buckman commenced the description of the different deposits from the basement-beds of the Inferior Oolite to the Portland Oolite, inclusive. In treating of the Inferior Oolite, the author reviewed the opinions of Murchison, Strickland, and Brodie on the relations of the members of this lowest part of the series, and criticised the views lately brought forward by Dr. Wright as to the association of the Inferior Oolite sands with the Lias, rather than with the Oolite. In this collocation Prof. Buckman does not agree, believing that, although some of the shells from the fossiliferous beds accompanying these sands are peculiar, and, especially as regards a few of the Ammonites, are sometimes Liassic in their type, yet by far the greater portion of the fauna, including the local and non-migrating mollusca, is characteristically Oolitic; and that the two particularly Liassic Ammonites that have been brought forward as evidences of the Liassic character of these sands were derived from the true Lias, far below the sands in question. After the consideration of these basement-beds of the Inferior Oolite, the author described in detail the geological characters of the several members of the Oolite series, viz. 1. the Inferior Oolite; 2. the Fuller's Earth; 3. the Great Oolite, with the Stonesfield Slate; 4. the Bradford Clay; 5. the Forest-marble; 6. the Cornbrash; 7. the Oxford Clay and Kelloway Rock; 8. the Coral-rag; 9. the Kimmeridge Clay; and 10. the Portland Oolite and Purbeck beds at Swindon. The author differed in the details of this description from previous writers in several respects; and, in conclusion, expressed a hope to be enabled to offer remarks on the physical conformation of this Oolitic district, on the palæontology of the strata, and the geological changes thereby indicated, at some future period.

3. "On the Geology of the Southern part of Andalusia between Gibraltar and Almeria." By Prof. D. T. Ansted, A.M., F.G.S.

In this paper the author first described the mica-schists of the Sierra Nevada, in which, on the south-west, irregular deposits of highly argentiferous copper occur. On the north-west, crystalline limestone, with galena, overlies the schists; and thick beds of tertiary marls repose on the limestone. Superficial detritus is irregularly spread over all. Thick beds of shale with copper-ore are at some places found between the old schists and the limestone; near Malaga they pass into a conglomerate and then into triassic and jurassic beds. These shales and the schists are both traversed by serpentine-veins. Not far east of Malaga the author observed a black fœtid magnesian limestone (distinct from the dolomites of the Sierra de Mijas adjacent), underlying shales and sandstones on which rests a grit containing remains of Calamites or Equisetites. This limestone corresponds in position with the conglomerates between the shales and sandstones near Malaga, which last extend along the coast from some distance west of Malaga to the eastern side of the Sierra Nevada. They comprise white and red sandstones with marls, and are occasionally gypsiferous and lignitiferous. Near the top of the series a white sandstone, used for building, has afforded the Equisetites above referred to.

Next in order are the blue and black limestones of the Sierra de Gador on the north side of the Sierra Nevada, passing towards the west into the light-coloured limestones of Gibraltar. These are probably of jurassic age, are much altered, and traversed by fissures containing enormous deposits of galena. The red marble of San Anton, probably of cretaceous age, was next noticed. A peculiar calcareous breccia, reposing on the jurassic and cretaceous rocks, near Malaga, is at the base of the tertiary series; on it lies a hard limestone of oolitic structure, with which is associated a compact rock of the nummulitic series, formed of *Alveolina, Orbitoides*, &c. The upper tertiary formation consists of the Tejares beds,—comprising, 1st. (lowest) blue clay loaded with univalve and bivalve shells, and exceedingly rich in *Foraminifera*; 2. Marly sands, with land, freshwater, and rolled fossils; 3. Coarse gravel, with sands, abounding in Pectens, Oysters, and some other shells, and occurring in the Caleta river-bed. Of later age than the above, are the raised beaches along the coast-line between Malaga and Almeria.

XVII. *Intelligence and Miscellaneous Articles.*

ON SOME SPECIAL LAWS OF ELECTRICAL FORCE.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

It is not without regret that I am led to offer a few remarks on the several notices of my papers on electricity by Professor Thomson and other writers, which have from time to time appeared in the Philosophical Magazine. In the first place, I observe (Phil. Mag. vol. viii. p. 42) Mr. Thomson states, that Dr. Faraday and myself have undertaken researches with a view of invalidating the theory of Coulomb,—certainly a gratuitous and unproved assumption to begin with. Then the experimental result I obtained, $F \alpha Q^2$, is represented as having been adduced by me in opposition to the theory; and the Report made to the British Association in 1837, by the Rev. Dr. Whewell, is referred to in support of Mr. Thomson's views. In this Report I am represented as having been surprised at finding the force to be as the square of the quantity; and in another place, Hist. of Induct. Sciences, vol. iii. p. 28, new edition, it is said that I considered the result as "inexplicable." Now if Mr. Thomson or the Rev. Dr. Whewell, for whom I must ever entertain the greatest regard and respect, will be so good as to point out where in any of my works even the shadow of all this is to be found, I should feel myself very greatly obliged. So far from having been surprised at the result, or considering it inexplicable, I distinctly say in my paper in the Philosophical Transactions for 1834, referred to by Mr. Thomson, p. 226, that "I do not advert to these experiments as containing any unexpected results, but rather in explanation of particular methods of research," &c.; and at page 236 I refer the law in question to electrical induction upon the attracted disc, &c., being just what Mr. Thomson has been so obliging as to repeat, virtually in as many words, and with a view of exposing the delusion he assumes I labour under in regard to the

Coulombian theory. In Mr. Thomson's paper, Phil. Mag. for 1854, vol. vii. p. 193, I observe the following remark:—"The amount of heat is proportional to the square of the quantity discharged, as was first demonstrated by Joule, although it had been announced by Sir Snow Harris, as an experimental result, to be simply proportional to the quantity." Riess, Joule, and Clausius appear to agree as to my having made this announcement, and confidently allude to the inaccuracy and to the refutation of my deductions. Now I do here most emphatically and most positively deny, ever having stated anything of the kind; but since these gentlemen say I *have* done so, and pretend to have corrected my error, I call upon them as a point of honour to say when I made such an announcement, or where in any of my published works it is to be found. The fact is, that so far from having stated the law of electrical heat to be simply as the quantity of electricity discharged, I was really the first to discover with precision, and demonstrate with exactitude, the law in question; and that, too, long before Mr. Thomson and the other gentlemen above mentioned were at all known in the world of science. If Mr. Thomson will turn to pages 67 and 68 of the Transactions of the Plymouth Institution, published in 1830, and quoted in the Journal of the Royal Institution, 1830–31, p. 380*, he will find in the former work a series of original experiments on the heating effects of the electrical discharge, and at page 68 the following announcement:—"It may be hence inferred that the effects of an electrical discharge on a metallic wire, all other things remaining the same, is *directly as the square of the quantity*." See also Journal of Royal Institution, p. 381 (vi.). Moreover the date of my paper is so far back as November 1825; and I may say in the words of Dr. Riess (Phil. Mag. 1854, vol. vii. p. 348), that "an assertion of such a general character as that ventured by Mr. Thomson ought to be the consequence of a careful examination," especially of what has been done at home.

As this subject is of importance, I may further remark, that the experiments of Cuthbertson and others, referred to by Becquerel and quoted by Joule, Phil. Mag. October 1841, can scarcely be said to have established the law in question, or even its probability. In the first place, they had no accurate measure of the quantity of electricity accumulated; they were unaware that twice the quantity accumulated on a given coated surface would counterpoise four times the weight, regulating Cuthbertson's own steel-yard electrometer, in which, a charge of 30 grains was taken as twice that of 15 grains. Their methods of research by the fusion of wires appears to have been anything but exact, and all sorts of tricks were played with the battery, as by breathing into the jars, &c. Cuthbertson says, 'Practical Electricity,' pp. 180 to 186, "If 18 inches of wire be taken and a given charge just causes it to run into balls, much shorter lengths will still be only converted into balls; if only 7 inches be taken, nothing but balls will appear; the only difference will be that the balls will be smaller, and will be dispersed to a greater distance,

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* See also a copy of my paper printed in 1828, in the Library of the Royal Society.

which may be easily overlooked." Now what confidence can be placed in all this as experimental research? Van Marum found his batteries produce a heating effect proportional to the coated surface. Cuthbertson, by his most exceptionable method of breathing into his jars when he wanted a greater effect, did, it is true, obtain a higher ratio, yet no direct satisfactory comparison between the quantity discharged and the heat produced was ever arrived at, and various results ensued. This we see admitted at page 182 of Mr. Cuthbertson's work, Exp. 149; here the law in question evidently failed: at page 185 we observe that when the quantities of electricity were said to be as 2 : 3, another result ensued; for experiments 150 and 151 show that the lengths of wire fused, instead of being as 4 : 9, were as 2 : 6, that is, as 1 : 3. It is quite impossible to repose any confidence in such a state of things. Indeed, we have only to examine Mr. Cuthbertson's experiments attentively as given in his work, and in Nicholson's Journal, 4to, vol. ii. p. 218, to be assured of the inexactitude of the experimental processes. In the latter we find the lengths of wire melted to be as the quantity of electricity: see p. 218. However true, therefore, it may be that Cuthbertson obtained results which led him to imagine that twice the quantity of electricity would melt four times the length of wire, he cannot be said to have demonstrated and established that law; and I may therefore, without any philosophical injustice, claim to have been the first to have clearly developed that law by exact electrical measurements, and by new methods of research, as my paper dated 1825, and quoted in the Journal of the Royal Institution, 1830–31, fully shows. There also will be found the hypothesis advanced by Mr. Joule, that increased velocity is probably the source of the quadruple heat. I endeavour to show that the heat is as the velocity with which the unit of charge traverses the wire, that a double quantity passes with a double velocity, and the effect is "as the momentum," or quantity into velocity. I again therefore ask Mr. Thomson to state when or where I ever announced "as an experimental result," that the heating effect of an electrical discharge was "simply proportional to the quantity of electricity." Mr. Joule, in referring to my paper in the 'Transactions' for 1834, appears to have confounded this question with that of the *same* quantity accumulated under different electrometer intensities, a question discussed in my late differences with Dr. Riess. What I said in my paper in the 'Philosophical Transactions,' and which I still insist on, was simply this: viz. that under whatever electrometer intensity you accumulate a given quantity of electricity, provided the battery surface be undivided, that quantity, when discharged through a metal wire, will still excite in it the same degree of heat. Thus if a quantity of electricity = A, for example, be collected on a Leyden jar, exposing 2·5 square feet of coating, and then be collected on a jar exposing 5 square feet of coating, I say, that notwithstanding the electrical intensity in these two cases may be as 4 : 1 nearly, yet that the discharge of the quantity A in each case will excite the same heat in a metallic wire; that is, if the same charging rod and circuit be employed. And I ask the gentlemen to whom I have just alluded, to make that experiment. Until they do so, they have no right to talk of "the memoir in which Riess refuted my statement," &c. (Phil. Mag. 1854, vol. vii. p. 297).

I remain, Gentlemen,
Your obedient Servant,
W. S. Harris.

Windsor Villas, Plymouth,
July 16, 1857.

ON THE COMPOSITION OF BEUDANTITE. BY M. RAMMELSBERG.

The author obtained Beudantite from Cork from Dr. Krantz; it formed small, green octahedra, partially covered with a rust-coloured coating, which, like those from Horrhausen, were seated upon a brownish-black, racemose brown hæmatite, or rather iron-sinter.

When heated in the air, Beudantite furnishes an acid liquid, but nothing else of a volatile nature, and becomes red. It is infusible before the blowpipe, but diffuses an odour of sulphurous acid, and deposits a yellow coating on charcoal; with fluxes it gives the reaction of iron and some copper; when reduced with soda it furnishes a black slag, a yellow coating, and granules of lead, together with spangles of iron.

If the powder be boiled with water, nothing is dissolved, and especially no sulphuric acid is extracted. Nitric acid behaves in the same way. Muriatic acid attacks the powder slowly when boiled; the reddish-yellow solution contains sulphuric acid and oxide of lead, and deposits chloride of lead on cooling.

Boiling solution of potash colours the powder brownish-red, but dissolves no oxide of lead; it dissolves phosphoric acid, however, as is proved by its behaviour with silver salts, salts of magnesia, and molybdate of ammonia.

The specific gravity of the purest fragments was = 4·295. The results of various experiments are,—

	I.	II.	III.	IV.
Sulphuric acid	12·40	12·32	13·55	13·96
Phosphoric acid	..	8·00	9·73	8·21
Arsenic acid	..	0·21	0·37	0·10
Oxide of lead	..	20·35	22·98	27·57
Oxide of copper	..	..	2·45	
Oxide of iron	..	38·11	40·42	40·96
Water	9·77	..	..	(9·30)
				100·00

The average of III. and IV. is,—

			Oxygen.
Sulphuric acid	13·76	..	8·26
Phosphoric acid	8·97	5·02	5·10
Arsenic acid	0·24	0·08	
Oxide of lead	24·05	1·72	2·21
Oxide of copper	2·45	0·49	
Oxide of iron	40·69	..	12·21
Water	9·77	..	8·68
	99·93		

One of the analyses previously made by Dr. Percy* nearly approaches those of the author, except that the arsenic acid prepon-

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* Phil. Mag. for September 1850.

derates. If the above proportions of oxygen may be expressed by the numbers 9 : 5 : 2 : 12 : 9, Beudantite is

$$4Fe^2O^3 + 2PbO + 3SO^3 + PO^5 + 9HO,$$

and is therefore a very peculiar double compound, which might be written $2PbO\,SO^3 + Fe^2O^3\,SO^3 + (Fe^2O^3)^3\,PO^5 + 9HO$,

or $2(PbO)^3\,PO^5 + (Fe^2O^3)^3\,PO^5 + 9Fe^2O^3\,SO^3 + 27HO$.

—Poggendorff's *Annalen*, vol. c. p. 579.

THE IMPROVED INDUCTION COIL.

To the Editors of the Philosophical Magazine and Journal.

128 Sloane Street,
July 4, 1857.

GENTLEMEN,

Mr. Hearder, in his last letter to you, has again asserted that the induction coil contrived by me is perfectly similar to the instrument invented by himself. This is the fourth time he has made this assertion; and as it is always accompanied by his claim to priority, it makes it appear that I had really copied his machine, or adopted his ideas on the subject.

As my coil has, for protection, been enclosed by a box which has not been opened in Mr. Hearder's presence, he may not be aware of the dissimilarity of our instruments. First, Mr. Hearder uses a gutta-percha bobbin for his secondary helix, and winds the wire to be placed thereon until it is close to the cheeks of the bobbin. This method was adopted by me at first, but I found that the sparks passed from the upper layer of wire to the lower one. To prevent this I constructed my present coil *without* a bobbin, and wound my wire so that each layer was shorter than the one beneath; the intervals thus left were filled by the gutta-percha tissue with which I insulated the layers of wire. The coil thus assumed the form of a cylinder having rounded ends, and the space thus gained was sufficient to prevent the discharge taking place between the inner and outer layers of wire. By adopting this arrangement, Mr. Ladd of Chancery Lane has constructed an instrument which gives sparks 4½ inches long in air of the natural pressure.

My iron core also differs from Mr. Hearder's, and is formed of very fine parallel wires, each wire being magnetically and electrically insulated from the others. I formed it thus because such a mass can be magnetized and demagnetized with greater celerity than any other arrangement, and allows me to break contact with the battery from 50 to 2000 times a second. This rapidity of action was noticed by Mr. Hearder at the London Institution as being peculiar; and it also allows me to obtain a beautiful arc of flame between the secondary terminals, instead of sparks following each other slowly.

I could name other peculiarities, such as using a great number of coatings of thin gutta-percha instead of fewer layers of thick tissue, &c., but I think the differences which I have pointed out will be sufficient to show that I have not copied Mr. Hearder's induction coils.

Trusting you will excuse me for again troubling you, I remain,

Yours very obediently,

C. A. BENTLEY.

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

[FOURTH SERIES.]

SEPTEMBER 1857.

XVIII. *On the Measurement of the Chemical Action of Light. By* JOHN W. DRAPER, *M.D., Professor of Chemistry and Physiology in the University of New York*[*].

THE recent experiments of Professor Bunsen and Dr. Roscoe encourage the hope that the attention of chemists will before long be particularly directed to photo-chemistry, which undoubtedly offers at this moment one of the most promising fields of research.

To be satisfied what a boundless opportunity for investigation is here presented, it is enough to recollect that in the decomposition of carbonic acid by the solar rays lies the starting-point of all organization, both vegetable and animal; and that if it were not for that effect, the whole surface of our globe would be a mere desolate waste, presenting no appearance of life. Besides this relation to the world of organization, the influences of light are now recognized as occasioning combinations and decompositions not inferior in number or importance to those produced by heat and electricity.

Impressed by such considerations, I devoted a great deal of time some years ago to the study of the chemical action of light, as the readers of this Journal know. But at that period the attention of chemists was so completely absorbed in the department of organic analysis, and in the application of the discoveries so made to vegetable and animal physiology, that it seemed impossible to divert it even to the fundamental fact which in reality is at the bottom of all those investigations. Organization im-

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[*] Communicated by the Author.

plies the prior action of light. The time has now probably come when the wants both of chemistry and physiology will require the conditions of that action to be determined. The field of organic analysis has been pretty completely reaped; there is not now much to be done except by the gleaners.

Even among those who have devoted themselves to experiments in optical chemistry, the tendency has been to the improvement of the art of photography, rather than to the examination of facts which are at its scientific basis. A great amount of information, destined ere long to be advantageously used, has, however, in that way been indirectly obtained.

It is quite evident that in the contemplated inquiry the first thing to be done is to invent some means for measuring with exactness the chemical force of light. More than twenty years ago I commenced making attempts with that view. These were, first by the comparison of stains made on paper covered with chloride or bromide of silver. Subsequently (December 1843) I described in this Journal, under the name of Tithonometer, an instrument which is well adapted to such inquiries. It consists of an arrangement by which there may be obtained from hydrochloric acid, decomposed by a voltaic battery, a mixture of equal volumes of chlorine and hydrogen. This mixture will remain without any change in the dark; but on exposure to the rays of a lamp, the two gases unite in proportion to the quantity of the incident light. So great is its sensitiveness, that an electric spark, which lasts, it is said, less than the millionth of a second, affects it powerfully even at a distance, and sometimes occasions an explosion which destroys the tithonometer.

By the aid of this instrument may be illustrated the change which I discovered that the sun's rays occasion in the properties of chlorine, and likewise the preliminary absorption of light which is necessary before chemical actions ensue. It is this period of preliminary absorption, in the case of the iodide of silver, which is of such interest in the art of photography—the period during which invisible impressions are made on the daguerreotype plate and collodion film; capable of development in the one case by vapour of mercury, and in the other by pyrogallic acid or protosulphate of iron.

The tithonometer is the instrument of which Professor Bunsen and Dr. Roscoe, in an improved form, have made such excellent use. In its original construction I can still recommend it to those who are disposed to engage in these inquiries, as possessing extraordinary sensitiveness; and if suitable corrections for variations of temperature and pressure be applied, of sufficient exactness.

To such I would in addition suggest another means for measuring the chemical action of light. It will be found well adapted where extreme sensitiveness is not desired. It is an aqueous solution of peroxalate of iron. This substance, which is of a golden-yellow colour, may be kept, as I found, for more than three years (probably for any length of time) without exhibiting any change, if in total darkness; but on exposure to a lamp or the daylight, it undergoes decomposition, carbonic acid gas escaping, and the lemon-yellow protoxalate of iron precipitating. If set in the sunshine, it actually hisses through the escape of the gas. The ray which chiefly affects it is the indigo, the same which affects the tithonometer, and the silver compounds used in photography. This ray, to produce its effect, undergoes absorption, as may easily be proved by causing a sunbeam to pass through two parallel strata of peroxalate, when it will be found that the light which has gone through the first portion is inoperative on the second.

Other properties which the solution of peroxalate of iron presents strongly recommend it as a photometric agent to the chemist. Unlike solution of chlorine, it may be very conveniently confined in glass tubes by mercury. In its use there are two points which must be attended to:—1st, the lemon-yellow protoxalate must not be permitted to incrust the side of the glass exposed to the light, and thereby injure its transparency; 2nd, the solution of peroxalate must be kept nearly at a constant temperature, for its colour changes with the heat. At the freezing of water it is of an emerald-green; at the boiling, of a brownish-yellow. With these variations of tint its absorptive action on light varies, and therefore its liability to be changed.

It may be remarked that the peroxalate of iron is an excellent photographic agent. A piece of tissue-paper made yellow by being dipped in a neutral solution of it, when dried in the dark is very sensitive. Its invisible impressions may be developed by a weak solution of nitrate of silver, two grains dissolved in an ounce of water answering very well.

In the application of peroxalate of iron to photometry, several different methods may be followed. The course I have most commonly taken has been to determine the quantity of carbonic acid produced—sometimes by volume, sometimes by weight. It is of course understood, that before any carbonic acid can be disengaged, the solution must become saturated therewith; and that before we can correctly measure the quantity of light by the quantity of acid produced, this dissolved portion must be ascertained. In one of my photometers the expulsion of the dissolved gas is accomplished by exposure to a small bath of boiling water, in another by a stream of hydrogen. Both processes yield satisfactory results.

But this method, by the determination of the produced carbonic acid, is only one of the numerous plans which the employment of peroxalate of iron suggests; for instance, we might use in the determination the weight of certain metals which the solution after exposure will precipitate. Thus a portion which has been made and kept in the dark, may be mixed with chloride of gold without any action ensuing; but if it has been illuminated, the amount of metallic gold precipitated is in proportion to the incident light. On this principle I commenced an attempt to determine the hourly and diurnal illumination of a given locality. At the bottom of a hollow metal tube, arranged as a polar axis, was placed a bulb containing a standard solution of the iron salt, and at the close of the proposed periods the weight of gold it could reduce was ascertained. There is something fascinating in determining the quantity of light which the sun yields us by the quantity of gold it can produce. Upon the whole, however, I would recommend to those who are disposed to renew these attempts, to select a method depending on the volume of carbonic acid, for it is always easier to make an observation than an experiment.

Among the important results which may be expected from these new modes of photometry, and which will doubtless be furnished at an early period, are the hourly, diurnal, and annual quantities of the sunlight. These are not only important in a meteorological point of view, but also as respects physical geography, and the great interests of agriculture. The sum of vegetable organization is in all climates and localities a function of the light distributed thereto. Even so far as heat is concerned, the indications of the thermometer are of little use. It is not the intensity, but the absolute quantity which should be measured. To each plant, from the moment of its germination to the moment of its maximum development, and the completion of its physiological functions, a definite quantity of heat and also of light must be measured out. As respects the heat in such inquiries, it is not the thermometer but the calorimeter which should be used; and as to the light, the photometers here recommended determine its quantity, but not its brilliancy, and therefore answer the indications required. And since it is the light of the sun, and not the temperature of a locality, which is the effective condition of vegetable growth, we see how important, even in agriculture itself, these proposed determinations really are.

I hope that these remarks may draw attention to the problem of the chemical action of light. To those who are disposed to devote themselves to such inquiries, I recommend as a photometric means a mixture of chlorine and hydrogen where great sensitiveness is required, and in other cases the peroxalate of iron.

University, New York,
July 29, 1857.

XIX. *On the Chemical Changes which Pig Iron undergoes during its conversion into Wrought Iron.* *By* F. Crace Calvert, *F.C.S., and M.R.A. of Turin; and* Richard Johnson, *Esq.**

WISHING to make some improvements in the manufacture of iron, we carefully examined the various analyses which had been made of pig iron and wrought iron; but we found that no comparison could be made between the recorded results, as the samples analysed had been obtained from different sources, and also as no detailed analysis had been published of the various chemical changes which pig iron undergoes in the process of puddling during its conversion into wrought iron. We therefore decided to undertake this task, with the hope of throwing some light upon this important operation in the manufacture of iron, and of thereby enabling practical men to make those improvements in the puddling of iron which on many accounts are so much to be desired.

To fully investigate and closely follow the progressive and interesting chemical changes which pig iron undergoes during its conversion into wrought iron, we took samples every five or ten minutes after the pig iron had melted in the furnace. These chemical actions are clearly defined in the furnace by the peculiar appearance which the mass assumes as the operation proceeds. Before describing the various chemical changes, the appearance of the melted mass as taken out of the furnace, and its chemical composition, we shall describe with some details the analytical processes which we have adopted to determine the elements which exist in pig and wrought iron, and in the samples taken during the operation. These details of analysis appear to us the more important, when it is remembered that most of the heterogeneous substances existing in pig iron are present only in minute quantities, and that it is on their gradual removal or decrease that the subsequent quality of the wrought iron depends. Also it is necessary to bear in mind, that we had to trust entirely to the exactitude of the analytical methods adopted, to appreciate the chemical changes which gradually took place in the melted mass during the time that the conversion of the pig iron into wrought iron lasted.

Iron.—The quantity of iron was determined by dissolving 1 gramme of iron in pure hydrochloric acid, reducing the solution to a perfect protosalt by a little pure zinc, and then determining the amount of iron by Marguerite's process.

Carbon.—To determine this element, we found after many trials that the best process was to reduce the iron into very fine

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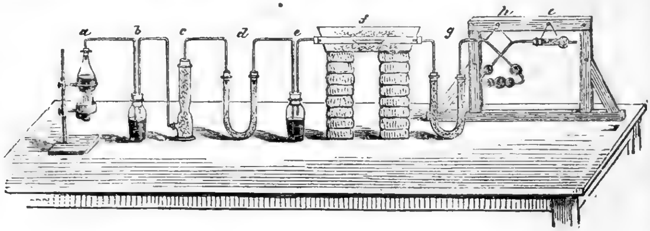
* Communicated by the Authors.

powder, either by pulverization or by means of a file, and then to burn the carbon under the influence of a red heat by a slow current of pure and dry oxygen gas. The apparatus which we used was the following:—



a, a flask containing a mixture of chlorate of potash and oxide of copper, which by gently heating gave off a regular current of oxygen.

b, a bottle containing a concentrated solution of caustic potash, so as to retain any chlorine or any oxygenated compound of this gas which might be produced.

c, a tube full of pumice-stone moistened with solution of caustic potash, and employed with the same view as the last.

d, a U-tube filled with pieces of solid melted caustic potash, also used for the same purpose.

e, a bottle containing sulphuric acid for retaining any moisture which might accompany the oxygen gas.

f, a porcelain tube in which was placed a small porcelain dish containing the pulverized iron.

g, a tube filled with small pieces of pumice-stone moistened with sulphuric acid, with the view of retaining any moisture.

h, a Liebig-tube full of concentrated solution of caustic potash, to determine the amount of carbonic acid produced by the combination of the oxygen with the carbon of the iron.

i, a small tube with fragments of caustic potash, to retain any trace of carbonic acid which might not be absorbed in the Liebig-tube.

To render the absorption of the carbonic acid complete, it is necessary to conduct the operation very regularly and slowly; therefore about two hours are required to burn all the carbon existing in about 3 grammes of iron.

By this method two analyses of the same sample seldom presented a greater difference than 0·05. We also took the precaution to dissolve the oxide of iron obtained after combustion, in order to see that no hydrogen gas was given off, and consequently that no metallic iron remained.

Silicium.—There is considerable difficulty in determining with precision this element in cast iron, and it was only after several fruitless trials of various processes that we adopted the following, which gave us very satisfactory and concordant results. 5 grms. of the iron were dissolved in aqua regia containing excess of nitric acid, the whole was then evaporated to dryness and fused in a platinum crucible with three times its weight of a mixture of pure carbonate of potash and carbonate of soda. The mass obtained was dissolved in water and boiled with aqua regia until the whole of the peroxide of iron had entered into solution, and then was evaporated a second time to dryness and heated carefully to about 200° C. The mass was then treated with hydrochloric acid and water, and the silica on being gathered on a filter, was washed with dilute hydrochloric acid until it was perfectly white, dried and calcined, when its weight gave the amount of silicium in the iron analysed.

Sulphur.—In consequence of the small proportion in which this element exists in pig iron and wrought iron, considerable difficulty is experienced in ascertaining with accuracy the various proportions of sulphur existing in iron, and this difficulty was increased by the fact that none of the methods recommended gave satisfactory results in our hands. Thus, for example, the method which consists in determining the sulphur in the state of sulphuretted hydrogen, failed, owing to the difficulty of removing the last traces of sulphuretted hydrogen held in solution in the liquid in which the iron is dissolved, and in which the gas is produced. As to the process which consists in dissolving the iron in aqua regia and boiling off the greater part of the acid, and then adding nitrate of baryta to the solution, it cannot be followed with security; for one of us has shown that sulphate of baryta is soluble in acid liquors, especially in those containing nitric acid, and often in such quantities as to make a greater difference between the analysis of two samples of the same iron, than the real difference which exists in two samples of iron from different ores. These considerations induced us to modify the last-mentioned process in the following way:—5 grammes of the sample of iron to be analysed were reduced to fine powder, and gradually and slowly added to a strongly oxidizing aqua regia, composed of four parts of fuming nitric acid and one part of hydrochloric acid. The iron being dissolved, the solution was evaporated to the consistence of a thin syrup, and then gradually mixed with four times its weight of a mixture of pure carbonate of potash and soda, and heated to redness for one hour in a platinum crucible. The fused mass was then heated with pure boiling water until all the soluble portion was dissolved. This liquor was then rendered slightly acid with hydrochloric acid, evaporated to dryness, and heated at 200° C. to render the silica insoluble. The whole was then treated with water slightly acidulated with acetic acid, and on the silica being separated by filtration, the amount of sulphate, and consequently of sulphur, was determined from the weight of sulphate of baryta obtained.

Phosphorus.—We also attached great importance to the exact valuation of this body, because, like sulphur, its presence even in small quantities is most injurious; the more so, as such minute quantities as a few thousandths, more or less, will completely alter the value of iron for many uses. To determine phosphorus, the process which we followed was similar to that employed for the sulphur; with this difference, that we added some muriatic acid, and to the liquid from which we had separated the silica, ammonia in excess instead of acetic acid, as we had done in the analysis of sulphur. The liquid was allowed to stand, to see if any alumina separated; and if not, we added hydrochloric acid in excess, then pure chloride of calcium, and then ammonia again, when phosphate of lime having the following formula, $PO^5\,3CaO$, was precipitated, from which the quantity of phosphorus was calculated. We always took care to operate on such a bulk of fluid as to prevent the precipitation of any sulphate of lime, and we also washed rapidly to prevent any carbonate of lime being formed. We verified this method several times during our analysis, by determining the amount of lime in our precipitates, and the amount of phosphoric acid, by M. Reynoso's process.

Aluminium.—If there was any aluminium, it was separated during the last process and its amount determined. We also made several fusions of iron dissolved in aqua regia and evaporated with a mixture of alkaline carbonates to which we had added a little caustic alkali, and we found no aluminium, or only mere traces in the iron analysed by us.

Manganese.—5 grammes of iron were dissolved in aqua regia, and the whole evaporated to dryness and calcined with alkaline carbonates. The fused mass was treated with boiling water, and to the solution were added small pieces of Swedish paper to reduce the manganate. The iron and manganese were then collected on a filter, well washed, and then dissolved in hydrochloric acid. This solution was again evaporated, and heated so as to render the silicic acid insoluble. The residue was then treated with some weak hydrochloric acid, and the solution filtered to separate the silica. Carbonate of baryta recently prepared was then added to precipitate the oxide of iron; this was separated by filtration, and to the liquid, sulphate of soda and a little hydrochloric acid were added to separate the baryta in solution;

and finally, the manganese was precipitated by a little caustic potash, washed, dried, calcined, and its amount ascertained.

It is necessary that we should describe in a rapid manner the physical conditions which pig iron assumes during its conversion into wrought iron. When first heated in a puddling furnace, it forms a thick, pasty mass, which gradually becomes thin, and as fluid as mercury. When it has reached this point it experiences a violent agitation, technically called "the boil," which is produced no doubt by the oxidation of the carbon, and the escape of the carbonic oxide then generated. During this period of the operation the mass swells to several times its primitive bulk, and the puddler quickly agitates the melted mass to facilitate the oxidation of the carbon. After a short time the mass gradually subsides; the puddler then changes his tool, and takes the "puddle" to gather with it the granules of malleable iron floating in the melted mass of scoria or slag. The granules or globules of iron gradually weld together and separate from the scoria; and this separation is hastened by the puddler gradually forming large masses, called balls, weighing about 80 lbs., from which the scoria drains out. This part of the operation requires great skill in the puddler; for nearly the whole of the carbon has been oxidized, so that if the current of air is not managed with great care, the iron itself is oxidized, or as it is technically termed, "burnt;" and thus not only does great loss ensue in the quantity of malleable iron produced, but also the iron containing a certain quantity of oxide of iron is brittle, and of bad quality.

We shall now examine the various chemical changes which pig iron undergoes during its conversion into wrought iron.

The iron we took for our experiments was a good cold-blast Staffordshire iron; the pig was rather gray, being of the quality used for making iron wire, or a gray No. 3. Its composition was as follows:—

	First analysis.	Second analysis.	Mean.
Carbon	2·320	2·230	2·275
Silicium	2·770	2·670	2·720
Phosphorus	0·580	0·710	0·645
Sulphur	0·318	0·288	0·301
Manganese and aluminium	traces	traces	
Iron	94·059	94·059	94·059
	100·047	99·957	100·000

224 lbs. of the above pig iron were introduced at 12 o'clock, on the 4th of April, 1856, into a puddling furnace which had

been cleaned out with malleable iron scraps. After thirty minutes the pigs began to soften and to be easily crumbled, and ten minutes more had hardly elapsed when they entered into a state of fusion. The first sample was taken out of the furnace at 12h 40m P.M., from the centre of the melted mass with a large iron ladle and poured on a stone flag to cool. The flue of the furnace, which up to this time had been kept open, was now nearly closed by a damper at the top of the chimney, so that the products of combustion came out by the door of the furnace and other openings, whilst little or none escaped by the chimney.

Appearance of the Sample.

On breaking the sample as taken out of the furnace, it had no longer the appearance of gray No. 3 pig iron, but a white, silvery, metallic fracture, similar to that of refined metal. The rapid cooling of the sample was no doubt the cause of the change noticed, for it contained quite as much carbon as the pig iron used; and further, the carbon was in a very similar condition, as in both cases a large quantity of black flakes of carbon floated in the acid liquors in which the iron was dissolved. The following is the amount of carbon and silicium which the above sample contained per cent.:—

	First analysis.	Second analysis.	Mean.
Carbon	2·673	2·780	2·726
Silicium . . .	0·893	0·938	0·915

These results are highly interesting, as they show that the iron had undergone during the forty minutes which it had been in the furnace, two opposite chemical changes; for whilst the proportion of carbon had increased, the quantity of silicium had rapidly decreased. This curious fact is still further brought out by the sample which we took out of the furnace at 1 P.M., or twenty minutes later than the last sample analysed, as is shown in this Table:—

	Carbon.	Silicium.
Pig iron used	2·275	2·720
1st sample taken out at 12h 40m .	2·726	0·915
2nd sample taken out at 1h 0m . .	2·905	0·197

Therefore the carbon had increased 0·625, or 21·5 per cent. of its own weight, and the silicium had decreased in the enormous proportion of above 90 per cent. It is probable that these opposite chemical actions are due, in the case of the carbon, to the excess of this element in a great state of division or in a nascent state in the furnace, and that under the influence of the high temperature it combines with the iron, for which it has a great affinity, whilst the silicium and a small portion of iron are oxidized and combined together to form protosilicate of iron, of which the scoria or slag produced during this first stage of puddling consists, and which plays such an important part in the remaining phænomena of the puddling process.

2nd Sample, taken out of the furnace at 1ʰ 0ᵐ P.M.

This sample contained the following quantities of carbon and silicium:—

	First analysis.	Second analysis.	Mean.
Carbon	2·910	2·900	2·905
Silicium. . . .	0·226	0·168	0·197

It had the same white, silvery appearance as No. 1; but had this difference, that it was slightly malleable under the hammer, instead of being brittle like No. 1. The scoria also was on the upper surface of the mass when cold, and not mixed with the metallic iron, as in succeeding examples.

3rd Sample, taken out at 1ʰ 5ᵐ P.M.

The mass in the furnace having become very fluid, and beginning to swell or enter into the state called "the boil," a small quantity was ladled out. When cold it was quite different from that of the two previous ones, being composed of small globules adhering to each other, and mixed with the scoria; the mass therefore was not compact, like the former ones, but was light and spongy; its external appearance was black, and the small globules when broken presented a bright metallic lustre, and were very brittle under the hammer. We had for some time considerable difficulty in separating the scoria from the globules of iron; but we found that by pulverizing the whole for a long time the scoria was reduced to impalpable powder, and by sieving we could separate it from the iron, which was much less friable. The iron thus cleansed from its scoria gave us the following results:—

	First analysis.	Second analysis.	Mean.
Carbon	2·466	2·421	2·444
Silicium. . . .	0·188	0·200	0·194

4th Sample, taken out at 1ʰ 20ᵐ P.M.

As soon as the last sample had been taken out, the damper of the furnace was slightly raised so as to admit a gentle current of air, which did away with the smoke which had been issuing from the puddler's door, and a clear and bright flame was the result. This was done, no doubt, to facilitate the oxidation of the carbon of the iron, and to increase this action the puddler quickly agitated the mass. Under these two actions the mass swelled up rapidly, and increased to at least four or five times its original bulk; and at $1^{h}\ 20^{m}$, the mass being in full boil, this 4th sample was taken out. Whilst cooling, it presented the interesting fact, that in various parts of it small blue flames of oxide of carbon were perceived, no doubt arising from the combustion of carbon by the oxygen of the atmosphere. It is curious that this phænomenon was not observed in the previous samples. It is due probably to the following causes: first, that the cast iron, having been brought by the boil to a state of minute division, offers a large surface to the action of the oxygen of the air, and thus the combination of the oxygen with the carbon of the iron is facilitated; and second, that at this period the carbon seems to possess little or no affinity for the iron; for one of us has often observed that when pig iron, rich in graphite, is puddled, the carbon is liberated from the iron; for if a cold iron rod is plunged into the mass of melted iron in the puddling furnace, it is covered with iron and abundant shining scales of graphite carbon.

The appearance of this No. 4 sample was most interesting; and the best idea that we can give of it is, that it is so light and formed of such minute granules as to be exactly like an ants' nest. The particles have no adherence to each other, for by mere handling of the mass it falls into pieces. This is due to each particle of iron being intimately mixed with scoria. The granules of iron have a black external appearance, are very brittle under the hammer, and when broken they present a bright, silvery, metallic fracture. The scoria was separated by the method above described for No. 3, and the quantities of carbon and silicium which the iron contained were as follows:—

	First analysis.	Second analysis.	Mean.
Carbon	2·335	2·276	2·305
Silicium. . . .	0·187	0·178	0·182

5*th Sample taken out at* $1^{h}\ 35^{m}$ P.M.

This sample is a most important one in the series, as it is the first in which the iron is malleable and flattens when hammered. It was ladled out of the furnace just as the boil was completed, and the swollen mass began to subside. The damper at the top of the chimney was drawn up, so that a very rapid draught was established through the furnace. The puddler also changed his tool, leaving the rubble and taking the puddle to work with. When cold it partakes of the appearance of Nos. 3 and 4 samples, the mass being spongy and brittle, as in No. 4, but less granulated, and like No. 3, being in separate globules, mixed with the scoria. The granules are black externally, but are bright and metallic when flattened. The analysis of these globules proves that the mass of iron in the furnace has lost during the quarter of an hour which has elapsed since the taking of No. 4 sample, a large proportion of its carbon, equal to 20 per cent. of its weight, whilst the silicium, on the contrary, has remained nearly stationary.

	First analysis.	Second analysis.	Mean.
Carbon	1·614	1·681	1·647
Silicium. . . .	0·188	0·178	0·185

6th Sample, taken out at 1^h^ 40^m^ P.M.

The reason why this sample was taken only five minutes after the last sample, was, that the mass in the furnace was rapidly transforming itself into two distinct products, viz. the scoria on the one hand, and small globules of malleable iron on the other. We attached some importance to this sample, as the workman was on the point of beginning the balling or agglomerating the globules of iron, so as to form large balls of about 80 lbs. weight, to be hammered and rolled out into bars. Whilst the mass taken out for analysis was cooling, small blue flames of oxide of carbon issued from it. These were similar to those observed in Nos. 4 and 5, but were not so abundant. The appearance of this sample was very similar to the last one, with the exception that the scoria was not so intimately mixed with the globules of iron, and that these were larger, and slightly welded together when hammered. The proportions of carbon and silicium were as follows:—

	First analysis.	Second analysis.	Mean.
Carbon	1·253	1·160	1·206
Silicium	0·167	0·160	0·163

When these figures are compared with those of the previous analysis, it is interesting to observe, that whilst the silicium remains nearly stationary, the carbon rapidly diminishes; for in the five minutes which elapsed between the taking out of the two samples, there was 28 per cent. of the carbon burnt out. This rapid decrease of carbon in the iron is maintained during the remaining ten minutes of puddling. In fact, in one quarter of an hour, viz. from $1^h\ 35^m$ to $1^h\ 50^m$, the iron lost 50 per cent. of the carbon which it contained at $1^h\ 35^m$.

7th Sample, taken out at $1^h\ 45^m$.

This sample was obtained when the puddler had began to ball. The appearance of the sample, although similar to the last, differs from it by the granules being rather larger, and nearly separated from the scoria, which forms a layer at the top and bottom of the mass. These granules are also much more malleable, for they are easily flattened under the hammer. This last fact is easily accounted for by the small amount of carbon which it contains, as stated above and shown by these results:—

	First analysis.	Second analysis.	Mean.
Carbon . .	1·000	0·927	0·963
Silicium . .	0·160	0·167	0·163

8th Sample, taken out at 1^h 50^m.

This last sample was taken a few minutes before the balls were ready to be removed from the furnace, to be placed under the hammer, and was a part of one of the balls which were separated and placed to cool. It was observed that no blue flame issued from the mass as it cooled. The appearance of the sample showed that the mass constituting the ball was still spongy, and granulated similar to the previous ones. The only difference was, that the granules adhered together sufficiently to require a certain amount of force to separate one from the other, and also that they were much more malleable under the hammer. They were found to contain the following quantities of carbon and silicium per cent.:—

	First analysis.	Second analysis.	Mean.
Carbon . .	0·771	0·773	0·772
Silicium . .	0·170	0·167	0·168

We should observe here, that the black coating which covers the granules of iron, even of No. 8 sample, preserves the iron from all oxidation; for none of the samples became oxidized during the nine months they were in the laboratory, exposed to the atmosphere, and to the various acid fumes floating about. This black coating is probably composed of a saline oxide of iron.

9th Sample.—Puddled Bar.

The balls taken out of the furnace were hammered, and then rolled into bars, and in these we found the following:—

	First analysis.	Second analysis.	Mean.
Carbon . .	0·291	0·301	0·296
Silicium . .	0·130	0·110	0·120
Sulphur . .	0·142	0·126	0·134
Phosphorus .	0·139	. . . .	0·139

10th Sample.—Wire Iron.

The puddled bars were cut into billets of about 4 feet in length, and heated in a furnace to a white heat, and then rolled into wire iron. The proportions of carbon, silicium, sulphur and phosphorus, were as follows :—

	First analysis.	Second analysis.	Mean.
Carbon . .	0·100	0·122	0·111
Silicium . .	0·095	0·082	0·088
Sulphur . .	0·093	0·096	0·094
Phosphorus .	0·117	. . .	0·117

To complete the series of products in the conversion of pig iron into wrought iron, we analysed the scoria or slag which remained in the furnace after the balls had been taken out, and found its composition to be as follows :—

Silica	16·53
Protoxide of iron . . .	66·23
Sulphuret of iron . . .	6·80
Phosphoric acid . . .	3·80
Protoxide of manganese .	4·90
Alumina	1·04
Lime	0·70
	100·00

Therefore in the scoria are found the silicium, phosphorus, sulphur and manganese which existed in the pig iron; and probably the phosphorus and silicium are removed from the iron by their forming fusible compounds with its oxide.

We shall conclude this paper by giving our results in a tabulated form, so that the removal of the carbon and silicium may be better appreciated by those who may consult it with the view of obtaining such information as may lead them to those improvements to which we think our investigations tend.

Pig iron used.	Time.	Carbon.	Silicium.
		2·275	2·720
Sample No. 1	12·40	2·726	0·915
,, ,, 2	1·0	2·905	0·197
,, ,, 3	1·5	2·444	0·194
,, ,, 4	1·20	2·305	0·182
,, ,, 5	1·35	1·647	0·183
,, ,, 6	1·40	1·206	0·163
,, ,, 7	1·45	0·963	0·163
,, ,, 8	1·50	0·772	0·168
Puddled bar, 9	....	0·296	0·120
Wire iron, 10	....	0·111	0 088

Finally, we wish to express to Mr. Siméon Stoikowitsch our best thanks for the ability and perseverance which he has shown in helping us in these long and tedious analyses.

XX. *Researches in Statical Electricity.*

By Sir W. Snow Harris, *F.R.S.*

[Continued from p. 100.]

Charging of the proof-plane and other insulated conductors.
Coulomb's experiment with a hollow globe and circular plate of twice the diameter.
Expression representing electrical charge.

23. A QUESTION has not unfrequently arisen in regard to the charging of the proof-plane and simple electrified conductors, which admits of an easy solution upon the elementary principles and subsequent experimental inquiries we have been considering. It has been doubted whether these bodies take up electricity upon all their surfaces, or upon one only. Now it is evident, (3) and Exp. 4, that neither the proof-plane nor any other conductor can take up electricity upon either face, except we displace from the face of contact a portion of its own electricity (3). The charge virtually consists of its own displaced electricity (9), consequently the charge it receives will be entirely dependent on this induced change, as we have already seen (17). In the case of an insulated conducting surface exposed to the operation of surrounding matter, a stratum of accumulated electricity must always be found upon all its surfaces upon the principles already exposed (10). Let, for example, *a, b, c,* fig. 18, be three concentric hollow spheres placed one within the other, and so sustained as not anywhere to touch; let the interior middle sphere be perfectly insulated, whilst the spheres *a* and *c* communicate with the ground; then in communicating electricity to the insulated sphere *b*, we find it charge on both its surfaces, and so produce an electrical stratum, *b c*, on each side of it. If we cut off the influence of the sphere *a* or *c*, then it charges on one surface only; at least the charge on the opposite surface is so small as to admit of being neglected in such an experiment. In the case of the double induction, twice the quantity of electricity may be accumulated under the same degree of the electrometer. The following experiments are instructive, and very conclusive.

Exp. 14. Let a light circular disc of gilded or silvered wood, *c*, fig. 19, about a foot in diameter and something less than ·25 of an inch thick, be suspended from a varnished glass arm by an insulating thread of varnished silk gut (2); connect it with the hydrostatic electrometer E, or with the fixed ball of the balance, as in fig. 17 (22). Let two other perfectly similar plates, *a, b,* be suspended in like manner at an equal distance upon each side of the plate *c*.

Connect one of the external plates, *a*, with the ground, and proceed to charge the centre plate *c* with a given number of measures of electricity, the opposite plate *b* being turned aside during this process. Observe now the electrometer intensity in degrees at a given distance of the attracting discs *p, n*. Suppose the number of measures 5; distance of the plates *a, c* ·5 of an inch, distance of the attracting discs *p, n* ·6 of an inch, and the index indication 10 degrees.

Replace plate *b*, and put it also in connexion with the ground. Discharge the air between *c* and *a* by a communication between the two coatings or plates *a, c* in the usual way, and repeat the former process; the number of measures now requisite to produce 10 degrees of force will be 10 measures, or just double the former. The plate *c*, therefore, or rather the air, has now charged in proportion to the two surfaces of the plate, which in this case have been each exposed to a free inductive action, and the electricity proper to the plate *c* has been enabled to retire, as it were, outward toward the stratum of air on each side of it.

24. This is precisely what happens in the case of what we term the charging simple insulated conductors, except that from the distance of other conducting matter taken as the opposite or uninsulated coating to the charge, the accumulation is less complete and more feeble. If we look at this question somewhat critically, we may probably be led to conclude that the essence of the charge is virtually the displaced electricity proper to the plate itself (9), (17). Suppose, for example, that we are about to communicate a given measure of electricity to the rectangular plate *a d b*, fig. 20, through the intervention of a small carrier-ball *c*, charged to saturation, suppose positively, and applied to any point *c* of the plate. The first action will be that of direct and reflected induction, already explained (3), fig. 2; that is to say, the electricity proper to the plate recedes in every direction, *ca, cd, cb*, from the point *c*, and a powerful negative space, *c*, is for a moment produced all around it; at the same instant, the charge, before distributed uniformly over the carrier-ball, concentrates itself by the reflected induction immediately upon the point *c* in contact with the plate; a neutralization of the opposite forces ensues, and the plate and surrounding air remain charged with the displaced electricity. If the magnitude of the plate be considerable in respect of the carrier-ball, all its charge, or nearly all, will have disappeared on a first contact; and the charge communicated as in the Leyden jar will be so greatly masked by the inductive action of surrounding matter, that comparatively little effect is produced on an electrometer, *n*, placed in connexion with the plate. On a repetition of this operation, the same actions are apparent: another quantity of electricity becomes displaced, and again the carrier-ball becomes robbed of its electricity. The

electrometer now begins to evince an increased action. A third application becomes still more sensible. The carrier-ball may yet yield up so large a portion of its electricity, that the residue on it may be neglected, and the march of an electrometer of measure E, fig. 13, will be as the square of the quantity of electricity displaced. A stratum of air everywhere surrounding the metal becomes in this way charged; or, according to Faraday, polarized and inductive action becomes propagated through contiguous or next particles to bodies at a distance: in this sense, simple insulated conductors, as distinguishable from a more regularly coated surface or stratum, may be conceived to charge upon all their surfaces. That this is not only possible but certain, is shown in our last experiment 14, the charge itself being resolvable into the displaced electricity of the plate itself. When the possible displacement becomes exhausted, the charge is at a maximum under the given circumstances and conditions of the experiment.

In the case of a coated stratum of air, fig. 19, Exp. 14, in which two coatings only, *a, c,* are employed, plate *b* being turned aside, the intervening air on one surface of the plate may be considered as the more immediate recipient of the charge: still the air in contact with the exterior surface of the plate may be conceived to participate in the operation. We have here merely to suppose, in the case of employing both the plates *a, b,* that plate *b* has been removed to an indefinite distance. It is only on account of the small induction of distant matter, as compared with that of the near and directly opposed coating on plate *a,* that we commonly neglect the charge by distant induction upon its opposite surface, and consider only the charge developed immediately between the two plates or coatings *a, c.* When we bring into operation, however, a third plate *b,* in the way shown, Exp. 15, then the equal induction upon the exposed surfaces of the centre plate *c* is complete, and we obtain a full or double charge.

25. It may be worth while to define here more explicitly what is to be understood by the term charge, considered as an expression of quantitative measurement. By the term electrical charge of an insulated conductor, we are to understand the measured quantity of electricity it can sustain under a given degree of the electrometer as referred to some unit of measure. Thus, fig. 21, let a globe S, a circular plate P, of twice the diameter of the sphere, and a rectangular plate R, of the same area as the sphere and plate, be put successively in connexion with the fixed disc *p* of the electrometer E, fig. 19, or with the fixed ball of the balance *p,* fig. 17 (22). Then the same quantity of electricity communicated to these three bodies, S, P, R, affects the electro-

meter very differently, notwithstanding that the surface is the same in each. And in order to give to the electrometer the same indication, different quantities will be required. Thus, if five measures communicated to the sphere indicate 10 degrees of force, seven measures will be required to be disposed on the plate to indicate the same force: for the rectangle some other number will be required. The number of measures thus requisite to bring the electrometer index to the same point, is the electrical charge of the given conductor as accepted by Cavendish, and may be supposed to represent the electrical capacity of any given surface.

26. This understood, we may now proceed to consider an interesting and beautiful experiment by Coulomb, on the relative charges of a hollow sphere and a circular plate of twice the diameter; from which it has been inferred that the plate charges in proportion to its two surfaces, and the sphere only in proportion to its outer surface. There can be but little doubt of the perfect accuracy of the experiments of this most ingenious and profound philosopher, nevertheless I am led to believe that they admit of a somewhat different interpretation from that generally given to them.

Exp. 15. Take two equal spheres, A, B, fig. 22, and a plate, P, of twice the diameter of the sphere; charge one of the spheres A with a given quantity of electricity, and subsequently touch it with the insulated plate P. The charge by the method of Coulomb will be found shared between the sphere and plate in the proportion of 1 : 2; that is to say, the plate will have abstracted two-thirds of the charge, and will have left one-third. Now, as already observed, although there can be little doubt of the accuracy of this result as stated by Coulomb, yet the sharing the charge between these bodies is not the same thing as charging the bodies separately with measured quantities of electricity (25). If we attempt to place a unit of quantity on the sphere, and twice that quantity on the plate under the same degree of the electrometer, we fail to do it: the plate will not receive it. And we have just seen (23), Exp. 14, that when a plate really takes up electricity as charge in proportion to both its surfaces, we then have a double accumulation, and we can place the same quantity of electricity on each surface as compared with a given unit of quantity. Now in the sharing of the charge between the sphere and plate, the plate would necessarily appropriate to itself an amount of electricity proportionate to the free inductive action between it and the sphere: in fact, as may be clearly seen, its remote surface P is twice that of the remote surface *p* of the sphere, and consequently has twice the direct inductive capacity, so that the near face *n* would be in a position to receive, in sharing the charge, twice the quantity of electricity which would be retained on the sphere A. Hence, when by contact the increased induced electrical charge of the sphere in the hemisphere *s* next the plate is communicated to the plate, and the sphere and plate are taken together as a whole, then it is that the plate receives a quantity proportionate to the magnitude of the inductive change in each, without which there would not be an equilibrium of distribution between the two bodies (3), fig. 2. The perfect success of this experiment is entirely dependent on insulation, and the absence of foreign induction; if any adventitious circumstance should arise calculated to increase the capacity of the sphere by induction, as by the presence of near matter, some particular hygrometric states of the air, or imperfect insulation, the plate and sphere will appear to share equally, as I have found in a great variety of instances, and as stated in my paper in the Philosophical Transactions for 1836. Such, however, I have since found is not really the case when the experiment is very perfectly conducted with due regard to disturbing influences.

27. The result of this experiment, therefore, although it may well determine the relative division of the electricity between the two bodies, does not really determine their relative capacity for electricity or their charge (25). In order to determine this, it is requisite to compare each with a third body, on the principles laid down by Cavendish, and which may be very well managed in employing a third body, suppose a sphere B, fig. 20, which may be either equal or not. For the sake of simplicity let it be equal.

Exp. 16. Let the sphere A, charged as before, be touched by an equal and similar neutral sphere B; then the charge becomes shared equally between the two spheres, and the capacity of a sphere equal to the plate P, of twice its diameter, may be represented by the fraction $\frac{1}{2}$. Repeat this experiment with the plate P; then, as just seen, its capacity deduced in a similar way may be represented by the fraction $\frac{2}{3}$. The capacity of the sphere, therefore, is to the capacity of the plate of twice the diameter in the proportion of $\frac{1}{2} : \frac{2}{3}$; that is, as $1 : \sqrt{2}$ very nearly, or as $1 : 1{\cdot}4$. And such is really the proportions of charge which the two bodies will sustain under a given degree of the electrometer (25).

Exp. 17. Place 5 measures of electricity on the sphere B, and 7 measures on the plate P. The respective reactive forces by the method of Coulomb will be precisely the same, the plate being touched by a free tangent plate (fig. 11), or otherwise near its edge; that is to say, 5 measures is to sphere as 7 to plate, that is, as $1 : \sqrt{2}$, or $1 : 1{\cdot}4$ very nearly.

Exp. 18. Place the plate and sphere successively in communication with the fixed disc of the hydrostatic electrometer (fig. 18), or the fixed disc of the balance, fig. 17. Deposit 5 measures on the sphere, and 7 measures on the plate: the index will stand at the same number of degrees in each case as before. A great many such experiments may be adduced to show that this is really the proportionate electrical capacities of a sphere and circular plate of twice the diameter of the sphere; and that if we attempt to place twice the quantity of electricity on the plate, according to the ordinary deduction, that it charges in proportion to its double surface, we entirely fail: the plate will not receive any other proportion of charge than that just stated.

28. The fact that simple insulated conductors do not always take up electricity in proportion to their surfaces has been long known. It was first observed by Le Monnier in 1746, by Volta in 1779, and was observed in certain cases by Coulomb himself in his justly celebrated memoirs on electricity in *L'Histoire de l'Académie*, in 1785. The law, however, of this species of electrical action has never been fully investigated in all its generality. In the case of spheres, circular plates, and plane surfaces, I have been enabled to arrive at a very simple expression for the relative quantities of electricity which such bodies can sustain under a given degree of the electrometer, that is to say, their charge; and which comes very near the result of experiment in almost every instance. If P represent the circumference or perimeter, S the surface, and C the charge, then we have $C = \sqrt{S \times P}$, taking P and S as abstract numbers. That this is true for spheres, circular plates, and plane surfaces of other forms, may be most satisfactorily shown in the way just described, Exps. 17 and 18. Take, for example, two spheres, Q, R, whose diameters are 5·67 inches and 9 inches respectively; then we have for charge of sphere Q, of diameter 5·67, and surface 101,

$$\sqrt{S+P} = \sqrt{101 \times 17} = 42{\cdot}4 \text{ nearly;}$$

and for charge of sphere R, of diameter 9, and surface 254·5,

$$\sqrt{S \times P} = \sqrt{254{\cdot}5 \times 28{\cdot}27} = 84{\cdot}8 \text{ nearly;}$$

that is to say, the relative quantities of electricity which may be accumulated on these two globes under the same electrometer indication, or in other words, their relative charge, will be as 42·4 : 84·8, or as 1 : 2, their surfaces being as 1 : 2·5.

Exp. 19. Place 5 measures on globe Q, and 10 measures on globe R, and take the reactive forces by the method of Coulomb; these reactive forces will be alike. If the relative charges had been in proportion to their surfaces, they should have been in the proportion of 1 : 2·5; that is to say, the globe should have taken 12·5 measures.

This result may be also examined by the process (Exp. 18).

29. A similar result ensues in the charges of circular plates, either as compared with each other, or with spheres of the same area, as we have just seen. The area of a plate and sphere being taken as equal, each surface to each surface, the relative charges will be as $\sqrt{P}$; that is, as 1 : $\sqrt{2}$, the circumference of a sphere being to that of a plate of twice the diameter as 1 : 2.

With respect to hollow cylinders and closed surfaces of other forms, their charges may be determined by comparing them with the charges of the plates into which they may be supposed to be expanded. Thus if a square plate, whose side is 10 inches, be turned up into a hollow cylinder, the charge of the cylinder will be to the charge of the square plate as 1 : 1·4, as in the case of a sphere and circular plate of twice the diameter, in which the total surfaces are the same.

The quantity of electricity which can be accumulated on a simple insulated conductor of rectangular figure, appears to vary with the surface and perimeter conjointly. If the surface be constant, it varies with $\sqrt{P}$; if the perimeter be the same or nearly so, or does not differ in any very great degree, then the charge will either be as $\sqrt{S}$, or come very near it.

Exp. 20. Take a square plate whose side is 10 inches, giving a unit of surface of 100 square inches, and a perimeter of 40 inches, and transform this square into a rectangular plate 37·3 inches in length by 2·7 inches in width, which exposes the same surface of 100 inches under a perimeter of 80 inches; then the relative charges of these two surfaces, as before tested (25), will be as 1 : $\sqrt{2}$.

Exp. 21. Take a rectangular plate of about 40 inches in length by 5 inches in width, and suppose it divided longitudinally into two parts; we have then in each rectangle one-half the area under nearly the same perimeter. In this case the quantity of electricity which can be accumulated upon the half surface will be to the quantity upon the whole surface also as 1 : $\sqrt{2}$, that is, it will be as $\sqrt{S}$. A double surface will not take up twice the quantity of electricity, except it be placed under twice the perimeter. If we take a plate 34·14 inches long by 5·86 inches wide, which gives twice the surface and twice the perimeter of a square whose side is 10 inches, then if we charge the square with 5 measures and the rectangle with 10 measures, the reactive forces, as in Exp. 17 (27), will be the same.

30. These phænomena are not only of great interest, but they have an important signification in any theoretical explanation of

electrical force we may be led to adopt. They evidently indicate a disposition in the electrical molecules to increased action when grouped closely about each other; when subjected to a state of linear extension, they become placed, as it were, further without each other's influence, and appear to repose with greater tranquillity on a conducting surface considered as a portion of a charged system (12). It is well known that when two similarly electrified bodies are brought into the presence of each other, the electrometer indications of these two bodies begin to increase, and continue to exhibit further excitement as the bodies are caused to approach each other. Now it is quite apparent that the molecules of an electrical stratum will be further without each other's influence when accumulated on a long rectangular plate, R, fig. 21, than on a circular plate P of equal surface. I am not, however, prepared to enter upon any theoretical explanation of these phænomena, much less do I adduce them in opposition to any accepted theory of electricity. Whether the several facts I have been considering be explicable or not upon a given theory, I by no means pretend to determine; my object is the progress of scientific truth untrammelled by hypothesis, not theoretical disquisition: there is, perhaps, nothing more calculated to retard the advance of natural knowledge than undue subserviency to what may be designated as philosophical doctrine. The mind enslaved by opinion looks beyond the truth to the establishment of a long-cherished hypothesis; and the question at last is not as to the value of recently discovered facts, but as to their agreement with theory. It was thus with the Ptolemaists: rather than endanger their doctrine of planetary motion, they encumbered the heavens with cycles and epicycles in order to maintain it. My own impression is, that so far from common electrical action being of the complex and difficult nature usually assigned to it, it is of an extremely simple form, and that all its statical phænomena are capable of being reconciled and calculated upon a few very general and very simple elementary principles.

6 Windsor Villas, Plymouth,
July 20, 1857.

XXI. *On Iodide of Acetyle.* *By* FREDERICK GUTHRIE[*].

THE iodide of the oxygen-containing radical acetyle, or othyle, is formed when the oxide of that radical, or anhydrous acetic acid, is brought into contact with either of the iodides of phosphorus.

To prepare it, an equivalent of anhydrous acetic acid is poured

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[*] Communicated by the Author.

upon considerably more than an equivalent of dry phosphorus in a dry, long-necked flask, and dry iodine added in small quantities at a time in rather more than equivalent weight. The flask is then warmed till action has ceased. The liquid containing uncombined iodine is poured into a retort containing a piece of dry phosphorus, and placed in an inverted position in connexion with a condenser. After boiling for a few minutes, the retort is reverted and the contents distilled off. The boiling-point remains tolerably constant at 108° C.; the temperature may, however, be pushed to 120° C., at which point the residue in the retort is inconsiderable. The distillate is vigorously shaken with mercury until it becomes transparent, and then quickly decanted and rectified at 108° C. However cautiously the distillation be effected, a portion of the iodide appears to suffer decomposition; hydriodic acid escapes, and a solid residue containing iodine is left in the retort. This is probably a substitution product, containing iodine in place of hydrogen.

In the above manner, by acting on 3¾ ozs. anhydrous acetic acid, with 9½ ozs. iodine and about 1 oz. phosphorus, I obtained 5 ozs. of iodide of acetyle.

Iodide of acetyle is a transparent liquid of brown colour. This coloration is specific, and does not arise from dissolved iodine, for it cannot be removed by agitation with mercury. It boils at 108° C. at 757 millims., and has the specific gravity 1·98 at 17° C. Its vapour-density could not be determined with accuracy, owing to the above-mentioned fact of its not being perfectly volatile without decomposition. It fumes strongly in the air, has a most suffocating odour, and an intensely acid and corrosive taste. Water decomposes it instantly and with violence, hydriodic and acetic acids being formed. To this reaction is due the energetic action it has on organic bodies containing the elements of water.

In the analysis of this body, the carbon and hydrogen were determined in the usual manner, moderately heated copper-turnings being employed in the anterior of the combustion-tube to arrest volatilized iodine. The iodine was estimated as iodide of silver. For this purpose a bulb containing a weighed quantity of liquid, and provided with two capillary tubes, was immersed in a solution of nitrate of silver. The point of the lower tube being then broken against the side of the vessel, and that of the upper with the fingers, the air was forced gradually through, and the bulb subsequently washed into the silver solution.

Burnt with oxide of copper, ·7827 grm. gave ·4416 grm. carbonic acid and ·1641 grm. water.

·3038 grm. yielded ·4140 grm. iodide of silver.

		Calculated.	Found.	
C^4	24	14·12	...	15·39
H^3	3	1·76	...	2·33
O^2	16	9·41	...	
I	127	74·71	73·68	

The excess in carbon and hydrogen, and the deficiency in iodine, show that the liquid was not quite free from anhydrous acetic acid.

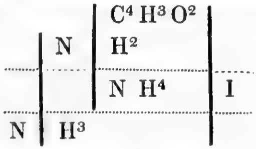
Zinc and sodium decompose iodide of acetyle at ordinary temperatures. Mercury, aided by the sun's light, also effects a recomposition, iodide of mercury being formed, and little or no permanent gas being evolved. The products of the action of these three metals appear to be alike, and to consist of a body or bodies, soluble in æther, and rapidly solidifying in contact with the air, probably owing to the absorption of oxygen. These bodies have not been obtained in a state fit for analysis.

If dry ammonia be passed over iodide of acetyle, an elevation of temperature occurs, the ammonia being abundantly absorbed. In the first place, two atoms of ammonia are concerned, the products being acetylamine and iodide of ammonium, according to the formula

$$2NH^3 + C^4H^3O^2I = NH^4I + NH^2C^4H^3O^2.$$

The iodide of ammonium, being insoluble in acetylamine, separates in the solid form. On continuing the action of ammonia upon this heterogeneous product, another atom of ammonia engages in the reaction, the iodide of ammonium dissolves, and a heavy, brown-coloured, oily liquid results, of spec. grav. 1·52 at 19° C., which becomes colourless on standing. This liquid yields up its third atom of ammonia even on exposure to the air, leaving behind acetylamine and iodide of ammonium. Heated with soda-lime, according to Will and Varrentrap's method, ·2684 grm. gave a platinum salt containing ·3376 grm. of platinum, or the liquid contained 21·82 per cent. of ammonia. The formula $C^4H^3O^2I + 3NH^3$ requires 23·08 per cent.

With respect to the rational constitution of this body little can be predicted. If the ammonium type be preserved, as is probable, it may be represented by the self-explicatory scheme,—



$$\begin{array}{l|l|l|l} & & C^4H^3O^2 & \\ & N & H^2 & \\ \hline & & N\ H^4 & I \\ \hline N & H^3 & & \end{array}$$

Treated with hydrochloric acid and bichloride of platinum, a salt was obtained, ·5508 grm. of which yielded ·2247 grm. of platinum, or 40·79 per cent. Now as the body

$$2(NH^4\,Cl\,Pt\,Cl^2)+C^4\,H^3\,O^2\,NH^3\,I\,Pt\,Cl^2$$

requires 40·14 per cent., it follows that the hydrochloric acid had determined the splitting up of the body, as indicated by the dotted lines in the above scheme. This result confirms also all the preceding analyses.

Owens College, Manchester,
August 1857.

XXII. *On the Preparation of the Double Æthers.*
By FREDERICK GUTHRIE*.

BY acting upon chloride of amyle with an alcoholic solution of caustic potash in a sealed tube at 100° C., M. Balard obtained a volatile ætherial liquid boiling at 112° C., which he described as oxide of amyle. The boiling-point induced Dr. Williamson to regard this product as the double æther of the composition $C^{10}\,H^{11}\,O,\,C^4\,H^5\,O$, being, in fact, identical with the æther he himself obtained by the action of iodide of amyle on æthylate of potash, or of iodide of æthyle on amylate of potash.

The latter chemist also describes this æther as a product of the action of sulphuric acid upon a mixture of the corresponding alcohols. As I required the æther in point in rather large quantity, I endeavoured to form it in the last-mentioned manner; but after modifying the process in many different ways, and employing large quantities of the mixed alcohols, I was forced to abandon it; for although the product had the characteristic smell of double æther, yet I was unable to deduce a liquid of fixed boiling-point. The cause of this appears to lie in the fact, that the minimum temperature sufficient for the ætherification of the æthylic alcohol effects a more profound decomposition of the amylic alcohol, amylene or even sulphurous acid being formed.

The experiment of M. Balard therefore naturally presented itself as suggesting a possible and, if possible, economical method of attaining my object. The following modification of it was found to answer very satisfactorily.

A retort connected with an inverted and well-cooled condenser was half-filled with amylic alcohol. Into this about two equivalents of very finely-powdered caustic potash were introduced in small quantities at a time, and the whole boiled for half an hour. To the contents of the retort, which became solid on cooling, an equivalent of iodide of æthyle was added gradually through a

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* Communicated by the Author.

funnel-tube. Double decomposition at once began; heat was evolved, the contents of the retort becoming liquid and boiling. The action was allowed to go on by itself for two or three hours, but was then assisted by the heat of a water-bath. After some hours' digestion the contents of the retort ceased to boil. The clear liquid was then decanted from the iodide of potassium and excess of caustic potash and rectified.

The rapidity with which the boiling-point rises from 100° to 110° C., its constancy at and about that point, and the smallness of the residue having a higher boiling-point, were evidences of the completeness of the reaction. In fact, after six rectifications the weight of the double æther obtained was two-thirds of that of the amylic alcohol employed. It boiled at 111°, 112°, 113° C., and gave on analysis (mean of two),

	$C^{10}H^{11}O, C^4H^5O$.	Found.
C . . .	72·41	71·66
H . . .	14·65	14·27

In the above process the excess of caustic potash combines with the water arising from the separation of the amylic æther from the amylic alcohol, so that no water appears in the free state,—

$$C^{10}H^{11}O, HO + 2KO, HO + C^4H^5I = KI + KO, 2HO$$
$$+ C^{10}H^{11}O, C^4H^5O.$$

There can be little doubt that analogous recompositions occur between the iodides of the other radicals and the solutions of potash in the several alcohols; the chief point to be attended to being, of course, such selection of the two that the boiling-point of the product shall differ as widely as possible from those of the iodide and alcohol employed. The advantage of this mode of preparation consists wholly in dispensing with metallic potassium or sodium, metals which of course entirely lose value as soon as oxidized.

It is clear that all such bodies may be regarded in three ways: as double æthers, as single æthers, or as alcohols, according to the general formulæ

$$C^{2m}H^{2m+1}O, C^{2n}H^{2n+1}O,$$

$$\left.\begin{matrix} C^{m+n}H^{m+n+1}O \\ C^{m+n}H^{m+n+1}O \end{matrix}\right\} \text{ equivalent to } C^{m+n}H^{m+n+1}O,$$

or

$$C^{2(m+n)}H^{2(m+n)+1}O, HO.$$

The action of pentiodide of phosphorus upon a member of this class seemed to promise to throw light upon the above question. Accordingly an ounce of the æthylate of amyle, prepared as above, was subjected to the action of 2½ ounces of iodine in presence of an excess of phosphorus, as in the preparation of iodide of æthyle. Reaction ensued, and the decanted liquid gave a distillate which was heavier than water; but on redistilling, this distillate showed no fixed boiling-point; and on rectification, two liquids were isolated, having the respective boiling-points and all the properties of iodide of æthyle and iodide of amyle. Now, according to the two latter of the above formulæ, we should in the one case expect a body having the composition $C^{m+n} H^{m+n+1} I$, and in the other a body having the composition $C^{2(m+n)} H^{2(m+n)+1} I$. The formation of two distinct iodides is incompatible with both of these, and supports, if it does not prove, the correctness of the formula

$$C^{2m} H^{2m+1} O, C^{2n} H^{2n+1} O.$$

Owens College, Manchester,
August 1857.

XXIII. *On the Formation of Craters, and the Nature of the Liquidity of Lavas. By* G. Poulett Scrope, *Esq., M.P., F.R.S., F.G.S.*

[Concluded from p. 142.]

II. THE *Nature of the Liquidity of Lavas.*—So much for that branch of my subject,—the formation of cones and craters. I wish now to ask attention to some circumstances respecting the mode of emission and nature of the lavas that proceed from them. I have already spoken of the comparatively tranquil manner in which some lava-streams are seen to well out from the flank of a volcano, or its summit, and the probability that differences in the liquidity or viscosity of the heated matter at the time of its efflux may occasion corresponding differences in the character of the phænomena. Observation confirms this expectation; and it has been remarked, that the very liquid and vitrified lavas, such as those of Kilauea and Bourbon, are poured out more or less tranquilly without any very violent explosions, their imprisoned vapours evidently escaping with comparative ease, while the more viscous and ultimately stony lavas, possessing a minor degree of liquidity, and consequently not allowing so easy a passage to the vapours that rise through, and struggle to escape from them, are protruded with fiercer explosive bursts, and the ejection of far greater quantities of scoria and other fragmentary matters.

This observation, coupled with other reasons to which I shall presently advert, led me to an opinion expressed in the works previously referred to, that the ordinary crystalline or granular lavas (making exception of the vitreous varieties), although at a white heat at the moment of their emission from a volcanic vent, are not in a state of complete fusion; that a large proportion at least, if not all, of the crystalline or granular particles of which, when cooled and consolidated, they appear composed, are already formed and solid, their mobility being aided by the intimate dissemination through the mass of a minute but appreciable quantity of some fluid,—in all probability water,—which is prevented from expanding wholly into vapour by the pressure to which it is subjected while within the volcanic vent, or in the interior of the current, until that pressure is sufficiently reduced to allow of its expansion in bubbles, or its escape through pores or cracks, by which it passes into the open air from the surface of the intumescent lava.

I was strengthened in this opinion by several concurrent considerations:—

1. If all lavas are (as they are usually supposed to be) in a state of complete fusion when they issue from a volcano, how is it that they do not all present the same glassy texture which is seen in some, the obsidians, the pitchstones, and pumiceous lavas especially, and in the ropy, cavernous, filamentous basalts of Kilauea, Iceland and Bourbon, and which these very crystalline and stony lavas themselves put on when melted under the blowpipe or in a furnace? The usual answer is, that the granular and crystalline texture is acquired subsequently to emission by slow cooling; and the experiments of Gregory Watt and Sir James Hall are cited in support of this assertion. In the present day, probably the processs by which Messrs. Chance and Co. of Birmingham, devitrify a mass of fused basalt (from the Rowley rag, near Dudley) by causing it to cool slowly in an "annealing furnace," would be considered as a strong confirmatory fact.

But there is no fact more certain than this, that the superficial portions at least of a lava-current flowing in the open air do not cool slowly. On the contrary, they are rapidly, I might say instantaneously, upon their exposure, consolidated and cooled down to a temperature which permits them to be handled and even walked upon without damage. How is it that this scoriform crust, or the solid cakes and slabs which so instantly form upon every exposed surface of lava, nay, even the scoriæ which are tossed up in a liquid state by the eruptive jets, and harden while yet in the air before they fall, exhibit on fracture no glassy texture, but much the same earthy or stony grain, and occasionally crystals of considerable size in the solid matter separating their cellular cavities, as is found in the interior of the current which is known to have cooled very slowly? How is it that some lava-currents are stony throughout, others vitreous throughout, as, for example, some of the large pumice-streams of Lipari, Iceland, and the Andes?

I have recently visited the manufactory of Messrs. Chance and Co., at Oldbury, near Birmingham, for the purpose of examining the mode in which the basalt used there (and which is the same upon which Mr. Gregory Watt experimented) conducts itself in their furnaces, and I found that when the liquid and fused contents of a furnace at a white heat are poured out upon a brick or other floor into the open air, so as to represent a stream of lava flowing out of a volcanic vent, the mass consolidates throughout, whatever its bulk, into a homogeneous and purely vitreous black obsidian, in fact an absolute glass, with a conchoidal fracture and sharp cutting edges. It is only when made to consolidate very slowly in an oven kept at a high temperature for some days, that it assumes the deadened and semi-crystalline texture of the manufactured article.

If this process be interrupted, it is found to have commenced by the formation, at numerous points within the vitreous mass, of globular concretions about the size of a small pea, of a lighter colour than the base, and having a pearly lustre and radiated structure. The multiplication and confusion of these crystallites or sphærulites ultimately destroy the glassy character of the substance altogether, and give to it a pearly semi-crystalline texture, without, however, restoring the far more crystalline aspect of the basaltic rock. A similar change may be often observed to have taken place in nature among the vitreous lavas, which pass into pearlstone and pitchstone by the formation of the same kind of sphærulitic concretions, and of course there is no question as to the complete state of fusion in which such lavas have been produced. But there is no trace of such a process in any of the ordinary earthy, and stony or crystalline and porphyritic lavas. I am not aware of a single current from either Etna or Vesuvius having ever exhibited, even on its most rapidly cooled surfaces, any passage into true obsidian, or sphærulitic pearlstone, or any portion of such vitrifactions. A pellicle or glaze of a semi-vitreous appearance coats the surface in some parts, or lines the cellular cavities; but it seems evident that the bulk of the matter could not have been at the time of its emission in that thoroughly fused condition which it assumes when melted in a furnace or under the blowpipe.

2. It struck me that temperature does not alone determine the fusion or liquefaction of substances; and that compression may prevent the liquefaction of a solid at a high temperature, just as it prevents the vaporization of a liquid, in the common experiment of boiling water at a lower temperature in a rarefied atmosphere. If so, the intense pressure to which heated lava must be subjected before it rises from the bowels of the earth to discharge itself on the surface, intensified by the reaction of its

own expansive force from the confining surfaces, might perhaps prevent its complete fusion, however high the temperature.

3. I had long been impressed by the vast volumes of aqueous and other elastic vapours evidently discharged from every volcano in eruption, and to all appearance the chief agents in the expulsion of lavas from the bowels of the earth. That this vapour is liable to be developed in every part of the mass of lava is shown by the formation of vesicles throughout its substance wherever the pressure is so reduced as to permit their expansion; for instance, in the superficial portions of a current, and in some lava-currents throughout the entire mass.

The experiments of Mr. Knox, related in a paper read before the Royal Society in 1824*, had taught me that water in an appreciable quantity is mechanically combined with the elementary particles of all the crystalline rocks of igneous origin. The questions therefore arose,—Might not the water thus intimately disseminated through a mass of crystalline lava, although at an intense temperature, remain unvaporized, owing to the still greater intensity of the pressure by which it is confined while yet within the bowels of the earth? And would it not, under these circumstances, exert an intense expansive force upon all the confining molecular or crystalline surfaces between which it lies, and thus occasion a tendency to separation among these solid particles whenever the compressing forces were relaxed, or the temperature increased sufficiently, so as to give a certain degree of mobility to these particles *inter se,* and an imperfect liquidity to the mass composed of them? And, supposing the intumescence thus occasioned to raise any portion of this semi-liquid matter into the open air, would not the instantaneous absorption of caloric from the contiguous particles that must accompany the vaporization of this water, and its escape in bubbles or pores and through cracks, owing to the nearly absolute cessation of pressure, account for the sudden cooling down and *setting,* or consolidation, of the exposed surfaces, without having undergone complete fusion (except in the case of mere superficial films), notwithstanding their previous intense temperature, amounting even to a white heat?

This supposition seemed to me to account satisfactorily, not only for the absence of a vitreous texture even in superficial portions of many lava-streams, and their instantaneous consolidation on exposure, in cellular or porous slabs and cakes, but also for several other characteristics of igneous rocks, not easily to be reconciled with the idea of their having always issued from the earth in a state of absolute fusion; such, for example, as the cracked and vitrified aspect of the felspar-crystals of many tra-

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* Phil. Trans. 1825.

chytes, the broken and dislocated appearance of the leucites, felspars, and other crystals in many basalts; the frequent arrangement of their longest axes in the direction of the bed of the rock, that is, of the movement of the lava when liquefied; the finer grain often exhibited towards the tail or extremity of a current than at its source, the brecciated lavas which appear to have enveloped fragments in great number of the same material without any fusion even of their finest angles. So also might be explained the more or less spongy, porous, and loosely crystalline texture of many trachytes, and their disposition in thick beds or dome-shaped bosses, attesting their protrusion in a very imperfect state of liquidity, more resembling the intumescence of some kinds of dough in an oven than the fusion of metal in a furnace.

And here let me remark, that Dr. Daubeny, and some other writers on volcanic phænomena, have spoken of the vesicles or air-bladders in lavas, as being proofs of their having been in a state of complete fusion. But have the loaves baked in our ovens been in fusion? The comparison of a cellular scoria with a loaf or a French roll will show that vesicles of precisely similar appearance to those of lavas are producible in substances of a pasty consistence, which owe their liquidity to an aqueous vehicle, the heat applied being only sufficient to develope the contained gases. Other kinds of baked cakes are porous rather than cellular, and aptly represent the texture of the earthy and porous trachytic lavas.

Plutonic Rocks.—This theory as to the nature of the liquidity of many lavas appeared to me so reasonable, that I proceeded to examine its applicability to the still more generally crystalline plutonic rocks, from the alteration of which by heat lavas are usually supposed to derive. I asked myself, what would probably be the effect on a mass of granite, for example, containing water intimately combined with its molecular particles, and confined beneath overlying rocks and seas, under circumstances of intense compression, and at the same time high and increasing temperature? Surely a tendency to intumescence, which, whereever, and in proportion to the extent to which it takes place, must elevate and fracture the overlying rocks, and likewise disintegrate more or less the crystalline particles of the swelling mass, through the irregularities of their internal movements and mutual friction. Many of the crevices broken through the neighbouring rocks would be injected by the intumescent matter. Some may be sufficiently enlarged to allow of its forcing its way into the open air as a lava, perhaps accompanied by eructations of the gases and vapours developed in the lower parts of the mass; or, should the liquefaction not be sufficient to admit of the rise of aëriform bubbles, as matter of a porous, pasty, or glutinous consistency, perhaps even semi-solid in texture and bulky in form.

It might happen that, circumstances occasioning in turn the preponderance of the compressing over the expansive forces (by reason, for example, of a diminution of temperature), portions of the subterranean crystalline mass will, after a partial intumescence of the kind supposed, return to a state of solidity. The result may be a more fine-grained rock, owing to the partial disintegration of the crystals; or, if the disintegration had proceeded sufficiently far, new mineral combinations might take place. Indeed, Watt long since proved that the particles of even apparently solid rocks are capable, through changes in temperature, of internal motion sufficient to admit their rearrangement according to polarity, that is, of crystallization. Still more likely is this result to occur on the condensation or escape of any fluid which had previously kept them from contact with each other, since the crystalline polarity can only exert itself within minute distances. And thus might be accounted for the frequently observed passages of granite and gneiss into syenite, greenstone, trap, or trachyte, and the varieties of mineral composition which these rocks at times exemplify. So also the transitions from the larger crystalline grain to the finer, and the dykes and veins which these rocks so often contain themselves, or intrude into their neighbours. So, too, the finer grain of the sides or selvages of such dykes might be owing to the greater disintegration of the crystals by friction along these sides as the matter was driven through them.

Another problematical fact which this theory of an aqueous vehicle in heated granite would account for, is the usual appearance of the quartz in this rock, not in crystals, but as a paste or base, seeming to be moulded upon the crystals of felspar. Had the rock crystallized from a state of fusion, the felspar, being far more fusible than quartz, might have been expected to be the last, not the first, to crystallize. But if the water disseminated through the rock were supposed to have taken the quartz into solution by aid of the alkalies present in the felspar, the fluid vehicle would in fact become a liquid or gelatinous silicate; and upon consolidation would naturally mould itself on the felspar crystals, or appear as a paste to them. I adduced the hot siliceous springs of Iceland and other volcanic districts as proofs that heated water under such circumstances could dissolve silex.

Those who will take the trouble to refer to the 2nd, 4th, 5th, and 6th chapters of my 'Considerations on Volcanoes,' will see that the above is a brief summary of the arguments there put forth, perhaps at too great length, and in a form which may have hindered their obtaining at the time of their publication the attention which I believe they merited.

Certain it is, that they were at that time, now thirty years back, neglected or generally discredited. I was told that my views were "unchemical." I was represented as asserting incandescent lava to be "cold or thereabouts*." The igneous and the aqueous origin of certain rocks had been so hotly contested, and fire and water were usually considered so antagonistic, that it seemed at first view an absurdity to imàgine that both could be combined in a substance seemingly in fusion. Probably also the idea was scouted at first through the notion that water could not be present within an incandescent mass of lava without causing it to explode like a mine; which might of course be the result of any considerable body of water being localized at one point. But the view I entertained, as has been explained, was that the water (and to some extent, perhaps, liquefied gases), to which I attributed much of the liquidity of some lavas, was disseminated throughout its mass, occupying minute interstices, and in intimate, though probably mechanical, combination with every molecule,—indeed intercalated between the plates even of its solid crystals; and moreover that the pressure to which the rock was subjected while beneath the earth was so enormous, as to prevent the vaporization of these minute portions of liquid anywhere except at points where the intensity of temperature, and consequently of expansive force, overcame the resisting forces, and thereby caused either the formation and rise of great bubbles of vapour from the lower depths of the subterranean lava-mass, or the inflation of minor bubbles and pores throughout it, or at least in the superficial portions which by intumescence were forced into the open air.

Of late, however, views precisely in accordance with the theory printed by me in 1824 have been put forward, and have attained extensive adhesion among continental geologists.

M. Delesse has proved by experiment the solubility of the silex of rocks in heated water containing either of the mineral alkalies. And, indeed, the manufacture of artificial stone is now carried on in this country (Messrs. Ransome's process) by saturating loose sand with an artificial hydrate of silica. Huge blocks of flint, I understand, are thrown into the hot alkaline water, and melt down like so much sugar.

Again, the experiments of Boutigny have shown that water at a white heat remains unvaporized, in the form of spheroidal globules, in which form it is obvious how readily it would communicate mobility to the solid particles among which it was

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* Westminster Review.

entangled; and how (according to these experiments) it might flash into bubbles of vapour on the reduction of its temperature by exposure to the air.

M. Deville, in his recent observations on the vapours disengaged from Vesuvius since the eruption of May in last year (for the perusal of which I am indebted to the kindness of my friend Dr. Daubeny), arrived at the conclusion, to use his own words, that "water in the proportion occasionally of 999 per mille must have formed an integral part of the Vesuvian lava at the moment of its emission; and, consequently, that in the interior of the incandescent lava there is such an arrangement of molecules as to permit the gaseous and volatile matters to remain there imprisoned until, in the progress of cooling and consolidation, they evolve themselves."

Above all, M. Scheerer of Christiania, the eminent Norwegian geologist, who is better acquainted perhaps than any other with the granites of that country, published in 1847 a theory, which, he says, his observations had suggested to him in 1833, on the production of granite, entirely identical with that which I had ventured to suggest in 1824–25. I take the following account of it from the paper read before the Geological Society of France in 1847, and published in the fourth volume of the *Bulletin de la Soc. Géol.* p. 468.

M. Scheerer attributes what he calls the "plasticity" of granite when protruded on or towards the surface of the earth (a condition evidenced by the veins it throws into the fissures of neighbouring rocks) to the combined action of *water* and heat. He describes the water as "intercalated between the solid atoms of the crystalline and other constituent minerals, endeavouring to escape by its tendency to vaporization, and consequent elasticity, but unable to do so owing to the pressure to which the enclosing mass is subject." He considers the water so contained in granite to be "primitive;" that is, one of the original bases of the rock, and not the result of infiltration. He attributes to it the solution of the quartz, aided by the alkali, and the consequent moulding of this mineral on the felspar-crystals. He even goes the length of styling the condition of granite before its protrusion by the term "une bouillie aqueuse," *a granitic broth.*

These theoretical opinions of M. Scheerer appear to have received the assent of M. Elie de Beaumont and other French geologists*. Their exact conformity with those which were first developed in my treatise on Volcanoes, published in 1824–25, and repeated in the Preface to my volume on Central France in

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* See *Bulletin de la Soc. Géol. France*, new series, vol. iv. p. 1312.

1826–27, will be evident to any one who will take the trouble to refer to those works.

It is not, however, for the vain purpose of claiming a priority in these views that I now ask the attention of the Society to them, but because the subject has not, I think, yet attained the consideration it deserves from the geologists of this country; and especially because of its leading, if followed out, to further inferences of considerable importance, which were likewise suggested by me in 1825, but have been hitherto only partially pursued to their legitimate consequences.

Laminated or schistose rocks, slaty cleavage, and folded rocks.—I refer to the mechanical changes in the texture and structure of the plutonic rocks which could not fail to have resulted from the mutual friction of the component crystalline particles attendant on their internal movements, whether caused by mere dilatation and recompression in place, or by a shifting of the entire mass in any direction, under intense and opposite, but irregular pressures.

I was led to reflect on this by observation of the ribboned pitchstones of Ponza and Ischia, in which, while in a state of vitreous fusion, crystallites had formed (just like those of the Oldbury obsidian), and subsequently been broken up by the movement of the semi-liquid mass, and drawn out into long stripes, giving a ribboned appearance to the rock.

Further examination proved to me that the ribboned trachytes of Ponza and Ischia, and some ribboned clinkstones, owed that character to a similar elongation of the felspar crystals and felspathic particles which they previously contained, in the direction in which the semi-liquid mass flowed, or rather was forced to move, and in which the pores or cells, when there are any, are equally elongated. These observations suggested to my mind the reflection, that the solid particles of any crystalline rock which is put in motion while in a state of imperfect solidity, and under the influence of opposing pressures, must be subject to a great amount of mutual friction or disturbance, by which their final arrangement when wholly consolidated will be determined.

Thus, suppose a mass of granite, of which A B (fig. 4) represents the section, consisting of crystals of felspar and mica irregularly disposed in a basis of more or less liquefied or gelatinous silex, exposed to movement in the direction A B, while under vast pressure both from above and below, that is, in the opposite directions C and D. Whether the surface C or D, or both, remained fixed, or merely moved, owing to resistances, at a slower rate than the other parts, the crystals in the latter would be turned round by internal friction, and rearranged and drawn out in stripes or planes in the direction of the motion, while the proportionate dimensions of the mass would be equally varied so as to produce a section something like E, F, G, H (fig. 5); in

Fig. 4.

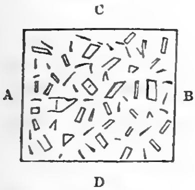
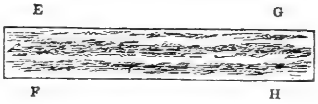


Fig. 5.



fact, a rock which, if no further change occurred in it except consolidation in place, would have all the characteristics of *gneiss*. The same movement, if still further continued, might, it appeared to me, be expected to disintegrate the angular crystals of felspar altogether, so as to cause them to disappear, perhaps to force their elementary molecules to melt into the intensely heated silicate, to which they would impart their alkalies. And the resulting rock, supposing the laminæ of the mica-crystals to slide readily past each other, when lubricated by the silicate, and not therefore to be so far disintegrated as those of felspar (as from their peculiar form might be expected), would put on a lamellar structure, and very much resemble *mica-schist*,—especially since the great flexibility of the mica would render its laminæ extremely liable to yield to the irregularities of pressure pervading the mass in a variety of directions, and consequently to take such wavings and contortions as are often exemplified in that rock. Whoever will examine the tortuous way in which the plates of mica envelope and bend round nodules of half-melted quartz or crystals of garnet in mica-schist, will be convinced, I think, that the whole mass has been subjected to great internal movement and consequent friction in the direction of the layers of mica, while under intense pressure, and in a comparatively softened state, the mica being lubricated, as it were, by a vehicle of liquid or gelatinous quartz. Whatever fissures or cracks were formed during this movement in the semi-solid rock, or subsequently, so long as the silicate remained unconsolidated, would be necessarily filled by it, and ultimately appear in the shape of the quartz-veins so frequent in this class of rocks.

Under this supposition, gneiss and mica-schist would bear the same relation to granite as the ribboned trachytes and schistose lavas (clinkstone) to ordinary crystallized or granular trachyte; and the quartz-rocks associated with granite represent the quartzose trachytes of Hungary, Fonza, and the Andes.

These views, developed by me in 1825, I cannot but think deserve the attention of geologists engaged in investigating the origin of the so-called "plutonic" and "metamorphic" rocks. It seems to me more probable that some process of this kind may have metamorphosed granite into the laminated rocks of plutonic origin, gneiss and mica-schist, than that these rocks should have been formed by the mere fusion and reconsolidation or crystallization in place of sedimentary strata *already laminated,* according to the usual "metamorphic" doctrine. I can understand the clay-slates and other fine-grained schists to have been formed through the mechanical disintegration of mica-schist, but not mica-schist by the baking or melting and cooling of the clay-slates in place, in the manner suggested by Sir C. Lyell.

In the formation of the clay-slates, perhaps, the action of heat was not concerned (except as engendering the pressure to which they have evidently been subjected), but that of water or an aqueous silicate only. Still in their case also internal movements and mutual friction of the component particles under extreme and irregular opposing pressures have, I am convinced, had a primary influence in occasioning that parallel arrangement of the scaly and flaky micaceous particles to which their slaty cleavage is due. This, at least, was the conviction forced upon my mind by a close examination of the fissile clinkstone of the Mont Dor and Mezen, which is used for roofing-slate, and is in its lamination and cleavage undistinguishable from many clay-slates. And that opinion I recorded at the time in my 'Considerations on Volcanoes*.'

I have since found this view of the origin of slaty cleavage supported by Mr. Darwin in his work 'On Volcanic Islands,' and by Mr. Sorby in his paper on slaty cleavage in the Edinburgh Philosophical Journal for 1853. I need not say that such support affords strong confirmation of its correctness.

Of course we are led to connect the movements under extreme pressure, to which this peculiar texture of the laminated rocks is here attributed, with the action of those same forces by which their beds have been so generally bent and contorted into a series of folds or wrinkles, more or less at right angles to the general strike.

If we seek to discover under what circumstances these flexures were brought about, we can hardly be wrong in ascribing them to the same violent process by which they have been elevated, usually on the flanks of some protruded ridge or enormous dyke of crystalline rock, which is seen to form the axis of the mountain range to which they belong.

Now what may we suppose to have been the character of this elevatory process?

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* See pp. 103, 144 and 202.

The phænomena of active volcanoes, and the protrusion of intumescent crystalline matter on so many points of the earth's surface, and at all periods of its history, may be admitted to prove the continued existence beneath a very large area of that surface—if not the whole—of a mass of intensely heated crystalline matter, having disseminated throughout its substance (in the manner already dwelt upon) some fluid or fluids, such as water, affording an imperfect liquidity to the mass, and, by its intense elastic force, communicating to it a powerful tendency to expansion. Now suppose any considerable diminution to occur locally in the amount of pressure confining this expansible mass beneath the crust of the globe,—such as might be brought about by any extraordinary concurrence of the ordinary barometric, tidal, oceanic, or excavating causes (not to suggest others),—or, on the other hand, any considerable increase of its expansive tendency, owing to a local increase of temperature, from some unknown, but easily imagined, cause,—we should anticipate, as the necessary result, the violent fracture and elevation of the overlying crust of rocks, and the extrusion through some principal fissure, or line of fracture, of a ridge of the subterranean intumescent crystalline matter.

It seems very probable that under such circumstances the central axis of the protruded ridge may retain its irregularly crystalline grain and structure, but that the portions of crystalline matter that from either side would rush or be thrust up by pressure from behind (consisting partly of the weight of the overlying rocks on the semi-liquid matter below them) towards the opening should be subjected to so much internal friction of their crystalline particles, and so much pressure at right angles, or nearly so, to the direction of the movement, as must stretch and draw them out into parallel planes,—just as happened evidently to the striped and ribboned trachytes in the protruded dykes of Ponza and Palmarola. This friction and pressure would be extreme, of course, along the lateral parts of the protruded mass, that is, the selvages of the great dyke; which, if the original mass were granite, would thus appear composed of an axis of granite, passing on either side into gneiss (or squeezed granite) and further on into mica-schist.

But every irregularity, whether on the large or the small scale, obstructing more or less the even motion of the layers, must create a waving or contortion in them, especially in the planes of slippery mica-plates, such as is exemplified even in hand-specimens of the Ponza trachytes, and also on the largest scale in the same locality. And the extreme irregularities of motion, occasioned on the upper layers of the intumescent mass by the pressure and resistance of the overlying beds, may be expected to carry their wavings still further, and at the throat of the fissure, where the squeeze and jam of the protruded matters must be at its maximum, to occasion those enormous and repeated zigzag foldings of the laminated beds, so frequently observed in mica- and chlorite-schists in such positions.

Meantime another influence would be similarly affecting the overlying stratified rocks above, or on the outer flanks of the elevated axis, namely their own specific gravity, urging them to slide or slip laterally when tilted up at (perhaps) a considerable angle on either side. The more compact and indurated strata would be partly fractured into cliffy masses, partly broken up into breccias and conglomerates by this movement; but the softer beds, especially those which were saturated with water (perhaps even yet under the sea), or which contained interstratified beds of silt, shale or clay, permeated with water, would glide laterally away from the axis in extensive landslips, and be wrinkled up into vast foldings under the intense pressure compounded of their own weight, and that perhaps of portions of the protruded matter thrust against them,—in a manner very similar to the contortions produced in the more crystalline laminated rocks by the violent squeeze which accompanied *their* protrusion. It may even be difficult to draw a line between the effects of these two replicating and fracturing forces. But, together, they seem to me sufficient to account for most of the phænomena of the kind observable in mountain-chains.

These were the ideas on this subject which I endeavoured to develope, though very imperfectly I am aware, in the more theoretic portion of my work on volcanoes, so often referred to, and they were illustrated by a rude ideal section of an elevated mountain-chain in the frontispiece to the volume. I still think they will be found a not improbable solution of this the greatest problem in the dynamics of geology. It appears to me that the results would be much the same, whether we suppose this elevatory action to have been *paroxysmal* and simultaneous or gradual, taking place by minor and successive expansive throes or shocks, or even still more slowly in the manner of a *creep,* as Sir Charles Lyell would probably conceive it to have operated, and to be still continuing. On these last assumptions, the earthquake-shocks which certainly accompany at present every effort of elevation, and appear to be propagated in waves through the substance of the earth's crust, in directions usually at right angles to the principal axes of elevation, or fissures of crystalline protrusion, may indicate the force by which the extreme replications and slaty cleavage of the laminated beds are occasioned.

I would ask of geologists to consider whether such a mode of protrusion of the laminated crystalline rocks, and of the lateral replication of the more earthy schists and marine strata, as is here suggested, does not accord with the general facts known respecting their position? Let me take two descriptions of the general position of the crystalline rocks from two writers of experience, judgement, and wholly impartial character, as respects the theory here indicated. Mr. Evan Hopkins* gives as the results of his extensive mining experience in the Andes and elsewhere, "that the great base [of all mountain-chains] is below more or less granitic, strongly saturated with mineral waters, and that this passes upwards by insensible gradations from a crystalline homogeneous compound into a laminated rock, such as gneiss, and still higher up into schists in vertical planes; the peculiar varieties of the higher rocks depending on the mineral character of the 'parent rock' below; the schistose rocks forming, in short, the external terminations of the great universal crystalline base,"—that is to say (as I would phrase it), the squeezed out, and therefore laminated, upper and lateral portions of the inferior crystalline mass.

Mr. Ruskin, in his recently published volume, having closely examined the structure of the Alps with the eye of a geologist no less than of a painter, but certainly without any theory to support, declares that the central axes of "irregular crystallines" (as he calls the granitic rocks) uniformly graduate on either side into the foliated or "slaty crystallines," *i. e.* into gneiss and ultimately mica- and chlorite-schists.

One point observed in the structure of the Alps and many other mountain-chains I may notice before I conclude, namely the occasional dip of the elevated strata towards the central axis of extruded crystalline rock, producing a synclinal instead of an anticlinal ridge. Another section copied loosely in the frontispiece to my work on volcanoes, from Von Buch's paper on the Tyrol, may show the mode in which I conceive this to have occurred through the injection of a mass of crystalline matter into a wedge-shaped fissure, opening downwards; such as must have frequently occurred among the fractures of the overlying strata—giving occasion in some cases to the further rise of the heated and intumescent matter into the hollow between the outer slopes of the synclinal valley. It would indeed accord with the theory suggested above, if such dykes or extravasations at synclinal axes were found to alternate frequently with the elevated anticlinal axes, for the cracks formed in indurated beds of overlying

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* Quart. Journ. Geol. Soc. vol. xi. p. 144.

rock would very frequently open alternately upwards and downwards[*].

Time will not allow of my dwelling now upon other points explanatory of geological problems, which are afforded by the theory of an expansive subterranean crystalline mass preserved by external pressure in a more or less solid condition beneath the crust of the globe, but always ready to expand and perhaps to intumesce upwards on any relaxation occurring in the overlying pressure. But I suggest it now, as I did thirty years since, as the solution most reconcileable with the known facts of the structure and relative position of the great elevated rock-formations of the globe, and as a theory founded, not upon mere guess-work, but on careful and extended observation of the phænomena of both active and extinct volcanoes, and the disposition of volcanic products of all ages.

XXIV. *On the Reciprocal Action of Metals and the Constituents of Well- and River-waters.* *By* HENRY MEDLOCK, *Esq.*[†]

I. *The Action of Lead.*

THE solvent action of water on lead is a subject of special importance, and one that has attracted the attention of many of the most able chemists of the day.

To determine by analysis whether or not a particular water will become impregnated with lead when placed in contact with that metal, is a question of high scientific interest, and one affecting the health of every civilized community. This question has occupied my attention for several years, and with the view of elucidating it still further I have undertaken a series of experiments, the result of which will be communicated in the following pages.

On referring to the opinions of the distinguished chemists who have investigated this subject, many discrepancies are observed. Some consider the action of water on lead to be due to the softness of the water, and absence of any considerable quantity of earthy salts; some attribute it to free carbonic acid; others to oxygen dissolved in the water; whilst some conjecture it to be due to the presence of nitric acid, which is one of the products resulting from the oxidation of organic matter containing nitrogen.

Dr. Smith[‡], in his able paper on the waters of the Dee and

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[*] See the diagram at p. 205 of 'Volcanoes.'

[†] Communicated by the Author.

[‡] Quart. Journ. Chem. Soc. vol. iv. p. 123.

Don, from the former of which the supply for the city of Aberdeen is obtained, states that he found in various samples of the Dee water, which had passed through leaden pipes and had been stored in leaden cisterns, quantities of lead varying from $\frac{1}{100}$dth to $\frac{1}{4}$th of a grain per gallon, the quantity rapidly increasing with the time the water remained in contact with the lead.

With the view of ascertaining the cause of the Dee water possessing such an energetic action on lead, Dr. Smith performed a series of experiments on the action of this water upon *bright* and on *tarnished* surfaces of lead. He found the quantity of lead dissolved varied from $\frac{1}{100}$dth to $\frac{1}{6}$th of a grain per gallon; and he observed that *tarnished* surfaces of lead were more readily acted upon than those which were perfectly bright. The conclusions arrived at by Dr. Smith are,—

"1st. That the action of water on lead is dependent on the *aëration* of the water; since in rainy weather, when the Dee water is coloured and contains but little air, the quantity of lead it dissolves is less than when the water is bright and well aërated.

"2ndly. That the water at all times acts more strongly upon old and tarnished lead than it does upon bright surfaces of the metal."

The following are the analyses of the waters of the Dee and of the Don given in grains per imperial gallon (70,000 grs.) :—

	Dee.	Don.
Lime	0·526	1·29
Magnesia	0·110	0·34
Potash and soda	0·382	0·58
Carbonic acid (combined)	0·374	0·96
Sulphuric acid	0·275	0·76
Chlorine	0·338	0·74
Silica	0·140	0·60
Iron, alumina and phosphates (precipitated by ammonia)	0·080	0·38
Organic matter and loss	1·775	3·00
Ammonia	traces	...
	4·000	8·65

These waters are remarkable for the small quantity of solid matter, and their consequent softness. They contain, however, a very large proportion of *organic matter*. The Dee water contains, as might be expected, traces of *ammonia*; but Dr. Smith failed to detect any nitric acid by operating on the residue of one gallon of the water, although nitric acid, or one of the oxidation products of ammonia, must undoubtedly have been present.

Neither ammonia nor nitric acid appear to have been looked for in the water of the Don.

A very able and important paper by Dr. Noad*, "On the Composition of certain Well-waters in the neighbourhood of London, with some observations on their action upon Lead," was published in 1852. The author was led to examine a sample of water from a well near the old church of Highgate, in consequence of the powerful action of the water on leaden pipes and cisterns, and also from the large amount of solid matter it contained. Of organic matter there was no appreciable quantity, but the proportion of nitric acid resulting from the decomposition of animal remains was present in extraordinarily large quantities, as will be seen from the subjoined analysis:—

	Grains in a gallon.
Silica	0·8960
Sulphate of potash	17·0448
Sulphate of soda	9·5152
Chloride of sodium	9·6320
Chloride of calcium	5·9120
Nitrate of lime	40·1200
Nitrate of magnesia	17·0640
	100·1840

From this analysis there appears to be the extraordinary quantity of 38·87 grains of actual anhydrous nitric acid in each gallon of the water!

Another sample of water from a spring at Clapham was analysed by Dr. Noad in 1848. This water likewise acted strongly upon lead. A leaden cistern filled with this water was examined at the same time, and the surface of the water was observed to be coated with a "thick, greasy scum, which proved on examination to consist almost entirely of oxide of lead." No signs of lead could be detected in the clear water beneath the scum. This water had the following composition:—

	Grains in a gallon.
Silica	0·24
Carbonate of lime	15·09
Carbonate of magnesia	13·97
Sulphate of lime	15·32
Sulphate of potash	6·79
Sulphate of soda	10·77
Chloride of sodium	11·46
Organic matter	4·10
	77·74

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* Quart. Journ. Chem. Soc. vol. iv. p. 20.

Here we have an instance of a water containing a large quantity of the so-called *preservative salts* acting strongly upon lead. It may be remarked, that, in making the analysis of this water, the author sought neither for nitric acid, ammonia, nor any other oxidation product of nitrogenous organic matter, although we may assume them to have been undoubtedly present. To what, the author inquires, are we to ascribe this remarkable action on lead? The reply is, "to the large quantity of organic matter." It was noticed that the corrosion of the lead occurred chiefly in the summer months, at a time when the decomposition of organic matter is assumed to be most active. The author explains the absence of lead in the clear water, on the assumption that the carbonic acid arising on the bottom and sides of the cistern from the decomposition of organic matter, combines with the oxide of lead floating on the surface of the water, and forms the carbonate which, unlike the carbonates of lime and magnesia, is almost absolutely insoluble in excess of carbonic acid. Other examples are quoted of water containing large quantities of sulphates and chlorides (which are supposed to have a protecting influence) acting strongly on lead; and in every instance, except in the case of the Highgate water, the water contained very large quantities of organic matter. Finally, the author found the deep well-waters of the London basin to act upon lead; and he attributes, in this case, the solvent action to be due to the *alkalinity* of the waters. Here we find, in the papers quoted, the action of water on lead attributed,—1st, to the presence of nitrates; 2ndly, to organic matter; 3rdly, to the presence of free alkali. Numerous other authors, amongst them Guyton Morveau, Christison, Captain Yorke, Dr. Lambe, &c., might be quoted; but as the investigations of these authors tend to throw but little light on the subject, I shall, before entering into details of my own experiments, allude only to the opinions of the Government reporters, Professors Graham, Hofmann and Miller, who, at the request of Government, paid particular attention to the action of waters upon lead*.

In reply to the question, "Whether any comparative inconvenience would arise from a supply of soft water to the metropolis," the authors remark upon the solvent action of soft waters upon lead as being calculated to excite alarm. It does not appear, however, that the soft waters from the Surrey Hills, which contain, on the average, only 6 grains of solid matter in a gallon and have a hardness of only 2 degrees, have any appreciable action on lead, with the exception of that from the Punch-bowl, of which the power of acting on lead seemed to be very considerable. Even river- and spring-water from the chalk strata, arti-

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* Quart. Journ. Chem. Soc. vol. iv. p. 400.

ficially softened to 3 degrees of hardness, had no solvent effect on lead. Thus, then, the notion that waters containing a small proportion of mineral matter necessarily act upon lead, seems to have no foundation in fact.

The conclusions resulting from many experiments arrived at by these able investigators may be briefly mentioned. They are as follows:—

1st. That certain salts, particularly sulphates, to which a protecting effect is usually ascribed, appear not uniformly to exercise this useful property.

2ndly. That some salts, such as chlorides, and more particularly *nitrates*, may increase the solvent action of water.

3rdly. That of all protecting agents, that of carbonate of lime dissolved by carbonic acid is the most considerable and surest.

The most important observation on this subject is in reference to the extraordinary influence of a small quantity of carbonic acid upon the lead actually in solution. This acid seizes upon the *soluble* oxide and converts it into *carbonate* of lead, of which not more than $\frac{1}{60}$th of a grain is soluble in a gallon of pure water. Even water *per se* free from carbonic acid, and containing as much as six grains of oxide of lead in a gallon, by mere exposure to the air for twenty-four hours was found to deposit all its lead as insoluble carbonate, except $\frac{1}{57}$th of a grain which still remained in solution. From this it will be seen, that although carbonic acid does not prevent certain waters acting upon lead, it nevertheless has the effect of precipitating the lead dissolved by other agencies, and so rendering it comparatively innocuous. The authors conclude their observations on this subject in the following words:—"*The properties of water which enable it to act at times with unusual vigour upon lead are little understood, and seem often to arise from the accidental action of local and very limited causes, such as the presence of decaying leaves and other impurities.*" That the conclusions of these and other investigators are very unsatisfactory cannot be denied; and the question, Why do some waters act on lead and others not? still remains unanswered. That this is a question of paramount importance, and one attended with unusual difficulties, is universally admitted; nevertheless I trust that the experiments detailed in the following pages will throw additional light upon the subject.

In looking at the analyses of waters which are known to act strongly on lead, we invariably find either a large proportion of *organic matter* or of its products of oxidation. To these, as will be presently shown, is entirely due the action of such waters on lead.

The organic matters held in solution by water, and which cannot be separated by filtration, are of two classes: one non-nitrogenous, the other nitrogenous. The former consist of carbon, oxygen and hydrogen, and in the natural progress of decay are resolved into carbonic acid and water; the other class contains in addition, nitrogen, sulphur and phosphorus. These elements unite in progress of time with hydrogen, and form *ammonia, sulphuretted hydrogen* and *phosphuretted hydrogen.* The ammonia, which is always formed in considerable quantity, is ultimately resolved by combination with oxygen into *nitrous* acid and water, according to the following equation:—

$$NH^3+6O=NO^3+3HO.$$

If Thames water, which, as is well known, contains a large quantity of nitrogenous organic matter, be allowed to remain for some time exposed to the air in an open vessel, the nitrogen of the organic matter will unite first with hydrogen to form ammonia; and this base may be readily detected by slightly acidulating the water with hydrochloric acid, evaporating to dryness on a water-bath, and testing the residue with potash. In progress of time, however, a portion of the ammonia is resolved, according to the above equation, into *nitrous acid,* which unites with an equivalent of ammonia, and may be found in the water as *nitrite of ammonia.* If to a portion of Thames water which has been exposed for some time to the atmosphere, a little pure potash is added and the water is then evaporated to dryness, abundance of *nitrous acid* may be found in the residue by first placing it in a flask, adding dilute sulphuric acid, and placing in the neck of the flask a slip of paper moistened with acidulated starch-paste and iodide of potassium. In a few seconds the paper will become intensely *blue.* In warm weather this change takes place in a few days, and may be effected almost instantaneously by application of heat.

When Thames water is distilled, the complicated organic substances contained in it are speedily resolved into more simple compounds, and amongst these ammonia and nitrous acid occupy a prominent position. On testing the first portions of water distilled from a 20-gallon still, it was found to be decidedly *acid* to test-paper. Two gallons of this water were neutralized with pure alcoholic potash, evaporated to dryness, and the residue tested for nitrous acid; the paper became *blue* instantly. To another two gallons of the same distilled water, acid to test-paper, was added a few drops of hydrochloric acid, and the water evaporated to dryness on a water-bath. On testing the residue with pure potash, abundance of *ammonia* was evolved. The experiment was repeated on the first four gallons of water distilled from six separate twenty gallons of Thames water, with the same results; thus proving the existence both of *ammonia* and *nitrous acid* in the water distilled from Thames water.

It has long been observed that certain specimens of distilled water act strongly upon lead, dissolving as much as five or six grains in a gallon; and this circumstance has given rise to the opinion that *soft waters* in general cannot safely be conveyed through leaden pipes, or stored for consumption in leaden cisterns. According, however, to the experiments of Professors Hofmann, Graham and Miller, neither the soft waters of the Surrey Hills, nor spring-water artificially softened, have the slightest action upon lead; although *certain* specimens of soft water, as in the case of the water of the Dee and of the Don, dissolve a considerable quantity of that metal.

Having found in water distilled from Thames water a considerable quantity of nitrous acid, free and in combination with ammonia, I was induced to study more fully the action of this water upon lead. Into a gallon of this distilled water I placed a quantity of perfectly pure and bright sheet-lead, exposing a surface of 560 square inches. In six hours the water became opake, and when shaken up was perfectly milky. The lead was much acted upon, and in twenty-four hours the water was filtered, and the quantity of carbonate of lead left on the filter amounted to 6·4 grains per gallon, $\frac{1}{60}$th of a grain only remaining in solution. Six similar experiments were made with the following results:—

Quantity of water.	Lead exposed for 24 hours.	Carbonate of lead produced.	Lead remaining in solution.
I. 1 gallon.	560 square inches superficial.	6·4 grs.	$\frac{1}{60}$th gr.
II. 1 gallon.	,,	4·7 grs.	$\frac{1}{36}$th gr.
III. 1 gallon.	,,	5·0 grs.	$\frac{1}{38}$th gr.
IV. 1 gallon.	,,	5·4 grs.	$\frac{1}{62}$nd gr.
V. 1 gallon.	,,	5·3 grs.	$\frac{1}{49}$th gr.
VI. 1 gallon.	,,	3·9 grs.	$\frac{1}{30}$th gr.

To determine whether the action of the distilled water on lead is due to the presence of free nitrous acid and nitrite of ammonia, I made the following experiments:—The 20-gallon still was refilled, and to the water was added a few sticks of caustic potash. As soon as the water became warm, there was at the end of the condenser a perceptible odour of *ammonia*, and the first portion of water which distilled over was strongly alkaline, the potash added having retained the nitrous acid and liberated the ammonia. The water as it distilled became less and less alkaline; and after two gallons had passed over, the water ceased to alter the colour of the most delicate red litmus-paper, being perfectly *neutral.* Six experiments with lead, similar to the preceding, were made with this neutral water, and in neither instance was the lead acted upon, nor was a trace of lead to be found in solution.

Having proved, by the preceding experiments, that the solvent action of distilled water upon lead is due entirely to the presence of nitrous acid, either free or combined with ammonia, and that water deprived of this acid has *no* action upon lead, as this appeared to afford a key to the difficult question regarding the solvent action of waters on lead in general, I instituted a number of additional experiments.

Nitrous acid, as is well known, forms four different combinations with oxide of lead:—

Neutral monobasic nitrite . .	PbO, NO^3
Bibasic nitrite	$2PbO, NO^3$
Tribasic nitrite (Bromeis) . .	$3PbO, NO^3$
Quadribasic nitrite	$4PbO, NO^3$.

If, through a solution of either of the basic nitrites of lead, a current of carbonic acid be passed, the basic atoms of oxide are precipitated as carbonate, and neutral nitrite of lead, PbO, NO^3, remains in solution, *e. g.*

$$4PbO, NO^3+3CO^2=3(PbO, CO^2)+PbO, NO^3.$$

If this neutral nitrite of lead be placed in contact with more metallic lead and allowed to remain, the solution acquires in a few minutes an alkaline reaction from the formation of a quadribasic salt and production of ammonia. This remarkable change is expressed in the following equation:—

$$3(PbO, NO^3)+3HO+6Pb=2(4PbO, NO^3)+PbO+NH^3*.$$

The quadribasic nitrite, by exposure to the atmosphere, is decomposed by carbonic acid, the three atoms of basic oxide combine with this acid and are precipitated as carbonate, *neutral nitrite* being left in solution, which again in contact with more lead forms another atom of ammonia, and is converted itself into the quadribasic salt. Thus a very minute quantity of nitrous acid

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* If a solution of neutral nitrite of lead be boiled with metallic lead, a quantity of ammonia is obtained which corresponds with the above equation. The experimental results will be communicated in a subsequent paper.

resulting from the decomposition of nitrogenous organic matter will go on attacking and dissolving lead *ad infinitum*. Each atom of nitrous acid, in acting upon lead, forms an atom of neutral nitrite of lead. Three atoms of this neutral nitrite, according to the above equation, attacks the lead vigorously, and forms the polybasic salt with simultaneous production of *ammonia**. This ammonia is in turn converted by the action of the lead into nitrous acid, which in its turn attacks and dissolves its quota of lead. Thus, although the quantity of nitrogenous organic matter in water may be very small, and the nitrous acid *directly* resulting from the oxidation of ammonia may be in very minute quantity, yet it will be evident from the foregoing remarks that the quantity will rapidly increase from a process of self-manufacture which is continually going on. The nitrous acid, in attacking the lead, produces *ammonia*: this ammonia in turn becomes transformed into nitrous acid by simple oxidation.

When nitrates are present in water, a similar action on lead is effected. The nitrate of lead produced in contact with the metal is speedily transformed into the quadribasic *nitrite* with evolution of binoxide of nitrogen, which, by combining with another atom of oxygen, forms again nitrous acid. The formation of quadribasic nitrite of lead, from the nitrate in contact with metallic lead, is represented by the following equation:—

$$5(PbO, NO^5)+11Pb=4(4PbO, NO^3)+NO^2.$$

The foregoing observations sufficiently explain why leaden pipes and cisterns are so rapidly destroyed by the action of certain waters, while they are not acted upon by others. The chief conclusions arrived at from these investigations are,—

1st. That the action of water upon lead is entirely due to the presence of *nitrous* and *nitric acids*, resulting primarily from the decomposition of organic matters and of ammonia contained in the water.

2ndly. That waters deprived of these acids, and of substances capable of producing them, have no action on lead, and may be conveyed with perfect safety through leaden pipes or stored in leaden cisterns.

3rdly. That carbonic acid exercises a most useful influence in decomposing the basic nitrite of lead formed, and precipitating the basic atoms of oxide as carbonate, leaving only very minute quantities of the neutral nitrite in solution.

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* Schönbein (Phil. Mag. vol. xii. p. 457) has shown that platinum and finely divided copper have the power of converting ammonia instantaneously into nitrous acid. Most other metals, especially those which have a powerful affinity for oxygen, possess the same property.

XXV. *Some Remarks on Heat, and the Constitution of Elastic Fluids.* *By* J. P. Joule, *F.R.S. &c.*[*]

To the Editors of the Philosophical Magazine and Journal.

Oakfield, Moss Side, Manchester,
August 22, 1857.

Gentlemen,

SHOULD you deem it to be of sufficient interest, I shall feel much obliged by your republishing the enclosed paper, referred to by Professor Clausius in his paper "On the Nature of the Motion which we call Heat," inserted in your last Number. I may observe in reference to the note at the foot of p. 109, that the Memoirs of the Literary and Philosophical Society of Manchester are at present regularly forwarded to the principal scientific societies of Europe and America.

I have the honour to remain,
Yours very respectfully,
James P. Joule.

In a paper "On the Heat evolved during the Electrolysis of Water," published in the seventh volume of the Memoirs of this Society, I stated that the magneto-electrical machine enabled us to convert mechanical power into heat; and that I had little doubt that, by interposing an electro-magnetic engine in the circuit of a voltaic battery, a diminution of the quantity of heat evolved, per equivalent of chemical reaction, would be observed, and that this diminution would be proportional to the mechanical power obtained.

The results of experiments in proof of the above proposition were communicated to the British Association for the Advancement of Science, in 1843[†]. They showed that whenever a current of electricity was generated by a magneto-electrical machine, the quantity of heat evolved by that current had a constant relation to the power required to turn the machine; and, on the other hand, that whenever an engine was worked by a voltaic battery, the power developed was at the expense of the calorific power of the battery for a given consumption of zinc, the mechanical effect produced having a fixed relation to the heat lost in the voltaic circuit.

The obvious conclusion from these experiments was, that heat and mechanical power were convertible into one another; and it became therefore evident that heat is either the *vis viva* of pon-

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[*] Read at a Meeting of the Manchester Literary and Philosophical Society, October 3, 1848, and published in the Society's Memoirs, November 1851.

[†] Phil. Mag. vol. xxiii. pp. 263, 347, 435.

derable particles, or a state of attraction or repulsion capable of generating *vis viva.*

It now became important to ascertain the mechanical equivalent of heat with as much accuracy as its importance to physical science demanded. For this purpose the magnetic apparatus was not very well adapted, and therefore I sought in the heat generated by the friction of fluids for the means of obtaining exact results. I found, first, that the expenditure of a certain amount of mechanical power in the agitation of a given fluid uniformly produced a certain fixed quantity of heat; and, secondly, that the quantity of heat evolved in the friction of fluids was entirely uninfluenced by the nature of the liquid employed; for water, oil and mercury, fluids as diverse from one another as could have been well selected, gave sensibly the same result, viz. that the quantity of heat capable of raising the temperature of a pound of water 1°, is equal to the mechanical power developed by a weight of 770 lbs. in falling through one perpendicular foot*.

Believing that the discovery of the equivalent of heat furnished the means of solving several interesting phænomena, I commenced, in the spring of 1844, some experiments on the changes of temperature occasioned by the rarefaction and compression of atmospheric air†. It had long been known that air, when forcibly compressed, evolves heat; and that, on the contrary, when air is dilated, heat is absorbed. In order to account for these facts, it was assumed that a given weight of air has a smaller capacity for heat when compressed into a small compass than when occupying a larger space. A few experiments served to show the incorrectness of this hypothesis: thus, I found that by forcing 2956 cubic inches of air, at the ordinary atmospheric pressure, into the space of 136½ cubic inches, 13°·63 of heat per pound of water were produced; whereas by the reverse process, of allowing the compressed air to expand from a stopcock into the atmosphere, only 4°·09 were absorbed instead of 13°·63, which is the quantity of heat which ought to have been absorbed, according to the generally received hypothesis. I found, also, that when strongly compressed air was allowed to escape into a vacuum, no cooling effect took place on the whole, a fact likewise at variance with the received hypothesis. On the contrary, the theory I ventured to advocate‡ was in perfect agreement with the phænomena; for the heat evolved by compressing the

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* The equivalent I have since arrived at is 772 foot-pounds. See Phil. Trans. 1850, part 1.—May 1851, J. P. J.

† Phil. Mag. vol. xxvi.

‡ I subsequently found that M. Mayer had previously advocated a similar hypothesis, without, however, attempting an experimental demonstration of its accuracy.—*Annalen* of Wöhler and Liebig for 1842.—May 1851, J. P. J.

air was found to be the equivalent of the mechanical power employed, and, *vice versâ*, the heat absorbed in rarefaction was found to be the equivalent of the mechanical power developed, estimated by the weight of the column of atmospheric air displaced. In the case of compressed air expanding into a vacuum, since no mechanical power was produced, no absorption of heat was expected or found. M. Seguin has confirmed the above results in the case of steam.

The above principles lead, indeed, to a more intimate acquaintance with the true theory of the steam-engine; for they have enabled us to estimate the calorific effect of the friction of the steam in passing through the various valves and pipes, as well as that of the piston in rubbing against the sides of the cylinder; and they have also informed us that the steam, while expanding in the cylinder, loses heat in quantity exactly proportional to the mechanical force developed*.

The experiments on the changes of temperature produced by the rarefaction and condensation of air give likewise an insight into the constitution of elastic fluids, for they show that the heat of elastic fluids is the mechanical force possessed by them; and since it is known that the temperature of a gas determines its elastic force, it follows that the elastic force, or pressure, must be the effect of the motion of the constituent particles in any gas. This motion may exist in several ways, and still account for the phænomena presented by elastic fluids. Davy, to whom belongs the signal merit of having made the first experiment absolutely demonstrative of the immateriality of heat, enunciated the beautiful hypothesis of a rotatory motion. He says, "It seems possible to account for all the phænomena of heat, if it be supposed that in solids the particles are in a constant state of vibratory motion, the particles of the hottest bodies moving with the greatest velocity and through the greatest space: that in fluids and elastic fluids, besides the vibratory motion, which must be considered greatest in the last, the particles have a motion round their own axes with different velocities, the particles of elastic fluids moving with the greatest quickness; and that in ætherial substances the particles move round their own axes, and separate from each other, penetrating in right lines through space. Temperature may be conceived to depend upon the velocity of the vibrations, increase of capacity on the motion being performed

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* A complete theory of the motive power of heat has been recently communicated by Professor Thomson to the Royal Society of Edinburgh. In this paper the very important law is established, that the fraction of heat converted into power in any perfect engine, is equal to the range of temperature divided by the highest temperature above absolute zero.—May 1851, J. P. J.

in greater space; and the diminution of temperature during the conversion of solids into fluids or gases may be explained on the idea of the loss of vibratory motion, in consequence of the revolution of particles round their axes at the moment when the body becomes fluid or aëriform, or from the loss of rapidity of vibration in consequence of the motion of the particles through greater space*." I have myself endeavoured to prove that a rotary motion, such as that described by Sir H. Davy, will account for the law of Boyle and Mariotte, and other phænomena presented by elastic fluids†; nevertheless, since the hypothesis of Herapath, in which it is assumed that the particles of a gas are constantly flying about in every direction with great velocity, the pressure of the gas being owing to the impact of the particles against any surface presented to them, is somewhat simpler, I shall employ it in the following remarks on the constitution of elastic fluids; premising, however, that the hypothesis of a rotatory motion accords equally well with the phænomena.

Let us suppose an envelope of the size and shape of a cubic foot to be filled with hydrogen gas, which, at 60° temperature and 30 inches barometrical pressure, will weigh 36·937 grs. Further, let us suppose the above quantity to be divided into three equal and indefinitely small elastic particles, each weighing 12·309 grs.; and further, that each of these particles vibrates between opposite sides of the cube, and maintains a uniform velocity except at the instant of impact; it is required to find the velocity at which each particle must move so as to produce the atmospherical pressure of 14,831,712 grs. on each of the square sides of the cube. In the first place, it is known that if a body moving with the velocity of $32\frac{1}{6}$ feet per second be opposed, during one second, by a pressure equal to its weight, its motion will be stopped, and that if the pressure be continued one second longer, the particle will acquire the velocity of $32\frac{1}{6}$ feet per second in the contrary direction. At this velocity there will be $32\frac{1}{6}$ collisions of a particle of 12·309 grs. against each side of the cubical vessel in every two seconds of time; and the pressure occasioned thereby will be $12{\cdot}309 \times 32\frac{1}{6} = 395{\cdot}938$ grs. Therefore, since it is manifest that the pressure will be proportional to the square of the velocity of the particles, we shall have for the velocity of the particles requisite to produce the pressure of 14,831,712 grs. on each side of the cubical vessel,

$$v = \sqrt{\left(\frac{14{,}831{,}712}{395{\cdot}938}\right)}\, 32\tfrac{1}{6} = 6225 \text{ feet per second.}$$

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* Elements of Chemical Philosophy, p. 95.

† Mr. Rankine has given a complete mathematical investigation of the action of vortices, in his paper on the Mechanical Action of Gases and Vapours.—Trans. Roy. Soc. Edinb. vol. xx. part 1.—May 1851. J. P. J.

The above velocity will be found equal to produce the atmospheric pressure, whether the particles strike each other before they arrive at the sides of the cubical vessel, whether they strike the sides obliquely, and thirdly, into whatever number of particles the 36·927 grs. of hydrogen are divided.

If only one-half the weight of hydrogen, or 18·4635 grs., be enclosed in the cubical vessel, and the velocity of the particles be as before, 6225 feet per second, the pressure will manifestly be only one-half of what it was previously, which shows that the law of Boyle and Mariotte flows naturally from the hypothesis.

The velocity above named is that of hydrogen at the temperature of 60°; but we know that the pressure of an elastic fluid at 60° is to that at 32° as 519 is to 491. Therefore the velocity of the particles at 60° will be to that at 32° as $\sqrt{519} : \sqrt{491}$, which shows that the velocity at the freezing temperature of water is 6055 feet per second.

In the above calculations it is supposed that the particles of hydrogen have no sensible magnitude, otherwise the velocity corresponding to the same pressure would be lessened.

Since the pressure of a gas increases with its temperature in arithmetical progression, and since the pressure is proportional to the square of the velocity of the particles, in other words, to their *vis viva*, it follows that the absolute temperature, pressure, and *vis viva* are proportional to one another, and that the zero of temperature is 491° below the freezing-point of water. Further, the absolute heat of the gas, or, in other words, its capacity, will be represented by the whole amount of *vis viva* at a given temperature. The specific heat may therefore be determined in the following simple manner:—

The velocity of the particles of hydrogen, at the temperature of 60°, has been stated to be 6225 feet per second, a velocity equivalent to a fall from the perpendicular height of 602,342 feet. The velocity at 61° will be $6225\sqrt{\frac{520}{519}} = 6230{\cdot}93$ feet per second, which is equivalent to a fall of 603,502 feet. The difference between the above falls is 1160 feet, which is therefore the space through which 1 lb. of pressure must operate upon each pound of hydrogen, in order to elevate its temperature one degree. But our mechanical equivalent of heat shows that 770 feet is the altitude representing the force required to raise the temperature of water one degree; consequently the specific heat of hydrogen will be $\frac{1160}{770} = 1{\cdot}506$, calling that of water unity.

The specific heats of the gases will be easily deduced from that of hydrogen; for the whole *vis viva* and capacity of equal bulks of the various gases will be equal to one another; and the velocity of the particles will be inversely as the square root of the specific gravity. Hence the specific heat will be inversely proportional to the specific gravity, a law which has been arrived at experimentally by De la Rive and Marcet.

In the following Table I have placed the specific heats of various gases determined in the above manner, in juxtaposition with the experimental results of Delaroche and Berard reduced to constant volume:—

	Experimental specific heat.	Theoretical specific heat.
Hydrogen	2·352	1·506
Oxygen	0·168	0·094
Nitrogen	0·195	0·107
Carbonic oxide . .	0·158	0·068

The experimental results of Delaroche and Berard are invariably higher than those demanded by the hypothesis. But it must be observed, that the experiments of Delaroche and Berard, though considered the best that have hitherto been made, differ considerably from those of other philosophers. I believe, however, that the investigation undertaken by M. V. Regnault, for the French Government, will embrace the important subject of the capacity of bodies for heat, and that we may shortly expect a new series of determinations of the specific heat of gases, characterized by all the accuracy for which that distinguished philosopher is so justly famous. Till then, perhaps, it will be better to delay any further modifications of the dynamical theory, by which its deductions may be made to correspond more closely with the results of experiment*.

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* If we assume that the particles of a gas are resisted uniformly until their motion is stopped, and that then their motion is renewed in the opposite direction, by the continued operation of the same cause, as in the projection upwards and subsequent fall of a heavy body; the maximum velocity of the particles will be to the uniform velocity required by the theory assumed in the text, as the square root of two is to one, and the comparison of the theoretical with the experimental specific heat will be as follows:—

	Experimental specific heat.	Theoretical specific heat.
Hydrogen	2·352	3·012
Oxygen	0·168	0·188
Nitrogen	0·195	0·214
Carbonic oxide . . .	0·158	0·136

I have just learned that the experiments of Regnault on the specific heat of elastic fluids are on the eve of publication, and doubt not that their accuracy will enable us to arrive at a decisive conclusion as to the correctness of the above hypothesis.—June 1851, J. P. J.

XXVI. *Note on the Action of Bromine on Acetic Acid.*

By W. H. Perkin, *F.C.S., and* B. F. Duppa, *Esq.*[*]

In a paper upon monochloracetic acid that appeared in your Magazine last July, Dr. R. Hoffmann stated that bromine does not act upon acetic acid even in the sunlight, which statement we are happy to confirm; but having by another mode of proceeding induced these substances to act on each other, we thought it advisable to send in a short account of our results.

When a mixture of 4 vols. of glacial acetic acid and 1 vol. of bromine is heated in a sealed tube to 120° or 130° C., the mixture becomes colourless, and on opening the tube torrents of hydrobromic acid are evolved. The liquid product, when submitted to distillation, sometimes commences to boil as low as 90° C., when large quantities of hydrobromic acid are present, and then gradually rises to 208° C., when the distillate commences to solidify in the condenser: this substance is bromoacetic acid, $C^4 (H^3 Br) O^4$. It crystallizes in rhombohedrons, is very deliquescent, and decomposes partially every time it is distilled, evolving hydrobromic acid.

The silver salt was prepared by adding a little carbonate of silver to a solution of the new acid to separate any free hydrobromic acid, filtered, and then nearly saturated with carbonate of silver, which produced a dense crystalline precipitate; the liquid was then filtered off, the crystalline mass washed with cold water and dried *in vacuo*. An aqueous solution of this salt decomposes slowly even at the ordinary temperature, bromide of silver being precipitated; by boiling it is rapidly decomposed. The dry salt, on being heated to about 90° C., suddenly decomposes, leaving bromide of silver, together with a substance that carbonizes at a higher temperature: it seems to be very hygrometric. A determination of the silver gave 43·61 per cent., theory requiring 43·9.

The lead salt crystallizes in small, brilliant needles, taking a slight pink hue if exposed to light; it is very insoluble in water, and is decomposed by long boiling into a viscid, uncrystallizable substance. An analysis of this salt gave—

42·774 per cent. of lead, and 33·1 of bromine; the formula $C^4 (H^2 Br Pb) O^4$ requires 42·878 of lead, and 31·117 of bromine.

When a mixture of 1 vol. of phenic acid and 2 vols. of bromoacetic acid is heated in a sealed tube to 130° C. for five hours, it becomes brown. This product, when neutralized with potash, produces a carmine-coloured liquid, which is the potassium salts of two acids; however, we refrain from giving an account of

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[*] Communicated by the Authors.

these until we know more about them; but it may be as well to state, that if a mixture of phenic acid, acetic acid, and iodine be heated in a sealed tube, there is a product obtained producing salts of a similar colour to those obtained with bromoacetic acid; and also if acetic acid be replaced by any of its homologues, similar acids are obtained, evidently showing the existence of a whole series of these acids. However, we expect soon to be able to give a full account of bromoacetic acid, and hope to obtain the bi- and tri-bromoacids, and also to give a complete account of the acids procured by treating phenic acid with bromoacetic acid.

August 1857.

XXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 153.]

January 8, 1857.—William Robert Grove, Esq., V.P., in the Chair.

THE following communications were read:—

"Memoir on the Symmetric Functions of the Roots of an Equation." By Arthur Cayley, Esq., F.R.S.

There are contained in a work, which is not, I think, so generally known as it deserves to be, the 'Algebra' of Meyer Hirsch, some very useful tables of the symmetric functions up to the tenth degree of the roots of an equation of any order. It seems desirable to join to these a set of tables, giving reciprocally the expressions of the powers and products of the coefficients in terms of the symmetric functions of the roots. The present memoir contains the two sets of tables, viz. the new tables distinguished by the letter (*a*), and the tables of Meyer Hirsch distinguished by the letter (*b*); the memoir contains also some remarks as to the mode of calculation of the new tables, and also as to a peculiar symmetry of the numbers in the tables of each set, a symmetry which, so far as I am aware, has not hitherto been observed, and the existence of which appears to constitute an important theorem in the subject. The theorem in question might, I think, be deduced from a very elegant formula of M. Borchardt (referred to in the sequel), which gives the generating function of any symmetric function of the roots, and contains potentially a method for the calculation of the tables (*b*), but which, from the example I have given, would not appear to be a very convenient one for actual calculation.

"Memoir on the Conditions for the Existence of given Systems of Equalities among the Roots of an Equation." By Arthur Cayley, Esq., F.R.S.

It is well known that there is a symmetric function of the roots of an equation, viz. the product of the squares of the differences of the roots, which vanishes when any two roots are put equal to each other, and that consequently such function expressed in terms of the coefficients and equated to zero, gives the condition for the existence of a pair of equal roots. And it was remarked long ago by Professor Sylvester, in some of his earlier papers in the 'Philosophical Magazine,' that the like method could be applied to finding the conditions for the existence of other systems of equalities among the roots, viz. that it was possible to form symmetric functions, each of them a sum of terms containing the product of a certain number of the differences of the roots, and such that the entire function might vanish for the particular system of equalities in question; and that such functions expressed in terms of the coefficients and equated to zero would give the required conditions. The object of the present memoir is to extend this theory, and render it exhaustive by showing how to form a series of types of all the different functions which vanish for one or more systems of equalities among the roots; and in particular to obtain by the method distinctive conditions for all the different systems of equalities between the roots of a quartic or a quintic equation, viz. for each system conditions which are satisfied for the particular system, and are not satisfied for any other systems, except, of course, the more special systems included in the particular system. The question of finding the conditions for any particular system of equalities is essentially an indeterminate one, for given any set of functions which vanish, a function syzygetically connected with these will also vanish; the discussion of the nature of the syzygetic relations between the different functions which vanish for any particular system of equalities, and of the order of the system composed of the several conditions for the particular system of equalities, does not enter into the plan of the present memoir. I have referred here to the indeterminateness of the question for the sake of the remark that I have availed myself thereof, to express by means of invariants or covariants the different systems of conditions obtained in the sequel of the memoir; the expressions of the different invariants and covariants referred to are given in my "Second Memoir upon Quantics," Phil. Trans. vol. cxlvi. (1856).

"Tables of the Sturmian Functions for Equations of the Second, Third, Fourth and Fifth Degrees." By Arthur Cayley, Esq., F.R.S.

The general expressions for the Sturmian functions in the form of determinants, are at once deducible from the researches of Professor Sylvester in his early papers on the subject in the 'Philosophical Magazine,' and in giving these expressions in the memoir "Nouvelles Recherches sur les Fonctions de M. Sturm," Liouville, t. xiii. p. 269 (1848), I was wrong in claiming for them any novelty. The expressions in the last-mentioned memoir admit of a modification by which their form is rendered somewhat more elegant; I propose, on the present occasion, merely to give this modified form of the general expression, and to give the developed expressions of the functions in question for equations of the degrees, two, three, four and five.

January 15.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"Photo-chemical Researches.—Part II. Phenomena of Photo-chemical Induction." By Prof. Bunsen of Heidelberg, and Henry Enfield Roscoe, B.A., Ph.D.

Chemical affinity, or the force which regulates the chemical combination of two bodies, is like all other forces, a certain definite quantity. Hence it is erroneous to say, that under different circumstances the same body can possess different affinities; more correctly we should say, that in the one case the bodies are able to follow the chemical attraction of their molecules, whilst in another case opposing forces render this combination impossible. These opposing forces may be considered as resistances similar to those exerted in the passage of electricity through conductors, in the distribution of magnetism in steel, and in the conduction of heat. We overcome these resistances when by agitation we increase the formation of a precipitate, or by insolation effect a decomposition.

We call the act by which these resistances to combination are lessened, and the formation of a chemical compound promoted, "chemical induction;" and we specify this as photo-chemical, thermo-chemical, electro-chemical, or idio-chemical, according as light, heat, electricity, or pure chemical action is the force which promotes the combination.

The phenomena of photo-chemical induction are particularly interesting, as affording starting-points from which we may gain a knowledge of this mode of action of affinity.

That on exposing a mixture of chlorine and hydrogen to the light the action does not commence to its full extent at once, was observed by Draper in 1843. An explanation of this fact was given by the supposition that the chlorine underwent on insolation a permanent allotropic modification, in which state it possessed more than usually active properties. This explanation is, as we shall show, erroneous, and the whole phenomenon is caused by the peculiar action to which we have given the name of photo-chemical induction. When the standard mixture of chlorine and hydrogen is exposed to a constant source of light, no action is at first perceptible; after a short time, however, the action becomes visible, and gradually increases until a constant maximum is reached. Experiments made with different amounts of light from different luminous sources, showed that the times which elapsed from the beginning of the exposure until the maximum was obtained, varied very much, according to circumstances. In one case the maximum action was reached in fifteen minutes, in others after an exposure of three and four minutes. The first action was in one case visible after six minutes' insolation, in others after one minute, whilst in some experiments a considerable action was observed in the first minute.

The condition modifying the action of the induction which we first examined, was the action of the mass of the insolated gas. From various experiments, it was found that the duration of the induction increased with the volume of exposed gas (by constant amounts of light), and curves have been drawn, representing the increase of the induction for the various volumes of gas employed.

We next examined the dependence of the duration of induction upon the amount of light to which a constant volume of gas was exposed, and experiment showed—

1. That the time necessary to effect the first action of the photo-chemical induction decreases with increase of light, and in a greater ratio than the increase of light.

2. That the time which elapses until the maximum is attained also decreases with increase of light, but in a much less ratio.

3. That the increase of the induction proceeds at first in an expanding series, attains a maximum, and then converges when the true maximum action is attained. The law regulating the increase of the induction by increase of light, we have rendered visible by curves.

The results of these experiments suggested the question, Is this condition of increased combining power, into which the mixture of chlorine and hydrogen passes by insolation, permanent, or is it confined to the time during which the gas is exposed to the light? In order to determine this question, the sensitive gas, which had stood for some time in the dark, was exposed to a constant source of light, and the time noted which elapsed before the maximum action was reached; the apparatus was then darkened for one minute, and then again insolated, and the time watched until the maximum action was again observed. These observations were repeated several times, each period of darkening being longer than the preceding. Thus conducted, the experiment led to the important conclusion, that the resistance to combination overcome by the influence of the light is soon restored when the gas is allowed to stand in the dark. Curves expressing the effect produced on induction by darkening, and by exposure to light, have been drawn.

We have explained the fact, that the mixture of chlorine and hydrogen does not combine in the dark, by the supposition of the existence of a resistance to combination which is overcome when the gaseous mixture is exposed to light. This resistance to combination can be increased by various circumstances. The presence of a very small quantity of foreign gas in the standard mixture of chlorine and hydrogen is sufficient to cause the resistance to be increased to a very great extent. An excess of $\frac{3}{1000}$ of hydrogen over that contained in the normal gas, reduced the action from 100 to 38.

In these experiments we have to do with the purest form of the so-called catalytic actions, to which the photo-chemical phenomena are closely related. The quantitative estimation of the relations which exist in the phenomena of contact, between the mass of the substance the time and other modifying conditions, has not as yet been possible, owing to the absence of any case in which these relations are exhibited in their simplest form. Our method of photo-chemical measurement points out a direction which promises to afford interesting results concerning these quantitative relations;

but in this communication we restrict ourselves to the consideration of these phenomena in so far as they influence the action of photo-chemical induction, intending on a future occasion to enter more fully into the new field of research indicated.

The contact action of foreign gases is still more strongly seen in the case of small quantities of oxygen. This gas, when present in quantities amounting only to $\frac{5}{1000}$ of the total volume of gas, diminishes the action from 100 to 4·7, whilst $\frac{13}{1000}$ reduced the action from 100 to 1·3. Excess of chlorine acted in a similar manner, though not to so great an extent, $\frac{10}{1000}$ of this gas reducing the action from 100 to 60·2, and $\frac{180}{1000}$ from 100 to 41·3. On examining the effect of small quantities of hydrochloric acid gas upon the induction maximum, we found, fortunately for the accuracy of the indications of our instrument, that an amount of $\frac{1·3}{1000}$ of this gas does not produce any appreciable effect on the action of the induction. Uninsolated gas was found to act similarly on the normal mixture, the admission of $\frac{6}{1000}$ of non-insolated gas reducing the action from 100 to 55. Curves have been drawn, representing the relation between the action and the amount of foreign impurity introduced. Several series of experiments also showed that a mixture of chlorine and hydrogen, which was so nearly pure that no alteration of the maximum action was observable, was longer in attaining the maximum than the perfectly pure gas; hence the duration of the induction serves as an exact measure of the absence of all foreign gases in the standard mixture.

An explanation of the laws of photo-chemical induction derived from the above-mentioned experiments, might easily be found in the assumption that the chlorine or the hydrogen, or both gases, undergo upon exposure to light a change similar to that between common and ozonized oxygen, or that these two gases can, under certain circumstances, be invested with active, and, under other circumstances, with passive properties. If this hypothesis be true, each gas must undergo this peculiar modification when separately exposed to the action of the light. That this is not the case was shown by the following experiment:—The two gases were separately evolved, and each led through a long glass tube, in which they could be separately exposed to the action of diffuse and direct sunlight. After this exposure, the gases passed through a connecting tube into the apparatus, in which a constant source of light gave the duration of the induction. Thus alternately insolating and darkening the separated gases, we observed the effect on the gases subsequently mixed and exposed to lamplight. No difference was perceptible in the duration of the induction between the gases previously insolated and those evolved in the dark. Hence we may conclude, that the light does not effect a permanent modification, either of the chlorine or hydrogen, but that the combination produced by the light must depend on photo-chemical action affecting only the increasing attractions of the chemically active molecules.

All the curves representing the increase of the induction under various conditions have a common form, and a point of flexure at which the maximum increase occurs. In order to determine whether this common property of the curves arises from the general mode of action of affinity, or whether the light plays an essential part, we have made experiments upon idio-chemical induction, *i. e.* action in which pure chemical attractions alone effect the alteration. For this purpose we employed a dilute aqueous solution of bromine with tartaric acid, which mixture, when left to itself in the dark, undergoes decomposition, hydrobromic acid being formed. By determining the amount of free bromine contained in the liquid at different times, we became acquainted with the rate at which the decomposition occurred. Analysis showed that the amount of hydrobromic acid formed was not the same in equal spaces of time; and curves representing this increase were found to have the form obtained for the photo-chemical induction. Hence the cause of this maximum increase appears not to lie in any peculiar property of light, but rather in the mode of action of affinity itself.

One of the many interesting applications of the laws of photo-chemical induction relates to the phenomena of photography. As an instance of this application we cite the remarkable observations of Becquerel, which induced him to assume the existence of certain rays which can continue, but not commence, chemical action. In order to explain the phenomenon observed by the French physicist, we do not need to suppose the existence of a new property of light, as the facts are easily explained by the laws of photo-chemical induction; and we are satisfied that these relations, which we have examined only in the case of chlorine and hydrogen, occur in a slightly modified form in other photo-chemical processes.

Having determined in this part of our investigation the most important phenomena of photo-chemical induction, we shall in the next section consider the laws which regulate the chemical action of light after the induction is completed.

January 22.—Dr. W. A. Miller, V.P., in the Chair.

The following communications were read:—

"On some of the Products of the Destructive Distillation of Boghead Coal."—Part I. By C. Greville Williams, Esq.

The paper, of which the following is a brief abstract, constitutes the first part of the author's examination of the hydrocarbons contained in boghead naphtha. In it he gives the results of his experiments on that portion of the fluid which resists the action of monohydrated nitric and sulphuric acids. He had previously stated the fact of his having obtained a substance possessing the composition and vapour-density of butyle*, and had expressed a belief that he should succeed in isolating not only that radical, but also propyle, amyle, and caproyle. The composition of the radicals varies so little, that to determine the boiling-points it was necessary to take the density of the vapour of all those fractions which distilled anywhere near their known boiling-points; and in each case he regarded that

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* Chem. Gaz., vol. xiv. p. 19.

fraction which gave the nearest result as representing the boiling-point of the radical as obtained from the source mentioned.

Propyle presented itself under the form of a colourless, very mobile fluid, having a pleasant odour, and boiling at 68° C. At 18° it had the very low density of 0·6745. Combustion gave results agreeing closely with the formula

$$C^{12} H^{14} = 4 \text{ vols.},$$

confirmed by a determination of the density of its vapour by Gay-Lussac's method, which gave 2·96, theory requiring 2·97. Propyle had not been previously obtained.

Butyle from the Torbane-hill mineral distils at 119°, and has a density of 0·6945 at 18°; its analysis coincided with the formula

$$C^{16} H^{18}.$$

The vapour-density was found to be 3·88, theory requiring 3·94.

Amyle distilled at 159°, and had a density of 0·7365 at 18°. On analysis, numbers were obtained agreeing perfectly with the formula

$$C^{20} H^{22}.$$

The vapour-density was found to be 4·93; theory requires 4·91.

Caproyle boiled at 202°; its density at 18° was 0·7568. The results of analysis accorded with the expression

$$C^{24} H^{26},$$

which indicates a vapour-density of 5·87, while experiment yielded 5·83.

The experiments detailed in the paper appear to demonstrate the radical nature of the hydrocarbons, and to negative the assumption of their being homologues of marsh-gas.

The paper concludes with a description of a method, by which, where numerous vapour-density determinations are to be made, the necessity is avoided of refilling the balloon with water or mercury in order to determine the residual air.

"On the Optical Characters of certain Alkaloids associated with Quinine, and of the Sulphates of their Iodo-compounds." By William Bird Herapath, M.D., in a Letter to Professor Stokes, Sec. R.S.

You will probably recollect that I sent you some time since a small portion of an alkaloid, which at that time was called quinidine in Germany, but it has since been distinguished from it and named cinchonidine. You then examined it for epipolism or fluorescence, and you pronounced the opinion that it possessed this property only in a minor degree, and you imagined that this arose from the presence of a small per-centage of α-quinine.

I have since obtained, through the kindness of Mr. J. E. Howard, specimens of the perfectly pure alkaloids quinidine and cinchonidine, and find that quinidine, which I can now identify as the β-quinine of Von Heijningen, possesses the phenomenon of fluorescence or epipolic dispersion as powerfully as α-quinine; whilst cinchonidine, if perfectly pure, is devoid of it altogether; and recent experiments have shown me that a small per-centage of quinidine was the cause of the epipolic dispersion found by you in the specimen of cinchonidine sent by me.

It may be as well to state that the cinchonidine tested by water of chlorine and ammonia gave no evidence of green tint, which it would have done if only $\frac{1}{5000}$dth part of either α-quinine or quinidine had been present, according to some recent experiments of my own.

I have also found that 1 gr. of pure quinine or quinidine in 35,000 of water will give an evident "*epipolic*" appearance; whilst when diluted with 70,000 grs. of water we have still very evident appearances of "fluorescence" upon the perpendicular wall of the glass vessel exposed to the incident light; whilst a bluish milkiness of "internal dispersion" may be seen when 1 gr. of either alkaloid is diluted with 700,000 grs. or 10 gallons of distilled water, well acidulated in all these cases with sulphuric acid.

Some other interesting results have followed from these investigations. When quinidine is dissolved in an excess of diluted sulphuric acid, and the solution mixed with about twice its bulk of spirit, and warmed to 130° F., and tincture of iodine then added in sufficient quantity, and subsequently set in repose, beautiful red acicular crystals are deposited; these, upon recrystallization from rectified spirit, acquire an increased size, become beautiful quadrilateral prisms, exhibiting a deep garnet-red by transmitted light, and possessing a clear bluish-purple reflected tint; they are optically doubly absorbent in a slight degree, and transmit a brownish-orange body-colour when polarized perpendicular to axis. The primary form appears to be a rhombic prism; and as far as my present analyses go, the compound seems to possess the following centesimal composition:—

Iodine	39·665
Sulphuric acid	6·273
Carbon	32·890
Hydrogen	3·960
Nitrogen	4·400
Oxygen	5·040
Water	8·504
	100·712

The excess arises from the hydrogen twice calculated in water of crystallization.

These numbers agree very closely with the formula

$$(C^{35} H^{19} N^{2} O^{4} + I^{2}) SO^{3} HO + 5HO,$$

and prove it to be the sulphate of iodo-quinidine, very analogous to the iodo-quinine compound, yet differing materially in its optical properties.

There is another alkaloid frequently associated with quinidine, which also crystallizes from spirit in the prismatic form like quinidine and cinchonidine, but is another example of epipolism or fluorescence. Its iodo-sulphate is deep olive-green in its reflected tint, orange-yellow by transmitted light, and possessing in an eminent degree optically doubly absorbent powers, thin laminæ being quite black, but still thinner ones give a bistre-brown "body-colour" when polarized perpendicular to the axis.

This alkaloid was also furnished me by the kindness of Mr. Howard, but has not yet been sufficiently purified, or in the quantity necessary to give certain results.

It possesses one very peculiar property. When dissolved in chloroform and evaporated spontaneously on glass, the gummy and uncrystalline residue, mounted in Canada balsam, at once shows a deep blue-green, epipolic, or fluorescent appearance.

Pure cinchonidine does not possess epipolic dispersion and does not become green by chlorine-water and ammonia; and when it is dissolved in acetic acid or chloroform and the fluid is exposed on glass plates to spontaneous evaporation, beautiful crystals in circular spots or drusæ develope themselves, which under polarized light exhibit black crosses and white or coloured sectors.

These appearances are not exhibited by pure quinine or true quinidine (β-quinine), both of which give a gummy, uncrystalline, and perfectly transparent residue.

Pure cinchonidine, thus optically and chemically distinguishable from either quinine or quinidine, is still further remarkable for producing with sulphuric acid and iodine an optical doubly absorbent compound of intense power, even equal to the sulphate of iodo-quinine compound; these crystals are very similar in form to my artificial tourmalines, and have long been mistaken by me for them; even at present I can only distinguish them by the tints in reflected light and the complementary body-colour, viz. whilst sulphate of iodo-quinine gives a cantharidine-green reflected tint, and a pink, ruby-red, reddish-brown or black body-colour when polarized perpendicular to the axis, according to the thickness of the plate examined, the sulphate of iodo-cinchonidine is golden-green by reflected light, and gives a sky or indigo-blue or black "body-colour" when polarized perpendicular to the axis. I have not yet made sufficient chemical analyses of this substance to enable me to decide on its formula, but I have obtained 39·307 per cent. iodine and 8·864 per cent. sulphuric acid, which sufficiently indicate a chemical difference in constitution from the sulphate of iodo-quinine, which, it may be remembered, contains 32·609 per cent. iodine and 10·61 per cent. sulphuric acid.

I hope soon to present these results in more detail when sufficient leisure is afforded me for the purpose.—W. B. H.

January 29.—Major-General Sabine, Treas. and V.P., in the Chair.

The following communications were read:—

"On the Nervous System of *Lumbricus terrestris*." By J. Lockhart Clarke, Esq., F.R.S.

"An Account of the two Methods of Reproduction in *Daphnia*, and of the Structure of the 'Ephippium.'" By John Lubbock, Esq., F.G.S.

"On the Thermo-electricity of Ferruginous Metals, and on the Thermal Effects of stretching Solid Bodies." By J. P. Joule, F.R.S.

The experiments on the above subjects were made with a thermo-multiplier placed in the vacuum of an air-pump. Its sensibility was such that with the junction antimony and bismuth, a thermometric effect not greater than $\frac{1}{8000}$ of a degree Centigrade could be estimated. In determining the thermo-electric position of the metals, it was necessary to increase the resistance of the instrument a hundred-fold, by placing in the circuit a coil of fine wire. In thermo-electric arrangement *steel* was found to be nearer copper than iron was. By hardening, steel was raised almost to the place of copper. *Cast iron* was found to surpass copper; so that the junction cast iron and copper is reverse to that of wrought iron and copper, and the arrangement cast iron and wrought iron is much more powerful than copper and wrought iron. A new test of the quality and purity of ferruginous metals is thus indicated, which will probably be found of value to the arts.

The experiments on the stretching of solids showed, in the case of the metals, a decrease of temperature when the stretching weight was applied, and a heating effect when the weight was removed. An iron wire $\frac{1}{4}$ of an inch in diameter was cooled $\frac{1}{8}$ of a degree Centigrade when stretched by a weight of 775 lbs. Similar results were obtained with cast iron, hard steel, copper, and lead. The thermal effects were in all these cases found to be almost identical with those deduced from Professor Thomson's theoretical investigation, the particular formula applicable to the case in question being $H=\frac{t}{J}\times Pe$, where H is the heat absorbed in a wire one foot long, t the absolute temperature, J the mechanical equivalent of the thermal unit, P the weight applied, and e the coefficient of expansion per 1°. With gutta-percha also a cooling effect on extension was observed; but a reverse action was discovered in the case of vulcanized india-rubber, which became *heated* when the weight is laid on, and *cooled* when the weight was removed. On learning this curious result, Professor Thomson, who had already intimated the probability of a reverse action being observed under certain circumstances with india-rubber, suggested to the author experiments to ascertain whether vulcanized india-rubber stretched by a weight is shortened by increase of temperature. Accordingly, on trial, it was found that this material, when stretched by a weight capable of doubling its length, has that length diminished by one-tenth when its temperature is raised 50° Centigrade. This shortening effect was found to increase rapidly with the stretching weight employed; and, exactly according with the heating effects observed with different stretching weights, entirely to confirm the theory of Professor Thomson.

February 12.—W. R. Grove, Esq., V.P., in the Chair.

The following communications were read:—

"On the Photography of the Moon." By William Crookes, Esq.

The subject of lunar photography is one which has engaged the attention of scientific men almost from the first announcement of the possibility of fixing the images in the camera. Owing to the extreme difficulty of satisfying all the conditions of the problem to be solved, there are few good photographs of the moon yet in existence. It was my good fortune in the autumn of 1855 to obtain several excellent pictures of this kind, and since these form the starting-point of the work which, by the assistance of a grant from the Donation Fund of the Royal Society, I have been pursuing during the greater part of the last year, a detailed account of the means employed for their production will not, I think, be considered out of place here.

The telescope in which these pictures were taken is the magnificent equatorial at the Liverpool Observatory. This, together with all the resources of the establishment, was placed at my disposal by my kind friend Mr. Hartnup, to whom it is but due to state, that, were it not for the invaluable assistance afforded me by his sterling advice as well as steady hand, the results would not have been worth keeping.

The mounting of the equatorial is quite unique; the polar axis and telescope together weigh about five tons, and whilst all parts are so truly and smoothly fitted that this enormous mass is moved equatorially by means of a small water-mill with such marvellous accuracy, that a star viewed through it appears absolutely stationary, its firmness is such that a hard blow against the side merely produces a scarcely perceptible momentary deflection. The object-glass is 8 inches in diameter, and has a sidereal focus of 12·5 feet—the diameter of the moon's image in this focus being about 1·35 inch.

The eyepiece was removed, and in its place the body of a small camera was attached, so that the moon's image might fall upon the ground glass or sensitive film in the usual manner. Much labour had been saved me in finding the true actinic focus, by several photographers of Liverpool, who were working for some time on the same subject when the British Association met in that city in 1854. They found that the object-glass had been over-corrected for the actinic rays—the plate being required to be placed at a distance of 0·8 of an inch beyond the optical focus: a few experiments were sufficient to enable me to verify this result.

During the time above referred to, and frequently since, Mr. Hartnup had taken many hundreds of pictures with chemicals recommended by various persons, but had not succeeded in obtaining a good negative at all, and not even a positive with a less exposure than from half a minute to a minute. As I succeeded in taking dense negatives in about four seconds, with the temperature of the room below freezing and the moon at a considerable distance from the meridian, and as I attribute the greater sensitiveness which I obtained to the great purity of the materials I employed, I think it right to give, after the mechanical arrangements are described, an account of the way in which these were prepared.

The clockwork movement was only sufficient to follow the moon approximately when on the meridian, but as the pictures were nearly all taken when she was some distance past the meridian, and when consequently the declination and atmospheric refraction were changing rapidly, it was necessary, notwithstanding the short time required to take the pictures, to correct for the imperfect motion of the telescope. This was done by means of slow-motion screws attached to the right ascension and declination circles, which are each 4 feet in diameter. The finder had an eyepiece with a power of 200 applied to it, having cross wires in its field.

The *modus operandi* of taking the picture was as follows:—The telescope having been moved until the moon's image was in the centre of the focusing glass, the water-mill was turned on and the dark slide containing the sensitive collodion plate was substituted for the ground glass. Mr. Hartnup then took his station at the finder, and with a tangent rod in each hand, by a steady and continuous movement, kept the point of intersection of the cross wires stationary on one spot on the moon's surface.

When the motion was most perfectly neutralized, I uncovered the sensitive plate at a given signal and exposed it, counting the seconds by means of a loud-ticking chronometer by my side.

From the ease with which on my first attempt I could keep the cross wires of the finder fixed on one point of the moon by means of the tangent rods, I confidently believe that with the well-tutored hands and consummate skill which guided this noble instrument, the moon's image was as motionless on the collodion plate as it could have been were it a terrestrial object.

The negatives which I obtained by these means were exquisitely beautiful, and so minute that I could not obtain paper with a sufficiently fine surface whereon to print copies which would do them justice. It was evident that they would bear magnifying several diameters and still remain sharply defined. The expense of carrying out this design here stopped me, when by the kind advice of Professor Wheatstone I applied to the Royal Society, whose munificence has so frequently been the cause of bringing to a successful termination investigations of the highest importance.

A half-plate photographic combination of lenses, by Ross, was screwed the reverse way into a large sliding camera body 10 inches high by 11 inches wide, and capable of sliding from 18 inches to 3 feet long. At the end of the sliding body opposite to the lens, was a groove to admit either a focusing glass or a dark slide for the sensitive plate. A smaller camera body was screwed into the other end of the brasswork of the lens, having also a groove in front to admit of a sliding box capable of holding the small negatives. A reflector was placed in front of all, so arranged as to move in altitude round a centre, and, being fixed in any required position, to reflect the diffused light of the sky through the negative and lens parallel with the axis of the latter.

Preliminary trials showed me that there was no good gained by magnifying the small pictures more than about 20 times, as after that the individual parts begin to get confused and indistinct; this magnifying cannot, however, be effected at once. In the small negatives the lights and shades are the reverse of what they are in nature, consequently a print on paper therefrom gives the light and shade correct. A photographic copy of a negative, however, produces a positive by transmitted light, and a print from this would have the shadows light and the light parts dark; consequently, in magnifying a negative with the intention of still producing a negative, an intermediate transmitted positive must first be taken, and this in its turn magnified, when it will produce a negative.

The relative distances of the negative and focusing glass from the intermediate lens were so adjusted, that an image of the negative, enlarged to about two diameters, was thrown upon the ground glass, care being taken that the light from the sky was reflected parallel through the centres of the negative and lens by means of the mirror. The aperture of the lens was then stopped down to half an inch by means of a diaphragm, and the focus most carefully obtained by sliding the end of the large camera in or out. I found it necessary to verify this by experimental trials at different distances on each side of the observed focus, as it was difficult to judge accurately with the eye on the ground glass, owing to the roughness of the latter and the feebleness of the light.

A picture, or rather many pictures, were now taken, and the one which by transmitted light most truthfully resembled a paper print from the small negative was reserved for further magnifying. This was effected absolutely in the same manner as the former: the negative being removed and the positive being placed in its stead, a further magnifying gave a large-sized negative.

Although this process seems very simple, it is impossible to estimate the difficulties, unless by an actual repetition of the experiment, which I had to overcome before arriving at the beautiful result which I have the honour to lay before the Society. The double copying had a tendency to slightly exaggerate the effect of light and shade, and this could only be obviated by exposing the plates for such a time, that with the feeble light at my command it was verging on decomposition; particles of dust, too, seemed most pertinaciously to fix themselves on the prominent mountains, giving rise to craters where none should be; and even my finished pictures are not perfectly free from these faults, although each negative is the representative of a month's work and upwards of a hundred failures.

I doubt if much better photographs of our satellite can be taken by the way I have pursued. The future of lunar photography lies in another direction: the image must not be received on a sensitive plate and this copy submitted to an after process of magnifying. Defects quite imperceptible to the naked eye on the small negatives, are expanded into great blotches when magnified. In fact, upwards of a dozen seemingly equally good negatives with which I started, have, with but one or two exceptions, shown spots when enlarged.

The magnifying must be conducted simultaneously with the photographing, either by having the eyepiece on the telescope, or better still, by having a proper arrangement of lenses to throw a magnified moon image at once on the collodion. The difficulty of want of light could not be any objection, as supposing the enlarged image to be equal to those which I have now taken, that would be an increase of area of about twenty times, consequently 20×6 seconds, or 2 minutes, would represent the average time of exposure; a period which, even were it prolonged four or five times, would not then be too severe a tax upon a steady and skilful hand and eye.

Description of the Photographic Process.

The glass employed for taking the original negative of the moon, was that known as "extra white colour patent plate," that for the intermediate positives and large negatives was ordinary patent plate. Cleaning the surface, which is an operation of especial importance, was effected in the following manner.

The glasses were dipped into and then well rubbed over with a hot solution of caustic potassa; then, after washing with water, they were transferred to hot nitric acid (one part strong acid to three of water), where they were allowed to remain for about half an hour.

A piece of soft wash-leather was plentifully rinsed, first in a warm dilute solution of carbonate of soda, afterwards in clean water, and then well wrung until all the superfluous water was squeezed out. The glass plates were taken from the nitric acid and rinsed in abundance of clean water, and then rubbed well on every part with the damp leather. This removed most of the superficial moisture; and the final drying was effected by means of another piece of wash-leather, prepared the same as before, but allowed to become perfectly dry.

Just previous to using, the plates, held in a pneumatic plate-holder, had the last polish given to them by briskly rubbing with a warm piece of fine diaper (which had also been previously washed in soda and water, and then well rinsed and dried) until the moisture condensed from the breath evaporated evenly and uniformly, especially guarding against the slightest contact between the surface of the glass and the fingers.

The plate was now held with its clean side downward until the collodion was about to be poured on, and every particle of dust (which was easily seen by bringing the source of light, the under surface of the plate and the eye, nearly in the same line) was gently wiped off by passing a warm piece of fine cambric lightly across.

Care was also taken to have the atmosphere of the room as free as possible from floating particles, and the dried collodion usually adhering to the neck of the bottle was scrupulously removed.

The collodion was poured on and the plate rendered sensitive in the usual way. As the temperature both of the equatorial and operating rooms was seldom far from the freezing-point, the great diminution of sensitiveness, which that circumstance would have occasioned, was obviated by having the nitrate of silver bath and developing solution warmed to about 30° C., and also by slightly warming the plates before using. The source of light was a fishtail gas burner in the outer room, and shining close to the orange glass window of the dark room.

The soluble paper for the collodion was prepared in the following manner:—a mixture was made of

	sp. gr.	
Commercial nitrous acid	1·43	4 fluid ounces.
Commercial nitric acid........	1·37	4 ,,
Sulphuric acid	1·82	8 ,,

When the temperature of the mixture had cooled down to 50° C., one sheet of Swedish filtering-paper, torn up into small pieces, was completely immersed in the mixture, and allowed to remain therein for about half an hour. It was then thrown into a large pail of water, and the paper removed and placed on a sieve under a running tap for a quarter of an hour; after washing in very dilute solution of ammonia and then in plenty of water, the paper was pressed between the folds of a cloth, and then allowed to dry spontaneously in the air.

The collodion was made with—

Ether, sp. gr. ·725 (previously freed from acid by rectification from dry caustic potassa)	5 fluid ounces.
Absolute alcohol	3 ,,
Soluble paper (dried at 100° C.)	50 grains.
Iodide of cadmium (*pure*)	30 ,,

The alcohol and ether were mixed together, and then the paper and iodide of cadmium were added: they dissolved in a few minutes with a little shaking. As soon as the solution was complete, it was allowed to stand for twenty-four hours, and then half of the clear supernatant fluid was decanted carefully into a clean well-stoppered bottle for use. I believe that collodion prepared in this way will remain uniform from one year's end to another.

The nitrate of silver bath was made by dissolving 1 ounce of crystallized nitrate of silver, perfectly pure and neutral, in 2 ounces of water, then, with constant stirring, adding a solution of 4 grains of iodide of cadmium in 1 ounce of water, and a quarter of an ounce of the above iodized collodion, and water to make up the volume to 10 ounces. This was allowed to stand for a few hours at a temperature of about 25° C., and then filtered from the undissolved iodide of silver and precipitated paper. A glass bath was used in preference to gutta percha, and, as above stated, it was heated to 30° C. when used.

The developing solution consisted of—

Pure pyrogallic acid	8 grains.
Crystallized citric acid	16 ,,
Water	8 fluid ounces.
Alcohol	½ ,,

This developing solution is very slow in its action, 15 to 20 minutes being frequently required, but it ultimately produces negatives of such vigour and freedom from stains, that I much prefer it to the usual formula.

The fixing solution employed was the ordinary nearly saturated solution of hyposulphite of soda. After its employment the pictures were well and carefully washed in warm water, dried before a fire, and, after scratching the description or name on a corner, varnished with the usual solution of amber in chloroform.

The subsequent operation of printing is so easily performed, and has been so fully described by persons of more experience than myself, that any further allusion to it will be needless.

Appendix.

Besides the pictures taken in America—which are almost valueless as moon maps, as the sides are reversed in the copying from the daguerreotype plate upon which they were originally taken,—the moon has been photographed by Professor Phillips, Father Secchi, MM. Bertsch and Arnauld, several Liverpool photographers, and Mr. Hartnup and myself. It is interesting and instructive to compare among themselves the means employed and the time occupied in taking the impression on these several occasions.

Professor Phillips's telescope has a sidereal focus of 11 feet, and an aperture of 6¼ inches; consequently the brilliancy of the moon's image in its focus is augmented 26 times over what she appears to the naked eye. The average time occupied for the collodion plate to receive the impression was about 3 minutes.

Father Secchi's telescope having a sidereal focus of 18 times its aperture, the moon's image was intensified 37·8 times, and the time required for the impression was an average of 6 minutes.

M. Porro's glass of 49 feet sidereal focus and 20 inches aperture, gave a moon image 12·3 times brighter than she appeared to the naked eye, and the average time of taking the picture was 17 seconds.

Mr. Hartnup's telescope being 12½ feet focus and 8 inches aperture, augments the intensity of the moon's image at its focus 35·1 times. The time which was required for the photograph of our satellite to be taken, on the occasion of the meeting of the British Association at Liverpool in 1854, was about 2 minutes; and under the same circumstances we ourselves succeeded in obtaining perfect and intense negatives in 4 seconds. These, however, were taken under very unfavourable circumstances, the temperature being below the freezing-point, and the moon at a considerable distance from the meridian, which necessarily caused both a diminution of the light and also a diminished sensitiveness of the collodion film.

The rapidity with which the above pictures were taken may be better understood by comparing them with those of terrestrial objects under similar circumstances. According to Herschel*—

"The actual illumination of the lunar surface is not much superior to that of weathered sandstone rock in full sunshine. I have frequently compared the moon setting behind the grey perpendicular façade of the Table Mountain, illuminated by the sun just risen in the opposite quarter of the horizon, when it has been scarcely distinguishable in brightness from the rock in contact with it. The sun and moon being nearly at equal altitudes, and the atmosphere perfectly free from cloud or vapour, its effect is alike on both luminaries."

Thus by comparing the Liverpool object-glass as to power with our ordinary camera lens, its focal length being nearly 19 times the aperture, and the moon's image being copied by its means in 4 seconds, we find that it is equivalent to copying sandstone illuminated

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* Herschel's Outlines of Astronomy, page 249.

by the sun in 4 seconds with a lens $4\frac{1}{2}$ inches focus and a little less than $\frac{1}{4}$ inch diaphragm; or with a compound lens having an aperture of one inch, and the same focal length, in a quarter of a second.

"Researches on the Reproductive Organs of the Annelids." By Thomas Williams, M.D., F.L.S.

"Addition to a Memoir on the Determination of Unknown Functions that are evolved under Definite Integrals." By J. Gomes de Souza, Esq.

In his previous communication (Phil. Mag., March 1857) the author developed ϕx in terms of the function $A_r e^{m_r x}$. In the present communication he developes in a more general way, using terms of the form $A_r \int_r^\delta e^{x\omega} \varpi(\omega_1 m_r) d\omega$, the function of ϖ being assumed at pleasure.

XXVIII. *Intelligence and Miscellaneous Articles.*

ON THE CALCULATION OF VAPOUR DENSITIES.

BY HERMANN KOPP.

WHEN the atomic weight of a substance is calculated for oxygen $=8$, and its vapour density for atmospheric air $=1$, supposing these data to be rigidly correct, it is found that the quotient of the atomic weight by the density is equal to one of the following numbers:—

28·88 14·44 7·22.

The number 28·88 corresponds with a condensation to 4 volumes.
„ 14·44 „ „ „ 2 „
„ 7·22 „ „ „ 1 „

These numbers represent what may be called the *normal quotients*.

When the density of the vapour of a substance has been approximatively determined, the quotient of the atomic weight by this density is a number very near one of these normal quotients. This operation immediately indicates the mode of condensation of the vapour. On the other hand, it is always easy to calculate the theoretical density of a vapour of a substance by dividing its atomic weight by one or other of the normal quotients. Thus, for the immense majority, if not the whole, of the organic compounds, the theoretical vapour density is found by dividing the atomic weight by the number 28·88.

Some examples will show the exactitude and the advantages of this mode of calculation. When the atomic weight of acetic æther, $C^8H^8O^4=88$, is divided by the vapour density, which experiment gives for this substance $=3{\cdot}112$, the quotient $\frac{88}{3{\cdot}112}$ is equal to 28·2, a number very near 28·88. This result indicates a condensation to four volumes. The theoretical vapour density of acetic æther is therefore $\frac{88}{28{\cdot}88}=3{\cdot}047$.

When the atomic weight of chloride of æthyle $=64{\cdot}5$ is divided by the vapour density of this substance, 2·22, the quotient is $=29{\cdot}0$. This result indicates a condensation to four volumes. The theoretical density of the vapour of chloride of æthyle is therefore $\frac{64{\cdot}5}{28{\cdot}88}=2{\cdot}233$.

When the atomic weight of chloride of titanium $=96$ is divided by the vapour density of this substance $=6{\cdot}8$, the quotient obtained is 14·12. This number indicates a condensation into two volumes. The theoretical density of the vapour of chloride of titanium is therefore $\frac{96}{14{\cdot}44}=6{\cdot}648$.

This mode of calculating the vapour density presents several advantages: it does not at all require the knowledge of the vapour densities of the elements of which a substance is composed.

The formula of a compound is only taken into consideration in as far as it serves to fix and calculate its equivalent. This equivalent being known, the nature and number of the elements contained in a compound are of little consequence. The density of the vapour may be calculated independently of these data.

With a great many substances, in fact, the vapour density is observed to depend entirely upon the weight of the equivalent. Thus caproic alcohol, $C^{12}H^{14}O^{2}$, propionate of æthyle, $C^{16}H^{10}O^{4}$, and anhydrous acetic acid, $C^{8}H^{6}O^{6}$, which with very different formulæ have the same equivalent, 102, have also the same vapour density, 3·332. Carbonic acid and protoxide of nitrogen have the same equivalent, 22, and the same density, 1·524. Formic acid, $C^{2}H^{2}O^{4}$, and alcohol, $C^{4}H^{6}O^{2}$, have the same equivalent, 46, and the same density of vapour, 1·592. Phenole, $C^{12}H^{6}O^{2}$, and bisulphuret of methyle, $C^{4}H^{6}S^{4}$, the composition of which is so different, have the same equivalent, 94, and the same density of vapour, 3·255.

In general the vapour densities of substances which have the same equivalent are equal, or stand in very simple relations. It therefore appears rational only to take into consideration the equivalent in calculating the vapour density.—*Comptes Rendus*, June 29, 1857, p. 1347.

ON THE FORMATION OF WATER BY PLATINUM ELECTRODES.

BY M. BERTIN.

Electricity, like heat, may give laminated platinum the property of producing the combination of hydrogen with oxygen either slowly or with explosion.

When platinum plates have transmitted the current of at least forty elements into acidulated water, if they are covered by a bell-glass to collect the mixture of oxygen and hydrogen, this mixture detonates spontaneously as soon as the bell-glass is nearly full of gas.

If the current be less energetic, the explosion of the mixture does not take place, but its slow recomposition maintains the volume of the gases constant, notwithstanding the incessant decomposition which takes place at the lower part of the electrodes.

When the acidulated water is replaced by common water, the current of a pile of fifty elements is incapable of producing the detonation of the mixture; but the slow recomposition becomes very energetic, and the liquid is seen to oscillate in the bell-glass in consequence of the unstable equilibrium which is established between the decomposition of the gases which takes place at the lower part of the electrodes, and the recomposition which is effected at their upper part.

These phænomena are manifested with platinum plates supported below by platinum wires to which they are soldered, whether the surface of the platinum be naked or platinized.—*Comptes Rendus*, June 15, 1857, p. 1273.

ON THE OPTICAL PROPERTIES OF MAGNETIC BODIES.—SECOND NOTE. BY M. VERDET.

In his previous note* the author stated that the compounds of manganese acquired a positive rotatory power under the influence of magnetism. He has since found a compound of that metal, the double cyanide of manganese and potassium (corresponding in its composition with the red ferrocyanide of potassium), the magnetic rotatory power of which is negative. Thus manganese represents to a certain extent the connexion between the two classes established by the author amongst the magnetic metals; that which is the rule for compounds of iron is the exception for those of manganese, and *vice versâ*.

The corresponding double cyanides of cobalt and chromium both possess a positive magnetic rotatory power. The double cyanide of cobalt and potassium is even diamagnetic.

The author has now no doubt as to the negative magnetic rotatory power of the salts of cerium. A sufficiently concentrated aqueous solution of chloride of cerium, under the action of magnetism, exerts upon polarized light an action opposite to that of water; it is easily ascertained in consequence of the perfect limpidity of the liquid.

The compounds of uranium and lanthanium have a negative magnetic rotatory power. Nitrate of uranium in a state of purity is diamagnetic; but the red and black oxides of uranium, which may be extracted therefrom by the action of heat, are both magnetic. Uranium must therefore be classed amongst the magnetic metals. On dissolving nitrate of uranium in water, æther or alcohol, liquids are obtained, the action of which upon polarized light is less than that of the proportion of solvent contained in them. The negative action of the dissolved salt is therefore indisputable.

Perfectly pure carbonate of lanthanium is strongly magnetic. The solution of chloride of lanthanium obtained by treating this carbonate with pure muriatic acid, when submitted to the action of magnetism, exerts a less action upon polarized light than water. The magnetic rotatory power of chloride of lanthanium is therefore to be regarded as negative.

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* See Phil. Mag. July 1857, p. 78.

Specimens of molybdenum received from M. Debray are magnetic; and as this property is also possessed by molybdic acid purified by several sublimations, it cannot be attributed to the presence of foreign matters. The molybdates of soda and ammonia are diamagnetic; the magnetic rotatory power is positive, but weak.

Lithium and glucinum are diamagnetic metals; the compounds of these metals, received by the author from MM. Troost and Debray, were most evidently repelled by magnets.—*Comptes Rendus*, July 6, 1857, p. 33.

THE IMPROVED INDUCTION COIL.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I had hoped that after the statement which I have been obliged to give, in the course of discussion, in the Mechanics' Magazine, of the proceedings of Dr. Noad and Mr. Bentley, that the latter gentleman would hardly have ventured to repeat his erroneous statements in proof either of his priority or his originality.

In his last communication he has prudently dropped the former question, but still endeavours to substantiate the latter by assertions which will be found to prove the reverse. Mr. Bentley states that his machine was not opened in my presence; but this is immaterial, I am content to take his own description of it; and when I explain the construction of mine, which was made twelve months before his was commenced, I think it will be seen that the two machines are perfectly identical.

1st. Mr. Bentley asserts, but I do not know upon what authority, except his peculiar propensity for guessing, that I wind my secondary wire close to the cheeks of the gutta-percha bobbin; and that he tried this plan himself at first, but could not succeed, and then adopted the plan of winding each layer shorter than the preceding one, so as to gain insulating space over the ends. Mr. Bentley has misrepresented my mode of arrangement, for *I do not* wind my wire close to the cheeks of the gutta-percha bobbin, but allow as much of the insulating medium to project beyond the layers as is sufficient to prevent the passage of sparks between them. I suppose that Mr. Bentley mistook the material with which the outside of my coil is finished, for wire; it is not wire, though it looks like it; and the cheeks of gutta-percha have nothing to do with the coil, but are merely put on for the sake of neatness. In this first point, then, the two machines are not dissimilar, but identical.

2ndly. Mr. Bentley says that "his own coil differs from Mr. Hearder's, and is formed of very fine parallel wires, each wire being magnetically and electrically insulated from the others;" but he does not say how mine is constructed, which I submit he ought to have done in order to prove the dissimilarity which he assumes. It happens, however, and his friend Dr. Noad was perfectly aware of the fact, that my iron wire is also composed of fine parallel wires, electrically and magnetically insulated from each other: so much for the second point of dissimilarity, or rather identity.

The interruption of contact in Mr. Bentley's machine, which he says I noticed as peculiar, did not proceed, as he states, from any modification of the wire, but from his having adopted my stiff spring, which is one of the distinctive characteristics of my machine, and a point of identity which Mr. Bentley does not find it convenient to allude to.

The rapid vibrations and modified appearances of the sparks are as peculiar to my machine as to his, and I have also the power of varying them at pleasure. I confess that I should like to know by what means Mr. Bentley has arrived at the conclusion, that his spring has ever vibrated 2000 times per second; I think he must have inadvertently inserted one cipher too many, or made an erroneous guess.

Again, Mr. Bentley states that another feature of dissimilarity is, that he uses a number of coatings of thin gutta-percha in preference to fewer layers of thick tissue, &c. If Mr. Bentley will refer to the Philosophical Magazine for December 1856, he will find that I use a stratum of oiled silk or sheet gutta-percha between the layers. I have also used thin vulcanized india-rubber tissue, and I use the thinnest material I can get, in a number of layers precisely as he describes. Thus it appears that Mr. Bentley's falsely assumed points of difference are really all points of the most perfect similarity, and that to identify his name with his coil is a complete piece of plagiarism. I will now take the liberty of pointing out the peculiarly distinctive features of my coil as compared with that of M. Ruhmkorff, and show how these distinctive peculiarities have all so strangely appeared in Mr. Bentley's machine.

1st, then, Ruhmkorff's secondary wire is covered with cotton; mine is covered with silk, and so is Mr. Bentley's.

2nd. Ruhmkorff insulates the layers of his secondary wire with paper; I use gutta-percha, and so does Mr. Bentley.

3rd. Ruhmkorff uses a hammer at the end of a lever, resting by its own gravity upon an anvil, to break contact; but I use a very stiff spring for the purpose, the vibrations of which I can modify at pleasure, and so does Mr. Bentley.

4th. Ruhmkorff uses iron rods about a quarter of an inch thick for the core of his machine; I use thin wires electrically and magnetically insulated from each other, and so does Mr. Bentley.

5th. Ruhmkorff for his condenser uses about twelve or fourteen feet of tinfoil folded between sheets of oiled silk. I have used oiled silk, vulcanized india-rubber, gutta-percha, cartridge-paper, and varnished paper, and I make my condenser of very large dimensions. Mr. Bentley uses varnished paper and does the same.

These peculiarities of my machine were all thoroughly known to Dr. Noad more than six months before he and Mr. Bentley brought out the machine in question; and I may be pardoned for reasserting, though it should be for the fifth time, my claim to be considered as the originator of this new form of induction coil in

England. I have no desire to press this discussion further; but if I find it at all necessary, I shall not hesitate to publish the whole of the correspondence betwixt Dr. Noad and myself, by which my position with regard to him and Mr. Bentley will be still better understood. In conclusion, I may state that the whole of this very unpleasant affair would have been avoided if Dr. Noad, when he introduced the machine to the public, had been candid enough to allude to my previous labours, which he himself was the means of keeping from the public.

I have the honour to remain, Gentlemen,

Yours very obediently,

JONATHAN N. HEARDER.

28 Buckwell Street, Plymouth,
August 10, 1857.

ON A MODIFIED FORM OF RUHMKORFF'S INDUCTION APPARATUS.

BY E. S. RITCHIE.

The induction apparatus made by Ruhmkorff and described by Du Moncel is probably familiar to many of your readers. By it is obtained a spark three-fourths of an inch through the atmosphere. Mr. Hearder has described in the London Philosophical Magazine (Nov. and Dec. 1856) certain improvements by which he has lengthened the spark to 3 inches.

The great difficulty experienced by him was in obtaining sufficient insulation between one stratum of the wire and the next above or below it, the entire thickness of the helix—including wire and insulation—being only about half an inch, and a tension of electricity sufficient to throw a spark 3 inches existing between the outer and inner strata.

M. Stöhrer has adopted the plan of dividing the coil into three divisions, thus lessening the difficulty; still, great danger exists of the spark passing, which would ruin the helix.

I have endeavoured to obviate this by winding the coil the *entire thickness as it progresses*. I commenced with a glass tube or bobbin, laying the first course on a cone at as great an angle as the wire could be conveniently laid—say about 50°. The diameter at the tube was about 2½ inches, and the greatest diameter 3½ inches, the length of the cone being nearly ½ an inch. When the stratum was laid and cemented by resin and bees-wax, a ring of thin vulcanized india-rubber was stretched over and cemented, the wire passed down to the glass cylinder, and this wire covered also by india-rubber; then another stratum was laid in the same manner, that is, the coil is built up precisely as a *cop* is laid by a mule-spinner. The advantages are, that the wire in each conical layer is very short, and only a slight tension can exist between them.

With a helix thus made, with less than 7000 feet of wire, I obtained a spark of 2¼ inches; and with one since constructed on the same principle, with 30,000 feet of wire, differing only so far as I found necessary to enable me to wind the helix by a machine which I constructed for the purpose, I have obtained sparks over 6 inches long.

I have constructed the condenser with oiled silk, with very thin gutta-percha, and with paper of different thicknesses; but find tissue-paper varnished and used double, according to Mr. Bentley's plan, the best. The surfaces used in the instruments above described are respectively about 30 and 75 square feet. I have used all the interruptors alluded to by the writers above mentioned, but prefer one which I have made thus:—The anvil is a wire or small rod of platinum secured in a plate by a binding-screw; over this a rod of platinum is secured in the same manner to a spring which presses them together; another spring loaded acts like a hammer upon the end of the first spring, to separate the platinum rods. A ratchet-wheel presses down this spring-hammer, and allows it to recoil and strike the other spring. By this the interruption is more instantaneously made, and the distance to which the platinum rods are separated easily regulated. This point appears to be of importance. The spark is lessened if the platinum rods are separated further than actually to break their contact. The usual primary helix of large wire and the interior bundle of iron wires are placed within the glass tube.

In my last instrument, I used a tube closed at the top, more effectually to cut off the passage of the current from one end to the other through the primary helix or iron wires. I have used a Bunsen's battery of four to six cells; four give the spark of as great length, but a few more cells increase the volume. I have applied a battery of eighteen cells, and also a plate battery of fifty-six pairs, without endangering the coil The instrument is undoubtedly capable of being greatly increased in size and power.

Boston, May 2, 1857.

P.S. Since writing my paper, I have constructed a helix in which the plane of the strata of wires is perpendicular to the tube, insulated as before. With one of the same length of wire as the largest one before mentioned,—throwing a spark, with six cells, 6 inches,—I have used a battery of eighteen cells (Bunsen's); but by using a battery of three series of six cells (that is, an *intensity* of six, and quantity of three), a very voluminous spark was obtained; as the action soon became feeble, I took the secondary coil from the glass cylinder and found that the current had *passed through the glass* near each end of the coil, forming a circuit through the primary wire; two minute holes, of a hair's breadth, from one-tenth to one-eighth of an inch in diameter, were drilled through, but the glass was *not fractured*; it also passed through several thicknesses of vulcanized india-rubber. The helix was uninjured, proving the insulation obtained by the mode of winding it. A more perfect insulation between the helices is readily made; and I now use a tube of gutta-percha over the glass. With powerful batteries the condenser of varnished paper is not sufficient, as the current passes entirely through, and with such I use oiled silk. I have put several condensers in the same instrument, connecting each by turning a screw, so that either or all can be used. Varied and beautiful effects are produced, particularly *in vacuo*, by using different amounts of surface of condenser.—June 10, 1857. *From* Silliman's *American Journal for* July 1857.

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

[FOURTH SERIES.]

OCTOBER 1857.

XXIX. *Observations on the Structure of Glacier Ice.*

By T. H. HUXLEY, *F.R.S. &c.*

The Government School of Mines,
Jermyn Street, September 14, 1857.

MY DEAR TYNDALL,

IN the following pages I have given you some account of the experiments and observations upon the structure of glacier ice, which, at your suggestion, I commenced during our sojourn at the Montanvert this autumn. No one knows better than yourself how much these subjects grow under the hands of the inquirer, and how little claim my brief investigations have to the character of completeness. Nevertheless my conclusions, so far as they go, are based on such clear and decisive evidence, and are so totally opposed to the views entertained by the highest authorities, that I feel I shall be doing more good by publishing than by withholding them.

I will in the first place state what I have myself observed with regard to the structure and the permeability of glacier ice, and afterwards I will compare my results with those which have been arrived at by others.

Structure of Glacier Ice.—A mass of ice freshly extracted from any part of the Mer de Glace, the Glacier du Géant, or the glacier of La Brenva, at a depth of 8 or 10 inches from the surface, always presented the following characters when examined either with the naked eye, or with a lens of a magnifying power of thirty or forty diameters.

It broke with a vitreous fracture; and when the surface was made even, either with a sharp knife, or by rubbing on a warm surface, it appeared perfectly smooth and glassy, exhibiting not the least trace of fissures. Minute shallow pits, however, were scattered over it, and became particularly obvious when a coloured fluid was poured on to the surface and then wiped away again, inasmuch as under these circumstances every pit retained a very small portion of the colour.

The mass was, as usual, traversed by a larger or smaller number of parallel blue veins (whose lenticular form was almost always very apparent, particularly in the Brenva); and when a thin section was made perpendicular to the plane of the veins and viewed by transmitted light, it became obvious that the ice formed one continuous mass, without fissures or interruptions of continuity of any kind. It contained, however, a multitude of small, closed, and perfectly distinct chambers, and it was to the absence or rarity of these in the course of the veins that the latter owed their transparency and blueness.

The form and contents of these chambers were exceedingly remarkable. In a blue vein, and in those parts of the intermediate "white ice" which were contiguous to a blue vein, they were always round or oval disks, with extremely flat and closely approximated sides; so that, viewed in one plane, they looked like circles; but in a plane at right angles to this, like narrow parallelograms. In the white ice midway between the blue veins, on the other hand, I very generally noticed an irregularity of form, which was in many instances so great that the cavities appeared to be ramified. The walls of the chambers very frequently appeared to be a little roughened, or, as it were, frosted.

Every chamber, without exception, which I carefully examined contained both water and air. The former was commonly present in larger quantities than the latter, which swam as a bubble in the water, and could very often be made to move about in the chamber like the bubble of a spirit-level. It seemed to me, though I will not pretend to lay it down as a rule, that the air was more abundant in proportion to the water in the more irregular chambers. Where the air was in large proportion to the water, the bubble of course became more or less completely supported by the walls of the containing cavity, and to a certain extent assumed its form; but where, as in the majority of cases, the air-bubble was small in proportion to the water, its figure was spheroidal, and totally different from that of the containing cavity. I mention this particularly, because, as I shall show below, the chambers (which for distinction's sake I will term the "water-chambers") have been confounded with the air-bubbles, and the form which is characteristic of the one has been erroneously ascribed to the other.

I had no means of measuring the dimensions of the water-chambers, but at a guess I should say they varied from a tenth to a fiftieth or a sixtieth of an inch in diameter.

The line of contact of the water in the water-chambers with the ice was optically perfectly well defined, and easily distinguishable. Hence I have no hesitation in saying, that if canals or fissures of any appreciable size filled with water had existed in the ice, I must, with the magnifying power employed, have discovered some trace of them; but, I repeat, nothing of the kind was discernible in perfectly fresh ice.

If the existence of fluid water dispersed through its substance in closed chambers is shown by future observations to be a universal character of glacier ice (and I cannot imagine that a structure universally prevalent in the Mer de Glace, the Géant, the Brenva, and, as I shall show by-and-bye, from M. Agassiz's figures, in the Aar glacier also, is a mere local peculiarity), it appears to me to be a fact of primary importance. For what I have described is the structure of the unchanged ice of the glacier—of ice which has been protected from solar or atmospheric influences by that which covered it; and it must be remembered that the ice which is within a foot of the surface on the Mer de Glace opposite the Montanvert, must have formed a part of the very depths of the tributary glaciers. In other words, the ice which is at this moment, say a hundred feet below the surface in the Glacier du Géant, will, in consequence of ablation, form the superficial ice of some part of the Mer de Glace years hence. Consequently, unless it can be shown that the substance of a glacier, as it approaches the surface, is exposed to some influences capable of developing the water-chambers and their contents, it is to be presumed that the structure found near the surface in the lower part of a glacier is the structure which prevails throughout the thickness of the higher part; and hence that the structure described is that of unaltered glacier ice in general. This conclusion, as I shall immediately show, is directly confirmed by the boring experiments and by the figures of M. Agassiz.

M. Agassiz's deductions, however, are totally at variance with mine; and he is so generally quoted as an authority in these matters, that I feel compelled, however unwillingly, to enter into a detailed criticism of his views, which are contained in the following extracts from his 'Système Glaciaire,' numbered, for the sake of more convenient reference, in successive order.

(1) "At its origin, near the Névé, the compact (or proper glacier ice) contains, like the ice of the Névé, a notable quantity of air. But there is this difference between the two, that in the compact ice the air, instead of being distributed through the whole mass, is united in small perfectly circumscribed

bubbles, whilst the interspaces of these bubbles are perfectly transparent, so that without being as diaphanous as ordinary water-ice, the compact ice has not the opacity of Névé ice. Moreover, it is more compact, and what is especially characteristic, it presents no trace of granular structure: a fragment exposed to the action of heat does not become resolved into grains of Névé, but breaks up into angular fragments.

"This difference of structure is accompanied by a greater impermeability; water no longer traverses the mass with the same ease and uniformity, but is seen to follow in preference certain angular routes which are the capillary fissures."—P. 151.

(2) "The means employed by nature to maintain this amount of plasticity and compressibility in glacier ice is the water which circulates throughout the mass, and which, while it lubricates it, contributes to maintain within it a constant temperature during the greater part of the year."—Pp. 152, 153.

(3) "*Superficial fissures which must not be confounded with the capillary fissures.*

"When during a fine summer day one travels over the upper regions of the compact ice (about the region of the Abschwung, on the Aar glacier), a continual crepitation is heard on all sides. It is caused by the bubbles of air which on approaching the surface escape through the ice, where they have been dilated by the effect of diathermanicity, and cause the parietes of the ice to burst when they are no longer sufficiently strong to resist the dilatation of the air."—P. 153.

(4) "The air-bubbles undergo no less curious modifications. In the neighbourhood of the Névé, where they are most numerous, those which one sees at the surface are all spherical or ovoid; but by degrees they begin to be flattened, and near the end of the glacier there are some which are so flat, that they might be taken for fissures when seen in profile. The drawing, pl. 6, fig. 10, represents a bit of ice detached from the gallery of infiltration. All the bubbles are greatly flattened. But what is most extraordinary is, that, far from being uniform, the flattening is different in each fragment, so that the bubbles, according to the face which they offer, appear either very broad or very thin. I know of no more significant fact than this, since it demonstrates that each fragment of ice is capable of undergoing in the interior of the glacier a proper displacement independently of the movement of the whole."—P. 167.

(5) "The same flattening of the bubbles is found at a greater depth. While engaged in my boring experiments, I observed attentively the fragments of ice brought up to the surface by the borer. I found in them almost flat bubbles, perfectly similar to those of the fragment figured above, at all depths from 10

to 65 metres. I observed, besides, that in the fragments which proceeded from a great depth, all the bubbles without exception were strongly flattened, whilst at less depths there were some less compressed and even altogether round, as at the surface.

"It follows, hence, that a strong pressure is exercised in the interior of the glacier."—P. 167.

(6) "I ought also to mention a singular property of these air-bubbles, which at first was very surprising, but afterwards admitted of very satisfactory explanation. When a fragment containing air-bubbles is exposed to the action of the sun, the bubbles insensibly enlarge. Soon, in proportion as they enlarge, a transparent drop shows itself on some point of the bubble. This drop in enlarging contributes its share to the enlargement of the cavity, and as it progresses it predominates over the air-bubble. The latter then swims in the midst of a zone of water, and incessantly tends to reach the most elevated point, at least if the flatness of the cavity does not hinder it."—Pp. 167, 168.

(7) In a note appended to this passage, M. Agassiz speaks of the irregularity of the walls of some of the bubbles, and adds, "The same effect has been produced upon the bubbles of the fragment fig. 10. There also all the bubbles have enlarged by diathermanicity, and a little drop has developed in the middle of each. But as the cavities are very small, the drops do not yet move freely in their cavity."

It will be observed that in Nos. 1, 4, 5, 6, M. Agassiz confounds together the water-chambers and the air-bubbles under the common term of "bubbles," and he affirms (6) that the presence of water in the "air-bubbles" is the effect of exposure to the sun's rays, and of the different diathermanicity of air and ice*. A careful analysis of M. Agassiz's facts, however, is very instructive. In the first place, I recognize in his fig. 10. pl. 6, a fair, though rough and sketchy, representation of the general arrangement and form of the water-chambers with their contained air-bubbles. The chambers are as usual flattened, but the artist has rightly represented their contained air-bubbles as spheroidal. The strangest thing is, however, that M. Agassiz has taken the air-bubbles for drops of water, and the drops of water for air-bubbles, as any one who is familiar with the microscopic appearance of bubbles of air will see, on comparing the description in (7) with the figure 10. In the next place, I repeatedly exposed thin plates of ice to the sun, carefully watching the air-bubbles, without being able to observe the phænomena

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* The Messrs. Schlagintweit (*Untersuchungen*, p. 17) adopt Prof. Agassiz's views on this point, and with him regard the presence of water as a local and partial phænomenon.

detailed by M. Agassiz in (6); and I must frankly confess I do not understand how such changes as those described are reconcileable with the commonest properties of ice and air. How do the bubbles enlarge when exposed to the sun? M. Agassiz has already admitted that the chambers are closed (1), and we know that ice is not readibly distensible; and therefore I hold it to be impossible that the bubbles should visibly dilate before the melting of the adjacent ice; and as to enlarging by the melting of the ice-wall, the fractional difference between the volume of water and the ice from which it proceeds, would be wholly imperceptible on such a scale. With regard to the explanation of the crackling noise given in (3), I can only say that I have repeatedly watched a thin lamina of ice melting, both by transmitted and reflected light, and that I have seen the walls of the chambers reduced to the thinnest pellicle without being broken by pressure from within. The air-bubbles escape quite quietly as soon as their wall is perforated. Furthermore, the cavities left where the air-bubbles have been, are not fissures at all, but, as I have said above, rounded pits. Indeed, this is a necessary consequence from M. Agassiz's own statements with regard to the shape of the bubbles.

M. Agassiz affirms in (5), that ice brought up from a depth of 65 metres was perfectly similar in structure to that represented in his figure 10. The fact is important; but surely it alone affords sufficient evidence that "diathermanicity" has nothing to do with the formation of the cavities and their watery contents. And indeed in (4) this same piece of ice (fig. 10) is said to have come from the "gallery of infiltration," a cavity perfectly shaded, and bored many feet below the surface of the glacier. So that either this figure does not represent the structure of the glacier at this point, or the structure is unaltered, and diathermanicity has nothing to do with it.

It follows, therefore, that there is no evidence to show that the influence of solar radiation has anything to do with the structure; on the contrary, M. Agassiz's *facts* strengthen my case.

If it be the universal character of glacier ice to be full of closed cavities containing fluid water and air, it becomes a matter of extreme interest to ascertain how the air and the water come there; how it is that the water retains its fluidity, and how it is that the water-chambers are compressed. It may seem a common-place comparison, but the ice and its cavities containing water remind me of nothing so much as a Gruyère cheese, in which one so often meets with closed cavities containing fluid and air. Let the Névé represent moist curds and the glacier valley the cheese-press, and the analogy is perhaps closer than it looks. But these are questions for you to solve; and I will only venture on one other supposition, viz. that the water-chambers have the value of a register thermometer, indicating that the minimum temperature to which the mass of a glacier descends is never for long less than 32°; otherwise I cannot conceive how the water should remain fluid; and if it were once frozen, how could it melt again?

M. Agassiz makes a very important observation in (4), and one which I am glad to be able to confirm in the main. I took some pains to ascertain the general direction of the planes of the water-chambers, and I found that in the substance of the blue veins they were sometimes parallel to the plane of the latter, while in the white ice their planes were always more or less inclined to the veins, usually forming an acute angle, and never, so far as I have seen, a right angle with them. Furthermore, as Prof. Agassiz points out, the water-chambers are arranged in groups, all the members of the same group having parallel planes, while their direction is more or less inclined to that of neighbouring groups. It seems to me very probable that, as Prof. Agassiz suggests, the different directions of the planes of the cavities may indicate internal changes of place of segments of ice corresponding with the groups; but, as I have already said, no fissures separating these segments are to be found in the deep ice of a glacier, and hence we cannot with propriety speak of them as "fragments."

Such is the structure which I have found to obtain in all "deep" glacier ice, by which I mean, all ice situated more than a few inches below the surface. It is as solid as glass or marble, and as devoid of any but accidental and gross fissures. The glacier, however, where exposed to the atmosphere, presents what may be called a "superficial layer" of very different character. Every one who has had occasion to cut an escalier, must have been struck with the difference between the resistance to the ice-axe at the first blow and that at the fourth or fifth. At the first, the jar to the hand is slight, and fragments of ice fly in all directions; but, at the last, one might almost as well be hewing some hard though splintery wood. The reason of this at once becomes apparent on examining the superficial ice. It is composed of larger or smaller granules of exceedingly irregular form *, separated by very obvious fissures, but nevertheless so fitted into one another as to cohere with some firmness. The distance to which the fissures extend into the interior of the glacier (and

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* The superficial layer is particularly well described by the Messrs. Schlagintweit in their *Untersuchungen über die physikalische Geographie der Alpen,* 1850.

hence the thickness of the superficial layer) varies a good deal; 7 or 8 inches is perhaps rather above than below their average depth; but however this may be, the important fact is, that whenever you clear away the superficial layer, you find beneath it what I have termed "deep" ice—that is, ice in which neither fissures nor granules are discernible; ice which tends to split parallel to the veins, and shows no disposition to break up into the angular fragments so characteristic of the superficial layer.

It has been said that mere optical examination is insufficient to disprove the existence of fissures in the deep ice, and that such fissures are present, though invisible in consequence of being filled with water. I have already shown that the line of contact of water and ice is optically well marked, and that there is every reason to believe that even the finest fissures would be visible under a sufficient magnifying power; but those who maintain the porosity of glacier ice, rest chiefly on the results of experiments made with coloured fluids. It is said that glacier ice becomes infiltrated throughout its substance with extreme readiness, the coloured liquid traversing fissures which are more particularly developed in the course of the blue veins. It became necessary, therefore, to repeat these infiltration experiments; and for this purpose, as you will recollect, I made use of the logwood infusion which you had prepared, and which by its combined clearness and intensity of colour was excellently fitted for the object in view.

If a little of the infusion were poured upon the natural surface of the glacier, it immediately soaked in, spreading itself in all directions between the granules (but more rapidly, as I often observed, in directions parallel with the veins), and staining the whole thickness of the superficial layer. Whatever quantity might be poured on to the surface, however, it penetrated no further than the superficial layer (unless there were some obvious crack in the deeper ice); and when the latter was cleared away with the axe, and the surface of the deep ice washed or even carefully rubbed with the hand, not a trace of the infusion could be found in it.

If a piece of the deep ice containing several blue veins were allowed to soak in the logwood infusion until it nearly melted away, it remained unstained, and either wiping it or passing it quickly through clean water rendered it perfectly clear and stainless.

But it is said that if cavities be made in the glacier and filled with a coloured infusion, the latter will soon, by means of the capillary fissures, infiltrate the surrounding mass. To determine this point, I selected a spot upon the north wall of a crevasse, just opposite the Montanvert, and between the centre and the west shore of the Mer de Glace, where the veins were well developed, their planes having a general north and south direction, but dipping at an angle of about 70° towards the centre of the glacier. On the northern aspect of the ice I cut away the superficial layer, so as to form two faces of a cube of about a foot in the side on the deep ice. One of these faces looked westward, and was consequently nearly parallel to the cleavage; the other looked northward, and was therefore nearly perpendicular to it. Perpendicular to the west face, and therefore to the structural planes, I bored a hole with an auger, about an inch in diameter and 9 inches long, and just sufficiently inclined to the horizon to hold the infusion of logwood, with which I filled it. I then thinned away the north face of the cube very carefully until the north wall of the hole was less than 2 inches in thickness—until, in fact, I could see the dark fluid through the substance of the several blue veins which it traversed with perfect distinctness.

For two hours not the slightest trace of leakage or infiltration into the substance of the ice forming the walls of this cavity could be observed; and the contour of the contained liquid remained perfectly sharp and well defined. It then began to leak at one point near its upper end through a small crack *in the white ice*, which led directly outwards. The liquid spread neither up nor down in the crack. Four hours afterwards no change whatever had taken place in the liquid contained in the lower part of the hole. At this time you joined me upon the ice, and you will recollect that I carefully thinned away the wall with a sharp knife until in some parts it was not more than $\frac{1}{4}$ of an inch thick. Still no infiltration occurred. The knife at length accidentally penetrated the wall, and the liquid at once flowed out. I then poured some clean water through the hole, and all trace of the coloured infusion was at once so thoroughly removed, that, on cutting away one wall, the other appeared perfectly clean and of its natural aspect.

I have given the details of this one experiment in order to show in what manner all were made; but it is unnecessary to be equally prolix with regard to the others. Suffice it to say, that, whether the holes were bored perpendicular to the structure or parallel with it, or at any intermediate angle, whether in white ice or in a blue vein, the result was precisely the same, not a particle of fluid making its way into the surrounding substance of the ice along the veins, nor in most cases in any other way. Occasionally a leakage would take place in the manner described above, but the fissures in these cases were gross and visible, and their direction had no reference whatsoever to that of the structure. Indeed, as the leakage always took place towards the surface, and not into the depth of the ice, I am inclined to think that these cracks were produced in cutting the ice to thin away the outer wall of the cavity.

I repeated these experiments in the neighbourhood of the Grand Moulin; on the Moraine du Noire, somewhat higher than the Couvercle; and on different parts of the Glacier du Géant, and everywhere with similar results. Furthermore, having carefully bored a vertical hole in the deep ice of the Mer de Glace, opposite the Montanvert, I filled it with the infusion, and having covered over the aperture with a roof of ice-blocks, I left it until the next morning. It rained hard during the night; and on revisiting the spot after an interval of about fifteen hours, I found that the covering blocks had slipped off, and that the liquid occupied only about the lower two-thirds of the cavity. No trace of infiltration could be discovered; but the lower part of the cavity had changed its figure from cylindrical to irregular and botryoidal. I conceive that the sinking of the fluid must be accounted for by the enlargement of the cavity consequent upon this botryoidal excavation of its walls; and I suppose that the ice-blocks proving an insufficient shelter, the rain poured into the hole, and keeping up a constant supply of comparatively warm liquid, eroded its walls in the way described. However this may be, the fact that the liquid had produced a fresh surface for itself, is important, as it shows that the absence of infiltration through the veins intersected by the cavities containing the coloured infusion is not dependent on a condensation of their walls by the auger.

To eliminate any error of this kind, however, I took a small block of the deep ice, and with a sharp knife fashioned it into a cup, whose walls varied in thickness from $\frac{1}{4}$ to $\frac{2}{3}$rds of an inch. Filled with the infusion and surrounded with ice, this cup remained for two hours without showing a trace of infiltration along its structural planes.

I can only conclude from these experiments, that the chief substance of a glacier is as essentially impermeable as a mass of marble or slate; and that though it may be traversed here and there by fissures and cracks, these no more justify us in speaking of glacier ice as "porous," than the joints and fissures in a slate quarry give us a right to term slate porous. We do not call iron porous because water runs out of a cracked kettle.

The extreme porosity of what I have termed the "superficial layer," however, is no less certain, and inasmuch as this layer is continually and rapidly wasting away at its surface, it must be as constantly reformed from the solid glacier ice beneath.

The fact observed by Professor Agassiz, that under a moraine (that is, where covered and protected by stone and gravel) the superficial ice is of the same character as the deep, suggested the idea that the superficial layer is the result of the operation of atmospheric influences; and that just as a bed of impervious rock becomes broken up into fragments, separated by permeable interstices, down to a certain depth wherever it is exposed to the atmosphere, so the glacier ice when left unprotected undergoes a similar weathering and disintegration. I submitted this notion to the test of experiment in the following way:—Not far from the upper end of the Moraine du Noire, and on one bank of a stream which cuts its way down the Glacier du Géant, I cleared away the superficial layer and cut out a block of the deeper ice, which was then divided into two equal portions of irregular cuboidal form, and about 8 inches in the side.

The logwood infusion was poured on both of these, and was retained only by such portions of the superficial layer as had been allowed to remain. Water poured on to the blocks ran off them as it would run from marble or glass, sinking only into the remains of the superficial layer. I then placed the two blocks side by side, on an elevated ridge of the glacier, with their natural upper faces turned towards the sun, at this time (1·15 P.M.) shining brightly; the one block I left without protection, while the other was just covered by a stone of 4 or 5 inches in thickness, resting upon its upper face. At 1·40, that is to say in less than half an hour, I removed the block of stone and poured the infusion over both pieces of ice. The covered one could be as little infiltrated as before, while the face of the uncovered became at once beautifully injected, the fluid instantly running into a network of little superficial fissures which had developed themselves, and out of which the infusion could be only partially extracted by washing.

Both pieces of ice were well washed, and the stone was replaced on the one, while the other was left uncovered as before.

In the course of the ensuing half-hour I examined both blocks several times. The covered ice remained unchanged; but in the uncovered, the fissures extended further and further into the mass, which gradually assumed throughout the granular aspect of the superficial layer. Water poured on its surface soaked into it immediately, and a small quantity of the infusion spread out, the moment it reached the block, in the most beautifully ramified figure through the fissures. Particularly large and apparent fissures could thus be frequently observed traversing the middle of the blue veins. At length the fissures extended completely through the mass, which thus became truly sponge-like. Water poured on its surface, filling the interstices, gave the mass a clear and semitransparent aspect, though by no means to be compared to that of a blue vein. But as soon as the supply of water ceased, the fissures of the side uppermost immediately began to lose their water, which drained away below, and becoming filled with air, a whitish opake hue succeeded. On reversing the block suddenly, what had been its under surface appeared at first clear, but the water soon deserting it, it rapidly whitened, while the previous upper and white surface became clear. Water poured upon the upper surface, traversed the mass and flowed out again below with the utmost ease. In fact it is impossible to conceive any more striking contrast in these respects, than that between the freshly extracted ice-block (or that which had remained under cover) and that which had been exposed.

So far as it may be permissible to draw a conclusion from the few experiments I made, I should say that direct exposure to the sun has much influence on the rapidity of this process of weathering; but it is by no means essential, for the northern faces of the walls of crevasses exhibit a well-developed superficial layer; and I have seen it even beneath huge boulders, where these were not in direct contact with the ice.

But one conclusion appears to me to be deducible from these experiments, and that is in perfect accordance with the results of ocular investigation. Glacier ice is essentially devoid of all pores, fissures and cavities, save the closed water-chambers; though of course, like all other brittle bodies, it is liable to become fissured and fractured by pressure from without. Fissures and cavities produced in this way, however, are accidental and not essential. But it is a remarkable feature of glacier ice, that it is liable to weather in a peculiar manner, becoming fissured and breaking up into irregular fragments to a certain depth. The superficial layer formed in this way is eminently porous, and absorbs fluids like a sponge.

In arriving at these results, however, I again regret to find myself in direct opposition to the current doctrine based on the statements of Prof. Agassiz, from whose 'Système Glaciaire' I continue my series of quotations.

(8) "*Capillary fissures.*—The true capillary fissures are very different from the superficial fissures which have just been described (3). They exist not merely at the surface, but are found on the walls of crevasses and in the interior of cavities where the rays of the sun never penetrate. They are larger than the little fissures which have just been mentioned, and far less numerous, particularly in the regions in which the latter abound. Their distribution is not uniform in the interior of the compact ice," p. 154; but (M. Agassiz goes on to explain) they are arranged in bands and zones, which, becoming more completely infiltrated with water than the intermediate ice, appear blue and transparent, and are the blue veins.

(9) "The quantity of bubbles with which the white ice is filled, is the reason why the fissures are more slowly propagated in it; the air, by its elasticity, being unfavourable to the formation of fissures (l'air qui est de sa nature élastique ne favorisant aucunement le crevassement). By degrees, however, and in proportion as the infiltration perpetuates itself, the rigidity increases, and the fissures multiply in proportion. Every bubble that a fissure meets in its course loses its aëriform contents. It becomes transparent, and the opacity of the mass is so far diminished. The consequence of this multiplication of fissures is continually to diminish the number of bubbles, and by this means to render the ice more and more transparent and blue.

"It will be evident to any one who has followed the progress of modern physics, that this phænomenon is due solely to the diathermanicity of ice. The air first and then the water becomes heated through the ice. However minute may be the degree of heat which is thus transmitted to them, it is enough to melt a part of the ice which surrounds them, and thereby to increase the cavity in which they are imprisoned. I do not think, however, that any very great importance should be ascribed to this phænomenon; and the fact that it is produced only when the ice is exposed directly to the rays of the sun, is in my eyes an indication that it exercises no notable influence on the mechanism of glaciers."—P. 157.

(10) "When the ice has acquired a certain degree of transparency, and the network of capillary fissures is fully established in it, water and air penetrate into the fissures with great facility. One may assure oneself of this in many ways, among others by the following experiment, which I have repeated many times. Let a cube of ice of a few decimetres on the side be detached from the bottom of a crevasse, in that part of the glacier where the ice is most transparent, and placed upon a rock. At first, a few fissures will appear on the surface, then these fissures will be gradually propagated into the interior, and the network becoming more and more complex, will by degrees reach the base. If, then, the block of ice be turned upside down, and water be poured upon it, all the fissures will disappear from above downwards, in the same order as they were formed. The block will remain perfectly transparent so long as it is saturated; but so soon as one leaves off watering, the fissures reappear where they last appeared when the block was reversed."—P. 161.

(11) "The angular fragments are the consequence and the product of the capillary fissures. When a morsel of compact ice is exposed for some time to the air, it becomes decomposed into a certain number of angular fragments, which are the smaller

the more numerous the fissures. The same thing would happen to the glacier if its thickness were less, and if the external heat had access to it on all sides. Nevertheless its surface decomposes more or less, the fissures dilate in consequence of the circulation, and the fragments are so dislocated as to be moveable on one another without however becoming detached." —P. 163.

(12) "The angular fragments and the capillary fissures seem to disappear the moment the ice is covered. Thus on sweeping clean a part of a moraine, or the side of a gravel cone, the ice beneath is found to be perfectly smooth, and apparently without a trace of a fracture. But it is sufficient to leave these same surfaces uncovered for some instants, and the capillary fissures immediately show themselves, and, in consequence, the angular fragments. They appear with such regularity, that one might be tempted to believe that they are formed spontaneously at the very moment of their appearance. But on examining them with a little attention, one becomes convinced that they are of older date.

"I by no means pretend to deny that heat, acting suddenly at the moment the moraine is uncovered, may not develope some cracks. I have myself seen such cracks form suddenly (par éclat), but I conceive they are but few. If it were otherwise, and if the fissures were formed as they appear, it would be necessary to suppose that there are none in the ice of the moraine before it is uncovered, which would be contrary to all we know of the transformations of the ice."—P. 165.

(13) "Let us now make the opposite experiment, and cover with sand and gravel a portion of the surface of the glacier. However fissured and disaggregated it may be, the fissures and angular fragments will disappear at the end of some time so completely, that on removing the gravel the surface will be found as compact and transparent as that of a portion of moraine which has never been uncovered. And yet it is not probable that the fissures have reunited during the interval. It is, on the contrary, the gravel, which, intercepting the air and keeping the fissures full of water, renders them invisible, and gives to the whole mass a false appearance of compactness, which ceases the moment the air again has access to the fissures."—P. 166.

If the extract (8) were to be taken merely as a description of the superficial layer of a glacier, I should only have to object, that, so far as I have been able to observe, the colour of the disintegrated blue veins is not much affected by the water they contain, and that no amount of watery infiltration will confer on the white ice the beautiful transparency and colour of the blue veins. But Prof. Agassiz over and over again affirms that the whole substance of the glacier is traversed by capillary fissures, and his infiltration experiments are supposed by himself conclusively to demonstrate the fact. I must confess, however, that I have neither been able to observe what Prof. Agassiz supposes he has observed, nor, were our observations in unison, could I admit his explanations.

Take for instance the citation (9). How can the elasticity of the air-bubbles influence the formation of fissures in the continuous mass of rigid and eminently brittle ice which encloses them? How is the statement, that the ice becomes more rigid as the fissures are developed in it, these fissures being supposed to be filled with water, compatible with that made in (2), that this same water is the chief source of the plasticity and compressibility of ice?

Again, I am at a loss to understand the "diathermanicity" theory. Prof. Agassiz brings forward no experimental proof that air contained within ice is more heated by the sun's rays than the ice itself; and, *à priori*, it seems improbable that the more diathermanous body should be more heated than the less. It is true, I cannot pretend to have "followed the progress of modern physics;" but I am emboldened to say this much by the fact, that you, who have, seem to find at least equal difficulty in adopting Prof. Agassiz's explanation.

With regard to the experiments detailed in (10), (12), and (13), it will be observed that my results in the main agree with those of Prof. Agassiz, if, as before, we confine ourselves to the superficial layer; but, as I have shown, it is an error to extend the conclusions drawn from the structure of the superficial layer to the deep ice. This, however, is what Prof. Agassiz has done; and it is curious to find him in (12) refusing to follow out a suggestion which would have led to the solution of his difficulty, because it is "contrary to all we know of the transformations of the ice." What do we know at present of the transformations of the ice?

It is important to remark again, that *as regards matters of fact*, Prof. Agassiz's statements with respect even to the deep ice are, so far as they go, not essentially different from mine. He admits (12) that no fissures are at first visible in the deep ice;—had he taken the trouble to make the experiment, he would have found also that coloured liquids cannot be made to enter it;—and he admits that the establishment of a complete system of fissures through a block of ice, and its consequent permeability, are matters of time and exposure (10). See also citation (1), and p. 289 of the 'Système Glaciaire.'

I omitted to make the experiment detailed in (13). It is singular that in (12) Prof. Agassiz states that "the angular fragments and the capillary fissures seem to disappear *the moment* the ice is covered," while in (13) the operation is said to take some time; but, supposing the fact to be as Prof. Agassiz says, it seems to me to be in the highest degree probable that the fissures *have* reunited during the interval. At any rate, I cannot admit Prof. Agassiz's explanation, for surely loose gravel is not exactly a substance calculated to " intercept air and keep fissures full of water."

It would take up too much space, and serve no useful purpose, to quote at length the account Prof. Agassiz gives of his infiltration experiments (Syst. Glaciaire, pp. 170–179). Those who will turn to the original, will find that they are all vitiated by the absence of any discrimination between the deep and the superficial ice, and between "capillary fissures" and accidental cracks. Not one of Prof. Agassiz's experiments affords the slightest evidence that capillary fissures are a primitive and essential constituent of the structure of the deep ice of a glacier.

The experiments of the Messrs. Schlagintweit (*l. c.* p. 12) appear to me to be equally inconclusive; these gentlemen, like Prof. Agassiz, having omitted to take the precaution of clearing away the superficial layer from the mouth of the cavity to be filled with the infiltration fluid. Unless this be done, the superficial layer sucks up the coloured liquid, which becomes diffused in the way they describe. And if the cavity (as may readily happen, especially with such large ones as those employed by these experimenters) communicates by an accidental fissure with some other part of the surface of the glacier (say the wall of a crevasse, or the roof of such a cavity as Prof. Agassiz's infiltration gallery), it should be well remembered that the fluid which drains through will not run out in a stream from the termination of the crack, unless the superficial layer has been cleared away; otherwise, it will fill the fissures of the superficial layer and appear as a great patch. The observer then, seeing nothing but fissures full of coloured infusion at each end of the course of the fluid, naturally enough imagines that in its intermediate course the fluid has traversed similar fissures. This conclusion would be at once dissipated by cutting away the superficial layer and laying open the infiltration cavity,—a precaution which does not seem to have occurred to either Prof. Agassiz or the Messrs. Schlagintweit.

I will conclude with a few words upon the relation of structure to the arrangement of dirt upon the surface of a glacier. The great "dirt-bands" have never been *proved* to be connected with any peculiar structure of the ice on which they lie, and it has been shown that they *may* be the mere result of the influence of the motion of the glacier upon the form of any patch of dirt scattered accidentally upon its surface; but besides these "dirt-bands," the dirt on a glacier frequently presents a definite arrangement upon a smaller scale, which *is* connected with the minute structure of the glacier. We have both observed, for instance, in those parts of the Mer de Glace in which the structure is vertical, that the superficial layer of the wall of a crevasse is weathered into granules of tolerably even size and similar form. Nevertheless, dirt (or a coloured infusion) accumulates in larger proportion in those fissures which are parallel with the cleavage, and thus, from a little distance, the surface of the ice appears as if striated or ruled with lines parallel to the structure. The lines are separated by the width of the granules, and there may be several interposed between two blue veins.

Why it is that those intergranular fissures which are parallel with the cleavage are the larger, is a question I will not for the present attempt to answer. It may be that the weathering takes place more rapidly in this direction, or it may be that these fissures being in the course of the flow of the water produced by the superficial waste of the ice, become enlarged more rapidly than the others.

These markings, and the similar ones frequently to be observed on the upper surface of a glacier, might be termed "dirt-lines," to distinguish them from the great "dirt-bands." There is a third mode of arrangement of dirt, which, like the "dirt-lines," is dependent on the weathering of the ice, but the resulting striæ are broad streaks, and not mere lines. These may perhaps be termed "dirt-streaks."

I became acquainted with these quite recently, when, induced by Prof. Forbes's description and representation of the "structure" of the glacier of La Brenva, I paid a visit to that glacier. Prof. Forbes states,—

"The alternation of bluish-green and greenish-white bands which compose this structure, gives to this glacier a most beautiful appearance as seen from the mule-road. An attempt has been made in plate 5 to give some idea of this most characteristic display, and which is better seen here than in any other glacier whatever with which I am acquainted. The sketch was taken by myself from the point marked *k* on the map in July 1842."—*Travels*, 2nd Ed., p. 203.

It must at once strike any one conversant with the ordinary character of the veined structure, that at the distance of the point on the mule-road from which Prof. Forbes's view is taken, any veins of the usual dimensions must be totally invisible; and I therefore approached La Brenva with the desire, if not the hope, of making the acquaintance of glacier structure on a new and gigantic scale.

Viewed from the mule-path, or from the old moraine at the commencement of the pine wood celebrated by De Saussure, the lower part of the glacier of La Brenva exhibits numerous crevasses, which appear to run nearly parallel with its length, so that the icy mass is divided into a series of parallel crests or ridges. The lateral faces of these ridges form perpendicular cliffs of ice, and present dark stripes directed in a longitudinal and nearly horizontal direction; but where an end view of a ridge is obtained, the stripes run either horizontally and transversely (as in the more central parts of the glacier), or are curved up towards the sides (as in the more lateral parts).

These markings are evidently those described and faithfully figured, as the "structure" of the glacier, by Prof. Forbes; but I cannot say I should have called them bluish-green. They looked to me simply dark and dirty. But I should state, that the weather, when I visited the glacier, was wet and cloudy.

Nevertheless, on descending on to the glacier itself, I found its structure, though very beautifully developed, to be in nowise remarkable for the size of its veins, which varied in length from an inch to eight or nine feet, and in breadth from a fraction of an inch to nine or ten inches. Veins of the latter dimensions, however, were rare; the majority having a thickness of less than an inch. The lenticular form was very well marked, and the veins were commonly separated by less than their thickness of white ice. I need hardly say that these veins became indistinguishable at a very short distance.

The streaks so conspicuous a long way off, on the other hand, became less sharply defined as I approached, and at length showed themselves to be nothing more than accumulations of the fine dirt—spread more or less over the whole cliff-like wall of ice,—in streaks of four to ten inches in breadth, and of variable length. They ran parallel with one another and with the structure, at a distance varying from a few inches to six or seven feet; and they were entirely superficial, the dirt never extending deeper than the weathered superficial layer.

It became clear, therefore, that the markings were neither structure nor stratification, but a peculiar kind of dirt-marks; and the next point was to ascertain the conditions of their formation.

On close examination, the face of the ice-cliff exhibiting these markings appeared to be worn into a sort of wavy or rippled surface, the length of the ripples having a general direction downwards. The close-set veins, on the other hand, traverse the face of the ice, as has been said, nearly horizontally. The whole surface of the ice is more or less dirty, not half-a-dozen square inches being without its little grains of sand and minute gravel, brought down, as I imagine, by the water which continually trickles from above; but the greater part of this impurity is invisible from a small distance, unless where it is specially accumulated.

Such accumulation takes place in two localities; in the first place, on the little shelves afforded by the upper and more southerly aspects of the "ripples" above referred to. Here the dirt accumulates quite independently of the structure, and as a consequence either of the form or of the aspect of the part on which it lodges.

From a little distance these aggregations appear as spots and patches, but further off they cease to be visible, and the glacier between the horizontal streaks appears white.

These streaks mark the second locality in which the dirt aggregates. Now whenever I carefully examined the surface of the glacier at these points, I found it to be weathered into large granules, separated by coarse fissures which extended for a considerable depth into the substance of the glacier; while the parts intermediate between the streaks, and which appear white from a distance, presented very much smaller granules, with fissures proportionately finer, and extending inwards for but a very small distance. In short, where the dark streaks existed, the ice was deeply weathered and coarsely granular, affording lodgment for dirt to a depth of two or three inches; while the intermediate substance had undergone only superficial weathering, and its finely granular structure afforded but little facility for the intrusion of foreign matters.

The "dirt-streaks," then, are due to the unequal weathering of the ice; but why does the ice weather unequally? On seeking for an answer to this question, I found that every dirt-streak corresponded either with a very large blue vein, or with a closer aggregation than usual of smaller blue veins, while the intermediate substance contained a preponderance of the smallest blue veins; so that the coarse granules were the result of the weathering of parts of the glacier, composed either exclusively, or for the most part, of blue ice, while those in which the proportion of white ice was larger, weathered less deeply and into finer granules.

The markings of La Brenva, then, are neither ordinary dirt-bands, nor direct expressions of structure, nor direct evidence of stratification, but they are produced by the more ready lodgment of dirt in some parts of the superficial layer of the glacier than in others, in consequence of the more coarse and deep weathering of these parts; which, again, is the result of the predominance of blue ice over white in these localities.

Why blue ice should predominate at intervals in the substance of this glacier,—whether the like alternation of structure holds good in glaciers generally,—and whether it has any relation to a primitive stratification, are problems of great interest and well worthy of investigation.

With regard to the second, I will merely express a belief that some such alternation of structure does obtain in glaciers generally; for the appearances presented by good sectional views of glaciers, such as that exposed on the north side of the Allalein, are so similar to those exhibited by La Brenva, that I cannot doubt the identity of their cause. I had been in the habit of regarding the appearances referred to as direct evidences of stratification; but if my supposition be correct, they will merely be evidences of an alternation of structure which may or may not depend on stratification.

Yours very faithfully,
T. H. Huxley.

XXX. *On certain Elementary Formulæ, &c.* *By* Henry Moseley, *M.A., F.R.S., Canon of Bristol, and Corresponding Member of the Institute of France*[*].

THE development of the value of one of the quantities x, y, z in the equation

$$1+y=(1+z)^x, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

in a series ascending by powers of another of them, yields—if it be x, in a series ascending by powers of y—the fundamental theorem of logarithms; if it be y, in a series ascending by powers of x, the exponential theorem; and if it be y, in a series ascending by powers of z, the binomial theorem. I have endeavoured in the following paper to effect these developments by simpler methods than those commonly in use, and thus to make easier to the student one of the first stages of analytical research.

First, let it be observed that when, in equation (1), x or $z=0$, then $y=0$. To determine x in terms of y, assume

$$x=Z_1y+Z_2y^2+Z_3y^3+Z_4y^4+, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where $Z_1, Z_2 \ldots$ are unknown functions of z. Now when x becomes $2x$ in equation (1), y becomes $2y+y^2$. Substituting these values for x and y in equation (2),

$$2x=Z_1(2y+y^2)+Z_2(2y+y^2)^2+Z_3(2y+y^2)^3+Z_4(2y+y^2)^4+\ldots.$$

But by (2),

$$2x=2Z_1y+2Z_2y^2+2Z_3y^3+2Z_4y^4+\ \ldots$$

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[*] Communicated by the Author.

Therefore (by the method of indeterminate coefficients) equating like powers of y in these equations, we have

$$\begin{array}{l|r|r} Z_1+4Z_2=2Z_2 & 4Z_2+8Z_3=2Z_3 & Z_2+12Z_3+16Z_4=2Z_4 \\ \therefore\ Z_2=-\tfrac{1}{2}Z_1 & \therefore\ 3Z_3=-2Z_2 & 14Z_4=-12Z_3-Z_2 \\ & Z_3=+\tfrac{1}{3}Z_1 & Z_4=-\tfrac{1}{4}Z_1 \end{array}$$

Substituting in equation (2),

$$x=Z_1\{y-\tfrac{1}{2}y^2+\tfrac{1}{3}y^3-\tfrac{1}{4}y^4+\ \ldots.\}.$$

Now when $y=z$, $x=1$ (see equation (1));

$$\therefore\ 1=Z_1\{z-\tfrac{1}{2}z^2+\tfrac{1}{3}z^3-\tfrac{1}{4}z^4\ \ldots.\}$$

$$\therefore\ x=\frac{y-\frac{1}{2}y^2+\frac{1}{3}y^3-\frac{1}{4}y^4+\ \ldots.}{z-\frac{1}{2}z^2+\frac{1}{3}z^3-\frac{1}{4}z^4+\ \ldots.}, \quad .\ .\ .\ . \quad (3)$$

which is the formula for determining the logarithm (x) of a given number $(1+y)$ to a given base $(1+x)$.

Next, to determine the value of y in a series ascending by powers of x, assume

$$y=Z'x+Z''x^2+Z'''x^3+Z''''x^4+\ \ldots.$$

$$\therefore\ 1+y=1+Z'x+Z''x^2+Z'''x^3+Z''''x^4+\ \ldots.\ .\ . \quad (4)$$

Squaring both sides,

$$(1+y)^2=1+2Z'x+(2Z''+Z'^2)x^2+(2Z'''+2Z'Z'')x^3$$
$$+(2Z''''+2Z'Z'''+Z''^2)x^4+\ \ldots.$$

But (by equation 1) x becomes $2x$ when y becomes $(1+y)^2$,

$\therefore$ by equation (4),

$$(1+y)^2=1+2Z'x+4Z''x^2+8Z'''x^3+16Z''''x^4+\ \ldots.$$

Equating the coefficients of like powers of x,

$$\begin{array}{l|l|l} 4Z''=2Z''+Z'^2 & 8Z'''=2Z''''+2Z'Z'' & 16Z''''=2Z''''+2Z'Z'''+Z''^2 \\ Z''=\dfrac{Z'^2}{1.2} & 3Z'''=Z'Z'' & 14Z''''=\dfrac{Z'}{3}+\dfrac{Z''''}{4} \\ & Z'''=\dfrac{Z'^3}{1.2.3} & Z''''=\dfrac{Z'_4}{1.2.3.4} \end{array}$$

Now if the numerator of the second member of equation (3) be represented by Y, and its denominator by Z, and if the resulting value of x be substituted in equation (4), we have

$$1+y=1+\frac{Z'}{Z}Y+\frac{Z''^2}{Z^2}Y^2+\frac{Z'''^3}{Z^3}Y^3+\ \ldots.$$

But Y is a multiple of y. No other term than the second in the above series contains therefore y in the first power only, so that the series may be written under the form

$$1+y=1+\frac{Z'}{Z}y+\text{ terms having powers of } y \text{ above the first.}$$

Equating, therefore, the coefficients of the first power of y in the two members of this equation,

$$\frac{Z'}{Z}=1, \text{ or } Z'=Z=z-\tfrac{1}{2}z^2+\tfrac{1}{3}z^3-\tfrac{1}{4}z^4+\ldots.$$

Substituting in equation (4) this value of Z', and the values before obtained for Z'', Z''', Z'''' ....

$$(1+z)^x=1+\frac{Zx}{1}+\frac{Z^2x^2}{1.2}+\frac{Z^3x^3}{1.2.3}+\frac{Z^4x^4}{1.2.3.4}+\ldots, \quad (5)$$

which is the exponential theorem.

Lastly, to expand y in a series ascending by powers of z, assume

$$y=X_1z+X_2z^2+X_3z^3+X_4z^4+\ldots.$$

$$\therefore\ (1+y)=(1+z)^x=1+X_1z+X_2z^2+X_3z^3+X_4z^4+\ldots. \quad (6)$$

Now in equation (5), let it be observed that Z is a multiple of z, and therefore that all the terms of the second member of that equation after the second, involve powers of z above the first; so that the equation may be written under the form

$$(1+z)^x=1+zx+\text{ terms multiplied by powers of } z \text{ above the first power.}$$

Equating the coefficients of the first power of z in the second member of this equation, and in equation (6), we have therefore $X_1=x$; and substituting this value in (6),

$$1+y=1+xz+X_2z^2+X_3z^3+X_4z^4+\ldots.$$

$$\therefore\ (1+y)^2=1+2xz+(2X_2+x^2)z^2+(2X_3+2xX_2)z^3 + (2X_4+2xX_3+X_2{}^2)z^4+\ldots;$$

but

$$(1+y)^2=\{(1+z)^2\}^x=\{1+(2z+z^2)\}^x,$$

$$\therefore\ (1+y)^2=1+x(2z+z^2)+X_2(2z+z^2)^2+X_3(2z+z^2)^3 +X(2z+z^2)^4+\ldots.$$

Equating coefficients of like powers of z,

$$x+4X_2=2X_2+x^2$$
$$2X_2=x^2-x$$
$$X_2=\frac{x(x-1)}{1.2}$$

$$4X_2+8X_3=2X_3+2X_2x$$
$$3X_3=X_2(x-z)$$
$$X_3=\frac{x(x-1)(x-2)}{1.2.3}$$

$$X_2+12X_3+16X_4=2X_4+2X_3+X_2$$

transposing, substituting, and reducing,

$$X_4=\frac{x(x-1)}{1.2.3.4}\{x^2-5x+6\}$$

$$X_4=\frac{x(x-1)(x-2)(x-3)}{1.2.3.4}.$$

Substituting these values of X_2, X_3, X_4, &c. in equation (6),

$$(1+z)^x=1+xz+\frac{x.(x-1)}{1.2}z^2+\frac{x.(x-1)(x-2)z^3}{1.2.3}$$
$$+\frac{x(x-1)(x-2)(x-3)}{1.2.3.4}z^4+\ldots.$$

which is the binomial theorem.

It will be observed, that whereas by the common method the binomial theorem is first proved, and the exponential theorem deduced from it, and from that again the logarithmic formula, the opposite order of investigation is here pursued; the logarithmic formula being first proved, the exponential theorem deduced from it, and from that the binomial theorem.

Olveston, Bristol,
September 5, 1857.

XXXI. *On the Gases of the Blood.* *By* LOTHAR MEYER, *M.D.*[*]

THE information which we have up to the present time possessed respecting the composition of the gases of the blood, is entirely derived from the valuable experiments which Magnus[†] has instituted upon this subject. The results obtained by this philosopher have been established and extended in the present research, of which the details have already been laid before the medical world in Henlé and Pfeufer's Journal for Rational Medicine[‡].

The object of the following investigation, which was carried out in the laboratory of Prof. Bunsen at Heidelberg, and completed in September 1856, was, in the first place, to determine the quantities of oxygen, nitrogen, and carbonic acid contained in normal, and more particularly in arterial blood; and secondly, to examine how far the absorption of these gases in the blood was dependent upon the well-known law first propounded by Dalton and Henry.

The gases dissolved in the blood were collected, according to Bunsen's method, by ebullition in a vacuous space, as described on pages 15 and 16 of the English edition of his 'Gasometric Methods[§].' In order to avoid the frothing and coagulation which ensue when blood is boiled, it was mixed with from ten to twenty times its own volume of distilled water

[*] Communicated by Henry E. Roscoe, B.A., Ph.D.

[†] Magnus, "Gase des Blutes," Poggendorff's *Annalen*, vols. xl. and lxvi.

[‡] See Henlé and Pfeufer's *Journal für rationelle Medecine*, N. F. vol. viii. part 2.

[§] Bunsen, 'Gasometry.' London: Walton and Maberly. 1857.

previously freed from air, and the ebullition effected at as low a temperature as possible by keeping the vacuous receiver cold. As soon as the free gases had been thus collected, the combined carbonic acid was expelled by allowing a few crystals of tartaric acid to fall into the liquid, and the gas thus liberated was collected by a repetition of the process of ebullition *in vacuo.* The details of the manipulation, together with several special precautions which the peculiar nature of the subject rendered necessary, are given at full length in the original memoir.

Eudiometric analysis gave the following composition of the gas thus collected from 100 volumes of blood, and reduced to 0° C. and $0^{m}\cdot76$ pressure of mercury:—

Date 1856.	Description of blood.	Free gas.	O.	N.	Free CO^2.	Combined CO^2.	Total CO^2.	Total volume of gas.
Jan. 17.	Carotid artery, Dog No. 2.	......	......	......	......	23·75		
Feb. 12.	,, ,, 1.	20·88	12·43	2·83	5·62	28·61	34·23	49·49
,, 19.	,, ,, 2.	......	(3·79)	(2·94)	......	......	(27·10)	(33·84)
,, 19.	,, ,, ,,	28·24	18·42	4·55	5·28	20·97	26·25	49·21
,, 28.	,, ,, 1.	25·50	14·29	5·04	6·17	28·58	34·75	54·08
	Defibrinated blood shaken with air at 21°·5 C. and $0^{m}\cdot7463$ pressure	17·04	11·55	4·40	1·09	18·12	19·21	35·16
		......	(5·81)	(4·12)	......	......	(21·56)	(31·49)

The dog No. 1 was full grown, and weighed 7·5 kilogrammes; that called No. 2 was not full grown, and weighed 9·5 kilogrammes. A distinct difference is observed in the composition of the gases from the blood of these two animals: that of the younger dog contained less combined carbonic acid and far more free oxygen. The gases from the blood of the same animal appear to possess a tolerably constant composition.

The numbers which are enclosed in parentheses represent experiments in which the crystals of tartaric acid were added to the blood before the free gases were removed by ebullition. These determinations gave the same quantity of carbonic acid and nitrogen as those in which the free gases were expelled before the addition of tartaric acid, but in the former, the quantity of oxygen which is expelled by boiling is much smaller, proving that the acid has caused an oxidation of some of the constituents of the blood to occur, without, however, forming carbonic acid.

The absorptiometer employed by Bunsen* is not applicable to experiments on the absorption of gases in the blood. A more convenient instrument for this purpose consists of a cylindrical tube containing the blood, connected with a bent tube in which the gas under examination is placed, and upon which the pressure

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* For description see Bunsen's 'Gasometry,' or Phil. Mag. Feb. 1855.

of mercury can be read off by means of a divided millimetre scale. The instrument, of which a more minute description is given in the cited memoir, may also be employed for the determination of the coefficients of absorption of other liquids which cannot be brought into contact with mercury. Experiments conducted with the view of determining the degree of accuracy attainable with this arrangement gave satisfactory results, and are detailed in the original paper.

For the purpose of determining the coefficient of absorption, I employed defibrinated blood, freed from all dissolved gases by ebullition *in vacuo*, at a temperature of about 30°.

Absorptiometric experiments conducted with the blood thus prepared, showed that with the three gases, carbonic acid, oxygen and nitrogen, the quantity absorbed varied with the pressure to which the gas was subject; and that the variation in the absorbed quantity is proportional to the increase or diminution of the pressure. The total amount of carbonic acid and oxygen taken up by the blood from a pure atmosphere of either of these gases, is, however, not proportional to the absolute pressure under which the absorption occurs. In other words, this total amount of dissolved gas consists of two separate portions (as is the case in a solution of chlorine in water*), one of which is independent of the amount of pressure, whilst the other obeys Dalton and Henry's law of absorption. The volume of gas dissolved by the volume of blood h, under the pressure P and at the temperature t, measured by the unit of pressure at 0° C., is

$$A = kh + \alpha h P\,;$$

in which α represents the coefficient of absorption, and k another constant, also dependent upon the pressure.

In the case of nitrogen, on the other hand, the total quantity of absorbed gas is proportional to the pressure, so that the solution appears to be purely a phænomenon of absorption. As, however, the quantity of nitrogen dissolved in the blood is very small, averaging about from 3 to 4 per cent. of the volume of blood employed, and as the errors of observation may possibly reach as high a limit as 1 per cent., the determination of the values of the absorption-coefficients could not be conducted with any great degree of accuracy according to the method described.

The coefficient α for oxygen is likewise small, so that the amount of this gas absorbed proportionally to the pressure, does not lie much beyond the limit of experimental error. The value obtained for α at a temperature of 18° C. is 0·04 (see Exps. 19 to 30 in the cited memoir).

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* Roscoe, "On the Absorption of Chlorine in Water," Quart. Journ. Chem. Soc. vol. viii. p. 14.

The value of the coefficients of absorption for carbonic acid were, lastly, found to be—

$$\alpha = 1{\cdot}15 \text{ reduced to } 0^\circ \text{ C.*}$$
$$\alpha = 1{\cdot}20 \text{ reduced to } 12^\circ \text{ C.}$$

This value of α is but slightly different from that found by Bunsen for carbonic acid in water, viz. 1·10†.

The cause of a retention of a portion of the oxygen and carbonic acid (which we have designated as kh), independent of the pressure, is to be sought in a special attraction which one or more constituents of the blood exert upon these gases, that is, in the action of chemical forces.

In the case of oxygen, k is much larger than α; so that the solution of oxygen in the blood is almost independent of the pressure exerted on the free gas. The dissolved quantity varies, on the other hand, with several circumstances; as, for instance, when the amount of solid constituents of the blood is reduced. A mixture of water and blood absorbs less oxygen than the blood itself, and it would appear that this diminution is directly proportional to the dilution. The absorbed quantity of oxygen also seems to depend upon the length of time which the blood, on issuing from the artery, remained in contact with the air before the dissolved gases were removed by ebullition in the vacuum. Defibrinated calf's blood, which was brought under the air-pump whilst still warm, gave for 18° C. (Exps. 19–30)

$$k = 0{\cdot}166,$$

reduced to 0° C. and $0^{\text{m}}{\cdot}76$ as unit of pressure. Other experiments gave a smaller value for k,

$$k = 0{\cdot}09.$$

The comparison of these numbers with those obtained by the ebullition of arterial blood, and also of that which had been saturated by agitation with atmospheric air, shows that the quantity of dissolved oxygen independent of the pressure remains the same, whether it be derived from pure oxygen, atmospheric air, or the air contained in the lungs. The presence of nitrogen or carbonic acid does not alter, at all events to a perceptible amount, the attraction exerted upon the oxygen.

The importance of this property of the blood for the living organism is very evident; without this, the residence in atmospheres of various composition, at various heights above the sea, for example, would not be possible without considerable disarrangement of the animal functions. This property explains also the fact observed by Reiset and Regnault, that the vital

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* See Bunsen's definition of absorption-coefficient 'Gasometry,' p. 128; and Phil. Mag. February 1855.

† See Bunsen's 'Gasometry,' p. 152.

actions of animals breathing an atmosphere very rich in oxygen were not observed to be perceptibly accelerated.

I have not attempted in the present research to determine which of the constituents of the blood exerts this attraction on the dissolved gases, nor have I examined whether a compound in definite atomic relations, that is, a chemical combination in the strictest sense, is formed. At any rate the combination is extremely unstable, as it is decomposed when the pressure of the free oxygen is entirely or even partially removed; the blood gives up the whole of its oxygen under the air-pump. The presence of an excess of free oxygen is therefore necessary for the existence of the compound. Many cases analogous to this are familiar to chemists; such, for example, as the loss of carbonic acid which bicarbonate of soda suffers in air free from that gas. If the blood which originally has an alkaline reaction be rendered acid, the unstable compounds become stable, and the greater part of the oxygen cannot now be removed by diminution of the pressure: at the same time the blood changes colour. From this fact it is seen that the oxidizing action of the dissolved gases does not take place in the blood itself, but in the tissues, and especially in the muscles, which have as a rule an acid reaction.

The reaction of the blood with carbonic acid differs essentially from that with oxygen, as a much larger quantity of carbonic acid is taken up from an atmosphere of this gas, independently of the pressure, than is the case with blood brought in contact with the air of the lungs.

Defibrinated calf's blood, which contained 33·8 per-centage volumes of combined carbonic acid, that is, carbonic acid only set free by boiling with excess of acid, measured at 0° and $0^{m}\cdot76$, took up from a pure atmosphere of carbonic acid, 63·0 volumes of this gas (at 12° C.) beyond the quantity previously absorbed, so that at the end of the absorption the blood contained its own volume of carbonic acid dissolved independently of the pressure.

Eudiometric analysis has shown that the quantity of carbonic acid contained in arterial blood is not nearly so large as this, and hence it follows that the venous blood also cannot contain so much of this gas; for it is easy to show, from the known rapidity of circulation of the blood through the lungs, and the quantity of exhaled carbonic acid, that even under the most favourable conditions, the venous blood can only contain a few more per-centage volumes of carbonic acid than the arterial. The total blood circulating throughout the body, therefore, does not contain nearly so much combined carbonic acid as it is able to take up from an atmosphere of pure carbonic acid. It is scarcely possible to explain the absorption otherwise than by the supposition that in the latter case the alkaline bicarbonates are formed partly from the normal- and sesqui-carbonates, and partly also from the alkaline phosphates which are contained in the blood. That these latter salts play a part in the reaction is seen from the fact, that the 33·8 per-centage volumes of combined carbonic acid combine with only 33·8 volumes of additional gas, and leave 29·2 volumes unaccounted for.

It appears from my experiments that little, if any, bicarbonates of the alkalies are present in the circulating blood; as, in the first place, the quantity of carbonic acid which the blood gives up in the vacuum without addition of acid, agrees very closely with the amount which must be absorbed, in the strict sense of the word, at the temperature and under the pressure of carbonic acid contained in the air of the lungs; and secondly, that the blood when boiled, after the separation of this first portion of gas, does not give out any appreciable quantity of carbonic acid, whereas bicarbonate of soda very rapidly gives out half an equivalent of carbonic acid, and continues to lose this gas, so that the solution gradually approaches the composition of the neutral salt. The absence of the bicarbonates in the blood is the more remarkable, as I have shown by absorptiometric experiments (*loc. cit.*) that a dilute solution of normal carbonate of soda takes up from an atmosphere containing but little free carbonic acid, so much gas, besides that properly speaking absorbed, as is necessary to form bicarbonate*. This phænomenon ceases when the amount of free carbonic acid in the gas diminishes to about 1 per cent.

From these experiments, however, it follows that if bicarbonates were once formed in the blood, they would not undergo any alteration in the atmosphere of the lungs. Hence it is evident that it is a mistake to imagine that these salts perform an essential function in the phænomena of respiration.

In opposition to the former views, my experiments show that the exchange of carbonic acid is, with a very great degree of probability, to be regarded simply as a phænomenon of absorption; whereas in the case of the solution of oxygen, chemical forces also come into play.

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* I found in solutions free from air, which contained—

	I.	II.	III.		
	1·041	0·998	0·998	vols. CO^2 at 0° and $0^{m}\cdot76$ in the form of $NaO\,CO^2$.	
at 23°·6 C $k=$	1·087	0·957	0·981	,,	,,
$\alpha=$	0·818	0·831	0·850	,,	,,

The numbers in column III. are calculated from experiments made with a mixture of carbonic acid and hydrogen.

Bunsen found at 22°·4 C. $\alpha=0\cdot864$. Hence it is seen that the presence of a small quantity of a salt in solution does not materially alter the coefficient of absorption.

XXXII. *Chemical Notices from Foreign Journals.*
By E. Atkinson, *Ph.D.*

[Continued from p. 108.]

WURTZ* has published a paper on the constitution and formula of oxalic acid, in which he establishes a relation between that acid and glycol, the diatomic alcohol discovered by himself†. By the action of weak nitric acid on glycol in the cold, glycolic acid is formed; but when glycol is boiled for a few minutes with weak nitric acid, a lively action ensues, red fumes are disengaged, and the liquor on standing some time solidifies to a mass of crystals, which are oxalic acid. Treated with stronger nitric acid, glycol yields besides oxalic acid, carbonic acid, and a certain amount of glycolic acid.

The products of the regular and successive oxidation of glycol are hence two acids, glycolic and oxalic, which stand to each other in the following relation:—

$$\underset{\text{Glycol.}}{\left.\begin{matrix}C^4\,H^4\\ H^2\end{matrix}\right\}O^4}\qquad \underset{\text{Glycolic acid.}}{\left.\begin{matrix}C^4\,H^2\,O^2\\ H^2\end{matrix}\right\}O^4}\qquad \underset{\text{Oxalic acid.}}{\left.\begin{matrix}C^4\,O^4\\ H^2\end{matrix}\right\}O^4.}$$

A similar relation exists between alcohol and acetic acid,

$$\underset{\text{Alcohol.}}{\left.\begin{matrix}C^4\,H^5\\ H\end{matrix}\right\}O^2}\qquad \underset{\text{Acetic acid.}}{\left.\begin{matrix}C^4\,H^3\,O^2\\ H\end{matrix}\right\}O^2.}$$

These acids are formed by the substitution of oxygen for a certain quantity of hydrogen in the radical of the alcohol. The radical of glycol is olefiant gas; when half the hydrogen is replaced by oxygen, glycolic acid is formed; and when the substitution is complete, oxalic acid is the result.

Oxalic acid stands in the same relation to glycol as acetic acid does to alcohol, and oxalic acid may be called the acetic acid of the diatomic alcohol series. Monatomic alcohol requires 4 equivalents of oxygen for its transformation into acetic acid; diatomic glycol requires 8 equivalents to experience the corresponding change,

$$\underset{\text{Glycol.}}{C^4\,H^6\,O^4} + O^8 = 4HO + \underset{\text{Oxalic acid.}}{C^4\,H^2\,O^8}.$$

It appears clear from these experiments that oxalic acid contains 4 equivalents of carbon. Wurtz considers that the other organic acids, such as succinic, suberic, sebacic, &c., which belong to the oxalic acid series, will be found to be derived from the higher glycols, the existence of which he has already demonstrated.

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* *Comptes Rendus*, June 29, 1857.

† Phil. Mag. Supplement, January 1857.

C. Wicke* has made an investigation of chlorobenzole, in the courseof which he has established the existence of a new diatomic alcohol, which occupies the same place in the aromatic acid series that glycol does in the fatty acid series.

Chlorobenzole, $C^{14}H^6Cl^2$, obtained by the action of pentachloride of phosphorus on hydruret of benzoyle (oil of bitter almonds),

$$\begin{array}{ccccccc} C^{14}H^6O^2 & + & PCl^5 & = & PCl^3O^2 & + & C^{14}H^6Cl^2, \\ \text{Hydruret of} & & \text{Pentachloride} & & \text{Oxychloride} & & \text{Chlorobenzole.} \\ \text{benzoyle.} & & \text{of phosphorus.} & & \text{of phosphorus.} & & \end{array}$$

has hitherto been generally regarded as hydruret of benzoyle in which oxygen was replaced by chlorine:

$$\left.\begin{matrix} C^{14}H^5 \\ H \end{matrix}\right\}O^2 \qquad \left.\begin{matrix} C^{14}H^5 \\ H \end{matrix}\right\}Cl^2.$$

Laurent denied that oxygen could replace chlorine, and the investigation was made with the view of ascertaining if this were the case. But Wicke shows that chlorobenzole has none of the characteristics of an aldehyde; and these are generally so pronounced as not to be easily mistaken. It is not acted upon by oxygen; it does not reduce metallic silver from its salts; it does not form a crystalline compound with ammonia, or with bisulphite of ammonia,—all properties of the aldehydes. Nor is it likely that it is a substitution product, for its chlorine is most readily separated as chloride of silver when it is treated with silver solution, while the chlorine in substitution products cannot generally be detected until after the decomposition of the organic substance.

Chlorobenzole belongs to another group of substances; it is the chloride of a biatomic alcohol, just as chloride of æthylene is the chloride of a biatomic alcohol, glycol. The formula of chlorobenzole is hence $\left.\begin{matrix} C^{14}H^6 \\ Cl^2 \end{matrix}\right\}$; that of the alcohol corresponding to it, which Wicke names benzolic alcohol, is $\left.\begin{matrix} C^{14}H^6 \\ H^2 \end{matrix}\right\}O^4$; the formula of chloride of æthylene is $\left.\begin{matrix} C^4H^4 \\ Cl^2 \end{matrix}\right\}$, and of glycol, the alcohol corresponding to it, $\left.\begin{matrix} C^4H^4 \\ H^2 \end{matrix}\right\}O^4$.

Wicke has not yet been able to obtain benzolic alcohol in a pure form; for it has the greatest possible tendency to pass into hydruret of benzoyle, from which it only differs by 2HO. The radical $C^{14}H^6$, or $C^{28}H^{12}$, he has also not been able to obtain: chlorobenzole may be heated with sodium or potassium to a temperature of 206° without the metal losing its lustre in the slight-

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* Liebig's *Annalen*, March and June 1857.

est degree. But evidence of the correctness of Wicke's view has been obtained in the various combinations formed by benzolic alcohol.

The double æthers were obtained by Williamson's method by double decomposition of chlorobenzole and the sodium combinations of the alcohols. *Methylbenzolic æther,* $\left.\begin{matrix} C^{14}\,H^{6} \\ (C^{2}\,H^{3})^{2} \end{matrix}\right\}O^{4}$, is obtained by adding an equivalent of chlorobenzole to 2 equivalents of sodium dissolved in pure and dry wood-spirit, and boiling for some time. The excess of wood-spirit is then distilled off, the residue mixed with water, and the liquid which rises to the surface removed by means of a pipette, dried and distilled. Methylbenzolic æther is a transparent liquid, insoluble in water, and heavier than that liquid; soluble in æther, alcohol and wood-spirit, and boils at 208° C., but not without partial decomposition. It has an odour resembling geranium.

Æthylbenzolic æther, $\left.\begin{matrix} C^{14}\,H^{6} \\ (C^{4}\,H^{5})^{2} \end{matrix}\right\}O^{4}$, was prepared in the same way as methylbenzolic æther, to which it is very similar in properties. This is also the case with amylbenzolic æther, $\left.\begin{matrix} C^{14}\,H^{6} \\ (C^{10}\,H^{11})^{2} \end{matrix}\right\}O^{4}$.

The compound æthers of benzolic alcohol are obtained by the double decomposition of chlorobenzole and the silver salts of the corresponding acids. The acetic benzolic æther has alone been obtained in a pure form; it crystallizes, while the others are thick viscous liquids, and cannot be distilled without decomposition.

Acetic benzolic æther, $\left.\begin{matrix} C^{14}\,H^{6} \\ (C^{4}\,H^{3}\,O^{2})^{2} \end{matrix}\right\}O^{4}$.—Two equivalents of dry acetate of silver are triturated with chlorobenzole, then placed in a flask and gently warmed. A violent action ensues, which results in decomposition if too large quantities are used. The mass is repeatedly extracted with æther, the filtered ætherial extracts distilled off in the water-bath: the residual oil, freed from acetic acid by washing with soda and then with water, is then dissolved in æther and left to crystallize. Acetic benzolic æther occurs in white lustrous crystals, very similar to the swallow-tail gypsum crystals. It melts at 36°, and solidifies to a crystalline mass, and cannot be distilled without decomposition. Treated with potash, it decomposes into hydruret of benzoyle and acetic acid.

Valerianic and benzoic benzolic æthers were also prepared, but they could not be obtained pure; still less could the compound benzolic æthers of the bibasic acids, sulphuric, oxalic, and succinic acids.

In the paper* in which he described the formation of a new series of diatomic alcohols, Wurtz expressed the opinion that the same method would lead to the formation of glycerine. Wurtz† has lately realized that anticipation, and succeeded in forming glycerine by synthesis. A glycerine may be derived from the bromide of a carbohydrogen, $C^n H^{n-1} Br^3$, by substituting for each equivalent of bromine an equivalent of water and an equivalent of oxygen. Such a bromine compound is readily prepared by the action of bromine on iodide of allyle, $C^6 H^5 I$. When bromine is added in small portions to iodide of allyle, an action takes place which must be moderated by placing the vessel in a freezing mixture; iodine is eliminated, and separates out in crystalline form, and 3 equivalents of bromine remain combined with the group $C^6 H^5$.

This terbromide of allyle, when purified, is a colourless heavy liquid, which is, however, generally of a beautiful rose tint, from the presence of a small quantity of iodine. At 10° C. it crystallizes in colourless prisms which melt at 17°; it distils without decomposition at 217°.

Three equivalents of acetate of silver were mixed with 1 equivalent of terbromide of allyle dissolved in crystallizable acetic acid; the mixture was heated for several days to 120° C., and the mass then extracted with æther. The ætherial solution was distilled on the water-bath, and the residue treated with lime and then with æther. The colourless ætherial solution left, after evaporation in the water-bath, a neutral yellowish oil, which after appropriate purification was analysed. It gave numbers corresponding to $C^{18} H^{14} O^{12}$, which is the formula of triacetine, $\left.\begin{matrix} C^6 H^5 \\ 3(C^4 H^3 O^2) \end{matrix}\right\} O^6$, and a portion of this treated with baryta gave the quantity of acetic acid required by that formula.

To obtain the glycerine contained in this triacetine, it was treated with baryta water, the excess of baryta removed, and the liquid filtered and evaporated to dryness. The residue was then extracted with absolute alcohol; the alcoholic solution evaporated on a salt-bath left glycerine, which was distilled *in vacuo*. The liquid which passed over had all the physical and chemical properties of glycerine, and gave on analysis the numbers required by that substance. When treated with iodide of phosphorus, PI^2, it gave iodide of allyle.

The action of chlorides of the negative radicals on sodium and potassium alcohols generally gives rise to compound æthers.

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* *Comptes Rendus*, July 25 and September 1. Phil. Mag. Supplement, January 1857.

† *Comptes Rendus*, April 13, 1857.

By the action of chloride of benzoyle on sodium-alcohol benzoic æther is formed:

$$\underset{\text{Sodium-alcohol.}}{C^4 H^5 NaO^2} + \underset{\text{Chloride of benzoyle.}}{C^{14} H^5 O^2 Cl} = NaCl + \underset{\text{Benzoic æther.}}{C^4 H^5 O, C^{41} H^5 O^3}.$$

The action of chloride of cyanogen on sodium-alcohol might be expected to give cyanate of æthyle. Such, however, is not the case: Cloez* has found that a new base isomeric with cyanate of æthyle is formed. When gaseous chloride of cyanogen is passed into a solution of sodium-alcohol in absolute alcohol, the gas is rapidly absorbed and chloride of sodium is separated. After the action is over, the mixture is distilled on the water-bath, and the residual syrupy liquid is washed with water to free it from admixed chloride of sodium. This liquid has the composition of cyanic æther, $C^4 H^5 O, C^2 NO$, but quite different properties. Cloez names it *Cyanætholine.* Its formation is thus:—

$$\underset{\text{Sodium-alcohol.}}{C^4 \left.\begin{matrix} H^5 \\ Na \end{matrix}\right\} O^2} + \underset{\text{Chloride of cyanogen.}}{C^2 N Cl} = NaCl + \underset{\text{Cyanætholine.}}{C^6 H^5 O^2 N.}$$

Cyanætholine is insoluble in water, but soluble in all proportions in alcohol and æther. It has a bitter taste, and an odour resembling that of sweet oil of wine. It cannot be distilled without decomposition. Concentrated potash decomposes it with formation of ammonia. It forms with most acids crystallizable salts, of which the hydrochlorate unites with bichloride of platinum to form a yellow double salt, and the nitrate with nitrate of silver to form a double salt, which occurs in large crystals.

By this action a series of bases homologous with cyanætholine may be obtained from other alcohols, which would run parallel with the series which includes glycocoll, alanine and leucine.

Schwanert† has investigated some of the decompositions of leucine, and has been led thereby to suggest a new view of its constitution. By the action of high temperatures it decomposes into amylamine and carbonic acid‡. Leucine dissolves in fuming sulphuric acid, and the solution is the more colourless the less the temperature has been allowed to rise; but if heated to 100° C., the solution becomes very brown: on diluting the solution with water, adding carbonate of baryta and then filtering, pure leucine is obtained from the filtrate. Anhydrous sulphuric acid acts on leucine with great energy; the acid is rapidly absorbed,

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* *Comptes Rendus*, March 2, 1857.

† Liebig's *Annalen*, May 1857.

‡ Phil. Mag. July 1857.

and forms a brown, inodorous, thick solution, which, when heated to 100° C., evolves carbonic and sulphurous acid. By mixing this solution with water, and submitting the liquid to distillation, the aldehyde of valerianic acid, $C^{10}H^{10}O^2$, was obtained.

By the action of chlorine on an aqueous solution of leucine, a reddish-yellow oily liquid distils over, which after purification and analysis proved to be valeronitrile, $C^{10}H^9N$, mixed with a small quantity of chlorvaleronitrile, $C^{10}H^8ClN$. A yellow acid liquor remains in the retort, from which crystalline scales deposit: these on recrystallization proved to be a compound of leucine with hydrochloric acid, $2(C^{12}H^{13}NO^4), HCl$. Similar compounds of glycocoll and alanine are known. The products of the action of chlorine on leucine are the same whether water be present or not: the leucine is decomposed into carbonic acid, valeronitrile and hydrogen; the latter forms with chlorine hydrochloric acid, which combines with a portion of undecomposed leucine, while the chlorine acts upon the valeronitrile and forms a substitution product. Bromine has the same action upon leucine. By the action of iodide of æthyle upon leucine no hydrogen is substituted.

These decompositions make it probable that leucine may have the same composition as anthranilic or carbanilic acid. The decomposition by heat is in both cases quite analogous: leucine gives amylamine and carbonic acid; anthranilic acid gives aniline and carbonic acid. Leucine would hence be amylcarbamic acid,—

$$C^{12}H^{13}NO^4 = N(C^{10}H^{11})(C^2O^2)\left.\begin{matrix}H\\H\end{matrix}\right\}O^2 = 2CO^2 + C^{10}H^{13}N$$

Leucine. Amylcarbamic acid. Amylamine.

$$C^{14}H^7NO^4 = N(C^{12}H^5)(C^2O^2)\left.\begin{matrix}H\\H\end{matrix}\right\}O^2 = 2CO^2 + C^{12}H^5N.$$

Anthranilic acid. Carbanilic acid. Aniline.

Leucine and anthranilic acid yield, when treated with nitrous acid, leucic acid and salicylic acid; both are corresponding members of different series. From this point of view Schwanert hoped to obtain the sulphamylaminic acid corresponding to sulphanilic acid, by treating leucine with sulphuric acid: this expectation was not fulfilled; a further decomposition appeared to set in. But the products of decomposition, valeraldehyde, carbonic and sulphuric acids and ammonia, may, however, be deduced from that formula without forcing the interpretation.

Kubel* has found a support for this view of the constitution

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* Liebig's *Annalen*, May 1857.

of leucine in the fact, that anthranilic acid forms, like leucine, compounds with acids; of these he has investigated the compounds with nitric, sulphuric, hydrochloric, and oxalic acids. They are well-marked, definitely crystallizable bodies.

Cahours*, in a communication on benzamic acid and its derivatives, points out the analogies which exist between the series to which benzamic acid belongs, and the members of the glycocoll series. While Schwanert shows that leucine has a similar constitution to carbanilic acid, Cahours views leucine as an alkaloid, and shows that benzamic acid has the properties of a true alkaloid. Benzamic acid, and its homologues, toluamic, cuminamic, anisamic acids, form compounds with phosphoric, oxalic, hydrobromic, and hydrochloric acids, which crystallize most readily, and of which the hydrochlorate forms with bichloride of platinum definitely crystalline compounds,—

$C^{14}H^7NO^4, HCl$, hydrochlorate of benzamic acid.
$C^{14}H^7NO^4, HCl, PtCl^2$, platinum compound of hydrochlorate of benzamic acid.

Glycocoll also forms with hydrochloric acid and bichloride of platinum, a compound crystallizing in brilliant orange prisms. Just as glycocoll has isomers, so benzamic acid has isomers in anthranilic acid and salicylamide. Benzamic acid has the same relation to salicylamide as glycocoll to glycocolamide, or alanine to lactamide. But salicylic acid, in becoming salicylamide, gains NH and loses O^2; while benzoic acid, in being converted into benzamic acid, simply gains NH. The position of the atoms is therefore different; as glycocolamide is produced, like salicylamide, by the reduction of its corresponding acid by ammonia, and benzamic acid by reduction of nitrobenzoic acid, might we not hope to obtain glycocoll by the reduction of nitroacetic acid?

$$\underbrace{C^{14}H^5(NO^4)O^4}_{\text{Nitrobenzoic acid.}} + 6SH = 6S + 4HO + \underbrace{C^{14}H^7NO^4}_{\text{Benzamic acid.}}$$

$$\underbrace{C^4H^3(NO^4)O^4}_{\text{Nitroacetic acid.}} + 6SH = 6S + 4HO + \underbrace{C^4H^5NO^4}_{\text{Glycocoll.}}.$$

By the action of chloride of benzoyle on glycocoll, hippuric acid is formed: Cahours has found, that by the action of chlorides of cumyle and anisyle on silver-glycocoll, new acids, cuminuric and anisuric acids, are obtained. The equation would be—

$$\underbrace{C^{20}H^{11}O^2, Cl}_{\text{Chloride of cumyle.}} + \underbrace{C^4H^4AgNO^4}_{\text{Silver-glycocol.}} = AgCl + \underbrace{C^{24}H^{15}NO^6}_{\text{Cuminuric acid.}}.$$

He has also found, that, by acting on benzamate of silver with chloride of benzoyle, a new acid is formed.

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* *Comptes Rendus*, March 16.

Between these acids and glycocol, leucine, &c., there are strong analogies, although we are not exactly able to state what is their rational constitution. It would be interesting to try whether cyanætholine* could be transformed into alanine by the simple fixation of water. The mode of production of both these substances has great analogies.

$$\underset{\text{Aldehyde.}}{C^4H^4O^2} + \underset{\text{Hydrocyanic acid.}}{C^2NH} + 2HO = \underset{\text{Alanine.}}{C^6H^7NO^4}$$

$$C^4H^5NaO^2 + C^2NCl = \underset{\text{Cyanætholine.}}{C^6H^5NO^2.}$$

In an investigation of phloretine, Hlasiwetz† had assigned to this substance the formula $C^{30}H^{15}O^{10}$, and considered it as composed of monobasic phloretic acid, $C^{18}H^{11}O^6$, and phloroglucine, $C^{12}H^4O^6$. This view he has now had reason to alter‡: phloretic acid is bibasic, and has the formula $C^{18}H^{10}O^6$; and instead of belonging to the group of lichen acids, belongs to the salicylic acid group, and is homologous with that acid. The salts of phloretic acid which he had previously described were acid salts, and he has now prepared and described a series of neutral salts which are obtained in the same way as the corresponding neutral salicylates.

Phloretate of æthyle, $\left.\begin{array}{l}C^{18}H^9\\C^4\,H^5\end{array}\right\}O^6$, is obtained by the action of iodide of æthyle on phloretate of silver or potash. It is a colourless viscous liquid, whose boiling-point is higher than 265°, has a weak odour and an irritating taste. Its optical properties were compared with those of salicylic æther prepared by the same method. Both substances have exactly the same refrangibility for one ray of the orange, but in general the dispersion of salicylic æther is greater than that of phloretic æther. By the action of nitric acid on this compound, *binitrophloretic æther,* $\left.\begin{array}{r}C^{18}H^7(NO^4)^2O^5\\C^4H^5\;O\end{array}\right\}$, is produced.

Binitrophloretic acid, $\left.\begin{array}{r}C^{18}H^7(NO^4)^2O^5\\H\quad O\end{array}\right\}$, is formed by the action of nitric acid on phloretic acid, and occurs in two modifications, according to the degree of concentration of the nitric acid used in its preparation. When concentrated acid is used, violent action ensues, nitrous fumes are evolved, and the acid dissolves up to a red liquid, which on cooling becomes filled with yellow granular crystals. On recrystallization they are

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* *Vide* ante, p. 273.

† Phil. Mag. March 1856.

‡ Liebig's *Annalen,* May 1857.

obtained in prisms. This acid forms salts with bases which are mostly crystalline, and have a yellow colour. The other modification is prepared by the gradual addition of nitric acid to an aqueous solution of phloretic acid; it has, when pure, the same degree of solubility as the other modification, and the same composition, but crystallizes in dark, golden-yellow plates and scales of the greatest beauty. There seems to exist between these two modifications the same relations as between nitrosalicylic and anilotinic acids[*]. All attempts to prepare a mononitrophloretic acid were ineffectual.

Bibromphloretic acid, $\left.\begin{matrix} C^{18}H^{7}Br^{2}O^{5} \\ H\ \ O \end{matrix}\right\}$, prepared by the action of bromine on phloretic acid, crystallizes in colourless, hard prisms, and forms salts with ammonia and baryta.

Phloretylaminic acid, $C^{18}H^{11}NO^{4}$, produced by the action of dry ammonia on phloretic æther, is homologous with salicylaminic acid, the body hitherto generally considered as salicylamide. Its acid character is not very decided; it does not decompose carbonates, but appears to form compounds with alkalies.

Chloride of phloretyle, produced by the action of pentachloride of phosphorus on phloretic acid, has the formula $C^{18}H^{8}O^{2}, Cl^{2}$.

Salicylate of baryta gives on destructive distillation, carbonate of baryta, and an oil which is phenylic alcohol, $C^{12}H^{6}O^{2}$. By treating phloretate of baryta mixed with caustic lime in the same manner, an oily brownish distillate is obtained, which when purified is colourless, strongly refracting, has an aromatic odour somewhat resembling phenylic alcohol, and a burning taste; coagulates albumen almost as rapidly as phenylic alcohol; forms with sulphuric acid a copulated acid, and with bromine and chlorine substitution products. Its boiling-point is about 220°. Its formula is $C^{16}H^{10}O^{2}$, and it forms with nitric acid a substitution product occurring in yellow crystals, which has the formula $C^{16}\begin{matrix} H^{7} \\ (NO^{4})^{3} \end{matrix}O^{2}$, and would thus be homologous with nitropicric acid. The body $C^{16}H^{10}O^{2}$ is evidently, from its properties, mode of formation and formula, the homologue of hydrated oxide of phenyle, and would be xylenyle alcohol, the alcohol of the xylole[†] series, whose theoretical boiling-point would be 225° C.

By the action of dry sulphurous acid on pentachloride of phosphorus, a clear, strongly refracting liquid is obtained, which has the formula $PCl^{5}S^{2}O^{4}$, and has been considered to be sul-

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[*] Phil. Mag. June 1856.

[†] Ibid.

phite of pentachloride of phosphorus. Schiff[*] has found, that on submitting this liquid to fractional distillation, it can be separated into oxychloride of phosphorus, and into a liquid boiling at 182° C., which has an extraordinary refrangibility, decomposes with water into sulphurous and hydrochloric acids, and with alcohol into hydrochloric and æthylosulphurous acids.

This body has the formula $\mathrm{S^2\,O^2\,Cl^2}$, and is formed according to the equation $\mathrm{PCl^5+2SO^2=PO^2\,Cl^3+S^2\,O^2\,Cl^2}$. If sulphurous acid and its derivatives be considered to be the compounds of a bibasic radical, $\mathrm{S^2\,O^2}$, which might be called *thionyle*, gaseous sulphurous acid would have the formula $\left.\begin{matrix}\mathrm{S^2\,O^2}\\ \mathrm{O^2}\end{matrix}\right\}$, and the above compound would be the chloride of this radical, $\left.\begin{matrix}\mathrm{S^2\,O^2}\\ \mathrm{Cl^2}\end{matrix}\right\}$, or chloride of thionyle. When treated with ammonia, violent action ensues, which must be moderated, and the result is the formation of a white uncrystalline product, which consists of a mixture of sal-ammoniac and the amide of this radical, *thionylamide*,—

$$\underset{\text{Chloride of thionyle.}}{\left.\begin{matrix}\mathrm{S^2\,O^2}\\ \mathrm{Cl^2}\end{matrix}\right\}}+\mathrm{4NH^3}=\underset{\text{Thionylamide.}}{\mathrm{N^2}\left\{\begin{matrix}\mathrm{S^2\,O^2}\\ \mathrm{H^2}\\ \mathrm{H^2}\end{matrix}\right.}+\mathrm{2NH^4\,Cl}.$$

The action of anhydrous sulphuric acid on pentachloride of phosphorus is quite analogous to that of sulphurous acid. A body, $\mathrm{PCl^5\,S^2\,O^6}$, is formed which is a mixture of oxychloride of phosphorus, and $\mathrm{S^2\,O^4\,Cl^2}$, or chlorosulphuric acid. Assuming the existence of a compound radical, $\mathrm{S^2\,O^4}$, *sulfuryle*, in sulphuric acid, anhydrous sulphuric acid would be $\left.\begin{matrix}\mathrm{S^2\,O^4}\\ \mathrm{O^2}\end{matrix}\right\}$; and this body, $\left.\begin{matrix}\mathrm{S^2\,O^4}\\ \mathrm{Cl^2}\end{matrix}\right\}$, chloride of sulfuryle. Schiff found that many substances resulting from the action of perchloride of phosphorus on mineral acids, as tungstic, molybdic, antimonic, &c., are not true compounds, but mixtures of pentachloride of phosphorus, and of the chlorides of the acids in question.

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[*] Liebig's *Annalen*, April 1857.

XXXIII. *On the Deviation from the Primary Laws of Elastic Fluids indicated by the experiments of* M. Regnault *and of* Messrs. Thomson *and* Joule. *By* J. J. Waterston, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

§ 1. THE results of Messrs. Thomson and Joule's experiments on the thermal effects of fluids in motion, are stated by the authors to prove that air and carbonic acid, on being compressed, evolve more heat than the amount mechanically equivalent to the work of compression (Phil. Trans. 1854, p. 341). This announcement being calculated to weaken our confidence in the principle of constancy in the mechanical equivalent of heat, I was induced to study closely the connexion between these experiments and those of M. Regnault, so as to obtain a clear idea of the modifying influence of a deviation from the law of Mariotte in experiments that involve the conversion of heat into work, and reconversion of work into heat.

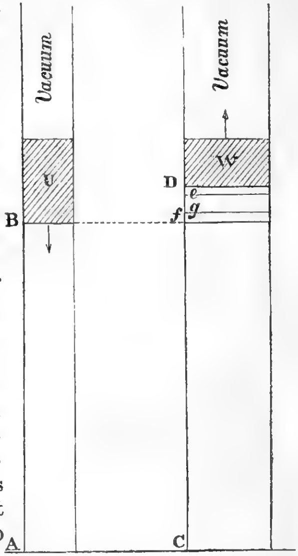
§ 2. It is first necessary to keep in view the exact extent of the information afforded by M. Regnault's researches, and the nature of the deviation indicated by them. He has determined the value of the difference of volumes under constant pressure, and the difference of pressures with constant volume, corresponding to the difference of temperature between the freezing- and boiling-point of water. He has done this for several gases, and for the same gas at different pressures, and found all to differ from each other, which they would not do if the primary laws were exactly maintained. But M. Regnault *has not determined the change of absolute volume between any two different pressures*: thus the absolute amount of deviation from the law of Mariotte between any two pressures has not been ascertained. We have also to keep in view, that *the influence of temperature* as affecting the amount of deviation has not been ascertained by M. Regnault. All his observations were made on differences of volume and of pressure between 0° and 100° C.; they tell us nothing of similar differences taken between 10° and 110°, or between 50° and 150°, &c.

§ 3. On examining the *rationale* of Messrs. Thomson and Joule's experiments with plugs, I find that by a simple process of computation we may deduce from them the two important items of information wanting in M. Regnault's researches.

The details of the process are as follows:—

The temperature of the experiment being, say 10° C.; the difference of pressure from two to one atmosphere; and the cooling effect in passing through the plug 0°·26 C.,—let C D, fig. 1, represent a cylinder of air at the pressure of one atmosphere and temperature 10° C. Let the elastic force of the air in this cylinder be wholly maintained by the counterbalancing weight of the piston W, so that we have to suppose a perfect vacuum to exist on the upper side of it. By placing a small additional weight *w* on W, suppose it to descend to *g*, so that the consequent rise of temperature in the air contained in the cylinder may be 0°·26 C. This increment of heat being withdrawn, the piston W descends from *g* to *f*. We may now suppose *w* withdrawn from W, which consequently rises from *f* to *e* by the elastic force of the air in the cylinder. There is a consequent loss of heat, and the temperature of the contained air sinks 0°·26. This decrement of heat being restored, the piston rises to D, its original position.

Fig. 1.



The ratio of *fg*, or its equal D*e*, to D C is nearly the same as that of 0°·26 to 273°+10°, or of the increment of temperature to the G temperature, or temperature reckoned from the zero of gaseous tension. The ratio of D*e* to D*f* is nearly 1 to 4, as determined by the experiments of MM. Clement and Desormes, also of MM. Gay-Lussac and Welter (*Mécanique Céleste,* book 12). The ratio of D*f* to DC is thus nearly 1·04 to 283, or 1 to 272; and this is the amount of deviation from the law of Mariotte in air at 10° C. between the pressure of two and one atmospheres, as derived from Messrs. Thomson and Joule's experiments on thermal effect.

The above result may be exhibited in diagram by A B, another cylinder resting on the same plane as C D, and having one-half the transverse area. The weight of the piston U being equal to W, and the weight of air in each cylinder being the same, as well as the temperature, we have AB$=f$C equal to $\frac{271}{272}$ of CD.

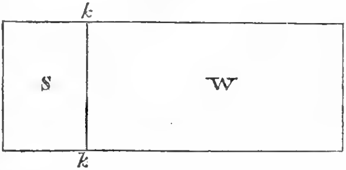
§ 4. This computation, expressed by symbols, is

$$\Delta=\frac{n\theta}{273+t};$$

in which Δ represents the deviation,
t the temperature on Centigrade scale,
θ the observed cooling effect,
and n the ratio $\frac{\mathrm{D}f}{\mathrm{D}e}$ (which is 3·78, taking the mean of the experiments from the *Mécanique Céleste*).

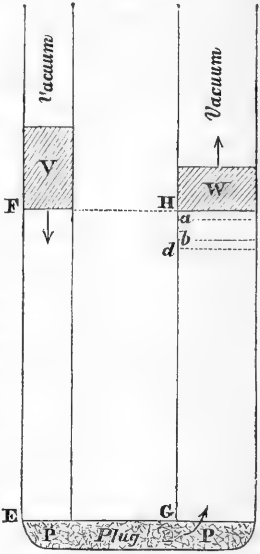
§ 5. In the demonstration of this process, it is assumed as an axiom that air, in passing from a higher to a lower state of density *without performing work*, does not gain or lose heat upon the whole. (This is called Mayer's hypothesis by Prof. Thomson.) Thus suppose S W, fig. 2, to represent a cylinder impervious to heat, and that it is divided by a partition $k\,k$. On one side of this (S) let there be air, and on the other (W) a perfect vacuum. If we now imagine $k\,k$ to be instantaneously withdrawn, the air in S rushes violently into W. At first, great motion in the direction S W is generated, and a corresponding lowering of temperature: then the motion is reconverted into heat. After the action has subsided, the resulting temperature of the expanded air is assumed to be the same as before, and this whether or not there was a vacuum originally in W. Mr. Joule has put this to the test of experiment (see Phil. Mag. May 1845, vol. xxvi. p. 369), and found no sensible deviation from it. (Note A, Appendix.)

Fig. 2.



If the resulting temperature were lower than before, then heat has disappeared without performing any *apparent* work; but there may be conversion of heat into work not apparent to the senses, into work concealed in the molecules of air, or in their physical habitudes with the higher agents of force; and *vice versâ*, the resulting temperature may be higher; heat appearing, without apparently the equivalent work being converted, the separation of the molecules being the antecedent of the phænomenon of absorption or evolution, as the case may be. To determine this is a fair subject for experiment; and if the deviation from the law of Mariotte was known by direct observation to the degree of accuracy required, we should, by comparing it with the amount assigned to the deviation by the experiments with plugs, have the means of testing the point in question. If the results differed sensibly, we *then* would have direct proof that air, on being compressed or dilated, did not alter its temperature in correspondence with the mechanical equivalent of the apparent work. Assuming, therefore, that there is no recondite evolution or absorption of heat in consequence of the molecules of gases changing their mutual distance, the following is the demonstration of the above process for computing the deviation by means of the thermal effect of Messrs. Thomson and Joule's experiments.

Fig. 3.



§ 6. Let the two upright cylinders G H and E F, fig. 3, of unequal diameters, be connected at P P by a plug of compressed cotton, as in Messrs. Thomson and Joule's experiments. Let the weight of the piston H W be first considered as equal to F V, and above them a perfect vacuum, so that the elastic force of the air in each cylinder may be maintained by the superincumbent weight of the pistons only. We have also to assume that the motion of each piston has attained uniformity, *i. e.* F V descends, and H W ascends with uniform velocity. The conversion of heat into motion, and of motion into heat, that took place through the whole space S W, fig. 2, now takes place insensibly within the pores of P. The space in which the alternate conversion and reconversion take place, instead of being, as in S W, one and of very sensible magnitude, now consists of an infinite number of indefinitely minute receptacles, in each of which the same kind of reciprocal action may be supposed to be going forward. If the law of Mariotte were perfectly maintained, the obvious result would be a rise of W equal to the descent of the same weight V, and the air changed from the higher density to the lower without either the performance of work or change of temperature.

If, in condensing air at a constant temperature, its elastic force increases in somewhat a less proportion than its density (which we can infer to be the case from M. Regnault's experiments, although not the absolute extent of the divergence between any two given densities), the weight of the piston V must be taken as somewhat less than that of W, to maintain the densities in the inverse ratio of the square diameter of the cylinders. In consequence of this diminution (say of $\frac{1}{100}$dth) of the weight of V, the work performed by it in descending (F E) a unit of height is less than the work required to raise W through (G H) a unit of height. It is thus obvious that the equivalent work would be completely transferred to W in raising it through $(\mathrm{G}d)=\frac{99}{100}$dths of the unit. But the density then, instead of being of the normal amount corresponding to the ratio that subsists between the weight W and the transverse area of its cylinder G H, would exceed that by $\frac{1}{99}$th: hence to arrive at an equilibrium, the ascent of W would be continued a little further beyond the $\frac{99}{100}$dth (viz. to a), and the work required to effect this must be derived from the conversion of some of the heat of the air contained in the $\frac{99}{100}$dth of the unit of the height of the cylinder GH. A lowering of its temperature is the consequence, and equilibrium is attained before W has reached fully to the top of dH, the last $\frac{1}{100}$dth of the unit.

The proportion da to dH of the $\frac{1}{100}$dth through which W is driven has been determined by the experiments above referred to, as detailed in the twelfth book of the *Mécanique Céleste.* It is nearly three-fourths; and it is such, that if the small amount of heat lost were exactly replaced from external sources, W would be carried through aH, the remaining fraction of the $\frac{1}{100}$dth. It will be remarked, that the ratio of da to dH is the well-known ratio of the increments of heat required for the same increment of temperature with volume constant and with pressure constant.

In the above demonstration we must keep in view, that, to facilitate our reasoning, it is allowable to assume the plug to extend from the last or lower position of V to the first or lower position of W; also that the unit of height may represent an incremental quantity, and thus the phænomena which, in order to fix our ideas, we consider as taking place in consecutive order, may be simultaneous.

§ 7. Professor Thomson assumes that the cooling effect in his experiments with plugs is proof that the gas, on being compressed, evolves more heat than the amount mechanically equivalent to the work of compression; that, in short, it does not alter its temperature in correspondence with the mechanical equivalent of the apparent work. It has not occurred to him that the depression of temperature may simply be caused by the conversion of heat into the mechanical force or work required to acquire against the atmospheric pressure that small augmentation of volume which a deviation from Mariotte's law demands. It is surely proper to take this first into consideration, yet the necessity of doing so seems never to have suggested itself. The experiments were originally proposed as a means of testing Mayer's hypothesis only* (see Phil. Mag. vol. iv. pp. 432, 433, §§ 73, 78), and up to the present time this is supposed to have been successfully accomplished (see Phil. Trans. 1854*).

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* See Appendix (1) (2).

§ 8. The formula, § 4, $\Delta=\frac{n\theta}{273+t}$, expresses the law of the deviation, viz. that for a constant difference of density it varies directly as the amount of the thermal depression or cooling effect, and inversely as the temperature of the experiment reckoned from the zero of gaseous tension. In the experiments of Messrs. Thomson and Joule with carbonic acid, the cooling effect was about four and a half times that of air, giving a deviation of $\frac{1}{60}$th for a difference of density equal to one atmosphere.

If the deviation at 100° were the same in absolute amount as at 0°, the cooling effect ought to be greater in the ratio of 373° to 273°. But if the deviation were less in proportion with the diminished density, the cooling effect ought to be unchanged.

If the cooling effect diminish as the temperature increases, the deviation must diminish for the same constant difference of pressure, not only with the density, but also in some inverse ratio of the temperature. This last seems to be the case, judging from Messrs. Thomson and Joule's experiments with carbonic acid.

§ 9. M. Regnault's observations provide us with the coefficients of expansion under constant pressure, and of augmentation of pressure with constant volume, all at the temperature of 50° C. or $273+50=323°$ G temperature, or temperature reckoned from the zero of gaseous tension. If we denote this G temperature by T, the volume by V, and tension or elastic pressure by P, the primary laws of gases in their entirety are expressed by $T=VP$, the equation of the hyperbola referred to its asymptote, in which we set out with a unit of each of the elements as a basis of comparison with standard measures.

From M. Regnault's experiments alone, we may infer that the actual coordinates of volume and pressure do not trace out this curve exactly, because his coefficients increase with the density; but the amount of the deviation cannot be inferred, because only one value of V for the same value of T has been supplied.

The actual coordinates of volume and pressure may nevertheless trace out the exact hyperbola, if they are assumed not to have reference to the actual asymptotes, but to a subordinate unknown hyperbolic curve lying near to and belonging to the same asymptotes, and that expresses by its equation the unknown cause of the deviation. Upon this assumption we obtain the means of computing the deviation by the coefficients alone. In this way the deviation comes out $\frac{1}{210}$. This value, compared with $\frac{1}{272}$, the value computed by the first process, does not show such accordance as to establish the hypothesis, but sufficient perhaps to make it worthy of passing remark.

Appendix.

(1) § 7 (Phil. Mag. vol. iv. p. 432, § 77, Prof. Thomson on the Dynamical Theory of Heat) : the following extracts prove this statement :—

"From equation III. it follows, that if Mayer's hypothesis be true, there is neither emission nor absorption of heat, on the whole, required to reduce the temperature of the air after passing through the orifice to its primitive value. . . . . . . Hence *the simplest conceivable test of Mayer's hypothesis* would be, to try whether the temperature of the air is exactly the same on the two sides of the orifice.

P. 433, § 78. "Should the differential method of experimenting just described indicate *any difference of temperature whatever* on the two sides of the orifice, Mayer's hypothesis would be shown to be not exactly fulfilled, and, according as the air leaving the orifice is found to be warmer or colder than the entering air, we should infer that the heat absorbed, when air expands at a constant temperature, is less than or greater than the equivalent of the mechanical effect produced by the expansion."

(2) Note A referred to in § 5. In Phil. Mag. vol. iv. p. 429, § 71, Prof. Thomson thus refers to those experiments of Mr. Joule :—

"In Joule's actual experiments, the test is simply this :—the total external thermal effect is determined when air is allowed to expand, through a small orifice, from one vessel to another previously exhausted by an air-pump. Here the first mechanical effect produced by the expanding gas is *vis viva* generated in the rushing of the air. By the time equilibrium is established, *all this mechanical effect has been lost in fluid friction* ; and no truth in physical science can be more certain, than that by the time thermal as well as mechanical equilibrium is established at the primitive temperature, the contents of the two vessels must have parted with just as much *more* heat than they would have parted with had the air in expanding pushed out the piston against an external resisting force, as is equivalent to the mechanical effect thus produced externally." How are we to reconcile this with the fact, that the contents of the two vessels were found not to have parted with any heat ? Perhaps the word *more* is a misprint, and should have been less. Prof. Thomson frequently makes use of the word friction. His view appears to be, that the motion of the air rushing *e. g.* from S to W, fig. 2, is lost in "fluid friction ;" and then the "heat of friction" restores the temperature to nearly what it was before, but not quite, the difference being what is shown in the experiments with plugs. Thus in the second paper on Thermal Effects (Phil. Trans. 1854, p. 339), he expresses himself as follows :—The thermal effect "shows *precisely* how much the heat of friction in the plug falls short of compensating the cold of expansion. But *the heat of friction* is the thermal equivalent of all *the work done* actually in the narrow passages by the air expanding as it flows through." Thus the plug does not allow the air to pass from a higher to a lower density without performing work. Again, a little further on, Prof. Thomson thus writes :—"*In any case,* w *deno-*

ting the whole work of expansion," &c. The value given to w is found at p. 341, viz. $w=\mathrm{PV}\log\frac{\mathrm{P}_{\prime}}{\mathrm{P}}$, the hyperbolic area of expansion at constant temperature. Prof. Thomson assumes that the work represented by this area must inevitably be done when air passes from one state of density to another at constant temperature. If not by "*pushing out a piston against external resisting force,*" then by generating "*heat of friction,*" "*stirring its own mass.*" P. 338.

(3) The dynamical theory of heat rests on the idea that heat is the motion of the elementary parts of bodies,—intestine *vis viva,* accompanied with apparent quiescence, because the motion exists in every direction alike, *e. g.* through every part of the air contained in S (fig. 2). By the removal of the partition $k\,k$, the intestine action in the direction SW has now scope to make its appearance; and when the particles of air have filled the whole of S W, the apparent motion in one direction relapses into intestine motion in all directions alike. This theory of heat involves no necessity for introducing the idea of friction. So far from fixing our ideas or facilitating our reasoning, its manifest tendency is to obscure and confuse. As an illustration, we may take the case of a rigid vessel impervious to heat, filled with air and moving at a high velocity in a straight line. Suppose it to be stopped instantaneously, and without fracture or any change in the vessel. At this instant the motion of translation of the air would necessarily be converted into heat, and there would be a rise of its temperature. Now it is impossible in such a case to introduce the idea of friction, but it is easy to conceive the apparent motion of the particles in the one direction changed into motion in all directions alike by simple impact with the sides of the vessel and with each other. In the arrangements for making the experiments with plugs, all work by the expansion of the air in passing through the plug seems to have been carefully prevented. If the air acted disruptively on the fibres of the cotton, then work would be performed. Also the extra velocity of the current of air in passing out of the plug would cause a slight effect equivalent at a maximum to about $\frac{1}{100}$dth of a degree. Even this might be prevented by having the plug shaped like the frustum of a cone, and thus equalize the velocity of the current on both sides. It may also be remarked, that as the cooling effect is a quantity of the differential kind, the differential thermometer seems to be the proper instrument to measure it by.

(4) There appears to have been considerable difficulty in equalizing the pressure of the air entering the plug; might not this be attained with greater facility by interposing between the condensing pump and plug a cylinder with a heavy piston, with its weight adjusted to the pressure required? The action would be similar in principle to the bellows of an organ. The induction and eduction pipes might be at the bottom of the cylinder, and the motion of the piston would probably be confined to so narrow limits as to enable it to act sufficiently as a regulator.

(5) In the second paper on Thermal Effects, at p. 336 a small Table of results appears, the last column of which is headed "*Theo-*

retical cooling effect for 100 *lbs. pressure*." On comparing this with the adjacent column of experimental results, we find such complete accordance that apparently Prof. Thomson's calculus has enabled him to predict in a satisfactory manner the results of observation. This is the test of a successful theory. On inquiring further what this theory is, we come to what Prof. Thomson calls an "*empirical formula*" (that is, a formula derived from experiment), communicated by Mr. Rankine, who states that its constant coefficient a "has been determined *solely* from Regnault's experiments on the increase of pressure at constant volume between 0° and 100° C." Mr. Rankine further states, that "it gives most satisfactory results for expansion at constant pressure, compression at constant temperature, and also (I think) for cooling by free expansion [*i. e.* the cooling effect in our (Messrs. Thomson and Joule's experiments)].

The formula is $$\frac{PV}{P_0V_0}=\frac{T+C}{C}-\frac{a}{T+C}\cdot\frac{V_0}{V}.$$

No explanation is given as to the method of determining a, nor is there any proof of it giving satisfactory results for compression at constant temperature, or how any results of this kind are possibly derived from M. Regnault's experiments.

The same remark applies to the formula $\frac{P'V'-PV}{PV}=f\frac{P-P'}{\Pi}$, given by Prof. Thomson at p. 340. No explanation is given of how f has been determined "from the results derived by Regnault from his experiments on the *compressibility* of air, of carbonic acid, and of hydrogen." Prof. Thomson must be sensible that upon these the proof of the soundness of his "theoretical deductions" mainly depends.

The value of f for air is ·00082 (p. 340), and the formula expresses that the deviation from the law of Mariotte (or of Boyle) for a difference of pressure equal to one atmosphere is ·00082 or $\frac{1}{1220}$.

How this has been deduced from M. Regnault's observations is not explained; but both formulæ cannot be right, inasmuch as *they contradict each other*. The deviation may be computed from either, and they give different results.

If, in Mr. Rankine's formula, we put $T=0$, $V=2V_0$, we have

$$\frac{2P}{P_0}=1-\frac{1\cdot9}{274\times2}\text{ and }P=\tfrac{1}{2}P_0\left(1-\frac{1}{288}\right).$$

Hence the deviation for carbonic acid is $\frac{1}{288}$. By the value of f given by Prof. Thomson at p. 340, it is ·0064 or $\frac{1}{156}$.

If we test the formula of Mr. Rankine by taking $T=0$, we have

$$PV=P_0V_0\left\{1-\frac{1\cdot9}{274}\frac{V_0}{V}\right\};$$

and when V approaches the value of V_0, we have

$$P=P_0\left\{1-\frac{1\cdot9}{274}\right\}.$$

Let PV represent 17145, the value given (p. 336) for the actual product of volume and pressure at 0° C., or when T=0, then

$$\frac{PV}{P_0V_0}=\frac{17145}{17116}=1+\frac{1}{590}=\frac{T+C}{C}-\frac{a}{T+C}\left(\frac{V_0}{V}\right)=1-\frac{1\cdot9}{274}\left(1-\frac{1}{590}\right).$$

Thus we have the arithmetical absurdity of

$$1+\frac{1}{590}=1-\frac{1\cdot9}{274}\left(1-\frac{1}{590}\right)=1-\frac{1\cdot9}{274}.$$

(6) Since M. Regnault's experiments on carbonic acid have afforded Mr. Rankine the means of determining the value of the constant *a*, and thus enabled Prof. Thomson to predict by his calculus the thermal effect in passing that gas through the plug (see Table, p. 336), we may ask, why has the same process not afforded the means of computing the results of the experiments on air? Some explanation of this may fairly be expected from Prof. Thomson.

(7) Table XII. p. 335, contains a series of experiments on air and carbonic acid at the temperature of about 90° C. The first part of this table compared with the last, shows such discrepancy, that it is impossible to accept the results, either in the case of air or carbonic acid, as established thereby. In the first part, the thermal effect in air would appear to be augmented by the high temperature; in the last, to be diminished. Prof. Thomson merely remarks that the lesser number is probably the more correct value, and thus that the thermal effect is lessened. The calculus that in Prof. Thomson's hands has so exactly anticipated results in the case of carbonic acid, appears to be of no avail in the case of air, although the experimental data to work upon are more abundant.

J. J. Waterston.

22 London Street, Edinburgh,
August 25, 1857.

XXXIV. *On the Occurrence of Indigo-blue in Urine.* *By* Edward Schunck, *Ph.D., F.R.S.**

THE occurrence of urine exhibiting various peculiar and abnormal colours is a phænomenon which has frequently attracted the attention and excited the curiosity of pathologists. Of these variously tinted urines the most remarkable and striking are the black and the blue, but they are at the same time so rare, that it has been deemed of importance to record minutely the symptoms exhibited in each case as well as the chemical and physical properties shown by the urine itself.

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* Communicated by the author from the Memoirs of the Literary and Philosophical Society of Manchester, vol. xiv. p. 239; having been read April 7, 1857.

These urines have been observed in diseases of the most different kinds, as well as in cases in which the general health seemed not to be in the least degree affected. The pigments themselves, to which the colours are due, have not until lately been subjected to any chemical examination, and great doubts still prevail regarding their true nature. The blue pigment to which I propose to confine myself on the present occasion, has been discovered in two states. In some cases it has been found ready formed so as to impart to the urine a blue colour, but merely in a state of suspension and therefore easily separated by simple filtration, whereas in other cases it has only made its appearance when the urine was left to stand or was subjected to the action of various reagents. In the cases described by Janus Plancus[*], Prout[†], Braconnot[‡], and Simon[§], it existed in the former state. Hassall developed the blue colour by means of putrefaction, in urines exhibiting the usual appearance, while Neubauer found the same effect to be produced by the addition of acids to the urine. As regards its chemical nature, the blue colouring matter seems to have been of three kinds, as far as can be ascertained from the descriptions given by the observers, which are not always very precise. In some cases, such as those described by Julia-Fontenelle[||], and Cantu[¶], the colour was evidently caused by prussian blue, the iron of which appears to have been derived in one case from a quantity of ink which the person had swallowed. The second kind of colouring matter has been minutely described by Braconnot, who obtained it simply by filtering the urine from the blue deposit found suspended in it. It was a dark blue powder, insoluble in water and alkalies, only slightly soluble in alcohol and yielding no crystalline sublimate when heated. From its dissolving in acids and its being reprecipitated by alkalies and other bases, Braconnot inferred that it consisted essentially of an organic base, to which he gave the name of *cyanourine*. If the substance which he examined was pure, it seems certainly to have been of a peculiar nature. Nevertheless no one has since then observed any colouring matter which could be with certainty pronounced identical with it, though instances have been met with in which the blue colour not being caused, as it seemed, by any well-known body, has been attributed to the presence of cyanourine. In the third class of cases the blue colour was produced by a substance, which, on examination

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[*] *Commentarii Instituti Bononiensis*, ad ann. 1767.

[†] On Stomach and Renal Diseases. 5th ed. p. 567.

[‡] *Annales de Chimie et de Physique*, vol. xxix. p. 252.

[§] Simon's Animal Chemistry, translated by Day, vol ii. p. 327.

[||] *Archives générales de Médecine*, vol. ii. p. 104.

[¶] *Journal de Chimie médicale*, vol. ix. p. 104.

of its properties and reactions, was found to be indigo-blue. Prout and Simon each mention a case in which indigo-blue was deposited from urine on standing, in the shape of a blue sediment. Neubauer* observed that the urine of a young man of 18, apparently in good health, when mixed with strong acids became first purple, then blue, and deposited a blue powder, which however he could not with positive certainty identify as indigo-blue. Hassall† was the first to point out that the occurrence of indigo-blue in urine was by no means so rare a phænomenon as had previously been supposed. The specimens of urine in which Hassall discovered it were mostly of a pale straw colour and acid. On standing they became thick and turbid and changed in colour from yellow to brown, then to bluish-green, while the surface became covered with a blue scum or pellicle, which was found to consist of impure indigo-blue. Hassall considers that the exposure of the urine to the oxygen of the atmosphere is essential for the formation of the colouring matter; however, I shall show that this exposure is by no means necessary. He also maintains that indigo-blue does not occur in healthy urine, that its presence is accompanied with strongly-marked symptoms of deranged health, and that its formation in urine must be regarded as a strictly pathological phænomenon,—conclusions which are, as will be seen, quite at variance with the results of my experiments.

Such in a few words is the present state of our knowledge on this rather obscure subject.

In my paper "On the Formation of Indigo-blue‡," I have shown that the colouring matter exists in plants in a very different state to what had hitherto been supposed, that it does not exist in them ready formed nor as reduced indigo, and that the presence of oxygen is not essential to its formation, but that it owes its origin to the presence of a peculiar substance, soluble in water, alcohol and æther, which by the action of acids is decomposed into indigo-blue, to which I have given the name of Indican, also a peculiar kind of sugar and a small quantity of other products. After having investigated the properties of this substance and its products of decomposition, I conceived it to be a matter of great interest to ascertain in what state indigo-blue exists in those urines, in which its presence is not indicated by the external appearance, but is only made manifest by treatment with various reagents. That such urines should contain a body

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* *Anleitung zur Analyse des Harns,* p. 19.

† Philosophical Magazine for September 1853; and Philosophical Transactions for 1854, p. 297.

‡ Phil. Mag. vol. x. [4] p. 73.

resembling indican seemed indeed exceedingly probable, since the same reagents which produce indigo-blue from indican lead in most cases to the development of the blue colour in particular kinds of urine. The extreme rarity, however, of these kinds of urine appeared to present an insuperable obstacle to the further investigation of the subject, and I therefore resolved to ascertain whether any conclusions could be arrived at from an examination of ordinary healthy urine.

When muriatic or sulphuric acid is added to urine, the mixture on being heated becomes brown and begins to deposit dark brown flocks, which increase in quantity when the heating is continued. When these flocks are filtered off, washed and dried, they form a compact dark brown mass, from which cold alcohol extracts a resinous matter, leaving undissolved a brown powder, which dissolves, however, in a boiling mixture of alcohol and ammonia. This powder contains nitrogen, and so much resembles indifuscine, one of the products of the decomposition of indican, as almost to lead one to suspect their identity. Its composition, however, though it stands, as I have ascertained, in a certain relation to that of indigo-blue, is quite different from that of indifuscine. Now if the liquid filtered from these flocks be mixed with a salt of oxide of copper and an excess of caustic soda, it becomes greenish, and if after being filtered it be heated for some time, it gradually deposits a tolerably large quantity of suboxide of copper, which is a proof of the presence of sugar. That the latter has been formed during the process and did not pre-exist, may be ascertained by previously heating a portion of the urine with a salt of copper and caustic soda, before treating the remainder of it with acid. Samples of urine, which, when tried in this way, afforded very doubtful or no indications of their containing sugar, were found, after being boiled with acid, then filtered and made alkaline, to reduce oxide of copper in a very marked manner. This reaction, which is so simple that it is only surprising it should never before have been observed, seems to me to prove that there is contained in urine some body, which by decomposition with acids yields sugar, the brown flocks precipitated at the same time being probably the substance with which the sugar was originally associated in the form of a copulated compound. From various considerations, which I need not detail, I was led to infer that this body could be no other than the very imperfectly known, so-called extractive matter of urine, and I accordingly commenced an investigation of this substance, which has led to conclusions of considerable interest. On discovering that the composition of the brown flocks formed by the action of strong acids on urine is expressed by the formula $C^{14} H^{7} NO^{4}$, which is also that of anthranilic acid, a product of the decomposition of indigo-blue, no further considerations were necessary to induce me to proceed with the investigation, notwithstanding the difficulties which I found attending it. Into the details of this investigation I shall at present only enter so far as they relate to the occurrence of indigo-blue in urine.

When acetate of lead is added to urine, it produces a cream-coloured precipitate, which consists of chloride, sulphate, phosphate, and urate of lead, and contains also a little of the extractive matter of urine, which is, as it were, merely attached to some of these lead compounds, since it is not precipitated from its watery solution by acetate of lead, when in a state of purity. The filtered liquid, which is much paler in colour than it was before the addition of acetate of lead, gives with basic acetate of lead a second precipitate of a pale cream colour, which consists of the lead compound of the extractive matter mixed with some basic chloride of lead. Both this and the first precipitate give, when treated with sulphuric or muriatic acid, yellow liquids, which, after being filtered from the sulphate or chloride of lead and boiled, yield brown flocks exactly like those obtained from urine itself. The liquid filtered from the precipitate with basic acetate of lead is almost colourless. It gives, however, on the addition of ammonia, an almost white precipitate, the quantity of which is much less than that of either of the two other precipitates. Now this precipitate exhibits a very remarkable peculiarity. It contains in most instances in combination with oxide of lead, a small quantity of a substance which, by decomposition with acids, yields indigo-blue. The first time that I treated this precipitate with acid I was surprised to observe that the liquid became immediately of a purplish-blue colour, and deposited after filtering and standing a small quantity of a substance, which on examination was found to consist chiefly of indigo-blue. This phænomenon was observed on so many occasions that I came to the conclusion that the occurrence in urine of an indigo-producing body similar to indican, was by no means an unusual circumstance.

In order to ascertain whether this body is present, I adopt the following method. The urine having been mixed with basic acetate of lead until no more precipitate is produced, is filtered, and after the precipitate has been washed with water, the liquid is mixed with an excess of ammonia, which always produces more or less of a white or yellowish-white precipitate. This precipitate is collected on a filter, slightly washed with water and then treated with dilute sulphuric or muriatic acid in the cold. After the whole of the oxide of lead has combined with the acid employed, the liquid is filtered. When there is much of the indigo-producing body present, the filter acquires a blue tinge, small particles of blue pigment are seen dotting the surface of the sulphate or chloride of lead, and the surface of the liquid, which is of a brownish-purple colour, in a very short time becomes covered with a thin pellicle, which is blue by transmitted and copper-coloured by reflected light, particles of the same blue substance being at the same time found attached to the sides of the vessel. When there is less of the indigo-producing body present, this pellicle only appears after some time, sometimes not until the next day. After twenty-four hours, however, the action of the acid is always completed, so that if no indigo-blue then appears or can be detected on examination of the deposit, the total absence of the indigo-producing body may be inferred. On the succeeding day, however large the quantity of blue deposit formed may be, the liquid no longer appears purplish, but brown, and after being filtered and boiled, deposits a dark brown powder, having exactly the same appearance as that produced by the action of acids on the ordinary extractive matter of urine. The matter left on the filter, after being washed, is treated with caustic soda, which dissolves a portion, acquiring thereby a brown colour. The portion which remains undissolved, after being again collected on a filter and washed, is treated with boiling alcohol. In most cases the alcohol acquires thereby a bright blue colour. When, however, the quantity of deposit formed is tolerably large, the boiling alcohol first dissolves another substance, which imparts to it a fine purple colour, and which I consider to be identical with indirubine*. That which the boiling alcohol leaves undissolved, is a bright blue powder having the properties of indigo-blue. It dissolves in an alkaline solution of protoxide of tin, and the solution on exposure to the air becomes covered with a blue film. It is soluble in concentrated sulphuric acid, forming a blue solution, which remains blue even after dilution with water. It imparts to boiling alcohol a bright blue colour, and the solution on cooling and standing deposits blue flocks. When heated in a tube, it gives a purple vapour which forms on the colder parts of the tube a blue sublimate.

Provided with this test, I proceeded to examine the urine of a number of individuals, and I succeeded in obtaining indigo-blue in so great a number of instances, that I have no hesitation in saying that the indigo-producing body, if not exactly one of the normal constituents of urine, occurs more frequently than any other of the abnormal ones. The urines containing it exhibit

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* It is very probable that Heller's urrhodine, as well as Golding Bird's purpurine, are also identical with indirubine, which, as I have shown, has the same composition as indigo-blue.

no remarkable or peculiar appearance whatever; they are acid, clear, and of the usual colour. Its occurrence, at least if its quantity is moderate, is not to be considered as a pathological phænomenon. I can at all events state from my own experience, that its presence is not attended by any symptoms of ill health or feelings of discomfort, and that neither from the state of the health nor the appearance of the urine, can any conclusions be drawn as to its presence or absence. The small number of samples of morbid urine which I had an opportunity of examining, yielded, with one exception, no more indigo-blue than the generality of healthy urines. Nevertheless, there are no doubt diseases in which the quantity of the indigo-producing body may become so large as to constitute a truly morbid symptom, and it may therefore become a matter of importance and interest for the medical man to have a ready means of detecting it. The delicacy of the test which I have described, as well as the small quantity of the substance usually present, may be judged of from the fact, that by working for several weeks on the urine of two individuals, which contained a comparatively large quantity, I obtained one grain of indigo-blue. Even when the amount of indigo-blue formed was very small, I always found that 16 fluid ounces of urine yielded an appreciable quantity of it.

The urine of forty different individuals, all of whom were apparently in a good state of health, yielded, with one exception only, more or less indigo-blue, when examined in the manner just described. These individuals belonged to both sexes, and they were of ages varying from 7 to 55. The majority were persons of the working classes. The largest quantity of indigo-blue was obtained from the urine of a man above the age of 50, a publican by trade. The urine of a young man, aged 32, a servant in my employment, yielded almost as large a quantity. Among the rest, the urine of a young man, aged 25, an engraver, that of a clerk, aged 23, and that of a girl, aged 12, who had been a cripple from infancy, were alone remarkable for the amount of indigo-blue which they yielded. In all these cases the indigo-blue was accompanied by the substance imparting to alcohol a purple colour, and which I suppose to be indirubine. The other specimens afforded much less, sometimes mere traces. In all cases, however, in which the urine of the same individual was examined at different times, the amount of indigo-blue obtained from it was found to vary exceedingly, it being sometimes considerable, and occasionally dwindling down to a mere trace. It was only very rarely, however, that none was found. In the case of the individual first referred to, the urine gave on one occasion not a trace, and this took place when he was engaged in performing labour, unusual for him both in its nature and amount. In my own case, as well as that of my assistant, the amount varied most capriciously from a tolerable quantity to a mere trace, occasionally even none at all being obtained.

I performed several experiments with different kinds of diet, in order to ascertain the effect on the amount of indigo-blue yielded by the urine. Only one experiment, however, led to any decisive result. Having selected an occasion when the night urine gave no indigo-blue, I took on the next night, before going to bed, a mixture of treacle and arrowroot boiled with water in as large a quantity as the stomach could bear, and the effect was that the urine of the following night gave a large quantity of indigo-blue. As, however, the same phænomenon was repeated for several succeeding nights without any additional quantity of food having been taken, it remained uncertain to what cause it was to be attributed, though a repetition of the experiment on a second occasion gave the same result.

I have hitherto not had an opportunity of examining many specimens of urine in disease. Of two samples of urine from patients with albuminaria, one gave a small quantity of indigo-blue, the other not a trace. Several specimens of diabetic urine yielded it. One of these, which I owed to the kindness of Dr. Browne of Manchester, gave a much larger quantity than I obtained from any other specimen of human urine.

The urine of the horse and the cow, when tried in the same way as human urine, gave comparatively very large quantities of indigo-blue, especially that of the horse.

I think it is highly improbable that the indigo-blue obtained in Hassall's experiments was produced, as he supposes, by the action of oxygen on the urine. Its formation was without doubt due to the decomposition of the indigo-producing body induced by the fermentation of the urine, the indigo-blue at the moment of its formation dissolving in the fermenting alkaline liquid and producing a true indigo vat, from which it was gradually deposited by the action of the atmospheric oxygen. When small quantities of indigo-blue only are formed in any specimen of urine, fermentation is not in my opinion to be recommended as a means of detecting it.

The occurrence of the indigo-producing body as an excretion, seems to me to be due to a disproportion between the oxygen absorbed by the system and the matter to be acted on by it, which again may be caused either by an excessive waste of the tissues or by an obstruction of the organs conveying oxygen, as the lungs and skin, or, as is probably the case in the majority of instances, by an excess of food being taken over and above the requirements of the system. As regards the constitution of this body, I think there can be no doubt that it contains the elements of indigo-blue and sugar, and that by oxidation within the system it is converted into the ordinary extractive matter of urine, which contains, as I have ascertained, the elements of sugar and of the black substance which is formed by the action of strong acids on urine, and which may be considered as a product of the oxidation of indigo-blue. Having prepared the extractive matter of urine in a state of purity, ascertained its composition, and examined its products of decomposition, I think it is probable that the indigo-producing body will be found, as regards its formation and composition, to occupy a place between the substance of the tissues and the ordinary extractive matter of urine. The very minute quantities of it ordinarily occurring in urine, and the difficulty of separating it from the extractive matter, make it, however, impossible to ascertain whether this is the case or not. My object in making known this portion of the investigation in its present fragmentary state, is to induce medical men, who have an opportunity of examining many varieties of urine, to endeavour to discover among these varieties some containing a sufficiently large quantity of this body to enable the chemist to ascertain its properties and composition.

The formation of a substance containing the elements of indigo-blue in the animal system, is a fact which may lead to important conclusions regarding the chemical composition of the complex bodies of which the blood and tissues consist.

XXXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 234.]

February 19, 1857.—Dr. W. A. Miller, V.P., in the Chair.

THE following communications were read:—

"Further Observations on the Anatomy and Physiology of *Nautilus*." By John D. Macdonald, Esq., Assistant Surgeon R.N.

"Brief Description of a Ctenostomatous Polyzoon, allied to Vesicularia, occurring on the Australian Coast." By John Denis Macdonald, Esq., Assistant Surgeon R.N.

"Observations on the Natural Affinities and Classification of Gasteropoda." By John Denis Macdonald, Assistant Surgeon R.N.

"On the Sea Saw-dust of the Pacific." By John Denis Macdonald, Esq., Assistant Surgeon R.N.

March 5.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"On what the Colonial Magnetic Observatories have accomplished." By Major-General Sabine, R.A., Treas. and V.P.R.S.

It has been suggested to me, that a brief review of what has been accomplished by the Colonial Magnetic Observatories, instituted on the joint recommendation of the Royal Society and British Association, would be acceptable; and that the officer who has been entrusted with the superintendence of these establishments is the person from whom such a review may most properly be expected. Fully assenting to both propositions, I have readily undertaken the task; and have availed myself of the occasion to add a few remarks and suggestions on the measures which appear to be required for the further prosecution of the objects for which the observatories were recommended.

The magnetic investigations designed to be carried into execution by the Colonial Observatories recommended by the Royal Society embraced a much wider scope than had been contemplated by any previous institutions, or than had been provided for by the arrangements or instrumental means of any then existing establishment, whether national or private. Not, as previously, limited to observations of a single element (the Declination),—or combining at the most one only of the components of the magnetic force,—the instructions of the Royal Society, and the instrumental means prepared under its direction, provided for the examination, in every branch of detail, of each of the three elements which, taken in combination, represent, not partially but completely, the whole of the magnetic affections experienced at the surface of the globe, classed under the several heads of absolute values, secular changes, and variations either periodical or occasional,—and proceeding from causes either internal or external. To meet the requirements of inductive reasoning, it was needful that the results to be obtained should comprehend all particulars under these several heads, attainable by an experimental inquiry of limited duration. That no uncertainty might exist as to the objects to which, in so novel an undertaking, attention was to be directed, the Report of the Committee of Physics, approved and adopted by the President and Council of the Royal Society, stated in a very few sentences, remarkable alike for their comprehensiveness and conciseness, the desiderata of magnetical science. It may be convenient to reproduce these, when desiring to show the degree in which the Observatories have fulfilled their contemplated purposes:—"The observations will naturally refer themselves to two chief branches, into which the science of terrestrial magnetism in its present state may be divided. The first comprehends the actual distribution of the magnetic influence over the globe, at the present epoch, in its mean or average state, when the effects of temporary fluctuations are either neglected, or eliminated by extending the observations over a sufficient time to neutralise their effects. The other comprises the history of all that is not permanent in the phenomena, whether it appear in the form of momentary, daily, monthly, or annual change and restoration; or in progressive changes not compensated by counter-changes, but going on continually accumulating in one direction, so as in the course of many years to alter the mean amount of the quantities observed."—Report, pp. 1, 2.

With reference to the first of these two branches, viz. the actual distribution of the magnetic influence over the globe at the present epoch, the Report goes on to state:—"The three elements, viz. the horizontal direction, the dip, and the intensity of the magnetic force, require to be precisely ascertained, before the magnetic state of any given station on the globe can be said to be fully determined . . . . and as all these elements are at each point now ascertained to be in a constant state of fluctuation, and affected by transient and irregular changes, the investigation of the laws, extent, and mutual relations of these changes is now become essential to the successful prosecution of magnetic discovery."

With reference to the second branch, viz. the secular and periodical variations, it is observed that—"The *progressive* and *periodical* being mixed up with the *transitory* changes, it is impossible to separate them so as to obtain a correct knowledge and analysis of the former, without taking express account of and eliminating the latter;" and with reference to the secular changes in particular, it is remarked—"These cannot be concluded from comparatively short series of observations without giving to those observations extreme nicety, so as to determine with perfect precision the mean state of the elements at the two extremes of the period embraced; which, as already observed, presupposes a knowledge of the *casual* deviations."

It is clear from these extracts that in the discussion of the observations the first point, in the order of time, ought necessarily to be an investigation into "the laws, extent, and mutual relations of the *transient* and," (as they were called at the time the Report was written,) "*irregular* changes," as a preliminary step to the elimination of their influence on the observations from which a correct knowledge and analysis of the progressive and periodical changes were to be obtained. It will be proper to show therefore, in the first place, what the Observatories have accomplished in regard to the so-called casual or transitory variations.

Casual Variations.—All that was known regarding these phenomena at the period when the Report of the Committee of Physics was written, was, that there occurred occasionally, and, as it was supposed, irregularly, disturbances in the horizontal direction of the needle, which were known to prevail, with an accord which it was impossible to ascribe to accident, *simultaneously* over considerable spaces of the earth's surface, and were believed to be in some unknown manner connected, either as cause or effect, with the appearances of the aurora borealis. The chief feature by which the presence of a disturbance of this class could be recognized at any instant of observation,—or by which its existence might be subsequently inferred independently of concert or comparison with other Observatories,—appeared to be, the deflection of the needle from its usual or normal position to an amount much exceeding what might reasonably be attributed to irregularities in the ordinary periodical fluctuations. The observations which had been made on the disturbances anterior to the institution of the Colonial Observatories had been chiefly confined to the declination. A few of the German Observatories had recently begun to note the disturbances of the horizontal force; but as yet no conclusions whatsoever had been obtained as to their laws: in the words of the Committee's Report, the disturbances "apparently observe no law." By the instructions cited above, the field of research was enlarged, being made to comprehend the disturbance-phenomena of the *three* elements; and the importance of their examination was urged, not alone as a means of eliminating their influence on the periodic and progressive changes, but also on the independent ground, that "the theory of the transitory changes might prove itself one of the most interesting and important points to which the attention of magnetic inquirers can be turned, as they are no doubt intimately connected with the general causes of terrestrial magnetism, and will probably lead us to a much more perfect knowledge of those causes than we now possess."

The feature which has been referred to as furnishing the principal if not the only certain characteristic of a disturbance of this class, viz. the *magnitude* of the departure from the usual or normal state at the instant of observation, has, in the discussion of the observations, been made available for the investigation of their laws: it has afforded the means of recognizing and separating from the entire mass of hourly observations, taken during several years, a sufficient body of observations to furnish the necessary data for investigating at three points of the earth's surface—one in the temperate zone of the northern hemisphere, a second in the temperate zone of the southern hemisphere, and a third in the tropics—the laws or conditions regulating or determining the occurrence of the magnetic disturbances. The method by which this separation has been effected has been explained on several recent occasions, and will be found fully described in the Phil. Trans. for 1856, Art. XV. By processes of this description, the disturbances of principal magnitude in each of the three elements, the Declination, Inclination and Total Force, have been separated from the other observations, at the three observatories of Toronto, Hobarton and St. Helena, and submitted to an analysis of which the full particulars will be found in the preliminary portions of the volumes which record the observations. By the adoption of a uniform magnitude as constituting a disturbance throughout the whole period comprised by the analysis, the amount of disturbance in the several years, months, and hours is rendered intercomparable. The result of this investigation (which could not be otherwise than a very laborious operation, since the observations at a single one of these stations, Toronto, considerably exceeded 100,000 in number, each of which had to be passed through several distinct processes,) has made known to us that the phenomena of this class, which may in future with propriety and advantage receive the appellation of "*occasional,*" are, in their mean or average effects, subject to periodical laws of a very systematic character; placing them, as a first step towards an acquaintance with their physical causes, in immediate connexion with the sun as their primary exciting cause. They have—1, a *diurnal* variation which follows the order of the solar hours, and manifests therefore its relation to the sun's position as affected by the earth's rotation on its axis; 2, an *annual* variation, connecting itself with the sun's position in regard to the ecliptic; and 3, a third variation, which seems to refer still more distinctly to the *direct* action of the sun, since, both in period and in epochs of maximum and minimum, it coincides with the remarkable solar period of about ten, or perhaps more nearly eleven, of our years, the existence of which period has been recently made known to us by the phenomena of the solar spots; but which, as far as we yet know, is wholly unconnected with any thermic or physical variation of any description (except magnetic) at the surface of the earth, and equally so with any other cosmical phenomena with which we are acquainted. The discovery of a connexion of this remarkable description, giving apparently to magnetism a much higher position in the scale of distinct natural forces than was previously assigned to it, may justly be claimed on the part of the Colonial Observatories, as the result of the system of observation enjoined (and so patiently and carefully maintained), and of the investigation for which it has supplied the data; since it was by means of the disturbance-variations so determined, that the coincidence between the phenomena of the solar spots and the magnitude and frequency of magnetic disturbances was first perceived and announced (Phil. Trans. 1852, Art. VIII.).

The extent and mutual relation of the disturbance-variations of the three elements, even at a single station, supply a variety of points of approximation and of difference, which are well suited to elucidate the physical causes of these remarkable phenomena; but valuable as such aids may be when obtained for a single station, their value is greatly augmented when we are enabled to compare and combine the analogous phenomena, as they present themselves at different points of the earth's surface. To give but a single example:—there are certain variations produced by the mean effects of the disturbances which attain their maximum at Toronto during the hours of the night; the corresponding variations attain their maximum, at Hobarton, also during the hours of the night, but with a small systematic difference as to the precise hour, and with this distinguishing peculiarity, that the deflection at Hobarton is of the opposite pole of the needle (or of the same pole in the opposite direction) to the Toronto disturbance; whilst at a third station, St. Helena, which is a tropical one, the hours of principal disturbance are those not of the night, but of the day. A very superficial examination is sufficient to show that for the generalization of the facts,—a generalization which is indispensable for their correct apprehension and employment in the formation of a theory,—the stations at which the phenomena are to be known must be increased. Those which were chosen for a first experiment were well selected to prove the importance of the investigation, and thus to lead to its extension. It is only at the Colonial Observatories that the disturbance-variations have hitherto been made out; and taking experience as our guide, we have before us the evidence of the means by which the inquiry may be further successfully prosecuted*.

Periodical Variations.—The anticipation expressed in the Report of the Committee of Physics, that for the purpose of obtaining a correct knowledge of the *regular periodical variations,* it would be found necessary to eliminate the "casual perturbations," has been fully confirmed. Had the latter been strictly "casual" (or accidental, in a sense contradistinguished from and opposed to periodical), a sufficiently extended continuance of observation might have occasioned their mutual compensation; but now that we have learned that the mean effects which they produce are governed by periodical laws, and that these laws and those of the regular periodical variations are dissimilar in their epochs, it is manifest that in their joint and undivided effects we have two variations, due to different causes and having distinct laws, superimposed upon each other; *to know the one correctly we must necessarily therefore eliminate the other.* A striking illustration of the importance of such elimination is furnished by the solar-diurnal variation of the total force. It will readily be imagined that the question must be an important one, whether a variation, which is supposed to derive its origin from the sun, be a single or a double progression; whether it have two maxima and two minima in the twenty-four hours, or but one maximum and one minimum in that period. When no separation is made of the disturbances, the progression appears to be a double one, having two minima, one occurring in the day and the other in the night. With the removal of the disturbed observations the night minimum disappears, and we learn that the regular solar-diurnal variation of the total force has but one notable inflection in the twenty-four hours, viz. that which takes place during the hours when the sun is above the horizon.

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* The Colonial Observatories under my superintendence were originally four in number, viz. Toronto, St. Helena, Cape of Good Hope, and Hobarton. In July 1846 the detachment of the Artillery at the Cape of Good Hope was withdrawn by orders from England, and the charge of the magnetical and meteorological observations transferred to Mr. Maclear, the Government Astronomer at that station. The magnetical observations made at the Cape, when the magnetic observatory was one of those under my superintendence, were published in 1851, with a discussion of certain of their results; and the disturbance-variation of the declination at the Cape has since been deduced by my assistant, Captain Younghusband, Phil. Trans. 1853, Art. VI. Since the transfer to Mr. Maclear, Mr. Pierce Morton, a gentleman of considerable mathematical attainments, who has been added as an assistant to Mr. Maclear in that branch of the Cape observations, has applied himself to the investigation of the lunar magnetic influence (as derived from the Cape observations), with a view of presenting the results to the Royal Society. For this, and other deductions,—such as, for example, the laws of the disturbances of the inclination and total force,—he will have the entire series of observations, viz. those as above-stated already published, and those which have been made since the transfer of the Observatory, up to the present time.

The night minimum is in fact the mean effect of the occasional disturbances. It is probable that the nocturnal inflection of the solar-diurnal variation of the Declination may be ascribed to the same cause, namely to the superposition of two distinct variations.

A careful analysis of the solar-diurnal variations of the Declination at the Colonial Observatories has brought to light the existence at all these stations, of an *annual inequality* in the direction of the needle concurrent with changes in the sun's declination, having its maxima (in opposite directions) when the sun is in or near the opposite solstices, and disappearing at or near the epochs of the equinoxes. An intercomparison of the results of the analysis at these stations has shown, that this inequality has the remarkable characteristics of having notably the same direction and amount in the southern as in the northern hemisphere, and in the tropical as in the temperate zones. An ingenious explanation of these phenomena has been suggested by Dr. Langberg of Christiania (Proceedings of the Royal Society, vol. vii. p. 434); but whether this explanation be or be not the correct one, the theoretical importance of the facts cannot be doubted, inasmuch as they appear to be wholly irreconcileable with the hypothesis which would attribute the magnetic variations to thermic causation. We may ascribe to the general and almost exclusive prevalence of the thermic hypothesis, and to its influence on magnetic reasonings, that the well-known erroneous opinion was so confidently promulgated by a deservedly high magnetic authority*, that a line *must* exist surrounding the globe, in which the needle would be found to have *no diurnal variation*. We have now, on the contrary, reason to be assured, by the facts of the annual inequality thus discovered, that there is no such line; but that everywhere in the regions of its supposed existence a diurnal variation subsists, having opposite characteristics in opposite parts of the year as influenced by the sun's position on either side of the equator, and disappearing only at the epochs when the sun passes from south to north or from north to south Declination.

Lunar Variation.—But if thermic relations have failed to supply a connecting link between the sun and those magnetic variations which are, without doubt, referable to the *sun* as their primary cause, the failure of that hypothesis is made still more obvious by the existence of variations governed by the *moon's* position relatively to the place of observation. We are indebted to M. Kreil, now holding the same position in Austria that I have filled in England, for the first suggestion of the existence of a lunar-diurnal variation of one of the elements, viz. of the Declination, founded on observations at Milan and Prague; and in the Phil. Trans. for 1856, Art. XXII., will be found an exposition of the facts of the moon's diurnal influence on each of the three magnetic elements at Toronto, viz. on the Declination, Inclination, and Total Force. In the case of this investigation, notwithstanding the smallness of the values concerned, the instrumental means supplied to the Colonial Observatories have

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* Arago, Annuaire, 1836, p. 284.

been found competent to determine, with an approximation sufficient for present theoretical purposes, the character and amount *for each element* of the regular daily effect of the moon on the terrestrial magnetic phenomena, the existence of which does not appear to have been even suspected at the time when the Report of the Committee of Physics was drawn up. The *discovery* of the moon's influence on any of the magnetic elements is due, as already stated, to M. Kreil; but Toronto is the first, and as yet the only, station, at which the numerical values at every lunar hour of the lunar-diurnal variations of the three elements have been published. Corresponding statements to that which has been given for Toronto, will be found for St. Helena and Hobarton, in the volumes of those observatories, which are now in preparation. All the results at the three stations present the same *general* characters. The lunar influence does not appear to participate in the decennial inequality which is found in all the solar variations (Phil. Trans. 1857, Art. I.). The lunar-diurnal variation of each of the elements is a double progression in the twenty-four hours, having epochs of maximum and minimum symmetrically disposed. In *character*, therefore, it differs from what might be expected to take place if the moon were possessed of inherent magnetism, *i. e.* if she were a magnet, as it is usually termed, *per se*; and accords with the phenomena which might be expected to follow if she were magnetic only by induction from the earth. On the other hand, it is believed that the *amount* of the variation, as observed at each of these stations, very far exceeds what can be imagined to proceed from the earth's inductive action reflected from the moon. In this theoretical difficulty we are naturally thrown back to seek a more extensive knowledge of the phenomena than we have yet obtained, and to the generalization which will follow, when sufficient materials for it have been procured. In subordinate particulars, a difference, which is apparently systematic, is perceived to exist in regard to the hours which constitute the epochs of maxima and minima at the three stations, as well as in regard to the amounts of the respective variations; these differences are no doubt intimately connected with the causes of the phenomena, and are likely to lead to their elucidation. It is therefore greatly to be desired that the number of stations furnishing complete determinations, such as the Colonial Observatories only have hitherto supplied, should be increased.

The domain of periodical variations has thus been considerably enlarged since the Report of the Committee of Physics was drawn up; and must henceforth be understood to comprise, in addition to the variations "whose amount is a function of the hour-angle of the sun, and of his longitude" (or of his declination) (Report, p. 10),—1st, those variations of the three elements whose amount is a function of the hour-angle of the moon; 2ndly, those variations which were classed in the Committee's Report as "irregular," or "apparently observing no law," but which are now known to be governed by laws depending on the sun's declination, and hour-angle; and 3rdly, those variations, both "regular" and "occasional," which have their epochs and amounts dependent apparently on a solar period of not yet perfectly ascertained duration, manifesting itself also by periodical changes in the frequency and amount of the solar spots. With the exception of the last-named class, all these variations require, for their generalization, that the phenomena should be investigated at several points of the earth's surface widely distant from each other; and we have now the knowledge, grounded on experience, that a very few years are sufficient for the observations at each station, with the instrumental means and methods recommended by the Royal Society, and when the investigation is made a primary object by those who engage in it.

Absolute Values and Secular Changes.—But interesting and valuable as is the acquisition of a fuller and more precise knowledge of the comparatively small magnetic variations produced at the surface of the earth by the action or influence of external bodies, even greater importance seems to attach,—when *terrestrial* magnetism is in question,—to the purposes of that distinct branch of the duties of a magnetic observatory, which consists in the determination of the absolute values and secular changes of the three magnetic elements. By the *absolute values* we seek to acquire a knowledge of the actual present order and distribution of the terrestrial magnetic influence at the surface of the earth, and to provide the materials by which the constancy, or otherwise, of the earth's magnetic charge may hereafter be examined; and by determinations of the present direction and amount of the *secular changes*, we seek to become acquainted with the laws, and ultimately with the causes, of that most mysterious change, by which the magnetic condition of the globe at one epoch passes progressively and systematically into that of another. It is specially by determinations of this class, obtained with the necessary precision in different parts of the globe, that, in the words of the Committee's Report, "the patient inductive inquirer must seek to ascend to the general laws of the earth's magnetism." At the time when the Report of the Committee of Physics was written, doubts were reasonably entertained, whether the limited time, during which the Colonial Observatories were likely to be maintained in action, would be sufficient for the determination of the secular changes; and it was therefore very properly argued, that "these changes cannot be concluded from comparatively short series of observations without giving to the observations *extreme nicety*, so as to determine with perfect precision the mean state of the elements at the two extremes of the period embraced." It is with much satisfaction, and with a well-deserved recognition of the pains which have been bestowed by the successive Directors of the Toronto Observatory, and their Assistants, on this branch of their duties, that I am able to refer to the determinations of the absolute values and secular changes of the three elements contained in the third volume of the Toronto Observations, in evidence that the instrumental means which were devised, and the methods which have been adopted, have proved, under all the disadvantages of a first essay, sufficient to determine these data with a precision which is greatly in advance of preceding experience, and, as far as may be judged, equal to the present requirements of theoretical investigation. This is the more deserving of notice, because Toronto is a station where the casual and periodical variations, which it was apprehended would seriously interfere with the determination of absolute values, are unusually large. We may derive, therefore, from the results thus obtained, the greatest encouragement to persevere in a line of research which is no longer one of doubtful experiment, and to give it that further extension which the interests of science require.

Amongst the results which have recompensed the labours of the Colonial Observatories in this branch of their inquiries, perhaps there is none of more importance in respect to the general theory of terrestrial magnetism, than the conclusion which has been established by means of the observations of the Declination at St. Helena, that the current annual amount of secular change takes place by *equal aliquot portions in every month, and even in every fortnight of the year.* The magnitude of the annual change of the Declination at St. Helena, $8'$ (or more precisely $7'\cdot93$ in each of the eight years in which the observations were maintained), and the comparative tranquillity of the tropical regions in regard to magnetic disturbances, were circumstances which rendered St. Helena a particularly eligible locality for an investigation of this nature. The result has been, to remove secular change altogether from the category of atmospheric or thermic relations, with which, in the absence of a correct knowledge of the facts, it has frequently been erroneously associated; and to show conclusively that it is a phenomenon of far more systematic order and regularity than has been generally apprehended (Proceedings of the Royal Society, vol. vii. pp. 67–75).

It has thus been shown, that, in each and all of the branches of inquiry for which the institution of the Colonial Observatories was recommended, they have accomplished the objects which were contemplated, and have in many respects exceeded the expectations on which their recommendation was founded. Nor has the scope of their performance been limited to a mere registry of the observations, or to their publication in a crude and undigested form. It was well remarked by an authority of the greatest weight, when addressing the British Association on the occasion of the assembly of the Magnetical and Meteorological Conference at Cambridge in 1845 (Herschel, Address, p. xxxv), that "A man may as well keep a register of his dreams, as of the weather or any other set of daily phenomena, if the spirit of grouping, combining, and eliciting results be absent." To advance by the simple and straightforward path of inductive inquiry, in a science such as terrestrial magnetism in which a physical theory has yet to be sought, the endeavour must be made "to grapple with the palpable phenomena, seeking means to reduce their features to measurement; the measurements to laws; the laws to higher generalizations; and so, step by step, to advance to causes and theories." The mere observational part is not, and ought never to be, viewed as the fulfilment of the duties of institutions such as magnetic observatories; those duties ought always to be held to include (either on the part of the Directors of the Observatories themselves, or on that of persons who, as Superintendents or otherwise, have constantly watched the progress of the work) "the systematic deduction from the registered observations, of the mean values, and of the local coefficients of diurnal, annual, and secular change;" because "no other class of persons stands in anything like so favourable a position for working out the first elementary laws of phenomena, and referring them to their immediate points of dependence," as those who have directed or superintended the processes by which the data required for the knowledge of the phenomena have been obtained. The introductory discussions prefixed to the several volumes which contain the observations at the Colonial Observatories, and a succession of papers presented to the Royal Society, and published in the Philosophical Transactions, bear testimony to at least unsparing labour, on the part of the Superintendent, to give a completeness to the experiment of Colonial Observatories, corresponding to its original conception; though no one is more sensible than himself that this portion of the duty might well have fallen into abler hands. One great advantage in the task has undoubtedly been enjoyed, viz. the union of the detailed knowledge above alluded to, with the opportunity of generalization and consequent insight, afforded by results admitting of strict comparison and combination, obtained from well-selected stations at such distant points of the globe, and by a uniform system of observation.

It may be useful on the present occasion to recall to more distinct recollection the views and opinions entertained by those who were the principal instigators of the proceedings by which the Royal Society became the responsible advisers,—and Her Majesty's Government the chief supporters,—of measures which have placed this country in the very conspicuous position of taking that lead in the advancement of certain branches of science, which other nations were willing and desirous that she should take. These views cannot be better stated than in the words of one to whom all will be willing to concede pre-eminence, as well in counselling the recommendation to Government as in conducting the several points connected with it to a successful issue (Herschel, in Quart. Review, No. CXXXI.). "Great physical theories, with their trains of practical consequences, are pre-eminently national objects, whether for glory or utility. In effect, such they ought to be considered by every nation calling itself civilized; and if we look to consequences, we have only to point to the history of science in all its branches to show, that every great accession to theoretical knowledge has uniformly been followed by a *new practice*, and by the abandonment of ancient methods as comparatively *inefficient* and *uneconomical*. This consideration alone we think sufficient to justify, even on utilitarian grounds, a large and liberal devotion of the public means to setting on foot undertakings and maintaining establishments, in which the investigation of physical laws, and the determination of exact data, should be the avowed and primary object, and practical application the secondary, incidental, and collateral one. That the time is now fully arrived when other great branches of physical knowledge must be considered as entitled to share in that public support and encouragement which has hitherto fallen to the lot of astronomy alone, will, we think, be granted without hesitation by all who duly consider the present state and prospects of science. The great problems which offer themselves on all hands for solution—problems which the wants of the age force upon us as practically interesting, and with which its intellect feels itself competent to deal—are far more complex in their conditions, and depend on data which, to be of use, must be accumulated in far greater masses, collected over an infinitely wider field, and worked upon with a greater and more systematized power, than has sufficed for the necessities of astronomy. The collecting, arranging, and duly combining these data are operations, which, to be carried out to the extent of the requirements of modern science, lie utterly beyond the reach of all private industry, means or enterprize. Our demands are not merely for a slight and casual sprinkling to refresh and invigorate an ornamental or luxurious product, but for *a copious, steady, and well-directed stream, to call forth from a soil ready to yield it, an ample, healthful, and remunerating harvest.* There are secrets of nature we would fain see revealed,—resources hidden in her fertile bosom for the well-being of man upon earth, we would fain see opened up for the use of the generation to which we belong. But if we would be enlightened by the one, or benefited by the other, we must *lay on power*, both moral and physical, without grudging and without stint."

If at the period when it was still doubtful what the Colonial Observatories then just established might be able to accomplish,—and when, in effect, the expectations from them were little more than the anticipations of what a voyage of discovery upon an unknown ocean might produce,—the propriety of embarking upon such investigations was thus unhesitatingly affirmed, how much more confidently may the duty of *perseverance* be insisted upon, when the results of the first experiment have already more than realized the hopes which caused it to be undertaken. They have indeed confirmed the belief that "the gigantic problem proposed to be resolved" is of a nature to yield in its full extent only to "continued and persevering inquiry;" but at the same time they may be said, in a certain sense, to have narrowed the field of inquiry, by showing more distinctly than was previously apprehended, both what is desired to be known, and how and where it is to be sought. If the history of magnetical science is to be something more than a fragment, the research must be persevered in.

In considering the means by which the researches thus opened out may be most advantageously prosecuted, it is natural that we should look, in the first instance, to the adoption, at other selected stations, of arrangements similar to those which were instituted at the stations which were chosen for a first, and as it has proved, successful experiment; and with this view I may be permitted to restate the opinions which I submitted to the Magnetical and Meteorological Conference at Cambridge in 1845, as all that has since taken place has served to confirm them.

"Before I close this communication, I wish to advert to the expediency of extending the system of observation now in operation at Toronto, St. Helena, and the Cape of Good Hope, to other of the British Colonies, where the same objects can be accomplished in an equally effective and economical manner.

"In cases where the institution of similar establishments is strongly urged by the Governor of a Colony,—where competent persons are present and disposed to superintend the observations, and where soldiers of the Artillery are stationed whose services may be available, and whose employment has been shown to be economical and effective in a high degree in the execution of a laborious and exact routine of observation,—there is wanting only a supply of instruments,—the temporary allotment of a building to contain them,—extra pay, such as the individuals at the above-named Observatories receive,—and an authorized connexion with a head-quarter establishment whence they may derive instruction and guidance.

"The cost of one of the Ordnance Observatories (including £100 a year for incidentals of all kinds) is £392 a year, exclusive of publication. It may be assumed that five years of hourly observation is a sufficient time of continuance for obtaining in any particular colony the mean values of the magnetical and meteorological elements, and their diurnal, annual, and secular variations, as well as the peculiarities of climate bearing on the health and industrial occupations of man. If the observations were printed *in full detail* for the five years, they would occupy two quarto volumes; but if it were thought sufficient hereafter that duplicate or triplicate manuscript copies should be deposited in different public libraries, and that publication should be confined to abstracts and an analysis, the cost of the publication would form but a small addition.

"The colonies of Ceylon, New Brunswick, Bermuda, and Newfoundland are in the described case; their respective Governors are recommending the establishment of Magnetical and Meteorological Observatories in them; competent directors are on the spot [this was written in 1845]; and they are all Artillery stations."

To the four stations thus named may be now added Mauritius and Demerara, as from both those Colonies, strong and repeated applications to the same effect have been sent through their respective Governors to the Secretary of State for the Colonies. Both these Colonies have offered to bear a portion of the expense of the proposed establishments; and have earnestly solicited to be placed in connexion with a head-quarter establishment, from which they might receive properly constructed instruments, with instructions and guidance in their use. Can it be said that we perform our duty as a mother-country when we put such applications on the shelf?—whilst, in the interests of science, it would be difficult to estimate too highly the value of such institutions,—in forming good observers, who might subsequently extend their activity over a wider range,—in affording to travelling observers the opportunity of testing and correcting their instruments, as well as keeping up and perfecting their skill in observation,—and in contributing to arouse, to nourish, and to extend to other parts of natural knowledge, that desire for the greatest possible accuracy, which was formerly met with only in astronomy and in geodesical operations of the highest class.

When it was first suggested that the officers and soldiers of the scientific corps of the army (Artillery or Engineers) stationed in the Colonies might, both beneficially to themselves and advantageously to the public interests, be made available for the performance of such temporary services, the suggestion, from its novelty, might have been open to many objections. None were, indeed, made by the military authorities of the time, who on the contrary approved and encouraged the proposition. There may have been doubts entertained in other quarters whether persons, whose ordinary occupations were so dissimilar, would be found to possess the necessary qualifications for carrying out a scheme of exact and varied observation, in which there was then no precedent to guide, and of which the performance would be sure to be extensively and closely scrutinized: but such doubts, if they existed, have probably long since subsided, as the successive volumes of the Colonial Observatories have appeared.

One great and unquestionable advantage which future institutions of this nature will have over those whose duties are accomplished, will be found in the assistance they will derive from the *Physical Observatory of the British Association* at Kew, as a head-quarter Observatory, in which their instruments can be prepared and verified, the constants, &c. carefully determined, new instruments be devised as occasion may require, and tested by experiment before they are sent out for use, and to which practical difficulties of all kinds, which may present themselves to the directors, may be referred.

The omission of a provision of this kind when the Observatories were first formed, was undoubtedly a great fault, which has been, and could only be, very imperfectly remedied by the Woolwich establishment, designed for a very different purpose, and insufficient even for the duties for which it was designed.

There is another advantage (if it be one) which might attend the early prosecution, viz. the opportunity of consulting (if it were desired to consult) the experience of the person who has conducted,—and, as he believes, successfully conducted,—the first experiment, from its commencement now almost to its close; but this, in the course of nature, can only be available for a few years to come.

The Colonial establishments were instituted at the instance of the Royal Society and British Association, with a more general concurrence and approval on the part of the cultivators of science in all parts of the globe than, it is believed, were ever before manifested in regard to any purely scientific undertaking; and with such a cordial and effective cooperation of the public authorities as is well deserving of being held in remembrance. It is for those two great scientific bodies to consider whether any, and what, steps should now be taken to procure the continuance of the researches.

March 12.—Major-General Sabine, Treas. and V.P., in the Chair.

"On the Immediate Principles of Human Excrements in the Healthy State." By W. Marcet, M.D., F.C.S.

In a previous paper I had the honour of communicating to the Royal Society the results of a first series of investigations on the immediate principles of the fæces of man and animals; since then I have continued my researches on human excrements, being most ably seconded by my assistant, Mr. Frederick Dupré, Ph.D.

The new results obtained were the following:—

1. *Margarate of lime*, *phosphate of lime*, and *margarate of magnesia* were discovered to be immediate principles of human evacuations.

2. I found a new method for obtaining excretine, and its chemical formula has been established.

3. The fact that vegetable food induces the presence of margaric acid in excrements has been confirmed.

4. The existence of a comparatively large quantity of cholesterine in the spleen, which I had mentioned before as probable, has been confirmed.

When human fæces are exhausted with boiling alcohol, the fluid being rapidly strained through a cloth, a clear extract is obtained, which, on cooling, yields a deposit; this substance, being collected on a filter, is partly soluble in boiling alcohol, and there remains undissolved a residue insoluble in ether and alcohol. The residue in question being boiled with a solution of potash, dissolves almost entirely, and the addition of hydrochloric acid induces the formation of a precipitate in the solution. On examining this precipitate, it was found to consist of a crystallizable substance fusing at 60° Cent.; its structure and other properties were precisely those of *margaric acid*.

The acid filtrate contained *phosphoric acid* and *lime*. From several quantitative analyses, I concluded that there was more lime than is required to combine with the phosphoric acid in the form of the neutral phosphate, the excess of lime being exactly that which was necessary to convert the margaric acid into a neutral margarate of lime, $C_{34} H_{33} O_3 + CaO$. Consequently it followed that the three substances existed in the form of *margarate of lime* and *phosphate of lime* as immediate principles of human fæces.

The alcoholic filtrate from the deposit being allowed to stand for twenty-four hours, deposited another substance, of a nearly white appearance, and which proved to be *margarate of magnesia*.

The peculiar action of a vegetable diet on human fæces was investigated by means of experiments undertaken upon myself, when I observed that an entirely vegetable diet was attended with the formation of a large quantity of margaric acid in the excrements,—most probably not in the form of a margarate, but in the free state, inasmuch as it was obtained from the decomposition, with hydrochloric acid, of the precipitate induced by adding milk of lime to the cold and clear alcoholic extract of fæces, after the separation of the above-described deposits.

In the month of December 1855, I had an opportunity of noticing that during a cold night, when the temperature falls below the freezing-point, *excretine* crystallizes readily and in large quantity in the clear alcoholic extract of fæces; this method I employed as often as possible, to prepare enough excretine for its chemical analysis; but the cold weather not lasting long enough, and this season having been remarkably mild, I was compelled to adopt a modification of the process by milk of lime, described in my former communication.

Having prepared a sufficient quantity of excretine, partly by the action of cold, and partly by means of milk of lime, the chemical composition of this substance was now determined. A qualitative analysis showed it to consist of carbon, hydrogen, sulphur and oxygen*; there was no water of crystallization present. Oxide of copper was employed at first for the combustions, but they were subsequently undertaken with chromate of lead, on account of the large proportion of carbon that excretine contains; no substance having been found to combine with it, its atomic composition was calculated from the assumption that one equivalent contained one equivalent of sulphur; and the following formula was obtained:—

78 eq. Carbon	468
78 eq. Hydrogen	78
1 eq. Sulphur	16
2 eq. Oxygen	16
Atomic weight of Excretine	578

In my former communication I had stated that when the tissue of the spleen is submitted to a process of analysis similar to that adopted for the extraction of excretine, a substance closely allied to cholesterine is obtained. This subject being one of great importance in a physiological point of view, I have resumed the investigation, and placed beyond doubt that this substance is really *cholesterine*. Its presence in the spleen is evidently independent of that which might exist in the blood retained by this organ after death. Is it that the spleen secretes cholesterine? This can only be determined by actual experiment; but it is very remarkable that a part of the blood which is supplied to the liver should come directly from an organ containing large quantities of a substance known to enter into the composition of the bile.

"Description of a Chronometer Compass." By Ralph Reeder, Esq., of Cincinnati, U.S.

This instrument is a combination of the Universal Dial and Chronometer, and is intended to show the errors of the magnetic needle,

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* In my former communication I had erroneously stated that excretine contained nitrogen, which resulted from my not having been able to prepare a sufficiently large quantity of the substance; and, moreover, it might not have been perfectly pure.

both at sea and on land, and in clear weather to perform in place of the needle.

"Anatomical Description of a Species of Asteroid Polypes, probably forming the type of a new genus of Alcyonidæ." By John Denis Macdonald, Assistant Surgeon R.N.

March 19.—Dr. W. A. Miller, Vice-President, in the Chair.

The following communication was read:—

"A System of Train-Signalling, by which also disabled Trains may telegraph for assistance without the aid of portable apparatus." By Charles V. Walker, Esq., F.R.S.

When, in the early days of telegraphy, messages were sent and trains were signalled on the same wires, no facilities existed for reducing the apparatus employed for the latter purpose, to a simple form. The case is now becoming different, special wires being largely devoted to train signals; hence the present system.

The *instrument* employed is a large electro-magnet, with a moveable armature, carrying a stem and a hammer, which latter strikes on a bell by the direct force of magnetism. It is provided with a contact-maker, a spring, the depression of which causes a current to circulate. The bobbins are 4 in. × 3 in.; and are filled with ten pounds of covered copper wire, No. 16 or No. 18. The core is of five-eight inch iron. The armature and appendages weigh $2\frac{1}{4}$ oz. Bells of this kind have been in action for five years without cleaning or repairing. The battery is zinc-graphite, and a solution of 1 sulph. ac. + 8 or 10 water. The plates, $7\frac{1}{2}$ in. × 3 in., are placed in stone pots that contain about a quart, the zinc standing in a gutta-percha slipper, containing mercury. Batteries of this kind will do their work untended for half a year and longer.

The *language* consists of blows on the bell; the number of blows varies according to the train-signal to be given. The distinctions required for ordinary purposes being few, the bell-language is very appropriate, from its addressing the ear, from its simplicity and from the facility with which the signals are given and taken. *One* blow is for the starting of an ordinary train; *two*, for an express; *three*, for the arrival of a train; *five*, for stopping all trains; *six*, for testing. This is a general code; other forms of code are used for protecting level crossings and junctions; but the fundamental signals of the general code are of universal application. This system was introduced five years ago on the South Eastern Railway; and at the present time consists of about 100 bells, to which additions are in progress.

The bells are connected in pairs, both bells being in a circuit that terminates in the earth in the usual way, at each station. The signal is made by depressing the spring from its earth-contact, upon the zinc end of the battery, the graphite end being in permanent connexion with the earth. The battery being thus introduced between the bell and the earth, a current circulates along the wire and produces one blow upon the bell. The home bell may be excluded or not from the circuit, when a signal is sent.

By the above arrangement signals are sent from station to station. But the extreme simplicity of the battery, the bell, and the language allows the arrangements to be so modified that signals may be made on a pair of bells from any joint, intermediate between two bell-stations, without the necessity of providing the signaller with any telegraph or battery, or any electrical apparatus whatever. The addition of this property to the bells does not in any way interfere with their being in perfect action and constant use for the ordinary work of train-signalling, and therefore if the guards of trains and the plate-layers of the permanent way are provided with a signal for expressing their wants, a great advance is made in telegraphy, and a large element of safety is gained for the travelling public.

It is well known to electricians that, if two equal and opposed currents are presented to the respective ends of a wire, no evidence is manifested of the circulation of electric force; the wire is in a null state, as much so as if no current was presented to it. Taking advantage of this law, in connexion with the simple bell-system above described, the circuit is made to contain the two batteries, one at each station, as well as the pair of bells; the same pole, the graphite, for instance, of each battery being connected with the earth.

When the home-station signaller desires to make a signal, he depresses the spring as before; but the connexions are such, that by this act he excludes his *own* battery from the circuit. The circuit then contains but one battery,—namely that at the pass station; the current of which is now able to circulate from end to end, being no longer counterbalanced by an equal and opposite current; and consequently the bells are sounded. This, then, is the process for ordinary train-signalling, under this arrangement.

By altering the contact-maker so that it inverts the battery in the circuit, instead of putting it out of circuit, both batteries are made available for each signal; and consequently the power and with it the cost of each may be reduced.

But the null state of the wire is equally well and very readily destroyed, by connecting it with the earth at any point intermediate between the two stations; for by this process a complete circuit is made or channel opened for the discharge of both ends of both batteries, each independently of the other, except that the attached wire between the earth and the telegraph wire is common to both circuits, and thus the bells at the respective stations are actuated by the batteries of the respective stations. If ten blows with a pause of a minute, and then ten more, is the signal that the engine is disabled; ten blows, and a minute of contact, that an accident has happened; a ringing continued beyond ten, that the permanent way is obstructed, the stations at either side are advised and can take the measures necessary to meet the case.

These contacts may be made by hooking a wire or rod on to the line wire and making the necessary contacts with the rail; or, which is better, by establishing contact-makers, properly secured at frequent intervals on the telegraph posts.

This system gives to those in charge of disabled trains a certain means of asking for assistance from any point of the open railway, without any training beyond that of *counting ten* slowly and correctly. In practice, as between Red-Hill and Reigate, no inconvenience or loss of electricity has been suffered from counterbalancing the two currents.

The author states that there are other properties of opposed currents to be communicated on another occasion.

XXXVI. *Intelligence and Miscellaneous Articles.*

NOTES ON FROTH. BY DR. GLADSTONE, F.R.S.

ALL liquids, when shaken up with air, form bubbles; but some allow these bubbles to break and disappear the moment they are at rest, while others give rise to a more or less permanent froth. This difference among liquids appears to be of a specific character, and cannot as yet be shown to be dependent on any other quality.

As a general rule, aqueous solutions of organic bodies are most disposed to froth. Beer is a familiar instance. In this case the formation of the froth is originally due in a great measure to the rising of carbonic acid gas through the liquid; but its permanence is totally independent of that, or indeed of any dissolved gas. This was proved by subjecting some beer to exhaustion by an air-pump till every trace of the carbonic acid and air it contained was removed, withdrawing it from the vacuum, and shaking it immediately, when a fine froth was produced which was as persistent, or nearly so, as if the beer had continued brisk.

Solutions of the acetates are peculiarly disposed to produce a permanent froth. To such an extent is this the case, that in making use of mixtures of salts, I have sometimes distinguished those containing an acetate by this property. The acetate of iron is pre-eminent; but the acetates of copper, lead, and other metals share this property to a considerable extent. Yet acetic acid itself shows no disposition to froth; and the bubbles made when alcohol or æther are shaken, instantly disappear. Acetate of iron gives off much dissolved air when it is exhausted by the pump, but it froths equally afterwards. The citrate of iron is analogous to the acetate.

This property of frothing is quite independent of specific gravity. A heavy solution of sulphindigotic acid froths, but a solution of chloride of ammonium of high density produces no persistent bubbles when shaken; while, on the other hand, a weak solution of soap, that differs little from distilled water, will produce, as every one knows, a very permanent froth.

The froth on the surface of a coloured liquid is always of a lighter tint than the liquid itself. This is what might be anticipated, when it is remembered that in looking at froth, we see the light which has traversed only very thin films of the liquid, and hence has been subjected to but little absorption. Sometimes, however, the colour of froth is totally different from that of the liquid on which it floats; cochineal, for instance, gives a deep red aqueous solution, which froths considerably when agitated, but the bubbles appear of a pale bluish-purple. This is due to the dichromatic character of the liquids in question; they admit many rays of the spectrum for a certain distance which are afterwards absorbed, and that in such a way that the sum of the rays transmitted by the thin stratum conveys to the observer a different impression of colour to that which is conveyed by those rays which can penetrate a thicker stratum. Thus the thin film of cochineal solution which constitutes a bubble transmits the red ray, a yellowish-green ray, a bluish-green ray, and nearly the whole of the blue and violet of the spectrum; the resulting colour is accordingly bluish-purple mixed with much white light: a thicker stratum of the same cuts off both the green rays, and a still thicker one transmits only the red.

Of a similar nature is an appearance observed in port-wine. The new wine, when shaken, forms a bubble which is faintly red, while old port forms one that is colourless. This, indeed, is sometimes taken advantage of as an indication of the age of port. The difference arises partly from a gradual change that takes place in the colouring matter of the wine, partly also on the diminished thickness of the film that constitutes the bubble of the older and 'thinner' wine. If old port be placed in a hollow glass wedge, and thus interposed between the eye and a slit in the window-shutter in such a way that the line of light is seen traversing the different thicknesses of the liquid, that line will appear white where the stratum is thinnest, and will become red very suddenly as the stratum increases; almost as intensely red, in fact, as when the thickness is considerable; and if this line of light be analysed by a prism, it will be seen that every ray of the spectrum traverses the thin stratum, but that suddenly they are cut off, all at very nearly the same distance, with the exception of the red ray and a portion of the orange, which are transmitted through almost any amount of the liquid.

Some bubbles, as is well known, exhibit that beautiful phænomenon designated 'the colours of thin plates,' or 'Newton's rings.' This is most strikingly displayed by the froth on the surface of the black liquid produced by adding a few drops of sulphuric acid to oil of turpentine: the play of colours is beautifully relieved against the black background.

NOTE ON CAPILLARY ACTION. BY G. WERTHEIM.

The theory of capillarity rests upon the hypothesis that the volume of liquid which is raised above the level is in proportion to the contour of the section of the solid wall, whatever may be the curvature of this contour. This hypothesis, enunciated by Laplace and coincident with that of Young, serves as a point of departure from which we descend to the development of all particular cases, and rise on the other hand to the study of molecular forces. It cannot therefore be too carefully verified; but unfortunately the integration of the fundamental differential equation is only capable of being effected in certain particular cases, so that its experimental verification has been confined within very narrow limits.

To effect this verification in a more general manner, I have operated as follows:—I commenced by observing the asymptotic curve generatrix of the surface of the meniscus raised by a plane; having measured the corresponding values of the ordinates above the level, for abscissæ very close to each other, this curve was afterwards constructed by points, and the area and position of the centre of gravity of the surface comprised between this curve and the two axes were determined by experiment. The same determinations were effected for menisci raised by a great number of convex cylinders of different diameters, and by means of Guldin's theorem the volumes of the solids generated by the revolution of these surfaces round the axes of their cylinders were calculated. In the same way the capillary surface of the liquid included between two parallel planes, placed at different distances (2α), was found: let h be the height above the level of the lowest point of this curve, b the area of the section of the semi-meniscus, and l the breadth of one plane; $2l(h\alpha+b)$ will be the volume of the liquid raised.

The quotients of all the volumes which we have just found for one liquid at the same temperature, divided by the corresponding contours of the solid bodies, must be equal amongst themselves if the hypothesis be correct, and must furnish the constant of capillarity $\frac{a^2}{2}\sin\phi$ (in which ϕ is the designation of the complement of the angle formed by the extreme element of the curve with the wall).

This process does not apply to concave cylindrical surfaces; in these we can only observe the elevation h of the lowest point of the surface, and this is what I have done with a great number of tubes of very small or very large diameter; in these two limiting cases the constant of capillarity is deduced from the value of h alone, by means of the well-known formulæ of Poisson.

Lastly, in order to avoid being entirely deprived of data with regard to tubes of middle size, I had several drawn in zinc; and after coating their inner surfaces with wax, I immersed one extremity in a capsule filled with fused wax, and maintained at a temperature slightly superior to that of fusion. The ascension took place, and after cooling, the interior column retained very nearly its primitive height, and the meniscus its original surface; so that after dissolving the wall by dilute sulphuric acid, there remained a cylinder of wax terminated above by this surface, and below by a plane, the position of which in relation to the level had been previously determined: h was then measured, and the value of b determined upon a vertical section passing through the axis. The following Table contains the averages of all the results which I have obtained:—

Volumes borne (in millimetres) by 1 millim. perimeter (at a temperature 50° to 59° F., except with wax).

		Distilled water.	Saturated solution of protochloride of iron.	Oil (olive).	Wax (virgin).	Alcohol.	Sulphuric æther.
	Internal diam. millims.						
Narrow tubes...	0·15—1·8	7·537	6·182	3·720	3·507	3·072	2·618
Middling tubes	2·104	”	”	”	3·362	”	”
	5·246	”	”	”	3·146	”	”
	5·510	”	”	”	3·158	”	”
	6·244	”	”	”	3·221	”	”
Large tubes ...	12—30	<5·8, >5·6	<5, >3·7	3·7	3·366	3·1	”
One plane	....	5·148	3·264	3·457	3·470	3·169	”
	Distance.						
Two parallel planes......	9·740	5·187	3·329	3·779	”	3·254	2·673
	7·930	5·196	3·340	3·623	”	3·277	2·672
	5·344	5·179	3·352	3·952	”	3·178	2·649
	3·940	5·272	3·345	3·833	”	2·979	2·542
	2·640	5·142	3·432	3·899	”	3·088	2·505
	1·332	5·088	3·361	3·927	”	3·133	2·692
	0·882	5·133	3·597	3·807	”	3·185	2·703
	0·394	5·197	3·554	3·776	”	3·170	2·622
	0·236	5·192	”	3·821	”	3·272	2·732
	External diam.						
Convex cylinders	34·25	5·071	2·915	3·397	”	3·021	”
	22·03	4·819	2·778	3·170	2·926	2·976	”
	14·92	4·727	2·615	2·778	”	2·764	”
	10·09	4·500	2·450	2·794	3·477	2·799	2·296
	4·86	3·953	2·187	2·753	2·691	2·730	2·371
	3·02	3·800	2·353	2·861	2·555	2·281	2·185
	1·648	3·057	2·262	2·451	2·089	1·732	2·470
	1·032	1·948	1·912	2·017	2·075	1·642	2·212
	0·676	1·988	1·674	1·564	1·790	1·398	”
	0·332	1·464	0·784	1·218	2·289	0·774	”

From the numbers contained in this Table we may draw the following conclusions:—

1. *Two parallel planes* raise a constant volume, whatever may be their distance, and even when this distance is infinitely great. As regards water, the curve which I have found for a single plane accords very well with that observed by Hagen; the values of h between two planes differ but little from those found by Simon (of Metz), only the latter physicist has wrongly attempted to apply to all distances, a law which mathematicians have never enunciated except as being approximatively true for very small distances.

2. The constant capillarity calculated from experiments made with *narrow tubes* with some liquids is equal, and with others far superior to that determined by means of two planes; it is accidental that with water the relation between these two values is evidently

equal to $\frac{\pi}{2}$, as has been pointed out by Simon: this relation is nearly 2 for the chloride of iron and 1 for our other liquids.

3. The *wide tubes* give a value which is comprised between the two preceding values when the latter differ, and equal to them when they coincide; this is what takes place with alcohol, and it is for this reason that the only experiment of verification cited by Laplace and Poisson gave a result which accords exactly with the formula: it would not have been so if, in this experiment, Gay-Lussac had made use of water instead of alcohol. It will be seen also why Frankenheim* found that experiment disagreed with the formula, even when he employed tubes of an internal diameter of 14 millims.

4. In proportion as the radii of *convex cylinders* diminish in departing from the plane where this radius is infinitely great, the volume raised continues diminishing for the first two liquids; with the others this diminution commences at a certain limit of curvature, and increases gradually and apparently indefinitely. Amongst the liquids which I have tried, æther is that which presents the most constant volume; but unfortunately the results relating thereto are less certain than the others, notwithstanding the precautions which I took to diminish the evaporation during the experiment. And, in any case, it is not to the absence of viscosity that this constancy would have to be attributed; comparative experiments with pure water and gum-water having shown that viscosity, although retarding the movement when the equilibrium is established, has no sensible influence upon this definitive state.

To explain these facts, one might be tempted to admit that the angle of contingence varies with the curvature of the wall; but it may be demonstrated that this is not the case. Thus, if we consider only the menisci of water and chloride of iron raised by one plane, for which we should have already $\phi < 90°$, the area of the section would be

$$A = \frac{a^2}{2}\sin\phi,$$

and the maximum ordinate

$$H = a\sqrt{2}\sin\frac{\phi}{2} > a\sqrt{\sin\phi};$$

whilst experiment constantly gives

$$\sqrt{2A} > H.$$

I also show that for $\phi = 90°$ we have the coordinates of the centre of gravity,

$$x' = \frac{2}{3}H, \quad y' = \frac{2}{3}H - \frac{\sqrt{2}}{3}H = 0\cdot19525\,H;$$

in proportion as ϕ diminishes, the centre of gravity removes from the axis of the ordinates, whilst it is in reality more approximated to this than it would be according to this formula.

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* Poggendorff's *Annalen*, vol. lxxii. p. 191.

It will be necessary, therefore, to have recourse to another hypothesis, and, I believe, to take into account the variable thickness of the liquid stratum or sheath which adheres to the solid body. This hypothesis has been suggested to me by a series of experiments which I have made upon the elevation of the solution of protochloride of iron between two parallel iron plates which I fixed to the poles of a Ruhmkorff's electro-magnet. The lower extremities of these plates were immersed in the solution, and the values h and b for the different distances 2α were already known. A current was then passed into the apparatus, of which the intensity was gradually increased and measured, when the magnetic liquid was seen to rise between the two planes, often to twice or three times its original height, whilst the surface acquired the curvature suitable to this new elevation; but for each intensity of magnetization the volume raised remains evidently constant whatever be the distance of the two planes; in one word, things go on as if the constant capillarity had been doubled or tripled. We know, however, from the experiments of Brunner and Mousson, that the attraction of the liquid upon itself is not altered by its magnetization; and, on the other hand, the minuteness of the changes of form which the liquid undergoes when the polar surfaces are not immersed in it, and the fact that the augmentation of the volume elevated is independent of the distance of the planes, prove that we have not to do with an effect of magnetic attraction exerted at a distance. I think, therefore, that these facts can only be explained by the increase of thickness of the adherent stratum, an increase which may be proved directly.

It will be understood, also, that as every change of temperature may cause this thickness to vary, the influence of temperature may be very different from that foreseen by the theory, only taking into account the dilatation of the liquid.—*Comptes Rendus,* May 18, 1857, p. 1022.

THE IMPROVED INDUCTION COIL.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I am surprised to find that Mr. Hearder, after his communication to the Mechanics' Magazine for July 11, has again accused me of having copied his induction coil. He there states, that at the conclusion of one of his lectures at the London Institution, Mr. Shuter of Farringdon Street, when speaking about my induction coil, asserted "that he (Mr. Shuter) had had almost as much to do with its construction as Mr. Bentley, since all the different plans for carrying out the mode of insulation, &c. were concocted between them at his house; that Mr. Bentley had tried various thicknesses of gutta-percha with different degrees of success, and at length on one occasion came running to him (Mr. Shuter) with breathless delight, telling him that he had actually produced a spark nearly two inches in length. These statements were made in the presence of my friend Dr. Letheby, and with such an air of honest frankness that I could not but credit them."

The whole of this statement, if made, is incorrect; but as Mr. Hearder says he could not help giving credence to it, it follows that he cannot really believe me guilty of appropriating his contrivances, although he has accused me of doing so. Mr. Hearder appears to think that I have acknowledged his claim to priority; I have done nothing of the kind; I merely consider the claim unworthy of dispute, and not of sufficient importance to occupy any space in your Journal.

Mr. Hearder wishes to know by what means I ascertained that my contact-breaker vibrated 2000 times a second, and suggests that I have added a cipher too many. The mere fact that my machine can be made to act ten times more rapidly than he imagined, shows that our instruments *work* differently, although Mr. Hearder will not admit any difference in their construction.

The method adopted to ascertain the rate of vibration was simply to press a card upon the periphery of a toothed wheel, which was rotated rapidly until the musical sound emitted by the card was in unison with the tone which was produced at the contact-breaker; the number of teeth on the wheel, multiplied by its revolutions per second, gave the number of vibrations for that particular sound, which was $200 \times 10 = 2000$.

These rapid movements need not excite surprise, as small springs have vibrated before the poles of electro-magnets 20,000 times a second.

If Mr. Hearder's statements about the coils which he exhibited in London be correct, I am not the only one deceived in their construction, for all those to whom I had spoken on the subject had formed the same opinion as I had of their structure.

Mr. Hearder seems determined to do what he can to deprive me of any slight credit I may deserve, and he does not seem particular about the means he adopts to do so. He first tries to make my coil the joint production of Mr. Shuter and myself; then he accuses me of having copied *his* machine.

It is evident that if I made my coil after Mr. Hearder's pattern, I could not have been indebted to Mr. Shuter for his assistance, yet Mr. Hearder says he believes I was. Mr. Hearder also intimates that my name ought not to be associated with the improved induction coils; I suggested this myself in my January communication, and said that my coil, and those made by others, ought still to be called Ruhmkorff's coils.

I am, Gentlemen,
Your obedient Servant,
C. A. Bentley.

[Our readers, we doubt not, see very clearly the respective merits of our correspondents, and the origin of their difference as to the question of priority. We hope the discussion will terminate here. Each of our correspondents has his coil. The next question is, to what good purpose is he about to turn it? The man who does most with his instrument will eventually stand highest.—J. T.]

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

NOVEMBER 1857.

XXXVII. *On the Influence of Light upon Chlorine, and some remarks on Alchemy.* By JOHN W. DRAPER, *M.D., Professor of Chemistry and Physiology in the University of New York*[*].

SEVERAL years ago I observed that when a mixture of chlorine and hydrogen is exposed to light, union does not occur at once, but that a certain interval must elapse, during which absorption takes place, the combination then proceeding in a uniform manner.

It is by the chlorine that this absorptive agency is exercised, the indigo ray being chiefly influenced. And not only is it that ray which is thus absorbed: to it also must be attributed the subsequent combination.

Among several other facts connected with this subject, which may be found in the Philosophical Magazine (July 1844), the American Journal of Science, vol. xlix., and other publications of that time, there is one to which I would particularly direct attention. Chlorine which has been exposed to the sun has obtained properties not possessed by chlorine which has been made and kept in the dark; and the change is by no means transient, but lasts for many hours and even days.

In their recent examination of this fact, Professor Bunsen and Dr. Roscoe do not appear to regard the modification in question as being of so permanent a nature. Perhaps it may have been that the insolation to which they submitted the chlorine was not continued sufficiently long, or perhaps the light was not sufficiently intense. My opinion was founded on three different conditions of the experiment:—1st, on the behaviour of chlorine

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[*] Communicated by the Author.

itself, confined over salt water; 2nd, on the effects of a mixture of chlorine and hydrogen in equal volumes, as disengaged from hydrochloric acid by a voltaic current; 3rd, on the action of a solution of pure chlorine in distilled water. In each of these instances, the active properties imparted to the chlorine by exposure to light were plainly perceptible for a long time after. Indeed I infer from the experiments of those chemists, that they found the effects to continue for a certain brief period. If they do so continue, though only in a momentary manner, after the light has been shut off, I do not see in what other way we are to explain the result, than on the principle of a change in the relations of the chlorine. In this interpretation it is very well known that Berzelius coincided, in his account of my experiments in the 'Annual Report' for 1847.

At first I thought that there was a general analogy between the case of chlorine thus thrown into an active state, and that of iron in its passive condition. An iron wire which has been made passive, will quickly revert to the condition of activity if submitted to any jarring, vibration, or other trivial disturbance; its passive state being in one sense permanent, though very easily lost. But subsequently I found many reasons for supposing that the impression is of a much more lasting nature, and resembles that on phosphorus after a similar exposure to the indigo rays. As an illustration of what is here meant, I may relate, that having obtained a thin stratum of perfectly white phosphorus between two pieces of glass, I exposed it to a motionless solar spectrum, and found that it turned of a dark brown colour in those spaces on which the more refrangible rays fell, the effect reaching a maximum under the indigo ray. The fixed lines of Fraunhofer were very prettily depicted as white streaks, particularly the larger ones at H. I kept this sample of phosphorus for several years without its showing any disposition to resume the active state.

Professor Bunsen and Dr. Roscoe dwell very appropriately on the disturbing effects of minute quantities of extraneous gases, mingled with chlorine, on photo-chemical induction. No one who has used a tithonometer can have failed to make a similar remark. My attention has been directed to that subject in its more general aspect; and I will ingenuously confess that I have made several attempts at the transmutation of metals, on the principle of compelling them, by the aid of solar light, to be disengaged from states of combination, in the midst of resisting or disturbing media.

The following is a description of one of these alchemical attempts. In the focus of a burning-lens, 12 inches in diameter, was placed a glass flask 2 inches in diameter, containing nitric acid diluted with its own volume of water. Into the nitric acid were poured alternately small quantities of a solution of nitrate of silver and of hydrochloric acid, the object being to cause the chloride of silver to form in a minutely divided state, so as to produce a milky liquid, into the interior of which the brilliant converging cone of light might pass, and the currents generated in the flask by the heat, might drift all the chloride successively through the light. The chloride, if otherwise exposed to the sun, merely blackens upon the surface, the interior parts undergoing no change; this difficulty I hoped therefore to avoid. The burning-glass promptly brings on a decomposition of the salt, evolving on the one hand chlorine, and disengaging a metal on the other. In one experiment the exposure lasted from 11 A.M. to 1 P.M.; it was therefore equal to a continuous midday sun of seventy-two hours. The metal was disengaged very well. But what is it? It cannot be silver, since nitric acid has no action upon it. It burnished in an agate mortar, but its reflexion is not like the reflexion of silver: it is yellower. The light must therefore have so transmuted the original silver as to enable it to exist in the presence of nitric acid. In 1837 I published some experiments on the nature of this decomposition in the Journal of the Franklin Institute.

Though this experiment, and several modifications of it which I might relate, fail to establish any permanent change in the metal under trial in the sense of an actual transmutation, it does not follow that we should despair of a final success. It is not likely that Nature has made fifty elementary substances of a metallic form, many of them so closely resembling one another as to be with difficulty distinguished; moreover, chlorine and other elementary substances can be changed by the sunlight in some respects permanently; and if silver has not thus far been transmuted into a more noble metal, as platinum or gold, it has at all events been made transiently into a something which is not silver. Those who will reflect a little on the matter, cannot fail to observe that the sun-rays really possess many of the powers once fabulously imputed to the powder of projection and the philosopher's stone.

XXXVIII. *On the Induction Apparatus. By the* Rev. N. J. Callan, *D.D., Professor of Natural Philosophy in the Roman Catholic College, Maynooth*[*].

It is now more than twenty years since I discovered the method of making the induction coil, or a coil by which an electric current of enormous intensity may be produced with the

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[*] Communicated by the Author.

aid of a single galvanic cell,—a coil which is now to be used for working the Atlantic Telegraph. Mr. Faraday was the first who developed the laws of electrical induction; but he did not discover the method of making a coil by which a current of very great intensity may be obtained by means of a very small battery. This was first discovered in Maynooth College in 1836. In the summer of 1837, I sent the late Mr. Sturgeon a small coil which he exhibited at a meeting of the Electrical Society in London, and from which he gave shocks to several of the members. After the meeting, I received a letter of thanks from him, in which he described the astonishment of those who experienced the extraordinary power of the coil. This was the first induction coil of great power ever seen outside the College of Maynooth. The first notice of the discovery of the coil is found in a paper of mine published in the London Philosophical Magazine for December 1836. In 1836 and 1837 I also discovered that the intensity of the current induced in the coil increased with the number of cells employed, and that a shock may be got from the coil at the moment of making as well as of breaking connexion with the battery. In April 1837 I published, in Sturgeon's 'Annals of Electricity,' a description of an instrument which I devised for producing a rapid succession of electrical currents in the coil by rapidly making and breaking communication with the battery. This, as Mr. Bachhoffner says in one of his papers published in Sturgeon's 'Annals,' was the first contact-breaker ever made. Thus, before April 1837 I had completed the coil as a machine for producing a regular supply of electricity. From 1837 till the end of 1854 my attention was directed to other matters. Since the beginning of 1855, I made a long series of experiments on the various parts of the induction coil and apparatus. Although my experiments are not yet finished, I thought it better to lay the results already obtained before the British Association*.

The following are the results of my experiments:—First, a method of getting a shock directly from the armature of a magnet at the moment of its demagnetization; secondly, the discovery of what I believe to be a new fact or law connected with the action of iron on a battery by which it is magnetized, viz. that if iron be put into a coil of covered wire, the ends of which are connected with a battery, the quantity of electricity flowing from the same battery through another coil connected with it will be considerably greater when the first coil is nearly filled with iron than when there is little or no iron in it; thirdly, a

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* This paper was read in Section A. (on Mathematics and Physics) at the late meeting of the British Association in Dublin. The paper being hastily written, some things were omitted which are here supplied.

form of core which has five advantages over the cores in common use, which will enable us to get intensity and quantity currents, and may therefore answer for the Atlantic Telegraph and for the electric light; fourthly, an improved method of insulating the secondary coil; fifthly, a contact-breaker in which the striking parts are copper, and which acts as well as if they were platina; sixthly, an explanation of the action of the condenser, which appears to me more satisfactory than any other I have seen; lastly, some new facts regarding the condenser, and an improved method of making it.

The first result is a means of obtaining, not from a coil surrounding the armature of a magnet, but from the armature itself, a voltaic current capable of giving a shock. This result is obtained by making a coil of fine insulated iron wire, and an electro-magnet of such a form that the coil will fit between its poles. The iron coil is then the armature of the magnet. If the helix of the electro-magnet be connected with a battery, the iron becomes magnetized; and on account of its proximity to the magnetized iron, the coil of iron wire, or the armature of the electro-magnet, will be also magnetized, and will lose its magnetism when the connexion between the battery and electro-magnet is broken, or when the electro-magnet is demagnetized. If, at the moment the iron coil loses its magnetism, the ends of it be held in the hands, a shock will be felt. If the ends of the iron coil be connected with a delicate galvanometer, the needle will be deflected at the moment the coil is magnetized by the electro-magnet. Hence at the moment of magnetization or demagnetization, an electric current is produced in each section of the iron at right angles to its magnetic axis. From this, two inferences may be drawn,—first, that if for the copper coils used in magnetic telegraphs, coils of iron wire were substituted, electrical currents of greater intensity might be obtained; secondly, that if iron wire were used in the secondary coil of induction coils, the intensity of the secondary currents would be increased.

Here I shall take occasion to explain the causes which produce the secondary current in the induction coil. I believe that this current is the result of the combined action of three inductive forces; one arising from the sudden cessation or destruction of the magnetism of the core, the second from the cessation of the magnetism of the primary coil, and the third from the destruction of the magnetism of the secondary coil at the moment the connexion between the battery and primary coil is broken. This supposes, first, that as long as the primary coil is connected with the battery, magnetic power is given, not only to the iron core, but also to the primary and secondary coils; and secondly, that in each of them, at the moment of losing its magnetism, an electric current is produced in each of them as well as in all contiguous conductors. Both, I think, may be satisfactorily proved. First, every one knows that the iron core is magnetized by the primary current. Secondly, the primary coil itself is a magnet as long as it is connected with the battery; for every wire or conductor through which a voltaic current flows has magnetic properties: one of its sides will attract the north pole of a magnetic needle, and the opposite will attract the south pole; so that if the wire be placed over the needle at rest, the latter will be deflected from the magnetic meridian. The wire, or conductor of a galvanic current has its magnetic poles, not at its extremities, but at its opposite sides; so that were the wire divided into two halves along its length, one half would be a north and the other a south magnetic pole. The magnetic axis of such a wire is one of its diameters, or a line joining its opposite sides. Thirdly, the secondary coil is a magnet when the primary coil is connected with the battery. This is evident when the secondary coil is made of iron wire; for the primary current magnetizes iron by which it is surrounded as well as iron enclosed within it: it induces in each section of the surrounding as well as of the enclosed iron, an electrical current which magnetizes the iron. I have found by experiment that iron outside the primary coil is not so strongly magnetized as iron enclosed within it. When, as is commonly the case, the secondary coil is made of copper wire, it is also a magnet; for the primary current induces an electrical current in each spiral of the secondary coil of copper, as well as in each section of the iron core. This current magnetizes each spiral of the copper coil, and makes the whole coil a magnet at the moment the primary coil is connected with the battery. Now we must suppose, that as the primary current, whilst it continues to flow, maintains in the iron core the magnetic power produced by the currents induced in each section of the iron at the moment the primary coil is connected with the battery, although these currents last but an instant, so also the same primary current will maintain in each of the spirals of the copper coil the magnetism given to them by the currents induced in them at the moment the battery connexion is made. There is no reason why the continuance of the primary current should not maintain its first effect in the copper spirals as well as in the iron, since the first effect is the same in both, viz. the magnetization of both. Hence, when the primary wire of an induction coil is connected with a battery, the secondary coil is always a magnet, as well as the core and primary coil; and therefore in every induction coil we have three magnets so long as its primary coil is connected with a voltaic battery; and the three lose their magnetism the moment the battery communication is broken. Now in every magnet, at the moment of the cessation of its magnetism, an electric current is produced in a direction at right angles to the magnetic axis, in the magnet itself and in all contiguous bodies. First, it has been already shown that at the moment iron loses its magnetic power, an electric current is produced in each section of it in a direction perpendicular to its magnetic axis. By the laws of induction, these currents induce parallel ones in every contiguous conductor. Secondly, when a current flowing from a battery through a copper wire ceases, the wire loses its magnetism; and it is found by experiment, that at the moment of losing its magnetism, an opposite electrical current is produced in the whole length of the wire, or in a direction at right angles to its magnetic axis. Hence, because in every induction coil excited by a battery there are three magnets, viz. the core, the primary and secondary coils, having a common axis, and because at the moment the connexion with the battery is broken the three lose their magnetic power, an electrical current is produced in each section of each of the magnets in a direction perpendicular to their common axis; and these currents in each magnet induce electrical currents in the other two. Therefore, when the connexion with the battery is broken, a current is produced in the secondary coil, which is the result of the combined action of three inductive forces arising from the suspension of the magnetism of the core, of the primary and of the secondary coil. When the secondary coil is made of iron wire, the magnetic power it will receive from the primary current, and from the magnetic inductive force of the core, will be far greater than if it be made of copper wire; and therefore the intensity of the secondary current in a coil of iron wire must be much greater than that of the secondary current in a coil of copper wire. I showed, at the late meeting of the British Association in Dublin, an induction coil in which the secondary wire was of iron: its length was about 21,000 feet, and its thickness about the $\frac{1}{100}$dth of an inch. With a single cell, 6 inches by 4, and without a condenser, this coil gave sparks half an inch long. Should a condenser of the proper size increase the length of the sparks, as it does in Mr. Gassiot's great coil, in a thirtyfold ratio, my coil ought to give sparks 15 inches long with a single cell. I have not yet tried it with a condenser: I made two large condensers, in which, when both were united, the acting surface of each plate exceeded 600 square feet. After being used for some time, the insulation of the plates gave way, and the action of the condenser became feeble, and once ceased altogether. I intend to reconstruct both condensers as soon as possible, and to try their effect on the coil, on which I have, since the meeting of the Association, coiled about 28,000 feet more of fine iron wire, so that at present the length of the secondary coil is nearly 50,000 feet. Since the increased length of wire was put on the coil, I have got from it, with a single cell, 6 inches by 4, and without a condenser, sparks $\frac{15}{16}$ths of an inch in length. I expect that with the same battery it will give sparks at least an inch long without a condenser. This is, I believe, the most powerful coil ever made.

The second result is, that if a bundle of iron wire be put into a coil of insulated thick copper wire connected with a battery, the quantity of electricity which will flow through another coil in contact with the same battery, will be considerably greater when the iron wires are in the first coil than when they are altogether or partly removed. This I found by using a contact-breaker worked by an electro-magnet, the helix of which was connected with the same battery by which an induction coil was excited. In trying the effect of the induction coil without an iron core in its primary coil, I found that the action of the electro-magnet of the contact-breaker was slow and feeble. When a few wires were put into the primary coil, the action of the contact-breaker was sensibly increased; and when the primary coil was filled or nearly filled with wire, the attraction of the electro-magnet became considerably stronger, and consequently the voltaic current flowing round it must have been considerably increased. Since the core of the induction coil increases the quantity of electricity flowing from the battery through the helix of the electro-magnet, we must suppose that the iron of the magnet reciprocally increases the quantity of electricity transmitted through the primary coil, and that therefore little or no battery power is lost by using an electro-magnet for making and breaking contact, instead of the magnetized core of the coil. Hence it appears also to follow, that a secondary current of greater intensity may be got with a battery of given power from a great number of small coils than from one large one, in which the conducting power of the primary coil is equal to the sum of the conducting powers of the primary wires of all the small coils; for the magnetic power of the core of each of the small coils will be increased by the magnetism of the cores of the others.

The third result is a form of core which has five advantages over all the cores in common use, and which may enable us to get electrical currents having at the same time great intensity and considerable quantity, and may therefore be very useful for working the Atlantic Telegraph, and for producing the electric light. In my experiments on the core, I have used cores of six different forms, and varying in weight from one pound to two hundred and a half of iron wire. I have used, first, a core of uninsulated iron wire coiled on an iron bar; secondly, the ordinary bundle of iron wires; thirdly, an elliptical or flat bundle of wires; fourthly, a coil of covered iron wire; fifthly, a core consisting of a coil of insulated iron wire and of a bundle of iron wire; lastly, a core consisting of two concentric coils of insulated iron wire, one made of fine, the other of thick wire.

When the uninsulated iron wire coiled on a bar of iron was employed as a core, the spark produced by the secondary coil was less in length and brightness than when the iron bar alone was used; because a complete circuit was formed between some of the spirals and those above them, whilst the other spirals were insulated from each other by the oxide of iron on the surface of the wire.

The elliptical or flat bundle of wire receives from a given voltaic current flowing through a primary coil made of wire of given length and thickness, greater magnetic intensity than a cylindrical bundle does; because when the length of the circumference of the two bundles is the same, a section of the former is smaller, and contains less iron than a section of the latter. Therefore, if the two coils be connected with the same battery, the same quantity of electricity will flow through both; and the quantity of iron in the flat or elliptical one being less than in the cylindrical one, it will be more intensely magnetized.

I find that all cores consisting of bundles of parallel wires have five defects. First, in each section of every wire in such cores an electrical current is induced by the primary current, and all those currents may return to the points where they originated; or there is a complete circuit for them, which is found to diminish the intensity of the secondary current. Some have imagined that by insulating the wires of the core from each other, they have prevented all complete circuits. But these persons seem to have forgotten, or not to have adverted to the fact, that when the wires of the coil are insulated from each other, the primary current induces an electrical current in each section of every wire.

The second defect consists in this, that the currents induced in each section of every wire are opposed by those in the corresponding sections of all the adjoining wires; and thus the magnetic power which the primary current is capable of producing in the core is greatly diminished, and is less than it would be if all the wires were in close contact with each other; and consequently the intensity of the secondary current is diminished.

The third defect is, that the immense quantity of electricity set in motion by the primary current in all the sections of each wire in the core is lost: it remains within the core, and cannot be used for producing any electrical effect.

The fourth defect is, that we cannot ascertain the effect which a condenser applied to the primary coil has on these currents.

The fifth defect is, that we cannot apply a Leyden jar or any condenser to the currents themselves.

I have found that a core consisting of a coil of insulated or covered iron wire is free from all these defects. In such a core there is no complete circuit for any current in any section of the iron: for the electrical currents produced by the primary current in the sections of an enclosed iron coil move in the directions of the spirals of the coil; and since no spiral returns to itself, no current can return to the point where it originated. Neither does the current in any spiral of the coil oppose those in the adjoining spirals; for the currents in all the spirals flow in the same direction, or in the direction of the primary current. Thirdly, since all the currents in the spirals of the iron coil flow in the same direction from the beginning to the end of the coil, they must unite and form one current, having an intensity equal to the sum of their intensities. This I have proved by using a coil of very fine insulated iron wire, about 10,000 feet in length, as the core of a copper coil. When the connexion between the ends of the copper coil and a single cell was broken, sparks about one-twelfth of an inch passed between the ends of the thin iron wire without using a condenser. Fourthly, by connecting the primary coil with a condenser, I have found that the intensity of the current in the core is increased as it is in the current of the secondary coil. Fifthly, by connecting the ends of the core or iron coil with a Leyden jar, the length of the spark is diminished and its brightness increased. The effect of the condenser on the currents in the core may assist us in understanding the action of the condenser, which has not yet been satisfactorily explained.

A core consisting of a coil of insulated iron wire, has not only the advantages of being free from the five defects to which all the cores in common use are subject, but it will also enable us to get electrical currents having at the same time great intensity and considerable quantity, and may therefore be very advantageous for working the Atlantic Telegraph, and for producing the electric light. If we make a core of thirty covered iron wires, each one-eighth of an inch thick and 100 feet long, and wind over the iron coil a covered copper wire one-fourth of an inch thick, we can, with the aid of two cells and a suitable condenser, obtain thirty electrical currents, each having a considerable quantity of electricity, because the wires are short and thick, and an intensity greater than that which is required for the electric light. Sixty covered iron wires, of the same length and thickness as those in the core, may be rolled on the copper coil. Another coil of copper wire, one-fourth of an inch thick, may be put over the second iron one, and over this copper coil we may wind sixty or eighty covered iron wires, each 100 feet long and one-eighth of an inch thick. Then the innermost iron coil will be the core of the first copper one; the second iron coil will be the secondary coil of the first copper coil, and the core of the second; the third iron coil will be the secondary coil of the second copper coil. If the copper wires be connected with a battery of six cells, each about 5 inches square, and a condenser of sufficient size, an enormous magnetic power will be given to the 150 or 170 wires of the iron coils; and consequently 150 or 170 electric currents of considerable quantity and intensity will be produced as often as the connexion between the copper wires and the battery is broken. If necessary, the number of iron coils, and therefore the number of electric currents, may be increased. Mr. Shephard has got a brilliant electric light from eighty electric currents produced in coils of copper wire on the armatures of permanent magnets. I think that 150 currents produced by the coil I have described would far exceed in quantity and intensity the eighty currents obtained from Mr. Shephard's machine.

The electric light may perhaps be produced by several coils, like the one I showed at the meeting of the Association, and which has given sparks the $\frac{15}{16}$th of an inch, with one cell and without a condenser. The secondary coil is divided into four parts, each of which will give sparks about a quarter of an inch. I intend to make four or five other coils of equal power, and to divide the secondary coil of each into six or eight parts. The ends of the wire of each part will be left projecting from the coil. Thus in the five or six coils there will be between thirty and forty small secondary coils, each containing about 8000 or 10,000 feet of fine iron wire. Each of these secondary coils will give sparks at least one-eighth of an inch, with a battery of five or six cells and without a condenser. With a good condenser we may fairly expect that each will give sparks nearly 2 inches in length. Thus with a battery of five or six cells I think I shall have between thirty and forty currents, each capable of passing through about 2 inches of air. If the opposite ends of the thirty or forty small coils be connected with the opposite coatings of several large Leyden jars, and the sparks be passed between two coke-points, a brilliant light may be produced. Besides the coil which I have described, and which was divided into four parts, I made another which was 40 inches long, was divided into nine parts, and in which there were at least 70,000 feet of fine iron wire. Unfortunately, the secondary coil was seriously injured before I was able to make a single trial of its power. In dividing the two coils into several parts, I had three objects in view. First, to secure better insulation. The division of the secondary coil for the purpose of preventing the passage of sparks from one layer of the coil to the layer above or below it, was first recommended by Professor Poggendorff. Although this mode of preventing sparks within the coil occurred to myself before I saw his excellent paper on the induction apparatus, I was doubtful whether it would be of use, until I tried it in the last coil I made. My second object in dividing the secondary coil into parts, was to try the combined effect of the currents produced in each part by connecting the beginnings of all the parts with one coke-point, and all the ends with another. My third object was to try the effect of a Leyden jar connected with each part of the secondary wire, as well on the sparks produced by the part itself, as on the sparks produced by the whole secondary coil.

In order to get currents of considerable quantity, and at the same time of very great intensity, the core and secondary coil should be one continuous wire, about one-eighth of an inch thick, and the end of the core should be connected with the beginning of the secondary coil. I made a flat coil of covered iron wire one-eighth of an inch thick. The length of the coil was about 18 inches, its breadth 14, and its thickness between 4 and 5 inches. The length of the wire was about 2000 feet. On this iron coil I wound 150 feet of copper wire nearly one-fourth of an inch thick. By connecting the ends of the copper wire with a battery of two or three 4-inch cells, and a condenser in which the surface of each plate was 400 square feet, sparks about the twentieth of an inch would be made to pass between the terminals of the iron core. I have reason to think that had the condenser been only one-third or one-fourth of the size, the sparks would have been longer. When the ends of the iron core were connected with a condenser in which the acting surface of each plate was about fifty square feet, and in which the plates were insulated from each other by waterproof gutta-percha cloth, the current passed from one plate of the condenser to the other as freely as if they were connected by a good conductor. When the terminals were connected with three large Leyden jars, the brightness of the spark was increased, whilst its length scarcely suffered any diminution. I intended, but had not time, to coil over the copper wire another iron one of great length, and the same thickness as the one in the core, and to unite both together. Had I been able to do so, the combined currents of the core and secondary coil would form one of enormous intensity and considerable quantity. Two coils of this kind, each having a bar of iron in the inner iron coil, and having the ends of the iron bars connected by iron armatures, in the same way as in Mr. Whitehouse's coils, would, I think, answer better than his for the Atlantic Telegraph.

It appears to me that Mr. Whitehouse's coils admit of three important improvements. First, they may be greatly improved in the core by substituting for his secondary coil of copper wire a coil of covered iron wire of the same length and thickness. The iron wire would be intensely magnetized by the primary current, and by the inductive magnetic power of the enclosed iron bar; and in losing its magnetism at the moment the battery connexion is broken, a current will be induced in it of far greater intensity than that of the secondary current in Mr. Whitehouse's coil. Mr. Whitehouse's object in connecting the ends of one core with the ends of another by iron armatures, is to prevent the rapid suspension of the magnetic power of the cores at the moment the connexion between the battery and primary coil is broken. By causing the cores to lose their magnetism gradually, a series of currents corresponding to the successive diminutions of magnetic power is induced in the secondary coil: this series of currents has the effect of a continuous current, which is found to be of use in working the telegraph. The same object may be attained by using a core consisting of an iron bar and a coil of insulated iron wire. The iron bars may be connected by iron armatures extending over the ends of the iron coils, but separated from them by a piece of gutta-percha about the one-fortieth of an inch in thickness. Mr. Whitehouse's object might perhaps be attained still better by connecting the cores of every two coils, by six or seven, or a greater number of armatures. This may be done by brazing or otherwise fastening to the iron bar in each coil, plates of iron about a quarter or three-eighths of an inch thick, and sufficiently large to project an inch or two beyond the iron coil of the core. A small piece should be cut out of each plate, that the primary wire may pass from one side of the plate to the other. The corresponding plates fastened to the two iron bars may be connected by a plate of iron. Thus the two iron bars will have as many armatures as iron plates, and the magnetic power of the core will be retained longer than if there be only two armatures, and consequently the series of induced currents will continue for a longer time. Secondly, a great improvement may be made in the primary coil. Mr. Whitehouse's primary coil consists of twenty-four copper wires, No. 14, or about the $\frac{1}{11}$th of an inch thick, and 100 feet long. Now if the primary coil were made of copper wire of the same length, and nearly half an inch thick, it would conduct as much electricity as the twenty-four wires used by Mr. Whitehouse, and would produce greater magnetic power in the core, because the electricity flowing in the thick wire would be nearer to the core than the electricity flowing through the twenty-four thin wires. A third improvement may be made by winding over the primary coil an insulated iron or copper wire of the same length and thickness as the wire in the core, or of greater length, and uniting the end of the coil in the core with the beginning of the coil outside the primary coil. Such a coil would produce with a given battery a current of far greater intensity than that which would be produced by one of Mr. Whitehouse's coils, or a current of equal intensity with a much smaller battery. It appears to me, then, that the use of coils such as I have described would be greatly to the advantage of the Atlantic Company, or any company having a very long telegraphic line.

The fifth form of core which I used consisted partly of a coil of insulated iron wire, and partly of a bundle of iron wire. In one core of this form the iron wire of the coil was about the $\frac{1}{100}$dth of an inch, in another it was one-eighth of an inch thick. From the part of the core which consisted of iron wire $\frac{1}{100}$dth of an inch thick, I got sparks a quarter of an inch with a single cell and without a condenser. The length of wire in this coil was about 15,000 feet.

The sixth form of core which I used consisted of two concentric coils of insulated iron wire: one of very fine, the other of thick wire. The coil of thick wire should be enclosed within the coil of fine wire, and should be nearly 2 or 3 inches in diameter, especially when the primary coil is made of thick wire. In making coils of thick iron wire, great care is necessary, for in such wire there are cracks or flaws. At these cracks there are sometimes sharp points, which cut the covering of a spiral in an adjoining layer, and thus make a complete circuit, which is most injurious to the intensity of all the currents induced in the various parts of the coil. It is necessary to know that the complete circuit which diminishes the intensity of the secondary current in the greatest degree, is that which is made by connecting the ends of a coil of thick wire. I have not had time to determine which of the forms of core I have used induces the most intense current in the secondary coil, or which of them makes the condenser act with the greatest effect. I once used for the core a bundle of wires, 9 inches in diameter and 26 inches long. The weight of the core exceeded two hundred and a half pounds. This core acted so badly, as to convince me that anyone who wishes to obtain currents of very great intensity, or very long sparks, should never employ cores of very large diameter.

The fourth result is an improved method of insulation for the secondary coil. In this mode the insulation is imperfect where imperfect insulation is sufficient, and perfect where such insulation is required, and consequently each spiral is brought nearer to the core, to the primary coil, and to the other spirals of the secondary coil, than in the ordinary manner of insulation, in which the parts of each layer for which very little insulation is required are as well insulated from the layer above and below it as the parts which require the best insulation. My mode of insulation differs from the ordinary one in two respects:—First, in the insulation of each spiral from the adjoining ones in the same layer; secondly, in the insulation of the spirals of every layer from the contiguous spirals of the layer above it. I do not cover the fine wire with thread of any kind; but I coat it with a very thin film of varnish by drawing it through melted rosin and bees-wax. I draw it through the hot varnish by winding it on the coil at the distance of about 25 feet from the stove by which the varnish is heated; I have found that at this distance the varnish is cool and hard, even when the wire is drawn through it at the rate of 8000 feet in the hour. Thus in this mode of insulating the fine wire, a coil may be made in a comparatively short time. The insulation is sufficient, because the difference between the intensity of any spiral and the adjoining ones of the same layer is indefinitely small. On every inch of each layer I can put eighty or eighty-two spirals of wire $\frac{1}{100}$dth of an inch thick. My mode of insulating the spirals of each layer from those of the layer above or below it, differs also from the way in which they are insulated by others. In the common mode of insulation, if, as in Mr. Gassiot's great coil, five thicknesses of gutta-percha, or of any other insulating substance, be thought necessary in order to insulate the extreme spirals of any layer from those of the layer below it, five thicknesses of the insulating substance are put between the whole length of every two adjoining layers; so that if there be twenty layers along with the first, there will be 100 thicknesses of the insulating substance. But, in my mode of insulation, there would, in such a case, be only sixty. In order to render my mode of insulation intelligible, I shall explain how the first layer of spirals is insulated from the second, and the second from the third. Every other layer, such as the third, fifth, seventh, &c. represented by an odd number, will be insulated from the one above it, in the same way as the first is insulated from the second; and every layer, such as the fourth, sixth, eighth, &c. represented by an even number, will be insulated from the one above it, in the same way as the second is from the third. In insulating the first layer from the second, when five thicknesses of the insulating substance to be used are deemed necessary for the insulation of the last spirals of the second layer from the first spirals of the first (there the difference of intensity is greatest), I divide the length of the layer into five equal parts. I then put one thickness of the insulating substance (let us suppose it to be what I use, viz. the paper employed for copper-plate engravings saturated with a solution of gutta-percha in oil) on the entire length of the first layer, and then roll the fine wire on one fifth of the layer. I next cover the whole length of the coil with another thickness of prepared paper, and coil the fine wire on the second fifth of the layer. I then put on a third thickness of paper, and wind the wire on the third fifth of the coil. I then put on another thickness of paper, and coil the wire on the fourth fifth, and so on. Then between the first fifth of the second layer and the spirals below it in the first, there is one thickness of paper; and one will insulate them as well as five will insulate the whole length of the two layers from each other. Between the second fifth of the second layer and the part of the first layer below it, there are two thicknesses of paper, and they will sufficiently insulate these two parts from each other. In the same way the third is insulated by three thicknesses, the fourth by four, and the last by five thicknesses of paper: thus the five parts of the coil are as well insulated from each other as if there were five thicknesses between the entire length of the two layers. To insulate the second layer from the third, as well as the first is insulated from the second, only one thickness of paper is necessary; for by putting a single thickness of paper on the second layer, the first fifth is covered by one, the second by two, the third by three, the fourth by four, and the last by five thicknesses of paper. Hence to insulate any two layers, only six thicknesses of the insulating substance are necessary, or three for the insulation of each layer; and therefore to insulate twenty layers, only sixty thicknesses of the insulating substance to be used are required. Thus in my mode of insulation, every spiral in the secondary coil is brought nearer to all the contiguous spirals and to the primary coil and core, than in the ordinary method of insulation; and consequently the inductive power of the core and of the primary coil on the secondary one, as well as the inductive power of the spirals of the secondary coil on each other, must produce a secondary current of far greater intensity in mine than in the common mode of insulation. The coil which was shown at the meeting of the British Association was insulated in the manner just explained. This coil and the contact-breaker, which will be presently described, were seen at work by Mr. Gassiot, Dr. Robinson, M. Foucault, Professor Rogers, and other members of the Association. Mr. Gassiot was so much pleased with their action and construction, that he ordered from Mr. Yeates, an optician in Dublin, a contact-breaker and two secondary coils like mine. In each of these secondary coils there will be nearly 60,000 feet of iron wire about the $\frac{1}{100}$dth of an inch thick.

The fifth result is a contact-breaker in which the striking parts are copper, and which act as well as if they were platina. The contact breaker consists, first, of a small electro-magnet; secondly, of its armature screwed to a board moveable on a hinge, and having attached to it a spring connected with the vibrating piece of copper; thirdly, of a spring for pressing the striking pieces together; and of a trough containing oil, in which these pieces are always immersed. By means of the spring attached to the board to which the armature is fastened, the armature is brought within the most convenient distance from the small electro-magnet. The spring presses the striking pieces together with the greatest force the electro-magnet is capable of overcoming, and the pressure is exerted immediately over the points of contact. The oil prevents in some measure the oxidation of the copper, and serves to stop the battery current more quickly; for as soon as the pieces of copper are separated, the oil rushes in between them, and being a non-conductor, instantly stops the galvanic current from the battery. In the first contact-breaker which I made of this kind, there were two vibrating pieces, one of platina, the other of copper; the former struck against another piece of platina, the latter against a piece of copper: the copper was immersed in oil. By means of two screws, both might be made to make and break contact together, or I could cause either to make and break contact. By first causing the platina, and afterwards the copper, to make and break contact, I found that the copper acted as well as the platina. In the contact-breaker which I showed at the meeting in Dublin, there were three vibrating pieces of copper, each about three-eighths of an inch thick. M. Foucault thinks that the contact will be made and broken as well by one as by several vibrating pieces. Though that should be the case, the addition of two other pieces will not be useless; for the three may be immersed in different fluids, and thus we can discover the fluid in which contact may be made and broken with the greatest advantage.

The sixth result is a more satisfactory explanation of the condenser. In order to understand the action of the condenser, we must examine the electrical state of the primary coil at the moment its connexion with the battery is broken, and the effect which this state has on the core and secondary current. At the moment the connexion between the battery and primary coil is broken, the electricity which it received from the battery continues to flow to the end of the coil to which it was moving; but being no longer urged forward by the battery, its velocity is constantly diminished by the resistance of the wire. This electricity moving more slowly than when the coil and battery were connected, and in the same direction as the battery current, is not able to maintain in the core, or in the primary or secondary coil, the magnetic power produced in them by the battery; but it maintains a part of it, and prevents the core, the primary and secondary coil, from losing their magnetism in an instant, and consequently diminishes the intensity of the secondary current. The condenser prevents the gradual diminution of the velocity of the electricity flowing in the primary coil at the moment its connexion with the battery is broken, and probably accelerates it; for in an instant after the battery connexion is broken, the end of the coil towards which the electricity is moving, and the plate of the condenser connected with it, become positive. This positive plate instantly renders the other plate negative: the latter then attracts electricity to the former with an enormous force, on account of their very close proximity; and if the plates of the condenser be of sufficient size, the electricity moving in the primary coil will be drawn with such force to the positive plate, that its velocity, instead of being diminished, will probably be increased. Thus the condenser removes the obstacle arising from the electrical state of the primary coil, to the sudden suspension of the magnetism of the core, and of the primary and secondary coil, and probably increases their magnetic power by accelerating the current in the primary coil after its connexion with the battery is broken. The condenser not only removes an obstacle to the instantaneous suspension of the magnetism of the core and of the primary and secondary coil, but it also supplies a force tending to destroy that magnetism; for as soon as all the electricity moving in the primary coil is drawn to the positive plate of the condenser, it instantly rushes back to the negative one through the primary coil, and is drawn to itself by that plate with an immense force; and in its passage through the primary coil, tends to reverse the magnetic poles of the core of the primary and secondary coil, and consequently to destroy their magnetism. Hence the effect of the condenser is to make the core, the primary and secondary coil, lose their magnetism instantaneously, and thus to increase the intensity of the secondary current, or the length of the sparks produced by that current. This explanation of the action of the condenser is confirmed by the effect which I have found it to produce on the electrical currents induced by the primary current in each section of the core, viz. an increase of their intensity. Now the only causes that can produce an increase of intensity in these currents are, an increase of the magnetism of the core, and of the rapidity with which it loses its magnetic power, or either of these two causes.

I have shown that the effect of the condenser is at least to increase the rapidity with which the core loses its magnetism, and probably to increase its magnetic power. Hence my explanation of the action of the condenser is confirmed by the effect of the condenser on the currents produced in each section of the core.

The principle of this explanation of the action of the condenser suggested to me a new form of condenser, which I expected would act more powerfully than the condenser now in use. The new condenser was to consist, not of sheets of tinfoil, but of a large number of very thin sheets of iron, arranged in such a way that one-half of them would form one plate, and the other half the other plate of the condenser; and that the electrical current by which each plate would be charged, one positively, the other negatively, should not enter simultaneously each of the iron sheets forming the positive plate, nor leave simultaneously all the iron sheets in the negative one, but should flow through the whole length of each sheet, before entering into the next. In order to make a condenser of this kind, I got 112 sheets of iron, each 28 inches long, 10 broad, and about $\frac{1}{80}$th of an inch thick. I intended to arrange them so that the current by which they would be charged, at the moment the connexion between the primary coil and battery would be broken, should flow successively through the whole length of the 112 iron sheets, or through one plate equal in length to the sum of their lengths, which exceeds 250 feet. Had I had time to make, as I intended, our iron condenser in this way, the iron plates would be strongly magnetized by the electrical current flowing through their entire length; and in losing their magnetism, would produce a powerful secondary current, tending to destroy or to reverse the magnetism of the core, and thus increase the intensity of the secondary current. In the ordinary condenser there is one electrical current tending to destroy the magnetism of the core: in an iron condenser made as I have described, there are two currents tending to produce the same effect; viz. the current arising from the rush of electricity from the positive to the negative plate of the condenser, and the current caused by the demagnetization of the iron plates. In order to save time and trouble, I made our iron condenser in the ordinary way. When I have leisure I may make it in the manner I have just described.

The seventh result consists in the discovery of some new facts regarding the condenser, which have not been hitherto noticed in any publication. First, I have found that the action of the condenser is feeble when the core is a solid bar of iron; secondly, when it is a coil of fine insulated iron wire not having a bundle of iron wire, or a coil of thick covered iron wire in the hollow part of it; thirdly, when the quantity of iron in the core is very great compared with the thickness of the primary wire. Secondly, I have found that the size of the condenser must be increased with the conducting power of the primary wire. Thus a thick primary wire requires a larger condenser than a thin one; a primary wire of copper requires a far larger condenser than one of iron of the same length and thickness; and a very short primary wire of any metal requires a condenser very much larger than that which is necessary for one of the same metal 100 feet long. I have found that when the primary wire is not more than 30 feet, a condenser of moderate size will not produce the slightest effect on it. Thirdly, I have found that when a condenser is very much larger than that which is required to produce the full effect of a condenser on a given coil, it not only does not increase the power of the coil, but it makes it less than it would be without a condenser, and sometimes destroys it; and that in general there is a limit to the size of the condenser, beyond which its effect on the coil will be diminished. Fourthly, I have found that a condenser so large as to diminish the power of a coil excited by one cell, will increase its power when the coil is excited by ten or twelve cells. Hence the same condenser will not answer for the same coil when batteries of very unequal powers are used. Then, every condenser should be made in such a way that the entire of it will produce the full effect of a condenser on the coil for which it is intended when the largest battery we wish to use is employed, and that a small or a large part of the condenser may be used when we wish to excite the coil by a weak or strong battery. I learned from Mr. Gassiot and M. Foucault during the late meeting of the Association, that they were aware of the necessity of making the condenser in this way.

Maynooth College,
Sept. 29, 1857.

P.S. I have abstained from saying anything about the primary coil, because my experiments on it have not led me to a satisfactory conclusion, and not because I think the primary coils in common use incapable of improvement. I believe that they are very badly calculated to attain their object, and that they have been made on a false principle.

XXXIX. *On the Gyroscope.* *By* JOHN BRIDGE, *M.A.*[*]

THE gyroscope, as usually constructed, consists of a solid of revolution, whose axis forms a diameter of a circular ring into which its extremities a, a' are inserted: this ring is move-

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[*] Communicated by the Author.

able about a diameter at right angles to the axis of the solid by means of pivots b, b' inserted into another ring; and this second ring is moveable about a fixed vertical diameter $c\,c'$ at right angles to $b b'$.

The facts to be explained are the following:—

1. When the solid is in rapid rotation about $a\,a'$, let a weight be hung to the first supporting ring at a. Then, instead of an accelerated motion about $b b'$, we have a uniform motion about cc'. The motion of a is in a direction a right angle *in advance* of that in which the weight alone would have caused it to move. The rate of this precessional motion about the axis $c\,c'$ does not depend on the elevation of the axis $a\,a'$, but is increased when the force applied is increased, or when the rate of rotation of the solid of revolution is diminished.

2. When the outer ring, or the axis $b\,b'$, is fixed, $a\,a'$ will only be moveable in a vertical plane. If in this case, while the solid is in rapid rotation, a force be applied at a, it will produce very nearly the same effect as if the solid had been previously at rest.

3. If $a\,a'$ be allowed to move with great freedom in one plane only, fixed with respect to the earth, the axis $a\,a'$ will, during the rotation of the solid, oscillate about the meridian line of the plane in a manner similar to a common pendulum, the time of oscillation being a minimum when the plane in which $a\,a'$ lies is parallel to the earth's axis.

The following investigation contains an approximate solution of these problems.

From Earnshaw's 'Dynamics,' art. 257, it follows, that if C be the moment of inertia about the axis of figure of a solid, and A that about any other principal axis, when the body is set in rotation with velocity ω about an axis making an angle θ with the axis of figure, the effect of the centrifugal forces is the same as that of a couple, from which the angular acceleration of the body originally at rest would be $\frac{C-A}{A}\omega^2 \sin\theta\cos\theta$, tending to bring the principal axis towards the position of the instantaneous axis. If θ be small, this is nearly equal to $\frac{C-A}{A}\omega^2\theta$, which I will call $c\omega^2\theta$.

The First Problem.

Let A be any point of the horizontal great circle on a sphere concentric with the solid, i the extremity of the instantaneous axis, a the extremity of the principal axis.

ϕ, ψ the spherical coordinates of i.
ϕ', ψ' the spherical coordinates of a.

Also let $ia=\theta$, and let ia make an angle χ with the vertical circle through i. Then α being the angular acceleration which would result from the given weight if it were applied at right angles to the axis at a, we have (Airy's Tract on Precession, art. 12),

$$\frac{d\phi}{dt}=\frac{\alpha}{\omega}-c\omega\frac{\theta\cos\chi}{\cos\psi}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(A)}$$

$$\frac{d\psi}{dt}=-c\omega\theta\sin\chi; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(B)}$$

also

$$\frac{d\theta}{dt}=-\sin\chi\cos\psi\frac{d\phi}{dt}+\cos\chi\frac{d\psi}{dt}; \quad . \quad . \quad . \quad . \quad . \quad \text{(C)}$$

since the change in θ arises entirely from the change in position of i, and not from the rotation about i. And

$$\theta\frac{d\chi}{dt}=\omega\theta-\cos\chi\cos\psi\frac{d\phi}{dt}-\sin\chi\frac{d\psi}{dt}; \quad . \quad . \quad . \quad \text{(D)}$$

the change in χ resulting in part from the rotation about i, and in part from the change in position of i.

By substituting (A) and (B) in (C) and (D), we obtain

$$\frac{d\theta}{dt}=-\frac{\alpha}{\omega}\sin\chi\cos\psi,$$

$$\theta\frac{d\chi}{dt}=-\frac{\alpha}{\omega}\cos\chi\cos\psi+(1+c)\omega\theta.$$

If $\theta\cos\chi=u$ and $\theta\sin\chi=v$, the preceding may be replaced by the system of equations

$$\frac{du}{dt}=-\omega(1+c)v,$$

$$\frac{dv}{dt}=+\omega(1+c)u-\frac{\alpha}{\omega}\cos\psi,$$

$$\frac{d\phi}{dt}=\frac{\alpha}{\omega}-\frac{c\omega u}{\cos\psi},$$

$$\frac{d\psi}{dt}=\quad -c\omega v.$$

Also

$$\cos\psi'\frac{d\phi'}{dt}=\omega u,$$

$$\frac{d\psi'}{dt}=\omega v.$$

Whence

$$\frac{d\psi}{du}=\frac{c}{1+c},\ \psi-\mu=\frac{c}{1+c}(u-\rho),\ \text{if } \psi=\mu \text{ when } u=\rho;$$

$$v\frac{dv}{d\psi}=\frac{\alpha\cos\psi}{c\omega^2}-\left(\frac{1+c}{c}\right).u$$

$$v^2=\frac{2\alpha(\sin\psi-\sin\mu)}{c\omega^2}+\rho^2-u^2,\ \text{if } u=\rho \text{ when } v=0.$$

v is a maximum or minimum when

$$\psi=\mu-\frac{c}{1+c}\rho+\frac{\alpha c}{\omega^2(1+c)^2}\cos\mu \text{ very nearly};$$

then

$$u=\frac{\alpha\cos\mu}{(1+c)\omega^2} \text{ and } v=\pm\left(\rho-\frac{\alpha\cos\mu}{\omega^2(1+c)}\right).$$

From this it follows that θ continues to be of a magnitude so small (for ρ and $\frac{\alpha}{\omega^2}$ are supposed very small) that $\sin\theta$ may still be regarded as equal to θ.

Again,

$$\frac{d^2v}{dt^2}=-\frac{\alpha}{\omega}\sin\mu.c\omega v-\omega^2(1+c)^2v;$$

whence

$$\left(\frac{dv}{dt}\right)^2=\left(\omega^2(1+c)^2+\alpha c\sin\mu\right)\left(\overline{\rho-\frac{\alpha\cos\mu}{\omega^2(1+c)}}\Big|^2-v^2\right);$$

and if it is supposed that $t=0$ when $v=0$,

$$v=\left(\rho-\frac{\alpha\cos\mu}{\omega^2(1+c)}\right)\cos\omega'(1+c)t,$$

where ω' is very nearly equal to ω, $\frac{\alpha}{\omega^2}$ being very small. Also

$$u=\frac{\alpha\cos\mu}{\omega^2(1+c)}-\left(\rho-\frac{\alpha\cos\mu}{\omega^2(1+c)}\right)\sin\omega'(1+c)t;$$

whence

$$\frac{d\phi}{dt}=\frac{\alpha}{\omega}-\frac{c\alpha}{\omega(1+c)}+\left(\frac{c\omega\rho}{\cos\mu}-\frac{\alpha c}{\omega(1+c)}\right)\sin\omega'(1+c)t.$$

The motion of the extremity of the *principal* axis is given by

$$\frac{d\phi'}{dt}=\frac{\alpha}{\omega(1+c)}-\left(\frac{\omega\rho}{\cos\mu}-\frac{\alpha}{(1+c)\omega}\right)\sin\omega'(1+c)t,$$

$$\frac{d\psi'}{dt}=\qquad\cos\mu\left(\frac{\omega\rho}{\cos\mu}-\frac{\alpha}{(1+c)\omega}\right)\cos\omega'(1+c)t.$$

Hence the motion of the extremity of the principal axis may be considered as compounded of a precessional motion with velocity $\frac{\alpha}{\omega(1+c)}$ about a vertical axis, and a nutational motion in a small circle of radius $\left(\frac{\rho}{1+c}-\frac{\alpha\cos\mu}{\omega^2(1+c)^2}\right)$. The expression $\frac{\alpha}{(1+c)\omega}$ represents all which it was required to account for. Also, since $(1+c)=\frac{\text{C}}{\text{A}}$, its value is $\frac{\alpha\text{A}}{\text{C}\omega}$; and αA is proportional to the moment of the force applied, so that the expression becomes $\frac{m}{\text{C}\omega}$, which is independent of the moment of inertia about an axis at right angles to the axis of figure.

That the rate of precessional motion does not depend upon the moment of inertia about a principal axis at right angles to the axis of figure, may be illustrated by means of a simple apparatus such as the following:—Two equal circular boards are made to slide on a steel rod passing perpendicularly through their centres. They are placed at equal distances from the middle of the rod, where there is a groove running in a socket in which it may be made to rotate, and by which it is freely suspended. When this is weighted, it will be found that the rate of precessional motion is independent of the distance of the boards from the groove. This may be best judged of by the constancy of the ratio which the square of the number of seconds, or ticks of a watch, in which the precessional motion increases by a given quantity, bears to the number of turns.

In the preceding investigation, the change in the value of ω, arising from the weight being applied to the principal axis instead of the instantaneous axis, is neglected. This will easily be seen not to affect the approximate results.

The Second Problem.

Let the axis $a\,a'$ be now free to move only in the plane A $a\,a'$; this plane will then be pressed upon at the points a, a' in a direction perpendicular to the plane, and an equal and opposite pressure will be exerted on the axis at the points $a\,a'$; let the angular acceleration due to these pressures be β, and that due to the external force applied in the direction of the plane, α.

The equations A, B then become

$$\frac{d\phi}{dt}=\frac{\beta}{\omega}-c\omega u,$$

$$\frac{d\psi}{dt}=\frac{\alpha}{\omega}-c\omega v;$$

whence we easily find

$$\frac{du}{dt}=\frac{\alpha}{\omega}-\omega(1+c)v,$$
$$\frac{dv}{dt}=-\frac{\beta}{\omega}+\omega(1+c)u,$$
$$\frac{d\phi'}{dt}=\omega u,\quad \frac{d\psi'}{dt}=\omega v.$$

Now $\frac{d\psi'}{dt}=0$, from the condition that the principal axis is to remain in the plane A a a'. Therefore

$$v=0,\quad u=\frac{\beta}{\omega^2(1+c)}=\frac{\alpha}{\omega}\cdot t,\text{ and }\frac{d^2\phi'}{dt^2}=\omega\frac{du}{dt}=\alpha.$$

The acceleration produced by the force is therefore constant; at least it may be so considered while θ remains small. If the force α ceases to act, ψ or u, and therefore β and $\frac{d\phi}{dt}$, will remain constant. In other words, if the principal axis of a solid of revolution is free to move in a given plane, it behaves nearly in the same manner under the action of forces, whether the body is in motion or not.

The Third Problem.

Let A a a' be the plane fixed with respect to the earth in which the principal axis of the body is free to move. Then the effect of the earth's rotation with velocity Ω may be found by resolving it into—

$\Omega\cos\gamma$ about the normal to the plane; $\Omega\sin\gamma\cos\phi'$ about the principal axis of the body, ϕ' being reckoned from the extremity of the meridian line of the plane, which is moving in the plane with velocity $\Omega\cos\gamma$; and $\Omega\sin\gamma\sin\phi'$ about an axis perpendicular to the other two.

The effect of the last component alone need be considered. From this it appears that the forces which act on the body must be such as to cause the extremity of the principal axis to move upwards, that is, perpendicular to the plane of constraint, with a velocity $\Omega\sin\gamma\sin\phi'$.

In the equations of the last problem, let α be 0, and

$$\frac{d\psi'}{dt}=\Omega\sin\gamma\sin\phi'.$$

Then we have

$$\frac{d\phi'}{dt}=\omega u-\Omega\cos\gamma,\quad \frac{du}{dt}=-\omega(1+c)v,$$
$$\frac{d\psi'}{dt}=\omega v,\qquad \frac{dv}{dt}=\omega(1+c)u-\frac{\beta}{\omega};$$

whence

$$\omega v = \Omega \sin\gamma \sin\phi',$$

$$\frac{du}{dt} = -\Omega \sin\gamma (1+c) \sin\phi',$$

and

$$\frac{d^2\phi'}{dt^2} = -\omega\Omega(1+c)\sin\gamma\sin\phi'.$$

This shows that at any instant the acceleration of the axis toward the meridian line of the plane is $(1+c)\omega\Omega\sin\gamma\sin\phi'$. The principal axis therefore oscillates about the meridian line with the law of the circular pendulum, and the time of vibration for a small arc is $\dfrac{\pi}{\sqrt{(1+c)\omega\Omega\sin\gamma}}$.

59 Stanhope Street, Hampstead Road,
September 19, 1857.

XL. *On the Time required by Compounds for Decomposition.*
By Thomas Woods, *M.D.*[*]

THE difference between quantity and intensity of a galvanic current has been long recognized. The former is generally taken to mean the effects produced by the current where no obstacle, or the least possible, exists; the latter the electromotive force, or its power of overcoming resistance. In order to come as near as possible to the cause of different degrees of intensity in different galvanic arrangements, let us analyse and compare any two; for instance, the usual voltaic pair of two metals and one fluid, zinc and copper in dilute sulphuric acid, and a Grove's pair consisting of two metals and two fluids, viz. zinc acted on by sulphuric acid, nitric acid being decomposed in contact with platina. The electromotive force of the latter is much stronger than that of the former; yet the only difference in the chemical actions taking place is, that at the platina end of the Grove's *nitric acid* is decomposed, whilst *water* is decomposed in the other. Now except for this difference, all things in the two arrangements being the same, if we measure the amount of electricity passing from one end to another of each circuit by means of a galvanometer, we shall find that the quantity as well as the intensity of the Grove's pair is greater than that of the other; the needle of the galvanometer will stand at three or four times as many degrees with the current of the former as with that of the latter. But, as might be expected, if we weigh the zinc both before and after the current has been passing for the same period

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[*] Communicated by the Author, having been read at the Meeting of the British Association in Dublin, August 26, 1857.

of time in each, we shall find that in the Grove's three or four times as much zinc has been dissolved as in the other. What, then, is the fact?—that in the Grove's pair the electricity passes with three or four times the *rapidity* it does in the pair with one fluid; and the only difference between the pairs being, that in the latter water is decomposed, and in the former nitric acid, it follows that *an equivalent of nitric acid is decomposed three or four times more quickly than one of water under like circumstances.*

To proceed more exactly. I put a porous vessel into a gutta-percha trough, and filled the latter with sulphuric acid, sp. gr. 1·1. In the sulphuric acid I placed an amalgamated zinc plate, and in the porous cell a platina plate surrounded by several solutions in succession, such as nitric acid, solution of sulphate of copper, solution of nitrate of silver, &c. These solutions in the porous cell suffered decomposition when the zinc and platina plates were joined externally, the base being thrown down on the platina plate. There was therefore no polarization; and I introduced a sufficient resistance externally to render the resistances of the solutions which were decomposed comparatively nothing. I had therefore several combinations similar in every way, except in the fluid that surrounded the negative or platina plate; and the only difference in the chemical actions of the pairs was in the *decompositions* which occurred. If, therefore, on transmitting the current produced by joining the metals through a galvanometer, there was any difference in the amount of electricity, this difference must have been occasioned by the decomposition alone.

Sulphuric acid surrounding the porous cell, and amalgamated zinc in the acid, I put the platina plate in the porous cell and filled it with nitric acid, thus forming a Grove's pair. When the metals were joined, a certain resistance and galvanometer being included in the circuit, the needle of the galvanometer stood at $10\frac{3}{4}$ degrees of the scale.

I now removed the nitric acid and substituted solution of sulphate of copper, thus forming a Daniell's pair: with the same resistance and arrangement as in the former experiment, the needle stood at $5\frac{1}{8}$ degrees.

With solution of nitrate of silver in place of the sulphate of copper, the needle stood at 9.

With solution of iodic acid, the needle stood at 10; with acidulated water, at $3\frac{1}{6}$.

Now each of these numbers represents the quantity of electricity passing in a given time, or its *rapidity*; and as the pairs only differ from each other in the decomposition cell, it follows *that an equivalent of each compound is decomposed in a different period of time.* The numbers likewise represent the electromotive force of the pairs; *this force is therefore synonymous with rapidity of current.* They also represent the difference between the heat generated at the positive end of the battery by the formation of sulphate of zinc, and that absorbed at the negative end by the decomposition. The latter statement may be proved by subtracting the amount of heat absorbed by decomposition in each particular case from that generated at the positive end. Regnault has proved it in a paper, published, I think, in the August number of the French 'Annals of Chemistry' for 1855; a paper I unfortunately did not see until I had made many experiments myself for the same purpose.

In all these pairs the same quantity of electricity is developed by the consumption of an equivalent of zinc, but the *time* it requires to make the circuit depends on the time the compound at the negative end takes to be decomposed; and this amount of time depends, again, on the quantity of heat which such decomposition absorbs. Now this proposition leads us to the interesting fact, that all compounds require the same time to absorb the same quantity of heat in decomposing into their elements. For instance, the decomposition of water absorbs 80* units of heat, that of nitric acid 16*, that of sulphate of copper 62*, of nitrate of silver 30*, &c.; and the time they require to do so is exactly proportional to these amounts.

I may here observe, parenthetically, that from the above we can understand the difference of facility with which compounds can be decomposed. All compounds absorbing much heat in decomposing, such as the alkalies, &c., require a comparatively long time to separate into their elements; whilst bodies which absorb very little heat, decompose at once and from the most trifling causes; for instance, all explosive compounds, such as the fulminates, hypochlorous acid, &c.: these, not requiring time to absorb heat when decomposing, allow the affinities which form the elements into new compounds to satisfy themselves most easily and rapidly.

But the *time* required by compound bodies to absorb heat in their decomposition is only equal under similar circumstances; for instance, when zinc is the positive pole of a galvanic couple, an equivalent of all compounds at the negative decomposes with a rapidity proportional to the quantity of heat they absorb. Thus nitric acid, which absorbs only about one-third of the heat that water does, decomposes three times as quickly, and hence an equivalent of zinc is dissolved in one-third of the time in a Grove's pair that it is in a Daniell's pair; and an equivalent of electricity passes with proportionate quickness. But if potassium be made the positive metal in one of the pairs, the rapidity of

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* Proceedings of the Royal Society, January 1857.

current increases in proportion to the increased affinity of that metal for oxygen. For it seems a curious fact, that some elements, when combining, evolve heat more or less quickly than others; but in separating, the heat necessary for decomposition is absorbed equally; and the greater the quantity of heat an element generates in combining, the more rapidly it evolves it. Hence, as was mentioned before, the rapidity of the electric current depends on the difference of amount of heat evolved at one end and absorbed at the other of the circuit. If a very rapid current is required, a metal which produces the greatest amount of heat is made the positive pole, and a liquid which absorbs the least quantity of heat is placed round the negative one; both ends thus entering into the development of the current.

Faraday says that the current always sets out from the metal most acted on; but this does not always hold good. It is quite possible to have a metal easily acted on by an electrolyte which is a good conductor, and also to have a good negative pole, and yet no current of electricity, because the *decomposition* may not be such as to allow the current to pass. For instance, copper placed in nitric acid in a porous cell, and platina in sulphuric acid around it, gives no current of electricity, although the copper is quickly acted on, because the *decomposition* of the water at the negative end requires more *time* to be accomplished than the combination at the positive end. If the position of the metals be reversed, a strong current passes. If nitric and sulphuric acid be separated by a porous diaphragm, and a plate of copper be placed in each, the copper in the sulphuric acid acts as the positive metal, although sulphuric acid attacks the metal much less strongly than the nitric acid. Whenever, therefore, we calculate the galvanic effect of a pair, one end of the circuit is of just as much consequence as the other, whether we regard the amount or direction. The electricity is developed at the positive end, but it must pass through the negative end; and on the latter depends not only the rapidity with which it does so, but on it depends also whether it passes at all or not; as, if a fluid is used which takes more *time* to be decomposed than the elements at the positive end require to combine, no current can be produced.

One of the objects of this paper is to direct the attention of scientific men to the importance of the study of the heat of chemical combinations. If the heat produced by the combinations of all the elements were known, we should have a key to almost all the phænomena connected with the principles of chemistry. The reactions of bodies depend on, or are inseparably connected with, the amount of heat their combinations produce: in every instance the result of contact of bodies with respect to affinity could be predicted if the heat of combination of the elements were known; for, *cæteris paribus,* the bodies which produce most heat in chemically uniting always combine to the exclusion of others. This is, however, only a repetition of the truth proved by the experiments of Mr. Joule and myself, that the heat of combination is in proportion to the affinity of the combining bodies. If, therefore, I say, experiments were instituted to find the amount of heat evolved by the combination of the elements, the results would enable us in a great degree to calculate *à priori* chemical phænomena.

The method which I have described for finding the heat absorbed when a fluid is decomposed, viz. by making it the electrolyte in contact with the negative plate of a pair, and taking the needle of the galvanometer as the index of the current passing, and consequently of its rapidity, and therefore of the heat absorbed (as one depends on the other), offers, I think, great facility for experimenting on this subject; the galvanometer here acts the part of a thermometer of chemical action, the needle varying with the amount of heat absorbed by the decomposition, just as the mercury does in the common thermometer for changes of temperature. I endeavoured last year to find the amount of heat produced by the chemical combination of oxygen with several elements; an abstract of the experiments is published in the Proceedings of the Royal Society for January 1856. The results of the experiments show, that when oxygen combines with several bodies, the quantity of heat produced is not in each instance the same, but is given out in multiple proportion. If the heat of combination be known, we can also precalculate with precision the quantity and intensity of any proposed galvanic arrangement. We have only to find the difference of the amount of heat generated at the positive end by the combination, and that absorbed at the negative end by decomposition. I would just mention, that in this way I prejudged that solution of iodic acid used instead of nitric acid, would give the same quantity and intensity as the latter, when substituted for it in a Grove's pair; and I found by experiment that the idea was correct. The decomposition of iodic acid does not absorb more heat than that of nitric acid, and therefore allows the current to pass with equal rapidity; consequently the electromotive force is the same. However, as iodine is thrown down, this accumulates on the platina, and the conducting power of the plate is interfered with. Still, as a proof of the value of a knowledge of the heat evolved by chemical combinations in precalculating the result of galvanic arrangements, the experiment was successful.

In conclusion, I would beg to remark that the subject of this paper, viz. the *time* that compounds take to decompose, has not to my knowledge before been taken notice of; and I think it may not be unimportant, if only proving the *definite* nature of another principle connected with the constitution of matter. We know that all the "*forces*," such as electricity, heat, &c., are *definitely*, and after an invariable manner produced by the changes of matter. I have here endeavoured to show that the *time* in which these changes are brought about is also *definite*, and invariably and similarly related under similar circumstances. In "*Mechanics*," without taking into account the *time* in which work is done, no estimate can be formed of the moving power. If we are able to add to the result of chemical action the *time* in which it is performed, do we not advance a step in our investigations, and assimilate chemical to mechanical work more closely?

I would only further ask,—If the equivalents of all the elements produce the same quantity of electricity, as Faraday has shown, and very different amounts of heat, can heat and electricity be modifications of the same agency? For instance, an equivalent of zinc by its oxidizement always evolves an invariable amount of heat and an invariable amount of electricity; an equivalent of copper acts similarly; but whilst the amount of electricity is the same as that from zinc, the quantity of heat is only half as much. All other elements, by combining with oxygen, likewise develope various amounts of heat, but the same quantity of electricity. Can therefore these two imponderables be either modifications of the same force, or be related as cause and effect?

The propositions, then, I have endeavoured to prove in this paper are,—

That electromotive force is the same as rapidity of current;—that with the same positive pole the rapidity is determined by the amount of heat absorbed at the negative end by decomposition, as the more heat is absorbed the longer it requires for absorption.

And that every compound requires a definite and specific period of time for decomposition.

Parsonstown, August 1857.

XLI. *On a Sun-gauge.* By W. S. Jevons, *Assayer, Sydney Branch of the Royal Mint*[*].

It is, I believe, allowed that meteorologists are very imperfectly supplied with instruments for measuring the heating effect of the sun's rays. The actinometer of Sir John Herschel, though unexceptionable in principle, has been found very expen-

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[*] Communicated by H. E. Roscoe, Ph.D., M.A.

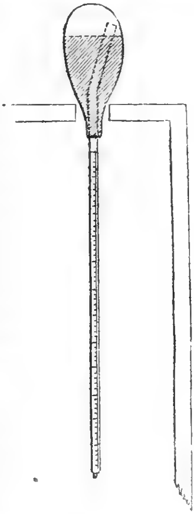
sive, and difficult to use; and the common black-bulb thermometer, which is generally employed, does not give results of any direct value or comparability.

But it appears to me, that even if we did possess a convenient true actinometer to give the intensity of the sun's rays at any given place and moment, that is, the *rate* of the sun's heating power, there is still a second instrument required in meteorology to measure the accumulated effect of the sun's rays during any given space of time. In short, the sun's heat should be gauged at every meteorological observatory as it arrives, day after day and year after year, precisely in the manner that falling rain is collected by the rain-gauge, and its accumulated amount measured at the end of any given period.

This instrument, which I propose to call a sun-gauge, is merely an adaptation of the invention familiarly known as *Wollaston's Cryophorus.* If the sun's rays be allowed to fall upon the surface of water contained in one of the bulbs of this instrument, the other bulb being sheltered from the sun but freely exposed to the air, the amount of water evaporated from the former and condensed in the latter bulb will afford a simple and perfect measure of the total amount of heat absorbed by the surface of water. For when pure water is contained in any exhausted vessel, the tension of the vapour therein can never remain above that due to the temperature of the coldest part of the vessel, otherwise rapid evaporation and condensation are produced until equilibrium is attained. When a moist surface therefore is exposed to the sun in a vacuous glass vessel, of which one part is sheltered from his rays and placed in a free current of air, the tension of the vapour within the glass can never rise perceptibly above that corresponding to the temperature of the air, and all the sun's heat absorbed by the exposed part will be carried over in a latent state into the sheltered part by the evaporation of a definite amount of water. The mere addition of a graduated measuring tube to Wollaston's cryophorus will thus convert it into a heat-gauge.

The only instrument which I have been able to make (without the aid to be obtained in London or other towns) is of a simple and rough form. It consists of an oblong glass-bulb, about 3 inches in length and $1\frac{1}{2}$ inch in diameter, into the mouth of which a half-inch glass tube is fused, in such a manner that a part of the tube reaches within the bulb almost to its opposite end, being bent, however, towards one side. The exterior part of the tube, 14 inches (or more) long, is straight, and graduated into *millimetres,* which read from the further end.

The water, which may be rendered opake by some fixed colouring matter, such as a weak solution of sulphate of indigo, is to be introduced in sufficient quantity to fill about two-thirds of the bulb, and the instrument being then perfectly exhausted of air by boiling, is completed by hermetically sealing the extremity of the graduated glass tube.



To use the sun-gauge, the whole of the liquid must first be poured into the bulb, which can readily be done on account of the bent position of the interior part of the tube. It is then inverted and placed in some support where the sun's rays may fall upon the bulb uninterruptedly throughout the day, while the tube beneath is completely sheltered from the sun, but freely exposed to the air. This may easily be done as shown in the figure, where the graduated tube hangs down into a box open only towards the side on which the sun never shines.

It is evident, that as long as the sun shines, or the heat of his rays is at all perceptible, water will evaporate from the bulb and condense colourless and pure in the graduated tube, by the divisions of which its amount may be quickly and easily read off. Except so far as an imperfection of the instrument, to be presently mentioned, may interfere, the results thus given by the same instrument will both be exactly proportional to the amounts of heat absorbed, and will also enable us to determine and express the absolute amount in a constant and convenient manner. But in constructing a number of instruments, the proportion of the sun's rays absorbed and gauged by the exposed bulb may not always be the same; so that it will be necessary to compare them against some standard instrument, and thus determine for each a correction factor which will reduce all results to complete uniformity.

Amounts of radiated solar heat may be conveniently expressed by the depth (in inches or millimetres) of evaporation from a surface of water exposed with perfect freedom to his rays, under the condition that all aqueous vapour of a greater tension than ·199 inch of mercury (the tension of the temperature of 32°) shall be immediately conveyed away. Or the relation of this *unit inch of water* to the *actine**, or "the abstract unit of solar radia-

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* The actine is "that intensity of solar radiation which, at a vertical incidence, and supposing it wholly absorbed, would suffice to melt one-millionth part of a metre in thickness from the surface of a sheet of ice horizontally exposed to its action per minute of mean solar time."—*Admiralty Manual*, p. 307.

tion," adopted by Sir J. Herschel, having been once determined, all the results could of course be equally well expressed according to this unit, leaving out the consideration of *time*. In practice, the vapour in the sun-gauge will not be removed by condensation in the sheltered part of the apparatus till its tension rises above that of vapour at the temperature of the air. As, however, the latent heat of vapour diminishes as the sensible heat rises, a small correction must be made according to the mean temperature of the air during the time of observation, as given by the following Table :—

Temperature of air.	Specific heat of vapour.	Correction factor.
32°	1092°	·000
42	1085	·006
52	1078	·013
62	1071	·020
72	1064	·026
82	1057	·032
92	1050	·038
102	1043	·045

If a = observed result of instrument,

$$\textit{True result} = a - a \times \textit{correction factor}.$$

One defect of the particular form of sun-gauge here described is, that when evaporation has proceeded to a considerable extent, the quantity of liquid in the bulb is diminished, and the absorbent area lessened in comparison with the surface of evaporation, so that the distillation of a millimetre's depth of water at the commencement of the exposure will indicate less amount of heat rays than the same distillation towards the end of the day. A definite and invariable horizontal surface of water is all that should be exposed to the sun's rays, and by an instrument of more complicated and perfect form this might perhaps be accomplished. If not, the inaccuracy might be experimentally determined for each instrument, and a tabular correction applied for each division of the graduated tube.

Likewise, in order to obtain very accurate and constant results, great care will be necessary with this instrument, as with the black-bulb radiating thermometer, that it may not be affected by heat communicated from the support or other neighbouring objects. This has not been much attended to with the present rough form of the sun-gauge.

The following are results obtained with the sun-gauge during two days' continuous observations :—

1857. Hours.	February 22.			April 26.			Sun-gauge. Mean.
	Temp. of air.	Black-bulb.	Sun-gauge.	Temp. of air.	Black-bulb.	Sun-gauge.	
A.M. 6·0	56·2	56·5	0	52·7	52·0	0	0
7·0	59·6	81·7	0	55·1	58·5	0	0
8·0	65·2	97·2	0	57·5	65·0	0	0
9·0	70·6	106·7	4	62·2	91·4	5	4·5
10·0	74·7	111·9	17	65·9	97·5	13	15·0
11·0	76·9	116·2	22	70·7	106·8	18	20·0
12·0	80·4	120·5	22·5	71·6	104·7	18	20·3
P.M. 1·0	79·3	115·2	19·5	72·9	111·5	18	18·7
2·0	76·9	107·4	17	73·4	112·8	15	16·0
3·0	75·3	93·2	9	71·8	106·0	15	12·0
4·0	74·9	95·9	2	70·4	99·0	8	5·0
5·0	72·4	77·2	0	66·9	67·7	2	1·0
6·0	70·1	70·9	0	62·1	60·7	0·5	0·3
Means and sums.	71·7	96·2	113·0	65·6	87·2	112·5	112·8

N.B. The place of observation is shaded till near 8 A.M. by elevated ground, hence the absence of indications before that hour. The days were neither quite cloudless.

The results of the sun-gauge given at each hour are the quantities, in millimetres, of water distilled since the last hour of observation.

The sun-gauge was refilled, with a better vacuum, between these two series of observations.

It must be borne in mind that the present paper is only intended to communicate the idea of an instrument which it would require the help of the most skilful optical instrument-maker to carry to perfection, or even to render of utility.

If the delicacy of the instrument be very much increased by enlarging the surfaces of absorption and evaporation and of condensation to a very great extent, compared with the divisions of the measuring tube, I see no reason why the instrument should not act as a simple and accurate *actinometer*.

The amount of distillation during one minute, for instance, accurately determined, will give the *rate* or *intensity* of the sun's rays, which may be supposed uniform during that period; just as the rate of expansion of a large bulb full of liquid in the actinometer of Sir J. Herschel is determined by watching and measuring its expansion in a capillary tube during five seconds of exposure to the sun's rays.

The total heating effect of the sun's rays falling on the earth's surface at the different latitudes and localities of the globe, varies immensely from several causes, the principal of which are,—

1st. The duration of the day, and the elevation of the sun.

2nd. The imperfect transparency of the atmosphere.
3rd. The screening effect of clouds of every thickness.
4th. The difference in the absorbent power of the ground, &c.

It is the variation in the temperature of the atmosphere thus produced which disturbs its equilibrium, and is the principal cause of all its great movements and changes; and it is surely therefore very desirable to possess an instrument which will enable us to estimate and record, however roughly, the actual amount and variation of this chief element in the science of Meteorology.

It may be observed that the sun-gauge will equally well measure terrestrial or night-radiation by distillation in the contrary direction, if a small quantity of liquid be left in the graduated tube. Or a special instrument of inverted form and action might be contrived, and continually exposed to the sky in the focus of a concave mirror, while being completely sheltered from the sun.

Royal Mint, Sydney, New South Wales,
May 14, 1857.

[Mr. Jevons is evidently not acquainted with the interesting researches of Pouillet (Poggendorff's *Annalen*, vol. xlv. for 1838) on the amount of the solar radiant heat falling on our globe. Pouillet gave a more perfect arrangement to Herschel's Heliometer; and by observing the amount and rate of increased temperature which a quantity of water contained in a metallic vessel exposed to the direct sunlight underwent, and observing also the rate of diminution of temperature when the sun's rays were cut off, he determined the coefficient of absorption for heat of the atmosphere, and hence calculated the thickness of a crust of ice which on the surface of the earth would be melted by the sun's rays in a day, and that which would on the sun's surface be liquefied by the solar heat.—Ed.]

XLII. *On the Influence of Metals upon Radiant Heat.* *By* H. Knoblauch, *Professor of Natural Philosophy in Halle*[*].

I.

With respect to radiant heat, the metals have been hitherto regarded as *adiathermanous* bodies, and they have therefore been employed as screens when it was necessary to cut off the calorific rays. With the thickness in which sheet-metal is found in commerce, this indeed might safely be done; but it decided nothing as to the real capability of metals to transmit radiant heat. This could only be ascertained by operating upon thin layers.

To test this question, M. Knoblauch first made use of a gold-

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[*] Abstracted from Poggendorff's *Annalen*, vol. ci. 1857.

leaf rolled out as thin as possible and stretched over a frame. As no terrestrial source of heat would be sufficiently intense, the leaf was placed in the track of the sun's rays, which were sent into a darkened room by a Silbermann's heliostat; a sensible action on a thermo-electric pile was the consequence. In order to augment the action, a glass lens was placed in the window-shutter. By this means a deflection of the astatic needle of a multiplying galvanometer connected with the thermo-electric pile was obtained, which amounted to 6 degrees.

A second experiment was made with a thin layer of mosaic gold, which was spread upon a smooth glass plate. The greater thickness of this layer reduced the deflection to 3 degrees.

Three gold precipitates, prepared chemically by Prof. Böttger of Frankfort, were peculiarly fitted for these experiments*. By transmitted light they showed a beautiful green colour, which deepened as the thickness of the layer augmented, while the leaf made use of in the experiment just recorded transmitted light of rather a bluish tint. The thicknesses of the layers were in the proportion 1 : 2 : 3. When the rays of heat, concentrated in the manner described, passed through these precipitates, which lay upon watch-glasses, the consequent deflections were,—for the thinnest, 33°·64; for the middle one, 4°·41; and for the thickest, 1°·42.

The same experiments were made with *silver*. Rolled leaves of this metal proved, however, too thick; and those only which were chemically prepared showed themselves capable of transmitting the radiant heat. The precipitates were transparent for blue light, the intensity of which indicated the thickness through which it had passed. Of four precipitates, the thickness of the strongest was about double that of the weakest. The deflections produced by the heat passing through these four layers, commencing with the thinnest, were 10°, 8°, 5°, and 1° respectively.

A similar result was obtained with platinum, which was also made use of as a thin coating upon the surface of a watch-glass. This permitted of the passage of the sun's rays to such a degree, that with a thin layer a deflection of 34°·7 was obtained, and with a thicker one a deflection of 8°·5.

Other metals than the above were either not thin enough, or not uniform enough to permit of similar experiments. The glass on which the precipitates rested could of course only have the effect of rendering the action more feeble.

But it might be asked whether the calorific rays pass really through the metal as they do through glass, crystals, or other diathermanous bodies; or whether they are not permitted to

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[* Faraday's method of preparing gold films might be turned to valuable account in such experiments as the above. See Phil. Trans. February 1857.]

pass through fine apertures in the leaves. Or the idea may suggest itself, that the thermo-electric pile was simply affected by the heat radiated by the exposed body itself, after it had been heated by the sun. At the end of this section a special proof shall be given that the action observed is not due to either of the causes referred to.

There is therefore no doubt that metals, such, for example, as *gold, silver, platinum, when in thin layers, are to be regarded as diathermanous bodies, which transmit calorific rays; the quantity transmitted naturally diminishing as the thickness of the metallic layer increases.*

It was of interest to ascertain whether the heat which had passed through the metallic leaves was of the same quality as the heat before its passage. To determine this, the heat was first examined before it had passed through the metal. The rays reflected from the heliostat were allowed to fall directly upon the thermo-electric pile, and the consequent deflection was observed. A diathermanous plate, for example a plate of yellow glass, was then interposed in the path of the rays, and the position of the needle was read off afresh. Supposing the first deflection to be expressed by 100, the latter, according to the observation, would be 59; a result which proved that for a direct quantity of heat equal to 100, a portion of it equal to 59 could pass through the yellow glass. In this way the heat was examined with different glasses: the following Table expresses the ratio of the incident to the transmitted heat in the respective cases:—

Yellow glass.	Blue glass.	Red glass.	Green glass.
100 : 59	100 : 41	100 : 46	100 : 20

The solar rays were next sent through a gold-leaf stretched over a frame, and afterwards allowed to fall upon the above glasses in succession: the object, it will be remembered, was to ascertain the ratio of the heat incident upon each glass to that which passed through it: the result is given in the following Table:—

Yellow glass.	Blue glass.	Red glass.	Green glass.
100 : 20	100 : 32	100 : 14	100 : 24

If these results be compared with the foregoing, remarkable differences appear. Before passing through the gold-leaf, the heat was capable of passing through the yellow glass in the ratio of 59 : 100; while, after crossing the metal, it was only able to penetrate the glass in the ratio of 20 : 100. A similar diminution of the ratio is observed in the case of the blue and red glasses, whereas in the case of the green glass the reverse is observed to be the case: the ratio here is augmented from one-fifth to nearly one-fourth. The experiments therefore show that the radiant heat, after having passed through the gold-leaf, is different in quality from what it was before.

The leaf of mosaic gold already referred to, when stretched upon a frame and tested in the same manner, gave a result very nearly coincident with the above. A precipitate of gold upon the surface of a watch-glass was also examined, and similar results were obtained with it. These experiments prove beyond doubt, that calorific rays which have passed through a layer of gold, possess the property of passing through diathermanous bodies in a totally different degree from those which have not passed through the metal.

The influence of the *thickness* of the metallic layer was also investigated. Four layers were made use of, which were successively placed in the path of the rays, and behind each of them the test with the coloured glasses was repeated. In the following Table the results are recorded; the second column of which shows the deflection produced when the rays, after having passed through the layer of gold, and without passing through the glass, were allowed to fall upon the pile. The last four columns show the ratios of the quantities of heat which passed through the gold alone, to those which passed through both gold and glass, in the respective cases:—

	Deflection after passing through gold.	Yellow glass.	Blue glass.	Red glass.	Green glass.
Thinnest layer...	33°·64	100 : 46	100 : 30	100 : 32	100 : 21
Thicker layer ...	31·28	100 : 38	100 : 30	100 : 25	100 : 24
Still thicker ...	4·41	100 : 26	100 : 32	100 : 18	100 : 24
Thickest layer...	1·42	100 : 22	100 : 34	100 : 14	100 : 54

It is here shown, that, as regards the yellow and red glasses, the transmissibility of the heat diminished as the thickness of the metallic layer increased. But the thicker the layer, the greater is the power of the heat transmitted to penetrate the green and blue glasses. For example: of the heat which passed through the thinnest layer, we find that 46 parts out of 100 can pass through the yellow; 21 out of 100 through the green glass. Of the heat, however, which has passed through the thickest layer, a quantity represented by 24 passes through the yellow glass, while 54 passes through the green. Hence the longer the path which the calorific ray describes through the diathermanous gold, the more decided are the changes produced in them by the metal.

That the results hitherto recorded were not produced by any modification of the heat in its passage through the glasses in which the gold precipitates spread, M. Knoblauch shows both by reasoning and experiment. We will give the results of the latter. The heat was first permitted to pass through the yellow, red, and blue glasses, direct to the pile, and the action was observed. An uncoated portion of the watch-glass which held the gold, or a second uncoated watch-glass, was then introduced in the path of the rays, before they reached the coloured glass. The ratios of the heat incident upon the latter to that transmitted in the respective cases are recorded in the following Table:—

	Yellow glass.	Blue glass.	Red glass.	Green glass.
Watch-glass absent..	100 : 59	100 : 41	100 : 46	100 : 20
Watch-glass present.	100 : 59	100 : 39	100 : 46	100 : 22

This experiment shows clearly that the introduction of the watch-glass had no sensible influence upon the transmissibility of the heat through the coloured glasses. It is, however, proved by the experiments of Delaroche and Melloni, that after passing through a transparent body, the heat acquires a power of passing more freely through a second body of the same kind: even this action is absent in the experiments of M. Knoblauch, and for the simple reason, that, before reaching the watch-glass, the heat had already passed through, and been sifted by, a glass lens of much greater thickness.

Experiments similar to those made with the gold were made with *silver*. The ratios of the heat incident upon the coloured glass to that transmitted, were first determined before the silver layer was introduced, and are stated in the following Table:—

Yellow glass.	Blue glass.	Red glass.	Green glass.
100 : 63	100 : 38	100 : 49	100 : 19

It was necessary to repeat this experiment in each case, as the same ratios could only be obtained when precisely the same experimental conditions were adhered to. It was, however, practically impossible to secure this in experiments conducted on the fine days of several successive summers.

Six different layers of silver, prepared chemically by Professor Böttger, were at M. Knoblauch's disposal. They were placed in succession in the path of the solar rays. The following Table shows the ratios of the heat incident upon the glasses to that which passed through them to the thermo-electric pile. The layers are arranged in the order of their thickness, the thinnest coming first:—

Layers of silver.	Deflection after passing through the silver.	Yellow glass.	Blue glass.	Red glass.	Green glass.
1	10°·00	100 : 42	100 : 34	100 : 27	100 : 21
2	9·91	100 : 35	100 : 34	100 : 26	100 : 21
3	7·81	100 : 36	100 : 36	100 : 26	100 : 23
4	5·11	100 : 33	100 : 38	100 : 22	100 : 25
5	1·00	100 : 31	100 : 43	100 : 20	100 : 28
6	0·94	100 : 30	100 : 43	100 : 19	100 : 28

According to this, 42 parts of every 100 which had passed through the thinnest layer of silver were transmitted by the yellow glass; but only 33 of every 100 which had passed through the thickest layer. Before passing through the silver, the heat was far more capable of passing through the yellow glass than afterwards, for in the former case 63 parts out of every 100 were transmitted.

Out of every 100 parts which had passed through the thinnest layer of silver, 34 were transmitted by the blue glass; but out of every 100 which had passed through the thickest layer, 43 were transmitted. Before passing through the silver, 38 parts out of every 100 were transmitted by the blue glass.

The experiments therefore show that the quantity of heat which passed through the yellow and red glasses diminished as the thickness of the layer of silver increased, while under the same circumstances, the quantity which passed through the blue and green glasses was augmented. All the experiments show that the properties of the heat—for example, its power of passing through the coloured glasses,—is changed by its passage through the silver, the difference increasing as the thickness of the layer of silver is increased. The experiments also show that the heat transmitted by the gold is different from that transmitted by the silver.

When the heat was sent through a layer of platinum and examined by the coloured glass, it yielded the results recorded in the second horizontal line of figures of the following Table. For the sake of comparison, the numbers obtained before the platinum was introduced are given in the first line:—

Yellow glass.	Blue glass.	Red glass.	Green glass.
100 : 59	100 : 41	100 : 46	100 : 20
100 : 61	100 : 44	100 : 46	100 : 22

In this case the numbers in the two series are hardly to be distinguished from each other. Similar results were obtained with a layer of platinum so thick as to permit less than one-fourth of the above quantity of heat to pass through it. Hence it follows, *that in the case of platinum, the incident and transmitted rays do not sensibly differ in their power of passing through coloured glasses.*

Certain metals, therefore, such as gold and silver, do not intercept all kinds of calorific rays in the same proportion: like coloured bodies in the case of light, they exert an elective absorption on the rays; while others, such as platinum, like colourless transparent bodies in the case of light, intercept or transmit all kinds of calorific rays with the same facility.

If it be desired to regard the deportment of bodies like platinum through an optical analogy, it would be necessary to call them *gray*; for to merit the name of white, they must be capable of transmitting a greater quantity of heat. Such substances would, according to this, show the same deportment to both luminous and calorific rays. M. Knoblauch remarks here, that he does not know a single diathermanous body which behaves in the same manner towards heat as a colourless transparent body towards light. Even in the case of rock-salt, which Melloni regarded as such a body, despite its incomparable diathermaneity, M. Knoblauch has always been able to perceive (when the plate was thick enough) an elective absorption.

In order to show that the heat transmitted by metals does not pass by means of the little holes and fissures in the metallic leaves, M. Knoblauch submits the following experiments.

In place of the thin layer of silver, a thicker one, in which fissures and holes were visible, was made use of; and the heat which had passed through such fissures and through the coloured glasses to the thermo-electric pile, was compared with that which fell upon the instrument when the silver layer was removed. Such rays showed no difference in quality whatever, as the following numbers prove: the first line gives the ratios of the incident to the transmitted heat when the metallic layer was absent; the second the same ratios when the silver was present:—

	Yellow glass.	Blue glass.	Red glass.	Green glass.
Silver absent	100 : 56	100 : 39	100 : 42	100 : 21
Fissured silver interposed.	100 : 57	100 : 39	100 : 43	100 : 22

This experiment proves that if the heat transmitted in the experiments recorded in the preceding pages had been due to its passage through the fissures and holes of the metal, the qualitative differences which subsequently showed themselves would not have appeared.

It is also known, that if to the heat passing through the apertures in the silver, that of the silver itself had been added, the resulting heat would be different in quality from that falling upon the silver. But the heat in the above experiments showed no such difference of quality, and hence no sensible portion of it can have been derived from the silver.

But supposing the thin metallic layer to be warmed and converted into a source of heat; the heat thus radiating from a less intense source must have passed through the coloured glasses in less quantity than the solar rays alone. Now the experiments show the reverse of this to be the case; for while of the direct rays only 20 in 100 pass through the yellow glass, of the rays which have passed through the layer of gold 54 out of 100 are transmitted.

M. Knoblauch next examined the heat which was reflected from the anterior surface of the layer of gold; an experiment was also suggested by the fact of his possessing a piece of glass (G) which had served to illustrate Goëthe's theory of colours. This glass transmitted yellow light, and reflected a bluish light. It was therefore antithetical to the gold, which transmitted a bluish light, but reflected yellow.

The solar rays were permitted to pass through the glass lens and to enter the dark room, where they were reflected from the dull surface of gold, and afterwards permitted to fall upon the thermo-electric pile. We will call the quantity of heat thus measured by the deflection of the galvanometer 100. A yellow glass was now placed in the path of the rays, and a quantity of heat expressed by the number 70 passed through the glass. When, however, the heat passed *through* the gold, only 14 parts out of 100 were capable of passing through the yellow glass. The other glasses were used in the same manner, with the following results:—

	Yellow glass.	Blue glass.	Red glass.	Green glass.
Heat reflected from gold	100 : 70	100 : 45	100 : 56	100 : 19
Heat transmitted through gold.	100 : 19	100 : 31	100 : 14	100 : 26

The experiments illustrate the extraordinary difference existing between the heat reflected by gold and the heat transmitted by it.

In the next place, the rays were suffered to fall upon the glass already mentioned. Calling the quantity reflected from the glass (G) and falling on the pile, 100; when the yellow glass was interposed, 59 parts out of the 100 passed through it. When the heat, instead of being reflected by the glass (G), was sent through it, 80 parts out of 100 passed through the interposed yellow glass. The other glasses were afterwards tried: the results are stated in the following Table:—

	Yellow glass.	Blue glass.	Red glass.	Green glass.
Heat reflected from glass (G)	100 : 59	100 : 44	100 : 44	100 : 20
Heat transmitted through glass (G).	100 : 80	100 : 51	100 : 64	100 : 19

Here also the deportment of the reflected heat towards the coloured glasses is very different from that of the transmitted heat.

The antithesis existing between the gold and the blue-yellow glass will also be remarked. While, in the case of gold, the reflected rays are more capable of passing through the yellow, blue, and red glass, and less capable of passing through the green glass than the transmitted rays; in the case of Goëthe's glass the reflected heat was less capable of passing through the yellow, blue, and red glass, and more capable of passing through the green than the transmitted heat.

II.

Twelve years ago M. Knoblauch had treated the question, whether, by its reflexion from rough metallic surfaces, radiant heat was so changed as to be distinguishable from unreflected heat. Melloni entertained the opinion that metallic surfaces behaved towards radiant heat as white bodies towards light. M. Knoblauch believed that he had confirmed this view; but since then MM. Provostaye and Desains have made experiments with metals, from which the authors conclude that different kinds of heat are unequally reflected from metallic surfaces. As to the properties of heat *diffusely* reflected from different metals, nothing further is known; but now that in the case of three metals differences in the heat transmitted by them have been detected, the examination whether changes occurred in the reflected heat could be undertaken with greater hope of success.

The beam reflected from the heliostat was first permitted to fall direct upon the thermo-electric pile, and the consequent effect was denoted by 100. The yellow, blue, red, and green glasses were then interposed successively in the path of the rays: the relative quantities of heat which passed through the glasses are given in the following Table:—

Yellow glass.	Blue glass.	Red glass.	Green glass.
100 : 66	100 : 36	100 : 51	100 : 18

The question now is, whether the calorific rays which have been diffusely reflected from metallic surfaces possess the same power of penetrating the coloured glasses as those which have not been thus reflected.

A rough surface of gold was first made use of. Expressing the action of the heat reflected from this surface, and falling direct upon the pile by 100, the proportional quantities which passed through the coloured glasses are given in the following Table:—

Yellow glass.	Blue glass.	Red glass.	Green glass.
100 : 73	100 : 37	100 : 55	100 : 17

It is here unmistakeable that a more copious passage takes place through the yellow glass than in the case where the heat was unreflected. The ratio in the latter case is only 66 : 100, while in the case now before us it is 73 : 100. Through the red glass also the transmission has been more copious, the ratio increasing from 51 : 100 to 55 : 100.

When the gold plate was exchanged for a rough plate of silver, or for a dull surface of platinum, the following ratios were obtained:—

	Yellow glass.	Blue glass.	Red glass.	Green glass.
Silver	100 : 72	100 : 38	100 : 55	100 : 17
Platinum......	100 : 66	100 : 38	100 : 51	100 : 18

Here we observe that the heat reflected from silver shows the same deportment as that reflected from gold. The rays in both cases are more capable of passing through the yellow and red glasses than the unreflected rays. The heat reflected from the platinum surface, on the contrary, is not to be distinguished in its action from the direct rays. The small difference which shows itself in the case of the blue glass, lies within the limits of the errors of observation.

Hence the same metals (that is, gold and silver) that alter the properties of the calorific rays which pass through them, exert also an influence upon the rays diffusely reflected from their surfaces, while platinum in both cases leaves the rays unchanged.

Other metals were next examined; the results are given in the following Table:—

	Yellow glass.	Blue glass.	Red glass.	Green glass.
Gold	100 : 69	100 : 37	100 : 56	100 : 17
Copper	100 : 67	100 : 38	100 : 55	100 : 16
Brass	100 : 66	100 : 37	100 : 53	100 : 18
Unreflected heat...	100 : 61	100 : 38	100 : 52	100 : 18

According to this, the heat reflected from copper and brass is also more copiously transmitted by the yellow glass; but the augmentation is not so great as in the case of gold. In the case of the red glass, the quantities transmitted from gold and copper are nearly equal; but with brass no sensible difference is observed between the reflected and unreflected heat. Here also, as in the former instance, the blue and green glasses show no difference between the reflected and unreflected rays.

A similar series of experiments was made with silver, mercury, and German silver. Each had a rough surface; the mercury was rubbed upon copper, and had a milky appearance. The following Table contains the results of the experiments:—

	Yellow glass.	Blue glass.	Red glass.	Green glass.
Silver	100 : 72	100 : 38	100 : 55	100 : 17
Mercury	100 : 72	100 : 38	100 : 55	100 : 18
German silver	100 : 66	100 : 38	100 : 52	100 : 18
Unreflected heat...	100 : 66	100 : 38	100 : 51	100 : 18

The rays reflected from mercury exhibit, according to this, quite the same deportment as those reflected from silver; while those reflected from German silver are not to be distinguished from those coming directly from the sun.

The heat reflected from lead, tin, and alloys of both metals gave the following values:—

	Yellow glass.	Blue glass.	Red glass.	Green glass.
Lead	100 : 64	100 : 42	100 : 51	100 : 15
Tin	100 : 64	100 : 42	100 : 51	100 : 15
Alloy of lead and tin.	100 : 64	100 : 42	100 : 51	100 : 15
Unreflected heat ...	100 : 64	100 : 42	100 : 51	100 : 15

The reflexion from these bodies has, according to this Table, no influence on the transmissibility of the heat through the coloured glasses.

Experiments with rough surfaces of zinc, iron, and black sheet-iron, also showed that no sensible difference existed between the heat reflected from them and the direct heat.

The results of all the observations on reflected heat are contained in the following Table, to construct which it was necessary to connect by new experiments the results exhibited separately in the foregoing Tables, and to reduce them afterwards to a common standard.

Diathermanous body.	Unreflected heat.	Gold.	Silver.	Platinum.	Mercury.	Black sheet-iron.	Tin.	Zinc.	Copper.	Lead.	Alloy of tin and lead.	Brass.	German silver.
Yellow glass..	100 : 65	73	72	65	72	66	65	65	71	65	65	70	65
Blue glass ...	100 : 40	39	40	40	40	40	40	40	40	40	40	39	40
Red glass ...	100 : 51	55	55	51	55	52	51	51	54	51	51	52	51
Green glass...	100 : 17	16	16	17	17	17	17	17	16	17	17	17	17

The results recorded in this Table are regarded by M. Knoblauch as placing beyond doubt the following proposition:—*that certain metals, as gold, silver, mercury, copper, brass, alter the calorific rays* (as coloured bodies alter those of light); *while other metals, as platinum, iron, tin, zinc, lead, an alloy of lead and tin* (similar to colourless transparent bodies as regards light), *do not change the calorific rays by reflexion.*

M. Knoblauch also shows, in a very convincing manner, that the results obtained could not be due to the heat emitted by the metals themselves after they had been warmed by the sun's rays.

We have seen that reflexion from certain metals can alter the quality of the heat; but the heat made use of in the experiments had been actually reflected from the mirror of the heliostat. It was necessary therefore to examine whether the variety of the sun's rays was not altered by this first reflexion. The heliostat made use of was that of Silbermann, and its mirror was of highly polished speculum metal.

M. Knoblauch first examined the influence of a steel mirror upon the calorific rays, and found it absolutely nothing; which, indeed, was to be inferred from the deportment of the rough iron surface already recorded. He then set a heliostat of August, which possessed a steel mirror, in the place of that of Silbermann, and compared the heat reflected from the mirror of the latter with that reflected from platinum. He knew from the observations already recorded, that platinum produced no change whatever in the calorific rays reflected from it. For the sake of making the conditions of the two experiments as much alike as possible, the heat reflected from platinum was preferred to the direct heat. The following Table gives the ratios of the heat incident upon, to that transmitted by the coloured glasses in the case of both metals:—

	Yellow glass.	Blue glass.	Red glass.	Green glass.
Polished speculum metal..	100 : 66	100 : 35	100 : 50	100 : 16
Platinum	100 : 66	100 : 35	100 : 50	100 : 16

From this it is plain that the heat is in no way changed by its reflexion from the mirror of Silbermann's heliostat.

In all the observations hitherto recorded, the source of heat was the sun's rays. M. Knoblauch repeated the same experiments, making use of the lamp of Locatelli with square wick. The experiments were made exactly in the manner already described, and the following Table contains a summary of the results :—

Diathermanous bodies.	Unreflected heat.	Gold.	Silver.	Platinum.	Mercury.	Sheet-iron.	Tin.	Zinc.	Copper.	Lead.	Alloy of lead and tin.	Brass.	German silver.
Yellow glass..	100 : 35	39	35	35	35	35	35	35	35	35	35	34	35
Blue glass ...	100 : 29	30	30	30	30	30	30	30	31	30	30	31	30
Red glass ...	100 : 37	39	37	36	37	37	37	37	38	37	37	39	37
Green glass...	100 : 14	14	13	13	14	13	14	14	13	14	14	13	14

This Table shows in what different degrees the heat of the sun and that of a Locatelli lamp are transmitted through the coloured glasses; and it also shows that when the lamp is used, the differences of transmission by the same glass, when heat reflected from different metals is made use of, almost vanish. In fact, of all the results obtained with the sun's rays, the only one that continues to assert itself here, is the more copious transmission of the heat diffusely reflected from gold through the yellow glass.

When, instead of the Locatelli lamp, a metal cylinder heated to 110° C. was made use of as the source of heat, the peculiarity observed in the case of gold also disappeared, and the heat reflected from it, like that reflected from the other metals, was not to be distinguished from the unreflected rays.

M. Knoblauch next examined the influence of the *state* of the surface on the heat reflected from it. A rough surface of gold was compared with a polished surface of the same metal, and the heat reflected from both was found to be acted upon in precisely the same manner by the coloured glasses. When, however, different thicknesses of the metal were precipitated by electrolysis on a plate of copper, it was found that the heat reflected from the thicker deposits was most copiously transmitted through the yellow glass. The result is stated in the following Table:—

	Yellow glass.	Red glass.
Thick layer.........	100 : 70	100 : 55
Thin layer	100 : 67	100 : 55
Unreflected heat...	100 : 63	100 : 52

Three surfaces of silver were next made use of. One was a dull plate of a whitish colour obtained by electrolysis; another was obtained by the so-called cold silvering process, and had more of a gray aspect; the third was a highly polished daguerreotype plate, which appeared almost black. The experiments with these three plates gave the following results:—

	Yellow glass.	Red glass.
Dull white surface......	100 : 72	100 : 56
Gray layer	100 : 68	100 : 51
Daguerreotype plate ...	100 : 65	100 : 50
Unreflected heat	100 : 66	100 : 50

In this case the heat reflected from the dull white surface passed most copiously through the yellow glass; that from the gray surface less copiously; while that reflected from the polished plate was transmitted least copiously. The latter heat was not to be distinguished from the unreflected calorific rays. Similar effects are observed in the case of the red glass.

A similar result was obtained with mercury rubbed upon a plate of copper until a dull whitish layer was obtained, when the heat reflected from this surface was compared with that reflected from the ordinary shining surface of the metal. The following Table gives the result of the experiment:—

	Yellow glass.	Red glass.
Dull whitish surface	100 : 73	100 : 55
Shining metallic surface ..	100 : 67	100 : 51
Unreflected heat	100 : 67	100 : 51

In experiments made with dull and polished surfaces of copper, and with rough and polished surfaces of brass, there was no difference observed. "It must be confessed," remarks M. Knoblauch, "that in the case of copper and brass, as in the case of gold, the perfection of the polish was not to be compared with that of the silver surface, for in optical respects the former did not cease to show its red, and the latter its yellow colour; while the polished silver and the metallic mercury, colourless in themselves, gave only the colours of the mirrored objects. With the former, therefore, the heat reflected under a definite angle was not purified from the diffuse heat; in the case of the latter alone was the pure and simple reflexion present."

The dependence of the diffuse reflexion on the inclination of the incident calorific rays to the reflecting surface remains now to be examined; and here, in the first place, we have to ascertain the *quantity* of heat which reaches the thermo-electric pile under different angles. The metal plate to be tested was placed in the path of the rays sent in from the mirror of the heliostat, and an arrangement was made which permitted the plate to be turned round a vertical axis, so as to permit the rays to fall upon it at any required angle. The surface of the thermo-electric pile was placed at right angles to a line which made the same angle with the reflecting surface as the rays which fell upon it from the heliostat. This arrangement permitted of an examination of all rays which fell upon the surface between the limits of 80 and of 2 degrees.

A uniform dull plate of gold was first examined, and a gradual augmentation of heat was observed as the rays fell more and more parallel to the surface. Thus, for example, when the inclination of the surface to the rays was 2°, the quantity of heat sent to the pile was five times the quantity sent when the inclination was 80°. With another rough surface of gold an augmentation in the ratio of 1 to 3·5 was observed, under the same circumstances; with polished gold the augmentation was only in the ratio of 1 to 1·36. The gradual increase is exhibited in the following Table, which also renders manifest the difference between the rough and polished surfaces.

Inclination.	80°.	60°.	40°.	20°.	10°.	2°.
Rough surface	1·00	1·44	2·06	2·94	3·95	5·08
Another rough surface ...	1·00	1·29	1·59	2·59	3·00	3·50
Polished surface...........	1·00	1·06	1·06	1·12	1·21	1·36

Fine grooves were afterwards drawn on the metallic plates, and the effect of these plates when the grooves were transverse, and when they were parallel to the plane of reflexion, was examined. The experiments were made with two plates of gold, on which the fine grooves had different degrees of distinctness. The following are the results of the experiment:—

Inclination.	80°.	40°.	10°.	2°.
Grooves transverse...	1·00	1·19	2·21	3·17
Grooves parallel......	1·00	1·40	3·04	4·04
Another plate.				
Grooves transverse...	1·00	1·20	3·30	4·40
Grooves parallel......	1·00	2·00	6·44	8·00

According to this, with one plate, the heat reflected from the plate at an inclination of 2°, was treble the quantity reflected at an inclination of 80°, when the grooves were transverse to the plane of reflexion; and quadruple of the same quantity when the grooves were parallel to the plane of reflexion. In the case of the other plate, at an angle of 2°, the heat was augmented 4·4 times when the grooves were perpendicular to the plane of reflexion, and augmented eight times when they were parallel to it. The dependence of the quantity of heat reflected from a finely-grooved metallic plate on the inclination of the markings to the plane of reflexion is here very manifest.

Silver was next submitted to the same examination: experiments with a uniform dull plate and one highly polished, led to the following results: the quantity reflected at an inclination of 80° is, as before, expressed by unity:—

Inclination.	80°.	60°.	40°.	20°.	10°.	2°.
Rough surface ...	1·00	1·23	1·45	1·98	3·32	3·98
Polished surface...	1·00	1·02	1·02	1·06	1·07	1·09

In this case the increase of the calorific action with the decrease of the inclination of the rays to the reflecting plate is very considerable, in the case of the rough surface, but barely indicated in the case of the polished one.

With *grooved* plates, the augmentation, as in the case of gold, was greater when the grooves were parallel to the plane of reflexion than when perpendicular to it. In the experiments three silver plates were used; two of which were covered with a galvanic deposit, and the third silvered by the process of rubbing. The following Table contains the results of the experiments:—

Inclination.	80°.	40°.	10°.	2°.
1st plate.				
Grooves transverse...	1·00	1·35	2·65	3·20
Grooves parallel......	1·00	1·55	3·30	3·60
2nd plate.				
Grooves transverse...	1·00	1·33	2·10	2·72
Grooves parallel......	1·00	1·57	3·66	4·29
3rd plate.				
Grooves transverse...	1·00	1·05	2·67	4·58
Grooves parallel......	1·00	1·18	2·90	5·15

Copper with a *dull* and with a *polished* surface gave the following results:—

Inclination.	80°.	40°.	10°.	2°.
Rough surface	1·00	1·13	1·75	2·50
Polished surface......	1·00	1·05	1·50	1·62

With a *grooved* surface the following results were obtained:—

Inclination.	80°.	40°.	10°.	2°.
Grooves transverse...	1·00	1·40	4·67	5·33
Grooves parallel......	1·00	1·55	5·33	6·50

The results with brass, platinum and iron are placed together in the following Table:—

Inclination.	80°.	40°.	10°.	2°.
Grooves transverse...	1·00	1·33	3·33	4·67
Grooves parallel......	1·00	1·50	5·00	6·33
Grooves transverse...	1·00	1·13	3·13	6·75
Grooves parallel......	1·00	1·62	5·06	7·50
Grooves transverse...	1·00	1·12	1·92	2·17
Grooves parallel......	1·00	1·42	2·17	3·00

The results of the foregoing experiments may be thus stated:—*The quantity of heat reflected from a metallic plate increases as the angle which the calorific rays make with the metallic surface diminishes; this increase depends upon the roughness of the surface, being in the case of a polished surface a vanishing quantity. In the case of grooved plates, the increase is least when the grooves are perpendicular to the plane of reflexion, and greatest when the grooves are parallel to this plane.*

It is shown by these experiments that the *quantity* of heat reflected from a rough metallic surface is dependent on the angles which the surface makes with the incident rays. M. Knoblauch next examined whether the *quality* of the heat reflected was affected by the inclination. The experiment was first made with a dull surface of gold, and the result was, that the rays reflected from it at an inclination of 80° were more capable of passing through the yellow and red glasses than rays reflected at an inclination of 2°. The following Table contains the results of the experiments:—

Gold.	Inclination.	Yellow glass.	Red glass.
Rough surface	80° 2	100 : 70 100 : 66	100 : 55 100 : 51
Polished surface	80 2	100 : 71 100 : 66	100 : 57 100 : 50
Unreflected heat	80 2	100 : 65	100 : 50

We see here that the qualitative difference, in the case of heat reflected from a roughened surface of gold, disappears wholly when the rays fall upon the plate at a very small angle.

Similar experiments were made with silver, mercury, copper and brass: the results are collected in the following Table:—

Silver.	Inclination.	Yellow glass.	Red glass.
Rough surface	80° 2	100 : 71 100 : 65	100 : 55 100 : 51
Polished surface	80 2	100 : 65 100 : 65	100 : 50 100 : 50
Unreflected heat	0	100 : 65	100 : 50
Mercury. Dull white surface	80 2	100 : 73 100 : 69	100 : 55 100 : 51
Shining metallic surface	80 2	100 : 67 100 : 67	100 : 51 100 : 50
Unreflected heat	0	100 : 67	100 : 51
Copper. Rough surface	80 2	100 : 73 100 : 66	100 : 53 100 : 48
Polished surface	80 2	100 : 73 100 : 66	100 : 53 100 : 48
Unreflected heat	0	100 : 65	100 : 48
Brass. Rough surface	80 2	100 : 70 100 : 66	100 : 48 100 : 48
Polished surface	80 2	100 : 69 100 : 64	100 : 48 100 : 48
Unreflected heat	0	100 : 65	100 : 48

In all these cases the qualitative peculiarity which distinguishes heat diffusely reflected, from the unreflected heat, disappears when the incidence is very oblique. *It is thus placed beyond doubt, that the properties of the reflected rays are dependent on the inclination at which they meet the reflecting surface.*

The principal results of this investigation may be stated to be as follows:—

1. Metals, as gold, silver and platinum, when in thin layers, are to be regarded as diathermanous bodies, which permit a portion of the calorific rays to pass through them; which portion naturally becomes less as the thickness of the layer increases.

In thus transmitting the calorific rays, certain metals, as gold and silver, exercise an elective absorption, similar to that of coloured transparent bodies upon light. Others, on the contrary, like platinum, act in the same manner upon all rays, and are therefore to be regarded as analogous to colourless bodies in the case of light.

2. In the case of diffuse reflexion, also, certain metals, such as gold, silver, mercury, copper and brass, similar to coloured and opaque bodies as regards light, exercise an elective absorption upon the calorific rays, in consequence of which the properties of the latter are altered. Others, on the contrary, for example, platinum, iron, tin, zinc, lead, alloy of lead and tin, German silver, reflect all kinds of calorific rays in the same proportion, exactly as colourless opaque bodies do with regard to light.

The properties which distinguish calorific rays reflected from metals, from unreflected heat, are so far dependent on the source of heat, that differences, for example, which exhibit themselves in a striking manner when solar heat is made use of, are diminished in the case of a Locatelli lamp, and completely disappear when the source of heat is a metallic cylinder not heated to redness.

The surface has the power either of causing the differences to appear in their maximum degree, or to disappear totally, according as the surface produces a diffuse or a regular reflexion.

The same is true of the change of the angle of incidence. In the case of a rough metallic surface, as the angle of the rays with the surface diminishes, the reflexion passes gradually from the diffuse to the regular, and at the same time the differences between the reflected and unreflected heat also gradually become less, until finally both have exactly the same character.

XLIII. *On the Calculation of the Numerical Values of a certain class of Multiple and Definite Integrals. By* Sir WILLIAM ROWAN HAMILTON, *LL.D., M.R.I.A., F.R.A.S. &c., Andrews' Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland**.

SECTION I.

[1.] THE results, in part numerical, of which a sketch is here to be given, may serve to illustrate some points in the theory of functions of large numbers, and in that of definite and multiple integrals. In stating them, it will be convenient to employ a notation which I have formerly published, and have often found to be useful; namely the following,

$$\mathrm{I}_t = \int_0^t dt; \quad \ldots \ldots \quad (1)$$

or more fully,

$$\mathrm{I}_t ft = \int_0^t ft\, dt; \quad \ldots \ldots \quad (1)'$$

with which I am now disposed to combine this other symbol,

$$\mathrm{J}_t = \int_t^\infty dt; \quad \ldots \ldots \quad (2)$$

in such a manner as to write,

$$\mathrm{J}_t ft = \int_t^\infty ft\, dt, \quad \ldots \ldots \quad (2)'$$

and therefore

$$\mathrm{I}_t + \mathrm{J}_t = \int_0^\infty dt. \quad \ldots \ldots \quad (3)$$

I shall also retain, for the present, the known notation of Vandermonde for factorials, which has been described and used by Lacroix, and in which, for any positive whole value of n,

$$[x]^n = x(x-1)(x-2)\ldots(x-n+1); \quad \ldots \ldots \quad (4)$$

so that there are the transformations,

$$[x]^n = [x]^m [x-m]^{n-m} = [x]^{n+m} : [x-n]^m, \text{\&c.}; \quad (4)'$$

which are extended by definition to the case of null and negative indices, and give, in particular,

$$[0]^{-n} = \frac{1}{[n]^n} = \frac{1}{1 \,.\, 2 \,.\, 3 \ldots n}. \quad \ldots \ldots \quad (4)''$$

For example,

$$(1+x)^n = \Sigma_{m=0}^{m=\infty} [n]^m [0]^{-m} x^m. \quad \ldots \ldots \quad (5)$$

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* Communicated by the Author.

It is easy, if it be desired, to translate these into other known notations of factorials, but they may suffice on the present occasion.

[2.] With the notations above described, it is evident that

$$\mathrm{I}_t^n 1 = [0]^{-n} t^n ; \quad \text{. . . . .} \quad (6)$$

and more generally, that

$$\mathrm{I}_t^n t^m = \frac{t^{m+n}}{[m+n]^n} = [m]^{-n} t^{m+n} \quad \text{. .} \quad (6)'$$

Hence results the series,

$$(1 + \mathrm{I}_t + \mathrm{I}_t^2 + \ldots) 1 = (1 - \mathrm{I}_t)^{-1} 1 = e^t ; \quad \text{. .} \quad (7)$$

and accordingly, we have the finite relation,

$$\mathrm{I}_t e^t = e^t - 1. \quad \text{. . . . .} \quad (7)'$$

The imaginary equation,

$$e^{t\sqrt{-1}} = (1 - \sqrt{-1}\,\mathrm{I}_t)^{-1} 1, \quad \text{. . . .} \quad (8)$$

breaks up into the two real expressions,

$$\cos t = (1 + \mathrm{I}_t^2)^{-1} 1, \quad \text{. . . . . . .} \quad (8)'$$

$$\sin t = \mathrm{I}_t (1 + \mathrm{I}_t^2)^{-1} 1. \quad \text{. . . . . . .} \quad (8)''$$

The series of Taylor may be concisely denoted by the formula,

$$f(x+t) = (1 - \mathrm{I}_t \mathrm{D}_x)^{-1} fx ; \quad \text{. . . . .} \quad (9)$$

and accordingly,

$$\mathrm{I}_t \mathrm{D}_x f(x+t) = \mathrm{I}_t f'(x+t) = f(x+t) - fx. \quad \text{.} \quad (9)'$$

And other elementary applications of the symbol I_t may easily be assigned, whereof some have been elsewhere indicated.

[3.] The following investigations relate chiefly to the function,

$$\mathrm{F}_{n,r} t = \mathrm{I}_t^n (1 + 4\mathrm{I}_t^2)^{-r-\frac{1}{2}} 1 ; \quad \text{. . . . .} \quad (10)$$

or

$$\mathrm{F}_{n,r} t = \mathrm{I}_t^n (1 + 4\mathrm{I}_t^2)^{-r} ft, \quad \text{. . . . .} \quad (10)'$$

where

$$ft = \mathrm{F}_{0,0} t = (1 + 4\mathrm{I}_t^2)^{-\frac{1}{2}} 1. \quad \text{. . . . .} \quad (11)$$

Developing by (5) and (6), and observing that

$$2^{2m} [-\tfrac{1}{2}]^m = (-1)^m [2m]^m, \quad \text{. . . . .} \quad (12)$$

and that therefore

$$2^{2m} [-\tfrac{1}{2}]^m [0]^{-m} [0]^{-2m} = (-1)^m \left([0]^{-m}\right)^2, \quad \text{.} \quad (12)'$$

we find the well-known series,

$$ft = 1 - \left(\frac{t}{1}\right)^2 + \left(\frac{t^2}{1\,.\,2}\right)^2 - \left(\frac{t^3}{1\,.\,2\,.\,3}\right)^2 + \text{\&c.}, \quad \text{.} \quad (13)$$

which admits of being summed as follows,

$$ft = \frac{2}{\pi}\int_0^{\frac{\pi}{2}} d\omega \cos(2t\cos\omega)\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (13)'$$

the function ft being thus equal to a celebrated definite integral, which is important in the mathematical theory of heat, and has been treated by Fourier and by Poisson.

[4.] It was pointed out* by the great analyst last named, that if there were written the equation,

$$y = \int_0^{\pi} \cos(k\cos\omega)\,d\omega, \quad . \quad . \quad . \quad . \quad . \qquad (14)$$

so that, in our recent notation,

$$y = \pi f\left(\frac{k}{2}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (14)'$$

then for large, real, and positive values of k, the function $y\sqrt{k}$ might be developed in a series of the form,

$$y\sqrt{k} = \left(\mathrm{A} + \frac{\mathrm{A}'}{k} + \frac{\mathrm{A}''}{k^2} + \&c.\right)\cos k$$
$$+ \left(\mathrm{B} + \frac{\mathrm{B}'}{k} + \frac{\mathrm{B}''}{k^2} + \&c.\right)\sin k\,; \quad . \quad . \qquad (15)$$

where a certain differential equation of the second order, which $y\sqrt{k}$ was obliged to satisfy, was proved to be sufficient for the successive deduction of as many of the other constant coefficients, A', A'', .. and B', B'', .. of the series, as might be desired, through an assigned system of equations of condition, after the two first constants, A and B, were determined; and certain processes of definite integration gave for them the following values,

$$\mathrm{A} = \mathrm{B} = \sqrt{\pi}\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (15)'$$

so that when k is very large, we have nearly, as Poisson showed,

$$y\sqrt{k} = (\cos k + \sin k)\sqrt{\pi}. \quad . \quad . \quad . \quad . \qquad (15)''$$

[5.] In my own paper on Fluctuating Functions†, I suggested a different process for arriving at this important formula of approximation, (15)″, which, with some slight variation, may

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* In his Second Memoir on the Distribution of Heat in Solid Bodies, *Journal de l'Ecole Polytechnique,* tome xii. cahier 19, Paris, 1823, pages 349, &c.

† In the Transactions of the Royal Irish Academy, vol. xix. part 2, p. 313; Dublin, 1843. Several copies of the paper alluded to were distributed at Manchester in 1842, during the Meeting of the British Association for that year: one was accepted by the great Jacobi.

be briefly stated as follows. Introducing the two definite integrals,

$$\left.\begin{aligned} A_t &= \frac{2}{\pi}\int_0^{\frac{\pi}{2}} d\omega \cos(2t \operatorname{vers}\omega), \\ B_t &= \frac{2}{\pi}\int_0^{\frac{\pi}{2}} d\omega \sin(2t \operatorname{vers}\omega), \end{aligned}\right\} \quad . \quad . \quad . \quad (16)$$

which give the following *rigorous transformation* of the expression (13)′, or of the function *ft*,

$$ft = A_t \cos 2t + B_t \sin 2t; \quad . \quad . \quad . \quad . \quad (16)'$$

and employing the *limiting values*,

$$\left.\begin{aligned} \lim_{t=\infty} . t^{\frac{1}{2}} A_t &= \frac{2}{\pi}\int_0^{\infty} dx \cos(x^2) = (2\pi)^{-\frac{1}{2}}, \\ \lim_{t=\infty} . t^{\frac{1}{2}} B_t &= \frac{2}{\pi}\int_0^{\infty} dx \sin(x^2) = (2\pi)^{-\frac{1}{2}}; \end{aligned}\right\} \quad . \quad . \quad (16)''$$

(which two last and well-known integrals have indeed been used by Poisson also,) I obtained (and, as I thought, more rapidly than by his method) the following *approximate expression*, equivalent to that lately marked as (15)″, for large, real, and positive values of t:

$$ft = (\pi t)^{-\frac{1}{2}} \sin\left(2t + \frac{\pi}{4}\right); \quad . \quad . \quad . \quad . \quad (17)$$

which is sufficient to show that the *large and positive roots* of the transcendental equation,

$$\int_0^{\frac{\pi}{2}} d\omega \cos(2t \cos\omega) = 0, \quad . \quad . \quad . \quad . \quad (17)'$$

are (as is known)* very nearly of the form,

$$t = \frac{n\pi}{2} - \frac{\pi}{8}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)''$$

where n is a large whole number.

[6.] Poisson does not appear to have required, for the applications which he wished to make, any more than the *two* con-

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* It must, I think, be a misprint, by which, in p. 353 of Poisson's memoir, the expression $k = i\pi + \frac{\pi}{4}$, is given, instead of $i\pi - \frac{\pi}{4}$, for the large roots of the transcendental equation $y=0$. It is remarkable, however, that this error of sign, if it be such, does not appear to have had any influence on the correctness of the physical conclusions of the memoir: which, no doubt, arises from the circumstance that the number i is treated as infinite, in the applications.

stants, which he called A and B, of his descending series (15); although (as has been said) he showed how all the *other* constants of that series *could* be *successively* computed, from them, if it had been thought necessary or desirable to do so. In other words, he seems to have been content with assigning the values (15)′, and the formula (15)″, as sufficient for the purpose which he had in view. In my own paper, already cited, I gave the *general term of the descending series* for *ft*, by assigning a formula, which (with one or two unimportant differences of notation) was the following:

$$(\pi t)^{\frac{1}{2}}ft = \Sigma_{m=0}^{m=\infty}\,[0]^{-m}\left([-\tfrac{1}{2}]^{m}\right)^{2}(4t)^{-m}\cos\left(2t-\frac{\pi}{4}-\frac{m\pi}{2}\right). \quad (18)$$

As an example of the numerical approximation attainable hereby, when t was a moderately large number, (not necessarily whole,) I assumed $t=20$; and found that *sixty terms* of the ultimately convergent, but initially divergent series (13), gave

$$\begin{aligned} f(20) &= \frac{2}{\pi}\int_0^{\frac{\pi}{2}} d\omega \cos(40\cos\omega) \\ &= +7\ 447\ 387\ 396\ 709\ 949\cdot\ 965\ 7957 \\ &\quad -7\ 447\ 387\ 396\ 709\ 949\cdot\ 958\ 4289 \\ &= +0\cdot007\ 3668; \quad (19) \end{aligned}$$

while only *three terms* of the ultimately divergent, but initially convergent series (18), sufficed to give almost exactly the same result, under the form,

$$f(20) = \left(1-\frac{9}{204800}\right)\frac{\cos 86^\circ\ 49'\ 52''}{\sqrt{20\pi}} + \frac{1}{320}\,\frac{\sin 86^\circ\ 49'\ 52''}{\sqrt{20\pi}}$$

$$= 0\cdot0069736 + 0\cdot0003936 = +0\cdot0073672. \quad (19)'$$

[7.] The function ft becomes infinitely small, when t becomes infinitely great, on account of the indefinite fluctuation which $\cos(2t\cos\omega)$ then undergoes, under the sign of integration in (13)′; so that we may write

$$F_{0,0}\,\infty = f\infty = 0. \quad (20)$$

But it is by no means true that the value of this *other* series,

$$F_{1,0}t = I_t ft = \frac{t}{1} - \frac{t}{3}\left(\frac{t}{1}\right)^2 + \frac{t}{5}\left(\frac{t^2}{1.2}\right)^2 - \&c., \quad (21)$$

which may be expressed by the definite integral,

$$F_{1,0}\,t = \frac{1}{\pi}\int_0^{\frac{\pi}{2}} d\omega \sec\omega \sin(2t\cos\omega), \quad (21')$$

is infinitesimal when t is infinite. On the contrary, by making

$$2t \cos \omega = x, \ d\omega \sec \omega = -\frac{dx}{x}\left(1 - \frac{x^2}{4t^2}\right)^{-\frac{1}{2}}, \quad . \quad (22)$$

the integral (21)′ becomes, at the limit in question,

$$\mathrm{F}_{1,0}\infty = \frac{1}{\pi}\int_0^\infty \frac{dx}{x}\sin x = \tfrac{1}{2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)''$$

Accordingly I verified, many years ago, that the series (21) takes *nearly* this constant value, $\frac{1}{2}$, when t is a large and positive number. But I have lately been led to inquire what is the *correction* to be applied to this approximate value, in order to obtain a more accurate numerical estimate of the function $\mathrm{F}_{1,0}\,t$, or of the integral $\mathrm{I}_t ft$, when t is large. In other words, having here, by (3) and (21)″, the *rigorous* relation,

$$\mathrm{F}_{1,0}\,t = \mathrm{I}_t ft = \tfrac{1}{2} - \mathrm{J}_t ft, \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

I wished to evaluate, at least *approximately*, this *other* definite integral, $-\mathrm{J}_t ft$, for large and positive values of t. And the result to which I have arrived may be considered to be a very simple one; namely, that

$$-\mathrm{J}_t ft = \mathrm{D}_t^{-1} ft; \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

where $\mathrm{D}_t^{-1} ft$ is a development analogous to the series (18), and reproduces that series, when the operation D_t is performed.

[8.] As an example, it may be sufficient here to observe that if we thus operate by D_t on the function,

$$f^{\backprime}t = \left(1 - \frac{129}{2^9 t^2}\right)\frac{\sin\left(2t - \frac{\pi}{4}\right)}{2\sqrt{\pi t}} - \frac{5\cos\left(2t - \frac{\pi}{4}\right)}{2^5 t\sqrt{\pi t}}, \qquad (25)$$

and suppress $t^{-\frac{7}{2}}$ in the result, we are led to this other function of t,

$$\mathrm{D}_t f^{\backprime}t = \left(1 - \frac{9}{2^9 t^2}\right)\frac{\cos\left(2t - \frac{\pi}{4}\right)}{\sqrt{\pi t}} + \frac{\sin\left(2t - \frac{\pi}{4}\right)}{2^4 t\sqrt{\pi t}}; \quad . \quad (25)'$$

which coincides, so far as it has been developed, with the expression (18) for ft: so that we may write, as at least approximately true, the equation

$$f^{\backprime}t = \mathrm{D}_t^{-1} ft. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)''$$

Substituting the value 20 for t, in order to obtain an arithmetical comparison of results, we find,

$$f^{\backprime}(20) = \left(1 - \frac{129}{204800}\right)\frac{\sin 86^\circ\, 49'\, 52''}{\sqrt{20\pi}} - \frac{\cos 86^\circ\, 49'\, 52''}{128\sqrt{20\pi}}$$

$$= +0{\cdot}062942 - 0{\cdot}000054 = +0{\cdot}062888; \quad . \quad . \quad (26)$$

which ought, if the present theory be correct, to be nearly equal to the definite integral, $-\mathrm{J}_t ft$, for the case where $t=20$. In other words, I am thus led to expect, after adding the constant term $\frac{1}{2}$, that the value of the connected integral,

$$\mathrm{I}_t ft = \pi^{-1}\int_0^{\frac{\pi}{2}} d\omega \sec\omega \sin(40\cos\omega), \quad . \quad . \quad . \quad (26)'$$

must be nearly equal to the following number,

$$+0{\cdot}562888. \quad . \quad . \quad . \quad . \quad . \quad (26)''$$

And accordingly, when this last integral $(26)'$ is developed by means of the *ascending* series (21), I find that the sum of the first sixty terms (beyond which it would be useless for the present purpose to go) gives, as the small difference of two large but nearly equal numbers, (which are *themselves* of interest, as representing certain *other* definite integrals,) the value:

$$\pi^{-1}\int_0^{\frac{\pi}{2}} d\omega \sec\omega \sin(40\cos\omega) =$$

$$\begin{array}{l} +3\ 772\ 428\ 770\ 679\ 800{\cdot}537\ 7058 \\ -3\ 772\ 428\ 770\ 679\ 799{\cdot}974\ 8177 \\ \qquad\qquad = +0{\cdot}562\ 888\ 1; \quad . \quad . \quad . \quad . \quad . \quad (26)''' \end{array}$$

which can scarcely (as I estimate) be wrong in its last figure, the calculation having been pushed to more decimals than are here set down; and which exhibits as close an agreement as could be desired with the result $(26)''$ of an entirely different method.

[9.] It must however be stated, that in extending the method thus exemplified to higher orders of integrals, the development denoted by $\mathrm{D}_t^{-n} ft$, or the definite and multiple integral $(-\mathrm{J}_t)^n ft$, to which it is equivalent, comes to be *corrected*, in passing to the *other* integral $\mathrm{I}_t^n ft$, not by a *constant term*, such as $\frac{1}{2}$, but by a *finite algebraical function*, which I shall here call $\mathrm{f}_n t$, and of which I happened to perceive the existence and the law, while pursuing some unpublished researches respecting vibration, a considerable time ago. Lest anything should prevent me from soon submitting a continuation of the present little paper, (for I wish to write on one or two other subjects,) let me at least be permitted now to mention, that the spirit of the process alluded to, for determining this finite and algebraical *correction**,

$$\mathrm{I}_t^n ft - (-\mathrm{J}_t)^n ft = \mathrm{I}_t^n ft - \mathrm{D}_t^{-n} ft = \mathrm{f}_n t, \quad . \quad . \quad . \quad (27)$$

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* Although this *algebraical part*, $\mathrm{f}_n t$, of the multiple integral $\mathrm{I}_t^n ft$, is *here* spoken of as a *correction* of the *periodical part*, denoted above by $\mathrm{D}_t^{-n} ft$,

(where $\mathrm{D}_t^{-n}ft$ still denotes a descending and periodical series, analogous to and including those above marked (18) and (25),) consists in *developing the algebraical expression* (10), (for the case $r=0$, but with a corresponding development for the more general case,) *according to descending powers of the symbol* I_t, and *retaining only those terms in which the exponent of that symbol is positive or zero*: which process gives the formula,

$$\mathrm{f}_n t = \tfrac{1}{2}\mathrm{I}_t^{n-1}(1+2^{-2}\mathrm{I}_t^{-2})^{-\frac{1}{2}}1 = (\tfrac{1}{2}\mathrm{I}_t^{n-1} - \tfrac{1}{16}\mathrm{I}_t^{n-3} + \tfrac{3}{256}\mathrm{I}_t^{n-5} - \ldots)1; \quad (28)$$

that is, by (5) and (6),

$$\mathrm{f}_n t = \Sigma_{m=0}^{m=\infty} 2^{-2m-1}[-\tfrac{1}{2}]^m[0]^{-m}[0]^{-(n-2m-1)}t^{n-2m-1}; \quad (28)'$$

where the series may be written as if it were an infinite one, but the terms involving negative powers of t have each a null coefficient, and are in this question to be suppressed.

For instance, I have arithmetically verified, at least for the case $t=10$, that the two finite algebraical functions,

$$\mathrm{f}_6 t = \frac{t^5}{240} - \frac{t^3}{96} + \frac{3t}{256}, \quad \ldots\ldots \quad (28)''$$

$$\mathrm{f}_7 t = \frac{t^6}{1440} - \frac{t^4}{384} + \frac{3t^2}{512} - \frac{5}{2048}, \quad \ldots \quad (28)'''$$

express the values of the two following sums or differences of integrals,

$$\mathrm{f}_6 t = \mathrm{I}_t^6 ft - \mathrm{J}_t^6 ft, \quad \ldots\ldots \quad (27)'$$

$$\mathrm{f}_7 t = \mathrm{I}_t^7 ft + \mathrm{J}_t^7 ft; \quad \ldots\ldots \quad (27)''$$

the calculations having been carried to several places of decimals, and the integrals $\mathrm{I}_t^6 ft$, $\mathrm{I}_t^7 ft$ having each been found as the difference of two large numbers.

Observatory of Trinity College, Dublin,
September 29, 1857.

[To be continued.]

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yet for *large* and *positive values* of t it is, *arithmetically* speaking, by much the *most important portion* of the whole: and accordingly I perceived (although I did not publish) it long ago, whereas it is only very lately that I have been led to *combine* with it the *trigonometrical series*, deduced by a sort of extension of Poisson's analysis.—When I thus venture to speak of any results on this subject as being my own, it is with every deference to the superior knowledge of other Correspondents of this Magazine, who may be able to point out many anticipations of which I am not yet informed. The formulæ (27) (28) are perhaps those which have the best chance of being new.

XLIV. *On the Electrical Light. By* H. W. DOVE*.

SINCE Fraunhofer first showed that the spectrum of the electric spark was distinguished from that of the light of the sun by a very bright line in the green, and a somewhat less luminous one in the orange, its prismatic analysis has been completed, especially by the investigations of Wheatstone, Masson and Ångström †. Wheatstone has shown that the lines are different according to the nature of the metals between which the spark passes; and that when it is produced between two different metals, the spectrum exhibits combined the lines which are perceived when it is produced consecutively between similar balls of each of the metals. According to Ångström, this also applies to similar balls formed of an alloy of the two metals. Masson has ascertained, that, leaving out of consideration the dissimilarity of the spectrum in the employment of different metals, determinate lines appear as common to it; and from this it has appeared the most natural opinion, that the light of the spark is mixed, consisting of a direct production of light in the medium in which the spark is produced, and glowing particles carried forward from the balls between which the spark is transmitted. Fresh proofs in favour of this view have been obtained, especially by Ångström, by the prismatic analysis of the sparks evolved in different gases.

A composite phænomenon may be indirectly investigated by changing the constituents which enter into it, or by attempting to produce these in an isolated condition. Draper has shown that the spectrum of a glowing platinum wire contains no lines, so that it is white in the strict sense; whilst with regard to the true electrical light without phænomena of incandescence, we are only able to judge of its colour by the naked eye. The luminous phænomena, known under the names of electrical brush, glow, and interruption of the spark, are so constant and so feebly luminous as to render an exact prismatic analysis extremely difficult, and in many cases even almost impossible. The judgement of the colour of a homogeneous luminosity is, however, very delusive. The ordinary gas-flame which is yellow by day, and even the orange light of an oil-lamp, appear white in the dark. For this reason we may presuppose that the eye will only furnish an uncertain judgement as to the colour of the weaker electrical luminous phænomena. Many of these are so faint, that for their exact perception they require the exclusion of every other source of light; and Prevost, as is well known, has already observed, that with coloured illumination the

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* Poggendorff's *Annalen*, No. 6. 1857.

‡ See Phil. Mag. vol. ix. p. 329.

brightest at last appears white. The colour of a source of light may, however, be investigated by allowing it to be absorbed by coloured dioptric media, or by investigating catoptric colours in its luminosity. I have availed myself of this method to compare the weaker electrical luminous phænomena with those of the spark.

The electrical brush may be produced in two ways: by attaching the point either to the positive primary conductor itself, or to a second conductor into which sparks pass continuously from the primary conductor. In the former case its rays are closer, but less branched and diffused; the brighter reddish-violet light, however, from which the rays are evolved is more intense, so that the whole brush appears to be illuminated by it. In the second case, the spark between the two conductors assumes, almost completely, the part of this bright basal point of the brush, the rays of which, however, are now much more branched. A similar difference is exhibited in the formation of the luminosity in a large exhausted electrical egg. If the superior conductor passing through the stuffing-box be in immediate contact with the primary conductor, the reddish-violet, perpendicularly descending stream of light is intense, whilst the diffused light of the rest of the space is weak; if, on the contrary, sparks be allowed to strike continually upon the superior conductor, the intensity of the perpendicular stream of light diminishes, whilst the whole space is filled with band-like whitish streaks of light, which incessantly change their form. This alone renders it probable that the perpendicular stream of light is the basal point of the brush which has become elongated *in vacuo,* and that the white bands correspond with its rays.

If the brush be looked at through a deep blue cobalt glass of half an inch thick which effaces the middle of the spectrum, its ramifications are still seen very distinctly, whilst they disappear completely in a red glass. A green glass which so obscures the red that when they are superimposed in ordinary daylight one seems to have a board before one's eyes, permits the passage of the rays, although more weakly than the cobalt glass. A blue picture on the red field appears to be brightly illuminated by the rays of the brush, upon a dark ground; a red picture on the blue field appears dark upon a bright ground, consequently just as when they are looked at in daylight through the deep blue glass. When looked at through an equilateral prism of Guinand's flint-glass, in which I can see several of Fraunhofer's lines of daylight with the naked eye, the rays of the brush appear nearly unchanged in colour, and only a little broader, whilst the bright basal point of the brush gives a spectrum in which red, green, and violet appear brilliantly, and which scarcely differs from that of a small spark. The comparison is best made when the second conductor in contact with the point is alternately brought in contact with the conductor and removed from it. The spectrum recognized at the basal point of the brush is then transferred to the point of interruption, where the sparks pass. The phænomenon in the electrical egg is exactly analogous. The perpendicular stream of light gives a many-coloured spectrum, consisting of a very broad blue border, a broad green and a narrow red streak; it is faintly visible through a red glass, whilst the band-like streaks are seen very clearly through the cobalt glass, but are completely absorbed in the red glass.

The light of an exhausted glass tube containing a little mercury, which appears brilliantly white in the dark, and in daylight a bluish-green, is not visible through the red glass, but very distinct through a green one, and rather less bright through the deep blue. If the tube be held to the conductor, it shines for a long time uninterruptedly; and besides blue and green, the spectrum contains a slight trace of red. I have sketched coloured spirals upon a white ground, which, when looked at through certain glasses of the same colour, disappear in such a way, that, on turning them round, the perfectly white hinder surface cannot be distinguished from that on which the coloured spiral has been drawn. This experiment succeeds without any coloured glass in the case of a spiral sketched with Schweinfurt green, when illuminated in the dark by the mercurial tube; the light in the mercurial tube consequently has the colour of this spiral.

The electric spark is distinctly visible through any coloured glasses, with the colour of the latter. Catoptric colours momentarily illuminated by it appear distinctly, as also do colours of interference when I concentrate the sparks of a self-discharging Leyden flask, by the object-lens of my polarizing apparatus, upon the aperture of the polarizing Nicol's prism; and the calc-spar plate, which appears colourless when the Nicol's prism is rapidly rotated in continual illumination, then exhibits the annular system distinctly, and therefore behaves exactly like the coloured sectors of a rotating colour-circle. Whilst the nature of the metals exerted an influence upon the spectrum of the spark, the absorption-phænomena of the rays of the brush remained unaltered, when I developed it from gold, platinum, iridium, nickel, iron, bismuth, tin, zinc and copper, or from a drop of water sprinkled on the conductor; this is in accordance with Faraday's observations. Whilst the introduction of a moist thread essentially modifies the light of the spark, the brush produced by a conductor united with the primary conductor by a wet thread remains unchanged. On the other hand, the luminosity of a uranium glass occurs with equal vividness with the brushes and sparks. I have found no essential difference between the luminosity of a Ruhmkorff's apparatus and that of an electrical machine, both as regards the sparks and brush in the air, and the luminosity in the electrical egg.

The spark of an electrical machine often appears interrupted at one spot by a weaker violet or reddish light. This interrupted spot generally lies nearest to the negative end; and by removing to an appropriate distance a non-insulated conductor placed near the principal conductor, a stream of sparks may easily be obtained which appears white at the primary conductor, and coloured at the conductor standing near it. This less luminous part is, however, very distinctly visible through a red glass, so that it is distinct from the light of the brush.

The preceding experiments, in connexion with the results of the prismatic investigation of the spark, appear to me to lead to the following conclusion.

A wire becoming red-hot by heat is first red, then orange, and lastly white, so that it behaves like the combination of light which is obtained when a screen is drawn away from the spectrum concealed by it in such a way that the red end first becomes visible, and to this the violet is finally added. The increase of brilliancy from the slightly luminous brush to the bright spark behaves quite otherwise. In this case it is as if the screen removed first set free the violet end, and then the other colours. This distinction of itself renders it improbable that the phænomena of electrical light in the state of less brilliancy can be ascribed to a gradually increasing ignition of solid particles. They rather resemble the weakly luminous flame of hydrogen, which becomes white by solid ignited carbon in the so-called gas-flames, or by other solid matters, as in the Drummond light. The true electrical light is produced at great distances in the surrounding, isolating, aëriform medium, when the latter is attenuated. With this coloured light belonging to the strongly refrangible part of the spectrum, phænomena of ignition may be combined, by particles torn away from the positive and negative bodies. If these particles be only at a red heat, the impression of a violet light is produced by their mixture with the electric light. To this class belong the column of light in the electrical egg, and the basal point of the brush, and lastly, the indented reddish sparks of an electrical machine, at distances to which a white spark does not pass. If particles at a white heat come together, the whole is white, as in the sparks of Leyden jars; in opposition to the bright light of incandescence, the less strongly luminous electric light disappears in the same way as the weak bluish lower part in a gas-flame appears black in opposition to the bright mass of light, whilst with the small brilliancy of a wax-light the latter betrays its colour even without optical aids of absorption. Only prismatic analysis and the action upon uranium glass indicate the presence of the electric light also. If the particles at a white heat do not reach each other, the spark acquires a spot of interruption, which, however, still shows red light besides the true electric light, when the particles previously at a white heat have become cooled to redness. The basal point of the brush, which retrogrades in proportion to the larger field in which the electric light becomes visible, is to be compared with the spot of interruption of the spark; the particles of the solid body which are here still red-hot may, on reaching a greater distance, be completely extinguished, so that then the electric light alone prevails. The brush could not be coloured by a spirit-flame coloured yellow with chloride of sodium held under it, as it then becomes converted into a spark. The phænomena of the exhausted tube with mercury indicate the modification which the electric light undergoes in media other than atmospheric air.

XLV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 314.]

March 19, 1857.—Dr. W. A. Miller, V.P., in the Chair.

THE following communications were read:—

"On the Action of Aqueous Vapour in disturbing the Atmosphere." By Thomas Hopkins, Esq.

In this paper it was maintained that the great disturber of the equilibrium of atmospheric pressure is the aqueous vapour which is diffused through the gases. These gases, when ascending, cool (say 5°) through expansion by diminution of incumbent pressure, whilst the vapour that is within them cools only 1°; and a consequence is, that when a mixed mass ascends, the vapour is condensed by the cold of the gases. It is well known that condensation of vapour gives out much heat, and this heat warms and expands the gases when they are forced to ascend, taking vapour with them; and the process being repeated and continued, an ascending current is produced in the atmosphere, cloud is formed, the barometer sinks, rain falls, and winds blow towards the part.

This was shown to take place in all latitudes, producing disturbances great in proportion to the amount of vapour condensed. In tropical regions, where the aqueous material is abundant, the disturbances are great, but take place principally in the higher regions of the air. The diminution of atmospheric pressure within the tropics at the surface of the earth, as measured by the barometer, extends over a large surface, but is not great in any one place. In cooler latitudes condensation takes place nearer to the surface of the globe, and then reduction of pressure is confined to a smaller area; but in parts on the surface within that area the reduction is great, because the lower and therefore heavier gases have been warmed and expanded; hence the falls of the barometer in certain cool localities are the greatest. In very cold and dry regions, as a consequence of there being but little vapour in the air to be condensed, the barometer sinks only a little, and that sinking is generally confined to a small area. In accordance with this view, it was shown that, in certain places, where much continuous rain falls, the barometer has a low average; and towards these areas winds blow from distant parts, as in the great trade- and other winds. Sea-breezes were also shown to be consequences of the condensation of vapour, which had been produced by the morning sun ascending to sufficient elevations; whilst the land-winds at night are attributable to the cooling of those elevated parts by evaporation during the absence of the sun.

Various objections that had been made to this theory of atmospheric disturbances were noticed by the author of the paper, which, though admitted to be plausible, were stated to be invalid, whilst the most important meteorological phenomena were asserted to be in accordance with it.

"On the Serpentines of Canada and their associated Rocks." By T. Sterry Hunt, Esq., of the Geological Survey of Canada.

The origin and formation of serpentine is still regarded as an unsettled problem by chemical geologists, and Sir William Logan having shown from structural evidence the undoubted stratified character and sedimentary origin of the serpentine of the Green Mountains in Canada, I have been induced to make a chemical and mineralogical investigation of these serpentines and the rocks associated with them. In the present note I propose to indicate briefly some of the results obtained, reserving for another occasion the details of my examination.

The serpentines of the Green Mountains, which have been traced for 150 miles in Canada, have been found by Sir William to belong to the upper portion of the Lower Silurian system, whose disturbance and metamorphism have given rise to the great Apallachian chain, of which the Green Mountains are the north-eastern prolongation. These mountains are composed of gneissoid, micaceous, argillaceous, talcose, and chloritic schists, with quartzite, limestone, dolomite, serpentine, pyroxenite, and the other rocks about to be mentioned.

The serpentines, which form immense beds, and often cover large areas, are sometimes homogeneous, and at other times conglomerate in their character, the cement being a ferruginous dolomite, or more rarely a carbonate of magnesia, exempt from lime but containing carbonate of iron. In some cases the serpentine is intimately mixed with a large amount of carbonate of lime. Chromic and magnetic iron, ilmenite, diallage, with the ordinary lamellar and fibrous varieties, picrolite and chrysotile, are common in these serpentine rocks. The results of a great number of analyses show a uniformity of composition in all the serpentines of this formation, and also show a curious fact hitherto overlooked,—that of the constant presence of a small portion of nickel, never exceeding a few thousandths. I have never failed to detect it in any variety of serpentine from this formation, not only in Canada, but in the States of Vermont, Connecticut, and New Jersey. Its presence seems still more widely spread, for I have also found nickel in serpentines from California, the Vosges in France, and in a *verde antique* marble from a Roman ruin. The association of nickel with the chromic iron and serpentines of Pennsylvania has been long known, and I have found the chromic iron of Canada to contain small portions both of nickel and cobalt, although the latter metal can rarely be detected in the nickeliferous serpentines.

The results of a number of analyses show that it is constantly present in the talcose slates and steatites of this region, and the same is to be observed of the magnesites and dolomites of the series; indeed the distribution of nickel would seem to be co-extensive with that of the magnesia in this formation, and the same thing may be said of chrome. I have not, however, met with any traces of chrome or nickel in the serpentines and talcose rocks of the Laurentian system, which underlies the Silurian and the still older copper-bearing rocks of Lake Huron, and probably corresponds to the oldest gneiss of Scandinavia. Both chrome and nickel, however, characterize the serpentines of the Vosges and of California. I have not yet been able to examine specimens from other foreign localities. The presence of traces of nickel in certain talcs was long since noticed by Stromeyer. Much of the so-called talcose slate of the Green Mountains is not magnesian, but consists of a hydrous aluminous silicate allied to pyrophyllite or pholerite, which are alumina-talcs.

The euphotides, which are associated with many of the European serpentines, are not wanting in the Green Mountains, although less distinctly marked to the eye than the foreign varieties. A tough, greenish or greyish-white rock, with a waxy lustre, forms in many places great stratified masses, which are associated with the serpentine, and is found on analysis to consist of a soda-felspar (albite) with a silicate of lime, magnesia, and protoxide of iron, having the composition of amphibole,—thus constituting a veritable euphotide. The two minerals are clearly distinguishable after calcination, which blanches the felspar, and reddens the ferruginous silicate. These rocks are by this means distinguished from others similar in their appearance and mode of occurrence, but consisting of petrosilex or compact siliceous felspar, and equally members of the sedimentary series. The specific gravity of these euphotides shows that the saussurite or felspar which forms their base has a density not greater than that of ordinary soda-felspar.

Immediately connected with the serpentines, there sometimes occurs a white compact rock, remarkable by its great hardness, and a density of 3·3 to 3·5. Analysis shows this rock to be a pure lime-alumina garnet, in some cases, however, mingled with another silicate which appears to belong to the amphiboles. This garnet is sometimes blended with serpentine, and at others forms distinct beds. In its general aspect it resembles closely the saussurite of the associated euphotides, and has probably often been confounded with that mineral by previous observers. Hence the densities of 3·2 and 3·3 assigned by different mineralogists to the saussurites of the Alps, while Delesse has shown that the true saussurite of the euphotide of Mount Genèvre, like that of the Vosges, is a felspar.

The magnesites of this region form great beds; they are crystalline, and consist of carbonate of magnesia with some carbonate of iron, and contain as imbedded minerals in some cases grains of quartz, in others felspar and talc, and at other times serpentine, but always holding chrome and nickel, the latter as a greenish carbonate, in the joints of the rock, or in the form of nickeliferous pyrites.

These magnesian rocks are not confined to the altered portions of this formation; beds of siliceous dolomite holding protocarbonate of iron are found, interstratified with pure fossiliferous limestones, near Quebec. The reaction between silica and the carbonates of lime, magnesia, and iron, which takes place at no very elevated temperature, in the presence of water, producing silicates of these bases with evolution of carbonic acid, enables us to understand the process which has given rise to the pyroxenes, serpentines, and talcs of this formation, while the argillaceous limestones, which are not wanting, contain all the elements of the garnet-rock.

The general conclusion deduced from these inquiries, and sustained by a great number of analyses, which I hope soon to submit to the Society, is, that the metamorphism of these Silurian strata has resulted from the chemical reaction, in the presence of water, of the elements existing in the original sedimentary deposits.

"On Determinants, better called Eliminants." By Professor Francis Newman, M.A.

1. This paper aimed at recommending the introduction into elementary treatises of the doctrine of Determinants; which, following Professor Boole, it called *Eliminants*. It exemplified the great aid to the *memory* which the notation affords. It undertook to show, that if only so much of new notation be used, as is needed in elementary applications, the subject becomes full as easy as the second part of algebra. The method of proceeding recommended may be understood by the following concise statement.

If n linear eqq. are given, connecting n unknown quantities; and every eq. is represented by $A_r x + B_r x_2 + C_r x_3 + \cdots + N_r x_n = P_r$ (where r is 1, 2, 3 ... n in the several eqq.), then, solving for any one of the unknowns, we of course obtain a result of the form $mx = a$. Very simple considerations then show, that m and a will be *integer functions of the coefficients*: namely, it is easy to prove, that *if* this is true for one number n, it must needs be true also for the number $(n+1)$; and consequently is generally true. Next, the same analysis exhibits, that $m=0$, is the result obtained, when $P\,P_2\,P_3 \dots P_n$ all vanish: moreover, that if the system presented for solution be the $(n-1)$ eqq.

$$\left.\begin{array}{l} Bv+B_2v_2+B_3v_3+\ldots+B_n=0 \\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ Nv+N_2v_2+N_3v_3+\ldots+N_n=0 \end{array}\right\}$$

and the solutions are denoted by

$$m'v=a_1\,;\ m'v_2=a_2\,;\ \ldots m'v_{n-1}=a_{n-1}\,;$$

we get the relations

$$\left.\begin{array}{l} m=Aa_1+A_2a_2+A_3a_3+\ldots+A_{n-1}a_{n-1}+A_nm'\,; \\ a=Pa_1+P_2a_2+P_3a_3+\ldots+P_{n-1}\,a_{n-1}+P_nm'\,; \end{array}\right\}$$

out of which flow all the rules for the *genesis* of Eliminants, and the application of them to solve linear eqq. of any degree.

In adapting the theory to the proof of elementary propositions, as, in forming the *Product* of two Eliminants, the paper urged the utility of the principle, that every Eliminant is *a linear function of any one of its columns*, and also, *of any one of its rows*;—which principle may often be so applied as to show by inspection, *à priori*, that certain constituents are excluded from this and that function, and thus enable us to obtain its value by assuming arbitrary values for such constituents. It deprecated (at least for elementary uses) the notations used by Mr. Spottiswoode* and others, not only as involving needless novelty to learners, but because no page can be broad enough to afford to write

$$(1,\,2)(1,\,1)'+(2,\,2)(1,\,2)'+(3,\,2)(1,\,3)' \text{ instead of } BX+bY+\beta Z,$$

and because accents, so related, are hard to see in a full page, and the general aspect of every element is so like that of every other element, that the fatigue of reading soon becomes confusing and intolerable.

2. But the main topic of the paper was to advocate the use of Eliminants in Geometry of three dimensions, especially in every systematic treatise on Surfaces of the Second Degree. Various illustrations and results were given, which the writer believed to be new; on which account, some of them may be briefly noticed here.

Problem. "To find the length of a perpendicular ρ, dropt from a given point $(a\,b\,c)$, on to a given plane $lx+my+nz+p=0$; when the axes are oblique, and the cosines of the angles $(xy)(xz)(yz)$ are *given*; viz.$=$D, E, F."

Result. Take G and H to represent the eliminants

$$G=\begin{vmatrix} 1 & D & E \\ D & 1 & F \\ E & F & 1 \end{vmatrix} \text{ and } H=\begin{vmatrix} 1 & D & E & -l \\ D & 1 & F & -m \\ E & F & 1 & -n \\ l & m & n & 0 \end{vmatrix};$$

then ρ is known from the eq.

$$\rho\sqrt{H}=(la+mb+nc+p)\sqrt{G}.$$

When ρ is given, this eq. determines the relations between $l\,m\,n\,p$,

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* It may be right to state, that Mr. Newman opened the paper by a grateful and honourable recognition of Mr. Spottiswoode's labours.

which are the test, that the plane may touch a sphere given in position.

Problem. To analyse the forms assumed by the locus of the general eq.

$$Ax^2+By^2+Cz^2+2A_2x+2B_2y+2C_2z$$
$$+2Dxy+2Exz+2Fyz+G=0 \text{ (axes oblique).}$$

Result. Let $V=\begin{vmatrix} A & D & E \\ D & B & F \\ E & F & C \end{vmatrix}$ and $W=\begin{vmatrix} A & D & E & A_2 \\ D & B & F & B_2 \\ E & F & C & C_2 \\ A_2 & B_2 & C_2 & G \end{vmatrix}$; then in the common treatises (only without this notation) it is shown that when V is finite, the surface (if real) has a centre. It is here added that when W is negative, the curvature is everywhere towards the same side of the tangent plane; when W vanishes, the tangent plane coincides with the surface in one straight line; but when W is positive, the surface is *cut* by the tangent plane in two intersecting straight lines, and the curvature bends partly towards one side of the tangent plane, partly towards the other.

Hence it appears that we have different sorts of surfaces, by combining $V=0$ or $V=\text{finite}$, with $W=0$ or $W=\text{positive}$, or $W=\text{negative}$.

The locus is *imaginary*, if W is >0, A and B finite, $CG-C_2^2>0$, and $C\begin{vmatrix} A & E & A_2 \\ E & C & C_2 \\ A_2 & C_2 & G \end{vmatrix}>0$.

The locus is *degenerate*, if of ABC one at least (as C) be finite, and if $V=0$, $\begin{vmatrix} A & E & A_2 \\ E & C & C_2 \\ A_2 & C_2 & G \end{vmatrix}=0$, $\begin{vmatrix} B & F & B_2 \\ F & C & C_2 \\ B_2 & C_2 & G \end{vmatrix}=0$: or if ABC all vanish, and if at the same time $D=0$, and $E:F:C_2=2A_2:2B_2:G$.

Problem. To investigate the nature of the plane intersections of the surface.

Result. If the cutting plane be $lx+my+nz+p=0$, the section is a hyperbola, parabola or ellipse, according as $\begin{vmatrix} A & D & E & l \\ D & B & F & m \\ E & F & C & n \\ l & m & n & o \end{vmatrix}$ is positive, zero, or negative.

The intersection *degenerates*, if $\begin{vmatrix} A & D & E & A_2 & l \\ D & B & F & B_2 & m \\ E & F & C & C_2 & n \\ A_2 & B_2 & C_2 & G & p \\ l & m & n & p & o \end{vmatrix}=0$.

In a non-centric surface, where $V=0$, we readily find that the former of these eliminants has the same sign as (D^2-AB); and consequently, that non-centric surfaces cannot have sections of opposite species. It also appears, that to determine in a non-centric surface the parabolic sections, we must take $l\,m\,n$ such as to verify one of the three eqq.

$$\begin{vmatrix} A & D & E \\ D & B & F \\ l & m & n \end{vmatrix}=0, \quad \begin{vmatrix} A & D & E \\ l & m & n \\ E & F & C \end{vmatrix}=0, \quad \begin{vmatrix} l & m & n \\ D & B & F \\ E & F & C \end{vmatrix}=0.$$

Problem. To determine the circular sections, when they exist.

Result. Take the larger question, of ascertaining when two surfaces of the second degree intersect in a plane curve. Denote the coefficients of the second surface by accents. Put $\alpha=A\rho-A'$; $\beta=B\rho-B'$; $\gamma=C\rho-C'$; &c. and determine ρ by the eq.

$$\begin{vmatrix} \alpha & \delta & \epsilon \\ \delta & \beta & \phi \\ \epsilon & \phi & \gamma \end{vmatrix}=0;$$

which involves ρ in the third degree.

Then $l\,m\,n$ will be determined (when the surds are real) by the proportion

$$l:m:n=\sqrt{(\epsilon^2-\alpha\gamma)}+\epsilon : \sqrt{(\phi^2-\beta\gamma)}+\phi : \gamma.$$

To apply this to the problem of circular sections, it is only necessary to suppose the second surface to be a sphere.

The surface becomes one of Revolution, if (with oblique axes) either system of three eqq. is fulfilled:

$$\begin{cases} (1) \quad \alpha\beta=\delta^2, \quad \alpha\gamma=\epsilon^2, \quad \beta\gamma=\phi^2, \\ (2) \quad \alpha\phi=\delta\epsilon, \quad \beta\epsilon=\phi\delta, \quad \gamma\delta=\epsilon\phi. \end{cases}$$

If out of each triplet we eliminate ρ^2 and ρ, (for it seems easiest to treat these as independent variables,) the result is two eqq. (expressible by eliminants), which are the two general conditions for a surface of revolution.

Problem. To find the system of rectangular conjugates. This of course is cardinal, and is treated everywhere: but is made far easier by Eliminants, as follows. Let us inquire after *that diameter, common to two given concentric surfaces, which shall have its conjugate planes the same for both.*

Take the centre for the origin, and $x=mz$, $y=nz$ for the common diameter sought. Then the central planes conjugate to it in the two surfaces are

$$\left.\begin{array}{l} (Am+Dn+E)x+(Dm+Bn+F)y+(Em+Fn+C)z=0 \\ (A'm+D'n+E')x+(D'm+B'n+F')y+(E'm+F'n+C')z=0. \end{array}\right\}$$

To identify these two planes, let

$$\frac{Am+Dn+E}{A'm+D'n+E'}=\frac{Dm+Bn+F}{D'm+B'n+F'}=\frac{Em+Fn+C}{E'm+F'n+C'}=\frac{1}{\rho},$$

or $$\alpha m+\delta n+\epsilon=\delta m+\beta n+\phi=\epsilon m+\phi n+\gamma=0.$$

Eliminate m, n, and you find that ρ is to be determined by the very same eq. as in the preceding; and since its eq. is of the third degree, it has always one real value.

Next, let the second surface be a sphere, and you find *at least one* diameter of the first surface *perpendicular* to its conjugate plane. Make this diameter the axis of x, and take for the axes of y and z the two *principal* diameters of the section in the conjugate plane. Then $D=0$, $E=0$, $F=0$; so that the general eq. is reduced to $Ax^2+By^2+Cz^2+G=0$. Moreover, the system of axes is now rectangular: hence the axis of y, and that of z, equally with that of x, are each perpendicular to its conjugate plane, and the eq. for ρ must have three real roots, corresponding to these three axes.

We might similarly investigate "the conditions of contact for two concentric surfaces;" which, when one of them is a sphere, gives the cubic whose roots are a^2, b^2, c^2, principal axes of an Ellipsoid.

Problem. To discuss the results of *Tangential Co-ordinates.* [This expression is employed as by Dr. James Booth in an original tract on the subject.]

Put
$$\left.\begin{array}{l} P=Ax+Dy+Ez+A_2 \\ Q=Dx+By+Fz+B_2 \end{array}\right| \left.\begin{array}{l} R=Ex+Fy+Cz+C_2 \\ S=A_2x+B_2y+C_2z+G \end{array}\right|$$

Then $Px+Qy+Rz+S=0$ is the eq. to the surface, and $Px'+Qy'+Rz'+S=0$ is the eq. to the tangent plane at (xyz). Hence if $x'y'z'$ are the three tangential co-ordinates (or intercepts cut from the co-ordinate axes by the tangent plane) we have $Px'+S=0$, $Qy'+S=0$, $Rz'+S=0$. Let $\xi\ \eta\ z$ be the *reciprocals* of $x'\ y'\ z'$. Then $P+\xi S=0$, $Q+\eta S=0$, $R+\zeta S=0$; and the eq. to the surface becomes $\xi x+\eta y+\zeta z-1=0$. Restore for PQR their equivalents; then eliminating $xyzS$ you get

$$\begin{vmatrix} A & D & E & A_2 & \xi \\ D & B & F & B_2 & \eta \\ E & F & C & C_2 & \zeta \\ A_2 & B_2 & C_2 & G & -1 \\ \xi & \eta & \zeta & -1 & 0 \end{vmatrix}=0;$$

general eq. to the surface, with axes oblique.

If the last eq. (developed) be represented by

$$a\xi^2+b\eta^2+c\zeta^2+2a_2\xi+2b_2\eta+2c_2\zeta+2d\xi\eta+2e\xi\zeta+2f\eta\zeta+g=0,$$

it is not difficult to obtain a system of eqq. in which $abc\ldots\xi\eta\zeta$ play the same part, as just before did $ABC\ldots xyz$. Whence again we have,

$$\begin{vmatrix} a & d & e & a_2 & x \\ d & b & f & b_2 & y \\ e & f & c & c_2 & z \\ a_2 & b_2 & c_2 & g & -1 \\ x & y & z & -1 & 0 \end{vmatrix}=0;$$

which is the *original* eq. of the surface under the form of an Eliminant.

The most arduous problems (as Dr. James Booth has shown) are often facilitated by these co-ordinates; but without Eliminants, the eqq. cannot be treated generally and simply.

The paper likewise contained the application of Eliminants to tangential co-ordinates in Curves of the Second Degree; and urged that eliminants ought to be introduced into the general treatment of these curves also, if only in order to accustom the learner to their use and gain uniformity of method. Thus, if the general eq. be

$$\mathrm{A}x^2+\mathrm{B}y^2+\mathrm{C}+2\mathrm{E}x+2\mathrm{F}y+\mathrm{G}=0,$$

then $\mathrm{V}=0$ is the test of degeneracy.

March 26.—Major-General Sabine, R.A., Tr. and V.P., in the Chair.

The following communication was read:—

"On the Theory of the Gyroscope." By the Rev. William Cook, M.A.

The explanation of the movements of the Gyroscope (as well as its mathematical theory) is founded on the principle enunciated in the two following verbal formulæ.

I. When a particle is made to move $\left\{\begin{matrix}\text{towards}\\ \text{from}\end{matrix}\right\}$ a plane by any applied force, but in consequence of its connexion with some rigid body on the same side of the plane, loses some of its momentum in a direction perpendicular to the plane; all the momentum so lost is imparted to the rigid body, which is consequently impelled $\left\{\begin{matrix}\text{towards}\\ \text{from}\end{matrix}\right\}$ the plane.

II. When a particle is made to move $\left\{\begin{matrix}\text{towards}\\ \text{from}\end{matrix}\right\}$ a plane by any applied force, but in consequence of its connexion with some rigid body on the same side of the plane, receives an extra momentum in a direction perpendicular to the plane; all the momentum so gained is taken from the rigid body, which is consequently impelled $\left\{\begin{matrix}\text{from}\\ \text{towards}\end{matrix}\right\}$ the plane.

The mass of the disc of the gyroscope is supposed to be compressed uniformly into the circumference of a circle of given radius (r), and to revolve round an axis with a given uniform angular velocity (ω). To facilitate the arithmetical computation of the formulæ, masses are represented by weights; so that any effective accelerating force f is supposed to be due to a pressure P acting on a mass W, and their relation expressed thus, $f=\frac{\mathrm{P}g}{\mathrm{W}}$.

The mass of any arc of the circle is denoted by $\frac{cr\theta}{l}$; θ being the angle at the centre, and c the mass of a given length l of the circumference. The terms of all the formulæ are thus made homogeneous.

The centre of gravity of the disc, axle, and the ring which carries the pivots of the axle is fixed, and the whole is moveable about that centre in any manner, subject to the condition that the line of the pivots of the ring is always horizontal, unless when detached from the stand. Let this straight line of the pivots be denoted by AB, the common centre of the disc and ring by O, the extremities of the axle by N and S; and $\mathrm{ON}=a$.

Let M denote the place of a particle of the disc, its position being determined by the angle AOM (θ), and let M′ be another point in the disc indefinitely near to M, but more remote from A, the direction in which the disc will presently be supposed to revolve being AMM′B.

A given force F is applied at N perpendicular to the plane ANBS, so that the disc may describe an angle ϕ round AB in the time t; whereby the points M and M′ describe the two arcs $MP=y$ and $M'P'=y'$ simultaneously. Suppose the circumference of the circle AMB to be divided into four quadrants, commencing at A, where $y=O$, and corresponding with the four ranges of value of θ through each of four right angles; suppose M and M′ to be in the first quadrant, so that y' is greater than y; then if the disc be supposed to revolve, a particle at M is carried from the line MP to the line M′P′, so as to acquire an increase of velocity from the plane AMM′ independently of the force F, and consequently (by the first of the two verbal formulæ) all the momentum so acquired by the particle is lost to the disc, ring, &c., which are thus impelled as by a force in the direction PM or P′M′, so as to oppose the rotation imparted by F, but to impart another round O in the direction ANB in the plane of the ring; *i. e.* in a plane perpendicular to that in which F acts. A force having the same tendency is found, by means of one or the other of the two verbal formulæ, in the other three quadrants, and thus every particle (dm) of the disc contributes to the same effect. This effect is due to the difference of the velocities $\frac{dy}{dt}$ and $\frac{dy'}{dt}$ at P and P′, or to the momentum $\left(\frac{dy'}{dt}-\frac{dy}{dt}\right)dm$ lost or gained by the particle dm in the time dt.

The value of $\frac{dy}{dt}$ is obtained from the equation $y=r\phi\sin\theta$, making both ϕ and θ to vary; but the value of $\frac{dy'}{dt}$ is obtained from that of $\frac{dy}{dt}$ by making θ only to vary. It is thus shown that

$$\left(\frac{dy'}{dt}-\frac{dy}{dt}\right)dm=\left(\cos\theta\,.\,\frac{d\phi}{dt}-\omega\,.\,\phi\sin\theta\right)r\omega dtdm.$$

It is thence shown, by taking the moments about AB, and applying D'Alembert's principle, that

$$\left(\frac{d^2\phi}{dt^2}+\omega^2\phi\right)\int\sin^2\theta d\theta-\omega\frac{d\phi}{dt}\int\sin\theta\,.\,\cos\theta d\theta=\frac{Fag}{cr^3},$$

the integrals applying to θ only, and between the limits O and 2π; *i. e.* to all the particles of the disc simultaneously and independently of ϕ or t. From this is obtained the result

$$\phi=\frac{4Fag}{Wr^2\omega^2}\,.\,\sin^2\left(\frac{\omega t}{2}\right);$$

W being the weight of the disc.

This value being periodical, and ranging between the limits O and the maximum $\frac{4Fag}{Wr^2\omega^2}$, shows that the disc makes oscillations which are of less extent and duration, as the spinning of the disc is more rapid; *i. e.* as ω^2 is made greater compared with $\frac{F}{W}$; and thus if F denotes a small weight (such as is usually supplied with the apparatus by the makers), the extent of the oscillation becomes insensible. This formula, applied to the apparatus with which the experiments were made, gives the theoretical maximum of ϕ about 18 minutes of a degree. It is evident that when F represents a weight, it should be replaced in the differential equation by F cos ϕ, but the result practically coincides with that actually obtained when F is not excessive.

That these oscillations must exist will be evident, when it is considered that the gyroscope, with the weight attached and the disc not spinning, becomes an ordinary pendulum: the effect of the spinning being to disturb its oscillations, and to lessen their extent to an unlimited amount, whenever the spinning of the disc is sufficiently rapid.

The preceding investigations, as well as the experiments, show that whenever a force is applied to the axis of a revolving disc, more or less of the momentum due to this force is converted into a momentum of rotation parallel to a plane which is perpendicular to that in which the force acts.

XLVI. *Intelligence and Miscellaneous Articles.*

ON DEMIDOVITE, A NEW SPECIES OF MINERAL FROM NIJNE TAGUIL IN THE URAL. BY N. NORDENSKIOLD.

For some years there has been brought from Taguil* a mineral which occurs but rarely, and to which the name of *blue malachite* has been given on the spot. The name cannot by any means be applied to the mineral in question, as it contains no traces of carbonate of copper, although it is found in thin layers with malachite. This mineral possessing, as will be seen hereafter, a new and very interesting composition, I venture to propose giving it the name of *Demidovite,* as a permanent homage to His Excellency M. Anatole de Demidov, who has so powerfully contributed to the advancement of mineralogy and geology by his munificence and his scientific travels.

* These works belong to Prince Anatole Demidoff, and to his nephew Paul Demidoff. They lie 150 wersts (about 100 English miles) to the north of Ekaterinenburg, on the east side of the frontier between Asia and Europe, but one part of the property is also on the European side.

The yearly produce amounts to more than 1,000,000 pud† of iron (the bar iron from there is known in this country under the name of old Sobel iron); nearly 240,000 pud of copper; and about 50 pud of native platina and 25 pud of gold is yearly washed from the sand. The platinum is only found on the European side of the Ural Mountains, but gold on both sides, although for the most part on the east, or Asiatic side. A great deal of the iron and copper goes directly to Asia.

The forests whence the charcoal is derived are very extensive, and give, when sixty years are allowed for their renovation, 275,000 korob of charcoal yearly, every korob containing 70½ cubic English feet.

The proprietors avail themselves of every new discovery in mining and metallurgy, and have in their employment natives of nearly every country of Europe,—England, France, Germany, Sweden and Finland.

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† The pud is somewhat less than 40 lbs.

Demidovite occurs upon the mammillated masses of malachite in layers of at the utmost 1 or 2 millimetres in thickness; sometimes several of these layers are seen alternating with malachite and Taguilite, and other phosphatic minerals. Between the Demidovite and the malachite there is often a very thin white layer, either composed of pure silica, or of silica mixed with white carbonate of copper*.

The external surface, where it has not been covered by malachite, is tolerably shining, but uneven and full of little clefts in different directions, scarcely visible by the naked eye.

It is of a sky-blue colour, sometimes passing a little into greenish.

Its fracture is dull, or of a waxy lustre.

It is transparent only at the edges, but becomes more so when placed in water. It is so porous that it adheres to the tongue.

Brittle.

Hardness nearly the same as that of gypsum.

When scratched it yields a white powder.

Its specific gravity is about 2·25.

Before the blowpipe it becomes black in the oxidizing flame; in the inner flame it fuses readily, and furnishes a black scoria, acquiring a metallic lustre on the surface. In a glass tube closed at one end it becomes black and furnishes water, which presents no trace of acid if the fragments be clean.

With borax it fuses readily, and without swelling, in the outer flame, furnishing a glass which is at first black, and acquires a bluish-green colour in cooling: in the inner flame the globule becomes nearly colourless, in consequence of the reduction of the copper. Every time that the globule is heated afresh, a greenish flame is seen; this no longer appears when the heat is continued.

With salt of phosphorus a yellow colour is obtained as long as the globule is warm; after cooling, it has the same colour as with borax, but the fragment leaves a skeleton of silica. The globule cannot be decolorized in the inner flame.

When fused for a long time with boracic acid into which an iron wire has been introduced, the wire dilates a little and acquires a very pale copper colour; the iron nevertheless remains malleable.

With a little soda it fuses and gives a black glass; if the quantity of soda be increased, the copper is reduced, but it requires a large amount of soda to cause the scoria to enter the charcoal. The copper obtained is a little paler than that procured by employing red copper.

In the laboratory which I was able to establish at Taguil, I made an analysis of this mineral, from which I saw that it possessed a new composition. I do not, however, regard the analysis as perfectly satisfactory with regard to the proportions of the substances of which it is composed; because, as I was only able to collect a very small quantity of the mineral in a state of perfect purity, I was obliged to operate upon very small quantities.

After having, by preliminary experiments, ascertained the presence

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* An artificial compound of carbonate of copper of a white colour has been investigated by M. H. Struve of St. Petersburgh.

of phosphoric acid in this mineral, I fused it, after reducing it to a very fine powder, with a mixture of potash and soda, and after getting rid of the phosphoric acid in the usual manner, I precipitated the copper from a slightly acid solution by means of sulphuretted hydrogen. The analysis, in which the quantity of phosphoric acid was determined by the loss, gave—

Silica	31·55	16·39
Alumina	0·53	0·25
Oxide of copper	33·14	6·67
Magnesia	3·15	0·60
Water	23·03	20·47
Phosphoric acid	10·22	5·73

I believe it is still too soon to attempt to determine, before the analysis has been repeated, the mode in which these substances are combined with each other; but we see nevertheless that Demidovite contains silicates and phosphates of copper, with water and phosphates of alumina and magnesia, but the latter in very minute proportions*. It must also be remarked, that one-third of the water escapes at a lower temperature than the other two-thirds.

Demidovite, like Wolkhonskite, Hisingerite, and several other minerals, is perfectly amorphous, and is no doubt still being produced. At Taguil, as in the whole of the Ural, a geological fact occurs to which sufficient attention has not been paid,—this is the great depth to which the rocks are either decomposed, or at least cleft into larger or smaller fragments. Even at the depth of more than 70 sagènes†, which is reached by the copper mine of Taguil, the rocks are almost all destroyed, except the limestone by which the mine is surrounded. In 1848 I saw a large isolated block of malachite laid bare at a depth of 45 sagènes; as might be supposed, all the mammillae were turned downwards, and the outer surface had so little consistency and was so friable, that it was detached by the finger; sometimes the specimens of malachite present in their interior a fragment of native copper, surrounded by red copper and malachite, sometimes mixed with Demidovite, Taguilite and phosphorocalcite. In the upper parts of the mines few metallic sulphurets are met with, but the quantity of these increases gradually in proportion as the bottom is approached.—*From the Bulletin des Naturalistes de Moscou.*

ON THE BEHAVIOUR OF THE NATIVE METALLIC SULPHURETS TOWARDS MURIATIC ACID UNDER THE INFLUENCE OF GALVANISM. BY M. VON KOBELL.

When copper pyrites is moistened with muriatic acid (1 vol. of concentrated acid to 1 vol. of water), no change takes place in the

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* If we take no notice of the phosphates of alumina and magnesia, we may deduce the formula $5CuO\ SiO^3 + CuO\ PO^5 + 18HO$, which requires—

$5SiO^3$	32·89
$6CuO$	33·88
PO^5	10·16
HO	23·06
	99·99

† The sagène = 7 English feet.

pyrites; but when the moistened place is touched with zinc, sulphuretted hydrogen gas is evolved, and the pyrites acquires a brownish colour. When iron is employed in place of zinc, no action appears to take place; nevertheless this is remarkably shown when fine powder of copper pyrites is mixed with fine powder of iron and treated with muriatic acid. Sulphuretted hydrogen gas is evolved in abundance; and if 2 parts of iron be taken to 1 part of pyrites, the latter is easily decomposed without the aid of heat, and the copper is precipitated, whilst in the absence of iron it is but slowly attacked by muriatic acid even when boiled.

Other sulphurets behave in the same way as copper pyrites, and this comportment may therefore be made use of in ascertaining the presence of sulphur in them. For this purpose the author employs a cylindrical glass, about 2½ inches in height and 1 inch in diameter, puts into it equal volumes of the specimen and iron powder (of each as much as can be taken up on the point of a knife), and pours over the mixture dilute muriatic acid to a depth of some lines. He then closes the glass with a suitable cork, which holds a strip of paper soaked in acetate of lead and then dried, in such a way that it passes across the whole width of the cork, and projects on each side when the glass is closed. With the following ores the reaction for sulphur makes its appearance within one minute, the paper becoming yellow, brownish, or gray.

Antimonite.
Pyrostibite.
Argentite.
Stephanite.
Proustite.
Pyrargyrite.
Polybasite.
Aftonite.
Chalkosine.
Covelline.
Tetraedrite.
Tennantite.
Polytelite.
Spaniolite.
Chalcopyrite.
Bornite.
Stannine.
Bismuthine.
Galenite.
Geokronite.
Bournonite, colour of paper pale yellow.
Boulangerite.
Plumosite.
Dufrenoysite.
Jamesonite.
Plagionite.
Zinkenite.
Kobellite.
Saynite.
Gersdorffite (as also amoibite).
Cobaltine, colour pale yellow.
Pyrites, marcasite, lonchidite, kyrosite.
Arsenopyrites.
Hauerite.
Sphalerite.
Cinnabar.
Linneite.
Nagyagite.
Tetradymite, colour pale yellow.

All these compounds, and of course also pyrrhotine (and sulphuret of iron and nickel), alabandine and Berthierite, which evolve sulphuretted hydrogen, even by themselves, when treated with muriatic acid, show the above-mentioned coloration of the lead-paper within one minute. On the other hand, realgar, orpiment, and molybdenite give no reaction. Sulphur itself, however, reacts in the way above mentioned. By this means very similar minerals may be distinguished at once, for example, Clausthalite and Galenite, for seleniuret of lead gives no reaction, chloanthite and arsenopyrites, &c.

Iron powder is a carburet of iron (known in the shops as *Ferrum alcoholisatum*); of course it must be free from sulphur when employed in these experiments.—*Gelehrte Anzeigen der königl. bayer. Akad. der Wiss.*, and *Journ. für Prakt. Chemie*, lxxi. p. 146.

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XLVII. *Experimental Relations of Gold (and other Metals) to Light.—The Bakerian Lecture. By* MICHAEL FARADAY, *Esq., D.C.L., F.R.S., Fullerian Prof. Chem. Royal Institution, &c.*[*]

THAT wonderful production of the human mind, the undulatory theory of light, with the phænomena for which it strives to account, seems to me, who am only an experimentalist, to stand midway between what we may conceive to be the coarser mechanical actions of matter, with their explanatory philosophy, and that other branch which includes, or should include, the physical idea of forces acting at a distance; and admitting for the time the existence of the æther, I have often struggled to perceive how far that medium might account for or mingle in with such actions, generally; and to what extent experimental trials might be devised which, with their results and consequences, might contradict, confirm, enlarge or modify the idea we form of it, always with the hope that the corrected or instructed idea would approach more and more to the truth of nature, and in the fulness of time coincide with it.

The phænomena of light itself are, however, the best and closest tests at present of the undulatory theory; and if that theory is hereafter to extend to and include other actions, the most effectual means of enabling it to do so will be to render its application to its own special phænomena clear and sufficient. At present the most instructed persons are, I suppose, very far from perceiving the full and close coincidence between all the facts of light and the physical account of them which the theory

[*] From the Philosophical Transactions, Part I. for 1857; having been received by the Royal Society Nov. 15, 1856, and read Feb. 5, 1857.

supplies. If perfect, the theory would be able to give a reason for every physical affection of light; whilst it does not do so, the affections are in turn fitted to develope the theory, to extend and enlarge it if true, or if in error to correct it or replace it by a better. Hence my plea for the possible utility of experiments and considerations such as those I am about to advance.

Light has a relation to the matter which it meets with in its course, and is affected by it, being reflected, deflected, transmitted, refracted, absorbed, &c. by particles very minute in their dimensions. The theory supposes the light to consist of undulations, which, though they are in one sense continually progressive, are at the same time, as regards the particles of the æther, moving to and fro transversely. The number of progressive alternations or waves in an inch is considered as known, being from 37,600 to 59,880, and the number which passes to the eye in a second of time is known also, being from 458 to 727 billions; but the extent of the lateral excursion of the particles of the æther, either separately or conjointly, is not known, though both it and the velocity are probably very small compared to the extent of the wave and the velocity of its propagation. Colour is identified with the number of waves. Whether reflexion, refraction, &c. have any relation to the extent of the lateral vibration, or whether they are dependent in part upon some physical action of the medium unknown to and unsuspected by us, are points which I understand to be as yet undetermined.

Conceiving it very possible that some experimental evidence of value might result from the introduction into a ray of separate particles having great power of action on light, the particles being at the same time very small as compared to the wave-lengths, I sought amongst the metals for such. Gold seemed especially fitted for experiments of this nature, because of its comparative opacity amongst bodies, and yet possession of a real transparency; because of its development of colour both in the reflected and transmitted ray; because of the state of tenuity and division which it permitted with the preservation of its integrity as a metallic body; because of its supposed simplicity of character; and because known phænomena appeared to indicate that a mere variation in the size of its particles gave rise to a variety of resultant colours. Besides, the waves of light are so large compared to the dimensions of the particles of gold which in various conditions can be subjected to a ray, that it seemed probable the particles might come into effective relations to the much smaller vibrations of the æther particles; in which case, if reflexion, refraction, absorption, &c. depended upon such relations, there was reason to expect that these functions would change sensibly by the substitution of different-sized particles of this metal for each other. At one time I hoped that I had altered one coloured ray into another by means of gold, which would have been equivalent to a change in the number of undulations; and though I have not confirmed that result as yet, still those I have obtained seem to me to present a useful experimental entrance into certain physical investigations respecting the nature and action of a ray of light. I do not pretend that they are of great value in their present state, but they are very suggestive, and they may save much trouble to any experimentalists inclined to pursue and extend this line of investigation.

Gold-leaf—effect of heat, pressure, &c.

Beaten gold-leaf is known in films estimated at the $\frac{1}{282000}$th of an inch in thickness; they are translucent, transmitting green light, reflecting yellow, and absorbing a portion. These leaves consist of an alloy in the proportions of 12 silver and 6 copper to 462 of pure gold. 2000 leaves $3\frac{3}{8}$ths of an inch square are estimated to weigh 384 grains. Such gold-leaf is no doubt full of holes, but having, in conjunction with Mr. W. De la Rue, examined it in the microscope with very high powers (up to 700 linear), we are satisfied that it is truly transparent where the gold is continuous, and that the light transmitted is green. By the use of the balance Mr. De la Rue found that the leaf employed was on the average $\frac{1}{278000}$th of an inch thick. Employing polarized light and an arrangement of sulphate of lime plates, it was found that other rays than the green could be transmitted by the gold-leaf. The yellow rays appeared to be those which were first stopped or thrown back. Latterly I have obtained some pure gold-leaf beaten by Marshall, of which 2000 leaves weighed 408 grains, or 0·2 of a grain per leaf; its reflected colour is orange-yellow, and its transmitted colour a warm green. Gold alloy containing 25 per cent. of silver produces pale gold-leaf, which transmits a blue purple light, and extinguishes much more than the ordinary gold-leaf.

So a leaf of beaten gold occupies in average thickness no more than from $\frac{1}{5}$th to $\frac{1}{8}$th part of a single wave of light. By chemical means, the film may be attenuated to such a degree as to transmit a ray so luminous as to approach to white, and that in parts which have every appearance of being continuous in the microscope, when viewed with a power of 700. For this purpose it may be laid upon a solution of chlorine, or, better still, of the cyanide of potassium*. If a clean plate of glass be breathed upon and then brought carefully upon a leaf of gold, the latter will adhere to it; if distilled water be immediately applied at the

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* The chlorine leaves a film of chloride of silver behind, the cyanide leaves only metal.

edge of the leaf, it will pass between the glass and gold, and the latter will be perfectly stretched; if the water be then drained out, the gold-leaf will be left well extended, smooth, and adhering to the glass. If, after the water is poured off, a weak solution of cyanide be introduced beneath the gold, the latter will gradually become thinner and thinner; but at any moment the process may be stopped, the cyanide washed away by water, and the attenuated gold film left on the glass. If towards the end a washing be made with alcohol, and then with alcohol containing a little varnish, the gold film will be left cemented to the glass*.

In this manner the leaf may be obtained so thin, that I think 50 or even 100 might be included in a single progressive undulation of light. But the character of the effect on light is not changed, the light transmitted is green, as before; and though that green tint is due to a condition of the gold induced by pressure, it as yet remains unchanged through all these varieties of thickness and of proportion to the progressive or the lateral undulation.

Gold-leaf, either fine or common, examined in the microscope, appears as a most irregular thing. It is everywhere closely mottled or striated, according as a part at the middle or the edge of a leaf is selected, minute portions which are close to other parts being four or five times as thick as the latter, if the proportion of light which passes through may be accepted as an indication. Yet this irregular plate does not cause any sensible distortion of an object seen through it, that object being the line of light reflected from a fine wire in the focus of a moderate microscope. Nor perhaps was any distortion due to consecutive convexities and concavities to be expected; for when the thicker parts of the leaf were examined, they seemed to be accumulated plications of the gold, the leaf appearing as a most irregular and crumpled object, with dark veins running across both the thicker and thinner parts, and from one to the other. Yet in the best microscope, and with the highest power, the leaf seemed to be continuous, the occurrence of the smallest sensible hole making that continuity at other parts apparent, and every part possessing its proper green colour. How such a film can act as a plate

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* Air-voltaic circles are formed in these cases, and the gold is dissolved almost exclusively under their influence. When one piece of gold-leaf was placed on the surface of a solution of cyanide of potassium, and another, moistened on both sides, was placed under the surface, both dissolved; but twelve minutes sufficed for the solution of the first, whilst above twelve hours were required for the submerged piece. In weaker solutions, and with silver also, the same results were obtained; from sixty to a hundred-fold as much time being required for the disappearance of the submerged metal as for that which, floating, was in contact both with the air and the solvent. An action of this kind has probably much to do with the *formation* of the films to be described hereafter.

on polarized light in the manner it does, is one of the queries suggested by the phænomena which requires solution.

When gold-leaf is laid upon glass and its temperature raised considerably without disturbance, either by the blowpipe or an ordinary Argand gas-burner, it seems to disappear, *i. e.* the lustre passes away, the light transmitted is abundant and nearly white, and the place appears of a pale brown colour. One would think that much of the metal was dissipated, but all is there, and if the heat has been very high (which is not necessary for the best results), the microscope shows it in minute globular portions. A comparatively low heat, however, and one unable to cause separation of the particles, is known to alter the molecular condition of gold, and the gold-beater finds important advantage in the annealing effect of a temperature that does not hurt the skins or leaves between which he beats the metal.

It might be supposed that the annealed metal, in contracting from the constrained and attenuated state produced by beating, drew up, leaving spaces through which white light could pass, and becoming itself almost insensible through the smallness of its quantity; and if gold-leaf unattached to glass be heated carefully with oil in a tube, it does shrink up considerably even before it loses its green colour, which finally happens. But if the gold-leaf laid upon glass plates by water only be carefully dried, then introduced into a bath of oil and raised to a temperature as high as the oil can bear for five or six hours, and then suffered to cool, the plates, when taken out and washed, first in camphine and then in alcohol, present specimens of gold which has lost its green colour, transmits far more light than before, and reflects less, whilst yet the film remains in form and other conditions apparently quite unchanged. Being now examined in the microscope, it presents exactly the forms and appearance of the original leaf, except in colour; the same irregularities appear, the same continuity, and if the destruction of the green colour has not been complete, it will be seen that it is the thicker folds and parts of the mottled mass that retain the original state longest.

This change does not depend upon the substance in contact with which the gold is heated*. If the leaf be laid upon mica, rock-crystal, silver or platinum, the same result occurs; the surrounding medium also may change, and be air, oil or carbonic acid, without causing alteration. Nor has the gold disappeared; a piece of leaf, altered in one part and not in another, was di-

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* The disappearance of gold-leaf as metal, when mingled with lime, alumina and other bodies, and then heated, has been already observed; and referred to oxidation (J. A. Buchner). See Gmelin's 'Chemistry,' vi. p. 206, "Purple oxide of gold."

vided into four equal parts, and the gold on each converted by chlorine gas into crystallized chloride of gold: the same amount was found in each division.

When the gold-leaf is laid by water on plates of rock-crystal, and then gradually heated in a muffle not higher than is necessary, an excellent result is obtained. The gold is then of a uniform pale brown colour by common observation, but when examined by a lens and an oblique light, all the mottle of the original leaf appears. It adheres but very slightly to the rock-crystal, and yet can bear the application of the pressure now to be described.

When gold rendered colourless by annealing is subjected to pressure, it again becomes of a green colour. I find a convex surface of agate or rock-crystal having a radius of from a quarter to half an inch very good for this purpose, the metal having very little tendency to adhere to this substance. The greening is necessarily very imperfect, and if examined by a lens it will be evident that the thinner parts of the film are rarely reached by the pressure, it being taken off by the thicker corrugations; but when reached they acquire a good green colour, and the effect is abundantly shown in the thicker parts. At the same time that the green colour is thus reproduced, the quantity of light transmitted is diminished, and the quantity of light reflected is increased. When the gold-leaf has been heated on glass in a muffle, it generally adheres so well as to bear streaking with the convex rock-crystal, and then the production of the reflecting surface and the green transmission is very striking. In other forms of gold film, to be described hereafter, the greening effect of pressure (which is general to gold) is still more strikingly manifested, and can be produced with the touch of a card or a finger. In these cases, and even with gold-leaf, the green colour reproduced can be again taken away by heat to appear again by renewed pressure.

As to the essential cause of this change of colour, more investigation is required to decide what that may be. As already mentioned, it might be thought that the gold-leaf had run up into separate particles. If it were so, the change of colour by division is not the less remarkable, and the case would fall into those brought together under the head of gold fluids. On the whole, I incline to this opinion; but the appearance in the microscope, the occurrence of thin films of gold acting altogether like plates, and yet not transmitting a green ray until they are pressed, and their action on a polarized ray of light, throw doubts in the way of such a conclusion.

It may be thought that the beating has conferred a uniform strained condition upon the gold, a difference in quality in one direction which annealing takes away; but when the gold is examined by polarized light, there is no evidence as yet of such a condition, for the green and the colourless gold present like results; and there is a little difficulty in admitting that such an irregular corrugated film as gold-leaf appears to be, can possess any general compression in one direction only, especially when it is considered that it is beaten amongst tissues softer than itself, and made up with it into considerable masses. The greening effect of pressure occurs with the deposited particles of electric discharges, and here it appears either amongst the larger particles near the line of the discharge, or amongst the far finer ones at a considerable distance. Such results do not suggest a dependence upon either the size of the particles or their quantity, but rather upon the relative dimensions of the particles in the direction of the ray and transverse to that direction. One may imagine that spherical or other particles, which, being disposed in a plane, transmit ruby rays or violet rays, acquire the power, when they are flattened, of transmitting green rays, and such a thought sends the mind at once from the wave of light to the direction and extent of the vibrations of the æther. For it does not seem likely that pressure can produce its peculiar result by affecting the relation of the dimension of the particle to the length-dimension of a progressive undulation of light, the latter being so very much greater than the former; but the relation to the dimension of the direct or lateral vibration of the particles of the æther may be greatly affected, that being probably very small and much nearer to, if not even less than, the size of the particles of gold.

Silver-leaf, as usually obtained by beating, is so opake, as perfectly to exclude the light of the sun. When this is laid by water on plates of rock-crystal and heated in a muffle, it begins to change, at a temperature lower than that required for gold, and becomes very translucent, losing at the same time its reflective power: it looks very like the film of chloride produced when a leaf of silver is placed in chlorine gas. When examined by a lens or an ordinary microscope, the leaf seems to be as continuous as in its original state; the finest hole, or the finest line drawn by a needle-point, appears only to prove the continuity of the metallic film up to the very edges of these real apertures. When pressure is applied to this translucent film, the compressed metal becomes either opake or of a very dark purple colour, and resumes its high reflective power. If a higher heat than that necessary for this first change be applied, then the leaf, viewed in the microscope, assumes a mottled appearance, as if a retraction into separate parts had occurred. At a still higher temperature this effect is increased; but the heat, whether applied in the muffle or by a blowpipe, which is necessary to fuse the metal and make it run together in globules, is very much higher than that which causes the first change of the silver: the latter is, in fact, below such a red heat as is just visible in the dark. Whatever the degree of heat applied, the metal remains as metallic silver during the whole time. When many silver leaves were laid loosely one upon another, rolled up into a loose coil, introduced into a glass tube, and the whole placed in a muffle and heated carefully for three or four hours to so low a degree that the glass tube had not been softened or deformed, it was found that the silver-leaf had sunk together a little and shaped itself in some degree upon the glass, touching by points here and there, but not adhering to it. But it was changed, so that the light of a candle could be seen through forty thicknesses: it had not run together, though it adhered where one part touched another. It did not look like metal, unless one thought of it as divided dead metal, and it even appeared too unsubstantial and translucent for that; but when pressed together, it clung and adhered like clean silver, and resumed all its metallic characters.

When the silver is much heated, there is no doubt that the leaf runs up into particles more or less separate. But the question still remains as to the first effect of heat, whether it merely causes a retraction of the particles, or really changes the optical and physical nature of the metal from the beaten or pressed state to another from which pressure can return it back again to its more splendid condition. It seems just possible that the leaf may consist of an infinity of parts resulting from replications, foldings and scales, all laid parallel by the beating which has produced them, and that the first action of heat is to cause these to open out from each other; but that supposition leaves many of the facts either imperfectly explained or untouched. The Arts do not seem to furnish any process which can instruct us as to this condition, for all the operations of polishing, burnishing, &c. applied to gold, silver and other metals, are just as much fitted to produce the required state under one view as under the other.

To return to gold: it is clear that that metal, reduced to small dimensions by mere mechanical means, can appear of two colours by transmitted light, whatever the cause of the difference may be. The occurrence of these two states may prepare one's mind for the other differences with respect to colour, and the action of the metallic particles on light, which have yet to be described.

Many leaves of gold, when examined by a lens and transmitted light, present the appearance of red parts; these parts are small, and often in curved lines, as if a fine hair had been there during the beating. At first I thought the gold was absolutely red in these parts, but am inclined to believe that in the greatest number of cases the tint is subjective, being the result of the contrast between the white light transmitted through bruised parts, and the green light of the neighbouring continuous parts. Nevertheless, some of these places, when seen in the microscope, appeared to have a red colour of their own, that is, to transmit a true red light. As I believe that gold in a certain state of division can transmit a ruby light, I am not prepared to say that gold-leaf may not, in some cases, where the effect of pressure in a particular direction has been removed, do the same.

Many of the prepared films of gold were so thin as to have their reflective power considerably reduced, and that in parts which, under the microscope and in other ways, appeared to be quite continuous: this agrees with the transmission of all the rays already mentioned, but it seems to imply that a certain thickness is necessary for full reflexion; therefore, that more than one particle in depth is concerned in the act, and that the division of gold into separate particles by processes to be described, may bring them within or under the degree necessary for ordinary reflexion.

As particles of pure gold will be found hereafter to adhere by contact, so the process of beating may be considered as one which tends to weld gold together in all directions, and especially in that transverse to the blow,—a point favourable to continuity in that direction, both as it tends to preserve and even reproduce it.

If a polarized ray be received on an analyser so that no light passes, and a plate of annealed glass, either thick or thin, be interposed vertically across the ray, no difference is observed on looking through the analyser,—the image of the source of light does not appear; but if the plate be inclined until it makes an angle of from 30° to 45°, or thereabouts, with the ray, the light appears, provided the inclination of the glass is not in the plane of polarization or at right angles to it, the effect being a maximum if the inclination be in a plane making an angle of 45° with that of polarization. This effect, which is common to all uncrystallized transparent bodies, is also produced by leaf-gold, and is one of the best proofs of the true transparency of this metal according to the ordinary meaning of the term. In like manner, if a leaf of gold be held obliquely across an ordinary ray of light, it partly polarizes it, as Mr. De la Rue first pointed out to me. Here again the condition of true transparency is established, for it acts like a plate of glass or water or air. But the relations of gold and the metals in different conditions to polarized light shall be given altogether at the close of this paper.

Deflagrations of Gold (and other metals)—heat—pressure, &c.

Gold wire deflagrated by explosions of a Leyden battery produces a divided condition, very different to that presented by gold leaves. Here the metal is separated into particles, and no pressure in any direction, either regular or irregular, has been exerted upon them in the act of division. When the deflagrations have been made near surfaces of glass, rock-crystal, topaz, fluor-spar, card-board, &c., the particles as they are caught are kept separate from each other and in place, and generally those which remain in the line of the discharge have been heated by the passage of the electricity. The deposits consist of particles of various sizes, those at the outer parts of the result being too small to be recognized by the highest powers of the microscope. Beside making these deflagrations over different substances, as described above, I made them in different atmospheres, namely, in oxygen and hydrogen, to compare with air; but the general effects, the colours produced, and the order of the colours, were precisely the same in all the cases. These deposits were insoluble in nitric acid and in hydrochloric acid, but in the mixed acids or in chlorine solution were soluble, exactly in the manner of gold. There is no reason to doubt that they consisted of metallic gold in a state of extreme division.

Now as to the effects on light, *i. e.* as to the coloured rays reflected or transmitted by these deposited particles, and first, of those in the line of the discharge where the wire had been. Here the mica was found abraded much, the glass less, and the rock-crystal and topaz least. Where abraded, the gold adhered; in all the other parts it could be removed with the slightest touch. The gold deposited in this central place was metallic and golden by reflected light, and of a fine ruby colour by transmitted light. On each side of this line the deposit had a dark colour, but when particularly examined gave a strong golden metallic reflexion, and by transmission a fine violet colour, partaking of green and ruby in different parts, and sometimes passing altogether into green. Beyond this, on each side, where the tints became paler and where the particles appeared to be finer, the transmitted tint became ruby or violet-ruby, and this tint was especially seen when the deposit was caught on a card. As to the reflected light, even at these faintest parts it is golden and metallic. This is easily observed by wiping off a sharp line across the deposit on glass in the very faintest part, and then causing the sun's rays collected in the focus of a small lens to travel to and fro across that edge; the presence of the metallic gold on the unwiped part is at once evident by the high illumination produced there. It is evident that all the colours described are produced by one and the same substance, namely gold, the only apparent difference being the state of division and different degrees of the application of heat. The thickest parts of these deposits are so discontinuous, that they cannot conduct the electricity of a battery of two or three pairs of plates, *i. e.* of a battery unable to produce a spark among the particles.

When any of these deposits of divided gold are heated to dull redness, a remarkable change occurs. The portions which before were violet, blue, or green by transmitted light, now change to a ruby, still preserving their metallic reflecting power, and this ruby is in character quite like that which is presented in the arts by glass tinged by gold. This change is often far better shown in the more distant and thinner parts of the deposit, than in those nearest to the line of discharge, for near the latter place, where the deposit is most abundant, the metal appears to run up into globules, as with gold-leaf, and so disappears as a film. I believe that the ruby character of the deposit *in* the line of discharge, is caused by the same action of heat produced at the moment by the electricity passing there. In the distant parts, the deposit, rubified by after-heat, is not imbedded or fused into the glass, rock-crystal, topaz, &c., but is easily removed by a touch of the finger, though in parts of the glass plate which are made very hot, it will adhere.

If the agate pressure before spoken of, in respect of gold-leaf, be applied to ruby parts not too dense, places will easily be found where this pressure increases the reflective power considerably, and where at the same time it converts the transmitted ray from ruby to green; making the gold, as I believe, then accord in condition with beaten gold-leaf. On the other hand, if parts of the *unheated* electric deposit, where they are purple-gray, and so thin as to be scarcely visible without care, be in like manner pressed, they will acquire the reflective power, and then transmit the green ray; and I think I am justified by my experiments in stating, that fine gold particles, so loosely deposited that they will wipe off by a light touch of the finger, and possessing one conjoint structure, can in one state transmit light of a *blue-gray* colour, or can by heat be made to transmit light of a *ruby* colour, or can by pressure from either of the former states be made to transmit light of a *green* colour; all these changes being due to modifications of the gold, as gold, and independent of the presence of the bodies upon which for the time the gold is supported; for I ought to have said, if I have not said so, that these changes happen with all the deposits upon glass, mica, rock-crystal, and topaz, and whatever the atmosphere in which they were formed.

When gold is deflagrated by the voltaic battery near glass (I have employed sovereigns laid on glass for the terminals), a deposit of metallic gold in fine particles is produced. The densest parts have a dark slate-violet colour passing into violet and ruby-violet in the outer thinner portions; a ruby tint is presented occasionally where the heat of the discharge has acted on the deposit. The deposited gold was easily removed by wiping, except actually at the spot where the discharge had passed. When these deposits were heated to dull redness they changed and acquired a ruby tint, which was very fine at the outer and thinner parts. The portions nearer the place of discharge presented ruby-violet and then violet tints, suggesting that accumulation of that which presented a fine ruby tint would, by stopping more and more light, transmit a ruby-violet or violet ray only. Pressure with the agate surface had a like effect as before, both with the heated and the unheated portions, *i. e.* with the violet and the ruby particles; but the effect was not altogether so good, and the tint of the transmitted ray was rather a green-violet than a pure green. Still the difference produced by the pressure was very remarkable. The unheated particles at the surface, away from the glass, presented by reflexion almost a black; being heated, they became much more golden and metallic in appearance.

I prepared an apparatus by which many of the common metals could be deflagrated in hydrogen by the Leyden battery, and being caught upon glass plates could be examined as to reflexion, transmission, colour, &c. whilst in the hydrogen and in the metallic, yet divided state. The following are briefly the results; which should be considered in connexion with those obtained by employing polarized light. *Copper*: a fine deposit presenting by reflexion a purplish-red metallic lustre, and by transmission a green colour, dark in the thicker parts, but always green; agate pressure increased the reflexion where it was not bright, and a little diminished the transmission, rendering the green deeper, but not changing its character as in the case of gold. *Tin* gave a beautiful bright white reflexion, and by transmission various shades of light and dark brown; agate pressure diminished the transmission and increased the reflexion in places before dull or dead; the effect appeared to be due simply to the lateral expansion of the separate particles filling up the space. *Iron* presented a fine steel-gray, or slate metallic reflexion and a dark brown transmission; agate pressure gave the same effect as with tin, but no change of colour. *Lead*: a bright white reflexion, the transmission a dark smoky brown; agate pressure appeared to change this brown towards blue. *Zinc*: the reflexion bright white and metallic; the transmission a dark smoky colour with portions of blue-gray, brown-gray and pale brown; agate pressure tended to change the blue-gray to brown. *Palladium*: the reflexion fine metallic and dark gray; the transmitted light, where most abundant, sepia-brown; agate pressure converted the tint in the thinner places from brown towards blue-gray. *Platinum*: the reflexion white, bright and metallic; the transmission brown or warm gray with no other colours; agate pressure increased the reflexion and diminished the transmission as with tin. *Aluminium*: the reflexion metallic and white, very beautiful; the transmitted light was dark brown, bluish-brown, and occasionally in the thinner parts orange; agate pressure caused but little change.

Films of Gold (and other metals) by Phosphorus, Hydrogen, &c.—effect of heat—pressure.

The reduction of gold from its solution by phosphorus is well known. If fifteen or twenty drops of a strong solution of gold, equal to about 1½ grain of metal, be added to two or three pints of water, contained in a large capsule or dish, if four or five minute particles of phosphorus be scattered over the surface, and the whole be covered and left in quietness for twenty-four or thirty-six hours, then the surface will be found covered with a pellicle of gold, thicker at the parts near the pieces of phosphorus, and possessing there the full metallic golden reflective power of the metal; but passing by gradation into parts, further from the phosphorus, where the film will be scarcely sensible except upon close inspection. If plates of glass be introduced into the fluid under the pellicle, and raised gradually, the pellicle will be raised on them; it may then be deposited on the surface of pure distilled water to wash it; may be raised again on the glass; the water allowed to drain away, and the whole suffered to dry. In this way the pellicle remains attached to the glass, and is in a very convenient condition for preservation and examination.

If phosphorus be dissolved in two or three times its bulk of sulphide of carbon, and a few drops of the fluid be placed on the bottom of a dry basin, vapour of the phosphorus will soon rise up and bring the atmosphere in the basin to a reducing state. If a plate of glass large enough to cover the basin have six or eight drops of a strong neutral solution of chloride of gold placed on it, and this be spread about by a glass stirrer, so as to form a flowing layer on the surface, the glass may then be inverted and placed over the dish. So arranged the gold solution will keep its place, but will have a film of metal reduced on its under surface. The plate being taken off after twenty, thirty, or forty minutes, and turned with the gold solution upwards, may then gradually be depressed in an inclined position into a large basin of pure water, one edge entering first, and the gold film will be left floating. After sufficient washing it may be taken up in portions on smaller plates of glass, dried, and kept for use. Mr. Warren De la Rue taught me how to make and deal with these films: they may by attention be obtained very uniform, of very different degrees of thickness, from almost perfect transparency to complete opacity, and by successive application of the same collecting glass plate may be superposed with great facility.

These films may be examined either on the water or on the glass. When thick, their reflective power is as a gold plate, full and metallic; as they are thinner they lose reflective power, and they may be obtained so thin as to present no metallic appearance, all the coloured rays of light then passing freely through them. As to the transmitted light, the thinner films generally present one kind of colour; it appears as a feeble gray-violet, which increases in character as the film becomes thicker and sometimes approaches a violet; a greenish-violet also appears; and the likeness of the gray-violet tint of these films to the stains produced by a solution of gold on the skin or other organic reducing substance, or the stain produced on common pottery, cannot be mistaken. Superposition of several gray-violet films does not produce a green tint, but only a diminution of light without change of colour. In those specimens made by particles of phosphorus floating on the solution of gold, very fine green tints occur at the thicker and golden parts of the film. The colour of the gold here may depend in some degree on the *manner* in which these films are formed: the thicker parts are not produced altogether by the successive addition of reduced gold from the portion of fluid immediately beneath them. When a particle of phosphorus is placed on pure water, it immediately throws out a film which appears to cover the whole of the surface; in a little while the film thickens around the particle and is easily distinguished by its high reflective power. It is this film which reduces the gold in solution, being itself consumed in the action; the result is a continued extension from the phosphorus outwards, which, after it has covered the solution with a thin film of gold, continues to cause a compression of the parts around the phosphorus and an accumulation there, rendering the gold at a distance of half an inch from the phosphorus so thick, that it is brilliant by reflexion and nearly opake by transmission, whilst near to the phosphorus the forming film is so thin as to be observed only on careful examination, and is still travelling outwards and compressing the surrounding parts more and more. The phosphorus is very slowly consumed; a particle not weighing $\frac{1}{100}$dth of a grain will remain for four or five days on the surface of water before it disappears.

Though the particles of these films adhere together strongly, as may be seen by their stiffness on water, still the films cannot be considered as continuous. If they were, those made by vapour of phosphorus could not thicken during their formation, neither could they dry on glass in the short time found sufficient for that purpose. Experimentally also, I find that vapours and gases can pass through them. Very thin films without folds did not sensibly conduct the electricity of a single pair of Grove's plates; thicker films did conduct; yet with these proofs that these films could not be considered as continuous, they acted as thin plates upon light, producing the concentric rings of colours round the phosphorus at their first formation, though their thickness then could scarcely be the $\frac{1}{100}$dth, perhaps not the $\frac{1}{500}$dth of a wave undulation of light. Platinum, palladium, and rhodium produced films, showing these concentric rings very well.

Many of these films of gold, both thick and thin, which being of a gray colour originally, were laid on a solution of cyanide of potassium to dissolve slowly, changed colour as they dissolved and became green; if change occurred, it was always towards green. On the other hand, when laid on a solution of chlorine, the change during solution was towards an amethyst or ruby tint. The films were not acted upon by pure nitric, or hydrochloric, or sulphuric acids, or solutions of potassa or brine. They dissolved in damp chlorine gas, not changing in colour during the solution. I believe them to consist of pure gold.

When these gold films were heated to dull redness they changed. The reflexion, though not much altered, was a little more metallic and golden than before; more light was transmitted after the heating and the colour had altered from greenish to violet, or from gray-green to ruby or amethyst; and now two or three films superposed often gave a very ruby colour. This action is like that of heat on the particles separated by electric explosions. If not overheated, the particles were not fused to the glass, but could be easily wiped off. Whenever these heated particles were pressed by the convex agate, they changed in character and transmitted green light. Heat took away this character of the gold, the heat of boiling oil, if continued, being sufficient; but on applying pressure at the same spot, the power of transmitting green light was restored to the particles. In many cases where the gold adhered sufficiently to the glass to bear a light drawing touch from the finger or a card, such touch altered the light transmitted from amethystine to green; so small is the pressure required when the particles are most favourably disposed.

Heating injured the conducting power for electricity of these films, no doubt by retraction of the particles, though there was no such evident appearance in these cases, as in the unattached gold-leaf of the particles running up into globules.

A given film, examined very carefully in the microscope by transmitted lamp-light, with an aperture of 90° and power of 700 linear, presented the following appearances. The unheated part was of a gray colour, and by careful observation was seen to be slightly granular. By very close observation this gray part was often resolvable into a mixture of green and amethystine striæ, it being the compound effect of these which in general produces the gray sensation in the eye. When a part of such a film was heated, the transmitted colour was changed from gray to purple, as before described, and the part thus heated was evidently more granular than before. This difference was confirmed in other cases. That the heated part should thus run up, seems to show that many of the particles must have been touching though they did not form a continuous film; and on the other hand, the difference between the effect here and with unattached gold-leaf, shows that the degree of continuity as a film must be very small. When these heated films were greened by agate pressure, or the drawing pressure of a card, the green parts remain granulated, apparently in the same degree as when purple. The green was not subjective or an effect of interference, but a positive colour belonging to the gold in that condition. Every touch of the agate was beautifully distinct as a written mark. The parts thus greened and the purple parts appeared to transmit about the same amount of light. Though the film appeared granulated, no impression was made upon the mind that the individual particles of which the film consisted were in any degree rendered sensible to the eye.

The unheated gold films when pressed by agate often indicated an improved reflective power, and the light transmitted was also modified; generally it was less, and occasionally tended towards a green tint; but the effect of pressure was by no means so evident as in particles which had been heated.

Films of some other metals were reduced by phosphorus in like manner, the results in all these cases being of course much affected by the strength of the solution and the time of action; they are briefly as follows. *Palladium*: a weak solution of the chloride gave fine films, apparently very continuous and stiff; the reflexion was strong and metallic, of a dark gray colour; the transmission presented every shade of Indian ink. *Platinum* chloride gave traces of a film excessively thin, and very slow in formation. *Rhodium* chloride in three or four hours gave a beautiful film of metal in concentric rings, varying in reflecting and transmitting power over light and also in colour; those which reflected well, transmitted little light; and those which transmitted, reflected little light; one might have thought there was no metal in some of the rings between other rings that reflected brilliantly, but the metal was there of transmitting thickness: the transmitting colour of rhodium varied from brown to blue. *Silver*: a solution of the nitrate gave films showing the concentric rings; the light transmitted by the thinner parts was of a warm brown, or sepia tint; the film becomes very loose and mossy in the thicker parts and is wanting in adherence; pressure brings out the full metallic lustre in every part, and in the thin places converts the colour from brown to blue, being in that respect like the result with pale gold-leaf, in which the silver present dominates over the colour of the gold. I do not think there is phosphorus combined with this silver; I did not find any, and considering the surface action on metals which float as films between air and water, it seems improbable that it should be there.

Hydrogen was employed to reduce some of the metals, their solutions being placed in an atmosphere of the gas. The action differed considerably from that of phosphorus, as might be expected. *Gold* produced a very thin film, too thin to be washed; it had a faint metallic reflexion, and transmitted a slate-blue colour like the former films. *Platinum* chloride was acted on at once; minute spots appeared here and there on the surface; these enlarged, became rough and corrugated at the middle, though brilliant at the edges, and at last formed an irregular coat over the fluid; at the part where the film was flat and brilliant, it resembled that produced by the electric explosion, and by transmission gave a dark gray colour. *Iridium* required much time, and formed a crust from centres like the platinum. *Palladium* gave an instant action, but most of the reduced metal sunk in a finely divided state; a film may be obtained, but it has very little adhesion. *Rhodium* is reduced, but the film consists of floating particles, having so little adhesion that it cannot be gathered up. *Silver* is reduced, but the film is very thin and has no tenacity.

A copper film of very beautiful character may be obtained as follows in all varieties of thickness. Let a little oxide of copper be dissolved in olive-oil to form a bath, and having immersed some plates of glass, for which purpose microscope plates 3 × 1 inches are very convenient, let the whole be heated up to the decomposing temperature of the oil; being left to cool, and the plates then drained and washed successively in camphine and alcohol, they will be found covered with a film of copper, having the proper metallic lustre and colour by reflexion; and by transmission, presenting a green colour, which, though generally inclining to olive, is in the thinner films often more beautiful than the green presented by pressed gold.

[To be continued.]

XLVIII. *On the Colour of Salts in Solution, each constituent of which is coloured.* *By* J. H. Gladstone, *Ph.D., F.R.S. &c.**

[With a Plate.]

FROM the general rule, that *a particular base or acid has the same effect on the rays of light, with whatever it may be combined in aqueous solution,* it may be inferred that *when two bodies combine, each of which has a different influence on the rays of light, a solution of the salt itself will transmit only those rays which are not absorbed by either,* or in other words, those which are transmitted by both. Thus if a red acid and a blue base combine, the resulting salt will certainly not be purple, but it may present the colour of some ray intermediate between red and blue—perhaps green.

This, indeed, was laid down by me as a general rule in a paper read before the Chemical Society, "On the Use of the Prism in Qualitative Analysis†;" but only one instance was given, namely chromate of copper. Since then I have examined several salts of the character above described, and many haloid salts, which have led to some unexpected, and I think suggestive results.

The method employed was that described in the paper already referred to. It is briefly as follows:—The solution to be examined is placed in a hollow wedge of glass, which is interposed between the eye of the spectator and a narrow slit in the window-shutter, in such a manner that the thin line of light is seen traversing the different thicknesses of liquid. This line of light is then analysed by placing a good prism between the hollow wedge and the eye. In this way it is seen at once what rays are absorbed by increasing thicknesses of the solution. The diagrams in Plate II. give the appearances thus presented; the lowest portion representing the prismatic spectrum as seen through the thinnest possible stratum of liquid, the higher portions showing the gradual absorption of different rays. The diagrams are not coloured, but the fixed lines will indicate with far greater accuracy than colour would, the different parts of the spectrum transmitted. The least refracted, that is, the red ray, is always to the right hand; then follow of course the orange, yellow, green and blue, while the violet ray is on the extreme left‡.

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* Communicated by the Author; having been read at the Meeting of the British Association at Dublin, 1857.

† Quart. Journ. Chem. Soc. vol. x. p. 79.

‡ Since writing the above, my attention has been directed by the Abbé Moigno to a description contained in his *Répertoire d'Optique Moderne,* 3^{me} partie, p. 1258, of some experiments by J. Müller, in which he has represented the transmission of light through varying thicknesses of a

CHROMATES.

Chromic acid in solution, and its combinations with colourless bases, such as potash, cut off instantly the more refrangible half of the spectrum, admitting only the blue rays near F for a short distance; but they transmit the less refrangible half perfectly. See fig. 1. Plate II. The red bi-salts, as those of potash or baryta, absorb the green in the neighbourhood of F likewise, and a little red.

Chromates were prepared by saturating the acid with the hydrated oxide or carbonate of the base.

Chromate of Copper is a green salt. The spectrum presented by it is represented in fig. 5, where evidently the absorption of the red ray is due to the base, that of the blue and violet to the acid. Compare figs. 1 and 9.

Chromate of Nickel is of a yellowish-green, and presents nearly the same spectrum as the copper salt, as might be anticipated on comparing figs. 1 and 10.

Chromate of Ferric Oxide is orange passing into red as the depth increases. It transmits only the red, orange, and yellow rays, which a comparison of figs. 1 and 4 will show to be transmitted in common by both constituents.

Chromate of Uranium is yellow, like any other salt of that base; but its prismatic appearance is totally different, the chromic acid having cut off all the remarkable luminous bands beyond F. Compare figs. 1 and 2.

Chromate of Chromium is of a reddish-brown colour. The rays transmitted by it are represented in fig. 7, from which it will be seen that the whole of the blue or green maximum of an ordinary chromium salt (see fig. 6) is cut off by the acid; but an absorption of light between D and E does not occur to such an extent as might be expected from the presence of chromic oxide. From the chemical reactions of this body, however, I have some doubt about its right to be considered a salt.

PERMANGANATES.

Permanganate of potash gives the very characteristic spectrum represented in fig. 3.

Permanganate of Uranium gives a purple solution. Its pris-

liquid by diagrams similar to my own. These experiments are an important step in the history of photo-chemical research, and would have been noticed by me in my previous paper had I been aware of their existence. M. Müller's mode of representing the optical effects produced by the transmission of light through coloured bodies is an improvement on the original attempt of Sir John Herschel, and should give precisely the same figures as my method; yet he arrives only, after a series of observations in each case, at the same result, which I, by using the hollow wedge, render apparent to the eye at once. His observations agree closely with mine; but they are very few in number, and have not led him to any of the generalizations deduced in my previous or my present paper.

matic spectrum shows the alternate bands of light and darkness due to the permanganic acid; but I did not recognize those due to uranium in the blue space. This, however, is not to be wondered at, since the permanganates admit very little blue light, and the uranium bands never show themselves till the light has traversed a considerable amount of salt. See figs. 2 and 3.

CARBAZOTATES.

Carbazotic acid gives a prismatic spectrum almost identical with that of chromic acid, but the line of distinction between the transmitted and the absorbed portions is about midway between *b* and F.

Carbazotate of Copper is a yellow salt, but its spectrum differs from that of carbazotic acid by the gradual absorption of the red ray—the universal effect of copper in solution.

Haloid Salts.

Chlorine, bromine, and iodine are, as all know, highly-coloured bodies; and their solutions in water are also coloured. Yet the acids which result from their combinations with hydrogen are colourless; and their compounds with most metals, including all those of the alkalies and earths, are colourless likewise, whether in the solid or the dissolved condition. Yet, as will be seen from the following observations, the peculiar effect exerted on light by the dissolved halogen itself appears also in its compounds with gold, platinum, and palladium: it appears, too, in its compounds with copper, nickel, and analogous metals; but in these cases only when the solution is very strong, the chromatic effect of the halogen wholly disappearing as water is added.

BROMIDES.

Bromine-water is red, and gives the prismatic appearance of fig. 13. The light transmitted near F is very dull.

Terbromide of Gold is intensely red in solution, and gives a spectrum identical with that of bromine. On comparing fig. 15, it will be seen that a compound of gold with a colourless acid transmits all the rays transmissible by bromine-water. The gold salt figured is the nitrate.

Bibromide of Platinum is intensely red, and likewise gives a spectrum identical with that of bromine. Fig. 16 will show that the remark made above in reference to the gold salt, applies here likewise.

Bromide of Palladium and Potassium also gives the ordinary bromine spectrum.

Bromide of Copper gives a saturated solution of a deep green colour; it transmits the spectrum represented in fig. 17, which is precisely similar to that of bromine, except that the red ray is gradually cut off, as with other copper salts. On diluting this solution a complete change of colour ensues; it acquires the blue colour and the prismatic appearance of a compound of oxide of copper with a colourless acid, as represented in fig. 9.

Oxybromide of Iron is intensely red. It gives the same prismatic spectrum as bromine-water does.

CHLORIDES.

Chlorine gas absorbs the more refrangible rays; chlorine-water, though yellow, cannot be obtained strong enough, even at a temperature of nearly 0° C., to show any very decided effect on the spectrum; nothing, in fact, beyond a diminution in the intensity of the violet. Yet in some of its compounds its absorbent power is well marked.

Terchloride of Gold gives a yellow solution, and absorbs more blue than the yellowish-green nitrate does. Compare figs. 19 and 15. The acid hydrochlorate of this salt is indistinguishable from it by the prism, though to the unaided eye it appears of a somewhat purer yellow.

Bichloride of Platinum transmits scarcely any rays more refrangible than *b*, unless the stratum be very thin, when a little blue and violet pass. See fig. 20. The sulphate, fig. 16, admits more rays, especially in the neighbourhood of F.

Bichloride of Palladium is red: it transmits the red and orange rays freely, the yellow and green but partially, and the blue only where the stratum is very thin. See fig. 23.

Chloride of Copper in saturated aqueous solution is green: it shows the absorption of the red ray due to the metal; but the rays more refrangible than *b* are likewise absorbed. See fig. 21. If water be added to this solution, it suffers the same change as the green bromide does, becoming blue, and admitting all the more refrangible rays, like any ordinary salt of copper.

Sesquichloride of Iron gives a solution of a reddish-orange colour when concentrated, and the prismatic appearance of fig. 8. If water be added, it becomes more yellow in colour, and admits the same rays as the nitrate does. See fig. 4. Of course in this and similar cases it will be understood that the light is made to pass through the same amount of salt, whether it be diffused through much or little water; a circumstance that in ordinary cases makes no difference in the absorption or transmission of rays.

Chloride of Nickel, when dissolved in very little water, is of a yellowish-green. It then absorbs, not only the red ray, as nickel salts always do, but nearly all the light beyond F besides. When more water is added it becomes of a bluish-green, and shows the absorption due to the metal alone. See figs. 11 and 10.

Chloride of Cobalt also varies in colour according to the state of dilution. The addition of water increases the transmissibility of both the yellow and the violet rays. See fig. 12.

IODIDES.

Iodine-water is too weak to show much absorption when placed in my hollow wedge, but an alcoholic solution of iodine gives the spectrum represented in fig. 14.

Iodide of Gold and Potassium gives a prismatic appearance resembling that of iodine. The remark made in respect to the terbromide of gold applies in this case also.

Iodide of Platinum and Potassium is of an intense red, and presents much the same prismatic appearance as the gold-salt; but while the light near F is wholly absorbed, it transmits some blue and more violet, which certainly iodine itself does not. See fig. 22.

Iodide of Palladium and Potassium is also intensely red, and resembles the corresponding platinum salt, except that the violet is not transmitted so freely as the blue.

Sesqui-iodide of Iron is of a deep red colour, and gives a spectrum resembling that of iodine.

Iodide of Nickel, when dissolved in a little water, is of a dark green, and affords a spectrum closely resembling that of iodine, except that the red ray is gradually absorbed by the metal. See fig. 18. When water is added to this, the same amount of salt transmits more and more blue, and assumes the appearance of fig. 10, a compound of nickel with a colourless acid.

Iodide of Cobalt, in saturated solution, is of a dark green, passing into a very deep red as the stratum increases. On dilution it becomes pinker.

Double Salts.

When the two bases in a double salt are both coloured, a similar result is obtained.

Double Chloride of Copper and Platinum is a definite salt, forming green crystals. A saturated solution of these gives the spectrum represented in fig. 24; but on addition of water, the same amount of salt transmits a somewhat broader band of green, and more blue, giving then the appearance of fig. 20, with the red ray gradually absorbed. This remarkable compound therefore shows, when in strong aqueous solution, the absorption due to each of its three constituents,—copper, platinum, and chlorine, see figs. 20 and 21; though, on dilution, the absorption due to chlorine is somewhat modified, as in the case of the uncombined copper-salt.

These results show that each coloured constituent of a salt retains its specific absorbent power when in combination. Two of the above-described cases, however, are slightly anomalous—the chromate of chromium, and the double iodide of platinum and potassium; and these are not the only, nor indeed the greatest exceptions, for the ferric ferrocyanide dissolved in oxalic acid transmits blue rays in great abundance, which are absorbed both by ordinary ferrocyanides and ferric salts. It must be borne in mind that the statement, "all the compounds of a particular base or acid, when dissolved in water, have the same effect on the rays of light," is not universally true, even when a coloured base is combined with a colourless acid; and hence we might anticipate, what actual observation has shown to be the case, that variations would sometimes occur when both the acid and base are coloured. It may be therefore laid down as a general, though not an invariable law, that *when an acid and a base combine, each of which has a different influence on the rays of light, a solution of the resulting salt will transmit only those rays which are not absorbed by either, or in other words, which are transmitted by both.*

XLIX. *On the Effect of Heat on the Colour of Salts in Solution.* *By* J. H. Gladstone, *Ph.D., F.R.S. &c.*[*]

[With a Plate.]

As a general rule, the solution of a salt has the same power of absorbing or transmitting the rays of light at all temperatures. I am not acquainted with any instance of a dissolved colourless salt which assumes a colour when the solution is either heated or cooled; nor does the converse seem ever to occur,—a salt coloured at the ordinary temperature, which loses that colour when heat is applied. Nevertheless it is not rare to find coloured salts, which, when dissolved in water, vary in shade or in tint according to the temperature.

In some instances, heating the solution seems merely to intensify the colour. This is the case with the following red, orange, yellow, and green salts:—

Meconate of iron—red.
Terbromide of gold—red.
Red nitrate of cerium.
Bichromate of potash—orange.
Ferrocyanide of potassium—yellow.
Molybdous chloride—green.

More frequently a change takes place in the character as well as in the intensity of the colour when the solution is heated. The following cases have been observed.

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[*] Communicated by the Author.

Bichloride of platinum, while it becomes more intense in colour, assumes also a redder tint.

Protochloride of platinum, held in solution by hydrochloric acid, changes also in the same way.

Bichloride of palladium acts precisely as the platinum salt does under the influence of heat.

Ferridcyanide of potassium gives a greenish solution, which, when heated, alters in colour, and if not too dilute, assumes a distinctly red appearance.

Polysulphide of potassium passes from yellow to a most intense red.

Sesquichloride of iron passes from orange to a deep and almost pure red.

Chloride of nickel passes from a bluish to a yellowish green.

Iodide of nickel, when dissolved in a little water, gives a clear green solution, which, on the application of heat, becomes of a nondescript shade, that appears distinctly red by gas-light.

Chloride of copper gives a green saturated solution, which on the addition of more water becomes blue. If this blue solution be heated (unless too dilute), the green colour is restored. On cooling, it again becomes blue.

Bromide of copper behaves in every respect like the chloride.

Sulphocyanide of cobalt in a minimum of water, gives a magnificent bluish purple colour, but on dilution it changes to the ordinary pink tint of cobalt salts in solution. If this be heated, provided it is not too dilute, it will reassume the purple hue.

Chloride of cobalt dissolves in water always of a pink, and in absolute alcohol always of a blue colour, while in mixtures of alcohol and water it will assume an immediate tint. By arranging properly the proportions of the two solvents, a liquid may be obtained which will show all the changes of an aqueous solution of the sulphocyanide, passing from pink through purple to blue when it is heated, and conversely, from blue to pink when it is cooled.

In all these cases it is to be understood that the change of colour lasts only as long as the heat continues. No permanent chemical change is effected; and the original colour of the solution returns in every instance as it cools.

A glance at the above observations will suffice to show that where the colour is not materially altered in character, it invariably becomes more intense when heated, that is to say, fewer rays are transmitted; and when the light is analysed by a prism, it is found that not in these only, but in every one of the instances, rays are absorbed by the hot solution which were transmitted by the same solution when cold.

Yet a distinction must be made between two classes of action, both of which influence the above observations, and influence them in the same direction. In the last seven instances it is more than probable that a temporary chemical change is effected in the liquid. In the preceding paper, I have shown that the chloride and bromide of copper, the chloride and iodide of nickel, the sesquichloride of iron, and one or two other analogous salts, give a saturated aqueous solution of a different colour to what is presented by the same solution when more water is added; and that this change is due to the fact, that in the saturated solution both the halogen and the metal exert their own absorbent power on the rays during their passage through the liquid, while in the dilute solution the metal alone influences the transmission of light. That this phænomenon results from some difference of arrangement among the elements of the salt and water, scarcely admits of a doubt; yet what that difference of arrangement may be is not so easily determined. This chemical change it is that is affected by temperature, an increase of heat having the opposite effect to an addition of water, and a diminution of heat having the same effect as dilution. Thus the light transmitted by a dilute solution of chloride of copper at the ordinary temperature, when analysed by a prism, shows the spectrum represented in Plate II. fig. 9, but on heating the solution, an absorption due to chlorine manifests itself, and the coloured band is reduced to the dimensions of fig. 21: and similarly, the bromide of copper, if dilute and cold, presents the ordinary prismatic appearance of copper salts, fig. 9; but if the same liquid be heated, it assumes the green colour and the modified spectrum of the saturated solution, fig. 17, in which the bromine exerts its absorbent power. In each case the influence of the halogen disappears as soon as the heat is withdrawn; and that this is not confined to the range of temperature above what we designate as ordinary, was proved by exposing a green solution of chloride of copper to a freezing mixture, when it assumed a distinctly bluish tint.

The case of the cobalt salts is evidently analogous: the dilute sulphocyanide of cobalt in water, or the chloride in aqueous alcohol, gives a prismatic appearance similar to, but not identical with, that represented in fig. 12; but when heated, additional dark spaces show themselves; the more refrangible red and less refrangible orange rays are wholly absorbed, and the more refrangible portion of the orange is allowed to penetrate but to a short distance, while a perfectly black line shows itself coincident with D, but somewhat broader. In fact the hot cobalt salt gives precisely the same prismatic appearance as that given by smalt-blue glass, or by an alcoholic solution of cobalt, as figured in the plate annexed to my paper "On the Use of the Prism in Qualitative Analysis," in the Quarterly Journal of the Chemical Society, April 1857.

No chemical change, however, appears to be concerned in the alteration of colour produced by heat in the other cases mentioned above. The elevation of temperature seems merely to heighten the absorbent power of the dissolved salt, so that the light absorbed by a certain quantity of the heated solution is the same as would have been absorbed by a larger quantity of the same solution, if cold. This will fully account for the changes, not merely in such salts as meconate of iron, where an increased intensity of colour is all that is observed, but for those changes which involve the character as well as the depth of the colour. These latter substances are in fact more or less dichromatic, that is, they present different colours according to the quantity of salt which the light has traversed before it reaches the eye; and the reason of this will be apparent on glancing at the spectrum of bichloride of platinum represented in fig. 20. It will be there seen that a thin stratum of the salt transmits all the rays from the extreme red to the fixed line F, and a little blue and violet beyond; but that as the thickness increases, the rays about *b* and afterwards about E are absorbed. The general impression conveyed by the rays that traverse a thin stratum is yellow; but when the green rays are absorbed, it changes naturally to orange, becoming more and more red as the stratum increases. Now the effect of heat upon a thin stratum, or a weak solution, is solely to produce the same amount of absorption as would be produced at the ordinary temperature by a thicker stratum, or a stronger solution.

In reference, however, to the change that ensues when a solution of polysulphide of potassium is heated, I doubt whether any increased thickness of the same liquid would give so intense a red. Can it be of the same nature as the modification of colour that takes place in melted sulphur at a far higher temperature? This is rendered more probable by the fact, that the yellow colour of the potassium salt in solution is due to the sulphur; yet on the other hand, sulphur dissolved in naphtha shows no indication of redness when the liquid is boiled.

It is scarcely necessary to remind either physicists or chemists of the observation made by Sir David Brewster, that the absorption bands of peroxide of nitrogen are increased by heating the gas; or of the observations of Schœnbein and others, that several solid substances, such as oxide of zinc, or gallate of iron, absorb, when heated, rays that they transmit or reflect when cold. The above observations on dissolved salts give, therefore, results which are perfectly in harmony with the little that was known before about the effect of heat on the chromatic phænomena presented by other pure chemical substances.

L. *A Demonstration of* Sir W. R. Hamilton's *Theorem of the Isochronism of the Circular Hodograph.* *By* ARTHUR CAYLEY, *Esq.*[*]

IMAGINE a body moving *in plano* under the action of a central force, and let h denote, as usual, the double of the area described in a unit of time; let P be any point of the orbit, then measuring off on the perpendicular let fall from the centre of force O on the tangent at P to the orbit, a distance O Q equal or proportional to h into the reciprocal of the perpendicular on the tangent, the locus of Q is the hodograph, and the points P, Q are corresponding points of the orbit and hodograph.

It is easy to see that the hodograph is the polar reciprocal of the orbit with respect to a circle having O for its centre, and having its radius equal or proportional to $\sqrt{h}$. And it follows at once that Q is the pole with respect to this circle of the tangent at P to the orbit.

In the particular case where the force varies inversely as the square of the distance, the hodograph is a circle. And if we consider two elliptic orbits described about the same centre, under the action of the same central force, and such that the major axes are equal, then (as will be presently seen) the common chord or radical axis of the two hodographs passes through the centre of force.

Imagine an orthotomic circle of the two hodographs (the centre of this circle is of course on the common chord or radical axis of the two hodographs), and consider the arcs intercepted on the two hodographs respectively by the orthotomic circle; then the theorem of the isochronism of the circular hodograph is as follows, viz. the times of hodographic description of the intercepted arcs are equal; in other words, the times of description in the orbits, of the arcs which correspond to the intercepted arcs of the hodographs, are equal. It was remarked by Sir W. R. Hamilton, that the theorem is in fact equivalent to Lambert's theorem, that the time depends only on the chord of the described arc and the sum of the two radius vectors. And this remark suggests a mode of investigation of the theorem. Consider the intercepted arc of one of the hodographs: the tangents to the hodograph at the extremities of this arc are radii of the orthotomic circle; *i. e.* the corresponding arc of the orbit is the arc cut off by the polar (in respect to the directrix circle by which the hodograph is determined) of the centre of the orthotomic circle; the portion of this polar intercepted by the orbit is the elliptic chord, and this elliptic chord and the sum of the radius vectors at the two extremities of the elliptic chord,

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[*] Communicated by the Author.

determine the time of description of the arc; and the values of these quantities, viz. the elliptic chord and the sum of the radius vectors, must be the same in each orbit.

The analytical investigation is not difficult. I take as the equation of the first orbit,

$$r=\frac{a(1-e^2)}{1+e\cos(\theta-\varpi)};$$

then the polar of the orbit with respect to a directrix circle $r=c$ is

$$r^2-\frac{c^2e}{a(1-e^2)}r\cos(\theta-\varpi)-\frac{c^4}{a^2(1-e^2)}=0.$$

And putting $c^2=k\sqrt{k}\sqrt{a(1-e^2)}$ (where k is a constant quantity, *i. e.* it is the same in each orbit), the equation becomes

$$r^2-\frac{k\sqrt{k}\,e}{\sqrt{a(1-e^2)}}r\cos(\theta-\varpi)-\frac{k^3}{a}=0.$$

But since a is supposed to be the same in each orbit, we may for greater simplicity write $k^3=m^2a$; it will be convenient also to put $e=\sin\kappa$; we have then

$$r=\frac{a\cos^2\kappa}{1+\sin\kappa\cos(\theta-\varpi)}$$

for the equation of the orbit, $r^2=ma\cos\kappa$ for the equation of the directrix circle, and

$$r^2-m\tan\kappa\, r\cos(\theta-\varpi)-m^2=0$$

for the equation of the hodograph.

We have in like manner

$$r=\frac{a\cos^2\kappa'}{1+\sin\kappa'\cos(\theta-\varpi')}$$

for the equation of the second orbit, $r^2=ma\cos\kappa'$ for the equation of the corresponding directrix circle, and

$$r^2-m\tan\kappa' r\cos(\theta-\varpi')-m^2=0$$

for that of the hodograph.

The equations of the two hodographs give at once

$$\tan\kappa\cos(\theta-\varpi)-\tan\kappa'\cos(\theta-\varpi')=0$$

for the equation of the common chord or radical axis of the two hodographs,—an equation which shows that, as already noticed, the common chord passes through the origin or centre of force. This equation gives $\theta=\alpha$ if

$$\tan\kappa\cos(\alpha-\varpi)-\tan\kappa'\cos(\alpha-\varpi')=0;$$

i. e. α is a quantity such that the expressions $\tan\kappa\cos\overline{\alpha-\varpi}$ and $\tan\kappa'\cos(\alpha-\varpi')$, which correspond to each other in the two orbits, are equal. We may take R, α as the polar coordinates of the centre of the orthotomic circle (where R is arbitrary); the equation of the polar of this point with respect to the directrix circle $r^2=ma\cos\kappa$, is then at once seen to be

$$r\cos(\theta-\alpha)=\frac{ma\cos\kappa}{\mathrm{R}},$$

which is the equation of the line cutting off the arc of the elliptic orbit

$$r=\frac{a\cos^2\kappa}{1+\sin\kappa\sin(\theta-\varpi)}.$$

Writing $\theta-\varpi=\theta-\alpha+(\alpha-\varpi)$, the two equations give

$$\cos(\theta-\alpha)=\frac{\mathrm{A}}{r},$$

$$\sin(\theta-\alpha)=\frac{\mathrm{B}}{r}+\mathrm{C},$$

if for shortness

$$\mathrm{A}=\frac{ma\cos\kappa}{\mathrm{R}},$$

$$\mathrm{B}=\frac{ma\cos\kappa}{\mathrm{R}}\,\frac{\cos(\alpha-\varpi)}{\sin(\alpha-\varpi)}-\frac{a\cos^2\kappa}{\sin\kappa\sin(\alpha-\varpi)},$$

$$\mathrm{C}=\frac{1}{\sin\kappa\sin(\alpha-\varpi)};$$

we have therefore

$$\frac{\mathrm{A}^2+\mathrm{B}^2}{r^2}+\frac{2\mathrm{BC}}{r}+\mathrm{C}^2=1,$$

or, what is the same thing,

$$(1-\mathrm{C}^2)r^2-2\mathrm{BC}r-(\mathrm{A}^2+\mathrm{B}^2)=0;$$

and thence, if r', r'' are the two values of r,

$$r'+r''=\frac{2\mathrm{BC}}{1-\mathrm{C}^2},$$

$$r'r''=-\frac{\mathrm{A}^2+\mathrm{B}^2}{1-\mathrm{C}^2}.$$

Let θ', θ'' be the corresponding values of θ, we have

$$\theta'-\theta''=\theta'-\alpha-(\theta''-\alpha),$$

and thence

$$\cos(\theta'-\theta'')=\frac{\mathrm{A}}{r'}\cdot\frac{\mathrm{A}}{r''}+\left(\frac{\mathrm{B}}{r'}+\mathrm{C}\right)\left(\frac{\mathrm{B}}{r''}+\mathrm{C}\right)$$

$$=\frac{\mathrm{A}^2+\mathrm{B}^2}{r'r''}+\mathrm{BC}\left(\frac{1}{r'}+\frac{1}{r''}\right)+\mathrm{C}^2.$$

Or adding unity to each side, multiplying by $r'r''$, and on the right-hand side substituting for $r'+r''$, $r'r''$ their values

$$r'r''\big(1+\cos(\theta'-\theta'')\big)=-\frac{2C^2A^2}{1-C^2},$$

the square of the chord is $r^2+r'^2-2rr'\cos(\theta'-\theta'')$, or, what is the same thing, $(r'+r'')^2-2r'r''\big(1+\cos(\theta'-\theta'')\big)$; hence to prove the theorem, it is only necessary to show that $r'+r''$ and $r'r''\big(1+\cos(\theta'-\theta'')\big)$ have the same values in each orbit, that is, that $\frac{2BC}{1-C^2}$ and $-\frac{2C^2A^2}{1-C^2}$ have the same values in each orbit. But observing that

$$\begin{aligned}1-\sin^2\kappa\sin^2(\alpha-\varpi)&=\cos^2\kappa+\sin^2\kappa\cos^2(\alpha-\varpi)\\&=\cos^2\kappa\{1+\tan^2\kappa\cos^2(\alpha-\varpi)\},\end{aligned}$$

the values of these expressions are respectively

$$-\frac{2a}{R}\frac{\big(m\tan\kappa\cos(\alpha-\varpi)-R\big)}{1+\tan^2\kappa\cos^2(\alpha-\varpi)},$$

$$\frac{2a^2}{R^2}\frac{m^2}{1+\tan^2\kappa\cos^2(\alpha-\varpi)},$$

which contain only the quantities m, a, R, $\tan\kappa\cos(\alpha-\varpi)$, which are the same for each orbit, and the theorem is therefore proved, viz. it is made to depend on Lambert's theorem. I may remark, that a geometrical demonstration which does not assume Lambert's theorem is given by Mr. Droop in his paper "On the Isochronism of the Circular Hodograph," Quarterly Math. Journal, vol. i. pp. 374–378, where the dependence of the theorem on Lambert's theorem is also shown.

By what precedes, the theorem may be stated in a geometrical form as follows:—"Imagine two ellipses having a common focus, and their major axes equal; describe about the focus two directrix circles having their radii proportional to the square roots of the minor axes of the ellipses respectively; the polar reciprocal of each ellipse in respect to its own directrix circle will be a circle (the hodograph), and the common chord or radical axis of the two hodographs will pass through the focus. Consider any point on the common chord, and take the polar with respect to each directrix circle; such polar will cut off an arc of the corresponding ellipse; and then *theorem*; the elliptic chord, and the sum of the radius vectors through the two extremities of the chord, will be respectively the same for each ellipse."

2 Stone Buildings, W.C.
June 24, 1857.

LI. *Experimental and Theoretical Researches on the Figures of Equilibrium of a Liquid Mass withdrawn from the Action of Gravity.*—Third Series. *By* J. Plateau.

[Concluded from p. 22.]

§ 15. In order to terminate that which relates to a note produced at a distance and different from unison, we have still to account for the facts stated in No. 10 of paragraph 3.

We proceed to show from a theoretical point of view, that these facts, with the exception of the last, depend upon a more general principle, which may be thus enunciated: if the vibrations of the instrument are sufficiently energetic with respect to those occasioned by the shock of the detached masses, and if at the same time the interval between the two notes is not too great, the note of the jet may be brought into unison with that of the instrument. We may remark that these are the circumstances cited in No. 10 of paragraph 3; for when the jet falls upon a body which can only render a determinate note, such as a diapason, and if we suppose for a moment, that the former suffers no modification with respect to the number of detached masses, then the vibrations due to the shock of these masses will in general have a period different from that of the vibrations of the body impinged upon, and hence the former can only be thus produced: every time a mass reaches the body, air is expelled from between the two, and then returns in order to be expelled anew on the arrival of the following mass, and so on. Now the sonorous waves thus produced are necessarily very feeble when compared with those resulting from the vibrations of the body which receives the shocks; besides this, the interval between the two tones can be diminished at pleasure by causing either the charge or the diameter of the orifice to vary.

As the vibrations of the instrument—or, in the case under consideration, those of the body receiving the shock—which are transmitted by the air to the vessel and to the liquid have not the same period as the passages of the contractions and expansions due to the forces of figure, there is, as we have explained (§ 12), a varying conflict between the two kinds of action; but if the interval between the two notes is not too great, it is conceivable that the transformation of the jet—a phænomenon susceptible of being influenced by extraneous causes*—may, under the action of vibrations, so lengthen or shorten the nascent contractions and expansions that the time of passage of each of the latter shall be precisely equal to the period of a vibration, and the two kinds of actions constantly in agreement: this point being attained, the note of the jet must necessarily be in unison with

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* Second Series, § 58.

that of the instrument. In order that the vibrations of the instrument may be capable of producing this effect, however, it is evidently necessary that they should have a sufficient intensity when compared with that of the vibrations of the note natural to the jet, since the tendency of the latter is in favour of the normal action of the forces of figure.

We shall understand the phænomenon still better by regarding it from a slightly different point of view. The vibrations, it will be remembered (§ 5), tend of themselves to produce contractions and expansions upon the jet; now if these expansions and contractions differ but little in length from those which the forces of figure tend on their part to produce, and if, moreover, the action of the vibrations is sufficiently intense to predominate over that of the forces of figure, the resulting system of contractions and expansions ought to be that which depends upon the vibrations, and afterwards the transformation, thus modified from its origin, ought to complete itself according to the new manner.

But this condition of the jet is a forced one, because the natural mode of transformation is altered. Consequently, if from any cause the regular succession or transmission of the vibrations becomes disturbed, the forces of figure ought immediately to preponderate again, and the contractions and expansions to reassume the length which corresponds to the free action of these forces. Thus is explained without difficulty the peculiarity in the experiment of No. 10 of paragraph 3, according to which a slight shock imparted to the apparatus, or a change in the position of the body upon which the jet falls, is often sufficient to cause the note of the jet to return suddenly to its natural pitch.

We have assumed that in this experiment the note of the jet puts itself in unison with that of the body receiving the shock, in conformity with the principle advanced at the commencement of this paragraph. Nevertheless, as may be concluded from the enunciation in the No. in question, Savart does not express himself on this point in precise terms; he says merely that the note of the body receiving the shock modifies that of the jet, whose pitch it changes; but other experiments which we shall soon have to discuss permit us to attribute to these words the meaning we have above given them.

§ 16. Lastly, No. 10 of paragraph 3 also informs us, that when the interval between the two notes is very small, both of them may be heard periodically, or even simultaneously. Let us endeavour to explain these facts also.

Let us suppose that the note natural to the jet has a somewhat lower pitch than that of the body receiving the shock. In the case of exact unison, the number of impulses proceeding from the detached masses in a given time would be equal to half the number of vibrations made by the body in the same time; hence the interval between two successive impulses would be equal to the duration of two of these vibrations: therefore, under the above supposition, the interval between two impulses will surpass by a little the duration of two vibrations, and if the reaction of these vibrations upon the nascent contractions and expansions is not sufficiently powerful to modify their length, and thus occasion unison, the small excess in the duration of the intervals in question will maintain itself. This being the case, let us set out from the first impulse. This will cause the body to perform a descending vibration, which will be followed by an ascending one; the second impulse will take place shortly after the commencement of a new descending vibration; the third will act during the third descending vibration, but at a more advanced phase of the same; the fourth impulse will arrive at a still somewhat more advanced phase of the fourth descending vibration; and so on until at length an impulse virtually coincides with the end of such a vibration. Under the influence of these repeated impulses, the amplitude of the vibrations of the body will necessarily increase until the arrival of the impulse last considered. But again, in consequence of the small excess in the interval between two impulses, the latter will next take place during ascending vibrations, and at more and more advanced phases of the same; so that after a number of impulses equal to that of the preceding, the body will again receive a shock at the termination of a vibration. Now this second group of impulses will evidently destroy whatever the first has produced; in other words, it will gradually diminish the amplitude of the vibrations, and conclude by annulling the same. A third group of impulses will revive these vibrations, a fourth will annul them again, and so on indefinitely. The note of the body which receives the shock ought therefore to be alternately more and less intense; on the other hand, on account of the relative velocities of the body and the detached masses, the note of the jet will be less intense when the masses reach the body during its descending, than when they strike it during its ascending vibrations; hence it is seen that the note of the jet is least when that of the body is most intense, and *vice versâ*. This granted, if the vibrations of the body acquire in their greatest amplitudes sufficient energy, and if at the same time the relative velocity of the impulses becomes sufficiently small, the note of the jet may be entirely disguised during the moments when that of the body is most intense, and in its turn may reappear and again predominate during the intermediate periods; consequently both notes will be heard periodically.

But if the body is only capable of performing vibrations of small amplitude, and if it is held at a great distance from the orifice, it may occur that the relative velocity of the impulses always continues to be considerable; so that the intensity of the jet's note is sensibly uniform, whilst that of the body's, even when greatest, is not able to overpower it. The former will then always be perceptible, and consequently during the most intense periods of the latter, both notes will be heard at the same time. Without doubt it is thus that we must interpret the words *or even simultaneously,* which are taken literally from Savart.

§ 17. Let us reconsider the case where a note in unison with that of the jet is produced by an instrument. If the latter, instead of acting at a distance, is placed in contact with the sides of the vessel from which the jet issues, it is evident that the vibrations communicated to these sides and propagated in the liquid will be much more energetic, and consequently that the modifications of the jet will be much more developed; moreover, it is manifest that the small irregularities of which we have spoken in paragraph 10 may then be entirely effaced. The contents of No. 11 of paragraph 3, therefore, require no further explanation.

§ 18. According to No. 12 of paragraph 3, another system of shorter and thinner ventral segments and nodes is now observable in the axis of the jet from the lower extremity of the continuous part downwards, which system, as Savart remarks, is due to the spherules that accompany the masses.

An apparent difficulty here presents itself. When the jet is protected from all vibratory action, its troubled part is free from ventral segments and nodes; it would appear, therefore, that under the sole action of the forces of figure, the masses arrive at the spherical form without executing sensible oscillations, and that oscillations of form take place solely in the cases where the forces of figure are activated by vibrations. Now the production of the spherules cannot in any manner be influenced by the vibrations; for the latter only act directly at the contracted section, their effect below this section being limited to acquired velocities (§§ 6 and 8) which accelerate the development of the expansions and contractions, and the subsequent conversion of each of the latter into a thread. The transformation of this thread, whence result the spherules, is effected solely by the forces of figure which are generated therein, as in every other sufficiently elongated cylinder of liquid; nevertheless these spherules execute oscillations of form, as is proved by the appearance of ventral segments and nodes presented by their passage before the eye.

In order to elucidate this point, let us attentively examine the circumstances relative to these spherules and to the large masses.

The thread, we must remember*, generally divides itself into three parts, of which the extreme ones proceed to reunite themselves respectively with the two large masses between which the thread extended itself, whilst the intermediate part at the same time contracts symmetrically above and below by expanding itself horizontally, so as to form the spherule under consideration. In virtue of this simultaneity and symmetry of action, the small portion of liquid in question attains the spherical form towards which it tends; but attaining this form with an acquired velocity, it necessarily goes past the same in such a manner that its vertical diameter becomes less, and its horizontal diameter greater, than that of the sphere of the same volume; hence the oscillations of form in the spherules and the consequent ventral segments and nodes.

But the transformation of the large mass, suspended at the end of the thread and becoming detached by the rupture of the same, does not take place in exactly the same manner; in fact, an instant before this separation the mass in question was already set free at its lower part by the rupture of the thread between it and the preceding mass; here, therefore, the ruptures below and above the mass, and consequently the two contractions which tend to flatten the same in a vertical direction, are not simultaneous. Moreover, as each of these contractions must be followed by an elongation, neither do the latter take place simultaneously; and the same remarks apply of course to the subsequent contractions and elongations. Hence each contraction of the bottom of the mass will take place wholly or partially during an elongation of the top, and *vice versâ*; but the first tend to increase, the second to diminish the horizontal diameter of the mass, so that their effects upon this diameter will more or less destroy one another; and if there is no vibratory influence, which, by the increased velocity which it imparts to the transformation, carries the diameter in question beyond that of the sphere, and thus determines an excess of pressure at the equator of the mass, this diameter ought to vary but little, and consequently a system of ventral segments and nodes will not be observable in the troubled part of the jet. We see then, that, even under the sole action of the forces of figure, the masses which detach themselves at the extremity of the continuous part are necessarily the seat of oscillations of form; these oscillations, however, can only be well developed in a vertical direction. We committed a slight error, therefore, when in paragraph 69 of the Second Series we asserted that, after becoming detached, the masses immediately formed themselves into spheres.

§ 19. Let us return for a moment to the spherules. When a

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* Second Series, § 62.

thread becomes transformed, the small contractions there produced also become changed into more attenuated threads, each of which becomes ruptured at two points, and thus furnishes by its middle portion a very small spherule*. The latter spherules are often thrown out of the axis of the jet, being carried away, no doubt, by currents of air; but as their mode of generation is the same as that of the larger spherules above considered, they ought also to perform oscillations of form. Savart, indeed, says that this takes place, though he does not indicate by what means he established the fact. The parabolic trajectory, described by those of the spherules which are thrown outside the jet, probably presents to the eye a trace sufficiently distinct to admit of the recognition of ventral segments and nodes therein; perhaps, too, the apparent figure resulting from the passage of those which remain in the axis may be distinguished.

§ 20. The sonorous instrument being still placed in contact with the sides of the vessel in order to impart greater energy to the action of the vibrations, let us now suppose that a note different in pitch from that of the jet is again produced. By No. 13 of paragraph 3, it will be seen that in this case the three last conclusions of paragraph 12 are exactly in accordance with Savart's observations. There is, it is true, some vagueness in the words *almost all tones*; though they cannot mean that the notes of both kinds, effective and ineffective, alternate with each other. For let us assume for a moment that certain intermediate notes are ineffective, and let us conceive the interval between the notes of the instrument and the jet to increase continuously; then on leaving one of these ineffective notes, one of two things must take place: either the action upon the jet will gradually increase from zero up to a certain point, or it will become suddenly developed. But the first of these deductions is contrary to the statement in the cited No., according to which the action diminishes as the interval between the two notes increases, and the second is scarcely admissible. It is very probable, therefore, that the notion of ineffective notes, involved in the words *almost all notes*, refers simply to those which are too distant from that of the jet, and which, in virtue of the statement in question, ought only to produce an insensible action.

§ 21. In paragraph 15 we have stated, that, within certain limits, vibrations differing in period from those of the note natural to the jet may predominate over the forces of figure in the generation of expansions and contractions; that the transformation thus commenced is then completed according to the new mode, and hence that the note of the jet is brought into unison with that of the instrument. Now the most favourable

* Second Series, § 62.

condition towards the production of this result must evidently coexist with the contact between the sonorous instrument and the sides of the vessel, because the vibrations are thereby transmitted more immediately. In fact, whilst in the case of No. 10 of paragraph 3, the phænomenon can only be realized within an interval of a minor third, here, as may be seen by No. 14 of the same paragraph, it extends to intervals of a fifth above, and more than an octave below the principal note. We may add, that Savart no longer expresses himself vaguely as before; he says distinctly, that the note of the jet puts itself into unison with that of the instrument.

§ 22. A superior limit elevated to the height of a fifth, appears at first sight to be in opposition to certain results in our second series; for, in order that the note of the jet may be elevated one-fifth, the number of detached masses which strike the stretched membrane in a given time must necessarily increase in the ratio 2 : 3; and the same remark applies also to the number of nascent divisions which pass the contracted section during the same time (§ 2); and as, under a constant charge, the length of the nascent divisions evidently varies inversely as the latter number, it follows that from the principal note to its fifth the nascent divisions become shortened in the ratio 3 : 2. But we know* that when a jet of water yields its natural note, the length of its nascent divisions is equal to 4·38 times the diameter of the contracted section†; if, therefore, by the sole action of a sonorous instrument the note of such a jet can be raised by a fifth, the length of the nascent divisions will be reduced to two-thirds of the above value, that is, to 2·92 times the diameter of the contracted section. Now this number is a little less than the limit of stability of liquid cylinders, which limit we know‡ to be comprised between 3 and 3·6; and nevertheless we have demonstrated§, that when a liquid cylinder is transformed, the length of its divisions cannot be less than this same limit.

The difficulty is only apparent however. The demonstration just cited assumes that the cylinder commences spontaneously to transform itself, and under this assumption it is rigorously correct; but it does not apply to the case where the contractions and expansions are originally formed by a sufficiently energetic foreign cause. In fact, the demonstration in question essenti-

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* Second Series, § 83.

† Such, at least, is the value of the ratio under moderate or strong charges; under a feeble charge, however, the ratio will be less, because according to the hypothesis of paragraph 2, the nascent divisions then assume a less volume, and consequently also a less length; but everything leads us to believe, that, in the experiment in question, the charge employed by Savart was not of the latter description.

‡ Second Series, § 46. § Ibid. § 57.

ally consists in showing, that if, in the first phases of transformation, we consider a portion of the cylinder consisting of a contraction and an expansion, the length of which is equal to that of a division, everything takes place in this portion in the same manner as if its two bases were solid, so that the transformation cannot establish itself spontaneously unless the distance between the bases be at least equal to the limit of stability; but if the transformation cannot of itself commence, in a cylinder actually realized between two solid discs, it is evident that it will continue of its own accord if it has once been commenced by a foreign cause, which has accumulated a certain quantity of liquid near one of the discs so as artificially to determine a sufficiently developed expansion and contraction; for it is evident, that, on passing from one side of the limit of stability to the other, there is no sudden transition from instability to an absolute stability. On leaving this limit the stability must at first be very feeble, since it sets out from zero; consequently at a little distance within this limit a deformation, artificially imparted to the cylinder, cannot, unless the same be small, become spontaneously effaced: if, on the contrary, it is considerable, it will progress spontaneously, and will bring about the disunion of the mass. The demonstration just recalled, therefore, cannot be appealed to when the nascent contractions and expansions of the jet of liquid are formed by energetic vibrations. In this case, if the sum of the lengths of one of these expansions and of one of these contractions, or what is the same, if the length of a division is a little less than the limit of stability, the transformation may commence in this abnormal manner; and the more intense the vibrations, the more the corresponding note for which the possibility of the phænomenon exists will be elevated above the principal note.

If the foreign note has a lower pitch than the principal one, and thus tends to give to the nascent divisions a length necessarily greater than the limit of stability, it will encounter no such resistance as that just signalized within this limit, so that the possibility of the phænomenon will be much more extended; in fact, we find from Savart's experiments, that the same extends over an interval of more than an octave.

There is also another reason why the phænomenon should be less limited below than above the principal note: in one and the same sonorous instrument the amplitude of the vibrations generally increases with the gravity of the note; now it is evident that the greater the amplitude of the transmitted vibrations, the greater the excess of liquid which each descending vibration tends to push into the jet in order to form a nascent expansion, and the greater also the withdrawal of liquid which each ascending vibration tends to produce in order to form a nascent contraction. If, therefore, as the note of the instrument deviates from the principal one, either below or above, the length of the nascent divisions which the vibrations tend to form becomes greater or less than that of the nascent division which the forces of figure tend, on their part, to produce; and if an increasing conflict with the latter forces thence arises, on the other hand, below the principal note, the vibrations act more and more energetically in order to cause the new mode of transformation to prevail, and this increased action must compensate more or less for the increased conflict.

We may here remark, that in the case of a note whose pitch is very low in comparison to that of the principal note, the new mode of transformation does not establish itself in the same manner as when the interval between the two notes is not great. In the latter case, indeed, in consequence of the small difference of length between the two kinds of nascent divisions, it is very probable that the action of the forces of figure becomes modified merely, and, as remarked in paragraph 15, lengthen or shorten their nascent divisions so as to make them coincide with those which correspond to the vibrations; but when the note of the instrument has a pitch so low that the length of the latter divisions considerably surpasses that of the others, when, for example, the note of the instrument is an octave below the note of the jet, and the vibrations are sufficiently intense to impose their mode of transformation upon the jet, we must admit that the action of the forces of figure is completely destroyed, so that there is no longer a modification of the first mode which adapts itself to the second, but an absolute substitution of the second for the first.

§ 23. Experiment fully confirms what has been above said with respect to the variations of stability within, and in the neighbourhood of the limit, in a cylinder of liquid adhering to solid bases. A cylinder of oil was formed, and immersed horizontally in an alcoholic mixture, after being placed between two discs* 31 millims. in diameter and 87 millims. apart; in this cylinder, therefore, the ratio of the length to the diameter was equal to 2·8, so that the figure was quite stable: this ratio, it will be seen, deviates from the limit still more than the one which, in the preceding paragraph, we found to belong to the nascent divisions of a jet of water, which, by the action of a sonorous instrument, is brought to yield a note a fifth above its principal one. In order to alter artificially the cylindrical form of the mass, the tip of the small syringe was slowly moved several times above the figure, each time starting from one of the discs

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* These discs were retained in position by a system similar to that represented in fig. 27 of the Second Series.

and stopping about midway between the two; by this means the oil was accumulated in greater quantity towards the other disc, in such a manner, however, that during the whole time the figure never ceased to regulate itself with respect to its axis; that is to say, it always remained a figure of revolution, and thus presented an expansion and a contraction similar to those which result from a spontaneous alteration. Now as long as the diameter of the greatest circular section of the expansion did not exceed about 41 millims., the mass, when left to itself, gradually reassumed the cylindrical form; but when the diameter in question attained 41 millims., the mass, when left free, continued spontaneously to deform itself, and concluded by becoming disunited.

In this experiment the artificial deformation necessary to determine the spontaneous continuation of the phænomenon was considerable; for, according to approximative measurements, when the diameter of the greatest circular section of the expansion was 41 millims., that of the least circular section of the contraction was only 15 millims., so that the latter was scarcely a third of the former: we must not forget, however, that the ratio between the length and the diameter of the cylinder was below that which in the jet of water corresponded to the fifth above its principal note*. Moreover, there are two other reasons why the passage of the note of the jet to a fifth above ought to be brought about by vibrations which directly would produce a much smaller deformation. For, in the first place, after the immediate action of the vibrations, the deformation ought to increase in virtue of the velocities acquired (§ 6); and, in the second place, since the divisions, and consequently the expansions and contractions, become elongated during their descent (§ 2*a*), the sum of the lengths of an expansion and a contraction, which was at first less than the limit of stability, soon approaches that limit; so that the progress of the transformation, according to the abnormal mode originally imparted, becomes more easy.

§ 24. Thus theory accounts for all the phænomena resulting from the action of vibrations upon jets descending vertically, at least for all those described by Savart in a precise manner; let us now pass to jets issuing in other directions.

In the first place, since in these jets there is also a gradual transformation into detached masses, foreign notes ought necessarily to exercise upon them an influence analogous to that which they exercise upon jets falling vertically; No. 15 of paragraph 3, therefore, requires no explanation.

§ 25. With respect to No. 16 it is otherwise. If all the divisions on attaining successively the extremity of the continuous

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* An error in the construction of the small apparatus caused the ratio to be 2·8 instead of 2·92.

part detached themselves in exactly the same manner, and if all the masses left this point with precisely the velocity which corresponds to the translatory motion of the liquid in the same, these masses would all describe exactly the same trajectory, and hence the discontinuous part could not become dispersed or present the appearance of a sheaf; there must, therefore, as Savart remarked, be irregularities in the emission of the detached masses from the extremity of the continuous part; these irregularities, however, must be very small, for the sheaf has not a great width. At first I thought that these irregularities resulted from the same causes as those considered in paragraph 10; but if this were the case, the suppression of foreign actions ought to cause the sheaf to disappear, and thus to reduce the whole to a single jet. But experiment does not confirm this; for by employing the means used by Savart in the case of jets descending vertically, that is to say, by receiving the discontinuous part upon a thick plank suitably inclined, and by placing soft bodies under the vessel whence the jet issues, under that which receives it, and under the supports, I have not been able to cause any notable diminution in the sheaf. One would infer from this that the irregularities are not due to vibratory motions, and consequently that they affect the action of the forces of figure themselves. It is manifest, in fact, from the nature of the phænomenon of transformation, that even slight disturbing causes must have an influence upon the perfect identity of all the divisions generated one after another at the contracted section; for instance, in the experiments of the paragraphs 50 to 55 of the Second Series, we have seen an external cause alter the equality in the lengths of the divisions of a cylinder. This granted, we will show that small differences of this kind in the nascent divisions of a jet issuing at a suitable inclination must necessarily cause the discontinuous part to be dispersed to some extent.

Let us consider more minutely two contractions, together with the expansion between them. As we know, each of these two contractions, very feebly indicated on leaving the contracted section, becomes gradually developed in traversing the continuous part by sending the half of its liquid into the expansion; the latter, therefore, receives in front the liquid sent thither in a direction contrary to the movement of translation, and in the rear the liquid driven thither in the same direction as that of translation, so that its velocity of translation tends to be diminished by the one afflux, and to be augmented by the other. Now although these two opposite actions are in general unequal, because at every moment the contraction in front is in a somewhat more advanced phase of transformation than that in the rear, nevertheless if the two contractions were perfectly identical at their respective origins, and if from that time up to their respective ruptures they have suffered precisely the same modifications, though not altogether at the same moments, it is evident that after these two ruptures, that is to say, when the expansion becomes a detached mass, the sum of the quantities of motion imparted to this mass by the preceding contraction will have been absolutely compensated by that of the quantities of motion in an opposite direction, imparted to it by the contraction which follows, and hence that this same mass will leave the continuous part with the velocity which corresponds exactly to the general motion of translation. But it is also clear, that the compensation would no longer be complete if at their origin the two contractions differed from each other; if, for example, they were unequal in length. Since when the divisions, and hence also the contractions, are longer the time of transformation is shorter*, it follows that the longer of the two contractions in question will develope itself more rapidly than the other; and as, in consequence of its greater length, it encloses more liquid, it will transfer to the expansion more particles of liquid with greater velocities, and consequently a greater quantity of motion. If this same contraction, therefore, follows the expansion, the latter will leave the continuous part as a detached mass with an excess of velocity; but if the contraction in question is that in front, the mass will depart from the continuous portion with a defect of velocity. Thus small differences in the lengths of the nascent contractions will occasion small inequalities in the velocities of the successively detached masses; on this account the masses will necessarily describe parabolæ of unequal amplitudes, and hence they will become dispersed in a vertical plane so as to form a sheaf.

This explanation assumes that the disturbing causes produce no irregularities in the contractions in directions perpendicular to the axis of the jet; and, in fact, from the experiment of paragraph 23, we must conclude that the contractions and expansions tend, with great force, to symmetrize themselves with respect to the axis, and hence that irregularities in directions normal to the axis cannot long exist.

According to this explanation, too, it is evident that there are two extreme limits for which the dispersion is zero, viz. when the jet descends and ascends vertically, for in these two cases all the detached masses describe one and the same rectilineal trajectory †;

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* Second Series, § 66.

† In a jet ascending vertically, the liquid, it is true, becomes dispersed when falling again; but it is scarcely necessary to remark, that the latter dispersion is due to a totally different cause, and has nothing in common with the phænomena here considered.

if then we pass from the first to the second limit, by causing the direction according to which the jet issues to vary by degrees, the sheaf will not begin to manifest itself distinctly until a certain angle between this direction and the descending vertical line is attained, and it will cease to be very apparent beyond another determinate angle. Further, as long as the jet issues in obliquely descending directions, and even in a horizontal direction, it is evident that at the extremity of the continuous portion—which portion is generally tolerably long—its direction will be already too near the vertical for the sheaf to manifest itself distinctly, so that the first direction at which the sheaf will commence to be visible will be an obliquely ascending one. All these conclusions agree with the facts of the No. under discussion.

As will be seen, we admit that the inequalities between the nascent contractions do not depend upon the direction according to which the jet issues; and, in fact, there is no plausible reason for attributing these inequalities to the ascending obliquity of the jet. In treating of vertically descending jets, these inequalities were not mentioned, because in such cases they cannot occasion any phænomena of a peculiar kind; their only effect, then, is to augment a little in the axis of the jet the inexactitude of the superposition of the individual systems of ventral segments and nodes, and thus they merely constitute an influence to be added to those mentioned in paragraph 10. The nature of the disturbances which produce the inequalities in question, it would no doubt be rather difficult to discover; but whatever it may be, the dispersion of the discontinuous part in jets issuing at a suitable angle reveals to us the presence of these causes.

§ 26. A jet being allowed to issue under an angle such as to cause the sheaf to be well formed, let us now conceive it to be submitted to the influence of a sonorous instrument. The note which will most shorten the continuous part will again evidently be that whose vibrations succeed each other at the same intervals as the passages at the contracted section of the expansions and contractions due to the forces of figure (§§ 5 and 12). But these vibrations being perfectly regular and isochronous, will, if sufficiently intense, prevent the disturbing causes from modifying the nascent contractions; in other words, when activating the transformation they will impart to the same their own regularity, so that the nascent contractions will have the same length, and thus all the detached masses will describe precisely the same trajectory (§ 25): under the influence of the note, therefore, the sheaf ought to disappear, and the whole ought to reduce itself to a single jet presenting a perfectly regular system of ventral segments and nodes.

§ 27. With respect to the singular effects whereby, under the influence of other notes, the sheaf is reduced to two or three jets, it would be necessary, before attempting an explanation, to know the relation between the notes in question, and the principal note; which relation Savart does not indicate. On this account, and because these phænomena are not the least curious amongst those which result from the action of vibrations upon jets of liquid, I decided to attempt the experiment.

The orifice I employed had a diameter of 3 millims.; it was made at the centre of a circular brass plate 12 centimetres in diameter*, so inclined that the jet issued upwards at an angle of about 35° to the horizon; this plate formed one of the bases of a cylindrical drum, which communicated by a wide and short horizontal tube with the lower part of a large Mariotte's vessel; the charge amounted to 34 centimetres; lastly, the sonorous instrument was a violoncello, the bottom of which rested on the supports of the apparatus.

The sheaf being well formed, repeated trials were made to find the principal note, or that which completely reduced the whole to a single jet with a well-regulated system of ventral segments and nodes, and which at the same time originated the first ventral segment very near the orifice. This being accomplished, the pitch of the instrument's note was raised by successive semitones. The influence of the vibrations was then found to diminish, the jet commenced to lose its regularity, afterwards the sheaf gradually reappeared, and then it remained without becoming reduced to either two or three jets. The principal note was next reproduced, and starting from it, the note of the instrument lowered in pitch by semitones. Again the alteration in the jet's regularity, and the progressive reappearance of the sheaf, manifested themselves; but on approaching the octave below, the sheaf showed a tendency to become changed into a double jet, and on arriving at the latter note the sheaf was decidedly replaced by two jets with regular systems of ventral segments and nodes. On continuing to lower the pitch of the note down to a third below the octave above mentioned, the two jets continued unaltered; lower still, and before reaching the second octave, sometimes two, sometimes three jets were obtained; the fifth alone occasionally gave a single jet; at length with the second octave below the principal note, three jets were always observed. In all these cases the jets invariably presented systems of ventral segments and nodes.

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* The object of this large diameter was to allow sufficient liberty to the vibrations of the plate; without which liberty, indeed, the vibrations of the liquid flowing towards the orifice would be impeded, and would thus lose their action on the jet.

These facts are less restricted than those enunciated in No. 16 of paragraph 3; in fact, according to this No., in which the meaning of Savart's expressions is reproduced, it is only under the influence of the principal note that the sheaf should become reduced to a single jet, and there should only be two other determinate and different notes capable of causing the appearance of two and three jets respectively. But the absence of any indications with respect to the relations between these notes and the principal one, is sufficient to show that Savart did not bestow his whole attention on phænomena of this kind, and that after having observed them in isolated cases, he did not investigate their susceptibility of extension.

§ 28. Let us now inquire whether theory can account for these same phænomena. Let us begin with the octave below the principal note. The period of a vibration corresponding to this note is double that of the passage of a contraction or an expansion at the contracted section, whence we conclude that the divisions which would be produced under the sole influence of the octave under consideration would be twice as long as those determined under the isolated action of the forces of figure. Hence we may admit that each of the first exactly embraces two of the second; for in this manner, at all the sections in which these pairs terminate, there is evidently an absolute concurrence between the two kinds of action, the sections in question constituting at once the middles of the contractions which would result from the vibrations, and the middles of the contractions due to the forces of figure. Let us now examine what ought to take place in any one of these pairs of divisions during transformation. As this pair consists of two entire divisions, it contains two expansions together with the intervening contraction, and is terminated by two semi-contractions; now whilst the whole contractions to which these terminations belong are, as we have seen, favoured by the vibrations, it is evident that the intermediate contraction is, on the contrary, in conflict, since its middle, which is the middle of the pair, corresponds to the middle of the division which the vibrations tend to produce, and hence to the middle of the expansion of the same; each of the expansions which the forces of figure produce in the jet is consequently adjacent to two unequally solicited contractions. Moreover, the contractions favoured by the vibrations must be also elongated by their influence, because the contractions which the latter would themselves produce would be twice as long; and as the length of each of the pairs of divisions above considered remains the same as it was in the absence of the note of the instrument, it follows that the contractions intermediate between the preceding, that is to say, those which occupy the middles of the pairs, and which are in conflict with the vibrations, must be shortened. We may assume, therefore, that the favoured contractions, although from their origin more attenuated than those in conflict, contain, nevertheless, more liquid than the latter because of their greater length; and since, being at once longer, and activated by the vibrations, they arrive more rapidly at their rupture, it is evident that they will transfer to the expansions more matter with greater velocity, and hence also a greater quantity of motion. All the expansions therefore will be in the condition analysed in paragraph 25, and consequently some of the detached masses on leaving the continuous part will have a small excess, and others a small defect of velocity. But here, inasmuch as the vibrations impart their regularity to the phænomena, they render all the favoured contractions identical at their origin, and similarly all the contractions in conflict identical amongst themselves; so that all the masses formed from the expansions which, during the course of the continuous part, had a favoured contraction in the rear, will leave that part with the same excess of velocity, and hence describe the same trajectory; and all the detached masses which result from the expansions which were preceded by the favoured contractions, when traversing the continuous part, will leave the latter with the same defect of velocity, and consequently describe another unique trajectory different from the former. Under the influence of the octave below the principal note, therefore, the sheaf should be replaced by two separate jets.

Nevertheless, it would not be impossible for the note under consideration to cause the sheaf to disappear; in fact, this note having already a very low pitch—at least with respect to the jet upon which I operated—its vibrations have a large amplitude, and they may act (§ 22) with sufficient energy to prevent the formation of the contractions in conflict, and thus leave only those divisions in the jet which they themselves tend to produce, in which case all the detached masses would necessarily have the same normal velocity.

Let us examine, in the second place, the influence of the fifth below the preceding note, or in other words, of the second fifth below the principal note. The vibrations of this note being three times less rapid than those of the principal note, we easily conclude that each of the divisions which they themselves tend to determine in the jet comprise exactly three of the divisions due to the forces of figure. It is clear, too, that of the three expansions contained in this group of divisions, the last has behind it a favoured contraction, and before it a contraction in conflict; that the first, on the contrary, is preceded by a favoured contraction and followed by one in conflict; and lastly, that the intermediate one is situated between two contractions in conflict which are identical with each other at their respective origins. According to this, the quantities of motion will necessarily distribute themselves amongst the detached masses proceeding from these three divisions in such a manner that the last, the first, and the intermediate one will leave the continuous part with velocities respectively superior, inferior, and equal to the normal velocity; and as, in consequence of the perfect regularity of the vibrations, exactly the same things will take place in each of the groups of three divisions, there can only be three different velocities in the discontinuous part. Hence if the action of the vibrations does not entirely efface that which, before their influence, the forces of figure exercised freely, the sheaf will resolve itself into three distinct jets; and if, on the contrary, the action of forces of figure is completely subordinated—which ought to occur more easily here than in the case of the octave below the principal note, in consequence of the still greater amplitude of the vibrations—there will, as we have shown above, be but one jet.

As to the separation into two jets, which, according to the experiments, also occurs under the influence of the note under consideration, it may be thus accounted for. When the action of the vibrations preponderates, and thus only the divisions which it determines are generated at the contracted section, the latter have a great length, since each occupies the place of three of the divisions which the forces of figure would originate; but we know* that every liquid figure, one dimension of which is considerable with respect to the two others, tends to divide itself into detached masses: we may assume, therefore, that new forces of figure develope themselves in the divisions in question, provided the acquired transversal velocities are not sufficiently great to oppose the same, and that these new forces divide each of the divisions into two new ones by forming a contraction in the middle; and hence, as each of the contractions thus produced is evidently in conflict, the reasoning employed with respect to the octave below the principal note shows that two jets ought to be obtained.

We may here remark, that the abnormal forces of figure just considered cannot form more than one contraction in each large division; for if they formed two, so as to divide each large division into three small ones, the latter would have the same length as those of the jet when not submitted to the influence of the sonorous instrument; but for this to be possible, it would be necessary that the new divisions should not experience more resistance to their formation than when every foreign action is absent; for from what takes place in cylinders†, we may con-

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* Second Series, § 85.

† Ibid. §§ 58 and 59.

clude that in every more or less analogous liquid figure the length of the divisions increases with the resistances; but the acquired transversal velocities, which in our large divisions determine a tendency to persevere in the mode of transformation imparted by the vibrations, constitute a resistance to an ulterior division.

Let us proceed, in the third place, to the second octave below the principal note. Here each of the divisions which would be generated under the sole action of the vibrations evidently comprises four of the divisions which would result from the forces of figure alone; but if these two actions combined together, it would appear that four distinct jets ought to result; for it is easy to see that the conflict would be unequal in the three contractions which would then form themselves, that it would be greater for the middle than for the two other contractions, so that each of the two expansions comprised between these three contractions would receive at its two sides unequal quantities of motion; and finally, that the differences would be greater for the extreme expansions, each of which would be situated between a favoured and opposed contraction. But, on the one hand, the vibrations in question having a considerable amplitude, it is clear that their action ought always to efface that of the forces of figure; and, on the other hand, the divisions thus formed being very long, it is also evident from what has been above said, that new forces of figure must arise to cause their subdivision. Again, in consequence of the resistance, also above indicated, this subdivision can at most give three parts, which, in consequence of the distribution of the conflicts and concurrences, and the regularity imparted by the vibrations, will convert the sheaf into three jets only.

We have still to consider, in the fourth place, the action of the notes included between the octave below the principal note and the fifth below the former, as well as those between this fifth and the second octave below the principal note. For these notes there is no longer any simple relation between the divisions which would result respectively from the vibrations alone and from the forces of figure alone; but it will without difficulty be admitted, that under the influence of the notes a little above or a little below the second fifth, and in the case where the effect of the vibrations does not entirely efface that of the forces of figure, the divisions due to these forces will be lengthened or shortened a little so as to permit the absolute concurrence of the two kinds of action at the limits which separate the successive systems of three of these divisions, and hence also the re-establishment of the simple ratio of 3 to 1 corresponding to the second fifth; hence the resolution into three jets. Under this influence, too, as under that of the double fifth, if the vibrations are preponderant, but not sufficiently so to oppose an ulterior development of the forces of figure, each large division will only be subdivided into two, so that the discontinuous part of the jet will present two jets only.

It will also be admitted, that notes in the neighbourhood of the octave below the principal note will cause the mode of transformation corresponding to the latter note to prevail, and thus the sheaf will never be changed otherwise than into two jets.

Lastly, it will likewise be admitted, that, for notes not too far distant from the second octave below the principal note, the vibrations will always have sufficient amplitude, and consequently sufficient action, to overcome the ordinary forces of figure, and that at the same time the divisions which they originate will always be sufficiently long to necessitate a subsequent subdivision of each of them at most into three parts, and possibly only into two, provided the vibrations offer a greater resistance; hence there will be either three or two jets.

As to the systems of ventral segments and nodes, which are observed in all the jets, they are evidently occasioned by the acquired transversal velocities which proceed from the action of the vibrations.

§ 29. It may be asked why, above the principal note and between this and the octave below, there are no notes except those in the neighbourhood of the two latter, which, in the experiments described in paragraph 27, occasioned phænomena analogous to those just studied; in fact, in the case of the fifth below the principal note, it will easily be found that the length occupied by a group of two divisions due to the vibrations alone is equal to that occupied by a group of three of the divisions due to the forces of figure; so that if we imagine these two groups to be superposed and combined, there will be concurrence in the two contractions which form the terminations of the system, and conflict in the two intermediate contractions belonging to the second of the groups above considered; and as the two conflicts would be equal, one would expect, according to our theory, to see the sheaf resolve itself into three jets; lastly, and for analogous reasons, one might also expect three jets to manifest themselves under the influence of the fourth, and two under that of the fifth above the principal note.

But in our theory, as we have seen, the manifestation of one, two, or three jets in place of the sheaf assumes that the vibrations communicated to the liquid regularize what takes place in the jet; and for this it is necessary that these vibrations should have an energy of action capable of neutralizing the disturbing causes which tend to establish, in the successive nascent contractions, unsymmetrically distributed inequalities of length. But, everything else being the same, inasmuch as the action of the vibrations upon the jet diminishes with their amplitude, we can conceive that above the octave below the principal note this action was probably simply insufficient; and that if it had been possible, by a more immediate transmission, or by a better disposition of the system of the orifice, to augment the amplitude of the communicated vibrations, the three notes above signalized would probably have no longer been inactive with respect to the sheaf. This will be evident if we notice that the vibrations act upon jets issuing obliquely in the same manner as upon those descending vertically, and if we remember that in Savart's experiments, mentioned in No. 14 of paragraph 3, and explained in the paragraphs 21 and 22,—which experiments were arranged so as to give a great intensity to the communicated vibrations—the mode of transformation imparted by the vibrations completely replaced that of the forces of figure in cases even where the notes extended to a fifth above the principal note.

I have mentioned the possible influence of a change in the nature of the orifice, because that employed in my experiments was made in a very thin plate*, and hence this plate perhaps vibrated with difficulty in unison with those notes which had not a low pitch.

§ 30. To complete our theoretical examination of the influence of vibrations upon jets of liquid, we have now merely to show the connexion between theory and experiment with reference to the facts of No. 17 in paragraph 3.

Since the principal note is also that for which the period of a vibration is equal to that of the passage of a contraction or an expansion at the contracted section (§§ 5, 12, and 26), and since, according to experiment, the number of vibrations corresponding to this note diminishes as the direction according to which the jet issues deviates more from the descending vertical, the number of nascent contractions and expansions, and hence the number of nascent divisions, passing the contracted section in a given time must also diminish. But as the velocity of efflux is sensibly independent of the direction according to which it takes place, the number of divisions generated in a given time cannot diminish notably except by an augmentation in the length of these nascent divisions; thus under the same charge and with the same orifice the nascent divisions become elongated as the direction of efflux deviates more from the descending vertical. This result may be immediately deduced from the hypothesis in paragraph 2; for whilst, on the one hand, a jet descending vertically tends to become thinner in consequence of the accelera-

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* It was only about half a millimetre in thickness.

tion due to gravity, on the other hand, a vertically ascending jet tends to become thicker in consequence of the retardation due to the same cause; and since, according to the hypothesis in question, and the therein assumed dependence between the divisions, the thinning of the vertically descending jet determines a diminution in the length of the nascent divisions, the thickening of the vertically ascending jet ought, for the same reason, to determine an increase in the length of the nascent divisions; whence it follows, that as the direction of efflux passes gradually from the first to the second of these cases, the nascent divisions will become elongated by degrees.

As may be seen from the No. under discussion, between the descending vertical and the horizontal direction of the jet the depression of its principal note is inconsiderable, but becomes greater between the latter and the vertically ascending direction, which necessitates the same relations between the elongations of the nascent divisions. This fact flows also from the hypothesis of paragraph 2; for the vertically ascending jet, in consequence of the gradual destruction of the velocity of the liquid, has a greater tendency to become thick (especially at its upper extremity), than has the vertically descending jet to become thin at an equal distance from the contracted section; hence, in virtue of the same mutual dependence of the divisions, when the jet, issuing first in a horizontal direction, is made to approach the ascending vertical, the successive increments in the length of the nascent divisions ought to be much greater than when the jet, starting from the vertically descending, gradually approaches the horizontal direction.

Observed facts being thus connected in a necessary manner with the hypothesis of paragraph 2, serve reciprocally to confirm the latter; and it was to these facts that we alluded when, in paragraph 2, we intimated that the hypothesis in question was supported by experimental results.

§ 31. In concluding the Second Series, we announced that in the present one, after having completed that which relates to jets of liquid, we should treat of the other figures of equilibrium besides the sphere and the cylinder; but, in order to avoid extending our memoir too much, we have resolved to reserve this subject for future series*.

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* At the end of the above memoir the author adds a note, wherein he briefly describes the researches of Hagen, Billet-Sélis, Dejean and Magnus, which appeared during the interval between the publications of his own second and third series of Researches. His last memoir, however, "On the recent Theories," &c., which appeared in the Philosophical Magazine for October 1856, renders the translation of this note here unnecessary.—TRANS.

LII. *On the Principle of* Nicol's *Rhomb, and on some improved forms of Rhombs for procuring Beams of plane-polarized Light.*

By Professor Potter, *A.M.*[*]

THE advantages of the compound rhomb of calc-spar, discovered by the late Mr. Nicol, in many of the interesting experiments of physical optics, are familiar to most who have tried experiments on the polarization of light. The theory of the mode of action of this rhomb by one of the doubly refracted rays undergoing total reflexion, had not however been published, that I am aware of, before it appeared in my experimental treatise on Physical Optics, published in the autumn of last year, although I had given that explanation to my friends for many years past. The explanation is as follows:—A ray of light entering a face of calcareous crystal becomes separated into two rays polarized in planes at right angles to each other; and in Nicol's rhomb one of these falls so obliquely upon a common surface of the crystal and a cement of Canada balsam as to be totally reflected and thrown aside, whilst the other traverses the film of cement, passes through the spar beyond it, and finally emerges from the rhomb as a ray polarized in one plane. A beam of light passing through the rhomb in this manner, becoming plane-polarized and of great brightness without colour, the rhomb is of continual use in optical experiments. M. Foucault has, it appears, adopted this explanation, and brought it before the British Association at the late Meeting in Dublin, as well as the form of rhomb which I described in the above-named treatise, with air in place of Canada balsam between the component prisms.

On examining a Nicol's rhomb of the original form, it will be found to be like fig. 1, where two like prisms of calc-spar are cemented together with Canada balsam in the common surface

Fig. 1.

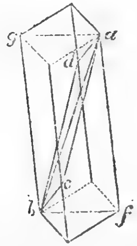
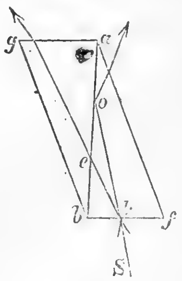


Fig. 2.



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[*] Communicated by the Author.

a d b c; and taking the principal section *a g b f*, the angle *a b f* will be found to be about a right angle. Let *a g b f*, fig. 2, represent the same section as in fig. 1. Let S *k* be a ray of light entering the rhomb at *k*, and separated by the double refraction into an ordinary ray *k o*, and an extraordinary one *ke*; and if the ray S *k* is incident nearly perpendicularly upon the surface *b f*, the ordinary ray *k o* will have very little deviation in direction; but the extraordinary ray, as Huygens found, will have the angle *e k o* about 6° 40′; an inclination which is towards the obtuse summit of the rhomb. Now the film of Canada balsam being represented by *b a*, the angle of incidence of the ordinary ray *k o* may be so great that the ray cannot pass into the Canada balsam, but will be totally reflected according to the rules of geometrical optics, because Canada balsam has less refractive power than calc-spar for the ordinary ray, although it has more refractive power than calc-spar for the extraordinary ray when it passes in certain directions; and the extraordinary ray may pass through the film and upper prism, as before stated, when *a b* has the proper direction with regard to the optic axis of the crystal. Now Canada balsam has been found to vary in its refractive index from 1·528 to 1·549, therefore putting $\mu_c = 1{\cdot}538$ for an average refractive index; also for calc-spar from M. Malus's measures, let $\mu_0 = 1{\cdot}6543$ be the constant refractive index for the ordinary ray, and let μ_e be the refractive index for the extraordinary ray, which varies from 1·6543 at its maximum value to 1·4833 at its minimum value. The luminiferous surfaces for light radiating around a luminous point within the crystal, take the forms of a sphere and an oblate spheroid, with their common axis the optic axis of the crystal, and the ray velocity is inversely proportional to the refractive index; so that if a and b are respectively the major and minor axes of the ellipse which generates the spheroid by its revolution round the axis, θ the angle which an extraordinary ray makes with the major axis, and ρ the corresponding radius vector, we have from the equation of the ellipse to the centre,

$$\rho = \frac{b}{\sqrt{1 - e^2 \cos^2 \theta}},$$

and

$$\frac{\mu_e}{\mu_0} = \frac{b}{\rho},$$

$$\therefore\ \mu_e = \mu_0 \sqrt{1 - e^2 \cos^2 \theta};$$

putting $\theta = 0$, we have $\rho = a$, and

$$e^2 = 1 - \left(\frac{1{\cdot}4833}{1{\cdot}6543}\right)^2 = {\cdot}19605,$$

$$\therefore\ \mu_e = 1{\cdot}6543 \sqrt{1 - {\cdot}19605 \cos^2 \theta}$$

To find the critical angles for the two rays as they fall upon the Canada balsam, we have the law of Snellius if i is the angle of emergence, i' the angle of incidence within the medium,

$$\sin i' = \frac{\sin i}{\mu};$$

and when i is 90° as its highest value, we have i' the critical angle, which is found from

$$\sin i' = \frac{1}{\mu}.$$

When the rays pass from one medium to another, μ becomes the relative refractive index for those media, and in the above formula is the quotient of the absolute refractive index for the medium in which the ray passes, divided by that for the medium into which it emerges; therefore we have for the ordinary ray,

$$\mu = \frac{\mu_0}{\mu_c} = \frac{1{\cdot}6543}{1{\cdot}538};$$

for the extraordinary ray,

$$\mu = \frac{\mu_e}{\mu_c} = \frac{\mu_0\sqrt{1-e^2\cos^2\theta}}{\mu_c}.$$

The former is greater than unity, and i' in $\sin i' = \frac{1}{\mu}$ has a definite value, which is found to be

$$i' = 68^\circ\ 23'.$$

The latter, when θ is small, is less than unity, and $\frac{1}{\mu}$ greater than unity, and there can be no critical angle; therefore the rays will pass into the Canada balsam at all angles of incidence upon it.

Whilst $\frac{\mu_e}{\mu_c} = \frac{\mu_0\sqrt{1-e^2\cos^2\theta}}{\mu_c}$ remains less than unity, this will occur; and when it equals unity, i' will be 90°. To find θ when this happens, we have

$$\frac{\mu_e}{\mu_c} = 1 = \frac{\mu_0\sqrt{1-e^2\cos^2\theta}}{\mu_c},$$

which gives $\theta = 33^\circ\ 43'$.

Now if bf, fig. 2, be a natural force of the crystal, the optic axis* makes an angle 45° 20′ with it; and adding $90^\circ - 33^\circ\ 43' = 56^\circ\ 17'$ to it, we have $45^\circ\ 20' + 56^\circ\ 17 = 101^\circ\ 37'$. If the angle abf were this value, an extraordinary ray falling upon the

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* Huygen's *Traité de la Lumière*, p. 98.

surface of the Canada balsam nearly parallel to $b\,a$ would pass into it, and *à fortiori* at smaller angles of incidence. If the angle $a\,b\,f$ equals 90° nearly, as in the older form of Nicol's rhomb, and the ordinary ray $k\,o$ makes the critical angle of incidence at o equal to $68^\circ\ 23'$, and the extraordinary ray an angle about $6^\circ 40'$ less than that, this latter will be transmitted, whilst the ordinary ray is reflected; and this will continue whilst the angle of incidence of the extraordinary ray increases from $68^\circ\ 23' - 6^\circ 40' = 61^\circ\ 43'$ to the critical angle of the extraordinary ray. If i'' be this critical angle, we have

$$\sin^2 i'' = \frac{\mu_c^{\,2}}{\mu_0^{\,2}(1-e^2\cos^2\theta)},$$

and

$$\theta = 45^\circ\ 20' - (90^\circ - i'') = i'' - 44^\circ\ 40',$$

which give

$$i'' = 82^\circ\ 43' \text{ nearly};$$

then $82^\circ\ 43' - 61^\circ\ 43' = 21^\circ\ 0'$ within the crystal.

If the refraction were ordinary refraction, multiplying this by 1·5, we should have the corresponding angle in air $31^\circ\ 30'$; but the true angle of the incident pencil would require to be calculated from the properties of the extraordinary rays, since these furnish the transmitted beam which is plane-polarized. The angle $31^\circ\ 30'$ is more than the visible angle of the polarized beam in the old form of Nicol's rhomb, and also of the dumpy rhombs now much used; but the Canada balsam employed for cement may be more dense than the average taken in the computations. The pair of large dumpy rhombs which I have had for many years in common use, as well as a rhomb of the old form, furnish a beam of polarized light of only about 27° for green light, and larger for blue, but less than this for red light.

From the above discussion it is clear that the separation of the polarized rays is effected by the first prism $a\,b\,f$, and the second part of the rhomb $a\,b\,g$ has only to transmit the extraordinary ray after it has passed through the film of Canada balsam. I therefore propose that the second prism shall be of glass, which is much easier to work than calc-spar, and the best quality of calc-spar has sometimes been very scarce.

In my descriptive and experimental treatise on Physical Optics, p. 31, I have described another form of rhomb which I had tried many years ago, with air in place of Canada balsam between the prisms, and those of much smaller angles than Nicol's rhomb, so that a much broader beam of polarized light could be obtained. I now propose that the prism nearest the eye shall be of glass, and the rhomb will then be like M. Biot's double-image rhomb, only with air between the two prisms instead of Canada balsam, and the angles of the prisms as investigated below. In fig. 3, let *acbd* be the plate of air between a prism of calc-spar *a c b d e f g*, and a like prism of glass *a c b d h k*. Now if fig. 4 represents a principal section of the rhomb, *a b* being the section of the plate of air, a ray of light S *r* being incident nearly perpendicularly at *r* on the surface *b f* will furnish an ordinary ray *r o* and an extraordinary ray *r e* with the angle *e r o* between them nearly 6° 40′. Then if the ordinary ray falls at the critical angle 37° 12′ and any higher angles, it will be totally reflected and thrown aside as *o m* in the figure. The critical angle for the extraordinary ray will depend upon the inclination of *b a* to the optic axis. If we take *b f* a natural face of crystal, and the angle *a b f* equal to 40°, we find the critical angle i'' from the expression

Fig. 3.

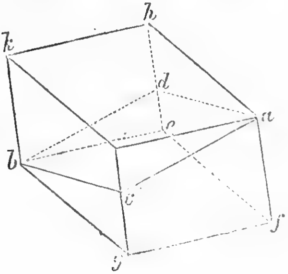
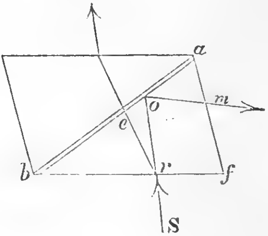


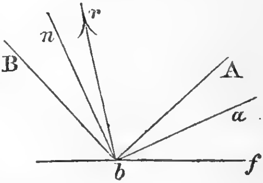
Fig. 4.



$$\sin i'' = \frac{1}{\mu_0 \sqrt{1-e^2\cos^2\theta}},$$

where θ is measured from the major axis of the generating ellipse of the oblate spheroid, the angle the minor axis makes with *b f* being 45° 20′.

Fig. 5.



In fig. 5, let *a b* and *b f* be as in fig. 4, let *b n* be perpendicular to *a b*; let *b* A be the direction of the optic axis and minor axis of the ellipse, *b* B that of the major axis, and *b r* the direction of the extraordinary ray; then the angle

$$\text{A}\,b\,a = 45^\circ\,20' - 40^\circ = 5^\circ\,20'$$

= angle B*bn*; and angle *r b n* = i'', therefore θ = angle *r b* B = $i'' + 5^\circ\,20'$, and

$$\sin i'' = \frac{\cdot 60419}{\sqrt{1-\cdot 19605\cos^2(i''+5^\circ\,20')}},$$

which gives i'' about 39° 32½′.

Since the critical angle for the ordinary ray is 37° 12′, and when the ordinary light commences to undergo total reflexion the extraordinary ray makes an angle of incidence $37° 12' - 6° 40' = 30° 32'$, we have the range between this and 39° 32′ within the crystal giving a pencil of the angle 9°, which is sufficient to furnish conveniently a broad beam of polarized light. From the nature of the double refraction, the angular separation of the ordinary and extraordinary rays diminishes as they approach the optic axis, and they coincide when in that direction; and hence the incident and emergent beams, when both the prisms are of calc-spar and similar, subtend angles of only about 8° to 9°.

When one of the prisms is of glass, we have other properties to consider, and have different results as the glass or spar is turned towards the eye, when the rhomb is used as an analyser. When the spar is cut along the optic axis, and the light is incident nearly perpendicular to the face so produced, the two rays coincide in direction, but have the greatest difference of refractive indices, that of the extraordinary ray being somewhat less than the one for common plate glass; a beam passing into a compound rhomb in this manner, we may have the extraordinary ray undergoing total reflexion at the last surface of the glass or that nearest the eye; and so an angular magnitude as well as breadth of pencil may be procured which will render the rhomb serviceable both as polarizer and analyser where Nicol's rhomb and the double-image rhomb would be much less applicable. When the experiments which I have in hand with this form of rhomb are completed, I intend to give an account of them in another paper. I have already obtained pencils of about 11° polarized in one plane with the *ordinary* rays transmitted.

London, November 7, 1857.

LIII. *On the Brine-springs of Cheshire.* *By* AUGUSTUS BEAUCHAMP NORTHCOTE, *F.C.S., Senior Assistant in the Royal College of Chemistry*[*].

THE existence of salt deposits in Cheshire, and of the brine-springs which flow from them, has from the earliest historic periods exercised a very peculiar influence upon the economic features of that county. It appears from the record of Domesday, that salt was even then one of the principal articles of its commerce, and that at a period anterior to the Norman Conquest it brought in a considerable revenue to the Crown; for as early as the time of Edward the Confessor, the Wiches, as they were called, are stated to have been very productive, and the tolls levied upon the amount of salt sold were divided in the pro-

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[*] Communicated by the Author.

portion of two-thirds to the king and one-third to the Earl of Chester. Upon the Conqueror's accession, the earldom was given to Hugh Lupus, his nephew; but the property attached to it had diminished in value; for it is recorded that the salt-works at Middlewich and Nantwich, which under the Saxon rule had produced a rental of £16 per annum, had fallen into complete disuse; and that those at Nantwich, from which an annual income of £20 had been derived, were almost as much neglected, for out of eight salt-works which had formerly flourished at this latter place, one only was at that time in operation. This period of depression was not, however, of long duration, for shortly afterwards a partial recovery had taken place, since the Nantwich salt-works are spoken of as being let to farm by the Crown at £10, the Middlewich at 25 shillings, and the Northwich at 35 shillings per annum*. It is thought probable that at this period the chief export of Cheshire salt was to Wales, for the Welsh gave to Nantwich the name of Hellath Wen, or the white salt-pit; much export trade could not have been carried on, for very long after this date, the salt manufacture of Cheshire did not exceed the consumption of the county itself, and a few of its immediately adjoining neighbours†.

It is curious to observe how the importance of places decreases in course of time through the effect of influences, at first apparently but slightly adverse, becoming under somewhat altered circumstances highly detrimental. Nantwich throughout the whole of its early history held the first rank among the salt-producing towns;—there were situated the brine-pits which Henry III. stopped up in order to distress the Welsh by cutting off their supply of that necessary article of food; and when this embargo upon the commercial activity of the town was withdrawn, we find it far surpassing its former self in the energy of its undertakings. This increasing prosperity and wealth continued until it reached a culminating point, from which it has ever since gradually but irretrievably descended. It seems to have attained its summit in the time of Henry VIII., for Leland states that at that time Nantwich contained 300 salt-works: their reduction then began, and in the early part of the reign of Queen Elizabeth their number had decreased to 216, whilst in 1624 they had dwindled nearly to 150. The cause of this declension was want of water-carriage, Nantwich being unfortunately situated beyond that point of the Weaver up to which it was navigable; a more advantageous locality was therefore sought for; the banks of the river were examined, and other springs found in more favoured situations, possessing moreover,

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* Lysons' *Magna Britannia*, vol. ii. part 2. p. 408 (1810).

† Ibid. pp. 408, 409.

in addition to superiority of position, the no less important advantage of greater concentration. From the date of that discovery the salt manufacture in Nantwich steadily declined, although the inhabitants still repaired annually on Ascension day to the "Old Biat," their most ancient salt-pit, and adorning it with flowers, ribands, and green boughs, they danced round it to their rustic music, and sung a hymn of thanksgiving for the blessing of the brine. This old custom was discontinued about the middle of the last century, for the blessing which they celebrated was fast leaving them. In 1810* one salt-work only existed in the town, and that is now, I am told, done entirely away with and the pit closed. But while Nantwich has been thus descending in the scale, Middlewich and Northwich have been increasing in importance; and along the lines of water communication, the rivers Weaver and the Grand Trunk Canal which runs parallel with the river Wheelock, other places have sprung up from time to time. Of these the most important appear to be,—Winsford on the Weaver considerably below Nantwich, Marston and Winnington on the same stream in the immediate neighbourhood of Northwich, and Anderton somewhat nearer the river's mouth; whilst on the Wheelock, in the vicinity of Sandbach, several works of great consequence have been established. A few statistics will show the immense increase which this branch of manufacture has undergone within a comparatively short period. Within the ten years from 1800 to 1810 the amount of salt produced in Northwich is said to have doubled, and the annual average of white or manufactured salt sent down the Weaver from Winsford and Northwich during that period was 139,317 tons †; this appears to have been the entire amount shipped on the Weaver, and is therefore comparable with the total quantity of white salt ‡ (as distinguished from rock-salt) which was carried down that river in the year 1832, which amounted to 383,669 tons, and which by the year ending April 5, 1856, had increased to 709,514 tons. With this, other accounts agree; for Lyson states that in 1805–1806 the total annual produce of the Cheshire brine-pits, those of Nantwich and Frodsham excepted, was 16,590 tons; whilst at the present time, single manufacturers, such as Mr. Blackwell of Wheelock, and Messrs. Kay of Winsford, produce respectively 70,000 tons, and from 50,000 to 60,000 tons of salt per annum.

The salt which is now produced in Cheshire is made, I understand, exclusively from natural brine-springs; but it has formerly at various times been obtained by dissolving rock-salt in water

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* Lysons' *Magna Britannia,* vol. ii. part 2. p. 703.

† Lysons' *Magna Britannia.*

‡ Ure's Dictionary of Arts and Manufactures, 3rd edit. p. 1091.

and evaporating the clear solution. The principal varieties manufactured are three: the salt of coarsest grain, or bay-salt; that of the finest grain, to which the name of table-salt is applied; and an intermediate variety, which is called common salt. The latter is made in the largest quantity, and forms the principal part of the vast export of Cheshire. More than three-fourths of the total amount of salt produced is used for foreign consumption; and of the remaining fourth, a considerable portion is employed in the supply of the British fisheries. It has, however, only attained its reputation after a somewhat severe struggle with foreign competitors; for in the year 1810, Mr. Henry* found it necessary to set forth an apology for British salt as an agent in curing provisions in no way inferior to that prepared from sea-water by evaporation on the shores of the Mediterranean, and to deprecate the folly of Great Britain in expending large sums of money in the purchase of an article, which she possessed the means, beyond almost any other country in Europe, of drawing from her own internal resources. Fortunately these remonstrances were not addressed in vain, and this country soon ceased to neglect her native produce, and to import that from abroad which she had in such abundance and of such excellent quality at home.

The great means, however, of extending the salt trade in this country, has been the gradual improvement in the mode of raising and evaporating the brine. Originally, in the early days of the manufacture, the method of obtaining salt from the brine by evaporation was unknown; and its preparation consisted only in pouring the brine upon burning branches of oak and hazel, from the ashes of which the deposited salt was afterwards collected. At length, however, the plan of evaporation was devised; but for a long time wood was the only fuel, of which such immense quantities were consumed at the salt-works in Droitwich in the time of Camden, that he represents Feckenham Forest and the neighbouring woods as becoming perceptibly thinner and thinner day by day. This havoc seems to have been continued notwithstanding until the middle of the seventeenth century, when the gradual introduction of coal superseded the more primitive kind of fuel†. In the methods adopted in raising the brine, great alteration also has taken place in the lapse of time; at Northwich, in Camden's time, a pit existed which furnished an abundant supply, but the way in which it was brought to the surface was crude in the extreme: the pit was provided with stairs, by means of which men descended with leathern buckets; these they filled with the water, and then ascending emptied their contents into troughs,

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* Phil. Trans. Royal Soc. 1810, p. 89.

† Holland's General View of the Agriculture of Cheshire, p. 71.

which served as reservoirs for the wich-houses*. From manual labour they passed to the employment of horses for this purpose; water-power and windmills were subsequently used; but all have been superseded of late years by the superior efficiency of the steam-engine. The methods of evaporation have also undergone vast extension and improvement. In early times this process was conducted in small leaden vessels, six of which they had in every house in Nantwich, and the salt was removed by women with little wooden rakes, placed in baskets, and drained†. These six leaden pans were afterwards exchanged for four iron ones, about 6 inches in depth and of a surface of about a square yard, capable of holding the same contents as the original leaden vessels. The limited extent of the operations thus conducted diminishes our wonder at the great proportion of wich-houses existing at these places during the middle ages, and so far surpassing the number in operation at the present day. Even so recently as a century ago, the largest pans at Northwich were only 20 feet long by 9 or 10 broad; whilst those employed forty years since had a superficies of 600, 800 and 1000 feet, with a depth of from 16 to 18 inches. The area of the pans has now, I suppose, almost reached its limit; some which I saw at Mr. Blackwell's works at Wheelock having a length of 70 feet and a width of 23 feet, making 1610 feet of surface. The heat is generally applied directly by the flue of from one to three furnaces, placed at one extremity of the pan, but in the works of Messrs. Kay of Winsford, I found part of the evaporation conducted upon a different principle: a small iron pan, heated by a furnace in the ordinary way, is made to communicate by a narrow channel, with a brick- or clay-lined basin; this again is in connexion with others of the same description disposed around a centre, and lastly, one is arrived at immediately adjoining the first-mentioned iron pan; the brine is here by a very simple kind of pump transferred to the heated iron vessel, by which means the level of the liquid in the brick basins is kept constantly below that in the original starting-place, and thus a continual circulation of the brine is maintained.

In the preparation of salt from brine, various substances have been at different times added from the idea of improving the quality of the product. Until recent times it was thought, that during the evaporation of an aqueous solution of chloride of sodium, hydrochloric acid was expelled, and soda formed: this doubtless arose from the decomposition of the chloride of magnesium contained in the brine with which the experiments were made,—an evolution of acid vapours having been probably observed during the

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* Holland's Agriculture of Cheshire, p. 48.

† Ibid. p. 50.

incipient drying of the salt. In order to counteract this supposed evil, acids were added, and it was imagined that the excellence of the Dutch salt was due to a skilful admixture of whey which the manufacturers were alleged to make with their brine during the evaporation, which prevented the injurious effects of the free alkali. Another very favourite class of adjuncts has always consisted of substances which contain constituents possessed of the property of coagulating upon the application of heat, which clarify a liquid by entangling all suspended particles of solid matter in the meshes of their coagulum, and carry them with it as it rises to the surface. Those substances which contain albuminous or gelatinous matters are peculiarly adapted for this purpose, and are constantly used in a variety of manufactures for the attainment of this end. Blood, white of egg, glue and cows' or calves' feet have long been used in this way in salt-making. In 1670 the Nantwich salt-makers are described as mixing twenty gallons of brine with two quarts of blood, and adding about two quarts of this clarifying liquid to a pan which held 360 quarts of brine. In 1810 this process was still adopted in some of the Cheshire works, but I am not aware that it is ever used at the present day. At Droitwich the use of white of egg seems to have prevailed. Various vegetable infusions, still containing albumen, as linseed mucilage and ale, were, in the early days of the manufacture, in great repute, but have, I believe, now fallen almost entirely into disuse.

In order to prevent that most unpleasant circumstance attendant upon the evaporation of all concentrated saline solutions,—the formation of a pellicle upon the surface of the liquid, which gradually becomes a thick layer of salt, and seriously impedes evaporation—another class of bodies are mixed with the brine; these are oils or butter, which, spreading over the whole area, by a peculiar molecular action prevent the formation of any pellicle, or "setting over" of the pan, as the workmen term it, and preserve that open surface which is most favourable to evaporation. The action of finely-powdered resin in effecting this is perfectly magical, the introduction of a very few grains being quite sufficient instantly to clear the surface of the largest pan, and to prevent any recurrence of the formation of the pellicle. This substance was also formerly thought to perform another function, viz. the production of a salt of finer grain; wheat-flour was also believed to exert the same action, whilst alum was added in order to facilitate the formation of larger crystals; but I believe it is now found that the regulation of the degree of heat employed in the evaporation will influence the size of the crystals with the utmost nicety, and that attention to that circumstance alone is sufficient to produce all the varieties which are found in the market. In Holland's 'General View of the Agriculture of

Cheshire,' to which I am so much indebted, and to which such frequent reference has been made, will be found a list of the varieties produced at diferent temperatures; according to it, the finest table-salt is deposited from the brine at its boiling temperature (226° F.), and the coarsest description by slow evaporation conducted at from 100° to 110° F.

The derivation of the brine in Cheshire is too well known to require any observation: the immense beds of rock-salt which occur in the new red sandstone of that county are familiar to every one. The existence of this substance appears to have first evidenced itself by the saline springs which at various places rose to the surface; and the majority of these spontaneous appearances seem to have occurred in the immediate vicinity of the course of the river Weaver, and of the lesser stream, the Wheelock. The former of these rises in the south-west portion of the county, and after running south for some miles, turns at Audlem to the north, passes Nantwich, and after some distance, Winsford; receives the Wheelock about Northwich, and passing Marston and Anderton, proceeds to the Mersey. The Wheelock rises in the south-east part of Cheshire, and passing the village of the same name, flows by Middlewich to its confluence with the Weaver. Along the banks of these streams a continual succession of places occurs, which have at various times been famous for the production of salt; and although great fluctuations have taken place, yet these have been due rather to incidental causes than to any failure of material;—imperfections in shafts allowing the admission of freshwater springs, and a slight distance from the convenience of water-carriage are reasons quite sufficient for the transfer of the manufacture from one place to another. Causes such as these probably led to the abandonment of the works at Dunham in the north-east of the county, and of those at Dirtwich in the south-west; yet the occurrence of brines at these places is interesting, as indicative of the position and extent of the beds of salt below, the two places being about thirty miles apart, and about equidistant from the centre of the district which is now the salt-producing one. This region may be said to lie north-west and south-east, and to be composed of three divisions,—the Northwich, the Middlewich, and the Sandbach: the first comprehends Northwich, Marston and Anderton, besides various other places near them; the second embraces Middlewich and Winsford, with the surrounding neighbourhood; and the third, of more limited extent with respect to actual operation, includes Sandbach, Wheelock, and a few villages in the immediate vicinity. In order to obtain fair samples of the Cheshire brines, I therefore took specimens from each of the above districts. From the Northwich district I obtained two, from springs a few miles apart, through the kindness of Mr. Johnson Fletcher; the places selected in this instance were Anderton and Marston; proceeding in the south-easterly direction, a specimen was chosen from the active department of Winsford, in the central district, and Messrs. Kay and Son furnished me with the brine which they employed; whilst in the extreme south-east, Mr. Blackwell provided me with samples from the spring which supplies his extensive works at Wheelock.

The depth at which the brine is found, and the level to which it rises, vary very much at different places. It appears that it is generally necessary to sink from thirty to fifty yards, in some cases even to a depth of eighty yards, before the spring is arrived at; the water then rises in the shaft to within from twenty to ten yards of the top, sometimes even to the surface. The general level of the brine in the pits is, however, far below this standard, as its removal by the pumps is so rapid as never to allow it to rise to its full height.

As in the case of the Worcestershire brines, I was desirous of ascertaining whether the composition of these springs varied with the different seasons of the year; with this view analyses were made in every instance of separate specimens taken respectively in January and August, and, as I had already found in the case alluded to, no difference worthy of note existed between them.

The analytical methods which have been adopted in the examination of these brines, are precisely those described in the former memoir on the Worcestershire springs; instead, however, of evaporating small portions of the waters to obtain mother-liquors and residues which might contain the rarer constituents, I obtained portions of the liquid which remains in the pans after the removal of the salt, and of the solid cake of earthy matter which adheres so tenaciously to the bottom of the pan as to require separation by the pick, and is called by the workmen "pan-scale." The former of these was tested for potassium, bromine, iodine and phosphoric acid; and the latter for arsenic, antimony, tin, iron, manganese, aluminium, strontium and fluorine. The brine itself was also examined for silica, for organic matter and its resultants, ammonia and nitric acid, and the metals precipitable by hydrosulphuric acid. The principal constituents of these waters are sodium, calcium, magnesium, chlorine and sulphuric acid; of the bodies occurring in less quantity,—a minute trace of potassium was found, a small quantity of iron with a little alumina and considerable traces of manganese, not, however, in proportions capable of determination. Bromine was ascertained to exist in rather large quantity, the unconcentrated brines becoming distinctly yellow upon the passage of a few bubbles of chlorine; this element was therefore determined by the method recommended by Fehling*, which depends upon the fact, that if a solution of chloride of sodium containing a small amount of bromide be insufficiently precipitated by nitrate of silver, the precipitate nevertheless contains all the bromine present; the bromine in the mixed precipitate of chloride and bromide of silver is then determined by the passage of chlorine over the fused mass in the usual way. The results yielded by this process, which were carefully and frequently repeated upon very different quantities of brine, agreed among themselves in the closest manner. The existence of iodine was proved in all the waters, but the quantity present was minute in the extreme. In one case also an indication of fluorine was obtained by the apparently etching action exerted upon a watch-glass in the application of the test now usually employed: these markings, however, on the parts uncovered by wax, although quite obvious when the newly-cleaned glass was breathed upon, were perfectly imperceptible when the same watch-glass was taken up a day or two later and re-wiped for the purpose of fresh examination†. Since that experiment, M. Nicklès' observations‡ on the fallacies incidental to this method of testing have appeared; and this doubtless was a case such as he describes, in which the vapour of any acid, or even of water, may exert such an action (not chemical, I presume, but physical), and fix upon glass any design at first traced upon the layer of wax. The search for the remaining substances above mentioned was unsuccessful, and they were therefore presumed to be absent.

I will now proceed to state the results which the analysis of these brines has afforded me, premising them simply by naming the sources from which they were derived. I. Anderton brine, from the pit of Lord Stanley of Alderley. II. Marston brine, from the works of the executors of the late C. W. Newman, Esq. III. Winsford brine, from the shaft of Messrs. Kay and Son. IV. Wheelock brine, from the works of Mr. Blackwell.

I. *Anderton Brine.*

Specific gravity (August) . .	1·2048
Temperature of brine . . .	...
Reaction	feebly alkaline.

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* Fresenius, Quant. Analysis, p. 345. 2nd Engl. edit.

† I accounted for this at first by imagining that a thin film of wax might be impressed by the needle upon the glass, which was not immediately removeable by the first cleansing.

‡ *Comptes Rendus*, March 30, 1857, p. 679.

Table of direct results of analysis, calculated to 100 parts.

	January 1856.			August 1856.		
	I.	II.	Mean.	I.	II.	Mean.
Potassa	......	......	......	......	......	trace
Soda	......	13·649	13·649	13·655	13·657	13·656
Lime	·190	·190	·190	·189	·187	·188
Magnesia	·035	·035	·035	·037	·036	·036
Protoxide of manganese	......	......	......	......	......	trace
Sesquioxide of iron	......	......	......	......	......	trace
Alumina	......	......	......	......	......	trace
Chlorine	15·623	15·629	15·626	15·619	15·618	15·618
Bromine				·0081	·0080	·0080
Iodine	......	......	......	......	......	trace
Sulphuric acid	·267	·267	·267	·266	·269	·267
Phosphoric acid	......	......	......	......	......	trace
Residue obtained by direct evaporation	26·205	26·245	26·225	26·272	26·276	26·274

Table of constituents, calculated in 100 parts.

	January.	August.
Chloride of sodium	25·749	25·736
Bromide of sodium		·0103
Iodide of sodium	...	trace
Sulphate of potassa	...	trace
Sulphate of lime	·454	·454
Carbonate of soda	·009	·023
Carbonate of lime	·007	·002
Carbonate of magnesia	·073	·075
Carbonate of oxide of manganese	...	trace
Phosphate of lime	...	trace
Phosphate of sesquioxide of iron	...	trace
Alumina	...	trace
	26·292	26·3003
Solid residue by direct evaporation.	26·225	26·274

Table of constituents, calculated to grains in the gallon.

	August.
Chloride of sodium	21704·712
Bromide of sodium	8·686
Sulphate of lime	382·885
Carbonate of soda	19·397
Carbonate of lime	1·686
Carbonate of magnesia	63·252
	22180·618

II. *Marston Brine.*

Specific gravity (August) . . .	1·2001
Temperature of brine	53°·6 F.
Reaction.	feebly alkaline.

Table of direct results of analysis, calculated to 100 parts.

	January 1856.			August 1856.		
	I.	II.	Mean.	I.	II.	Mean.
Potassa	......	......	......	......	......	trace
Soda	13·378	13·362	13·370	13·452	13·455	13·453
Lime	·176	·160	·168	·160	·162	·161
Magnesia	·051	·051	·051	·050	·051	·051
Protoxide of manganese	......	......	......	......	......	trace
Sesquioxide of iron	......	......	......	......	......	trace
Alumina	......	......	......	......	......	trace
Chlorine Bromine	15·228	15·220	15·224	15·293 ·0085	15·319 ·0087	15·306 ·0086
Iodine	......	......	......	......	......	trace
Sulphuric acid	·315	·320	·317	·313	·312	·313
Phosphoric acid	......	......	......	......	......	trace
Residue obtained by direct evaporation	25·769	25·772	25·770	25·872	25·868	25·870

Table of constituents, calculated in 100 parts.

	January.	August.
Chloride of sodium	25·087	25·222
Bromide of sodium		·011
Iodide of sodium	...	trace
Sulphate of potassa	...	trace
Sulphate of soda	·137	·146
Sulphate of lime	·408	·391
Carbonate of soda	·028	·036
Carbonate of magnesia	·107	·107
Carbonate of oxide of manganese .	...	trace
Phosphate of lime	...	trace
Phosphate of sesquioxide of iron .	...	trace
Alumina	...	trace
	25·767	25·913
Solid residue obtained by direct evaporation	25·770	25·870

Table of constituents, calculated to grains in the gallon.

	August.
Chloride of sodium	2118·7234
Bromide of sodium . . .	9·240
Sulphate of soda	122·650
Sulphate of lime	328·467
Carbonate of soda	30·242
Carbonate of magnesia . .	89·887
	21767·720

III. *Winsford Brine.*

Specific gravity (August) . .	1·2049
Temperature of brine	55°·4 F.
Reaction	slightly alkaline.

Table of direct results of analysis, calculated to 100 parts.

	January 1856.			August 1856.		
	I.	II.	Mean.	I.	II.	Mean.
Potassa	......	......	......	......	......	trace
Soda	13·790	13·793	13·791	13·548	13·550	13·549
Lime	·194	·190	·192	·188	·190	·189
Magnesia	·086	·090	·088	·089	·087	·088
Protoxide of manganese	......	......	......	......	......	trace
Sesquioxide of iron	......	......	......	......	......	trace
Alumina	......	......	......	......	......	trace
Chlorine }	15·782	15·774	15·778	15·485	15·495	15·490
Bromine }				·0122	·0124	·0123
Iodine	......	......	......	......	......	trace
Sulphuric acid	·262	·267	·264	·266	·264	·265
Phosphoric acid	......	......	......	......	......	trace
Residue obtained by direct evaporation	26·552	26·553	26·552	26·061	26·044	26·052

Table of constituents, calculated in 100 parts.

	January.	August.
Chloride of sodium . . . . }	26·000	25·525
Bromide of sodium . . . . }		·0158
Iodide of sodium	...	trace
Sulphate of potassa	...	trace
Sulphate of lime	·449	·450
Carbonate of soda	·023	·032
Carbonate of lime	·012	·006
Carbonate of magnesia	·184	·184
Carbonate of manganese	...	trace
Phosphate of lime	...	trace
Phosphate of sesquioxide of iron .	...	trace
Alumina	...	trace
	26·668	26·2128
Solid residue obtained by direct evaporation . . . . . . }	26·552	26·052

Table of constituents, calculated to grains in the gallon.

	August.
Chloride of sodium	21528·550
Bromide of sodium	13·326
Sulphate of lime	379·543
Carbonate of soda	26·989
Carbonate of lime	5·060
Carbonate of magnesia	155·191
	22108·659

IV. *Wheelock Brine.*

Specific gravity	1·2013
Temperature of brine	54° F.
Reaction	feebly alkaline.

Table of direct results of analysis, calculated to 100 parts.

	January 1856.			August 1856.		
	I.	II.	Mean.	I.	II.	Mean.
Potassa	......	......	......	......	......	trace
Soda	13·648	13·631	13·639	13·429	13·435	13·432
Lime	·214	·193	·203	·200	·201	·201
Magnesia	·113	·105	·109	·122	·120	·121
Protoxide of manganese	......	......	......	......	......	trace
Sesquioxide of iron	......	......	......	......	......	trace
Alumina	......	......	......	......	......	trace
Chlorine }	15·712	15·699	15·705	15·492	15·512	15·502
Bromine }				·0157	·0157	·0157
Iodine	......	......	......	......	......	trace
Sulphuric acid	·253	·265	·259	·244	·248	·246
Phosphoric acid	......	......	......	......	......	trace
Residue obtained by direct evaporation	26·447	26·464	26·455	26·068	26·074	26·071

Table of constituents, calculated in 100 parts.

	January.	August.
Chloride of sodium	25·737	25·333
Bromide of sodium		·0202
Iodide of sodium	...	trace
Chloride of magnesium	·109	·171
Sulphate of potassa	...	trace
Sulphate of lime	·440	·418
Carbonate of lime	·039	·052
Carbonate of magnesia	·150	·107
Carbonate of oxide of manganese	...	trace
Phosphate of lime	...	trace
Phosphate of sesquioxide of iron	...	trace
Alumina	...	trace
	26·475	26·1012
Solid residue obtained by direct evaporation	26·455	26·071

Table of constituents, calculated to grains in the imperial gallon.

	August.
Chloride of sodium . . .	21302·773
Chloride of magnesium . .	143·795
Bromide of sodium . . .	16·986
Sulphate of lime	351·500
Carbonate of lime	43·727
Carbonate of magnesia . .	84·977
	21948·758

Comparative Table.

Solid constituents of Cheshire Brines in 100 parts.

August 1856.

	Anderton.	Marston.	Winsford.	Wheelock.
Chloride of sodium	25·736	25·222	25·525	25·333
Bromide of sodium	·0103	·011	·0158	·0202
Iodide of sodium	trace	trace	trace	trace
Chloride of magnesium	......	......	......	·171
Sulphate of potassa	trace	trace	trace	trace
Sulphate of soda	......	·146	......	......
Sulphate of lime	·454	·391	·450	·418
Carbonate of soda	·023	·036	·032	·052
Carbonate of lime	·002	......	·006	......
Carbonate of magnesia	·075	·107	·184	·107
Carbonate of manganese	trace	trace	trace	trace
Phosphate of lime	trace	trace	trace	trace
Phosphate of sesquioxide of iron ...	trace	trace	trace	trace
Alumina	trace	trace.	trace	trace
	26·3003	25·913	26·2128	26·1012
Solid residue by direct experiment.	26·274	25·870	26·052	26·071

Towards the latter part of the autumn of last year, a considerable panic was created by a sudden outcry being raised that the supply of brine was rapidly diminishing over the whole of Cheshire. I made at the time many inquiries as to the truth of this statement, which had found its way into the public journals, and it proved, as is usual in such cases, that the account, although based upon truth, was very much exaggerated. A diminution in the supply of brine is a phænomenon of no unfrequent occurrence, and is in fact of two kinds—periodical and occasional. A periodical sinking of the water in the brine-shaft is found to occur in the summer season, the brine rising with the approach of winter, and attaining its highest level about December or January: these alternations have generally been very regular, but during the year 1856 a total departure from the usual variations took place, which, ending in a rapid sinking of the brine during the months of November and December, originated the fears which were entertained of a general failure being at hand. I have been enabled to obtain a statement of the fluctuations in the level of the brine in a pit at Anderton, during the two normal years 1854–55, and the abnormal year 1856, by which the matter is clearly exemplified. The shaft upon which the observations were made, was between 70 and 80 yards deep, and the numbers given express the height in yards to which the brine rose.

	1854.	1855.	1856.
January	15—16	30	24—30
February......		35	23—24
March.........	10—7—35—20	gradually receding	18—24
April	gradually receding		18—19
May			17—19
June			17½—19
July	6	4	17—19½
August	7	4	19—19½
September ...	7	4	19—20
October	8—20	increasing	20—22
November ...	20—25	22	18—11
December ...	30	30	10—11

By this account it is seen that in ordinary years the average depth of brine in the shaft is thirty yards in the depth of winter, whilst in the midst of summer five yards may be taken as the mean; and the origin of the alarm in 1856 was evidently the observation of a diminution setting in at a period at which an increase had usually happened hitherto. The fact, however, appears to have been disregarded, that in the warmer period of the year the supply of brine had been far more abundant than was generally the case, nor am I aware that any attempt was made to explain this part of the phænomenon. The sudden decrease was attributed by those on the spot best able to form an opinion, to one of those dislocations of strata which frequently on a smaller scale cause the occasional failures in the supply of brine: a sudden subsidence of land in one part of the Northwich district, to the depth of a three-storied building, is described as having occurred simultaneously with this sinking of the brine, and it was believed that in some of the deserted mines which honeycomb the rock-salt strata of that region, the pillars of salt, which are always left to support the roof, had been removed by the action of water, and the immense weight of the superincumbent mass had then caused the excavation to collapse. By an extensive disarrangement of this nature, either some new channel of escape might have been opened for the brine, or at least the communication between some of the streams or reservoirs might have been closed or interrupted. Similar occurrences, although with a converse effect, have happened in earlier times; and two instances are mentioned by Holland, in one of which, near Bickley, and also in the neighbourhood of Combermere Abbey, the ground suddenly sank to a depth of many feet, and the brine not being removed with the same rapidity as in the present day, forced its way to the surface upon its old subterranean channel or reservoir being filled up, and formed a pool of considerable size in the depression caused by the collapse of the strata.

The failure of 1856 appears to have been partial only, as would be naturally expected if due to the alleged cause; it seems to have been almost wholly confined to the Northwich district: at Winsford it was slightly felt, but was there attributed more to increased pumping than to any natural cause; whilst in the Sandbach department I believe no complaint of any kind was raised. The derangement, such as it was, seems to have been only temporary, for no complaints have since been made of the occurrence of any scarcity.

LIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 397.]

March 26, 1857.—Major-General Sabine, R.A., Tr. and V.P., in the Chair.

THE following communication was read:—

“On an Element of Strength in Beams subjected to Transverse Strain, named by the author ‘The Resistance of Flexure.” By William Henry Barlow, Esq., F.R.S.

In his former paper on this subject the author pointed out the existence of an element of strength in beams when subjected to transverse strain,—the resistance of flexure—which had been omitted in the generally received theory; and the object of the present experimental inquiry is to elucidate more clearly the general bearing of the subject, and determine more precisely the laws which govern this resistance.

The forms of beam employed in the experiments formerly described were only of two kinds—solid rectangular bars and open girders; in the present experiments other forms have been used, namely, square bars broken on their sides, square bars broken on their angles, round bars, beams of the **I** section broken with the flanges horizontal, and similar beams broken with the flanges vertical ⊢⊣.

The results of these experiments are exhibited in Tables, together with those of the former series; and the author employs them, in the first place, to test the accuracy of the existing theory, by comparing the resistance of the outer fibres or particles of each of the forms of beam, calculated on that theory, with the actual tensile strength of the metal as obtained by direct experiment. From this comparison applied to the different forms of beam, it would follow that the resistance at the outer fibre varies from 25,271 lbs. to 53,966 lbs., while the tensile strength of the metal, obtained by experiments on direct tension, averages only 18,750 lbs.; and the discrepancy and variation will be found to arise from the received theory not taking into account the resistance consequent on the molecular disturbance accompanying curvature.

In his former paper the author gave a formula by which the difference between the tensile strength and the apparent resistance at the outer fibre could be computed, approximatively, in solid rectangular beams and open girders; and he now proposes to trace the operation of the resistance of flexure, considered as a separate element of strength, and to show its effect, in each of the forms of section above indicated. Observing that the usual supposition of only two resistances in a beam, tension and compression, fails to account either for the strength, or for the visible changes of figure which take place under transverse strain, he proceeds to discuss the effects involved in such change of figure, and thence arrives at the following conclusions applicable to the resistance of flexure:—

1. That it is a resistance acting in addition to the direct extension and to compression.

2. That it is evenly distributed over the surface, and consequently (within the limits of its operation) its points of action will be at the centres of gravity of the half-section.

3. That this uniform resistance is due to the lateral cohesion of the adjacent surfaces of the fibres or particles, and to the elastic reaction which thus ensues between the portions of a beam unequally strained.

4. That it is proportional to and varies with the inequality of strain, as between the fibres or particles nearest the neutral axis and those most remote.

Formulæ are then given, according to these principles, exhibiting the relation between the straining and resisting forces in the several forms of section experimented on, as resulting from the joint effect of the resistances of tension, compression and flexure. The application of these formulæ to the actual experiments yields a series of equations with numerical coefficients, in which, were the metal of uniform strength, the tensile strength f, and the resistance of flexure ϕ, would be constant quantities, and their value might be obtained from any two of the equations; but as the strength varies even in castings of the same dimensions, and as a reduction of strength per unit of section takes place when the thickness is increased, the values of f and ϕ will necessarily vary, and can only be ascertained in each experiment by first establishing the ratio they bear to each other. For this purpose the first ten experiments are used, in all of which the metal was from $\frac{3}{4}$ to 1 inch in thickness, and its mean tensile strength ascertained by direct experiment to be 18,750 lbs. per inch. The resulting mean value of ϕ is $=$ 16,573 lbs., and the ratio of f to ϕ as 1 to ·847.

By using results obtained by Prof. Hodgkinson on the breaking weight of inch bars of ten different descriptions of iron, where the tensile strength was ascertained by direct experiment, it would appear that the ratio between the resistance of tension and the resistance of flexure varies in different qualities of metal, an inference which seems to be confirmed by other experiments on rectangular bars given in the Report of the Commissioners on the application of iron to railway structures. The mean result, however, accords nearly with that of the author's experiments, and gives the ratio of f to ϕ as 1 to ·853. Hence, according to these data, the resistance to flexure, computed as a force evenly distributed over the section, is almost nine-tenths of the tensile resistance.

This ratio of the values of f and ϕ being applied to the equations resulting from the several experiments, gives the tensile strength of the metal as derived from each form of section, and the results, though not perfectly regular, are found to be within the limits of the variation exhibited by the metal as shown by the experiments on direct tension in the former paper. Classified and condensed, these results are as follows :—

The mean tensile strength as obtained from

The open girders, is	18,282
The solid rectangular bar of 2 inches sectional area	17,971
The inch bars—square and round, and square broken diagonally	19,616
The bars of 4 inches sectional area, square and round, and square broken diagonally	16,800
The compound sections in which the metal was $\frac{1}{2}$ inch thick	19,701

Having thus found that his formulæ, when applied to his own experiments, gave consistent and satisfactory results, the author next tested them by other known experiments, and especially refers to those by Major Wade on the transverse strength of square and round bars of cast iron of different qualities, related in the "Reports on the Strength and other Properties of Metals for Cannon," presented to the United States Government by the Officers of the Ordnance Department. The unit of strength, as computed by Major Wade from these experiments, came out uniformly much higher in the round than in the square bars of the same kind of iron, whence he was led to doubt the correctness of the formula employed ; but the author shows that when his formula is used, which includes the resistance of flexure, the discrepancy referred to disappears, and the tensile resistance, whether obtained for the round or the square bars, agrees very nearly with that derived from the experiments on direct tension under like circumstances.

As to the ratio between the resistance of flexure and the tensile resistance, it is remarked that, were the metal homogeneous, the former resistance would probably be precisely equal to the latter, instead of bearing the ratio of nine-tenths, as found by experiment ; but the ratio evidently varies in different qualities of metal ; and accordingly from Major Wade's experiments, it appears that with the same metal subjected to different modes of casting, an increase of transverse strength may accompany a decrease in the tensile resistance.

Respecting the limit of action of the resistance of flexure, the author observes, that in all the simple solid sections, the points of action are evidently the centres of gravity of the half-section; while in the compound sections it is necessary to compute the centre rib and flanges as for two separate beams in which the resistance of flexure is different, and has its point of action at the centre of gravity of the separate portions. It would appear that the elastic reaction developes this resistance to the full extent when the section is such that a straight line may be drawn from every point at the outer portion to every point at the neutral axis within the section; but that if the form of section is such that straight lines drawn from the outer fibres or particles to the neutral axis fall without the section, then it must be treated as two separate beams, each having that amount of resistance of flexure due to the depth of the metal contained in it.

The last section of the paper is devoted to the consideration of the resistance of flexure in wrought iron; and experiments are first given to determine the position of the neutral axis, from which it is found to be at the centre of gravity of the section, as in cast iron; so that the action is the same in both materials, except as to the amount of the extensions and compressions with a given strain; and the formulæ given for cast iron will also apply to wrought iron. As wrought iron yields by bending and not by fracture, the relative value of f and ϕ are not so easily ascertained; moreover the ultimate compressive strain which wrought iron can sustain is little more than half its ultimate tensile strength; nevertheless the force required to overcome the elasticity of the material is nearly the same, whether applied as a compressive or tensile strain; the difference being, that the force which overcomes elasticity when applied as a compressive strain leads to the destruction or distortion of the material, while, in the case of the tensile strain, the elasticity may be overcome long before the material yields by absolute rupture.

A statement is given of the results of experiments made by Professor Barlow, in 1837, to show the weights which overcome the elasticity of the metal when applied transversely as compared with the weight necessary to produce the same result when applied by direct tension, and from these it is concluded that the resistance of flexure in wrought iron, considered as a force acting evenly over the surface, is nearly equal to one-half of the tensile resistance.

In an Appendix to this paper, by Professor Barlow (read at the following meeting), the preceding principles are applied to beams and rafters of non-symmetrical section.

With this view, the case of the double-flanged girder with unequal flanges is selected and discussed, and formulæ deduced, which are then tested by comparison with the results of experiments by Prof. Hodgkinson, published in the 'Manchester Memoirs;' a selection being made of those in which the girders differed most from each other in section, dimensions, and bearing-distance. The chief particulars of these experiments are given, with diagrams showing the forms of sections, and the values as obtained from the formulæ are stated. The value of the direct tensile strength of cast iron thus derived, falls between the limits of 1400 and 1700.

In the Reports of the Commissioners of Inquiry into the "Application of Iron to Railway Structures," are given the results of about fifty experiments on the direct tensile resistance of one-inch square cast-iron bars, under the direction of Professor Hodgkinson. The bars consisted of seventeen different kinds of iron, each set of bars being of the like quality and manufacture; and in several of these sets, which might have been expected to yield the same results, the difference is fully as great as in the cases here exhibited. From this fact an inference may be drawn in favour of the general applicability of the principles developed in the foregoing pages to cast-iron beams and girders of every variety of section.

April 2.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"Researches on Silica." By Colonel Philip Yorke, F.R.S.

This communication is principally devoted to an attempt to determine the formula of silica, and to the relation of some remarkable results obtained in this research. After giving some account of the grounds on which the three different formulas now in use among chemists (viz. SiO_3, SiO_2, and SiO) had been advocated, the author proceeds to state, that it appeared to him that the direct method which had been followed by Rose deserved the preference. This method consists in determining the quantity of carbonic acid which is displaced from excess of an alkaline carbonate in fusion, by a given weight of silica. The number 22 being the equivalent for carbonic acid on the hydrogen scale, the equivalent of silicic acid is obtained by the proportion $x=\frac{22\times\text{weight of silica used}}{\text{weight of carbonic acid expelled}}$.

Four experiments are detailed, made with carbonate of potash, which give as a mean result the number 30·7 for the equivalent of silica. This agrees with the formula SiO_2, and nearly with the previous results of H. Rose. Then follow seven experiments made in a like manner with carbonate of soda, which give as a mean result the number 21·3 as the equivalent of silicic acid—a number agreeing nearly with half that represented by the formula SiO_3, or $\frac{45{\cdot}2}{2}$.

Some experiments are then related, which go to show that the increased loss resulting with carbonate of soda could not be caused by the action of heat alone.—The author had next recourse to carbonate of lithia, and obtained as the mean result of four experiments with this substance, agreeing well together, the number 14·99 —a number which accords very closely with the formula SiO. These different numbers, obtained with silica, led the author to inquire whether any other body acting as an acid produces similar results with the fused carbonates of potash and soda. With this view, experiments were made with dry sulphate of magnesia, as a substitute for sulphuric acid, with bi-borate of soda, for boracic acid, with alumina and sesquioxide of iron. Of these bodies only boracic acid gave results similar to those obtained with silicic acid. The other substances all gave the equivalent numbers usually assigned to them, equally with the carbonates of potash and soda.

Directing his attention then to determine whether the equivalent of silicic acid could be found in other volatile acids than the carbonic, the author relates some experiments made with the hydrates of potash and soda, but he explains that there are circumstances which render it much more difficult to obtain accordant numbers with these bodies than with the carbonates.

Six experiments made with hydrate of potash gave as mean result the same number as that obtained with the carbonate, viz. 30·8. But with hydrate of soda the mean of three experiments gave the number 17·2 as the equivalent of silicic acid,—a result approaching that previously obtained with carbonate of lithia.

A silicate of soda was formed by fusing together silica and carbonate of soda, in proportions indicated by the previous experiments—*i. e.* 23 silica to 54 carbonate of soda = 31 soda. The fused mass was crystallized. It was dissolved in water, and the solution evaporated *in vacuo* yielded a crystallized salt, which contained about 5 per cent. of carbonate of soda; when this, calculated as the ordinary 10-hydrated salt, was subtracted, the silicate agreed nearly with the formula $NaO.SiO_2+7HO$. A crystallized salt of like composition was obtained when hydrated silica was dissolved in a solution of caustic soda, the silica and soda being in the same proportions, *i. e.* 31 : 23. In these experiments it appears, therefore, that in the process of solution and crystallization a portion of soda is extruded. When exposed to a temperature of about 300° F., nearly all the water was driven off from these salts, less than 1 per cent. remaining.

The fused silicate of lithia in like manner, when treated with water, appeared to split up into different compounds.

After guarding himself from drawing any decided inference from the experiments recorded, the author concludes by observing that at present he can see no alternative but to admit of more than one equivalent for silicic acid (that is to say of more than one acid), the value of which is determined by circumstances,—such as the presence of water and the nature of the base to which it is presented. The existence of such different silicic acids has been already suggested by chemists on different grounds, particularly by Ebelman and Laurent, and lately by M. Fremy.

April 23.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"On the Magnetic Induction of Crystals." By Professor Julius Plücker of Bonn, For. Memb. R.S., Hon. M.R.I. &c.

The author commences by referring to his discovery of the peculiar action of magnets on crystalline bodies, and to the researches to which he was thereby led. With reference to the form in which he ennunciated the law regulating the action of a magnet on a uniaxal crystal—that the optic axis is attracted or repelled by the poles of the magnet—he disclaims any intention of assigning a physical cause to the phenomenon, or doing anything more than expressing the results of observation, which are *as if* such a force existed. In the case of crystals of a more complicated character, he was led, in the first instance, to assume the existence of two magnetic axes, possessing a similar character as to attraction and repulsion with the one axis of optically uniaxal crystals. But finding that the proposed law did not hold when the crystal was examined in all directions, and not solely along peculiar axes, he abandoned, nearly two years ago, a hypothesis respecting which serious doubts had arisen long before. For the hypothesis of one or two axes acted upon by the magnet, he substituted another similar hypothesis. In the case of uniaxal crystals he now conceived an ellipsoid of revolution, consisting of an amorphous paramagnetic or diamagnetic substance, and having within the crystal its principal axis coincident with the principal crystallographic axis. It is easy to verify that both crystal and ellipsoid, the poles of the magnet not being too near each other, will be directed between them in exactly the same way. In the generalization, an ellipsoid with three unequal axes, having a determinate direction in the crystal, must be substituted for the ellipsoid of revolution. In this hypothesis too, two "magnetic axes" are met with, that is, according to the new definition, directions which possess, in common with the single crystallographic axis of uniaxal crystals, the property that if the crystal be suspended so that either of these axes is vertical, and the body is at liberty to turn freely round it, no extraordinary magnetic action is exhibited, but the crystal behaves like an amorphous substance.

According to observation, a crystal under favourable circumstances is directed in the same way as the smallest of its fragments. Hence, according to the new hypothesis, each of its particles may be regarded as acted on like an amorphous ellipsoid. But such an amorphous molecular ellipsoid, when influenced by a magnetic pole at a finite distance, will be directed like an ellipsoid of finite dimensions under the influence of an infinitely distant pole. Here Poisson's theory presented itself for the verification of the hypothetical conclusions and their consequences, to which the author had been led by considerations of a different kind. This verification had the most complete success. But before proceeding to it, it was found necessary to confirm Poisson's theory itself (or rather the results following from it), with respect to an ellipsoid of finite dimensions influenced by an infinitely distant pole. By means of a beautiful theorem lately published by Professor Beer, by which the results relating to the influenced ellipsoid are simply and elegantly expressed by means of an auxiliary ellipsoid, the author was enabled to deduce immediately the analytical expressions. These were afterwards compared with experiment, by observations made on two carefully worked ellipsoids of soft iron, executed by M. Fessel of Cologne.

The results thus obtained from theory, and verified by experiment, with reference to an amorphous ellipsoid, were compared with the results obtained from the observation of crystals, and manifested a complete agreement. According to this theory, the magnetic induction within a crystal is, like the elasticity of the luminiferous ether, determined by means of an auxiliary ellipsoid. As there are three rectangular axes of optical elasticity, so there are three principal axes of magnetic induction, characterized by the property that if a crystal be suspended along any one of them, the two others set, one axially, and the other equatorially. As there are two optic axes, situated in the plane of the axes of greatest and least elasticity, so there are two magnetic axes, characterized by the property already mentioned.

Among crystals, the author selected for special examination red ferrocyanide of iron, sulphate of zinc, and formiate of copper. The first is paramagnetic, the second diamagnetic, and in both cases the principal axes of magnetic induction are determined by the planes of crystalline symmetry. The setting of elongated prisms, as well as of long cylinders and short cylinders or circular plates, cut in various selected directions from the crystals, is described in detail. The use of both cylinders and circular plates, cut with their axes in the same direction, obviated any objection which might be raised attributing the setting to the external form, since, so far as was due to mere form, a cylinder and a circular plate would set with their axes in rectangular directions.

Formiate of copper differs from the former crystals in having but one plane of crystalline symmetry, and accordingly in having but one principal axis of magnetic induction determined by the crystalline form. The existence of three principal magnetic axes, having the property already mentioned, was demonstrated experimentally, and the directions of those two which were not determined by the crystalline form, were ascertained by experiment. In this crystal the axes of greatest and least induction, and consequently the magnetic axes, lie in the plane of symmetry; and the existence of two magnetic axes was demonstrated, and their positions were determined.

In conclusion, the author gives a list of crystals, classified according to their paramagnetic or diamagnetic characters, and the order of magnitude of the magnetic inductions in the direction of their principal axes. He also remarks that some crystals, of which instances are given, though belonging according to their form to the biaxal class, have two of their principal magnetic inductions so nearly equal that they cannot be distinguished from magnetically uniaxal crystals; while others, though not belonging to the tesseral system, have all their principal inductions so nearly equal that they cannot be distinguished from amorphous substances.

LV. *Intelligence and Miscellaneous Articles.*

ON A NEW STEREOSCOPIC PHÆNOMENON. BY M. A. CIMA.

I TAKE a drawing of a head seen in front, of 3 or 4 centimetres in height; it may be lithographed, or engraved, or drawn with a pencil. I cut this drawing in the direction of the vertical axis of the nose, and arrange these two parts of the drawing in the same vertical plane, before the eyes, at a less distance than that of distinct vision. I remove or bring together the two parts of the drawing until the two images which result from their duplication, combine so as to form the entire face. The image of this face thus obtained presents the appearance of a solid object, or of a modelled figure in which the nose, the cheek-bones, the chin, and the eyebrows are detached, as in an object in relief. This sensation of relief increases in proportion to the length of time during which the two images are viewed; to obtain the maximum of effect, the two half-faces must be held at a suitable distance, which varies according to the observer. A similar, but much less perfect effect is obtained by looking at the drawing of the entire face, either with one or both eyes, at a distance much less than that of distinct vision.

I think that the explanation of these phænomena must be founded upon the two following facts, which led me to make the observation which is the subject of this note. The eye sees of a gray colour a series of very small alternate black and white spaces, very close to each other, and seen at a distance less than that of distinct vision. In looking at a single face divided into small squares alternately black and white, at a distance less than that of distinct vision, the white squares appear larger, the lines of contact between the white and black squares become of a gray colour, which extends gradually, and the black squares appear larger; at the same time the white squares appear to be raised at the centre and the black squares to be hollowed. This double property of our eye, which is due to *ocular irradiation*, furnishes us with a sufficient explanation of this new stereoscopic appearance.—*Comptes Rendus*, October 26, 1857, p. 664.

NOTE ON M. RUHMKORFF'S INDUCTION COIL.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Boston, Mass., Oct. 20, 1857.

I notice, in the September Number of your Magazine, my paper from Silliman's Journal, giving a description of my construction of the Ruhmkorff induction coil. I have since (in July last) constructed one with 60,000 feet of wire on the same principle, which gives sparks of $10\frac{1}{2}$ inches in the atmosphere with two cells, Bunsen's battery. This instrument is in the possession of Columbia College, New York.

Respectfully yours,

E. S. RITCHIE.

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SUPPLEMENT TO VOL. XIV. FOURTH SERIES.

LVI. *Observations upon the Structure of Glaciers.*

By JOHN BALL, *M.R.I.A., F.L.S.*[*]

NOT a few of those who have paid attention to the interesting questions connected with the movement and structure of glaciers, have long felt that, in spite of the efforts of many able men who had observed the phænomena on the spot, and of the progress made towards understanding their physical theory, for which we are chiefly indebted to Professor J. Forbes, much still remained both of fact and theory that called for further investigation on the part of men who should combine habits of careful observation with a mastery over the principles of physical science.

The paper read last January before the Royal Society, by Professors Tyndall and Huxley, followed by a lecture delivered at the Royal Institution by the first-named gentleman, has been welcomed by such persons as a material advance towards the completion of our knowledge on these subjects.

The theory of *glacier motion* may, indeed, be now considered complete. Professor Forbes had proved that glaciers in their gradual descent move after the manner of viscous fluids; Prof. Tyndall has now brought to light that property of ice by which rigid masses of glacier are enabled to conform themselves to the laws that regulate the motion of imperfect fluids.

The question remains, whether the able and ingenious authors of the paper read before the Royal Society have been equally successful in explaining the physical causes of the singular and complicated structure which careful examination detects in all known glaciers; and with a view to satisfy some doubts on this subject, I endeavoured, during a recent visit to the Swiss Alps, to observe with attention such peculiarities in the structure and arrangement of those glaciers which I was able to visit, as might help to test the sufficiency of the new theory.

[*] Communicated by the Author.

Although circumstances did not permit me to undertake any continuous series of observations, accompanied by accurate measurements, it has seemed to me that some notes made at the time, and of the accuracy of which, as far as they go, I feel confident, may deserve the notice of those who are interested in the subject. If I have ventured to draw some conclusions from facts, or to point out what appear to be objections to the deductions of other more competent physical observers, I am well aware that it becomes me to do so with great hesitation. That hesitation is somewhat lessened by the expectation that the subject is about to receive further illustration from the continued labours of Professor Tyndall, and to no more able and candid judge could any facts or arguments seeming to tell against his own conclusions be proposed.

The chief, but not the only peculiarities of glacier structure that have attracted the attention of observers, are the following:—

1. Stratification of the *Névé.*
2. Veined structure of the middle and lower region.
3. Dirt-bands of Professor Forbes.
4. Dirt-bands of M. Agassiz*.
5. Crevasses.
6. Capillary fissures in the compact ice.
7. Air-bubbles included in the ice.

These are all so familiar to those who have either observed for themselves, or who have studied with care the writings of Forbes, Agassiz, and other glacier authorities, that it would be superfluous to describe them, and I shall merely offer such remarks in regard to each of them as have some bearing on the theory of glacier structure.

1. The stratification of the *névé,* its arrangement in parallel layers of more and less crystalline snow, the diminished thickness of the layers in the lower part of exposed sections, accompanied by a consolidation of the substance of the *névé* into ice, more or less filled with air-bubbles, are facts familiar to those who have dwelt much in the upper regions of the ice-world, where in concave hollows, or still more often, on slightly convex *plateaux,* the vast accumulations are formed which give birth to the greater glaciers.

On one point, many of those who have described the formation of these beds seem to be certainly in error.

Each of the horizontal beds seen in an exposed vertical section of the *névé* is supposed to correspond with the whole annual fall of snow at the spot where the bed was formed. But this inference rests either on a great exaggeration of the effects of

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* Recently named Dirt-Streaks by Professor Huxley.

evaporation and melting in diminishing the mass of snow, or on an under-estimate of the amount that annually falls in the upper regions.

Comparing the observations made at the Great St. Bernard, and those of M. Dolfuss at his Pavilion on the Aar Glacier, we cannot estimate the average annual fall of snow, in the region of the Swiss Alps from 8000 to 10,000 feet above the sea, at less than 60 English feet. From the experiments of Agassiz on the relative density of snow, water, and different varieties of glacier ice, it appears that this thickness of snow would be equivalent to a bed of *névé* ice rather more than 8 feet in thickness. But in the lower part of the sections of *névé*, where the snow is converted into *névé* ice, the thickness of the distinct beds is seldom more than 6 or 8 inches, and often much less.

If each layer corresponds to the produce of one year's fall of snow, we should have to believe that the effect of melting and evaporation is to reduce the annual deposits of snow to one-twelfth or a still smaller proportion of their original bulk. This estimate, I feel persuaded, would be a very great mistake if applied to the region where the great accumulations take place, 10,000 feet, and more, above the sea. It is my strong impression, derived from frequent observations in the upper regions of the Alps, that the visible beds of *névé* represent each considerable fall of snow that has taken place, without any absolute limit between those of one year and another. Where a slight fall has occurred, or where a more considerable one has been followed by a sufficient interval of hot weather, it leaves no trace among the strata of the *névé*, save that the water which it produced has helped by percolation to consolidate the icy seams in the beds immediately below it. Again, when one fall of snow follows another with but a short interval, and before a strong crust of granular ice has formed over the first, the two become so nearly confounded together in the stratification that subsequently ensues as not to be distinguishable to the eye at a short distance. But it happens many times in each year, that after a heavy fall of snow dry and clear weather succeeds. I see no reason to doubt, that in winter as well as in summer, the joint effect of the sun and air is to produce a crust of granular ice, whereon the wind often deposits dust and other light bodies. At the next fall of snow, the main source of waste, evaporation from the surface, is put a stop to, and the future diminution in the thickness of the new stratum is owing to the gradual consolidation which arises from internal melting, from the percolation of water coming from above, and the pressure of the fresh strata of snow which continually accumulate overhead.

This explanation accounts for the great irregularity in the thickness and appearance of the *névé* beds, their unequal *glacification*, and the fact that while, at intervals, some are clearly defined to the eye by a dark line along the outcropping edge of their upper surface, the greater number are but very faintly traced. The dark lines betray the increased effect of the wind during long intervals of fine weather in summer, when larger surfaces of earth and rock are exposed, small particles of which, with dead insects, leaves, &c., are then carried far and wide over the smooth and crisp snow-plains.

Passing from this point, I would wish to fix attention on the fact, that while all observers admit the stratification of *névé*, no one of those who believe that all trace of that original structure disappears in the next stage of its existence—the glacier ice of the middle region—has attempted to account for so extraordinary and, as far as I know, so unique a fact, as the utter destruction of all trace of the internal arrangement of vast masses of matter. Granting the possibility that pressure and movement combined may effect such a result, it would be most desirable to obtain some insight into the *modus operandi*, and, if possible, to detect the process in Nature's own laboratory.

Until some approach has been made to clearing up this mystery, either by well-devised experiments, or by successful descents into some of the great crevasses of the upper regions of the glaciers, it can scarcely be said that our knowledge of glacier structure is complete.

Professor Forbes has indeed asserted, in more than one passage of his writings, that all traces of structure, including as well the veined structure of the middle and lower region, as the stratification of the *névé*, are utterly destroyed in the ice-cataracts that occur on many glaciers when the frozen stream is forced down a steep declivity. With all respect for so great an authority, I have never been able to accept this statement, and my incredulity was recently confirmed by a rather careful observation of the ice-cataract of the glacier of Rosenlani. Confining myself, of course, to those falls where the continuity of the glacier is not interrupted, the whole mechanical effect upon the mass of the glacier is simply to cause the production of many deep and wide crevasses, or, very frequently, of a double system of crevasses, breaking up the superficial portion of the ice into irregular quadrilateral prisms. As the whole mass advances, wasting of the ice takes place along the exposed surfaces, but especially on those that are most affected by the sun, and so the prisms become narrowed upwards and obliquely cut away on one side. When the glacier stream reaches a gentler slope at the foot of the fall, and flows over a concave, instead of a convex bed, the sides of adjacent prisms are brought nearer to each other, and gradually, owing to diminished motion in front, the parts of the ice become pressed together again into a compact whole; but in the whole process there is no disturbance of the relative position of adjoining portions of the same mass of ice, nor anything to affect the internal arrangement of its parts. The effect has been exactly the same as if great wedges had been cut out of the surface, and the sides of the trenches so formed forced together under great pressure. Professor Tyndall has shown us how under such circumstances the surfaces brought into contact become absolutely welded together.

Anticipating some of what I have to say on the veined structure, I may observe that I this year noticed on the glacier of La Brenva, near Cormayeur, what I believe to be direct evidence of the process above described.

In a part of the glacier where the veined structure is particularly well marked, I observed two transverse, almost vertical bands of very clear blue ice about an inch and a half in thickness, cutting the planes of veined structure pretty nearly at right angles, and inclined to each other at an acute angle. In the three portions of ice lying between the transverse bands, and at either side of them, respectively, the planes of the veined structure were not conformable but slightly inclined at obtuse angles. The cohesion between the ice of the transverse bands and that of the veined structure was quite perfect, so that I was able with ease to detach hand specimens, in which, if they could have been preserved, the unequal inclination of the veined structure on either side of the transverse band would have remained distinctly visible.

It seems to me clear that in this case the two transverse bands represented former crevasses which had been closed together late in the autumn, when the increasing cold was sufficient to freeze the enclosed water before the junction of the opposite walls had been quite completed. Such crevasses must at one time have been of considerable width, or else the planes of the veined structure at opposite sides of the transverse bands would not have been inclined, and the appearance would have been merely that of a fault in parallel strata.

Such an appearance may not be rare, but as I had never observed it before, and have not seen it noticed by glacier writers, I have been a little particular in describing it. Lines indicating closed crevasses are common enough, and can sometimes be traced far on the surface of the glacier; perhaps, if attention be given to the point, it will be found that they often correspond to a shifting of the planes of the veined structure.

2. The *veined structure* of the middle and lower part of the great glaciers, and, more or less, of all glaciers in which the

ice is well consolidated, has hitherto been the point as to which science seemed most at fault in endeavouring to explain the phænomena of the ice-world.

As one of those who had never been able to accept the conjectural explanation given by Professor Forbes, who was the first to make known the true character of this phænomenon, and to perceive its importance, I could not fail to be much interested in the new hypothesis, which supplies a simple, and apparently an adequate explanation of the known facts. But the result of some consideration and recent observation has not removed all the difficulties which seem to me to stand in the way of an unqualified acceptance of this new theory, and with very sincere diffidence I venture to state them.

In the first place, then, I would observe that while Professor Tyndall has abundantly proved the extensive influence of great pressure in modifying the internal arrangement of various bodies, he has not shown any instance in which the resulting structure at all resembles that which we find in glacier ice. In every case of lamination and consequent cleavage, attributable to pressure, the result seems to be due, either to a rearrangement of the rigid particles contained in the mass by which their flatter surfaces are disposed in planes perpendicular to the direction of pressure, or else to the compression of minute and comparatively soft nodules or accretions*, which alter their form under the force to which they are exposed, and each particle, from a rounded or irregular form, assumes that of a lamina or plate with its faces perpendicular to the direction of pressure. In either case the phænomenon of cleavage is the direct result of the arrangement of the particles of the mass in layers with flattened faces approximately disposed in the same plane. Now, whether we examine the bands of compact blue ice, or the intermediate layers of white opake ice, we find no trace of lamination, or anything approaching to it, in the internal structure of the mass.

At times, indeed, I have found, as Professor Forbes first noticed, a distinct tendency to cleavage along the surface of junction between the veins of blue and white ice, but on either side, both the one vein and the other have always appeared to me absolutely devoid of any tendency to split in the direction of the veined structure, nor does close examination show traces of molecular arrangement which could produce such a tendency. The cleavage planes above described seem to be by no means invariably present, and the phænomenon has to me the air of being superinduced by pressure upon the already existing veined structure, rather than its essential concomitant.

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* I apprehend that this is true of wax as well as the other substances cited by Professor Tyndall.

Again, if the veined structure be produced by pressure alone, how does it happen that the part of the mass which must be most amenable to the action of external force, I mean the white ice filled with air-bubbles, does not show in the form of the cavities which pervade its whole substance, a record of the process by which it is assumed that the internal condition of the ice has been so profoundly modified? But as Agassiz has pointed out, and I have frequently verified his observations on this point, though the air-cavities often show traces of compression reducing them to the form of flattened lenses, the directions in which they are flattened are most various, and show no constant relation to the planes of the veined structure. Here then we have direct evidence that separate portions of the ice have been acted on by pressure sufficient in amount to modify their internal arrangement, but that these pressures have not acted in the same, or nearly the same direction. But surely this is not consistent with the supposed action of force on a great scale upon an enormous mass of ice—able to destroy first the primitive structure, and then to impress upon it a new and different one. Such a force must have entered into composition with any local pressures which can be supposed to have existed in the interior of the mass, and the resultants would have approximated closely to the general direction of its action.

A further difficulty which I think deserves some attention is this, that the pressure hypothesis gives no explanation whatever of the irregularity in the appearance and the thickness of the veins. Even if we are to admit that such pressure as exists in glaciers is competent to impress upon masses of *névé*, sometimes many square miles in extent, a veined structure consisting of alternate bands of nearly compact ice and ice filled with air-bubbles—a conclusion, let me say, which is not absolutely inconsistent with what we know of the action of pressure on seemingly homogeneous masses of matter, but which has not yet been shown to be possible, either by experiment or by any close accordance with observed facts,—is there anything that would lead us to expect that the veins should be otherwise than moderately uniform in structure? Every one, however, who has looked carefully at the veined structure knows that this is not the fact: some of the blue veins are very thin and faintly marked, others on the contrary much thicker, more compact, and therefore more transparent than those on either side of them. But there is a further fact to which I wish to call particular attention, viz. the recurrence at regular intervals of these thicker and more transparent blue veins. I do not feel justified in asserting that this is constantly to be seen, but I have reason to think it very frequent. My attention was first called to it on a small glacier rather high up on the north-north-east side of the Töde, above the Upper Sand Alp, and I have since seen it elsewhere, *e. g.* on the glacier of La Brenva.

I next proceed to ask whether, supposing it possible that pressure alone can produce in beds of *névé* alternate layers of denser and of less compact ice perpendicular to the direction of pressure, the form and position of the veined structure as we find it in nature is consistent with the new hypothesis? It is well known that if, by a comparison of the dip and strike of the veined structure in different parts of a glacier, we construct surfaces to pass through the apparently continuous layers of blue or white ice, such surfaces, at the lower end of great glaciers, usually assume the form of a scallop shell, or flattened spoon, with its concave side turned up, and dipping outwards towards the lower end of the glacier. In the middle region of glaciers whose length is great compared to their breadth, this spoon-shaped structure becomes much elongated, and it seems to be distinctly traceable only near the sides, while in the central part the veined structure is arranged in parallel planes nearly vertical, and directed towards the lower end of the glacier. In smaller glaciers, which, instead of flowing in deep valleys, occupy depressions on the slopes of the higher Alps, the spoon-shaped arrangement is usually traceable more clearly throughout their whole breadth, and the dip towards the centre is nearly conformable to the form of the solid bed of the glacier.

It will readily be admitted that there are cases where large masses of ice are forced in their onward course into channels with steep sides, where the greatest pressure is probably that due to the lateral constriction of the ice-stream between the containing walls, and where we might accordingly expect that the veined structure, if produced by pressure alone, should be developed in planes parallel to the direction of motion. The Mer de Glace of Chamouni, and the great glacier of the Aar, are familiar instances of this class. But I can scarcely admit that this holds true of many other glaciers in which the direction of the veins is the same. Let us take for instance the Aletsch glacier, the greatest, I believe, of all known ice-streams. Omitting many small affluents which cannot produce much effect, it is nourished by three main branches, proceeding respectively from the north side of the Aletschhorn, from the col connecting the Mönch and the Jungfrau, and from the valley leading by the north side of the Viescherhörner to the Finster Aarhorn. After the junction of these separate streams, the great glacier flows on with remarkable uniform slope and breadth, with no projecting promontories of rock to disturb its course, for a distance of eight or nine English miles, until opposite to that singular and beautiful lake, the Märjelen See. There it experiences resistance from the mass of the Aeggischhorn which lies in its direct course, the inclination becomes steeper, the stream is forced to the right, is rapidly narrowed from about three miles to little more than one mile in width, and finally descends through a steep gorge below the Bell Alp, until it pours out a stream which soon unites with the Rhone, and more than doubles the volume of its waters.

It cannot be doubted that the part of the glacier which is most subjected to lateral pressure, is that below the Märjelen See, where it abuts against the rocks of the Aeggischhorn, and is turned from its direct course by the resistance which it encounters. Yet in that part of the glacier the veined structure is much less perceptible than usual; a fact which Professor Forbes also noticed and explained in accordance with his own theoretical views, by pointing out the probability, or almost certainty, that the motion of the glacier of Aletsch is much slower than that of other great glaciers; and as a consequence, that the difference between the rate of progress at the centre and at the sides is proportionately diminished. But if this explanation were correct, it would apply with still more force to the upper part of the glacier, where the inclination is still less, and where we must assume that the onward motion is very slow and sensibly uniform, except close to the sides.

In that long unbroken reach of the ice-stream, again, there can be far less lateral pressure than opposite the Aeggischhorn; and I apprehend that if there be any pressure at work capable of modifying the internal structure of the ice, it must be that due to the weight of the mass acting vertically on its lower strata, compounded with that which causes its onward movement. We have, at all events, direct evidence that there is no appreciable amount of lateral compression. So long as the glacier is confined on both banks by solid rock, the point might remain doubtful; but at the point already referred to, the rocks which had confined the glacier on its eastern shore subside, and give place to the depression which is occupied by the Märjelen See. Instead of a barrier, there is a hollow partly filled by the blue waters of the lake, which is on one side shut in by the glacier. True it is, that this hollow is of moderate depth compared to that of the central part of the glacier; but above the level of the lake the ice-cliffs are often 50 or 60 feet in height, to which must be added the depth of the water below. If the veined structure at the surface of the glacier was due to lateral compression, when that pressure was removed the ice would be pressed outwards into the vacant space. The fact that no such lateral expansion takes place, and that the glacier moves steadily forward along the main valley, seems to me to prove conclusively that the chief pressure acting upon it in the plane of the horizon, must be in the direction of its motion, and that surfaces formed perpendicular to the lines of maximum pressure should intersect the glacier in flattened curves, slightly convex towards its lower extremity. The fact, however, is otherwise. The upper portion of what I may call the *long reach* of the glacier, was covered with snow when I traversed it; but opposite to the Märjelen See, and for three or four miles up the stream, I found the veined structure quite well developed in the central region of the glacier, arranged as usual in vertical planes parallel to the direction of motion. If it be said that this structure was originally produced by the mutual pressure of the three main ice currents which unite in the upper basin already referred to, I would reply, that the point of junction is fully eight miles from the Märjelen See; that it is probably a considerable overestimate if we allow 200 feet as the average annual advance of that part of the glacier; and, consequently, that the veined structure at the lower station must, according to this latter hypothesis, have been produced more than 200 years before we observe it. For the greater part of that time we have proof that the ice has been subject to little or no lateral pressure, but constantly to a force acting in the direction of motion, and to the vertical pressure of the upper on the lower parts of the mass. At the most moderate estimate of the superficial waste along the intermediate portion of the glacier, we must suppose that a thickness of 400 or 500 feet of overlying ice has been removed during the long interval in question. But if the long-continued action of pressure in new directions is incompetent to alter the structure impressed upon the ice 200 years before, what becomes of the assumed obliteration of the original stratification of the *névé* through the agency of pressure; and how are we to believe that an effect so universal as this must be, if indeed that original stratification is everywhere effaced, should not be repeated in a field where the same causes must have been in operation with remarkable uniformity for a period unusually protracted?

Even in those glaciers where it is conceded that lateral compression has probably been at work to a great extent, such as the Mer de Glace of Chamouni, there are many parts of the glacier where it appears to me that the hypothesis of Professor Tyndall would require that the veined structure should be developed at right angles to the motion of the centre of the glacier*; but I do not venture to urge that view against the

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* Professor Tyndall's mud experiments illustrate this, by the flattening of the circular rings impressed on the centre of a stream moving over a bed of irregular slope.

possible reply, that, although there are portions of the glacier where the frontal resistance must greatly predominate over the lateral pressure, this new force does not operate long enough to modify sensibly the structure already impressed upon the ice*. Even with this qualification it is hard to reconcile the new theory with such facts as are presented on the lower part of the glacier of La Brenva. Professor Forbes has well described the way in which this great glacier, descending through a steep gorge on the southern side of Mont Blanc into the Allée Blanche, a valley running nearly at right angles to its original course, encounters first an enormous moraine, which it has itself piled up below, then abuts against the rocks on the opposite side of the main valley, bridging over the torrent of the Doire, and is thus gradually bent round, until the lower part of its course follows the direction of that valley. Now if it were possible to examine the conditions of internal pressure and tension across the glacier opposite to the point where it abuts against the south side of the Allée Blanche, and where the process of flexure is chiefly accomplished, it cannot be doubted that these conditions would be found to be far other than symmetrical as regards the centre of the glacier. On the south side, we must admit the existence of powerful lateral pressure from the resistance of the rocks to the onward movement of the glacier†, while on the

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* This view of the permanence, within certain limits, of the veined structure, which I assume to be an essential portion of Mr. Tyndall's theory, is, of course, at variance with the belief expressed by Professor Forbes, that that structure is destroyed in ice-cataracts, where the surface is completely cut up by deep crevasses, and redeveloped within a short distance, where the glacier resumes its normal mode of progress, and of which he gave one instance in the Mer de Glace, opposite to the promontory of Trelaporte. I have already stated reasons for doubting this conclusion; and I may here add, that considering the great difficulty of obtaining actual access to the most crevassed parts of the ice, observations on its condition are to be received with great caution. Where glacier ice presents a moderate slope, the action of running water in warm weather acts so as to scour away the disintegrated portions of the surface, and to bring to light the real structure of the interior; but where the form of the ice is such that the water melting from the surface runs off at once without forming rills, as is the case on the steep faces of ice pinnacles, and on the upper edges of crevasses, the whole surface is weathered into a crust of disintegrated fragments of ice, and no sign of internal structure is visible to the eye. This is even true, in some states of the weather, on parts of the glacier where the veined structure is generally visible, and affords ground for doubting some alleged instances of its non-appearance.

† "Intense pressure" Professor Forbes calls it in his Twelfth Letter on Glaciers. I may remark that this most interesting glacier has subsided at the present time (September 1857) nearly to the dimensions described and figured by Professor Forbes in 1842, while in September 1853 it had pretty nearly those which he found in 1846. On both occasions, however, it seemed to me that his sketches somewhat underrated the length of the

opposite side the mechanical condition must be completely different; the feeble moraine can exert little or no lateral pressure, while it is probable that a considerable amount of frontal resistance is encountered in the glacier itself, in consequence of the retarded motion of that side towards which the mass of the ice is twisted. Contrary, however, to my expectation, I found in crossing the glacier from a point immediately below the Chapel of Notre Dame, that the arrangement of the surfaces of the veined structure is remarkably symmetrical. Measures of dip taken with a clinometer near the edge of a glacier are, indeed, very fallacious. The ice does not melt uniformly from its upper surface; the action of the sun and of radiant heat reflected from the shore is most energetic upon the exposed sides, and the steep banks of ice which rise terrace-like, one above the other, at the edge of the glacier, are formed of beds or layers of ice, of which the outermost coming into contact with the rock, must have been originally much lower in the interior of the glacier, before the processes of movement and ablation had brought it into its exposed position. The inward dip of the veined structure opposite the Chapel, as I have constantly found it in similar positions, increases from the outermost and lower, to the inner and higher ridges of the ice. Where in actual contact with the rock, at the only point which I was able to attain, it did not exceed 30°, which was at that point, as nearly as I could judge, the inclination of the underlying rock, while at the opposite edge of the glacier, where it forms a steep ridge thinly covered with gravel, sometimes more than 80 feet in height, and with no retaining wall of rock, the dip towards the centre seems to vary from 30° to 40°. Contrary to the experience of Professor Forbes in 1846, I found the veined structure better developed towards the centre than close to the sides; but I lay no stress upon this circumstance, for I believe that the ice varies exceedingly in this respect within short distances, and no two observers pass over exactly the same course upon the glacier, while, as I have already observed, there is a constant source of error in the various degree of weathering of the surface of the ice. But the fact to which I wish to call particular attention, is the regularity of the so-called spoon-shaped arrangement of the veined structure on this glacier, subjected as it is, on one side, to intense lateral pressure. I was unable to detect any difference in this respect between the lower extremity of the Brenva and those glaciers which are most free from disturbance from this cause.

In taking leave of this part of the subject, I would suggest that a theory which would explain the structure of glaciers by

lower end of the glacier, extending from below the Chapel of Notre Dame towards Cormayeur.

the effects of pressure arising from the form of the solid channels through which they move, assumes a degree of elasticity in the substance of the ice, which, until it is proved to exist, can scarcely be taken for granted. Seeing the extent to which a large portion of the mass is penetrated by air-bubbles, omitting all consideration of the fissures and crevasses which extend through most portions of it, it might be expected that before an intense force acting from without could be transmitted so as to modify the structure half a mile away from the point at which it is applied, it would first exercise such a crushing force upon the part of the glacier near at hand as would greatly modify its actual structure. If experiment should show that a measurable amount of pressure applied to a block of glacier ice does actually cause a flattening of the air-bubbles at right angles to the direction of pressure, we must conclude that no pressure of equal amount has previously been transmitted through that block, and we shall be better able to estimate the pressure, *from sources external to the glacier itself*, that can have acted on the interior of the mass. My own impression is, that the cleavage which is sometimes, but not invariably, developed in the veined structure, is produced by pressure acting on ice in which that structure already existed, and that it is accompanied by a partial flattening of the air-bubbles adjoining the surfaces of the blue veins, and in the planes in which cleavage takes place. I do not pretend, however, to advance such a conclusion with confidence on the strength of such few and imperfect observations as I have been able to make.

One more remark I venture to offer upon a question of fact, rather than of theory, though it has an important bearing upon the whole question under discussion.

The accomplished authors of the paper read before the Royal Society, starting for the first time to visit the glaciers of the Alps, with a surmise that the phænomena of veined structure might be accounted for as particular cases of slaty cleavage produced by pressure under new conditions, were naturally struck by every fact that appeared to confirm that conclusion. They noticed instances in which the veined structure, instead of exhibiting the blue veins in continuous surfaces, showed merely flattened cakes of blue ice imbedded in a matrix of white vesicular ice. More frequently they found that the blue veins, while seldom more than an inch or two in thickness, thin out and disappear after being traceable for a distance of some feet. Hence they have been led to apply the term *"lenticular structure"* as descriptive of that which previous observers had called the *veined*, or *laminar* structure of the ice. So long as the new expression was confined to the particular and unusual condition of the ice first spoken of, no objection could be made to it; but if I am not under a grievous misconception in believing that the blue veins may usually be traced for a distance of many yards, and almost constantly for several feet, I may be permitted to appeal to the subsequent and wider experience of the authors of this new term against the use of it as generally descriptive of the phænomenon to which they seem disposed to apply it.

3. *Dirt-Bands* of Professor Forbes.—The appearance first described by Professor Forbes under this name has been confounded by subsequent writers with a totally different phænomenon to which I shall presently refer, and this makes it necessary to distinguish it by his name. It is generally to be recognized only from a distance, when a large extent of glacier is seen under such conditions of light and shade as enable the eye to seize slight variations of hue; but when once seen it is impossible not to come to the conclusion that it is an indication of the working of some mechanical law, which has operated over the whole mass of the glacier.

Professor Tyndall has illustrated, by ingenious experiments, the conclusion drawn in the paper so often cited, which refers the production of these broad bands, or zones, of ice alternately white or discoloured, to the effect of steep falls or ice-cataracts separating into distinct portions the dirt which had before been evenly distributed over the surface. The form subsequently assumed is easily shown by him to be a consequence of the law of glacier motion. Though I believe that the effect of ice-cataracts in breaking up the surface of the glacier has been somewhat overrated*, I am inclined to think that the appearances produced in Professor Tyndall's models are really traceable on some glaciers, but that they are different, both in form and origin, from those described by Professor Forbes. The latter were most accurately described in the third and fifth of his published letters. They traverse the whole breadth of a glacier stream, and are arranged at tolerably uniform intervals of several hundred feet, the breadth of the dirt-bands and of the intervening whiter spaces being approximately equal. I am not aware whether accurate measures have verified Professor Forbes's surmise, that they correspond to slight undulations in the surface of the glacier, a fact which, if well established, would go far to prove his conclusion, which has always seemed to me probable, that they are the indications of an undulation in the flow of the glacier, depending on the different rates of progress in summer and winter. Fresh snow interfered with my observations on two or three occasions during the last season, but I have an impression that during former visits to the Alps I have noticed an ar-

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* They certainly do not seem to affect the continuity of moraines.

rangement of dirt and debris in irregular curved streaks, much narrower than the "*dirt-bands,*" and not regularly disposed across the whole width of the glacier. These probably corresponded with the phænomena observed on the Rhone Glacier and that of Grindelwald by Messrs. Tyndall and Huxley, and subsequently explained in their paper in the Philosophical Transactions.

4. *Dirt-Bands* of M. Agassiz.—The language of M. Agassiz and his friends, in treating of the appearances which they supposed identical with the dirt-bands of Professor Forbes, is not always so clear and consistent as to enable the readers of the 'Système Glaciaire' to recognize with certainty the phænomena which they describe. This is evidently due to the want of accurate principles of physical science on the part of the author and his fellow-labourers. But, as a record of observations, it seems to me that their work contains much information that cannot properly be neglected, and amongst other facts, the system of dirt-lines, which Agassiz regards as evidence of the stratification of the glacier, deserves, I think, further careful examination and discussion. I have frequently remarked the recurrence at pretty uniform intervals of broader and more strongly marked blue bands in the veined structure, sometimes accompanied by the appearance of a thin stratum of dirt-fragments which may be traced along the walls of intersecting crevasses, and into the substance of the glacier. These are the lines which M. Agassiz has identified with the dirt-bands; and to these, if I am not mistaken, most of the statements in his writings apply. But at times it would appear as if the observations recorded had reference to two completely distinct phænomena, of less general occurrence. One of these is the existence of lines indicating closed crevasses, of which the lips often retain for a considerable time enough of dirt to mark their presence on the glacier. When seen from a moderate height above the glacier, where their direction can be compared with that of existing crevasses, these are easily recognized, but on its surface they may readily be confounded with the class of dirt-lines now under consideration. The other phænomenon, which has, as I believe, been a source of confusion, is peculiar to the upper region of great glaciers. The annual fall of snow, and the corresponding destruction of it by heat and evaporation, are, as is well known, very variable in amount. New beds of *névé*, and even small glaciers, are sometimes formed on the lower mountains, and disappear again in subsequent seasons. The same thing happens on the surface of great glaciers. After a layer of dirt has been deposited on a part of the glacier which has been exposed for one, or perhaps for several successive seasons, it may easily become covered over by beds of *névé*, which may finally, if they

subsist long enough, be converted into glacier. Thus we often have *névé* beds, and may easily find glacier, formed upon an underlying glacier at a place far removed from that where the lower part was originally consolidated. In such cases, one instance of which is described at page 204 of the *Nouvelles Etudes sur les Glaciers*, it is obvious that no sort of conformity can be expected to exist between the structure of the upper and the lower beds. When, either through a temporary change of seasons, or through the descent of the newly-formed glacier or *névé* beds into a warmer climate, these latter begin to waste away, this process will usually go on most rapidly along the outer edges of the new deposits, and especially where adjacent rocks reflect heat upon the glacier, and the same operation may easily cut into the underlying mass of old glacier. In this way we may have exposed to view layers of dirt and debris of rock, either underlying the new formation, or intercalated between its beds during the period of its growth, but having no relation whatever to the surfaces which indicate the structure and mode of formation of the old glacier. When the new formation is completely melted away, no difficulty remains, because the whole of the foreign matters mixed up in it will be deposited on the surface of the main glacier, now again brought to light, and will follow the ordinary laws which regulate the distribution of superficial debris.

5. Crevasses.
6. Capillary fissures.
7. Air-bubbles.

This paper has grown to such undue length, that I forbear to add more than a single remark upon these topics to those which I have included in the foregoing pages.

It seems to me, that in regard to the existence and extent of capillary fissures in the compact ice, those portions of a glacier which are subjected to the pressures and tensions which must accompany the onward movement of the mass, may be expected to be in a different condition from those which by their position are exempted from the action of these forces. Parts of the ice sometimes appear completely solid, and show to the eye no trace of capillary fissures. Perhaps the existence of these is not so universal as has been commonly supposed.

I may now be permitted to point out very briefly how far the facts discussed in the foregoing pages are consistent with a theory of glacier structure which has hitherto found little favour with the eminent men who have taken part in the controversy. According to this hypothesis, the stratification of the *névé* passes by a gradual consolidation of the ice into that laminar or veined structure which is found to be an almost universal characteristic of glaciers.

In favour of this view it may be urged that we have, in the first place, the positive fact that the *névé* is a mass made up of beds alternately more and less compact and crystalline,—that, so far as we are able to trace them downwards, these beds continually diminish in thickness,—and finally, that the glacier ice formed out of these beds has a proper structure, which differs only in degree from that of the *névé*. The irregularity in the thickness and appearance of the veins, and the recurrence at regular intervals of larger and more strongly-marked bands, are facts exactly in accordance with this explanation of the origin of the veined structure. At first sight it seems, however, impossible to reconcile it with the position which is generally assumed by the surfaces of the veined structure, but perhaps a little consideration may lessen, though I do not pretend to say that it will completely remove the difficulty. All great glaciers are made up from the confluence of separate ice-streams. We suppose each of these to consist in its origin of beds of *névé* transformed into glacier ice, the strata of which must gradually have conformed their slope to the form of the channel down which they have moved. Hence at the point of junction of two glaciers, we have brought side by side two great masses of ice made up of concave shells lying one within the other. In the gradual advance of the glacier, two, or it may be several, such masses are pressed together within a channel much narrower than the aggregate of those which they separately occupied. Those who have followed Professor Tyndall's experiments, will readily understand that under such conditions concave shells of ice would be gradually brought to the shape of deep troughs, of which the transverse section would have the form of an elongated letter U. It is obvious that the appearance presented by a horizontal section through a mass made up of such surfaces would be that of lines parallel to its length. But a horizontal section does not correctly represent the condition of things seen in descending over the surface of a glacier. Each successive portion of the glacier has undergone in an increased degree the process of ablation, by which it is estimated that a thickness of from 10 to 12 feet is in many cases annually removed from the surface of the glacier. We should therefore obtain a nearer approximation to the truth by supposing an elongated solid, composed of deep trough-shaped surfaces lying one within the other, to be cut through by a plane inclined at a small angle to its longer axis, so as to reduce its thickness at the lower end to a mere tongue. The likeness to a glacier would be further increased if the edges were rounded off so as to make the upper surface somewhat convex. Any one who has a difficulty in following the description may easily construct a miniature model, by doubling together fifteen or twenty thicknesses of leather or strong paper, and then cutting through the fold in a direction slightly inclined to it. On paring the edges of the new section with a sharp knife, it will be found that the arrangement of the lines exposed on the upper face of the model, and the inclination to its surface of the laminæ of leather or paper, very closely correspond to the ordinary disposition of the veined structure in glaciers that have flowed through a narrow valley. If it be objected that, as the hypothesis supposes that several separate streams, each with its own system of veined structure, were pressed together to make up the main glacier, the terminal section presented by the front of the glacier should present an equal number of systems of shell- or spoon-shaped structure, the answer may be found in observations detailed by M. Agassiz, and represented on his map of the Lower Aar glacier, which have the advantage of being supported by a great degree of anterior probability. Each separate ice-stream that goes to make up the glacier can subsist only so long as the supply of ice from its source meets the waste that arises during the process of ablation. It is not likely that any two of the streams that supply the glacier are exactly alike in this respect: one will bring with it a more abundant supply than the other, and survive the longest. Hence that one will by itself constitute the extreme front of the glacier, its structural surfaces will alone be perceived, and little if any trace of the other affluents will there be visible.

By analogous considerations the position of the veined structure may in most cases be explained; but there is one point which it has always seemed to me difficult to reconcile with the stratification theory,—I allude to the increased dip inwards of the constituent veins, which is constantly observed in ascending the frontal portion of glaciers. This disposition is common to the great glaciers which have flowed through long channels, and to smaller glaciers of the second order that have moved but very slowly in depressions upon the flanks of the higher Alps. In particular cases the pressure theory appears to be more competent to account for it, but it seems to me that a complete and satisfactory explanation is yet to be sought for.

The stratification theory is in no way inconsistent with the occasional occurrence of "lenticular structure" in the fragments of blue ice, or the more common appearance of thinning out in the blue veins. The unequal advance of different portions of the glacier cannot take place without frequent disruption of the surfaces of blue ice, and though these may again be united so as to form a nearly continuous mass, some degree of discontinuity must occasionally remain.

The cleavage of the veined structure is accounted for by the effect of great pressure transverse to the already existing surfaces, probably accompanied by the partial flattening of the air-bubbles in a portion of the mass.

So far, I venture to think that the stratification theory, though not free from difficulties of its own, may well compare with its rivals. There is, however, a class of glaciers which differ widely from all others in the mode of their formation, and whose structure has been regarded as incompatible with that theory. I allude to the "*glaciers remaniés*" which are formed at the base of lofty precipices when the latter are surmounted by steep slopes of snow or ice, which from time to time descend in dust-like showers, forming avalanches, such as are watched by tourists from their habitual haunts on the Wengern Alp and the Great Scheideck. Below the latter pass, in a hollow at the very base of the precipitous face of the Wetterhorn, is a glacier of this class, called the Lower Glacier of Schwarzwald, which has already attracted the attention of M. Desor and of M. Charles Martins. Being sufficiently easy of access, this glacier offers one of the most favourable opportunities for studying the structure of such glaciers, and I thought myself fortunate in being able to visit it on the 13th of August last. The form of the glacier is that of a segment of a cone, whose apex, abutting against the rocks, is formed by the snow or fragments of ice that descend about 4000 feet from a steep slope below the peak of the Wetterhorn. The glacier preserves on all sides a tolerably regular slope of about 30° with the horizon*, but where it abuts upon the nearly level bottom of the valley, the front is in some places cut away into steep faces from 20 to 40 feet in height. On examining this part of the glacier, it was found to be formed for the most part of a uniform mass of white ice, everywhere filled with air-bubbles, and without any appearance of veined structure. The bubbles showed very slight and occasional traces of compression in any direction, in this respect differing from all ordinary glacier ice. Nevertheless, when masses of the ice were forcibly struck with a heavy iron-pointed stick, it became evident that there was a distinct tendency to cleavage in parallel planes. On further examination the veined structure was found here and there imperfectly developed, and in a few instances single bands of blue ice, 1 or 2 inches thick and several feet in length, perfectly clear and without air-bubbles, were seen in positions conformable to the direction of the cleavage planes. Along the steep terminal slopes nearly

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* This measure is given from recollection, my notes being defective on this point.

horizontal lines of gravel were seen at intervals, and, as far as I was able to get access to them, I was led to the belief that these corresponded to the outcrop of surfaces extending into the interior of the glacier, and strictly conformable to the direction of cleavage. I was not able to cut far into the ice, but it appeared to me that the lines of gravel were merely superficial, and caused by the slight projection of the edges of surfaces of somewhat harder ice retaining minute fragments in their descent over the face of the glacier. Subsequent observations made higher up led me to doubt the complete accuracy of this conclusion. The direction of the strike of the cleavage planes, marked as it was to the eye by the lines of gravel, and by the occasional blue veins, varied a good deal, partly from lateral contortion, owing, as it appeared, to inequalities in the rocky bed of the glacier, and partly to a variation in the dip, which was nearly horizontal, but sometimes inclining outwards at a small angle, just as we find it in ordinary glaciers.

On ascending the glacier, the lower part of the surface was found to consist of the same white ice that has been already described, without any trace of veined structure, but having, at intervals of from 10 to 15 feet, transverse dirt-lines, evidently marking the same structure as the gravel-lines on the terminal slopes. Crevasses were pretty numerous, disposed in lines radiating from the apex of the cone, and often 6 or 8 feet in width. On the walls of these crevasses the dirt-lines were well marked, and had all the appearance of being produced by thin layers of dust penetrating the ice, but instead of being horizontal or dipping outwards they invariably dipped inwards, towards the centre of the glacier, at an angle which increased from below until about the middle of the glacier, where it varied from 30° to 40°. In no instance was I able to detect traces of veined structure in the walls of the crevasses.

Above its middle region the glacier was covered with the remains of *éboulements,* which had taken place during the past winter and spring. The surface was irregular, but not so much so as I have seen it in places where frequent falls of the consolidated *névé* take place. Very few fragments of ice were seen, and it seemed to me that the greater part of the mass must have fallen from the upper region in the state of snow very loosely agglomerated. The very fact that this glacier can be traversed in summer without danger, is a proof that it is not chiefly formed of fragments of ice falling from a higher glacier, as has been incautiously asserted by Agassiz*, on the authority of M. Desor; for in glaciers of the latter class the surface is covered with large

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* *Nouvelles Etudes,* p. 242.

iceblocks, which are frequently descending from above, and few explorers like to remain longer than they can avoid within their reach. A genuine example of this kind is seen on a terrace of the Wetterhorn, further removed from the Scheideck than that which I visited, and much higher above the valley leading to Rosenlani. Circumstances did not allow me to attempt to examine it. The ascent would be rather difficult, and the passage of the glacier, if at all practicable, would involve some risk, unless made in the early morning, before the ice commonly begins to descend from the upper plateaux.

The upper part of the Schwarzwald glacier has but few crevasses, and it is therefore difficult to trace the dirt-lines which are there covered with the fresh snow fallen from above. I was, however, able to convince myself that the inward dip attains its maximum about the middle of the glacier, and gradually diminishes in the upper part, not exceeding 10° at the highest point where I was able to measure it.

I should add, that I had direct proof of the motion of the ice in this glacier, though this is probably slight. At the bottom, opposite to the centre of the terminal face, a miniature moraine, about 30 feet long and 3 high, had been quite freshly thrown up by the advance of the ice during the early part of the summer, but owing to its rapid waste during a warm season the ice had again receded 3 or 4 feet.

I leave it to more competent authorities to decide on the significance of the facts above stated. To myself, it appears that they are not completely reconcileable with any of the hypotheses that have been framed to explain glacier structure. If the whole phænomenon were one of cleavage in planes dipping inwards at the middle of the glacier, and horizontally or outwards at the bottom, it might with much plausibility be ascribed to pressure. But if the clear blue veins occasionally present are to be referred to the same cause, how is it that the air-bubbles in the white ice show no trace of its action? But still more difficult it is to understand how any amount of pressure could cause the production at regular intervals of those surfaces whose presence is revealed by the dirt-lines, whether these are due to the presence of dirt * in the mass of the ice or not.

The advocates of the stratification theory may maintain that

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* The determination of this point would require a sharp cutting instrument, which I had not with me, and careful examination of the ice with a powerful lens. It must be recollected that an exceedingly thin film of dust, dispersed over a surface enclosed in a mass of ice, might be scarcely perceptible, but that the melting of a thickness of several feet of the same ice, leaving much of the enclosed dust upon the face of the glacier, would render its presence plainly visible to the eye.

the condition of the greater part of the materials of this glacier is such as to make the formation of regularly stratified beds quite possible, and it may even be admitted that the interval between the dirt-lines would probably correspond with the annual deposit, but as each bed so formed would have the shape of a hollow cone with its apex turned upwards, it seems all but impossible that its upper surface can correspond with that traced by a dirt-line in the middle of the glacier, whose form must be that of an inverted cone or deeply concave curved surface.

It is not for me to pronounce whether the facts and laws of physical science yet known are adequate to solve these difficulties, but I may be allowed to express the conviction, that in a complete theory of glacier structure, some further account than has yet been taken will be made of the process by which the materials of the glacier are originally deposited and consolidated. I am not less sure that the potent influence of pressure in modifying internal structure, which has been so fully developed by Professor Tyndall, will be recognized hereafter as one of the physical principles that will help to explain the intricate phænomena of glacier structure.

P.S. Nearly the whole of the foregoing pages were written before I had an opportunity of reading the interesting paper by Professor Huxley, contained in the October Number of this Journal. I will venture to make a few remarks on the conclusions which Mr. Huxley has drawn from his recent observations.

Absence of capillary fissures in the deep ice.—Mr. Huxley has fully proved the incorrectness of the common opinion, founded mainly on the statements and observations of Agassiz, which supposed the whole mass of the ice to be penetrated by capillary fissures through which water could percolate; but I do not think that his experiments enable us to conclude that there are no such fissures in the interior of the glacier. If we are to adopt that conclusion, we must then cease to believe with Professor Tyndall, that glacier ice is enabled to advance in conformity with the law of viscous motion, by *fracture* and *regelation*. For in the midst of new observations we must not lose sight of the main facts already well established, and we owe to Professor Forbes the certainty that the glacier advances by the gradual differential motion of contiguous portions of the mass, and not by the separate advance of large masses which are divided by crevasses or open cracks. I may observe, that all the conclusions of Professor Forbes on this head were confirmed by observations made by me in 1845 on the Gorner and Findelen glaciers near Zermatt, which have remained unpublished, because I conceived that the proofs given by Professor Forbes were already sufficient and complete.

I cannot, indeed, conceive any explanation of the facts known as to glacier movement which can consist with the entire absence of minute cracks (whether capillary or not) penetrating that part of the ice which is in course of gradual motion. True it is that under pressure these cracks will be closed up again, and therefore those parts of the ice not actually undergoing the process which causes the motion may well be free from fissures. Pending further observations, which I trust Mr. Huxley may be induced to make, I shall venture to adhere to the belief that the irregular network of fissures which pervades the surface of the ice when exposed to air and warmth, represents a structure already existing in the ice before it came to the surface; and that former fissures, though possibly closed so as to show no trace under the microscope, may yet be *surfaces of easy melting,* which on the first application of heat are recalled into existence.

Air-bubbles in the ice.—In common, as I suppose, with many readers of Mr. Huxley's very interesting observations, the absence of more precise details as to the circumstances under which they were made is to me a matter of regret. It is allowable to suppose that his visits to the glaciers were generally made in the middle of the day, and during fine weather, and in that case I feel some doubt whether the ice within 8 or 10 inches of the surface is in the same conditions of temperature as that of the interior of the glacier. I would submit that the hour of the day, the state of the weather, and of the thermometer, at the time and previous to the observations, and also a statement of the precautions taken to prevent the pieces of ice taken out from undergoing changes by exposure to sun and air, would be a valuable addition to the interesting particulars for which we are indebted to Mr. Huxley. Such details seem necessary for satisfactory proof of Mr. Huxley's conclusion, that the entire ice of the glacier is pervaded by cells containing both water and air, a fact which seems, however, no way inconsistent with rational views of the conditions of temperature in the interior of glaciers.

Dirt-streaks.—I believe that there is no material inconsistency between what I have said as to the dirt-bands of M. Agassiz, and Mr. Huxley's observations on the same phænomenon, for which I gladly adopt the name "dirt-streaks." I would observe, however, that the illustration given at pl. 5 of the 'Travels in the Alps,' to which Mr. Huxley refers his readers, is far less accurate than most of those contained in the same work. A glance at the plate is sufficient to show that the lines represented on the glacier cannot possibly represent the same thing in the foreground, sixty or eighty yards from the eye, and in the middle distance more than half a mile from the point whence the sketch is taken. It is true that something of the structure observed by Mr. Huxley may be traced for some distance along the side of the glacier above the great moraine; but neither on the Brenva, nor on any other glacier, have I ever seen anything like a system of continuous lines or streaks distinctly visible at a distance, and over a wide breadth of glacier, as represented in this illustration.

One word more as to the so-called lenticular structure. When Mr. Huxley speaks of veins of ice 9 or 10 inches in thickness and apparently lenticular, which I suppose means that the exposed section thins out on either side, I venture to question whether such veins form any part of the real veined structure, and whether they are not instances of what is by no means rare,—the filling-up of small crevasses with ice generally differing in colour and appearance from the surrounding mass. I do not recollect to have attended to the length of the veins, or the exposed sections of them on the glacier of the Brenva, but I can only say that if 9 feet be the greatest length that can there be traced, the structure of that glacier must differ from all those of the same class that I have ever been able to examine.

LVII. *On the Influence of Light upon Chlorine.*

By Prof. H. E. Roscoe, *Ph.D. &c.*

To the Editors of the Philosophical Magazine and Journal.

Owens College, Manchester,
November 1857.

Gentlemen,

I WISH to make a few observations upon a communication of Dr. Draper's, published in the last (November) Number of the Philosophical Magazine, in which he refers to the researches on the chemical action of light, recently published by Professor Bunsen and myself, in a manner calculated to mislead many of your readers as to the results we obtained, and the conclusions which must follow therefrom.

In 1843, Dr. Draper observed that when a mixture of chlorine and hydrogen is exposed to light from various sources, a certain time elapses before combination begins, and that, when commenced, the action continually increases, until at length a permanently constant maximum has arrived. In our experiments we have observed the same fact, and have devoted one complete section of our research to the minute consideration of these remarkable relations. We differ widely, however, from Dr. Draper as to the cause of this phænomenon; and it is the experimental evidence upon which we rest our explanation which I think it advisable here briefly to state.

On first thoughts one would naturally conclude that the cause of this peculiar phænomenon was to be sought in some allotropic change which one or both gases undergo on exposure to light, in which state the combining powers are heightened, and the subsequent combination on insolation thus rendered possible. This is, in fact, the explanation given by Dr. Draper. He supposes that during the first insolation it is the chlorine which undergoes a change of properties; and he convinced himself by various experiments that this is the case, and that the chlorine retains this condition of heightened chemical activity when preserved in the dark for a long period, extending in some cases over several weeks. We have with great care frequently repeated these experiments, avoiding the errors to which they were subject, and we have not in any one case succeeded in corroborating Dr. Draper's statements. We found, that to whatever kind or amount of light the gas had been subjected, it rapidly returned to its inactive condition on exclusion of light; that on standing in the dark for about half an hour, the gas did not possess any properties different from those which it possessed before it had been exposed to light. Hence Dr. Draper's position, as regards a permanent allotropic condition of either gas, becomes untenable. That Dr. Draper arrived at these conclusions by help of the experiments mentioned in his former papers in your Journal, will not surprise us when we become acquainted with the difficulty of obtaining by any means accurate results in this subject, and the impossibility of so doing by methods so imperfect as those which Dr. Draper employed.

We have shown, in our second paper read before the Royal Society, that the presence of a trace of foreign gas, not exceeding $\frac{1}{100,000}$dth of the total volume, is sufficient to alter in the most marked degree the photo-chemical sensibility of the mixture. Dr. Draper derives the proof of the existence of a permanent allotropic modification of chlorine from (amongst others) experiments in which that gas was collected over a solution of chloride of sodium, then exposed to the sun, and afterwards mixed with its own volume of hydrogen, and the rate observed at which the mixture combined on subsequent insolation, compared with a similar mixture not previously exposed. The limits of error introduced by this and the other modes of experimentation adopted by Dr. Draper to determine this question, extend, as we have shown in our paper, even beyond the differences in the action arising from the phænomenon itself, and hence his conclusions cannot be relied upon.

In order to determine whether either gas undergoes on exposure any change in properties, whether permanent or not, we passed (with all due precautions) the gases, evolved electrolytically, separately through long tubes which could be exposed to the direct solar rays; and on observing the photo-chemical properties of the mixed chlorine and hydrogen (by gas-light), first, when the separated gases had been strongly insolated, and secondly, when they had been carefully protected from light, we found that in the one case the combination did not take place more rapidly than it did in the second; conclusively proving that the previous insolation of the separated gases, and hence any allotropic modification of either gas, cannot account for this slow action on the first exposure to light. Dr. Draper's objection, that we did not expose the gases for a sufficient time to solar action, is not valid, inasmuch as the direct solar light to which they were separately exposed was several thousand times as intense as the small gas-flame which in four minutes effected the maximum action.

The phænomenon in question depends upon a totally different cause. It belongs to a most important, but hitherto disregarded phase of general chemical action. In all chemical combinations and decompositions a certain time must elapse before the full action is set up. In no case does chemical action commence instantly on bringing the bodies under the requisite conditions; and in many instances the length of time which elapses before the full action occurs is very considerable, so that the progressive phases of the combination can be distinctly traced.

The phænomenon which we have considered is a striking example of this general condition of chemical action, of this increase of the chemical activity with the time under which the forces act. This general mode of action has been named by us Chemical Induction. We have shown in our paper on Photo-chemical Induction, that precisely similar relations are observable in the photographic phænomena, and also in pure chemical actions; our investigations form, in fact, starting-points for the consideration of time as affecting chemical action, a subject which I am at present engaged in following out.

It is almost superfluous for me to mention, regarding Dr. Draper's experiment on the solar decomposition of chloride of silver, that before one can believe that the black substance insoluble in nitric acid is either a modification of the metal or "a something which is not silver," we must know that it is not a subchloride, particularly as Wöhler states that suboxide of silver receives a metallic lustre on burnishing.

I am yours truly,

Henry E. Roscoe.

LVIII. *Methods in the Integral Calculus.* *By the* Rev. Robert Carmichael, *A.M., Fellow of Trinity College, Dublin**.

1. An interesting exemplification of the reciprocal aid which geometry and analysis lend to each other is afforded by the fact, that there is a large class of differential equations whose integration may be much facilitated by the employment of geometrical considerations, more especially those associated with the transformation to polar coordinates. It is obvious, too, that where differential equations admit of geometrical interpretations, such interpretations are more likely to be suggested in this way than where their integration is conducted by methods purely analytical.

Thus, as a simple example, if it be proposed to integrate the equation

$$xdx+ydy=m(ydx-xdy),$$

by transformation to polar coordinates we get at once

$$rdr=mr^2d\theta, \text{ or } dr=mrd\theta;$$

whence, instantly, the curve denoted by the given equation is the logarithmic spiral

$$r=\mathrm{C}e^{m\theta},$$

or, in rectangular coordinates,

$$(x^2+y^2)^{\frac{1}{2}}=\mathrm{C}e^{m\tan^{-1}\left(\frac{y}{x}\right)}.$$

The immediate geometrical interpretation of the given equation is easily seen by dividing both sides of its transformed type by ds. In fact, then, we get

$$r\frac{dr}{ds}=m\frac{r^2d\theta}{ds};$$

and, if Q be the intercept between the point on the curve and the foot of the perpendicular from the origin on the tangent (P), this equation is at once equivalent to

$$\mathrm{Q}=m\mathrm{P};$$

or the given differential equation represents, as is known, the plane curve for which the perpendicular from the origin on the tangent is in a constant proportion to the intercept between the corresponding point of contact and the foot of the perpendicular. The curve which satisfies this condition is, as we have seen, the logarithmic spiral.

Thus the immediate geometrical interpretation of the given equation, as well as the more remote, are with equal facility ob-

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* Communicated by the Author.

tained; the latter being the curve required, the former the property of the curve indicated by the given equation.

2. Again, if it be proposed to integrate the differential equation $(xdx+ydy+zdz)^2=m^2\{(ydy-zdy)^2+(zdx-xdz)^2+(xdy-ydx)^2\}$, and at the same time to determine its geometrical interpretations, both immediate and remote, the same method can be employed with advantage. The equation obviously represents a curve of double curvature; and, remembering the expressions for the projections on the coordinate planes of the infinitely small triangle standing on the element of a curve with its vertex at the origin, we have at once for the immediate geometrical interpretation of the given equation

$$\mathrm{Q}=m\mathrm{P},$$

Q and P having the same meanings as before.

For the remote geometrical interpretation, or the integration of the given equation, we have

$$rdr=mr^2d\phi,$$

whence

$$r=\mathrm{C}e^{m\phi},$$

where ϕ is the whole angle swept out by the radius vector.

3. An inquiry obviously suggested by the last example, is the integration of the *partial* differential equation resembling it in form, namely,

$$\left(y\frac{du}{dz}-z\frac{du}{dy}\right)^2+\left(z\frac{du}{dx}-x\frac{du}{dz}\right)^2+\left(x\frac{du}{dy}-y\frac{du}{dx}\right)^2=m^2\left\{x\frac{du}{dx}+y\frac{du}{dy}+z\frac{du}{dz}\right\}^2,$$

and the determination of its geometrical interpretation, if it have any.

The equation obviously represents some surface; and if both sides of the equation be divided by

$$\left(\frac{du}{dx}\right)^2+\left(\frac{du}{dy}\right)^2+\left(\frac{du}{dz}\right)^2,$$

it will be easily seen that, ψ being the angle between the radius vector to the point (x, y, z) and the perpendicular on the tangent plane at that point, the given equation is equivalent to

$$r^2\sin^2\psi=\mathrm{Q}^2=m^2\mathrm{P}^2,$$

or the given equation represents the surface for which the perpendicular from the origin on any tangent plane is in a constant ratio to the intercept between the point of contact and the foot of the perpendicular.

Now putting

$$\mathrm{Q}^2=r^2-\mathrm{P}^2,$$

and substituting, we get

$$r^2=(1+m^2)P^2;$$

and, remembering that in polar coordinates

$$P=\frac{r^2\sin\theta}{\sqrt{\left(\frac{dr}{d\phi}\right)^2+r^2\sin^2\theta+\sin^2\theta\left(\frac{dr}{d\theta}\right)^2}},$$

we get

$$\left(\frac{dr}{d\phi}\right)^2+r^2\sin^2\theta+\sin^2\theta\left(\frac{dr}{d\theta}\right)^2=(1+m^2)r^2\sin^2\theta,$$

or

$$\left(\frac{dr}{d\phi}\right)^2=\sin^2\theta\left\{m^2r^2-\left(\frac{dr}{d\theta}\right)^2\right\}.$$

This, then, is the result of the transformation of the given equation to polar coordinates, the deduction of which by direct substitution would have been tedious and laborious, and the expression thus reduced may be integrated by series. It can be easily proved that the quadrature of the surface represented by this equation is given by the simple formula

$$\Sigma=(1+m^2)^{\frac{1}{2}}\iint r^2\sin\theta\,d\theta\,d\phi,$$

a result which indeed might have been anticipated from geometrical considerations.

4. There are some differential equations whose integration may be facilitated by a partial employment of the above method.

Thus if it be proposed to integrate the equation

$$ydx-xdy=(x^2+y^2)^{\frac{1}{2}}dx,$$

by partial transformation to polar coordinates we get

$$r^2d\theta=rdx,\ \text{or}\ rd\theta=dx,$$

or

$$\frac{d\theta}{\cos\theta}=\frac{dx}{x},$$

whence, at once, the integral

$$\tan\left(\frac{\pi}{4}+\frac{\theta}{2}\right)=Cx.$$

It is not difficult to reduce this solution to the form

$$\tan^{-1}\left(\frac{y}{x}\right)=2\tan^{-1}\left(\frac{Cx-1}{Cx+1}\right).$$

5. Again, let it be proposed to integrate the equation

$$y(ydx-xdy)=(x^2+y^2)e^{-\frac{y}{x}}dx;$$

employing partial transformation as before, we get

$$r \sin \theta \, d\theta = e^{-\tan \theta} dx,$$

or

$$e^{\tan \theta} \tan \theta \, d\theta = \frac{dx}{x}.$$

Thus the variables are separated, and the equation is reduced to an integrable form.

6. If the equation to be integrated were

$$y(ydx - xdy) = (x+y)^2 . \, e^{-\frac{y}{x}} dx,$$

we have

$$r \sin \theta d\theta = (\sin \theta + \cos \theta)^2 e^{-\tan \theta} dx,$$

or

$$\frac{e^{\tan \theta} \tan \theta \, d\theta}{1 + \sin 2\theta} = \frac{dx}{x},$$

where, as before, the variables are separated, and the equation reduced to an integrable form.

7. There are some cases in which the solution of partial differential equations may be facilitated by transformation to polar coordinates. In these cases the partial differential equations reduce themselves to complete differential equations in a single variable. Thus the equation

$$x \frac{dz}{dy} - y \frac{dz}{dx} = \mathrm{A}_n \left(\frac{y}{x}\right)^n + \mathrm{A}_{n-1} \left(\frac{y}{x}\right)^{n-1} + \&c.$$

is reduced at once to the form

$$\frac{dz}{d\theta} = \mathrm{A}_n \tan^n \theta + \mathrm{A}_{n-1} \tan^{n-1} \theta + \&c.,$$

which is easily integrated, and θ replaced by x and y.

8. Let it be proposed to integrate the system of simultaneous partial differential equations

$$\left.\begin{aligned} \frac{du}{dx} &= c_1 y + b_1 z, \\ \frac{du}{dy} &= a_1 z + c_1 x, \\ \frac{du}{dz} &= b_1 x + a_1 y. \end{aligned}\right\}$$

Multiply the first equation by x, the second by y, and the third by z; add; and we obtain

$$\left(x \frac{d}{dx} + y \frac{d}{dy} + z \frac{d}{dz}\right) . \, u = 2(a_1 yz + b_1 zx + c_1 xy),$$

whence at once

$$u=(a_1yz+b_1zx+c_1xy)+u_0,$$

where u_0 denotes an arbitrary homogeneous function in x, y, z of the order zero.

It is to be observed, however, that u_0 *cannot contain any inverse powers of* x, y, z. For instance, u_0 cannot be of the form

$$\frac{\alpha x+\beta y}{\gamma x+\delta y},$$

as in that case u would not satisfy *the separate* partial differential equations.

Thus the ultimate form of the required solution is

$$u=\tfrac{1}{2}(a_1yz+b_1zx+c_1xy)+k,$$

where k is an arbitrary constant.

9. Again, let it be proposed to integrate the system of partial differential equations

$$\left.\begin{aligned}\frac{d^2w}{dx^2}&=\lambda,\\ \frac{d^2w}{dx\,dy}&=\mu,\\ \frac{d^2w}{dy^2}&=\nu,\end{aligned}\right\}$$

where λ, μ, ν are given constants; or, more generally, the system

$$\left.\begin{aligned}\frac{d^2w}{dx^2}&=ax^2+by^2+\lambda,\\ \frac{d^2w}{dx\,dy}&=\qquad bxy+\mu,\\ \frac{d^2w}{dy^2}&=cy^2+bx^2+\nu.\end{aligned}\right\}$$

Multiply the first equation by x^2, the second by $2xy$, and the third by y^2; add; and putting the symbol

$$x\frac{d}{dx}+y\frac{d}{dy}+z\frac{d}{dz}=\nabla,$$

we obtain

$$\nabla(\nabla-1)\,.\,w=(ax^4+4bx^2y^2+cy^4)+(\lambda x^2+2\mu xy+\nu y^2),$$

whence at once

$$w=\tfrac{1}{12}(ax^4+4bx^2y^2+cy^4)+\tfrac{1}{2}(\lambda x^2+2\mu xy+\nu y^2)+u_1+u_0,$$

where u_1, u_0 are arbitrary homogeneous functions in x, y, z of the degrees unity and zero respectively.

For the same reason as in the last article, u_1 and u_0 cannot contain any inverse powers of x, y, z.

Thus the ultimate form of the required solution is

$$w=\tfrac{1}{12}(ax^4+4bx^2y^2+cy^4)+\tfrac{1}{2}(\lambda x^2+2\mu xy+\nu y^2)+(\alpha x+\beta y+\gamma z)+\delta,$$

where $\alpha, \beta, \gamma, \delta$ are arbitrary constants.

10. Again, let it be proposed to integrate the system

$$\left.\begin{array}{l} \dfrac{d^2u}{dx^2}+\dfrac{2}{x}\dfrac{du}{dx}=2ax+by+\dfrac{cy^2}{x} \\[2ex] \dfrac{d^2u}{dy^2}+\dfrac{2}{y}\dfrac{du}{dy}=2dy+cx+\dfrac{bx^2}{y} \end{array}\right\}.$$

Multiplying the first equation by x^2, the second by y^2, and adding, as before, we get

$$\nabla(\nabla+1)\,.\,u=2(ax^3+byx^2+cxy^2+dy^3),$$

whence

$$u=\tfrac{1}{6}(ax^3+byx^2+cxy^2+dy^3)+\alpha,$$

the arbitrary constants in the second arbitrary function being of necessity reduced to cipher.

Trinity College, Dublin,
October 10, 1857.

LIX. *Experimental Relations of Gold (and other Metals) to Light.—The Bakerian Lecture.* By Michael Faraday, *Esq., D.C.L., F.R.S., Fullerian Prof. Chem. Royal Institution, &c.*

[Concluded from p. 417.]

Diffused particles of gold—production—proportionate size—colour—aggregation and other changes.

AGENTS competent to reduce gold from its solution are very numerous, and may be applied in many different ways, leaving it either in films, or in an excessively subdivided condition. Phosphorus is a very favourable agent when the latter object is in view. If a piece of this substance be placed under the surface of a moderately strong solution of chloride of gold, the reduced metal adheres to the phosphorus as a granular crystalline crust. If the solution be weak and the phosphorus clean, part of the gold is reduced in exceedingly fine particles, which, becoming diffused, produce a beautiful ruby fluid.

This ruby fluid is well obtained by pouring a weak solution of gold over the phosphorus which has been employed to produce films, and allowing it to stand for twenty-four or forty-eight hours; but in that case all floating particles of phosphorus should be removed. If a stronger solution of gold be employed, the ruby fluid is formed, but it soon becomes turbid and tends to produce a deposit. When the gold is in such proportion that it remains in considerable excess, still the ruby formation is not prevented, and being formed, it mingles unchanged with the excess of gold in solution. If an exceedingly weak solution of gold be employed, the production of ruby appears to be imperfect and retarded. The nearer the solution is to neutrality at the commencement the better; when a little hydrochloric acid was added, the effect was not so good, and the colour of the fluid was more violet than ruby.

If a pint or two of the weak solution of gold before described be put into a *very clean* glass bottle, a drop of the solution of phosphorus in sulphide of carbon added, and the whole well shaken together, it immediately changes in appearance, becomes red, and being left for six or twelve hours, forms the ruby fluid required; too much sulphide and phosphorus should not be added, for the reduced gold then tends to clot about the portions which sink to the bottom.

Though the sulphide of carbon is present in such processes and very useful in giving division to the phosphorus, still it is not essential. A piece of clean phosphorus in a bottle of the gold solution gradually produces the ruby fluid at the bottom, but the action is very slow. If the phosphorus be attached to the side of the bottle, but always beneath the surface of the solution, the streams of ruby fluid may be seen moving both upwards and downwards against the side of the glass, and forming films in the vicinity of the phosphorus perfect in their golden reflexion, and yet transmitting light of ruby, violet, and other tints, thus giving, first a proof that the particles are gold, and then connecting the present condition of the gold with that of the films already described. On the other hand, the phosphorus may be excluded and the sulphide of carbon employed alone; for when it and the solution of gold are shaken together, the gold is reduced and the ruby fluid formed; but it soon changes to purple or violet.

A quick and ready mode of producing the ruby fluid, is to put a quart of the weak solution of gold (containing about 0·6 of a grain of metal) into a clean bottle, to add a little solution of phosphorus in æther, and then to shake it well for a few moments: a beautiful ruby or amethystine fluid is immediately produced, which will increase in depth of tint by a little time. Generally, however, the preparations made with phosphorus dissolved in sulphide of carbon, are more ruby than those where æther is the phosphorus solvent. The process of reduction appears to consist in a transfer of the chlorine from the gold to the phosphorus, and the formation of phosphoric or phosphorus acids and hydrochloric acid, by the further action of the water.

The fluids produced may easily be tested for any gold yet remaining unreduced, by trial of a portion with solution of protochloride of tin. If any be found, it is easily reduced by the addition of a little more of the phosphorus in solution. After all the gold is separated as solid particles, the fluid may be considered in its perfected state. Occasionally it may smell of phosphorus in excess, even after it has been poured off from the deposited particles of it and the sulphide. In that case it is easy to deprive it of this excess by agitation in a bottle with air. When kept in closed vessels mouldiness often occurs. If this be in groups, it is collected with facility at the end of a splinter of wood and removed, or the whole fluid may be poured through a wet plug of cotton in the neck of a funnel, the reduced gold passing freely. All the vessels used in these operations must be very clean; though of glass they should not be supposed in proper condition after wiping, but should be soaked in water, and after that rinsed with distilled water. A glass supposed to be clean, and even a new bottle, is quite able to change the character of a given gold fluid.

Fluids thus prepared may differ much in appearance. Those from the basins, or from the stronger solutions of gold, are often evidently turbid, looking brown or violet in different lights. Those prepared with weaker solutions and in bottles, are frequently more amethystine or ruby in colour and apparently clear. The latter, when in their finest state, often remain unchanged for many months, and have all the appearance of solutions. But they never are such, containing in fact no dissolved, but only diffused gold. The particles are easily rendered evident, by gathering the rays of the sun (or a lamp) into a cone by a lens, and sending the part of the cone near the focus into the fluid; the cone becomes visible, and though the illuminated particles cannot be distinguished because of their minuteness, yet the light they reflect is golden in character, and seen to be abundant in proportion to the quantity of solid gold present. Portions of fluid so dilute as to show no trace of gold, by colour or appearance, can have the presence of the diffused solid particles rendered evident by the sun in this way. When the preparation is deep in tint, then common observation by reflected light shows the suspended particles, for they produce a turbidness and degree of opacity which is sufficiently evident. Such a preparation contained in a pint bottle will seem of a dull pale-brown colour, and nearly opake by reflexion, and yet by transmission appear to be a fine ruby, either clear or only slightly opalescent.

That the ruby and amethystine fluids hold the particles in suspension only, is also shown by the deposit which occurs when they are left at rest. If the gold be comparatively abundant, a part will soon settle, *i. e.* in twenty-four or forty-eight hours; but if the preparation be left for six or eight months, a part will still remain suspended. Even in these portions, however, the diffused state of the gold is evident; for where, as in some cases, the top to the depth of half an inch or more has become clear, it is seen that the ruby portion below is as a cloud sinking from it; and in the part which has apparently been cleared from colour by the settling of the particles, the lens and cone of light still show the few, or rather the fine diffused particles yet in suspension, though the protochloride of tin can show no gold in solution. The mould or mucus before spoken of, often collects the larger, heavier particles, and becomes of a dark blue colour; it may then be taken out by a splinter of wood, and being shaken in water, disengages the particles, which issue from it in clouds like the sporules from a ripe puff-ball.

A gradual change goes on amongst the particles diffused through these fluids, especially in the cases where the gold is comparatively abundant. It appears to consist of an aggregation. Fluids, at first clear or almost clear to ordinary observation, become turbid; being left to stand for a few days, a deposit falls. If the supernatant fluid be separated and left to stand, another deposit may be obtained. This process may be repeated, and whilst the deposition goes on, the particles in the fluid still seem to aggregate; it is only when the fluid is deprived of much gold that the process appears to stop. Even after the fluid has attained a fine marked ruby tint, if allowed to stand for months in a place of equable temperature, the colouring particles will appear in floating clouds, and probably the aggregation is then still going on. That the particles of gold when they touch each other do in many cases adhere together with facility, is shown in many experiments. In order to test this matter mechanically, I gave much agitation to a dense ruby fluid, but did not find it cause any sensible change in the character. When gold particles of a much larger size were agitated in water, they did cohere together, and the fluid, which required a certain time for settling at the beginning of the experiment, settled in a much shorter time at the termination.

If these fluids be examined generally their appearances differ, not merely under different circumstances, but also under the same circumstances, though they always consist of a colourless liquid and diffused particles of gold. A certain fluid in a bottle or glass, looked at from the front, *i. e.* the illuminated side by general daylight, may appear hazy and amethystine, whilst in bright sunlight it will appear light brown and almost opake.

From behind, the same fluid may appear of a pure blue in both lights, whilst from the side it may appear amethystine or ruby. These differences result from the mixture of reflected and transmitted lights, both derived from the particles, the former appearing in greatest abundance from the front or side, and the latter from behind. The former is seen by common observation in a purer state if a black background be placed behind the fluid; when a white background is there, much of the transmitted light from that source comes to the eye, and the appearance is greatly altered. A mode of observing the former by a strong ray of light and a lens has been already described; but even in that case some effects of transmitted light are observed if the focus is thrown deep into the fluid; and it is only the particles near the surface, whether illuminated by the base or the apex of the cone, which give the nearly pure effect of reflexion. In order to observe the transmitted ray in an unmingled state, a glass tube closed at one end was surrounded with a tube of black paper longer than itself, and with the black surface inwards. When a fluid (or the particles in it) was to be examined, it was put into this tube, and a surface of white paper illuminated by daylight or the sun, regarded through it, other light being excluded from the eye; or the tube was sometimes interposed between the eye and the sky, and sometimes the rays of the sun itself were reflected up to the eye through it. In speaking hereafter of the tints of the light transmitted by the particles (which will of course vary with the proportion of different rays in the original beam of light), a pure white original light is to be understood, but occasionally differently-tinted papers were employed with this tube as sources of different coloured lights.

The very oblique angle at which reflected light comes to the eye from the diffused particles, is well seen when the lens cone, or a direct ray of the sun, is passed into the fluid and observed from different positions; it is only when the eye is behind and nearly in the line of the ray, that the unmixed transmitted ray is observed. In the dark tube I think that no reflected light arrives at the eye: for if half an inch in depth of water be introduced, white light passes; if a drop of the washed deposit, to be hereafter described, be introduced, the light transmitted is either blue or ruby, or of other intermediate tint, according to the character of the deposit; but if water be then added until the column is six inches or more in length, the quantity of light transmitted does not sensibly alter, nor its tint; a fact which I think excludes the idea of any light being reflected from particle to particle, and finally to the eye.

If a given ruby-tinted fluid, containing no gold in solution, be allowed to stand for a few days, a deposit will fall from which the fluid may be removed by a siphon; being now allowed to stand for a week, a second deposit will be produced; if the fluid be again removed and allowed to stand for some months, another deposit will be obtained, and the fluid will probably be of a bright ruby; if it be now allowed to stand for several months, it will still yield a deposit, looking however more like a ruby fluid than a collection of fine particles at the bottom of the fluid, whilst traces of yet finer particles of gold in suspension may be obtained by the lens. All these deposits may be washed with water and will settle again; the coarser are not much affected, but the finer are, and tend to aggregate; nevertheless specimens often occur, especially after boiling, which tend to preserve their fine character after washing, if the water be very clean and pure.

The colour of these particles whilst under, or diffused through water, is, by common reflected light, brown, paler and richer, sometimes tending to yellow, and sometimes to red. The same difference is shown when illuminated by sunlight. Everything tends to show that the light reflected is very bright considering the size of the particles, and therefore of the reflecting surfaces; yet comparing by the cone of light a ruby fluid when first prepared and before it has become very sensibly turbid, with the same fluid after the evident turbidity is produced, in both of which cases I believe the gold to be in solid metallic particles, though of different sizes, it would seem that more light is transmitted and absorbed and less reflected by the finer particles than by the coarser set, the same quantity of gold being in the same space. I believe that there may be particles so fine as to reflect very little light indeed, that function being almost gone. Occasionally some of the fluids containing the very finest particles in suspension, when illuminated by the sun's rays and a lens, appeared to give a fine green reflexion, but whether this is a true colour as compared to white light, or only the effect of contrast with the bright ruby in the other parts of the fluid, I am not prepared to say.

When the deposits were examined in the dark tube by transmitted light, being first diffused in more or less water to give them the form of fluid, those first deposited, and therefore presumed to be the heavier and larger, transmitted a pure blue light. The second and the third had the same character, perhaps the fourth, if the subdivision into portions had been numerous; then came some which transmitted an amethystine ray from the white of paper; and others followed progressing to the finest, which transmitted a rich ruby tint. It is probable that many of these deposits were mixtures of particles having different characters, and this is perhaps the reason that in some cases, when the fluids were contained in round-bottomed flasks, the lens-like deposit was ruby at the edges, though deep violet in the middle, the former having settled last; but as a pure blue deposit could be obtained, and also one transmitting a pure ruby ray, and as a comparatively pure intermediate preparation transmitting a ruby violet, or amethystine ray, was obtained, it is probable that all gradations from blue to ruby exist; for the production of which I can see no reason to imagine any other variation than the existence of particles of intermediate sizes or proportions.

When light other than white was passed through the fluids, then of course other tints were produced, yet some of these were unexpected. A fluid of a pure blue colour, whilst in the dark tube, would in an open glass and by reflected light appear of a strong ruby-violet tint. Dropping some of the wet deposit into pure water, the striæ which it formed would in one part be ruby in colour and in another violet: these effects were referable to the light reflected from the solid particles back through the fluid to the eye, but it seemed redder than any which light reflected from gold was likely to produce. However, upon regarding the surface of dull gold-leaf, or the thick wet deposit of gold, or the hand, it was found that the red rays easily passed through the blue fluid and formed a ruby-violet tint. Prevost showed in old times, how much the red and warm rays are reflected by gold, in preference to the others contained in white light.

The supernatant fluid in specimens that had stood long and deposited, was always ruby; yet because it showed no dissolved gold, because it showed the illuminated cone by the lens, and because by standing ruby clouds settled in it, there was every reason to believe that the gold was there in separated particles, and that such specimens afforded cases of extreme division, which by long standing would form deposits of the finest kind.

Those fluids which on standing gave abundance of deposits, transmitting blue light, consisted in the first instance of particles transmitting a ruby light, and in these cases it would seem that the particles at their first separation were always competent to transmit this ruby light; and if the preparation were not too rich in gold the ruby condition appeared to be retained, the division being then most extreme. But purple or amethystine fluids could be procured, which, containing no colouring particles other than suspended gold, still retained them in suspension for many months together, so that they must have been as light or as finely divided as those in the ruby fluids. When the phosphorous æther was employed for the reduction of the gold, such fluids occurred; also when the solution of the phosphorus in sulphide of carbon was used, provided the solution of gold had a very little chloride of sodium contained in it. They appear to show that the mere degree of division is not the only circumstance which determines the aptitude to transmit in preference this or that ray of light.

Considering the fluids as owing their properties to diffused particles, it may be observed, that many of them which in small quantities in the dark tube transmit an amethystine light, send forward a ruby light when the quantity is increased; and this appears to be the general progression. I have not found any which by increase in quantity tended to transmit the blue rays in preference to the red.

Elevation of temperature had an effect upon these fluids which is advantageous in their preparation. On boiling an apparently clear ruby fluid for some time, its colour passed a little towards amethystine, and on boiling a like amethystine fluid, its tint passed towards blue. The separation of the gold particles was also facilitated, for now they would settle in three or four days from a fluid which, prior to this operation, would not have deposited them in an equal degree for weeks. In the case of the ruby fluids the colour often became more rosy and luminous, and by reflected light the fluid seemed to have become more turbid, as if the particles had gained in reflective power; in fact, the boiling often appeared to confer a sort of permanency on the particles in their new state. When settled, they formed collections looking like little lenses of a deep ruby or violet colour, at the bottom of the flasks containing the fluid; when all was shaken up, the original fluid was reproduced, and then, by rest, the gold re-settled. This effect could be obtained repeatedly. The particles could fall together within a certain limit, but many weeks did not bring them nearer or into contact; for they remained free to be diffused by agitation. The space they occupied in this lens-like form must have been a hundredfold, or even a thousandfold, more than that which they would have filled as solid gold. Whether the particles be considered as mutually repulsive, or else as molecules of gold with associated envelopes of water, they evidently differ in their physical condition, for the time, from those particles which by the application of salt or other substances are rendered mutually adhesive, and so fall and clot together.

In preparing some of these fluids, I made the solution of gold hot and boiling before adding the solution of phosphorus. The phænomena were the same in kind as before: but when the phosphorus was dissolved in sulphide of carbon, the gold soon fell as a dark flocculent deposit; when it was dissolved in æther, a more permanent turbid ruby fluid was obtained, which, if it does not go on changing in aggregation, may give a good ruby deposit.

The particles in these fluids are remarkable for a set of physical alterations occasioned by bodies in small quantities, which do not act chemically on the gold, or change its intrinsic nature; for through all of them it seems to remain gold in a fine state of division. They occur most readily where the particles are finest, *i. e.* in the ruby fluids, and so readily that it is difficult to avoid them; they are often occasioned by the contact of vessels which are supposed to be perfectly clean. An idea of their nature may be obtained in the following manner. Place a layer of ruby fluid in a clean white plate, dip the tip of a glass rod in a solution of common salt and touch the ruby fluid; in a few moments the fluid will become blue or violet-blue, and sometimes almost colourless: by mingling up the neighbouring parts of the fluid, it will be seen how large a portion of it can be affected by a small quantity of the salt. By leaving the whole quiet, it will be found that the changed gold tends to deposit far more readily than when in the ruby state. If the experiment be made with a body of fluid in a glass, twelve or twenty-four hours will suffice to separate gold which in the ruby state has remained suspended for six months.

The fluid changed by common salt or otherwise, when most altered, is of a violet-blue, or deep blue. Any tint, however, between this and the ruby may be obtained, and, as it appears to me, in either of two ways; for the intermediate fluid may be a mixture of ruby and violet fluids, or, as is often the case, all the gold in the fluid may be in the state producing the intermediate colour: but as the fluid may in all cases be carried on to the final violet-blue state, I will, for brevity sake, describe that only in a particular manner. The violet or blue fluid, when examined by the sun's rays and a lens, always gives evidence showing that the gold has not been redissolved, but is still in solid separate particles; and this is confirmed by the non-action of protochloride of tin, which, in properly prepared fluids, gives no indication of dissolved gold. When a ruby solution is rendered blue by common salt, the separation of the gold as a precipitate is greatly hastened; thus when a glass jar containing about half a pint of the ruby fluid had a few drops of brine added and stirred into the lower part, the lower half of the fluid became blue whilst the upper remained ruby; in that state the cone of sun's rays was beautifully developed in both parts. On standing for four hours the lower part became paler, a dark deposit of gold fell, and then the cone was feebly luminous there, though as bright as ever in the ruby above. In three days no cone was visible in the lower fluid; a fine cone appeared in the upper. After many days the salt diffused gradually through the whole, first turning the gold it came in contact with blue, and then causing its precipitation.

Such results would seem to show that this blue gold is aggregated gold, *i. e.* gold in larger particles than before, since they precipitate through the fluid in a time which is as nothing to that required by the particles of the ruby fluid from which they are obtained. But that the blue particles are always merely larger particles does not seem admissible for a moment, inasmuch as violet or blue fluids may be obtained in which the particles will remain in suspension as long as in the ruby fluids; there is probably some physical change in the condition of the particles, caused by the presence of the salt and such affecting media, which is not a change of the gold as gold, but rather a change of the relation of the surface of the particles to the surrounding medium.

When salt is added in such quantity as to produce its effect in a short time, it is seen that the gold reflexion of the particles is quickly diminished, so that either as a general turbidness or by the cone of rays it becomes less visible; at last the metal contracts into masses, which are comparatively so few and separate, that when shaken up in the fluid, they confer little or no colour or character, either by reflected or transmitted light. In these cases no re-solution of the metal is effected, for neither the salt nor hydrochloric acid, when used in like manner, have any power to redissolve the gold. The same aggregating effect is shown with all the fluids whatever their colour, and also with the deposits that settle down from them. When salt is added to the solution of gold *before* the phosphorus, and therefore before the reduction of the gold, the fluid first produced is always ruby; but it becomes violet, purple, or blue, with a facility in proportion to the quantity of salt present. If that be but small, the ruby will remain for many days unchanged in colour, and the violet-ruby for many weeks, before the gold will be deposited, the degree of dilution or concentration always having its own particular effect, as before described; the more finely divided preparations, *i. e.* the ruby and amethystine, appear to be more permanent than when the salt is added after the separation of the gold.

Many other bodies besides salt have like action on the particles of gold. A ruby fluid is changed to or towards blue by solutions of chlorides of calcium, strontium, manganese; sulphates of magnesia, manganese, lime; nitrates of potassa, soda, baryta, magnesia, manganese; acetates of potassa, soda and lime; these effect the change freely: the sulphate of soda, phosphates of soda and potassa, chlorate of potassa, and acetate of ammonia acted feebly. Sulphuric and hydrochloric acids produce the change, but show no tendency to dissolve the gold. Nitric acid acts in the same manner, but not so strongly: it often causes re-solution of the gold after some time, because of the hydrochloric acid which remains in the fluid.

Amongst the alkalies, potash produces a similar action in a weak degree. So also does soda. Lime-water produces a change in the same direction, but the gold quickly precipitates associated with the lime.

Ammonia causes the ruby fluid to assume a violet tint; the deposit is slow of formation and often ruby in colour; the alkali apparently retards the action of common salt.

Chlorine or nitromuriatic acid turns the ruby fluid blue or violet-blue before they dissolve the gold.

Solution of sulphuretted hydrogen changes the ruby slowly to purple, and finally to deep blue. Æther, alcohol, camphine, sulphide of carbon, gum, sugar, and glycerine cause little or no change in the fluids; but glycerine added to the dense deposits causes serious condensation and alteration of them, so that it could not be employed as a medium for the suspension of particles in the microscope.

All endeavours to convert the violet gold back into ruby were either failures, or very imperfect in their results. A violet fluid will, upon long standing, yield a deposit and a supernatant ruby fluid, but this I believe to be a partial separation of a mixture of violet and ruby gold, by the settlement of the blue or violet gold from ruby gold, which remains longer in suspension. Mucus, which often forms in portions of these fluids that have been exposed to the air, appears sometimes to render a fluid more ruby, but this it does by gathering up the larger violet particles; it often becomes dark blue or even black by the particles of gold adhering to it, many of which may be shaken out by agitation in water; but I never saw it become ruby coloured as a filter can, and I think that in these cases it is the gathering out of the blue or violet particles which makes the fluid left appear more ruby in tint. I have treated blue or violet fluid with phosphorus in various ways, but saw no appearance of a return in any degree towards ruby. Sometimes the fluids possess a tendency to re-solution of the gold, a condition which may often be given by addition of a very little nitric acid, but in these cases the gold does not become ruby before solution. It would rather appear that the finer ruby particles dissolve first; for the tint of the fluid, if ruby-violet at the commencement, changes towards blue. One effect only seemed to show the possibility of a reversion. Filtering-paper rendered ruby by a ruby fluid was washed and dried; being wetted by solution of caustic potash, it did not change; but being heated in a tube with the alkali, it became of a gray-blue tint; pouring off the alkali, washing the paper, and then adding dilute sulphuric or nitric acid to it, there was no change; but on boiling the paper in the mixed acids there was a return, and when the paper was washed and dried, it approached considerably to the original ruby state. Again, potash added to it, rendered it blue, which by washing with water, and especially with a little nitric acid, was much restored towards ruby. These changes may be due to an affection of the surface, or that which may be considered the surface of the particles.

The state of division of these particles must be extreme; they have not as yet been seen by any power of the microscope. Whether those that are ruby have their colour dependent upon a particular degree of division, or generally upon their being under a certain size, or whether it is consequent in part upon some other condition of the particles, is doubtful; for judging of their magnitude by the time occupied in their descent through the fluid, it would appear that violet and blue fluids occur giving violet deposits, which still consist of particles so small as to require a time equally long with the ruby particles for their deposition, and indeed in some specimens to remain undeposited in any time which has yet occurred since their formation. These deposits, when they occur, look like clear solutions in the fluid, even under the highest power of the microscope.

I endeavoured to obtain an idea of the quantity of gold in a given ruby fluid, and for this purpose selected a plate of gold ruby glass, of good full colour, to serve as a standard, and compared different fluids with it, varying their depth, until the light from white paper, transmitted through them, was apparently equal to that transmitted by the standard glass. Then known quantities of these ruby fluids were evaporated to dryness, the gold converted into chloride, and compared by reduction on glass and otherwise with solutions of gold of known strengths. A portion of chloride of gold, containing 0·7 of a grain of metal, was made up to 70 cubic inches by the addition of distilled water, and converted into ruby fluid: on the sixth day it was compared with the ruby glass standard, and with a depth of 1·4 inch was found equal to it; there was just one hundredth of a grain of gold diffused through a cubic inch of fluid. In another comparison, some gold leaves were dissolved and converted into ruby fluid, and compared; the result was a fluid, of which 1·5 inch in depth equalled the standard, a leaf of gold being contained in 27 cubic inches of the fluid. Hence looking through a depth of 2·7 inches, the quantity of gold interposed between the light and the eye would equal that contained in the thickness of a leaf of gold. Though the leaf is green and the fluid ruby, yet it is easy to perceive that more light is transmitted by the latter than the former; but inasmuch as it appears that ruby fluids may exist containing particles of very different sizes (or that settle at least with very different degrees of rapidity), so it is probable that the degree of colour, and the quantity of gold present, may not be always in the same proportion. I need hardly say that mere dilution does not alter the tint sensibly, *i. e.* if a deep ruby fluid be put into a cylindrical vessel, and the eye look through it along the axis of the vessel, dilution of the fluid to eight or ten times its volume does not sensibly alter the light transmitted. From these considerations, it would appear that one volume of gold is present in the ruby fluid in about 750,600 volumes of water; and that whatever the state of division to which the gold may be reduced, still the proportion of the solid particles to the amount of space through which they are dispersed must be of that extreme proportion. This accords perfectly with their invisibility in the microscope; with the manner of their separation from the dissolved state; with the length of time during which they can remain diffused; and with their appearance when illuminated by the cone of sun's rays.

The deposits, when not fixed upon glass or paper, are much changed by drying; they cannot be again wetted to the same degree as before, or be again diffused; and the light reflected or refracted is as to colour much altered, as might be expected. Whilst diffused through water, they seem to be physical associations of metallic centres with enveloping films of water, and as they sink together will lie for months at the bottom of the fluid without uniting or coming nearer to each other, or without being taken up by metallic mercury put into the same vessel. This is consistent with what we know of the manner in which gold and platinum can be thoroughly wetted if cleaned in water, and of the difference which occurs when they are dried and become invested with air. I endeavoured to transfer the gold particles unchanged into other media, for the purpose of noting any alteration in the action on light. By decanting the water very closely, and then carefully adding alcohol with agitation, I could diffuse them through that fluid; they still possessed a blue colour when looked through in the dark tube, but seemed much condensed or aggregated, for the fluid was obscure, not clear, and the particles soon subsided. I could not transfer them from alcohol to camphine; they refused association with the latter fluid, retaining a film of alcohol or water, and adhering by it to the glass of the vessel; but when the camphine was removed, a partial diffusion of them in fresh alcohol could be effected, and gave the colour as before. All these transfers, however, injured the particles as to their condition of division. In one case I obtained a ruby film on a white plate; on pouring off the water and allowing parts to become dry, these became violet, seen by the light going through them to the plate and back again to the eye. I could not wet these places with water, a thin feebly reflecting surface remained between it and them. Using alcohol, the parts already dry remained violet, when wetted by it; but wetting other parts with alcohol before they were dry from water they remained rosy, became bluish when dry from the alcohol, and became rosy again when re-wetted by it.

It will be necessary to speak briefly of the reduction of gold into a divided state by some other chemical agents than those already described*. If a drop of solution of protosulphate of iron be introduced to, and instantly agitated with, a weak neutral solution of chloride of gold in such proportion that the latter shall be in excess, the fluid becomes of a blue-gray colour by transmission and brown by reflexion; and a deposit is formed of a green colour by transmitted light, greatly resembling the colour of beaten or pressed metal. It is not however pure gold, but an association of it and oxide of iron. Hydrochloric or other acids remove the iron and reduce the gold to a dark, dense, insoluble set of particles, in very small quantity apparently, yet containing all that was present in the bulky green deposit. If the solution of gold be made slightly acid beforehand, then the change and precipitation is to appearance much less; the reflexion by the particles is feeble but of a pale-brown colour, the general transmitted light is amethystine; in the dark tube the tint is blue; the particles are much condensed and settle quickly, but occasionally leave a good ruby film on the side of the glass, which has all the characters of the ruby films and particles before described. The loose gold particles quickly adhere together. Hence it appears that the green precipitate often obtained by protosulphate of iron is not pure gold in a divided state; and that when care is taken to produce such pure divided gold, it presents the appearances of divided gold obtained by other means, the gold being competent to produce the ruby, amethystine, and blue colours by transmission. Usually the gold rapidly contracts and becomes almost insensible, and yet the test of protochloride of tin will show that *all* has been separated from solution; it then forms a *striking* contrast to the depth of colour presented by the same solution of gold precipitated by phosphorus, and most impressively directs attention to the molecular condition of the metal in the latter state.

A very small quantity of *protochloride of tin*, added to a dilute solution of gold, gave, first the ruby fluid, showing diffused particles by the cone of rays; this gradually became purple, and if the gold were in sufficient quantity, a precipitate soon began to fall, being the purple of Cassius. If the chloride of tin were in

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* See Gmelin's 'Chemistry,' vol. vi. p. 219, "Terchloride of gold," for numerous references in relation to changes of these kinds.

larger quantity, a more bulky precipitate fell and more quickly. Acid very much reduced this in quantity, dissolving out oxide of tin, and leaving little else than finely-divided gold, which, when diffused and examined in the dark tube, transmitted a blue colour. I believe the purple of Cassius to be essentially finely-divided gold, associated with more or less of oxide of tin.

Tartaric acid being added to a weak solution of gold gradually reduced it. The amethystine tint produced by diffused particles first appeared, and then a blue deposit of larger particles, whilst the side and bottom of the glass became covered by an adhering film of finer particles, presenting the perfect ruby tint of gold.

Æther added to a weak solution of gold gradually reduced it; the fluid was brown by reflected light, fine blue by transmitted light, and gave a good cone by the sun's rays and lens. The blue colour was not deep, though all the gold had been separated from solution; the preparation closely resembled that made with protosulphate of iron and a little acid.

A weak solution of gold, mingled with a little sugar, being heated, yielded a very characteristic decomposition. The gold was reduced into diffused particles, which rendered the fluid of a ruby-amethystine colour, and which, upon standing for twenty-four hours, gave signs of separation by settling as on former occasions. A little glycerine with solution of gold reduces it at common temperatures, producing a fluid, brown by reflexion, blue by transmission, giving a fine cone of rays by its suspended particles. Heat quickens the action, and causes a blue deposit.

Organic tissues often reduce solutions of gold, light if present assisting the action; and they afford valuable evidence in aid of the solution of the question relative to the condition of the metal in the divided state. If the skin be touched with a solution of gold, it soon becomes stained of a dull purple colour. If a piece of the large gut of an ox be soaked first in water, then in a solution of gold, and be afterwards taken out and allowed to dry, either exposed to light or not, the inner membrane will become so stained, that though of a dull purple colour by common observation, a transmitted ray will show it to be generally a very fine ruby, equal to that of ruby-coloured glass, or the gold fluids already described, though perhaps in places of a beautiful violet hue. The character of the particles which are here located and not allowed to diffuse and aggregate, as in the fluids, will be resumed when dealing with the whole question of the metallic nature of the particles of the variously-divided gold.

Chloride of gold is reducible by heat alone. If a drop of solution of chloride of gold be evaporated in a watch-glass, or on a plate of rock-crystal, and then heated over a spirit-lamp until the gold is reduced, it will generally be found that the vapour has carried a portion of gold on to the neighbouring part of the glass, and that this part, when placed over a sheet of white paper, has the ruby tint. With the rock-crystal both ruby and blue parts are produced; and when the ruby parts are subjected to rock-crystal pressure, they become beautifully green. In the arts also glass is oftentimes coloured ruby by gold; I think that glass in this state derives its colour from diffused divided gold; and if either the ruby glass or the watch-glass be examined by a lens and the cone of rays, it will be seen that the colours are not due to any gold dissolved, but to solid and diffused particles. There is nothing in any of the appearances or characters, or in the processes resorted to to obtain the several effects, that point at any physical difference in the nature of the results; and without saying that gold cannot produce a ruby colour whilst in combination or solution, I think that in all these cases the ruby tint is due simply to the presence of diffused finely-divided gold.

Metallic character of the divided gold.

Hitherto it may seem that I have assumed the various preparations of gold, whether ruby, green, violet, or blue in colour, to consist of that substance in a metallic divided state. I will now put together the reasons which cause me to draw that conclusion. With regard to *gold-leaf* no question respecting its metallic nature can arise, but it offers evidence reaching to the other preparations. The green colour conferred by pressure, and the removal of this colour by heat, evidently belong to it as a metal; these effects are very striking and important as regards the action on light, and where they recur with other forms of gold, may be accepted as proof that the gold is in the metallic state. Although I do not attach equal importance to the fact already described, that gold-leaf frequently presents fine parts that appear to be ruby in colour, I am not as yet satisfied that they are not in themselves ruby; and if they should be so, it will be another proof by analogy of the metallic nature of other kinds of preparations eminently ruby.

The *deflagrations* of gold wire by the Leyden discharge can be nothing but divided gold. They are the same whatever the atmosphere surrounding them at the time, or whatever the substance on which they are deposited. They have all the chemical reactions of gold, being, though so finely divided, insoluble in the fluids that refuse to act on the massive metal, and soluble in those that dissolve it, producing the same result. Heat makes these divided particles assume a ruby tint, yet such heat is not likely to take away their metallic character, and when heated they still act with chemical agents as gold. Pressure then confers the green colour, which heat takes away, and pressure reconfers. All these changes occur with particles attached to the substances which support them by the slightest possible mechanical force, just enough indeed to prevent their coalescence and to keep them apart and in place, and yet offering no resistance to any chemical action of test agents, as the acids, &c., not allowing any supposition of chemical action between them and the body supporting them. Still this gold, unexceptionable as to metallic state, presents different colours when viewed by transmitted light. Ruby, green, violet, blue, &c. occur, and the mere degree of division appears to be the determining cause of many of these colours. The deflagrations by the voltaic battery lead to the same conclusion.

The *gold films* produced by phosphorus have every character belonging to the metallic state. When thick, they are in colour, lustre, weight, &c. equal to gold-leaf, but in the unpressed state, their transmitted colour is generally gray, or violet-gray. The progression of their lustre and colour is gradual from the thickest to the thinnest, and the same is generally true, if thick films are gradually thinned and dissolved whilst floating on solvents; the thick and the thin films must both be accepted as having the same amount of evidence for their metallic nature. When subjected to chemical agents, both the thick and the thin films have the same relations as pure metallic gold. These relations are not changed by the action of heat, yet heat shows the same peculiar effect that it had with preparations of gold obtained by beating, or by electric deflagrations. The remarkable and characteristic effect of pressure is here reproduced, and sometimes with extraordinary results; since from the favourable manner in which the particles are occasionally divided and then held in place on the glass, the mere touch of a finger or card is enough to produce the result. Yet with gold thus proved to be metallic, colours including gray, gray-violet, green, purple, ruby, especially by heat, and green again by pressure, and by thinning of gray films, may be obtained by transmitted light, almost all of them at pleasure.

It may be thought that the *fluid preparations* present more difficulty to the admission, that they are simply cases of pure gold in a divided state; yet I have come to that conclusion, and believe that the differently-coloured fluids and particles are quite analogous to those that occur in the deflagrations and the films. In the first place they are produced as the films are, except that the particles are separated under the surface and out of the contact of the air; still, when produced in sufficient quantity against the side of the containing vessel to form an adhering film, that film has every character of lustre, colour, &c., in the parts differing in thickness, that a film formed at the surface has. Whilst the particles are diffused through the fluid it is difficult to deal with them by tests and reagents; for their absolute quantity is very small, and their physical characters are very changeable, chiefly as I believe by aggregation; still there are some expedients which enable one to submit even the finest of them to proof. In several cases particles from ruby and amethystine fluids adhered to the sides of the bottles or flasks in which the fluids had been preserved, and the process of boiling seemed to favour such a result; the adhesion was so strong, that when the fluid contents were removed and the bottles well washed, the glass remained tinged of a ruby or a violet colour. These films, in which the fine particles were fixed mechanically apart and in place, were then submitted to the action of various chemical agents. Drying and access of air did not cause any marked alterations in them. Strong nitric acid produced no change, nor hydrochloric acid, nor sulphuric acid. Neither did a solution of chloride of sodium, even up to brine, cause any alteration in the colour or any other character of the deposit. A little solution of chlorine or of nitromuriatic acid dissolved them at once, producing the ordinary solutions of gold. I can see no other mode of accounting for these effects (which are in strong contrast with what happens when ruby fluid is acted on by these agents), than to suppose that the gold particles, being in a high state of division, were retained in that state for the time by their adhesion to the glass. Of course chemical change was free to occur, but not a change dependent upon their mutual aggregation; yet they were not held by any special chemical attraction to, or combination with, the glass; for a touch with a card, a feather, or the finger, was sufficient to remove them at once, and if rubbed off with a point of wood, they coated it with brilliant metallic gold.

Again, though these particles are so finely divided that they pass easily through ordinary filters, still a close filter catches some; and if a ruby fluid be passed through again and again, the paper at last becomes of a rosy hue, because of the gold which adheres to it; being then well washed, and, if needful, dried, the gold is again ready for experiment. Such gold paper, placed across the middle of the dark tube and examined by transmitted light, was of the same ruby tint as when looked through in the open air. It was unaffected by salt or brine, though these, added to the rosy fluid which had passed the filter, instantly changed it to violet-blue. Portions of the paper were put into separate glasses with brine, solutions of hydrochloric, nitric and sulphuric acids, ammonia, potassa, soda and sulphuretted hydrogen, but no change occurred with any of them in two days. On the other hand, a very dilute solution of chlorine immediately turned the ruby to blue, and then gradually dissolved the gold. A piece of the ruby paper immersed in a strong solution of cyanide of potassium suffered a very slow action, if any, and remained unaltered in colour; being brought out into the air, the gold very gradually dissolved, becoming first blue. A portion of the ruby paper was dried and heated in oil until the oil and the paper began to change their hue; the gold had not altered in its colour or character. Another portion was heated in the vapour of alcohol and also of æther until the paper began to alter; the gold remained unaltered. A blue fluid being passed oftentimes through a filter gave a blue paper, which, being washed and tried in the same manner, was found to contain particles unchanged by the simple acids or alkalies, or by heat or vapours, but dissolving, as gold would do, in chlorine or nitromuriatic acid. These tests are, I think, sufficient to prove the metallic nature and permanence of the gold as it exists in the ruby, amethystine, violet, and other coloured fluids.

The production by such different agents as phosphorus, sulphide of carbon, æther, sugar, glycerine, gelatine, tartaric acid, protosulphate of iron and protochloride of tin, of gold fluids all more or less red or ruby at the commencement, and all passing through the same order of changes, is again a proof that only gold was separated; no single one or common compound of gold, as an oxide or a phosphide, could be expected in all these cases. Many of the processes, very different as to the substances employed to reduce the gold, left good ruby films adhering to the glass vessels used, presenting all the characters of the gold described already; this was the case with phosphorus, sugar, tartaric acid, protosulphate of iron, and some other bodies.

Again, the high reflective power of these particles (unalterable by acids and salts), when illuminated by the sun's rays and a lens, and the colour of the light reflected, is in favour of their metallic character. So also is their aggregation, and their refusal to return from blue, violet or amethystine to ruby; for the cohesive and adhering force of the gold particles and their metallic nature and perfect cleanliness is against such a reverse change. Particles transmitting blue light could be obtained in such quantity as to admit of their being washed and dried in a tube, and being so prepared they presented every character of gold: when heated, no oxygen, water, phosphorus, acid of phosphorus, nor any other substance was evolved from them: they changed a little, as the film when heated changed, becoming more reflective and of a pale brown colour, and contracted into aggregated porous masses of pure ordinary gold.

Gold is reduced from its solution by organic tissues; and *stained gut* has been quoted as a case. I have a very fine specimen which by transmitted light is as pure a ruby as gold-stained glass, and I believe that the gold has been simply reduced and diffused through the tissue. The preparation stood all the trials that had been applied to the ruby films on glass or the gold deposit on filtering-paper. Portions of it remained soaking in water, solution of chloride of sodium and dilute sulphuric acid for weeks, but these caused no change from ruby to blue, such as could be effected on loose ruby particles. Strong hydrochloric acid caused no change as long as the tissue held together; but as that became loose, the gold flowed out into the acid in ruby-amethystine streams, finally changing to blue. Caustic potassa caused no change for days whilst the tissue kept together, but on mixing all up by pressure, the loosened gold became at last blue. Strong nitric acid caused no change of colour until, by altering the tissue, the gold particles first flowed out in ruby and amethystine streams, and then were gradually changed to the condition of common aggregated gold. All these effects, and the actions on light, accord with the idea that the stain was simply due to diffused particles of finely-divided gold; and I am satisfied that all such stains upon the skin, or other organic matter, are of exactly the same nature.

As to the gold in ruby glass, I think a little consideration is sufficient to satisfy one that it is in the metallic condition. The action of heat tends to separate gold from its state of combination, and when so separated from the chloride, either upon the surface of glass, rock-crystal, topaz, or other inactive bodies, a ruby film of particles is frequently obtained. The sunlight and lens show that in ruby glass the gold is in separated and diffused particles. The parity of the gold glass, with the ruby-gold deflagrations and fluids described, is very great. These considerations, with the sufficiency of the assigned cause to produce the ruby-tint, are strong reasons, in the absence of any to the contrary, to induce the belief that finely-divided metallic gold is the source of the ruby colour.

When a pure, clean, stiff jelly is prepared, and mixed, whilst warm and fluid, with a little dilute chloride of gold, as if to prepare a ruby fluid, it gelatinizes when cold, and if left for two or three days may become a ruby jelly; sometimes, however, the gold in the jelly changes but little or changes to blue, or it may happen that it is reduced on the surface as a film, brilliant and metallic by reflected light, and blue-gray by transmitted light. I have not yet ascertained the circumstances determining one or the other state. If a trace of phosphorus in sulphide of carbon be added to the solution of gold in a dilute state, and some salt be added to the warm jelly, and the latter be then mixed gradually and with agitation with the gold solution, a ruby jelly is generally produced. In such ruby jelly the reduced particles of gold preserve their state and relative place, and the tint does not pass to blue, even though a considerable proportion of salt be present. Such jelly will remain in the air for weeks before it decays, and has every character, in colour and appearance, of gold ruby glass. It is hardly possible to examine the series of ruby glass, ruby membrane, ruby jelly cold and gelatinous, ruby jelly warm and fluid, and the ruby fluids, to consider their production, and then to conclude that the cause of their common ruby colour is not the same in all.

When the warm ruby jelly is poured into a capsule or on to a plate, allowed to gelatinize and then left in the air, it gradually becomes dry. When dry, some of these jellies remain ruby; others will probably be of an amethystine violet colour, or perhaps almost blue. When one of the latter is moistened with water, and has absorbed that fluid, it becomes gelatinous, and whilst in that state resumes its first ruby colour; but on being suffered to dry again, it returns to its amethystine or blue colour. This change will occur for any number of times, as often as the jelly is wetted and dried. Here the gold remains in the same metallic state through this great change of colour, the association or the absence of water being the cause: and the effect strengthens in my mind the thought before expressed, that in the ruby fluids the deposited particles are frequently associates of water and gold. It is a striking case of the joint effect of the media and the gold in their action on the rays of light, and the most striking case amongst those where the medium may be changed to and fro.

When a ruby jelly is prepared with salt, and being warm is poured out in thin layers on to glass or porcelain, it first gelatinizes and then dries up; in which case the salt is excluded and crystallizes. When the dry jelly is put into cold water, the salt dissolves and can be removed. The jelly then swells to a certain amount, after which it can be left soaking in water for a week or longer, until everything soluble is separated. No change takes place in the ruby tint, no gold is removed. When the last water is poured off and the remaining jelly warmed, it melts, forming a fine ruby fluid, which can either be dissolved in more water, or regelatinized, or be dried and preserved for any length of time. It is perfectly neutral; gives no signs of dissolved gold by any of the tests of the metal; is not changed by sulphuretted hydrogen, gallic acid, pyrogallic acid, dilute caustic alkalies, or carbonated alkalies or lime-water; or by dilute sulphuric, hydrochloric or nitric acids, the actions being continued for fourteen days:—being boiled with zinc filings it does not change; and even when dilute sulphuric or hydrochloric acid is added to evolve nascent hydrogen, still the ruby character undergoes no alteration. Strong sulphuric, or nitric, or hydrochloric acid do not alter it whilst cold; but when warmed, the first causes the gold to separate as dark aggregated metallic particles, and the two latter gradually cause the change to amethyst and blue formerly described. Chlorine, or a mixture of hydrochloric and nitric acids, dissolves the gold, the ruby colour disappears, and the ordinary solution of gold is produced. In all these cases the ruby gold behaves exactly as metallic gold would do with the same agents, and quite unlike what would be expected from any possible combination of oxygen and gold.

In some of these jellies the ruby particles are so determinate as to give the brown reflexion by common observation; in others they are so fine as to look like ruby solutions, unless a strong sunlight and a lens be employed; and the impression again arises, that gold may exist in particles so minute as to have little or no power of reflecting light. Ruby particles of extreme fineness, when present in small amount in water, appear to remain equally diffused for any length of time; if in larger amount, that which settles to the bottom will remain for weeks and months as a dense ruby fluid, but without coming together: both circumstances seem to imply an association of the particles of gold with envelopes of water. Many circumstances about the ruby jellies imply a like association with that animal substance, and many of the stains of gold upon organic substances probably include an affinity of the metal of the like kind.

Relations of Gold (and other metals) to polarized Light.

It has been already stated, that when a ray of common light passes through a piece of gold-leaf inclined to the ray, the light is polarized. When the angle between the leaf and the ray is small, about 15°, nearly all the light that passes is polarized; but as the leaf is really very irregular in thickness, and ill-stretched as a film, parts inclined at different angles are always present at once. The light transmitted is polarized in the same direction as that transmitted by a bundle of thin plates of glass, inclined in the same direction. The proportion of light transmitted is small, as might be expected from the high reflective power of the metal. The polarization does not seem due to any constrained condition of the beaten gold, for it is produced, as will be shortly seen, by the annealed colourless leaf-gold, and also by deposits of gold particles; but is common to it with other uncrystallized transparent substances. It would seem that a very small proportion of the gold-leaf can be occupied by apertures, since the light which passes is nearly all polarized. On subjecting thin gold-leaf, or heated gold-leaf, or films of gold, or any preparations which required the support of glass, results of polarization were obtained, but the observations were imperfect because of the interfering effect of the glass.

Proceeding to employ a polarized ray of light, it was found that a leaf of gold produced generally the same depolarizing effect as other transparent bodies. Thus, if a plate of glass be held perpendicular to the ray, or inclined to it either in the plane of polarization or at right angles to it, there is no depolarization; but if inclined in the intermediate positions, the ray is more or less depolarized. So it is with gold-leaf; the same effects are produced by it. Further, the depolarization is accompanied by a rotation of the ray, and in this respect the quadrants alternate, the rotation being to the right-hand in two opposite quadrants, and to the left in the intervening quadrants. So it is with gold-leaf; the same effects are produced by it, and the rotation is in the same direction with that produced by glass, when inclined in the same quadrant.

As further observation in this direction was stopped by the necessity of employing glass supports for the leaves, films, &c., I sought for a medium so near glass in its character, as should either reduce its effect to nothing, or render it so small as to cause its easy elimination. Either camphine or sulphide of carbon was found to answer the purpose with crown-glass; but the latter, as it possesses no sensible power of rotation under ordinary circumstances, is to be preferred. Should a medium of higher optic force be required, it would probably be supplied by the use of that dangerous fluid, phosphorus dissolved in sulphide of carbon. A rectangular glass cell being provided, which did not itself affect the polarized ray, was placed in its course and filled to a certain height with sulphide of carbon. A plate of crown-glass was then introduced perpendicularly to the ray; it did not affect it; being inclined as before described, the effect on the ray was still insensible, the glass appearing to be, for all ordinary observations such as mine, quite as the medium about it. I could now introduce gold-leaf attached to glass into the course of the polarized ray, its condition as a flat film or plane being far finer than when stretched on a wire ring as before. It proved to be so far above the sulphide of carbon, as to have powers of depolarization apparently as great as those it had in air, and being inclined, brought in the image at the analyser exceedingly well. It was indeed very striking to see, when the plate was moved parallel to itself, the darkness when mere glass intervened, and the light which sprung up when the gold-leaf came into its place; the opake metal and the transparent glass having apparently changed characters with each other. By care I was able to introduce a stretched piece of gold-leaf (without glass) into the sulphide of carbon: its effects were the same with those just described.

In all the experiments to be described, the plane of polarization and the plane of inclination had the same relation to each other: the figure shows the position of the polarizing Nicol prism, as the eye looks through it at the light, and *a*, *b* represents the vertical axis, about which the plates were inclined. Whether they were inclined in one direction or the other, or had the glass face or the metal face towards the eye, made no difference. In all cases with gold-leaf, it was found that the ray had been rotated; that it required a little direct rotation of the analyser to regain the minimum light; that short of that red tints appeared, and beyond it blue or cold, these being necessarily affected in some degree by the green colour of the gold-leaf. Thinned gold-leaf produced the same results; but as holes appeared in those that were thinnest, the results were interfered with, because the light passing through them was affected by the analyser in a different manner, and yet mingled its result with that of the light which had passed through the gold.

The gold-leaf plates, deprived of green colour by heating in oil, were found with the glass in such good annealed condition, as not to affect the ray; but when they were moved, until the oblique colourless gold came into the course of the ray, it was depolarized; a red image appeared; direct rotation of the analyser reduced this a little in intensity and then changed the colour to blue. The reduction was not much, and both in that and the first appearance of the red image there is a difference between the heated and the unheated gold: probably the green tint of the latter, which would tend to extinguish the red and produce a minimum, may be sufficient to account for the effect. Gold which had been re-greened by agate pressure acted in like manner on the polarized ray, but the experiments were imperfect.

A glass plate having gold-leaf on one part of it, had a second glass plate put over it and gummed at the edges. In the sulphide of carbon, therefore, it represented in one part a plate of air, and in another a compound plate of air and gold; both acted in the same direction, but the air and gold much more than the air. Gold on glass in this medium, or gold in air, or glass in air, all gave results in the same direction, *i. e.* required direct rotation of the analyser to compensate for them.

I proceeded to examine the other forms of gold; and first, the deposits on glass obtained by *electric deflagration*. These affected the ray of polarized light exactly in the manner of gold-leaf, and that even at the distant parts of the deposit. It was most striking to contrast the thinnest and faintest portion of such a film with the neighbouring parts of the glass from which it had been wiped off. It must be remembered that such a preparation is a layer of separate particles; that these particles are not like those of starch or of crystals, for they have no action whilst in a plane perpendicular to the polarized ray; nor have they a better action for being in a thick layer, as in the central parts of the deposit. The particles seem to form the equivalent of a continuous plate of transparent substance; and as in such a plate it is the two surfaces which act, so there appears to be the equivalent of these two surfaces here; which would seem to imply that the particles are so small and so near, that two or more can act at once upon the individual atoms of the vibrating æther. Their association is such as to present as it were an optical continuity.

The gold films by phosphorus were then submitted to experiment, and gave exactly the same result. All of them depolarized, and required direct rotation of the analyser to arrive at a minimum, or to pass from the red to the blue tints. Graduated films, of which I should judge from the depth of tint that one place was at least twenty times as thick as another, gave the effect as well in the thinnest as the thicker or any intermediate part; indicating that thickness of the plate, and therefore any quality equivalent to crystalline force of the particles, had nothing to do with the matter. A glass beaker, which had been employed to contain ruby fluid, had a film of gold deposited on its inner surface so thin, as to be scarcely perceptible either by reflexion or otherwise, except by a ruby tint which appeared upon it in certain positions; but being examined by a polarized ray, it gave an effect as strong and as perfect as gold-leaf, showing how thin a film of gold was sufficient for the purpose. This thin film appeared to be almost perfect in its continuity, for when the red image was brought in, direct rotation of the analyser reduced it to a minimum which was quite dark; after which, further rotation brought in a good blue image. The least touch of the finger removed the film of gold and all these effects with it. These films, though they are certainly porous to gas, and to water in some form, for it can evaporate from beneath them through its body, have evidently optical continuity.

In order to submit the gold fluids to experiments, cells were made of two glass plates, separated by the thickness of a card, and fastened at the edges by varnish internally and gum externally. These being filled with dense ruby or blue fluid, gave no indication of action on the ray, showing that the diffused particles were inoperative. The same fluids, dried on plates of glass so as to leave films, did act just as the gold deflagrations had done;

for though the particles were very irregularly spread, parts of the general deposit, and these not the thickest, could be selected, which produced the effect excellently well.

When the coloured jellies are laid upon glass plates and allowed to dry, the plates introduced obliquely into the sulphide of carbon affect the ray, but not as gold films; the light image becomes visible, but the plane of polarization is not changed; the light is coloured by the ruby or blue tint of the gold present, but a film of jelly without gold makes it visible to the same extent. In this case the gold is not in one plane, but diffused through the dry jelly, and the effect is the same as if it were diffused through water, being negative.

Such are the effects with the various preparations of divided gold. I will hastily notice what occurs with some other metals. *Platinum deflagrated* in hydrogen: it depolarized the ray, required direct rotation of the analyser to attain a minimum, therefore rotated the plane of polarization; but did not present sensible colour on either side of the minimum of light. *Palladium deflagrated* in hydrogen: it depolarized, producing a red image; direct rotation of the analyser lessened the light to a minimum, and then brought in a blue image. The films of palladium obtained by phosphorus acted well in the same manner. These films appear to be exceedingly continuous, and it could be observed in them, that though the thickest were not the best, yet films could be obtained so thin as to be distinctly inferior to other parts a little thicker; also that where the brilliancy of reflexion which indicates perfect smoothness passed in any degree into dulness, the action of the film was injured: the perfect condition of the surfaces of the films seems to be essential to their good action. *Rhodium films* by phosphorus gave good actions, like those produced by gold. *Silver deflagrations,* either in air or hydrogen, gave depolarizing results like those with gold. Silver films also gave excellent results of the like kind. A thin pale-brown film was much better than a thicker one. *Copper* deflagrated in hydrogen: depolarized, bringing in a red image, which by direct rotation of the analyser was lowered a little and then converted to blue. The copper films obtained from oil acted in the same manner; the red and blue images appeared in their order; but very little direct rotation of the analyser was required to produce the minimum of light. *Tin* deflagrated in hydrogen: depolarized and rotated the ray, as with gold; the images were only feeble in colour. *Lead* deflagrated in hydrogen: acted as tin. *Iron* deflagrated in hydrogen: acted as tin. *Zinc* deflagrated in hydrogen: acted as tin. *Aluminium* deflagrated in hydrogen: had like action with the rest; the image brought in by it was red, which direct revolution of the analyser reduced at a little distance to a minimum and then converted to blue. A film of *mercury* produced by sublimation, a film of *arsenic* produced in like manner, and a film of *smoke* from a candle, though all of them sufficiently pervious to light, did not produce any result of depolarization. Films of the smoke of burning *zinc*, of *antimony*, or of *oxide of iron* produced no effect.

I placed some metallic solutions in a weak atmosphere of sulphuretted hydrogen. Gold and platinum gave no films; silver so poor a film as to be of no use; and lead one so brittle as to be unserviceable. That obtained with palladium I believe to be the metal itself. The films of sulphuret of mercury, sulphuret of antimony which was orange, and sulphuret of copper which was pale brown, all acted on the light and depolarized it. The sulphuret of copper presented a difference from the metals generally, worth recording: it depolarized the light, producing an image which, if not blue at once, was rendered blue by a little direct rotation of the analyser; after which the same motion brought in a minimum and then produced an orange or red tint, *i. e.* with the sulphuret of copper the warm and cold tints appear on opposite sides of the minimum to those where they occur when films of the metals are employed, though the minimum in both cases is in the same direction.

Many of the results obtained in the sulphide of carbon were produced also in camphine, the analyser being in each case adjusted to the minimum of light before the metallic plate or film was introduced. I pass, however, to a very brief account of some polarizations effected by the metals themselves in the sulphide of carbon, in which case the polarizing Nicol prism was dispensed with. The results show that all the dry forms of gold accord in giving the same manifestation of action on light, whatever the state of their division, provided they be disposed in a thin regular layer, equivalent to a continuous film. It was first ascertained that a plate of crown-glass in an inclined position in sulphide of carbon gave no signs of polarity to a ray of light passing through it. When fine gold-leaf was on the glass and inclined to the ray, it polarized the light, and exactly in the same manner and direction as a bundle of glass plates in the same position in the air. More light passed than when the gold-leaf was in air, but it could not be so completely polarized; the minimum light was of a pale bluish colour. A thinned gold-leaf produced the same effect, but let more common light through. I think the difference between gold-leaf and sulphide of carbon is sensibly less than that between the metal and air. The depositions of deflagrated gold, the films of gold obtained by phosphorus, and even the heated deflagrated gold, produced polarizing effects, which, though not large, were easily recognized and distinguished from the non-action of the glass. Gold-leaf and gold films on glass produced a like effect in a camphine-bath, the results being easily distinguished from those of the glass and camphine only, in places where the glass had been cleared from gold.

Films of palladium, rhodium, silver, a plate with deposited gold particles, and a layer of deflagrated silver particles gave a like result, the effect varying in degree. The sulphuret of copper before spoken of as in contrast with the metals, gave only a doubtful result, if any.

Before concluding, I may briefly describe the following negative results with the preparations of gold. I prepared a powerful electro-magnet, sent a polarized ray across the magnetic field, parallel to the magnetic axis, and then placed portions of the ruby and violet fluids, also of their deposits wet and dry, also portions of the gold films, of gold-leaf, the results of deflagrations &c., in the course of the ray; but on exciting the magnet could not obtain any effect beyond that due to the water or glass, which in any case accompanied the substance into the magnetic field. In some cases very dense preparations of the ruby and blue deposits were employed, the intense electric lamp light being required to penetrate them.

I passed the coloured rays of the solar beam through the various gold fluids and films that have been described. For this purpose a beam of sunlight entering a dark room through an aperture $\frac{1}{6}$th of an inch in width, was sent through two of Bontemp's flint-glass prisms, and its rays either separated, or at once thrown on to a pure white screen; the different objects were then interposed in the course of the ray, but I could not perceive when any portion of a ray passed (and that was generally the case) that it differed sensibly in colour or quality from the ray passing into the preparation. In like manner, the objects were put into the differently coloured rays and observed by the reflected light, a lens being sometimes employed to concentrate the light; but I could not find any marked difference between the colour or character of the ray reflected and the impinging ray, except in quantity.

LX. *Solution of a Question in the Theory of Numbers.*

By ARTHUR CAYLEY, *Esq.*[*]

IT is well known that if $N=a^{\alpha}b^{\beta}\ldots$ where $a, b, \ldots$ are primes, then ϕN, the number of numbers less than and prime to N, is equal to $a^{\alpha-1}b^{\beta-1}(a-1)(b-1)\ldots$; the question arises to find

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[*] Communicated by the Author.

$\phi^{-1}N'$, that is, the several numbers each of which has a given number N' of numbers less than and prime to itself. This may be effected as follows: let a be any prime number, and multiply together all the different series of the form

$$1+(a-1)[a]+a(a-1)[a^2]\ldots+a^{\alpha-1}(a-1)[a^\alpha]+\ldots,$$

where the bracketed factors are to be multiplied together by enclosing the product in a bracket; the general term of the product is obviously

$$a^{\alpha-1}b^{\beta-1}\ldots(a-1)(b-1)\ldots[a^\alpha b^\beta\ldots].$$

Or putting $a^\alpha b^\beta\ldots=N$, the general term is $\phi N[N]$; and if $\phi N=N'$, then the general term is $N'[\phi^{-1}N']$. Hence in the product in question, each of the bracketed numbers which are multiplied by the coefficient N' will be a value of $\phi^{-1}N'$, that is, a number having N' numbers less than and prime to itself. The only values of a which need be considered are obviously those for which $a-1$ is a divisor of N'; and each series need only be continued during so long as the coefficient $a^{\alpha-1}(a-1)$ is a divisor of N'. Thus if $N'=12$, the divisors of N' are 1, 2, 3, 4, 6 and 12, and the values of a are 2, 3 (4 is not a prime number), 5, 7 and 12: the series to be multiplied together are

$$\begin{aligned}&1+1[2]+2[2^2]+4[2^3],\\&1+2[3]+6[3^2],\\&1+4[5],\\&1+6[7],\\&1+12[13],\end{aligned}$$

and the product contains the terms

$$12[2^2 3^2]+12[2^2 7]+12[2.3.7]+12[2.13]+12[3.7]+12[13],$$

i. e. the required numbers are 36, 28, 42, 26, 21 and 13; or in order of magnitude, 13, 21, 26, 28, 36 and 42. The rule may be also stated as follows: write down all the numbers of the form $a^{\alpha-1}(a-1)$ where a is prime, which are divisors of N', and combine these divisors in every possible way so as to give the product N'; the corresponding values of $a^\alpha b^\beta\ldots$ are the required series of values. The question is in fact one of the partition or decomposition of N' into factors of a given form.

2 Stone Buildings, W.C.
June 25, 1857.

LXI. *Acoustic Experiments.* *By* Count SCHAFFGOTSCH*.

A GLASS tube open at both ends, when simply blown upon by the mouth, gives its fundamental tone, *i. e.* the deepest tone belonging to it, as an open organ-pipe, feebly but distinctly. On placing the open hand upon one of the openings and rapidly withdrawing it, the tube yields two tones one after the other; first the fundamental tone of the closed pipe, and then the tone of the open pipe, already mentioned, which is *an octave* higher. By the application of heat these fundamental tones, of which only the higher one will be taken into consideration here, are raised, as is well known; this is observed immediately on blowing upon a tube heated externally, or by a gas-flame burning in its interior. For example, a tube 242 millims. in length and 20 millims. in diameter, heated throughout its whole length, when blown upon even before it reaches a red heat, gives a tone raised a major third, namely the second G sharp in the treble clef, instead of the corresponding E. If a gas-flame 14 millims. in length and 1 millim. in breadth at the bottom, is burning in the tube, the tone rises to the second treble F sharp. The same gas-flame raises the tone of a tube 273 millims. in length and 21 millims. in width, from the second treble D to the corresponding E. These two tubes, which for brevity will hereafter be referred to as the E-tube and the D-tube, served for all the following experiments, the object of which was to show a well-known and by no means surprising fact in a striking manner, namely that the column of air in a tube is set in vibration when its fundamental tone, or one nearly allied, for example an octave, is sounded outside the tube. The existence of the aërial vibrations was rendered perceptible by a column of smoke, a current of gas, and a gas-flame.

1. A glimmering smoky taper was placed close under the E-tube held perpendicularly, and the smoke passed through the tube in the form of a uniform thread. At a distance of 1·5 metre from the tube, the first treble E was sung. The smoke curled, and it appeared as if a part of it would be forced out at the upper, and the other part at the lower opening of the tube.

2. Two gas-burners, 1 millim. in the aperture, were applied near each other to the same conducting tube. Common gas flowed from both of them; one projected from below into the D-tube for about one-fifth of its length; the gas-flame of the other was 3 millims. in height. At a distance of 1·5 metre therefrom, the first treble D was sung; the flame instantaneously increased several times in breadth and height, and consequently

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* From the *Monatsbericht der Königl. Akademie zu Berlin* for April 30 1857.

in size generally; a larger quantity of gas therefore flowed out of the outer burner, which can only be explained by a diminution of the stream of gas in the inner burner, that is, in the one surrounded by the glass tube.

3. A burner, with an aperture of 1 millim. projecting from below into the D-tube, about 80 millims., yielded a gas-flame 14 millims. in length. At 5·6 metres therefrom, the first treble E was sung; the flame was instantaneously extinguished. The same thing takes place at a distance of 7 metres, when the flame is only 10 millims. in height, and the first treble D sharp is sung.

4. The last-mentioned flame is also extinguished by the note G sharp sounded close to it. Noises, such as the clapping of hands, pushing a chair, or shutting a book, do not produce this effect.

5. A burner with an aperture of 0·5 millim., projecting from below 60 millims. into the D-tube, yielded a globular gas-flame 3 to 3·5 millims. in diameter. By gradually closing a stopcock, the passage of gas was more and more limited. The flame suddenly became much longer, but at the same time narrower, and nearly cylindrical, acquiring a bluish colour throughout, and from the tube a piercing second treble D was sounded; this is the phænomenon of the so-called chemical harmonics, which has been known for eighty years. When the stopcock is still further closed, the tone becomes yet stronger, the flame longer, narrower, and nearly spindle-shaped; at last it disappears.

An effect exactly similar to that caused by cutting off the gas, is produced upon the small gas-flame by a D, or the first treble D sung or sounded from instruments; and in this case it is to be observed that the flame generally becomes the more sensitive the smaller it is, and the further the burner projects into the glass tube.

6. The flame in the D-tube was 2 or 3 millims. in length; at a distance of 16·3 metres (more than 51 feet) from it the first treble D was sounded. The flame immediately acquired the unusual form, and the second treble D sounded and continued to sound from the tube.

7. While the second treble D of the preceding experiment was sounding, the first treble D was sounded loudly close to the tube, when the flame became excessively elongated and then disappeared.

8. The flame being only 1·5 millim. in length, the first treble D was sounded. The flame gave out the second treble D (and perhaps sometimes also a higher D) only for a moment and disappeared. The flame is also affected by various Ds of an ad-

justible labial pipe, by the contra D, D, D, the first treble D, and the second treble D of a harmonium, but by no single C sharp or D sharp of this powerful instrument. It is also affected by the third treble D of a clarionet, although only when quite close. The sung note also acts when it is produced by inspiration (in this case the second treble), or when the mouth is turned from the flame.

9. In immediate proximity the note G sung is effective.

Some influence is exerted by noises, but not by all, and often not by the strongest and nearest, evidently because the exciting tone is not contained in them.

10. The flame burning quietly in the interior of the D-tube was about 2·5 millims. in length. In the next room, the door of which was open, the four legs of a chair were stamped simultaneously upon the wooden floor. The phænomenon of the chemical harmonics immediately occurred. A very small flame is of course extinguished, after sounding for an instant, by the noise of a chair. A tambourine, when struck, acts sometimes, but in general not.

11. The flame burning in the excited, singing condition in the interior of the D-tube, the latter was slowly raised as high as possible without causing the return of the flame to the ordinary condition. The note, the first treble D, was sung strongly and *broken off suddenly* at a distance of 1·5 metre. The harmonic tone ceased, and the flame fell into the state of repose without being extinguished.

12. The same result was produced by acting upon the draught of air in the tube by a fanning motion of the open hand close above the upper aperture of the tube.

13. In the D-tube there were two burners close together; one of them, 0·5 millim. in aperture, opened 5 millims. below the other, the diameter of which was 1 millim. or more. Currents of gas, independent of each other, flowed out of both; that flowing from the narrower burner being very feeble, and burning, when ignited, with a flame about 1·5 millim. in length, nearly invisible in the day; the first treble D was sung at a distance of 3 metres. The strong current of gas was immediately inflamed, because the little flame situated below it, becoming elongated, flared up into it. By a stronger action of the tone the small flame itself is extinguished, so that an actual transfer of the flame from one burner to the other takes place. Soon afterwards the feeble current of gas is usually again inflamed by the large flame, and if the latter be again extinguished alone, everything is ready for a repetition of the experiment.

14. The same result is furnished by stamping with the chair, &c. It is evident that in this way gas-flames of any desired size

and any mechanical action may be produced by musical tones and noises, if a wire stretched by a weight be passed through the glass tube in such a way that the flaring gas-flame must burn upon it.

15. If the flame of the chemical harmonic be looked at steadfastly, and at the same time the head be moved rapidly to the right and left alternately, an uninterrupted streak of light is not seen, such as is given by every other luminous body, but a series of closely approximated flames, and often dentated and undulated figures, especially when tubes of a metre and flames of a centimetre in length are employed.

This experiment also succeeds very easily without moving the eyes, when the flame is looked at through an opera-glass, the object-glass of which is moved rapidly to and fro, or in a circle; and also when the picture of the flame is observed in a hand-mirror shaken about. It is, however, only a variation of the experiment long since described and explained by Wheatstone, for which a mirror turned by watchwork was employed.

[Note. It is perhaps but right that I should draw attention to the relation of the foregoing paper to one that I have published on the same subject. On the 6th of May, and the days immediately following, the principal facts described in my paper were discovered; but on the 30th of April the foregoing results were communicated by Prof. Poggendorff to the Academy of Sciences in Berlin. Through the kindness of M. v. Schaffgotsch himself, I received his paper at Chamouni, many weeks after the publication of my own, and until then I was not aware of his having continued his experiments upon the subject. We thus worked independently of each other, but as far as the described phænomena are common to both, all the merit of priority rests with Count Schaffgotsch.—J. T.]

LXII. *On the Action of Bromine on the Iodide of Acetyle.*

By Dr. Maxwell Simpson*.

We know from the researches of Regnault, that the iodide of ethylene of Faraday ($C^4H^4I^2$), when treated with an alcoholic solution of potash, gives amongst other products an ethereal liquid boiling at 56° Cent. This is the iodide of acetyle (C^4H^3I). Chemists usually regard this body as the homologue of the iodide of allyle (C^6H^5I). As the latter compound is converted by an excess of bromine into a terbromide, with which M. Wurtz has recently succeeded in regenerating the glycerine of the fats, I thought it would be interesting to ascertain whether or not the compound C^4H^3I, submitted to the

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* Communicated by the Author.

same treatment, would give the terbromide of acetyle ($C^4H^3Br^3$), and also whether or not this body, should I obtain it, would give the glycerine of the acetyle series of compounds $C^4\left.\begin{matrix}H^3\\H^3\end{matrix}\right\}O^6$.

To prepare the terbromide of acetyle, 1 equivalent of the iodide of acetyle is treated with 3 equivalents of bromine. As a great deal of heat is evolved during the reaction, the iodide must be introduced into a long tube surrounded with a mixture of ice and salt, and the bromine very gradually added to it. Iodine is set free in large quantity, communicating a dark red colour to the liquid, and separating from it on standing in large brilliant crystals. As soon as all the bromine has been added, the tube is agitated for some time, still retaining it in the freezing mixture, then sealed and exposed to the temperature of 100° Cent. for about 12 hours. At the expiration of this time it is opened*, and the contents washed with dilute potash in order to remove the free iodine. Finally, to complete the reaction, it is boiled for a few minutes with a little free bromine, washed again with potash, and subjected to distillation, reserving the portion which passes over between 185° and 190° Cent. With the exception of a few drops, the entire liquid distils over at this temperature. The reaction which takes place is exceedingly simple: the three equivalents of bromine combine with the radical C^4H^3, displacing the 1 equivalent of iodine.

The liquid thus prepared is usually of a beautiful rose tint, from the presence of a trace of free iodine; it can, however, be obtained colourless. It has a sweet taste and agreeable fragrant odour, recalling that of chloroform. It is insoluble in water, but freely soluble in alcohol, ether, and acetic acid. It boils at about 186° Cent., and is remarkably heavy, having a specific gravity of 2·663 at 0° Cent. Its composition is $C^4H^3Br^3$, as proved by the following analyses.

I. 0·5967 grm. of the fluid gave 0·2000 grm. carbonic acid and 0·0687 grm. water.

II. 0·4989 grm. of the fluid gave 0·1660 grm. carbonic acid and 0·0523 grm. water.

III. 0·3255 grm. of fluid gave 0·6972 grm. bromide of silver.

Per-centage composition.

	Theory.		Experiment.	
			I.	II.
4 equivs. carbon . .	24	8·99	9·14	9·07
3 equivs. hydrogen .	3	1·11	1·27	1·16
3 equivs. bromine . .	240	89·90	...	91·1
	267	100·00		

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* This can be done with perfect safety, as no gas is evolved during the heating.

With the view of preparing the glycerine from this fluid, I mixed 56 grms. or 1 equiv. of it, with 105 grms. or 3 equivs. of acetate of silver, adding at the same time some crystallizable acetic acid, introduced the mixture into a glass globe with long neck, and exposed it to the temperature of 120° Cent. for six days. At the expiration of this time, the greater part of the silver was converted into bromide; but it was not possible to separate from the product of the reaction a single drop of fluid boiling above 200°. This attempt then to obtain the glycerine of the acetylic series failed. Is the negative result to be attributed to some accidental circumstance, as, for example, the employment of too weak acetic acid in the process; or does it appear to indicate that the bromine compound on which I operated is not really the homologue of the terbromide of allyle? I am disposed to be of the latter opinion. Indeed, one cannot fail to notice the remarkable resemblance that exists between the properties of the bromine compound I have obtained, and those of the *bromure d'ethylène bromè*, obtained and described by M. Wurtz. These bodies have the same odour, the same density, and the same boiling-point. But we know that the *bromure d'ethylène bromè*, $C^4 H^3 Br, Br^2$, is the homologue of the *bromure de propylène bromè* of Cahours, $C^6 H^5 Br, Br^2$, and not that of the terbromide of allyle. If then the body obtained by me is really identical with that obtained by M. Wurtz, we must come to the conclusion that the iodide of allyle, submitted to the action of bromine, behaves in a different manner from the iodide of acetyle, which is usually regarded as its homologue.

Paris, November 25, 1857.

LXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Concluded from p. 479.]

April 2, 1857.—The Lord Wrottesley, President, in the Chair.

THE following communication was read:—

"On the Application of Parabolic Trigonometry to the Investigation of the Properties of the Common Catenary." By the Rev. James Booth, LL.D., F.R.S.

April 30.—The Lord Wrottesley, President, in the Chair.

The following communication was read:—

"Inquiries into the Quantity of Air inspired throughout the Day and Night, and under the influence of Exercise, Food, Medicine, Temperature, &c." By Edward Smith, M.D.

This communication consists of three parts, and contains the results of 1200 series of observations. The author was himself the subject of all the investigations. He is thirty-eight years of age, six feet in height, healthy and strong, and with a vital capacity of the lungs of 280 cubic inches.

The paper concludes with a summary of the principal results obtained and a series of deductions, applicable especially to the solution or elucidation of hygienic questions. From the former the following facts are extracted :—

The total quantity of air inspired in 24 hours (allowance being made for intervals amounting altogether to 40 minutes, during which it was not recorded) was 711,060 cub. ins. ; or an average of 29,627 cub. ins. per hour and 493·6 per minute. The quantity was much less during the night than during the day. There was an increase as the morning advanced and a decrease at about $8^h\,30'$ P.M., but most suddenly at about 11 P.M. During the day the quantity increased immediately after a meal, and then subsided before the next meal; but in every instance it rose again immediately before a meal. The rate of frequency of respiration generally corresponded with the quantity, but the extremes of the day and night rates were greater. The period of greatest parallelism was between tea and supper. An increase was occasioned by one meal only, namely breakfast. The average depth of respiration was 26·5 cub. ins., with a minimum of 18·1 cub. ins. in the night, and a maximum of 32·2 cub. ins. at $1^h\,30'$ P.M. The mean rate of the pulse was 76 per minute, the minimum at $3^h\,30'$ A.M., the maximum at $8^h\,45'$ A.M. ; the difference being more than one-third of the minimum rate.

Sleep came on in two of the series of continuous observations, and the time of its occurrence was also that of the lowest quantities of air inspired.

The amount of breathing was greater in the standing than in the sitting posture, and greater sitting than lying. It was increased by riding on horseback, according to the pace, also by riding in or upon an omnibus. In railway travelling the increase was greater in a second- than in a first-class carriage, and greatest in the third-class and on the engine. An increase was also produced by rowing, swimming, walking, running, carrying weights, ascending and descending steps, and the labour of the tread-wheel; and in several of these cases the rate of increase was determined for different degrees of exertion used. Reading aloud and singing, and the movement recommended by Dr. Hall for restoring suspended respiration, increased the quantity; bending forwards whilst sitting, lessened it.

The quantity of inspired air was increased by exposure to the heat and light of the sun, and lessened in darkness. Increase and decrease of artificial heat produced corresponding effects; and the depth of respiration was greatly increased by great heat. An increase in quantity was caused also by cold bathing, and sponging, and the cold shower-bath; by breakfast, dinner, and tea—when tea actually was taken, but when coffee was substituted there was a decrease. Supper of bread and milk also caused a decrease. Milk by itself or with suet caused an increase.

An increase was obtained with the following articles of diet, viz. eggs, beef-steak, jelly, white bread (home-made), oatmeal, potatoes, sugar, tea, rum (1 oz.). The following caused a decrease, viz. butter, fat of beef, olive oil, cod-liver oil, arrow-root, brandy (1 oz. to $1\frac{3}{4}$ oz.), and kirchenwasser. Ether ($\frac{1}{2}$ drachm) increased the quantity and depth of inspiration. A decrease in quantity was caused by sp. ammon. co. (ʒiss), sp. ammon. fœt. (ʒiss), tincture of opium (20 ♏), morphia ($\frac{1}{6}$ and $\frac{1}{8}$ gr.), tartarized antimony ($\frac{1}{2}$ gr.), and chloride of sodium.

Carbonate of ammonia (15 grains) caused a small increase at first and then a small decrease; febrifuge medicines had a like effect. Chloroform (25 ♏ and ʒss), by the stomach, varied the quantity from an average increase of 28 cub. ins. to an average decrease of 20 cub. ins. per minute; with a maximum increase of 63 cub. ins. per minute. Chloric ether (ʒss) also varied the quantity, but there was an average increase of 17 cub. ins. per minute, and of 1·8 per minute, in the rate; whilst the pulse fell on the average 1·7 per min. Chloroform, by inhalation (to just short of unconsciousness), lowered the quantity a little during the inhalation, and more so afterwards. The rate was unchanged, but the pulse fell, on an average, 1·7 per min. Amylene similarly administered and to the same degree, increased the quantity during inhalation 60 cub. ins. per min., but afterwards decreased it to 100 cub. ins. per min. less than during the inhalation. The rate of respiration was unchanged: the pulse fell 6 per min. at the end of the observation.

Digitalis (infusion ℥i) varied the quantity, increasing it at first and then decreasing it. The rate of inspiration was unaffected, whilst that of pulsation somewhat increased.

The paper is accompanied by tables of numerical statements, and by diagrams exhibiting the results in a series of curves.

May 7.—The Lord Wrottesley, President, in the Chair.

The following communications were read:—

"On the Plasticity of Ice, as manifested in Glaciers." By James Thomson, A.M., C.E. Belfast.

The object of this communication is to lay before the Royal Society a theory which I have to propose for explaining the plasticity of ice at the freezing-point, which is shown by observations by Professor James Forbes, and which is the principle of his Theory of Glaciers.

This speculation occurred to me mainly in or about the year 1848. I was led to it from a previous theoretical deduction at which I had arrived, namely, that the freezing-point of water, or the melting-point of ice, must vary with the pressure to which the water or the ice is subjected, the temperature of freezing or melting being lowered as the pressure is increased. My theory on that subject is to be found in a paper by me, entitled "Theoretical Considerations on the Effect of Pressure in Lowering the Freezing-Point of Water," published in the Transactions of the Royal Society of Edinburgh, vol. xiv. part 5, 1849[*]. It is there inferred that the lowering of the freezing-point, for one additional atmosphere of pressure, must be ·0075° Centigrade; and that if the pressure above one atmosphere be denoted in atmospheres as units by n, the lowering of the freezing-point,

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[*] The paper here referred to is also to be found in the Cambridge and Dublin Mathematical Journal for November 1850 (vol. v. p. 248), where it was republished with some slight alterations made by myself.

denoted in degrees Centigrade by t, will be expressed by the formula

$$t = \cdot 0075\, n.$$

The phenomena which I there predicted, in anticipation of direct observations, were afterwards fully established by experiments made by my brother, Professor William Thomson, and described in a paper by him, published in the Proceedings of the Royal Society of Edinburgh (Feb. 1850) under the title, "The Effect of Pressure in lowering the Freezing-Point of Water experimentally demonstrated*."

The principle of the lowering of the freezing-point by pressure being laid down as a basis, I now proceed to offer my explanation, derived from it, of the plasticity of ice at the freezing-point as follows:—

If to a mass of ice at 0° Centigrade, which may be supposed at the outset to be slightly porous, and to contain small quantities of liquid water diffused through its substance, forces tending to change its form be applied, whatever portions of it may thereby be subjected to compression will instantly have their melting-point lowered so as to be below their existing temperature of 0° Cent. Melting of those portions will therefore set in throughout their substance, and this will be accompanied by a fall of temperature in them on account of the cold evolved in the liquefaction. The liquefied portions being subjected to squeezing of the compressed mass in which they originate, will spread themselves out through the pores of the general mass, by dispersion from the regions of greatest to those of least fluid pressure. Thus the fluid pressure is relieved in those portions in which the compression and liquefaction of the ice had set in, accompanied by the lowering of temperature. On the removal of this cause of liquidity—the fluid pressure, namely,—the cold which had been evolved in the compressed parts of the ice and water, freezes the water again in new positions, and thus a change of form, or plastic yielding of the mass of ice to the applied pressures, has occurred. The newly-formed ice is at first free from the stress of the applied forces, but the yielding of one part always leaves some other part exposed to the pressure, and that, in its turn, acts in like manner; and, on the whole, a continual succession goes on of pressures being applied to particular parts—liquefaction in those parts—dispersion of the water so produced, in such directions as will relieve its pressure,—and recongelation, by the cold previously evolved, of the water on its being relieved from this pressure. Thus the parts recongealed after having been melted must, in their turn, through the yielding of other parts, receive pressures from the applied forces, thereby to be again liquefied, and to enter again on a similar cycle of operations. The succession of these processes must continue as long as the external forces tending to change of form remain applied to the mass of porous ice permeated by minute quantities of water.

Postscript received 22nd April, 1857.

It will be observed that in the course of the foregoing communica-

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* The paper by Prof. William Thomson, here referred to, is also to be found republished in the Philosophical Magazine for August 1850.

tion, I have supposed the ice under consideration to be porous, and to contain small quantities of liquid water diffused through its substance. Porosity and permeation by liquid water are generally understood, from the results of observations, and from numerous other reasons, to be normal conditions of glacier ice. It is not, however, necessary for the purposes of my explanation of the plasticity of ice at the freezing-point, that the ice should be at the outset in this condition; for, even if we commence with the consideration of a mass of ice perfectly free from porosity, and free from particles of liquid water diffused through its substance, and if we suppose it to be kept in an atmosphere at or above 0° Centigrade, then, as soon as pressure is applied to it, pores occupied by liquid water must instantly be formed in the compressed parts in accordance with the fundamental principle of the explanation which I have propounded —the lowering, namely, of the freezing- or melting-point by pressure, and the fact that ice cannot exist at 0° Cent. under a pressure exceeding that of the atmosphere. I would also wish to make it distinctly understood that no part of the ice, even if supposed at the outset to be solid or free from porosity, can resist being permeated by the water squeezed against it from such parts as may be directly subjected to the pressure, because the very fact of that water being forced against any portions of the ice supposed to be solid will instantly subject them to pressure, and so will cause melting to set in throughout their substance, thereby reducing them immediately to the porous condition.

Thus it is a matter of indifference as to whether we commence with the supposition of a mass of porous or of solid ice.

"On the Comparison of Transcendents, with certain applications to the Theory of Definite Integrals." By George Boole, Esq., Professor of Mathematics in Queen's College, Cork.

The following objects are contemplated in this paper:—

1st. The demonstration of a fundamental theorem for the summation of integrals whose limits are determined by the roots of an algebraic equation.

2ndly. The application of that theorem to the comparison of algebraical transcendents.

3rdly. Its application to the comparison of functional transcendents, *i. e.* of transcendents in the differential expression of which an arbitrary functional sign is involved.

4thly. Certain extensions of the theory of definite integrals both single and multiple, founded upon the results of the application last mentioned.

In the expression of the fundamental theorem for the summation of integrals, the author introduces a symbol, Θ, similar in its definition to the symbol employed by Cauchy in the Calculus of Residues, but involving an additional element. The interpretation of this symbol is not arbitrary, but is suggested by the results of the investigation by which the theorem of summation is obtained. All the general theorems demonstrated in the memoir either involve this symbol in their expression, or are immediate consequences of theorems into the expression of which it enters.

The author directly applies his theorem of summation both to the solution of particular problems in the comparison of the algebraical transcendents, and to the deduction of general theorems. Of the latter the most interesting, but not the most general, is a finite expression for the value of the sum

$$\Sigma\int\phi\psi^{\frac{m}{n}}dx,$$

where ϕ and ψ denote any rational functions of x; the equation by which the limits of the integrals are determined being of the form $\psi^{\frac{m}{n}}=\chi$, in which χ is also a rational function of x.

The forms of ϕ, ψ, and χ are quite unrestricted, except by the condition of rationality. Previous known theorems of the same class, such as Abel's, suppose ψ a polynomial and specify the form of ϕ. In the author's result, the rational functions ϕ, ψ, and χ are not decomposed. In a subsequent part of the paper, after investigating a general theorem applicable to the summation of all transcendents which are irrational from containing under the sign of integration any function which can be expressed as a root of an equation whose coefficients are rational functions of x, he explains by means of it, the cause of the peculiarity above noticed.

In the section on functional transcendents, a remarkable case presents itself in which the several integrals under the sign of summation, Σ, close up, if the expression may be allowed, into a single integral taken between the limits of negative and positive infinity. The result is an exceedingly general theorem of definite integration, by means of which it is demonstrated, that the evaluation of any definite integral of the form

$$\int_{-\infty}^{\infty}\phi(x)f\left(x-\frac{a_1}{x-\lambda_1}-\frac{a_2}{x-\lambda_2}\,.\,.-\frac{a_n}{x-\lambda_n}\right)dx,$$

in which $\phi\,(x)$ is a rational function of x, and in which $a_1\ a_2\,.\,.\,a_n$ are positive, and λ_1, $\lambda_2\,.\,.\,\lambda_n$ are real, the number of those constants being immaterial, may be reduced to the evaluation of a definite integral of the form

$$\int_{-\infty}^{\infty}\psi(v)f(v)dv,$$

in which $\psi(v)$ is a rational function of v of the same order of complexity as the function $\phi(x)$. Two limited cases of this theorem are referred to as already known,—one due to Cauchy, the other published by the author some years ago.

The remainder of the paper is occupied with applications of the above general theorem of definite integration. Of the Notes by which the paper is accompanied, the first discusses the connexion between the author's symbol and Cauchy's, and contains two theorems, one exhibiting the general solution of linear differential equations with constant coefficients, the other the general integral of rational fractions. Both these theorems involve in their expression the symbol Θ. The second Note is devoted to the interpretation of some theorems for the evaluation of multiple integrals, investigated in the closing section of the paper.

LXIV. *Intelligence and Miscellaneous Articles.*

ON A NEW POLARIZER OF ICELAND SPAR.—EXPERIMENT ON FLUORESCENCE. BY LÉON FOUCAULT.

WHEN the object is to polarize a pencil of white light completely, the best known method is to have recourse to the use of the Nicol's prism; but for operating upon a sheaf of a certain volume, of 4 to 5 centimetres in diameter, for example, the Nicol's prism becomes expensive and difficult to procure, in consequence of the rarity of fine specimens of spar.

The cutting adopted in the construction of the Nicol's prism is necessarily attended with a great expense of material. The prism to be complete, must be taken from a piece of spar the longitudinal edges of which are at least equal to three times one of the equal sides which terminate the bases. The piece is then cut from one obtuse angle to the other, through a plane inclined at 88° upon the plane of the bases, and perpendicular to the plane of their small diagonals. The two faces thus obtained are polished, and fastened together by means of Canada balsam.

When a parallelopiped thus formed is turned towards a uniformly illuminated ground, and we look through the piece in the direction of its axis, a field of polarization is seen included between two curved bands, one red, and the other blue, which correspond with the limit directions in which the ordinary and extraordinary rays are transmitted. These bands enclose an angular space of 32°, which renders the Nicol's prism an analyser applicable in all circumstances when the inclination of the rays, which are to be observed simultaneously, does not exceed 32°.

But this angular extent of the field of polarization, which is prized in the Nicol's prism considered as an analyser, no longer possesses the same interest when the apparatus is simply to play the part of a polarizer; for in that case the action which is to be produced in general only affects a pencil of light of nearly parallel rays. So that, under such circumstances, there would be an advantage in increasing the transverse dimensions of the prism, even when this would cause a certain reduction in the extent of the angular field of polarization.

In reflecting on the data of the question, I have in fact ascertained that the cutting of the Nicol's prism may be modified so as to diminish its length considerably without injury to the effects which it may produce as a polarizer.

I take a parallelopiped of spar, of which the longitudinal edges are only equal to five-fourths of one of the sides of the bases; I carry from one obtuse angle to the other a section inclined at 59° upon the plane of the bases, and the new faces being polished, I replace the two fragments in their natural position *without glueing them*, and taking care to preserve between the new faces a little space in which air remains, and which, under suitable incidence, causes the complete reflexion of the ordinary ray.

On looking through a rhomb cut in this way, and mounted otherwise like a Nicol's prism, we again find that there exists an angular field of polarization; but the index of refraction of the air being considerably less than those of the two rays propagated by the spar, complete polarization only takes place in an extent of about 8°, and the field which it occupies is included between two red bands.

The new combination consequently does not satisfy the conditions necessary to form a good analyser; but when we have simply to polarize a pencil of solar light, of which the extreme rays are only inclined to one another by a half-degree, the prism with a sheet of air and its 8 degrees of field is more than sufficient to polarize all the elements of such a pencil. This kind of polarizer is in some respects even preferable to the Nicol's prism, inasmuch as the ordinary ray is reflected almost normally to the intersection of two of the lateral faces, and has therefore no tendency to become again reflected, or to escape afterwards by the base and mix, as in the Nicol's prism, with the extraordinary ray. Moreover, when the substance of the spar is very pure, and traversed neither by planes of cleavage nor by hemitropic plates, complete extinction is produced by an analyser upon the whole extent of the transmitted pencil. It is to be expected that, in the circumstances in which the Nicol's prism has been employed as a polarizer, the new cutting will be preferred, as it produces a more complete effect, at the same time saving nearly two-thirds of the mass of spar.

These trials having called my attention to all the peculiarities observed in the Nicol's prism, I have been struck by finding the tints inverted in the band of total reflexion which corresponds with the limit direction of transmissibility of the extraordinary ray. This inversion is certainly due to the fact, that, notwithstanding the slight difference of the average indices of refraction, the dispersive power of Canada balsam is greater than that of the spar for the limit direction of the extraordinary ray. It follows from this that the relative indices of the various simple rays go on increasing from the violet to the red, which explains why these different rays are totally reflected in the inverse order of their absolute refrangibilities.

We may avail ourselves of this remark to obtain, by means of the Nicol's prism, a pencil exclusively formed of the most refrangible rays contained in the solar light. For this purpose it is sufficient to place the prism in the course of the luminous pencils, and to incline it gradually in the direction in which complete extinction is produced; we then see the transmitted pencil pass to blue, then to violet, and lastly become reduced to a radiation which is nearly invisible, but eminently adapted to develope with intensity the phænomena of fluorescence discovered by Mr. Stokes. Sulphate of quinine, uranium-glass, and some diamonds immersed in this sheaf immediately acquire a very great brilliancy.—*Comptes Rendus*, August 17, 1857, p. 238.

ON THE INFLUENCE OF STRUCTURE UPON THE MAGNETIC PROPERTIES OF IRON. BY F. P. LE ROUX.

With regard to the experiments of MM. Plücker and Faraday upon magneto-crystalline phænomena, MM. Tyndall and Knoblauch have shown, by ingeniously varied experiments, that the position taken by crystalline masses in relation to the poles of a magnet did not depend, as had been at first supposed, so much upon the direction of the crystallographic axis, as upon that of the dominant cleavage. The phænomena observed, whether with natural crystals or with diamagnetic or slightly magnetic masses, prepared so as to imitate the molecular arrangement of crystals, leave no doubt upon this subject. The planes of cleavage place themselves equatorially in diamagnetic, and axially in magnetic substances. From this fact these physicists have thought they might conclude that the *elective polarity* of crystals, or of bodies which resemble these, depended on the condensation of the molecules (each of them being regarded as a centre of action), which is greater in one direction than in the other.

Serious objections may be made against this opinion. The most important is that of M. Matteucci, who has ascertained that a needle of crystallized bismuth, in which the planes of cleavage are parallel to the length, tended to place itself equatorially with greater force when these planes are vertical than when they are horizontal. Still more recently (*Comptes Rendus*, Sept. 14, 1857), M. Matteucci has published some experiments on magnetism by rotation which appear to prove that the direction of the planes of cleavage, relatively to the lines of force of magnets, acts otherwise than by the distribution of the molecules considered separately, in respect of their magnetic mass. Hitherto experiments of this kind have only been made upon diamagnetic or slightly magnetic bodies. I proposed to try whether strongly magnetic bodies, such as iron, would not disclose analogous phænomena.

I had in my possession some fragments of materials prepared for the manufacture of cast steel by the process of M. Chenot. These are very pure iron ores, reduced either by hydrogen or by carbonic oxide, and then compressed by the hydraulic press. In this operation each grain of the reduced mineral is crushed and forms a little lamella, which places itself perpendicularly to the direction of the pressure. These materials were carefully cut into square prisms, in such a way that the direction of the lamellæ was parallel to the length of the prisms, and at the same time to one of the lateral faces. I suspended these prisms either above a small magnetized bar at the distance of a few centimetres, or over a strong horseshoe magnet; in the latter case the distance was increased to 24 centimetres. Such a distance evidently prevents all objection relative to some difference of thickness which the prisms might have presented, notwithstanding the care taken in their formation.

By means of a watch with a seconds-hand, I counted the duration of 40 oscillations of a determined magnitude. The want of a proper chronometer slightly diminishes the exactitude of my observations; but I have convinced myself that the utmost error of reading was not more than one second. Moreover, the differences upon which the proof of the phænomenon rests, were never less than 12 seconds. The numbers given are the average of several experiments, between which, however, there were but slight differences.

To avoid the influence of a certain quantity of magnetism remaining in the specimen to be investigated, each determination included eight observations made by presenting successively each of the lateral faces of the prism to the magnet, and turning the bar each time.

The specimen which gave me the most marked effects, is a small prism of 21·55 millims. in length by 12·55 millims. in breadth, weighing 16·94 grs.: its density is 6·25. I had a great deal of trouble in cutting it, in consequence of the slight mutual adherence of the lamellæ. I found that the action of the magnet upon this material was stronger when the cleavages were vertical than when they were horizontal. Before annealing, the relation of the forces which caused the bar to oscillate was as 1 : 1·16. After annealing, this relation was notably weaker: it was no more than 1 : 1·09. These numbers are the averages of several determinations made with the large magnet at a distance of 24 centimetres. With a small straight magnet placed at a distance of 2 centimetres only, this relation was always stronger. It was found to be 1 : 1·21 before, and 1 : 1·12 after annealing.

Another prism produced by the reduction of a compact mineral fragment, and also compressed, but in which the lamellated structure was not apparent, furnished differences of the same nature as the preceding, but very slight.

I also operated upon a specimen in which the iron was intermixed with lamellæ of copper. In consequence of the slight aggregation of the material, I was compelled, to enable me to work it, to render the contact of the particles more intimate, by heating it to a high temperature, and submitting it in this state to a powerful pressure. In this way I prepared two pieces: the first was heated to a welding white heat, when the copper was fused. Its density is 7·27. I found no difference in the duration of its oscillations, according as the stratification was vertical or horizontal. The second piece was only brought to a very bright cherry-red heat. Its density is only 6·20; so that the approximation was less intimate than in the preceding case. There is also a considerable difference; the relation of the forces in the two cases being as 1 : 1·047.

It is important to observe, that, at least in accordance with what is generally admitted, these results are independent of the influence of the currents induced in the body which oscillates above the magnet. In fact, the fundamental character of the phænomenon discovered by Arago, of which induction furnishes the explanation, is the rapid diminution of the oscillations, without any sensible alteration of their duration.

It is also remarkable that the arrangement of the cleavages which, in our experiment, developes a more powerful magnetism, is also that which, if we pay attention only to the possible currents of induction in the moveable mass, would give greater energy to these currents.

Lastly, to draw the last and most important consequence from the fact to which I have just referred, we must observe that this influence of the arrangement of the molecules must be intimately connected with magnetism or diamagnetism itself. We find it, in fact, equally sensible with bismuth and iron, although the diamagnetism of the former is about 25,000 times weaker than the magnetism of the latter. We also find that the same circumstance, namely, the vertical position of the cleavages, which increases the diamagnetism of bismuth, also increases the magnetism of iron.—*Comptes Rendus*, October 5, 1857, p. 477.

INDEX TO VOL. XIV.

END OF THE FOURTEENTH VOLUME.

ALERE FLAMMAM.

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RED LION COURT, FLEET STREET.

Fig. 1.

Fig. 2.

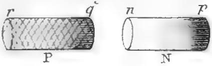


Fig. 3.

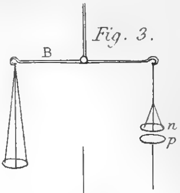


Fig. 4.

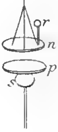


Fig. 5.

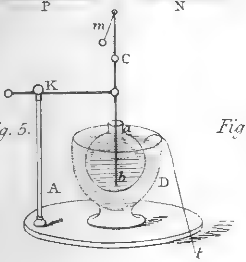


Fig. 6.

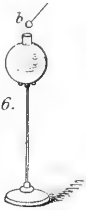
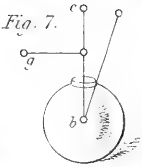


Fig. 7.



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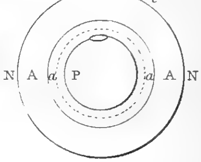

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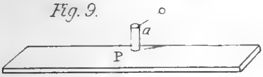
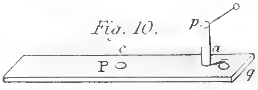


Fig. 10.



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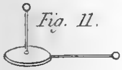

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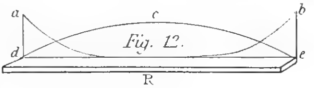


Fig. 13.

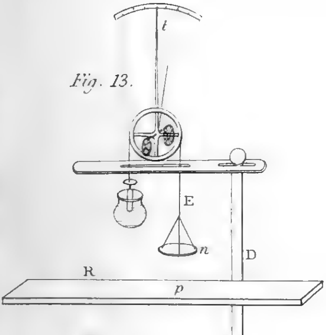
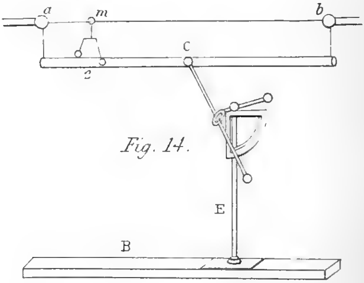


Fig. 14.



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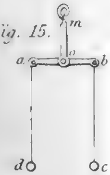
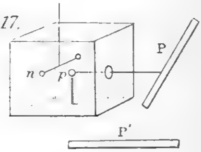


Fig. 16.

Fig. 17.



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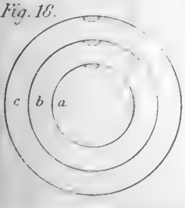

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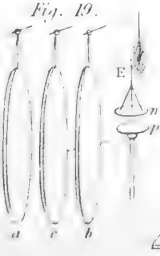
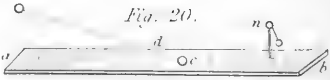


Fig. 20.



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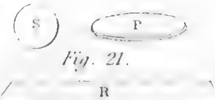

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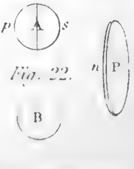

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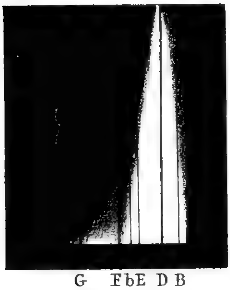
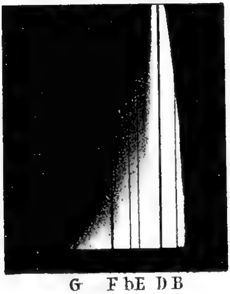
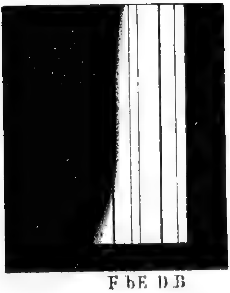
XVII. *Bromide of Copper.*



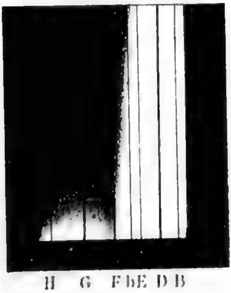
XVIII. *Iodide of Nickel.*



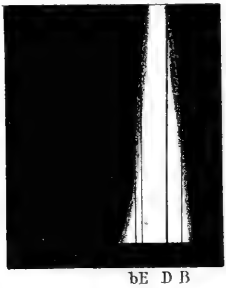
XIX. *Chloride of Gold.*



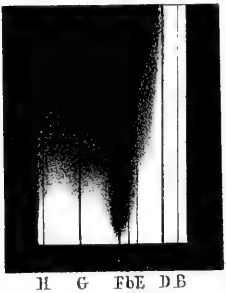
XX. *Chloride of Plat.m*



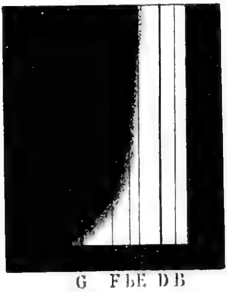
XXI. *Chloride of Copper.*



XXII. *Iodide Plat. & Pot.m*



XXIII. *Chloride of Palladium.*



XXIV. *Chlor. Plat. & Copper.*

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I Chromic Acid

II Uranium Salt

III Permanganate

IV. Ferric Salt

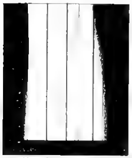
V Chromate of Copper

VI Chromium Salt

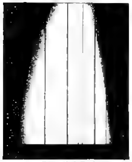
VII Chromate of Chromium

VIII Ferric Chloride

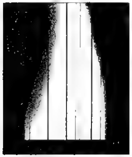
IX Copper Salt



X Nickel Salt



XI Chloride of Nickel



XII. Chloride of Cobalt

XIII Bromine

XIV Iodine

XV Gold Salt

XVI Platinum Salt

XVII Bromide of Copper

XVIII Iodide of Nickel

XIX Chloride of Gold

XX Chloride of Plat^m

XXI Chloride of Copper

XXII Iodide Plat. & Pt.

XXIII Chloride of Palladium

XXIV. Chlor Plat & Copper

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